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

CONDUCTED BY

LORD KELVIN, LL.D. F.R.S. &c.
GEORGE FRANCIS FITZGERALD, M.A. Sc.D. F.R.S.
AND
WILLIAM FRANCIS, Ph.D. F.L.S. F.R.A.S. F.C.S.

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

VOL. XLV.—FIFTH SERIES.
JANUARY—JUNE 1898.

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

SOLD BY SIMPKIN, MARSHALL, HAMILTON, KENT, AND CO., LD.; WHITTAKER AND CO.;
AND BY ADAM AND CHARLES BLACK;—T. AND T. CLARK, EDINBURGH;
SMITH AND SON, GLASGOW;—HODGES, FIGGIS, AND CO., DUBLIN;—
PUTNAM, NEW YORK;—VEUVE J. BOYVEAU, PARIS;—
AND ASHER AND CO., BERLIN.

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

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

J. B. Pinelli ad Mazonium.

ALERE FLAMMAM.

CONTENTS OF VOL XLV.

(FIFTH SERIES).

NUMBER CCLXXII.—JANUARY 1898.

NUMBER CCLXXIII.—FEBRUARY.

NUMBER CCLXXIV.—MARCH.

NUMBER CCLXXV.—APRIL.

NUMBER CCLXXVI.—MAY.

NUMBER CCLXXVII.—JUNE.

PLATES.

I. to X. Illustrative of Mr. C. F. Brush's Paper on the Transmission of Radiant Heat by Gases at Varying Pressures.
XI. to XVIII. Illustrative of Messrs. Michelson and Stratton's Paper on a New Harmonic Analyser.
XIX. Illustrative of Prof. H. A. Rowland's Paper on Electrical Measurement by Alternating Currents.
XX. to XXII. Illustrative of Mr. J. H. Vincent's Paper on the Photography of Ripples.
XXIII. Illustrative of Mr. Thomas Preston's Paper on Radiation Phenomena in the Magnetic Field.
XXIV. Illustrative of Mr. R. W. Wood's Paper on Phase-Reversal Zone-Plates, and Diffraction-Telescopes.

ERRATA.

January Number.

Prof. Nagaoka's paper:

Page 10, last line, *for* $\frac{}{\pi}$ *read* $\frac{2}{\pi}$.
Page 13, line 4, *for* 0·4832 *read* 0·4782.
Page 15, line 12, *for* $(1+2h+2h^4\ldots)$ *read* $(1+2h+2h^4\ldots)^2$.
Page 16, line 1, *for* $\frac{E}{k'}+K$ *read* $\frac{1}{2}\left(\frac{E}{k'}+K\right)$.
Page 16, line 9, *for* 2·0113 *read* 2·0613.

Mr. J. H. Michell's paper:

The twelve lines at the bottom of page 111 should come after line 7 on p. 108.

February Number.

Page 156, as heading of 3rd column of Table. *For* $\frac{1}{4}K_2SO_4$ *read* $\frac{1}{2}K_2SO_4$.

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

[FIFTH SERIES.]

JANUARY 1898.

I. *Diffraction Phenomena in the Focal Plane of a Telescope with Circular Aperture, due to a Finite Source of Light.*

By H. NAGAOKA, *Rigakuhakushi, Professor of Applied Mathematics, Imperial University, Tōkyō*[*].

THE diffraction of a circular aperture has for a long time attracted the attention of physicists, on account of its intimate connexion with the theory of optical instruments. For a point source of light, the problem was treated by Airy[†], Schwerd[‡], and Lommel[§]; the last-mentioned author has brought our knowledge of the subject to the highest state of perfection, both with regard to Fraunhofer's and Fresnel's diffraction phenomena. For practical purposes, however, it is necessary to modify the problem slightly, and investigate the diffraction due to a finite source of light. In connexion with the resolving power, or for the explanation of drop-formation during the transit of inferior planets, the problem was treated by Lord Rayleigh[||], André[¶], Struve[**], and Strehl[††]. In the present paper I give, in the first place, a

---

[*] Communicated by the Author.

[†] Airy, Camb. Phil. Trans. vol. v. p. 283 (1834).

[‡] Schwerd, *Beugungserscheinungen*, Mannheim (1835).

[§] Lommel, *Abhandl. d bayer. Akad.* Bd. xv. (1884).

[||] Rayleigh, Wave-Theory of Light, *Encycl. Brit.*, 9th edition.

[¶] André, *Annales de l'Ecole normale*, [2] tom. v. p. 275 (1876).

[**] Struve, *Mémoires de l'Académie des Sciences de St. Pétersbourg*, tom. xxx. (1882); Wied. *Ann.* Bd. xvii. p. 1008 (1882).

[††] Strehl, *Zeitschrift für Instrumentenkunde*, Bd. xv. p. 362 (1895); Bd. xvi. p. 257 (1896); Bd. xvii. p. 165 (1897).

general discussion of Fraunhofer's diffraction-phenomena of a circular aperture for a finite source of light, and show how the intensity of illumination in the focal plane can be mechanically evaluated for a luminous source having any given shape. I then pass to the consideration of the intensity both inside and outside of a circular image; further, it will be shown how, by the superposition of two systems of lines of equal intensity we can explain the formation of a ligament during the ingress or egress of a dark disk from a luminous source, as verified by the experiments of André and Angot*.

1. *General Expression for the Intensity.*

It is well known that a circular aperture gives rise to a diffraction pattern which, for a point source of light, consists of concentric rings surrounding the image of the luminous point. If, instead of a luminous point, we have a finite source of light, each element of the source will produce similar phenomena; the illumination in the focal plane of the telescope is thus the integral effect of the source of light extending over a given geometrical area. The intensity of the image of a uniform source is not of a simple character, but, as observed in the focal plane of the telescope, will be distributed according to a certain law depending on the shape of the source and the size of the aperture.

Let the circular opening of the telescope be taken for the plane of xy, and denote the cosines of the angles which the incident ray makes with xy-axes by α, β, and those for the diffracted ray by α', β'; then, putting R=radius of the telescope-aperture, λ=wave-length of light,

$$r=\frac{2\pi\sqrt{(\alpha-\alpha')^2+(\beta-\beta')^2}}{\lambda}\mathrm{R},$$

we know that the intensity of the diffracted light in the focal plane of the telescope is proportional to

$$\frac{\mathrm{J}_1{}^2(r)}{r^2},$$

where $\mathrm{J}_1(r)$ is a Bessel function of the first kind and of order 1. If the source of light be not a geometrical point, we must consider α, β as variable in finding the illumination at points corresponding to α', β', and sum the effects due to all the

---

* André et Angot, *Annales de l'Ecole normale,* [2] tom. x. p. 323 (1881).

elements of the luminous source. In other words, the illumination is proportional to

$$\iint \frac{J_1^2(r)}{r^2} d\sigma,$$

where $d\sigma$ represents an element of the luminous area.

For effecting the integration we can conveniently put for nearly normal incidence

$$r = \frac{2\pi RA}{\lambda},$$

where A denotes the angular interval between the incident and the diffracted ray. It is to be remarked that for $\lambda = 0{\cdot}589\,\mu$, $r=1$, and $A=1''$, $R=1{\cdot}93$ centims. Thus, expressed in polar coordinates, r, θ, the present problem reverts to the evaluation of

$$\iint \frac{J_1^2(r)}{r} dr\,d\theta,$$

the integration extending over the whole luminous source. The intensity of illumination at α', β' in the focal plane is consequently given by

$$I = K\iint \frac{J_1^2(r)}{r} dr\,d\theta,$$

K being a constant to be afterwards determined.

Since

$$-\frac{1}{2}\frac{d}{dr}\left(J_0^2(r) + J_1^2(r)\right) = \frac{J_1^2(r)}{r}\text{*},$$

we can write

$$I = -\frac{K}{2}\iint \frac{d}{dr}\left(J_0^2(r) + J_1^2(r)\right) dr\,d\theta.$$

If the luminous source be a plane of infinite extent,

$$I_\infty = K\,.\,\pi,$$

since

$$J_0(0)=1, \quad J_1(0)=0, \quad J_0(\infty)=0, \quad J_1(\infty)=0.$$

We shall henceforth assume the intensity for an infinite source to be unity; thus, on this assumption,

$$K = \frac{1}{\pi}$$

and

$$I = \frac{1}{\pi}\iint \frac{J_1^2(r)}{r} dr\,d\theta. \quad . \quad . \quad . \quad . \quad (\text{I.})$$

---

* See Lommel, *Mathematische Annalen*, Bd. xiv. p. 510 (1879); Rayleigh, Phil. Mag. [5] vol. xi. (1881).

Denoting the limits of integration with respect to r by r_0 and r_1, which are generally functions of θ, we obtain

$$I=\frac{1}{2\pi}\int\left(J_0^2(r_0)+J_1^2(r_0)-J_0^2(r_1)-J_1^2(r_1)\right)d\theta, \quad \text{(II.)}$$

which at the centre of the image of a circular source of radius r reduces to

$$I=1-J_0^2(r)-J_1^2(r), \quad . \quad . \quad . \quad . \quad \text{(II. }a\text{)}$$

and at the vertex of a circular sector including angle α,

$$I=\frac{\alpha}{2\pi}\left(1-J_0^2(r)-J_1^2(r)\right). \quad . \quad . \quad . \quad \text{(II. }b\text{)}$$

2. *On the Curve* $y=J_0^2(x)+J_1^2(x)$.

Before entering into further discussion, it will be worth while to examine the term $J_0^2(r)+J_1^2(r)$, which enters into the expressions for the intensity of the diffracted light. Although the values of $J_0^2(r)$ and $J_1^2(r)$ are in themselves oscillating, the sum of these two functions presents a remarkable aspect, as will be easily seen by representing it as a curve,

$$y=J_0^2(x)+J_1^2(x).$$

In the first place, the curve is confined to the positive part of the ordinate ; further, the relation

$$J_0^2(x)+2J_1^2(x)+2J_2^2(x)+\ldots=1$$

shows that y cannot be greater than 1, which it will attain only for $x=0$.

Since

$$\frac{dy}{dx}=-2\frac{J_1^2(x)}{x}$$

and

$$\frac{d^2y}{dx^2}=-2\frac{J_1^2(x)}{x}\left(2J_0-\frac{3J_1(x)}{x}\right),$$

we see that points corresponding to the roots x_n of $J_1^2(x)=0$ are the points of inflexion, and have tangents parallel to the axis of x. The coordinates of these points are x_n, $J_0^2(x_n)$.

From the nature of the roots of $J_1(x)=0$, we see that these points occur at nearly equal intervals of the abscissæ little greater than π.

In addition to these, we find another series of inflexion-points given by the equation

$$\frac{3J_1(x)}{x}=2J_0(x)\,;$$

or since
$$\frac{2J_1(x)}{x}=J_0(x)+J_2(x),$$

the above equation can also be written
$$\frac{J_0(x)}{J_2(x)}=3.$$

For any value of x, $J_1(x)<1$; the relation to be satisfied at the inflexion-points,
$$\frac{J_1(x)}{J_0(x)}=\frac{2x}{3},$$

shows that for large values of x, $J_0(x)$ must be very small; thus the abscissæ of the inflexion-points belonging to this set will ultimately coincide with the roots of
$$J_0(x)=0.$$

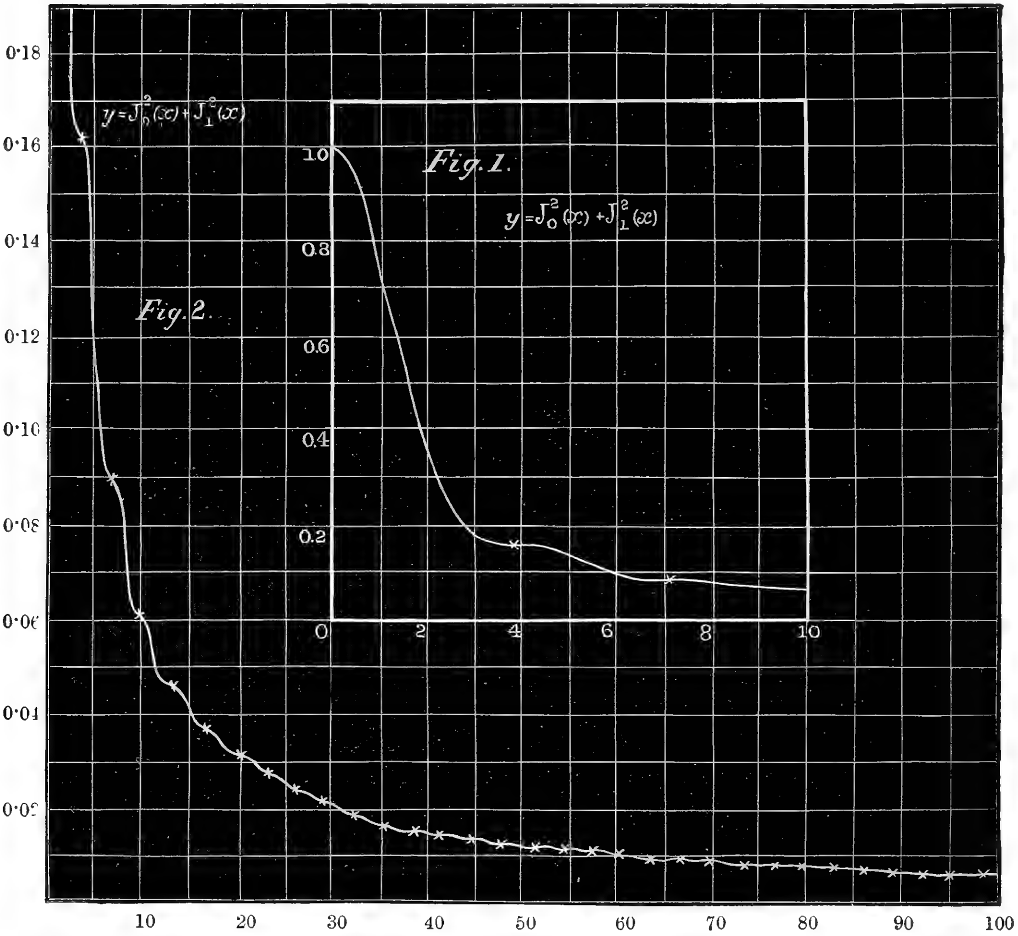
If in the relation given above $J_0(x)$ and $J_1(x)$ be expanded in semiconvergent series, we get the equation
$$\tan\left(x-\frac{\pi}{4}\right)=\frac{8}{11}x-0{\cdot}5392\,.\,\frac{1}{x}+0{\cdot}603\,.\,\frac{1}{x^3}-\dots$$

The ordinates for these inflexion-points are given by
$$y=J_1^2(x)\left(1+\frac{9}{4x^2}\right).$$

We shall distinguish the inflexion-points given by the roots of $J_1(x)=0$ from those given by $\dfrac{J_0(x)}{J_2(x)}=3$, by calling the former inflexion-points of the *first* set, and the latter inflexion-points of the second set. The above considerations show that the inflexion-points of the second set lie nearly midway between those of the first set, the approximation becoming closer for increasing values of x. Further, we conclude that the curve has neither maximum nor minimum (excepting the point $x=0$, $y=1$), but has an infinite number of inflexion-points (of the first set) where the tangents are parallel to the x-axis.

Between the inflexion-points of the two sets, there must be places of maximum curvature; these points are given by
$$J_0^2(x)-J_1^2(x)-\frac{5J_0(x)J_1(x)}{x}-\frac{2J_1^2(x)}{x^2}\left[3+2J_1^4(x)+4J_0^2(x)J_1^2(x)\right]$$
$$+16\frac{J_0(x)J_1^5(x)}{x^3}-51\frac{J_1^6(x)}{x^4}=0.$$

If x be large, the leading term is evidently $J_0^2(x)-J_1^2(x)$, while the remaining terms decrease very rapidly with increasing values of x; thus the places of maximum curvature will be approximately given by $J_0(x) = \pm J_1(x)$. This equation shows that these points lie nearly midway between the inflexion-points of the first and of the second set. Thus, if the successive inflexion-points of the first set be joined by a series of straight lines, one part of the curve will lie to the right and the other to the left of the line; the points of intersection of the line with the curve will, for large values of x, lie nearly midway between the two points, and nearly coincident with the inflexion-points of the second set.



Representing the curve graphically, it will show a succession of steps, at nearly equal horizontal intervals; the height of the consecutive steps becomes smaller, and the rate of decrease diminishes, as we recede further from the axis of y. Fig. 1 shows the curve from $x=0$ to $x=10$; for larger values of x, the curve is given in fig. 2. The positions of the inflexion-points of the first set are shown by the asterisks.

3. *Different Expressions for* $J_0^2(x)+J_1^2(x)$.

Different expressions can be found for y; for small values of x, we may expand it in powers of x, by means of well-known definite integrals :—

$$J_0^2(x)=\frac{1}{\pi}\int_0^\pi J_0(2x\sin\omega)\,d\omega,$$

$$J_1^2(x)=\frac{1}{\pi}\int_0^\pi J_0(2x\sin\omega)\cos 2\omega\,d\omega,$$

or

$$J_0^2(x)+J_1^2(x)=\frac{2}{\pi}\int_0^\pi J_0(2x\sin\omega)\cos^2\omega\,d\omega.$$

By expanding $J_0(2x\cos\omega)$ in powers of $2x\cos\omega$, and integrating, we obtain

$$J_0^2(x)+J_1^2(x)=\Sigma\frac{(-1)^n\,\Pi(2n)}{2^{2n+1}(\Pi(n))^3\,\Pi(n+1)}\cdot x^{2n}. \quad . \quad . \quad \text{(A)}$$

The ascending series converges very slowly for large values of x; we may conveniently employ the following semi-convergent series, which can be easily found from the corresponding expansions for $J_0(x)$ and $J_1(x)$. Thus,

$$J_0^2(x)=\frac{1}{\pi x}\left(1-\frac{1}{8x^2}+\sin 2x-\frac{\cos 2x}{4x}-\frac{5\sin 2x}{32x^2}\right),$$

$$J_1^2(x)=\frac{1}{\pi x}\left(1+\frac{3}{8}\frac{1}{x^2}-\sin 2x-\frac{3}{4}\frac{\cos 2x}{x}-\frac{3}{32}\cdot\frac{\sin 2x}{x^2}\right),$$

whence

$$J_0^2(x)+J_1^2(x)=\frac{2}{\pi x}\left(1+\frac{1}{8x^2}-\frac{\cos 2x}{2x}-\frac{\sin 2x}{8x^2}\right). \quad . \quad . \quad \text{(B)}$$

The above series (B) is rapidly convergent, and can be conveniently used for values of x greater than the first root $x_1=3{\cdot}8317$ of $J_1(x)=0$; at the last-mentioned value of x, the number obtained for $J_0^2(x)+J_1^2(x)$ will be accurate to the fourth decimal place.

In the neighbourhood of the inflexion-points of the first set, y remains nearly constant; we can thus expand $J_0^2(x)+J_1^2(x)$ in Maclaurin's series. Denoting the roots of $J_1(x)=0$ by $x_0, x_1, \ldots\ldots$, and putting

$$y_n=J_0^2(x_n), \quad \xi=x-x_n,$$

we shall obtain the following series for y in the neighbourhood of the point x_n, y_n :—

$$y=J_0^2(x_n)\left[1-\frac{4}{3!\,x_n}\cdot\xi^3+\frac{12}{4!}\frac{\xi^4}{x_n^2}-4\left(\frac{39}{x_n^2}-4\right)\frac{\xi^5}{5!\,x_n}+\ldots\right]. \quad \text{(C)}$$

Of these three expressions (A), (B), (C), we shall have occasion to use the second form (B) most frequently, as it expresses the nature of the curve $y = J_0^2(x) + J_1^2(x)$ in the simplest manner. It shows that the curve is to the first approximation a rectangular hyperbola,

$$xy = \frac{2}{\pi}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (D)$$

if x be not small. To the second approximation, we have to introduce the term $\frac{\cos 2x}{\pi x^2}$, which gives the curve an undulating appearance; the effect of the third and fourth terms is still smaller, so that for practical purposes it is sufficient to assume the mean curve to be an hyperbola, as it is very tedious to push the calculation to the fourth decimal place. To show the difference in y_n calculated from (B) and (D), I give the following table :—

$x_1 = 3{\cdot}832$,	$y_1 = 0{\cdot}1622$	by (B),	$y_1 = 0{\cdot}1661$	by (D).
$x_2 = 7{\cdot}016$,	$y_2 = 0{\cdot}0921$	”	$y_2 = 0{\cdot}0907$	”
$x_3 = 10{\cdot}173$,	$y_3 = 0{\cdot}0624$	”	$y_3 = 0{\cdot}0626$	”
$x_4 = 13{\cdot}324$,	$y_4 = 0{\cdot}0477$	”	$y_4 = 0{\cdot}0478$	”

Thus the coincidence becomes closer with increasing values of x.

4. *Intensity at the Centre of a Circular Image.*

Equation (II. a) shows that the intensity of light at the centre of a luminous disk as seen through a telescope is given by

$$\begin{aligned} I(r) &= 1 - J_0^2(r) - J_1^2(r) \\ &= 1 - y. \end{aligned}$$

If the disk be divided by a series of concentric circles of radius x_n into a number of zones, whose breadth is equal to the difference between the successive roots of $J_1(x) = 0$, we find that the illumination at the centre due to each of these zones is given by the height of the corresponding step in the curve $y = J_0^2(x) + J_1^2(x)$. The diminution in the height of these steps, with increasing x, shows that the effect of the zone of nearly the same breadth varies almost inversely proportional to its distance from the centre. The same reasoning will apply to a circular sector.

The following table gives the intensity at the centre of the luminous disk whose radius is equal to the root of $J_1(r) = 0$:—

n.	r_n.	$\mathrm{I}(r_n)$.	$\mathrm{I}(r_n)-\mathrm{I}_n(r_{n-1})$.
0.	0·00000	0·00000	——
1.	3·83171	0·83778	0·83778
2.	7·01559	0·90994	0·07216
3.	10·17347	0·93765	0·02771
4.	13·32369	0·95232	0·01467
5.	16·47063	0·96140	0·00908
6.	19·61586	0·96758	0·00618
7.	22·76008	0·97205	0·00447
8.	25·90367	0·97544	0·00339
9.	29·04683	0·97809	0·00265
10.	32·18968	0·98023	0·00214
20.	63·61136	0·98999	0·00052
30.	95·02923	0·99330	0·00023
40.	126·44614	0·99497	0·00013
50.	157·86266	0·99597	0·00008

It will be seen from the table that the effect of the first zone is five times greater than all the rest combined. Thus the illumination at the centre of a disk of small radius will not sensibly differ from that of infinite extent. For values of r less than r_1 we have the following table :—

r.	$\mathrm{I}(r)$.	Difference.	r.	$\mathrm{I}(r)$.	Difference.
0·0	0·00000	0·00995	2·0	0·61726	0·06147
0·2	0·00995	0·02926	2·2	0·67873	0·05077
0·4	0·03921	0·04684	2·4	0·72950	0·03946
0·6	0·08605	0·06170	2·6	0·76896	0·02894
0·8	0·14775	0·07412	2·8	0·79790	0·01951
1·0	0·22187	0·07942	3·0	0·81741	0·01177
1·2	0·30129	0·08368	3·2	0·82918	0·00600
1·4	0·38497	0·08286	3·4	0·83518	0·00220
1·6	0·46783	0·07842	3·6	0·83738	0·00040
1·8	0·54625	0·07101	3·8	0·83778	

These two tables enable us to integrate mechanically the expression (II.),

$$\mathrm{I}=\frac{1}{2\pi}\int\left(\mathrm{J}_0^2(r_0)+\mathrm{J}_1^2(r_0)-\mathrm{J}_0^2(r_1)-\mathrm{J}_1^2(r_1)\right)d\theta,$$

for a source of any given shape. About a point at which the intensity is sought, describe a number of concentric circles dividing the region of integration into a series of zones whose breadths are equal to the differences between the successive roots of $\mathrm{J}_1(r)=0$. If the angle subtended at the point by these zones be known, we can, by summation of these separate effects, find the intensity at the required point. If the luminous source subtend several seconds of arc, the value of r will be a large quantity. Thus, if the boundary does not show great irregularities, we can approximately assume the bounding edge to be straight for contiguous zones. If the mean angle α_n subtended by the zone at the given point be found, the intensity

$$I=\frac{1}{2\pi}\Sigma\alpha_n(I(r_n)-I(r_{n-1}))\text{ approximately.}$$

We notice that for the first zone, $I(r_1)-I(r_0)=0{\cdot}8378$, while for all the rest $I(r_\infty)-I(r_1)=0{\cdot}1622$; it is thus necessary to subdivide the first zone into a series of subsidiary zones and sum their effect as for the other zones, for which the second table will be of use. In addition to this, we shall have to add a small correction for a few zones near the given point, inasmuch as we multiply the height of the step by the arithmetical mean of the bounding angles of zones, instead of taking some other proper value of a_n, which will leave no error during the mechanical integration. The calculation of the correction will be simplified by assuming the zones in the manner above described.

5. *Intensity at the Rim of a circular Disk.*

We have already seen how the intensity at the centre of a circular disk can be calculated; we shall now proceed to the discussion of the intensity at the rim of the disk, and then obtain the result for a general case, when the point lies inside or outside the disk.

Let the radius of the circular source be a; then the distance of a point on the periphery of the circle will be

$$r=2a\cos\theta.$$

The intensity at the rim $r=0$ is given by

$$I=\frac{2}{\pi}\int_0^{\frac{\pi}{2}}\int^{2a\cos\theta}\frac{J_1^2(r)}{r}\,dr\,d\theta.$$

Integrating it at first with respect to θ, we can bring the integral under the form

$$I=\frac{2}{\pi}\int_0^{2a}\int_0^{\cos^{-1}\frac{r}{2a}}\frac{J_1^2(r)}{r}\,dr\,d\theta,$$

$$=\frac{}{\pi}\int_0^{2a}\frac{J_1^2(r)}{r}\cos^{-1}\frac{r}{2a}\,dr.$$

Integrating by parts,

$$I = \frac{1}{2} - \frac{1}{\pi}\int_0^{2a} \frac{J_0^2(r)+J_1^2(r)}{\sqrt{4a^2-r^2}}\,dr,$$

or

$$= \frac{1}{\pi}\int_0^{2a} \frac{(1-J_0^2(r)-J_1^2(r))dr}{\sqrt{4a^2-r^2}}.$$

The above integral shows that the intensity at the rim approaches the value $\frac{1}{2}$, as the radius of the disk is indefinitely increased.

We have already seen how $J_0^2(r)+J_1^2(r)$ can be expanded in ascending as well as descending powers of r. For values of r smaller than x_1 the semiconvergent series cannot be used, while for values of r somewhat greater than x_1 it is disadvantageous to use the the ascending series. We shall, therefore, have to divide the integral into two parts: namely, one extending from 0 to x_1, for which the expansion in ascending powers of r should be used; and from x_1 to $2a$, for which the semiconvergent series should be employed.

It is to be noticed that the variation of $J_0^2(r)+J_1^2(r)$ is very small in the neighbourhood of $r=x_1$, so that it would be advantageous to fix the limit of integration at $r=x_1(=3{\cdot}8317)$, in order to diminish the error in integration.

Thus,

$$I = \frac{1}{\pi}\int_0^{x_1}(1-J_0^2(r)-J_1^2(r))\frac{dr}{\sqrt{4a^2-r^2}}$$

$$+\frac{1}{\pi}\int_{x_1}^{2a}(1-J_0^2(r)-J_1^2(r))\frac{dr}{\sqrt{4a^2-r^2}}.$$

For practical application, a is generally large compared to x_1; we shall therefore assume $a>x_1$; then

$$\int_0^{x_1}(1-J_0^2(r)-J_1^2(r))\frac{dr}{\sqrt{4a^2-r^2}}$$

$$= \frac{1}{2a}\int_0^{x_1}(1-J_0^2(r)-J_1^2(r))\left(1-\frac{1}{2}\cdot\frac{r^2}{4a^2}+\frac{1.3}{2.4}\cdot\frac{r^4}{(4a^2)^2}-\ldots\right)dr$$

$$= \frac{0{\cdot}9615}{a}+\frac{1{\cdot}579}{a^3}+\frac{1{\cdot}543}{a^5}+\ldots\ldots,$$

which converges very rapidly when a is large.

In the second integral we have to put

$$J_0^2(r)+J_1^2(r)=\frac{2}{\pi r}\left(1-\frac{\cos 2r}{2r}+\frac{1-\sin 2r}{8r^2}\right).$$

Thus we shall have to calculate the following integrals :—

$$\int_{x_1}^{2a}\frac{dr}{\sqrt{4a^2-r^2}}=\frac{\pi}{2}-\sin^{-1}\frac{x_1}{2a};$$

$$\int_{x_1}^{2a}\frac{dr}{r\sqrt{4a^2-r^2}}=-\frac{1}{2a}\log\tan\frac{\theta_1}{2},\quad\text{where } \theta_1=\sin^{-1}\frac{x_1}{2a};$$

$$\int_{x_1}^{2a}\frac{dr}{r^3\sqrt{4a^2-r^2}}=\frac{1}{8a^3}\cdot\frac{\cot\theta_1}{\sin\theta_1}-\frac{1}{8a^3}\log\tan\frac{\theta_1}{2};$$

$$\int_{x_1}^{2a}\frac{\cos 2r\,dr}{r^2\sqrt{4a^2-r^2}}=\frac{1}{2}\sqrt{4a^2-x_1^2}\,\frac{\cos 2x_1}{x_1^3}+a\int\sqrt{1-\frac{r^2}{4a^2}}\,d\left(\frac{\cos 2r}{r^3}\right)$$

$$=-\frac{0{\cdot}01367}{a}-\frac{3}{32}\frac{\cos 4a}{a^2}\text{ nearly};$$

$$\int_{x_1}^{2a}\frac{\sin 2r\,dr}{r^3\sqrt{4a^2-r^2}}=\frac{0{\cdot}00322}{a}-\frac{5}{16}\frac{\cos 4a}{a^2}\text{ nearly.}$$

Arranging the integrals in a suitable way and writing

$$\frac{1}{\pi}\sin^{-1}\frac{x_1}{2a}=\frac{0{\cdot}60984}{a}-\frac{0{\cdot}3731}{a^3}+\frac{0{\cdot}6162}{a^5}-\dots,$$

$$\log\tan\tfrac{1}{2}\theta_1=\log x_1-\log 4a-\frac{x_1^2}{48a^2}-\frac{3}{128}\frac{x_1^4}{a^4}\dots,$$

we find for the intensity at the rim of a circular disk whose radius is large compared to x_1

$$I_R=\frac{1}{2}-\frac{\log 4a}{\pi^2 a}-\frac{0{\cdot}1654}{a}+0{\cdot}016\frac{\cos 4a}{a^2}\text{ nearly.}\quad\text{(III.)}$$

As was before remarked, the intensity at the rim approaches $\frac{1}{2}$ with increasing values of a; it is, moreover, seen how the fluctuation due to the term $\frac{\cos 4a}{a^2}$ is negligibly small. By applying the above formula, the following table was calculated for $a>20$:—

a.	I_R.	a.	I_R.
20	0·4695	250	0·4965
25	0·4747	300	0·4971
30	0·4832	400	0·4977
35	0·4809	500	0·4981
40	0·4830	600	0·4984
45	0·4846	700	0·4986
50	0·4860	800	0·4988
60	0·4880	900	0·4989
70	0·4895	1000	0·4990
80	0·4906	1500	0·4993
90	0·4915	2000	0·4995
100	0·4923	2500	0·4996
150	0·4946	3000	0·4996
200	0·4958	∞	0·5000

6. *Intensity at any Point of the Focal Plane due to a Circular Source of Light.*

Let the intensity of light be required at any point O in the focal plane of a telescope, where the point O may lie either external or internal to the image of the circular source (see figs. 3 and 4).

Fig. 3.

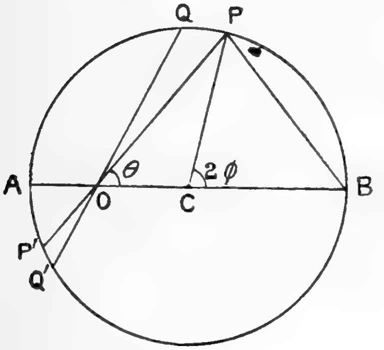
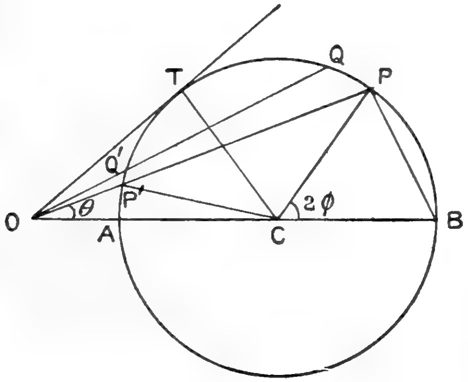


Fig. 4.



Put ∠BCP $=2\phi$; ∠BOP $=\theta$; AO $=\delta$; then

$$OP^2=\rho^2=(a\mp\delta)^2+a^2+2a(a\mp\delta)\cos 2\phi$$
$$=(2a\mp\delta)^2-4a(a\mp\delta)\sin^2\phi\,;$$

when O lies within the circle,

$$\rho_i=(2a-\delta)\sqrt{1-k_i^2\sin^2\phi},\quad\text{where } k_i^2=\frac{4a(a-\delta)}{(2a-\delta)^2}\,;$$

and when O lies external to the circle

$$\rho_e = (2a+\delta)\sqrt{1-k_e^2\sin^2\phi}, \quad \text{where } k_e^2 = \frac{4a(a+\delta)}{(2a+\delta)^2}.$$

Thus k^2 is always less than 1, whether the point be inside or outside the circle.

We can easily prove that

$$d\theta = \left(1 + \frac{\delta}{(2a-\delta)(1-k_i^2\sin^2\phi)}\right)d\phi\,*$$

$$= \left(1 + \frac{k_i'}{1-k_i^2\sin^2\phi}\right)d\phi \text{ for an internal point,}$$

and

$$d\theta = \left(1 - \frac{k_e'}{1-k_e^2\sin^2\phi}\right)d\phi \text{ for an external point,}$$

k_i' and k_e' being defined by

$$k_i' = \sqrt{1-k_i^2} = \frac{\delta}{2a-\delta},$$

$$k_e' = \sqrt{1-k_e^2} = \frac{\delta}{2a+\delta}.$$

The intensity at an internal point becomes

$$I_i = \frac{1}{\pi}\int_0^{2\pi}\int_0^{\rho}\frac{J_1^2(r)}{r}\,dr\,d\theta$$

$$= 1 - \frac{1}{\pi}\int_0^{\frac{\pi}{2}}\left(J_0^2(\rho_i) + J_1^2(\rho)\right)\left(1 + \frac{k_i'}{1-k_i^2\sin^2\phi}\right)d\phi.$$

For an external point we get

$$I_e = \frac{1}{\pi}\int_{-\alpha}^{+\alpha}\int_{\rho'}^{\rho}\frac{J_1^2(r)}{r}\,dr\,d\theta,$$

where $\rho' = OP'$, $\rho = OP$, and $\alpha = \angle AOT$.

Putting the values of ρ_e and $d\theta$, we obtain

$$I_e = -\frac{1}{\pi}\int_0^{\pi}\left(J_0^2(\rho_e) + J_1^2(\rho_e)\right)\left(1 - \frac{k_e'}{1-k_e^2\sin^2\phi}\right)d\phi.$$

If the point O be not very near the rim of the circle, we can put

$$J_0^2(\rho) + J_1^2(\rho) = \frac{2}{\pi\rho} \text{ nearly,}$$

---

* See Halphen, *Traité des Fonctions Elliptiques*, tom. i.

whence it follows that

$$\mathrm{I}_i=1-\frac{2}{\pi^2(2a-\delta)}\int_0^{\frac{\pi}{2}}\left(1+\frac{k_i'}{1-k^2\sin^2\phi}\right)\frac{d\phi}{\sqrt{1-k^2\sin^2\phi}},$$

$$=1-\frac{2}{\pi^2(2a-\delta)}\left(\mathrm{K}+\frac{\mathrm{E}}{k_i'}\right), \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (\mathrm{IV.})^*$$

Similarly

$$\mathrm{I}_e=\frac{2}{\pi^2(2a+\delta)}\left(-\mathrm{K}+\frac{\mathrm{E}}{k_e'}\right). \quad . \quad . \quad (\mathrm{IV}.\,a)$$

Putting

$$l=\frac{1-\sqrt{k'}}{1+\sqrt{k'}}, \qquad l_1=\frac{1-\sqrt{k}}{1+\sqrt{k}}, \qquad h=e^{-\pi\frac{\mathrm{K}'}{\mathrm{K}}},$$

we have

$$h=\frac{l}{2}+2\left(\frac{l}{2}\right)^{\frac{5}{2}}+15\left(\frac{l}{2}\right)^9+\ . \ . \ . \ . \ ,$$

$$h_1=\frac{l_1}{2}+2\left(\frac{l_1}{2}\right)^{\frac{5}{2}}+15\left(\frac{l_1}{2}\right)^9+\ . \ . \ . \ . \ , \qquad \log h\,.\log h_1=\pi^2,$$

and

$$\mathrm{K}=\frac{\pi}{2}\,(1+2h+2h^4+2h^9+\ . \ . \ . \ .)\,;$$

further

$$\mathrm{E}=k'^2\mathrm{K}+\frac{2\pi^2}{\mathrm{K}}\cdot\frac{h+4h^4+9h^9+\ . \ . \ .}{1+2h+2h^4+2h^9+\ . \ . \ .},$$

or

$$\mathrm{E}=\frac{\pi^2}{4\mathrm{K}}\cdot\frac{1+3^2\,.\,h^{1\cdot2}+5^2\,.\,h^{2\cdot3}+7^2\,.\,h^{3\cdot4}+\ . \ . \ .}{1+h^{1\cdot2}+h^{2\cdot3}+h^{3\cdot4}+\ . \ . \ .}\dagger,$$

whence the intensity of illumination for both cases can be easily calculated.

The following table gives the values of $\mathrm{K}+\frac{\mathrm{E}}{k'}$ and $\frac{\mathrm{E}}{k'}-\mathrm{K}$:—

---

* In the expansion of $\mathrm{J}_0{}^2(\rho)+\mathrm{J}_1\,(\rho)$ in semiconvergent series, only the first term was retained; the effect of the succeeding terms after the integration is almost insignificant.

† See Weierstrass-Schwarz, *Formeln und Lehrsätze zum Gebrauch der elliptischen Functionen*, 2te Auflage, Berlin, 1893, pp. 44, 61.

k.	$\frac{E}{k'}+K$.	$\frac{E}{k'}-K$.
0·1	1·5748	0·0000
0·2	1·5869	0·0001
0·3	1·6084	0·0009
0·4	1·6416	0·0031
0·5	1·6901	0·0088
0·6	1·7516	0·0196
0·7	1·8727	0·0511
0·8	2·0113	0·1328
0·9	2·4848	0·4080

The formula given above applies only to points far from the rim of the circular image. The most interesting case connected with the present problem is the investigation of the intensity in the very neighbourhood of the rim, where the well-known phenomenon of drop-formation makes its appearance. As the semiconvergent expansion for $J_0^2(\rho) + J_1^2(\rho)$ is no longer allowable in the vicinity of the rim, we must have recourse to another method of integration for that portion of the region, where $J_0^2(\rho) + J_1^2(\rho)$ must be expanded according to ascending powers of ρ.

We shall divide the region of integration into two parts by describing a circle with radius x_1 about the point O (figs. 5 and 6), where the intensity is sought; at points of the periphery not included within the circle thus described, we can use the semiconvergent series for $J_0^2(\rho) + J_1^2(\rho)$, and apply the method of integration given above, while in the interior we must have recourse to the following mode of procedure.

Fig. 5.

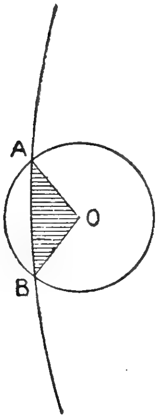
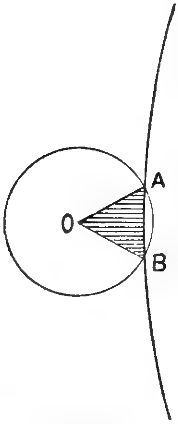


Fig. 6.



As the point lies very near the periphery, δ is a small quantity and k very near unity; whence, if ρ be expressed in terms of ϕ, ϕ is nearly a right angle for points of the periphery AB included within the circle.

Let

$$\phi = \frac{\pi}{2} - \psi,$$

then

$$1 - k^2 \sin^2 \phi = \sqrt{k'^2 + k^2\psi^2 - k^2\frac{\psi^4}{3}} \text{ nearly.}$$

Denoting the angle subtended by AB at the centre of the circular image by $4\psi_1$, we obtain by the formula already given

$$\left(\frac{x_1}{2a-\delta}\right)^2 = k'^2 + k^2\left(\psi_1{}^2 - \frac{\psi_1{}^4}{3}\right).$$

Expressing k' in terms of a and δ, we obtain

$$\psi_1{}^2 - \frac{\psi_1{}^4}{3} = \frac{x_1{}^2 - \delta^2}{4a(a-\delta)}.$$

Practically, a is very large compared to x_1, and ψ_1 is a very small angle, so that we may safely neglect $\frac{\psi_1{}^4}{3}$ and put

$$\psi_1{}^2 = \frac{x_1{}^2 - \delta^2}{4a(a-\delta)},$$

and for points on the periphery AB

$$\rho = (2a-\delta)\sqrt{k'^2 + k^2\psi^2}.$$

Putting

$$\xi = \sqrt{4a(a-\delta)}\,\psi, \quad \xi_1 = \sqrt{4a(a-\delta)}\,\psi_1 = x_1{}^2 - \delta^2 \text{ nearly,}$$

we have

$$\rho = \sqrt{\delta^2 + \xi^2},$$

$$d\phi = -d\psi = \frac{-d\xi}{\sqrt{4a(a-\delta)}}.$$

Returning to formula (II.) we find the intensity at O due to the sector OAB,

$$\begin{aligned} \mathrm{I}_s &= \frac{1}{\pi}\int_0^\alpha \left(1 - \mathrm{J}_0{}^2(\rho) - \mathrm{J}_1{}^2(\rho)\right) d\theta \\ &= \frac{\alpha}{\pi} - \frac{1}{\pi}\int_0^\alpha \left(\mathrm{J}_0{}^2(\rho) + \mathrm{J}_1{}^2(\rho)\right) d\theta, \end{aligned}$$

where 2α is the angle subtended by AB at O.

Replacing ρ and θ by the values already given, we obtain for an internal point

$$\mathrm{I}_s = \frac{\alpha}{\pi} - \frac{1}{\pi}\int_0^{\xi_1} \Big(\mathrm{J}_0^2(\sqrt{\delta^2+\xi^2}) + \mathrm{J}_1^2(\sqrt{\delta^2+\xi^2})\Big)\left(1+\frac{\delta(2a-\delta)}{\delta^2+\xi^2}\right)\frac{d\xi}{\sqrt{4a(a-\delta)}}$$

$$= \frac{\alpha}{\pi} - \frac{\mathrm{I}_s'}{2\sqrt{a(a-\delta)}} - \mathrm{I}_s'' \text{ nearly,}$$

where

$$\mathrm{I}_s' = \frac{1}{\pi}\int_0^{\xi_1} [\mathrm{J}_0^2(\sqrt{\delta^2+\xi^2}) + \mathrm{J}_1^2(\sqrt{\delta^2+\xi^2})]\, d\xi,$$

$$\mathrm{I}_s'' = \frac{\delta}{\pi}\int_0^{\xi_1} \frac{\mathrm{J}_0^2(\sqrt{\delta^2+\xi^2}) + \mathrm{J}_1^2(\sqrt{\delta^2+\xi^2})}{\delta^2+\xi^2}\, d\xi.$$

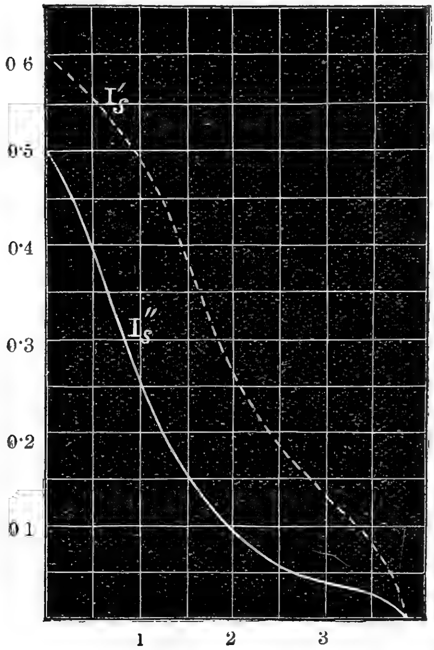
When the point O is external to the circle,

$$-\mathrm{I}_{s'} = \frac{\alpha}{\pi} - \frac{\mathrm{I}_s'}{2\sqrt{a(a+\delta)}} + \mathrm{I}_s'' \text{ nearly.}$$

Fig. 7.

$$y = \frac{1}{\pi}\int_0^{\sqrt{x_1^2-\delta^2}} (\mathrm{J}_0^2(\sqrt{\delta^2+\xi^2}) + \mathrm{J}_1^2(\sqrt{\delta^2+\xi^2}))\, d\xi,$$

$$x = \delta.$$



$$y = \frac{\delta}{\pi}\int_0^{\sqrt{x_1^2-\delta^2}} \frac{\mathrm{J}_0^2(\sqrt{\delta^2+\xi^2}) + \mathrm{J}_1^2(\sqrt{\delta^2+\xi^2})}{\delta^2+\xi^2}\, d\xi.$$

I have calculated these two integrals I_s' and I_s'' by mechanical quadrature, for which Gauss's method seems specially suited. The course of I_s' and I_s'' is shown in fig. 7.

The effect due to the remaining part of the circular source can be easily calculated by assuming the semiconvergent series for $J_0^2(\rho)+J_1^2(\rho)$. Retaining the first term of the series only, we get for the illumination at an internal point O due to the part external to the sector AOB,

$$I_i' = \frac{\pi-\alpha}{\pi} - \frac{2}{\pi^2(2a-\delta)} \int_0^{\phi_1} \left(1+\frac{k_i'}{1-k_i^2 \sin^2\phi}\right) \frac{d\phi}{\sqrt{1-k^2\sin^2\phi}}$$

$$= \frac{\pi-\alpha}{\pi} - \frac{2}{\pi^2(2a-\delta)} \left(F(\phi_1) + \frac{E(\phi_1)}{k_i'} - \frac{k_i^2}{2k_i'} \frac{\sin 2\phi_1}{\sqrt{1-k_i^2\sin^2\phi_1}} \right),$$

where F and E denote elliptic integrals of the 1st and 2nd kind respectively.

For an external point we shall have

$$I_e' = \frac{\alpha}{\pi} + \frac{2}{\pi^2(2a-\delta)} \left(F(\phi_1) - \frac{E(\phi_1)}{k_e'} + \frac{k_e^2}{2k_e'} \frac{\sin 2\phi_1}{\sqrt{1-k_e^2\sin^2\phi_1}} \right).$$

Thus we obtain, for the total intensity of illumination at an internal point in the very neighbourhood of the rim,

$$I_i = 1 - \frac{2}{\pi^2(2a-\delta)} \left(F(\phi_1) + \frac{E(\phi_1)}{k_i'} - \frac{k_i^2}{2k_i} \frac{\sin 2\phi_1}{\sqrt{1-k_i^2\sin^2\phi_1}} \right) - \frac{I_s'}{2\sqrt{a(a-\delta)}} - I_s''. \quad . \quad . \quad (V.)$$

At an external point,

$$I_e = \frac{2}{\pi^2(2a-\delta)} \left(F(\phi_1) - \frac{E(\phi_1)}{k_e'} + \frac{k_e^2}{2k_e'} \frac{\sin 2\phi_1}{\sqrt{1-k_e^2\sin^2\phi_1}} \right) + \frac{I'}{2\sqrt{a(a-\delta)}} - I_s''. \quad . \quad . \quad (V.a)$$

The expression within the bracket can be calculated by means of Legendre's table; the course of the function for different values of ϕ and δ is shown in fig. 8. The curves on the right-hand side apply to I_i, and those on the left to I_e.

Having found the values of these different integrals, we can now discuss the illumination near the rim of the circular image.

We have already found that the intensity of light at the centre of a circular disk is nearly equal to 1, when the diameter is tolerably large, and at the rim, nearly equal

Fig. 8.

$$y = \frac{2}{\pi^2}\left\{F(\phi) + \frac{E(\phi)}{k'} - \frac{k^2 \sin 2\phi}{2k'\sqrt{1-k^2\sin^2\phi}}\right\}.$$

$$x = \frac{\delta}{2a}.$$

$$k^2 = \frac{4a(a-\delta)}{(2a-\delta)^2},\ k' = \frac{\delta}{2a-\delta}.$$

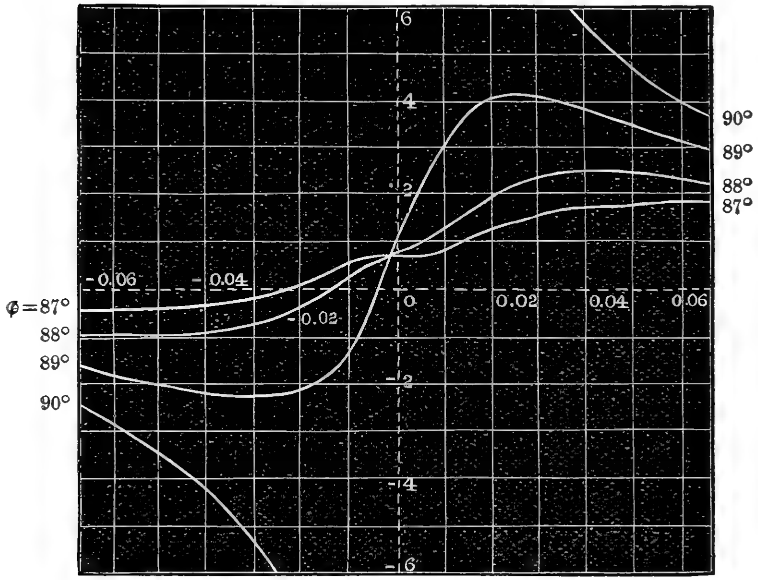
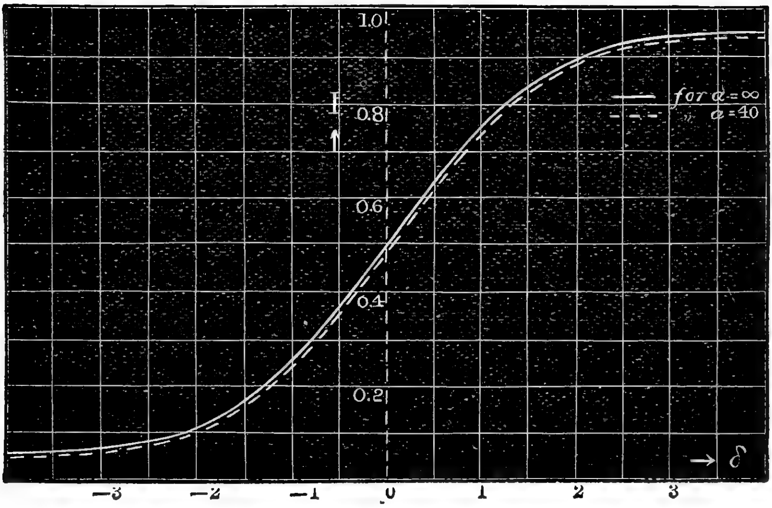


Fig. 9.



to half the intensity at the centre. As will be seen from the expression for I_i, the intensity increases very rapidly from the rim towards the centre; and from that for I_e, the decrease takes place very rapidly as we pass from the rim outwards. Thus the variation of intensity is greatest near the rim, but the change does not take place abruptly; the intensity fades away gradually in the neighbourhood of the rim, as illustrated in the accompanying diagram[*] (fig. 9). The full line is for $a=\infty$, and the dotted line for $a=40$.

The image of a luminous disk, as seen through a telescope, is thus not sharply defined at the geometrical rim, where the change of intensity takes place continuously. If the intensity for the limit of visibility be less than I_R, the image of the disk will appear to a slight extent broadened.

7. *Lines of Equal Intensity.*

For practical purposes it is sometimes convenient to draw the lines of equal intensity. For a circular source of light they consist of a series of concentric circles, which, if drawn for equal difference of intensity, crowd together near the geometrical edge.

When there are different sources of light we can superpose the separate effects and graphically represent the distribution of illumination in the following manner:—

Draw the lines of equal intensity for the image of each source; at the point of intersection of any two lines the intensity will be the sum of the two. We thus obtain a system of points of equal intensity. By drawing the lines at small intervals we can obtain a sufficient number of points to draw curves of equal intensity, which will represent the distribution of illumination due to different sources. In fact, the process of drawing the lines of equal intensity is analogous to that of drawing equipotential lines.

Suppose that the luminous source is a circular disk, on which there is a small dark circular space touching the rim of the disk. The image, as seen through a telescope, will form a drop, as the following consideration of the lines of equal intensity will show.

We can imagine such a source to be produced by the superposition of two different sources, one of which consists of a circular disk of uniform intensity without any dark space, while the other consists of a circular disk occupying the place of the dark space, and of such an intensity that it is equal but of *opposite* sign. This consideration immediately gives the

---

[*] The curve will in reality show minute fluctuations, due to the terms of the series for $J_0^2(\rho)+J_1^2(\rho)$ which we have neglected in finding (V.) and (V.a).

72623

solution of the problem either as mathematical formula or as graphical representation.

For the latter purpose we draw the concentric circles representing lines of equal intensity for the luminous source, and similar lines of equal *negative* intensity for the imaginary source occupying the dark space; by the process above described we get a series of lines of equal intensity due to these sources. In the annexed diagrams (figs. 10 and 11) I give a portion of these lines in the neighbourhood of the rim of the luminous source ($a=\infty$), when there is a dark circular disk ($a=40$); the lines are drawn for equal intervals 0·1 of the intensity. Fig. 10 represents the lines when the dark disk touches the rim, and fig. 11 when the nearest distance between it and the rim is equal to 1. In both figures the dotted lines give the boundaries of the luminous source.

Fig. 10.

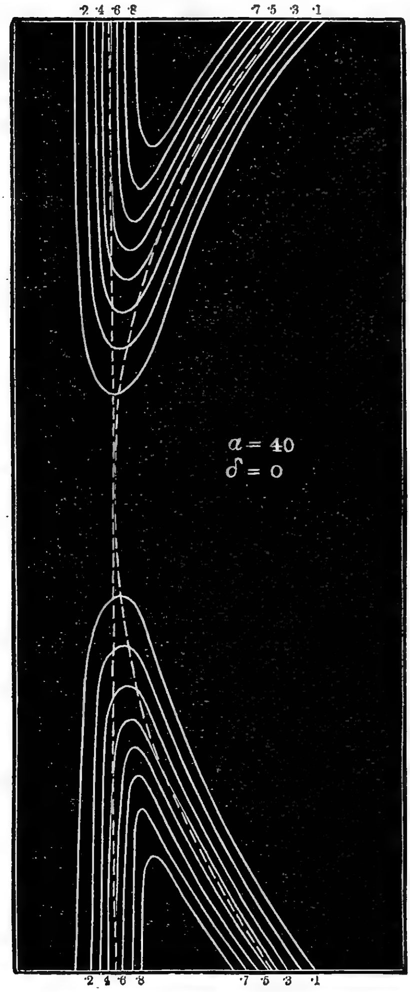


Fig. 11.

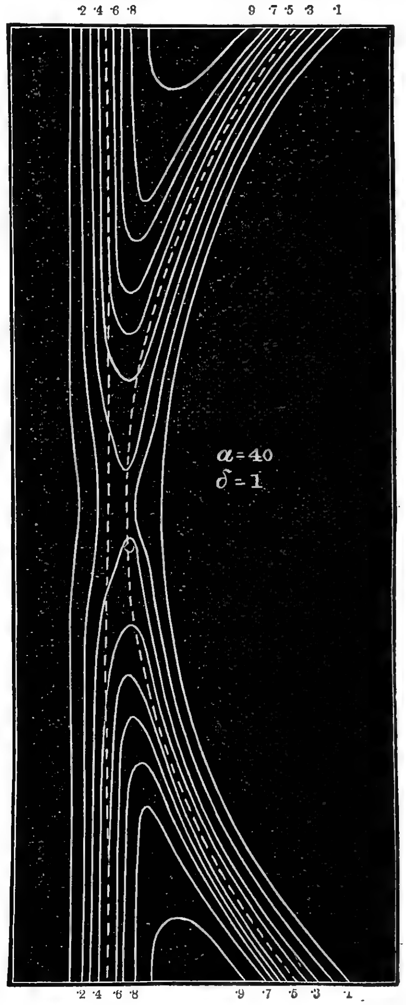
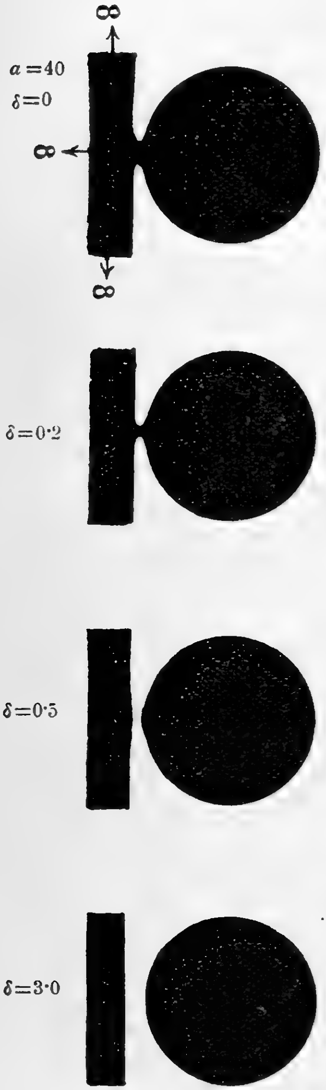


Fig. 12.



The inspection of these figures shows that when the dark space is in geometrical contact with the edge of a luminous source, it appears as a dark protuberance projecting from the surrounding dark space. With the receding of the dark space towards the interior of the luminous source, the connecting ligament becomes thinner, and finally disappears. The inner dark disk is, however, a little elongated, and assumes a pear-shaped appearance; while the external dark space bulges out towards the luminous source. The change is only transient; with further ingress the dark disk becomes circular, and the swelling of the external dark space vanishes. The accompanying diagram (fig. 12) will represent approximately the various stages of dark space within a luminous source, as observed by means of a telescope.

Thus, to the first approximation, we have arrived at the explanation of drop formation during the transit of inferior planets.

If the dark space be taken nearly equal to the luminous source and have small protuberances, we can, by similar process, obtain a result which has close analogy with Baily's beads.

II. *Relative Motion of the Earth and Æther.*

By WILLIAM SUTHERLAND [*].

THE experiment of Michelson and Morley, described in the Philosophical Magazine [5] xxiv. p. 449, has created quite a dilemma in the physics of the æther; for while the great body of general evidence tends to show complete independence of the æther near the earth on the earth's motion, this celebrated experiment has been supposed to prove defi-

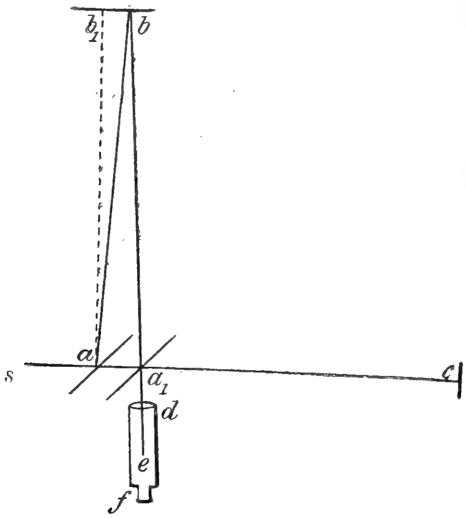
---

[*] Communicated by the Author.

nitely that the earth's surface and the adjacent æther have no relative motion. I propose now to show how a slight alteration in the point of view of the theory of that experiment will make it appear that, until a special adjustment for sensitiveness of the optical apparatus has been made, it is not competent to decide as to the relative rest or motion of earth and æther.

For the sake of clearness let us briefly repeat the authors' account of the theory of their experiment along with their diagram. b and c are two mirrors at right angles to one another (fig. 1), and at equal distances D from a a piece of glass inclined at $\pi/4$ to them, and intended to divide a beam of light sa into reflected and transmitted parts going to b and c respectively. Suppose the whole apparatus to be moving in the direction sc with velocity v relative to the æther in which the beam of light is moving with velocity V; then while the reflected beam is going to b and back a is moving to a_1, so that the path of the reflected part is aba_1, while that of the transmitted part is aca_1: at a_1 the former is partly transmitted and the latter partly reflected to the telescope under conditions favourable to the occurrence of interference.

Fig. 1.



Along ac the beam moves with velocity $V-v$ relative to c, so that the time of traversing ac is $D/(V-v)$; similarly the time for ca_1 is $D/(V+v)$, and hence

$$ac+ca_1=DV\left(\frac{1}{V-v}+\frac{1}{V+v}\right)=\frac{2D}{1-v^2/V^2}.$$

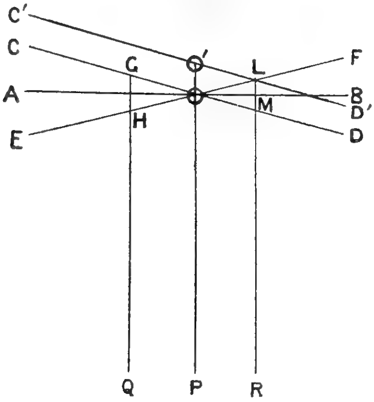
The path of the other beam is taken to be aba_1, whose length is evidently $2D(1+v^2/V^2)^{\frac{1}{2}}$, so that with omission of the third and higher powers of v/V the difference between aca_1 and aba_1 can be set down as Dv^2/V^2. If v is the velocity of the earth in its orbit, then if the whole apparatus is turned through $\pi/2$ the longer path becomes the shorter, and *vice versâ* : so that the difference of path which occasioned the interference-fringes in the first position is altered in the second by $2Dv^2/V^2$, which ought to produce a difference in the position of the fringes in the telescope. And this motion of the fringes is what Michelson and Morley found not to occur, even after they had given the apparatus a sensitiveness which they supposed to be quite adequate to its accurate measurement, minute as is v^2/V^2, namely about 10^{-8}. Hence the startling conclusion that the relative motion of earth and æther must be small compared to the earth's orbital velocity, in short that the earth drags the æther with it.

In the supplement to their paper Michelson and Morley (p. 460) show that the angle which ab makes with the mirror at b is more strictly $v/V+v^2/2V^2$, while ca_1 after reflexion makes an angle with the normal to b which is more strictly $v/V-v^2/2V^2$, so that the two rays which are to interfere after passing a_1 are inclined at an angle v^2/V^2 to one another. This is of no importance in the actual experiments, because to get interference-bands of a convenient width it is necessary to have the rays inclined at a much larger angle than this, though still at a very small angle, which is obtained by slight derangements of the mirrors and plate from the ideal mathematical positions assigned to them in the theory of the experiment.

To ascertain what really happens with the two interfering rays, let us take a small area of plane wave equal to the area of the pupil of the observer's eye, and take its image in b as it appears immediately after reflexion at a, and let us take the image of its transmitted part after reflexion at c and a_1 ; then if the whole apparatus and æther were at rest, and the angles were all exactly as supposed, we should have the two images coincident as represented in section at AB (fig. 2).

Now suppose that by slight derangement the images are separated as in CD and EF. Fringes now appear whose width depends on the angle COE. When the images coincided they represented coincident trains of waves with double illumination along their path. If the derangement, which separated the image AB into two, left them still parallel, there would be only circular fringes visible in any plane parallel to them, and only a uniform resultant central illumination to an infinitely small eye moving along a normal, which will have the special value zero when the distance between the separated images is equal to half a wave-length of the light employed. Thus, then, while the images are parallel no straight fringes can be seen, and the effect of varying the distance between the images is to cause motion among the circular fringes. Returning now to the case of a symmetrical angular separation of the images as at CD and EF, we see that the central bright fringe (really dark in the actual experiments because of the opposite conditions of reflexion of the two beams) will be seen anywhere along OP; and the next may be said to be approximately along the line GH (and only approximately straight), at such a distance from O that GH is equal to λ, the wave-length. Thus the interval between the fringes PQ is approximately given by the equation

Fig. 2.



$$PQ = \lambda/2 \tan COA.$$

For brevity we will confine the discussion to two-dimensioned space, that is, to straight-line waves travelling in a plane.

Next let the relative motion of the æther and the apparatus cause the separation of the images contemplated by Michelson and Morley, namely by a distance $s = Dv^2/V^2$, which is also accompanied by the angular rotation of the two images through $v^2/2V^2$ in opposite directions; but when COA is large compared to v^2/V^2 this last effect can be neglected. Suppose, then, that CD is moved to C′D′ so that OO′ $= s$, and let C′D′ cut EF in L. It seems to be assumed by Michelson and Morley that the locus of the central fringe moves along to LR, so that

$$OM = PR = LM/\tan COA = s/2 \tan COA, \text{ and } \therefore\ PR = PQs/\lambda;$$

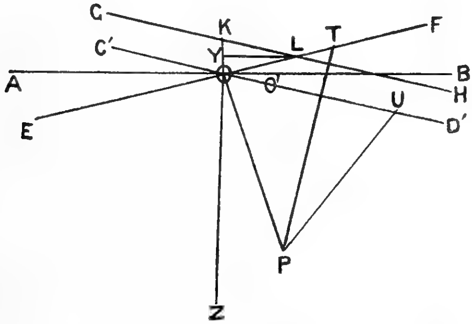
and therefore that the whole system of fringes is moved laterally by the same fraction of the width of a fringe as s is of λ.

Now this can be true only if the images are symmetrical with respect to L, or if each wave-front is absolutely uniform in character. But in dealing with wave-fronts of light in connexion with interference we must be careful to conjoin the effects of parts only of identical origin, as, for instance, that of C with that of E, or that of O′ with that of O, or that of L in C′D′ with that of L in EF.

But in the actual experiments it is practically impossible to secure that the two images intersect in a point that corresponds to itself as L does. In general we must assume that the two images C′D′ and EF intersect under the conditions represented in the next diagram (fig. 3), where O in EF and O′ in C′D′ are images of the same point in the original wave-front.

Let $OO'=c$; then if T is any point at distance x from O in EF, the corresponding point U in C′D′ is at distance $c+x$ from O. Let us bisect FOD′ by AB, and find the value of x which determines a pair of corresponding points so that they are equidistant from a point whose polar coordinates relative to O and OB are r and θ. Denote the angle FOB by α; then the equality of PT and PU gives

Fig. 3.



$$r^2+x^2-2rx\cos(\theta+\alpha)=r^2+(c+x)^2-2r(c+x)\cos(\theta-\alpha), \quad (1)$$

$$\therefore\ 2x(c-2r\sin\theta\sin\alpha)-2rc\cos(\theta-\alpha)+c^2=0\,;$$

and treating α as a small angle

$$2x(c-2r\alpha\sin\theta)-2rc\cos\theta-2rc\alpha\sin\theta+c^2=0 \text{ nearly.}$$

Denote $r\cos\theta$ by q, and $r\sin\theta$ by p, and then

$$2x(c-2p\alpha)+c(c-2p\alpha)-2cq=0. \quad . \quad . \quad . \quad (2)$$

In this equation first suppose that

$$c-2p\alpha=0,$$

then either $c=0$ or $q=0$. If c happens to be zero, then either $p=0$ or $\alpha=0$. The case of $p=0$ is of no interest, and with $c=0$, $\alpha=0$ we have the case of absolutely coincident images with no fringes at all. Thus the case with $c-2p\alpha=0$ and $c=0$ can be dismissed, and we have to consider next $c-2p\alpha=0$ and $q=0$, which amounts to this, that at the particular position of P given by $p=c/2\alpha$ and $q=0$, both of each pair of corresponding points are nearly equidistant from P, so that both of each pair of corresponding disturbances reach this position of P in the same phase. In this case there is one point $p=c/2\alpha$ and $q=0$ which stands nearly in the same sort of symmetry with respect to the two images as O does when O′ and O are identical. We will return to this as a special case after we have studied the general case in which $c-2p\alpha$ is not equal to zero. Here we have

$$x=\frac{c\{q-(c-2p\alpha)/2\}}{c-2p\alpha}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

Let the state of affairs we have been discussing so far be that in which there is no relative motion of the apparatus and the æther, so that the want of coincidence of the two images is due entirely to experimental imperfection; and now suppose the apparatus to acquire its velocity v relative to the æther, the effect of which is to shift C′D′ relatively to EF in the manner contemplated by Michelson and Morley. Let us suppose the shift to be the simplest possible, namely, that of C′D′ parallel to itself to GH through a distance $\mathrm{OK}=s=\mathrm{D}v^2/\mathrm{V}^2$ along the normal to AB, and let GH intersect EF in L, which is now to be regarded as a new origin. c has not been altered by the shift, p has been increased by $\mathrm{OY}=s/2$, and q has been diminished by $\mathrm{YL}=s/2\tan\alpha=s/2\alpha$ nearly; thus then for the distance x' defining the distance of a point along LF from L which is at the same distance from P as its corresponding point in LH, we get by making in (3) the changes indicated

$$x'=\frac{c\{q-s/2\alpha-(c-2p\alpha-s\alpha)/2\}}{c-2p\alpha-s\alpha}. \quad . \quad . \quad . \quad (4)$$

so that approximately as s and α are small

$$x-x'=\frac{c}{c-2p\alpha}\cdot\frac{s}{2\alpha}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

But x' is measured from L, so that the actual shift of the corresponding points is $x-x'-\mathrm{OL}=x-x'-s/2\alpha$

$$=\frac{s}{2\alpha}\frac{2p\alpha}{c-2p\alpha}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

Therefore if c is large compared to $2p\alpha$ the shift of corresponding points is only a small fraction of that $(s/2\alpha)$ contemplated by Michelson and Morley for the fringes. Now to see how the consideration of corresponding points bears upon the problem we have only to remember that each wave-front propagates itself normal to itself, and that therefore the best interference effects are to be sought along the normal to AB which is equally inclined to the normals to the two image waves; if we fix our attention on two small elements of the image waves around two corresponding points, we see that their most vivid interference effects will be along the normal to AB which is at distance $(x+c/2)\cos\alpha$, or nearly $x+c/2$ along AB from O. In order that q, which is $x(c-2p\alpha)/c + (c-2p\alpha)/2$, should be nearly this $x+c/2$, the fraction $2p\alpha/c$ is to be small, or c is to be large compared to $2p\alpha$, which is the condition just found in order that the shift of corresponding points should be small compared to that of the fringes sought by Michelson and Morley. If the eye of the observer is placed in OZ (fig. 3), that is if $q=0$, then $x=-c/2$, and in the whole system of possible fringes there is along OZ a peculiar central region characterized by a certain symmetry; but at the same time to an eye placed anywhere the central band in its field may be characterized by maximum clearness, yet it will have none of the peculiar character of the one absolute symmetrical central one. To an eye placed anywhere the central band in its field is at such a point that the mean distance from equal corresponding areas of the two images is the same; and according to what we have just seen if the one image is shifted through a distance s along the normal to the mean position of the two images, then the position of corresponding points is only changed by the small fraction $2p\alpha/(c-2p\alpha)$ or $2p\alpha/c$ of $s/2\alpha$, and therefore the fringes will be seen in the eye to move approximately only the small fraction $2p\alpha/c$ of the expected $s/2\alpha$. If things were adjusted so that the absolutely central region of the fringes appeared in the eye, then just as in fig. 3 O moves to L, the complete shift $s/2\alpha$ of the central region would be observed; but this adjustment would be a tedious business.

In the final experimental arrangements the simplicity of the scheme given in fig. 1 was somewhat departed from, because each half of the divided beam was reflected backwards and forwards four times along its initial path in order to increase the effective value of D; the images were brought into apparent coincidence and so adjusted that 2α was such as gave fringes of convenient width; but c was quite unknown and might be large compared to $2p\alpha$, or say $2D\alpha$, without detection, because while α was adjusted for by reason of the necessity of making the fringes of convenient width, there was no adjustment for making c zero, beyond the comparatively rough one of causing images of an object to coincide apparently, and therefore on the average c may be assumed to be large compared to $2p\alpha$. The case in which c becomes not very different from $2p\alpha$ has already been partly discussed in the extreme form when $c=2p\alpha$, a relation which we saw makes x indeterminate. When $c-2p\alpha$ is small but not zero, since $x=-c/2+qc/(c-2p\alpha)$ we can, by keeping q small, also keep x as near $-c/2$ as we please, so that the central region of the fringes is still peculiar and characteristic. In this case the approximation given for $x'-x$ in (5) is no longer of use; and we must reason in the following way, that before the shift (fig. 3), the central region will lie along OZ, and after the shift along a parallel to OZ through L, so that the system of fringes shifts as expected by Michelson and Morley; in short, when q is small we are observing near the central fringe which moves the full $s/2\alpha$. Thus we see that while an improper use of the formula (6) might make it seem as if we could get infinite magnification of the effect looked for in the experiment, in reality the shift $s/2\alpha$ of the fringes is the largest practically obtainable, and can be secured only by making either q nearly equal to 0, or as we saw before (fig. 2) $c=0$; while if c is allowed to be larger than $2p\alpha$ the shift may be any fraction of this maximum. In other words, the shift expected by the experimenters can be obtained in only two cases: first when the intersection of the two images corresponds to itself, in which case the eye may observe anywhere; and second, when there is a lateral shift of one image relative to the other, and the eye is in the axis of quasi-symmetry OZ. If the contention here advanced is sound, it appears that the failure of Michelson and Morley to get evidence of the relative motion of earth and æther is due to the absence of a certain adjustment required to give their method the sensitiveness aimed at, and not to any real defect in the theory of the experiment. Their method has been since applied to other attempts to measure motion of the æther, as in Lodge's experiment with the whirling steel disks (Phil. Trans. clxxxiv. 1893), Threlfall and Pollock's on the Effect of Röntgen Rays (Phil. Mag. [5] xlii.), and Henderson and Henry's (Phil. Mag. [5] xliv.) on the Motion of Æther in an Electromagnetic Field; in all these experiments only negative results have been obtained. But it is to be remembered that in the last three the two parts of the divided beam are sent in opposite directions round the same path, whereas in Michelson and Morley's experiment the two beams travel in independent paths: now in the former case if the two beams travel D between separating and meeting, and meet again at an angle 2α, then the linear separation of corresponding points may be taken as $2\alpha D$, which amounts to the same as our $2\alpha p$, and hence the conditions are such as favour the degree of sensitiveness expected by Michelson and Morley; while in their own experiment, as each of the separated beams is reflected fifteen times in its own independent path there is opportunity for a considerable lateral shift of the one beam relative to the other when they meet, although both are adjusted as nearly to parallelism as is necessary, that is to say that c is independent of $2\alpha p$, and the sensitiveness of the system of fringes is unknown, but in all probability small compared to that expected. In their celebrated repetition of Fizeau's great experiment on the effect of running water on the æther Michelson and Morley got their well-known positive result, but in this case the divided beams were sent in opposite directions round the same path, so that the optical arrangement had the sensitiveness expected. It is the use of multiple reflexion along different paths in the experiment on the relative motion of earth and æther that introduces the possibility of comparatively large lateral shift.

If the argument in this paper is correct it ought to be possible by careful adjustment for the requisite smallness of c, or for getting the absolute central band into the field of view, to give the Michelson and Morley apparatus the sensitiveness desired for measurement of the relative motion of earth and æther; and in any case an experimental examination of the effect of lateral shift seems desirable.

Melbourne, Sept. 1897.

III. *The Transmission of Radiant Heat by Gases at Varying Pressures.* *By* CHARLES F. BRUSH*.

[Plates I. to X.]

BEFORE describing my own investigations on the transmission of heat by gases, I shall refer briefly to the classical work of a somewhat similar nature by MM. Dulong and Petit early in the present century. Their memoir entitled "Researches on the Measure of Temperatures, and on the Laws of the Communication of Heat," gained the prize voted by the Academy of Sciences in 1818. A translation of this important paper may be found in the 'Annals of Philosophy,' for February, March, April, and May, 1819.

---

* Communicated by the Author, having been read before the American Association for the Advancement of Science, August 10, 1897.

In their researches on the "Communication of Heat," Dulong and Petit used as the cooling body a very large thermometer bulb filled with mercury; and as the recipient of the heat, a large copper bulb or "balloon" about three decimetres in diameter, in the centre of which the thermometer bulb was placed. The copper balloon was coated with lamp-black on the inside, and kept at any desired constant temperature by means of a water-bath or melting ice. The thermometer tube was of such length as to bring the zero of the scale outside the balloon; and the thermometer was adapted to be removed, heated, and quickly replaced, air-tight. The balloon was connected with an air-pump capable of rapidly exhausting it down to about two millimetres pressure; and also with a gas-holder from which it could be quickly filled with the gas whose cooling properties were to be determined. The rate or "velocity" of cooling of the thermometer bulb was deduced from observations of the falling temperature at equal intervals of time.

With this apparatus Dulong and Petit made many carefully conducted experiments at differences of temperature between the thermometer and balloon ranging as high as 300 degrees; and with several different gases besides air, ranging in pressure from atmospheric to two millimetres. From the results of these experiments they deduced several laws of cooling which they held to be general in their application. They sharply divided the cooling into two parts; that due to convection—the actual contact of the surrounding cooler gas renewed by its own currents, and that due purely to radiation—the same as would occur in an "absolute vacuum." They derived a constant value for the latter, and values for the former varying with different gases and different pressures. They generally used the thermometer bulb naked, with its natural vitreous surface, but sometimes they silvered it. While this radical change in the character of surface greatly changed the loss of heat due to radiation, it apparently had no effect on that due to convection. They also tried thermometers with different-sized bulbs; and again, various shaped vessels, filled with various liquids surrounding a small thermometer bulb, were tried as the cooling body. None of these changes affected the general laws of cooling which they had deduced. But they did not try a smaller copper balloon. Had they tried a very small one they would have found some of their general deductions untenable.

MM. Dulong and Petit devoted a lengthy chapter to the discussion of the cooling of bodies in vacuo. But they fell into the grave error of deducing the behaviour of the last few millimetres of gas from that of the rest. In this way they arrived at the following "Sixth Law:"—

"The cooling power of a fluid diminishes in a geometrical progression when its tension itself diminishes in a geometrical progression. If the ratio of this second progression is 2, the ratio of the first is 1·366 for air; 1·301 for hydrogen; 1·431 for carbonic acid; and 1·415 for olefiant gas."

My own observations show that this law can be approximately true only in the case of a large balloon; and at pressures from a few millimetres upward. There is no suggestion of it when a small balloon is used; and at small pressures it does not obtain with either large or small balloons. I find that in a small balloon the cooling effect of the last millimetre of air is nearly ten times as great as that of all the rest, up to atmospheric pressure, combined.

It was through misplaced confidence in their Sixth Law that Dulong and Petit were led to place a value on the rate or velocity of cooling in vacuo, something like a hundred per cent. too high, as I shall show later; and as they derived the cooling values of gases by deducting the cooling effect of a vacuum from the total cooling observed, all their values for gases are much too low. These large errors vitiate much of their otherwise excellent work, and render the numerical values of the ratios given in the Second and Third Laws extremely doubtful.

Other experimentalists also have studied the transfer of heat by air and other gases at various pressures. Kundt and Warburg (Pogg. *Ann.* 1874–5) and Winkelmann (Pogg. *Ann.* 1875–6) observed that the rate of heat transmission remained substantially constant through a long range of diminishing pressure; and then decreased with further exhaustion. But as they made no measurements of pressure below one millimetre (1316 millionths of atmospheric pressure), their results have no quantitative value for low pressures.

Crookes, in his paper "On Heat Conduction in Highly Rarefied Air (Proc. Roy. Soc. 1880), described a similar experiment in which he carried the pressure measurements as low as 2M. (two millionths). From the fall in the rate of heat loss which occurred between the pressures of 760 millimetres and 1 millimetre, and 5 M. and 2 M., he concludes: "We may legitimately infer that each additional diminution of a millionth would produce a still greater retardation of cooling, so that in such high vacua as exist in planetary space the loss of heat—which in that case would only take place by radiation—would be exceedingly slow."

In this conclusion Sir Wm. Crookes was, I think, wrong. I find that the curve representing the rate of cooling does not break down materially at pressures as low as a twentieth of a millionth.

My own investigations on "The Transmission of Radiant Heat by Gases at Varying Pressures" form a part of a general study of the properties of high vacua, in which I have long been engaged, and which is yet far from being completed.

In the course of my work it became necessary to know how much of the heat communicated by a good radiating body at ordinary temperatures, to a neighbouring body at a slightly lower temperature, through an intervening gas, is transmitted by the so-called æther, and how much by the gas; and whether any of that transmitted by the gas is communicated otherwise than by the process of convection. Also why, and to what extent, do the gases differ from each other in their heat transmitting capacities.

In the drawings herewith, Plate I. is a diagram of the apparatus used in my experiments. A is the thermometer whose cooling was observed. It has a very open scale divided into two-tenths degrees C. The zero-point is placed a long distance (about 170 millimetres) above the bulb, for obvious reasons. The bulb is cylindrical, about 20 millim. long and about 7 millim. in diameter, and is coated with lampblack applied with a very thin alcoholic solution of shellac. After several hours' baking at 100 degrees in a good vacuum, this bulb gave constant radiation results. The thermometer is suspended by a platinum wire, with its bulb in the centre of the large pear-shaped glass bulb B, about 112 millim. in diameter. The stem of the thermometer hangs freely in the long neck of the large bulb. I shall hereafter call the glass bulb B, the "large radiation bulb," or simply the "large bulb," to distinguish it from a smaller one used later. The bulb B is surrounded by a copper tank C, lagged with woollen cloth, and filled with crushed ice and distilled water. A wire netting C′ serves to keep some of the ice always below the lowest point of B. The tank C is movable on vertical guides, whereby it may quickly be raised to, or lowered from, the position shown, thus exposing the bulb B alternately to the ice-bath and the atmosphere of the laboratory. The bulb B communicates freely with the large barometer-tube D, which is used for measuring all but very small pressures. E is a standard boiled barometer, dipping into the mercury-cistern F, common to both barometers. G is a McLeod gauge giving very accurate measurements of small pressures, and H is a drying-bulb containing phosphorus pentoxide. The glass stopcock I serves to admit other gases than air, and is of course made absolutely tight when closed. The mercury valve K prevents any leakage backward from the pump when the latter is stopped during observations. Exhaustion is effected by an automatic Sprengel pump having five fall tubes. L is a fine cathetometer placed in front of the whole apparatus, and by rotation on its vertical axis is adapted to read the McLeod gauge, both barometers, and the thermometer. It has a vertically divided scale with vernier and microscope, for reading the barometers, and a micrometer for reading the gauge. A watch N is mounted close beside the thermometer on a sliding frame, so as to be easily kept in the field of view of the cathetometer telescope when the latter is used to observe the falling temperature.

Before using this apparatus I always exhausted to a good vacuum, and heated the bulb B by means of a water-bath, and all other vacuous parts by means of an air-bath, to 100 degrees for several hours. This was found necessary in the first instance with air, in order to divest the inner glass surfaces of that portion of their coating of adherent gas most easily given off in a vacuum. This gas was pumped out; and not being principally air, was not largely re-absorbed when air was admitted. Without this precaution I was unable to obtain constant results at very low pressures. When other gases were tried successively, the preliminary heating prevented gas from one operation attaching itself to the glass and remaining to contaminate the succeeding gas at very low pressures.

I next introduced the proper gas up to atmospheric pressure, and made a preliminary cooling of the thermometer by raising the ice-tank C. This preliminary cooling was found to have a slight effect on the readings next following, and was done to make the first set of readings on any day entirely comparable with the others. I then lowered the ice-tank, and when the temperature had risen to 18 degrees, stirred the ice and water thoroughly, raised the tank again, and observed the thermometer through the telescope—noting by the watch N the instant when the falling mercury passed each degree of the scale. Then, with the ice-tank still up, I noted the pressure by measuring with the cathetometer the difference in height of the barometer columns in D and E. The barometer D showed that the gas in the radiation bulb cooled nearly to zero with very great rapidity when the ice-tank was raised. I always measured pressures with the radiation bulb cold. It was usual to repeat the whole operation to confirm results before reducing the pressure by the pump.

Observations were thus made at pressures varying from atmospheric down to the best vacuum obtainable. In some instances many series of observations were made at varying pressures all within the last millionth. The gauge could be relied upon to measure these small pressures with very great accuracy. But it was difficult to maintain them long at any exactly constant value on account of the continual, though slight, evolution of gas from the glass of the apparatus.

As I desired only comparative results, no correction was made for the probable slight inequalities in the callibration of the thermometer; nor for heat conducted to or from the bulb by the stem; nor for the change of zero-point due to changing external pressure. The mercury fell exactly to zero at atmospheric pressure, and about one-fiftieth of a degree lower at no pressure. The pressure error due to differences of capillary depression in the two barometers was ascertained at high exhaustions, and found nearly constant. It was always corrected. The different gases used were carefully prepared and dried, and were introduced quite free from any admixture with air.

My observations have extended over a long period, and are far too voluminous to be recorded here in detail. But I have embodied their most salient features in a series of curves which render them readily apparent to the eye. In these curves the abscissæ represent the pressure, and the ordinates represent the rate of heat transmission through the gas, from the thermometer bulb to the ice-cold envelope. The rate of transmission at any particular pressure is expressed by the reciprocal of the number of seconds required for the temperature to fall through a given number of degrees. For convenience of scale all the reciprocals are multiplied by 500.

Pl. II. shows the curve for air. The heavy line represents the rate of cooling from 15 degrees to 10 degrees. It is in three sections, A, B, and C. Section A embraces the whole range of pressure from nothing to atmospheric; section B embraces the range of pressure from nothing to ·01 of atmospheric; and section C embraces the range of pressure from nothing to ·0001 of atmospheric, *i. e.* 100 M. (one hundred millionths). Atmospheric pressure is taken at 760 millim. Thus it will be seen that section B is the last hundredth of A, magnified a hundred times; and section C is the last hundredth of B, magnified a hundred times. The cross-section paper on which these curves are drawn is 50 centimetres wide; hence if the curve were completed on the scale of B, it would be 50 metres long; and if completed on the scale of C, it would be five kilometres long. This magnification of the abscissæ without change of the ordinates enables us to study every part of the curve with ease. The small circles represent the points in the curve established by observation. These points are shown exactly as found, without any attempt to smooth out rough places in the curve. The same is true of the curves of other gases. The heavy dotted line parallel with the base represents that portion of the total heat transmission due to the æther; while all above it represents that due to the air.

Starting at the left-hand end of section A, representing the rate of heat transmission at atmospheric pressure, we observe that the curve drops regularly at a rate faster than the diminution of pressure, during 95 per cent. of the whole range of pressure from atmospheric to zero. Beyond this point the rate of heat transmission remains substantially constant, as shown by section B and the latter part of A, down to a pressure of about ·0003—a range of nearly ninety-nine and a half per cent. of that remaining. Here the curve suddenly begins to drop again, and falls steadily, as shown by section C and the latter part of B, until it meets the æther line at the zero of pressure.

Under the curve A I have drawn curves with finer lines representing the rate of heat transmission at smaller differences of temperature between the thermometer and ice-bath. As before stated, A represents the cooling from 15 degrees to 10 degrees. On the same scale *a* represents the cooling from 9 degrees to 6 degrees; *a a* from 6 degrees to 4 degrees; and *a a a* from 3 degrees to 2 degrees. Now, Newton's law of cooling requires that the rate shall vary directly with the difference of temperature between the cooling body and the surrounding medium. While this law is known to be incorrect for large differences of temperature, it is generally accepted for very small differences. If it were correct under the conditions of the present experiment, then the ratios of the times required for the temperature to fall through the several ranges above indicated, would all equal unity, and the curves A, *a*, *aa*, *aaa* would coalesce. But they are very far from doing this. It will be observed that all of these curves preserve their relative values very closely indeed, until they approach the point of pressure where the curve A reverses itself; then they begin to bunch themselves very much closer together, especially the lower ones, and shortly reach a greatly reduced, as well as varied ratio of values which they retain substantially unchanged to the end, as shown in connexion with section C. To avoid confusion of lines I have omitted the secondary curves corresponding with section B.

Pl. III. shows the curves for carbon monoxide. This gas was chosen for comparison with air, because its absorptive power for radiant heat is ninety times greater, while its specific heat is almost exactly the same. The principal curve, representing the rate of heat transmission from 15 degrees to 10 degrees, differs very little from that of air. It shows a slightly better rate than air at very small pressures; not quite so good a rate as air at intermediate pressures; and the same rate at atmospheric pressure. But the curves *a*, *a a*, *a a a*, representing equivalent amounts of cooling at smaller temperature differences, are materially unlike those of air. At high pressures they have about the same ratio values as with air; but the ratio diminishes much less at intermediate and low pressures; that is to say, the curves remain further apart. It is equally noticeable that the curves *a a*, *a a a* retain their full relative ratio values at low pressures, while with air they nearly coalesce.

Pl. IV. shows the curve for ethylene. It was thought that this gas might transmit heat more rapidly than air, because of its much higher specific heat. But it does not do so. Its curve has the same form as those of air and carbon monoxide. It transmits heat nearly as well as air at atmospheric pressure, but not nearly so well at intermediate pressures. At a very few millionths, however, it conducts a trifle better than air. The curves *a*, *a a*, and *a a a* have the same characteristics, and about the same ratios, as those of carbon monoxide.

Hydrogen was next tried, on account of its very low coefficient of viscosity, as well as its very high specific heat. Pl. V. illustrates the hydrogen curve on the same scale as the others. While in general form it resembles the air curve, all the ordinates are immensely increased. It is noticeable that the intermediate section B of the curve lies much nearer A than C, quite different from its relative position in the curves of the other gases. This section of the curve shows that hydrogen retains about two-thirds of its initial heat-transmitting power at a pressure nearly two hundred times smaller than does air. The curves A, *a*, *a a*, and *a a a* have something like the same ratios as they have in the cases of carbon monoxide and ethylene. In general, it may be said of hydrogen in the large radiation bulb, that it transmits heat nearly four times as fast as air at atmospheric pressure; more than twice as fast at a very few millionths, and more than seven times as fast through a long range of intermediate pressures.

As evidence of the accuracy of the observations on which the curves thus far described are based, it is gratifying to note that the vacuum, or æther line, locates itself exactly the same in all.

In making the above-described observations I looked for some change in the phenomena when the exhaustion reached the point at which the mean free path of the gas molecules equalled the distance between the thermometer bulb and the cold walls of the enclosing globe. This should have been at a pressure of about two millionths. No such change was observable, however, in any case. Partly in pursuance of the same idea, I resolved to repeat some of my experiments, using a very much smaller radiation bulb. This I expected would also reduce that portion of the total cooling effect due to convection currents. I accordingly employed the bulb or tube P (Pl. I.) in my further experiments. This is made from a thin glass tube slightly less than 20 millimetres internal diameter, and in it hangs the same thermometer A which was used before. In transferring the thermometer, great care was taken to avoid any disturbance of the coating of lampblack on its bulb. At *b* is a contraction of the tube P to prevent the thermometer bulb swinging against the inside of the tube. The contraction *b* is, however, much larger than the thermometer stem, so that normally the latter does not touch it. The thermometer bulb hangs exactly in the centre of P, near its bottom, and is separated from it by a space of a trifle more than six millimetres—almost exactly a quarter of an inch; instead of two inches, as in the case of the "large bulb." The tube or bulb P, I shall hereafter designate the "small radiation bulb," or simply "small bulb," to distinguish it from the large one.

Pl. VI. shows the curve for hydrogen, with the small bulb. It differs radically in size and form from that obtained with the large bulb. Section A, instead of drooping rapidly with decreasing pressure, maintains almost its full value throughout. Section B starts with nearly double its old value, but breaks down much earlier. Section C starts with a little higher value, but is much straighter, and consequently has a lower value throughout most of its length. The curves *a*, *a a*, *a a a* are very peculiar. They start at atmospheric pressure with much smaller total, and very different relative ratios than in Pl. V., and are successively absorbed into A. They reappear later, however, as shown in section C, but with smaller ratios than in Pl. V.

Pl. VII. gives the curve for air, with the small bulb. It differs from that with the large bulb quite as much as did the hydrogen curve. Section A droops slightly, and then regains almost its full atmospheric value at one per cent. pressure. Section B has the same form as with the large bulb (Pl. II.), but more than double its value; and section C also has a much higher value throughout. The curves *a*, *a a*, *a a a* have small ratio values at the beginning, and are absorbed into section A the same as with hydrogen. But *a a* and *a a a* coalesce when they reappear, and coincide to the end; while the ratio between *a* and *a a* remains constant at a very small value.

Pl. VIII. is the curve for carbon dioxide, with the small bulb. It closely resembles the air-curve in form, but has a very much smaller value throughout. While the curves *a a* and *a a a* are soon united, and remain so to the end, *a* and *a a* never disappear as they did in the cases of hydrogen and air.

With the small bulb, as with the large, no change in the character of the phenomena was observable when the exhaustion had reached the point at which the mean free path of the molecules equalled the space through which the heat was conducted. This point was reached in the small bulb at a pressure of about fourteen millionths.

It seems reasonable to assume that the radical difference between sections A of the curves obtained with the large and small bulbs respectively was due to an almost complete suppression of convection-currents in the latter case. In the absence of convection-currents, that part of the heat transmitted by the gas was probably carried by a process analogous to conduction in solids. The shortness of conductor in the case of the small bulb may account for the greatly increased rate of conduction. But why the conductivity of a gas remains nearly constant through a very wide range of pressure is not clear. Sir Wm. Crookes's explanation of this phenomenon seems to me very unsatisfactory.

It will be noticed that the "æther-line" is about four per cent. lower with the small bulb than with the large one. This may be due to the greatly decreased amount of surface presented by the small bulb for absorption of the radiant heat.

The enormous heat-conducting capacity of gases at very small pressures is strikingly shown in all the curves. But hydrogen is preeminent in this respect. Thus, in the large bulb, hydrogen at a pressure of only twenty-six millionths of an atmosphere transmits heat as rapidly as the æther! At seventy-six millionths it equals air at atmospheric pressure; that is to say, it does the work of nearly two hundred thousand times its weight of air!

It is remarkable that at pressures up to a few millionths all of the curves are nearly straight lines. This is especially noticeable in the small-bulb curves, showing that at these small pressures the heat-transmitting power of a gas varies directly with its amount. Hence it seems reasonably certain that if the very small fraction of a millionth of the gas examined, which remained at the end of each experiment, could have been entirely removed, the heat-transmitting power of the vacuum would not have been materially diminished. It was customary at the end of the experiments with each gas to close the gauge permanently when the pressure had fallen to a tenth of a millionth or so, and with the capacity of the whole apparatus thus reduced run the pump continuously from one to two hours. Several sets of observations were always made during this extreme exhaustion, and while the change in the rate of cooling of the thermometer was generally appreciable it was always very small indeed. In my earlier experiments I took the greatest care to ensure the absence of mercury-vapour in the final vacuum. But the presence or absence of mercury-vapour made no difference distinguishable from the errors of observation.

Of course the best vacuum producible by a Sprengel-pump still contains many thousands of millions of gas-molecules per cubic centimetre. This may be regarded as a prodigiously large or exceedingly small quantity of gas, according to our point of view. While it has no apparent effect on the general heat-transmitting capacity of the vacuum, it does seem to interfere with or modify some function of the æther. This is the only explanation of certain phenomena that I can offer. I refer to the different behaviour of the vacua with different residual gases, and in different-sized bulbs, in the matter of adherence to, or departure from, Newton's simple law of cooling. The curves *a*, *a a*, *a a a* illustrate these differences in the several cases at the extreme end of section C of the principal curves. These differences are too large to be attributed to errors of observation. This is one of several reasons which lead me to suspect that at higher pressures all the gases examined interfere materially with and retard the transmission of heat by the æther. In other words, I suspect that the dotted æther line of my curve sheets should not be drawn parallel with the base, and have a constant value at all gaseous pressures as shown, but should have a decreasing value as the gas pressure rises from zero. On this interesting phase of my subject I hope to have more to say at a future date.

Before closing I will call attention to two more curve sheets. Pl. IX. is an air-curve plotted from figures given in Dulong and Petit's paper. It is drawn to such a scale that the rate of heat-conduction at atmospheric pressure is the same as in my own experiment with air in the large bulb, and illustrated in Pl. II. The first five stations in the curve are the ones from which they deduced their "Sixth Law" of cooling. The rest of the curve is drawn in accordance with that law, and the vacuum line represents exactly the value they assigned to the cooling power of an absolute vacuum. Comparison with Pl. II. shows how much they erred in their deductions.

Pl. X. embodies the results obtained with a mixture of three volumes of hydrogen and five volumes of carbon dioxide in the small bulb. A study of this curve in connexion with Pl. VI. shows that the carbon dioxide interfered very greatly with the performance of the hydrogen. Before any exhaustion was made, the hydrogen *alone* would have done more than three times the work of both gases. It was not until the pressure had fallen to about one hundred millionths that both gases combined did as well as the hydrogen would have done alone. Below this pressure both gases contributed to the result.

This interference of mixed gases is a very interesting phenomenon, and seems to warrant the careful investigation which it is my intention to give it.

IV. *The Stresses and Deflection of Braced Girders.*

By W. H. MACAULAY[*].

A STRUCTURE consisting of a number of bars hinged together (or pin-jointed) at their ends is called a frame. The points at which hinges occur are called joints. A frame is said to be *stiff* if the number and arrangement of bars are such that the frame cannot be distorted without stretching one or more of them. If this is not the case it is said to be loose. A frame is said to be *just stiff*, if it is stiff and the removal of any one bar could make it loose. If a frame is stiff, but not just stiff, it is possible, by removing a set of one or more bars, to make it just stiff without depriving it of any joints; such bars are called *redundant bars*. A frame may have more than one set of redundant bars, that is to say it may be possible to reduce it to being just stiff, without loss of joints, in more than one way. The same terminology may be applied to a frame in one plane; it is then assumed that no distortion is admissible except in that plane, and that any forces applied to the frame are in its plane.

[*] Communicated by the Author.

The problem of determining the tensions of the members of a stiff frame with redundant bars, and its deformation (assumed to be small) in any direction under the action of given forces applied at the joints, was completely solved by Clerk Maxwell. His solution was published in the Philosophical Magazine in 1864, series 4, vol. xxvii., and is reprinted at the end of vol. i. of his collected papers. The only step in this direction previously published was due to Clapeyron, and dealt only with the deformation of a frame without redundant bars. The method employed is sometimes referred to as Mohr's, although Mohr made his earliest contributions to the subject some years after the publication of Maxwell's solution, and then only attempted some particular examples of very simple character.

The application of Maxwell's solution to any practical case involves no difficulty, though the calculations may be rather long. Several examples of such applications have been given from time to time by Mr. Max am Ende in the 'Engineer.' The primary practical use of the method is for such applications to examples with particular numerical data, and in this connexion it deserves rather more attention than it has received. General results, sufficiently concise to be of interest, can only be obtained for frames of rather simple and symmetrical types with simple arrangements of loading. The object of the present investigation is to obtain from Maxwell's equations some general results for one or two simple types of girders.

It may be convenient to begin with a statement of Maxwell's solution of the problem. The question which he proposes is this:—Let K, L, M, N be four joints of a stiff frame, and let the forces applied to it consist of a tension F between K and L, in consequence of which the points M and N approach each other through a small distance x; to find x and the tensions of all the bars of the frame, assuming that the strains are all very small, and that Hooke's law is applicable. And the answer is as follows:—Select out of the bars of the frame a set which form a just stiff frame, connecting all the joints of the given frame; let the extensibilities of these be e_1, e_2, &c., extensibility being the ratio of extension to tension, or, for a uniform bar, length ÷ (area of section × Young's modulus); let their tensions be T_1, T_2, &c.; let ϵ_1, ϵ_2, &c. be the extensibilities and R_1, R_2, &c. the tensions of the remaining or redundant bars; let p_1, p_2, &c. be the tensions of the selected bars, taken alone as a just stiff frame, due to a unit tension between K and L, q_1, q_2, &c. their tensions due to a unit tension between M and N, r_1', r_1'', &c. their tensions due to a unit tension in the line of the first redundant bar, r_2', r_2'', &c.

their tensions due to a unit tension in the line of the second redundant bar, and so on; then R_1, R_2, &c. are given by the equations

$$\left.\begin{array}{l} R_1\epsilon_1 + R_1\Sigma e r_1^2 + R_2\Sigma e r_1 r_2 + R_3\Sigma e r_1 r_3 + \ldots + F\Sigma e p r_1 = 0 \\ R_2\epsilon_2 + R_1\Sigma e r_1 r_2 + R_2\Sigma e r_2^2 + R_3\Sigma e r_2 r_3 + \ldots + F\Sigma e p r_2 = 0, \end{array}\right\} \quad (1)$$

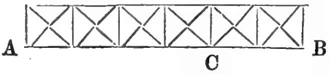
&c.

Also
$$T_1 = p_1 F + r_1' R_1 + r_2' R_2 + \ldots \quad . \quad . \quad . \quad (2)$$
&c.

and
$$x = F\Sigma e p q + R_1\Sigma e q r_1 + R_2\Sigma e q r_2 + \ldots \quad . \quad . \quad (3)$$

To apply this solution to the case of a girder on two rigid supports with a load at one of the joints K, we may suppose that a base connecting the supports with the necessary rigidity forms part of the frame, and that a force equal to the load acts vertically between K and a point of this base; the formulæ given by Maxwell then apply, and, if we take N to be a point of this base vertically below M, the value we get for x will be the deflection of the joint M. If there are several loads, we must investigate their effects separately and compound the results.

Let us apply these equations to the case of a pin-jointed girder of the type



with N panels, with all the horizontal members of equal extensibility λ, all the sloping members of equal extensibility μ, all the $N-1$ intermediate vertical members of equal extensibility ν, and the two end vertical members of equal extensibility ν'. Suppose the sloping bars not to be pinned together at their intersections. Let θ be the inclination to the vertical of each of the sloping bars, and let us write s and t for sec θ and tan θ respectively.

Let the ends (lower joints) of the girders rest on rigid supports A, B, and a load W be applied at one of the lower joints C, and let the weight of the girder be neglected. Let m be the number of panels between A and C, and n the number between C and B.

Distinguish the vertical bar at A by the suffix 0, and the panels from A to B each consisting of the bars  by the suffixes 1, 2, ... N. Select for a just stiff frame that obtained by rejecting all the vertical bars except the one at A, then the tensions of these vertical bars are R_1 R_2 .... R_N

respectively. Distinguish the four bars of a panel ⧖ of the just stiff frame by the letters a, b, c, d; a belonging to the top bar, b to the bottom bar, c to the one which slopes upwards from left to right, and d to the one which slopes downwards from left to right. The tension of a bar of the just stiff frame being denoted by T with a suitable suffix, T_{a1} is written for the tension of the bar a in the panel 1. The p's of Maxwell's equation can be distinguished by the same double suffixes, and the r's by triple suffixes, of which the first indicates the redundant bar referred to, the second the position in the panel, while the third is the suffix of the panel. The bar 0 is of course an exceptional case, requiring only the single suffix 0.

A number of relations among the tensions can be written down without reference to the strengths of the bars; in fact the whole number of bars being $5N+1$, we can find $4N+1$ such relations independent of one another.

Using the method of sections we get

$$\rho=1 \text{ to } N \qquad T_{a\rho}+T_{b\rho}+(T_{c\rho}+T_{d\rho})\sin\theta=0 \text{ (N equations)}, \quad . \quad (4)$$

$$\left.\begin{array}{ll} \rho=1 \text{ to } m & T_{c\rho}-T_{d\rho}=-s\frac{n}{N}W \\ \rho=m+1 \text{ to } N & T_{c\rho}-T_{d\rho}=s\frac{m}{N}W \end{array}\right\} \text{(N equations)}, \quad . \quad . \quad . \quad . \quad (5)$$

$$\left.\begin{array}{ll} \rho=1 \text{ to } m & T_{b\rho}-T_{a\rho}=(2\rho-1)t\frac{n}{N}W \\ \rho=m+1 \text{ to } N & T_{b\rho}-T_{a\rho}=\{2(N-\rho)+1\}t\frac{m}{N}W \end{array}\right\} \text{(N equations)}, \quad (6)$$

$$\left.\begin{array}{ll} & T_0+T_{d1}\cos\theta=0 \\ \rho=1 \text{ to } m & R_\rho+(T_{d\rho}+T_{d,\rho+1})\cos\theta=\frac{n}{N}W \\ \rho=m+1 \text{ to } N-1 & R_\rho+(T_{d\rho}+T_{d,\rho+1})\cos\theta=-\frac{m}{N}W \\ & R_N+T_{dN}\cos\theta=-\frac{m}{N}W \end{array}\right\} \text{(N+1 equations)}. \quad (7)$$

We want N more equations to enable us to determine the tensions of all the bars of the frame, and these are furnished by Maxwell's system of N linear equations for $R_1\ R_2 \ldots\ R_N$. Let us now solve these equations, replacing F by W.

The first step is to calculate the coefficients for our particular case. The r's and p's are readily found, the only difficulty being to write them down systematically, with due regard to all the different cases which arise according as m and n are odd or even.

We get $r_{\rho 0}=(-1)^{\rho+1}$,

if $\sigma<\rho+1$, $\qquad r_{\rho a\sigma}=r_{\rho b\sigma}=(-1)^{\rho+\sigma}t$,

$$r_{\rho c\sigma}=r_{\rho d\sigma}=(-1)^{\rho+\sigma+1}s,$$

if $\sigma>\rho$, $\qquad r_{\rho a\sigma}=r_{\rho b\sigma}=r_{\rho c\sigma}=r_{\rho d\sigma}=0$;

thus $\qquad (-1)^{\rho+\sigma}\Sigma e r_\rho r_\sigma=\nu'+2\rho(\lambda t^2+\mu s^2)$,

where $\qquad \rho<\sigma+1$,

the summation being for all bars of the just stiff frame, r_ρ referring to the ρth, and r_σ to the σth redundant bar.

$$p_0=\tfrac{1}{2}(-1)^m-\frac{1}{2\mathrm{N}}\{(-1)^{\mathrm{N}}m+n\}.$$

For the panels 1 to m:

if $\mathrm{N}-\sigma$ is even, $p_{a\sigma}=-\sigma\dfrac{n}{\mathrm{N}}t+[t]$, $\quad p_{b\sigma}=(\sigma-1)\dfrac{n}{\mathrm{N}}t+[t]$,

$$p_{c\sigma}=-[s], \qquad p_{d\sigma}=\frac{n}{\mathrm{N}}s-[s]\ ;$$

if $\mathrm{N}-\sigma$ is odd, $p_{a\sigma}=-(\sigma-1)\dfrac{n}{\mathrm{N}}t-[t]$, $\quad p_{b\sigma}=\sigma\dfrac{n}{\mathrm{N}}t-[t]$,

$$p_{c\sigma}=-\frac{n}{\mathrm{N}}s+[s], \qquad p_{d\sigma}=[s],$$

where the terms in square brackets are to be kept if n is odd, and to be omitted if n is even.

For the panels $m+1$ to N:

if $\mathrm{N}-\sigma$ is even, $p_{a\sigma}=-(\mathrm{N}-\sigma)\dfrac{m}{\mathrm{N}}t$, $\quad p_{b\sigma}=(\mathrm{N}-\sigma+1)\dfrac{m}{\mathrm{N}}t$,

$$p_{c\sigma}=0, \qquad p_{d\sigma}=-\frac{m}{\mathrm{N}}s\ ;$$

if $\mathrm{N}-\sigma$ is odd, $p_{a\sigma}=-(\mathrm{N}-\sigma+1)\dfrac{m}{\mathrm{N}}t$, $\quad p_{b\sigma}=(\mathrm{N}-\sigma)\dfrac{m}{\mathrm{N}}t$,

$$p_{c\sigma}=\frac{m}{\mathrm{N}}s, \qquad p_{d\sigma}=0.$$

Thus for the panels 1 to m,

$$p_{a\sigma}+p_{b\sigma}=(-1)^{\mathrm{N}-\sigma}\left\{-\frac{n}{\mathrm{N}}t+[2t]\right\},$$

$$p_{c\sigma}+p_{d\sigma}=(-1)^{\mathrm{N}-\sigma}\left\{\frac{n}{\mathrm{N}}s-[2s]\right\};$$

and for the panels $m+1$ to N,

$$p_{a\sigma}+p_{b\sigma}=(-1)^{\mathrm{N}-\sigma}\frac{m}{\mathrm{N}}t,$$

$$p_{c\sigma}+p_{d\sigma}=-(-1)^{\mathrm{N}-\sigma}\frac{m}{\mathrm{N}}s.$$

Accordingly if $\rho \quad m+1$,

$$\Sigma epr_\rho=(-1)^{\rho+1}p_0\nu'+(-1)^{\mathrm{N}+\rho+1}\rho\left\{\frac{n}{\mathrm{N}}-[2]\right\}(\lambda t^2+\mu s^2)$$

$$=(-1)^{\rho+1}p_0\nu'+(-1)^{\mathrm{N}-\rho}\rho\left\{\frac{m}{\mathrm{N}}-(-1)^n\right\}(\lambda t^2+\mu s^2)\ ;$$

and if $\rho>m$,

$$\Sigma epr_\rho=(-1)^{\rho+1}p_0\nu'+(-1)^{\mathrm{N}+\rho+1}m\left\{\frac{\mathrm{N}-\rho}{\mathrm{N}}-[2]\right\}(\lambda t^2+\mu s^2)$$

$$=(-1)^{\rho+1}p_0\nu'+(-1)^{\mathrm{N}-\rho}m\left\{\frac{\rho}{\mathrm{N}}-(-1)^n\right\}(\lambda t^2+\mu s^2).$$

On examining these results we find that

$$\Sigma er_\sigma r_{\rho-1}+2\Sigma er_\sigma r_\rho+\Sigma er_\sigma r_{\rho+1}$$

is zero, except when $\rho=\sigma$, and that in this case it is equal to $2(\lambda t^2+\mu s^2)$; also that

$$\Sigma epr_{\rho-1}+2\Sigma epr_\rho+\Sigma epr_{\rho+1}$$

is zero except when $\rho=m$, and that in this case it is equal to $-(\lambda t^2+\mu s^2)$.

Accordingly, if we take the $\rho-1$th, ρth, and $\rho+1$th of Maxwell's equations and add them together after multiplying the ρth by 2, all the R's will be eliminated except three, the resulting equation being

$$\nu(\mathrm{R}_{\rho-1}+2\mathrm{R}_\rho+\mathrm{R}_{\rho+1})+2(\lambda t^2+\mu s^2)\mathrm{R}_\rho=0,$$

subject to the following exception, namely, that when $\rho=m$ we must add the term $-(\lambda t^2+\mu s^2)\mathrm{W}$, and that in the last equation $\nu\mathrm{R}_\mathrm{N}$ must be replaced by $\nu'\mathrm{R}_\mathrm{N}$.

Let $\nu'/\nu=\alpha$, and $2(\lambda t^2+\mu s^2)/\nu=\beta$, then each three successive R's of the series $\mathrm{R}_1\ \mathrm{R}_2\ \ldots\ \mathrm{R}_m$ satisfy the difference equation

$$\mathrm{R}_{\rho-1}+(2+\beta)\mathrm{R}_\rho+\mathrm{R}_{\rho+1}=0.\quad.\quad.\quad.\quad.\quad(8)$$

Accordingly, this series of R's are given by the equation

$$\mathrm{R}_\rho=\mathrm{A}u^{m-\rho}+\mathrm{B}v^{m-\rho},\quad.\quad.\quad.\quad.\quad.\quad(9)$$

where u and v are the roots of the equation

$$u^2+(2+\beta)u+1=0,$$

that is to say, $u+v=-2-\beta$, and $uv=1$. It is clear that u and v are both negative; let us denote by u the root which is a proper fraction.

Similarly R_m, R_{m+1}, . . . R_{N-1} are given by the equation

$$R_\rho=A'u^{\rho-m}+B'v^{-m}, \qquad (10)$$

and

$$aR_N=A'u^n+B'v^n, \qquad (11)$$

where

$$A'+B'=A+B. \qquad (12)$$

We have now used N−3 of our N equations (1), and have three left to complete the determination of A B A′ B′, namely the equation

$$R_{m-1}+(2+\beta)R_m+R_{m+1}-\tfrac{1}{2}\beta W=0, \qquad (13)$$

and the first two of the equations (1), which are

$$R_1+\{R_1-R_2+R_3-\ .\ .\ .\ -(-1)^N R_N\}(a+\beta)+\frac{1}{\nu}W\Sigma epr_1=0, \quad (14)$$

$$-R_1(a+\beta)+R_2+\{R_2-R_3+\ .\ .\ .\ +(-1)^N R_N\}(a+2\beta)$$
$$+\frac{1}{\nu}W\Sigma epr_2=0. \qquad (15)$$

Now from equation (2)

$$T_0=p_0W+R_1-R_2+\ .\ .\ .\ -(-1)^N R_N. \qquad (16)$$

So equations (14) and (15) may be written

$$R_1+T_0(a+\beta)+W\{-p_0(a+\beta)+\frac{1}{\nu}\ \Sigma epr_1\}=0,$$

$$R_1\beta+R_2-T_0(a+2\beta)+W\{p_0(a+2\beta)+\frac{1}{\nu}\ \Sigma epr_2\}=0,$$

or

$$R_1+T_0(a+\beta)=-\tfrac{1}{2}W\frac{n}{N}\beta, \qquad (17)$$

$$R_1\beta+R_2-T_0(a+2\beta)=W\frac{n}{N}\beta. \qquad (18)$$

We have now to solve equations (12), (13), (16), (17), and (18) for A, B, A′, B′.

Since $\beta=-(1+u)^2/u$, equation (13) gives

$$A'u+B'v=Av+Bu-\frac{(1+u)^2}{2u}W.$$

Combining this with (12) we get

$$\left.\begin{aligned} A' &= B + \frac{1+u}{1-u}\frac{W}{2} \\ B' &= A - \frac{1+u}{1-u}\frac{W}{2} \end{aligned}\right\}; \quad \ldots\ldots \quad (19)$$

from (17) and (18) $R_1(2+\beta)+R_2+T_0a=0$,

$$\therefore\ T_0a = Au^m + Bv^m. \quad \ldots\ldots \quad (20)$$

Substituting this value of T_0 in (17) we get

$$Au^{m+1}\{v - \frac{1}{a}(1+v)\} + Bv^m\{u - \frac{1}{a}(1+u)\} = \tfrac{1}{2}W\frac{n}{N}(1+u).$$

Let $\frac{1}{a}(1+u) - u = \phi$, and $\frac{1}{a}(1+v) - v = \psi$,

so that

$$\phi - u\psi = 1 - u;$$

then this equation may be written

$$A\psi u^{m+1} + B\phi v^m = -\tfrac{1}{2}W\frac{n}{N}(1+u). \quad \ldots \quad (21)$$

To get another equation for A and B we must sum the series in equation (16): the result is

$$\begin{aligned} \frac{1}{a}(Au^m + Bv^m) &= p_0W - (-1)^m A\frac{1-(-u)^m}{1+u} \\ &- (-1)^m B\frac{1-(-v)^m}{1+v} + (-1)^m A'u\frac{1-(-u)^{n-1}}{1+u} \\ &+ (-1)^m B'v\frac{1-(-v)^{n-1}}{1+v} - (-1)^N\frac{1}{a}(A'u^n + B'v^n), \end{aligned}$$

which reduces to

$$\begin{aligned} Av^n + (-1)^N Bv^m = \left\{\frac{\phi - \psi u^{2n+1}}{1-u}v^n - \frac{m+(-1)^N n}{N}\right\} \\ \times \frac{1+u}{\phi + (-1)^N\psi u^{N+1}}\frac{W}{2}, \quad \ldots \quad (22) \end{aligned}$$

and solving equations (21) and (22) we get

$$A = \left\{\frac{\phi(1-u^n)(\phi+\psi u^{n+1})}{(1-u)(\phi+\psi u^{N+1})} + \frac{n}{N}u^n\right\}\frac{1+u}{\phi-\psi u^{N+1}}\frac{W}{2}, \quad (23)$$

$$B = -\left\{\frac{\psi(1-u^n)(\phi+\psi u^{n+1})u^{m+1}}{(1-u)(\phi+\psi u^{N+1})} + \frac{n}{N}\right\}\frac{(1+u)u^m}{\phi-\psi u^{N+1}}\frac{W}{2}. \quad (24)$$

We get A' and B' by interchanging m and n in these results, or we can use equations (19).

Thus the solution of Maxwell's equations for the tensions of the redundant bars of our frame has been obtained in a fairly simple and concise form.

$$R_\rho(\rho \text{ from } 1 \text{ to } m) = Au^{m-\rho} + Bu^{\rho-m},$$

$$R_\rho(\rho \text{ from } m \text{ to } N-1) = A'u^{\rho-m} + B'u^{m-\rho} = Au^{m-\rho} + Bu^{\rho-m} + \frac{1+u}{1-u}\frac{W}{2}(u^{\rho-m} - u^{m-\rho}).$$

Thus the general expression for R_ρ (ρ from 1 to $N-1$) may be written

$$\frac{W}{2}\frac{(1+u)(1-u^n)(\phi+\psi u^{n+1})(\phi-\psi u^{2\rho+1})}{(1-u)(\phi^2-\psi^2 u^{2N+2})}u^{m-\rho} + \frac{n}{N}\frac{W}{2}\frac{(1+u)(u^{N-\rho}-u^\rho)}{\phi-\psi u^{N+1}} + \left[\frac{W}{2}\frac{1+u}{1-u}(u^{\rho-m}-u^{m-\rho})\right], \quad (25)$$

where the term in the square brackets is to be omitted when $\rho - m$ is negative.

This expression is equal to αT_0 when $\rho = 0$, and to αR_N when $\rho = N$. It should be noted that the result involves the extensibilities of the bars and the inclinations of the sloping ones only in the form in which these quantities occur in a and β. When $\nu' = \nu$, ϕ and ψ are each unity. Now $-u$ is a positive proper fraction (except in the limit when ν is infinite); and it is clear that if the intermediate vertical bars are not very slender compared with both horizontal and diagonal bars, u^N will be small, and good approximate results of a simple form will be obtained by neglecting this, and possibly some other powers of u, in comparison with unity.

We have $(1+u)/a = \phi + u$; thus

$$T_0 = \left\{\frac{(1-u^n)(\phi+\psi u^{n+1})}{\phi+\psi u^{N+1}}u^m - \frac{n}{N}(1-u^N)\right\}\frac{\phi+u}{\phi-\psi u^{N+1}}\frac{W}{2}. \quad (26)$$

If we put $\nu = \infty$ in this equation we get results for the girder  which can be easily verified.

The remaining tensions may be found either from Maxwell's equation (2) or from the equations (4) to (7). As equations (7) are not very convenient in form, let us employ equations (2) to find T_a and T_c.

$$\begin{aligned}T_{a\rho}&=p_{a\rho}W+r_{1a\rho}R_1+r_{2a\rho}R_2+\ \ldots\ +r_{Na\rho}R_N\\&=p_{a\rho}W+t\{R_\rho-R_{\rho+1}+\ \ldots\ +(-1)^{N-\rho}R_N\}.\end{aligned}$$

Let ρ be greater than m; then

$$\begin{aligned}T_{a\rho}=p_{a\rho}W&+tA'u^{\rho-m}\left\{1-u+u^2-\ \ldots\ +(-1)^{N-\rho}\frac{1}{a}u^{N-\rho}\right\}\\&+tB'v^{\rho-m}\left\{1-v+v^2-\ \ldots\ +(-1)^{N-\rho}\frac{1}{a}v^{N-\rho}\right\}\end{aligned}$$

$$=p_{a\rho}W+\frac{t}{1+u}\{A'u^{\rho-m}+B'v^{\rho-m-1}+(-1)^{N-\rho}(A'\psi u^{n+1}+B'\phi v^n)\}$$

$$=p_{a\rho}W+\frac{}{1+u}(A'u^{\rho-m}+Bu^{m-\rho+1})-(-1)^{N-\rho}t\,.\,\tfrac{1}{2}W\frac{m}{N}$$

$$=-(N-\rho+\tfrac{1}{2})\frac{m}{N}tW+\frac{t}{1+u}(A'u^{\rho-m}+B'u^{m-\rho+1})$$

$$=-(N-\rho+\tfrac{1}{2})\frac{m}{N}tW+\frac{t}{1+u}(Au^{m-\rho+1}+Bu^{\rho-m})$$

$$-\tfrac{1}{2}tW\frac{1}{1-u}(u^{m-\rho+1}-u^{\rho-m}).$$

When $\rho<m+1$ we have by symmetry

$$T_{a\rho}=-(\rho-\tfrac{1}{2})\frac{n}{N}tW+\frac{t}{1+u}(Au^{m-\rho+1}+Bu^{\rho-m}).$$

Similarly

$$\begin{aligned}T_{c\rho}&=p_{c\rho}W+r_{1c\rho}R_1+\ldots\\&=p_{c\rho}W-s(R_\rho-R_{\rho+1}+\ \ldots).\end{aligned}$$

We have the same summation as before, and when $\rho>m$,

$$T_{c\rho}=p_{c\rho}W-\frac{s}{1+u}(A'u^{\rho-m}+B'u^{m-\rho+1})+(-1)^{N-\rho}s\,.\,\tfrac{1}{2}W\frac{m}{N}$$

$$=\frac{1}{2}\frac{m}{N}sW-\frac{s}{1+u}(A'u^{\rho-m}+B'u^{m-\rho+1})$$

$$=\frac{1}{2}\frac{m}{N}sW-\frac{s}{1+u}(Au^{m-\rho+1}+Bu^{\rho-m})+\tfrac{1}{2}sW\frac{1}{1-u}(u^{m-\rho+1}-u^{\rho-m}).$$

We at once deduce from this the value of $T_{d\rho}$ for the case of $\rho<m+1$; and all the other tensions can be written down with the help of equations (5) and (6), the results being as follows in terms of A and B:—

$\rho<m+1$:—

$$T_{a\rho}=-(\rho-\tfrac{1}{2})\frac{n}{N}tW+\frac{t}{1+u}(Au^{m-\rho+1}+Bu^{\rho-m}$$

$$\mathrm{T}_{b\rho}=(\rho-\tfrac{1}{2})\frac{n}{\mathrm{N}}t\mathrm{W}+\frac{t}{1+u}(\mathrm{A}u^{m-\rho+1}+\mathrm{B}u^{\rho-m}),$$

$$\mathrm{T}_{c\rho}=-\tfrac{1}{2}\frac{n}{\mathrm{N}}s\mathrm{W}-\frac{s}{1+u}(\mathrm{A}u^{m-\rho+1}+\mathrm{B}u^{\rho-m}),$$

$$\mathrm{T}_{d\rho}=\tfrac{1}{2}\frac{n}{\mathrm{N}}s\mathrm{W}-\frac{s}{1+u}(\mathrm{A}u^{m-\rho+1}+\mathrm{B}u^{\rho-m}),$$

$$\mathrm{R}_{\rho}=\mathrm{A}u^{m-\rho}+\mathrm{B}u^{-m}\ \text{(this being } \alpha\mathrm{T}_0 \text{ when } \rho=0\text{)};$$

$\rho>m$:

$$\mathrm{T}_{a\rho}=-(\mathrm{N}-\rho+\tfrac{1}{2})\frac{m}{\mathrm{N}}t\mathrm{W}+\frac{t}{1+u}(\mathrm{A}u^{m-\rho+1}+\mathrm{B}u^{\rho-m})$$
$$-\tfrac{1}{2}t\mathrm{W}\frac{1}{1-u}(u^{m-\rho+1}-u^{\rho-m}),$$

$$\mathrm{T}_{b\rho}=(\mathrm{N}-\rho+\tfrac{1}{2})\frac{m}{\mathrm{N}}t\mathrm{W}+\frac{t}{1+u}(\mathrm{A}u^{m-\rho+1}+\mathrm{B}u^{\rho-m})$$
$$-\tfrac{1}{2}t\mathrm{W}\frac{1}{1-u}(u^{m-\rho+1}-u^{\rho-m}),$$

$$\mathrm{T}_{c\rho}=\tfrac{1}{2}\frac{m}{\mathrm{N}}s\mathrm{W}-\frac{s}{1+u}(\mathrm{A}u^{m-\rho+1}+\mathrm{B}u^{\rho-m})$$
$$+\tfrac{1}{2}s\mathrm{W}\frac{1}{1-u}(u^{m-\rho+1}-u^{\rho-m}),$$

$$\mathrm{T}_{d\rho}=-\tfrac{1}{2}\frac{m}{\mathrm{N}}s\mathrm{W}-\frac{s}{1+u}(\mathrm{A}u^{m-\rho+1}+\mathrm{B}u^{\rho-m})$$
$$+\tfrac{1}{2}s\mathrm{W}\frac{1}{1-u}(u^{m-\rho+1}-u^{\rho-m}),$$

$$\mathrm{R}_{\rho}=\mathrm{A}u^{m-\rho}+\mathrm{B}u^{\rho-m}-\tfrac{1}{2}\mathrm{W}\frac{1+u}{1-u}(u^{m-\rho}-u^{\rho-m})$$

(this being $\alpha\mathrm{R}_{\mathrm{N}}$ when $\rho=\mathrm{N}$).

In these expressions the values which have been found for A and B can be inserted. We get at once

$$\frac{1}{1+u}(\mathrm{A}u^{m-\rho+1}+\mathrm{B}u^{\rho-m})=\tfrac{1}{2}\mathrm{W}\frac{1}{\phi-\psi u^{\mathrm{N}+1}}$$
$$\times\left\{\frac{(1-u^{n})(\phi+\psi u^{n+1})(\phi u^{-\rho}-\psi u^{\rho})u^{m+1}}{(1-u)(\phi+\psi u^{\mathrm{N}+1})}\right.$$
$$\left.+\frac{n}{\mathrm{N}}(u^{\mathrm{N}-\rho+1}-u^{\rho})\right\}. \quad . \quad (27)$$

The full expression for R_{ρ} has already been written down.

Let us now calculate the deflection of the girder at a point distant k bays from A and l bays from B. The deflection is by Maxwell's formula

$$\mathrm{W}\Sigma epq+\mathrm{R}_1\Sigma eqr_1+\mathrm{R}_2\Sigma eqr_2+\dots \quad . \quad . \quad (28)$$

and to find the q's we have only to substitute k and l respectively for m and n in the expression for the p's. Let us first deal with the case in which $k<m+1$; when we have found the deflection for this portion of the girder the deflection for the remainder can be obtained by symmetry,

$$\Sigma epq=\nu' p_0 q_0+\lambda t^2\Bigg\{\sum_{\rho=0}^{\rho=k-1}\left(\rho\frac{n}{\mathrm{N}}+1'\right)\left(\rho\frac{l}{\mathrm{N}}+1''\right)$$

$$+\sum_{\rho=1}^{\rho=k}\left(\rho\frac{n}{\mathrm{N}}-1'\right)\left(\rho\frac{l}{\mathrm{N}}-1''\right)+\sum_{\rho=k}^{\rho=m-1}\left(\rho\frac{n}{\mathrm{N}}+1'\right)(\mathrm{N}-\rho)\frac{k}{\mathrm{N}}$$

$$+\sum_{\rho=k+1}^{\rho=m}\left(\rho\frac{n}{\mathrm{N}}-1'\right)(\mathrm{N}-\rho)\frac{k}{\mathrm{N}}+(\mathrm{N}-m)^2\frac{mk}{\mathrm{N}^2}$$

$$+2\sum_{\rho=m+1}^{\rho=\mathrm{N}-1}(\mathrm{N}-\rho)^2\frac{mk}{\mathrm{N}^2}\Bigg\}+\mu s^2\Bigg\{k'''+k\left(\frac{n}{\mathrm{N}}-1'\right)\left(\frac{l}{\mathrm{N}}-1''\right)$$

$$+(m-k)\left(-\frac{n}{\mathrm{N}}+1'\right)\frac{k}{\mathrm{N}}+n\frac{mk}{\mathrm{N}^2}\Bigg\},$$

where single dashes indicate that the term in question is to be omitted if n is even, and double dashes that it is to be omitted if l is even. Performing the summations indicated we find that

$$\Sigma epq=\nu' p_0 q_0+\tfrac{1}{3}\lambda t^2\frac{kn}{\mathrm{N}}(\mathrm{N}^2+1-n^2-k^2)+\mu s^2\frac{kn}{\mathrm{N}}$$

$$+(\lambda t^2+\mu s^2)\frac{k}{\mathrm{N}}(2\mathrm{N}'''-n'-n'');$$

and the last term may be written

$$(\lambda t^2+\mu s^2)\frac{k}{2\mathrm{N}}\{[1-(-1)^n-(-1)^l]m-n+(-1)^{n+l}\mathrm{N}\}.$$

Now when $\rho<k+1$

$$\Sigma eqr_\rho=(-1)^{\rho+1}q_0\nu'+(-1)^{\mathrm{N}-\rho}\rho\left\{\frac{k}{\mathrm{N}}-(-1)^l\right\}(\lambda t^2+\mu s^2),$$

and when $\rho>k$

$$\Sigma eqr_\rho=(-1)^{\rho+1}q_0\nu'+(-1)^{\mathrm{N}-\rho}k\left\{\frac{\rho}{\mathrm{N}}-(-1)^l\right\}(\lambda t^2+\mu s^2).$$

Thus $R_1 \Sigma eqr_1 + R_2 \Sigma eqr_2 + \ldots$

$$= q_0 \nu'(R_1 - R_2 + \ldots) - (-1)^N \frac{k}{N}(R_1 - 2R_2 + 3R_3 - \ldots)(\lambda t^2 + \mu s^2)$$

$$+ (-1)^k \{R_1 - 2R_2 - \ldots + (-1)^{k+1} k R_k\} (\lambda t^2 + \mu s^2)$$

$$+ k\{R_{k+1} - R_{k+2} + \ldots + (-1)^{l+1} R_N\} (\lambda t^2 + \mu s^2).$$

Now

$$R_1 - R_2 + \ldots = \frac{1}{\alpha}(Au^m + Bv^m) - p_0 W,$$

and the other sums can be evaluated in terms of A, B, u, v, and, finally, we get for the value of the whole expression

$$\frac{\nu}{2}\left\{ -2\alpha p_0 q_0 W - \frac{l}{N}(Au^m + Bv^m) - \frac{k}{N}(A'u^n + B'v^n) + Au^{m-k} + Bv^{m-k} \right\}$$

$$-\tfrac{1}{2}(\lambda t^2 + \mu s^2) W \frac{k}{N} \{[1 - (-1)^n - (-1)^l] m + (-1)^{k+m} N\}.$$

Thus

$$W\Sigma epq + R_1 \Sigma eqr_1 + R_2 \Sigma eqr_2 + \ldots = \tfrac{1}{3} W \lambda t^2 \frac{kn}{N}(N^2 - n^2 - k^2 - \tfrac{1}{2})$$

$$+ \tfrac{1}{2} W \mu s^2 \frac{kn}{N} - \frac{\nu}{2}\left\{ \frac{\alpha}{N}(lT_0 + kR_N) - R_k \right\}. \quad . \quad (29)$$

This is the deflection at the point k, l when $k < m+1$, and by symmetry we see that the deflection at this point when $k > m$ is

$$\tfrac{1}{3} W \lambda t^2 \frac{lm}{N^2}(N^2 - m^2 - l^2 - \tfrac{1}{2}) + \tfrac{1}{2} W \mu s^2 \frac{lm}{N}$$

$$- \frac{\nu}{2}\left\{ \frac{\alpha}{N}(lT_0 + kR_N) - R_k \right\}. \quad . \quad (30)$$

If we substitute for T_0, R_N, and R_k the values which we have found for them (25), we get for the deflection when $k < m+1$,

$$\tfrac{1}{3} W \lambda t^2 \frac{kn}{N}(N^2 - n^2 - k^2 - \tfrac{1}{2}) + \tfrac{1}{2} W \mu s^2 \frac{kn}{N}$$

$$+ \tfrac{1}{4} W \nu \frac{1+u}{\phi - \psi u^{N+1}} \left\{ \frac{(1-u^n)(1-u^k)(\phi + \psi u^{n+1})(\phi u^{m-k} + \psi u^{m+1})}{(1-u)(\phi + \psi u^{N+1})} \right.$$

$$\left. + \frac{1}{N^2}(mk + ln)(1 - u^N) + \frac{k}{N}(u^m - u^n) - \frac{n}{N}(u^k - u^l) \right\}; \quad .(31)$$

and when $k > m$,

$$\tfrac{1}{3}\mathrm{W}\lambda t^2\frac{lm}{\mathrm{N}^2}(\mathrm{N}^2-m^2-l^2-\tfrac{1}{2})+\tfrac{1}{2}\mathrm{W}\mu s^2\frac{lm}{\mathrm{N}}$$

$$+\tfrac{1}{4}\mathrm{W}\nu\frac{1+u}{\phi-\psi u^{\mathrm{N}+1}}\left\{\frac{(1-u^n)(t-u^k)(\phi+\psi u^{n+1})(\phi u^{m-k}+\psi u^{m+1})}{(1-u)(\phi+\psi u^{\mathrm{N}+1})}\right.$$

$$\left.+\frac{1}{\mathrm{N}^2}(mk+ln)(1-u^{\mathrm{N}})+\frac{k}{\mathrm{N}}(u^m-u^n)-\frac{n}{\mathrm{N}}(u^k-u^l)\right\}$$

$$+\tfrac{1}{4}\mathrm{W}\nu\frac{1+u}{1-u}(u^{k-m}-u^{m-k}). \quad \dots \dots \dots \quad (32)$$

We can obtain another expression for the deflection at a point at which $k>m$ by interchanging m, n and l, k throughout the expression (31).

We are now in a position to find by a process of summation the tensions of the bars and the deflection at any point for the case of uniform loading, a weight W being attached to each of the lower joints, the total load being (N−1)W. It will be seen that the results in this case are, in consequence of the symmetry of the loading, somewhat simpler than those obtained for the case of a single load. To obtain R_ρ for this case we have to sum the values of the expression (25) for a single load for all values of m from 1 to N−1, n varying from N−1 to 1. The result is

$$\tfrac{1}{2}\mathrm{W}\frac{(1+u)(\phi-\psi u^{2\rho+1})}{(1-u)(\phi^2-\psi^2u^{2\mathrm{N}+2})}\left\{(\phi u^{-\rho}-\psi u^{\mathrm{N}-\rho-1})\frac{u-u^{\mathrm{N}}}{1-u}-(\mathrm{N}-1)u^{\mathrm{N}-\rho}(1-u)\right\}$$

$$+\tfrac{1}{4}(\mathrm{N}-1)\mathrm{W}\frac{(1+u)(u^{\mathrm{N}-\rho}-u^{\rho})}{\phi-\psi u^{\mathrm{N}+1}}+\tfrac{1}{2}\mathrm{W}\frac{1+u}{1-u}\sum_{\sigma=0}^{\sigma=\rho-1}(u^\sigma-u^{-\sigma}),$$

which reduces to

$$-\tfrac{1}{2}\mathrm{W}\frac{(1+u)(u^\rho+u^{\mathrm{N}-\rho})}{\phi+\psi u^{\mathrm{N}+1}}\left\{\frac{\mathrm{N}-1}{2}+\frac{\phi+\psi u^2}{(1-u^2)}\right\}+\tfrac{1}{2}\mathrm{W}\left(\frac{1+u}{1-u}\right)^2; \quad (33)$$

this is $\alpha\mathrm{T}_0$, which is equal to $\alpha\mathrm{R}_{\mathrm{N}}$, when ρ is 0 or N.

We have some simple relations as before between the other tensions, but these it is hardly necessary to use, for the principal summation is the same for all of them, that is to say we have to evaluate

$$\frac{1}{1+u}\sum_{m=1}^{m=\mathrm{N}-1}(\mathrm{A}u^{m-\rho+1}+\mathrm{B}u^{\rho-m})-\tfrac{1}{2}\mathrm{W}\frac{1}{1-u}\sum_{m=1}^{m=\rho-1}(u^{m-\rho+1}-u^{\rho-m}).$$

Call this expression Σ, then

$$\mathrm{T}_{a\rho}=-\sum_{n=1}^{n=\mathrm{N}-\rho}(\rho-\tfrac{1}{2})\frac{n}{\mathrm{N}}t\mathrm{W}-\sum_{m=1}^{m=\rho-1}(\mathrm{N}-\rho+\tfrac{1}{2})\frac{m}{\mathrm{N}}t\mathrm{W}+t\Sigma$$

$$=-\tfrac{1}{2}\{(\mathrm{N}-\rho+\tfrac{1}{2})(\rho-\tfrac{1}{2})-\tfrac{1}{4}\}t\mathrm{W}+t\Sigma.$$

Similarly,

$$T_{b\rho}=\tfrac{1}{2}\{(N-\rho+\tfrac{1}{2})(\rho-\tfrac{1}{2})-\tfrac{1}{4}\}tW+t\Sigma,$$
$$T_{c\rho}=-\tfrac{1}{4}(N-2\rho+1)sW-s\Sigma,$$
$$T_{d\rho}=\tfrac{1}{4}(N-2\rho+1)sW-s\Sigma.$$

Now

$$\Sigma=\sum_{m=1}^{m=N-1}\tfrac{1}{2}W\frac{1}{\phi-\psi u^{N+1}}\left\{\frac{(1-u^{n})(\phi+\psi u^{n+1})(\phi u^{-\rho}-\psi u^{\rho})u^{m+1}}{(1-u)(\phi+\psi u^{N+1})}\right.$$
$$\left.+\frac{n}{N}(u^{N-\rho+1}-u^{\rho})\right\}-\frac{1}{2}\frac{1}{1-u}\sum_{m=1}^{m=\rho-1}(u^{m-\rho+1}-u^{\rho-m})$$
$$=-\tfrac{1}{2}W\frac{u^{N-\rho+1}+u^{\rho}}{\phi+\psi u^{N+1}}\left\{\frac{N-1}{2}+\frac{\phi+\psi u^{2}}{(1-u)^{2}}\right\}+W\frac{u}{(1-u)^{2}}.$$

Thus the tensions of all the bars are found.

To find the deflection at the point k, l for the loading under consideration we have to sum the expressions (29) and (30), the former for values of m from l to $N-1$, and the latter for values of m from 1 to $l-1$. Thus the deflection is

$$\tfrac{1}{3}W\lambda t^{2}\frac{l}{N}\frac{k(k+1)}{2}\left\{N^{2}-\frac{k(k+1)}{2}-l^{2}-\tfrac{1}{2}\right\}+\tfrac{1}{2}W\mu s^{2}\frac{l}{N}\frac{k(k+1)}{2}$$
$$+\tfrac{1}{3}W\lambda t^{2}\frac{k}{N}\frac{l(l-1)}{2}\left\{N^{2}-\frac{l(l-1)}{2}-k^{2}-\tfrac{1}{2}\right\}+\tfrac{1}{2}W\mu s^{2}\frac{k}{N}\frac{l(l-1)}{2}$$
$$-\frac{\nu}{2}\left\{\frac{\alpha}{N}(lT_{0}+kR_{N})-R_{k}\right\},$$

T_0, R_N, and R_k being here the tensions for the case of the continuous loading, these being the results of summing the corresponding tensions for the case of a single load. Remembering that $lT_0 + kR_N = NT_0$, and substituting the values which we have found for the tensions T_0 and R_k, the expression for the deflection becomes

$$\tfrac{1}{6}W\lambda t^{2}kl\{N^{2}-1-\tfrac{1}{2}(k^{2}+kl+l^{2})\}+\tfrac{1}{4}W\mu s^{2}kl$$
$$+\tfrac{1}{4}W\nu\frac{1+u}{\phi+\psi u^{N+1}}\left\{\frac{N-1}{2}+\frac{\phi+\psi u^{2}}{(1-u)^{2}}\right\}(1+u^{N}-u^{k}-u^{l}),$$

or

$$\tfrac{1}{12}W\lambda t^{2}kl(N^{2}+kl-2)+\tfrac{1}{4}W\mu s^{2}kl$$
$$+\tfrac{1}{4}W\nu\frac{(1+u)(1-u^{k})(1-u^{l})}{\phi+\psi u^{N+1}}\left\{\frac{N-1}{2}+\frac{\phi+\psi u^{2}}{(1-u)^{2}}\right\}. \quad . \quad . \quad (34)$$

The results obtained for continuous uniform loading may be written concisely as follows. Let

$$\frac{1}{\phi+\psi u^{N+1}}\left\{\frac{N-1}{2}+\frac{\phi+\psi u^2}{(1-u)^2}\right\}=\omega,$$

and let x and y be the number of bays from A and B respectively to centre of bar in the case of a tension formula, or to point of deflection in the case of the deflection formula, then

$$T_a=-\tfrac{1}{2}tW\left\{xy-\tfrac{1}{4}+(u^{x+\frac{1}{2}}+u^{y+\frac{1}{2}})\omega-\frac{2u}{(1-u)^2}\right\}, \quad . \; . \quad (35)$$

$$T_b=\tfrac{1}{2}tW\left\{xy-\tfrac{1}{4}-(u^{x+\frac{1}{2}}+u^{y+\frac{1}{2}})\omega+\frac{2u}{(1-u)^2}\right\}, \quad . \quad (36)$$

$$T_c=\tfrac{1}{2}sW\left\{\tfrac{1}{2}(x-y)+(u^{x+\frac{1}{2}}+u^{y+\frac{1}{2}})\omega-\frac{2u}{(1-u)^2}\right\}, \quad . \; . \quad (37)$$

$$T_d=\tfrac{1}{2}sW\left\{\tfrac{1}{2}(y-x)+(u^{x+\frac{1}{2}}+u^{y+\frac{1}{2}})\omega-\frac{2u}{(1-u)^2}\right\}, \quad . \; . \quad (38)$$

$$R=\tfrac{1}{2}W(1+u)\left\{-(u^x+u^y)\omega+\frac{1+u}{(1-u)^2}\right\}, \quad . \; . \; . \quad (39)$$

$$\text{Deflection}=\tfrac{1}{4}W\left\{\tfrac{1}{3}\lambda t^2xy(N^2+xy-2)+\mu s^2xy\right.$$
$$\left.+\nu(1+u)(1-u^x)(1-u^y)\omega\right\}. \quad . \; . \; . \quad (40)$$

Here, as before, the expression (39), when $x=0$ and $y=N$, is equal to αT_0, which is equal to αR_N.

These results appear to be sufficiently simple and concise. The numerical values of u and ω can readily be calculated for a given girder. Referring to the quadratic equation which u satisfies, we see that

$$\frac{2u}{(1-u)^2}=-\frac{2}{4+\beta}, \quad \left(\frac{1+u}{1-u}\right)^2=\frac{\beta}{4+\beta},$$

and $$u=-1-\tfrac{1}{2}\beta+\tfrac{1}{2}\beta^{\frac{1}{2}}(4+\beta)^{\frac{1}{2}}.$$

The data of the question, admitting only four different sections of members of the frame, do not permit the sections to be all proportioned to the tensions. The results obtained can, however, be compared with those given by other less accurate methods.

Let us regard the girder as a uniform beam, loaded with N − 1 weights, each equal to W, at equal intervals along it, and apply the ordinary Bernoulli-Euler method for determining the deflection at any point. If, in calculating the moment of inertia of a section of the beam, we take account only of the two horizontal booms, neglecting the web, we readily find that the deflection at a point at a distance x from one end of the beam and y from the other end (the unit of length being the length of a bay) is

$$\tfrac{1}{12}W\lambda t^2 xy(N^2+xy) \; . \quad . \quad . \quad . \quad . \quad . \qquad (41)$$

In comparing this result with the expression (40), we must suppose that x and y can only have integral values from 1 to $N-1$; thus the value of xy ranges from $N-1$ to $\frac{1}{4}N^2$ or to $\frac{1}{4}(N^2-1)$ according as N is even or odd. Accordingly (41) comprises what may be expected to be generally the most important part of (40).

However, if, as will naturally be the case, $\mu s^2 > \frac{2}{3}\lambda t^2$, the remaining terms of (40), which may be by no means insignificant, will certainly be positive; whereas the terms omitted in (41) by neglecting the moment of inertia of the web will be negative.

Let us take a numerical example. Let the cross-sections be 8ζ for horizontal bars, 2ζ for diagonals, 4ζ for end verticals, and ζ for intermediate verticals; let $N=9$ and $\theta=45°$, so that $t=1$ and $s=\surd 2$. Then, taking the depth of the girder or the length of a horizontal bar to be unity as before, and writing E for Young's modulus,

$$\lambda=\frac{1}{8\zeta E}, \quad \mu=\frac{\surd 2}{2\zeta E}, \quad \nu=\frac{1}{\zeta E}, \quad \nu'=\frac{1}{4\zeta E},$$

$$\beta=3\cdot078, \quad u=-\cdot205, \quad v=-4\cdot873,$$

$$\frac{1}{\alpha}=4, \quad \phi=3\cdot38, \quad \psi=-10\cdot62, \quad \omega=1\cdot78,$$

$$\mu s^2-\tfrac{2}{3}\lambda t^2=1\cdot33\frac{1}{\zeta E},$$

and the values of the several terms of our formula are given by the following table:—

	$x=1,\ y=8.$	$x=2,\ y=7.$	$x=3,\ y=6.$	$x=4,\ y=5.$
$\frac{1}{3}\lambda t^2 xy(N^2+xy)\zeta E$	29·7	55·4	74·2	84·2
$(\mu s^2-\frac{2}{3}\lambda t^2)xy\zeta E$	10·6	18·6	23·9	26·6
$\nu(1+u)(1-u^x)(1-u^y)\omega\zeta E$ ...	1·7	1·4	1·4	1·4

We see that in this case the term $(\mu s^2-\frac{2}{3}\lambda t^2)xy$ is of considerable importance, and that to make it small compared with (41) the value of μ must be considerably reduced. Let us suppose the diagonal members to be of the same section as the horizontals, keeping the latter of the same section as before; then

$$\mu s^2 - \tfrac{2}{3}\lambda t^2 = \cdot 27 \frac{1}{\zeta E},$$

and the four successive values of $(\mu s^2 - \frac{2}{3}\lambda t^2) xy \zeta E$ become respectively 2·2, 3·8, 4·9, 5·4;

$$\beta = \cdot 96,\quad u = -\cdot 39,\quad v = -2\cdot 57,\quad \phi = 2\cdot 8,\quad \psi = -3\cdot 7,\quad \omega = 1\cdot 83,$$

and the four successive values of $\nu(1+u)(1-u^x)(1-u^y)\omega\zeta E$ are 1·5, ·9, 1·2, and 1·1.

Here the expression (41) gives a comparatively good result.

It is clear that we ought not to correct (41) by introducing a value for moment of inertia of web, for this would be an alteration in the wrong direction.

The expression ω may be written

$$\frac{\frac{1}{2}(N-1) + \frac{1}{\alpha}\left(\frac{1+u}{1-u}\right)^2 - \frac{2u}{(1-u)^2}}{\frac{1}{\alpha}(1+u)(1+u^N) - u - u^N};$$

so it is easy to estimate the importance of the term involving ω provided that u is not nearly equal to -1.

In the limiting case in which $\nu = \infty$ we get the frame

 ;

we have $\alpha = 0$, $\beta = 0$, $u = v = -1$; when N is even $\omega = 0$, when N is odd $\omega = \frac{1}{2(N+1)}$; and the expression

$$\nu(1+u)(1-u^x)(1-u^y)\omega$$

has the following values:—

N even and x even . . .	0,
N even and x odd . . .	$2(\lambda t^2 + \mu s^2)$,
N odd and x even . . .	$\frac{2x}{N+1}(\lambda t^2 + \mu s^2)$,
N odd and x odd	$\frac{2y}{N+1}(\lambda t^2 + \mu s^2)$.

In the general case let us suppose that u^N is negligible compared with unity, then

$$\omega=\frac{\frac{1}{2}(\mathrm{N}-1)+\frac{1}{\alpha}\left(\frac{1+u}{1-u}\right)^2-\frac{2u}{(1-u)^2}}{\frac{1}{\alpha}(1+u)-u},$$

and

$$\mathrm{T}_0=-\tfrac{1}{2}\mathrm{W}\frac{1+u}{1+u-\alpha u}\left\{\tfrac{1}{2}(\mathrm{N}-1)-\frac{u}{1-u}\right\}.$$

Thus the condition that $-\mathrm{T}_0$ is equal to a quarter of the total load, namely $\frac{1}{4}(\mathrm{N}-1)\mathrm{W}$, is

$$\tfrac{1}{2}(\mathrm{N}-1)=\frac{1}{\alpha}\frac{1+u}{1-u},$$

or

$$\alpha=\frac{2}{\mathrm{N}-1}\left(\frac{\beta}{4+\beta}\right)^{\frac{1}{2}}.$$

Thus, when $\mathrm{N}=9$ and $u=-\cdot205$, $-\mathrm{T}_0$ will be equal to a quarter of the total load if the end verticals are of about 6 times the strength of the intermediate ones; when $\mathrm{N}=9$ and $u=-\cdot39$ the same result will be secured if the end verticals are about 9 times the strength of the intermediate ones. By means of such results as these, we may form an estimate of the conditions under which the method, sometimes employed, of calculating the tensions of the bars of a redundant frame by considering it as the result of the superposition of two just stiff frames, can be regarded as in any degree trustworthy.

Certain results for the girder  can, as we remarked above, be deduced from the case already considered by making the extensibilities of the intermediate vertical bars all infinite. But, as we have in this frame only one redundant member, we can, without inconvenience, take a more general case than we have hitherto dealt with with regard to the sections or extensibilities of the bars, or even suppose these to be all different. Let us use the same notation as before for distinguishing the bars of the frame, and take as our redundant bar the vertical bar which bounds the Nth panel, its extensibility being ϵ and its tension R.

Then

$$r_0=(-1)^{\mathrm{N}+1},\quad r_{a\sigma}=r_{b\sigma}=(-1)^{\mathrm{N}-\sigma}t,\quad r_{c\sigma}=r_{d\sigma}=-(-1)^{\mathrm{N}-\sigma}s,$$

and for a single load at the lower joint m, n, the p's are the same as before. For the single load W, R is given by the equation

$$\mathrm{R}(\epsilon+\Sigma er^2)+\mathrm{W}\,\Sigma erp=0, \quad . \quad . \quad . \quad . \qquad (42)$$

and R having been determined, all the other tensions can be written down at once by resolving the forces at each joint, and by summing we can deduce results for any system of loading.

Although no case presents any difficulty, it is well to assume some symmetry of structure in order to obtain results which are concise enough to be of interest. It will be noticed that if N is even and the structure and loading are both symmetrical about the vertical line through the centre of the girder (the loading being wholly on the lower boom), $\mathrm{T}_{c\frac{1}{2}\mathrm{N}}$ and $\mathrm{T}_{d\frac{1}{2}\mathrm{N}+1}$ must both be zero; accordingly, the bars c, $\frac{1}{2}\mathrm{N}$ and d, $\frac{1}{2}\mathrm{N}+1$ may be removed without any effect upon the tensions of the other bars. But this removal reduces the frame to the state of being just stiff, so all the tensions are independent of the distribution of extensibilities, and can be found by resolving the forces at the joints.

Let us suppose that the girder is symmetrical about the horizontal line through its centre and in its plane. With this assumption very simple expressions can be found for the tensions. We will also suppose that it is symmetrical about the vertical line through its centre, since all actual girders have this property. We have then $e_{a\sigma}=e_{b\sigma}$, let us call each of these λ_σ; we have also $e_{c\sigma}=e_{d\sigma}$, let us call each of these μ_σ. And by virtue of the symmetry about the vertical line $\lambda_\sigma=\lambda_{\mathrm{N}-\sigma+1}$, and $\mu_\sigma=\mu_{\mathrm{N}-\sigma+1}$; also $e_0=\epsilon$, let us call each of these ν.

Then for a single load at the lower joint m, n, we have

$$\begin{aligned}\Sigma erp&=\nu r_0p_0+t^2\left\{-\frac{n}{\mathrm{N}}+[2]\right\}\sum_1^m\lambda+t^2\frac{m}{\mathrm{N}}\sum_{m+1}^{\mathrm{N}}\lambda\\&\qquad-s^2\left\{\frac{n}{\mathrm{N}}-[2]\right\}\sum_1^m\mu+s^2\frac{m}{\mathrm{N}}\sum_{m+1}^{\mathrm{N}}\mu\\&=\nu r_0p_0+\frac{m}{\mathrm{N}}(t^2\Sigma\lambda+s^2\Sigma\mu)\\&\qquad-(-1)^n\Big(t^2\sum_1^m\lambda+s^2\sum_1^m\mu\Big),\end{aligned}$$

and

$$r_0p_0=-\tfrac{1}{2}(-1)^n+\frac{1}{2\mathrm{N}}\{m+(-1)^{\mathrm{N}}n\},$$

also

$$\epsilon+\Sigma er^2=2(\nu+t^2\,\Sigma\lambda+s^2\Sigma\,\mu).$$

Thus the expression for R is very simple, namely,

$$-\tfrac{1}{2}\frac{m}{\mathrm{N}}\mathrm{W}+\frac{(-1)^n(\frac{1}{2}\nu+t^2\sum_1^m\lambda+s^2\sum_1^m\mu)+\dfrac{\nu}{2\mathrm{N}}\{m-(-1)^{\mathrm{N}}n\}}{2(\nu+t^2\Sigma\,\lambda+s^2\Sigma\,\mu)}\mathrm{W}.$$

If N is even and $m=n$,

$$\Sigma\,\lambda=2\sum_1^m\lambda,\qquad \Sigma\,\mu=2\sum_1^m\mu,$$

and

$$\mathrm{R}=-\tfrac{1}{4}\{1-(1)^n\}\mathrm{W},$$

which verifies the remark made above.

For the case of a uniform loading W on each of the N−1 lower joints,

$$\mathrm{R}(\epsilon+\Sigma er^2)+\mathrm{W}\Sigma\Sigma erp=0.$$

Here $\Sigma\Sigma erp$ means the summation of all values of Σerp for different values of m and n. To evaluate it let us find first the part of it which belongs to the two bars $a\sigma$ and $b\sigma$. We have

$$p_{a\sigma}+p_{b\sigma}=(-1)^{\mathrm{N}-\sigma}\left(-\frac{n}{\mathrm{N}}+\;[2]\right)t\quad(\sigma=1\text{ to }m),$$

$$p_{a\sigma}+p_{b\sigma}=(-1)^{\mathrm{N}-\sigma}\frac{m}{\mathrm{N}}t\qquad\qquad(\sigma=m+1\text{ to N}),$$

the term in square brackets to be kept only if n is odd. Thus the part of $\Sigma\Sigma erp$ for the bars $a\sigma$ and $b\sigma$ is

$$\lambda_\sigma t^2\frac{1}{\mathrm{N}}\left\{\frac{\sigma(\sigma-1)}{2}-\frac{(\mathrm{N}-\sigma)(\mathrm{N}-\sigma+1)}{2}+(\mathrm{N}-\sigma)\mathrm{N}+[\mathrm{N}]\right\},$$

the term in square brackets here to be kept only if N−σ is odd ; this reduces to

$$\tfrac{1}{2}\lambda_\sigma t^2\{\mathrm{N}-(-1)^{\mathrm{N}-\sigma}\}.$$

Similarly the part of $\Sigma\Sigma erp$ belonging to the bars $c\sigma$ and $d\sigma$ is

$$\tfrac{1}{2}\mu_\sigma s^2\{\mathrm{N}-(\quad 1)^{\mathrm{N}-\sigma}\},$$

and

$$\Sigma \nu r_0 p_0 = -\frac{\nu}{2}\Sigma(-1)^n + \frac{\nu}{2\mathrm{N}}\Sigma m + (-1)^{\mathrm{N}}\frac{1}{2\mathrm{N}}\Sigma n$$
$$= \tfrac{1}{4}\nu\mathrm{N}\{(-1)^{\mathrm{N}}+1\},$$

thus

$$\Sigma\Sigma erp = \frac{(-1)^{\mathrm{N}}+1}{2}\cdot\tfrac{1}{2}\mathrm{N}\nu + \tfrac{1}{2}\mathrm{N}(t^2\Sigma\lambda + s^2\Sigma\mu)$$
$$-\tfrac{1}{2}\{t^2\Sigma(-1)^{\mathrm{N}-\sigma}\lambda_\sigma + s^2\Sigma(-1)^{\mathrm{N}-\sigma}\mu_\sigma\}.$$

Thus we get

$$\mathrm{R} = -\tfrac{1}{4}\mathrm{NW} + \frac{\tfrac{1}{2}\{1-(-1)^{\mathrm{N}}\}\mathrm{N}\nu + t^2\Sigma(-1)^{\mathrm{N}-\sigma}\lambda_\sigma + s^2\Sigma(-1)^{\mathrm{N}-\sigma}\mu_\sigma}{2(\nu + t^2\Sigma\lambda + s^2\Sigma\mu)}\mathrm{W}$$
$$= -\tfrac{1}{4}(\mathrm{N}-\gamma)\mathrm{W}. \quad \ldots\ldots\ldots \quad (43)$$

When N is even, $\Sigma(-1)^{\mathrm{N}-\sigma}\lambda_\sigma$ and $\Sigma(-1)^{\mathrm{N}-\sigma}\mu_\sigma$ are both zero on account of the symmetry about the vertical line through the centre of the girder, and thus γ is zero.

When N is odd,

$$\gamma = 2\frac{\mathrm{N}\nu - t^2\Sigma(-1)^\sigma\lambda_\sigma - s^2\Sigma(-1)^\sigma\mu_\sigma}{\nu + t^2\Sigma\lambda + s^2\Sigma\mu}. \quad . \; . \; . \quad (44)$$

The values of the remaining tensions can readily be deduced; they are as follows:—

$$\mathrm{T}_{a\sigma} = -\tfrac{1}{2}\mathrm{W}t\{(\sigma-1)(\mathrm{N}-\sigma) + \tfrac{1}{2}\mathrm{N} + \tfrac{1}{2}(-1)^\sigma\gamma\},$$
$$\mathrm{T}_{b\sigma} = \tfrac{1}{2}\mathrm{W}t\{(\sigma-1)(\mathrm{N}-\sigma) + \tfrac{1}{2}\mathrm{N} - 1 - \tfrac{1}{2}(-1)^\sigma\gamma\}$$
$$\mathrm{T}_{c\sigma} = \tfrac{1}{2}\mathrm{W}s\{-\tfrac{1}{2}\mathrm{N} + \sigma + \tfrac{1}{2}(-1)^\sigma\gamma\},$$
$$\mathrm{T}_{d\sigma} = \tfrac{1}{2}\mathrm{W}s\{\tfrac{1}{2}\mathrm{N} - \sigma + 1 + \tfrac{1}{2}(-1)^\sigma\gamma\},$$
$$\mathrm{T}_0 = -\tfrac{1}{4}\mathrm{W}(\mathrm{N}-\gamma).$$

These expressions for the tensions do not depend upon the assumption of any symmetry about the vertical through the centre of the girder, except that the two end vertical bars are equal, and to suppose them to be unequal would not introduce any complication.

If ν is not too great compared with the λ's and μ's, that is to say if the vertical bars are strong enough, we may expect γ to be of a magnitude less important than N; and so we can choose the sections of bars roughly proportional to the tensions throughout, and then calculate γ and determine the exact values of the tensions, and so decide whether it is desirable to make any corrections in the assumed sections, the process being repeated with corrected values of sections if necessary. In the limiting case in which the vertical members are very strong γ will certainly be small, and T_0 will be nearly equal to $\frac{1}{4}$NW.

The case of the common type of girder

is very simple, since Maxwell's problem has only to be applied to the centre panel, and the results are hardly worth writing down.

In practice the joints of a girder are generally rivetted and stiffened with gussets, and the horizontal booms are continuous. The effect of stiffness of the angles is doubtless a matter of secondary importance, but the effect of continuity of the horizontal booms and their resistance to bending, when they are no longer regarded as lines, seems to deserve more attention. Accordingly, let us see how the comparison of results (40) and (41) is modified by this consideration.

Let the lines which we have hitherto taken for the horizontal members in our original girder be replaced by the neutral lines of continuous booms. Let ζ be the area of cross-section of each, and k the radius of gyration of the area about the horizontal line in its plane through its centre of gravity. Then to obtain the Bernoulli-Euler deflection, neglecting the web as before, we must multiply the expression (41) by $\frac{\zeta}{4t^2} \div \left(\frac{\zeta}{4t^2} + \zeta k^2\right)$; thus we get

$$\tfrac{1}{12} W\lambda t^2 xy (N^2 + xy)(1 + 4t^2k^2)^{-1}, \quad . \quad . \quad (45)$$

here $2tk$ is certainly less than the ratio of the depth of a boom to the depth of the girder between the neutral lines.

Now consider the girder as a frame, but with the bottom boom continuous, and let us aim at adding such loading to the bottom boom, at the joints, as shall give it at those points the deflection (40). Write δ for the deflection (41), $\delta + \delta'$ for the deflection (40), and δ_1, δ_1' for the greatest values of δ and δ'. First add a uniform loading $2t^2k^2W$, this by the Bernoulli-Euler theory will give the boom by itself the deflection δ. Now we want to add a loading which will give the boom by itself a deflection δ'; this will not be a uniform loading, but a uniform loading $2t^2k^2W\frac{\delta_1'}{\delta_1}$ which gives the central deflection correctly will give nearly the deflection δ'

elsewhere. Thus for the girder considered as a frame, with the bottom boom only continuous, and a uniform loading $W\left\{1+2t^2k^2\frac{\delta_1+\delta_1'}{\delta_1}\right\}$, we have a deflection agreeing with $\delta+\delta'$ at the centre and very nearly equal to it elsewhere. Now $4t^2k^2$ is a small fraction, squares of which cannot be worth keeping, so the reduction in deflection under loading W, due to the bottom boom being continuous, is very nearly $2t^2k^2\frac{\delta_1+\delta_1'}{\delta_1}(\delta+\delta')$. And if we suppose the effect of continuity of the top boom to be about the same as for the bottom one (it is probably less and hardly can be greater), the whole reduction of deflection due to continuity of both booms is about $4t^2k^2\frac{\delta_1+\delta_1'}{\delta_1}(\delta+\delta')$.

Now the reduction in the result (41) already found (45) is $4t^2k^2\delta$, and the difference between these reductions is

$$4t^2k^2\left\{\delta'+\frac{\delta_1'}{\delta_1}(\delta+\delta')\right\}.$$

So we see that the effect of taking account of the area of section and continuity of booms in both calculations is to reduce the divergence between the results (40) and (41) by the fraction

$$4t^2k^2\left\{1+\frac{\delta_1'}{\delta_1}\left(1+\frac{\delta}{\delta'}\right)\right\}$$

of itself. We shall not be far wrong in substituting for this the fraction $4t^2k^2\left(2+\frac{\delta_1'}{\delta_1}\right)$. Thus, if we calculate the deflection of the girder as a frame with continuous booms, and compare the result with that given by the Bernoulli-Euler theory (leaving out the web in the moment of inertia of the section), we still have substantially the divergence already noted, though it is reduced by a small percentage. Moreover, the remark made above as to the omission of the web still applies.

The reduction in the divergence between the two results, since it can be roughly estimated, may as well be taken account of. Thus, if the depth of boom is $\frac{1}{10}$ of the depth of the girder between the neutral lines, the reduction will be about 2 per cent. or a little more.

V. *Electrical Measurement by Alternating Currents.* By HENRY A. ROWLAND, *Johns Hopkins University, Baltimore, Md.**

[Plate XIX.]

THE electrical quantities pertaining to an electric current which it is usually necessary to measure, in addition to current, electromotive force, watts, &c., are resistances, self and mutual inductances, and capacities. I propose to treat of the measurement of alternating currents, electromotive force, and watts in a separate paper. Resistances are ordinarily best dealt with by continuous currents, except liquid resistances. I propose to treat in this paper, however, mainly of inductances, self and mutual, and of capacities together with their ratios and values in absolute measure as obtained by alternating currents. I also give a few methods of resistance measurement more accurate than usually given by means of telephones or electrodynamometer as commonly used and specially suitable for resistances of electrolytic liquids.

I have introduced many new and some old methods depending upon making the whole current through a given branch circuit equal to zero. These always require two adjustments and they must often be made simultaneously. However, some of them admit of the adjustments being made independently of each other, and these, of course, are the most convenient. But none of these zero methods admit of any great accuracy unless very heavy currents are passed through the resistances. The reason of this is that an electrodynamometer cannot be made nearly as sensitive for small currents as a magnetic galvanometer. The deflexion of an electrodynamometer is as the square of the current. To make it doubly sensitive requires double the number of turns in *both* the coils. Hence we quickly reach a limit of sensitiveness. It is easy to measure an alternating current of ·0001 ampere and difficult for ·00001 ampere. A telephone is more sensitive and an instrument made by suspending a piece of soft iron at an angle of 45°, as invented by Lord Rayleigh, is also probably more sensitive.

For this reason I have introduced here many new methods depending upon adjusting two currents to a phase-difference of 90°, which I believe to be a new principle. This I do by passing one current through the fixed and the other through the suspended coil of an electrodynamometer. By this means a heavy current can be passed through the fixed coils and a

---

* Communicated by the Author.

minute current through the movable coil, thus multiplying the sensitiveness possibly 1000 times over the zero current method.

I have also found that many of the methods become very simple if we use mutual inductances made of wires twisted together and wound into coils. In this way the self-inductances of the coils are all practically equal and the mutual inductances of pairs of coils also equal. Hence we have only to measure the minute difference of these two to reduce the constants of the coil to one constant, and yet by proper connexions we can vary the inductances in many ratios. Three wires is a good number to use. However, the electrostatic induction between the wires must be carefully allowed for or corrected if much greater accuracy than $\frac{1}{100}$ is desired.

By these various methods the measurement of capacities and inductances has been made as easy as the measurement of resistances, while the accuracy has been vastly improved and many sources of error suggested.

Relative results are more accurate than absolute as the period of an alternating current is difficult to determine, and its wave-form may depart from a true sine-curve.

Let self-inductances, mutual inductances, capacities, and resistances be designated by L or l, M or m, C or c, and R or r with the same suffixes when they apply to the same circuit, the mutual inductance having two suffixes. Let b be 2π times the number of complete periods per second, or $b=2\pi n$. The quantities $b\mathrm{L}$, $b\mathrm{M}$ or $\frac{1}{\mathrm{C}}$ are of the dimensions of resistance and thus $\frac{\mathrm{L}}{\mathrm{M}}$, $b^2\mathrm{LC}$ or $b^2\mathrm{MC}$ have no dimensions. $b^2\mathrm{LM}$, $\frac{\mathrm{L}}{\mathrm{C}}$ or $\frac{\mathrm{M}}{\mathrm{C}}$ have dimensions of the square of resistances.

Where we have a mutual inductance M_{12}, we have also the two self-inductances of the coils L_1 and L_2. When these coils are joined in the two possible manners, the self-inductance of the whole is

$$\mathrm{L}_1+\mathrm{L}_2+2\mathrm{M}_{12} \text{ or } \mathrm{L}_1+\mathrm{L}_2-2\mathrm{M}_{12}.$$

In case of a twisted wire coil the last is very small. Likewise $\mathrm{L}_1\mathrm{L}_2-\mathrm{M}^2_{12}$ will be very small for a twisted wire coil, as is found by multiplying the first two equations together.

If there are more coils we can write similar equations. For three coils we have

$$\begin{array}{ll} & \mathrm{L}_1+\mathrm{L}_2+\mathrm{L}_3+2\mathrm{M}_{12}+2\mathrm{M}_{13}+2\mathrm{M}_{23} \\ 1. & \mathrm{L}_1+\mathrm{L}_2+\mathrm{L}_3-2\mathrm{M}_{12}-2\mathrm{M}_{13}+2\mathrm{M}_{23} \\ 2. & \mathrm{L}_1+\mathrm{L}_2+\mathrm{L}_3-2\mathrm{M}_{12}+2\mathrm{M}_{13}-2\mathrm{M}_{23} \\ 3. & \mathrm{L}_1+\mathrm{L}_2+\mathrm{L}_3+2\mathrm{M}_{12}-2\mathrm{M}_{13}-2\mathrm{M}_{23} \end{array}$$

Connecting them in pairs, we have the self-inductances

$$\begin{array}{lll} L_1+L_2+2M_{12} & L_1+L_3+2M_{13} & L_2+L_3+2M_{23} \\ L_1+L_2-2M_{12} & L_1+L_3-2M_{13} & L_2+L_3-2M_{23} \end{array}$$

There are many advantages in twisting the wires of the standard inductance together, but it certainly increases the electrostatic action between the coils. This latter source of error must be constantly in mind, however, and, for great accuracy, calculated and corrected for. But by proper choice of method we may sometimes eliminate it.

For the most accurate standards, I am rather doubtful about the use of twisted wire coils, at least without great caution. But for many purposes it certainly is a great convenience, especially where only an accuracy of one per cent. is desired. In some calculations I have made, I have obtained corrections of from one to one-tenth per cent. from this cause.

For twisted wires the above results reduce to $3L+6M$, $3L-2M$. Similar equations can be obtained for a larger number of wires. For twisted wire coils, n wires joined abreast, the self-induction is $\frac{L+(n-1)M}{n}$, which is practically equal to L or M. The resistance is R/n.

When we have $n=p+m$ wires twisted and wound in a coil and we connect them p direct and m reverse, the resistance and self-induction will be

$$\frac{nR^3+b^2R[AC+BC-nAB]}{(nR)^2+(bC)^2} \quad \text{and} \quad \frac{R^2[n(A+B)-C]+b^2ABC}{(nR)^2+(bC)^2}$$

where R is the resistance of one coil and

$$\begin{aligned} A&=L+(n-1)M \\ B&=L-M \\ C&=nL+(4mp-n)M. \end{aligned}$$

This gives self-inductances and resistances equal to or less than L and R. The correction for electrostatic induction remains to be put in. For the general case, the equation is very complicated for coils abreast, with mutual inductances.

The number of mutual inductances to be obtained is M for two wires, 0, M, 2M for three wires, 0, M, 2M, 3M for four wires, &c. From these results we see that we are always able to reduce mutual to self-inductance. Measuring the self-inductance of a coil connected in different ways, we can always determine the mutual inductances in terms of the self-inductances.

Thus we need not search for methods of directly comparing mutual inductances with each other, although I have given two of these, but we can content ourselves with measuring self-inductances and capacities. Fortunately most of the methods are specially adapted to the latter, the ratio of self-inductance to capacity being capable of being determined with great exactness by many methods.

In the use of condensers I have met with great difficulty from the presence of electric absorption. I have found that this can be represented by a resistance placed in the circuit of the condenser, which resistance is a function of current period.

I have developed Maxwell's theory of electric absorption in this manner. Correcting his equations for a small error, I have developed the resistance and capacity of a condenser as follows:

Let a condenser be made of strata of thicknesses a_1, a_2, &c., and specific inductive capacities k_1, k_2, &c., and resistances p_1, p_2, &c. Then we have

$$R = \frac{B_0}{b^2} - \frac{B_2}{b^4} + \frac{B_4}{b^6} - \text{\&c.},$$

$$\frac{1}{C} = A_0 - \frac{A_2}{b^2} + \frac{A_4}{b^4} - \text{\&c.},$$

where

$$B_0 = \frac{a_1}{r_1 k_1^2} + \frac{a_2}{r_2 k_2^2} + \text{\&c.}$$

$$B_2 = (4\pi)^2 \left\{ \frac{a_1}{r_1^3 k_1^4} + \frac{a_2}{r_2^3 k_2^4} + \text{\&c.} \right\}$$

$$B_4 = (4\pi)^4 \left\{ \frac{a_1}{r_1^5 k_1^6} + \text{\&c.} \right\}$$

&c.

$$A_0 = 4\pi \left\{ \frac{a_1}{k_1} + \frac{a_2}{k_2} + \text{\&c.} \right\}$$

$$A_2 = (4\pi)^3 \left\{ \frac{a_1}{r_1^2 k_1^3} + \frac{a_2}{r_2^2 k_2^3} + \text{\&c.} \right\}$$

&c.

Mr. Penniman has experimented in the Johns Hopkins University laboratory with condensers by method 25 and found some interesting results. With a mica standard condenser of $\frac{1}{8}$ microfarad he was not able to detect any electric absorption, although I have no doubt one of the more accurate methods will show it.

With a condenser, probably of waxed paper, he found :—

Number of complete periods per second.	Capacity in microfarads.	Apparent resistance in ohms.
14·0	4·64	139·6
32·0	4·96	34·1
53·3	4·96	20·5
131·1	4·94	5·2

The first value of the capacity seems to be in error, possibly one of calculation. However, the result seems to show a nearly constant capacity but a resistance increasing rapidly with decrease of period, as Maxwell's formula shows. The constant value of the capacity remains to be explained.

Mr. Penniman will continue the investigation with other condensers, liquid and solid, as well as plates in electrolytic liquids.

The results in the other measurements have been fairly satisfactory, but many of the better methods have only been recently discovered and are thus untried. But we must acknowledge at once that work of the nature here described is most liable to error. Every alternating current has, not only its fundamental period, but also its harmonics, so that very accurate absolute values are almost impossible to obtain without great care. To eliminate them, I propose to use an arrangement of two parallel circuits, one containing a condenser and the other a self-inductance, each with very little resistance. The long-period waves will pass through the second side and the short ones through the condenser-side. By shunting off some of the current from the second side, it will be more free from harmonics than the first one.

However, in a multipolar dynamo, especially one containing iron, there is danger of long-period waves also, which this method might intensify. A second arrangement, using the condenser side, might eliminate them. However, many dynamos without iron and without too many poles and properly wound produce a very good curve without harmonics, especially if the resistance in the circuit is replaced by a self-inductance having no iron. These remarks apply only to absolute determinations. Ratios of inductance, self and mutual, and capacity are independent of the period, and thus it can always be eliminated. Measurements of resistances also are independent.

But there are other errors which one who has worked with continuous currents may fall into. Nearly all alternating currents generate electromagnetic waves which are so strong that currents exist in every closed circuit with any opening between conductors in the vicinity.

We eliminate this source of error by twisting wires together and other expedients. But in avoiding one error, we plunge into another. For, by twisting wires we introduce electrostatic capacity between them, which may vitiate our results. Thus, in methods 23 or 24 for comparing mutual inductances, if there is electrostatic capacity between the wires, a current will flow through the electrodynamometer in the testing circuit and destroy the balance.

Various expedients suggest themselves to eliminate this trouble, as, for instance, the variation of the resistance A in the above ; but I shall reserve them for a future paper. I may say, however, that it is sometimes possible, as in method 12 for instance, to choose a method in which the error does not exist.

However, with the very best method, much rests with the experimenter, as errors from electromagnetic and electrostatic induction are added to errors from defective insulation when we use alternating currents.

These errors are generally less than one per cent., however, and intelligent and careful work reduces them to less than this.

The following methods generally refer by number to Plate XIX., on which the resistances &c. are generally marked. One large circle with a small one inside represents an electrodynamometer. Of course the circuit of the small coil can be interchanged with the large one. Generally we make the smaller current go through the hanging coil.

By the methods 1 to 14, we adjust the electrodynamometer to zero by making the phase-difference in the two coils 90°. For greatest sensitiveness, the currents through the two coils must be the greatest possible, heating being the limit. This current should be first calculated from the impedance of the circuit, as there is danger of making it too great.

In the second series of methods, 15–26, the branch circuit in which the current is to be 0 is indicated by 0.

Resistances in the separate circuits are represented by R R′ R, &c., and r r' r, &c. Corresponding self-inductances and capacities in the same circuits are L L′ L, &c., and l l' l, &c., or C C′ C, &c., and c c' c, &c. $b=2\pi n$, where n is the number of complete current waves per second.

The currents must be as heavy as possible, $\frac{1}{10}$ ampere or more, and it is well to make those that require a current of more than $\frac{1}{100}$ ampere of larger wire freely suspended in oil. A larger current can, however, be passed through an ordinary resistance-box for a second or two without danger. A few fixed coarse resistances of large wire in air or oil with ordinary resistance-boxes for fine adjustment, are generally all that are required. Special boxes avoiding electrostatic induction are, however, the best, but are not now generally obtainable.

In some methods, such as 8, 9, 10, &c., we can eliminate undesirable terms containing the current period by using a key which suddenly changes the connexions before the period has time to change much.

In using twisted-wire mutual inductances, methods 7 and 12 are about or entirely free from error due to electrostatic action between the wires. In all the methods this error is less when the resistance of the coils is least, and in 23 and 24 when A is least. In method 8 the error is very small when the coil-resistances and R are small and r great. In this method with 1 henry and 1 microfarad, the error need not exceed 1 in 1000. Probably the same remarks apply to 9, 10, 11 also. By suitable adjustment of resistances in the other method, the error may be reduced to a minimum. It can, of course, be calculated and corrected for.

An electrodynamometer can be made to detect ·0001 ampere without making the self-inductance of the suspended coil more than ·0007 henry or that of the stationary coils more than ·0006 henry, the latter coil readily sustaining a current of $\frac{1}{10}$ ampere without much heating.

An error may creep in by methods 1–14 if the current through the suspension is too great, thus heating it and possibly twisting it. This should be tested by short-circuiting the suspended coil or varying the current. This error can be eliminated by always observing by reversing the current in one coil.

Inductances containing iron introduce harmonics and vary with current strength. Thus they have no fixed value.

Closed circuits or masses of metal near a self-inductance diminish it, and increase the apparent resistance, which effects vary with the period. Short circuits in coils are thus detected.

Electrolytic cells act as capacities which, as well as the apparent resistance, vary with the current period. They also introduce harmonics. The same may be said of an electric arc.

An incandescent lamp or hot wire introduces harmonics into the circuit.

Hysteresis in an iron inductance acts as an apparent resistance in the wire almost independent of the current period, and does not, of itself, introduce harmonics. The harmonics are due to the variation of the magnetic permeability with the amount of magnetization.

Electric absorption in a condenser acts as a resistance varying with the square of the period, the capacity also varying, as I have shown above.

In general any circuit containing resistances, inductances, and capacities combined acts as a resistance and inductance or capacity, both of which vary with the current period, the square of the current period alone entering. For symmetry the square of the current period can alone enter in all these cases and those above.

Hence only inductances containing no iron or not near any closed metallic circuits have a fixed value. The same may be said of condensers, as they must be free from electric absorption or electrolytic action to have constants independent of the period. There is no apparent hysteresis in condensers and the constants do not apparently vary with the electrostatic force.

In Plate XIX. I have drawn the diagrams for the various methods, which are nearly self-explanatory. In methods 1 to 14 inclusive, the concentric circles are the coils of the electrodynamometer. Either one is the fixed coil and the other the hanging coil. Oblong figures are inductances, and when near each other are mutual inductances. A pair of cross-lines is a condenser. When a condenser and inductance are on the same circuit, we may, in general, leave out one or the other.

The following numbers indicate both the number of the method and the figures in Plate XIX.

Method 1.

$$\frac{L'}{c}=\frac{[r(R_{,}+R'')+R_{,,}(r+R_{,})][R'(R_{,}+R_{,,})+R''(R_{,}+R')]}{(R_{,}+R''+R_{,,})^2}.$$

Method 2.

$$\frac{L'}{c}\text{ or }b^2LL'\text{ or }-\frac{1}{b^2cc'}=$$

$$\frac{[R_{,,}R'-R_{,}R''][R_{,}(r+R'')+R_{,,}(r+R_{,})]}{R_{,,}(R_{,}+R_{,,})}.$$

Method 3.

In (1) make $R'=R''=R_{,,}=0$, or in (2) make $R''=R_{,}=0$,

$$R_{,,}=\infty.\quad\text{Hence }\frac{L'}{c}=rR'.$$

In case the circuit r contains some self-inductance, l, we can correct for it by the equation

$$\frac{L'}{c}=rR'\left(1-\frac{1}{b^2lc}\right).$$

Method 4.

$$\frac{L_1}{c}=\frac{[R'(r+R_{\prime\prime})+R''(R'+R_{\prime\prime})][R'(R''+R_{\prime\prime})+R''(R_{\prime}+R_{\prime\prime})]}{R'R''}.$$

Method 5.

$$\frac{L_1}{c}=\frac{[R_{\prime}(R''+R_{\prime\prime})+R_{\prime\prime}(R'+R'')][R'(R''+R_{\prime\prime})+r(R'+R'')]}{(R'+R'')(R''+R_{\prime\prime})}.$$

Method 6.

$$\frac{L}{c} \text{ or } \frac{l}{C}=(R+R')(R''+r).$$

We can correct for self-inductions L', L'' in the circuits R', R'' by using the exact equation

$$b^2\left[[L'(r+R'')+\left(L''-\frac{1}{b^2c}\right)R'\right]\left[L''(R+R')+R''(L+L')\right]+$$
$$R'R''(r+R'')(R+R'),$$

or approximately

$$\frac{L}{c}=(R+R')(R''+r)-\frac{L'}{c}-\frac{L''}{c}\frac{R+R'}{R''}+b^2\frac{L[L'(r+R'')+L''R']}{R'}$$
$$+\ \&c.$$

Method 7.

$$R_2R_3M_{12}M_{13}+b^2[L_3M_{12}-M_{23}M_{13}][L_2M_{13}-M_{23}M_{12}]=0.$$

For a coil containing three twisted wires, $M_{12}=M_{13}=M_{23}$ and the self-inductions of the coils are also equal to each other and nearly equal to the mutual inductions. Put an extra self-induction L_3 in R_3 and a capacity C_2 in R_2. Replace L_3 by $L+L_3$ and L_2 by $L-\frac{1}{b^2C_2}$ and we can write

$$\frac{L_3+L-M}{C_2}=R_2R_3+b^2(L-M)(L_3+L-M).$$

As $L-M$ is very small and can be readily known, the formula will give $\frac{L_3}{C_2}$. When $L-M=0$ we have

$$\frac{L_2}{C_3} \text{ or } \frac{L_3}{C_2}=R_2R_3.$$

Method 8.

$$b^2M(M+L)=rR \qquad 2b^2M^2=rR+(rR)'$$

or

$$b^2M(M-L)=(rR)' \quad 2b^2LM=rR-(rR)'.$$

Placing a capacity in the circuit R, we have also

$$b^2M(M+L)-\frac{M}{C}=rR,$$

or

$$b^2M(M-L)+\frac{M}{C}=rR.$$

To correct for the self-induction, l, of r we have the exact equations

$$b^2M(M+L)=rR+b^2l(L+M)$$

$$b^2M(M-L)=rR+b^2l(L-M)$$

$$b^2M(M+L)-\frac{M}{C}=rR-b^2l\left(L+M-\frac{1}{b^2C}\right)$$

$$b^2M(M-L)+\frac{M}{C}=rR-b^2l\left(L-M-\frac{1}{b^2C}\right).$$

If the condenser is put in r, we have

$$\frac{L+M}{c}=rR-b^2M(L+M)$$

or

$$\frac{L-M}{c}=rR+b^2M(L-M).$$

Method 9.

$$b^2L'M-\frac{M}{C'}=R_{,}\left[R'+R_{,}+\frac{rR''}{r+R''}\right]$$

or

$$-b^2L'M+\frac{M}{C'}=R_{,}\left[R'+R_{,}+\frac{rR''}{r+R''}\right].$$

Making $R''=\infty$ and $r+R'=r$, we have

$$-b^2L'M+\frac{M}{c'} \quad \text{or} \quad b^2L'M-\frac{M}{C'}=R_{,}(r+R_{,}).$$

Taking two observations we can eliminate $b^2L'M$, and we have

$$\frac{M}{C'}=R_{/}\{r-(r)'\}.$$

Knowing L′M we can find C′. Throwing out C′ (*i. e.* making it ∞) we can find $b^2L'M$ in absolute measure: then put in C′ and find its value as above.

To correct for self-induction in $R_{/}$, we have for case $R''=\infty$ the exact equation

$$b^2L'M-\frac{M}{C'}=R_{/}(r+R_{/})+b^2[L'+L_{/}-M]L_{/}-\frac{L_{/}}{C'}.$$

The correction, therefore, nearly vanishes for two twisted wires in a coil where $L'-M=0$ and C is taken out.

Method 10.

$$-b^2lM+\frac{M}{c}\quad\text{or}\quad b^2lM-\frac{M}{c}=$$

$$\frac{[R_{/}R''-R_{//}R']\{r[R'+R''+R_{/}+R_{//}]+(R'+R_{/})(R''+R_{//})\}}{[R'+R''+R_{/}+R_{//}]^2}$$

This can be used in the same manner as 9, to which it readily reduces. But it is more general and always gives zero deflexion when adjusted, however M is connected. To throw out C makes it ∞.

Method 11.

$$\frac{L-M}{c}=rR+b^2(l-M)(L-M),$$

$$\frac{L+M}{c}=rR+b^2(l+M)(L+M).$$

For the upper equation the last term may be made small and the method may be useful for determining L−M when c is known. Method 8, however, is better for this.

Method 12.

$$\frac{L'}{l}=\frac{R+R'}{r}.$$

Should the circuits R and r also have small self-inductances, L and l, we can use the exact equation

$$\mathrm{L}'+\mathrm{L}=\frac{\mathrm{R}+\mathrm{R}'}{r}\,l\,\frac{1+\dfrac{\mathrm{L}r}{l\mathrm{R}}}{1-\dfrac{b^2\mathrm{L}l}{r\mathrm{R}}}=\frac{\mathrm{R}+\mathrm{R}'}{r}\,l\left[1+\frac{\mathrm{L}r}{l\mathrm{R}}+\frac{b^2\mathrm{L}l}{r\mathrm{R}}+\text{\&c.}\right]$$

When L′ and l are approximately known, we can write the following, using the approximate value on the right side of the equation,

$$\frac{\mathrm{L}'}{l}=\frac{\mathrm{R}+\mathrm{R}'}{r}\left[1+\frac{\mathrm{L}r}{l\mathrm{R}}-\frac{\mathrm{L}'}{l}\,\frac{r}{\mathrm{R}+\mathrm{R}'}+\frac{b^2\mathrm{L}l}{r\mathrm{L}}+\text{\&c.}\right]$$

Taking out L′ and putting a condenser, C, in R we have

$$\frac{l}{\mathrm{C}}=r\mathrm{R}'-b^2l\mathrm{C}\mathrm{R}(\mathrm{R}+\mathrm{R}').$$

For a condenser, R can be small or zero.

Method 13.

$$\text{(A)}\quad\left[b\mathrm{L}''-\frac{1}{b\mathrm{C}''}\right]^2=\frac{[\mathrm{R}_{\prime\prime}\mathrm{R}'-\mathrm{R}_{\prime}\mathrm{R}''][\mathrm{R}_{\prime\prime}(r+\mathrm{R}_{\prime})+\mathrm{R}_{\prime}(r+\mathrm{R}'')]}{\mathrm{R}_{\prime}^2}.$$

This determines capacities or self-inductions in absolute value. As described above, mutual induction can also be determined by converting it into self-induction.

$$\text{(B)}\quad\left[b\mathrm{L}_{\prime\prime}-\frac{1}{b\mathrm{C}_{\prime\prime}}\right]^2=\frac{[\mathrm{R}''\mathrm{R}_{\prime}-\mathrm{R}'\mathrm{R}_{\prime\prime}][\mathrm{R}_{\prime\prime}(r+\mathrm{R}_{\prime})+\mathrm{R}_{\prime}(r+\mathrm{R}'')]}{\mathrm{R}'(r+\mathrm{R}_{\prime})}.$$

$$\text{(C)}\quad\left[b\mathrm{L}_{\prime}-\frac{1}{b\mathrm{C}_{\prime}}\right]^2=\frac{[\mathrm{R}'\mathrm{R}_{\prime\prime}-\mathrm{R}''\mathrm{R}_{\prime}][\mathrm{R}_{\prime}(r+\mathrm{R}'')+\mathrm{R}_{\prime\prime}(r+\mathrm{R}_{\prime})]}{\mathrm{R}''[r+\mathrm{R}''+\mathrm{R}_{\prime\prime}]}.$$

Method 14.

$$\left[b\mathrm{L}'-\frac{1}{b\mathrm{C}'}\right]^2=$$

$$\frac{[\mathrm{R}_{\prime}\mathrm{R}''-\mathrm{R}_{\prime\prime}\mathrm{R}']\{r[\mathrm{R}'+\mathrm{R}_{\prime}+\mathrm{R}''+\mathrm{R}_{\prime\prime}]+[\mathrm{R}'+\mathrm{R}_{\prime}][\mathrm{R}''+\mathrm{R}_{\prime\prime}]\}}{\mathrm{R}_{\prime\prime}[r+\mathrm{R}''+\mathrm{R}_{\prime\prime}]}.$$

Of course, in any of these equations, methods 13 or 14, L″ is eliminated by making L″ = 0 or the condenser C is omitted by making C = ∞.

Method 15.

$$-\frac{1}{b^2C_{\prime}C''} \quad \text{or} \quad b^2L_{\prime}L'' \quad \text{or} \quad -\frac{L_{\prime}}{C'} = \frac{R'R_{\prime\prime}(R_{\prime}+R_{\prime\prime\prime})(R''+R''')-R''R_{\prime}R'''R_{\prime\prime\prime}}{R'R_{\prime\prime}-R'''R_{\prime\prime\prime}}.$$

$$\frac{C''}{C_{\prime}} \quad \text{or} \quad \frac{L_{\prime}}{L''} \quad \text{or} \quad -b^2L_{\prime}C'' = \frac{R_{\prime\prime\prime}R'''R_{\prime}-R'R_{\prime\prime}(R_{\prime}+R_{\prime\prime\prime})}{R'R_{\prime\prime}(R''+R''')-R''R_{\prime\prime\prime}R'''}.$$

(A) When $R_{\prime\prime\prime}=\infty$ we have

$$\frac{L_{\prime}}{C''} = \frac{R'R_{\prime\prime}(R''+R''')-R''R_{\prime}R'''}{R'''} = R'R_{\prime\prime} - \frac{R''}{R'''}[R'''R_{\prime}-R'R_{\prime\prime}];$$

$$b^2L_{\prime}C'' = \frac{R'''R_{\prime}-R'R_{\prime\prime}}{R''R'''}.$$

If we adjust by continuous current, we shall have

$$R'''R_{\prime} - R'R_{\prime\prime} = 0.$$

For a condenser we can make $R''=0$ provided there is no electric absorption. In this case $b^2L_{\prime}C''$ is indeterminate and we can adjust to find $\frac{L_{\prime}}{C''}$. However, two simultaneous adjustments are required.

But I have shown that the presence of electric absorption in a condenser causes the same effect as a resistance in its circuit, the resistance, however, varying with the period of the current. Hence R'' must include this resistance. However, the value of R'' will not affect the first adjustment much and so the method is easy to work. If it is sensitive enough it will be useful in measuring the electric absorption of condensers in terms of resistance.

It has the advantage of being practically independent of the current period for $\frac{L}{C}$, as it should be.

For comparison of capacities the same simplification does not occur.

Indeed, the method is of very little value in this case, being surpassed by 16.

Method 16.

(A) $[R_{\prime}R''-R_{\prime\prime}R'][W+r'+r'']+W[R_{\prime}r''-r'R_{\prime\prime}]=0;$

$$\frac{L'}{L_{\prime}} \quad \text{or} \quad \frac{C_{\prime}}{C'} = \frac{R''}{R_{\prime\prime}} + \frac{(Wr'')}{R_{\prime\prime}(W+r'+r'')}.$$

The first equation is satisfied by adjusting the Wheatstone bridge so as to make

$$(\mathrm{R}_{,}\mathrm{R}''-\mathrm{R}_{,,,}\mathrm{R}')=0\ ;\qquad \mathrm{R}_{,}r''-\mathrm{R}_{,,}r'=0\ ;$$
$$\mathrm{R}_{,}(\mathrm{R}_{,,}+r'')-\mathrm{R}_{,,}(\mathrm{R}'+r')=0.$$

That is

$$\frac{\mathrm{R}_{,}}{\mathrm{R}_{,,}}=\frac{\mathrm{R}'}{\mathrm{R}''}=\frac{r'}{r''}.$$

We can then adjust W with alternating currents. This is a very good method and easy of application, but requires many resistances of known ratio. Many of these, however, may be equal without disadvantage. A well-known case is given by making r' and $r''=0$.

(B) By placing self-inductions or condensers in $\mathrm{R}_{,}$ and r'' instead of the above we have the following

$$\frac{c''}{\mathrm{C}'}\text{ or }-b^2\mathrm{L}_{,}c''\text{ or }\frac{\mathrm{L}_{,}}{l''}=\frac{\mathrm{R}_{,}(\mathrm{W}+\mathrm{R}'')-\mathrm{R}'\mathrm{R}_{,,}}{\mathrm{R}''(\mathrm{W}+r'+r'')\mathrm{W}r''}$$

$$-\frac{1}{b^2\mathrm{C}_{,}c''}\text{ or }\frac{\mathrm{L}_{,}}{c''}\text{ or }-b^2\mathrm{L}_{,}l''=$$
$$\frac{(\mathrm{W}''+r'+r'')(\mathrm{R}_{,}\mathrm{R}''-\mathrm{R}_{,,}\mathrm{R}')+\mathrm{W}(\mathrm{R}_{,}r''-\mathrm{R}_{,,}r')}{\mathrm{W}+\mathrm{R}''}.$$

Making $\mathrm{R}''=0$, we have

$$\frac{c''}{\mathrm{C}_{,}}\text{ or }-b^2\mathrm{L}_{,}c''\text{ or }\frac{\mathrm{L}_{,}}{l''}=\frac{\mathrm{R}_{,}\mathrm{W}-\mathrm{R}'\mathrm{R}_{,,}}{\mathrm{W}r''}$$

$$-\frac{1}{b^2\mathrm{C}_{,}c''}\text{ or }\frac{\mathrm{L}_{,}}{c''}\text{ or }-b^2\mathrm{L}_{,}l''=\mathrm{R}'\mathrm{R}_{,,}+r'\mathrm{R}_{,,}\left(1+\frac{\mathrm{R}'}{\mathrm{W}}\right)$$
$$-\frac{r''}{\mathrm{W}}(\mathrm{R}_{,}\mathrm{W}-\mathrm{R}'\mathrm{R}_{,,}).$$

In case we adjust the bridge to $\mathrm{R}_{,}\mathrm{W}-\mathrm{R}'\mathrm{R}_{,,}=0$ and a condenser is in r'' so that we can make $r''=0$, the value of $-b^2\mathrm{L}_{,}c''$ will be indeterminate, and we can find $\frac{\mathrm{L}_{,}}{c''}$ by the adjustment of W alone.

This is an excellent method, apparently, as only one adjustment is required.

However, see the remarks on method 15. This present method with $r''=0$ for $\frac{\mathrm{L}}{c}$ is Anderson's, with, however, alternating currents instead of direct as in his.

The other two values are imaginary in this case. Indeed the whole method (B) is only of special value for $\frac{L}{c}$, as two adjustments are needed for the others.

Method 17.

(A) $W=\infty$, $R=\infty$:

$$b^2ML' = R_{\prime}R'' - R_{\prime\prime}R',$$

$$\frac{L'}{M} = \frac{R' + R_{\prime} + R'' + R_{\prime\prime}}{R_{\prime\prime}}.$$

By this method the self-induction of the mutual induction-coil is eliminated. But it is difficult to apply, as two resistances must be adjusted, and the adjustment will only hold while the current period remains constant. The same remarks apply to (B) and (C) following.

(B) $R=\infty$:

$$b^2ML' = \frac{W[R_{\prime}R'' - R'R_{\prime\prime}]}{W''' + R_{\prime} + R_{\prime\prime}},$$

$$\frac{L'}{M} = \frac{W'''[R' + R_{\prime} + R'' + R_{\prime\prime}] + (R' + R_{\prime})(R'' + R_{\prime\prime})}{R_{\prime\prime}W}.$$

(C) $W=\infty$:

$$b^2ML' = \frac{R}{R + R'' + R_{\prime\prime}}(R_{\prime}R'' - R'R_{\prime\prime}),$$

$$\frac{L'}{M} = \frac{R(R' + R_{\prime} + R'' + R_{\prime\prime}) + (R' + R_{\prime})(R'' + R_{\prime\prime})}{RR_{\prime\prime}}.$$

Method 18.

$$R_{\prime}R'' - R'R_{\prime\prime} = 0,$$

$$\frac{L'}{M'} = 1 + \frac{R''}{R_{\prime\prime}} + \frac{R' + R''}{W}.$$

L' and M' belong to the same coil. By adjusting the Wheatstone bridge first, W can then be afterwards adjusted.

To find the ratio for any other coil independent of the induction-coil, we can first find $\frac{L'}{M'}$ as above. Then add L to the same circuit, and we can find $\frac{L+L'}{M'}$. Whence we can get L. This seems a convenient method if it is sensitive enough, as the value of $\frac{L'}{M'}$ should be accurately known for the inductance standard.

Method 19.

$$b^2(L'l-M^2)=\frac{r}{R_{,,}}[R'R_{,,}-R''R_{,}],$$

$$\frac{L'}{M}=\frac{R'+R_{,}}{r}-b^2\frac{L'l-M^2}{r^2}\left(\frac{l}{M}+1\right)=\frac{R'+R_{,}}{r}-\frac{R'R_{,,}-R''R_{,}}{rR_{,,}}\left(\frac{l}{M}+1\right).$$

This is useful in obtaining the constants of an induction standard. For twisted wires $L'l-M^2$ should be nearly 0, depending, as it does, on the magnetic leakage between the coils. $\frac{l}{M}$ is often known sufficiently nearly for substitution in the right-hand member. It can, however, be found by reversing the inductance standard.

Method 20.

$$R'R_{,,}-R''R_{,}=0,$$

$$\frac{M}{L}=\frac{R''}{R_{,}+R_{,,}};\ \frac{M}{L'}=\frac{W}{R''}\frac{R_{,,}^2}{(R_{,}+R_{,,})^2};\ \frac{L}{L'}=\frac{W}{R'+R''}\ L>M;\ L'$$ any value.

In case of a standard inductance, M and L are known, especially when the wires are twisted.

The method can then be used for determining any other inductance, L′, and is very convenient for the purpose.

$R_{,,}$ and $R_{,}+R_{,,}$ are first calculated from the inductance standard. The Wheatstone bridge is then adjusted and W varied until a balance is obtained. This balance is independent of the current period, as also in the next two methods.

Method 21.

$$R'R_{,,}-R''R_{,}=0,$$

$$\frac{l}{M}=\frac{R'+R_{,}}{R_{,}};\ \frac{L'}{M}=\frac{(R'+R_{,})^2}{rR_{,}};\ \frac{L'}{l}=\frac{R'+R_{,}}{r}\ l>M.$$

This is Niven's method adapted to alternating currents. See remarks to method 20.

Methods 20 and 21 are specially useful when one wishes to set up an apparatus for measuring self-induction, as the resistances R', R'', $R_{,}$, $R_{,,}$ can be adjusted once for all in case of a given inductance standard and only W or r need be varied afterwards.

Method 22.

$$\frac{L''}{M}=\frac{R'+R_{\prime}}{R_{\prime}}\,;\quad \frac{M}{C'}=R_{\prime}R''\,;\quad \frac{L''}{C'}=R''(R'+R_{\prime}).$$

This is Carey Foster's method adapted to alternating currents and changed by making R'' finite instead of zero.

The ratio of $R'+R_{\prime}$ to $R_{\prime}$ is computed from the known value of the induction standard. R'' is then adjusted and C' obtained. In general the adjustment can be obtained by changing $R_{\prime}$ and R''. The adjustment is independent of the current period.

Method 23.

$$b^2 mL'=rR_{\prime}+R[r+R'+R_{\prime}],$$

$$\frac{Mr-L'R}{m}=r+R'+R_{\prime}.$$

If we make $R=0$ we have

$$b^2 mL'=rR_{\prime}$$

$$\frac{M}{m}=\frac{r+R'+R_{\prime}}{r}.$$

This method requires two simultaneous adjustments. M must also be greater than m. As M and L' belong to the same coil, we can consider this method as one for determining m in terms of the M and L' of some standard coil.

The resistance, A, can be varied to test for, or even correct, the error due to electrostatic action between the wires of the induction standard.

Method 24.

$$\frac{L_{\prime}}{M_{\prime}}=\frac{L'}{M'}\frac{r_{\prime}}{r'}\,;\quad \frac{M_{\prime}}{M'}=\frac{r'(r_{\prime}+R_{\prime}+R_{\prime\prime})}{r_{\prime}(r'+R'+R'')}.$$

This is a good method for comparing standards. We first determine $\frac{L_{\prime\prime}}{M}$ for each coil by one of the previous methods. Then we can calculate $\frac{r_{\prime}}{r'}$ and adjust the other resistances to balance.

It is independent of the period of the current and suitable for standards of equal as well as of different values, as the mutual inductances can have any ratio to each other.

For twisted wire coils $r_{\prime}=r'$ very nearly. See method 23 for the use of the resistance, A.

Method 25.

In fig. 6 remove the shunt R′ and self-induction L.

This method then depends upon the measurement of the angular deflexion when a self-induction or a capacity is put in the circuit of the small coil of the electrodynamometer, and comparing this with the deflexion when the circuit only contains resistance.

The resistance of the circuit, r, is supposed to be so great compared with R that the current in the main circuit remains practically unaltered during the change.

There is also an error due to the mutual induction of the electrodynamometer-coils which vanishes when r is great.

$$\frac{1}{b^2c^2} \text{ or } b^2l^2 = \mathrm{R}''[r+\mathrm{R}'']\left[\frac{r_{,}+\mathrm{R}_{,}''}{\mathrm{R}_{,}''}\,\frac{\theta_{,}}{\theta}-\frac{r+\mathrm{R}''}{\mathrm{R}''}\right].$$

These formulas assume that the deflexion is proportional to θ. This assumption can be obviated by adjusting $\theta=\theta'$, when we have

$$\frac{1}{b^2c^2} \text{ or } b^2l^2 = \frac{(r+\mathrm{R}'')(r_{,}\mathrm{R}''-r\mathrm{R}_{,}'')}{\mathrm{R}}.$$

These can be further simplified by making $\mathrm{R}''=\mathrm{R}_{,}''$.

The method thus becomes very easy to apply and capable of considerable accuracy. As the absolute determination depends on the current period, however, no great accuracy can be expected for absolute values except where this period is known and constant, a condition almost impossible to be obtained. The comparison of condensers or of inductances is, however, independent of the period and can be carried out, however variable the period, by means of a key to make the change instantaneously.

Method 26.

Similar results can be obtained by putting the condenser or inductance in R″ instead of r, but the current through the electrodynamometer suspension is usually too great in this case unless r is enormous. We have in this case for equal deflexions,

$$\frac{1}{b^2\mathrm{C}''^2} \text{ or } b^2\mathrm{L}''^2 = \mathrm{R}''(\mathrm{R}''+r)\left(\frac{r\mathrm{R}_{,}''-r_{,}\mathrm{R}''}{r_{,}\mathrm{R}''}\right),$$

where $r_{,}$ and $\mathrm{R}_{,}''$ are the resistances without condenser or self-induction.

This is a very good method in many respects.

For using 25 and 26, a key to make instantaneous change of connexions is almost necessary.

Method 27.

To measure resistance by alternating currents, a Wheatstone bridge is often used with a telephone.

I propose to increase the sensitiveness of the method by using my method of passing a strong current through the fixed coils of an electrodynamometer while the weaker testing current goes through the suspended system.

Using non-inductive resistances, methods 10, 13 A, B, C, and 14 all reduce to proper ones. 10 or 14 is specially good and I have no doubt will be of great value for liquid resistances. The liquid resistances must, however, be properly designed to avoid polarization errors. The increase of accuracy over using the electrodynamometer in the usual manner is of the order of magnitude of 1000 times.

By this method, a resistance of 10,000,000 ohms can be measured to 1 part in 1000 or even more. This method should play a great part in future work on liquid resistances.

Since writing the above I have tried some of the methods, especially 6 and 12. By the method 12, results to 1 in 1000 can be obtained. Replacing L' by an equal coil, the ratio of the two, all other errors being eliminated, can be obtained to 1 in 10,000, or even more accurately.

The main error to be guarded against in method 12, or any other where large inductances or resistances are included, arises from twisting the wires leading to these. The electrostatic action of the leads or the twisted wire coils of an ordinary resistance-box, may cause errors of several per cent. Using short small wire leads far apart, the error becomes very small.

Method 6 is also very accurate, but the electric absorption of the condensers makes much accuracy impossible unless a series of experiments is made to determine the apparent resistance due to this cause.

In method 12 I have not yet detected any error due to twisting the wires of coils l. However, the electrostatic action of twisted wire coils is immense, and the warning against their use which I have given above has been well substantiated by experiment. Only in case of low resistances and low inductances, or in cases like that just mentioned, is it to be tolerated for a moment. Connecting two twisted wires in a coil in series with a resistance between I have almost neutralized the self-inductance, which was one henry for each coil, or four henrys for them in series.

Altogether the results of experiment justify me in claiming that these methods will take a prominent place in electrical measurement, especially where fluid resistances, inductances, and capacities are to be measured. They also seem to me to settle the question as to standard inductances or capacities, as inductances have a real constant which can now be compared to 1 in 10,000 at least.

It is to be noted that all the methods 15 to 24 can be modified by passing the main current through one coil of the electrodynamometer and the branch current through the other. The deflexion will then be zero for a more complicated relation than the ones given. If, however, one adjustment is known and made, the method gives the other equation.

Thus method 18 requires $R_{\prime}R''-R'R_{\prime\prime}=0$. Hence, when this is satisfied we must have the other condition alone to be satisfied. Also in method 22, when we know the ratio of the self and mutual inductances in the coil, the resistances can be adjusted to satisfy one equation, while the experiment will give the other, and hence the capacity in terms of the inductance. Again, pass a current whose phase can be varied through one coil of the electrodynamometer, and connect the circuit to be tested through the other. Vary the adjustments of resistances until the deflexion is zero, however the phase of current through the first coil may be varied.

The best methods to apply the first modification to are 15 A, 16 A and B, 18, 20, 21, 22, and 24. In these either a Wheatstone bridge can be adjusted or the ratio of the self and mutual inductances in a given coil can be assumed known and the resistance adjusted thereby.

The value of this addition is in the increased accuracy and sensitiveness of the method, an increase of more than one hundredfold being assured.

As a standard I recommend two or three coils laid together with their inductances determined, and not a condenser, even an air condenser.

VI. *A New Harmonic Analyser.*

By A. A. MICHELSON *and* S. W. STRATTON*.

[Plates XI.-XVIII.]

EVERYONE who has had occasion to calculate or to construct graphically the resultant of a large number of simple harmonic motions, has felt the need of some simple

---

* Communicated by the Authors.

and fairly accurate machine which would save the considerable time and labour involved in such computations.

The principal difficulty in the realization of such a machine lies in the accumulation of errors involved in the process of addition. The only practical instrument which has yet been devised for effecting this addition is that of Lord Kelvin.

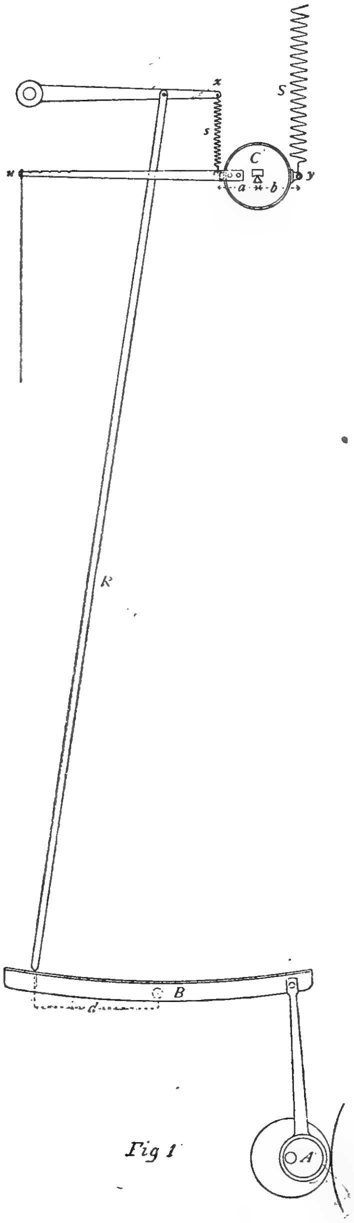


Fig 1

In this instrument a flexible cord passes over a number of fixed and movable pulleys. If one end of the cord is fixed the motion of the other end is equal to twice the sum of the motions of the movable pulleys. The range of the machine is, however, limited to a small number of elements on account of the stretch of the cord and its imperfect flexibility; so that with a considerable increase in the number of elements, the accumulated errors due to these causes would soon neutralize the advantages of the increased number of terms in the series.

It occurred to one of us some years ago that the quantity to be operated upon might be varied almost indefinitely, and that most of the imperfections in existing machines might be practically eliminated. Among the methods which appeared most promising were the following:—addition of fluid pressures, elastic and other forces, and electric currents. Of these the simplest in practice is doubtless the addition of the forces of spiral springs.

The principle upon which the use of springs depends may be demonstrated as follows:—

Let a (fig. 1) = lever-arm of small springs (but one of which is shown in the figure),
b = lever-arm of large counter-spring S,
l_0 = natural length of small springs,
L_0 = ” ” large spring,
$l+x$ = stretched length of small springs,
$L+y$ = ” ” large spring,
e = constant of small springs,
E = ” large spring,
n = number of small springs,
p = force due to one of the small springs,
P = ” ” large spring;

then

$$p=\frac{e}{l_0}\left(l+x-\frac{a}{b}y\right),$$

$$P=\frac{E}{L_0}\left(L+y\right),$$

$$a\Sigma p=bP,$$

whence

$$y=\frac{\Sigma x}{n\left(\frac{l}{L}+\frac{a}{b}\right)}.$$

From this it follows that the resultant motion is proportional to the algebraic sum of the components, at least to the same order of accuracy as the increment of force of every spring is proportional to the increment of length.

To obtain the greatest amplitude for a given number of elements the ratios l/L and a/b should be as small as possible, but of course a limit is soon reached when other considerations enter.

About a year ago a machine was constructed on this principle with twenty elements, and the results obtained* were so encouraging that it was decided to apply to the Bache Fund for assistance. The present machine of eighty elements was constructed with the aid of a grant from this fund.

Fig. 1 shows the essential parts of a single element. s is one of the eighty small springs attached side by side to the lever C, which for greater rigidity has the form of a hollow cylinder, pivoted on knife-edges at its centre. S is the large counter-spring. The harmonic motion produced by the eccentric A is communicated to x by the rod R and lever B, the amplitude of the motion at x depending on the adjustable distance d. The resultant motion is recorded by a pen connected with u by a fine wire; under the pen a slide moves with a speed proportional to the angular motion of the cone D (Plate XI.).

To represent the succession of terms of a Fourier series the eccentrics have periods increasing in regular succession from one to eighty. This is accomplished by gearing to each eccentric a wheel the number of whose teeth is in the proper ratio. These last are all fastened together on the same axis and form the cone D (Pl. XI.).

Turning the cone will produce at the point x motions corresponding to $\cos\theta$, $\cos 2\theta$, $\cos 3\theta$, &c., up to $\cos 80\theta$, with amplitudes depending on the distances d. The motion of the elements may also be changed from sine to cosine by disengaging the cone and turning all of the eccentrics through 90° by means of a long pinion, which can be thrown in gear with all of the eccentric wheels at once.

The efficiency and accuracy of the machine is well illustrated by an inspection of the summations of Fourier series shown by curves 1 to 12 (Pls. XII. to XIV.).

Curves 13 to 24 (Plates XIV. to XVI.) are added to illustrate the use of the machine in constructing curves representing functions which scarcely admit of other analytical expression.

Pl. XVII. shows well the dependence of the accuracy of the representation of a particular function on the number of terms of the series.

The machine is capable not only of summing up any given trigonometric series, but can also perform the inverse process of finding for any given function the coefficients of the corresponding Fourier series. Thus if

$$f(x)=a_0+a_1\cos x+a_2\cos 2x+,$$

---

* Paper read before the National Academy of Sciences, April 1897.

we have

$$a_k = \frac{2}{\pi}\int_0^{\pi} f(x)\cos kx\,dx.$$

On the other hand, if n is the number of an element of the machine and α the distance between any two elements, and the distance d (fig. 1) is proportional to $f(n\alpha)$, the machine gives

$$\sum_0^m f(n\alpha)\cos n\theta = \sum_0^{\pi} f(x)\cos\frac{m}{\pi}\theta x,$$

which is proportional to a_k if $k = \frac{m\theta}{\pi}$. Hence to obtain the integral, the lower ends of the vertical rods R (Pl. XI.) are moved along the levers B to distances proportional to the ordinates of the curve $y = f(n\alpha)$.

The curve thus obtained for a_k is a *continuous* function of k, which approximates to the value of the integral as the number of elements increases. To obtain the values corresponding to the coefficients of the Fourier series, the angle $\theta = \pi$, or the corresponding distance on the curve, is divided into m equal parts. The required coefficients are then proportional to the ordinates erected at these divisions.

Curve No. 25 (Pl. XVIII.) is the approximate value of $\int \phi(x)\cos kx\,dx$ when $\phi(x)$ = constant from 0 to a, and is zero for all other values. The exact integral is $\frac{\sin ka}{k}$. The accuracy of the approximation is shown by the following table, which gives the observed and the calculated values of the first twenty coefficients for $a = 40$.

$$\int_0^a \cos kx\,dx.$$

n.	Obs.	Calc.	Δ.
0	100·0	100·0	0·0
1	65·0	64·0	1·0
2	0·0	0·0	0·0
3	−20·0	−21·0	1·0
4	0·0	0·0	0·0
5	12·5	13·0	−0·5
6	− 1·5	0·0	−1·5
7	− 9·0	− 9·0	0·0
8	0·0	0·0	0·0
9	6·0	7·0	−1·0
10	− 2·0	0·0	−2·0

Table (*continued*).

11	− 6·0	− 6·0	0·0
12	0·0	0·0	0·0
13	4·0	5·0	−1·0
14	− 2·0	0·0	−2·0
15	− 4·0	− 4·5	0·5
16	0·5	0·0	0·5
17	3·5	4·0	−0·5
18	− 1·0	0·0	−1·0
19	− 3·5	− 3·0	−0·5
20	0·0	0·0	0·0

The average error is only ·65 of one per cent. of the value of the greatest term.

Another illustration is given in curve No. 27 (Pl. XVIII.), in which

$$\phi(x) = e^{-a^2x^2}.$$

For $a=1$ the following are the values of the coefficients of the first twelve terms of the equivalent Fourier series :—

$$\int_0^\infty e^{-a^2x^2} \cos kx \, dx.$$

n.	Obs.	Calc.	Δ.
0	100	100	0
1	97·5	96	1·5
2	88	86	2
3	73·5	70	3·5
4	56	54	2
5	40	38	2
6	26·5	25	1·5
7	16·5	15	1·5
8	9·5	8	1·5
9	5·5	4·5	1
10	3	2	1
11	1	1	0
12	0	0·5	− 0·5

Here the average error is somewhat greater, being 1·5 per cent. of the value of the greatest term, but by a suitable selection of the constant it may be made less than 1 per cent.

It appears, therefore, that the machine is capable of effecting the integration $\int \phi(x) \cos kx \, dx$ with an accuracy comparable with that of other integrating machines; and while it is scarcely hoped that it will be used for this purpose where great accuracy is required, it certainly saves an enormous amount of labour in cases where an error of 1 or 2 per cent. is unimportant.

The experience gained in the construction of the present instrument shows that it would be quite feasible to increase the number of elements to several hundred, or even to a thousand, with a proportional increase in the accuracy of the integrations.

Finally, it is well to note that the principle of summation here employed is so general that it may be used for series of any functions by giving to the points *p* the motions corresponding to the required functions, instead of the simple harmonic motion furnished by the eccentrics. A simple method of effecting this change would be to cut metal templates of the required forms and mount them on a common axis. In fact, the harmonic motion of the original machine was thus produced.

VII. *A Telemetrical Spherometer and Focometer.*

By Prof. W. Stroud, *D.Sc., M.A.*[*]

HAVING been engaged for the past eight years in conjunction with Prof. Barr in the construction and perfecting of naval range-finders, it was natural that I should turn my attention to the possible application of telemetrical principles to physical measurement.

One very obvious application is to the determination of (1) the radius of curvature of a concave and especially of a convex surface, and (2) the focal length of a convex and especially of a concave lens. The problem of finding the position of the image of an object in a convex mirror or a concave lens is essentially a range-finding problem.

For the particular purpose in view the most suitable form of range-finder is a constant-range instrument, *i. e.* an instrument (whose optical parts are rigidly fixed) which can be translated along an optical bank and will indicate when an object or image is at the specified range. The mode of action of the instrument will thus be very like that of an ordinary short-focus telescope as commonly used on optical banks, but a short-focus telescope is essentially bad as a range-finder, and that for the following reasons:—

(1) As is well known, when using such a telescope it is advisable to cover up the middle portion of the objective

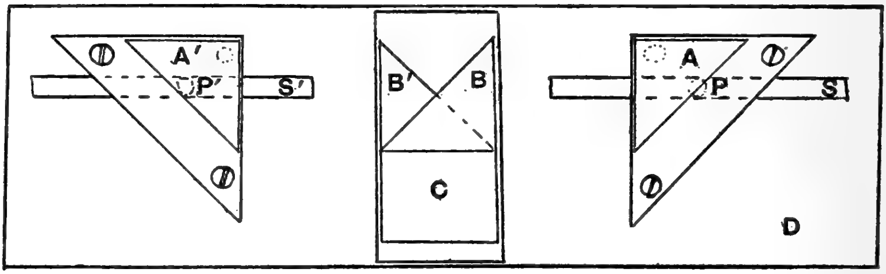
---

[*] Communicated by the Physical Society: read Oct. 29, 1897.

so that only the extreme beams are used to form the image. In this way a crude range-finder is formed, which suffers from a radical defect, arising from the fact that the beams enter the eye in different directions, so that any alteration in the accommodation of the eye produces a duplication of the image. An attempt is made to avoid error from this cause by keeping the eye focussed all the time on cross-wires fixed in the focal plane of the eyepiece. (2) The short-focus telescope has not usually *base* enough for accurate telemetry (the maximum available base being the diameter of the objective), or if it should have base enough, the minimum range which it can measure is too great.

Looked at from the range-finding point of view the problem of optical spherometry and focometry is one of excessive simplicity. What is required is a range-finder whose prime reflectors can be fixed at any distance apart (to furnish the adjustable base) and at any angle to that base (to furnish the adjustable range).

Fig. 1.



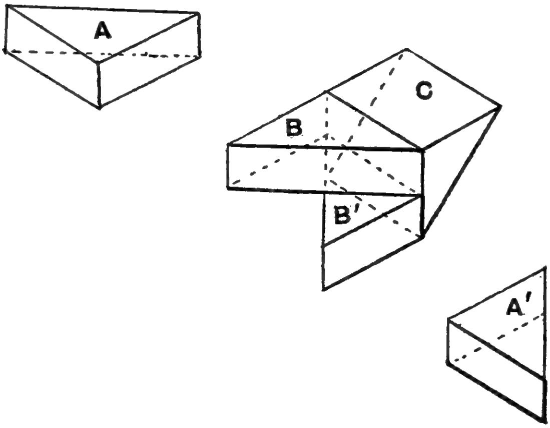


Fig. 2.

One arrangement is shown in plan in fig. 1 and in perspective in fig. 2. A, A′ are two triangular reflecting prisms forming the prime reflectors supported on stands provided with levelling-screws. Slots S, S′ are provided in the frame-piece D through which pass stout pins P, P′ provided with clamping-screws below. B, B′ are fixed reflectors opposite A and A′ respectively. C is an extra reflector (made into one block with B and B′) so as to reflect the two beams upward to the eye. If now the prime reflectors be adjusted for a particular range, the two portions of the object viewed will appear in coincidence in the field of view furnished by C. If the two portions are not in coincidence they may be brought into coincidence by translating the range-finder to or from the object or image. The instrument works in fact just like a short-focus telescope, the only difference being that the correctness of the range is determined by observing whether or not the two portions of the object viewed are in alignment.

The instrument was used to determine the radius of curvature of a convex mirror by the ordinary method. It is clear that u should be made as large as possible so as to make v large.

The object (a thin rod or slit well illuminated at the back) was placed at the extremity of the bank, and the mirror at the other extremity; the R.F. (range-finder) was brought up to read (1) the image of the rod, (2) the surface of the mirror, (3) the position of the object. Measurement (1) is first done because the R.F. should be adjusted on the image so as to have maximum available base-length and minimum range (the conditions for maximum accuracy). The portion of the mirror used was restricted to 10 cm. diameter, so as to make the measurements comparable with direct measurements made with our spherometer, which has its adjustable point 5 cm. distant from each of the fixed legs. The R.F. is then brought up to within a few cms. of the mirror and the prime reflectors are angled until the images appear nearly in coincidence, the prime reflectors are then rigidly fixed and the R.F. is moved to and fro along the bank until exact coincidence has been obtained, when the reading on the bank is taken. If the base-length is 10 cm. and the image about 70 cm. away, the extreme variations cannot amount to 1 mm. and should not amount to more than $\frac{1}{2}$ mm. From fig. 1 it will be seen that the R.F. is so constructed that it does not itself obstruct the beams forming the two separate images provided its distance from the mirror is sufficient (say 8 or 10 cm.).

Measurement (2) is effected by suspending a weight by a very fine wire so that the wire rests in contact with the convex surface. Our mirror was silvered at the back, the wire resting against the front glass surface. Under these circumstances an image of the wire will be formed by reflexion in the surface. The R.F. is adjusted (*a*) on the wire, (*b*) on its image. If t is the thickness of the glass, $\frac{2t}{\mu}$ is the difference between these readings, from which t can be calculated with sufficient accuracy.

Measurement (3) is effected most conveniently by shifting the rod or slit a measured large distance (say 100 cm.) towards the mirror, so that the R.F. can be brought to bear upon it.

The following results were obtained :—

u.	v.	r.	r (corrected).
143·62	26·82	65·50	65·37
128·79	26·09	65·44	65 31
118·78	25·66	65·46	65·33
124·12	25·88	65·39	65·27
119·12	25·67	65·44	65·31

The last column gives the value for the radius corrected for aberration. The radius of the same mirror measured by our spherometer gave 65·31 and 65·37 cm. It should be remarked (1) that our spherometer has received a good deal of rough usage, (2) that the spherometer measures the curvature of the external surface, while the telemeter was used to measure the curvature of the silvered surface. Still the results show that this telemetrical method is nearly as accurate as direct spherometry.

The constants of a concave lens were next determined. One face was obviously concave, the other looked nearly flat. The radius of the concave face was first got in this way :— the rod, suitably and strongly illuminated, was placed between the R.F. and the concave surface, and roughly adjusted so that it and its image were in close proximity near the centre of curvature. In other words u and v were made nearly equal and the value of each determined. In this case we are dealing with a real image and can bring our R.F. as near as we please to the image.

Now a little elementary calculation shows that if R be the range, dR the error in R, B the base, and $d\theta$ the error in angular estimation, then $d\theta = \frac{dR}{R} \cdot \frac{B}{R}$; but $\frac{B}{R}$ is practically fixed, as its maximum value $= \frac{\delta}{\rho}$, where δ, ρ are the diameter and radius of the surface respectively. Hence as $d\theta$ is fixed ($\doteq$ 1 minute of arc for snapshot naked-eye work and $\doteq \frac{1}{3}$ minute when great care is taken), dR will be a minimum when R is a minimum. To determine the range of a real image as accurately as possible, we must then take the minimum base and minimum range for maximum accuracy. In this case it may be advisable to place on the face of the prism a convex lens whose focal length is equal to the range required.

In the case of the lens used $\delta = 8$ cm. $\rho \doteq 16$ cm.; so that

$$dR = R \, . \, \frac{R}{B} \, . \, d\theta = 2R \, d\theta.$$

If, then, $d\theta = \frac{1}{3}$ minute $\doteq \frac{1}{10{,}000}$ and $R = 5$ cm., then

$$dR = \tfrac{1}{100} \text{ mm.}$$

In order to compete in accuracy with this a spherometer would require to indicate to $\frac{1}{3000}$ mm. In short, as a R.F. could be constructed to work at much shorter ranges than even 5 cm., there can be no question that for concave spherical surfaces the telemetrical method is more accurate than the spherometer.

In this way (though not with a very restricted base, for I did not realize at the time that the accuracy would be much increased by restricting the base and diminishing the range) the radius of curvature was found to be 15·81. A small spherometer gave 15·87 cm.—the lens face was too small for the large spherometer previously mentioned; the radius of the circle of contact of the small spherometer was only 1·5 cm., and as the instrument was home-made and only ordinary care had been bestowed on cutting the screw, I attribute the discrepancy to errors in the spherometer.

The second face when measured telemetrically turned out to be very slightly concave, the mean of several measurements giving a radius of 3300 cm.

The thickness of the lens can be readily got on the optical bank in this way:—The R.F. stand is so constructed that it can be turned through 180° about a vertical axis. A fine wire carrying a weight at its lower end is supported about the middle of the bank and readings of its position are taken with the R.F. from both sides.

The wire is now replaced by the lens and the operation repeated with the R.F., the readings for the one face being taken from the one side, the readings for the second face from the other.

The differences between the sets of readings for the wire and the corresponding sets for the lens gives the thickness required.

The principal points, the focal length, and the refractive index can now be obtained in the ordinary way.

The following results were obtained for the focal length for sodium light, u and v being referred to the principal points.

u.	v.	f.	μ.
24·10	13·33	29·82	1·5276
31·40	15·31	29·87	1·5268
32·13	15·45	29·75	1·5287
35·21	16·13	29·77	1·5284
	Mean	29·80	1·5279

The results are remarkably good when it is remembered that all the observations are simple naked-eye observations without the assistance of any magnifying-power.

Fig. 3.

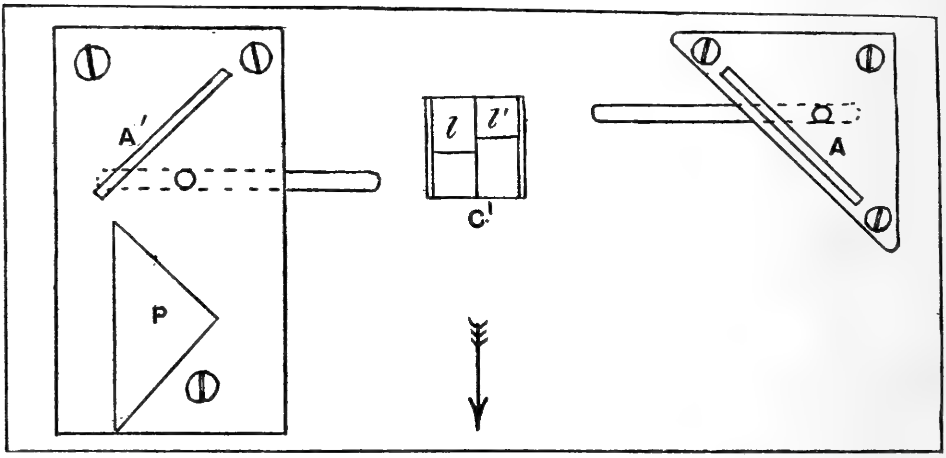


Fig. 3 shows in plan another form which the R.F. may take. A, A′ are, as before, the prime reflectors. The composite block of prisms B, B′, and C (figs. 1 and 2) is now replaced by a single right-angled polished speculum-prism C′ turned edge upwards, as shown in the plan. As it stands, this instrument would not be a range-finder at all, but an azimuth indicator. A right-angled glass prism P is, however, mounted on the stage supporting the reflector A′, so that the beam of light is reversed right for left before falling on A′. This converts the instrument into a range-finder. The kind of appearance that is presented in the instrument before correct alignment has been attained is shown by the lines *l*, *l′*, as seen in the prism C′. Here the object being observed is a line perpendicular to the plane of the figure at some distance away in the direction of the arrow. A moment's reflection will show that the R.F. is too near the object. To produce alignment we should require to move the instrument in the opposite direction to that of the arrow. The advantage which this arrangement presents over the preceding is that the speculum-prism C′ obstructs very little of the beams. Experiment shows that the accuracy obtainable with it is not appreciably different from that obtainable with the instrument first described.

Conclusions.

1. The telemetrical method described is not much inferior to a spherometer for measuring the radii of curvature of convex and approximately plane surfaces.
2. The method is superior in accuracy (though not of course in convenience) to the spherometer for measuring the radii of curvature of concave surfaces.
3. The method is available for determining all the geometrical or optical constants of either a convex or a concave lens on the optical bank alone.

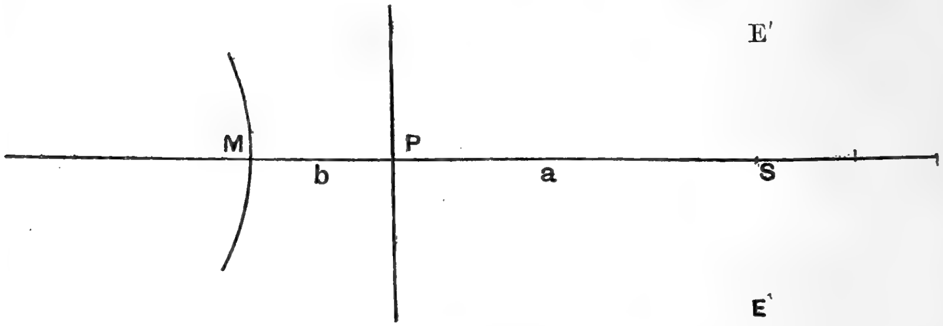
APPENDIX.

In connexion with this subject there remains to be described a simple apparatus for measuring optically the radius of curvature of a convex mirror or the focal length of a concave lens.

I venture to think that it will be found as useful in elementary teaching in other laboratories as it has been found at the Yorkshire College.

M is the convex mirror, P is a slip of silvered plate-glass (assumed plane), S is a pin well illuminated or narrow slit of light. The plate-glass P is arranged so that its upper edge is nearly on the horizontal axis of the mirror. Either S or P is shifted nearer to or further from M, until the image of S in P coincides with the image of S in M, whether viewed from E or from E′. Under these circumstances $u=a+b$, $v=a-b$, whence r can be obtained. As the image formed by reflexion in M is smaller than that formed in P, it is advisable to use a very narrow slit at S.

Fig. 4.



To determine the focal length of a concave lens, the mirror is removed, the concave lens is placed so that its face touches the silvered back of P, and is half covered by it. A stout vertical rod is placed at some considerable distance behind the mirror on the prolongation of the axis of the lens, and the pin S is shifted to or from the mirror until the image of the pin seen by reflexion in the plane mirror coincides with the image of the rod as seen directly through the lens, whether we look from E or from E′. This gives the position of the virtual image of the rod, whence the focal length can be calculated.

The pin or rod can with advantage be replaced by a vertical straight edge well illuminated.

Both these methods will be found very satisfactory for elementary students. The apparatus is very simple, and the method seems to me instructive in illustrating the subject of parallax.

VIII. *High Electromotive Force.* *By* JOHN TROWBRIDGE[*].

LORD KELVIN, in a paper dated April 12, 1860[†], entitled "Measurement of Electromotive Force Required to Produce a Spark," states that "there is a much less rapid

---

[*] Communicated by the Author.

[†] 'Papers on Electrostatics and Magnetism,' Macmillan: London, 1872. P. 258.

variation with distance, of the electrostatic force preceding a spark at the greater than at the smaller distance. It seems most probable that at still greater distances the electrostatic force will be found to be sensibly constant, as it was certainly expected to be at all distances.

Having at my command a very much improved Planté rheostatic machine with sixty condensers of 15 × 80 inches coated surface, I have been enabled to greatly extend my studies of high electromotive force, and to investigate the conditions necessary to produce sparks of great body forty-eight to fifty inches in length. The accumulator which was used to charge the rheostatic machine in parallel had a voltage of 20,000, and the machine could then give me 1,200,000 volts. The length of spark which corresponds to this voltage is very closely forty-eight inches. Professor Elihu Thomson, by means of transformers, has obtained sparks of fifty to sixty inches in length, and has estimated the necessary voltage to produce a spark of 80 centim. at 500,000.

This estimate A. Heydweiler* thinks is very much too great, and he believes that 100,000 would be nearer the truth. My investigations show conclusively that the estimate of Professor Thomson is far nearer the truth than that of Heydweiler; and instead of being lessened it should be increased.

In a recent paper on the tension at the poles of induction-apparatus, A. Oberbeck† states that a potential-difference of 60,000 volts under given conditions can produce a spark of more than 10 centim. in length. It is difficult to obtain consistent results by means of induction-coils and transformers.

My results show that Lord Kelvin's conjecture that the electrostatic force necessary to produce a spark in air remains sensibly constant for all distances beyond the limit he describes, is correct; for when lengths of spark are plotted as abscissas, and the corresponding electromotive forces as ordinates, a straight line is obtained. Planté calls attention to the fact that the loss of energy resulting from the transformation of dynamic into static electricity is, in the case of his apparatus, much less than in induction apparatus. This remark applies with great force to the modern use of transformers for the production of high-tension effects. With the improved form of rheostatic machine which I have constructed, one third of a horse-power will produce the effects which have hitherto required the employment of from thirty to forty horse-power. The condensers in this apparatus are very uniform

---

* Wied. *Ann.* xlviii. p. 231 (1893).

† Wied. *Ann.* No. 9, p. 109 (1897).

in thickness, and the charging and discharging method is a complete departure from that adopted by Planté. All forms of rotating cylinders carrying contact-pieces were abandoned, and two lever-arms were used to charge the condensers in parallel, and then to discharge in series. Great uniformity of action was thus secured.

The sparks produced by the apparatus have great disruptive effects, and produce reports like the discharge of a pistol. The electrostatic field in the neighbourhood of the terminals is very intense, and the wires leading to the spark-gap emit brush-discharges which extend at least 12 inches from the surface of the wire. I was interested to observe the effect of such powerful discharges through Crookes-tubes, and therefore set up a tube which had been exhausted to such a degree that sparks of 8 inches in length preferred to pass through the air from terminal to terminal of the tube rather than to pass through the rarefied space. The disruptive discharge produced by the machine passed readily through such a tube, and showed the x-rays with great brilliancy: moreover, the degree of rarefication of the tube was not sensibly altered by single discharges of such high electromotive force.

The results so far obtained by the employment of this apparatus prove (1) that the length of spark is proportional to the electromotive force ; (2) rarefied spaces hitherto considered too high vacua to conduct electricity cease to act like such vacua to very high electromotive force.

Jefferson Physical Laboratory,
Harvard Universsty, Cambridge, U.S.

IX. *Phosphorescence produced by Electrification.*
By JOHN TROWBRIDGE *and* JOHN E. BURBANK *.

VARIOUS investigators have stated that the x-rays can communicate an electrical charge to bodies. We were interested to discover whether this statement could be verified by means of the phenomenon of phosphorescence. When fluorite, in the state of a comparatively coarse powder, is heated to a low red heat for about an hour, it loses completely its power of phosphorescing under the effect of a low red heat; if it is then electrified by means of the brush-discharge of an electrical machine and then submitted to a low red heat it suddenly phosphoresces. In this connexion it is interesting

---

* Communicated by the Authors.

to note a statement of E. Becquerel upon the effect of electric sparks on phosphorescent bodies. In his work entitled *La Lumière, ses Causes et ses Effets**, he says:—"In order to submit the substances to the action of electric discharges they were placed directly upon an insulated stand if they were in fragments, or in tubes if they were in the state of powder, in such a manner that the discharge-terminals were two or three centimetres apart. One or many electrical discharges were then passed from leyden-jars between the terminals. One then finds that not only the bodies in question become phosphorescent after the passage of the discharge, but also, as one sees later, they are made phosphorescent by these electrical discharges when subjected to heat, even if before having been submitted to the action of the discharges, they had been calcined so long as to be entirely deprived of the power of phosphorescence by elevation of temperature. The electric light acts, therefore, like sunlight, but with greater energy by reason of its greater intensity on account of the position of the bodies; and it is capable of making bodies acquire phosphorescence which did not previously possess it. There is no necessity for submitting fluorspar or the diamond to heat immediately after having been exposed to the discharges. The elevation of temperature can take place even after the lapse of some months."

Becquerel evidently attributed the effects observed to the light of the discharge; for he says, further (p. 55, *ibid.*), "The electric spark acts only by its light, but its action is more energetic than that of sunlight by reason of its great intensity and the proximity of the source."

In an article on luminescence by E. Wiedemann and G. C. Schmidt † the conclusion is reached that the violet light alone of electrical discharges does not cause phosphorescence; but the phosphorescence is due to peculiar discharge-rays analogous to cathode rays. M. W. Hoffman ‡ has confirmed Wiedemann's work. The latter states, "no *entladungsstrahlen* could be detected in the brush-discharge."

With the preparation of fluorite we have mentioned above no effect was produced by the light of the electrical discharge. It was submitted according to the method of Becquerel to the light of the spark; and it was also exposed to the light of burning magnesium and to that of the voltaic arc, and no phosphorescent effects could be produced, nor by subsequent

---

* Paris, 1867, vol. i., p. 55.

† Wied. *Ann.* lvi. 1895, p. 237.

‡ Wied. *Ann.* No. 2, 1897, p. 273.

heating. It phosphoresced, however, on heating after having been electrified by the brush-discharge. The phosphorescence was evidently an expression of the electrical energy which was communicated to the substance, and this energy was suddenly converted into light by the action of the heat in dispelling the electricity. The substance was next subjected to the action of the *x*-rays, in a brass box closed by an aluminium window. The box was also connected to the ground. When the substance was taken from the metallic box it showed no phosphorescence, but on being heated it exhibited a bright phosphorescence which quickly disappeared.

The action of the *x*-rays on this mineral was exactly similar to that of electrification; and we cannot but regard our experiments as an evidence of the electrical nature of the action of the *x*-rays. By them an electrical charge is communicated to fluorescent and phosphorescent substances. The resulting electrical energy in being dissipated reduces the secretion of light. By heating crystals which have been electrified we produce a sudden dissipation of the electrical energy which has been communicated to the particles of the phosphorescent substance, either by direct electrification or by the electrification produced by the *x*-rays.

Most phosphorescent substances, after having been submitted to sunlight or daylight, glow more strongly when heated. The phosphorescence, however, dies out more quickly after the process of heating, which seems to cause the energy communicated to the substance by light to become dissipated more quickly. The same thing is true of the state of phosphorescence communicated by the *x*-rays. Can we therefore conclude that the phosphorescence excited by sunlight or daylight is due to an electrical condition which is dissipated by heat? This supposition seems to us not inconsistent with the electromagnetic theory of light, and phosphorescence may be an evidence of the electrical stresses which produce the phenomena of ultra-violet light.

Jefferson Physical Laboratory,
Harvard University, Cambridge, U.S.

X. *On the Isothermals of Ether.* *By* J. Rose-Innes, *M.A.*, *B.Sc.*[*]

IN a paper "On the Isothermals of Isopentane," read before the Physical Society last May[†], I gave an account of some results obtained by investigating Prof. Young's

---

[*] Communicated by the Physical Society: read Nov. 12, 1897.

[†] Phil. Mag. [5] xliv. p. 76.

experimental work on Isopentane. The main conclusions arrived at were as follows:—

(i.) The study of a diagram constructed by plotting $\frac{1}{av^2}$ against $v^{-\frac{1}{3}}$, where a is the internal pressure and v is the volume of a gram, suggests that there is a discontinuity in the slope of $\frac{1}{av^2}$. Even if there is not discontinuity in the exact sense of the word, there is an extremely rapid change of behaviour, amounting practically to the same thing.

(ii.) The temperature for which the pressure is accurately given by the laws of a perfect gas at a given volume, remains practically constant for all large volumes, until we approach the neighbourhood of the critical volume. At the critical volume this temperature has diminished somewhat from its value for large volumes, but the diminution is only slight.

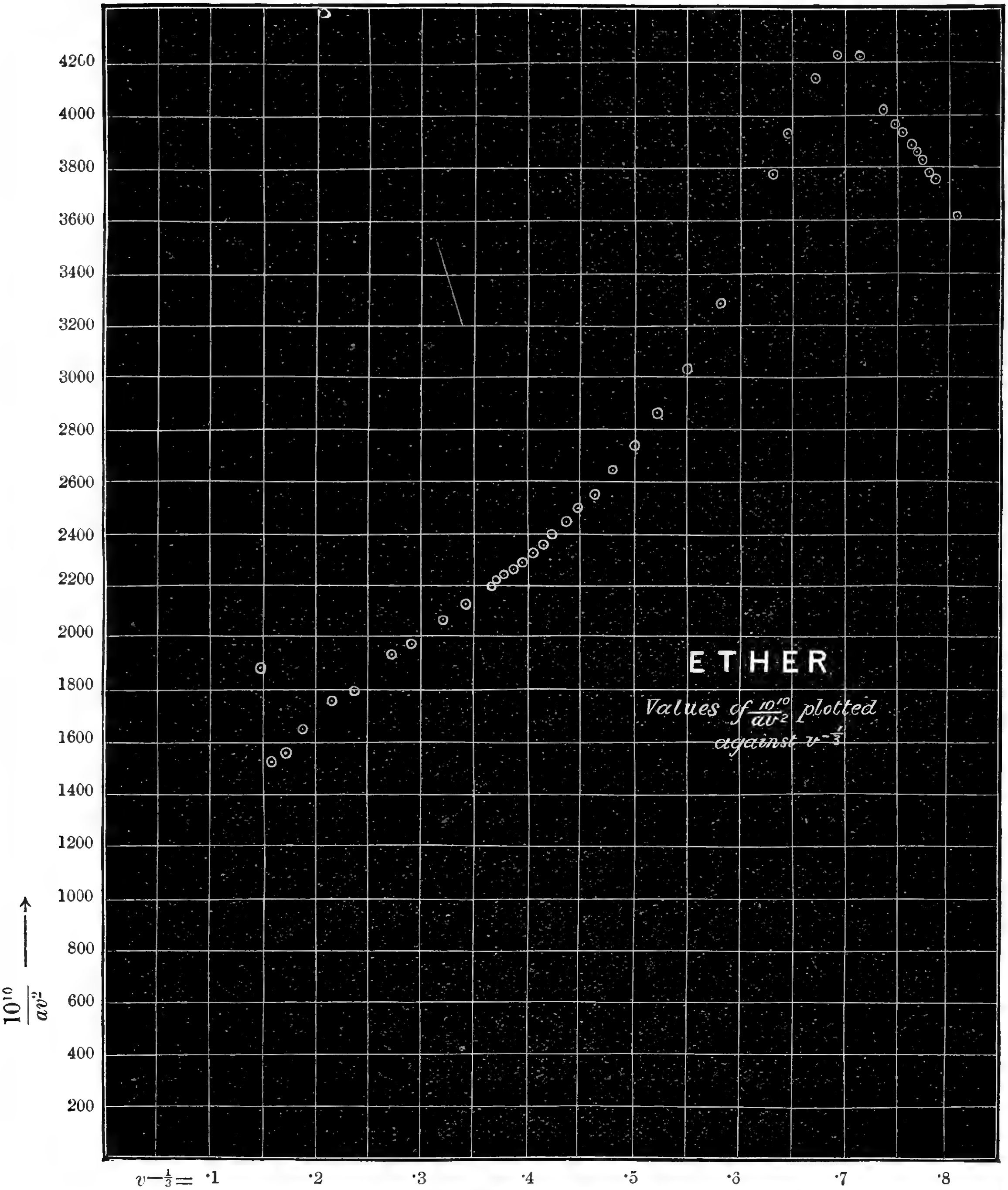
These conclusions were embodied in a formula giving the pressure in terms of the temperature and volume, and a comparison between calculation and experiment was effected by means of a diagram. It is not suggested that this formula is incapable of further improvement, but the close correspondence between calculation and experiment seems to show that the main conclusions on which the formula was founded are correct.

A natural extension of the above investigation is to try how far these general conclusions are true of some other substance. For this purpose I resolved to employ the experimental results obtained by Profs. Ramsay and Young with ether, as it appears that the linear law connecting temperature and pressure at constant volume holds accurately in this case. [Phil. Trans. vol. 178 A, pp. 57–93; and Phil. Mag. vol. xxiii. pp. 435–458.]

Profs. Ramsay and Young have given the values of a for a large number of volumes (Phil. Mag. vol. xxiii. p. 441), so that it was easy to calculate the values of $\frac{1}{av^2}$, and to plot them against $v^{-\frac{1}{3}}$. The resulting diagram is given on p. 104, and it suggests very strongly that there is a discontinuity of slope at about vol. 3·3. In this respect, accordingly, the ether results show a striking likeness to those of isopentane, the discontinuity in the case of the last substance occurring at about the same volume.

It was shown in my former paper that when Ramsay and Young's linear law holds, there is one and only one temperature for each volume at which the pressure is given by the laws of a perfect gas; if we call this temperature τ we easily have

$$\tau=\frac{av}{bv-\mathrm{R}}.$$



The values of b and a are given by Profs. Ramsay and

Young (*loc. cit.* p. 441); the values of τ were thence obtained, and the results are given in the following table:—

Volume.	τ.	Volume.	τ.	Volume.	τ.
300	1035	17	792·2	4	815·3
250	860·6	16	795·2	3·7	813·7
200	827·3	15	795·5	3·3	796·7
150	841·1	14	796·7	3·0	769·7
100	797·5	13	798·0	2·75	735·5
75	766·9	12	800·7	2·5	681·9
50	783·8	11	801·6	2·4	662·6
40	774·3	10	802·9	2·3	643·2
30	775·3	9	805·5	2·2	621·5
25	780·2	8	805·0	2·1	597·2
20	790·1	7	805·6	2·0	571·2
19	789·2	6	805·5		
18	789·4	5	807·2		

In calculating this table, the value of R was taken $=\frac{1}{\cdot 00119}$.

An examination of the table shows that τ remains nearly constant for all large volumes down to about vol. 3; its numerical value throughout this range is roughly 800. The only exception occurs in the case of vol. 300, but the value of τ is here so erratic that it is clearly subject to a large experimental error.

We have still to try how far the formula found for isopentane can be made by alterations of the constants to suit the experimental results with ether. For isopentane it was found that fairly good concordance with experiment could be secured by the use of the formula

$$p=\frac{\mathrm{RT}}{v}\left\{1+\frac{e}{v+k-gv^{-2}}\right\}-\frac{l}{v(v+k)},$$

where R, e, k, g, and l are constants characteristic of the gas*. I have calculated suitable numerical values for these constants in the case of ether from Ramsay and Young's original experimental data; the values are given in the following table, and those of isopentane are added for the sake of comparison:—

	Ether.	Isopentane.
R	840·34	863·56
e	7·485	7·473
k	3·188	3·636
g	4·4539	6·2318
l	5,095,070	5,420,800

---

* This formula was incorrectly stated in my former paper "On the Isothermals of Isopentane;" the correct form is that given above.

The formula was tested by drawing a system of continuous isothermals giving pv plotted against $v-\frac{1}{3}$, and then the experimental values were put in as dots; there is on the whole a fair agreement between calculation and experiment, as may be seen on inspection of the diagram. It is generally difficult in these investigations to know how much may be reasonably allowed for experimental errors. Fortunately in this instance we have a clue to guide us, as Messrs. Ramsay and Young in testing their linear law published tables comparing pressures found with pressures calculated (*loc. cit.* pp. 438–440 and pp. 442–445), and from these it is seen that they were willing to allow over 1 per cent. as a possible experimental error. In this connexion they remark: "It is to be noticed that the greatest divergence is at the temperatures 250° and 280°, but the deviations are in opposite directions and must therefore be ascribed to experimental error" (*loc. cit.* p. 444).

I likewise found in testing my formula that the greatest divergence is at temperatures 280°·35 C. and 250° C., and that the deviations are in opposite directions, and therefore consider it justifiable to attribute them mainly to the same cause. For the remaining temperatures discrepancies occur fairly often of over 1 per cent., but none so great as 2 per cent., so that they still seem to lie within the limits of experimental error.

Finally we may infer that both the general conclusions obtained in the former paper with regard to isopentane hold good also in the case of ether.

XI. *The Wave-Resistance of a Ship.* *By* J. H. Michell.*

THE object of this paper is to give a general solution of the problem of the waves produced by a ship of given form moving with uniform velocity in an inviscid liquid, and to determine the consequent wave-resistance to the motion of the ship. The only assumption made as to the form of the ship is that the inclination of the tangent plane at any point of its surface to the vertical median plane is small. This condition is not satisfied near the bottom of the middle body of a modern ship, but it seems probable that this will not much affect either the waves produced or the resistance, for the waves arise rather from the parts at the bow and stern at which the tangent to the surface is inclined to the direction of the ship's motion, than from the approximately cylindrical

---

* Communicated by the Author.

middle body. The neglect of friction is probably of little consequence. The eddying water close to the side will no doubt slightly alter the virtual shape of the ship, but the change in the inclination of the virtual tangent plane, on which the wave-making depends, will, almost certainly, be very small. Further, the effect of viscosity in destroying the waves produced by the bow will modify to some extent the interaction of bow and stern waves; but, seeing that it is the waves of length comparable with that of the ship which chiefly give rise to the resistance, the effect must be small. The conclusion is, therefore, that the course followed by W. Froude, of considering frictional resistance and wave-resistance separately and adding the two, will probably give a close approximation to the truth.

A summary of the experimental work on this question, as well as a sketch of the theoretical work of Russell, Rankine, and the two Froudes, is given in White's interesting 'Manual of Naval Architecture' (1894), chap. xi. Recent mathematics on the subject has been devoted chiefly to explaining, in a general way, the interesting wave-patterns observed, but exception must be made of the papers of Sir W. Thomson (Lord Kelvin), Phil. Mag. (1886–7), in which the critical speed of a canal boat, examined experimentally long before by Russell, was mathematically discussed. Reference may be made to Lamb's 'Hydrodynamics' (1895), chap. ix., and to Sir W. Thomson's 'Popular Lectures on Navigation' (1891), p. 450, for the discussion of wave-patterns.

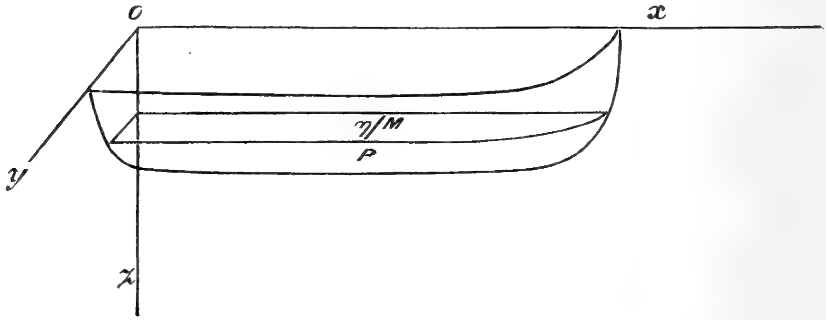
None of these writers has, however, considered the waves produced by an actual ship, and the present paper is intended to supply the necessary investigation. The formula obtained for the wave-disturbance is rather complex, but that for the resistance is much simpler, as the most complicated term in the former represents a local disturbance not leading to any resistance in an inviscid liquid. There will therefore be no difficulty to those who have the necessary apparatus in making comparisons with experiment. As to general results, for deep water the theory leads to a resistance which increases with the velocity, in an oscillating manner, to a maximum and then decreases to zero as the velocity is indefinitely increased. That the resistance is an oscillating function of the velocity has been experimentally found by Mr. W. Froude and his son *, to whom also we owe the general explanation in terms of the interference of bow and stern waves. But the

---

* "On the Leading Phenomena of the Wave-making Resistance of Ships," Trans. Inst. Naval Architects, 1881.

ultimate vanishing of the resistance has not, so far as I know, been anticipated. From general considerations it is clear that, so far as the wave-form is concerned, the effect of increasing the velocity is the same as that of decreasing the acceleration of gravity, and, if gravity vanishes, there is no propagation of waves; but this is not quite the theorem to be obtained.

Fig. 1.



Take the vertical median plane of the ship as $y=0$, and the surface of the undisturbed water as $z=0$, the axis Ox being in the direction of motion of the ship and Oz vertically downwards. We may suppose the ship at rest and the water moving backwards with uniform velocity v apart from the wave-disturbance. The motion is assumed steady and the velocity potential written $-vx+\phi$. Since the inclination of the ship's surface to the plane $y=0$ is everywhere small, ϕ will be small, and we shall neglect the squares of the velocities due to ϕ in comparison with their first powers. At the surface of the water let ζ be the depression at (x, y) below the mean level. Then

$$\frac{d\phi}{dz}=-v\frac{d\zeta}{dx} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

is the kinematic surface condition, and

$$p/\rho+\tfrac{1}{2}q^2-g\zeta=\text{const.}$$

the equation of pressure, which, since

$$q^2=\left(-v+\frac{d\phi}{dx}\right)^2+\left(\frac{d\phi}{dy}\right)^2+\left(\frac{d\phi}{dz}\right)^2$$

$$=v^2-2v\frac{d\phi}{dx}, \quad \text{(q. p.)}$$

gives

$$v\frac{d\phi}{dx}+g\zeta=0$$

and, therefore, with (1)

$$\frac{d\phi}{dz}=\frac{v^2}{g}\frac{d^2\phi}{dx^2}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

On account of the symmetry of the ship with respect to the median plane $y=0$, we have $d\phi/dy=0$ when $y=0$, except over the ship, where, if η is the semi-breadth at (x, z),

$$\begin{aligned}\frac{d\phi}{dy}&=-v\frac{d\eta}{dx}\\&=-vf(x,z) \quad \text{(say)}, \quad . \quad . \quad (3)\end{aligned}$$

and this condition is taken to hold at the plane $y=0$, instead of at the surface of the ship, the justification being the same as that for equation (1). Finally, $d\phi/dz=0$ at the bottom, $z=h$, of the water.

We now consider the solution for ϕ, in the part of the water where y is positive, with the given boundary conditions at $z=0$, $z=h$, $y=0$.

The typical term in the solution is

$$a\cos n(z-h)\cos(mx+\alpha)\cos(py+\beta),$$

where $m^2+n^2+p^2=0$. Here m must be taken real as the water extends from $x=-\infty$ to $x=+\infty$; n and p may be either real or imaginary, but if p is imaginary $[=ip']$ the last factor must take the form $e^{-p'y}$.

This term satisfies $d\phi/dz=0$ at $z=h$, and it also satisfies equation (2), if

$$n\tan nh=-v^2m^2/g. \quad . \quad . \quad . \quad . \quad (4)$$

This equation has an infinite number of real roots and *one* pure imaginary root given by

$$n'\tanh n'h=v^2m^2/g, \quad [n=in'].$$

We shall see that the imaginary root is alone responsible for the wave-making resistance. As for p it is always imaginary for the real roots of n, and is so for the imaginary root if $m>n'$.

The condition (3) will now require the expansion of the given function $f(x, z)$ in the form

$$\Sigma\Sigma a_{mn}\cos n(z-h)\cos(mx+\alpha),$$

where the summation with respect to m will take the form of an integral.

Suppose at first the function periodic in x so that

$$f(x+l,z)=f(x-l,z),$$

and put

$$f(x,z)=\Sigma_r\Sigma_n\left\{\mathrm{A}_{rn}\cos\frac{\pi r x}{l}+\mathrm{B}_{rn}\sin\frac{\pi r x}{l}\right\}\cos n(z-h),$$

where r is a positive integer.

By Fourier's method

$$\int_{-l}^{l} f(x,z)\cos\frac{\pi r x}{l}\,dx=l\,\Sigma_n \mathrm{A}_{rn}\cos n(z-h),$$

$$\int_{-l}^{l} f(x,z)\sin\frac{\pi r x}{l}\,dx=l\,\Sigma_n \mathrm{B}_{rn}\cos n(z-h),$$

where A_{on} is to be halved as usual.

Since the functions $\cos n(z-h)$ are all conjugate, as is easily proved, from these we get

$$\int_0^h\int_{-l}^{l} f(x,z)\cos\frac{\pi r x}{l}\cos n(z-h)\,dx\,dz=l\,\mathrm{A}_{rn}\int_0^h\cos^2 n(z-h)dz,$$

$$=l\,\mathrm{A}_{rn}\frac{1}{4n}(2nh+\sin 2nh)$$

and

$$\int_0^h\int_{-l}^{l} f(x,z)\sin\frac{\pi r x}{l}\cos n(z-h)dx\,dz=l\,\mathrm{B}_{rn}\frac{1}{4n}(2nh+\sin 2nh)\;;$$

where A_{oo} is to be halved; and the coefficients of the terms given by the imaginary roots, here, as always below, are got by putting $n=in'$; so that

$$\int_0^h\int_{-l}^{l} f(x,z)\cos\frac{\pi r x}{l}\cosh n(z-h)dx\,dz=l\,\mathrm{A}_{rn'}\frac{1}{4n'}(2n'h+\sinh 2n'h)$$

and so for $\mathrm{B}_{rn'}$.

Hence the theorem

$$f(x,z)=\Sigma_r\Sigma_n\frac{4n\cos n(z-h)}{l(2nh+\sin 2nh)}\int_{-l}^{l}\int_0^h f(\xi,\zeta)\cos\frac{\pi r}{l}(\xi-x)$$
$$\times\cos n(\zeta-h)d\zeta\,d\xi$$
$$+\Sigma_r\frac{4n'\cosh n'(z-h)}{l(2n'h+\sinh 2n'h)}\int_{-l}^{l}\int_0^h f(\xi,\zeta)\cos\frac{\pi r}{l}(\xi-x)$$
$$\times\cosh n(\zeta-h)d\zeta\,d\xi.$$

Now let l become infinite, and putting

$$\pi r/l=m$$
$$\pi/l=dm,$$

we get

$$f(x,z) = \frac{4}{\pi}\Sigma_n \int_0^{\infty}\int_{-\infty}^{\infty}\int_0^h f(\xi,\zeta)\,\frac{n\cos n(z-h)\cos n(\zeta-h)}{2nh+\sin 2nh} \times \cos m(\xi-x)\,d\zeta\,d\xi\,dm$$

$$+\frac{4}{\pi}\int_0^{\infty}\int_{-\infty}^{\infty}\int_0^h f(\xi,\zeta)\,\frac{n'\cosh n'(z-h)\cosh n'(\zeta-h)}{2n'h+\sinh 2n'h} \times \cos m(\xi-x)\,d\zeta\,d\xi\,dm.$$

In particular, suppose the depth of water infinite, we have then

$$nh = r\pi+\epsilon,$$

$$hdn = \pi,$$

$$\tan nh = \tan\epsilon,$$

$$\cos n(z-h) = (-)^r\cos(nz-\epsilon),$$

$$2n+(\sin 2nh)/h = 2n,$$

$$n\tan\epsilon = -km^2. \quad [k=v^2/g],$$

$$\sin 2\epsilon = \frac{-2km^2n}{n^2+k^2m^4},$$

$$\cos 2\epsilon = \frac{n^2-k^2m^4}{n^2+k^2m^4}.$$

Also

$$\tanh n'h = 1,$$

$$n = km^2,$$

$$\frac{\cosh n'(z-h)\cosh n'(\zeta-h)}{2n'h+\sinh 2n'h} = \tfrac{1}{2}e^{-n'(z+\zeta)}.$$

The result receives some confirmation from what has been observed with torpedo-boats at high speeds. It has been found that the *total* resistance varies as a power of the velocity which at first is nearly the second, but which, increasing to a maximum, ultimately becomes less than the second *. A very simple investigation, given below, shows that in shallow water, if we neglect all but the long waves, the wave-resistance varies ultimately as the first power of the velocity.

I may mention that somewhat similar work to that of the present paper gives a theory of the damping of the oscillations of ships due to wave-making. This I hope to give in a subsequent paper.

---

* White, p. 470.

Substituting, we get

$$f(x,z) = \frac{2}{\pi^2}\int_0^\infty\int_0^\infty\int_0^\infty\int_{-\infty}^\infty f(\xi,\zeta)\cos(nz-\epsilon)\cos(n\zeta-\epsilon) \times \cos m(\xi-x)d\xi\, d\zeta\, dm\, dn$$

$$+\frac{2v^2}{\pi g}\int_0^\infty\int_0^\infty\int_{-\infty}^\infty f(\xi,\zeta)m^2e^{-km^2(z+\zeta)}\cos m(\xi-x)d\xi\, d\zeta\, dm,$$

which is the theorem on which the rest of the present paper is built. It is curiously easy to give an *à posteriori* proof of the theorem. Using the value of ϵ given above we find

$$\cos(nz-\epsilon)\cos(n\zeta-\epsilon) = \cos nz\cos n\zeta - \cos n(z+\zeta)\frac{k^2m^4}{n^2+k^2m^4},$$

$$-\sin n(z+\zeta)\frac{km^2n}{n^2+k^2m^4}.$$

Integrate the last two terms with respect to n, viz.

$$k^2m^4\int_0^\infty\frac{\cos n(z+\zeta)}{n^2+k^2m^4}dn = \frac{\pi}{2}km^2e^{-km^2(z+\zeta)},$$

$$km^2\int_0^\infty\frac{n\sin n(z+\zeta)}{n^2+k^2m^4}dn = \frac{\pi}{2}km^2e^{-km^2(z+\zeta)},$$

and the quadruple integral becomes

$$\frac{2}{\pi^2}\int_0^\infty\int_0^\infty\int_0^\infty\int_{-\infty}^\infty f(\xi,\zeta)\cos nz\cos n\zeta\cos m(\xi-x)d\xi\, d\zeta\, dm\, dn$$

$$-\frac{2k}{\pi}\int_0^\infty\int_0^\infty\int_{-\infty}^\infty f(\xi,\zeta)m^2e^{-km^2(z+\zeta)}\cos m(\xi-x)d\xi\, d\zeta\, dm.$$

The former integral is $f(x,z)$, and the latter disappears with the triple integral in the given formula.

Considering now, for simplicity, the water infinitely deep, it appears at once that the required solution for ϕ is

$$\phi = \frac{2v}{\pi^2}\int_0^\infty\int_0^\infty\int_0^\infty\int_{-\infty}^\infty f(\xi,\zeta)\frac{\cos(nz-\epsilon)\cos(n\zeta-\epsilon)}{\sqrt{m^2+n^2}}\cos m(\xi-x)e^{-\sqrt{m^2+n^2}y} \times d\xi\, d\zeta\, dm\, dn$$

$$-\frac{2v^3}{\pi g}\int_{g/v^2}^\infty\int_0^\infty\int_{-\infty}^\infty f(\xi,\zeta)\frac{me^{-m^2v^2(z+\zeta)/g}}{\sqrt{m^2v^4/g^2-1}}\sin\ m(x-\xi)+m\sqrt{m^2v^4/g^2-1}y\} \times d\xi\, d\zeta\, dm$$

$$+\frac{2v^3}{\pi g}\int_0^{g/v^2}\int_0^\infty\int_{-\infty}^\infty f(\xi,\zeta)\frac{me^{-m^2v^2(z+\zeta)/g}}{\sqrt{1-m^2v^4/g^2}}\cos m(\xi-x)e^{-m\sqrt{1-m^2v^4/g^2}y}\, d\xi\, d\zeta\, dm; \quad \ldots\ldots (5)$$

for this gives

$$\frac{d\phi}{dy} = -vf(x,z) = -v\frac{d\eta}{dx} \text{ when } y=0.$$

In this expression attention must be called to the factor

$$\sin\{m(x-\xi)+m\sqrt{m^2v^4/g^2-1}\,y\}$$

in the second integral. This form is not required to satisfy the boundary conditions formulated above; and it is evident that the solution is to a certain extent indeterminate with those conditions, for we may superpose any system of free waves symmetrical with respect to $y=0$ on a particular solution satisfying them. The form of the factor in question is chosen in order to make the elementary diverging waves trail aft; in other words, to satisfy the condition that the ship advances into still water.

Leaving the reduction of the integrals on one side, for the present, we proceed to calculate the wave-resistance (R).

Let δp be the increase of pressure due to the wave-disturbance. Then

$$\mathrm{R} = -2\iint \delta p\frac{d\eta}{dx}dx\,dz,$$

the double integral extending over the median plane of the ship. Now measuring from the undisturbed surface

$$p = \varpi + g\rho z - \tfrac{1}{2}\rho q^2 + \tfrac{1}{2}\rho v^2,$$

and therefore

$$\delta p = \rho v\frac{d\phi}{dx}, \qquad \text{(q.p.)}$$

so that

$$\mathrm{R} = -2\rho v\iint\frac{d\phi}{dx}\frac{d\eta}{dx}dx\,dz.$$

Substituting the value of $d\phi/dx$, we see that the first and third integrals in the expression for ϕ add nothing to the resistance because

$$\iiiint f(\xi,\zeta)f(x,z)\sin m(x-\xi)\,d\xi\,d\zeta\,dx\,dz = 0,$$

on account of the odd factor $\sin m(x-\xi)$,
and hence

$$\mathrm{R} = \frac{4\rho v^4}{\pi g}\int_{g/v^2}^{\infty}\int_{0}^{\infty}\int_{-\infty}^{\infty}\int_{0}^{\infty}\int_{-\infty}^{\infty} f(x,z)f(\xi,\zeta)\frac{m^2e^{-m^2v^2(z+\zeta)/g}}{\sqrt{m^2v^4/g^2-1}}\cos m(x-\xi)\,dx\,dz\,d\xi\,d\zeta\,dm$$

$$= \frac{4\rho v^4}{\pi g}\int_{g/v^2}^{\infty}(\mathrm{I}^2+\mathrm{J}^2)\frac{m^2dm}{\sqrt{m^2v^4/g^2-1}}$$

$$= \frac{4\rho g^2}{\pi v^2}\int_{1}^{\infty}(\mathrm{I}^2+\mathrm{J}^2)\frac{\lambda^2d\lambda}{\sqrt{\lambda^2-1}}, \quad \ldots\ldots\ldots \quad (6)$$

where $$\lambda = mv^2/g,$$
and

$$\mathrm{I} = \int_0^\infty \int_{-\infty}^\infty f(x, z) e^{-\lambda^2 gz/v^2} \cos \lambda gx/v^2 \, dx \, dz,$$

$$\mathrm{J} = \int_0^\infty \int_{-\infty}^\infty f(x, z) e^{-\lambda^2 gz/v^2} \sin \lambda gx/v^2 \, dx \, dz.$$

If the ship is similarly formed at bow and stern $\mathrm{I}=0$, the origin being at midship.

We can now prove that the resistance vanishes when the velocity is infinite.

Observe that

$$\int_0^\infty f(x, z) e^{-\lambda^2 gz/v^2} dz = \mathrm{F}(x) \int_0^\infty e^{-\lambda^2 gz/v^2} \, dz$$
$$= \frac{1}{\lambda^2} \frac{v^2}{g} \mathrm{F}(x), \quad \ldots \ldots \quad (7)$$

where $\mathrm{F}(x)$ is less than the greatest value of $f(x, z)$ for a given value of x; and, therefore, if we substitute a large number t instead of ∞ as the upper limit of λ, the part of R neglected is of order not greater than

$$v^2 \int_t^\infty \frac{d\lambda}{\lambda^3} \qquad \text{or} \qquad v^2 t^{-2},$$

and this vanishes when $v=\infty$ if we take

$$t = \left(\frac{\;}{g} \right)^{2/3}.$$

In the part of R retained $\lambda g/v^2$ is small throughout, so that we may expand the circular functions and write

$$\mathrm{I} = \int_0^\infty \int_{-\infty}^\infty f(x, z) e^{-\lambda^2 gz/v^2} \, dx \, dz$$
$$- \frac{1}{2} \lambda^2 \frac{g^2}{v^4} \int_0^\infty \int_{-\infty}^\infty f(x, z) x^2 e^{-\lambda^2 gz/v^2} \, dx \, dz + \ldots$$

and

$$\mathrm{J} = \lambda \frac{g}{v^2} \int_0^\infty \int_{-\infty}^\infty f(x, z) x e^{-\lambda^2 gz/v^2} \, dx \, dz$$
$$- \frac{1}{6} \lambda^3 \frac{g^3}{v^6} \int_0^\infty \int_{-\infty}^\infty f(x, z) x^3 e^{-\lambda^2 gz/v^2} \, dx \, dz + \ldots$$

Now

$$\int_{-\infty}^{\infty} f(x, z)dx = \int_{-\infty}^{\infty} \frac{d\eta}{dx}\, dx = 0,$$

the ship being of finite length and $\eta = 0$ at both ends. Hence, using the formula (7)

$$\mathrm{I} = \mathrm{A}\frac{g}{v^2} + \mathrm{B}\lambda^2\left(\frac{g}{v^2}\right)^3 + \dots,$$

$$\mathrm{J} = \mathrm{A}'\frac{1}{\lambda} + \mathrm{B}'\lambda\left(\frac{g}{v^2}\right)^2 + \dots.$$

and

$$\mathrm{R} = \frac{4\rho g^2}{\pi v^2}\int_1^t \left[\mathrm{A}''\frac{1}{\lambda^2} + \mathrm{B}''\left(\frac{g}{v^2}\right)^2 + \mathrm{C}''\lambda^2\left(\frac{g}{v^2}\right)^4 + \dots\right]\frac{\lambda^2 d\lambda}{\sqrt{\lambda^2 - 1}}.$$

The successive terms are of orders

$$\frac{1}{v^2}\log v^2, \qquad v^{-10/3}, \quad \dots$$

and all vanish when $v = \infty$. The resistance therefore ultimately vanishes. Of course this result is only proved for a ship which is very short in comparison with the depth of the water.

We now proceed to the reduction of the integral which gives the resistance due to two elements of the surface.

Consider two elementary areas σ, σ' at (x, z), (x', z') on the side of the ship, and let θ, θ' be the inclinations of the horizontal lines in these areas to the axis of x. The resistance due to these two elements is

$$\frac{8\rho g^2}{\pi v^2}\sigma\,\sigma'\,\theta\,\theta'\int_1^{\infty} e^{-\lambda^2 g(z+z')/v^2}\cos\lambda g(x - x')/v^2 \frac{\lambda^2}{\sqrt{\lambda^2 - 1}}\,d\lambda,$$

or, say,

$$\frac{8\rho g^2}{\pi v^2}\sigma\,\sigma'\,\theta\,\theta'\int_1^{\infty} e \quad {}^2\cos\varepsilon\lambda\frac{\lambda^2}{\sqrt{\lambda^2 - 1}}d\lambda.$$

Now, writing for the moment $z \equiv x + iy$, and taking the integral

$$\int \frac{e^{isz}}{\sqrt{1 - z^2}}dz$$

around the circuit enclosing the region x and y positive, we get

$$\int^{\infty} \frac{e}{\sqrt{1 - x^2}}dx + i\int_1^{\infty} \frac{e^{isx}}{\sqrt{x^2 - 1}}dx - i\int_0^{\infty} \frac{e^{-sx}}{\sqrt{1 + x^2}}dx = 0,$$

and, therefore [*], realizing,

$$\int_1^\infty \frac{\cos s\lambda}{\sqrt{\lambda^2-1}}\,d\lambda=\int_0^\infty \frac{e^{-s\lambda}}{\sqrt{1+\lambda^2}}d\lambda-\int_0^1 \frac{\sin s\lambda}{\sqrt{1-\lambda^2}}\,d\lambda$$

$$=\int_0^\infty e^{-s\sinh\theta}\,d\theta-\int_0^{\frac{\pi}{2}}\sin(s\sin\theta)\,d\theta$$

$$=\kappa J_0(s)-Y_0(s), \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

where $\kappa=\log 2-\gamma=\cdot 11593\ldots.$

From which

$$-\frac{d^2}{ds^2}\int_1^\infty \frac{\cos s\lambda}{\sqrt{\lambda^2-1}}=-\{\kappa J_0''(s)-Y_0''(s)\}$$

$$=\{\kappa J_0(s)-Y_0(s)\}$$

$$-\frac{1}{s}\{\kappa J_1(s)-Y_1(s)\},$$

since

$$J_0''(s)+J_0'(s)/s+J_0(s)=0$$

$$J_0'(s)=-J_1(s),$$

and so for $Y_0(s)$.

Now taking

$$H=\int_1^\infty e^{-r\lambda^2}\cos s\lambda\frac{d\lambda}{\sqrt{\lambda^2-1}},$$

and putting

$$e^{-r\lambda^2}=\frac{1}{\sqrt{\pi r}}\int_0^\infty e^{-\mu^2/4r}\cos\lambda\mu\,d\mu,$$

$$H=\frac{1}{2\sqrt{\pi r}}\int_0^\infty\int_1^\infty e^{-\mu^2/4r}\{\cos\lambda(s+\mu)+\cos\lambda(s-\mu)\}\frac{d\lambda d\mu}{\sqrt{\lambda^2-1}}$$

$$=\frac{1}{2\sqrt{\pi r}}\int_0^\infty e^{-\mu^2/4r}\{\kappa J_0(s+\mu)-Y_0(s+\mu)$$

$$+\kappa J_0(s-\mu)-Y_0(s-\mu)\}d\mu, \quad . \quad (9)$$

from equation (8).

From which H can be readily calculated by mechanical quadrature in the case in which $4r$ is not large, and this is the case for ordinary ships. Elaborate tables of J_0 and J_1

---

[*] See Gray and Mathews, 'Bessel Functions,' p. 65, and Ex. 18, p. 230. The formula (8) was given by Weber.

are now available, and tables* of Y_0 and Y_1 have been calculated by Mr. B. A. Smith, who has kindly prepared tables of $\kappa J_0 - Y_0$ and $\kappa J_1 - Y_1$, appended to the present paper.

We now have

$$-\frac{d^2H}{ds^2} = \frac{1}{2\sqrt{\pi r}} \int_0^\infty e^{-\mu^2/4r}\{F(s+\mu)+F(s-\mu)\}d\mu,$$

where

$$F(s+\mu) = \kappa J_0(s+\mu) - Y_0(s+\mu)$$
$$-\frac{1}{s+\mu}\{\kappa J_1(s+\mu) - Y_1(s+\mu)\},$$

and the expression for the mutual resistance is

$$\frac{4\rho g^2}{\pi^{3/2} v^2 \sqrt{r}} \sigma\sigma'\theta\theta' \int_0^\infty e^{-\mu^2/4r}\{F(s+\mu)+F(s-\mu)\}d\mu,$$

where

$$r = g(z+z')/v^2$$
$$s = g(x-x')/v^2.$$

For elements at opposite ends of the ship s will in general be large compared with unity and with $\sqrt{4r}$, and in this case we can put

$$\kappa J_0(s+\mu) - Y_0(s+\mu) = \sqrt{\frac{\pi}{2(s+\mu)}} \sin\left\{\frac{\pi}{4} - (s+\mu)\right\} \text{ q. p.}$$

and so for $(s-\mu)$, and then approximately

$$H = \frac{1}{\sqrt{2rs}} \sin\left(\frac{\pi}{4} - s\right) \int_0^\infty e^{-\mu^2/4r} \cos\mu\, d\mu$$
$$= \sqrt{\frac{\pi}{2s}} \cdot \sin\left(\frac{\pi}{4} - s\right) e^{-r},$$

and the resistance is

$$\frac{8\rho g^2}{\pi v^2} \sigma\sigma'\theta\theta' \sqrt{\frac{\pi}{2s}} \cdot \sin\left(\frac{\pi}{4} - s\right) e^{-r}$$
$$= \frac{8\rho g^{3/2}}{v\sqrt{2\pi(x-x')}} \sigma\sigma'\theta\theta' \sin\left\{\frac{\pi}{4} - g(x-x')/v^2\right\} e^{-g(z+z')/v^2}.$$

Now if l is the length of the free wave which travels with the velocity of the ship

$$v^2 = gl/2\pi,$$

---

* 'Messenger of Mathematics,' 1896.

and the formula can be written

$$\frac{8g\rho\sigma\sigma'\theta\theta'}{\sqrt{l(x-x')}}\sin\pi\left\{\frac{1}{4}-2(x-x')/l\right\}e^{-2\pi(z+z')/l}.$$

This gives a maximum resistance, approximately, when

$$x-x'=(n+7/8)l \qquad [n \text{ an integer}]$$

and a maximum assistance when

$$x-x'=(n+3/8)l.$$

These formulæ correspond to the interference of the bow and stern waves, which has been so skilfully discussed by Mr. R. E. Froude. When the two elements are on the same vertical cross-section of the ship, another form of reduction may be given. Putting $x-x'=0$, the integral to be considered is

$$G=\int_1^\infty e^{-r\lambda^2}\frac{d\lambda}{\sqrt{\lambda^2-1}}.$$

Put $$\lambda^2=\tfrac{1}{2}(1+\mu),$$

so that $$d\lambda=\frac{1}{2\sqrt{2}}\frac{d\mu}{\sqrt{1+\mu}},$$

and $$\lambda^2-1=\tfrac{1}{2}(\mu-1);$$

whence $$G=\tfrac{1}{2}e^{-r/2}\int_1^\infty e^{-r\mu/2}\frac{d\mu}{\sqrt{\mu^2-1}};$$

or, if $$\mu=\cosh\phi,$$

$$G=\tfrac{1}{2}e^{-r/2}\int_0^\infty e^{-(r\cosh\phi)/2}\,d\phi$$
$$=\tfrac{1}{2}e^{-r/2}K_0(r/2),$$

where K_0 is the Bessel function, so indicated by Gray and Mathews (pp. 67, 90).

Hence

$$\int_1^\infty e^{-r\lambda^2}\frac{\lambda^2\,d\lambda}{\sqrt{\lambda^2-1}}=-\frac{dG}{dr}$$
$$=\tfrac{1}{4}e^{-r/2}\{K_0(r/2)-K_0'(r/2)\}$$
$$=\tfrac{1}{4}e^{-r/2}\{K_0(r/2)-K_1(r/2)\}.$$

since $$K_0'=K_1,$$

and the corresponding term in the resistance is

$$\frac{2\rho g^2\sigma\sigma'\theta\theta'}{\pi v^2}e^{-g(z+z')/2v^2}\left[\mathrm{K}_0\{g(z+z')/2v^2\}-\mathrm{K}_1\{g(z+z')/2v^2\}\right].$$

The functions K do not appear to have been calculated, but their general course is known. It will be sufficient at present to refer to Gray and Mathews, chap. vii.

As an illustration of the process of calculating the resistance of a given ship at any speed, we may consider one of simple analytical form which is fairly ship-shape and easily realisable. Experimental confirmation of the result was not practicable, and the matter must be left in the hands of those who have the necessary apparatus at command.

Let the surface of the ship be

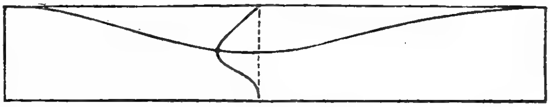
$$y=\pm c(1+\cos ax)(1+\cos bz),$$

between

$$x=\pm\pi/a,$$

$$z=0 \quad\text{and}\quad \pi/b\,;$$

Fig. 2.



so that, for y positive,

$$f(x,z)=\frac{dy}{dx}=-ac\sin ax(1+\cos bz).$$

Here $\mathrm{I}=0$,

and

$$\mathrm{J}=-ac\int_0^{\pi/b}(1+\cos bz)\,e^{-\lambda^2 z/k}dz\int_{-\pi/a}^{\pi/a}\sin ax\sin\lambda x/k\,dx$$

$$=-ac\frac{k}{\lambda^2}(2\lambda^4+b^2k^2-e^{-\pi\lambda^2/bk}\,b^2k^2)\frac{2ak^2}{(\lambda^4+b^2k^2)(a^2k^2-\lambda^2)}\sin\pi\lambda/ka,$$

where $k=v^2/g$; and therefore

$$\mathrm{R}=\frac{16g\rho}{\pi}a^4c^2k^5\int_1^\infty(2\lambda^4+b^2k^2-e^{-\pi\lambda^2/bk}\,b^2k^2)^2\frac{\sin^2\pi\lambda/ka}{(\lambda^4+b^2k^2)^2(a^2k^2-\lambda^2)^2}\frac{d\lambda}{\lambda^2\sqrt{\lambda^2-1}}$$

which is best calculated by mechanical quadrature.

Suppose, for example, in foot-second units

$v=20$ (velocity of ship),
$2\pi/a=200$ (length of ship),
$\pi/b=20$ (depth below water-line),
$8c=32$ (greatest breadth),

then the integral is found to be ·620, and the resistance is

$$R = 940 \text{ lbs. wt. about.}$$

This seems to be about what one would expect from the experimental results available; but I know of no formula with which to compare it, and experiment alone can decide whether the theory has numerical value. Of course the method of successive approximations can be applied if necessary.

To examine the case of the ship in shallow water in which all but the long waves are neglected, we may proceed as follows:—We make the motion steady as before and use the same set of axes. The pressure-equation gives at the surface

$$g\zeta + v\frac{d\phi}{dx} = 0, \text{ as before ;}$$

while the equation of continuity for long waves gives

$$\frac{d}{dx}\left\{(h-\zeta)\left(-v+\frac{d\phi}{dx}\right)\right\} + \frac{d}{dy}\left\{(h-\zeta)\frac{d\phi}{dy}\right\} = 0,$$

where h is the depth of the water, and this is

$$v\frac{d\zeta}{dx} + h\left(\frac{d^2\phi}{dx^2} + \frac{d^2\phi}{dy^2}\right) = 0 \quad \text{(q. p.)}$$

Differentiating with respect to x and substituting for ϕ from the pressure-equation, we obtain

$$(gh - v^2)\frac{d^2\zeta}{dx^2} + gh\frac{d^2\zeta}{dy^2} = 0,$$

or

$$(c^2 - v^2)\frac{d^2\zeta}{dx^2} + c^2\frac{d^2\zeta}{dy^2} = 0,$$

where c is the velocity of free long waves.

The ship being wall-sided, and extending to the bottom of the water, the kinematic equation over the ship is

$$\frac{d\phi}{dy} = -v\frac{d\eta}{dx},$$

which on differentiation with respect to x and use of the pressure-equation gives

$$g\frac{d\zeta}{dy} = v^2\frac{d^2\eta}{dx^2}.$$

and, of course,

$$\frac{d\zeta}{dy} = 0$$

over the rest of the plane $y = 0$.

Now if $v>c$, i.e. if the velocity of the ship is greater than that of the free wave, the equation

$$\frac{d^2\zeta}{dy^2}=\frac{v^2-c^2}{c^2}\frac{d^2\zeta}{dx^2}$$

is solved in the form

$$\zeta=\mathrm{F}\left(x+\sqrt{\frac{v^2-c^2}{c^2}}y\right), \quad . \quad . \quad . \quad . \quad (10)$$

where the boundary condition gives

$$\sqrt{\frac{v^2-c^2}{c^2}}\,\mathrm{F}'(x)=\frac{v^2}{g}\frac{d^2\eta}{dx^2},$$

or

$$\zeta=\mathrm{F}(x)=\frac{v^2c}{g\sqrt{v^2-c^2}}\frac{d\eta}{dx}. \quad . \quad . \quad . \quad . \quad (11)$$

The form of solution (10) is employed in order to make the diverging waves trail aft.

The disturbance therefore consists of two bands at an angle $\tan^{-1}(c/\sqrt{v^2-c^2})$ with the line of the ship's motion, the front of each band being a hump above the mean level and its back part a hollow, which is similar to the hump if the ship is similarly shaped fore and aft.

The resistance (R) is given by

$$\begin{aligned}\mathrm{R}&=-2h\int\delta p\,\frac{d\eta}{dx}\,dx\\&=2g\rho h\int\zeta\,\frac{d\eta}{dx}\,dx\\&=2\rho h\frac{v^2c}{\sqrt{v^2-c^2}}\int\left(\frac{d\eta}{dx}\right)^2dx,\end{aligned}$$

so that it is infinite when the velocity of the ship is equal to that of the free wave, and ultimately varies as the velocity.

If $v<c$, the differential equation for ζ takes the potential form

$$\frac{d^2\zeta}{dx^2}+\frac{d^2\zeta}{dy'^2}=0,$$

putting

$$y'=\frac{\sqrt{c^2-v^2}}{c}y.$$

The solution is now

$$\zeta(x',y')=\frac{1}{\pi}\frac{v^2}{g}\frac{c}{\sqrt{c^2-v^2}}\int\frac{d^2\eta}{dx^2}\log r'\,dx+\mathrm{C},$$

where $$r'^2 = (x'-x)^2 + y'^2,$$

and there is no wave-resistance.

If we add the solutions for an equally-spaced infinite number of ships moving abreast, we get the case of a ship in the centre of a canal.

In the Table appended there may be an error of 1 in the last place or possibly of 2 in the values for $x > 3$ or 4.

Melbourne University,
August 9, 1897.

Tables of $\kappa J_0 - Y_0$ and $\kappa J_1 - Y_1$.

By Mr. B. A. Smith, M.C.E.

x.	$\kappa J_0(x)-Y_0(x)$.	$\kappa J_1(x)-Y_1(x)$.	x.	$\kappa J_0(x)-Y_0(x)$.	$\kappa J_1(x)-Y_1(x)$.
·00	∞	∞	·41	·9243	2·7384
·01	4·7209	100·0261	·42	·8972	2·6822
·02	4·0274	50·0453	·43	·8706	2·6286
·03	3·6215	33·3951	·44	·8446	2·5773
·04	3·3331	25·0767	·45	·8190	2·5282
·05	3·1091	20·0903	·46	·7940	2·4813
·06	2·9258	16·7695	·47	·7694	2·4362
·07	2·7705	14·4002	·48	·7453	2·3929
·08	2·6359	12·6255	·49	·7216	2·3514
·09	2·5168	11·2470	·50	·6983	2·3114
·10	2·4099	10·1457	·51	·6753	2·2729
·11	2·3133	9·2459	·52	·6528	2·2357
·12	2·2245	8·4971	·53	·6306	2·1999
·13	2·1428	7·8645	·54	·6088	2·1653
·14	2·0670	7·3230	·55	·5873	2·1319
·15	1·9961	6·8545	·56	·5661	2·0995
·16	1·9297	6·4450	·57	·5453	2·0681
·17	1·8671	6·0843	·58	·5248	2·0377
·18	1·8079	5·7642	·59	·5046	2·0083
·19	1·7517	5·4780	·60	·4846	1·9798
·20	1·6982	5·2209	·61	·4650	1·9521
·21	1·6472	4·9888	·62	·4456	1·9251
·22	1·5983	4·7779	·63	·4264	1·8988
·23	1·5515	4·5855	·64	·4076	1·8732
·24	1·5066	4·4094	·65	·3890	1·8483
·25	1·4633	4·2476	·66	·3707	1·8241
·26	1·4216	4·0983	·67	·3525	1·8005
·27	1·3813	3·9603	·68	·3346	1·7775
·28	1·3424	3·8323	·69	·3169	1·7550
·29	1·3046	3·7131	·70	·2995	1·7329
·30	1·2680	3·6020	·71	·2823	1·7114
·31	1·2326	3·4982	·72	·2653	1·6904
·32	1·1981	3·4007	·73	·2485	1·6699
·33	1·1645	3·3094	·74	·2319	1·6496
·34	1·1319	3·2233	·75	·2155	1·6299
·35	1·1000	3·1423	·76	·1993	1·6105
·36	1·0690	3·0656	·77	·1833	1·5914
·37	1·0387	2·9932	·78	·1675	1·5728
·38	1·0092	2·9245	·79	·1518	1·5544
·39	·9803	2·8593	·80	·1363	1·5365
·40	·9519	2·7973	·81	·1211	1·5188

Table (*continued*).

x.	$\kappa J_0(x) - Y_0(x)$.	$\kappa J_1(x) - Y_1(x)$.	x.	$\kappa J_0(x) - Y_0(x)$.	$\kappa J_1(x) - Y_1(x)$.
·82	·1060	1·5015	4·8	·4277	−·3354
·83	·0910	1·4843	4·9	·4588	−·2847
·84	·0763	1·4674	5·0	·4846	−·2323
·85	·0617	1·4509	5·1	·5052	−·1786
·86	·0473	1·4346	5·2	·5203	−·1244
·87	·0330	1·4185	5·3	·5300	−·0699
·88	·0189	1·4026	5·4	·5343	−·0159
·89	·0050	1·3870	5·5	·5332	+·0373
·90	−·0088	1·3715	5·6	·5269	·0892
·91	−·0224	1·3563	5·7	·5155	·1393
·92	−·0359	1·3413	5·8	·4991	·1873
·93	−·0493	1·3264	5·9	·4782	·2326
·94	−·0625	1·3117	6·0	·4527	·2749
·95	−·0756	1·2972	6·1	·4233	·3139
·96	−·0885	1·2829	6·2	·3900	·3492
·97	−·1012	1·2688	6·3	·3535	·3805
·98	−·1138	1·2548	6·4	·3140	·4077
99	−·1263	1·2408	6·5	·2721	·4306
1·0	−·1386	1·2271	6·6	·2281	·4488
1·1	−·2548	1·0966	6·7	·1826	·4625
1·2	−·3583	·9757	6·8	·1358	·4715
1·3	−·4501	·8616	6·9	·0884	·4758
1·4	−·5308	·7526	7·0	·0408	·4755
1·5	−·6008	·6477	7·1	−·0065	·4704
1·6	−·6604	·5460	7·2	−·0531	·4609
1·7	−·7101	·4473	7·3	−·0986	·4471
1·8	−·7500	·3513	7·4	−·1424	·4290
1·9	−·7805	·2583	7·5	−·1842	·4071
2·0	−·8017	·1682	7·6	−·2237	·3814
2·1	−·8142	·0812	7·7	−·2604	·3524
2·2	−·8180	−·0023	7·8	−·2940	·3203
2·3	−·8138	−·0821	7·9	−·3243	·2854
2·4	−·8017	−·1580	8·0	−·3510	·2484
2·5	−·7824	−·2293	8·1	−·3739	·2093
2·6	−·7560	−·2959	8·2	−·3928	·1684
2·7	−·7234	−·3576	8·3	−·4076	·1266
2·8	−·6848	−·4139	8·4	−·4182	·0840
2·9	−·6407	−·4649	8·5	−·4244	·0408
3·0	−·5920	−·5100	8·6	−·4264	−·0020
3·1	−·5390	−·5492	8·7	−·4242	−·0438
3·2	−·4823	−·5823	8·8	−·4177	−·0851
3·3	−·4227	−·6092	8·9	−·4071	−·1251
3·4	−·3606	−·6299	9·0	−·3926	−·1637
3·5	−·2970	−·6444	9·1	−·3744	−·2001
3·6	−·2320	−·6525	9·2	−·3527	−·2343
3·7	−·1666	−·6546	9·3	−·3277	−·2657
3·8	−·1014	−·6505	9·4	−·2996	−·2940
3·9	−·0368	−·6406	9·5	−·2688	−·3194
4·0	+·0266	−·6251	9·6	−·2359	−·3412
4·1	+·0881	−·6042	9·7	−·2012	−·3594
4·2	·1472	−·5781	9·8	−·1642	−·3738
4·3	·2035	−·5472	9·9	−·1261	−·3845
4·4	·2566	−·5120	10·0	−·0874	−·3912
4·5	·3058	−·4728	10·1	−·0481	−·3941
4·6	·3510	−·4300	10·2	−·0087	−·3923
4·7	·3917	−·3842	10·3	+·0302	

XII. *Intelligence and Miscellaneous Articles.*

ON THE CONDUCTIVITY OF CARBON FOR HEAT AND ELECTRICITY.

BY L. CELLIER.

THE results of this investigation are contained in the following table, in which T is the temperature; K_u the conductivity for heat; K_e the conductivity for electricity; and c_1 the specific heat of unit volume:—

	T.	c_1.	K_w.	K_e.	$\frac{K_w}{K_e}$.
Graphite	$6^{\circ}84$	0·3055	0·701	$13{\cdot}049\times10-9$	$53{\cdot}72\times10^6$
Gas Graphite	9·04	0·2782	0·400	$185{\cdot}36\times10-9$	$2{\cdot}158\times10^6$
Arc Light Charcoal of Paris	7·83	0·2667	0·494	$267{\cdot}88\times10-9$	$1{\cdot}844\times10^6$
Arc Light Charcoal of Siemens and Halske	9·04	0·2488	0·367	$146{\cdot}32\times10-9$	$2{\cdot}509\times10^6$

Professor Weber found for metals that

$$\frac{K_w}{K_e}=a+bc_1, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where

$$a=5{\cdot}28\times10^4,$$
$$b=8{\cdot}19\times10^4.$$

The table of the values of the quotient K_w/K_e shows distinctly that *such a relation does not hold in the case of carbon;* further, that carbon cannot be at all compared with metals, for while in the latter the ratio K_w/K_e varies between $0{\cdot}07\times10^6$ and $0{\cdot}12\times10^6$, in the former it varies between $1{\cdot}8\times10^6$ and $53{\cdot}72\times10^6$. The thermal conductivity is 15 to 20 times as much as would be deduced from the above ratio by means of the specific heat and the electrical conductivity. This relation between the two conductivities appears to be connected with the metallic nature of the substances.—Wiedemann's *Annalen*, No. 7, 1897.

THE GENESIS OF DALTON'S ATOMIC THEORY.

BY H. DEBUS, PH.D., F.R.S.

Sir H. Roscoe and Mr. A. Harden have published in the Phil. Mag. (March 1897) a paper on the Genesis of Dalton's Atomic Theory, wherein several statements are made against the views held by me. These statements of Messrs. Roscoe and Harden are, in my opinion, erroneous. I have fully answered them in a paper published in the *Zeitschrift für physikalische Chemie* von W. Ostwald and J. H. van't Hoff, vol. xxiv. p. 325, to which I beg to refer readers of the Philosophical Magazine who take an interest in the subject.

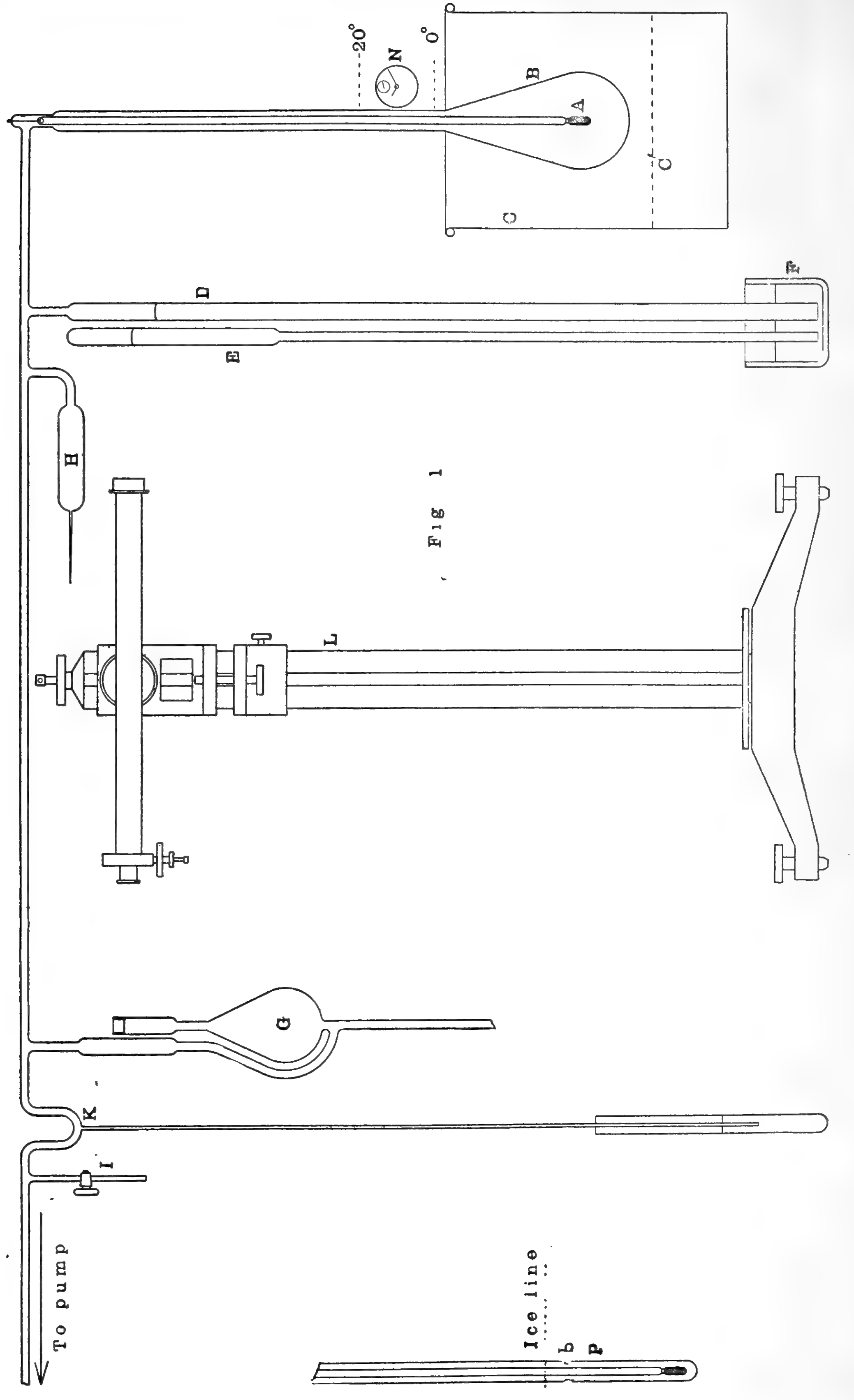


Fig 1

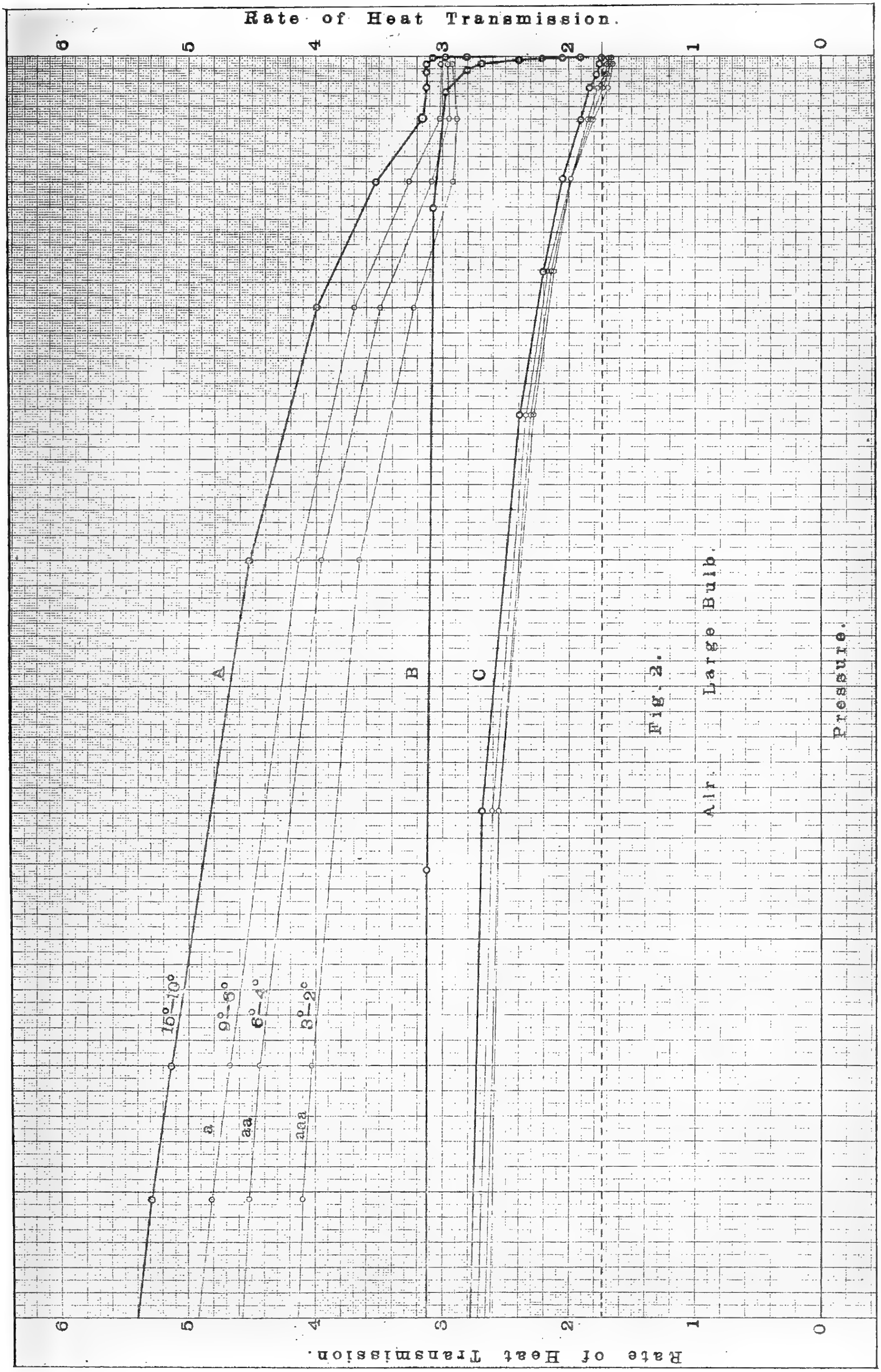


Fig. 2. Air. Large Bulb.

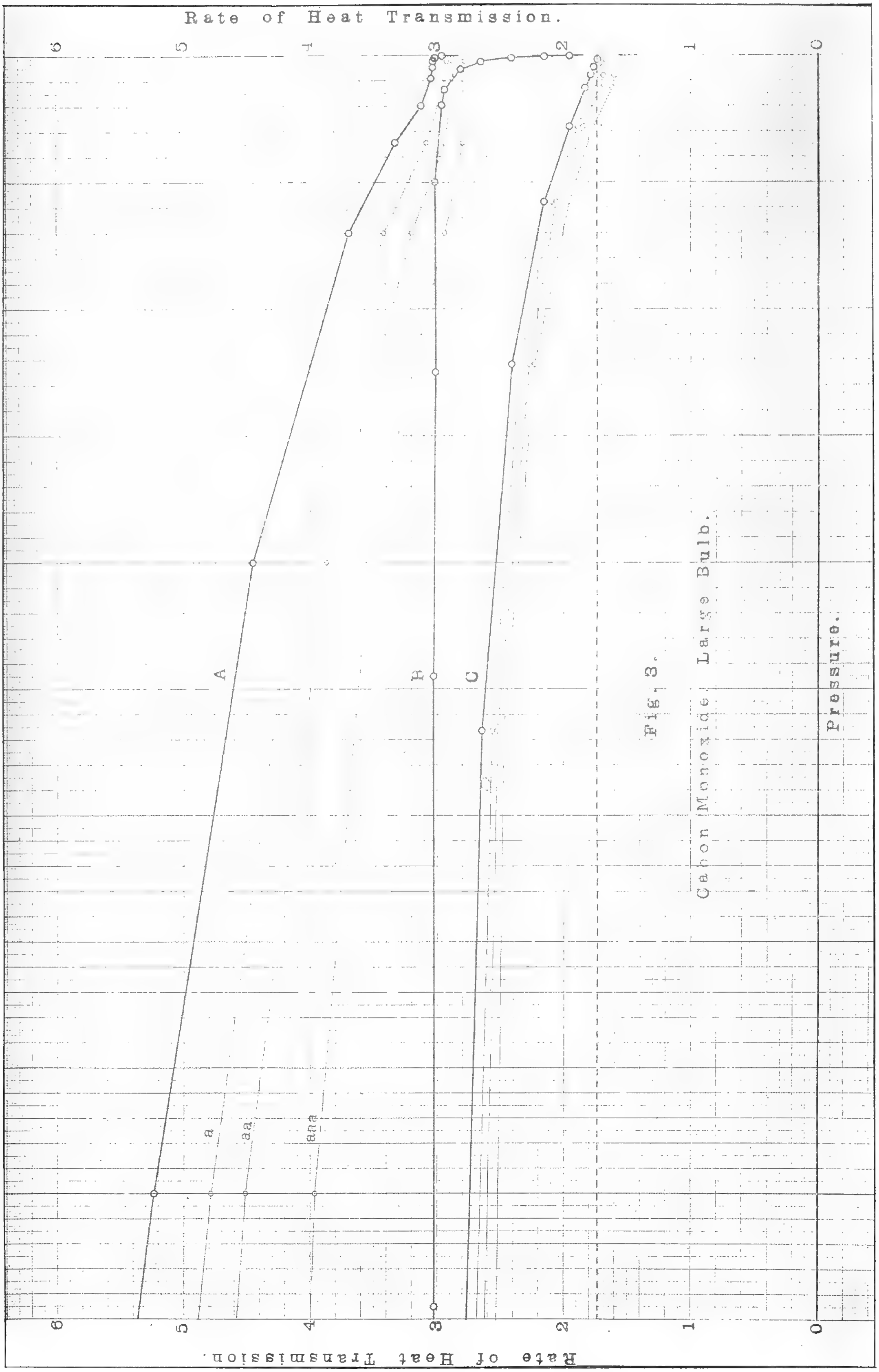


Fig. 3.

Cabon Monoxide. Large Bulb.

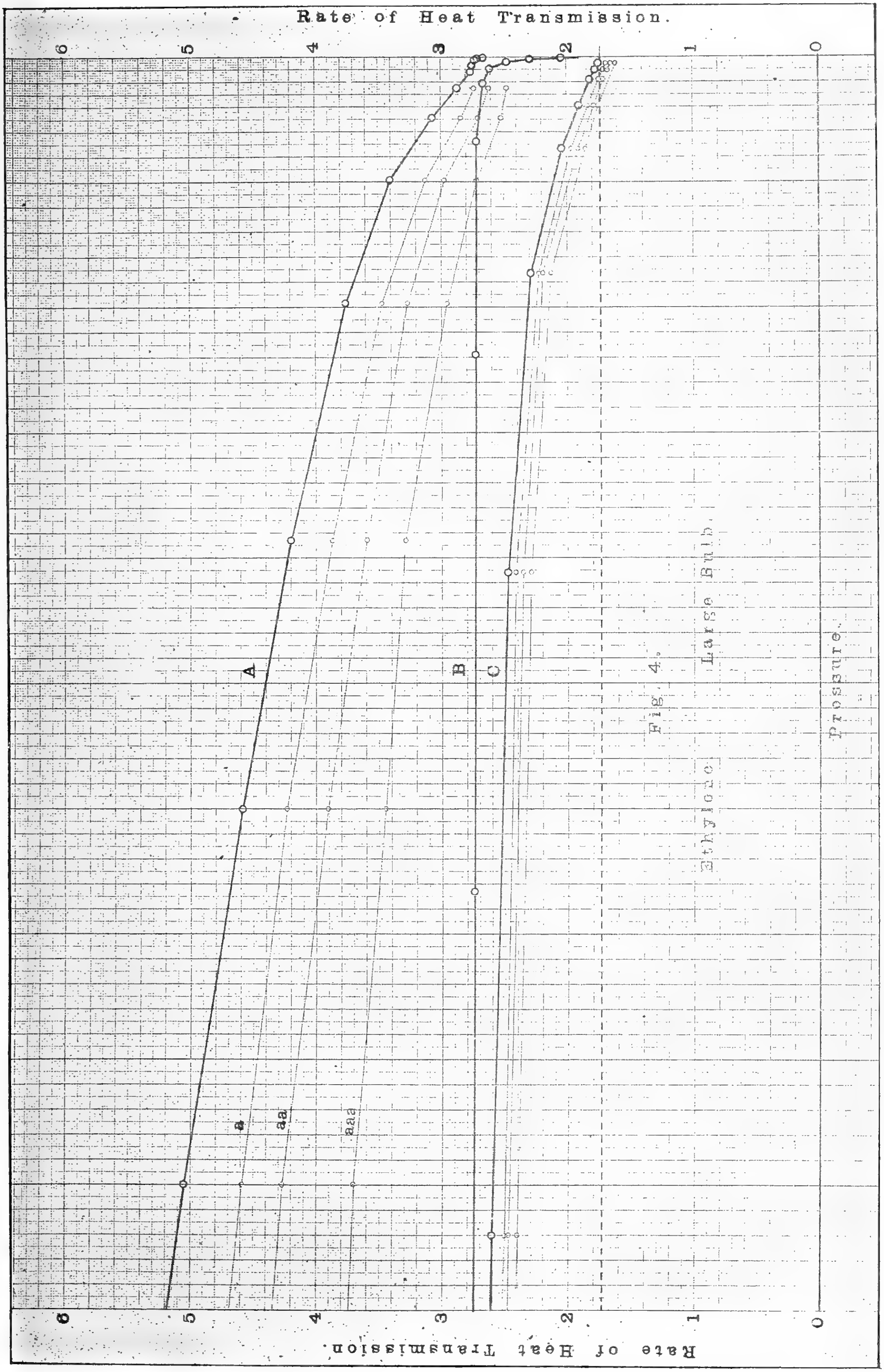
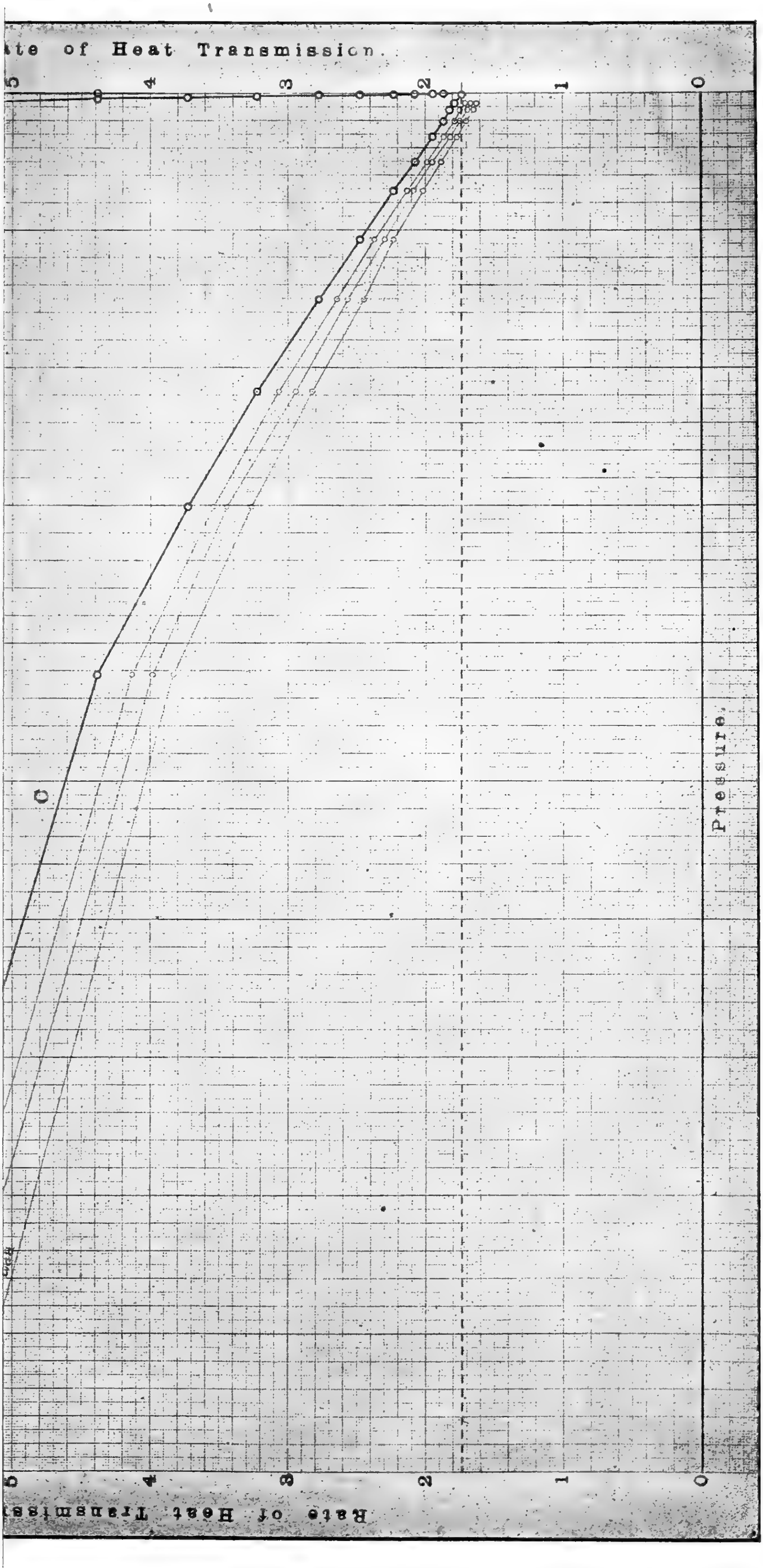


Fig. 4. Ethylene. Large Bulb.


te of Heat Transmission.
5
4
3
2
1
0
C
Pressure.
5
4
3
2
1
0
Rate of Heat Transmissi

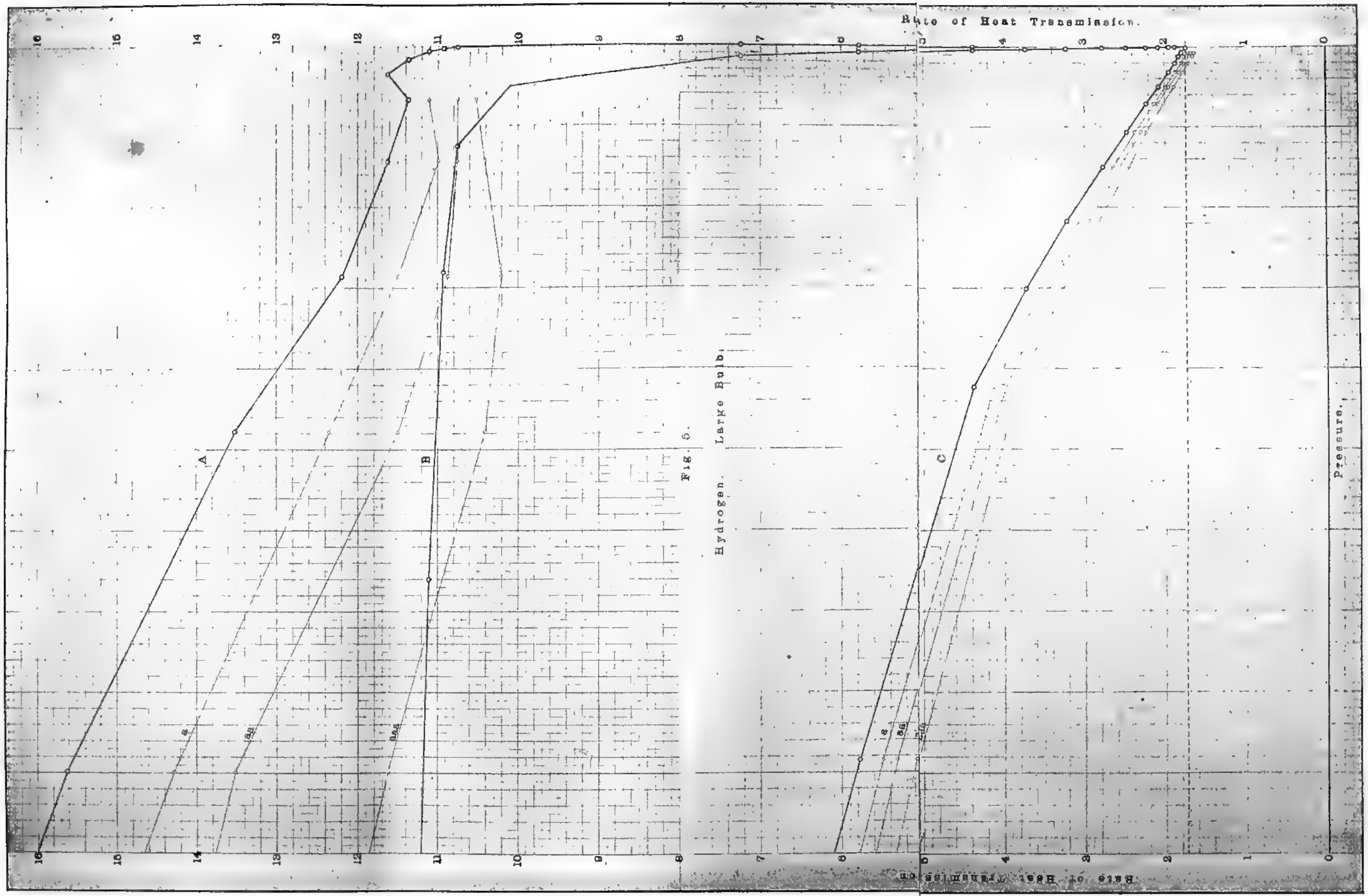
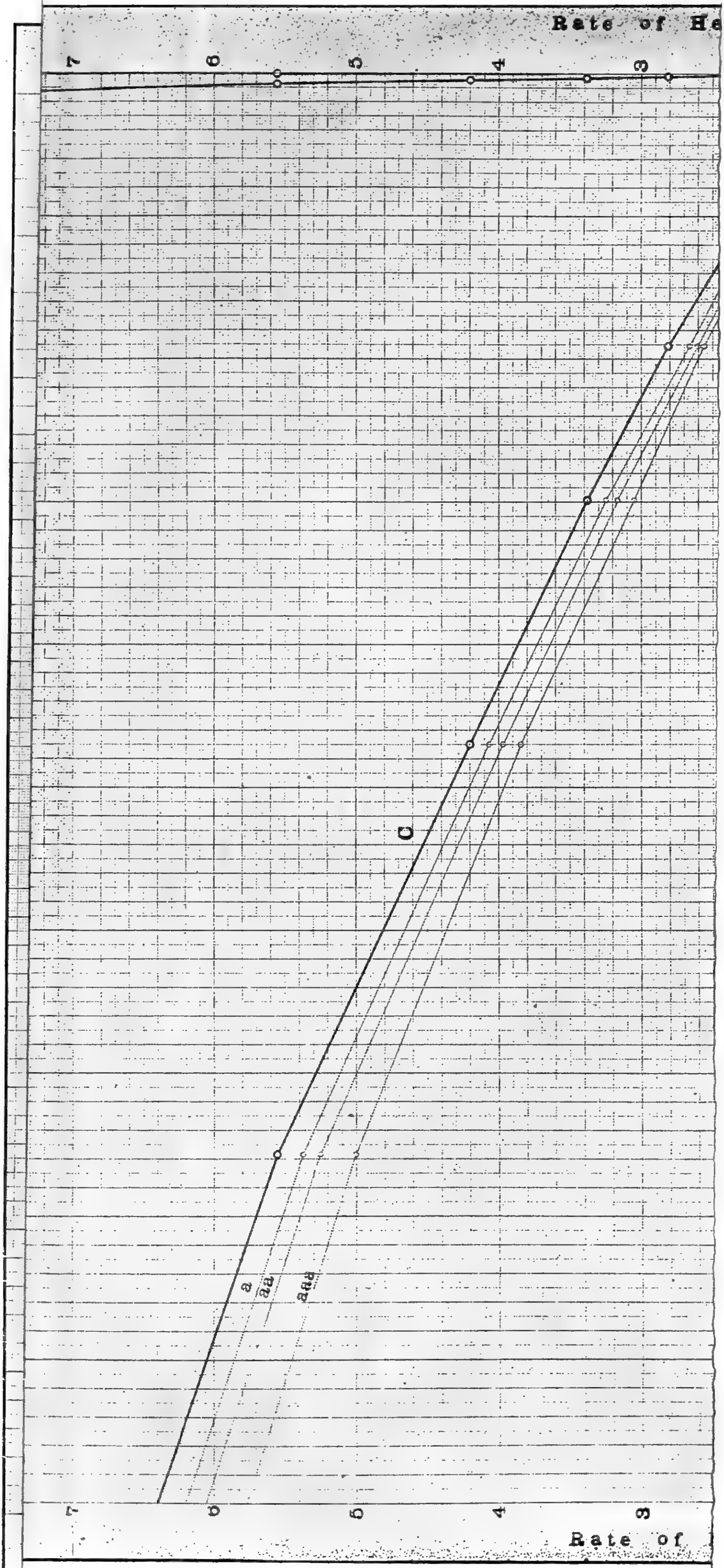


Fig. 5. Hydrogen. Large Bulb.


Rate of He
7
6
5
4
3
C
a
aa
aaa
7
6
5
4
3
Rate of

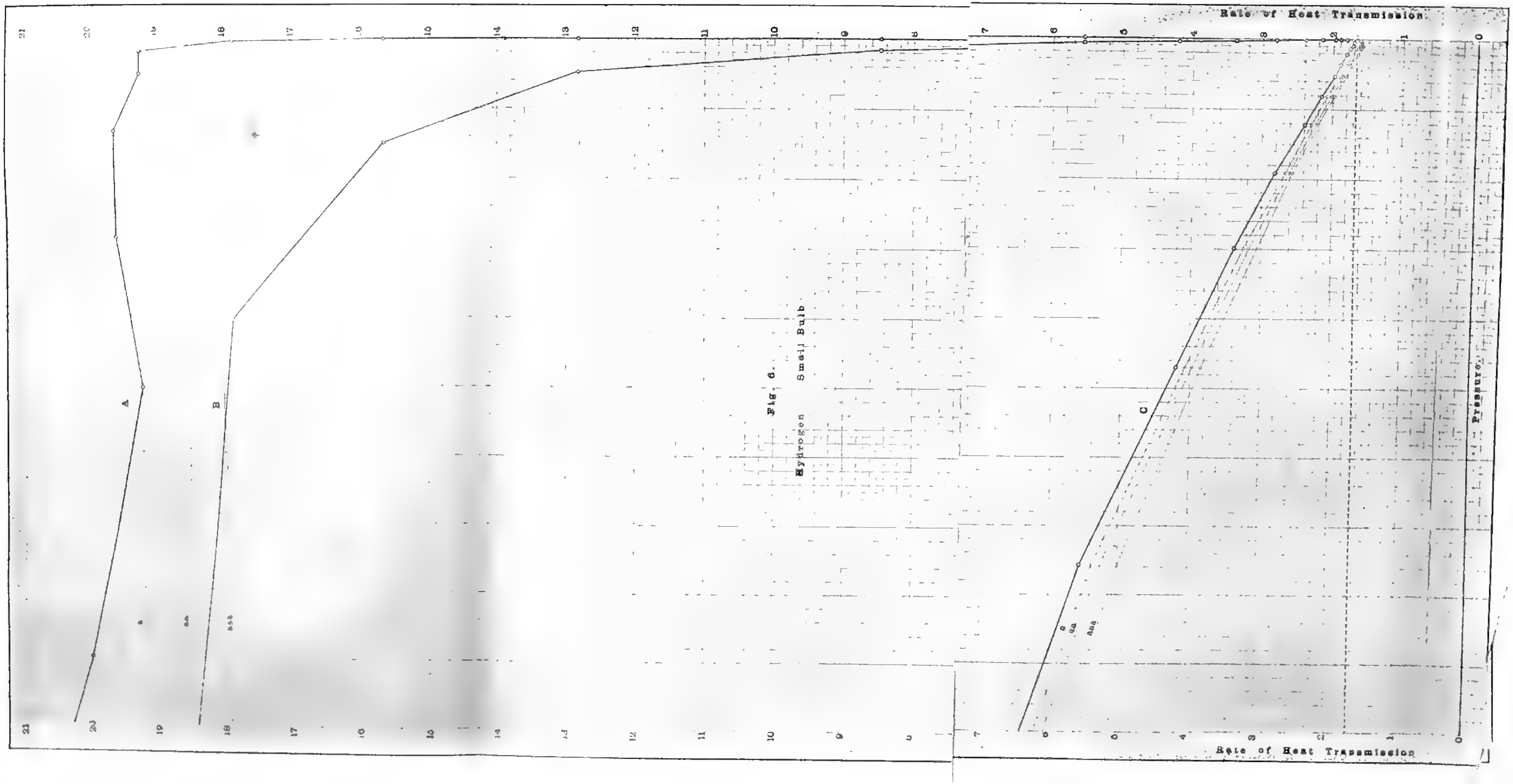
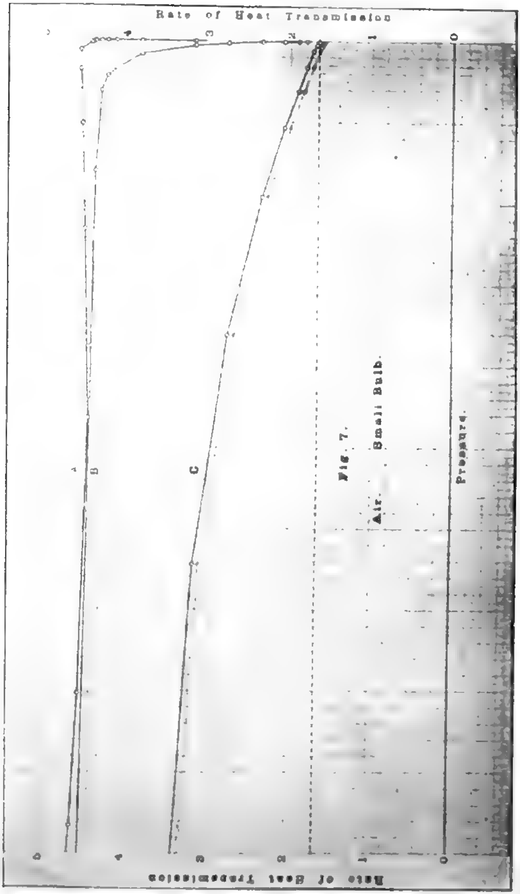


Fig. 6. Hydrogen Small Bulb


Rate of Heat Transmission
5
4
3
2
1
0
B
C
Fig. 7.
Air. Small Bulb.
Pressure.
Rate of Heat Transmission

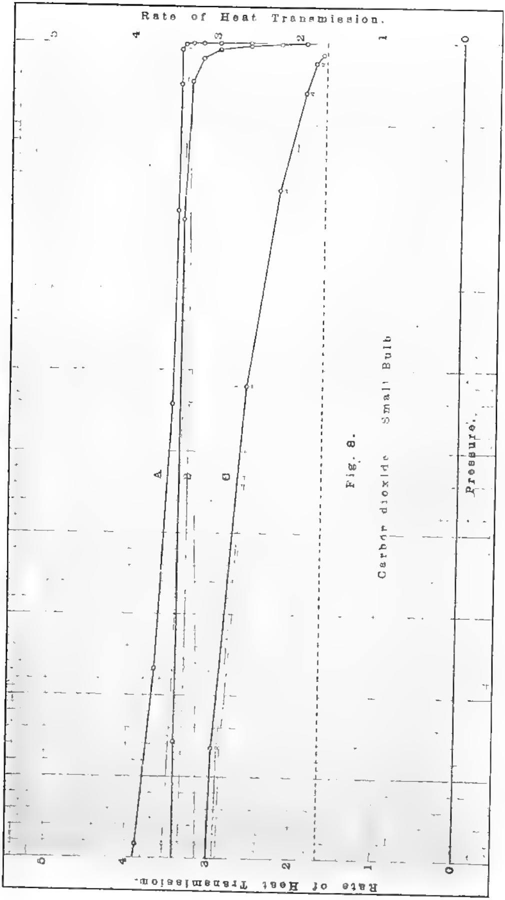


Fig. 8.

Carbon dioxide Small Bulb

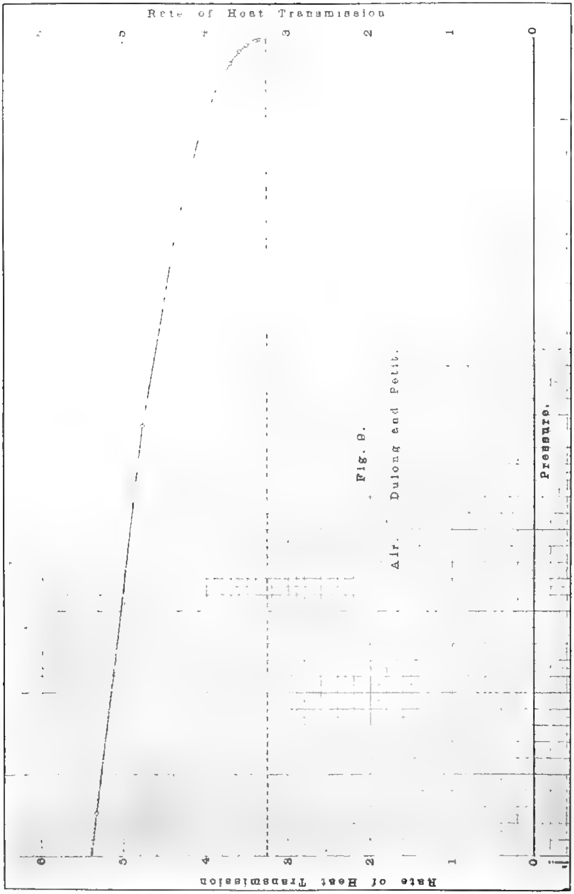


Fig. 8.

Air. Dulong and Petit.

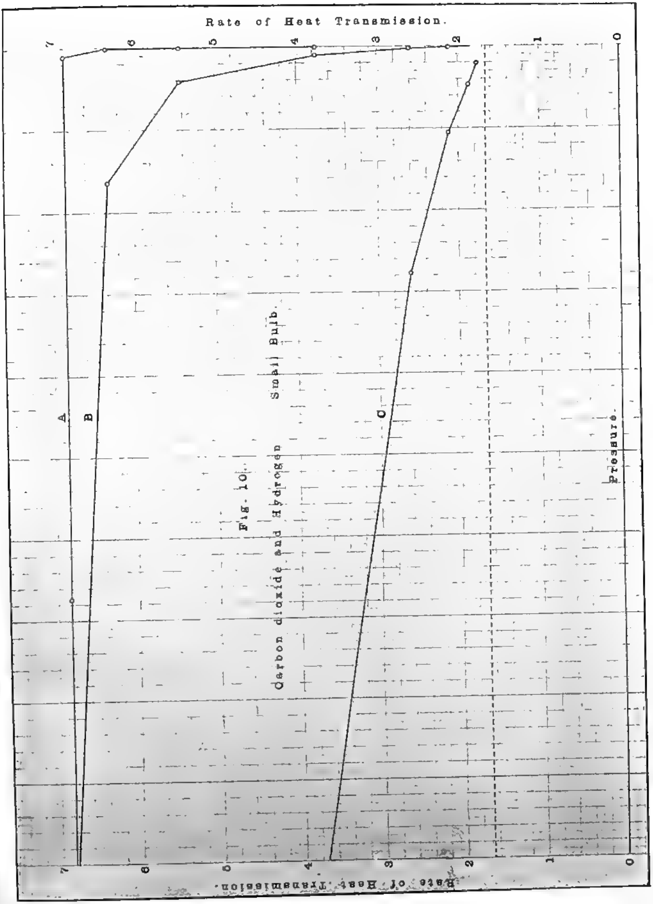
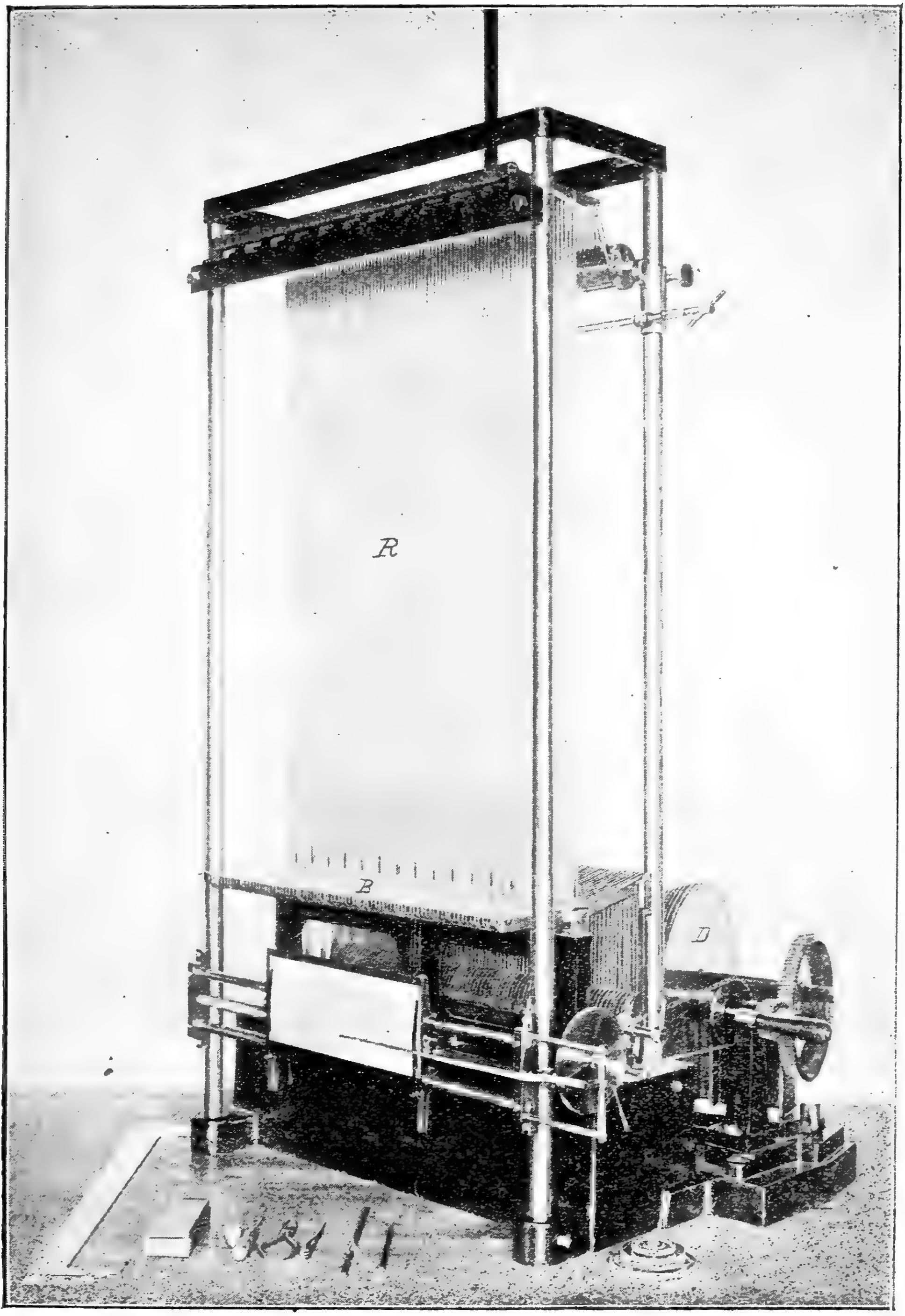
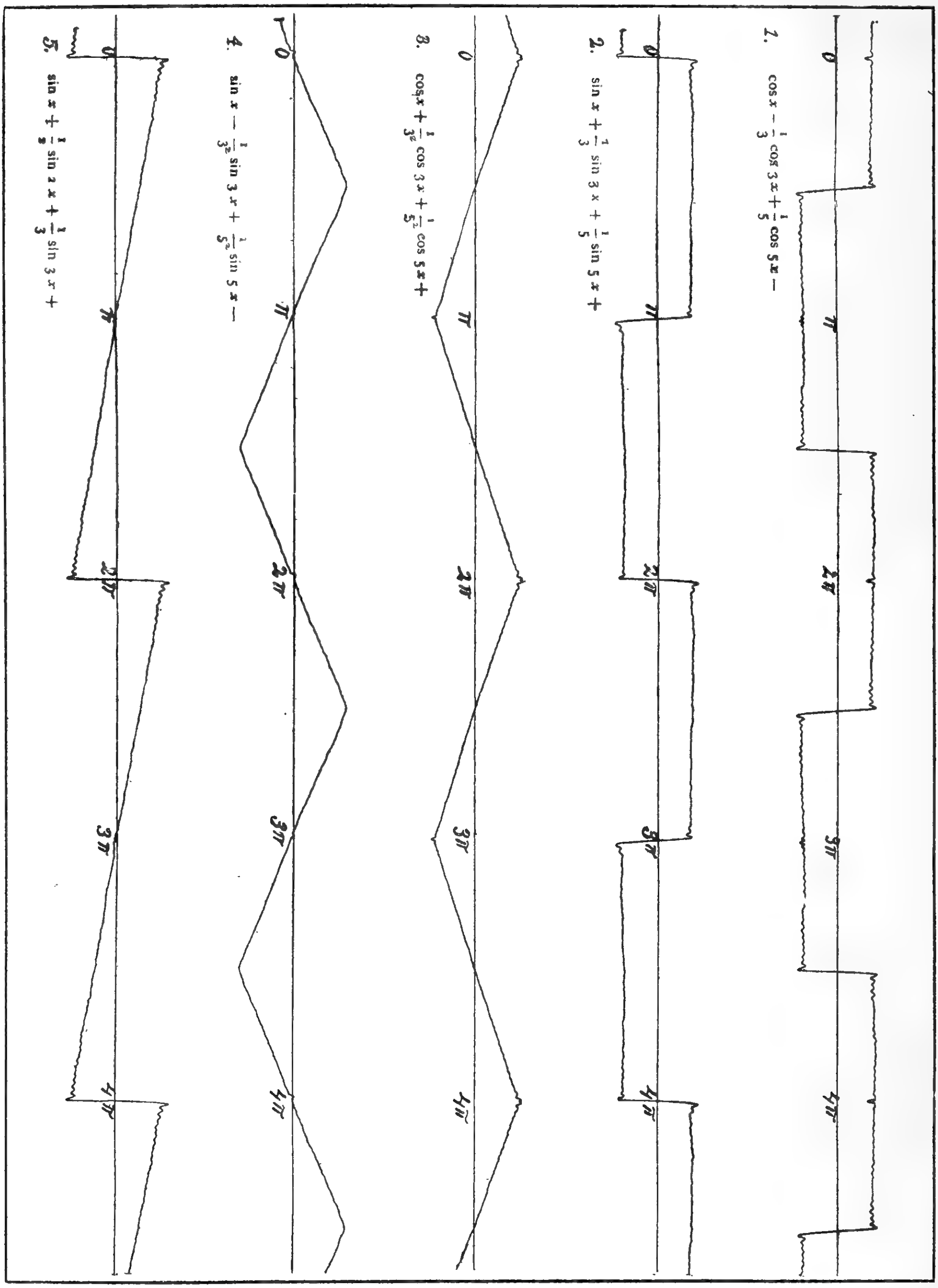


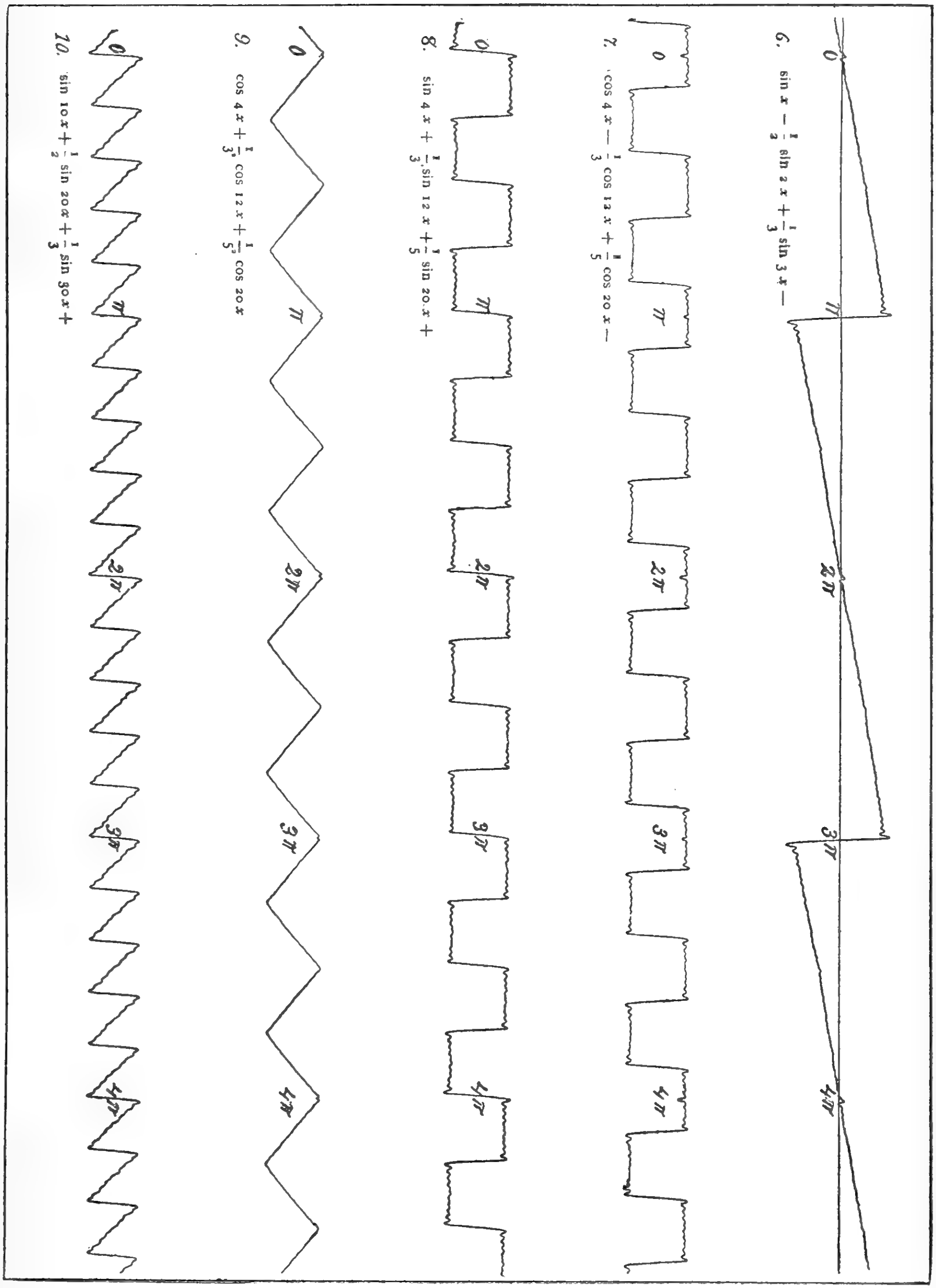
Fig. 10.

Carbon dioxide and Hydrogen. Small Bulb.


R
B
D

Phil. Mag. S. 5. Vol. 45. Pl. XII.

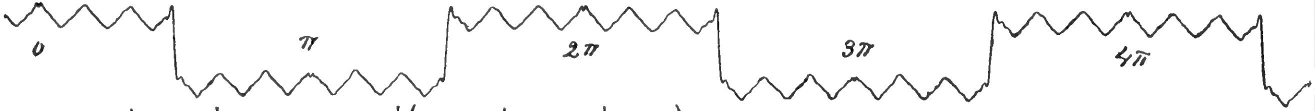



6. $\sin x - \frac{1}{2}\sin 2x + \frac{1}{3}\sin 3x -$
0
π
2π
3π
4π
7. $\cos 4x - \frac{1}{3}\cos 12x + \frac{1}{5}\cos 20x -$
0
π
2π
3π
4π
8. $\sin 4x + \frac{1}{3}\sin 12x + \frac{1}{5}\sin 20x +$
0
π
2π
3π
4π
9. $\cos 4x + \frac{1}{3^2}\cos 12x + \frac{1}{5^2}\cos 20x$
0
π
2π
3π
4π
10. $\sin 10x + \frac{1}{2}\sin 20x + \frac{1}{3}\sin 30x +$
0
π
2π
3π
4π

11. $\sum_{0}^{80} \frac{\sin\frac{n\pi}{10}}{\frac{\pi n}{10}} \cos nx$

12. $\sum \frac{1}{n^2 - 8^2} \cos n x$ (odd terms)

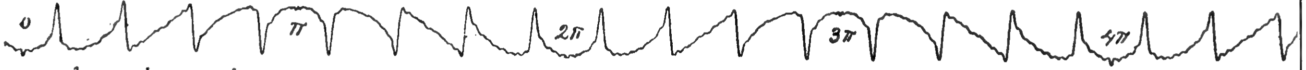
13 $\cos 4x + \frac{1}{3^2} \cos 12x + \frac{1}{5^2} \cos 20x + \qquad + \frac{1}{10} 80x$



14. $\cos x - \frac{1}{3} \cos 3x + \frac{1}{5} \cos 5x + \qquad + \frac{1}{4}\left(\cos 12x + \frac{1}{3^2} \cos 36x + \frac{1}{5^2} \cos 60x\right)$

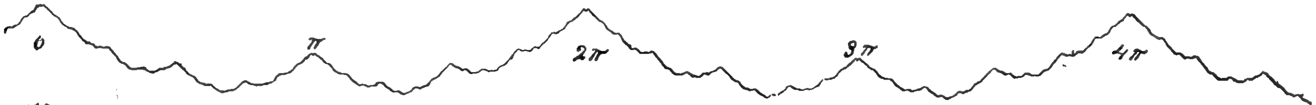
15. $\cos x + \frac{1}{3^2} \cos 3x + \frac{1}{5^2} \cos 5x + \qquad + \frac{1}{4}\left(\cos 9x - \frac{1}{3} \cos 27x + \frac{1}{5} \cos 45x + \right)$

16. $\cos x + \frac{1}{3^2}\cos 3x + \frac{1}{5^2}\cos 5x + \quad + \frac{1}{4}\left(\cos 11x + \frac{1}{3^2}\cos 33x + \frac{1}{5^2}\cos 55x\right)$

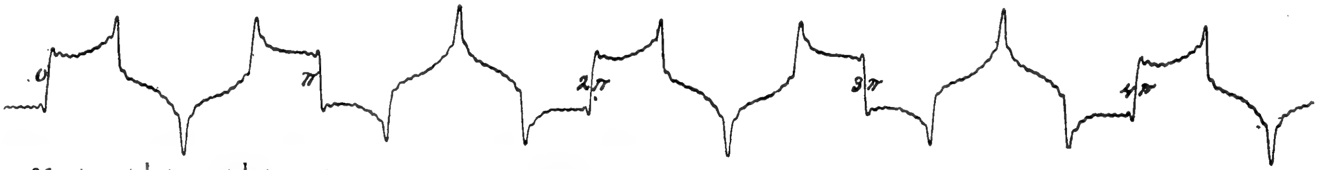


17. $-\frac{3}{7}\cos x - \frac{1}{3}\cos 9x + \frac{1}{5}\cos 17x - \frac{1}{7}\cos 25x +$

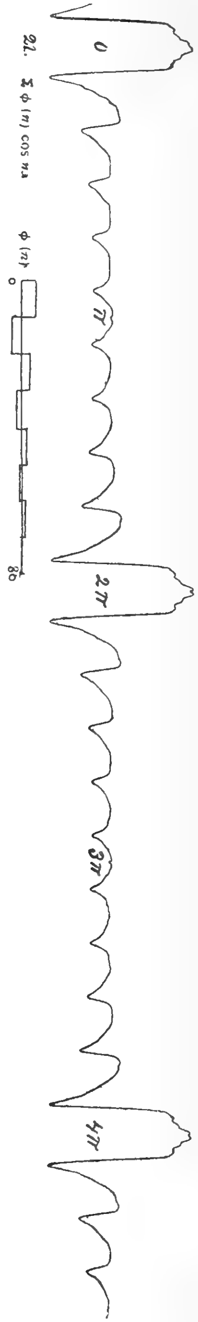
18. $\cos 5x + \frac{1}{3}\cos 12x + \frac{1}{5}\cos 20x + \frac{1}{7}\cos 28x +$



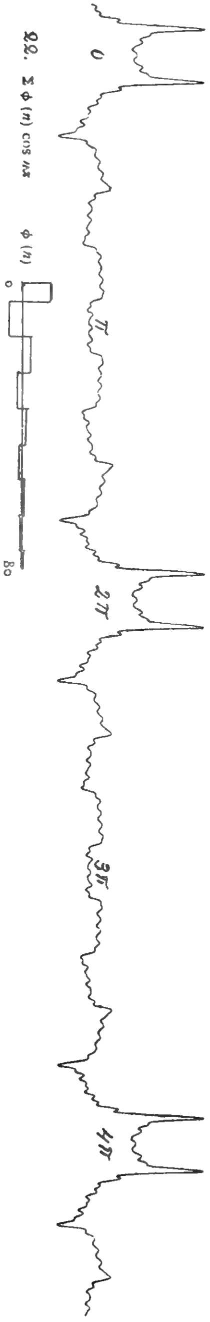
19. $\cos x + .7\cos 2x + .4\cos 4x + .2\cos 8x +$



20. $\sin 3x + \frac{1}{3}\sin 11x + \frac{1}{5}\sin 19x +$

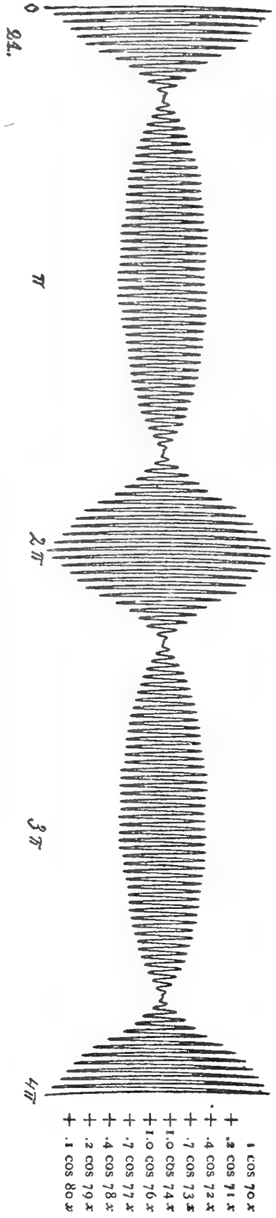


21. $\Sigma\phi(n)\cos nx$

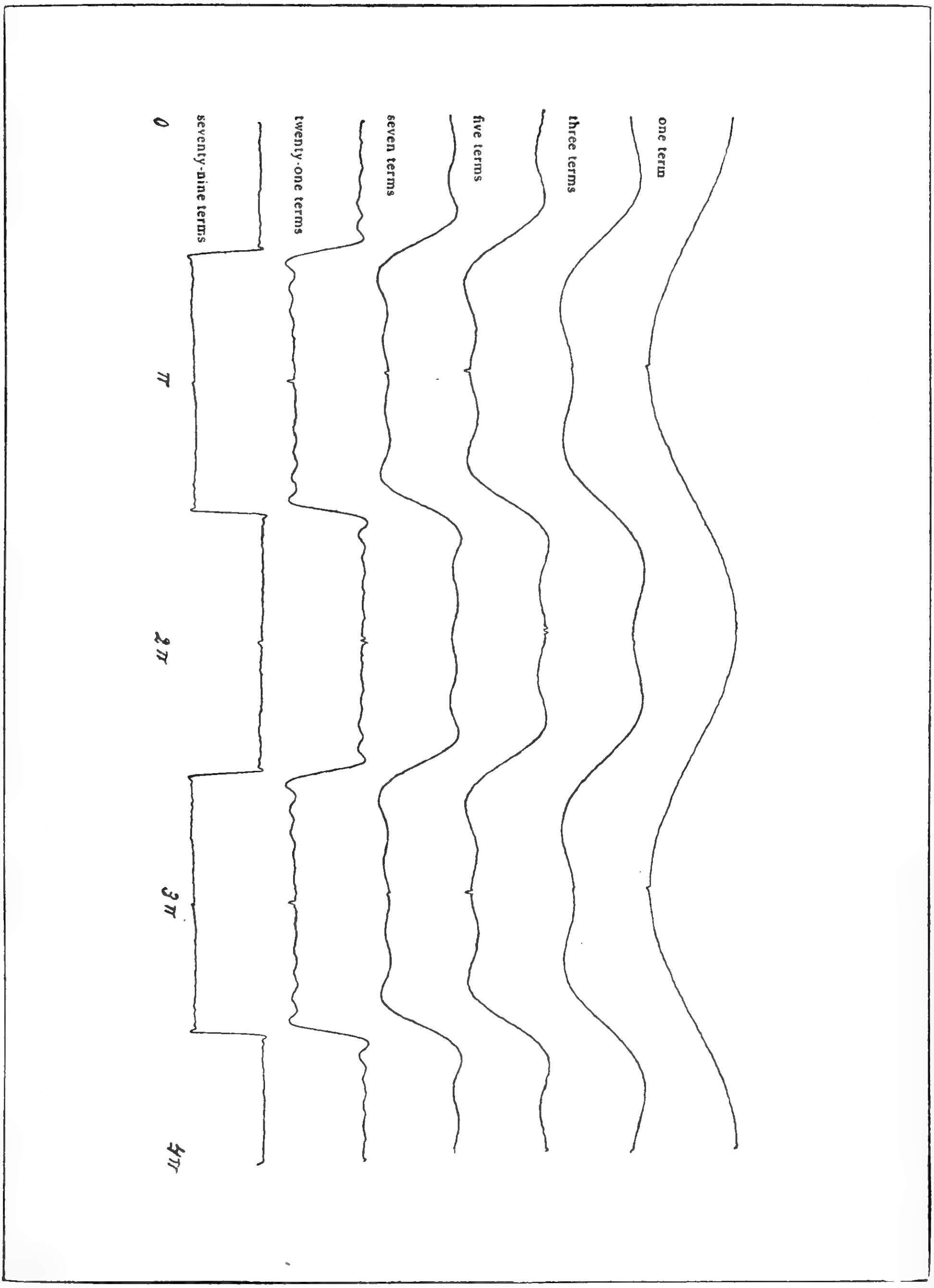


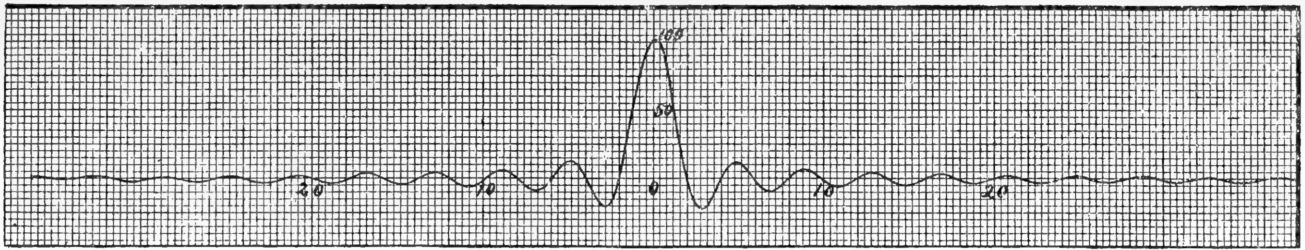
22. $\Sigma\phi(n)\cos nx$

23. $-\cos 40x - \frac{1}{2}\cos 76x$

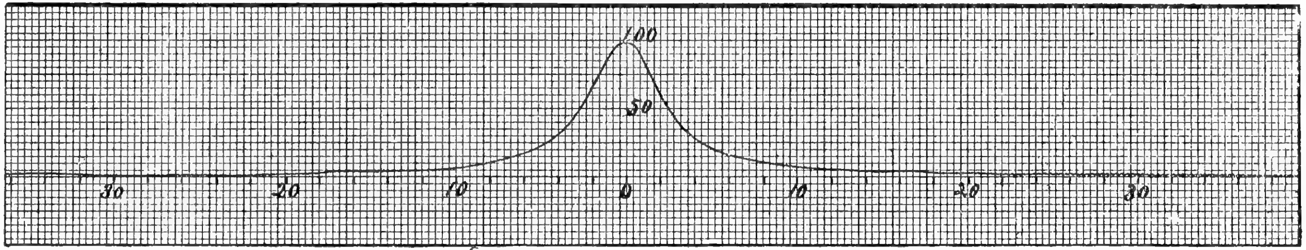


24.

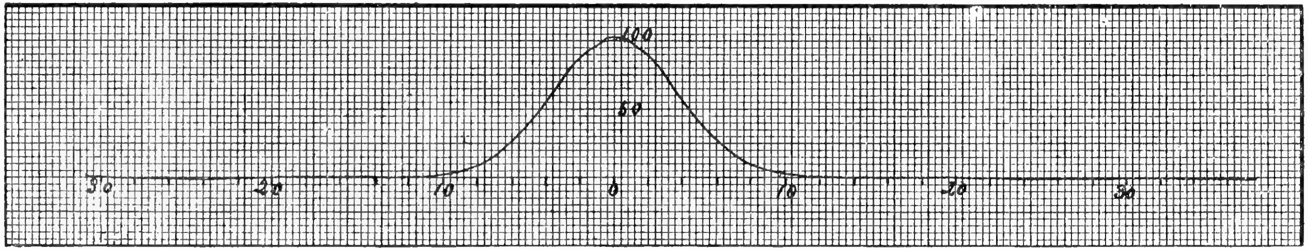

one term
three terms
five terms
seven terms
twenty-one terms
seventy-nine terms
0
π
2π
3π
4π



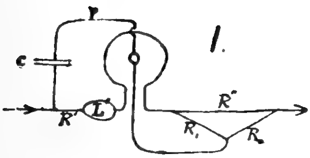
25. $\int \phi(x) \cos kx \, dx$ $\phi(x) = c$ from o to a $a = 40$.

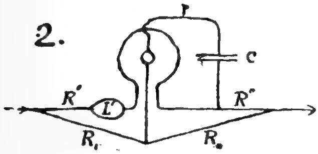


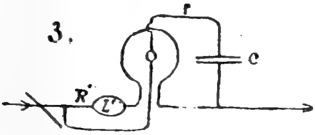
26. $\int \phi(x) \cos kx \, dx$ $\phi(x) = e^{-ax}$ $a = .1$

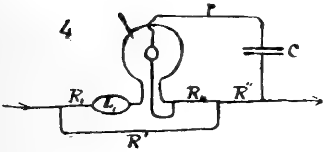


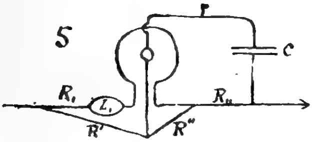
27. $\int \phi(x) \cos kx \, dx$ $\phi(x) = e^{-a^2x^2}$ $a = .1$

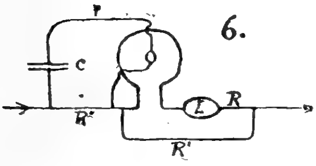

1.

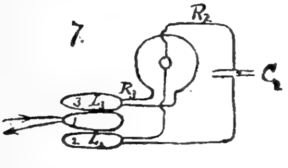

2.

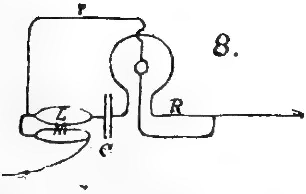

3.

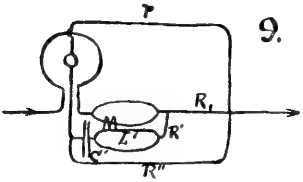

4

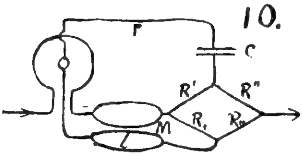

5

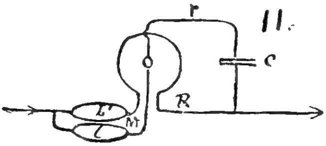

6.

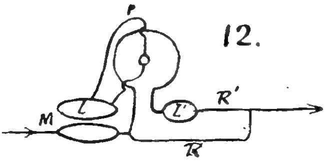

7.

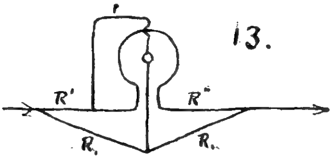

8.

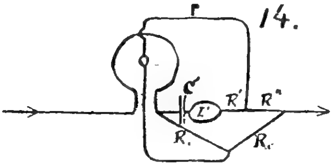

9.

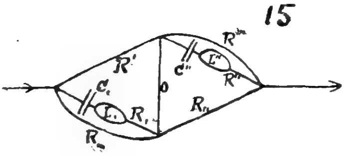

10.

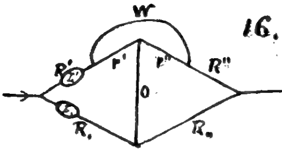

11.

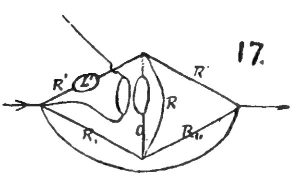

12.

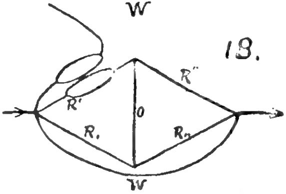

13.

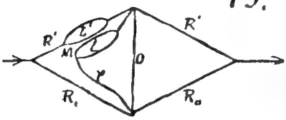

14.

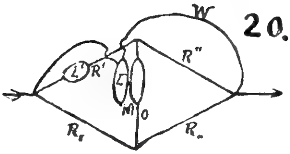

15

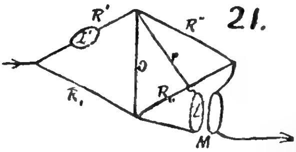

16.

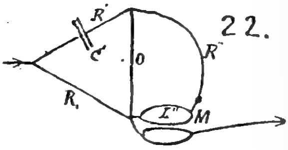

17.

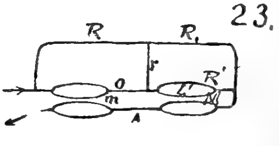

18.

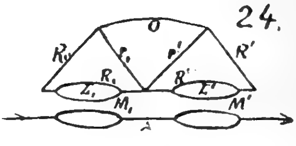

19.


20.


21.


22.


23.


24.

THE

LONDON, EDINBURGH, AND DUBLIN

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[FIFTH SERIES.]

FEBRUARY 1898.

XIII. *Electrical Properties of Newly Prepared Gases.*

By JOHN S. TOWNSEND, *M.A., Cavendish Laboratory, Cambridge**.

1. THE experiments which are described in this paper form a continuation to those which have already been published in the 'Proceedings of the Cambridge Philosophical Society,' vol. ix. pt. v. It was there shown that the gases given off by the electrolysis of sulphuric acid or caustic potash carry with them an electric charge, a large percentage of which remains in the gas after it has been bubbled through a liquid, and passed through glass wool to remove the spray. Another property of these gases is their power of condensing moisture to form a cloud. No such cloud could be observed in newly prepared gases unless they were charged, and, further, the weight of the cloud was found to be proportional to the charge on the gas. These results go to show that the condensation of the moisture is connected with the charge; and the experiments described in Section 16 of the above paper and in Section 19 of this paper prove that when the cloud is formed in a charged gas the electrification resides on the drops forming the cloud. So that we have definite proof of the fact that the drops are formed round the carriers of the electric charge.

2. These results were used to find the charge on each carrier, and it was found to coincide with what we can

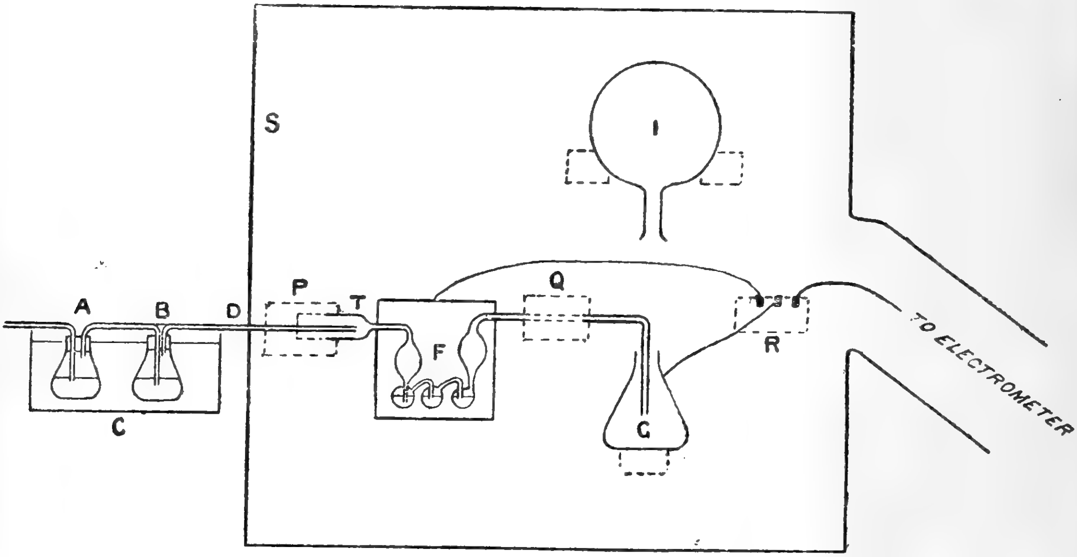
---

* Communicated by Prof. J. J. Thomson, F.R.S.

calculate as being the atomic charge, on the supposition that at ordinary temperature and pressure there are 10^{20} molecules in each cubic centimetre of a gas. The experiments giving the weight of the cloud corresponding to the charge being of importance were repeated by the following method, which gave the same proportionalities, in the different gases, between the charge per c.c. and weight of cloud per c.c. as were previously obtained.

3. The apparatus which was used is shown in fig. 1. The positively-charged oxygen and hydrogen given off from a

Fig. 1.



dilute sulphuric-acid cell were first bubbled through a solution of potassium iodide in the small flask A, and then through distilled water in B. Both A and B were immersed in a trough of water C, so that they should remain at a fixed temperature while an experiment was being conducted. The charged gas thus formed a cloud and carried it along the delivery-tube D, which led into the paraffin block P inside a large metallic screen S. The block P was arranged as shown in the figure, in order that the moisture should not break down the insulation by settling on the paraffin. By this means the moist gas entered the larger tube T connected to three sulphuric-acid bulbs, F, set up between the two paraffin tunnels P and Q. After the gas had bubbled through the acid it was not only cleared of the moisture which under ordinary conditions it would have carried from B, but the cloud was also completely removed. The increase of weight in the bulbs thus arises from two causes; and when the weight of the moisture necessary to saturate the volume of the gas which passes through F is subtracted from the total, the weight of the cloud is obtained. The gas on leaving F entered one of the insulated inductors—the smaller one G being used for oxygen, and the larger one I for hydrogen. The inductor and the bulbs were both covered with tinfoil and connected to mercury cups in the paraffin block R, so that either of them could be readily connected to the insulated quadrants of the electrometer.

The experiments were conducted in the following manner:—The sulphuric-acid bulbs were carefully weighed, and then connected by indiarubber tubing to the glass tubes, which were imbedded in the paraffin blocks, as shown in the figure. The current through the cell was switched on for a few minutes before joining the bulbs into the series, so as to drive the air out of A and B. As soon as the gas began to bubble through the acid in F, a stop-watch was started. By connecting the conductors F and G alternately to the quadrants of the electrometer, the rate at which each was acquiring a charge was found. The sum of the charges acquired by F and G denotes the total charge passing out of B, and that acquired by F divided by the total is the discharging-power of the bulbs. After the stream of gas from the cell had been running through the apparatus for a sufficient time, the bulbs were removed and dry air was drawn through them before weighing them a second time. In each experiment a current of 14 amperes was used, which was kept constant by having an ammeter and an adjustable resistance in the circuit.

In order to reduce the readings on the electrometer-scale to absolute units, it was found that when F was connected to the quadrants, and the other conductors to earth, each scale-division represented ·0040 electrostatic unit of quantity, and when G was connected to the quadrants the scale-divisions each represented ·0036 electrostatic unit. The inductor I, which was used with hydrogen, had a larger capacity than either of the other two inductors, and when it was connected to the quadrants it required ·0042 electrostatic unit to make the spot of light on the electrometer-scale move through a division.

The following tables (p. 128) give the results of the experiments which were made with the different gases. θ is the temperature, in degrees centigrade, of the water in B; W the increase in the weight of the sulphuric-acid bulbs; and n_1 and n_2 the numbers of divisions that the spot of light on the electrometer-scale moves per minute when F and G

TABLE I. +Oxygen from H_2SO_4 electrolyte.

θ.	n_1.	n_2.	W.	w.	$\frac{W-w}{v}$.	ρ.	$\frac{W-w}{\rho v}$.
1	22	31	·0270	·0050	$22·3\times10^{-6}$	$4·04\times10^{-3}$	$5·5\times10^{-3}$
1	13	20	·0187	·0050	$14·1\times10^{-6}$	$2·58\times10^{-3}$	$5·4\times10^{-3}$
13	21	30	·0330	·0118	$20·8\times10^{-6}$	$3·8\times10^{-3}$	$5·5\times10^{-3}$
14	9	19	·0240	·0125	$11·5\times10^{-6}$	$2·05\times10^{-3}$	$5·6\times10^{-3}$

TABLE II. +Hydrogen from H_2SO_4 electrolyte.

θ.	n_1.	n_2.	W.	w.	$\frac{W-w}{v}$.	ρ.	$\frac{W-w}{\rho v}$.
0	21·5	28·5	·0264	·0092	$8·8\times10^{-6}$	$2·1\times10^{-3}$	$4·2\times10^{-3}$
0	27	34	·0296	·0092	$10·5\times10^{-6}$	$2·57\times10^{-3}$	$4·1\times10^{-3}$
15	32	37	·0486	·0264	$10·8\times10^{-6}$	$2·8\times10^{-3}$	$3·8\times10^{-3}$
15·3	26	31	·0445	·0271	$8·5\times10^{-6}$	$2·2\times10^{-3}$	$3·9\times10^{-3}$

TABLE III. −Oxygen from KOH electrolyte.

θ.	n_1.	n_2	W.	w.	$\frac{W-w}{v}$.	ρ.	$\frac{W-w}{\rho v}$.
0	15	26	·0188	·0035	21×10^{-6}	$3·1\times10^{-3}$	$6·8\times10^{-3}$
0	22	32	·0217	·0035	25×10^{-6}	$4·1\times10^{-3}$	$6·1\times10^{-3}$
12	14·5	24·5	·0230	·0081	20×10^{-6}	$2·86\times10^{-3}$	$7·0\times10^{-3}$
11·5	12·5	22·5	·0206	·0078	17×10^{-6}	$2·51\times10^{-3}$	$6·8\times10^{-3}$

[or 1] are connected to the insulated quadrants ; w is the calculated weight of the moisture necessary to saturate the volume of gas v at temperature θ which passes through F in the course of the experiment, $\frac{W-w}{v}$ the weight of the cloud per c.c., and ρ the charge per c.c. on the gas. The volume v is easily calculated, as the gas is evolved by a known current.

Table I. gives the results obtained with positive oxygen. In each experiment a current of 14 amperes was used, and the stream of the evolved gas passed through F for 20 minutes.

Table II. gives similar results for positively-charged hydrogen.

Table III. refers to negatively-charged oxygen from a caustic potash cell, the stream of gas being passed through F for 15 minutes in each experiment.

The experiments were performed with widely different densities of electrification ρ, in order to find how the weight of the cloud varied with the electrification. The ratio of the weight of the cloud to the electrification, $\frac{W-w}{\rho v}$, is given in the last column for each experiment. The variations in ρ were obtained by varying the temperature of the cell from which the gas was given off. Both in the case of a sulphuric-acid electrolyte and a caustic-potash electrolyte the charge on the gas which is evolved by a given current increases as the temperature of the electrolyte is raised.

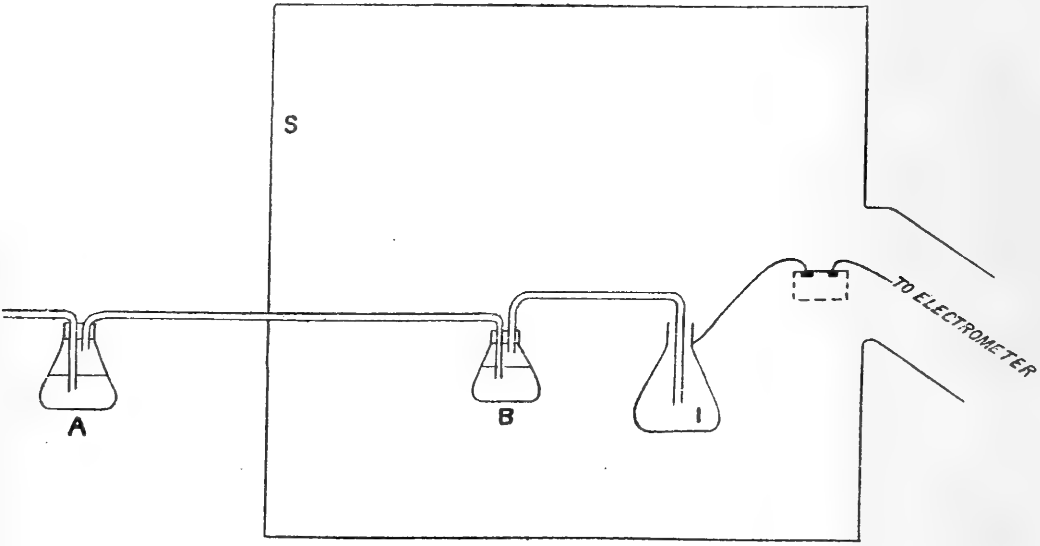
4. The numbers given in the tables show that in each case the weight of the cloud is proportional to the charge, and that this proportionality does not vary with the temperature of the water from which the cloud is formed, at least within the limits of 0° and 14° C. The ratio is not so exact in the case of the negative oxygen as it is for the other gases. The only chemical difference that was detected was that the former was alkaline and the latter slightly acid in its reaction. Since the vapour rising from hot caustic potash does not change red litmus to blue, the alkalinity of the gas must be due to small particles of caustic potash which are carried with the gas from the electrolyte and give it an alkaline reaction even after bubbling through sulphuric acid; this could easily be detected at all temperatures of the cell by letting the oxygen or hydrogen pass along a tube with red litmus.

The oxygen given off by the electrolysis of caustic potash has no appreciable charge till the temperature of the cell is up to about 20° C., and the cloud begins to appear at the same time as the charge on the gas; so that the presence of the alkaline spray can have only a very small effect, as no cloud is observed in the oxygen coming from the cell at a temperature 10° C., although the presence in the spray of caustic potash can be easily detected.

5. The following simple experiments show how intimately connected the charge on the gas is with the formation of the cloud :—

The apparatus used is shown in fig. 2. The oxygen given off, by a current of 10 amperes, from a caustic-potash cell of specific gravity 1·3 was bubbled through a solution of potassium iodide in the flask A, to remove any traces of ozone. Before entering the inductor I, the gas bubbled through water in B. The inductor I was insulated and connected to the quadrants of the electrometer so that the charge on the gas that entered it could be read off on the electrometer-scale. The gas was not passed through any glass wool before entering I, so that as much spray as possible should be carried from the cell.

Fig. 2.



The cell was at a low temperature (11°) to start with, and a current of 10 amperes was switched on, which gradually warmed the electrolyte. During the first nine minutes that the current was on and the gas entering the inductor, no electrification whatsoever was detected, and no cloud could be observed over the water in B. During these nine minutes the temperature of the cell rose from 11° to 18°.

During the next five minutes a deflexion, nine divisions negative, was obtained, and the temperature of the cell had risen to 21°·5.

At a temperature between 22° and 23° a thin cloud began to appear over the water in B, and the spot of light was then moving at the rate of four divisions per minute, so that before the cloud becomes distinctly visible a small electrification of about 2×10^{-4} electrostatic units per c.c. is necessary. When the current is continued and the cell becomes hotter, the electrification on the gas increases and the cloud becomes denser.

6. In order to show that the cloud disappears when the charge on the gas is removed, the temperature of the cell was raised to 48°, and, with the same apparatus as was used for the last experiment, the three following results were obtained:—

(*a*) When the gas passes from the cell to the inductor without filtering through glass wool, a dense cloud was observed over the water in B, and a deflexion of twenty-nine divisions negative per minute was obtained as the gas entered I.

(*b*) The tube connecting A and B was removed, and a tube containing glass wool was substituted. With the same current through the cell a slight cloud was seen over the water in B, and the deflexion was reduced to six divisions per minute. The electrification of the gas was thus reduced to one-fifth of its original value by passing through fifteen centimetres of glass wool. The amount of spray carried through this length of wool must have been extremely small, but the cloud was distinctly visible.

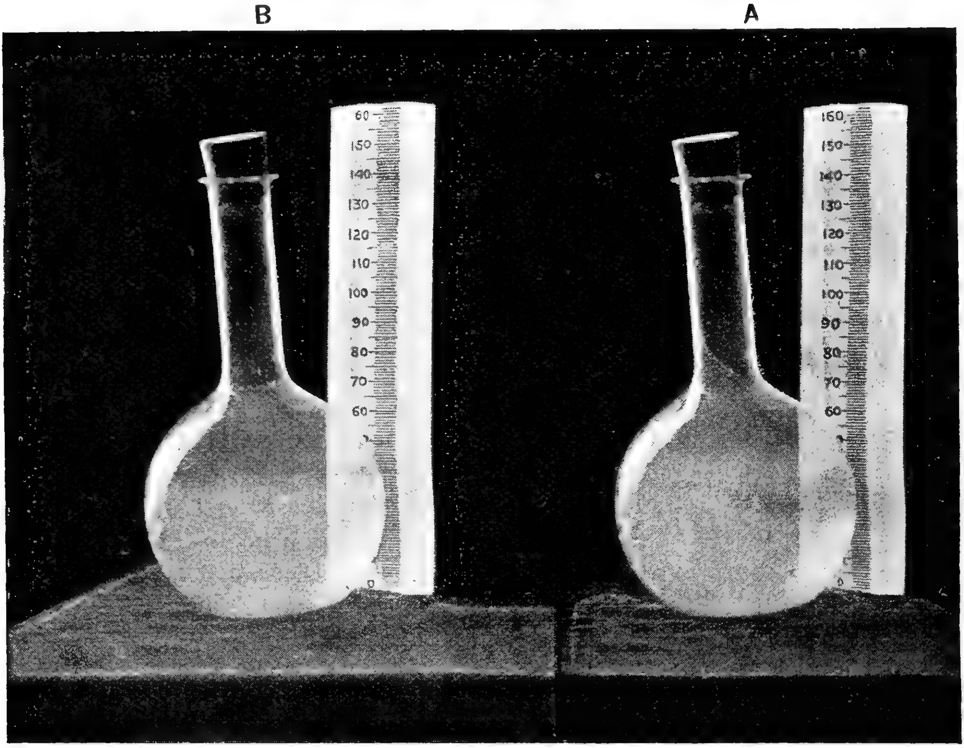
(*c*) The tube containing the glass wool was heated with a Bunsen burner, and its discharging-power was considerably increased, as the spot of light then gave only two divisions per minute, and no cloud was observed over the water in B.

It is thus evident that the formation of the cloud and the presence of the charge are phenomena which accompany one another.

7. The clouds which are formed are slightly different in appearance, and for equal electrifications those formed in oxygen are whiter than those formed in hydrogen. A difference is also to be noticed between the positive and negative oxygen clouds, that formed in the latter being the whiter. This would point to the fact that the drops formed in the negative oxygen are larger than those in the positive, and that those formed in either positive or negative oxygen are larger than those in the hydrogen. A fairly approximate value for the radius of the drop may be obtained by observing the rate

at which the cloud falls in a vessel. The velocity of the drop through the gas was obtained by taking two photographs of the cloud, allowing some minutes to elapse between the two exposures. Fig. 3 represents two such photographs taken of a cloud formed by bubbling the charged oxygen from a sulphuric-acid electrolyte through water. Three minutes were allowed to elapse between the two exposures, and the scale shows that in that time the cloud had fallen between nine and ten millimetres. Similar experiments were made with the other gases, but in the case of the hydrogen the outline of the cloud never became so distinct as it did in the oxygen.

Fig. 3.



The sizes of the drops were obtained from the formula $6\Pi\mu a V = \frac{4}{3}\pi a^3 g$ (Lamb, 'Motion of Fluids,' p. 229).

This gives for the radius of the drop in the positive oxygen $6{\cdot}8 \times 10^{-5}$, the rate of fall being ten millimetres in three minutes, and the radius of the drop in negative oxygen $7{\cdot}9 \times 10^{-5}$, the rate of fall being eighteen millimetres in four minutes.

We thus know the weight of each drop, and dividing this into the weight of the corresponding cloud, we obtain the number of drops per cubic centimetre.

When the number of drops is divided into the charge per cubic centimetre the charge on each is obtained.

The charge on the positive carrier was thus found to be $2{\cdot}4 \times 10^{-10}$, and that on the negative carrier to be $2{\cdot}9 \times 10^{-10}$. When we take into account all the experimental errors, these two charges may be considered equal and approximately 3×10^{-10}. The charge on the hydrogen was found to be between one-third and two-thirds of this value, but could not be arrived at very accurately owing to the difficulty of finding the size of the drop. For present purposes this charge will be taken as $\frac{3}{2} \times 10^{-10}$.

Velocity of the Carrier when acted on by an Electric Force.

8. It has been shown that the radius of the drop forming the cloud is in the case of negatively charged oxygen 8×10^{-5}, so that its weight is 2×10^{-12} grammes, and when acted on by gravity it moves at the rate of eighteen millimetres in four minutes. The force acting on the drop is practically 2×10^{-9}, so that it would require an electric force of $\frac{2 \times 10^{-9}}{3 \times 10^{-10}}$ absolute units to produce an equal velocity, or 2000 volts per centimetre. As we are only dealing with small electromotive forces it is evident that we may disregard the effect of mutual repulsion of the carriers when the gas is carrying a cloud. When the gas is bubbled through sulphuric acid the radius of the carrier is so much reduced that the effect of the mutual repulsion of the particles carrying the charge is easily detected.

9. The following is a general method of investigating the motion of a gas in a vessel of any shape, the initial distribution being uniform. Let ρ be the density of electrification in any part of the gas, u, v, and w the velocities of the carriers along the axes of x, y, and z.

The equation of continuity is $\frac{1}{\rho}\frac{\delta\rho}{\delta t} + \frac{du}{dx} + \frac{dv}{dy} + \frac{dw}{dz} = 0$, the notation being the same as that used in Lamb, 'Motion of Fluids.' Let ϕ be the electric potential; $-\frac{d\phi}{dx}$, $-\frac{d\phi}{dy}$, and $-\frac{d\phi}{dz}$ are the forces which act on the charged carriers, and their velocities u, v, and w are given by the equations

$$ku = -e\frac{d\phi}{dx}; \quad kv = -e\frac{d\phi}{dy}; \quad kw = -e\frac{d\phi}{dz}, \quad . \quad . \quad (1)$$

where e is the charge on the carrier and k is a constant to be determined experimentally. Substituting these values for u, v, and w in the equation of continuity we obtain

$$\frac{k}{\rho}\frac{\delta\rho}{\delta t}-e\nabla^2\phi=0, \quad \text{but } \nabla^2\phi=-4\pi\rho,$$

therefore

$$\frac{k}{\rho^2}\frac{\delta\rho}{\delta t}=-4\pi e.$$

Integrating we obtain

$$\rho=\frac{\rho_0}{1+\frac{4\pi e\rho_0 t}{k}}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

where ρ_0 is the initial density, which is uniform throughout the space considered.

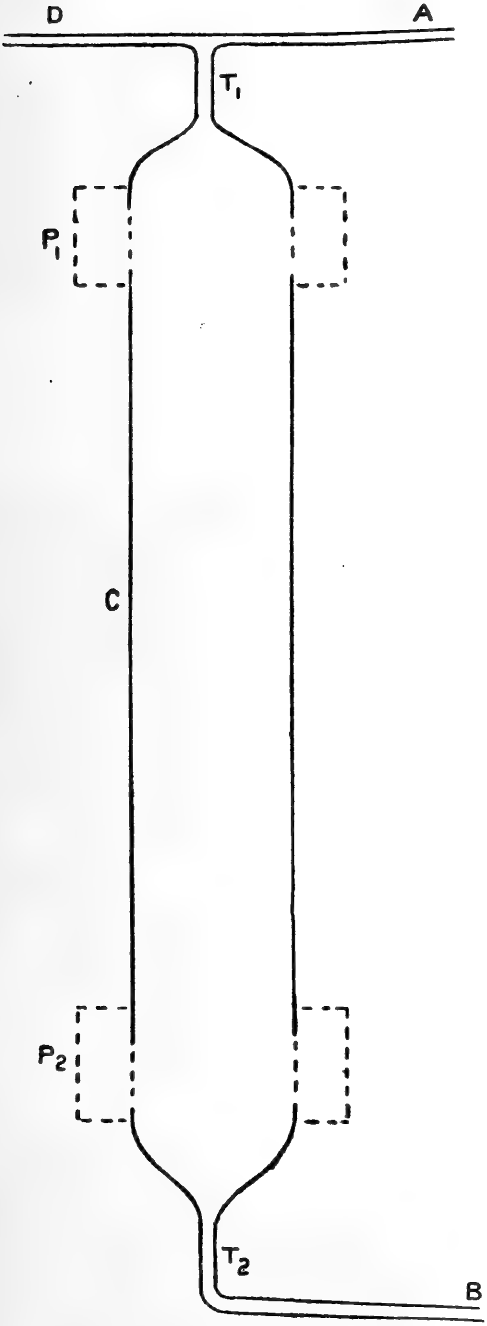
Equation (2) shows that the motion takes place in such a way that the density ρ is a function of the time only, and does not vary from point to point in the gas; on this account no variation in the pressure of the charged gas takes place, and the terms $\frac{dp}{dx}, \frac{dp}{dy}, \frac{dp}{dz}$, which have in general to be taken account of when dealing with the motion of gases, can be omitted in equations (1) since p does not vary from point to point.

When the gas is in a closed vessel the mutual repulsion of the carriers of the electricity drives them to the sides, where they either remain so close to the walls that they cannot be blown out, or else get discharged against the sides. The charged gas remaining in the vessel has a uniform density given by equation (2), and if the vessel which contains it is an insulated conductor connected to a pair of quadrants of an electrometer the deflexion on the electrometer-scale will be proportional to ρ when the charged gas is blown out of the vessel.

10. In order to investigate experimentally the rate at which the gas loses its charge to the sides of an inductor, or in other words to find how the density ρ will vary with the time, a metal cylinder C (fig. 4), 30·2 centimetres long and 1·6 centimetres in diameter, was used. The two ends of the cylinder fitted into paraffin blocks, P_1 and P_2, into which were also fixed the glass tubes T_1 and T_2. The tube T_1, through which the cylinder was filled, had several layers of fine copper gauze across the broad end, which tended to distribute the

gas evenly across the section. The whole apparatus was contained inside a screen, the ends A and B of the tubes leading to the cylinder projecting outside the screen; by this means the charged gas could be blown out of the cylinder without stirring any apparatus or tubes inside the screen; so that the deflexion on the electrometer scale, when C is connected to the insulated quadrants, was solely due to the removal of the charge inside C. Any effect of gravity, on the motion of the carriers towards the sides of the tube, was prevented by placing it in a vertical position.

Fig. 4.



11. The gases that were examined were the oxygen and hydrogen given off by electrolysis of dilute sulphuric acid. The cell was raised to a temperature about 20° above the room, at which it could easily be kept by running the current. By means of an ammeter and variable resistance in the circuit the current could be kept constant. A method is thus provided of filling the cylinder several times with a gas having a constant electrification per c.c. When filling the cylinder with the hydrogen it is best to lead the gas in at the top, and when using oxygen at the bottom, in order that the air may be driven out in as short a time as possible, as it is not desirable to run the current for

more than five minutes at a time, when a number of experiments have to be performed requiring the same electrification in each case.

The gases were first bubbled through potassium iodide and then through strong sulphuric acid, so that they should enter the tube D perfectly dry. The cylinder and tubing connected with it were thoroughly dried by heating them with a Bunsen-burner and then blowing dry air through them. This precaution is absolutely necessary as it only requires a very slight amount of moisture to form a cloud, and this would impede the motion of the carriers.

12. A series of experiments of the following type were then performed. The cylinder was connected to a pair of quadrants of the electrometer, which were kept to earth while the cylinder was being filled and insulated before blowing out the gas, so as to get the readings on the same part of the scale. The tube A was closed, and D connected to the delivery-tube of the sulphuric-acid wash-bottle, and a current of fourteen amperes sent through the cell. It only required the current to run for about five minutes to completely fill C with the charged gas. The circuit was then broken and the tube B closed. At the same time a stop-watch was started. After the lapse of t minutes the ends A and B are opened, and dry unelectrified air blown through C to expel the charged gas. This caused the spot of light on the electrometer-scale to move n divisions, which is proportional to ρ the density of the electrification after the gas has remained in the cylinder for a time t.

The numbers thus obtained for n and t are given in Tables IV. and V., the first being for charged hydrogen and the second for charged oxygen.

TABLE IV.

n.	t.
32	0
25·5	1
20·5	2
17·5	3
15·5	4
14	5

TABLE V.

n.	t.
49	0
37·5	2
29·5	4
24·5	6
22	8

It will be seen that these numbers are nearly exactly the same as those given by the formula $n=\frac{n_0}{1+\theta t}$ if we put $n_0=32$ and $\theta=\cdot 0043$ in the case of hydrogen, and $n_0=49$ and $\theta=\cdot 00255$ in the case of oxygen. The numbers calculated from this formula are given in the two following tables:—

TABLE VI.

n.	*t.*
32	0
25·4	1
21·1	2
18	3
15·7	4
14	5

TABLE VII.

n.	*t.*
49	0
37·5	2
30·3	4
25·5	6
22	8

this shows that $\rho=\frac{\rho_0}{1+\theta t}$. Hence we have from equation (2) (p. 134) $\theta=\frac{4\pi\rho_0 e}{k}$.

13. The velocity of the charged carrier when acted on by an electric force can be found from the above numbers. The electrometer having been standardized it was found that ·0037 electrostatic unit of quantity would give a deflexion of one division on the scale, so that we get

$$\rho_0=\frac{32\times\cdot 0037}{\pi\times\cdot 8^2\times 30\cdot 2}=2\,.\,10^{-3}$$

in the hydrogen experiments, and

$$\rho_0=\frac{49\times\cdot 0037}{\pi\times\cdot 8^2\times 30\cdot 2}=3\,.\,10^{-3}$$

in the oxygen experiments. Let k_H and e_H denote the values of k and e for hydrogen, and k_o and e_o similar values for oxygen, and we get

$$\frac{4\pi\rho_0 e}{k_H}=\cdot 0043, \text{ where } \rho_0=2\,.\,10^{-3},$$

and
$$\frac{4\pi\rho_0 e}{k_{\text{O}}} = \cdot 00255, \text{ where } \rho_0 = 3\,.\,10^{-3}.$$

Hence
$$\frac{k_{\text{H}}}{e_{\text{H}}} = 5\cdot 6, \quad \text{and} \quad \frac{k_{\text{O}}}{e_{\text{O}}} = 15. \quad . \quad . \quad . \quad . \quad (3)$$

From the equation $k\,\text{V} = \text{F}e$, which is similar to the equations (1) (§ 9), V being the velocity due to an electromotive force F, we obtain for the motion of the charged carrier in hydrogen $\text{V} = \frac{\text{F}}{5\cdot 6}$, and for the motion of the charged carrier in oxygen $\text{V} = \frac{\text{F}}{15}$. Hence under an electromotive force of one volt per centimetre, the hydrogen carrier travels at the rate of $\frac{1}{300 \times 5\cdot 6}$ centimetres per second, and the oxygen carrier at the rate of $\frac{1}{300} \times \frac{1}{15}$ centimetres per second. These results do not require a knowledge of e.

The conclusions arrived at from the above investigations are based on the assumption that in each case we are dealing with a gas containing carriers all charged with the same sign, either positive or negative. Experiments on conductivity have been carried out in order to test this point, and it has been found that we are in reality dealing with mixtures. Thus in the case of oxygen, or hydrogen, from a sulphuric-acid cell having a positive charge equal to $3\,e$, it is possible to obtain from the gas a positive charge of $4\,e$ and a negative charge of e. If we suppose the positive and negative to act independently the charge on the oxygen carrier would be $5\,.\,10^{-10}$ instead of $3\,.\,10^{-10}$, also the velocities under a volt per centimetre would be smaller in the ratio of $3:4$ than those given above.

We can arrive at an approximate value for the size of the carrier if we assume that the viscosity of a gas affects the motion of a small sphere and a large one according to the same law $6\pi\mu a\,\text{V} = \text{P}$ (Lamb, *loc. cit.*) ; substituting

$$\text{V} = \frac{1}{300 \times 5\cdot 6}, \quad \mu = 10^{-4}$$

for hydrogen and

$$\text{P} = \frac{1}{300} \times \frac{3}{2}\,10^{-10} = \frac{1}{2}\,10^{-12},$$

we get for the radius, a, of the hydrogen carrier $4\cdot 5\,.\,10^{-7}$. Similar substitutions give the radius of the oxygen carrier

$12 \cdot 10^{-7}$. Hence the carriers are large compared with molecular dimensions.

The velocities obtained for the carriers in the conductivity due to Röntgen rays are far greater than the above. Thus for oxygen and hydrogen Mr. Rutherford obtains velocities of 5·2 and 1·4 centimetres per second under a volt per centimetre (Phil. Mag. Nov. 1897).

If we assume that the charges on the carriers are of the same order as those obtained in Section 7, we see that the dimensions of the carriers are smaller than those with which we are here dealing.

The Discharging Power of Tubes.

14. It has been shown that if a gas with a uniform charge ρ_0 per c. c. be left in a tube for a time t, the density of the electrification falls to a uniform value ρ, given by the equation $\rho = \frac{\rho_0}{1 + a\rho_0 t}$ where a is a constant. Now, if we suppose that the velocity of a gas as it passes along a tube be perfectly uniform, and that no discharging takes place due to accidental circumstances, the values of t, ρ, and ρ_0 would be connected by the same equation, where now t denotes the time that any portion of the gas takes to traverse the tube, ρ_0 and ρ being the densities of electrification on entering and escaping from the tube. When the gas is given off by a current through an electrolyte, the volume q which enters the tube per second is known when the current is known; so that $t = \frac{V}{q}$ where V is the volume of the tube, and the above equation becomes $\frac{\rho_0 - \rho}{\rho} = \frac{a\rho_0 V}{q}$. The values of ρ_0 and ρ are easily found and satisfy this equation roughly, so that a can be determined; its value, however, is greater than $\frac{4\pi e}{k}$ found already. This is due to accidental discharging, as it can be shown that, even when the carrier is so large that the small E.M.F. driving it to the side of the tube could have no appreciable effect, still a measurable discharge takes place (see Section 21).

15. If the gas be run into an insulated inductor after traversing the tube, $\frac{\rho_0 - \rho}{\rho}$ is the ratio of the charge gained per minute by the tube to the charge gained per minute by the inductor. These charges can be easily found by connecting the tube and inductor alternately to the insulated quadrants

of an electrometer. The tube C insulated in the manner described in Section 10 was used in these experiments, the gas examined being the hydrogen given off by a current of fourteen amperes from a dilute sulphuric-acid electrolyte. The following deflexions n_1 and n_2 were obtained, n_1 being the deflexion per minute when the tube is connected to the insulated quadrants, and n_2 the deflexion per minute when the inductor is connected to the insulated quadrants:—

n_1.	n_2.
16	47
9·5	36

The relative value of n_1 and n_2 in absolute units of quantity was 12 : 13·5, so that $\frac{\rho_0-\rho}{\rho}=\frac{12n_1}{13{\cdot}5n_2}$.

We thus obtain the following numbers showing the connexion between $\frac{\rho_0-\rho}{\rho}$ and ρ_0 :—

$\frac{\rho_0-\rho}{\rho}$.	ρ_0.	$\frac{\rho_0-\rho}{\rho\rho_0}$.
·30	2×10^{-3}	150
·23	$1{\cdot}46\times10^{-3}$	157

so that $\frac{\rho_0-\rho}{\rho}$ is (within 5 per cent.) proportional to the densities of the electrification.

16. Similar values were obtained for $\rho_0-\rho$ and ρ for the oxygen given off from the same electrolyte by a current of fourteen amperes. A different method was used in this case, which did not require an inductor. When the tube C (experiment, Section 10) was being filled with a charged gas, which enters with an electrification ρ_0 and escapes with an electrification ρ, the quantity $\rho_0-\rho$ can be obtained by observing the rate (n_1 per minute) at which the spot of light moves on the electrometer-scale, when the tube is full of the charged gas and the stream kept running through it. The mean density of electrification in the tube is practically

$\frac{\rho_0+\rho}{\rho}$, and was obtained by blowing out the charged gas and observing the deflexion n_2, which is proportional to $\frac{\rho_0-\rho}{2}\times V$, where V is the volume of the tube.

The following values of n_1 and n_2 were obtained :—

n_1.	n_2.
14·5	62
19·5	49

The rate q at which the gas is supplied to the tube is ·85 cubic centimetres per second ; so that $(\rho_0-\rho)\times 51$ is the charge given to the tube per minute as the gas passes through it and is proportional to n_1, and $\frac{\rho_0+\rho}{2}\times V$ is proportional to n_2, where V, the volume of the tube, is 60·4 cubic centimetres.

Thus we get the following values for $\frac{\rho_0-\rho}{\rho}$ and ρ_0 :—

$\frac{\rho_0-\rho}{\rho}$.	ρ_0.	$\frac{\rho_0-\rho}{\rho\rho_0}$.
·32	$4{\cdot}3\times10^{-3}$	74
·25	$3{\cdot}3\times10^{-3}$	76

17. The ratio $\frac{\rho_0-\rho}{\rho}$ was also found to increase when the current through the electrolyte diminished, thus showing that the ratio depended upon the rate at which the gas is supplied to the tube. Another tube of equal length and 1·12 centimetres in diameter was examined by similar methods, and it was found to have a far less discharging-power than the larger tube, so that, when the supply q and density ρ_0 are given, the smaller the bore of the tubing that is used to convey the gas from one vessel to another, the less will be the loss of the charge. The above results showing the dependence of $\frac{\rho_0-\rho}{\rho}$ on ρ_0 are important, as we see from the

formula $\frac{\rho_0-\rho}{\rho}=\frac{4\pi e}{k}\times\frac{\rho_0 V}{q}$ that the increase in the charge due to heating the cell arises from an increase in the number of carriers with the same charge, and not from an increase of the charge on the carriers already present; for, if the increase in density were due to an increase in e, we should have found $\frac{\rho_0-\rho}{\rho}$ to be proportional to the square of ρ_0.

18. It would be almost impossible to arrange to pass the gas along a tube in such a way that the discharging-power would be as small as the theoretical value, calculated from the numbers given for k (Section 13). Small differences in temperature between the gas and the sides of the tube, and other effects which would give rise to currents in the gas, considerably increase the discharging-power, but here a comparatively small effect on the loss of charge when the gas is allowed to stand in the tube. The velocity of the carrier towards the sides of the tube, due to an electromotive force, will on this account be too large when calculated from the formula $\frac{\rho_0-\rho}{\rho}=\frac{4\pi e,}{k}\times\frac{\rho_0 V}{q}$.

Thus, when we substitute the values given in Sections 15 16 for $\frac{\rho_0-\rho}{\rho}$ and ρ_0, we obtain for k the values: $k_H=3e_H$, and $k_O=12\,e_O$.

Causes which Influence the Discharging Power.

19. In the above experiments the gases were always treated in the same way before entering the tube C, being bubbled first through a solution of potassium iodide, and then through strong sulphuric acid. When the gases are bubbled through water instead of sulphuric acid, they will enter C carrying with them a cloud, the effect of gravity upon which makes itself very apparent by comparing the charges acquired per minute by the tube when in the vertical and horizontal positions. Thus in the case of a stream of highly charged hydrogen, which bubbled through water before entering C, it was found that the charge acquired was fourteen scale-divisions per minute as the gas passed through C in the vertical position; and forty-seven divisions per minute with C in the horizontal position. The electric capacity of the conductor, consisting of C and the quadrants, was not altered by more than three per cent. by turning the tube round, so that the large increase in the discharging-power must have

been caused nearly entirely by the force of gravity acting on the drops forming the cloud. The experiment shows that the charge resides on the drops forming the cloud.

A similar result was obtained in the case of charged oxygen carrying a cloud; the corresponding numbers of divisions of the scale being 4 and 20, showed that five times the charge is removed by the tube when it is turned from the vertical to the horizontal position.

Rough values for the size of the drop might be deduced from these numbers; but the variations in density per c. c. (due to the cloud) are so great, that it would be impossible to arrive at satisfactory values, especially in the case of hydrogen, where the weight of the cloud can be as great as $\frac{1}{7}$th of the weight of the gas in the same volume.

20. Even when the gas has been bubbled through sulphuric acid before entering C, a slight effect of this kind is still to be found. Thus, when the tube is turned from a vertical to a horizontal position, the charge acquired by it per minute increases from 16 to 19 divisions of the electrometer-scale per minute. If the coefficient of viscosity can, even roughly, be applied as in Section 13 to determine the size of the carrier, the effect of gravity could not exert on a carrier a force that would cause it to move through the gas surrounding it with a velocity of the same order as $\frac{1}{300}$ centimetre per second. In fact, if we take the density of the carrier to be unity, its radius should be as big as $3 . 10^{-5}$ in order that the force of gravity should make it move with a velocity $\frac{1}{500}$ centimetre per second, in hydrogen; whereas we have found that its radius is of the order $5 . 10^{-7}$ (Section 13).

But if, instead of considering a single carrier, we take the case of a finite quantity of gas (occupying unit volume say) as differing from the surrounding gas by ·05 per cent. in density, which could arise from various causes, a vertical motion far greater than the above would ensue, which would affect the discharging-power of a horizontal tube.

The gases can be sent through a tube containing phosphorus pentoxide, after bubbling through sulphuric acid, so as to remove all traces of moisture, and if the powder is placed loosely in a horizontal tube about 8 centimetres long, with glass wool at the ends to prevent dust being carried along, not more than half the charge on the gas will be lost. The hydrogen when dried in this manner loses more of its charge, in passing along the vertical tube C, than if it had been dried by sulphuric acid alone; but the loss of charge was not found to be increased by turning the tube from the vertical to the horizontal position.

21. The discharging-power of a tube in a vertical position is, as we should expect, greatly diminished by having the gas moist, as the electric force is then too small to have any appreciable effect in causing the drops to move towards the sides. Thus, in an experiment similar to that explained in Section 15, the hydrogen, instead of being bubbled through sulphuric acid before entering C, was bubbled through water, and it was found that when C acquired a charge of fourteen per minute, the inductor acquired seventy-five per minute; whereas if a similarly charged dry gas had been used, C would have acquired a charge at the rate of twenty-eight per minute, and the inductor at the rate of sixty-two.

The same property holds in the case of charged oxygen. Thus when the gas carries a cloud, only four divisions per minute were acquired by C, whereas a similarly charged dry gas would have given seven divisions per minute. The loss of charge of the clouded gas in passing through a vertical tube is due to the unevenness in the motion and the accidental contact of the cloud with the sides.

Diffusion.

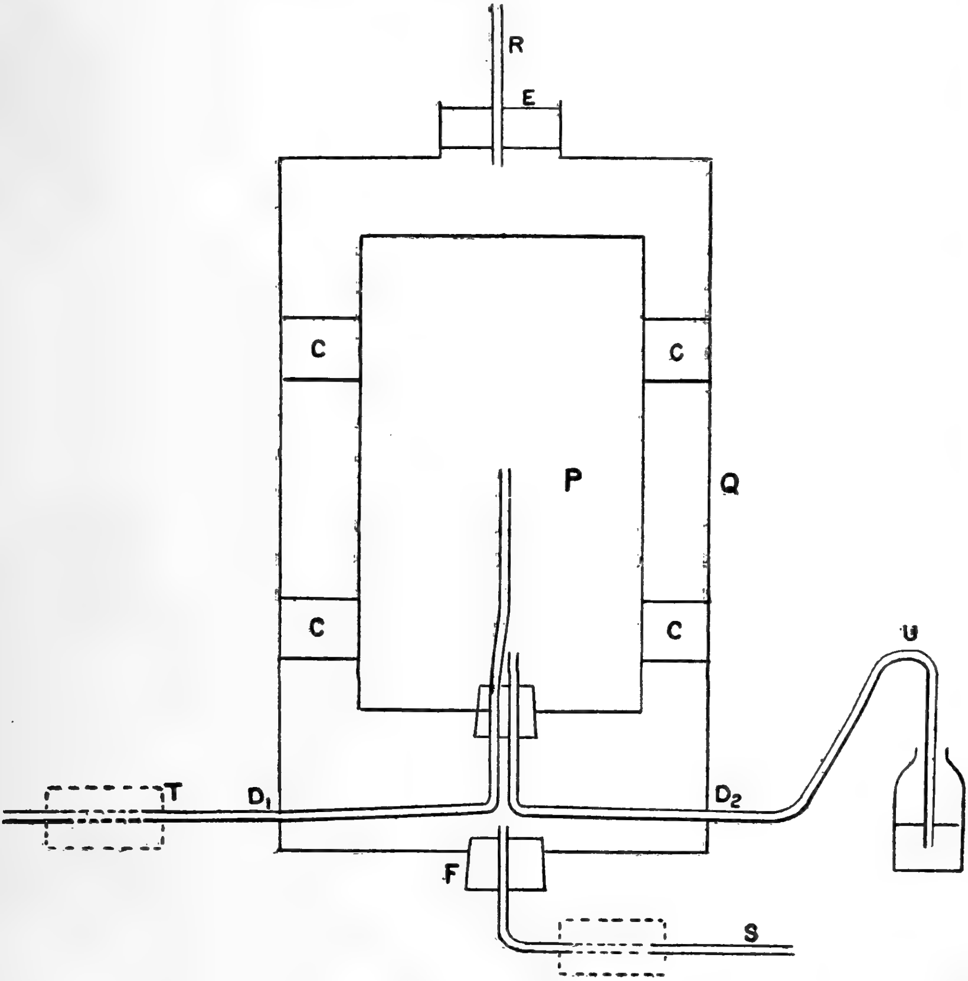
22. Experiments were performed on the diffusion of charged gases in order to find out whether any of the charge can pass through porous earthenware. The apparatus which was used in these experiments is shown in fig. 5. It consists of two cylinders, the outer Q was of tin, and the inner P was a porous porcelain vessel fixed by means of corks (C) to the tin cylinder. The porous vessel had one aperture which was fitted with an indiarubber stopper carrying two tubes projecting through the sides of Q at D_1 and D_2. The tube T was broken, and fitted with a paraffin tunnel for purposes of insulation, and the tube U dipped into a sulphuric-acid bottle, and so acted as a manometer. The outer cylinder had two apertures at E and F; the tube R from E led into a large insulated inductor; and the tube from F was connected through a paraffin tunnel to the delivery-tube of a sulphuric-acid bottle.

Before performing an experiment it is necessary to thoroughly dry the whole apparatus; this was done by blowing dry air through T and F. The charged hydrogen from a sulphuric-acid electrolyte was dried and allowed to enter the cylinder P through T.

The bellows are connected to the tube S, and a gentle current of air is blown into the cylinder through F. The cylinder Q is connected to earth, and the inductor into which

R leads is connected to the insulated quadrants of the electrometer. By this means the hydrogen which diffuses through P is carried with the air-current along the tube R into the

Fig. 5.



inductor. The sulphuric acid in U during the course of the experiment rose in the tube, showing that the hydrogen diffused as quickly as the tube T supplied it.

Only a very small fraction of the original charge was found to be on the gas after diffusion, as the deflexion per minute, as the diffused gas enters I, was found to be less than 2 per cent. of the deflexion obtained by allowing the gas to enter I directly.

23. Having seen that the hydrogen in diffusing carries with it no appreciable charge, it is interesting to know whether the electricity is carried to the sides and gets

caught in the pores of the porcelain, or whether the carriers remain distributed throughout their original volume surrounded by the air which diffuses in to take the place of the hydrogen.

In order to test this the whole apparatus was insulated and experiments were conducted as follows:—

The outside cylinder Q was connected to the insulated quadrants of the electrometer and the hydrogen from the cell, after bubbling through strong sulphuric acid, was admitted into P by means of the tube T. The draught of air between P and Q was maintained in order to make the diffusion as rapid as possible; no inductor was used, so that the hydrogen after diffusion was carried into the atmosphere of the room through F. As the charged gas entered P, the conducting cylinder Q being connected to the quadrants, the deflexion on the electrometer-scale registered the total charge that entered P.

When a sufficient charge had entered P, the tube T was closed and the sulphuric acid in the manometer U rose rapidly and showed a maximum difference of pressure between the atmosphere inside and outside P of about three centimetres of acid. As the blowing into E was continued the sulphuric acid in U began to fall gradually, and in a few minutes the difference of pressure was reduced to one or two millimetres, which showed that practically all the hydrogen had diffused out.

The question was then decided by observing what charge could be blown out of P by blowing through T, and it was found that a large fraction of the original charge could be thus removed from inside P. The charge which is thus blown out does not represent quite all the electricity left in the atmosphere in P after the hydrogen had diffused out, as about ten per cent. got discharged in bubbling through the sulphuric acid before escaping through U.

The numbers obtained in three experiments which were performed in the above manner are given in the following table: n_1 is the charge (in divisions of the scale) put into P;

n_1.	t_1.	h_1.	h_2.	t_2.	n_2.
210	3	3·0	·1	6	130
200	3	3·2	·2	5	125
132	5	3·0	·2	4	98

t_1 the time in minutes required to admit the charge; h_1 the maximum height in centimetres that the sulphuric acid rises in U after the tube T is closed; t_2 the time that elapses while the manometer falls from h_1 to h_2; and n_2 the charge that is blown out of P. The current through the electrolyte was in each case fourteen amperes.

In the last experiment the charge on the gas was reduced by passing it through a tube of phosphorus pentoxide before it entered T.

We thus see that the carriers of the electricity in the charged hydrogen can with ease be transferred to an atmosphere of any other gas by diffusion. Also, by continuing the stream of hydrogen into P, the density of the charge in P increases, so that diffusion not only affords a method of removing a charge from a gas, but also gives us a means of increasing the electrification per unit volume.

Experiments with Hydrochloric Acid.

24. When gases are given off by electrolysis both the quantity and sign of the electricity which they carry are affected by various causes. These changes which the charge undergoes are well illustrated by a series of experiments which were performed with a twenty-per-cent. solution of hydrochloric acid.

The electric state of the gases was examined by leading them into an insulated inductor, connected to one pair of quadrants of an electrometer. The other pair of quadrants, the case of the electrometer, and the screen inside which the inductor was placed, were connected to earth.

The hydrogen was bubbled through a strong solution of caustic potash and the chlorine through water, before entering the inductor.

25. The charge carried by the hydrogen evolved from a hydrochloric-acid electrolyte varies in a very peculiar manner. When new carbon electrodes are used and the current turned on, the electrometer shows that the gas, at first, has a positive charge, but although the current is kept constant, this charge diminishes gradually, and after some minutes becomes negative; it soon reaches its maximum negative value, and the charge then remains constant, except for a small variation due to temperature. When the electrolyte is cooled, and the same current again sent through the acid, the hydrogen begins to come off with a negative charge.

26. The chlorine which has been evolved from the positive

electrode will be seen to have dissolved in the acid not only round the positive electrode, but also round the negative, before the change of sign is observed in the hydrogen. If now this acid be removed from G and the electrodes well washed, and a fresh quantity of acid used, it will be found that the electrometer will indicate charges on the hydrogen exactly similar to those at first observed, except that the positive charges are not so large.

After washing the electrodes a second time, and experimenting again with a fresh quantity of acid, the same effect will be observed without any further diminution in the positive charges.

The number of divisions of the scale obtained in an experiment of this kind are :—

1st minute, 14 divisions positive.
2nd „ 11 „ „
3rd „ 5 „ „

After a few minutes the hydrogen came off with a negative charge, corresponding to five divisions per minute. The current through the electrolyte being ten amperes, and the value of divisions on the electrometer-scale in absolute units of quantity being obtained by multipling them by ·003.

When the chlorine was similarly examined it was found to have a small negative charge, corresponding to four divisions of the electrometer-scale per minute. It did not vary in sign like the charge on the hydrogen.

27. It is probable that the remarkable change of sign that occurs in the charge carried by the hydrogen is due more to the absorption of chlorine gas by the carbon electrode, than to the presence of chlorine gas in the electrolyte; for when platinum electrodes were used the hydrogen was given off with a large positive charge and did not change when the chlorine got dissolved in the electrolyte.

It is improbable that the effect is due merely to the change from platinum to carbon, as there was no corresponding change when similar experiments were performed with sulphuric acid; the positive charge on the hydrogen evolved being large whether platinum or carbon terminals were used; and no change occurred by running the current for half-an-hour, except a gradual increase in the charge, which can be shown to be due to a rise in temperature. It was also found that the charge on the hydrogen was increased by using a new carbon electrode, instead of that used with the hydrochloric acid.

28. In this case of direct electrolysis where no secondary action takes place, or other chemical effects at the electrode considerations of polarization would lead us to expect that gases would come off carrying with them a charge of the same sign as that which they carry in the electrolyte. Thus in the case of polarization due to a layer of hydrogen with a positive charge surrounding the electrode, it is conceivable that, when the evolution of hydrogen takes place, some of the gas composing this layer would come off without losing its charge.

29. In seeking to explain the changes of sign that occur, as in the case of the hydrogen evolved from hydrochloric acid, it must be remembered that, once the gas has acquired a charge, only a small quantity of electricity is lost by bubbling through liquids, so that causes which influence the sign of the charge should most naturally be sought for at the place where the gas is being evolved, and may be due to the state of the electrode or to impurities in the electrolyte in the immediate vicinity of the electrode.

30. There are some well-known phenomena which point to an explanation of the results obtained with hydrochloric acid, namely, the electrification of gases by glowing metals (J. J. Thomson, Phil. Mag. Dec. 1896). The typical effects of this kind are, the positive electrification of oxygen when a metal is being oxidized, and the negative electrification of hydrogen when blown past a hot oxidized metal; the metal in the former case uniting more readily with the negative oxygen atoms, and the oxygen in the latter uniting more readily with the positive hydrogen atoms. The only necessity for the high temperature of the metal is to break up some of the molecules of the gas into atoms, but in the cases we are dealing with the gases at the electrode are already in the atomic state. We should therefore expect somewhat similar effects to take place when the gases which are evolved can form chemical compounds with the electrode, or with bodies either dissolved in it or in its immediate vicinity.

31. If the above principles are applied to the case of hydrochloric acid, we should expect when the gases are evolved by electrolysis, that the hydrogen would carry a positive charge, and the chlorine a negative charge. The subsequent change from positive to negative, in the case of the hydrogen, can be explained by supposing a small quantity of hydrochloric acid to be formed at the negative electrode, due to the action of the nascent hydrogen on the chlorine, and that the positive hydrogen atom is more active in forming a compound with chlorine than the negative. A similar

explanation would apply to the change which occurs from negative to positive in the charge carried by the chlorine when platinum is used instead of carbon for an electrode, since a chloride of platinum is formed.

32. The charged hydrogen given off from a platinum electrode, in the electrolysis of hydrochloric acid, has the same property of forming a cloud when bubbled through water as the highly charged gases obtained by other methods. The chlorine, however, has only a small charge, and, moreover, contains a quantity of hydrochloric-acid vapour which would be difficult to remove without making the charge too small to form a visible cloud. The hydrogen was treated in the following manner, in order to get rid of the acid vapour which would form a cloud when bubbled through water :—The delivery-tube from the cell was connected to the tube T, which dipped into a strong solution of caustic potash contained in the flask H_1, and the gas then passed along a tube containing glass wool soaked in caustic potash before it bubbled through the water in the flask H_2. The hydrogen which was evolved from the platinum electrode, after passing through this apparatus, was led into an insulated inductor connected to the electrometer, and, with ten amperes through the electrolyte, the spot of light on the electrometer-scale was deflected thirty-seven divisions per minute, and with fourteen amperes, sixty-two divisions per minute, the temperature of the cell being 36°. In both cases a dense cloud was observed above the water in H_2.

In order to test the efficiency of the apparatus for removing the acid vapour, and at the same time to show that newly prepared hydrogen does not form a visible cloud unless it has a considerable charge, the gas evolved from the carbon electrode was led into H_2. It was found that the electrometer was giving only three divisions per minute, and no cloud could be seen over the surface of the water in H_2, although the temperature of the cell and current through it were the same as when thirty-seven divisions per minute were obtained by using a platinum electrode.

No weighing experiments were done with this cloud, but from its appearance it could be easily seen to be less dense than that which was obtained by bubbling hydrogen prepared from a sulphuric-acid electrolyte through water, although the electrification was in each case the same.

In addition to the above experiments, the effect of Röntgen rays in discharging the gas has been examined. It is hoped that the results of this investigation, and the experiments on conductivity alluded to in Section 13, will be ready for publication in the near future.

In conclusion, I desire to express my thanks to Professor Thomson for the valuable assistance his suggestions have afforded.

XIV. *On the Calculation of the Conductivity of Aqueous Solutions containing Two Electrolytes with no Common Ion.*

By Prof. J. G. MacGregor *and* Mr. E. H. Archibald, *Dalhousie College, Halifax, N.S.*[*]

ACCORDING to the dissociation theory of electrolysis the specific conductivity of a complex solution, volume v of which contains N_1, N_2, N_3, &c., gramme-equivalents of the electrolytes 1, 2, 3, &c., respectively, is given by the expression

$$\frac{1}{v}\left(\alpha_1 N_1 \mu_{\infty 1} + \alpha_2 N_2 \mu_{\infty 2} + \alpha_3 N_3 \mu_{\infty 3} + \&c.\right),$$

where the α's are the coefficients of ionization of the electrolytes and the μ_∞'s their specific conductivities per gramme-equivalent at infinite dilution. If the quantities involved in this expression can be determined in the case of any solution, its conductivity can be calculated.

Since but little change in the ionic velocities of one electrolyte in a solution can be produced by small quantities of others, the value of μ_∞ for an electrolyte in a complex solution may be taken to be the same as in a simple solution, provided the complex solution be sufficiently dilute. Hence for such solutions, the μ_∞'s of the above expression may be determined by observations of conductivity made on sufficiently extended series of simple solutions of 1, 2, 3, &c.

In a former paper by one of us[†] it has been shown that equations sufficient for the determination of the α's and the N's may be obtained, provided the following assumptions be admissible, viz., (1) that any dissolved electrolyte, which is in dissociational equilibrium, may be regarded as being in this state not only throughout the whole volume of the solution, but also throughout any finite part of it, and (2) that each electrolyte in a complex solution, with its undissociated and its dissociated parts, though disseminated throughout the whole volume, may be regarded as occupying a definite portion of the volume, which may be spoken of as its region. It would follow from these assumptions that the law of equilibrium may be applied either to one such region or to

---

[*] Communicated by the Authors.

[†] MacGregor, Trans. Roy. Soc. Canada [2], ii. sec. iii. (1896), p. 65.

the regions of two or more electrolytes which have common ions.

We restrict ourselves here to solutions containing two electrolytes with no common ion, which therefore will in general contain also two other electrolytes formed from the former by double decomposition. We may refer to the former electrolytes as 1 and 2, and to the latter as 3 and 4. 1 and 2 will have no common ion; nor therefore will 3 and 4.

The application of the law of equilibrium to electrolyte 1 throughout its own region and throughout the regions occupied by it and 3, and by it and 4, gives the equations:—

$$\alpha_1/V_1=\alpha_2/V_2=\alpha_3/V_3=\alpha_4/V_4, \quad . \quad . \quad . \quad . \quad (a)$$

where the α's are the ionization coefficients of the respective electrolytes, and the V's their regional dilutions, *i. e.*, the volumes of their regions divided by their content in gramme-equivalents of electrolyte. These equations are obtained by the same reasoning, *mutatis mutandis*, as that by which Arrhenius* showed that equality in the concentration of ions is the condition which must be fulfilled in order that two dilute simple solutions of electrolytes with a common ion may undergo no change of ionization on being mixed. They need not therefore be deduced here.

Another equation may be obtained by applying the law of equilibrium, in addition, to each electrolyte throughout the whole solution, viz., the equation

$$v_1v_2=v_3v_4,$$

where the v's are the volumes of the regions of the respective electrolytes. This equation may also be written

$$N_1V_1N_2V_2=N_3V_3N_4V_4. \quad . \quad . \quad . \quad . \quad . \quad (b)$$

In addition to these equations obtained from the conditions of dissociational equilibrium we have also one expressing the equality of the volume v of the solution to the sum of the volumes of the regions occupied by the electrolytes, viz.,

$$N_1V_1+N_2V_2+N_3V_3+N_4V_4=v. \quad . \quad . \quad . \quad . \quad (c)$$

We have also equations stating that the ionic concentrations at a given temperature depend only upon the regional dilutions, viz.,

$$\alpha_1/V_1=f_1(V_1),\ \alpha_2/V_2=f_2(V_2),\ \alpha_3/V_3=f_3(V_3),\ \alpha_4/V_4=f_4(V_4), \quad . \quad (d)$$

where the functions f may be determined for dilute solutions by means of conductivity observations on simple solutions of the respective electrolytes, provided we may assume that

---

* *Ztschr. f. phys. Chem.* ii. (1888), p. 284.

the relation between ionization and regional dilution in a complex solution is the same as between ionization and dilution in a simple solution.

Finally, from the nature of the process of double decomposition, if n_1 and n_2 are the numbers of gramme-equivalents of 1 and 2 added to water in preparing the solution, we have the equations:

$$n_1 = N_1 + N_3,\quad n_2 = N_2 + N_4,\quad \text{and } N_3 = N_4. \quad . \quad . \quad (e)$$

We have thus twelve equations for determining the four α's, the four V's, and the four N's involved in them.

The functions, *f*, are too complex to admit of an algebraical solution of these equations. Were it possible to measure conductivity with sufficient exactness, however, it would be possible to solve them by a partially graphical process. For this purpose, reducing equations (*b*), (*c*), and (*e*) to two, we should have

$$N_3 = (v - V_1 n_1 - V_2 n_2)/(V_3 + V_4 - V_1 - V_2), \quad . \quad . \quad (1)$$

$$N_3^2(V_3V_4/V_1V_2 - 1) + N_3(n_1 + n_2) = n_1 n_2. \quad . \quad . \quad . \quad (2)$$

We may apply equations (*d*) by drawing curves which give the relation of concentration of ions to dilution for simple solutions of the electrolytes. We may then apply equations (*a*) by reading off from these curves the dilutions (V_1, V_2, &c.) of simple solutions having what we may guess at as being the concentration of ions of the complex solution. We may then determine N_3 from (1) and see if this value is, first, a possible value, and, if so, secondly, one that will satisfy (2). If, though a possible value, it will not satisfy (2), we must make another, and this time a more intelligent, guess, and so on, until a value of the concentration of ions has been selected giving values of V_1, V_2, &c. which will satisfy both (1) and (2), when, N_3 being thus determined, N_1, N_2, and N_4 may be found, and all the data required for calculation are known.

We have tried this method but without a satisfactory result. The denominator in expression (1) is a small quantity, and even when the curves referred to above are drawn with the best available data as to conductivity, it is affected by too large an error.

It is possible, however, by the aid of the above equations to determine the constitution of a complex solution with a known concentration of ions and therefore a calculable conductivity, to prepare the solution, and measure its conductivity, and thus to apply a somewhat severe test to the dissociation theory as a working hypothesis.

For this purpose select a low value of the concentration of ions (that the solution may be dilute) and read off from the experimental curves referred to above, the values of the dilutions, V_1, V_2, &c., of simple solutions of 1, 2, &c., which will have this concentration of ions. If these solutions be prepared and mixed in proper proportions as to volume, no change in ionization or composition will occur. To find the proper proportions, select arbitrarily any value of v_4, the volume of the solution of 4 which is to be mixed with the others. It will contain $N_4 = v_4/V_4$ gramme-equivalents of 4. From equations (*e*) we must have $N_3 = N_4$. Hence the volume of the solution of 3 to be mixed with the others will be $v_3 = V_3v_4/V_4$. Next select arbitrarily any value of v_2. Then since by (*b*) we must have $v_1v_2 = v_3v_4$,

$$v_1 = v_3v_4/v_2 = V_3v_4^2/V_4v_2.$$

These volumes being mixed, we have a complex solution in which the α's and N's are known, the conductivity of which is therefore calculable by the dissociation theory.

A series of observations and calculations of the conductivity of solutions containing sodium and potassium chlorides and sulphates has been made by the second of us with the object of testing the calculability of the conductivity in such cases. As a full account* of these experiments is to be published elsewhere it will be unnecessary here to enter into detail. They were made by Kohlrausch's method, with fairly good though not the best appliances, and full use was made of the experience of other observers as to precautions against error. Taking account of all sources of error, the measurements of conductivity may probably be regarded as accurate to about 0·25 per cent.

The following tables give the observations made on simple solutions of the salts mentioned for the purpose of obtaining data for drawing the curves referred to above. Dilutions are expressed in litres per gramme-equivalent. The conductivities are specific molecular conductivities (*i. e.* per gramme-equivalent), and are expressed in terms of 10^{-8} times the conductivity of mercury at 0° C. All solutions were prepared and all observations made at 18° C. The values of the ionization-coefficient used in calculating the concentration of ions are quotients of the specific molecular conductivity observed by Kohlrausch's values† of the specific molecular conductivity at infinite dilution.

---

* Mr. Archibald's paper has been communicated to the Royal Society of Canada, and is to be published in vol. iii. of the Second Series of its Transactions.

† Wied. *Ann.* xxvi. (1885) p. 198.

Potassium Chloride.			Sodium Chloride.		
Dilution.	Conductivity.	Concentration of Ions.	Dilution.	Conductivity.	Concentration of Ions.
20·00	1085	·0445	20·00	898·2	·0436
15·62	1070	·0561	15·62	890·0	·0553
12·50	1058	·0694	12·50	879·8	·0683
10·00	1050	·0861	10·00	866·2	·0841
6·915	1036	·123	6·915	852·6	·120
5·760	1025	·146	5·760	838·1	·141
3·456	997	·236	4·800	827·6	·167
2·880	986	·281	4·000	807·1	·196
2·400	975	·333	2·880	791·6	·267
2·000	959	·393	2·400	777·7	·315
1·713	953	·456	2·000	759·0	·368
1·428	935	·537	1·571	732·2	·452
1·190	924	·636	1·309	720·4	·534
1·091	921	·692	1·091	701·6	·624
1·000	918	·753	1·000	694·5	·674

Potassium Sulphate.			Sodium Sulphate.		
Dilution.	Conductivity.	Concentration of Ions.	Dilution.	Conductivity.	Concentration of Ions.
20·00	959·0	·0375	20·00	784·2	·0370
15·62	933·9	·0467	15·62	771·1	·0466
12·50	918·0	·0574	12·50	752·7	·0568
10·00	898·0	·0702	10·00	733·5	·0692
8·605	893·2	·0811	7·047	663·2	·0888
7·173	879·1	·0957	5·882	651·2	·104
5·973	856·3	·112	5·313	648·1	·115
4·977	839·0	·132	3·692	622·7	·159
3·456	791·2	·179	2·918	597·9	·193
2·880	771·1	·209	2·431	582·5	·226
2·400	753·0	·245	2·022	561·5	·262
2·073	741·3	·279	1·689	540·6	·302
2·000	737·0	·288	1·408	521·2	·349
1·440	707·2	·384	1·176	495·5	·397
1·200	689·1	·449	1·016	477·5	·443
1·000	671·8	·525	·847	455·5	·507

In preparing the mixtures, the proper dilutions having been determined by the aid of the dilution-ionic concentration-curves referred to above, and the simple solutions made up, equal volumes (25 c.c.) of the solutions of the sulphates were mixed with volumes of the solutions of the chlorides calculated as shown above, the volumes of the chloride solutions being therefore also equal, but different in the case of different mixtures.

The following table gives the concentrations, in gramme-equivalents per litre, of the constituent solutions, the volumes of the chloride solutions mixed with 25 c.c. each of the sulphate solutions, the common concentration of ions, the observed and calculated values of the specific conductivity, and the differences between observed and calculated values expressed as percentages. Conductivities are expressed as in the former tables, and, as before, the solutions were prepared and their conductivities determined at 18° C. Observations of density made before and after mixing showed that no change of volume occurred which required to be taken into account in the calculations.

Constituent Solutions.					Mixtures.			
Concentration.				Volumes of Chloride Solutions (c.c.).	Concentration of Ions.	Conductivity.		
KCl.	NaCl.	$\frac{1}{4}K_2SO_4$.	$\frac{1}{2}Na_2SO_4$.			Observed.	Calculated.	Difference per cent.
·5814	·6410	·8460	1·019	43·81	·454	521·7	518·3	−0·65
·5000	·5393	·7902	·8375	41·87	·393	460·4	458·1	−0·50
·4166	·4484	·5903	·6711	40·26	·333	380·2	380·0	−0·05
·3930	·4166	·5540	·6289	40·00	·315	356·9	358·4	+0·42
·3566	·3787	·5000	·5617	39·38	·288	331·1	328·8	−0·69
·3401	·3571	·4739	·5291	38·88	·274	316·0	313·9	−0·67
·2380	·2500	·3225	·3478	36·55	·196	223·0	223·8	+0·36
·2008	·2083	·2702	·2902	36·13	·167	190·9	191·3	+0·21
·1675	·1736	·2201	·2374	35·42	·141	162·3	161·5	−0·49
·1310	·1349	·1674	·1834	34·99	·112	127·8	128·0	+0·16
·1219	·1265	·1552	·1700	34·85	·104	119·2	118·8	−0·34
·1032	·1063	·1287	·1419	34·36	·089	104·7	104·4	−0·29
·0787	·0800	·0969	·0984	31·25	·068	78·00	78·23	+0·29
·0648	·0659	·0791	·0800	30·88	·0568	65·29	65·03	−0·40
·0527	·0536	·0640	·0644	30·57	·0467	52·92	53·03	+0·21
·0500	·0512	·0607	·0611	30·52	·0445	50·75	50·91	+0·32

It will be seen that the differences between observed and calculated values range from 0·7 to 0·2 per cent.; that the larger differences occur in the case of the more concentrated solutions and for the most part have the same sign, and that in the case of the more dilute solutions the differences are either within or but little beyond the limit of error of the observed values and are about equally distributed as to sign. For the stronger solutions it was to be expected that the differences would be beyond the limit of observational error; because (1) the ratios of the specific molecular conductivity to the specific molecular conductivity at infinite dilution, which were taken to be equal to the ionization-coefficients, are so, rigorously, only at infinite dilution; and (2) the value of the specific molecular conductivity at infinite dilution for an electrolyte in a mixture may be taken to be the same as the value determined by experiments on a simple solution, only at infinite dilution. It was to be expected also that, except for the accumulation of accidental errors, the differences in the case of the stronger solutions would have the same sign; for the errors in the values of the α's and the μ_∞'s would have the same signs for different solutions. When we consider the large number of sources of error involved in the determination of the constituent solutions, the preparation of the mixtures, and the finding of their conductivity, the agreement between the observed and the calculated values in the case of the weaker solutions must be regarded as quite satisfactory, and as warranting the conclusion that it is possible by the aid of the dissociation theory to calculate the conductivity of dilute aqueous solutions containing sodium chloride and potassium sulphate, and therefore also sodium sulphate and potassium chloride, within the limit of observational error.

XV. *The Failure of German Silver and Platinoid Wires.*

By Rollo Appleyard[*].

THE object of the present paper is to direct attention to the serious mechanical defectiveness of certain alloys used for electrical wires. The question is the more important because mechanical weakness implies electrical instability. In what follows I endeavour to bring together facts enough to indicate the general behaviour of german-silver and platinoid, and especially to settle the case as regards the conditions external to the wires.

It is necessary to remember that great differences exist between different samples of the same nominal quality of alloy; so that, of a hundred pounds' weight of wire, ninety-nine may be proof against all ordinary climates and conditions to all time, while the remaining pound may become fragile under the same treatment in a few weeks.

Many suggestions have been made to me, and are yet current, as to the cause of the failure of these wires. A well-known expert in electrical matters attributes it to sulphur in the ebonite of the electrical apparatus. It will, however, be shown that failure occurs apart from all ebonite and sulphur. Paraffin-wax has been mentioned by another authority as the probable cause of these defects. Instances will be given to

---

[*] Communicated by the Physical Society: read Nov. 26, 1897.

prove that paraffin-wax is an absorbent of moisture, and consequently useless for purposes of protection against climate; but I have no evidence that it is aggressively harmful to the wires. Chemists have variously attributed the failure to the presence of zinc, copper, antimony, arsenic, sulphur, phosphorus, and tungsten in the wires: the chemistry of the question must therefore be regarded as *sub judice.*

Tropical heat and moisture accelerate failure, but not in all cases. Moisture has been proved by Brereton Baker to be essential to certain chemical combinations; and there is evidence enough in what follows that the same agency is effective in bringing about changes in the constitution of alloys. The deteriorating effect of heat and moisture is not limited to mixed metals; for I have recently examined a large tube made of electrolytic copper which, under the influence of steam, quickly became very seriously "pitted." Another tube, of the same electrolytic copper, became similarly "pitted" when used for conveying sea-water. When these were replaced by tubes of ordinary copper, there was no further trouble.

During the past six years some very remarkable instances of the failure of alloys have come to my notice, particularly with regard to wires of german-silver and platinoid used in the construction of resistance-coils. Specimens of these wires, insulated with white silk, were submitted to various conditions of climate; they were sent respectively to India, Brazil, Chile, Peru, Ecuador, Nicaragua, Mexico, and Texas. For comparison and reference, similar wires were in some instances kept in England. Several thousands of bobbins were thus distributed in widely different latitudes, *i. e.*, to Valparaiso, Iquique, Chorillos, Santa Elena, Panama, San Juan del Sur, Salina Cruz, Coatzacoalcos, Vera Cruz, Galveston, Pernambuco, Bahia, Rio Janeiro, Monte Video, and Calcutta.

A few years after this distribution faulty bobbins were reported from San Juan del Sur, Santa Elena, Panama, Vera Cruz, Bahia, and Calcutta. These six towns all lie on or near sea-coasts, and they are nearly on the same terrestrial isotherm, *i. e.* the isotherm including the area of high terrestrial mean temperature, 25° C.

In all cases of failure the alloy had become brittle and the wires had broken, not only at the outer layers, but also within the coils. The following is a short history of the wires that failed:—

(1) In April 1891 some german-silver wire, 16 mils diameter, doubly covered with silk, was wound on small boxwood bobbins and then treated with paraffin-wax. Each bobbin was afterwards lapped with a strip of leather. They were all

packed in tin-lined soldered cases containing straw, and were shipped to Vera Cruz, on the Mexican Coast—a voyage of twenty-six days. A month after leaving England the cases were unpacked. It was then noticed that the leather lappings of the bobbins had deteriorated, the silk coverings were discoloured, the german-silver had become brittle, and breaks had occurred in the wire. The packing-straw showed signs of dampness. The case had been stowed in the hold with other cargo in rather a hot part of the ship. No current was ever sent through this wire except the small fraction of a milliampere used momentarily while testing the resistance before shipment.

(2) In February 1891 similar bobbins, of nominally the same german-silver wire, were shipped to Valparaiso and Iquique. These were not treated with paraffin-wax, they were merely lapped with leather. During the Chilian war the apparatus was dismantled. I have had no information as to the state of these bobbins since January 1893, when the wire was reported to be in good condition. The mean temperature of Valparaiso is lower by about 10° C. than the mean temperature of Vera Cruz.

(3) In June 1895 bobbins of platinoid were shipped to the town of Bahia, on the Brazilian coast. The wire was silk-covered and treated with paraffin-wax. No leather or ebonite was used in connexion with this apparatus. An outer box of mahogany protected the bobbins from insects. At the end of a very wet and stormy season the wire failed. It was reported "faulty" in January 1896, about six months after its arrival in Brazil. When unwound, the wire showed several fractures; it had become "short" locally. Bahia lies almost on the same isotherm as Vera Cruz, 25° C.

(4) Between the months of June and August 1893 bobbins of platinoid were shipped to Valparaiso, Iquique, and Chorillos. The wire was silk-covered and treated with paraffin-wax. Each bobbin had an outer tube of ebonite. There have been no reports of failure from any of these towns. This goes to prove that the presence of ebonite is not itself sufficient to account for the fracture of platinoid wires.

(5) In November 1893 similar bobbins were shipped to Galveston and Coatzacoalcos: no faults have, so far, developed in them. Here, again, ebonite has done no evident harm. Coatzacoalcos is nearer to the equator than is Vera Cruz.

(6) In December 1894 similar bobbins were shipped to Santa Elena, on the coast of Ecuador, and to San Juan del Sur, in Nicaragua. Two years later two of the Santa Elena bobbins were reported "faulty," and the same number failed at

San Juan del Sur during the same time. Both towns are within about 10 degrees of latitude of the equator.

(7) In September 1895 similar bobbins were shipped to Panama. The wire of one was reported "broken" in November 1896.

(8) Between October 1894 and September 1895 about a thousand bobbins of platinoid wire, made and protected as above described, treated with paraffin-wax and sheathed with ebonite, were shipped to Pernambuco, Rio Janeiro, and Salina Cruz. No reports of failure have, so far, been received from these towns. They are all within the tropics.

(9) In March 1895 a voltmeter, wound with silk-covered german-silver wire, was shipped to Calcutta. The wire was not treated with paraffin-wax, and no ebonite was used in connexion with it. In February 1897 every inch of the wire was rotten. As the coil was unwound, the wire fell to pieces. Slight electrical heating had possibly accelerated the structural change of the alloy.

(10) The specimens of various german-silver and platinoid wires on bobbins kept in England are still quite good. This applies to the wire such as was used for the voltmeter, as well as to the platinoid such as was used for the resistance-coils. This autumn I selected some of these home-specimens and formed them into small coils. They were then exposed just above the surface of a tank of water that was boiled all day and allowed to cool all night. This process was continued for six weeks with no apparent deteriorating effect upon any of the coils.

(11) A piece of bare platinoid wire kept for some years in the laboratory has become discoloured, and there are several black spots in it; but I can find no "short" places and no mechanical weakness anywhere.

(12) Some suspended helices of bare german-silver wire have broken in several places; these have had current through them from time to time. One of the fractures occurred at or very near a brazed joint. The helices were under slight torsional stress. There were no fractures in the short horizontal german-silver wires connecting these helices.

The constitution of german-silver is stated to vary as follows:—

Copper	50–66	parts.
Zinc.	19–31	,,
Nickel	13–18	,,

Platinoid is generally described as german-silver with two per cent. of tungsten. According to Brannt, the tungsten, in the form of phosphor-tungsten, is first melted with a certain quantity of copper. The nickel is next added, then the zinc, and finally the remainder of the copper. In order to remove phosphorus and a portion of the tungsten, both of which separate as dross, the resulting compound is several times remelted. It is probable that traces of arsenic and phophorus are present in the alloy.

If nickel is in too great proportion in nickel-copper alloys, oxygen is absorbed during fusion, and liberated on cooling. The result is a porous metal. Again, if the temperature of casting is too high, or if the cooling is irregular, cavities may be expected in the final alloy.

The surprising diminution in tensile strength produced by traces of impurities, for some alloys, has been studied by Prof. Roberts-Austen and other metallurgists. A very small percentage of arsenic in nickel-copper-zinc compounds causes extreme brittleness. So also does a small addition of lead or iron. And as arsenical nickel ore is the source of much of the nickel of commerce, the failure of german-silver and platinoid may possibly be due to traces of arsenic.

At this point, however, I propose to discriminate between two kinds of brittleness, my object being to simplify the discussion of the problem. That such a distinction is necessary will perhaps be best illustrated by an example. Prof. Roberts-Austen demonstrated that the addition of 1 per cent. of lead reduces the tensile strength of gold by more than two-thirds. Similarly, he observed that arsenic renders gold very fragile, and that 0·2 per cent. of bismuth *instantly* converts gold into an alloy that crumbles under the die. In all these cases, brittleness is characteristic of the alloy from the moment of solidification; it may, in fact, be regarded as a definite function of the atomic volumes of the constituent elements. I propose to call this "primary" brittleness.

But the brittleness of german-silver and platinoid is of a different order. It is a *subsequent* phenomenon. For example: the alloys from which electrical wires are made are necessarily strong in the first instance; if they were mechanically weak, *i. e.*, if they possessed "primary" brittleness, they would fail in the process of "drawing" through the die. Hence, the distinction between "primary" and "secondary" brittleness. The one is an accident of birth, the other is a disease that develops with age and circumstance.

The fracture of platinoid, as seen under the microscope, is granular or crystalline. In new and good specimens the colour is silver-grey, and uniform except for a few well-defined patches of brown. Occasional specimens of brittle wire retain, in parts, the silver-grey fracture. As a rule, however, bad specimens have, encroaching on the silver-grey area, patches of purple, yellow, copper-colour, and brown, with frequent cracks and fissures that communicate more or less with the surface of the wire. Very bad specimens have, at times, scarcely any of the silver-grey colour at the fracture. The patches of dark-purple, yellow, and copper-colour are clearly defined, as though each of them represented a distinct alloy, or a separate mass of "liquated" metal. On a particular bobbin, brittleness is often restricted to a foot or two of the wire. Similar remarks apply to the appearance of the fractures of german-silver.

The crevices and fissures in bad specimens are easy to observe with a microscope. They are probably developed during wire-drawing, as the result of pores in the cast metal. Another explanation of them may be looked for in the "liquation" of some of the metals constituting the alloy. By this action, the portion of the alloy that first solidifies rejects the yet molten portions, as ice rejects foreign matter. As cooling continues, the various constituents become isolated, homogeneity is lost, and I think we may fairly assume that, in consequence, the strength of the material varies from point to point of its mass, so that in passing afterwards through the die, the weaker constituents give way, and the general structure is loosened. Crystallization and internal electrical actions may also result in local weakness, crevices, and fissures.

Keeping in mind the existence of these fissures it is a simple matter to account for the fact that wires deteriorate more quickly under a small stress than when they are submitted to no stress. Apart from the mechanical weakening, it is evident that any extending force opens the crevices and makes way for air and moisture—it may also facilitate crystallization. And clearly, if the wires are being used for electric currents, as is the case with resistance-coils on arc-light circuits, great local heating occurs at these weakened sections, and fracture is inevitable. This was probably the fate of the voltmeter-wire mentioned in example (9) above. It was almost certainly the fate of the suspended helices (12).

From what precedes, there is plenty of evidence that secondary brittleness is fostered by tropical heat and moisture. A good wire, free from incipient cracks, may last for years, or may be proof against heat and moisture, as were the wires in example (10). But if there are any fissures, there is no doubt that moisture intrudes upon the alloy through these capillary channels, with destructive effect.

There is a kind of tradition that paraffin-wax is a perfect safeguard against moisture; as a matter of fact it is highly absorbent. The wax on the bobbins returned from the tropics is completely choked with moisture. Shellac, or the old-fashioned sealing-wax varnish, resists much better. There is no evidence to show that ebonite is harmful to german-silver or platinoid, but it may be well to keep the metal out of actual contact with ebonite.

Can metallurgists tell us the difference, in constitution and structure, between a german-silver wire that decays in four weeks, and another that under similar conditions never fails? Or, what is even more important, can they make us platinoid that shall never fail? If they cannot, it becomes necessary to surrender those cheaper and better electrical materials, and fall back upon the more expensive alloys, beginning at platinum-silver. If sufficient time and means were at the disposal of metallurgists they might discover the secret of permanence in alloys. Germany, with the advantages of a National Laboratory, has already attacked the question, and "manganin" is the result. Its adoption there as a satisfactory alloy is directly due to work done upon it by the Reichsanstalt. But it has yet to be proved that manganin will endure the conditions imposed by the tropics. Moreover, the doubt arises as to whether it is desirable to obtain from abroad material that ought to be produced by our own country. British cable-manufacturers are already importing thousands of tons annually of sheathing-wire from Germany; and it seems probable that, for the want of a National Laboratory, instrument-makers will now get their resistance-wire from that same adventurous foreign source.

XVI. *Analogy between the Cathodic Rays and those of Röntgen.* *By* Prof. A. Battelli, *of the Pisa University*[*].

IN the 'Electrician' of the 8th of January, 1897, Prof. Silvanus Thompson describes a very simple experiment which induces him to admit that there exists within Crookes's tubes a special kind of rays, which differ from the cathode rays and from those of Röntgen. He observes that on bringing a magnet near the focus-tube, as was already known, the fluorescence on the side of the tube facing the small reflectors *changes* its position; whereas, on the other hand, if a fluorescent plate be placed before the tube, the luminous stain on it does not shift its position through the effect of the magnet.

[*] Communicated by the Author.

From this he concludes that there must exist internally the above-said special rays (those deviated through the effect of the magnet), which, according to the author, are of a different nature not only from those of Röntgen but also from the cathode rays, only as they do not follow the regular laws of reflexion.

Now Thompson's experiment as regards the rays emanating from the focus-tube is perfectly similar to what I had already had occasion to observe in 1896 in conjunction with Prof. Garbasso* respecting the cathode rays. We were thereby led to admit, as most probable, that the Röntgen and cathodic rays were of an identical nature, with this difference, however, that the first only constitute a part of the latter.

It seems to me that Thompson's experiment may agree with this hypothesis without the necessity of recurring to a third species of rays of a different nature. In fact, reflexion of cathodic rays cannot be spoken of, if we admit that when they strike against any kind of fluorescent plate they are converted into other sorts of radiation. Whilst on the other hand, if such reflexion be admitted it cannot indeed be said that in its complex action it follows the laws of regular reflexion. We can instead observe in the focus-tubes other facts which I shall forthwith describe, and which are more in harmony with our hypothesis than with Prof. Thompson's.

For the study of these facts I have made use of "focus-" tubes or bulbs, in the inside of which we could place, in various positions, photographic films covered with black paper impenetrable to light, folded (as I had done in preceding experiments) round small metallic cylinders.

The bulbs were of spherical form (fig. 1) ; from the horizontal tubes A and B penetrated the anode and cathode, from the vertical tube C a small glass rod supporting the small reflector.

Through the same tube C one of the small photographic cylinders projected into the spherical bulb ; and through the other vertical tube D one or more similar cylinders were introduced into the bulb.

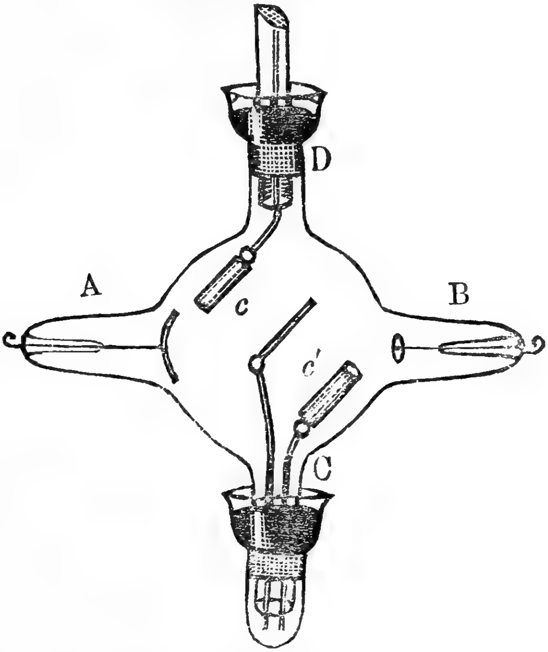
The reflector and the small cylinders were supported by appropriate framing joined to the respective supports by means of small hinges, so that one could easily give to the reflector and to the cylinders any desired position and inclination.

---

* A. Batelli, "Richerche sulle azioni fotografiche nell' interno dei tubi di scarica," *N. Cimento*, ser. 4, vol. iii. p. 193 (1896) ; A. Battelli ed A. Garbasso, "Raggi catodici e raggi X," tom. cit. p. 289 (1896); A. Battelli ed A. Garbasso, "Azioni dei raggi catodici sopra conduttori isolati," *ibid.* vol. iv. p. 129 (1896).

Finally, the tube D was kept in permanent communication with the air-pump.

Fig. 1.



By means of this arrangement the conditions of the apparatus could rapidly be changed, and on the other hand the closure of the tube C and the communication of the tube D with the air-pump were established in a perfect manner and with much rapidity, by means of joints surrounded with mercury, as indicated in the figure. With these tubes I performed the following series of experiments:—

1*st Experiment.*—The small reflector was a plate of platinum, and besides the two small cylinders *c* and *c′* situated in front and at the back of the same small reflector, other three were placed in front laterally to *c* distributed at equal intervals between *c* and the surface of the reflector, all at nearly the same distance from the point struck by the small pencil of cathodic rays. The experiment was conducted under the influence of both high and medium rarefaction, and an impression was invariably obtained on all the films placed in front of the platinum reflector, which impression gradually diminished in strength from the film *c* towards the one placed in the plane of the reflector [*]. In this last

---

[*] Whenever I speak of a photographic impression on a film placed in front or at the back of the reflector of a focus-tube, I mean to indicate the impression on that part of the film that was facing the reflector, because in any other part of the same film the impression was always very faint or small.

impression a sharp demarcation is generally observed between the part acted upon (corresponding to the region above the reflector) and the part which remained unaltered (corresponding to the region beneath).

Observing then the interior of the bulb, one could easily perceive, so long as the rarefaction was not very high, the luminous pencil of the reflected cathodic rays forming with the reflector an angle, which, by the way, could not be reckoned as equal to that of the incident pencil; but meanwhile the fluorescence, though slight, spread over all the inner side of the tube placed above the plane of the reflector.

Whilst by degrees the rarefaction was gradually being increased, the reflected pencil of rays became less and less distinct; and near the surface of the reflector there could then be noted, especially by means of an opaque surface provided with a small opening, a feeble halo (rays expanding in all directions), whilst the demarcation-line between the inner and outer part of the tube grew more and more distinct.

2nd Experiment.—The cathode of the bulb used in this experiment was a plane disk, and the reflector had a spherical surface. The luminous cathodic rays striking on the spherical reflector were not reflected in a convergent pencil, but in a diminishing and rather divergent one, which, as the rarefaction increased, reduced itself into a halo near the surface, with an aspect not very dissimilar to the one it assumed when the reflector was formed by a plane surface.

Besides this the fluorescence expanded on the inner sides of the tube in a manner similar to the case of a plane reflector with a line of demarcation corresponding to the edge of the spherical reflector.

These first two experiments prove that it is quite out of the question to speak of a completely regular reflexion of the cathodic rays.

3rd Experiment.—On the direction taken by the small luminous pencil of the cathodic rays, reflected by the ordinary reflector of the focus-bulb, a second plane reflector was placed with an inclination of about 45° to the pencil.

This second reflector sent back the rays that fell on it in a less condensed pencil than when these same rays reached it, but yet its action was similar to that of the first reflector, with this difference, that the halo round its surface had a certain preponderance over the short reflected pencil, which appeared faint and fading, and was often invisible.

4th Experiment.—In the bulb of fig. 1 instead of the photographic cylinder *c*, a spherical reflector S′ was placed with its axis in a horizontal position, and the cylinder *c*′ was fixed in the posterior part of the tube, but high up in front of the spherical reflector S′ and at some distance from the axis of this same reflector.

The cathodic rays reflected by the plane reflector on the spherical one S′, were by this thrown back with the same luminous appearances as surrounded the spherical reflector exposed to the direct cathodic rays in the 2nd experiment; but the small pencil which showed sufficiently in said 2nd experiment was scarcely visible in front of S′ and could only be followed up to a very limited distance from the reflector.

The photographic cylinder received then a much more vivid impression on the part facing the reflector than on any other portion of it.

The two experiments, 3rd and 4th, show that the cathode rays reflected by the reflector of a focus-tube can subsequently undergo a new anomalous reflexion similar to the first reflexion of the same rays.

5th Experiment.—In this experiment two bulbs of equal size, with the reflector and the small cylinder disposed as in fig. 1, were placed in simultaneous communication with the air-pump.

In the first bulb, however, the reflector was a sheet of platinum of the thickness of $\frac{1}{10}$ of a millimetre, and in the second bulb a sheet of aluminium of the same thickness.

The electrical discharge was sent alternately in the two bulbs minute after minute.

On the two films which faced respectively the two reflectors, strong impressions were always received and of the same intensity. To a feeble impression obtained at the back of the sheet of aluminium, no impression whatever corresponded on the film placed at the back of the sheet of platinum. But to a sufficiently marked impression on the first, a very feeble impression showed on the second.

6th Experiment.—The same arrangement was maintained as in the preceding experiment; but in the first bulb the reflector was now of aluminium of the thickness of $\frac{6}{10}$ of a millimetre, and in the second the reflector was also of aluminium, but of the thickness of but $\frac{1}{10}$ of a millimetre.

The impressions on the films in front were, as usual, of the same intensity. And as for the posterior films, a stronger impression was obtained on the one situated behind the thinner sheet.

7th Experiment.—The same disposition as in the preceding experiment. The reflector of one of the bulbs was of black paper of the thickness of $\frac{8}{100}$ of a millimetre, and that of the other was a sheet of aluminium of the same thickness.

The impression on the photographic cylinder in front of the sheet of aluminium was very slightly more intense than on the cylinder placed in front of the sheet of black paper. Whereas in the cylinder placed behind the first reflector the impression appeared sensibly weaker than that at the back of the second.

8th Experiment.—Still the same arrangement as in the preceding experiments. The reflector of one of the bulbs was of sheet aluminium of $\frac{1}{10}$ of a millimetre, and the other was also of sheet aluminium of $\frac{3}{1000}$ to $\frac{4}{1000}$ of a millimetre.

The electrical discharge was sent alternately in the two bulbs with intervals of two seconds each time.

Of the films in front, the one that was the more strongly acted on was the one facing the thicker sheet, and the reverse effect was obtained on the films on the back.

Besides this, the two impressions in front and behind the very thin sheet of aluminium were very nearly of the same intensity. And, lastly, at the back of the thin sheet the usual shadow projected in the focus-tube by the reflector was no longer discernible.

Shortly after the action of the bulb, the sheet of aluminium was pierced through by the cathode rays, but we still had sufficient time to complete the experiment.

The same experiment with similar results was performed with two bulbs, the first of which had as reflector a sheet of black paper of the thickness of $\frac{8}{100}$ of a millimetre, and the other a small sheet of black paper even thinner than $\frac{1}{100}$ of a millimetre.

9th Experiment.—In one of the bulbs, with the preceding arrangement, a reflector of black wove-paper was placed of the thinness of less than $\frac{1}{100}$ of a millimetre, and in the other a reflector of sheet platinum covered with the same thin wove paper.

In front of the two surfaces facing the cathode an impression was received slightly more intense on the film facing the sheet of platinum, whereas in front of the opposed surfaces a much stronger impression was naturally received on the other film belonging to the sheet of the thin wove paper.

The experiments from 5th to 9th included prove, not only that the photographic action on the sheet struck by cathodic rays is much more intense on the side facing the cathode than on the contrary one (which was already known), but they demonstrate, moreover, that the agent to which such an effect is due is the more fully transmitted from the first face to the second the thinner the stratum is which is to be penetrated, and the less dense the substance.

This last fact would prove in harmony with the hypothesis that the photographic action is due to the cathodic rays themselves forced back by the obstacle or piercing through it.

And next, what could not easily be foreseen, is the other conclusion suggested by these experiments, namely, that the action of the surface facing the cathode is the more feeble the less the thickness of the sheet. It would be difficult to place this result in harmony with the hypothesis that the Röntgen rays emanating from any substance whatever are generated by the cathodic rays which strike that same substance.

10*th Experiment.*—Two bulbs of equal size were placed simultaneously in communication with the air-pump, each bulb containing the platinum reflector, and also several photographic cylinders disposed as in the 1st experiment.

One of these reflectors was, however, surrounded by a spherical covering of thin black wove paper of the thinness of $\frac{2}{100}$ of a millimetre. This covering had a small aperture in front of the cathode, through which the small pencil of cathodic rays penetrated so as to strike direct on the reflector.

The results obtained with both bulbs were identical, with, perhaps, a very slight difference in the intensity of the impression. Besides which, in the bulb with the spherical covering of black thin wove-paper the fluorescence on the inner side of the tube was not altered in its distribution nor sensibly so in its intensity; similarly, the shadow projected by the reflector on the aforesaid inner side of the tube remained unaltered.

Therefore an object surrounding the reflector does not in the least way cause any deviation in the action which may be obtained within the tube through the effect of the cathodic rays reflected by the reflector itself.

11*th Experiment.*—Two bulbs were used in this experiment prepared in the same way as in the first, both having a reflector of aluminium of from three to four thousandth parts of a millimetre in thickness. To one of these an electromagnet was applied behind the reflector in such a way that the luminous mark, which was visible in the direction of the incidental cathodic pencil, should strongly shift its position.

In the photographic cylinders placed behind the two reflectors, impressions were obtained of very nearly the same intensity.

I then placed, in the next experiment, two more photographic cylinders respectively behind the two small reflectors, but in a lateral position. So soon as the first bulb was excited I rapidly pushed, by means of the electromagnet, the centre of the luminous mark at the back on the line which joined the reflector to the small cylinder.

In this manner I only obtained a very slight variation in the photographic impression.

This experiment goes to prove that the radiation emanating from the posterior surface of a sheet struck by the cathodic rays, acts in the same manner as the radiation emitted by the front surface of the sheet. This shows that the power, observed by Thompson as possessed by the rays emanating from the reflector of a focus-tube, could also be noted in the rays emanating from the anticathodic surface of an ordinary Crookes's tube, if this surface be sufficiently thin.

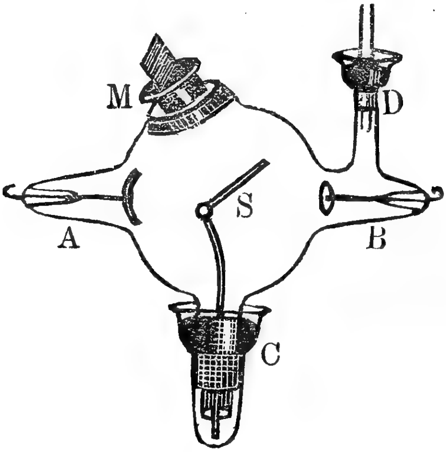
12*th Experiment.*—In a bulb similar to that of fig. 1 the two photographic cylinders were composed, each of them, of three small cylinders placed in a line and respectively covered with black paper, with a sheet of aluminium and one of platinum, all of the thickness of $\frac{8}{100}$ of a millimetre.

In the small cylinder covered with aluminium an impression was obtained weaker than the one noted in the small cylinder covered with paper; and on the small cylinder covered with platinum, a still weaker one than on the one covered with aluminium, both in front and at the back of the reflector.

13*th Experiment.*—Three small cylinders prepared respectively in the same manner as in the 12th experiment were placed at the same moment on the direct line followed by the cathodic rays in an ordinary Crookes's tube.

The same results were obtained as in the preceding experiment with the same relations between the respective photographic impressions.

Fig. 2.



14*th Experiment.*—For this experiment a bulb was constructed slightly differing from the preceding ones. This also was spherical, and the two electrodes entered it through the two horizontal tubes A and B (fig. 2), and the reflector penetrated in it likewise through the vertical tube C. But on the part of the bulb facing the said reflector was soldered a large tube M closed by a bimetallic disk, composed, namely, half of an aluminium sheet of the thickness of *half a millimetre*, and the other half also of a sheet of aluminium of the thickness of *one millimetre*. On the diameter along which the two sheets fitted together a thick sheet of zinc was soldered perpendicularly to the plane of the disk.

In front of both the two sheets that formed the disk a small photographic cylinder was placed, on the extremities of which two figures were respectively fixed; one made with the thinner aluminium sheet ($\frac{1}{2}$ millimetre) and the other with the thicker sheet (1 millimetre).

Naturally the two sides of the cylindrical surfaces, wherein the figures showed, were turned towards the disk.

In the region of the film which stood opposite the two thin sheets (the thin disk and figure of the thin sheet) a strong impression was received; whereas on the points placed in a line with the two thicker sheets a rather feeble one was obtained. But the parts where the figure of the thin sheet fronted the thicker half-disk were impressed in an equal degree to the parts placed under the figure of the thicker sheet and the thinner half-disk.

The same results were obtained when the tube M of the bulb was closed by disks formed (*a*) half of aluminium and half of glass of the same thickness, (*b*) consisting half of magnesium and half of glass still of the same thickness.

I also tried to use aluminium and zinc; but the difference between the impressions derived from the rays emanating from both the one and the other metal (of equal thickness) was so great that no decisive result was obtainable.

15*th Experiment.*—The preceding experiment was repeated, the cathodic rays striking directly on the metallic disk.

The result was the same.

These four last experiments show at any rate very approximately that the transparencies of those substances which are photographically efficacious for the cathodic rays and the Röntgen rays have the same value.

Conclusions.

From the bulk of the experiments above described it seems to me that the following deductions can principally be drawn:—

(*a*) It cannot be asserted that the cathodic rays are reflected, as a whole, according to the laws of regular reflexion.

(*b*) The rays thrown back from the reflector of a focus-tube we may confidently assume to possess the same properties as the direct cathodic rays.

(*c*) The rays emanating from the posterior part of a very thin sheet, the anterior part of which is struck by the cathodic rays, also possess the same properties as these.

(*d*) A pencil of cathodic rays appears as made up of various rays of different natures. When they strike on a substance of a very small thickness they seem to possess the power of passing through it, but in the same way as if they passed through a filter, which would allow a passage more or less easy to some of them than to the others.

XVII. *A Theory of the Connexion between Cathode and Röntgen Rays.* *By* J. J. Thomson, *M.A., F.R.S., Cavendish Professor of Experimental Physics, Cambridge*[*].

A MOVING electrified particle is surrounded by a magnetic field, the lines of magnetic force being circles having the line of motion of the particle for axis. If the particle be suddenly stopped, there will, in consequence of electromagnetic induction, be no instantaneous change in the magnetic field; the induction gives rise to a magnetic field, which for a moment compensates for that destroyed by the stopping of the particle. The new field thus introduced is not, however, in equilibrium, but moves off through the dielectric as an electric pulse. In this paper we calculate the magnetic force and electric intensity carried by the pulse to any point in the dielectric.

The distribution of magnetic force and electric intensity around the moving particle depends greatly on the velocity of the particle, if this velocity is so small that the square of its ratio to the velocity of light can be neglected, then the electric intensity is symmetrically distributed round the particle, and at a distance r from it is equal to e/r^2, where e is the charge on the particle; the lines of magnetic force are circles with the line of motion of the particle for axis; the magnitude of the magnetic force at a point P is $we \sin \theta / r^2$, where w is the velocity of the particle, and θ the angle a radius from the particle to P makes with the direction of motion.

When, however, the velocity of the particle is so great that we can no longer neglect the square of its ratio to the velocity of light, the distribution of electric intensity is no longer uniform, the electric intensity, along with the magnetic force, tends to concentrate in the equatorial plane, that is, the

---

[*] Communicated by the Author.

plane through the centre of the particle at right angles to its direction of motion ; this tendency increases with the velocity of the particle until, when this is equal to the velocity of light, both the magnetic force and the electric intensity vanish at all parts of the field except the equatorial plane, and in this plane they are infinite.

The pulses started by the stopping of the charged particle are, as might be expected, different when the ratio of the velocity of the particle to that of light is small, and when it is nearly unity. But even when the velocity is small, the pulse started by stopping the particle carries to an external point a disturbance in which the magnetic force is enormously greater than it was at the same point before the particle was stopped. The time the pulse takes to pass over a point P is, if the charged particle be spherical, equal to the time light takes to pass over a distance equal to the diameter of this sphere ; the thickness of this pulse is excessively small compared with the wave-length of visible light. When the velocity of the particle approaches that of light two pulses are started when it is stopped. One of these is a thin plane sheet whose thickness is equal to the diameter of the charged particle ; this wave is propagated in the direction in which the particle was moving ; there is no corresponding wave propagated backwards : the other is a spherical pulse spreading outwards in all directions, whose thickness is again equal to the diameter of the charged particle, and thus, if this particle is of molecular dimensions, or perhaps even smaller, very small compared with the wave-length of ordinary light. The theory I wish to put forward is that the Röntgen rays are these thin pulses of electric and magnetic disturbance which are started when the small negatively charged particles which constitute the cathode rays are stopped.

We shall now proceed to calculate the disturbance propagated through the dielectric when a charged particle is suddenly stopped.

The components of the magnetic force and the electric intensity all satisfy Poisson's equation

$$\frac{d^2\phi}{dt^2}=\mathrm{V}^2\left\{\frac{d^2\phi}{dx^2}+\frac{d^2\phi}{dy^2}+\frac{d^2\phi}{dz^2}\right\};$$

the solution of this equation was shown by Poisson to be

$$\phi=\frac{d}{dt}(t\omega_1)+t\omega_2,$$

where ϕ is the value of the function at a point P at the time t ; ω_1 the mean value of ϕ when $t=0$ over the surface of a sphere whose centre is at P, and whose radius is Vt; ω_2 is the mean value of $d\phi/dt$ when $t=0$ over the surface of the same sphere.

Let $t=0$ be the time when the particle is suddenly brought to rest. Take the centre of the particle when it is brought to rest as the origin of coordinates, and the line of motion of the centre of the particle as the axis of z. Then α, β, γ, the components of the magnetic force when the particle is stopped, are for all points outside the particle given by the equation*

$$\left.\begin{aligned} \alpha &= -\frac{eVw}{(V^2-w^2)^{\frac{1}{2}}}\frac{y}{\left(x^2+y^2+\dfrac{V^2}{V^2-w^2}z^2\right)^{\frac{3}{2}}}; \quad \frac{d\alpha}{dt}=-w\frac{d\alpha}{dz} \\ \beta &= \frac{eVw}{(V^2-w^2)^{\frac{1}{2}}}\frac{x}{\left(x^2+y^2+\dfrac{V^2}{V^2-w^2}z^2\right)^{\frac{3}{2}}}; \quad \frac{d\beta}{dt}=-w\frac{d\beta}{dz} \\ \gamma &= 0. \end{aligned}\right\} \quad (1)$$

At all points inside the particle, which we shall take to be a sphere of radius a,

$$\alpha=\beta=\gamma=0.$$

In these equations V is the velocity of light through the dielectric, w the velocity of the charged sphere before it was stopped, e the charge on the sphere. To get the values of α, β, γ at any time after the particle is stopped, we have by Poisson's method to integrate the values just given over the surfaces of certain spheres; in the general case this integration leads to complicated elliptic integrals. We shall get a clearer idea of the physical nature of the disturbance if we consider two special cases, (1) when we can neglect the square and higher powers of w/V; (2) when w/V is very nearly unity.

In the first case, when we neglect w^2/V^2, α, β, $d\alpha/dt$, $d\beta/dt$ when $t=0$ all satisfy Laplace's equation, hence the mean value of any of these quantities over the surface of a sphere which does not enclose the origin, nor cut through any part of the electrified sphere, is equal to the value of this quantity at the centre of the sphere; we can easily see, too, that when the sphere entirely surrounds the electrified sphere the mean value of any of these quantities over its surface is zero. Thus we have, by Poisson's solution, the following values for the components of the magnetic force after a time t from the stoppage of the electrified sphere,

---

* Heaviside, Phil. Mag. April 1889; J. J. Thomson, 'Recent Researches,' p. 19.

$$\left.\begin{aligned}\alpha &= -\frac{ewy}{(x^2+y^2+z^2)^{\frac{3}{2}}} - \frac{3ew^2yzt}{(x^2+y^2+z^2)^{\frac{5}{2}}} \\ \beta &= \frac{ewx}{(x^2+y^2+z^2)^{\frac{3}{2}}} + \frac{3ew^2xzt}{(x^2+y^2+z^2)^{\frac{5}{2}}}\end{aligned}\right\} \quad . \quad . \quad . \quad (2)$$

As we are neglecting w^2 we may leave out the second terms in these equations. These values hold from $t=0$ to $t=(r-a)/\mathrm{V}$. When $t>(r+a)/\mathrm{V}$,

$$\alpha=\beta=0.$$

We must now allow for the absence of magnetic force inside the sphere of radius a; the easiest way to do this is to suppose that the expressions (1) hold right up to the centre of this sphere, and superpose on the distribution represented by (1) a distribution inside the sphere given by

$$\alpha=\frac{ewy}{r^3}, \qquad \beta=-\frac{ewx}{r^3}, \quad . \quad . \quad . \quad . \quad (3)$$

where $$r^2=x^2+y^2+z^2\,;$$

while outside the sphere we have for this distribution

$$\alpha=\beta=0.$$

If we superpose this distribution we may suppose that at any time

$$\alpha=\alpha_1+\alpha_2,$$
$$\beta=\beta_1+\beta_2,$$

where α_1, β_1 are the values given by equations (2) which may be now supposed to hold from $t=0$ to $t=r/\mathrm{V}$, while, when $t>r/\mathrm{V}$, α_1, β_1 both vanish; α_2, β_2 are the magnetic forces arising from the disturbance given initially by (3). This disturbance will begin to be felt at a point P at a time $(\mathrm{OP}-a)/\mathrm{V}$, and will cease after a time $(\mathrm{OP}+a)/\mathrm{V}$; O is the centre of the charged sphere. Thus the thickness of the pulse due to this distribution is equal to the diameter of the sphere.

We can easily show that

$$\int w\frac{ex}{r^3}\,d\mathrm{S},$$

taken over the part of a sphere whose centre is at P and radius is Vt, which is within the sphere whose radius is a, is equal to

$$\frac{2\pi we\,.\,\mathrm{V}^2t^2}{\mathrm{OP}^2}\left\{1-\frac{(\mathrm{OP}^2-a^2-\mathrm{V}^2t^2)}{2\mathrm{V}t\,.\,a}\right\}\frac{x}{\mathrm{OP}},$$

where x is the x coordinate of P.

Hence ω_1, the mean value of wex/r^3, is

$$\frac{ew}{2\,.\,\mathrm{OP}^2}\left\{1-\frac{(\mathrm{OP}^2-a^2-\mathrm{V}^2t^2)}{2\mathrm{V}t\,.\,a}\right\}\frac{x}{\mathrm{OP}}.$$

Thus

$$\frac{d}{dt}(t\omega_1)=\frac{1}{2}\frac{ew}{\mathrm{OP}^2}\left\{1+\frac{\mathrm{V}t}{a}\right\}\frac{x}{\mathrm{OP}}.$$

Hence

$$\beta_2=-\frac{1}{2}\frac{ew}{\mathrm{OP}^2}\left(1+\frac{\mathrm{V}t}{a}\right)\frac{x}{\mathrm{OP}},$$

$$\alpha_2=\ \frac{1}{2}\frac{ew}{\mathrm{OP}^2}\left(1+\frac{\mathrm{V}t}{a}\right)\frac{y}{\mathrm{OP}},$$

from $t=(r-a)/\mathrm{V}$ to $t=(r+a)/\mathrm{V}$. If the sphere is small, $\mathrm{V}t/a$ is large compared with unity, and $\mathrm{V}t$ is approximately equal to OP; hence

$$\alpha_2=\ \frac{1}{2}\frac{e}{a}\frac{yw}{\mathrm{OP}^2},$$

$$\beta_2=-\frac{1}{2}\frac{e}{a}\frac{xw}{\mathrm{OP}^2},$$

α_2 and β_2 are, when a is small, very large compared with α_1 and β_1.

We have now the complete solution of the problem, and we see that after the sphere is stopped, the magnetic force at a point P remains unaltered until $t=(r-a)/\mathrm{V}$, when a very thin pulse of intense negative magnetic force arrives, the intensity of the field being $ew\sin\theta/2a\,.\,\mathrm{OP}$, where θ is the angle between OP and the axis of z; the magnetic force previously at P was in the opposite direction, and equal to $ew\sin\theta/\mathrm{OP}^2$. This very intense pulse only lasts for a very short time; and the view I wish to put forward is that this pulse constitutes one kind of Röntgen radiation. The reasons for this view will be given after we have considered the case of a sphere moving with the velocity of light. We may, however, point out that since the state represented by $\alpha_1\beta_1$ lasts for the time r/V, while that for the state α_2, β_2 only for the time $2a/\mathrm{V}$,

$$\int\alpha\,dt=\int\beta\,dt=0.$$

This must evidently be the case, for the line integral of the magnetic force round a circuit is equal to 4π times the current through the circuit; in this case the currents are dielectric currents, and equal the rate of increase of the electric displacement through the circuit, so that the time integral of the line integral is equal to the change in the displacement; but when we neglect w^2/V^2, the distribution of the displacement is the same when the sphere is moving as when in the steady state at rest: thus the time integral must vanish.

Let us now consider the case when the velocity of the particles is nearly equal to that of light. In the limiting case when $w=V$ we see, from the expressions given for α, β, that they vanish unless $z=0$, when they become infinite; in this case the original magnetic field is confined to a plane through the centre of the sphere at right angles to its direction of motion. When w is nearly but not quite equal to V, the disturbance is practically confined between the two cones whose semi-vertical angles are $\frac{\pi}{2}-\vartheta$ and $\frac{\pi}{2}+\vartheta$, where ϑ is a small angle. To simplify the analysis and yet retain the essential physical features of the case, we shall suppose that the initial disturbance, instead of being confined between these two cones, is confined between the planes $z=+d$ and $z=-d$, where d is a small quantity; and that both the magnetic force and the electric intensity are parallel to the planes, the lines of electric intensity being radial at right angles to the axis of z, and the lines of magnetic force circles with their centres on the axis of z. Let E be the electric intensity at a point distant ρ from this axis; then the total normal induction over the surface of a cylinder passing through this point and with the axis of z for its axis is equal to

$$E \times 2\pi\rho \times 2d,$$

this must equal $4\pi e$; hence

$$E=\frac{e}{d\rho}.$$

Hence if α, β are the components of the magnetic force just after the particle is stopped,

$$\alpha=-\frac{Vey}{d\rho^2},$$

$$\beta=\frac{Vex}{d\rho^2},$$

$$\frac{d\alpha}{dt}=-V\frac{d}{dz}, \qquad \frac{d\beta}{dt}=-V\frac{d\beta}{dz}.$$

Both $d\alpha/dt$ and $d\beta/dt$ are zero except when $z=\pm d$, when they are infinite.

These equations give the initial state of the field outside the charged particle; inside this particle, which we shall take to be a sphere of radius d, we shall suppose that the electric intensity and the magnetic force both vanish.

Thus the original distribution of the field is confined between two parallel planes; and from this space we must exclude that inside the sphere as this is free from magnetic force.

Let us now consider how this distribution will spread through space. Consider what will happen at a point P. There will be no effect at P until a sphere of radius Vt and centre P cuts the space between the planes. This will not happen until $t=(c-d)/\mathrm{V}$, where c is the distance of P from the plane through the centre of the sphere perpendicular to the direction in which the sphere was moving before it was stopped. When t is greater than this value, the sphere will cut the space between the planes; and to apply Poisson's solution we have to find the mean value of the magnetic force over the surface of this sphere. Take the plane of xz to pass through P. Let Q be a point on the surface of the sphere, dS an element of the area of this surface, ϕ the angle the plane through Q and the axis of z makes with the plane of xz, ρ the distance of Q from the axis of z, and θ the angle between ρ and the normal to the sphere at Q; then the element of the surface included between z and $z+dz$, ϕ and $\phi+d\phi$ is given by the equation

$$d\mathrm{S}=\rho\frac{d\phi\,dz}{\cos\theta}.$$

Now initially

$$\beta=\frac{e\mathrm{V}\cos\phi}{\rho d};$$

so that

$$\beta d\mathrm{S}=\frac{e\mathrm{V}}{d}\frac{\cos\phi}{\cos\theta}\,d\phi\,dz.$$

Now if a is written for Vt the radius of the sphere, and if the x coordinate of P is b, then we may easily prove that

$$\cos\theta=\frac{1}{a}\sqrt{a^2-(z-c)^2-b^2\sin^2\phi};$$

hence

$$\beta d\mathrm{S}=\frac{ea\mathrm{V}}{d}\frac{\cos\phi\,d\phi\,dz}{\sqrt{a^2-(z-c)^2-b^2\sin^2\phi}}.$$

The limits of ϕ are

$$\pm\sin^{-1}\frac{\sqrt{a^2-(z-c)^2}}{b}=\pm\sin^{-1}\vartheta,\text{ say,}$$

In finding the mean value of β over the sphere we must double this value, for to each value of ϕ and z there correspond two elements of the surface of the sphere which contribute equally to the integral; hence

$$\int \beta d\mathrm{S} = 4e\frac{a\mathrm{V}}{d}\int\int_0^{\sin^{-1}\vartheta} \frac{\cos\phi\, d\phi\, dz}{b\sqrt{\sin^2\vartheta - \sin^2\phi}}$$

$$= 2\pi e\mathrm{V}\frac{a}{b}\int dz.$$

Now the limits of z depend upon whether the sphere does not or does cut right through the slab between the two parallel planes; in the former case $\mathrm{V}t$ is less than $c+d$, and the limits of z are $c-\mathrm{V}t$ and d; then

$$\int \beta d\mathrm{S} = 2\pi e\frac{\mathrm{V}a}{db}(d-c+\mathrm{V}t);$$

in the latter case $\mathrm{V}t$ is greater than $c+d$, and the limits of z are $-d$ and $+d$; hence in this case

$$\int \beta d\mathrm{S} = 2\pi e\frac{\mathrm{V}a}{db}\,2d.$$

Hence ω_1, the mean value of the initial value of β over the surface of this sphere, is

$$\frac{1}{2}\,\frac{e\,.\,\mathrm{V}}{bd}\,\frac{\{d-c+\mathrm{V}t\}}{\mathrm{V}t}$$

in the first case, and

$$e\frac{\mathrm{V}}{b\,.\,\mathrm{V}t}$$

in the second; hence

$$\frac{d}{dt}(t\omega_1) = \frac{1}{2}\,\frac{e\mathrm{V}}{db} \text{ or } 0,$$

according as $\mathrm{V}t <$ or $> c+d$.

This value of $\frac{d}{dt}(t\omega_1)$ is the same whether the point P is in front or behind the plane.

We now proceed to find the value of

$$\int \frac{d\beta}{dt}d\mathrm{S} = -\mathrm{V}\int\int \frac{d\beta}{dz}\cdot\frac{\cos\phi\, d\phi dz}{\cos\theta}.$$

Now $d\beta/dz$ is zero except at the surface of the plane; hence when the sphere cuts $z=d$ and not $z=-d$, we have

$$\int \frac{d\beta}{dt} d\mathrm{S} = \mathrm{V} \int (\beta_{z=d}) \frac{\cos\phi \,.\, d\phi}{\cos\theta}$$

$$= 2e \frac{\mathrm{V}^2 a}{bd}.$$

When the sphere cuts both $z=d$ and $z=-d$, then

$$\int \frac{d\beta}{dt} d\mathrm{S} = 0.$$

Thus ω_2, the mean value of the initial value of $d\beta/dt$ over the surface of the sphere, is given by the equation

$$t\omega_2 = \frac{1}{2}\frac{e\mathrm{V}}{bd}, \text{ when the sphere cuts } z=d \text{ and not } z=-d,$$

$$= -\frac{1}{2}\frac{e\mathrm{V}}{bd} \text{ when the sphere cuts } z=-d \text{ and not } z=d,$$

$$= 0 \text{ when the sphere cuts both.}$$

Hence by Poisson's formula

$$\beta = \frac{e\mathrm{V}}{bd} \text{ when the sphere cuts } z=d \text{ and not } z=-d,$$

$$= 0 \text{ when the sphere cuts } z=-d \text{ and not } z=d,$$

$$= 0 \text{ when the sphere cuts } z=d \text{ and also } z=-d.$$

Thus the distribution of magnetic force between the planes $z=\pm d$ is propagated forwards unchanged with the velocity V, there is no corresponding pulse propagated in the negative direction.

In addition to the plane pulse there will also, as in the previous case, be a spherical one, whose thickness is $2d$; we can calculate the magnetic force at any point in this pulse as follows:—Let H be the magnetic force at a point in this pulse at a distance b from the axis of z, then the line integral of this magnetic force round the circle whose radius is b and whose axis is the axis of z is $2\pi b\mathrm{H}$; the magnetic force lasts for a time $2d/\mathrm{V}$, so that the time integral of the line integral is $4\pi bd\mathrm{H}/\mathrm{V}$.

At any point in front of the particle the time integral of the magnetic force due to the plane pulse round the same circuit is

$$2\pi b \frac{e\mathrm{V}}{bd} \times \frac{2d}{\mathrm{V}} = 4\pi e.$$

Hence the time integral of the whole magnetic force round this circuit is equal to

$$4\pi b\mathrm{H}\frac{d}{\mathrm{V}}+4\pi e.$$

This is equal to 4π times the change in the electrostatic polarization through the same circuit: now when the particle was stopped, this polarization was zero, and when the field has reached a steady state, the electric intensity is uniformly distributed, so that the polarization through the circle is

$$\frac{e}{2\pi}(1-\cos\theta),$$

where θ is the acute angle between OP and the axis of z, P being a point on the circumference of the circle; hence

$$4\pi b\mathrm{H}\frac{d}{\mathrm{V}}+4\pi e=2\pi e(1-\cos\theta),$$

or

$$\mathrm{H}=-\frac{1}{2}\frac{\mathrm{V}e\cot\frac{\theta}{2}}{rd},$$

where r is the distance OP. The minus sign denoting that the magnetic force in the spherical pulse is in the opposite direction to that in the plane pulse.

At a point behind the charged particle there is no plane pulse, so that

$$4\pi b\mathrm{H}'\frac{d}{\mathrm{V}}=-2\pi e(1-\cos\theta'),$$

where θ' is the acute angle between OP and the axis of z; thus

$$\mathrm{H}'=-\frac{1}{2}\frac{\mathrm{V}e\tan\frac{\theta'}{2}}{rd}:$$

hence if θ is the angle between OP and the positive direction of the axis of z, the magnetic force at *any* point in the spherical wave is given by

$$\mathrm{H}=-\frac{1}{2}\frac{\mathrm{V}e\cot\frac{\theta}{2}}{rd}.$$

Thus we see that the stoppage of a charged particle will give rise to very thin pulses of intense magnetic force and electric intensity; when the velocity of the particle is small there will be one spherical pulse; when the velocity is nearly equal to that of light there will in addition to the spherical pulse be a plane one propagated only in the direction in which the particle was originally moving. It is these pulses which I believe constitute the Röntgen rays. As they consist of electric and magnetic disturbances, they might be expected to produce some effects analogous to those of light. If they were so thin that the time taken by them to pass over a molecule of a substance were small compared with the time of vibration of the molecule, there would be no refraction, and the thinness of the pulse would also account for the absence of diffraction.

In the preceding investigation we have supposed that the stoppage of the particle is instantaneous; if the impact lasts for a finite time T the negative pulse will be broadened out, so that its thickness, instead of being $2a$, will be $2a+\mathrm{VT}$, where V is the velocity of light. The intensity of the magnetic force in the pulse will vary inversely as the thickness of the pulse, so that when the collision lasts for the time T, the magnetic force in the negative pulse will be $2a/(2a+\mathrm{VT})$ of the value given above. The more sudden the collision, the thinner the pulse and the greater the magnetic force and the energy in the pulse; the pulse will, however, possess the properties of the Röntgen rays until T is comparable to one of the times of vibration of a substance through which it has to pass. In the case of the cathode rays all the circumstances seem favourable to a very sudden collision, as the mass of the moving particles is very small and their velocity exceedingly great. In some experiments which I described in the Philosophical Magazine for Oct. 1897 on cathode rays, the velocity of the negative particles was about one third of that of light, and in some more recent experiments made on the Lenard rays, with the apparatus described by Des Coudres, considerably higher velocities were found. A change in the time of the collision will alter the thickness of the pulse and so change the nature of the ray.

If we suppose that part of the absorption of the rays is due to the communication of energy to charged ions in their path, we find that the thicker the pulse the greater the absorption. For suppose that E is the electric intensity in the pulse, m the mass, and e the charge on an ion; then if u is the velocity communicated to the ion when the pulse passes over it, t the time taken by the pulse to pass over it,

$$mu=\mathrm{E}e\,.\,t,$$

or if d is the thickness of the pulse

$$mu=\mathrm{E}e\,.\,\frac{d}{\mathrm{V}};$$

thus the energy $\frac{1}{2}mu^2$ communicated to the ion is equal to

$$\frac{1}{2}\frac{E^2d^2e^2}{V^2}.$$

Now the energy in the pulse is proportional to E^2d/V^2, so that the ratio of the energy communicated to the ion to the energy in the pulse is proportional to d. Thus the broader the pulse, the greater the absorption and the less the penetrating power. The energy in the pulse is inversely proportional to its thickness.

If we return to the expression for the intensity of the magnetic force in case (1), we see that it is proportional to $\sin\theta$, so that the disturbance is greatest at right angles to the cathode rays: thus, if the cathode particles are stopped at their first encounter, the Röntgen rays would be brightest at right angles to the cathode rays; if, however, as would seem most probable, the cathode particles had to make several encounters before they were reduced to rest, changing their direction between each encounter, the distribution of the cathode rays would be much more uniform. Experiments on the distribution of Röntgen rays produced by the impact of the cathode particles directly against the walls of the discharge-tube are, as Sir George Stokes has pointed out, affected by the much greater absorption of the oblique rays produced by the greater thickness of glass traversed by them. Experiments on rays produced by focus-tubes would give results more easily interpreted.

The result to which we have been led from the consideration of the effects produced by the sudden stoppage of an electrified particle, viz.: that the Röntgen effects are produced by a very thin pulse of intense electromagnetic disturbance, is in agreement with the view expressed by Sir George Stokes in the Wilde Lecture ('Proceedings of Manchester Literary and Philosophical Society,' 1897), that the Röntgen rays are not waves of very short wave-length, but impulses.

Cambridge,
Dec. 16, 1897.

XVIII. *A New Method of Measuring the Torsional Angle of a Rotating Shaft or Spiral Spring. By* FREDERICK J. JERVIS-SMITH, *M.A., F.R.S., Millard and University Lecturer in Mechanics, Oxford*[*].

WHEN an elastic shaft is used to transmit motion, the power transmitted can be found, when the number of rotations in unit time and the torsional couple of the shaft

---

[*] Communicated by the Author.

are known. Thus, suppose E to denote the HP transmitted, N the number of rotations of the shaft per minute, the work done per minute in inch-lbs. is $12 \times 33000 \times E$; and this equals the twisting moment T in statical inch-lbs., multiplied by the angular motion of the shaft $2\pi N$ per minute, *i. e.*

$$2\pi NT = 12 \times 33000 \times E,$$

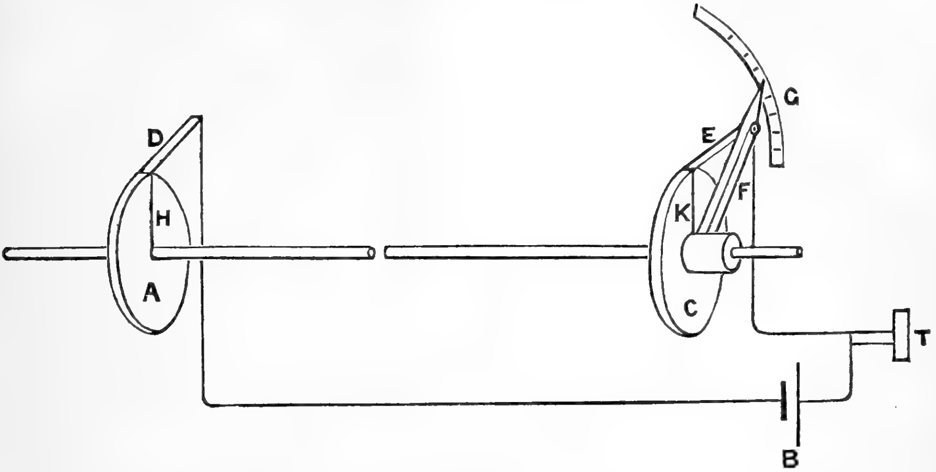
$$\therefore \quad E = \frac{2\pi NT}{12 \times 33000}.$$

To find E, N and T must be known. When variation in rotation is but slight, N may be found by means of a counter such as that of Harding, T then remains to be determined. In 1894, May 2nd, the writer of this paper exhibited at the Royal Society several models of ergometers, which showed how the torsion of a rotating shaft might be measured by mechanical and optical methods, and a description was given in a pamphlet entitled "A Torsion Ergometer or Work-Measuring Machine;" a rotostat was also exhibited, in which an inverting prism rotated in front of the index of the ergometer, brought it to rest, and made an easy reading possible although the shaft of the machine might be making any number of revolutions per minute. The author has been subsequently informed that a similar device was used by Lord Rayleigh for blending colours. [Reprint of some Optical Papers. Lord Rayleigh, 1883.] At the time of devising the rotostat the author did not know that a rotated inverting prism had been so used for blending colours or similar purpose.

Since 1894 the problem of measuring the torsion of a very long shaft arose, the shaft being used to drive a dynamo, and also the torsion of a solenoidal spiral spring used as a flexible shaft to drive screw-propellers of different forms, in water, at different depths.

While the apparatus required is of a simple character the results obtained are sufficient and reliable.

The method is as follows (see diagram) :—Two disks of insulating material A, C are fixed at the ends of the shaft, the bearings of which are not shown, narrow slips of copper H, K are attached to the disks at their circumferences, and to the shaft. Two brushes D, E (single flat wires answer well), press on the edge of the disks, the brushes are connected to an electric circuit, including a battery B, and a telephone T. Then when H and K are in the same plane, at each revolution of the shaft a click is heard in the telephone; but if while the shaft is rotating the disk C has an angular advance on the disk A, due to torsion, no click will be heard, since D and E do not touch the conductors H and K at the same instant.



Now, in order to hear the click again, the brush-holder F must be moved through an angle equal to that of the angle of advance of C on A, and this is the angle of torsion which is found. It is shown on a divided scale G.

The author has applied the method to find the torsional angle of an ergometer in which a solenoidal spiral spring is used instead of a shaft as shown in the figure.

The couple due to any angle is found by fixing A and suspending known weights from a thin steel tape attached to a pulley of known radius keyed on to the shaft close to the disk C.

XIX. *The Source of the* x-*Rays.*

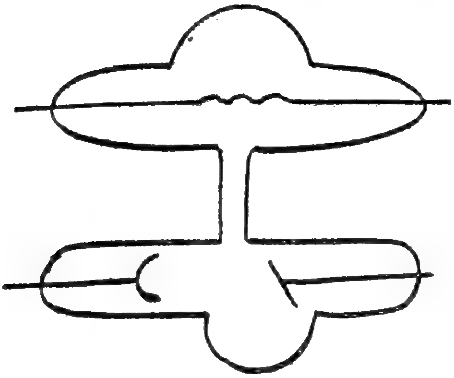
By JOHN TROWBRIDGE *and* JOHN E. BURBANK*.

THE experiments described in this paper were conducted with Crookes tubes containing no interval between the anode and the cathode: and no discharge, therefore, in the usual sense occurred in the tubes. A continuous conductor was led through the rarefied tube, and it was discovered that the x-rays were given off from every element of this conductor at right angles to its surface when a disruptive discharge occurred in the circuit of which the tube formed a part. This remarkable result was obtained by means of the very high electromotive force furnished by a Planté rheostatic machine which was charged by ten thousand storage-cells. We shall describe our experiments with the various tubes we employed, and in conclusion state our results.

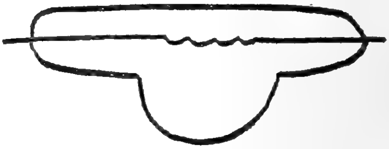
* Communicated by the Authors.

The first tube is shown in fig. 1. It consisted of a straight-wire tube joined to an ordinary Crookes tube of the focus pattern. This latter tube was joined to the straight-wire tube in order to test the vacuum in the latter, and to be sure

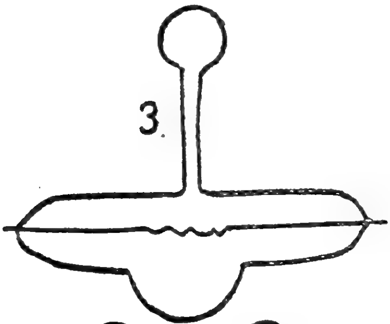
1



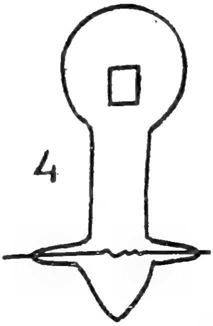
2



3

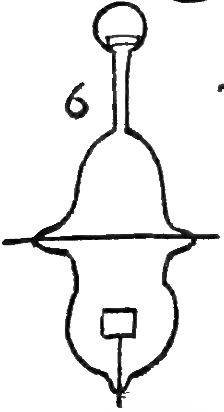


4

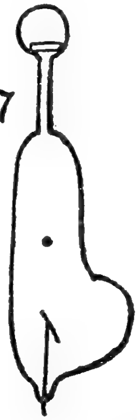


5

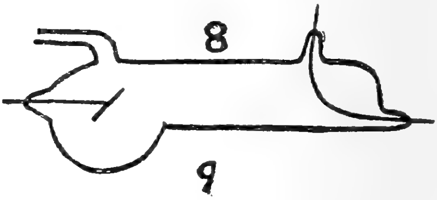
6



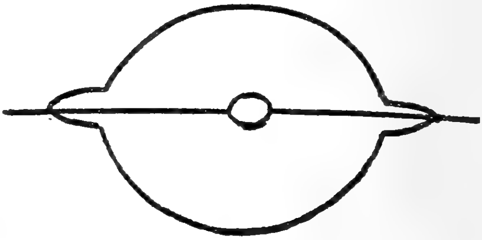
7



8



9



that the necessary conditions existed for the production of the *x*-rays. When the terminals of the straight-wire tube were connected to the Planté machine and the latter was excited, the entire tube fluoresced brilliantly. This fluorescence was especially bright in the connecting tube between the straight-wire tube and the Crookes tube, and a beam of light passed across the Crookes tube and formed a fluorescent spot on its bulb.

Photographic plates were exposed opposite to the thin bulb on the straight-wire tube. These plates were carefully insulated from the ground, and were covered in one case with a sheet of hard rubber $\frac{1}{8}$ of an inch in thickness, and in another case by a sheet of glass about $\frac{1}{2}$ an inch in thickness. A powerful brush-discharge was seen in the dark room to pass from the thin bulb of the tube to the insulator which covered the plates ; and on developing the plates, they were found to be covered with star-like clusters surrounded by nebulous patches. It was evident that the brush-discharge had produced discharges at the surface of the dry plates, even through plates of glass $\frac{1}{2}$ an inch in thickness. Moreover, there was a general darkening of the surface of the plate which indicated the action of x-rays. No metallic objects could be placed upon the dry plates, for a powerful spark immediately passed to them and punctured the tubes. The darkening, however, was apparently diminished under strips of glass, although the effect of the brush-discharge masked the effect of the x-rays. Before the straight tube was exhausted powerful brush-discharges were given off at the ends of the tube. When the tube was exhausted, these brushes were much diminished, and were replaced by a powerful brush which came off from the straight wire through the thin bulb of the tube, and speedily punctured the latter when any object, even an insulator, was brought within six inches of the bulb. The most interesting result obtained with this form of tube was the production of the so-called x-ray burn, by means of the brush-discharge from its bulb. When the back of the hand was exposed to the brush-discharge, which assumed a peculiar forked nature in the dark room, a peculiar prickling sensation was experienced, and all the symptoms of the well-known x-ray burn developed. The skin, when examined under a microscope, exhibited an appearance similar to that shown by the photographic plate. There were centres of inflammation surrounded by regions of lesser degrees of burn. It seems evident that the so-called x-ray burn is due to an electrification—a discharge at the surface of the skin—and this electrification may or may not be accompanied by the x-rays. This first form of tube was then abandoned, and a straight-wire tube alone (fig. 2) was employed. Similar results were obtained with this tube. It was significant that the whole interior of this tube fluoresced brilliantly when it formed part of a circuit through which a disruptive discharge passed. This latter form of tube was replaced by the form represented in fig. 3. A side tube ending in a thin bulb was added to the straight-wire tube. The same phenomenon was exhibited by this tube; in addition, a brilliant fluorescence filled the side tube, which appeared to flow in or flow out of the narrow tube which ended in the small bulb. We were reminded by this phenomenon of Poynting's hypothesis of the flow of energy into a wire. This form of tube was replaced by a straight-wire tube which is represented in fig. 4 and fig. 5 (side view). A straight wire passed completely through the tube, and was in circuit with a line on which there was a rapid change of potential. At one end of this tube opposite a thin bulb blown on the tube was a piece of platinum foil inclined like the ordinary focus-plane in a focus-tube. At first we connected this focus-plane with the ground, and having ascertained that *x*-rays were given off very strongly from this platinum, we removed the ground connexion and substituted for it a sheet of zinc. The tube still gave off *x*-rays. We then removed the sheet of zinc and found that *x*-rays were given off with undiminished strength. This tube was then modified into the form represented in fig. 6, fig. 7 (side view). The straight wire occupied the middle of a tube; in one end of this tube was placed a focus-plane of platinum, and at the other there was a long narrow tube which ended in a thin bulb. In this bulb was a crystal of calcite, which was confined in the tube by the narrowness of the bore of the connecting tube. The tube showed that *x*-rays were given off at right angles to the straight wire, for the calcite fluoresced a brilliant red, and the fluoroscope showed *x*-rays proceeding from the inclined piece of platinum. This form of tube, moreover, showed that the *x*-rays are reflected, so to speak, from the interior surface of the glass, for there were multiple shadows of the wire on the sides of the tube which were produced by the *x*-rays of varying intensity which were developed on the surface of the glass, and which in turn proceeding from this surface at different angles produced elongated shadows. In the next form of tube the wire passing through the tube was no longer straight, but was bent in the manner represented in fig. 8. In this form of tube there was a brilliant caustic formed on the side of the tube opposite the concave side of the wire, and this brilliant caustic threw distorted shadows of the bent wire on the opposite side of the tube. It was evident that the *x*-rays were given off at right angles to the surface of this wire, and therefore coincided in direction with the lines of electrostatic force.

We next experimented with the form of tube represented in fig. 9. This consisted of a large thin bulb 5 inches in diameter, enclosing a continuous conductor, the centre of which consisted of an aluminium mirror. When this tube was exhausted to a very high degree, the mirror formed a bright fluorescent spot on the bulb, the position of which could be readily changed by means of a magnet. When the exhaustion was carried to a very high degree, reversing the current from a Ruhmkorf coil through the tube caused no marked difference in the appearances in the tube; at a lower degree, however, a marked difference resulted. With the employment of a powerful Ruhmkorf coil giving sparks of at least 8 inches x-rays could be detected in this tube; and when the tube was connected to the Planté machine, the x-rays gave strong effects in the fluoroscope.

In order to test the question whether the so-called cathode rays and the x-rays are generated primarily only at the cathode, a very large resistance of distilled water was interposed in the circuit with the continuous wire tube (fig. 9), in order to damp any oscillations which might arise. The circuit thus consisted of the tube, the water-resistance, a spark-gap, and the secondary coil of a large Ruhmkorf. The tube was connected at first permanently to the air-pump. As the exhaustion advanced a beam of rays proceeded from the mirror on the continuous conductor, which was focussed on the wall of the tube. This beam was more brilliant and produced a stronger fluorescence on the tube when the wire was negative than when it was positive. At a higher stage of the vacuum, however, very little, if any, difference could be detected in the appearance of the tube, and x-rays could be detected outside the tube opposite the fluorescent spot caused by the mirror. That is, the x-rays were given off both when the wire constituted the cathode of the circuit and also when it formed the anode.

It seems, therefore, that the term cathode-rays is not a general one. It would seem that electric rays might be a more comprehensive one for both cathode-rays and x-rays.

Furthermore, the phenomenon of electrostatic induction plays an important part in the phenomena of the so-called x-rays. When the tube represented in fig. 9 had reached a certain stage of exhaustion, a bit of tinfoil connected to a zinc plate 20 × 25 cm. and 1 mm. thick was stuck upon the outside of the tube where the mirror formed the fluorescent spot. This zinc plate was carefully insulated from the ground. It was seen that a bundle of rays was reflected by the tinfoil to the opposite wall of the tube, throwing a well-defined shadow of the mirror and the continuous conductor on this wall. The direction of this shadow could be changed at will by changing the position of the tinfoil. This phenomenon was produced both when the wire was the cathode and when it was the anode. It can be explained on the hypothesis that a layer of electrified particles is held by a condenser-action on the wall of the tube, and that the fresh-coming particles are strongly repelled by those that have accumulated at the spot.

The behaviour of aluminium toward the *x*-rays is so remarkable that it merits especial investigation. Can it be that it manifests a remarkable condenser action toward the high electromotive forces which produce the *x*-rays, similar to the action which has been observed at lower voltages? *

We connected to the air-pump at the same time two exactly similar tubes, one of which had two pointed terminals of platinum, the other two pointed terminals also; but one consisted of aluminium and the other of platinum. The discharge from a Ruhmkorf coil was sent through these tubes, which were in multiple circuit. At a certain stage of the exhaustion it was seen that the discharge passed more easily when the aluminium wire was made a cathode than when it constituted the anode. When the wire terminals in both tubes were made of thin disks the difference was less marked. This might have been surmised from previous investigations on the effect of form of electrodes on resulting polarization †. It may be that the anomalous action of aluminium in respect to the *x*-rays is due to a species of dielectric polarization on the surface of the platinum, and that thus the surface becomes a new source of electrostatic stress similar to that which was observed by connecting a bit of tinfoil and a capacity to the tube. Since we are dealing with very high differences of potential, and with high charges on the ions, the instantaneous exhibition of electrical energy is very great, and might probably explain the diffusion of this energy through the air. According to this hypothesis the light manifestations of the *x*-rays arise only at the fluorescent screens, or at other suitable surfaces.

Conclusions.

1. A Crookes tube enclosing a continuous conductor is well suited, with the employment of high electromotive force, for the study of electric lines of induction.
2. The direction of the so-called *x*-rays and cathode-rays can be changed by electric induction.
3. The so-called *x*-ray burn can be produced by an intense state of electrification.

---

* L. Graetz, Wied. *Ann.* No. 10, 1897, p. 323.

† Karl Robert Klein, *ibid.* p. 259.

4. The so-called cathode-rays and x-rays are given off from every element of a continuous conductor at a high stage of the vacuum in a Crookes tube, both when this conductor constitutes the cathode and when it forms the anode of the electrical circuit. The term electric rays, possibly rays of polarization, would appear to be more comprehensive than the terms cathode-rays and x-rays.

Jefferson Physical Laboratory,
Harvard University, Cambridge, U.S.

4. 132

XX. *On the Photography of Ripples.—Second Paper.*
By J. H. VINCENT, *B.Sc., A.R.C.Sc.*[*]

[Plates XX.–XXII.]

IN a former paper[†], an apparatus was described by which the photographs accompanying the paper were obtained. In continuing the photographic method of recording experiments on ripples, the same principles have been employed. That is to say, the light which fell on the mercury surface was rendered parallel before incidence, and only that light which was reflected in a parallel beam was allowed to fall on the sensitive plate.

But in order more faithfully to reproduce the phenomena, it appeared that light of practically normal incidence should be employed. This was brought about by using only one large condensing lens, and by placing the first spark-gap (*i. e.* the one which furnishes the light to take the photographs) close to the brass mounting of the camera lens. The camera was thus pointing vertically downwards, and was so placed that the vertical projection of the lens-stop and the first spark-gap fell equidistant from, and on opposite sides of, the centre of the condensing lens.

The camera lens was provided with an iris diaphragm. The stop used was F 64 except where otherwise stated.

It was found that the time of duration of the spark was not unduly increased by the use of magnesium terminals. The focal lengths of the front achromatic lens of the camera combination and of the condensing lens were determined for blue light, and the large lens was placed at a calculated distance from the camera, so that the light from the spark-gap was brought to a focus on the centre of the lens-stop.

In selecting a position in the laboratory to set up the apparatus the desirable conditions are those of steadiness and

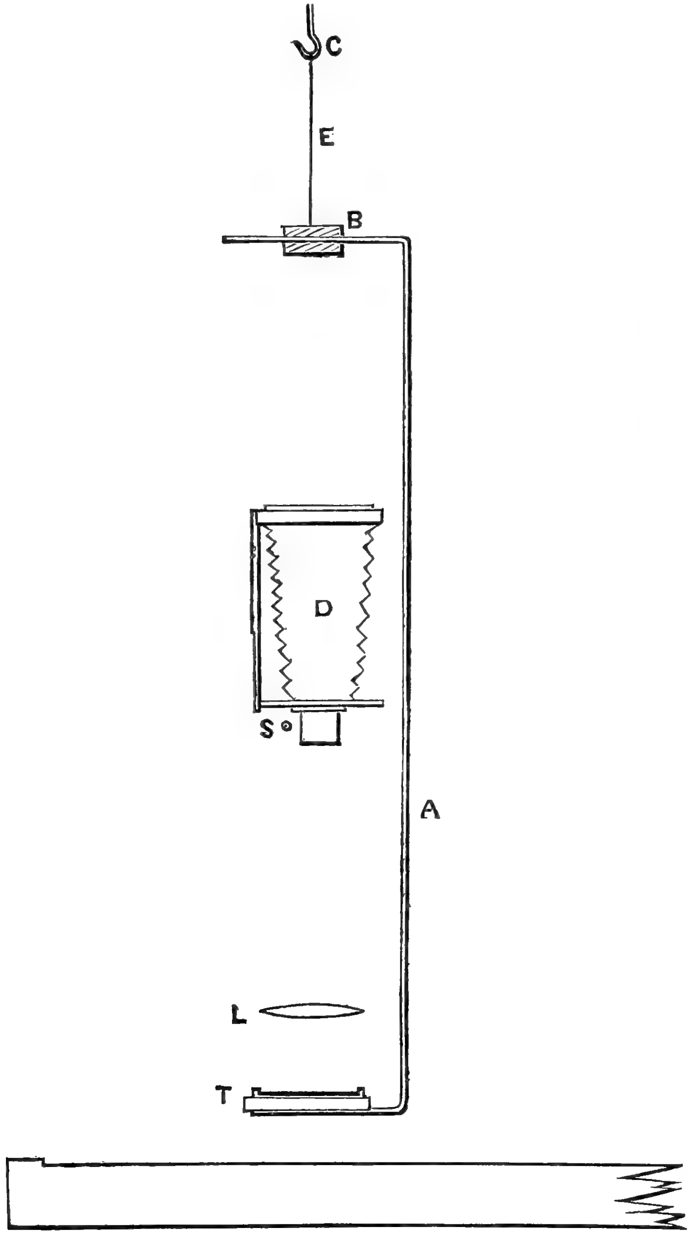
---

[*] Communicated by Prof. J. J. Thomson, F.R.S.

[†] Phil. Mag., June 1897; Proc. of Phys. Soc., July 1897.

dryness. The latter was sacrificed to obtain the former, and the apparatus was set up in a cellar with a concrete floor and bricked roof. The disadvantage of the induction-machine

Diagram of Apparatus.



$\frac{1}{15}$ nat. size.

T. Mercury trough.
A. Iron standard supporting T.
B. Wooden block.
C. Iron hook in the roof.
E. String.
S. First spark-gap.
L. Lens.
D. Camera.

giving trouble, owing to the damp, was amply compensated by the advantage of having a steady roof. This allowed of the suspension of the trough from the roof by a string; the annoyance due to the gradual stretching of a rubber cord was thus avoided.

Description of Photographs.

The figures are about $\frac{1}{2}$ natural size, with the exception of figs. 6 and 7 which are $\frac{3}{4}$ natural size.

A number of photographs were taken to test whether it was possible to avoid suspending the trough. Even when the motor and induction-machine were placed upon soft rubber bungs, the vibration was still so strong as to cause the surface of the mercury to appear furrowed in directions parallel to the sides of the trough; this was not much remedied by placing the trough on soft felt pads. When the motor was driven slowly, and the table on which the trough stood was agitated by the vibration of a fork, the phenomena became regular as shown in fig. 1.

Fig. 1.—Frequency 236.

The ripples due to the sides of the trough are of two kinds. The small ones are those caused by the fork and the large ones are caused by the shaking of the table by the motor and induction-machine.

Fig. 2.—The disturbance is here caused by stamping with the foot on the concrete floor on which the table stands.

Fig. 3.—Frequency 60.

The trough having been suspended, the mercury surface is now free from waves due to adventitious causes. The figure shows a point source and its image equidistant from the circular mirror.

The production of conjugate foci with ripples is a matter of some difficulty. (No example of conjugate foci was given in the first paper.) This is owing to a phenomenon which often accompanies the reflexion of ripples; a focus may appear in a position not in accordance with the laws of reflexion, or a normal focus may be accompanied by a number of points having the appearance of foci. This would occur if the waves on reflexion were split up into several sets of different wave-length, one set having the wave-length of the incident waves.

Fig. 4.—Frequency 60.

This illustrates the abnormal foci referred to above. There are two or perhaps three of these to the left of the normal focus. That the focus which is the most obvious is really the normal one is proved by its obeying the formula

$$\frac{1}{v}+\frac{1}{u}=\frac{1}{F}.$$

The calculated length of v on the negative was 3 99 cm. It actually measured 4·05 cm.

Fig. 5.—Frequency 120.

This is from a negative taken with the first form of apparatus. The source is placed at the centre of curvature of the circular mirror and we should expect to find no focus conjugate to the source. But there is a well-defined centre of a series of concentric circles ; this centre is distant from the mirror less than half the radius of curvature; no real source can have such a conjugate focus if the ripples obey the ordinary laws of reflexion. For a normal focus to be formed in this position it would be necessary to allow a series of circular ripples to impinge on the reflector in such a way that the centre of the contracting circles should be behind the mirror. The sides of the trough are rectilinear, and the virtual images of the source due to the sides of the vessel are outside the trough.

From figs. 1 and 2 we see that no casual trepidation is likely to cause curvilinear waves. Perhaps these spurious foci are positions of maximum disturbance consequent upon the interference of the ripples from all the real and virtual sources.

Fig. 6.—Frequency 236.

This is another illustration of conjugate foci. The reflector was accurately elliptical. It does not look so in the figure, owing to the whole of the enclosed area not being photographed, due to slight curvature of the surface near the boundary. The reflector was cut out of thin ebonite. The plate was floated in the mercury and was then buoyed up by underlying chips of wood until the mercury surface was as plane as could be arranged. If this is not attended to, little reflexion occurs at the edge.

The source can be distinguished by the style of glass, which shows as a black mark on the print. The image is approximately at the other focus of the ellipse. This is an interesting case of conjugate foci and reminds us that the principle of least time is really the principle of maximum or minimum time.

Fig. 7.—Frequency 236.

If the source is placed at the centre of the ellipse, the surface of the mercury becomes so furrowed by the large number of ripples going in different directions that it is, regarded as a whole, almost dark on the ground glass of the camera. The phenomena were also so complicated that no regularity was observable. By opening the lens-stop we may in effect smooth down the less violent ripples, and thus obtain a photograph which shows a regular diffraction-pattern. (Stop F 16 was used.)

The bright bands are lines of minimum disturbance produced by the interference between the direct waves from the centre and the reflected waves from the ends of the ellipse.

Two foci conjugate to the centre are seen near each end of the ellipse. Let us assume that the portions of the ellipse which are most concerned in deciding the position of these foci, are closely adjacent to the extremities of the major axis, and calculate the distance of the foci from the centre on this assumption. This distance is

$$a - \frac{ab^2}{2a^2 - b^2},$$

using the ordinary notation. From the known dimensions of the ellipse and the scale of reduction of the photograph (·765) this distance becomes 1·315 centim. On measuring the negative the value 1·30 centim. was obtained.

Fig. 8.—Frequency 236.

A comb of ten teeth was used to agitate the surface. The points were in the same straight line and equally spaced. The distance between the centres of the teeth at each end was 3·18 centim.

The print shows that such a comb behaves like a diffraction-grating. The length of a bar and slit on the negative is

$$\frac{3{\cdot}18 \times {\cdot}765}{9} = {\cdot}27 \text{ centim.}$$

The wave-length (on the negative) is ·105 centim. Thus the direction of normals to the rectilinear waves due to refraction is given by

$${\cdot}105n = {\cdot}27 \sin \theta_n,$$

or

$$n = 2{\cdot}57 \sin \theta_n,$$

where n is the order of the spectrum. Thus the greatest value n can have is 2; and we could not expect to find evidence of more than five sets of linear waves on the photograph. At a distance from the comb the central band can be made out. This corresponds to the undiffracted image one sees when looking through an ordinary grating. On either side of this band the first diffracted band is seen. The angle between the linear waves in these bands was measured with a protractor and found to be 49°. Thus $\theta_1 = 24{\cdot}5$. This is in fair accord with theory, which gives $\theta_1 = 23°$.

Fig. 9.—Frequency 236.

In this figure we have the analogue of a zone-plate in optics. The mercury was agitated by a card cut to represent a median section of a circular zone-plate. The central portion was 2 centim. long. This was bounded on each side by a space $\sqrt{2}-\sqrt{1}$. Then followed a length of uncut edge $\sqrt{3}-\sqrt{2}$ centim. long, and so on. It need hardly be remarked that the effect at the focus is not twice that of the uncut edge, as it would be if we were dealing with areal sources, as in light.

The focal distance as measured on the negative is about 5·5 centim., and λ measures ·115 centim. The half-periods are successively $\sqrt{\lambda d}$, $\sqrt{2\lambda d}$, &c.; whence

$$\cdot 765 = \sqrt{\lambda d},$$

or

$$d = 5\cdot 1 \text{ centim.}$$

Fig. 10.—Frequency 60.

The focal length of a zone-plate is less for red light than for blue. The corresponding effect is shown in this figure. (Stop F 22.)

Fig. 11.—Frequency 60.

In attempting to produce refraction of ripples many methods naturally suggest themselves. One of the most obvious is that of sending ripples from a deep to a shallow portion of liquid. But in order to considerably affect the velocity of propagation, it is necessary to have the depth of the shallow portion only a small fraction of the wave-length. Now mercury will not allow itself to be spread out on a surface it does not wet to a less depth than about 3 millim. This difficulty can be overcome by amalgamating the surface of a piece of metal and fixing it in the trough.

Experiments by this method have been unsuccessful. The ripples move onwards into the shallow region up to a certain depth without change of wave-length; then they are apparently quenched, and the very shallow parts appear quite unaffected by the disturbance in the rest of the liquid.

The figure shows another method of refracting ripples. The kinematic surface-tension and the effective value of gravity are both lowered when the surface is covered with water. A triangle of water was made on the mercury and kept in position with glass styles at the corners. The glass styles were attached to the sides of the trough.

Rectilinear waves are sent through the triangle and are bent towards the base on entrance and emergence. The effect is analogous to but very much less marked than in the case of light passing through a glass prism.

Fig. 12.—Frequency 60.

The three black circular patches are drops of water placed on the mercury near together, and such that their centres are on a normal to the waves, which are rectilinear before passing under the drops; but the portions of the waves which pass under the water are delayed, and on emergence a distinct bending is produced.

There is a white mark on each of the photographs of the drops. These are photographs of the virtual image of the spark in the convex water surfaces. This shows that the ripples pass only on the mercury, and do not disturb the water surface.

The Cavendish Laboratory,
Cambridge.

Erratum.

In the first paper on this subject (Phil. Mag. June, 1897) an error occurs in the description of fig. 5 (page 416). *For* "half" *read* $\frac{1}{2\sqrt[3]{2}}$. This fraction equals $\frac{1}{2\cdot520}$ to four significant figures. The fraction obtained from measurements of the negative is $\frac{1}{2\cdot503}$.

XXI. *Measurements concerning Radiation-Phenomena in the Magnetic Field.*—(I.) *By* Dr. P. Zeeman*.

1. In § 12 of my paper on "Doublets and Triplets"† I mentioned already that I was occupied in photographing the spectrum of a source of light placed in a magnetic field. However, it was not until October last that good photographs of the characteristic phenomena were obtained; some specimens were shown at the October meeting of the Amsterdam Academy‡. In order to study further quantitatively the radiation-phenomena in the magnetic field I have made the following measurements. For purposes of measurement there are several advantages in photographing the outer components of the magnetic triplets, quenching the light of the middle line by means of a nicol. By this latter device§ we also are enabled to measure accurately the magnetic effect in lines not entirely separated into triplets.

---

[*] Communicated by Dr. Oliver J. Lodge, F.R.S.

[†] Phil. Mag. xliv. pp. 55, 255 (1897).

[‡] *Cf.* 'Nature,' vol. lvii. p. 192. That it is not very easy to photograph the characteristic phenomena is shown by a paper in 'Nature' (vol. lvi. p. 420). The authors obtained only negative results. Mr. Preston, working apparently with the same grating, has quite recently succeeded in actually photographing all the appearances I have described. ('Nature,' vol. lvii. p. 173.)

[§] And also by the method of Cornu (*C.R.* Oct. 18, 1897) and König (Wied. *Ann.* Bd. lxii. p. 240, 1897).

I have measured negatives, obtained by means of a Rowland grating, using electrodes of various metals and a very strong field. The measurements, however, only refer to a relatively small part of the spectrum. I regret that it is not greater. Much time was lost because the grating was not mounted in the manner of Rowland, the various parts of the apparatus not being ready in time. Grating and camera-box were placed upon separate stands; it was now necessary to find the position of slit, grating, and camera-box for each part of the spectrum by a laborious process of error and trial. The dimensions of the camera-box permitted only the use of plates 13 or 18 centim. long. The results of this paper are hence to be regarded only as preliminary. I intend to go over the measurements once more and to make negatives of the whole spectrum, using a larger grating and the new apparatus, just now finished.

2. Measurements of the distance between the outer components of the triplet will also put to test the possible hypothesis, that it is only one kind of ions, having charges proportional to their effective masses, that are free to vibrate in the atom or molecule. On such an hypothesis, in a definite field the intervals between the outside components of the triplets should be (measured in change of wave-length) proportional to the square of the wave-length or (measured in difference of frequency) the same for all lines of the spectra of all substances*. The measurements communicated prove that for different substances the magnetic change is at least *of the same order* of magnitude, and certainly not directly dependent upon the atomic weight. The variation of the phenomenon with wave-length, however, seems more complicated than follows from the hypothesis mentioned, and in fact in Lorentz's theory there is no *à priori* reason to be given for its probability†.

3. The Rowland grating used was one of radius 6 feet and 14,438 lines to the inch. The second spectrum was used in most cases. With a medium-sized induction-coil and one large leyden-jar twenty minutes exposure was sufficient in the most sensitive part of the spectrum. Pure metals were used as electrodes. In each position of the apparatus always two (and often several) negatives were taken, one with and one without the magnetic field. The plates used were Cadett and Neal and Marion instantaneous; they were developed with hydroquinone. In some photochemical difficulties Dr. Ernst Cohen was kind enough to give me his assistance.

---

* Becquerel, *C. R.* Nov. 8, 1897; Larmor, Phil. Mag. vol. xliv. p. 503, § 4 (1897).

† Lorentz, Wied. *Ann.* Bd. lxiii. p. 278 (1897); Larmor, *l. c.* p. 506.

The lines on the plates were identified by comparison with the solar spectrum. The negatives were measured on an ordinary dividing-engine, with a microscope reading directly to $\frac{1}{100}$ of a millimetre, or by estimating the tenths of divisions on the head to $\frac{1}{1000}$ of a millimetre. The accuracy of the measurement of the interval between the components of course largely varies with the metal used as electrode. In a spectrum with sharp lines, for instance that of zinc or cadmium, this interval can be measured with an accuracy exceeding that of the magnetic measurements in our case. In the case of other substances, for instance copper or tin, the lines are so hazy, or of so small intensity, that it is only possible to make an estimation.

As an example of the accuracy obtained when magnetic triplets resulting from bright sharp lines are measured by means of the microscope of the dividing-engine, I will give some measurements concerning the line 4722 of the spectrum of zinc. The numbers in the following table are the readings ($\frac{1}{100}$ of a millimetre) on the head of a micrometer-screw when the first and the second component respectively was under the wire.

Determination of Distance between the Outer Components of the Triplet. (Zinc, $\lambda = 4722$.)

	Component I.	Component II.
	28·8	49·8
	28·8	48·7
	28·9	49·9
	28·2	49·5
	28·8	48·7
	28·2	48·9
Mean	28·6	49·3

Difference $= 20{\cdot}7 \times \frac{1}{100}$ millim.

The negative was taken in the second spectrum. On the negative 1 millim. corresponded to about 4·41 Ångström-units.

The following results were obtained concerning lines in the blue, violet, and ultra-violet part of the spectrum.

4. *Zinc.*—The following tables contain under λ the wave-lengths of the spectral lines. The meaning of the different columns will be clear from the headings. The intensity H of the magnetic field was measured by means of a bismuth spiral. There is a slight uncertainty in the value of H, the temperature-coefficient of the bismuth wire not being accurately known.

λ.	Distance between components in $\frac{1}{100}$ millim.	Scale of negative.	Order of spectrum.	H.
4811	18·6	1 millim.	2	$32 . 10^3$
4722	20·7	=4·41 A.U.		
4680	25·1			
3345	Imperceptible	1 millim.	3	$32 . 10^3$
3303	......	=2·94 A.U.		
3282	0			

It appears, as is also indicated in the table, that some lines are *not* changed under the influence of the field, or at least so little that the magnetic change, if it exists, is extremely small*. It deserves to be noted that between the first three lines in the table *clearly* showing the magnetic change, and the last three not showing it, there exists also another interesting difference. Indeed, the first are the group of three lines resulting from Kayser and Runge's *second* subordinate series for the value 3 of the coefficient n in their formula†. The second group of three lines follows from the *first* subordinate series, n being 4. Further inquiry must decide whether *all* lines of the first series are *not* influenced, only those of the second being changed.

5. *Cadmium.*—

λ.	Distance between components in $\frac{1}{100}$ millim.	Scale of negative.	Order of spectrum.	H.
4800	22·0	1 millim.	2	$32 . 10^3$
4678	24·2	=4·41 A.U.		

These are lines of Kayser and Runge's *second* subordinate series ‡, n being 3 (*cf.* zinc). The line 5086, also of this series, unhappily could not be measured, being situated too near the border of the negative.

6. *Copper.*—The lines of the copper spectrum are, at least between 4800 and 4300, so hazy that they could not be measured. A rough estimation of some lines gave about 30 hundredths of a millimetre for the distance between the components.

7. *Tin.*—

λ.	Distance between components in $\frac{1}{100}$ millim.	Scale of negative.	Order of spectrum.	H.
4585	±35	1 millim.	2	$32 . 10^3$
4525	Imperceptible	=4·46 A.U.		
4447	± ?			
4184	± ?			

---

* *Cf.* Lorentz, *l. c.* p. 284.

† Kayser u. Runge, Wied. *Ann.* Bd. xliii. p. 394 (1891).

‡ Kayser u. Runge, *l. c.* p. 399.

The lines of the tin spectrum are very feeble. With the magnetic field off, the lines indicated in the table all clearly appeared on the negative after an exposure of 15 minutes. Exposing, however, with the field on, only the first two lines were on the plate, notwithstanding that the time of exposure was prolonged to 23 minutes, the other circumstances being the same as before. Apparently the first and the last two lines undergo a magnetic change; the last lines (being the feeblest) did not impress their components on the plate. The components of 4585 were so feeble that they disappeared if looked at through the microscope. For this reason I have made a scratch at each of the components by means of a needle, and have measured the distance of these scratches.

Amsterdam, December 31, 1897.

XXII. *Notices respecting New Books.*

The 'Opus Majus' of Roger Bacon, edited, with Introduction and Analytical Table, by JOHN HENRY BRIDGES (vol. i. pp. 188+404, vol. ii. pp. 568). Oxford: Clarendon Press.

"ROGER BACON, l'un des plus puissants génies du Moyen âge, occupe le premier rang parmi les promoteurs de la renaissance générale des lettres et des sciences. Il contribua particulièrement aux progrès des Mathématiques en montrant, dans plusieurs de ses ouvrages (*Op. Maj.*: quatrième, cinquième et sixième parties), le rang qu'elles tiennent dans l'ensemble des connaissances humaines, et les secours qu'elles peuvent procurer dans toutes les recherches scientifiques, dont elles sont le fondement. Son Optique contient, comme tout le monde le sait, de savants aperçus et des découvertes réelles en théorie, et l'invention de plusieurs instruments devenus de la plus haute utilité. Ses connaissances en Astronomie lui firent reconnaître les erreurs du calendrier, dont il conçut la réformation. Le calendrier qu'il calcula, et qui est resté manuscrit, se distingue par sa correction et par l'usage des chiffres arabes, qui sont les mémes que ceux de Sacro Bosco." So writes Chasles in his *Aperçu* (p. 517), and the same verdict is pronounced by Hallam and other writers whom we have consulted.

"The 13th century, an age peculiarly rich in great men, produced few, if any, who can take higher rank than Roger Bacon. He is in every way worthy to be placed beside such thinkers as Albertus Magnus, Bonaventura, and Thomas Aquinas" (*Encyc. Britannica*). The same writer quotes Dühring to the effect that Bacon was not appreciated by his age because he was so completely in advance of it; he is a 16th or 17th century philosopher, whose lot has been by some accident cast in the 13th century; he is no schoolman, but a modern thinker, whose conceptions of science are more just and clear than are even those of his more celebrated namesake. With this agrees Hallam's remark, "the mind of Roger Bacon was strangely compounded of almost prophetic gleams of the future course of science, and the best principles of the inductive philosophy, with a more than usual credulity in the superstitions of his own time" ('Literary History,' pt. i. cap. ii.).

The present editor adduces the extreme rarity of Jebbs's edition (1733)—which was reprinted seventeen years subsequently in Venice—as a sufficient reason for this new edition. The *Introduction* contains a life of Bacon (pp. xxi–xxxvi), a discussion of Bacon's position in the metaphysical controversies of the 13th century (pp. xxxvi-xliii), an account of his "Scriptum principale" (pp. xliii–xlviii), of his Philology (pp. xlviii–liv), and of his Mathematics (pp. lv–lix). "It would seem that Bacon had made himself acquainted with the highest mathematics of his time; though no evidence is forthcoming to show that he contributed personally to the advance of the science, otherwise than by strongly insisting on its culture, and by pointing out new fields for its practical application, in the better government of the Church, and in the development of industry. His interest, like that of Galileo, lay in applied rather than in abstract mathematics." Bacon says "Scientiarum porta et clavis est Mathematica." The introduction further gives an account of Bacon's Astrology (pp. lix–lxv), of his views of the Propagation of Force (pp. lxv-lxix), of his Optics (pp. lxix–lxxiv), of his Alchemy, of his views on experimental science and on Moral Philosophy (pp. lxxix–lxxxviii), concluding with an account of the general characteristics of the *Opus Majus* (pp. lxxxviii–xcii). "It appears on the surface that Bacon belongs to the order of thinkers, typified by Pythagoras rather than by Aristotle, who engage in speculation, not for its own sake alone, but for social or ethical results, that are to follow.... In wealth of words, in brilliancy of imagination, Francis Bacon was immeasurably superior. But Roger Bacon had the sounder estimate and the firmer grasp of that combination of deductive with inductive method which mark the scientific discoverer. Finally, Francis Bacon was of his time; with Roger Bacon it was far otherwise."—Before the text, an account of which is given in the preface (pp. vii–xix), comes an excellent analysis of the *Opus Majus* (pp. xciii–clxxxvii.) This is of great utility for a study of the work. The editor's sufficient reason is stated above, he adduces as a more cogent reason the fact that Jebbs's edition is incomplete. Here we regret to find that reviewers who have had access to the MSS., some additional to those consulted by our author, join issue with him, and so far from pronouncing it to be the definitive edition which has been looked for, pronounce it to be "full of gross errors." We close with stating another good point, viz., that there is an Index at the end.

Applied Mechanics. *By* JOHN PERRY, *M.E., D.Sc., F.R.S., Professor of Mechanics and Mathematics in the Royal College of Science, London.* London: Cassell & Co., 1897.

IT may safely be said that no teacher of engineering is more anxious that the subject should be properly taught, and has given more attention to discovering better methods of instruction, than Professor Perry. The present treatise is therefore the result of great experience and much experiment in educational method, and as such it is addressed to the teacher as well as to the student of the subject. According to the author—and every teacher of any experimental science will, doubtless, agree with him—no course of instruction in applied mechanics can be considered complete or even satisfactory, which does not include a considerable amount of laboratory practice. Laboratory work will, however, have a far greater educational value if it be performed with apparatus possessing such imperfections as are present in actual machinery. The student should not only learn the simple laws of mechanics, but he ought to investigate for himself how, why, and to what extent these simple laws are departed from in practical contrivances. For example, the familiar Atwood's machine should be devoid of friction-wheels, and its pulley should be a heavy one; the student will then be able to study and allow for the effects of friction, and he will learn much concerning the dynamics of rotation. To teachers of physics this may appear at first sight quite heterodox, but it must be borne in mind that physical and engineering laboratories perform different functions; in the former natural laws are learned and verified, and the physical properties of substances are measured, while in the latter the student is taught the application of these laws and substances to the requirements of everyday life. The engineer may therefore assume the truth of his formulæ; it is the duty of the physicist and the mathematician to establish and prove them.

The author experienced difficulties in teaching applied mechanics on account of his students' ignorance of the rudiments of differential and integral calculus, and eventually was obliged to include these among the subjects of his course of lectures. We know of one college where the same difficulty is felt in connexion with the physics course, and doubtless there are other cases. Having regard to the increasing importance of science subjects and of applied mathematics, it appears very desirable that the teachers of mathematics in schools should be asked and encouraged to alter the order in which their subject is taught, so as to give an earlier and more prominent place to the elements of the calculus. The Science and Art Department might possibly assist matters by introducing the calculus into the syllabus of their examinations at some stage earlier than the sixth; the difficulty will, however, not be completely removed until those who direct school education recognize to a much greater extent the necessity for increasing the amount of time devoted by the scholars to mathematical studies.

The subjects treated in the volume cover a wide range, and it is full of suggestion both to teacher and student, while the characteristic manner in which the author gives advice and caution seems to bring the reader into personal contact with him, and completely distinguishes his treatise from any of the ordinary text-books. J. L. H.

15 *Lezioni Sperimentali su la Luce considerata come Fenomeno Elettromagnetico. By* A. Garbasso, *Lecturer in the University of Pisa.* Milan: Publishers of *L'Elettricità*, 1897.

In 1895 the author delivered a course of lectures in the University of Turin, on light considered as an electromagnetic phenomenon. The book before us contains an account of the experimental portion of these lectures; it deals with the researches of Hertz and his successors, and describes the principal experiments in the form in which they were repeated by the author in his lectures. The volume contains over 100 illustrations, the majority of which are of very poor quality and several almost unintelligible. The experiments are fairly well described, but the work suffers greatly by the omission of the non-experimental parts of the subject.

J. L. H.

XXIII. *Intelligence and Miscellaneous Articles.*

ON REAL AND APPARENT FREEZING-POINTS AND THE FREEZING-POINT METHODS. BY MEYER WILDERMANN, PH.D.*

Errata.

For solid solvend *always read* solidified solvent.
,, $C=0{\cdot}003^{\min^{-1}}$,, $C=0{\cdot}003\times 2{\cdot}3026^{\min^{-1}}$.
,, $K>6$,, $K>6\times 2{\cdot}3026$.
,, $C''=14$ or 15 ,, $C''=14$ (or 15) $\times 2{\cdot}3026$.

Page 465, line 37, for $\frac{dt}{dz}=K(t_o-t)$ read $\frac{dt}{dz}=K(t_o-t)(t_o-t_{ov})$.
,, 474, *for* $C(z_2-z_1)=\log(t_g-t_2)-\log(t_g-t_1)$ *read* $C(z_2-z_1)=\log(t_g-t_1)-\log(t_g-t_2)$.
,, ,, *for* $C(z_3-z_1)=\log(t_g-t_3)-\log(t_g-t_1)$ *read* $C(z_3-z_1)=\log(t_g-t_1)-\log(t_g-t_3)$.
,, 479, lines 26–32, *for* $3^\circ\min^{-1}$, $5^\circ\min^{-1}$ *read* $3^\circ\times 2{\cdot}3026\min^{-1}$, $5^\circ\times 2{\cdot}3026\min^{-1}$.
,, 480, *for* C′ *read* C″.
,, 481, line 34, *for* K, 5 or 6 *read* K, 5 (or 6) $\times 2{\cdot}3026$.
,, 483, in Table IV., *for* $\frac{C''(t_g-t')}{C(t_o-t_{ov})}$, $\frac{C''(t_g-t_g')}{C(t_o-t_{ov})}$ *read* $\frac{C(t_g-t')}{C''(t_o-t_{ov})}$, $\frac{C(t_g-t_g')}{C''(t_o-t_{ov})}$.
,, 485, line 11, *for* $C=0^\circ{\cdot}003$, $C(t_g-t')=0^\circ{\cdot}006$, *read* $C=0{\cdot}003$, $C(t_g-t')=0^\circ{\cdot}0006$.
,, ,, lines 13 and 14, *for* 1·25 per cent., $0^\circ{\cdot}004$ per cent., *read* $1{\cdot}25\times 2{\cdot}3$ per cent., $0{\cdot}004\times 2{\cdot}3$ per cent.
,, ,, line 18, *for* 12 to 36 *read* 6 to 18.
,, ,, ,, 21, *for* 36 to 110 *read* 18 to 55.
,, ,, ,, 28, *for* 150 *read* 75.
,, ,, ,, 1, *for* during 5 minutes of the experiment *read* during five minutes of the experiment after the freezing-point is reached.

---

* It was too late to make the corrections in the proof itself, so I send them now.

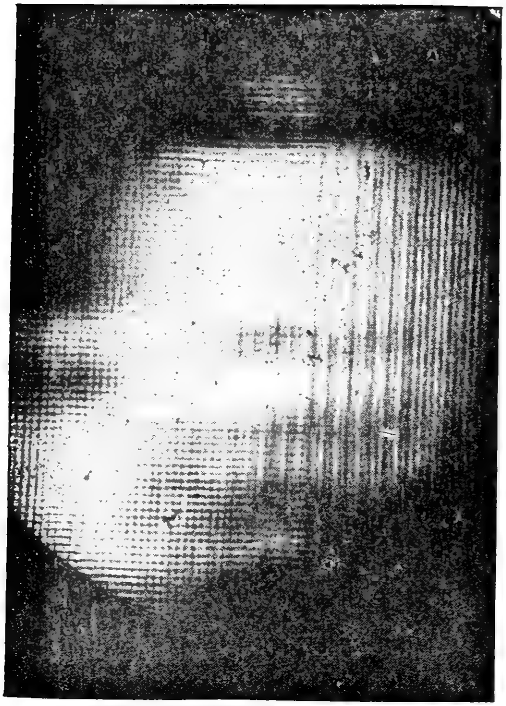


Fig. 1.

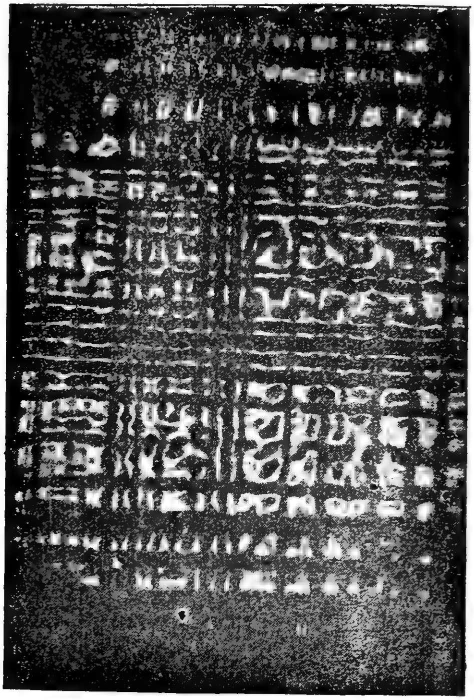


Fig. 2.

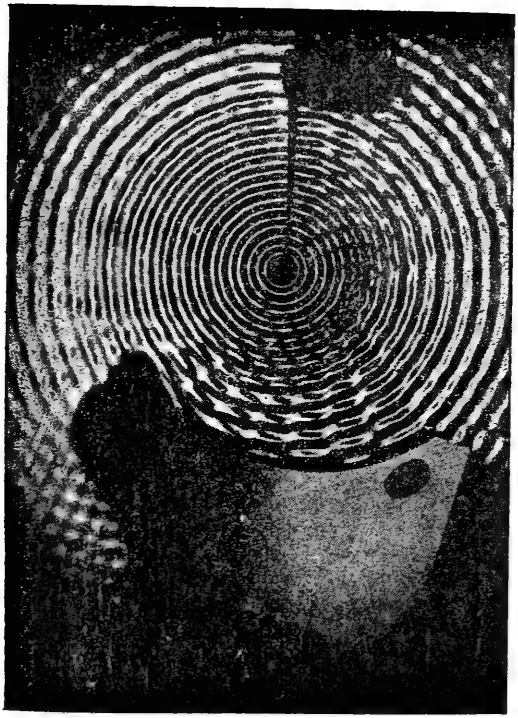


Fig. 3.

Fig. 4.

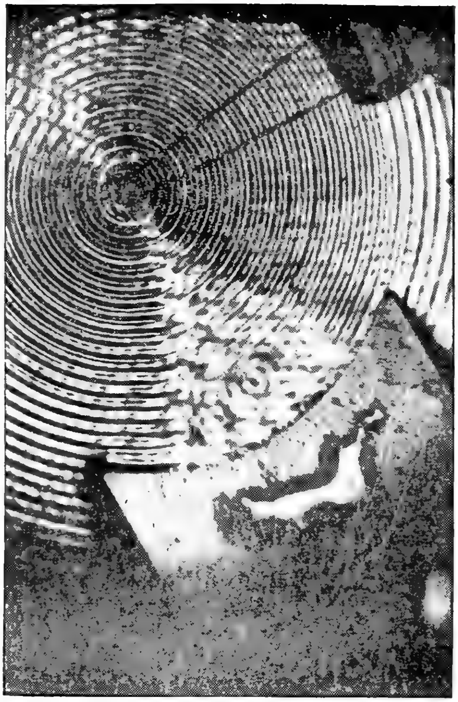


Fig. 5.

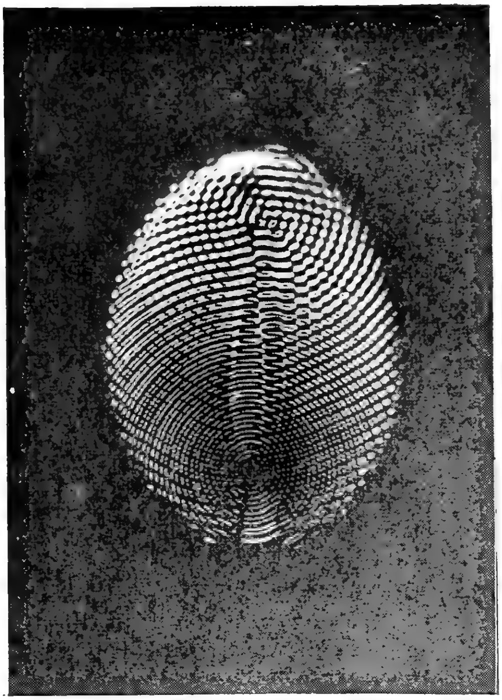


Fig. 6.

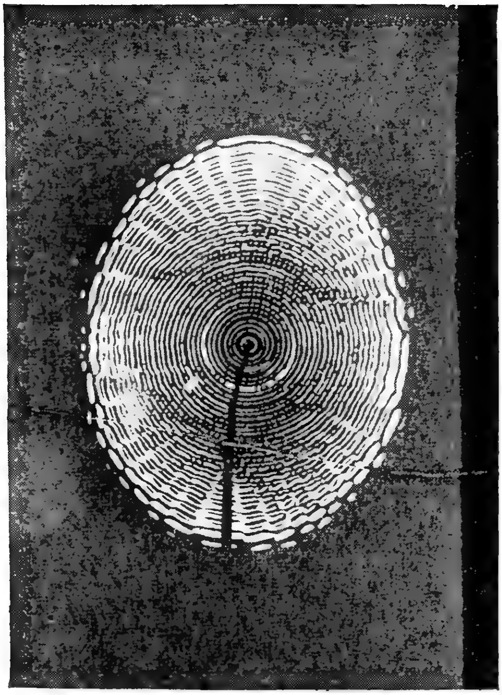


Fig 7.

Fig. 8.

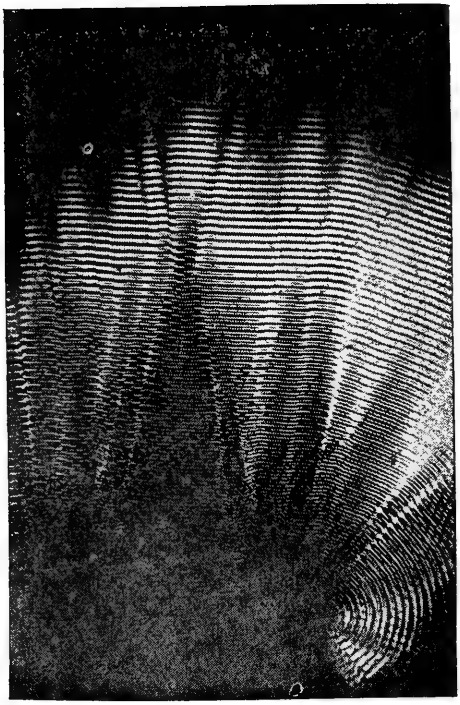


Fig. 9.

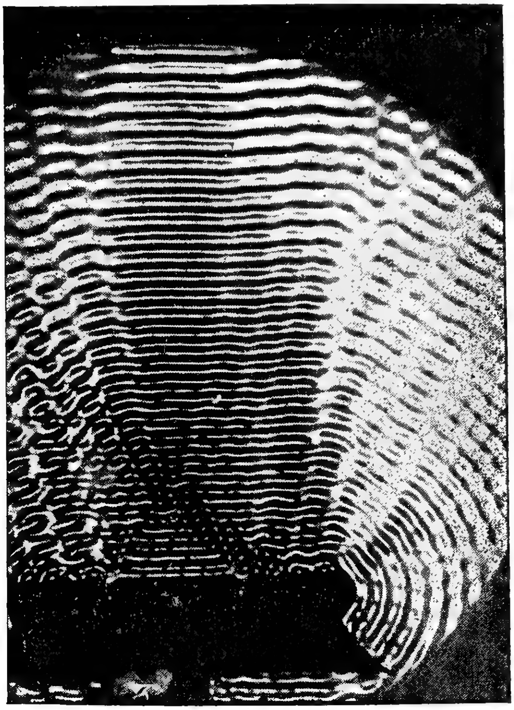


Fig. 10.

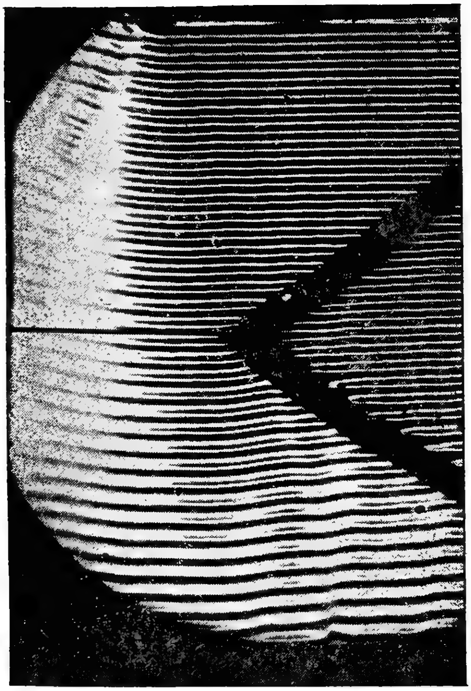


Fig. 11.

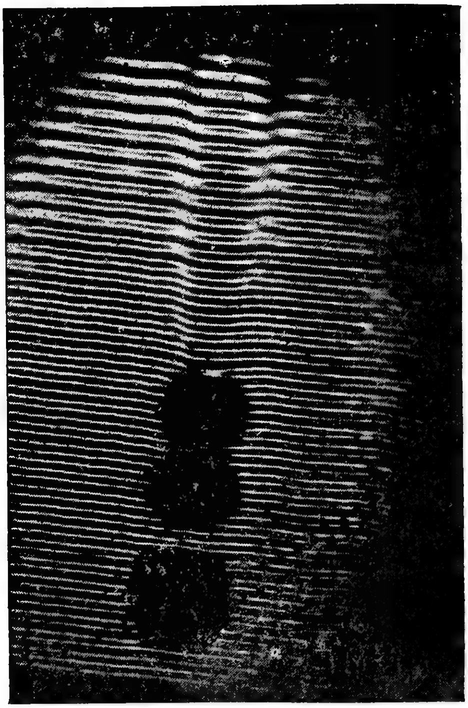


Fig. 12.

THE

LONDON, EDINBURGH, AND DUBLIN

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[FIFTH SERIES.]

MARCH 1898.

XXIV. *Notes on Thermometry.* *By* C. Chree, *Sc D., F.R.S.**

Occurrence of Constant Coefficients.

A_1, A_2, &c., in law of thermal expansion of mercury.
a_1, a_2, &c., " " " glass (fixed zero).
b_1, b_2, &c., " " " " (movable zero).
b_1', b_2', &c., " after-effect in glass.
d_1, d_2, &c., " depression of thermometers' zero with temperature.
$e_1 = A_1 - a_1$, &c.
e = mean coefficient of expansion of mercury in glass between 0° and 100° C.

Introductory.

§ 1. Of late years much has been done, especially in France and Germany, to increase the accuracy of the measurement of temperatures by means of mercury thermometers. An excellent account, with illustrations, of some of the more important practical results is given in Chap. II. of Waldo's 'Modern Meteorology'; but the scope of this work hardly allows of the subject being treated in detail except where meteorological questions are directly concerned. With this partial exception, I know of no

---

* Communicated by the Kew Observatory Committee.

English text-book where particulars of recent work may be found. The following discussion is intended partly to supply this want. Few of the subjects it deals with are novel, except in details; and several are discussed at much greater length in Dr. Guillaume's admirable work '*Thermométrie de Précision,*' in the publications of the Bureau International, and in those of the Phys. tech. Reichsanstalt at Charlottenburg. I am indebted to these sources for many of the details mentioned here. I have, however, adopted an independent standpoint, and have further considered the bearing of recent work on the methods in general use in this country.

The ideal mercury-thermometer is one which accommodates itself at once and completely to the temperature it is for the time being exposed to, and which when exposed to a given temperature supplies an invariable reading.

The departures from this ideal will be discussed presently, after we have considered the nature of the temperature-scale of an imaginary glass-mercury thermometer free from all the deficiencies.

§ 2. For the laws of thermal expansion of mercury and glass we may assume provisionally

$$V=V_0(1+A_1t+A_2t^2+\ldots), \quad . \quad . \quad . \quad . \quad (1)$$

$$v=v_0(1+a_1t+a_2t^2+\ldots). \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Here t denotes temperature on a standard scale, which we may suppose that of the hydrogen-thermometer of the Bureau International; V_0 is the volume at 0° C., and V at $t°$ C. of a given mass of mercury, A_1, A_2, &c., constants defining its expansion; v_0 is the volume at 0° C., and v at $t°$ C. of a given mass of glass, a_1, a_2, &c. expansion-constants *for the particular kind of glass.*

The *natural scale* of the thermometer corresponds to the case when its stem between the divisions 0 and 100—which answer to the freezing- and boiling-points of pure water under a standard pressure—is subdivided into 100 equal volumes. Degree divisions below 0° C. or above 100° C. should equal in volume those of the fundamental interval 0° to 100°. Suppose that t and $t+x$ are corresponding temperatures on the hydrogen and glass-mercury scales. At 0° C. the volume of the mercury is equal to the internal volume V_0 of the thermometer bulb, including the stem up to the zero mark. At temperature $t°$ C. the volume of the mercury is equal to that of the bulb and of $t+x$ stem-divisions. If then v_0 be the volume at 0° C. of one stem-division we have, referring to (1) and (2),

$$V_0(1+A_1t+A_2t^2+\ldots)=\{V_0+v_0(t+x)\}(1+a_1t+a_2t^2+\ldots). \quad (3)$$

When $t=100°$, x vanishes, thus

$$v_0/V_0=(e_1+100\,e_2+100^2e_3+\ldots)\div(1+100\,a_1+100^2a_2+\ldots), \quad (4)$$

where $e_1\equiv A_1-a_1$, $e_2\equiv A_2-a_2$, &c.

If both glass and mercury had a linear law of expansion, *i. e.* if all the constants except A_1 and a_1 were zero, (4) would become

$$v_0/V_0=e_1\div(1+100a_1), \quad . \quad . \quad . \quad . \quad . \quad (5)$$

giving as first approximation

$$v_0/V_0=e_1.$$

Substituting from (4) in (3) and reducing, we get

$$x=\frac{t(100-t)}{1+a_1t+a_2t^2+\ldots}[e_1\{a_1+a_2(100+t)+a_3(100^2+100t+t^2)+\ldots\}$$
$$-e_2\{1-100\,a_2t\ldots\}-e_3\{100+t+\ldots\}+\ldots]\div(e_1+100\,e_2+100^2e_3+\ldots). \quad (6)$$

The preceding formulæ are not of course legitimate for all values of t. The freezing- and boiling-points of mercury assign limits to the application of (3), and in the immediate neighbourhood of these points—especially of the boiling-point—its accuracy is somewhat uncertain. At ordinary temperatures the series on the right of (1) converges rapidly, A_1t being much the most important term. The glass of our present problem is ideal. There is perhaps no actual glass of whose expansion we know enough to judge whether it is expedient to retain any term higher than a_2t^2 in (2). In all ordinary kinds of glass a_1/A_1 is of the order 1/7, and so e_1 may be assumed necessarily positive. The sign of e_2 is more uncertain, and as regards e_3 and e_4 nothing is known except that the terms depending on them appear to be of little importance.

Supposing our ideal glass to resemble ordinary glass in the general laws of its expansion, $(1+a_1t+a_2t^2+\ldots)^{-1}$ is nearly unity and replaceable by a factor $(1-a_1t\ldots)$, proceeding in ascending powers of t. Thus (6) may be regarded as of the general type *

$$x=t(100-t)(B_0+B_1t+B_2t^2+\ldots), \quad . \quad . \quad . \quad (7)$$

---

* *Cf.* Guillaume's *Thermométrie*..., p. 195.

where the B coefficients vary with the nature of the glass. Doubtless (7) is convergent for the restricted values of t occurring in our problem; but it would not be safe to assume that B_0 is always the most important term. For instance, if we neglect all constants with suffixes greater than 2, we reduce (6) to

$$x=t(100-t)(1+a_1t+a_2t^2)^{-1}\left\{\frac{e_1(a_1+100a_2)-e_2}{e_1+100e_2}+a_2t\right\}, \quad (8)$$

and *conceivably* $e_1(a_1+100a_2)-e_2$ might be very small.

It is important to notice that even if mercury and glass had both a linear law of expansion, on the hydrogen scale, x would not vanish, but would be given as a first approximation by

$$x=a_1t(100-t). \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

This is a relation of the same form as that proposed by Callendar for the difference between the platinum-resistance and air-thermometer scales.

The divergences between actual glass-mercury and hydrogen scales depend partly on the departures of glass from our ideal; their discussion is thus postponed until the properties of glass have been dealt with.

Properties of Real Glass.

§ 3. The principal departure of real glass from our ideal is that its volume at a given temperature is not invariable. It is well known that if a mercury-thermometer employed at ordinary temperatures be tested at intervals in ice it usually reads higher as time progresses, *i. e.* its zero rises. Its behaviour is explicable on the hypothesis of a secular contraction of the bulb, becoming gradually slower as time elapses. By properly annealing the tubes, and storing them for several years before dividing them, this defect may be very largely if not entirely got rid of; and it can in any but a very new thermometer be easily allowed for in reducing observations. This change of zero, be it noticed, is believed to occur however uniform be the temperature at which a thermometer is kept.

When we expose a thermometer to changes of temperature a further defect presents itself, which for thermometry of the highest accuracy is much more troublesome. Within certain limits, the increase of volume in glass accompanying rise of temperature from, say, t to t' does not wholly disappear at once when the temperature is rapidly reduced to t. A thermometer taken through such a cycle reads lower on the second exposure to the lower temperature. This phenomenon is conspicuous when we compare ice-readings taken immediately before and after exposure to temperatures between 50° C. and 100° C.; the "depression of zero," as it is called, is greater the higher the previous temperature. Exposure for only a minute or two to a high temperature is only partially effective; but 20 or 25 minutes exposure usually produces practically the full effect. On the other hand, the depression takes a considerable time to disappear; it is a question of days or even weeks if the high temperature has approached 100° C.

Hitherto at the Bureau International attention has been mainly directed to a standard glass, French *verre dur*, at temperatures between $-30°$ C. and 100° C. At the Reichsanstalt certain Jena glasses, notably 16^{III} and 59^{III}, have been examined in similar detail. The conclusion reached for these glasses throughout at least this range, is that the difficulty can be met by treating a reading z_t in ice immediately *after* the measurement of any temperature t as the zero of the thermometer for the preceding observation. This is equivalent to the conclusion that the residual or after-effect, whose existence is made manifest by the depression of zero, remains practically unchanged during the brief interval required to observe both t and z_t. If t has been preceded by a higher temperature, the residual effect is in reality larger, but it depresses equally the readings t and z_t. This method of a "movable zero" requires that in determining the fundamental interval the boiling-point 100° C. be first determined, and that the ice-reading z_{100} be taken immediately after.

The ordinary practice in this country proceeds on the hypothesis of a "fixed zero." In determining the fundamental interval the observation of the ice-point precedes that of the boiling-point; and in ordinary use the observed departure from 0° in a preliminary observation in ice is applied as a constant correction at all points of the scale. The relative merits of the movable zero and fixed zero methods will be discussed later; in the mean time let us consider what modifications are required in the theory developed for the ideal thermometer.

§ 4. In ordinary investigations of thermal expansion the temperature is gradually raised, and the volume compared at intervals with that originally possessed by the material at 0° C. This is a fixed zero method; thus supposing a_1, a_2, &c. to have the values obtained in ordinary experiments, (6) is immediately applicable in ordinary British thermometry, provided readings be always taken with the temperature slowly rising, and the zero employed answer really to prolonged exposure to the temperature 0° C.

The movable zero method is analogous to experiments in which the volume v of glass at temperature t is compared with a volume $v'_{t,0}$ obtained after suddenly cooling the glass to 0° C. Instead of attempting to modify (6) to suit these circumstances, I shall develop the theory independently.

§ 5. If the changes in temperature are slow, as is really assumed in the movable zero method, except in zero determinations, then so long as t is increasing we may regard $v'_{t,0}$ as a continuous function of t, which for ordinary values of t differs little from a constant. We may thus provisionally assume

$$v'_{t,0}=v_0(1+b_1't+b_2't^2+b_3't^3+\ldots); \quad . \quad . \quad (10)$$

where $v_0\equiv v'_{0,0}$ is the volume answering to prolonged exposure to 0° C., while b_1', b_2', &c., are absolute constants for the particular glass.

Though not essential for our present purpose, we may notice that combining (2) and (10) we deduce a relation of the type

$$v=v'_{t,0}(1+b_1t+b_2t^2+b_3t^3+\ldots)\ ; \quad . \quad . \quad (11)$$

where

$$a_1=b_1+b_1', \quad a_2=b_2+b_2'+b_1b_1'\ldots \text{ \&c.}\ ; \quad . \quad (12)$$

or, in general, to a first approximation

$$b_1=a_1-b_1', \quad b_2=a_2-b_2'.$$

Let S represent the volume at 100° C. of the bulb and tube up to the division 100, and at the same temperature let $100\,s$ denote the volume of the fundamental interval. Then the mercury at 100° C. has a volume S, and at any other temperature t (on the hydrogen scale) it has by (1) a volume

$$\mathrm{S}(1+\mathrm{A}_1t+\mathrm{A}_2t^2+\ldots)\div(1+100\,\mathrm{A}_1+100^2\mathrm{A}_2+\ldots).$$

The volume of the bulb, including the stem up to the division 0, when the glass has been suddenly cooled to 0° C. after exposure to 100° C., is equal to the volume $\mathrm{S}\div(1+100\mathrm{A}_1+100^2\mathrm{A}_2+\ldots)$ of the mercury at 0° C. Thus its volume at temperature t under the conditions supposed is by (2)

$$\mathrm{S}(1+a_1t+a_2t^2+\ldots)$$
$$\div\{(1+100\,\mathrm{A}_1+100^2\mathrm{A}_2+\ldots)(1+100b_1'+100^2b_2'+\ldots)\}.$$

Thus supposing the thermometer to read $t+y'$ when the hydrogen temperature is t we get, reasoning as in § 2,

$$(t+y')s\frac{1+a_1t+a_2t^2+\dots}{1+100a_1+100^2a_2+\dots}=\mathrm{S}\left\{\frac{1+\mathrm{A}_1t+\mathrm{A}_2t^2+\dots}{1+100\,\mathrm{A}_1+100^2\mathrm{A}_2+\dots}\right.$$
$$\left.-\frac{1+a_1t+a_2t^2+\dots}{(1+100\,\mathrm{A}_1+\dots)(1+100\,b_1'+\dots)}\right\}, \quad (13)$$

where

$$s/\mathrm{S}=\left\{\mathrm{A}_1+100\,\mathrm{A}_2+\dots-\frac{(a_1-b_1')+100\,(a_2-b_2')+\dots}{1+100b_1'+\dots}\right\}$$
$$\div(1+100\,\mathrm{A}_1+100^2\mathrm{A}_2+\dots). \quad . \quad (14)$$

As will be seen presently in § 6, $100b_1'+100^2b_2'+\dots$ is of the order 1/50,000 in ordinary glass, and is hardly likely to be worth retaining in the denominator of (s/S) when multiplied by y'. Omitting it

$$s/\mathrm{S}=\{\mathrm{A}_1-a_1+b_1'+100(\mathrm{A}_2-a_2+b_2')+\dots\}$$
$$\div(1+100\,\mathrm{A}_1+100^2\mathrm{A}_2+\dots). \quad . \quad . \quad (15)$$

Using (15) for (s/S) in the coefficient of y' in (13), and (14) in the coefficient of t—since t/y' may be very large—and, after the substitutions and reductions are effected, neglecting $100b_1'+100^2b_2'$ relatively to 1, we get

$$y'=$$
$$\frac{t(100-t)\left[(\mathrm{A}_1-a_1+b_1')\{a_1+a_2(100+t)\}-(\mathrm{A}_2-a_2+b_2')(1-100a_2t)\dots\right]}{(1+a_1t+a_2t^2+\dots)\{\mathrm{A}_1-a_1+b_1'+100(\mathrm{A}_2-a_2+b_2')+\dots\}}$$
$$+\frac{(100-t)\{b_1'+b_2'(100+t)+\dots\}(1+100a_1+100^2a_2\dots)+}{\{\mathrm{A}_1-a_1+b_1'+100(\mathrm{A}_2-a_2+b_2')+\dots\}(1+a_1t+a_2t^2+\dots)}. \quad . \quad (16)$$

Strictly speaking, the terms in $b_1'a_2$ and $b_2'a_2$ in the first line of (16) should be omitted, as others of the same order have been neglected. Their retention, however, can do no harm, and it facilitates calculation to group b_1' with A_1-a_1 and b_2' with A_2-a_2, the principal terms in their respective multipliers being the same.

We must next determine the ice-reading $y'_{t,0}$ taken after exposure to temperature t.

Let s' be the volume of the stem-division 0 to 1 when $y'_{t,0}$ is observed, then obviously

$$s'y'_{t,0}=\mathrm{S}\left\{1-\frac{1+b_1't+b_2't^2+\dots}{1+100b_1'+100^2b_2'+\dots}\right\}$$
$$\div(1+100\mathrm{A}_1+100^2\mathrm{A}_2+\dots). \quad . \quad . \quad . \quad (17)$$

As the ice-reading is taken very rapidly, and the stem where read must emerge from the ice, the temperature to which s' corresponds is uncertain. In ordinary glass, however, the volume at 100° C. exceeds that at 0° C. by only about 1 part in 400, and $y'_{0,0} \sim y'_{100,0}$ is seldom as much as $0^\circ{\cdot}3$ C. Thus, at least within the range 0° to 100° C., we can hardly introduce an error so large as $0^\circ{\cdot}001$ C. by supposing s' in (17) to answer to the temperature t.

Doing so, neglecting $100b_1'+100^2b_2' : 1$, and using (15), we get

$$y'_{t,0}=\frac{(100-t)\{b_1'+b_2'(100+t)+\ldots\}(1+100a_1+100^2a_2+\ldots)}{\{A_1-a_1+b_1'+100(A_2-a_2+b_2')+\ldots\}(1+a_1t+a_2t^2+\ldots)} \quad . \quad (18).$$

Thus the second line in the expression (16) for y' is simply $y'_{t,0}$, and hence for the difference y between the glass and hydrogen scales we get

$$y=y'-y'_{t,0}=$$
$$\frac{t(100-t)[(A_1-a_1+b_1')\{a_1+a_2(100+t)+\ldots\}-(A_2-a_2+b_2')(1-100a_2t)+\ldots]}{(1+a_1t+a_2t^2+\ldots)\{A_1-a_1+b_1'+100(A_2-a_2+b_2')+\ldots\}}. \quad . \quad . \quad . \quad . \quad (19)$$

Comparing (19) and (6), we see that to the degree of approximation reached in (19), y differs from x only in replacing

$$e_1 \equiv A_1-a_1 \text{ by } A_1-a_1+b_1', \text{ or } A_1-b_1,$$
$$e_2 \equiv A_2-a_2 \text{ by } A_2-a_2+b_2', \text{ or } A_2-b_2.$$

Thus (19) might have been arrived at by using (11) instead of (2) and following a method more analogous to that by which (6) was obtained.

§ 6. To see more exactly what we are doing it is convenient at this stage to consider the order of magnitude of the several constants. For this purpose *verre dur* may be selected as an example, taking the first approximations to the constants, which are quoted by Guillaume*, viz.:—

$$A_1=182\times10^{-6},\quad A_2=\ \ 3\times10^{-9},$$
$$a_1=\ \ 22\times10^{-6},\quad a_2=24\times10^{-9},$$
and hence
$$e_1=160\times10^{-6},\quad e_2=-21\times10^{-9}.$$

These figures make $100\,e_2/e_1=-1/80$ approximately,
$$100\,a_1+100^2\,a_2=\ \ 1/400 \quad ,,$$
$$e_1+100\,e_2\ =\ \ 1/6300 \quad ,, \quad .$$

To arrive at an idea of the size of b_1' &c. we utilize the fact that in *verre dur* the depressed zero after 100° C. is

---

* '*Thermométrie*,' p. 217.

almost exactly 0°·1 C. lower than that answering to 0° C. Thus from (18)

$$\frac{100(b_1'+100\,b_2'+\ldots)(1+100\,a_1+100^2\,a_2+\ldots)}{e_1+100\,e_2+\ldots+(b_1'+100\,b_2'+\ldots)}=\frac{1}{10}.$$

Having regard to the small value found above for $100\,a_1+100^2a_2$, we thence deduce as a close approximation

$$(b_1'+100\,b_2'+\ldots)/(e_1+100\,e_2+\ldots)=1/1000,$$

and so, from above, $b_1'+100\,b_2'+\ldots=1/6300000$ roughly.

The size of a_1 seems fairly uniform in different kinds of glass, so that e_1 never differs much from the value 160×10^{-6} found above. The size of a_2 and e_2 is probably much more variable, but $100\,e_2/e_1$ is, doubtless, in all cases a small fraction. Similarly the size of $(b_1'+100\,b_2'+\ldots)/(e_1+100\,e_2+\ldots)$ is certainly somewhat variable; it may be as large in some kinds of glass as 1/300, but we are unlikely to commit any serious error if we treat it as a quantity of the order 1/1000 in English glass.

§ 7. Thus in neglecting $b_1'+100\,b_2'+\ldots$ in the denominator of (19) we should only make an error of the order 1/1000 in the value of y. Now between 0° C. and 100° C. the largest value of y seldom approaches 0°·5 C., so throughout at least this range the neglect is abundantly justified, even when accuracy of the order 0°·001 C. is aimed at. At temperatures much above 100° C. the neglect of $b_1'+100\,b_2'+\ldots$ would require more consideration. Neglecting it provisionally, we deduce from (6) and (19)

$$x-y=\frac{t(100-t)\{b_2'-a_1\,b_1'\ldots\}}{(1+a_1t+\ldots)(e_1+100\,e_2+\ldots)}\quad . \quad . \quad . \quad (20)$$

As we have already seen, a_1 is of the order 2×10^{-5}, and b_1'/e_1 appears seldom to approach 1/400. Thus for values of t between 0° and 100°

$$a_1b_1't(100-t)\div(1+a_1t\ldots)(e_1+100\,e_2\ldots)$$

is a quantity of the order 0°·0001 C., and so may be neglected. For the same range of temperature $x-y$ is a small fraction of a degree, and we may thus write 1 for $1+a_1t+\ldots$ in (20). Thus we conclude that for practical purposes—so long at least as t is not considerably in excess of 100—we may replace (20) by

$$x-y=t(100-t)b_2'\div(e_1+100\,e_2+\ldots).\quad . \quad . \quad . \quad (21)$$

Our reasoning does not necessarily imply that b_2'/a_1b_1' is large. There may be cases where it is not large; only for them $x-y$ is negligible.

§ 8. As our treatment of the movable zero method rests on (10), it is desirable to indicate the experimental basis for this formula, especially as by doing so we shall see more clearly how to obtain numerical results for b_1', b_2', and $x-y$.

The experimental basis is simply that in ordinary thermometers used in a definite way the depression D_t in the zero reading, after exposure to moderate temperature t, is given satisfactorily by a formula of the type

$$D_t=d_1t+d_2t^2+\ldots, \quad . \quad . \quad . \quad . \quad . \quad (22)$$

where d_1, d_2 are constants for the particular thermometer.

If v_0' be the volume of a scale-division, and V_0 that of the bulb up to the fixed zero mark after prolonged exposure to 0° C., we have

$$v_0'D_t=V_{t,0}-V_0,$$

where $V_{t,0}$ is the volume of the bulb after sudden cooling from $t°$ to 0°. Hence by (22)

$$V_{t,0}=V_0\{1+(d_1v_0'/V_0)t+(d_2v_0'/V_0)t^2+\ldots\}. \quad . \quad (23)$$

This is a formula of the assumed type (10), with

$$d_1v_0'/V_0=b_1', \quad d_2v_0'/V_0=b_2', \quad \&c. \quad . \quad . \quad (24)$$

In practice, as explained above, v_0' may differ from v_0, the value corresponding to prolonged exposure to 0° C. The divergence is, however, negligible in (23) for values of t not exceeding 100°. Neglecting it also in (24), we clearly have b_1' and b_2' determined in terms of v_0/V_0, a quantity known when the glass is known, and of d_1 and d_2, constants determined by experiments on the depressed zero readings after a series of temperatures.

In deducing (21) from (20) we neglected $(a_1t+a_2t^2)/1$. Thus to this degree of accuracy we may also neglect $100\,a_1+100^2a_2+\ldots$; and when we do so we have from (4)

$$v_0/V_0=e_1+100\,e_2+\ldots$$

Hence, referring to (24), we see that (21) is equivalent to

$$x-y=t(100-t)\,d_2. \quad . \quad . \quad . \quad . \quad . \quad . \quad (25)$$

In reality $100\,a_1+100^2a_2+\ldots$ is of the order 1/400, while, for values of t between 0 and 100, D_t or $t(100-t)d_2$ is of the order 1/10; thus their product is negligible.

If d_2 be zero, *i.e.* if D_t be a linear function of t,

$$x-y=0,$$

or the fixed and movable zero methods supply the same scale. This is pointed out in Guillaume's '*Thermométrie*'[*], and has also been remarked on recently by Schuster and Gannon[†].

§ 9. As examples of the size of d_1 and d_2 in different glasses are of interest, I have collected the following data from Guillaume's '*Thermométrie*'[‡], adding the corresponding *calculated* values of the zero depressions answering to temperatures of 50° C. and 100° C. The results for the French glasses appear to be due to Guillaume himself, those for the Jena glass to Bottcher[§].

TABLE I.

Glass.	$d_1\times10^7$.	$d_2\times10^7$.	Depression after 50° C.	Depression after 100° C.
Verre dur (experiments $-10°$ to 100° C.)	+8886	+11	$0\overset{\circ}{\cdot}046$ C.	$0\overset{\circ}{\cdot}099$ C.
,, (,, $-10°$ to 190° C.)	+8557	+12	...	·098
French crystal, hard........................	+7972	+329	·122	·409
Jena glass 16^{III}...............................	+7100	-8	·034	·063

The *observed* depressions in *verre dur* are ·047 after 50° C. and ·100 C. after 100° C.

I have seen no actual figures for Jena glass 59^{III}, but it is stated[||] that the depression after 100° C. does not exceed 0°·02 C.

The depression in the French crystal is only slightly larger than in the other kinds of glass at atmospheric temperatures, but it increases much more rapidly at high temperatures, a defect necessarily shared by all glasses in which d_2 has a large positive value.

§ 10. In the following Table II. I have calculated from (25) the algebraical excess of the reading on the fixed zero over that on the movable zero scale at certain points, for the kinds of glass given in Table I. For *verre dur* I have taken $d_2=115\times10^{-8}$ throughout the whole range. The application

---

[*] P. 198.

[†] Phil. Trans. A, for 1895, p. 428.

[‡] Pp. 149 and 318.

[§] *Zeitschrift für Instrumentenkunde,* vol. viii. p. 409 (1888).

[||] *Wiss. Abhandl. der Phys. tech. Reichsanstalt,* vol. i. 1894, p. 10.

of the formula to the temperatures −38° C. and 200° C. is somewhat of an extrapolation from the experimental point of view.

TABLE II.

Value of $x-y$ (in centigrade degrees) from (25).

Temperature centigrade.	*Verre dur.*	French crystal (hard).	Jena glass 16^{III}.
−38°	−0°·006	−0°·173	+0°·004
+50	+0·003	+0·082	−0·002
+200	−0·023	−0·658	+0·016

Obviously in the case of French crystal the distinction between the fixed and movable zero scales must be taken into account, even for accuracy of the order 0°·1 C.

Generally speaking, English thermometer glass resembles more nearly French crystal than the other two glasses, in so far as at temperatures above 50° C. the term d_2t^2 tends to predominate.

Some experiments, however, at Kew Observatory, still incomplete, show notable differences in the behaviour of different English thermometers, all nominally of the same glass. In some the depression of zero after exposure to 100° C. is only a shade larger than in Jena glass 16^{III}, in others the depression is twice, or even thrice, as great. If the cause is difference as regards annealing or in treatment, one would expect to hear of like differences in *verre dur* and Jena glass thermometers.

In all the English glass thermometers I have met with, d_2 was positive. This implies, it will be noticed, that they would read lower at −38° C. on the fixed zero than on the movable zero scale.

Relations of different Temperature Scales.
Experimental Results.

§ 11. Of the kinds of glass used in the higher thermometry the most notable are probably *verre dur* and the two Jena glasses 16^{III} and 59^{III}. The following particulars as to their chemical composition are derived from p. 10 of vol. i. of the Reichsanstalt's *Wiss. Abhandlungen.*

TABLE III.

	Verre dur.	16^{III}.	59^{III}.
SiO_2	71·5	67·3	71·95
Na_2O	11·0	14·0	11·0
CaO	14·5	7·0	
B_2O_3	...	2·0	12·0
Al_2O_3	1·5	...	5·0
$Al_2O_3+Fe_2O_3$ ...	...	2·5	
ZnO	...	7·0	
K_2O	0·4	...	
SO_3	0·7	...	
M_2O_3	...	0·2	0·05
	99·6	100·00	100·00

The data for 16^{III} and 59^{III} refer apparently to the ingredients as put into the melting-pot. Actual analysis of 16^{III} by Tornöe gave about 1 per cent. of SO_3. The data for *verre dur* are from an analysis by Tornöe. Similar data will be found in Guillaume's '*Thermométrie*,' p. 144 and p. 160. The first-mentioned page gives details as to the degree of variability of composition in different samples of *verre dur*.

The relations to one another of the natural scales of *verre dur* (movable zero), hydrogen, and nitrogen thermometers have been examined experimentally at the Bureau International. From the observations formulæ of the type (7) have then been calculated. For instance, the formula* found to connect t, temperature on the hydrogen scale, and t', temperature on the *verre dur* scale, throughout the range 0° to 100° C. is

$$t'-t=t(100-t)\times 10^{-6}(62{\cdot}296-0{\cdot}48946t+{\cdot}0012805t^2).$$

These formulæ—which accord very closely with the actual observations—are finally employed to calculate tables† of differences between the several scales.

At the Reichsanstalt‡ the scales of 16^{III}, 59^{III}, and other Jena glasses have been compared with that of *verre dur*, and formulæ of the Callendar type have then been calculated to suit the observed differences; such, for instance, as

$$t_{16}-t=0^\circ{\cdot}0259\{(t/100)-(t/100)^2\};$$

---

* Guillaume's *Thermométrie*, p. 284.

† *Ibid.* pp. 328–332; also Bureau International's *Travaux et Mémoires*, vol. x. 1894 (*Thermomètres Étalons*, par Ch. Ed. Guillaume, pp. 32, 33).

‡ *Wiss. Abhandl.* vol. ii. pp. 39, 40.

TABLE IV.

Algebraic excess of reading on scale specified over that of the hydrogen thermometer.

Scale	10°.	20°.	30°.	40°.	50°.	60°.	70°.	80°.	90°.
Nitrogen	+·006	+·010	+·011	+·010	+·009	+·005	+·001	−·002	−·003
Verre dur	+·052	+·085	+·102	+·107	+·103	+·090	+·072	+·050	+·026
French crystal (hard)	+·064	+·107	+·130	+·138	+·134	+·119	+·097	+·069	+·036
,, (ordinary)	+·067	+·112	+·137	+·147	+·144	+·130	+·107	+·076	+·041
Jena glass 16^{III}	+·057	+·093	+·113	+·119	+·116	+·102	+·083	+·058	+·031
Jena glass 59^{III}	+·024	+·036	+·037	+·033	+·026	+·016	+·007	+·001	−·002
English crystal (Wiebe)	·00	+·02	+·03	·00	−·03				

where t_{16} is temperature on the scale of 16^{III}, and t is the mean for the scales of 16^{III} and *verre dur*. From these formulæ are deduced differences between the several scales, proceeding apparently to 0°·0001 C. Particulars of a comparison of the natural scales of *verre dur* and English crystal by Wiebe*—proceeding to 0°·01 C. only—are quoted in Guillaume's '*Thermométrie*,' p. 214.

Combining these data I have compiled the table on p. 218, which refers all the scales to that of the hydrogen thermometer at the Bureau International.

§ 12. As most of the data in the table depend on the comparison of the *verre dur* and hydrogen scales, it is only proper to mention that a more recent comparison has been made at the Bureau International over part of the fundamental interval. Full particulars do not appear to have been given as yet, but the following preliminary details are contained in a recent publication of the Bureau †:—

TABLE V.

Excess of reading of *verre dur* over hydrogen thermometer.

Temp. Cent.	10°.	20°.	30°.	40°.
Old determination ...	·052	·085	·102	·107
New „ ...	·046	·078	·098	·106

The new determination was made by M. Chappuis, using a hydrogen thermometer having its reservoir of glass instead of, as previously, platinum-iridium.

The close agreement of the two determinations at 40°, where the difference between the scales is greatest, is emphasized in the text; but the very appreciable discrepancies at 10° C. and 20° C. seem to me hardly in harmony with the conclusion "Ainsi les anciennes mesures . . . sont complètement confirmées par les nouvelles . . ."

§ 13. At the Reichsanstalt comparisons have been made of thermometers of various kinds of glass and the air-thermometer. Formulæ have then been calculated to fit the observed differences between the natural scales as well as possible. These formulæ, though really of the type (7), are arranged somewhat differently. For instance, over the range −20° C.

---

[*] *Sitz. der k. preuss. Akad. der Wiss.* 1885, p. 633.

[†] *Procès-Verbaux des Séances de* 1895, Paris, 1896, p. 46.

to 100° C. the difference δ between the natural scale of the air-thermometer and Jena glass 59^{III} is represented by

$$\delta = t(100-t)\{\cdot 00000487 - \cdot 0000002638(100-t)\}^*.$$

This appears to be the algebraic excess of the reading on the air-thermometer, though I do not see an explicit statement to that effect. At least it makes

$$\delta = -0^\circ\cdot 021 \text{ at } 50^\circ \text{ C., and } +0^\circ\cdot 064 \text{ at } -20^\circ \text{ C.}$$

High-Temperature Results.

§ 14. At temperatures above 100° C. the most exact method of comparing thermometers is probably to use the boiling-points of a series of liquids. By modifying the pressure under which the liquid boils, a single liquid may suffice for comparisons over a very considerable range. Thus Crafts[†], in 1882, covered a range of 70° C. with water-vapour alone. According to Crafts, the method was suggested primarily by Regnault. A liquid bath has certain advantages from its simplicity; but the stirring must be very good, and the whole of the mercury in the thermometer-stem should, if possible, be immersed.

In addition to instrumental difficulties, troubles are apt to arise from the behaviour of the thermometer-glass. A thermometer exposed for some hours, or even minutes, to a high temperature, in excess of what it has been exposed to since its construction, usually experiences a *permanent* rise in zero. This is more especially true of temperatures of 300° C. and upwards. In some experiments by Guillaume[‡], exposure for ten hours to a temperature of between 360° and 380° C. raised the zeros of three thermometers of *verre dur*, Jena glass, and French crystal by 1°·8, 3°·0, and 7°·0 C. respectively. Further heating for periods of twelve and ten hours at the same temperature produced a considerable further rise. Probably the most complete experiments on this subject have been those by Crafts nearly twenty years ago. In 1879–80 he exposed eight thermometers, four of French crystal and four of a German soda-glass, to a variety of temperatures, the highest being 355° C.[§] The exposures were of various durations, from 20 minutes to several months. The rise in the zero varied from 10° to 18°—or, including rise under preliminary treatment, even 26° C. The conclusions drawn by Crafts were that the rise is much most rapid at the begin-

---

[*] "Thätigkeit der Phys. Tech. Reichs." (*Zeits. f. Instr.* 1894, p. 305).

[†] *Comptes Rendus* of the French Academy, vol. xcv. 1882, p. 838. (*Cf.* also Ramsay and Young, Phil. Trans. for 1884, p. 37, and W. Watson, Phil. Mag. July 1897, p. 116).

[‡] *Thermométrie*, p. 147.

[§] *Comptes Rendus* of the French Academy, vol. xci. 1880, p. 291.

ning of the exposure to a fixed temperature, that it tends probably to a maximum with prolonged exposure, and that it is greatest in glasses containing lead. Together with the permanent rise of zero there may exist a temporary depression, and as the latter wears off, on the thermometer's being exposed to a normal temperature, the permanent rise apparently increases.

Owing to the permanent rise of zero, wholly erroneous conclusions would be drawn as to the natural scale of a thermometer unless it had previously been exposed to a course of treatment tending to reduce to a minimum the permanent effect of exposure to any temperature within the range to which the comparison is to be extended*. Generally speaking, the treatment consists in exposing the thermometer for some time, preferably on more than one occasion, to a temperature higher than any at which the comparison is to be made, and allowing it to cool slowly †.

§ 15. At the Bureau International the comparison of the *verre dur* and hydrogen scales has been extended to the range 100° C. to 200° C., use being made of the variable boiling-point method already referred to. The following *resumê* of the results is given in the "Procès-Verbaux" ‡ of the International Committee.

TABLE VI.

Temperature centigrade ...	110°.	120°.	130°.	140°.	150°.
$\delta \equiv$ *verre dur* — hydrogen-reading.	$-\cdot037$	$-\cdot063$	$-\cdot075$	$-\cdot074$	$-\cdot060$
Temperature centigrade ...	160°.	170°.	180°.	190°.	200°.
δ *verre dur* — hydrogen-reading.	$-\cdot034$	$+\cdot001$	$+\cdot043$	$+\cdot086$	$+\cdot125$

The results are said to accord fairly with the formula §

$$\delta = t(100-t)10^{-3}\{4\cdot22377\times10^{-2} + \cdot88486164\times10^{-3}(100-t) + \cdot39993\times10^{-5}(100-t)^2\},$$

where t is *verre dur* temperature.

---

* *Cf.* Crafts, *Comptes Rendus*, vol. xciv. 1882, p. 1298.

† It is desirable that such treatment should precede calibration or division. If it alters the coefficients a_1, a_2, &c. in (2) it really alters the natural scale of the glass.

‡ *Séances de* 1895, Paris, 1896, p. 44.

§ The factor 10^{-3} is not in the original but appears necessary.

As the "Procès-Verbaux" remarks, "cette fonction ne se raccorde pas très bien avec celle qui exprime la différence de marche entre 0° et 100°" (*cf.* § 11).

The Bureau do not regard the results as very satisfactory, owing to the fact that the differences between different *verre dur* thermometers "peuvent à 200° atteindre 5 à 6 centièmes de degré, quantité du même ordre de grandeur que la différence de marche par rapport au thermomètre à hydrogène" (*l. c.* p. 45). For practical purposes, however, it is interesting to know that even up to 200° C. the (movable zero) temperature-reading from a *verre dur* thermometer is likely to differ from the hydrogen scale by less than 0°·2 C.

At the Reichsanstalt* comparisons of Jena-glass thermometers with the air-thermometer in an oil-bath over the range 100° to 300° C. are assigned an accuracy of 0°·02 C. Even at 500° C. the comparison of thermometers of Jena glass 59^{III}—with the mercury prevented from boiling by gaseous pressure—is apparently assigned an accuracy of the order 0°·1 C., when auxiliary thermometers (Faden-thermometer) of a special pattern are used to supply a correction for the emergent column (see § 38).

For the difference δ between the natural scale of 59^{III} and that of the air-thermometer over the range 100° to 300° C. the formula proposed is

$$\delta = t(100-t)\{-{\cdot}000007233 - {\cdot}0000004259(100-t)\}.$$

This differs somewhat conspicuously from the corresponding formula for the range −20° to 100° C. quoted in § 13.

According to the formula, the two scales differ by 0·7° C. at 200° C., and by 4°·7 C. at 300° C.

§ 16. Of earlier comparisons the best known are those of Regnault, whose results are quoted in several English works (*e. g.* Balfour Stewart's 'Heat,' Lord Kelvin's 'Mathematical and Physical Papers,' vol. iii., Smithsonian 'Physical Tables,' 1896). Two useful warnings with respect to these data are given by Crafts †. Firstly, Regnault used a fixed zero method; secondly, French crystal glass for thermometers has been considerably altered since his time. The Choisy-le-Roi crystal glass used by Regnault in particular contained nearly twice as much lead as modern French crystal. Crafts himself, in 1882, compared a number of thermometers of modern French crystal glass, from two different makers, and likewise

---

* "Die Thätigkeit der Phys. tech. Reichsanstalt" (*Zeits. f. Instr.* Sept. 1894, p. 303).

† *Comptes Rendus*, vol. xcv. 1882, p. 836.

a thermometer of German soda-glass with a hydrogen thermometer. He gives a table * comparing his results with Regnault's. This I partly reproduce below. It should be noticed, however, that the gas-thermometer was air in the case of Regnault, hydrogen in the case of Crafts, and that the latter employed a movable zero method—differing, however, apparently in some particulars from that followed at the Bureau International. The figures are *corrections* required to reduce the reading of the mercury-thermometer to the scale of the gas-thermometer. The first and second lines A and B supply the corrections to Regnault's Choisy-le-Roi and *verre ordinaire* thermometers, respectively; the last line C refers to the modern crystal thermometers used by Crafts.

The centigrade scale is that employed throughout.

Table VII.

Temp.	120°.	140°.	160°.	180°.	200°.	220°.	240°.	260°.	280°.	300°.	320°.	340°.
Correction A ...	−·12	−·29	−·52	−·80	−1·25	−1·82	−2·55	−3·44	−4·48	−5·72	−7·25	−9·30
„ B ...	+·05	+·15	+·26	+·37	+ ·30	+ ·20	+ ·10	− ·20	− ·52	−1·08	−1·80	−3·00
„ C ...	+·04	+·16	+·33	+·34	+ ·27	+ ·08	+ ·14	− ·39	− ·63	−1·21	−2·03	

Crafts's table really gives the corrections for every 10°. It stops at 330°, but I have obtained the data under 340° from the other sources mentioned. In Crafts's opinion the ordinary French, German, and English thermometers of fifteen years ago possessed fairly similar natural scales. It is important to notice that even when, as in line C of the table, the departure from a gas scale is small up to 250° C., it may be very considerable at temperatures over 300° C.

The fact that an accurately divided glass-mercury thermometer, with its zero- and boiling-points correct, may differ

---

* In the paper last quoted, p. 838.

by 9° or 10° C. from a gas thermometer near the top of its scale is mentioned in several English books *, but has hardly met with the recognition it merits.

Low-Temperature Results.

§ 17. At temperatures below 0° C. the natural scales of mercury thermometers show much larger departures from the hydrogen scale than are met with between 0° and 100° C. Thus, according to the determinations of the Bureau International, "*verre dur* reading less hydrogen reading"

$$= -0^{\circ}{\cdot}170 \text{ at } -20^{\circ}\text{ C.},$$
$$= -0^{\circ}{\cdot}426 \text{ at the freezing-point of mercury.}$$

For this latter point the Bureau has found, "avec une approximation de 2 centiémes de degré" †,

$$-38^{\circ}{\cdot}80 \text{ C. on the hydrogen scale,}$$
$$-39^{\circ}{\cdot}22 \text{ C. on the scale of } \textit{verre dur}.$$

As to the scale of English glass at temperatures below 0° C., the only published result I know of is a determination of the freezing-point of mercury on the natural (fixed zero) scale of a Kew Standard, No. 45, by Balfour Stewart ‡. The mean of three separate experiments—giving an extreme difference of 0°·1 F.—was

$$-38^{\circ}{\cdot}2 \text{ F. } (-39^{\circ}{\cdot}0 \text{ C.}).$$

This comes much nearer the hydrogen scale than does the *verre dur* reading.

Results from a single thermometer are, however, at best somewhat inconclusive, and some doubt might reasonably be entertained as to the closeness of the temperature produced by freezing mercury in Balfour Stewart's apparatus to that produced in the Bureau International's experiments.

As to the latter point some collateral evidence exists. The experiments on the Kew Standard, No. 45, are mentioned in Balfour Stewart's paper on the determination of the freezing-point of mercury on the scale of the air-thermometer, and seem to have been carried out under fairly similar conditions to those of the main object of the research.

The hydrogen and air scales are unlikely to differ much

---

* *E.g.* Lord Kelvin's 'Mathematical and Physical Papers,' vol. iii. p. 147, and Glazebrook and Shaw's 'Practical Physics,' p. 189 of first edition.

† *Comptes rendus . . . de la deuxième Conférence Général. . . .* Paris, 1896, p. 29.

‡ Phil. Trans. for 1863, p. 428.

at −39° C., and Balfour Stewart's * determination of the freezing-point of mercury on the scale of the air-thermometer, viz. −37°·93 F. (or −38°·85 C.), differs by only 0°·05 C. from the Bureau's determination on the hydrogen scale. This collateral evidence is, however, somewhat weakened by the fact that Balfour Stewart states that the mercury used in the experiments on thermometer No. 45 was not the same as that used with the air-thermometer.

§ 18. Since Balfour Stewart's investigation there have been numerous observations of the freezing-point of mercury on calibrated Kew standard thermometers of English glass. The results deduced at Kew Observatory for the freezing-point of mercury on the natural (fixed zero) scales of twenty of these thermometers are as follows:—

highest	−38°·15 F.	(−38°·97 C.),
mean	−38°·35 F.	(−39°·08 C.),
lowest	−38°·50 F.	(−39°·17 C.).

The thermometers were read to the nearest 0°·05 F. only.

In sixteen out of the twenty thermometers the reading lay between −38°·25 F. and −38°·45 F., and as errors of reading of 0°·05 F. might easily arise, there can be little doubt that the mean found above is fairly representative for ordinary English glass.

To test the accuracy of the method, and obtain a connexion with the hydrogen scale, I had three of the twenty thermometers—which had remained at the Observatory—partially recalibrated and retried together in the mercury bath, while two independent observations were made in the bath with a Tonnelot thermometer standardized at the Bureau International. The results obtained with the three Kew standards, after allowing for the secular change of zero, were practically identical with those found on separate occasions many years ago. While the results obtained on the two occasions with the Tonnelot thermometer, read to 0°·01 C. by the unaided eye, differed by only 0°·02 C., and gave for the freezing-point of mercury

−39°·286 C. on the *verre dur* scale,

or, applying the Bureau's table,

−38°·86 C. on the hydrogen scale.

The reading with the Tonnelot thermometer is 0°·06 C. lower than that accepted by the Bureau, and I am not certain

---

* *L. c.* p. 435.

that the determination of the zero corresponding to the temperature $-39°$ C. was carried out exactly in the way approved by the Bureau, but any failure in this respect would, I think, be more likely to increase than reduce the apparent difference.

Thus the experiments with the Tonnelot thermometer show that if the Kew apparatus gives a different result from that of the Bureau, this difference tends to exaggerate the apparent departure of the natural English glass scale from that of the hydrogen thermometer. It has also to be borne in mind that, as stated in § 10, an English glass thermometer treated like the *verre dur* thermometer on a movable zero method would give a reading algebraically higher, and so nearer that of the hydrogen thermometer, than when treated as at Kew Observatory.

§ 19. It may appear that undue space has been given to the question of the temperature answering to the freezing-point of mercury, and the corresponding reading of English glass thermometers. Since Balfour Stewart's determination, however, very considerable importance has attached to this point. For many years it was not unusual to treat the freezing-point of mercury as a third fixed point (taken as $-37°{\cdot}9$ F.) on the scale of mercury thermometers, subdividing the tube below 32° F. by reference to the observed readings in freezing ice and freezing mercury. This produced a thermometer in which the degree volumes below 32° F. were equal amongst themselves but unequal to those above 32° F.

The object, of course, was to produce a mercury thermomometer whose readings should lie nearer those of the air-thermometer than if it were divided into identically equal volumes throughout; and so would give better results when no corrections were applied, other than for observed change of ice reading. This practice, no doubt, may be regarded with far from silent contempt by the present generation of physicists; but to our ruder ancestors, who thought an accuracy of from $0°{\cdot}1$ to $0°{\cdot}2$ F. at temperatures below 32° F. something on which to congratulate themselves, the idea seemed an ingenious one. However bad, or the reverse, the idea was, the fact remains that a good many mercury-thermometers divided in this way have been used in this country and its colder dependencies.

Even at the present day it is the practice of English opticians to take $-37°{\cdot}9$ F. as a fixed point in the construction of spirit thermometers [*] used for low-temperature

---

[*] *Cf.* Balfour Stewart's 'Heat,' 1st edition, Art. 26.

meteorological investigations; and many, perhaps most, low Canadian temperatures have been observed on these instruments.

My own belief is that for ordinary meteorological uses the idea was by no means bad, and that it might, for very low temperatures, when accuracy of 0°·1 or 0°·2 F. is good enough, be carried farther.

Instead of trying to make the scale of the mercury or spirit thermometer coincide with that of a gas thermometer at three points only, one might, if one knew the natural scale well enough, make it coincide at a considerably larger number of points. The calculated alterations to make to the data supplied by calibration would not entail any very serious trouble.

The other reason for dwelling on the facts is that they are at least strongly suggestive that the glass employed in this country for making thermometers is not quite so uniform as one would like it to be. The figures given in § 18 for the freezing-point of mercury on the natural scales of twenty English glass thermometers showed a difference of 0°·35 F. between the extremes. I was inclined to attribute the greater part of this to possible errors of reading, or impurity of the mercury used on one or two occasions in the bath. A direct comparison, however, of the two extreme thermometers, freshly recalibrated, in the same bath of freezing mercury fairly confirmed the previously observed difference. The result is perhaps not absolutely conclusive, because the purity of the mercury inside thermometers has to be taken for granted; and if this confidence is misplaced, the point at which the consequences would be most serious would likely be the freezing-point of mercury itself.

[To be continued.]

XXV. *On Lord Kelvin's Absolute Method of Graduating a Thermometer.* *By* J. Rose-Innes, *B.Sc.**

IN a paper "On the Thermal Effects of Fluids in Motion" Lord Kelvin has given the cooling effects exhibited by various gases in passing through a porous plug; and he found that the effects for any one gas kept at the same initial temperature were proportional to the difference of pressure on the two sides of the plug (Reprinted Papers, vol. i. pp. 333–455). He also found that the cooling effect for any one gas per unit difference of pressure varies as the inverse square of the absolute temperature; and this rule succeeds

---

* Communicated by the Physical Society: read Dec. 10, 1897.

very well in the case of air. For carbonic acid, however, the results furnished by this rule are not so satisfactory, as may be seen by inspecting the table given by him comparing the actual with the theoretical cooling effect (*loc. cit.* p. 429). Moreover in the case of hydrogen it is found that there is a *heating* effect, which increases, if anything, when the temperature rises, so that here the law of the inverse square of the temperature is wholly inapplicable. It seemed to me that it might be possible to hit upon some simple algebraic expression which should reproduce the experimental results rather better than Lord Kelvin's rule does; and in fact it was found that a satisfactory agreement between observation and calculation might be obtained by putting

$$\text{cooling effect} = \frac{\alpha}{\mathrm{T}} - \beta,$$

where α and β are constants characteristic of the gas, and T is the absolute temperature.

The following values of α and β were found from the experimental data:—

	α.	β.
Air	441·5	·697
Carbonic acid . .	2615	4·98
Hydrogen . . .	64·1	·331

A comparison of the actual results with those calculated by the new formula is given in the following table:—

Name of Gas.	Temp.	Actual cooling effect.	Calculated cooling effect (Kelvin's formula).	Calculated cooling effect (New formula).
Air	0°	0°·92	°·92	·920
	7·1	·88	·87	·879
	39·5	·75	·70	·716
	92·8	·51	·51	·510
Carbonic Acid......	0	4·64	4·64	4·60
	7·4	4·37	4·40	4·35
	35·6	3·41	3·63	3·49
	54·0	2·95	3·23	3·02
	93·5	2·16	2·57	2·16
	97·5	2·14	2·52	2·08
Hydrogen .	4·5	−0·100	......	−·100
	91·0	− ·155	......	−·155

It will be seen that the formula proposed in this paper, regarded simply as an empirical formula, is more efficient than Lord Kelvin's; there is nothing astonishing in this as it contains two disposable constants instead of only one. But it has the following further advantages:—

(i.) It includes the three cases of air, hydrogen, and carbonic acid under one form, and therefore enables us to treat them all in one common investigation.

(ii.) It renders more manageable the differential equation concerned in the thermodynamic scale of temperature, and leads to simpler algebraic results after integration.

This last proposition we must now proceed to prove. It is shown by Lord Kelvin that when a gas passes through a porous plug we must have

$$t\frac{dv}{dt}-v=\frac{\mathrm{JK}}{\Pi}\theta,$$

where t and v denote the temperature and volume of the gas respectively, K its specific heat, θ the cooling effect per atmo of differential pressure, Π the value of one atmo, and J the value of Joule's mechanical equivalent (Reprinted Papers, vol. iii. p. 179). Hence

$$\frac{1}{t}\frac{dv}{dt}-\frac{v}{t^2}=\frac{\mathrm{JK}}{\Pi}\frac{\theta}{t^2}$$

$$\frac{d}{dt}\left(\frac{v}{t}\right)=\frac{\mathrm{JK}}{\Pi}\left\{\frac{\alpha}{t^3}-\frac{\beta}{t^2}\right\}.$$

Estimate of the Absolute Value of the Freezing-point of Water.

Integrate the last equation between the limits t_0 and t_1, and we obtain

$$\frac{v_1}{t_1}-\frac{v_0}{t_0}=\frac{\mathrm{JK}}{\Pi}\left\{\frac{\alpha}{2t_0^{\,2}}-\frac{\alpha}{2t_1^{\,2}}-\left(\frac{\beta}{t_0}-\frac{\beta}{t_1}\right)\right\}$$

$$=\frac{\mathrm{JK}}{\Pi}\left\{\frac{1}{t_0}-\frac{1}{t_1}\right\}\left\{\frac{\alpha}{2}\left(\frac{1}{t_0}+\frac{1}{t_1}\right)-\beta\right\}$$

$$=\frac{\mathrm{JK}}{\Pi}\left\{\frac{1}{t_0}-\frac{1}{t_1}\right\}\frac{\theta_1+\theta_0}{2},$$

where θ_1 and θ_0 are the values of θ at the temperatures t_1 and t_0 respectively.

Multiply the equation by $t_0\,t_1$ and we get

$$v_1t_0-v_0t_1=\frac{\mathrm{JK}}{\Pi}(t_1-t_0)\frac{\theta_1+\theta_0}{2}$$

$$(v_1-v_0)t_0-v_0(t_1-t_0)=\frac{\mathrm{JK}}{\Pi}(t_1-t_0)\frac{\theta_1+\theta_0}{2}.$$

Hence

$$t_0 = \frac{t_1 - t_0}{v_1 - v_0}\left(v_0 + \frac{\mathrm{JK}}{\Pi}\frac{\theta_1 + \theta_0}{2}\right)$$

$$= (t_1 - t_0)\frac{v_0}{v_1 - v_0}\left(1 + \frac{\mathrm{JK}}{\Pi v_0}\frac{\theta_1 + \theta_0}{2}\right).$$

If t_1 and t_0 are taken as the boiling-point and freezing-point of water respectively, then this equation gives us the value of the freezing-point t_0 in terms of the interval $t_1 - t_0$; it is usual, as pointed out by Lord Kelvin, to take the interval $t_1 - t_0$ as containing 100 degrees (*loc. cit.* p. 175). It is evident that we should have obtained the same value for t_0 if θ had been constant throughout the range of temperature t_0 to t_1, and equal to $\frac{\theta_1 + \theta_0}{2}$. This shows that the proper mean cooling-effect is simply the arithmetic mean of the cooling effects at the boiling-point and freezing-point. The following table gives us the value of the freezing-point derived from experiments on the three gases.

	Uncorrected estimate of temperature of freezing-point.	Correction.	Corrected estimate.
Hydrogen . . .	273·13	—·13	273·00
Air	272·44	·72	273·16
Carbonic acid . .	269·5	4·35	273·85

The first column of figures is taken from Lord Kelvin's paper (*loc. cit.* p. 177).

Thermodynamic Correction for a Constant-pressure Gas Thermometer.

Suppose now we have a temperature t lying above both t_1 and t_0, and fixed by some definite physical phenomenon. We require to know exactly how it lies with respect to t_0 and t_1.

We start as before with the differential equation

$$\frac{d}{dt}\left(\frac{v}{t}\right) = \frac{\mathrm{JK}}{\Pi}\left\{\frac{\alpha}{t^3} - \frac{\beta}{t^2}\right\}.$$

We may put this

$$\frac{d}{dt}\left(\frac{v}{t}\right) = \frac{\mathrm{A}}{t^3} - \frac{\mathrm{B}}{t^2},$$

if $\mathrm{A} = \frac{\mathrm{JK}\alpha}{\Pi}$ and $\mathrm{B} = \frac{\mathrm{JK}\beta}{\Pi}$.

Integrate between the limits t_0 and t,

$$\frac{v}{t}-\frac{v_0}{t_0}=\frac{1}{2}\left(\frac{A}{{t_0}^2}-\frac{A}{t^2}\right)-B\left(\frac{1}{t_0}-\frac{1}{t}\right)$$

$$=\left(\frac{1}{t_0}-\frac{1}{t}\right)\left\{\frac{A}{2}\left(\frac{1}{t_0}+\frac{1}{t}\right)-B\right\}.$$

Hence

$$\frac{v}{t}-\frac{v_0}{t_0}=\frac{t-t_0}{tt_0}\left\{\frac{A}{2}\left(\frac{1}{t_0}+\frac{1}{t}\right)-B\right\}.$$

Multiply by $t_0(t-t_1)$ and we shall have

$$v\left(t_0-\frac{t_0t_1}{t}\right)-v_0(t-t_1)=\frac{(t-t_1)(t-t_0)}{t}\left\{\frac{A}{2}\left(\frac{1}{t_0}+\frac{1}{t}\right)-B\right\}.$$

Similarly by interchanging t_0 and v_0 with t_1 and v_1, we might obtain

$$v\left(t_1-\frac{t_0t_1}{t}\right)-v_1(t-t_0)=\frac{(t-t_1)(t-t_0)}{t}\left\{\frac{A}{2}\left(\frac{1}{t_1}+\frac{1}{t}\right)-B\right\}.$$

Subtract the equation last but one from the last,

$$v(t_1-t_0)-(v_1-v_0)t+v_1t_0-v_0t_1=\frac{(t-t_1)(t-t_0)}{t}\,\frac{A}{2}\left(\frac{1}{t_1}-\frac{1}{t_0}\right).$$

We may write this

$$(v-v_0)(t_1-t_0)-(v_1-v_0)(t-t_0)=\frac{(t-t_1)(t-t_0)}{t}\,\frac{A}{2}\left(\frac{1}{t_1}-\frac{1}{t_0}\right).$$

Divide by v_1-v_0 and transpose,

$$t-t_0=\frac{v-v_0}{v_1-v_0}(t_1-t_0)+\frac{(t-t_1)(t-t_0)}{t}\,\frac{A}{2(v_1-v_0)}\left\{\frac{1}{t_0}-\frac{1}{t_1}\right\}.$$

Finally,

$$t=t_0+\frac{v-v_0}{v_1-v_0}(t_1-t_0)+\frac{JK(\theta_0-\theta_1)}{2\Pi(v_1-v_0)}\,\frac{(t-t_1)(t-t_0)}{t}.$$

This last expression for t may be said to consist of three parts :—

(i.) There is the quantity t_0, whose absolute value may be considered to have been determined once for all by means of its own equation. We may regard it as known numerically with sufficient accuracy.

(ii.) There is the term $\dfrac{v-v_0}{v_1-v_0}(t_1-t_0)$. This term gives us the degrees above freezing-point on the equi-expansion method of graduation. It is what is usually called the "temperature."

(iii.) The quantity $\dfrac{JK(\theta_0-\theta_1)}{2\Pi(v_1-v_0)}\,\dfrac{(t-t_1)(t-t_0)}{t}$ is due entirely to the Joule-Thomson effect. It may be regarded as a correction necessary owing to the deviation of the substance from a perfect gas. It is this quantity which is calculated and tabulated so as to give us the means of arriving at the absolute scale. We notice that in order to calculate this term, we require to know the value of t, the very quantity we are seeking to find. Sufficiently accurate, however, for the purpose of calculating this small term, will be the value of t found by means of a first approximation.

Thermodynamic Correction for Constant-volume Gas-Thermometer.

We will now compare the indications of a gas-thermometer kept at constant volume with the thermodynamic scale of temperature.

Let us start as before with the differential equation

$$\frac{d}{dt}\left(\frac{v}{t}\right) = \frac{\mathrm{A}}{t^3} - \frac{\mathrm{B}}{t^2}.$$

Integrate this between the limits t and ∞ and we have

$$\mathrm{P} - \frac{v}{t} = \frac{1}{2}\frac{\mathrm{A}}{t^2} - \frac{\mathrm{B}}{t},$$

where P is the value of $\frac{v}{t}$, as v and t are made to grow indefinitely large with p constant. To determine the form of P we must appeal to experiment. We know that when a gas is made to expand to larger and larger volumes it obeys Boyle's law more and more closely; hence we infer that when v and t are made indefinitely large, the value of $\frac{v}{t}$ must vary inversely as the pressure. We may therefore write

$$\mathrm{P} = \frac{\mathrm{C}}{p}$$

where C is a constant characteristic of the gas.

Writing in this value we have as the complete solution

$$\frac{\mathrm{C}}{p} - \frac{v}{t} = \frac{1}{2}\frac{\mathrm{A}}{t^2} - \frac{\mathrm{B}}{t}.$$

Multiply this by pt, and transpose,

$$pv = \mathrm{C}t - p\left(\frac{1}{2}\frac{\mathrm{A}}{t} - \mathrm{B}\right).$$

If we neglect the Joule-Thomson effect we have as a first approximation $pv = \mathrm{C}t$, or $t = \frac{pv}{\mathrm{C}}$, which is the value for t in the case of a perfect gas; and this approximate value for t may be used in the term involving the Joule-Thomson effect on the right-hand side of the equation. We then get

$$pv = \mathrm{C}t - \frac{\mathrm{CA}}{2v} + p\mathrm{B}.$$

This equation represents our *second* approximation to the correct formula. We may also write it

$$p(v-\mathrm{B}) = \mathrm{C}t - \frac{\mathrm{CA}}{2v}.$$

Imagine now that we keep v constant, and use the equation to determine t when p is measured. Let the suffix 0 refer to the freezing-point, and the suffix 1 to the boiling-point as before. We then have

$$p_0(v-\mathrm{B}) = \mathrm{C}t_0 - \frac{\mathrm{CA}}{2v},$$

$$p_1(v-\mathrm{B}) = \mathrm{C}t_1 - \frac{\mathrm{CA}}{2v}.$$

By subtraction

$$(p_1-p_0)(v-\mathrm{B}) = \mathrm{C}(t_1-t_0),$$

$$(p-p_0)(v-\mathrm{B}) = \mathrm{C}(t-t_0),$$

$$\therefore \quad \frac{p-p_0}{p_1-p_0} = \frac{t-t_0}{t_1-t_0};$$

or

$$t = t_0 + \frac{p-p_0}{p_1-p_0}(t_1-t_0).$$

To the degree of approximation to which we are working, therefore, there is no thermodynamic correction needed for a constant-volume gas thermometer. There may be a correction involving *squares* of small quantities, which would appear on a nearer approximation. Such a correction, however, would not be worth taking into account in the case of a thermometer constructed with air or hydrogen, as the unavoidable errors of experiment would certainly be much larger than the correction. It is satisfactory to know that for all practical purposes absolute temperature is to be obtained with very great accuracy from Regnault's thermometric system by simply adding the value of t_0 to his numbers for temperature on the centigrade scale.

This result differs from that obtained by Rowland *, who employed Lord Kelvin's law of the inverse square of the

---

* Proceedings Amer. Acad. Arts & Sciences, xv. (n. s. vii.) p. 114.

temperature, and inferred that there was a correction needed involving the first power of the cooling-effect. Both his investigation and my own involve the assumption that an empirical formula found to hold through a short range of temperature can be used for any temperature however high, and hence neither his result nor mine is conclusively established; but it seems interesting to show that the employment of a new expression, at least as good as Lord Kelvin's, for the cooling-effect, leads to a smaller value for the correction.

XXVI. *On the Simple Isomorphisms of a Substitution-Group to itself.* *By* G. A. MILLER, *Ph.D.*[*]

MOST of the difficulties connected with the study of groups reside either in the simple groups or in the general problem of isomorphisms. One of the fundamental elements of this problem consists of the simple isomorphisms of a group to itself. It is our object to give a general outline of this element, together with a few details which appear new and of considerable importance in the study of the intransitive substitution-groups.

The simplest method of making a group (G) simply isomorphic to itself is that by which we write after each one of its substitutions[†] the transform with respect to some substitution that is commutative to G. That we obtain a simple isomorphism in this way, follows directly from the equation

$$s^{-1}t_1s \,.\, s^{-1}t_2s = s^{-1}t_1t_2s.$$

From this equation it also follows that we obtain a simple isomorphism when the transforming substitution is not commutative to G. In this case it would, however, not be a simple isomorphism of G *to itself.*

Instead of transforming every substitution of G by the same substitution, we may employ different transformers for the different substitutions. It is necessary and sufficient that all such transformers have the same effect upon the substitutions to which they are applied, regarded as operators, as a given transforming operator has upon the corresponding operators of the simply isomorphic operation-group.

Suppose that a regular group (R) is made simply isomorphic to itself in any one of the possible ways. We may suppose that all the substitutions, except identity, begin with

---

[*] Communicated by the Author; having been read at the Detroit Meeting of the American Association for the Advancement of Science, 1897.

[†] Unless the contrary is stated, the groups under consideration are supposed to be general substitution-groups.

the same element. The second elements in each of the corresponding substitutions may be made identical by transforming one of the two constituent groups by means of a substitution which does not contain the first element of all the substitutions. As the groups remain simply isomorphic, and their corresponding substitutions coincide with respect to their first two elements, they must coincide throughout. Hence, *every simple isomorphism of* R *to itself may be obtained by writing after each substitution of* R *its transform with respect to a given substitution which does not contain an arbitrary element of* R.

All the substitutions which transform R into itself, and contain only elements of R, form a transitive group of degree n, n being the order of R. A subgroup of this group, which contains all its substitutions which do not contain a given one of its n elements, cannot contain any substitution besides identity that is commutative to every substitution of R. Hence, no two substitutions of this subgroup can transform all the substitutions of R in the same manner. From the preceding paragraph it follows that this subgroup transforms R into itself in every possible manner.

Since the order of the given subgroup is equal to the order of the entire group divided by n, the latter must contain just n substitutions that are commutative to every substitution of R. These form a regular group, which coincides with R only when R is commutative. Each one of these two regular groups is said to be the associate* of the other. Before proceeding further in the consideration of the simple isomorphisms of R to itself, it seems well to give some definitions which apply to the general substitution-group.

§ 1. *Definitions and General Considerations.*

If we regard the substitutions of G as elements, we observe that a substitution of these elements corresponds to every simple isomorphism of G to itself and a substitution-group to all the possible isomorphisms. This substitution-group (I) has been called the *group of isomorphisms* of G. Its degree is the order of G diminished by the number of its substitutions that correspond to themselves in every simple iso-

---

* 'Quarterly Journal of Mathematics,' vol. xxviii. p. 249. The preceding seems to be an easy proof of Jordan's theorem in regard to the number of substitutions that are commutative to every substitution of a given regular group. Cf. *Traité des Substitutions*, p. 60.

morphism of G to itself. Hence it cannot exceed the order of G diminished by unity.

If an isomorphism of G to itself can be obtained by transforming it with respect to one of its own substitutions, it is said to be *cogredient*. All the other simple isomorphisms of G to itself are said to be *contragredient*. Two simple isomorphisms of G to itself which cannot be transformed into each other may be called *distinct*. Hence, the number of distinct isomorphisms of G is equal to the number of different intransitive groups of twice the degree of G that may be formed by making G simple isomorphic to itself.

When two isomorphisms are not distinct, they may be said to be *transform* with respect to each other. All the cogredient isomorphisms of G are transforms of the identical, but they do not necessarily form a complete system of transforms. The cogredient isomorphisms correspond to a self-conjugate subgroup in the group of isomorphisms. The subgroup of this group, which corresponds to the transforms of the identical isomorphism, includes this self-conjugate subgroup; but it is not necessarily self-conjugate. From the fact that the transforms of the identical isomorphism correspond to a subgroup of the group of isomorphisms, it follows that the total number of the simple isomorphisms of G to itself is divisible by the number of the transforms of the identical isomorphism.

It may happen that a subgroup of G corresponds to itself in all the possible simple isomorphisms of G to itself. Frobenius has called such a subgroup *characteristic* [*]. Subgroups which correspond to each other in any of these isomorphisms may be called *isomorphic*, and those which correspond in any of the transforms of the identical isomorphism may be called *transform*. Hence, the isomorphic subgroups include the transform and the latter include the conjugate.

If a subgroup corresponds to itself in all the transforms of the identical isomorphism it may be called *self-transform*. Hence the self-conjugate subgroups include the self-transform, and the latter include the characteristic. The last might be called self-isomorphic, in harmony with the other notation. While no special attention is called to these names, yet it is very important to observe the special properties of these six types of subgroups.

When a group, like the symmetric groups whose order is not 720 [†] and all the metacyclic groups, admits only co-

---

[*] *Sitzungsberichte der Berliner Akademie*, 1895, p. 183.

[†] Hölder, *Mathematische Annalen*, vol. xlvi. p. 345; Miller, 'Bulletin of the American Mathematical Society' (1895), vol. i. p. 258.

gredient simple isomorphisms to itself, these six types of subgroups reduce to two, since the characteristic, self-transform, and self-conjugate subgroups become identical, as well as the isomorphic, transform, and conjugate subgroups. With respect to groups like the operation or abstract groups and the regular substitution-groups, which admit only transform simple isomorphisms to themselves, they reduce to four types, since the characteristic subgroups and the self-transform become identical, as well as the isomorphic and the transform.

There is a special type of characteristic subgroups to which attention should be called, viz. that formed by all the *commutators* of G. It has been called by Dedekind the *commutator subgroup* of G. As G has only one such subgroup, it must correspond to itself in all the simple isomorphisms of G to itself. In fact, a subgroup belonging to any one of the seven types that have been defined must correspond to one of the same type in every simple isomorphism.

As the cogredient isomorphisms correspond to a self-conjugate subgroup of I, this group must have an α, 1 isomorphism to some other group (I′), α being the order of the given self-conjugate subgroup of I. Those α isomorphisms which correspond to the same operator of I′ are said to be of the same *class* *. All of them may be obtained from any one by transforming it with respect to substitutions of G; for the substitution of I which corresponds to the transform of a given isomorphism is obtained by multiplying the substitution which corresponds to the isomorphism into that which corresponds to the transforming operator.

§ 2. *Simple Isomorphisms of* R *to itself.*

Suppose that all the substitutions of R except identity begin with the same element (a_1), and that each one of them is denoted by its second element. No two substitutions will thus be denoted by the same element. Each substitution of the subgroup, which contains all the substitutions that do not involve a_1, of the largest group of degree n that transforms R into itself transforms the substitutions of R in exactly the same manner as its own elements. Hence this subgroup may be considered the group of isomorphisms of R. By changing the notation of the substitutions of R we may obtain any one of the conjugates of this subgroup for the I of R.

Since a transformer must permute at least half of the substitutions of a group if it permutes one, it follows directly from the preceding paragraph that the class of the group of

---

* Hölder, *Mathematische Annalen*, vol. xlvi. p. 326.

degree n which transforms R into all its simple isomorphisms to itself cannot be less than $n \div 2$. In other words, a substitution which transforms a regular group of degree n into itself must be of degree $\overline{\overline{>}} n \div 2$, if it is not identity. We proceed to determine this degree somewhat more accurately.

Suppose that m is the order of the largest maximal subgroup of R, and that some substitution that is commutative to every substitution of this subgroup transforms R into itself; then will the class of the group that transforms R into all its simple isomorphisms to itself be $n-m$. This number cannot be less than $\frac{n(p-1)}{p}$, p being the smallest prime number which is a factor of the order of R. When $p=2$, we have the limit at which we arrived in the preceding paragraph.

The largest group of degree n that transforms R into all its simple isomorphisms to itself has a n, 1 isomorphism to the I of R. To identity in I we may let either R or its associate correspond. The average degree of the n substitutions which correspond to any substitution of I is $n-1$. Hence, some of these must be of degree n, in case at least one of them is of a lower degree than $n-1$.

When R contains no substitution besides identity that is commutative to all its substitutions, it and its associate have only one common substitution. The largest group of degree n which transforms R into all its cogredient isomorphisms is of order n^2. Its subgroup which contains all its substitutions that do not involve a given element, is simply isomorphic to R as well as to its associate. Hence at least such an R must be the transform of its associate.

In general, the substitutions that are common to R and its associate form a characteristic subgroup of both. The quotient group of R and its associate with respect to this subgroup must be of a composite order. The largest group of degree n that transforms R only according to its cogredient isomorphisms is of order $n^2 \div m$, m being the order of the given characteristic subgroup. The subgroup of order $n \div m$ which contains all the substitutions of this group that do not involve a given element is the group of cogredient isomorphisms of R and its associate. When $m=n$, R is commutative, and *vice versâ*.

By multiplying all the substitutions of R, first on the right and then on the left, by each one of them we obtain two squares containing n^2 substitutions, each substitution occurring n times in each square. The substitutions by means of which we obtain all the lines of a square from any one of them form a group which is simply isomorphic to R. Each of these substitutions of one square is commutative to every one of those of the other square. If a substitution is commutative to every substitution of R, it must clearly be of degree n. This proof of the given theorem of Jordan is due to Frattini. It follows directly from this method of proof *that every* R *is the transform of its associate.*

Since all the simple isomorphisms of R to itself can be obtained by transforming the identical isomorphism, we need to transform only one of the two constituent groups in any one such isomorphism in order to obtain all the others. We shall see in the next section that it is not generally possible to obtain all the transforms of a given isomorphism of a general substitution-group in this manner.

§ 3. *Simple Isomorphisms of* G *to itself.*

Let
$$s_1, s_2, s_3, \ldots s_m$$
be m substitutions of order two such that no two of them contain any common element, and that the degree of each exceeds the sum of the degrees of all those which precede it. The group of order 2^α which is generated by any α of these substitutions contains no two substitutions of the same degree. This group has
$$(2^\alpha-1)(2^\alpha-2)(2^\alpha-2^2)(2^\alpha-2^3)\ldots(2^\alpha-2^{\alpha-1})$$
simple isomorphisms to itself. None of these except the identical can be obtained by transforming the identical. The transforms of the others will, in general, lead to different isomorphisms.

The G which we have just given is an extreme case. It is generally possible to obtain a number of different isomorphisms by transforming the identical. If the G which is made simple isomorphic to itself is transitive, and its order exceeds 2, this is always possible. For when it is not commutative, we may obtain such isomorphisms by transforming it by its own substitutions. When it is commutative it must be regular and must have different simple isomorphisms to itself unless it has only one substitution of a given order. In this case it must be the transitive group of order 2.

When the transforms of the identical isomorphism of G do not give the total number of its simple isomorphisms to itself, G has more than one distinct isomorphism. It is important to find the numbers of the substitutions of I which belong to each distinct isomorphism of G. We have observed that the substitutions of I which belong to the distinct isomorphism represented by any transform of the identical form a subgroup of I. We shall represent the substitutions of this subgroup by $s_1, s_2, s_3, \ldots s_\lambda$.

Let t be a substitution of I that belongs to any other distinct isomorphism. Then will

$$ts_1, ts_2, ts_3, \ldots ts_\lambda$$

belong to the same distinct isomorphism. Since the inverse of each one of these corresponds to the interchange of the two constituents in the corresponding isomorphism it must belong to the same distinct isomorphism. If one of these $(ts_a)^{-1}$ is not included in the given line, the following substitutions of I must belong to the same distinct isomorphism,

$$s_a^{-1}t^{-1}s_1, s_a^{-1}t^{-1}s_2, s_a^{-1}t^{-1}s_3, \ldots, s_a^{-1}t^{-1}s_\lambda.$$

All of these are evidently different from the preceding substitutions. If these two lines do not contain all the inverses of their 2λ substitutions, we add a new line by multiplying such an inverse into the given s's, &c.

Since all the substitutions of I that may be obtained in this manner are of the form

$$s_\beta t^\gamma s_\delta \quad (\beta, \delta = 1, 2, \ldots, \lambda : \gamma = 1, -1)$$

their number cannot exceed $2\lambda^2$. It is easy to verify that this maximum is reached in the group of order 168 and degree 7 if we take for the s's its subgroup of order 6. This is the group of isomorphisms of the group of order 8 which contains no substitution whose order exceeds 2*.

An isomorphism may be transformed into another with the same constituent groups (1) by transforming either or both of its constituents, (2) by interchanging the constituents, and (3) by transforming a part of one constituent into a part of the other. All the isomorphisms which may be obtained by the first two methods or their combination correspond to substitutions of I that are included in the given rectangular form. As the last method does not apply to transitive groups we observe that the given form includes all the substitutions of I which correspond to a given distinct isomorphism when G is transitive. When G is intransitive, several of these rectangular forms may belong to the same distinct isomorphism.

From what precedes it is evident that a study of the group

---

* *Cf.* Moore, Bulletin of the American Mathematical Society (1894) vol. i. p. 61.

of isomorphisms with respect to the arrangement of its substitutions in the given form is of great importance in the study of simple isomorphisms. For instance, if the group of isomorphisms of G is the symmetric group of order 6, and its subgroup that corresponds to the transforms of the identical is of order 2 or 3, then will G have just two distinct simple isomorphisms to itself. This is evident when the subgroup is of order 3. When it is of order 2 we may arrange the substitutions of the symmetric group as follows :

$$\begin{array}{lccc} s_1,\ s_2 & \equiv & 1 & ab \\ ts_1,\ ts_2 & \equiv & ac & acb \\ s_2^{-1}t^{-1}s_1,\ s_2^{-1}t^{-1}s_2 & \equiv & abc & bc \end{array}$$

It is clear that we should have arrived at the same result by using either one of the other two subgroups of order 2, since these three subgroups are conjugate. When G is transitive the problem of finding the number of its distinct simple isomorphisms is thus reduced to the following two more elementary problems :—(1) To determine the subgroup of its I which corresponds to all the transforms of the identical isomorphism ; and (2) To arrange the substitutions of I in the given form. When G is intransitive the number of distinct simple isomorphisms cannot exceed the number found in this way.

In making G simply isomorphic to itself it is frequently convenient to inquire whether a given subgroup may correspond to another given subgroup. We have observed that such subgroups must be of the same type. Suppose that this condition is satisfied, and that the corresponding substitutions are, in order,

$$s_1,\ s_2,\ s_3,\ \ldots,\ s_\lambda;\quad s_1',\ s_2',\ s_3',\ \ldots,\ s_\lambda'.$$

Let t_1 and t_2 be two substitutions of G such that (1) all the substitutions of two larger subgroups of G (or of G itself) are of the form

$$t_1^\alpha s_\beta \quad t_2^\alpha s_\beta' \quad (\alpha=1,\ 2,\ \ldots,:\ \beta=1,\ 2,\ \ldots,\ \lambda)\ ;$$

(2) the first power of t_1 that occurs among the s's corresponds to the same first power of t_2 that occurs in the given simple isomorphic subgroups ; and (3) the same powers of t_1 and t_2 transform the corresponding generating substitutions of the given subgroups into the same power of the t's multiplied into corresponding s's : then will these larger subgroups (or G) be also simply isomorphic.

The proof of this statement is evident if we arrange the substitutions as follows:—

$$\begin{array}{ccccc@{\qquad}ccccc}
s_1 & s_2 & s_3 & \ldots & s_\lambda & s_1' & s_2' & s_3' & \ldots & s_\lambda' \\
t_1s_1 & t_1s_2 & t_1s_3 & \ldots & t_1s_\lambda & t_2s_1' & t_2s_2' & t_2s_3' & \ldots & t_2s_\lambda' \\
t_1^2s_1 & t_1^2s_2 & t_1^2s_3 & \ldots & t_1^2s_\lambda & t_2^2s_1' & t_2^2s_2' & t_2^2s_3' & \ldots & t_2^2s_\lambda' \\
\cdot\cdot & \cdot\cdot & \cdot\cdot\cdot\cdot & & & \cdot\cdot & \cdot\cdot & \cdot\cdot\cdot\cdot & & \\
t_1^\rho s_1 & t_1^\rho s_2 & t_1^\rho s_3 & \ldots & t_1^\rho s_\lambda & t_2^\rho s_1' & t_2^\rho s_2' & t_2^\rho s_3' & \ldots & t_2^\rho s_\lambda'
\end{array}$$

$t_1^{\rho+1}$, $t_2^{\rho+1}$ being the first power of the t's that is found in the first row. Under the given conditions the products of corresponding substitutions in the two rectangles must correspond.

§ 4. *The Group* I.

If m is the number of substitutions of G that correspond to themselves in all the simple isomorphisms of G to itself, the degree of I is $g-m$, g being the order of G. It is evident that $m \geqq 1$. Professor Moore has examined the case when I is transitive and of degree $g-1$ *, arriving at some very interesting results. As a rule I is intransitive. Instead of considering the entire group it is frequently convenient to consider a constituent which is simply isomorphic to it. As elements of such a constituent we may take those which correspond to any system of generators of G and to the substitutions to which these generators correspond in any simple isomorphism of G to itself.

I has always a $1, \alpha$ isomorphism to the largest group of degree n that transforms G into itself, n being the degree of G. We have seen that $\alpha = n$ when G is regular. When G is a non-regular transitive group the substitutions that are commutative to each of its substitutions must be of degree 0 or n. Since they form a group their number cannot exceed n. As every substitution of this group is commutative to substitutions whose degree is less than n it must be intransitive. Hence α cannot exceed $n \div 2$ for such a G. If G contains a subgroup of degree $n-1$, α is evidently 1. When I is simply isomorphic to G it is said to be a *complete* group †, and *vice versâ*.

Paris, July 1897.

---

* Moore, Bulletin of the American Mathematical Society (1895), vol. ii. p. 33.

† Hölder, *Mathematische Annalen*, vol. xlvi. p. 325.

XXVII. *On the Electric Resistance of Cobalt, Iron, and Nickel Films in Magnetic Fields of various strengths.* *By* J. C. Beattie, *D.Sc., F.R.S.E.*[*]

THE fact that the paramagnetic metals iron, cobalt, and nickel have a different electric resistance according as the specimens examined are magnetized or not was first observed by Lord Kelvin. Attempts have been made to determine how the change in resistance is related to the magnetic properties of the specimen. Some have found that the variation is proportional to the magnetic moment; others, again, have found other relations. Certain it is that, in these three metals, the variation is an increase when the resistance is measured parallel to the lines of magnetic force, a decrease when perpendicular to them. In the following pages experiments will be described which had for their object the investigation of the relation of the resistance variation to the magnetization in films of cobalt, nickel, and iron transversely magnetized. For such films, Kundt [†] has shown that the Hall effect is proportional to the magnetization. The method employed in the experiments to be described in this paper, was to measure for any one film the Hall effect and the resistance perpendicular to the lines of magnetic force; then to compare these two effects at different field-strengths; and see whether or not any simple relation between the two existed. Did such a relation exist, we could then say in what manner the variation of resistance depended on the magnetization.

The films were deposited on platinized glass by electrolysis. Nickel was obtained from a solution of the double sulphate of nickel and ammonium; cobalt from the double sulphate of cobalt and ammonium; and iron from a solution of four parts of iron vitriol and three parts sal ammoniac in thirty parts water.

The specimens used were cut in the form of a rectangle usually 9 mm. long by 7 or 8 mm. broad; the thickness of the films varied from $\frac{1}{20,000}$ of a mm. to $\frac{1}{5,000}$ of a mm. Care was taken to have the thickness of each film as uniform as possible.

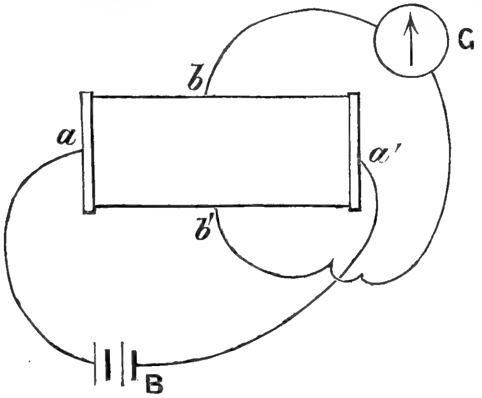
To measure the Hall effect the film had the usual two primary electrodes soldered along the whole extent of the ends, wires from a secondary battery of two cells were soldered

---

[*] Communicated by the Author, having been read before the Philosophical Society of South Africa, Nov. 24th, 1897.

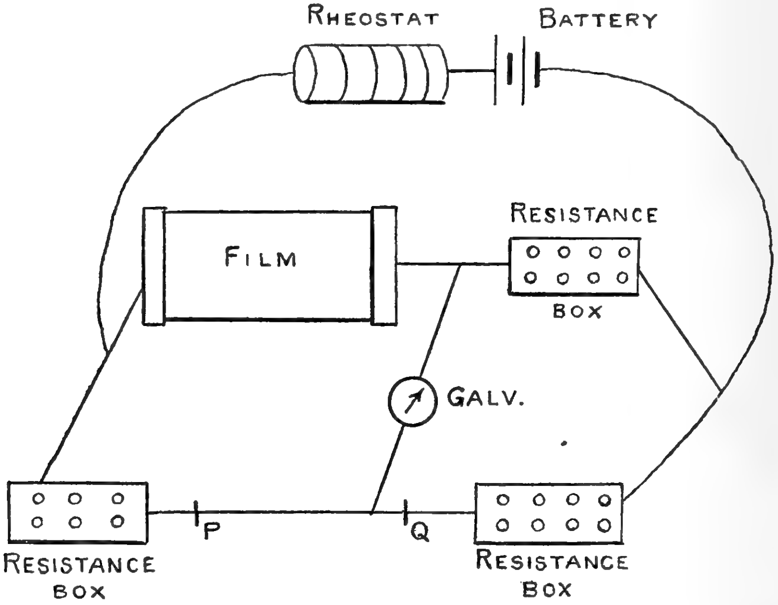
[†] Kundt, Wiedemann's *Annalen*, 1893.

to these ; the current—which we shall call the primary—could be regulated by the insertion of resistances in the primary circuit. Two secondary electrodes were soldered on to two approximately equipotential points lying on opposite sides of the film ; these were joined to the galvanometer.



a, *a'*. Primary electrodes.
b, *b'*. Secondary electrodes.

In those films where the secondary electrodes were not on the same equipotential line when there was no magnetic field, the electrodes could be brought to the same potential by inserting a suitable resistance between one of the secondary electrodes and the properly chosen primary electrode.



To measure the variation of resistance the arrangement shown in the diagram was used.

The film formed one arm of a Wheatstone bridge with resistance-boxes in its other arms, and in addition a graduated german-silver wire, PQ, with a movable contact-maker. The current was supplied by the battery of two secondary cells already referred to; this current had to be very weak—usually a few milliamperes—in order to avoid heating.

The resistances of the four arms of the bridge were made as nearly equal as possible.

When a balance had been obtained in the Wheatstone bridge, the magnetic field was created; the balance previously existing was disturbed, that is, a deflexion was now observed on the galvanometer due to the fact that the resistance of the film had changed. This reading was then assumed proportional to the current flowing through the galvanometer, that is proportional to Δa, the variation of the resistance, a, of the film. Such an assumption gives a result less than the true one by 0·003 of the total result in the most unfavourable case; in other words, with a reading of 300 mm. the error is 1 mm.

The steady magnetic field was created by means of a small Ruhmkorff electromagnet with pole-pieces 10 mm. in diameter and between 3 and 4 mm. apart. The electromagnet with its pole-pieces was pierced through the centre so that the field-strength could be determined by the rotation of the plane of polarization in a piece of glass.

The galvanometer was placed in such a position as to be uninfluenced by the magnet. It had a resistance of 10 ohms. Its scale was placed about 3 metres distant from its mirror.

The Hall effect is given in the results only in scale-readings. The variation of resistance is given in scale-readings and in absolute measure.

To find what variation of resistance in the film was indicated by a galvanometer reading of one scale-division, the following method was employed. A suitable resistance—usually 100,000 ohms—was placed in parallel with an arm of the bridge; the point on the graduated scale which was such as to give no current through the galvanometer was found when the parallel circuit of 100,000 ohms was broken; this latter was then made and the reading observed. Evidently, knowing the resistance of the arm of the bridge concerned, of the shunt, and knowing the deflexion produced, the variation of resistance represented by one scale-division deflexion on the galvanometer could be calculated.

A plus sign denotes an increase, a minus sign a decrease of resistance.

In most of the films the Hall effect was observed first;

then the variation of resistance. The order in which the two are measured affects the result in the case of nickel; in that metal there is a hysteresis effect in both phenomena. In cobalt the hysteresis effect is so small as to be negligible.

To discover whether or not the resistance of the platinized glass and the connecting wires was modified by the presence of the magnetic field, a nickel film was first used; its variation of resistance was observed and the nickel afterwards taken off by means of acid; the platinum mirror and its connecting wires showed no change of resistance due to the creation of the magnetic field.

Cobalt.

The following are some of the results obtained with cobalt films:—

No. XXIII.—Resistance between primary electrodes 2·492 Ω. Primary current $\frac{4}{500}$ amp. ($8\frac{3}{4} \times 13\frac{1}{2}$ mm.)

Field in C.G.S. units.	Hall effect.	Variation of resistance.	$\frac{\text{Hall effect}}{\sqrt{\text{variation}}}$	$\frac{R_1 - R}{R}$
2,400	37·75	−13·75	10·2	−0·0003644
5,100	58·00	−35·0	10·2	−0·0009275
6,600	85·75	−66·25	10·5	−0·001756
8,500	117·75	−130·5	10·3	−0·002458
10,300	135·75	−179·0	10·1	−0·004744
11,700	154·5	−229·5	10·2	−0·006082
13,800	163·75	−243·0	10·5	−0·006440
16,160	168·00	−256·5	10·5	−0·006796
			10·3	

No. XXVI.—Resistance between primary electrodes 2·019 Ω. Primary current $\frac{4}{500}$ amp. ($9\frac{3}{4} \times 7\frac{1}{4}$ mm.)

Field in C.G.S. units.	Hall effect.	Variation of resistance.	$\frac{\text{Hall effect}}{\sqrt{\text{variation}}}$	$\frac{R_1 - R}{R}$
4,300	46·25	−19·0	11·0	−0·000699
5,400	73·0	−34·5	12·3	−0·001269
8,520	114·5	−104·0	11·2	−0·003827
10,300	135·25	−135·75	11·6	−0·004968
11,700	167·0	−183·5	12·9	−0·006726
13,800	183·5	−208·0	12·6	−0·007654
16,160	188·0	−225·0	12·5	−0·008290
			12·0	

The films XXIII. and XXVI. were deposited from a solution of the double salt of cobalt and ammonium; another film was then made from a roseo-cobalt chloride solution. With it the following results were obtained.

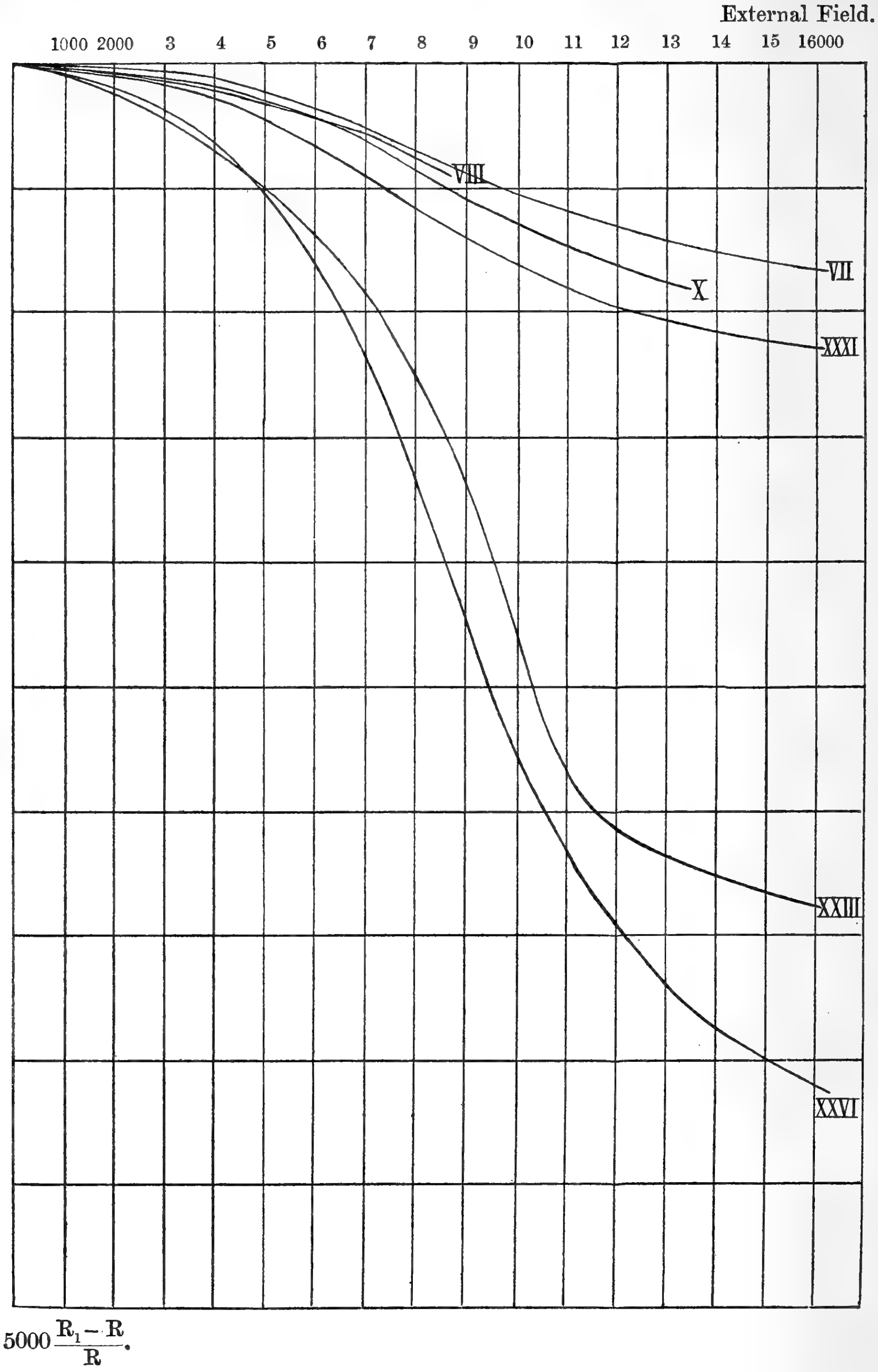
No. XXXI.—Resistance between primary electrodes 4 Ω. Primary current $\frac{4}{500}$ amp. (Size 10 × 7 mm.)

Field.	Hall effect.	Variation of resistance.	$\frac{\text{Hall effect}}{\sqrt{\text{variation}}}$.	$\frac{R_1 - R}{R}$.
2,500	30·0			
4,370	53·5	26·25	10·3	−0·000355
5,590	69·5	43·60	10·6	−0·000592
8,560	105·0	95·0	10·5	−0·001287
11,360	129·5	137·0	11·0	−0·001856
14,070	145·0	155·0	11·6	−0·002100
16,275	152·0	171·0	11·6	−0·002317
			10·9	

Other mirrors were used; some of the results are given in the appended graph. The form of the curve is the same in all. We have first a parabolic part starting at the origin and continuing up to a field-strength of about 9000 units; the form then undergoes a change, and finally the concave part is turned towards the axis. This form and the fact that the Hall effect divided by the square root of the change of resistance is constant justifies the conclusion that in cobalt the difference in the resistance of a thin film of it when transversely magnetized from its resistance when unmagnetized is proportional to the magnetization squared.

The value of this variation differs very much in different mirrors. This can best be seen from the curves, which are drawn to the same scale. No hysteresis effect was observed, although in the Hall effect a slight effect was obtained.

Variation of Resistance in Cobalt.



Nickel Films.

The results obtained with nickel differed very much for different plates both qualitatively and quantitatively. The results were also much complicated by a very great hysteresis effect in the variation of resistance; this caused a greater variation in the film's resistance after it had been magnetized and demagnetized several times.

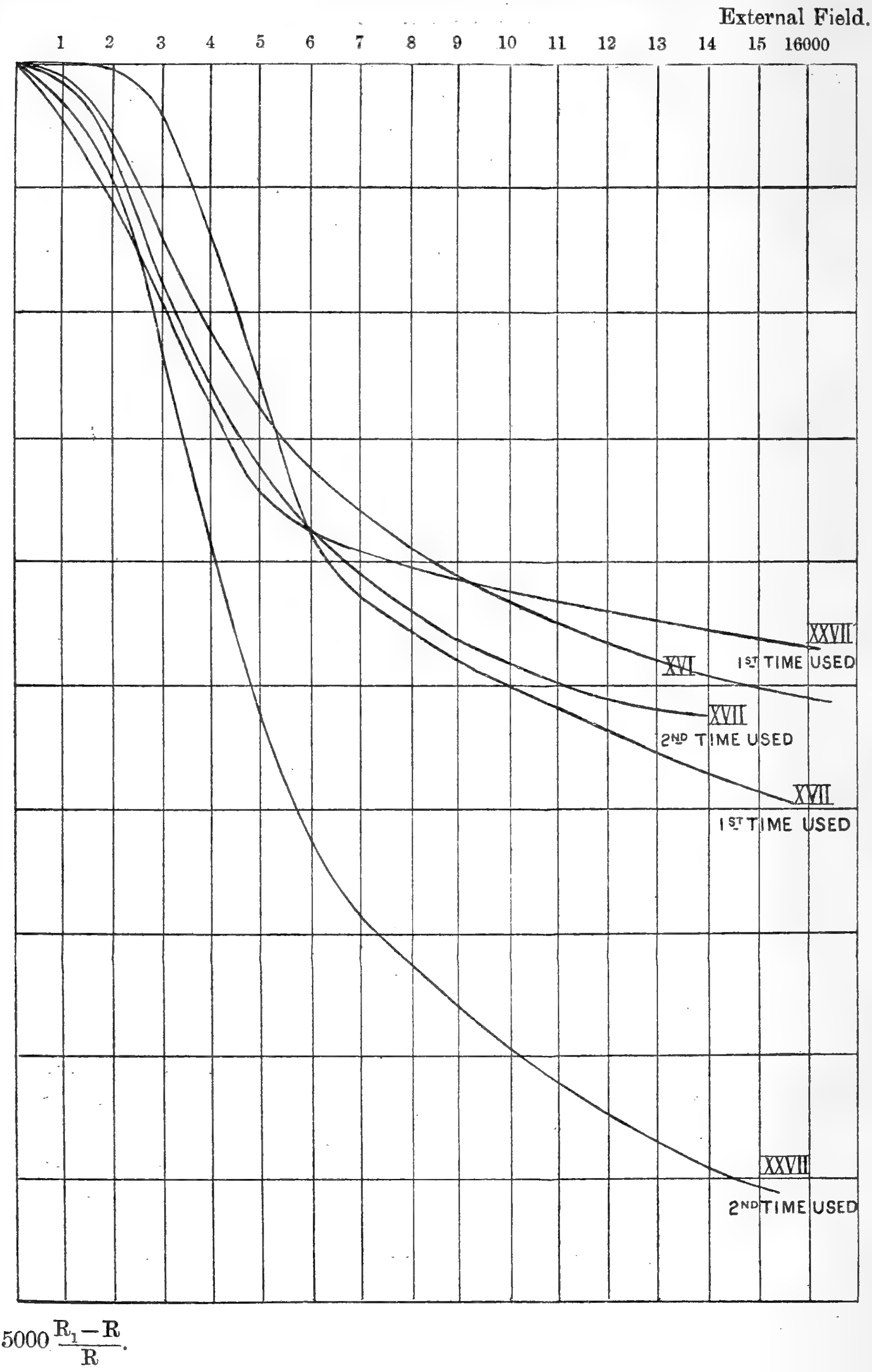
For example, with film XXVII. the following results were obtained.

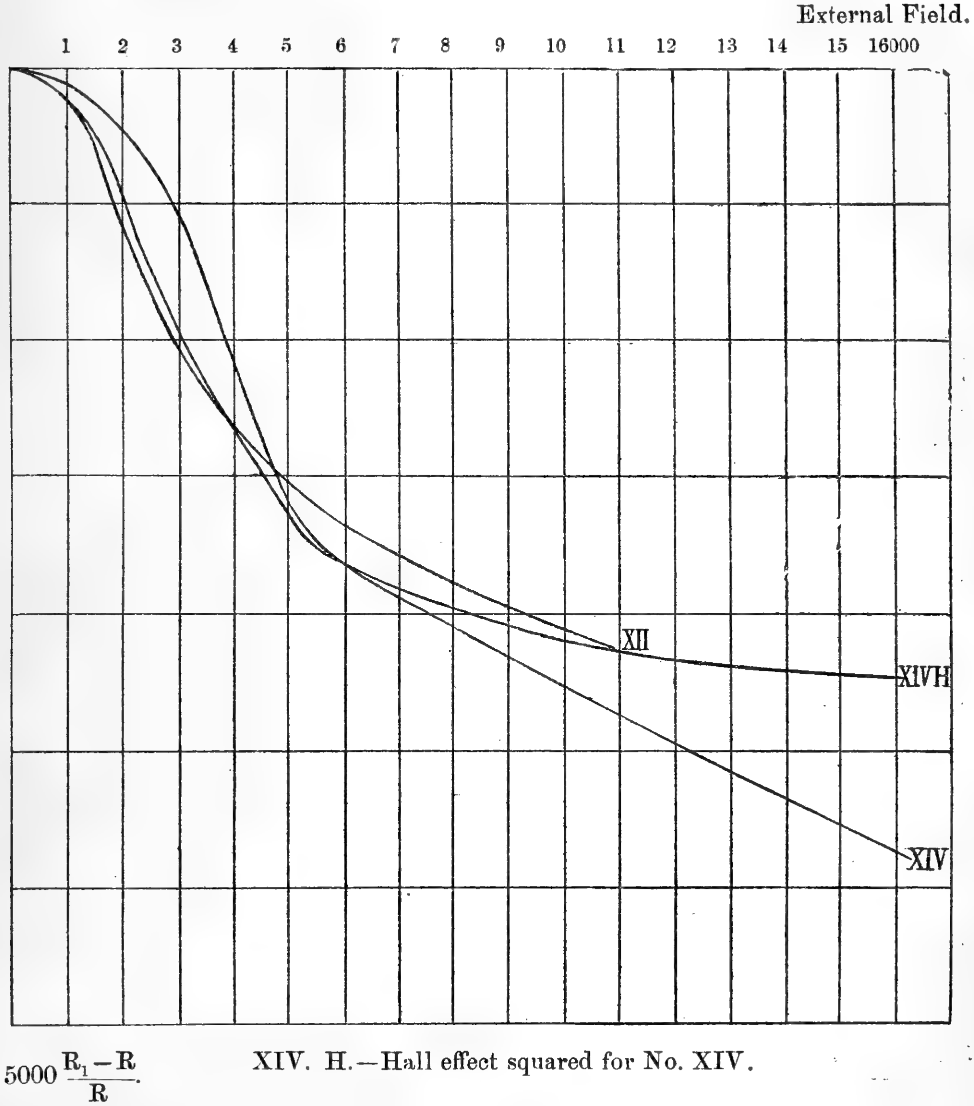
No. XXVII. First time used. ($13\frac{1}{2} \times 8\frac{3}{4}$ mm.)

Field.	Hall effect in millim.	Variation of resistance in millim.	$\frac{\text{Hall effect}}{\sqrt{\text{variation}}}$.
2,620	152·25	− 50·0	21·4
4,370	216·0	− 90·0	22·7
5,590	228·4	−104·5	22·3
8,560	234·5	−116·7	21·7
11,360	241·25	−123·0	21·7
14,070	242·75	−125·0	21·6
16,000	245·5	−134·0	21·2
			21·8
Second time used.			
2,000	111·0	− 45·5	16·4
2,620	152·25	− 86·0	16·4
4,370	216·0	−192·2	15·5
5,590	228·4	−253·0	14·4
8,560	234·5	−316·0	13·1
11,360	241·25	−347·0	12·9
14,070	242·75	−370·0	12·6
16,000	245·5	−388·5	12·4

A great number of other films behaved in the same manner as XXVII. The results for some of them are given in the curves on page 250.

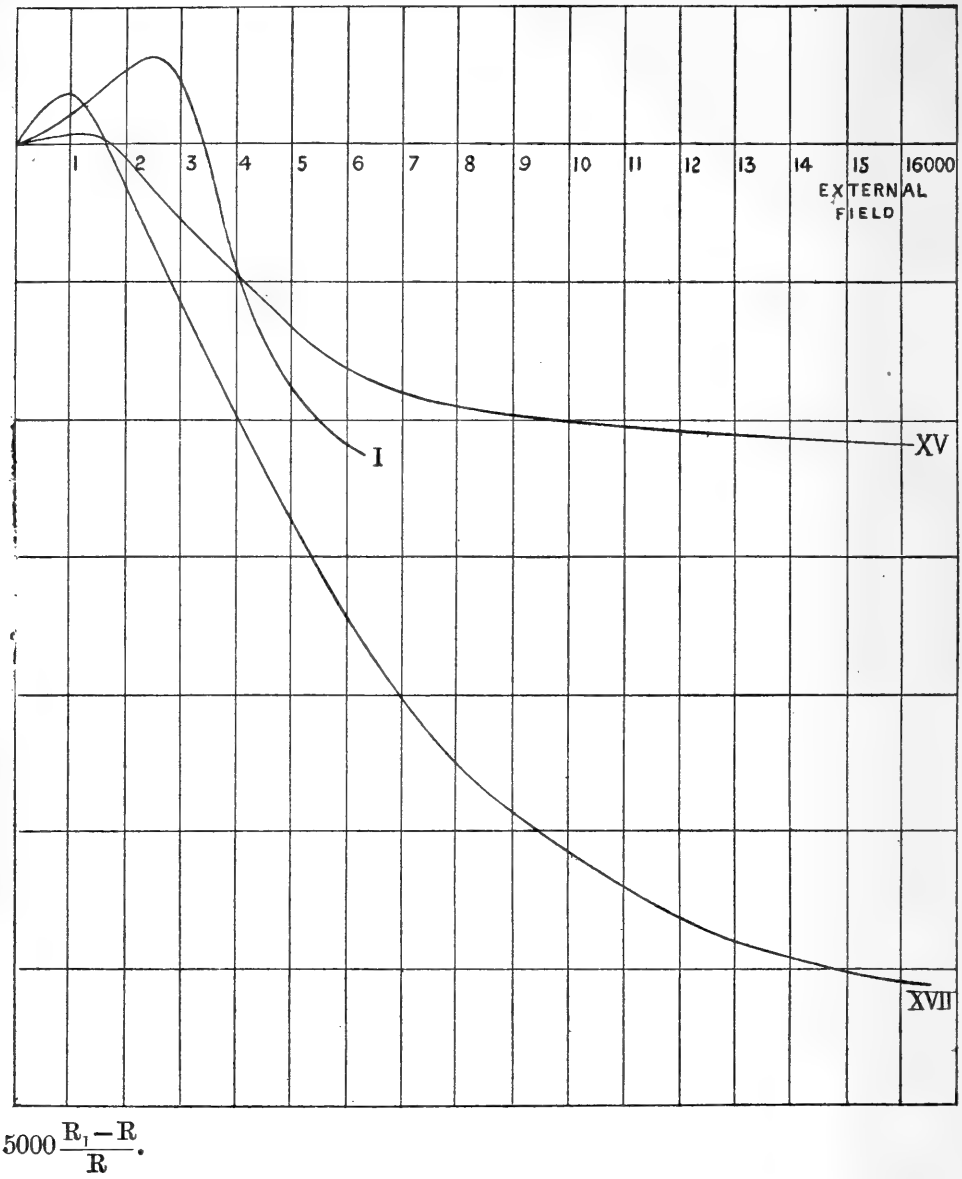
It will be noticed that the form of the curve is first parabolic, with the convex side towards the horizontal axis; that there is a point of inflexion usually between a field-strength of 5000 and 6000 C.G.S. units; and that after that strength the concave part of the curve is turned towards the horizontal axis. Roughly this is the same thing that we had with cobalt; but here we have not usually a constant quantity when the Hall effect is divided by the square root of the resistance variation. The difference between the resistance variation and the Hall effect squared is shown for film XIV. in the graph on page 251.


External Field.
1
2
3
4
5
6
7
8
9
10
11
12
13
14
15
16000
XXVII
1ST TIME USED
XVI
XVII
2ND TIME USED
XVII
1ST TIME USED
XXVII
2ND TIME USED
$5000\frac{R_1-R}{R}$.



XIV. H.—Hall effect squared for No. XIV.

Finally, in some films it was found that the resistance variation was for low fields an increase, and for higher fields a decrease. The results for three films of this nature are shown in the following diagram :—



Iron.

In the iron films the variation of resistance with fields up to 16,000 C.G.S. units due to transverse magnetization was so small that even in the most favourable case not more than 30 scale-divisions' reading could be observed on the galvanometer-scale. So far as conclusions from such slight effects could be relied upon, it appeared as if the resistance variation was not proportional to the magnetization squared.

Conclusions.

(1) Films of nickel, cobalt, and iron show a change in their electrical resistance when brought into a sufficiently strong

magnetic field so as to be transversely magnetized. This variation is greatest in nickel and in cobalt, and least in iron; the variation can be observed in nickel at much lower fields than in cobalt, in some cases with a strength of 1000 C.G.S. units: in cobalt it can be with certainty observed from 2000 C.G.S. units onwards. In iron the variation can usually not be observed with fields weaker than 6000 C.G.S. units.

(2) In cobalt films a direct relation exists between the variation of the resistance and the Hall effect: from it we conclude that the latter is proportional to the magnetization squared.

(3) In nickel the variation of resistance differs very considerably according to the specimen used. In all the mirrors, however, it shows signs of attaining a maximum, but at a higher field than that required for the Hall effect maximum.

Again, the variation becomes greater in some plates after they have been used, so that a plate which originally had a resistance variation proportional to the magnetization squared has, after first being used, a higher variation than this relation would lead us to expect.

(4) In iron the variation of resistance is less than proportionality to magnetization squared would lead us to expect.

I have to express my thanks to Professors Warburg and Rubens for their great kindness and assistance to me while engaged in carrying out the above experiments.

South African College, Capetown,
December 1897.

XXVIII. *On the Determination of the Conductivity of Liquids in Thin Layers.* *By* G. B. BRYAN, *B.Sc.*, 1851 *Exhibition Science Scholar, University College, Nottingham, St. John's College, Cambridge* *.

INTRODUCTION.

INTEREST in this subject has been aroused by the work of Koller †, who found that, with certain liquids, the conductivity decreased as the thickness of the layer was decreased; and it was suggested by Prof. J. J. Thomson that it might be worth while repeating some of Koller's experiments. In doing this the apparatus and method of measuring the resistance differed from Koller's, and therefore his method of experimenting will be briefly described.

To obtain a thin layer of a liquid, Koller took two flat circular metal plates, and placed one horizontally on the bottom of a glass vessel; the other was laid on this and

---

* Communicated by Prof. J. J. Thomson, F.R.S.

† *Wien. Ber.* 98 ii. *a*, p. 201 (1889).

separated from it by three small pieces of glass, all of the same thickness, which was carefully measured. These plates formed the electrodes, and as the glass vessel was of very little greater diameter than the plates, the specific resistance of the liquid was calculated from the formula, Sp. R. $= ra/d$, where a is the area of the plates, d the thickness of the layer, and r its resistance. Two pairs of plates were used; one pair of brass, gilded and polished, and the other pair of zinc, carefully amalgamated. The diameter of the gilded plates was 6·6 cm.; that of the zinc plates was slightly less.

To measure the resistance between the electrodes they were connected in series with a standard resistance and a battery of constant E.M.F. The potentials at the terminals of the electrodes and the standard resistance were compared by means of an electrometer, and the unknown resistance thus determined. Readings were taken at a given time, T, after starting the current.

Koller tested a large number of substances, including turpentine, benzol, toluol, xylol, and CS_2, with which the gilded electrodes were used. The following is a specimen of the results obtained:—

Turpentine.

E.M.F. = 1 Daniell Cell. Temp. = 24° C.

d.	Specific Resistance in C.G.S.	
	T = $1\frac{1}{2}$ minutes.	T very great.
0·15 mm.	$228 . 10^{19}$	$273 . 10^{19}$
1·135 „	$20·5 . 10^{19}$	$176 . 10^{19}$
7·49 „	$2·37 . 10^{19}$	$158 . 10^{19}$

Water, alcohol, and ether were also tested, and the following figures obtained:—

Water.

E.M.F. = 1 Daniell Cell. Temp. = 24° C.

d.	I. T = 1 minute.	II. T = 2 minutes.
	Sp. Resistance.	Sp. Resistance.
1·135 mm	$109 . 10^{12}$	$136 . 10^{12}$
3·015 „	80	74
7·49 „	58	51

Alcohol.

E.M.F. = 1 Daniell Cell. Temp. = 24° C.

d.	I. T = 4 minutes.	II. T = 1 minute.
	Sp. Resistance.	Sp. Resistance.
1·135 mm.	$251 . 10^{13}$	$162 . 10^{13}$
3·015 „	123	135
7·49 „	101	84

Ether.

E.M.F. = 1 Daniell Cell. Temp. = 24° C.

d.	T = 1 minute.
	Sp. Resistance.
1·135 mm.	$160 . 10^{13}$
3·015 „	118
7·49 „	111

With water, alcohol, and ether the zinc electrodes were used.

The above figures were taken from a large series of readings which all agreed in showing that the specific resistance increased as the thickness of the layer decreased.

Koller concluded, from his experiments, that this effect was due neither to "transition resistance" nor to polarization, but to dielectric effects. If this were the case it was thought that the subject would be well worth further investigation.

DESCRIPTION OF APPARATUS.

In carrying out the present investigation it was decided, after some preliminary experiments, not to use glass separators between the plates, for being small they were troublesome to work with, and also necessitated the removal of the upper plate and disturbance of the liquid when it was required to change the thickness of the layer. It was therefore resolved to attach the upper plate to a vertical screw, and a piece of apparatus was found in the laboratory which, it was thought, would answer the purpose very well. It is shown in fig. 1. The micrometer-screw S carries two parallel brass plates p_1, p_2; p_1 is fixed at right angles to the screw, and p_2 attached to p_1 by three adjusting screws $s_1 s_1 s_1$ round the circumference. From p_2 is suspended the upper electrode A, by means of a glass rod g_1.

Fig. 1.

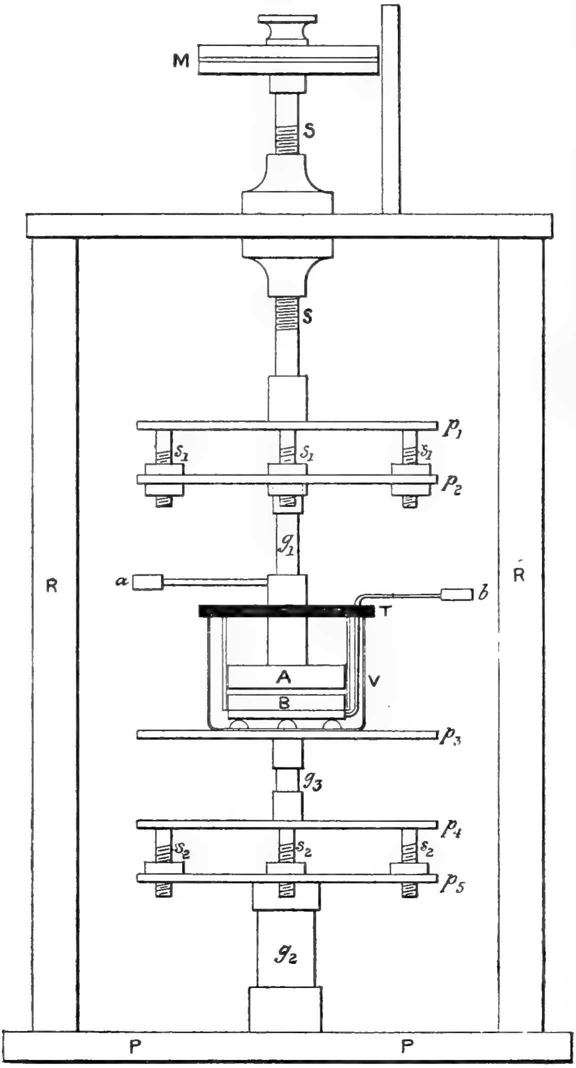


Diagram of First Form of Apparatus. ¼ nat. size.

To the iron base-plate P is attached a vertical glass rod g_2, which carries two brass plates p_5, p_4 connected together by adjusting screws $s_2 s_2 s_2$ similarly to the plates p_1, p_2. p_3 is another brass plate connected to p_4 by the glass rod g_3, and on this plate is placed the glass vessel V which contains the lower electrode B, resting on three glass points. The glass vessel is covered by an ebonite plate T which fits loosely round the glass rod g_1. The lead wire, b, from the lower plate is brought up through a thin glass tube.

The micrometer-screw has 20 threads to the inch, and passes through a very long nut; and as it carries a considerable weight there is no backlash. The micrometer head is divided into 100 parts, so that one division represents $\frac{1}{2000}$ inch. The screw was examined by observing the edge of the upper plate A with a microscope, and the error was found to be so small as to be negligible.

Adjustment of the Plates.—(1) As the upper plate A rotates with the screw it must be made to move parallel to itself. To do this the lower edge of A was observed with a horizontal microscope containing a micrometer-scale; each of the three screws $s_1 s_1 s_1$ was brought opposite the microscope in turn (the exact position being read on the divided head) and adjusted until the downward or upward motion during each third of a revolution was the same.

(2) The lower plate B must then be made parallel to the upper one. To do this a strong light was placed behind the plates and looked at through the space between the plates. The screws $s_2 s_2 s_2$ were adjusted until, on screwing down the upper plate, the light was extinguished right across at the same instant. The light was moved round the plate, and when it disappeared at the same moment, whatever its position, the adjustment was assumed to be correct, and the reading of the divided head at that instant was taken as the zero. This method was tested by making the adjustments with the glass vessel removed, and then measuring the thickness of a piece of thin glass at several points round the edges of the plates. The measurements were found to agree very closely amongst themselves and with the thickness of the glass, as measured by a spherometer, to ·01 mm. The adjustment was probably not always as good as this; but it could not be tested when the glass vessel was in position.

The Plates.—The plates first used were of brass, 10 cm. in diameter and 3 mm. thick. They were ground flat on an emery-wheel, and then on plate-glass with fine emery-powder. They were then carefully amalgamated. It was found impossible to get these plates perfectly flat on account of their springiness; both were found to be slightly concave. The correction for this was estimated to be 0·013 turn of the screw $=0·0165$ mm., to be added to the distance between the plates given by the reading of the divided head.

As these plates were not considered satisfactory two others were made. These were 5·64 cm. in diameter and about 1 cm. thick, so as to avoid springiness. The metal was zinc.

They were turned up all over and the surfaces were then ground on plate-glass with fine emery until, after washing, one plate would hold the other up when they were pressed together. These plates were then sent to a jeweller's to be gilded, and unfortunately the gilder, contrary to instructions, cleaned the surfaces in the usual way and rather spoilt them; they were found to be slightly irregular, so that a correction could not be estimated. Thus the only advantage of these plates over the others was the gilding.

METHODS OF DETERMINING THE RESISTANCE.

Three methods of finding the resistance were tried in various cases:—

(1) Comparison with a standard resistance by direct deflexion, using a continuous current from a battery of secondary cells. The galvanometer used had a resistance of 17,380 ohms and gave a deflexion of one division with a current of about 10^{-8} amperes.

Fig. 2.

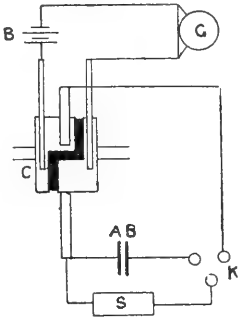


Fig. 4.

AB. Electrodes.
C. Commutator.
B. Battery.
G. Galvanometer.
S. Standard resistance.
K. Key.
PQR. Slide wire.

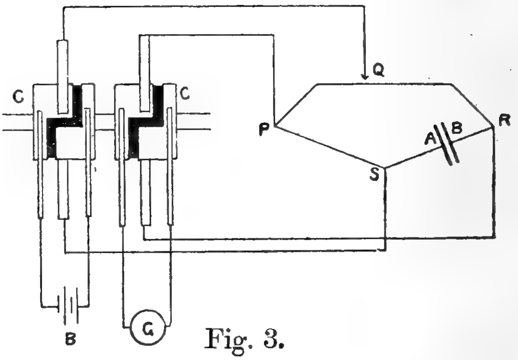


Fig. 3.

(2) Comparison with a standard resistance by direct deflexion, using an alternating current. The same galvanometer was used as in (1). The alternating current was obtained by means of a revolving commutator. This commutator had two brass segments on an ebonite drum, and is shown, together with the connexions, in fig. 2. The commutator was

driven by means of a small water-motor. It will be seen from the diagram that the current through the galvanometer was always continuous, but the sensibility was reduced slightly owing to the insulating spaces between the segments of the commutator. When the four brushes were set right the deflexion was quite steady and was not affected by slight changes in speed, with the liquid layer in circuit.

(3) The third method was the modification of the Wheatstone bridge described by T. C. Fitzpatrick *. By means of a double commutator, designed by Prof. Threlfall, an alternating current is supplied to two corners of a Wheatstone bridge, whilst the connexions to the galvanometer are reversed at the same time as the current, so that any current through the galvanometer is always in one direction. The commutator employed at first had two drums, each with two brass segments and four brushes. It is shown in fig. 3, with the connexions. Driven at full speed the commutator gave about 60 alternations per second.

METHOD OF EXPERIMENTING.

With the direct deflexion methods of measuring the resistance the upper electrode was set at the required distance from the lower one, the current switched on, and a reading of the galvanometer taken at a definite interval of time, T, afterwards; usually about 1 minute. A series of readings was taken, increasing the thickness of the layer before each reading, and then a series decreasing the thickness. The means of these two sets, when they agreed fairly closely, were usually taken.

In the case of the bridge-method the measurements were made as quickly as possible one after the other, so as to avoid any changes in the state of the liquid, or of the surfaces of the electrodes.

The specific resistance is calculated from the formula $\text{Sp. R.} = \frac{ra}{d}$.

RESULTS WITH FIRST FORM OF APPARATUS.

The first liquids tried were paraffin-oil, turpentine, benzol, and CS_2, using method (1) to find the resistance; but their resistance was so high that only a small deflexion could be obtained with the thinnest layer (·317 mm.), about 50 mm at the most. No results were therefore obtained for these substances.

---

* B. A. Report, 1886.

Aniline was then tested, its conductivity being much higher. The amalgamated plates were used. The standard resistance was a megohm box; the battery, five secondary cells. The galvanometer was shunted.

A large number of readings were taken with aniline, which was distilled several times during the experiments. Its resistance was found to change considerably with the duration of the current, increasing as a rule *.

Thus for a sample of brown aniline :—

Table I.

E.M.F.=10 volts. $\theta=11°$ C.

d, mm.	T, minutes.	*r*, ohms.
·65	½	11,800
	1	14,000
	5	21,000
	20	23,800
1·28	½	16,400
	1	20,800
	5	34,000
Same aniline distilled.		
·65	½	298,000
	4½	329,000
	20	400,000
	120	343,000
1·28	½	542,000
	1½	590,000
	8½	573,000
	120	623,000

These figures show that the change is very considerable, especially in the first few minutes. Two causes were found to be producing this change: (1) due to the metal electrodes, which were found to produce a slow darkening of the aniline, and a slow *decrease* in the resistance. (2) Due to the passage of the current; this produces an increase of resistance, and the rate of this increase depends upon the purity of the aniline. Thus before distilling, with a thickness of 0·65 mm., the resistance rose in 20 minutes from 11,800 to 23,800, *i. e.* 100 per cent., whilst after distilling, the resistance rose from 298,000 to 400,000, *i. e.* 30 per cent., in the same time.

After passing the current in one direction for some time,

---

* *Cf.* Warburg, Wied. *Ann.* liv. 1895.

on reversing it the resistance appeared to drop suddenly to a minimum, and then begin to rise again gradually. The following figures show this effect with a layer ·628 mm. thick. D is the deflexion of the galvanometer:—

TABLE II.

T, minutes.	D.	r, ohms.
2½	156	28,300
13½	114	38,700
Reversed.		
13¾	*144*	*30,600*
14	124	35,600
24	108	40,900
Reversed.		
24½	*182*	*24,200*
31½	126	35,000

With a thicker layer the fall of resistance after reversal was not so sudden. Thus for a thickness of 2·55 mm.:

TABLE III.

T, minutes.	D.	r, ohms.
½	153	104,000
5½	74	236
Reversed.		
5¾	86	203
6	93	188
8½	*134*	*131*
9½	131	133
15	85	206
Reversed.		
15¼	96	182
15½	110	159
17½	*145*	*121*
22½	102	171
Reversed.		
22¾	118	148
23	139	126
23½	*144*	*121*

The same results were obtained after distilling the aniline and thus decreasing its conductivity about 20 times.

These figures show that there is probably considerable polarization of the electrodes, or else some change in the liquid which is reversed with the current, and most likely takes place at the electrodes.

The polarization was measured in some cases, just after stopping the current, by balancing against a Clark cell. As much as 0·3 volt was found with a thickness of 0·628 mm., which would not account for the large increase in resistance during the passage of the current, since the applied E.M.F. was 10 volts.

The polarization, however, fell off very rapidly and might have been much greater whilst the current was on.

Hence it would appear that although the conductivity of aniline might not vary with the thickness of the layer, it would be very difficult to prove it by means of a continuous current.

A large number of readings were taken with different thicknesses, and on the whole they appeared to show that the conductivity decreased with the thickness. The following are specimens of the results obtained:—

TABLE IV.

Distilled Aniline. E.M.F. = 10 volts. $\theta = 9°$ C.

T=1 min.			*d*, mm.	T=1 min.	T=17 min.
d, mm.	*r*, ohms.	Sp. R. C.G.S. $\times 10^{-15}$.		Sp. R. $\times 10^{-15}$.	Sp. R. $\times 10^{-15}$.
0·245	30,300	119	·334	85·1	134
0·651	89,300	110	·651	84	125
1·29	150,000	94	1·29	81·8	93·3
2·55	232,000	73·4	10·17	41·7	51
5·10	370,000	58·3			
7·64	464,000	49			
10·17	880,000	46·6			

In these experiments the current-density is much greater in the thin layers than in the thick ones. The following are some readings in which the current-density was kept constant by varying the E.M.F. by means of a liquid rheostat until the deflexion of the galvanometer was approximately the same in each case. The time taken to do this was about 1 minute:—

TABLE V.

d, mm.	Sp. R. $\times 10^{-15}$.
·334	85·6
·651	48·1
1·29	35·0
2·55	35·8
5·10	27·7
7·64	25·8
10·17	25·2

The difference of conductivity between the thick and thin layers is here much greater than before.

As this difference might be due to the rise in resistance caused by the current immediately after starting being greater with the thinner layers than with the thicker ones, it was decided to try the effect of an alternating current, and the second method of measuring the resistance was used.

The aniline was redistilled and the plates were cleaned with alcohol and readjusted.

The results at first obtained appeared to show that the conductivity *increased* as the thickness of the layer decreased. But it was found that, as time progressed, whilst the conductivity of the thinnest layer remained practically constant, that of the thicker ones gradually increased until a steady state was reached, when it was nearly the same for all layers. This is shown in the following tables:—

TABLE VI.

E.M.F. = 10 volts. Temp. = 11°.

d, mm.	Specific Resistance × 10^{-15}.			
	(1)	(2) 5½ hours after (1).	(3) 29 hours after (1).	(4) 49 hours after (1).
·175	...	...	43·9	37·9
·334	39·9	42·5	44·8	34·0
·651	50·8	49·1	45·1	
1·290	80·0	59·1	45·2	37·4
2·55	123	74·0	45·0	
5·10	165	69·5	44·2	
10·17	192	64·2	43·4	36·5

The readings in the fourth column are one set out of a large number, taken on the same day, which agreed in showing that the conductivity was practically the same for all layers.

The aniline was redistilled and again tested. The conductivity was now much less, but the same gradual change to a steady state was found to occur.

TABLE VII.

E.M.F. = 10 volts. $\theta = 11°$.

d, mm.	Specific Resistance, C.G.S. × 10^{-15}.		
	(1)	(2) 18 hours after (1).	(3) 40 hours after (1).
0·175	105	124	114
·334	111	127	107
·651	130	133	111
1·29	159	139	108
2·55	209	143	110
5·10	242	147	112
10·17	263	147	112

Water.—The ordinary water from the laboratory-still was used. This had a comparatively high conductivity, and was in fact a dilute solution. The gilded zinc electrodes were employed, and to keep the conduction as much as possible to the space between the plates they were surrounded by a close-fitting inner cylinder of glass, as shown in fig. 1.

Before putting the plates into position they were cleaned with dilute acid and alcohol.

The resistance was measured by method 3, two Daniells being used to give the E.M.F.

A large number of readings were taken, and they all agreed in showing that the apparent specific resistance was considerably greater in the thin layers. The plates were removed and cleaned several times, but the results were always the same. The following is one set of readings:—

TABLE VIII.—*Distilled Water.*

Temp. 12° C.

Turns of screw.	d, mm.	r, ohms.	Sp. R. C.G.S. $\times 10^{-11}$.	Differences between the nos. in col. 3.	Resistance of 1 turn = ·127 mm. from last column.
·1	·127	65·9	1055	66·1	330
·3	·381	132	704	70·0	350
·5	·635	202	646	173	346
1·0	1·270	375	600	365	365
2·0	2·54	740	592	698	349
4·0	5·08	1438	575	1002	334
7·0	8·89	2440	559		
					345·7 mean.

The values of the specific resistance in the fourth column show a large falling off as the thickness increases. In the fifth column are given the differences between consecutive values of the resistance in column 3, and these are multiplied by constants to give the resistance of a thickness of ·127 mm. The approximate equality of these numbers shows that the great increase in specific resistance in the thin layers is probably due to the plates not being perfectly clean to begin with; to polarization, and changes at the electrodes produced by the current; and to errors of adjustment. As the end effect was probably rather different with different thickness, the following experiment was made to find the conductivity at each position of the plates.

At each position the resistance was measured, then the upper plate was screwed down one-tenth of its distance from the lower plate and the resistance again measured. The plate was then brought back to its original position, and one-tenth above it, the resistance being measured in each case. This series was repeated several times and the differences between consecutive readings gave the resistance of a layer equal to one-tenth the distance between the plates.

From the mean of these differences was calculated the resistance of a layer equal to one turn of the screw (1·27 mm.). Thus, at the position $d = \cdot 5$ turn, the readings were :—

Table IX.

d, turns.	r, ohms.	Differences.
·50	202·5	16·5
·55	219	16·5
·50	202·5	17·0
·45	185·5	17·0
·50	202·5	17·5
·55	220	18·5
·50	201·5	17 9
·45	183·6	16·4
·50	200	
		17·16 mean resistance of 0·05 turn.

Whence resistance of 1 turn = 343·2 ohms.

The mean values for all positions were :—

Table X.—*Distilled Water.*

d, turns.	d, mm.	Resistance of 1 turn at each position.
·1	·127	357
·3	·381	363
·5	·635	343
1·0	1·27	340
2·0	2·54	353
4·0	5·08	349
7·0	8·89	343
		349·7 mean.

The greatest difference from the mean is about 4 per cent., and the mean is about 1 per cent. greater than that in Table VIII.

Alcohol (absolute).

TABLE XI.

d, turns.	d, mm.	r, ohms.	Sp. R. C.G.S.$\times 10^{-12}$.	Differences.	Resist. of 1 turn.
·1	·127	572	915		
·2	·254	921	737	349	3490
·3	·381	1286	686	365	3650
·5	·635	2005	641	719	3595
1·0	1·27	3815	610	1810	3620
2·0	2·54	7410	592	3595	3595
					3590 mean.

Here again there is a great increase in the specific resistance in the thin layers, whilst the differences between the layers give an approximately constant value for the resistance of one turn, as was the case with water. For this reason it was expected that by improving the apparatus the differences in the apparent conductivity of the layers could be considerably reduced.

DESIGN OF NEW APPARATUS.

The chief improvements to be made were :—

(1) To make the upper plate move parallel to itself without turning with the screw. This would increase the accuracy of the adjustment as well as reduce the time required for it.

(2) To so fix the plates that they could be removed after adjusting, to be cleaned, and replaced without spoiling the adjustment. The liquid could then be poured into the vessel immediately after refixing the plates, so that there would be much less time for them to get dirty again than in the old arrangement.

(3) To improve the surface of the plates.

(4) To increase the rapidity of alternation of the current so as to reduce the polarization.

With the rate of alternation already used (about 60 per sec.) the balance-point with the very thin layers of water was not quite steady, and it was thought that this was probably due to slight polarization.

The new arrangement for holding the plates is shown in fig. 5. My best thanks are due to Professors Heaton and Robinson, of University College, Nottingham, for the use of their workshops while I was making this apparatus.

Condition (1) is fulfilled by attaching the upper plate A to a steel rod r which slides without turning in the two v's, v_1 v_2.

Fig. 5.

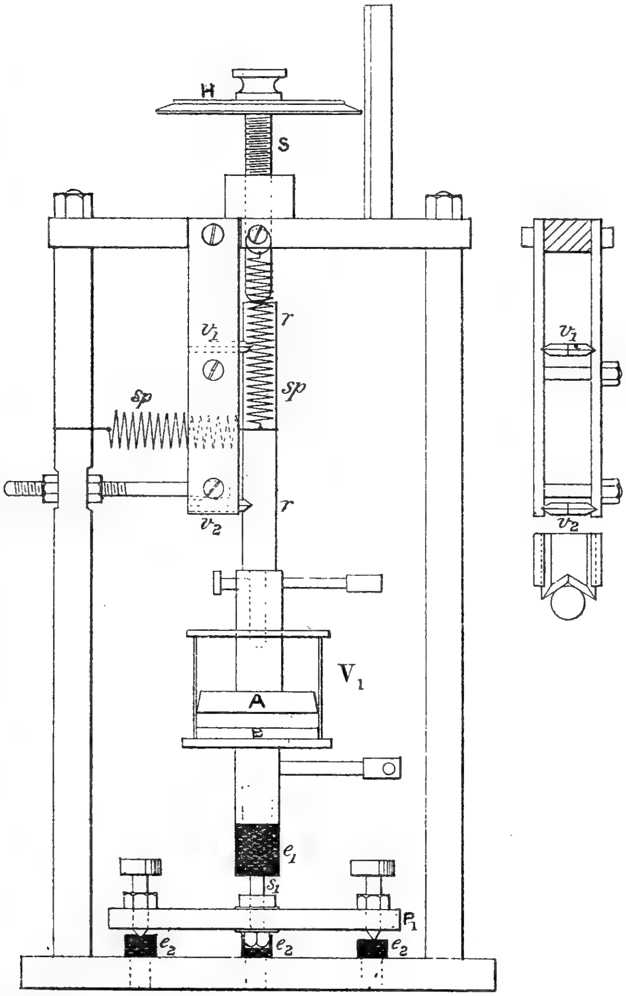


Diagram of Second Form of Apparatus. $\frac{1}{4}$ nat. size.

Condition (2) is fulfilled thus:—The shaft of the upper plate has a $\frac{1}{4}$ in. hole bored in it, and slips without shake on to the reduced end of the steel rod r, and is fixed by a set-screw which screws into a small hole in the rod and draws the shaft tight up against the shoulder at the same time. The lower plate B screws on to an ebonite rod e_1 which is attached by means of the iron screw s_1 to the iron plate P_1; s_1 passes through a hole much larger than itself so as to admit of some lateral adjustment. The plate P_1 is fixed laterally by the three levelling-screws which rest, by hole slot and plane, on three ebonite plugs e_2, e_2, e_2. After adjusting the lower plate B parallel to the upper one the levelling-screws are fixed by lock-nuts, and the plate can then be removed and replaced without impairing the adjustment. The glass vessel V_1 consists of a cylinder ground to fit the shoulder on the lower plate, and of such internal diameter that the upper plate just slides easily inside it. Just before pouring in the liquid to be tested this cylinder was fastened down with clean paraffin wax.

The Plates.—These were of cast brass, turned to the shape shown in the figure. The surfaces were very carefully scraped up to a good surface-plate, until, after cleaning, one would hold the other up (weight of each plate about 1 lb.). They were then gilded, great care being taken not to damage the surfaces in any way, and only a very thin coating of gold was put on, just enough to prevent oxidation. These plates were very satisfactory, and the error due to irregularities of the surfaces negligibly small. Area of plates $=20{\cdot}82$ sq. cm.

To realize condition (4) a new commutator was made. It is similar to the one shown in fig. 3, but each drum has eight segments instead of two, thus increasing the number of reversals per revolution four times. If the drum were rolled out it would appear as in fig. 4. Two brushes press on the outer rims, and two others on two consecutive teeth.

This commutator was driven by an electromotor, and the speed could be varied so that from 100 to 250$\sim$ per second of the current could be obtained. The speed used gave about 200$\sim$ per second.

The galvanometer used was a dead-beat one of 1430 ohms resistance, and figure of merit, with the interrupted current, of 10^{-7} ampere.

The connexions were the same as in fig. 3. The adjustment of the lower plate parallel to the upper one was made in the same way as before: but could now be easily tested by placing a piece of glass of known thickness between the plates. The error was found to be about ·01 mm., but was probably sometimes less than this.

The micrometer-screw s has 30 threads per inch, and the head H is divided into 100 parts. The screw was compared with a standard.

RESULTS WITH SECOND FORM OF APPARATUS.

Battery, 2 Leclanché's. Temp. about 16° C.

TABLE XII.—*Distilled Water.*

d, turns of screw.	d, mm.	r, ohms.	Sp. R. C.G.S. $\times 10^{-12}$.
·1	·0846	54	133
·2	·169	95	117
·5	·423	225	110·5
1	·846	433	106·6
2	1·692	875	107·5
5	4·23	2149	105·6
7	5·92	3026	106·3
10	8·46	4310	106

TABLE XIII.—$CuSO_4$. 1 *gm. in* 5,000 *c.c. water.*

d, turns of screw.	d, mm.	r, ohms.	Sp. R. C.G.S. $\times 10^{-10}$.
·2	·169	5·15	633
·5	·423	12·2	599
1	·846	24·0	590
2	1·692	47·4	582
5	4·23	119	584
10	5·92	237	582
20	8·46	472	581

TABLE XIV.—$CuSO_4$. 1 *gm. in* 50,000 *c.c. water.*

d, turns.	d, mm.	r, ohms.	Sp. R. C.G.S. $\times 10^{-11}$.
·4	·338	54	331
·5	·423	66	322
·6	·508	77	315
·8	·676	100	307
1	·846	122	300
2	1·692	243	299
5	4·23	621	305
10	8·46	1260	310
20	16·92	2510	309

TABLE XV.—KCl. 1 *gm. in* 10,000 *c.c.*

d, turns.	d, mm.	r, ohms.	Sp. R. C.G.S. $\times 10^{-10}$.
·5	·423	12·4	609
1	·846	23·6	582
2	1·692	47·1	579
5	4·23	117·7	579
10	8·46	236·5	582
20	16·92	471·7	580

TABLE XVI.—*KCl.* 1 *gm. in* 50,000 *c.c.*

d, turns.	*d*, mm.	*r*, ohms.	Sp. R. C.G.S. × 10^{-11}.
·1	·0846	10·5	258
·2	·169	18·8	231
·5	·423	45·5	223·4
1	·846	90	221·5
2	1·69	182	224
5	4·23	458	225
10	8·46	917	225·5
20	16·92	1833	225

TABLE XVII.—*Absolute Alcohol.*

d, turns.	*d*, mm.	*r*, ohms.	Sp. R. C.G.S. × 10^{-13}.
·1	·0846	459·5	113
·2	·169	959	118
·5	·423	2436	120
1	·846	4970	122·3
2	1·692	9965	122·6
4	3·384	19960	122·4
8	2·768	39800	122·2
12	10·152	60300	123·8

TABLE XVIII.—*Aniline.*

(Pure, but had been in store some time, and was slightly brown.) Battery, 5 accumulators.

d, turns.	*d*, mm.	*r*, ohms.	Sp. R. C.G.S. × 10^{-14}.
·2	0·1692	20,500	252
·4	0·3384	41,100	252
·5	0·423	51,500	253
1	0·846	103,900	256
2	1·692	205,800	253
5	4·23	505,000	248
10	8·46	1,010,000	248

In all the above readings the current was not kept on longer than was necessary to obtain a balance, so as to avoid changes in the liquid which might be produced by the passage of the current, especially in the case of the thinnest layers of dilute solutions, where the resistance was small. With aniline quickness was very necessary: for, as was found previously, its conductivity is altered very much by the passage of the current.

CONCLUSION.

It will be seen that the expectation of reducing the apparent differences in conductivity of the different thicknesses (Tables VIII. and XI.) has been realized to a great extent; in most cases reduced within the limits of error of adjustment. This is partly due to the improvement in the surfaces of the plates, and partly to better cleaning. The plates were washed in alcohol, after dipping in dilute sulphuric acid, until the liquid to be tested would thoroughly wet them.

The readings, in the case of the very thin layers of dilute solutions, depended very much on the state of cleanliness of the plates. Thus for water (Table XII.) the Sp. R. of ·1 turn is 27 per cent. greater than that for 10 turns (of this difference about 12 per cent. can be accounted for by error of adjustment); whilst for very dilute KCl (Table XVI.) when the plates were more carefully cleaned, the corresponding difference is only 15 per cent., although the resistance is five times less.

With alcohol (Table XVII.) the readings from 1 to 12 turns agree very closely, and below 1 they are within the limit of error of adjustment.

With aniline (Table XVIII.) all the values of the Sp. R. agree fairly closely, the greatest difference from the mean being less than 2 per cent.

The range of the experiments may be considered to be from ·5 turn (·423 mm.) to 20 turns (16·92 mm.); below ·5 the values of the Sp. R. show a difference from the rest which could not be got rid of; above 20 turns several experiments were made with a tube 16 cm. long and about 4 cm. in diameter. The values of the Sp. R. obtained were in very close agreement with those at 10 or 20 turns for the same liquid.

The above results show that for dilute solutions, alcohol, and aniline, the conductivity is the same for all thicknesses within the limits of the experiments. It has been found that the great differences obtained by Koller, for water and alcohol, can be eliminated by taking care that the plates are true and clean, and by using an alternate current instead of a continuous one. Koller allowed for polarization by estimating it immediately after stopping the current, but this is not satisfactory, since the polarization falls off very rapidly. He estimated that the greatest polarization could not have been greater than ·02 volt., which, though great compared with the whole E.M.F. (1 Daniell) would not account for the difference of

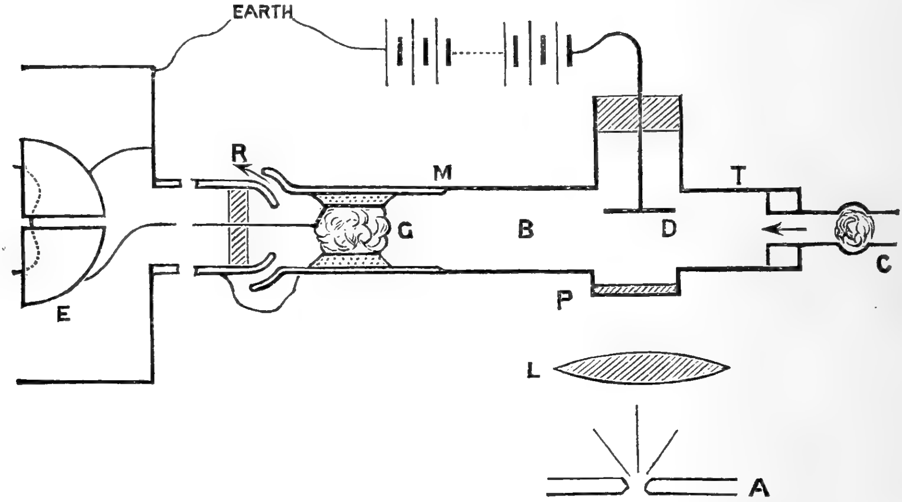
100 per cent. between the conductivity of a layer 7·49 mm. thick and one 1·135 mm. thick. I think that what is not accounted for by polarization can be explained by unevenness and uncleanliness of the plates.

In conclusion, I wish to thank Prof. J. J. Thomson for his kind encouragement during this investigation.

XXIX. *On Air Electrified by the Discharging Action of Ultra-violet Light.* By JOHN ZELENY, *B.Sc., Assistant Professor of Physics, University of Minnesota*[*].

THE following experiments were made to compare some of the electrical properties of air into which a body is being discharged by the action of ultra-violet light, with those when Röntgen rays are the cause of the discharge.

The apparatus was arranged as is shown in the figure. T is a brass tube. At M it is joined by a lead cylinder which contains on the inside an insulated metal tube filled with glass wool G, which latter is connected to a pair of quadrants of the electrometer E.



The light of an arc lamp A was concentrated by the quartz lens L, and after passing through the quartz window P fell upon the freshly cleaned, negatively charged, zinc plate D.

If air was blown into the apparatus at C, it passed by D, through G, and out at R. When now the air was at rest and the light was turned on, or when the air was blown through while the light was cut off, the glass wool at G received no charge. But if both the light and air-blast were in action,

---

[*] Communicated by Prof. J. J. Thomson, F.R.S.

the electrometer showed that G was receiving a negative charge.

The deflexion increased continuously until it passed off the scale.

When, however, another plug of glass wool was introduced into the tube at B, no charge whatever reached G, showing that here also, as with gases charged under the action of Röntgen rays (J. J. Thomson and E. Rutherford, Phil. Mag. Nov. 1896, p. 393), a plug of glass wool completely discharges the gas.

It was found that even with a fairly strong blast only a small part of the charge lost by D reached G. The much larger part went to the walls of the tube T.

This was shown by noting the rate of leak under the action of the light when first D was connected to a pair of charged quadrants and G was to earth ; and next when both D and G were connected to the same charged quadrants. In the latter case any charge that reached G went to replenish the original charge.

It was further found, when a source of Röntgen rays (replacing A and L, and tinfoil being placed over P) was so arranged as to discharge the plate D at the same rate as did the ultra-violet light, that by using the same blast of air and having the same negative charge on D, the quantity of positive electrification which reached G was considerably less than the quantity of negative obtained by the use of the light.

The gas blown away from the plate had a proportionally greater density of free electrification when ultra-violet light was used than was obtained by the use of the Röntgen rays. In the case when light is used the negative carriers start from the negative plate and enter the stream of air. With Röntgen rays, however, the positive ions, though distributed throughout the whole gas, are free from the negative ions only when near the plate towards which they are moving, and so are less easily blown away. In addition some of the free negative ions near the walls of the tube are carried into the glass wool and diminish the resultant charge.

These experiments were performed last June at the Physical Institute in Berlin, and other work prevented a more thorough consideration of the subject.

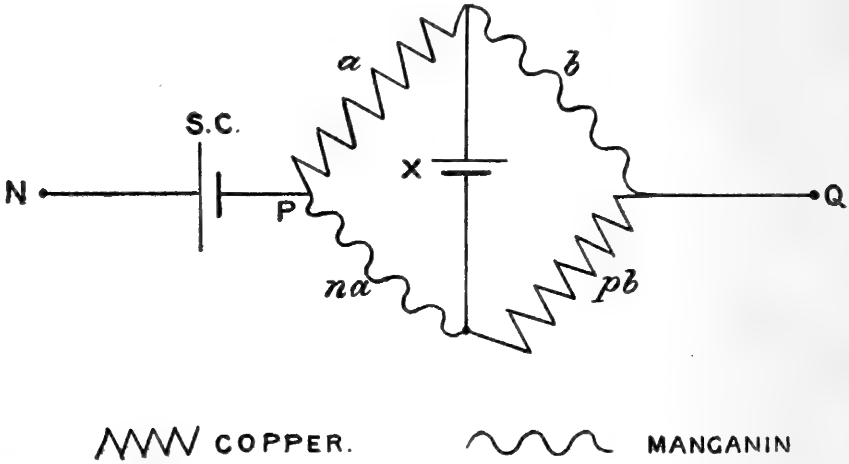
Cavendish Laboratory,
Dec. 30, 1897.

XXX. *On Apparatus for Self-acting Temperature Compensation of Standard Cells.* *By* ALBERT CAMPBELL, *B.A.*[*]

IN ordinary testing work when using a Clark cell as the working standard of difference of potential, it is desirable to get rid of the trouble of continually having to make the temperature correction. Some time ago ('The Electrician,' pp. 601–603, September 6, 1895) I proposed and investigated three methods by which a self-acting correction could be attained. Quite recently I have constructed the apparatus for two of the methods, and I have found it an easy matter to reach the accuracy aimed at (viz. maximum error 1 in 2000).

The arrangement of the first piece of apparatus is shown in fig. 1.

Fig. 1.



S.C. is the standard cell. Four resistances of values a, b, na, and pb are connected as in a Wheatstone's bridge with an auxiliary cell X, whose voltage must be known within 5 or 6 per cent. A Leclanché cell is found suitable for this. The resistances a and pb are of a metal such as copper or iron with a large temperature-coefficient, while b and na are of manganin or a similar alloy. As shown in the paper cited above, we can choose n and p so as to have a constant voltage of, say, 1·400 volts between N and Q at all temperatures from 0° to 20° C. when S. C. is a Clark cell. The arrangement thus not only corrects for temperature but can be made to give a more even figure by cutting off the 0·034 volts at the same time. In one of the coils exhibited the resistances have the following values at 15° C :—

---

[*] Communicated by the Physical Society: read December 10, 1897.

$$a = 48{\cdot}46 \text{ ohms.}$$
$$na = 482{\cdot}7 \quad ,,$$
$$b = 63{\cdot}63 \quad ,,$$
$$pb = 495{\cdot}0 \quad ,,$$

As the absolute values of a and b are of no importance so long as they are high enough to keep the auxiliary Leclanché cell from running down, the copper coils a and pb did not require to be adjusted but were merely measured; their resistances were reduced to 15° C. and the manganin coils na and b were then adjusted to give $n=9{\cdot}96$, $p=7{\cdot}78$*. It is clearly a great saving of trouble not to have to adjust the coils which have a high temperature-coefficient. The four coils were wound on a brass cylinder 7 or 8 centim. in diameter, and then well protected with silk ribbon, four leads being brought out. To test the whole directly the cylinder well wrapped in wadding was placed in a tin canister, the lid of which was then *soldered on* so as to be watertight. The leads were brought out through a long tube soldered to the lid. A thermometer also passed down this tube into the canister. A Leclanché cell was connected as in fig. 1. The tin was then well immersed in water at various temperatures, and steady temperatures inside the tin were got by stirring the water until a thermometer in it showed the same temperature as the one inside the tin. The potential-difference from P to Q was measured by a potentiometer method at various temperatures with the following results:—

Temperature.	P.-D., volt.
15°·5 C.	0·0338
34·2 C.	0·01472
48·5 C.	0·0000

It will be seen that the actual observed values of the auxiliary voltage are just what would be required along with the Clark cell to give 1·400 volts at all ordinary temperatures.

The arrangement in fig. 1 was chosen so as to give the maximum possibility of correction. Mr. C. W. S. Crawley has pointed out to me that some trouble is saved in the construction of the coils by making only one of the four of

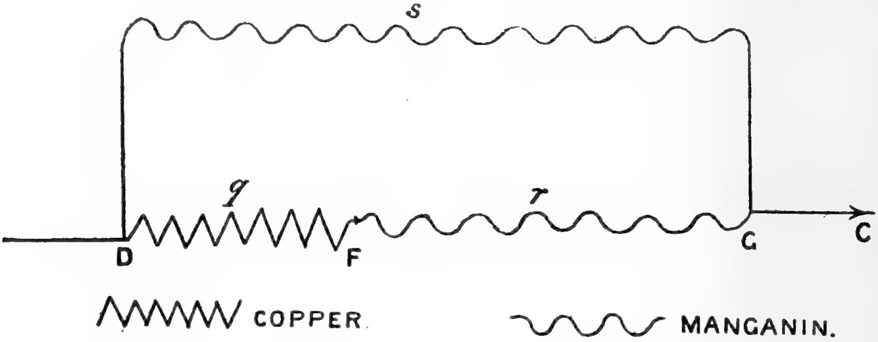
---

* The numerical values of p and n given in my paper mentioned above are not quite right owing to a slip in signs in the equation there given.

copper. This also simplifies the calculation of the ratios n and p, and still gives sufficient variation to balance that of a Clark cell. It is convenient to place the cell inside the wide brass tube on which the coils are wound.

In the other arrangement exhibited three resistance-coils are connected as in fig. 2.

Fig. 2.



The resistances r and s are of manganin, and q is of copper. A current C is sent from D to G from an outside source. The values of q, r, and s are so chosen that a fixed value of C produces *at all temperatures* a potential-difference from F to G which would balance the voltage of the standard.

In the specimen shown $3{\cdot}75\ q=r+s$ and $r=s$. The actual values at 15° C. are

$$r=s=3{\cdot}6335 \text{ international ohms.}$$
$$q=1{\cdot}9382 \quad \text{,,} \quad \text{,,}$$

A current of 1·000 ampere gives a balance with a Clark cell at F and G at all ordinary temperatures (to within 1 in 2000). This definite current, obtainable at all temperatures, can be passed through known resistances and so used to measure unknown voltages by potentiometer methods.

If the resistance r be subdivided other exact currents such as 2, 3, 4 ... amperes can be got directly.

It should be noted that although I have made special reference to the Clark cell, either of the two forms of compensator may be arranged for any other type of standard cell so as to wipe out the greater part of the temperature variation. For instance, if a Carhart 1 volt cell has a coefficient of 0·0097 per cent. per degree C., used with a compensator it would not vary by more than 0·001 per cent. per degree. It will thus be seen that in some cases a compensating arrangement such as I have described above would prove of value not only in ordinary testing work but also in measurements where much higher accuracy is required.

XXXI. *On Electric Equilibrium between Uranium and an Insulated Metal in its Neighbourhood.* *By* Lord KELVIN, *G.C.V.O.*, J. CARRUTHERS BEATTIE, *D.Sc.*, *F.R.S.E.*, *and* M. SMOLUCHOWSKI DE SMOLAN, *Ph.D.**

THE wonderful fact that uranium held in the neighbourhood of an electrified body diselectrifies it was first discovered by H. Becquerel. Through the kindness of M. Moissan we have had a disk of this metal, about 5 centim. in diameter and ½ centim. in thickness, placed at our disposal.

We made a few preliminary observations on its diselectrifying property. We observed first the rate of discharge when a body was charged to different potentials. We found that the quantity lost per half-minute was very far from increasing in simple proportion to the voltage, from 5 volts up to 2100 volts; the electrified body being at a distance of about 2 centim. from the uranium disk. [*Added* March 9.—We have to-day seen Prof. Becquerel's paper in *Comptes Rendus* for March 1. It gives us great pleasure to find that the results we have obtained on discharge by uranium at different voltages have been obtained in another way by the discoverer of the effect. A very interesting account will be found in the paper above cited, which was read to the French Academy of Sciences on the same evening, curiously enough, as ours was read before the Royal Society of Edinburgh.]

These first experiments were made with no screen placed between the uranium and the charged body. We afterwards found that there was also a discharging effect, though much slower, when the uranium was wrapped in tinfoil. The effect was still observable when an aluminium screen was placed between the uranium, wrapped in tinfoil, and the charged body.

To make experiments on the electric equilibrium between uranium and a metal in its neighbourhood, we connected an insulated horizontal metal disk to the insulated pair of quadrants of an electrometer. We placed the uranium opposite this disk and connected it and the other pair of quadrants of the electrometer to sheaths. The surface of the uranium was parallel to that of the insulated metal disk, and at a distance of about 1 centim. from it. It was so arranged as to allow of its easy removal.

With a polished aluminium disk as the insulated metal and with a similar piece of aluminium placed opposite it in place of the uranium, no deviation from the metallic zero was found

---

* Communicated by Lord Kelvin: read before the Royal Society of Edinburgh, March 1, 1897.

when the pairs of quadrants were insulated from one another. With the uranium opposite the insulated polished aluminium a deviation of −84 scale-divisions from the metallic zero was found in about half a minute. [Sensibility of electrometer 140 scale-divisions per volt.] After that the electrometer-reading remained steady at this point, which we may call the uranium rays-zero for the two metals separated by air which was traversed by uranium rays. If instead of having the uranium opposite to the aluminium, with only air between them, the uranium was wrapped in a piece taken from the same aluminium sheet, and then placed opposite to the insulated polished aluminium disk, no deviation was produced. Thus in this case the rays-zero agreed with the metallic zero.

With polished copper as the insulated metal, and the uranium separated only by air from this copper, there was a deviation of about +10 scale-divisions. With the uranium wrapped in thin sheet aluminium and placed in position opposite the insulated copper disk a deviation from the metallic zero of +43 scale-divisions was produced in two minutes, and at the end of that time a steady state had not been reached.

With oxidized copper as the insulated metal, opposed to the uranium with only air between them, a deviation from the metallic zero of about +25 scale-divisions was produced.

When the uranium, instead of being placed at a distance of 1 centim. from the insulated metal disk, was placed at a distance of 2 or 3 millim., the deviation from the metallic zero was the same.

These experiments show that two polished metallic surfaces connected to the sheath and the insulated electrode of an electrometer, when the air between them is influenced by the uranium rays, give a deflexion from the metallic zero, the same in direction, and of about the same amount, as when the two metals are connected by a drop of water.

XXXII. *Notices respecting New Books.*

Recherches Expérimentales sur quelques Actinomètres Electrochimiques. Par H. Rigollot, *Chef des travaux de Physique à l'Université de Lyon.* Paris: Masson, 1897.

THIS volume, published as one of the series of annals of the University of Lyons, contains a full account of Dr. Rigollot's work on the electromotive force between two plates, one of which is illuminated and the other kept in the dark. Two metallic plates are taken, and one side of each is coated with the substance to be examined; the other side of each plate is then varnished or paraffined, and the plates are placed in an electrolyte in the dark and short-circuited until there is no difference of potential between them. One of the plates is then exposed either to white light or to a portion of the spectrum, and the difference of potential produced between the plates is compared with a standard Daniell cell by a potentiometer method. Tables and curves are given, showing the behaviour of various substances when illuminated, and the author sums up the results of his investigation as follows:—

1. Plates of oxide, sulphide, fluoride, chloride, bromide, or iodide of copper, plates of oxide or sulphide of tin, and of sulphide of silver, may, under certain conditions, form actinometers of great sensitiveness.

2. For a given actinometer, other things being equal, the electromotive force depends on the electrolyte.

3. In the spectrum each actinometer shows a maximum of sensitiveness for a wave-length depending only on the nature of the sensitive plate, and independent of the electrolyte.

4. When the plates of an actinometer of oxide, fluoride, chloride, bromide, or iodide of copper are immersed in solutions of different colouring-matters, the electromotive force produced by light of given intensity is increased. In the spectrum the electromotive force is increased for radiation of all wave-lengths, but the maximum of sensitiveness corresponds to a wave-length depending on the colouring-matter chosen. The position of this maximum is independent of the copper salt of which the actinometer is formed.

5. In the spectrum, for an actinometer with coloured plates, the maximum electromotive force due to the colouring-matter occurs always with longer waves than those corresponding to the maximum absorption of light by the colouring matter itself.

The author points out several possible applications of electrochemical actinometers, their great advantage being that their electromotive force returns to zero on removal of the illumination.

J. L. H.

Observational Astronomy. By ARTHUR MEE, *F.R.A.S.* 2nd Edition. Western Mail, Limited: Cardiff and London.

THE first edition of this work appeared in 1893, and met with well-deserved appreciation from amateur astronomers. It has now been thoroughly revised, partly re-written, and brought up to date. Profusely illustrated and abounding in facts tersely, pleasantly, and correctly described, the new volume is well calculated to prove a safe and efficient guide to the young telescopist. Written in a chatty attractive style, the information imparted holds the attention; and the work forms not only a compendium of astronomical history, but a comprehensive handbook of instruction for beginners. It matters not in what direction or department the student may essay to achieve distinction, he will find in these pages the stepping-stones to success clearly indicated. The author, in preparing the volume, has received valuable help in several branches from specialists, and this, in combination with his own excellent writings, has enabled him to produce a work which must prove of great utility to the ever increasing class of amateur observers. He dilates on Naked-eye Astronomy, The Telescope and its employment, the Moon, Planets, the Sun, Comets and Meteors, the Sidereal Firmament, Astronomical Photography; and the close of the book contains a memoir of that able, amiable, and greatly respected friend of the amateur astronomer, the Rev. T. W. Webb. A mass of accurate and recent information is contained in these pages, and everyone who makes astronomy a hobby and possesses a telescope should procure the book. It is useful as a reliable means of reference in regard to past events and the position of the science to-day, while it also points out the road to new discoveries. The volume bears evidence of careful revision, and contains very few misstatements or misprints. On p. 3 we notice Fomalhant instead of Fomalhaut. It is stated on p. 29 that Herschel's highest magnifier (6000) was applied to his largest instrument (48-inch); but the power was really tried on a 6·3-inch of 7 feet focal length in 1782. It was employed upon α Lyræ, and was thought to be 6450; but a later determination made it 5787. On p. 66 Lassell and Bond are said to have discovered a seventh and eighth moon to Saturn; but only one satellite was found by them in 1848. On p. 103 Canis Venat. should be Canes Venat. and on p. 98, Kreuger should be Krueger. These are, however, very trivial blemishes in a generally very accurate handbook which we believe will admirably fulfil the purpose for which it is intended and become the *vade mecum* of the astronomical amateur.

XXXIII. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from vol. xliv. p. 443.]

November 3rd, 1897.—Dr. Henry Hicks, F.R.S., President, in the Chair.

THE following communication was read:—

1. 'A Contribution to the Palæontology of the Decapod Crustacea of England.' By the late James Carter, F.R.C.S., F.G.S.

November 17th.—Dr. Henry Hicks, F.R.S., President, in the Chair.

The following communications were read:—

1. 'The Geology of Rotuma.' By J. Stanley Gardiner, Esq., B.A.

The author describes the relationship of the island of Rotuma (situated in lat. 12° 30′ S., long. 177° 1′ E.) to the adjoining isles. It is almost separated into two parts, which are united by a narrow neck of sand. The interior is composed of volcanoes, which have emitted lavas and fragmental rocks. Around the volcanic rocks are stratified deposits composed of sea-sand with volcanic fragments These are partly denuded, and are mantled round by coral-reef and beach sand-flats. A remarkable cavern in the lava of Sol Mapii, with lava-stalactites, is described; there is a similar cavern in Au Huf Huf.

An account of the prevalent meteorological conditions is also given.

In an Appendix by Mr. H. Woods, M.A., some of the rocks are described. They consist of olivine-dolerites and basalts and associated fragmental rocks.

2. 'A Geological Survey of the Witwatersrand and other Districts in the Southern Transvaal.' By Frederick H. Hatch, Ph.D., F.G.S.

After giving an account of the physical characters of the area, the author proceeds to describe the various rocks, referred to

(1) The Karoo System,
(2) The Cape System,
(3) The Primary or Archæan System.

The Archæan rocks protrude in a few places through the sedimentary beds, which form the greater part of the area, and consist of an igneous complex of rocks of varied composition.

The Cape System is capable of division into five distinct series:—

UPPER BEDS.	Magaliesberg and Gatsrand series; alternating quartzites, shales, and lava-flows. 16,000 to 20,000 feet.
	Dolomite and cherts, thickly bedded. 6000 to 8000 feet.
	Black Reef; a bed of quartzite and conglomerate, 20 to 50 feet, and Klipriversberg amygdaloid; a basic volcanic rock, 5000 to 6000 feet.
LOWER BEDS.	Witwatersrand Beds; sandstones and conglomerate (in part auriferous). 11,000 to 15,000 feet.
	Hospital Hill Series; quartzites and ferruginous shales. 8000 to 10,000 feet.

A full description of each of the series, and the associated volcanic and igneous rocks, is given in the paper.

The Karoo formation is represented by the Coal Measures of Vereeniging and the district south of Heidelberg, and by the measures of other coal-areas. They have furnished plants which Mr. Seward refers to in a note as being of Permo-Carboniferous age.

The age of the Cape System is doubtful. The Upper beds rest unconformably on the Lower ones, and if the latter be of Devonian age, as has been inferred, the former may represent the Lower Carboniferous rocks.

In conclusion, the author makes some observations upon the geotectonic relations of the area.

3. 'Observations on the Genus *Aclisina*, de Koninck, with Descriptions of British Species, and of some other Carboniferous Gastropoda.' By Miss J. Donald, of Carlisle.

December 1st.—Dr. Henry Hicks, F.R.S., President, in the Chair.

The following communications were read:—

1. 'A Revindication of the Llanberis Unconformity.' By the Rev. J. F. Blake, M.A., F.G.S.

In a paper published in the Quarterly Journal of the Society for 1893, the author of the present paper maintained that certain conglomerates and associated rocks occurring for some distance north-east and south-west of Llanberis, which had hitherto been considered to lie below the workable slates of the Cambrian rocks of that area, were in reality unconformable deposits of later date than those slates. In the year 1894 a paper appeared in the same Journal, in which the authors maintained that in no case which had been examined could any valid evidence be found in favour of the alleged unconformity, and that in one (on the north-east side of Llyn Padarn) which they supposed to afford the most satisfactory proof of it, the facts were wholly opposed to the notion.

The present paper is a reply to these authors, in which their objections, founded on general considerations, on field observations, and on microscopic examination of rock-specimens, are discussed, and the author gives the results of further observations on the rocks of the district. The Moel Tryfaen sections, and those on each side of Llyn Padarn in the Llanberis district are considered, and he maintains that the post-Llanberis (using this term in the sense of being after the deposition of the main workable slates) age of the conglomerates which are under discussion is established; though the more he considers the correlation of these conglomerates with the Bronllwyd Grits the less he likes it, and as far as the stratigraphy is concerned, they may be much newer,—their age is at present an open question; but of their unconformable position he has no doubt.

2. 'The Geology of Lambay Island, Co. Dublin.' By Messrs. C. I. Gardiner, M.A., F.G.S., and S. H. Reynolds, M.A., F.G.S.

The authors, who have previously described the neighbouring district of Portraine (Quart. Journ. Geol. Soc., Dec. 1897), undertook an examination of this island, with the intention of comparing the rocks with those of Portraine, and of investigating the nature of the rock familiar to geologists under the name of 'Lambay porphyry.' The sedimentary rocks are similar to some of those of Portraine, and are of Middle or Upper Bala age. Associated with them are pyroclastic rocks and andesitic lava flows. some of the lavas having flowed beneath the sea. The sediments and volcanic rocks were exposed to denudation, and a conglomerate composed of their fragments was accumulated round the volcano. The 'Lambay porphyry,' which has been determined as a diabase-porphyry by Dr. von Lasaulx, is partly intrusive in the other rocks, but has in places come to the surface as a lava-flow.

Petrographical descriptions of the various rocks are given by the authors.

December 15th.—Dr. Henry Hicks, F.R.S., President, in the Chair.

The following communications were read :—

1. 'On the Pyromerides of Boulay Bay, Jersey.' By John Parkinson, Esq., F.G.S.

After briefly noticing the literature of the subject, the author describes the altered rhyolites of Boulay Bay. One variety, the commonest, is of a dark red colour, showing flow-structure; another is porphyritic; a third, near the centre of the Bay, has a pale greenish matrix enclosing fragments, which, however, are due to flow-brecciation. Large pyromerides occur in two localities: in the more interesting, that north of the jetty, the structure of the rock indicates either a very peculiar magmatic differentiation *in situ* or (more probably) the mixture of two magmas differing in their stage of consolidation.

From study of a series of specimens of the pyromeridal rock, the author arrives at the following conclusions :—(1) The rock shows marked flow-structure and at times bands which indicate a slight difference in its composition, the latter tending to assume a moniliform outline. In such the microscopic structure corresponds with that of the pyromerides, and exhibits traces of radial crystallization. (2) These afford a passage into somewhat oval pyromerides, with rather tapering ends and irregularly mammillated surfaces. (3) From these sometimes a single one seems to be thrown off, while lines of pyromerides or little lumps of similar material are scattered about the matrix. (4) Many of the pyromerides are solid throughout; others have a central cavity filled with quartz.

The author describes varieties of the pyromerides. They are generally deep red in colour, and exhibit (*a*) fluxion-structure, made more distinct by minute black microliths; (*b*) a radial structure; (*c*) a 'patchy' devitrified structure (with crossed nicols); the second being not always present. The matrix is usually of a greenish tint, showing devitrification-structure and sometimes a trace of perlitic structure.

The pyromerides frequently exhibit more or less crescentic cracks, due apparently to contraction, which have been filled by quartz. Sometimes also they scale off in rudely crescentic shells. In one locality a variety with good spherulites, about as large as a pea, passes into one showing a fluxion-structure and pyromerides, having traces of radial structure as well as clots and irregular 'wisps,' suggestive of a stiffer material broken up by one more liquid.

As the result of his studies, the author thinks that while very regular spherulites do occur, apparently in consequence of radial crystallization round a centre, the pyromerides are due to the mixture of two magmas slightly different in composition and fluidity, the less plastic of the two being sometimes drawn out into streaks, but at others forming lumps, in which, where their form is suitable, a radial structure is subsequently developed. He concludes by comparing the pyromerides of Boulay Bay with specimens from other localities described by MM. Delesse and Lévy, Prof. Iddings, and Miss Raisin, or collected by himself, and by discussing the quartz-filled cavities which occur in certain cases. These he regards as originally vesicles, and not due to any subsequent decomposition.

2. 'On the Exploration of Ty Newydd Cave near Tremeirchion, North Wales.' By the Rev. G. C. H. Pollen, S.J., F.G.S.

In November 1896 a Committee was formed, consisting of Dr. H. Hicks, Dr. H. Woodward, and the author, for the purpose of exploring this cavern, which is situated in the same ravine on the east side of the Vale of Clwyd as the well-known caverns of Ffynnon Beuno and Cae Gwyn, explored about 12 years ago by Dr. H. Hicks and Mr. E. B. Luxmoore. Grants have been made by the Royal Society and by the Government Grant Committee for the purpose of carrying on the explorations; and though a considerable time must elapse before the work is completed, the results already obtained are of so much importance that the author has thought it advisable to bring them before the Society. In the work of exploration he has throughout been ably assisted by the theological students of St. Beuno's College. The cavern had been in part broken into by quarrying operations, but the chambers and tunnels were completely filled up with more or less stratified deposits, and had remained entirely untouched.

Although the ground above the cavern is strewn over with drift and erratics from the North and from the central areas of Wales, not a fragment of anything but immediately local material has been discovered in the cavern itself, showing clearly that the deposits in the cavern had been carried in by water before the Northern and Western ice had reached this area. The work has been carried on almost continuously throughout the year, and most of the material has been removed for a distance of over 60 feet from the entrance. The height of the cavern above sea-level is 420 feet, or about 20 feet above the floor of the Cae Gwyn Cave.

The following points appear to the author to be now fully established:—

1. The material in the Ty Newydd Cave, as in the lower parts of those of Ffynnon Beuno and Cae Gwyn, is of purely local origin. Of this he can speak with confidence, as the question was before him from the beginning and the gravels were examined with minute care for erratics.

2. This local deposit is of earlier date than the Boulder Clay with Western and Northern Drift. This was proved by the finding of granite- and felsite-boulders abundantly at higher levels and over the Cave, and in one case filling the upper part of one of the fissures communicating from above with the Cavern.

3. The occurrence of the tooth of a large mammal (*Rhinoceros*) in the lower part of the Cave shows that the animal was contemporary with, or of earlier date than the infilling of the Cavern by the local drift.

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

[FIFTH SERIES.]

APRIL 1898.

XXXIV. *Variations in the Electromotive Force of the* H-*form of Clark Cells with Temperature.* *By* F. S. Spiers, *B.Sc.*, F. Twyman, *and* W. L. Waters.

THE work described in this paper was intended as a completion of the investigation carried out some years ago by Prof. Ayrton and Mr. W. R. Cooper [†] on the effect of variations of temperature on the E.M.F. of the Board of Trade form of Clark cell. Their results showed that it was not possible, by applying the ordinary temperature-correction, to obtain the true value of the E.M.F. of the cell with a greater accuracy than 0·1 per cent. It was thought that the H-form of cell would show some improvement over the ordinary type in this respect; and this was confirmed by some results published recently by Dr. Kahle [‡]. The experiments to be described in this paper were undertaken with the object of seeing how far this was correct, and in general to investigate the behaviour of this form of cell under variations of temperature. The experiments were carried out at the Central Technical College.

The tests were made on four cells set up in accordance with the specification issued by Dr. Kahle from the Reichsanstalt [§]. The cells were in every way alike [||], and were mounted

---

[*] Communicated by the Physical Society: read Nov. 12th, 1897.

[†] Proc. Roy. Soc. December 1895.

[‡] *Zeitschrift für Instrumentenkunde,* August 1893.

[§] 'Electrician,' vol. xxxi. p. 265, July 1893.

[||] They were set up by Mr. Cooper, Nos. 1, 2, and 4 on 19th March, 1895, and No. 3 on 12th February, 1896.

together inside a glass vessel in paraffin oil, and during the experiments they each underwent precisely the same treatment. The diameter of each of the two legs of the cell was about $\frac{3}{4}$ inch, and their length about one inch.

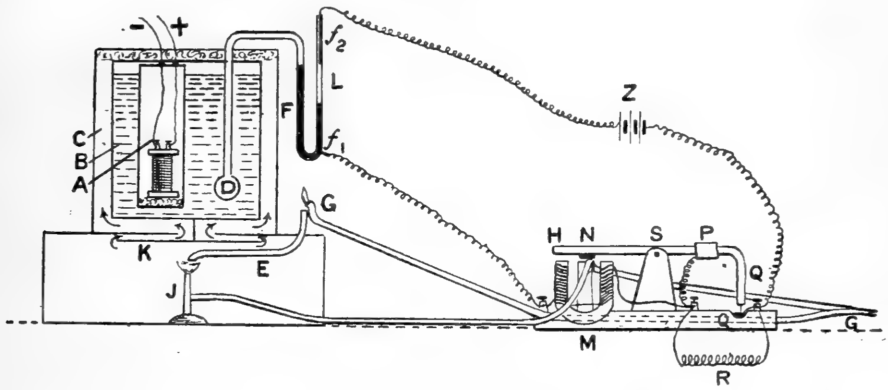
The method used for the determination of the E.M.F. was as follows. Current from a storage-cell traversed a potentiometer-wire in series with a resistance, the value of which was such that the fall of potential along the wire amounted to one ten-thousandth of a volt per centimetre-length. The cell to be tested was joined up in opposition to the standard, and the difference in their E.M.Fs. was balanced against the fall of potential along a certain length of the stretched wire.

The standard used in comparison consisted of one or other of two cells of the Muirhead type, which had been found to have an E.M.F. approximately constant during several years. The two cells were in one case, and during the experiments were placed in a thermostat in which the temperature could be kept constant to 0°·01 C. The E.M.F. of the standard was determined absolutely by balancing it against the P.D. between the terminals of a resistance of known value of about three ohms, due to a current of about half an ampere passing through it. The resistance had been made specially for the purpose of manganin wire, wound upon an ebonite frame and immersed in paraffin oil. It had been previously aged by heating, and had assumed a steady value, as shown by comparison with standard manganin coils. The current was measured by a Kelvin centiampere balance, the constant of which had been frequently checked by means of a silver voltameter. By this means the E.M.F. could be determined absolutely, correct to 0·0001 volt.

The thermostat consisted of an inner air-space, in which was placed the standard, surrounded by an outer water-bath which was heated by a gas-jet, the gas being automatically cut off when the temperature rose to a given value, and being allowed to come on again as soon as the temperature had fallen below that value.

In fig. 1, A B C are zinc vessels: A is the air-bath, containing the standard resting on cottonwool; B the surrounding water-bath, closed at the top and containing the thermometer D; C the enclosing jacket, between which and B the hot air from the gas-jet passes, heating the water-jacket on its way. D is an alcohol thermometer with a mercury-index F, which, on the expansion of the alcohol, is pushed up the narrow tube L, and electrically connects the two platinum wires f_1 and f_2. J is the gas-jet for heating the water-bath; K a baffle-plate over the gas-jet, to prevent the flame heating the water-bath directly. E is an escape-tube, which serves to carry off some of the heat of the jet; G is a little by-pass, burning at the mouth of the tube E, and serves to relight the main jet when extinguished. M is an electromagnet; H, its armature, is pivoted at S and weighted at P, and carries a platinum wire Q which dips into the mercury cup Q′.

Fig. 1.



When the temperature of the water-bath has reached a certain arbitrary value, the mercury-index electrically connects f_1 and f_2, completing the circuit of the electromagnet M and the battery Z. The electromagnet being thus excited, attracts its armature H, which compresses the indiarubber tube T that feeds the main gas-jet J, between the brass piece N and the adjustable wooden block N′, and so extinguishes the light. The water-bath now cools slowly till its temperature has fallen below the fixed value, and the contact is broken at f_2 by the falling of the mercury-index. When this happens, the armature of the electromagnet is released and pulled up at once by the weight P, the gas coming on again at the jet J; some of the gas passing up the tube E is ignited at the top by the by-pass G, and then striking back, lights the main jet J, restarting the heating, and the cycle is repeated.

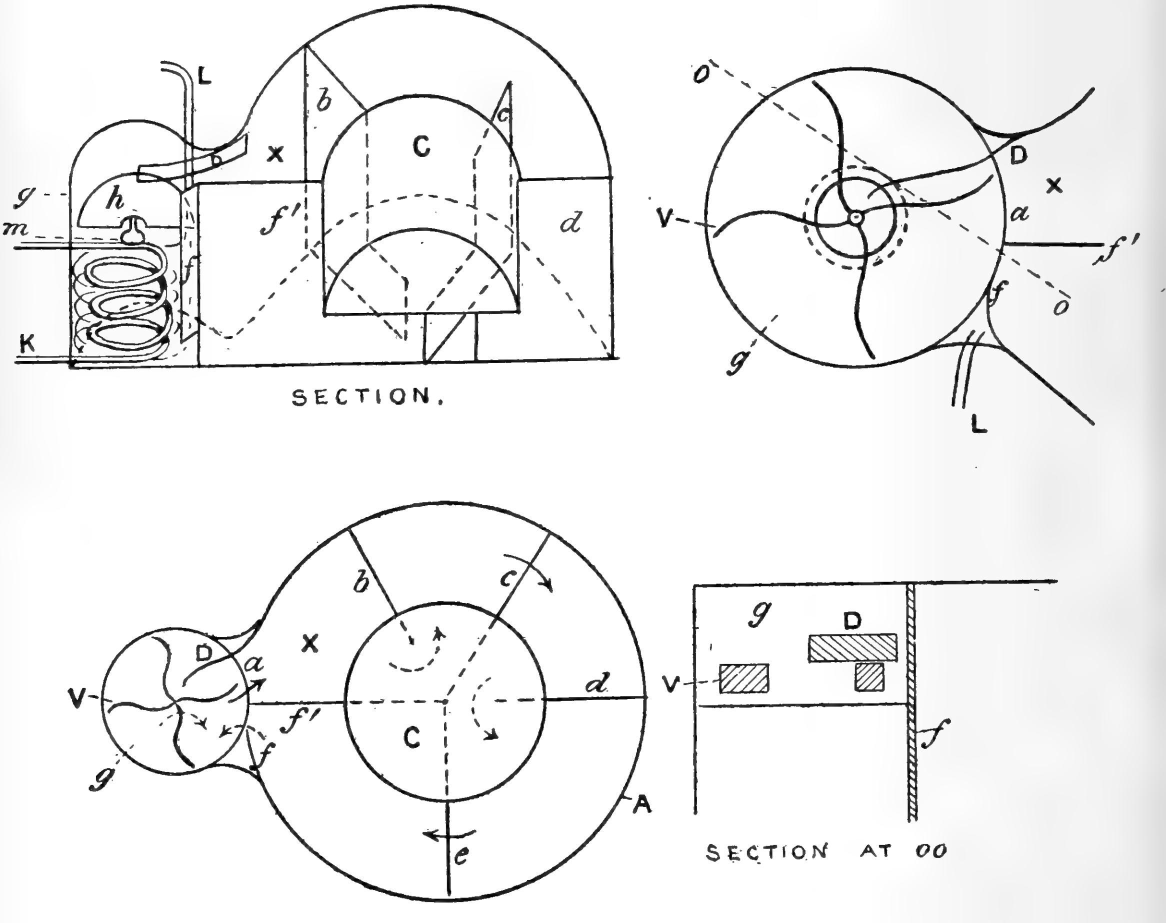
As it requires a less current to hold the armature down than it takes originally to attract it, the current circuit of the magnet is so arranged that when the armature is attracted it breaks the direct-current circuit at the mercury cup Q′, and inserts into the circuit a resistance of 50 ohms, thus economizing the energy.

The temperature at which the cut-off acts can be adjusted several degrees by altering the distance which the wire f_2 projects down the tube L.

By carefully adjusting the size of the jet, the by-pass, and the height of the block N′, we could keep the temperature of the air-bath constant to 0°·01 C., the gas being cut off and coming on again about every five minutes.

To investigate the effect of temperature on the cells, it was necessary to be able to vary their temperature so as to get a uniform rise or fall. To effect this, some special form of heating-bath was needed. In its final form, this heating-bath consisted of an outer zinc bath, in which the vessel containing the Clark cells was placed, which was divided up by means of a number of partitions, reaching from the bottom of the vessel to above the surface of the oil. The oil was forced alternately under one partition and over the next, round the whole of the bath, and from the last compartment was drawn off into a separate chamber containing a heating-coil and a coil of lead tubing for an iced-water

Fig. 2.



SECTION.

SECTION AT OO

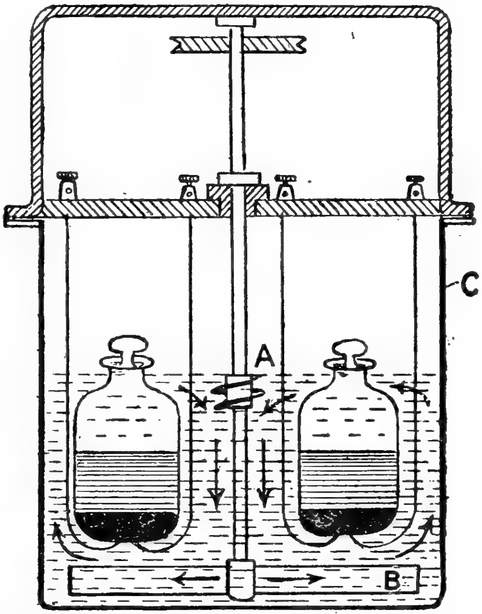
PLAN.

circulation, and from this chamber it was again circulate round the compartments by means of a set of centrifugal vane

A (fig. 2) is the outer zinc bath, containing the oil, and divide up into compartments by means of the zinc plates, *a*, *b*, *c*, . . . C, the inner glass vessel, contains the cells, and rests about one and a half inches from the bottom of the outer bath on the parts of the partitions *a*, *b*, *c*, . . . which reach to the centre. *g* is the separate compartment containing the heating-coil L, the coil of lead tubing K, and the centrifugal pumping-vanes V. The vanes V are directly coupled to a motor with a vertical shaft, this being found to be the most satisfactory way of driving them. The ring *h* separates the chamber *g* into two parts: in the upper, the vanes work and the oil is rotating; in the lower, the heating and cooling coils are placed, the oil in this part not rotating. *m* is a distributing plate under the hole in the centre of the ring *h*, which makes the circulating oil pass over the heating and cooling coils. D is a stationary guide vane which helps to deflect the rotating oil into the next chamber X.

The oil passes under the division *b*, over *c*, under *d*, over *e*, and finally under *f* into the heating-chamber *g*, where it passes over the heating-coil, and is sucked up through the hole in the plate *h*, and, being thrown out by the vanes V, is again circulated.

Fig. 3.

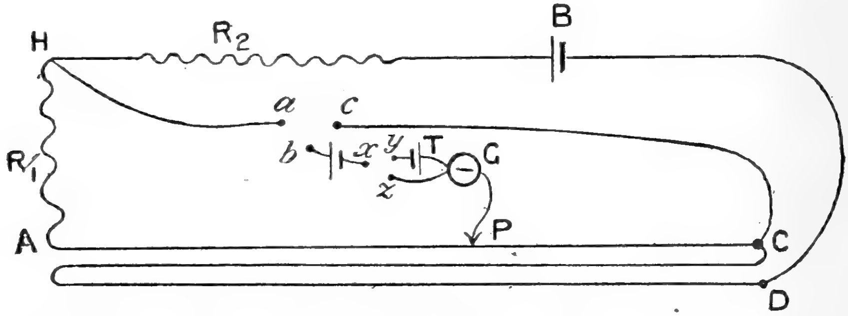


The circulation obtained by this means was excellent. When the bath was tested at a rate of change of temperature of 1° in eight minutes, there was found to be a maximum difference of temperature between any two points of the bath of 0°·2; while at a rate of change of 1° in fifteen minutes, no difference in temperature was detected.

In the inner vessel containing the cells (fig. 3) the oil was circulated by means of two sets of vanes; the upper set, A, in the form of a screw-thread, was arranged so as to send the oil downwards on rotation; the lower set, B, were radial near the centre, and at the outer ends curved so as to force the oil upwards. Thus, as the oil was forced downwards from the first set A, it was caught, and first sent outwards and then upwards by the second set B, thus producing a constant circulation. During our experiments the temperature in this bath was always uniform to at least 0°·02 C.

The stretched wire (fig. 4), three metres in length, was employed as the potentiometer-wire. The resistance of this being known, the strength of current necessary to give a drop of potential of one ten-thousandth of a volt per centimetre length was calculated, and also the resistance required to be traversed by this current to produce a potential-difference between its extremities equal to the E.M.F. of our standard. The resistance R_1 was adjusted so as to be as nearly as possible equal to that value, and the deficit was found in terms of a length AP of the stretched wire. For example, suppose the resistance necessary to produce a potential-difference equal to the E.M.F. of our standard to be found by calculation to be 214·30 ohms. Plugs would be taken out of R_1 to the value of 214 ohms, and the point P on the wire would be selected such that the resistance of AP equalled 0·30 ohm. Then the mercury cups a, b and x, z being joined, the current sent through the wire by the storage-cell B was adjusted by means of the auxiliary variable resistance R_2 until the E.M.F. of the standard equalled the potential-difference between the points H and P. In this way the wire was rendered direct reading for any subsequent comparisons, one centimetre on the scale corresponding with one ten-thousandth of a volt. By joining the mercury cups a, b and x, z, the value of the current could be readily checked at any time during the experiment without interrupting the rest of the circuit. As a rule, very little variation was found

Fig. 4.



to occur. In this method we measure the difference of the E.M.Fs. of the cells on the stretched wire, and not the actual E.M.F. of the cell under test, consequently considerable variations of the current have very little effect on the result.

The cell under test was placed at T, and by joining the mercury cups b, c and x, y, this could be joined up with its E.M.F. in opposition to that of the standard; and then by moving the contact P, we could find a point such that the potential-difference between P and C equalled the difference in E.M.F. between the cell to be tested and the standard. If the difference between the E.M.Fs. was positive, then the point P lay on one side of the point C, while if the difference was negative, it lay on the other side.

To find how the changes in E.M.F. lagged behind those of the temperature, it was found best to subject the cells to a cycle of temperature, the temperature being first raised at a uniform rate through a certain number of degrees, and then cooled again at the same uniform rate to the original temperature. Supposing no lag to exist, the curve connecting temperature and E.M.F. would be the same for both rising and falling temperatures. But if there is a lag of short duration, *i. e.* not cumulative, then, provided that the temperature varies at the same rate throughout, two distinct curves, the second parallel to the first for the greater part of its length, will result, and will enclose an area between them. If there is any lag of a semi-permanent character, it will be shown by the curve not returning to its original starting-point till after a considerable time.

When we proceeded to take the cells through a cycle of temperature, we first of all determined absolutely the E.M.F. of the standard, which had been kept at a constant temperature for some days in the thermostat. The current for heating the cell-bath was then turned on, the required value of the current to give a certain rate of rise of temperature having been previously determined by experiment. The current in the stretched wire was then adjusted to its proper value, and the readings of the E.M.Fs. of the cells were taken every few minutes. The temperature of the cells was observed every five minutes and a curve plotted connecting temperature and time. The heating curve between time and temperature always gave practically a straight line, without readjustment of the current. When we started to cool our bath, we turned on the iced-water circulation full for the first few minutes, to get a sharp bend in the temperature and time curve, afterwards it was made to go much slower, and the flow required readjusting from time to time to keep the curve straight.

Considering that this adjustment was done purely by the method of trial and error, the curves obtained may be regarded as highly satisfactory. They approximate so closely to straight lines that it was thought unnecessary to reproduce them. In some of the cycles we cooled first and and heated afterwards, thus reversing the cycle. At the conclusion of each experiment the E.M.F. of the standard was again tested absolutely.

The accuracy of the method was all that was desired. The thermostat would keep the temperature of the standard constant to 0°·01 C., and the temperature of the cell-bath was uniform to 0°·02 at least. The difference in the E.M.Fs. could be read on the stretched wire to one hundred-thousandth of a volt. The E.M.F. of the standard never varied more than one ten-thousandth of a volt during an experiment, and in general it probably varied much less.

Three different rates of change of temperature were tried: 1° C. in seven minutes; 1° in fifteen minutes; and 1° in thirty minutes. A cycle generally lasted about five or six hours. The curves are plotted for E.M.F. and temperature, and, to avoid confusion, the curves for the different cells have been separated by displacing the origin of E M.F. differences.

It will be seen that the curve connecting E.M.F. and temperature is not the same for both the rising and the falling temperatures in a cycle. As explained above, this indicates the existence of lag. The lag of E.M.F. at a rate of change of temperature of 1° in seven minutes is about four ten-thousandths of a volt; at a rate of change of 1° in fifteen minutes, it is about two-and-a-half ten-thousandths; at a rate of 1° in thirty minutes, it is about one twenty-thousandth of a volt. By this lag is meant half the difference between the E.M.Fs. given by the rising and falling curves for any particular temperature. If these lags are compared with those obtained for the B.T.-type of cell, in the paper above referred to, we see that the lag in the case of the H-form of cell is less than one quarter of that in the ordinary form. (See fig. 7. N.B.—The zero-lines on this diagram are purely arbitrary; the curves are merely for the purpose of comparison.)

The temperature at the close of a cycle was sometimes not exactly the same as at the commencement, hence the temperature-time curves are not closed. It is highly probable that if the temperature had been the same, the E.M.F. would have returned to its original value. On keeping the temperature constant at the end of the cycle, the lag passed off rapidly; at a rate of variation of temperature of 1° in fifteen minutes it disappeared in about ten minutes.

The probable reason for the superiority of the H-form over the ordinary form is that when the changes of temperature produce variations in the density of the zinc sulphate solution, the zinc amalgam, being at the bottom, is always in contact with a saturated solution, while in the B.T.-form we have the solution in contact with the mercury saturated, but that in contact with zinc will not be saturated till the whole of the paste has attained its equilibrium condition and again become saturated with zinc sulphate at the new temperature.

Thus in general it appears that in the H-form of Clark cell the lag is much less than in the B.T.-form, and that under ordinary conditions, when the rate of variation of temperature is not greater than 1° in half an hour, by applying the ordinary temperature-corrections we can obtain a result accurate to less than a ten-thousandth of a volt. If we wish to apply a temperature-correction for a greater rate of variation than this, we must either find the temperature-coefficient for variations at that rate from curves such as those given here, or else slightly diminish the ordinary coefficient. Subsequent experiments showed that if an H-cell were placed in a water-bath, which was heated through 3° or 4° as quickly as possible, then the E.M.F. of the cell would reach an approximately steady value as soon as the temperature of the bath, and by taking the temperature of the cell as being that of the bath, we could apply the temperature correction accurate to a ten-thousandth of a volt.

The two Muirhead cells used as standards in the above experiments were also taken through a single cycle at a rate of variation of temperature of 1° in fifteen minutes. From the curves (fig. 8) it will be seen that one of the cells is but slightly inferior as regards lag to the H-form, while the other comes about midway between the H and the B.T. forms. These two cells have been recently dismounted, and it was found that they were almost dried up and that the zinc sulphate had crystallized out over the top of the cells. They had thus practically become examples of Prof. Callendar's crystal cell*, and this probably accounts for their small lag. The cell which appeared the drier of the two had the larger lag.

Our best thanks are due to Prof. Ayrton and Mr. T. Mather for their many valuable suggestions and kind help, and to Mr. Cooper for the loan of the cells tested.

---

* Proc. Roy. Soc. October 1897.

APPENDIX.

Since the above investigation was completed, some experiments have been made to test whether the lag of E.M.F. observed was due to the lag of the temperature of the cell behind that of the bath or to diffusion lag. A B.T.-form of cell was made up in a tube 2 cm. diameter, and a thermometer inserted in a position similar to that occupied by the zinc. The cell was immersed in a water-bath so that the upper surface of the paste was about 2 cm. below the surface of the water. The temperature of the bath was then raised uniformly, the bath being kept stirred, and the temperature of the cell observed from time to time and compared with that of the water-bath. The lag of the temperature of the cell behind that of the bath was, at a rate of variation of temperature of 1° in six minutes, 0°·15; at a rate of 1° in fifteen minutes 0°·10; at a rate of 1° in thirty minutes less than 0°·05. The same experiment was tried with the thermometer half in the mercury and half in the paste so as to represent the condition of things in the H-form. The temperature lags in this position were found to be very slightly less than in the other.

Experiments were also made in which the cell at a temperature of 12° C. was placed in the bath at 30° C. and the bath stirred; the temperature of the cell was observed at different times. The observations showed that at the end of five minutes the lag behind the temperature of the bath was 0°·10, at the end of eight minutes practically zero. The temperatures in these experiments were observed accurate to 0°·05 C.

A Callendar crystal cell was tested in the same manner as the above and gave almost identical results.

It appears from these experiments that about half the lag of E.M.F. observed in the H-form of cell is due to the lag of the temperature of the cell behind that of the bath, and the rest to diffusion lag.

Hence in all experiments with Clark cells, where the temperature effects are likely to be serious, it is better to discard the B.T.-form of cell and use either Prof. Callendar's crystal cell, Prof. Carhart's modification of the B.T.-form, or the H-form. The Callendar crystal cell, in which the zinc rests in a mass of moist zinc-sulphate crystals, has a very small diffusion lag, but has the disadvantage that there is a possibility of particles of zinc falling and reaching the mercury. This fault is, however, eliminated in his inverted type. The Carhart cell, in which the solution is of constant strength, also possesses a very small diffusion lag, but it has two serious drawbacks, firstly, an error of a few tenths of a degree in the temperature at which the cell is saturated makes an error of several ten-thousandths of a volt in the E.M.F., and, secondly, every time a current passes through the cell a certain amount of zinc is dissolved, so in time the solution becomes stronger. This point is of particular importance where the cells are used with condensers.

Fig. 5.—Curves connecting E.M.F. and Temperature. Rate of Variation of Temperature, 1° in 15 minutes.

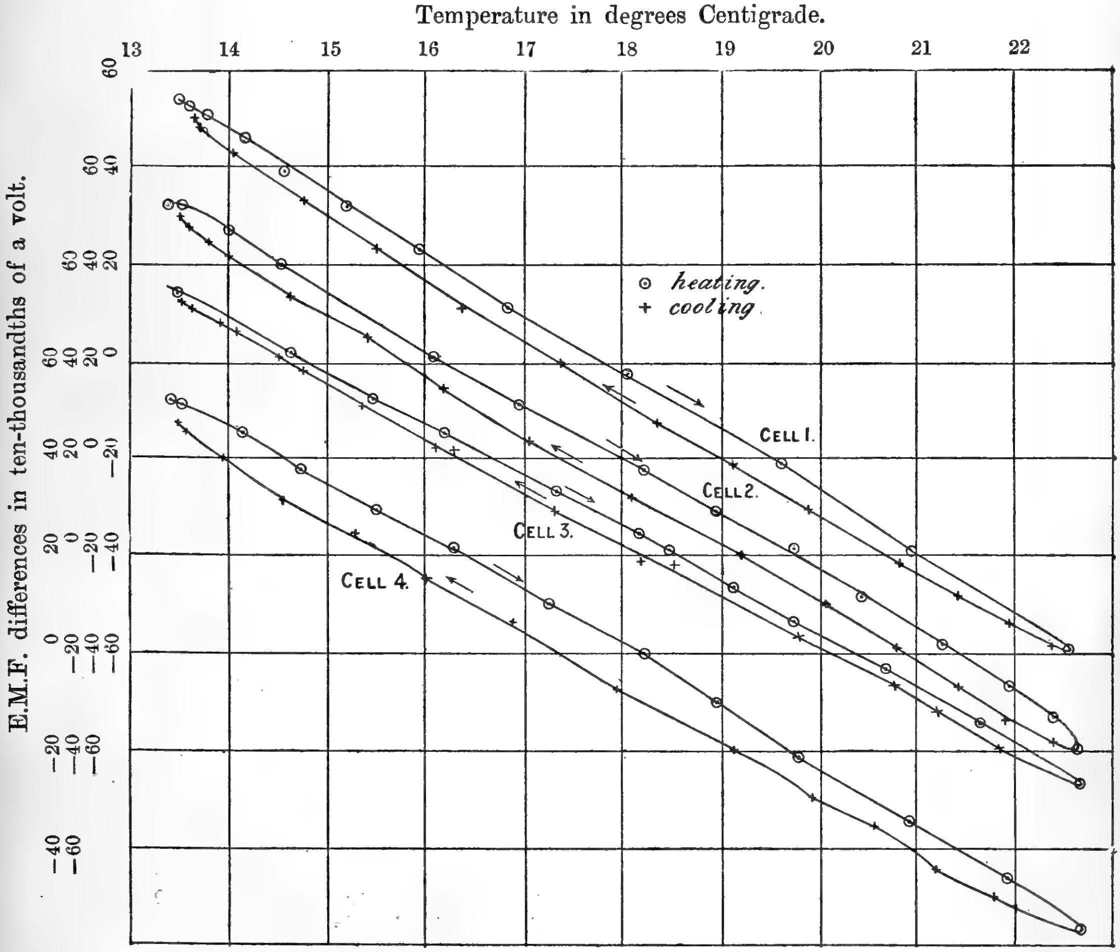


Fig. 6.—Curves connecting E.M.F. and Temperature; Rate of Change of Temperature 1° in 30 minutes.

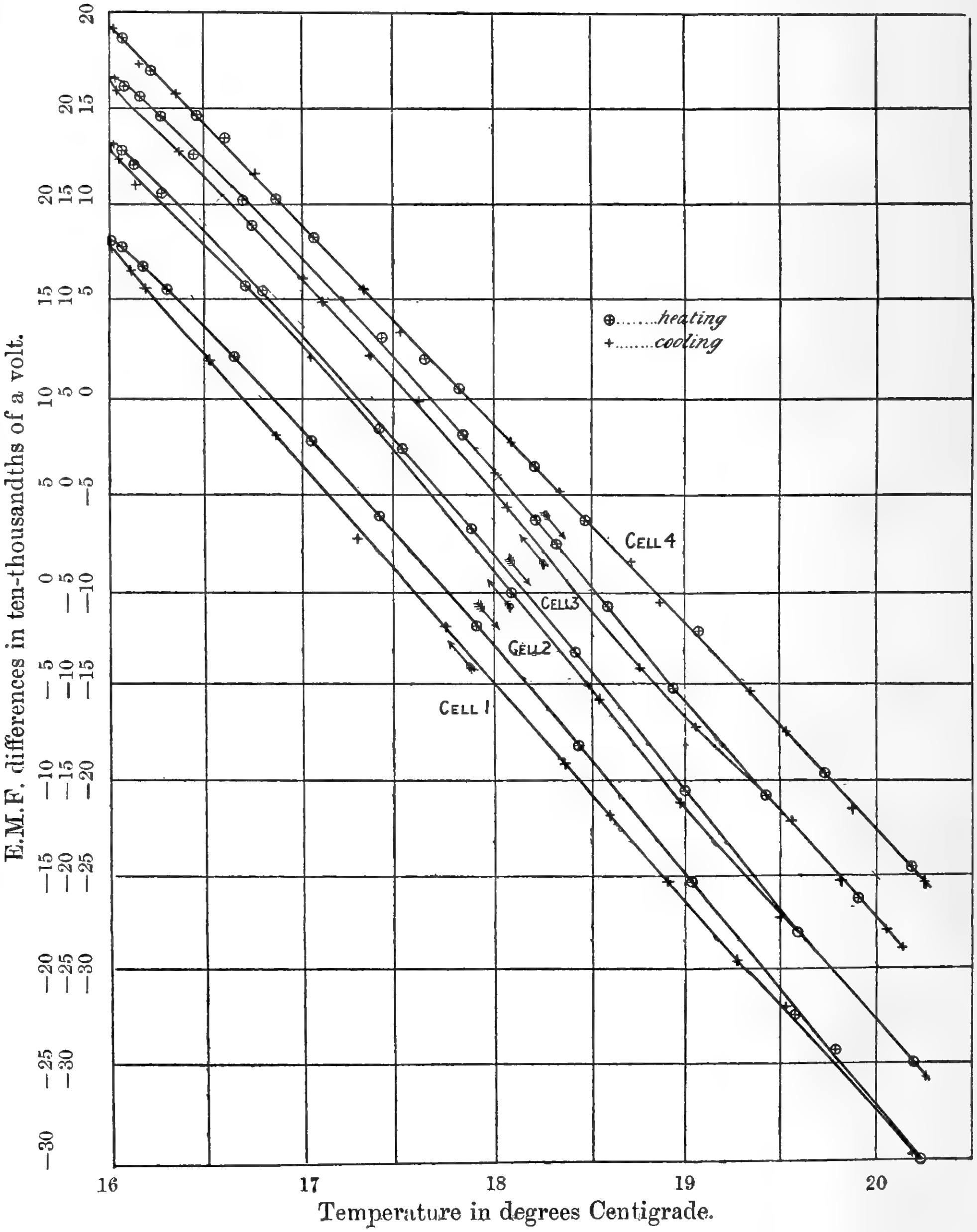


Fig. 7.—Curves comparing the lag in the H-form of cell with that in the B.T.-form.

A. An average curve for Board of Trade cell. Rate of change of temperature, 1° in 15 min. (From paper of Prof. Ayrton and Mr. Cooper.)
B. H-form. 1° in 15 min.
C. H-form. 1° in 30 min.
D. H-form. 1° in 7 min.

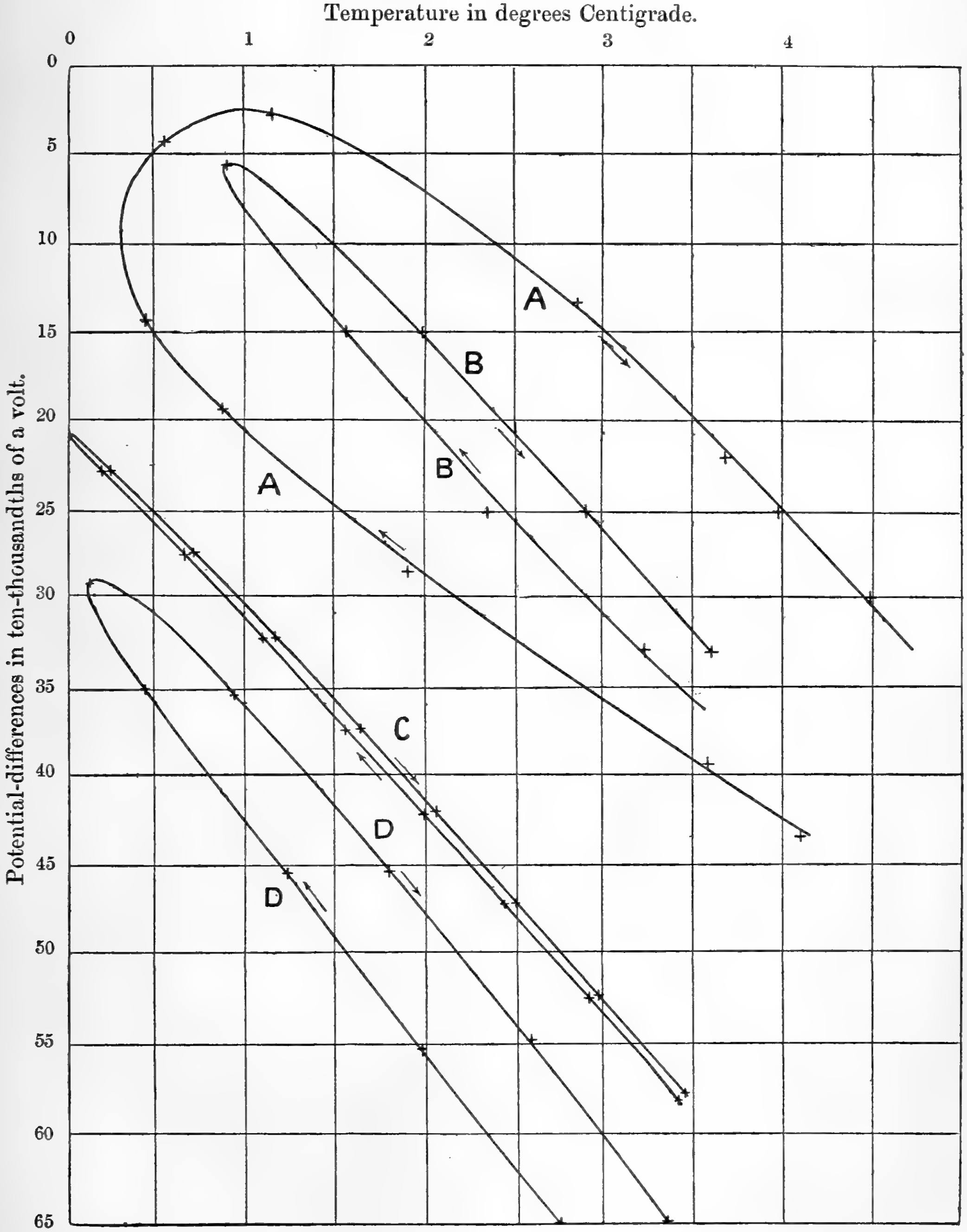
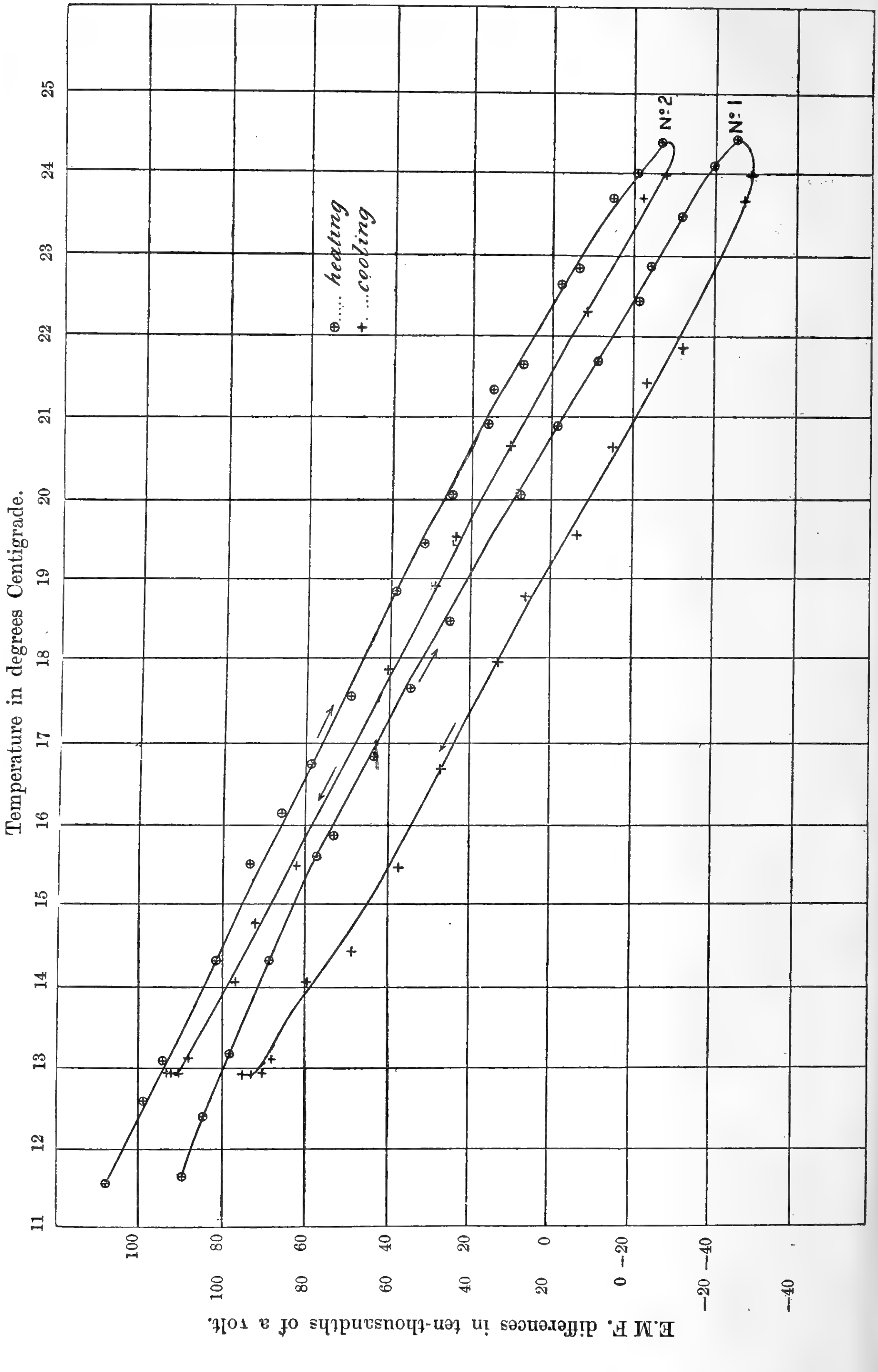


Fig. 8.—Curves of E.M.F. and Temperature. Muirhead Cells. Rate of Variation of Temperature, 1° in 15 minutes.



XXXV. *Notes on Thermometry.* By C. Chree, *Sc.D., F.R.S.*

[Concluded from p. 227.]

Lag.

§ 20. GLASS-MERCURY thermometers, and probably all others, differ from the ideal of our definition in requiring a sensible time to follow a change of temperature. This *lag* in a mercury-thermometer increases with the mass of the mercury and the thickness of the glass. It also depends on the nature of the surrounding medium. A clinical thermometer, for instance, initially at 15° C., will rise to the temperature of the body faster in a moist than in a dry mouth, and much faster in a well-stirred bucket of water than in either. In still air where temperature is altering rapidly, two adjacent thermometers of different sluggishness may differ by degrees.

If T denote time, t the thermometer reading, τ the temperature of its surroundings, the formula usually advanced to represent the phenomena is * :

$$\frac{dt}{d\mathrm{T}} + \lambda(t-\tau) = 0,$$

where λ is a constant. When $t-\tau$ is small, this is probably at least a close approximation to the facts. When, however, $t-\tau$ is considerable—as, for instance, when a thermometer initially at 15° C. is suddenly exposed to a temperature of 40° C.—the initial phenomena, in my experience, do not follow so simple a law.

When τ is constant, the solution of the differential equation is

$$t = \tau + (t_0 - \tau)e^{-\lambda\mathrm{T}},$$

where t_0 is the value of t when $\mathrm{T}=0$.

---

* *Cf.* Guillaume's *Thermométrie*, p. 185.

Thus if the temperature τ be absolutely constant, the difference between t and τ tends ultimately to vanish, and the temperature of the medium may be obtained directly, to any required degree of accuracy, by allowing sufficient time to elapse before reading the thermometer. Usually, however, τ is to some extent variable[*], and to arrive at a sufficiently exact interpretation of the readings of the thermometer it may be necessary to carefully observe the rate of variation of t and employ some mathematical analysis. The mere presence of the thermometer tends frequently to introduce variation in the temperature of the medium surrounding its bulb. When, for instance, there is only partial immersion in a liquid bath or in a freezing solution, whose temperature differs appreciably from that of the room, there is a constant transfer of heat along the thermometer-stem, which influences to some extent the temperature recorded. This influence depends largely on the character of the thermometer and the nature of the bath.

The phenomena discussed in this paragraph have an important bearing on the accuracy of ordinary thermometric measurements, which is perhaps somewhat apt to be overlooked by those whose experience has centred in laboratories provided with elaborate thermo-regulators.

Freezing-point of Water.

§ 21. Particulars of the method of determining (depressed zero) freezing-points at the Bureau International will be found in Guillaume's *Thermométrie,* p. 116. The object is to reduce the temperature to 0° C. as rapidly as is consistent with the safety of the thermometer. The ice must be pure, the presence of the least trace of salt being especially objectionable. It should be very finely divided, and sufficiently but not excessively moist. What Guillaume speaks of most favourably is good lake ice or freshly fallen snow moistened with pure distilled water. He quotes the following results so observed by Pernet :—

---

[*] The case where τ is a linear function of the time has been treated recently by Grossmann (*Annalen der Hydrographie der maritimen Meteorologie,* vol. x. 1897, p. 483). The same paper contains references to recent work on the subject and also a variety of interesting experimental results.

TABLE VIII.

	Temperature centigrade.
Distilled water-ice, free from air	0°·0000
Distilled water-ice	−0·0014
Natural ice mixed with distilled water	−0·0017
Natural ice moist	−0·0003
Natural ice, finely divided, dry	−0·0035
Snow, dry	−0·0060
Snow, moistened with distilled water	−0·0004
Snow moistened, after water flowed out	−0·0038

These are to be regarded rather as indications of the order of probable differences under ordinary conditions than as rigid physical data. Thus Marek, whose results Guillaume also quotes *, got for dry artificial ice −0°·0519 or −0°·0919, according as it was finely divided or only pounded.

When ice is dry, its temperature may in reality be very much below the freezing-point. Of course under ordinary experimental conditions the ice is exposed to a temperature over 0°, and its surface is usually moist. The tendency for the ice in the centre of large blocks to be at a lower temperature than 0° C. has been noticed at the Reichsanstalt †, where it is the practice to employ artificial ice from distilled water.

In making artificial ice, the purity of the water seems of some importance, ice from tap or cistern-water giving freezing-points too low by from 0°·002 C. to 0°·003 C. †

To obtain results agreeing to 0°·001 C. a very uniform procedure must be followed, and it is quite conceivable that the adoption of different procedures at different places might lead to slightly different scales of temperature ‡.

One disturbing agent, to which attention may be called, is conduction of heat down the thermometer stem. Though specially to be feared in depressed-zero readings following high temperatures, its absence can hardly, perhaps, be safely assumed in ordinary fixed-zero determinations, especially in a hot room.

In dealing with toluene or other spirit-thermometers, in which the liquid wets the tube, the depressed zero method has been found impracticable at the Bureau International §,

---

* *Loc. cit.* p. 118.

† *Wiss. Abhandl.* vol. i. p. 83.

‡ Cf. *Comptes Rendus . . . de la deuxième Conférence général des Poids et Mesures*, Paris, 1896, pp. 125 and 133.

§ See their pamphlet, *Mesures des basses Températures*, Paris, 1893, p. 4.

owing to the slowness with which the spirit drains down the tube after a fall of temperature.

On the fixed zero method, the thermometer ought theoretically to be kept at 0° C. for an indefinite time. In practice, however, prolonged exposure to a temperature of about 15° C. is a much more usual preliminary. Now a *verre dur* thermometer after long exposure to 15° C. would give, even after 10 or 15 minutes immersion in ice, a zero reading about 0°·015 C. below the true fixed zero, and this, of course, is a very serious difference in thermometry aiming at accuracy of the order 0°·001 C. Even with English glass, where the depression of zero after ordinary atmospheric temperatures is considerably less than with *verre dur*, it is hopeless to think of accuracy above 0°·01 C. unless attention is paid to the previous temperature.

For work of the highest accuracy, the pressure to which the ice is exposed may become of importance. According to the theory of Prof. James Thomson, and the experiments of Lord Kelvin, an increase of one atmosphere in the pressure lowers the freezing-point about 0°·0075 C.* Thus in vacuum-tubes where the pressure is practically nil, or in laboratory experiments with compressed gases, a very sensible correction might be necessary. Even under natural conditions a correction of the order 0°·003 C. might be required at lofty stations.

It must not be overlooked that such variations in external pressure affect the reading in another totally different way, through their direct influence on the glass of the thermometer (see § 26). In fact, care is required to ensure that no external pressure effect is produced by too tight packing of the ice, or by allowing the bulb to rest unsupported on largish ice-crystals.

In determining freezing-points of metals or solutions it is customary to cool down the liquid and observe the stationary point on an immersed thermometer. This method, though not usually adopted, is of course applicable to the freezing-point of water. A discussion of it by Dr. M. Wildermann has appeared recently in the Phil. Mag. †, in which will be found references to recent work on the subject.

---

* Lord Kelvin's 'Mathematical and Physical Papers,' vol. i. pp. 156–169; or Baynes' 'Thermodynamics,' art. lxxxix.

† December, 1897, p. 459.

Boiling-point of Water.

§ 22. In Balfour Stewart's 'Heat' (art. xxi. 1st ed.), 212° F. is defined as the temperature of steam under the pressure of 29·905 inches of mercury reduced to 32° F. at the latitude of London. Balfour Stewart points out a difference between 212° F. and 100° C., defining the latter as the temperature of steam under the barometric pressure of 760 mm. of mercury reduced to the temperature of freezing water in the latitude of Paris. The Bureau International, however, now generally recognized as the central authority, have substituted latitude 45° for that of Paris in the definition *. This has apparently the fortunate result of bringing 212° F. and 100° C. into almost exact agreement.

According to Kater's determination,

$$760 \text{ mm.} = 29{\cdot}9218 \text{ inches};$$

and so

$$759{\cdot}573 \text{ mm.} = 29{\cdot}905 \text{ inches};$$

and, according to Guillaume's *Thermométrie*, p. 325,

$$\{\text{Gravity in London (Standards' Office)}\} \div \{\text{Gravity in latitude } 45^\circ\} = 1{\cdot}0005815.$$

These data make the pressure of 29·905 inches of mercury in London equal to that of 760·031 mm. in latitude 45°, and by Regnault's table (*cf.* Guillaume's *Thermométrie*, p. 327) the boiling-point corresponding to the latter pressure is 100°·001 C.

Recent comparisons †, both American and British, give apparently for the ratio (yard/metre) a value something like one part in 40,000 larger than Kater's, and it is uncertain what accuracy is assignable to values of g, and to what extent it presents local fluctuations. Thus the closeness of the above agreement between 212° F. and 100° C. may not be absolutely relied on. If it should prove that g has a daily or annual variation of one part in 25,000, an uncertainty of the order 0°·001 C. would exist in the standard boiling-point.

---

* Guillaume's *Thermométrie*, p. 4.

† A preliminary discussion of the most recent comparison, initiated by Mr. Chaney, will be found on pp. 37–40 of the *Procès-Verbaux* for 1895 (Paris, 1896) of the International Committee of Weights and Measures. Owing to the absence of a legally defined scale of temperature, there seems some uncertainty as to the interpretation to be put on the standard temperature 62° F. At this point the *verre dur* and hydrogen scales differ by about 0°·075 C.

Also for this degree of accuracy it may prove that specification of the standard latitude, 45°, alone is not sufficient. It is desirable, of course, that the definitions of 212° F. and 100° C. should be absolutely equivalent.

§ 23. When we leave definitions and come to actual determinations of boiling-points, we encounter, as has recently been emphasized by Mr. E. H. Griffiths [*], uncertainties in barometric determinations. Differences of ·002 inch, or more, in the readings of the same barometer by two skilled observers, due mainly to personal equation in setting the ivory pointer, are not unusual. Mr. Griffiths does not refer to this point, but directs his attention to possible differences between barometers themselves. A comparison of a Tonnelot barometer examined at the Bureau International and an English barometer verified at Kew Observatory leads him to the conclusion [†] "that a discrepancy of as much as ·2 mm. would appear to exist between the Sèvres (Bureau International) and Kew standards." He adds, however, that the conditions of the comparison "were not entirely satisfactory."

Several more direct comparisons of the standard barometers at the principal European institutions have been made by skilled observers, using portable barometers as intermediaries. The more important comparisons prior to 1890 are summarized in a table in Waldo's 'Modern Meteorology,' from which I have extracted the following results:—

TABLE IX.

Place and Institution.		Type of Barometer.	Corrections in mm. to reduce to normal barometer (Wild's) at St. Petersburg.			
			Chistoni, 1881.	Waldo, 1883.	Sundell, 1886.	Brounow, 1887.
Paris.	Meteorological Bureau.	Regnault.	−·04	−·05	+·16	+·11
,,	Bureau International.	Wild-Pernet	......	−·24		
		Marek.	......	−·20	+·21	+·10
Berlin.	Normal Standard Commission.	Normal.	......	−·25	−·05	
Vienna.	Central Meteorological Institute ...	Pistor.	−·17	−·08	+·13	+·11
Hamburg.	Deutsche Seewarte.	Normal.	+·11	−·04	+·14	+·07
Kew.	Observatory	Standard.	−·01	−·10	−·05	

If the discrepancies shown by Table IX. actually existed,

---

[*] 'Camb. Phil. Soc. Proceedings,' vol. ix. p. 224.

[†] *Loc. cit.* p. 229.

accuracy even of the order 0°·005 C.[*] in the absolute value of a boiling-point determination would appear highly problematical. It is not unlikely, however, that many of the discrepancies arose in great part from changes in the portable barometers during transit. Barometers, even with contracted tubes, are awkward things to carry by rail, and trustworthy results can only be obtained after the transported instrument has been hung up for a considerable time. Not unlikely Prof. Mohn's[†] suggestion, viz. to compare barometers by the aid of a travelling hyposometric thermometer, might prove a good one, at least in the case of stations where appliances exist for observing both boiling- and freezing-points with the highest degree of accuracy.

§ 24. There are other sources of uncertainty in boiling-point determinations. Every thermometer has a time-lag, and so probably has every barometer; hence simultaneous readings of thermometer and barometer may not really correspond to simultaneous temperature and pressure phenomena. The columns of wide-tube barometers, in which the lag is least, generally pulsate slightly even to the eye, unless under exceptionally steady conditions of pressure. Under normal conditions, one may expect changes of several thousandths of an inch in barometric pressure during the time occupied by a boiling-point determination.

In the paper already referred to, Mr. Griffiths raises the question whether the temperature of steam accommodates itself immediately to the barometric pressure when the latter changes. On his p. 230 he, however, expresses the opinion that a table on his p. 231 "will . . . convince the most sceptical that the temperature-lag of the steam may be disregarded." I am not sure his argument will be universally conceded, as the data—consisting of observed boiling-points by a platinum thermometer, and boiling-points calculated from simultaneous readings of a barometer in the course of one afternoon—might have been advanced, with perhaps equal force, in support of the contention that barometers and platinum thermometers are instruments free from lag. The uncertainties arising from lag are usually small, but still it is desirable

---

[*] Since writing the above, I have observed it stated on p. 102 of the Reichsanstalt's *Wiss. Abhandl.* vol. i. that their determination of the fundamental interval of a certain Tonnelot thermometer differed by ·005 C. from that of the Bureau International. It is significantly added that ·005 C. answers to a difference of ·14 mm. in the barometer-reading at the boiling-point.

[†] Report of the International Meteorological Conference, Paris, 1896, pp. 76–78.

to select as far as possible times of steady barometric pressure for boiling-point determinations.

Calibration[*].

§ 25. This is too technical and mathematical a subject to be profitably discussed here in detail. The methods in use at the Bureau International are described in several of their publications[†] and in Guillaume's *Thermométrie*, pp. 43 *et seqq.* In calibrating an ordinary thermometer, range 0° to 100° C., use is made of threads of 50, 40, 30, 20, 12, 10, 8, 6, 4, and 2 degrees.

The publications of the Reichsanstalt recommend that the shortest mercury-threads used do not exceed in length one degree-division, and speak of an error as large as 0°·01 C.[‡] arising in the calibration of a Tonnelot thermometer when the shortest thread equalled 2°·5. They also recommend a double calibration with each thread, proceeding from one end of the thermometer to the other and back again. This is to serve as a protection against the error that might arise from gradual heating of the mercury-thread. Alleged "nicht unerheblichen" errors[§] in the calibration-data supplied by the Bureau International for one of the Reichsanstalt's *verre dur* thermometers are ascribed to the neglect of this precaution.

The object of calibration at both these institutions is to supply a table of corrections to be applied habitually to every reading of the thermometer. Degree-divisions represent equal lengths, not equal volumes, on the French and German standard thermometers.

Kew standard thermometers, on the other hand, are calibrated before they are subdivided, and the calibration results are used to guide one in dividing the stem into equal volumes, not equal lengths. Most authorities take for granted that this attempt is bound to fail when accuracy of the order 0°·001 C. is in question. This is very probably a true conclusion, but it is not always safe to prophecy failure before the attempt is made. The fact is that no attempt has ever been made to construct Kew standards suitable for this degree of accuracy. They are intended to be read easily by the unaided eye, and the width of the divisions is usually a very considerable multiple of the $\frac{1}{1000}$ of a degree interval. The accuracy ordinarily aimed at in the construction of these instruments is really 0°·05 F., and if we may judge from

---

[*] For a description of the principal methods see a report by Profs. Balfour Stewart, Rücker, and Thorpe, B. A. Report for 1882, p. 145.

[†] *Travaux et Mémoires,* vol. x. 1894 (*Thermomètres Étalons,* pp. 10–16).

[‡] *Wiss. Abhandl.* vol. i. 1894, pp. 39, 40.

[§] *Ibid.* vol. ii. 1895, p. 6.

careful calibrations by Dr. Thorpe and Prof. Rücker *, Prof. Rowland, Mr. Waldo †, and others, the accuracy actually attained is usually considerably higher than this. What can be done depends very much on the natural uniformity of the bore. If the preliminary calibration shows the tube destined for a Kew standard to be unduly conical or erratic in bore it is laid aside. When calibration-tables are constructed, a tube of this kind may seem to be as good as another; but I, for one, would be sorry to pin my faith to it even when calibrated with threads of 1° length.

Pressure Coefficients.

§ 26. The application of increased pressure to the outside of a thermometer, whether through rise of barometric pressure or immersion in a liquid, compresses the glass and reduces its internal volume. This makes the mercury rise in the stem, and, supposing the increase in pressure uniform over the outer glass surface, the rise bears to the increase in pressure a constant ratio, known as the "external pressure coefficient." The unit of pressure usually employed is that of a millimetre column of mercury at 0° C., the rise in the stem being measured in degrees. Knowing the external pressure coefficient, one can calculate the correction necessary to reduce the readings taken with the thermometer under any known external pressure to what they would have been under the standard pressure.

Again, when the horizontal position is adopted as the standard one—as is done at the Bureau International and the Reichsanstalt—a correction is necessary when the thermometer is read in the vertical position, to allow for the influence of the increased internal pressure. This pressure expands the bulb and compresses its contents, both effects contributing to lower the reading. If we treat the internal pressure as uniformly distributed, and proportional to the length of the mercury column measured from the centre of the bulb, we can, by observing the difference in the readings of a thermometer when vertical and when horizontal at any one temperature, calculate an "internal pressure coefficient." This may conveniently be the ratio of the observed rise of reading in degrees—when the thermometer is transferred from the vertical to the horizontal position—to the length of the mercury column measured in millimetres. Knowing this internal pressure coefficient, one can calculate a table giving the correction for internal pressure to be applied to any reading taken with the thermometer vertical.

---

* B. A. Report, 1881, p. 541.

† American Journal of Science, vol. xxi. 1881.

Applying the mathematical theory of elasticity, Guillaume[*] has deduced a very simple relation between the external and internal pressure coefficients, measured of course in the same units. Calling these coefficients β_e and β_i respectively, as Guillaume does, this relation may be written

$$\beta_i - \beta_e = c,$$

where c is proportional to the difference between the compressibility of mercury and that of the particular glass of which the thermometer is made. Thus in all thermometers of the same glass $\beta_i - \beta_e$ should be constant. Taking the units recommended above, viz. rise of 1° in reading and 1 mm. of mercury pressure, the results deduced at the Bureau International[*] and the Reichsanstalt[†] are as follows:—

Glass.	$(\beta_i - \beta_e) \times 10^7$.
Verre dur	154[‡]
Jena glass 16^{III}. . . .	143
" " 59^{III}. . . .	138

§ 27. In determining β_e the thermometer, immersed in a liquid, is exposed to various air pressures, from an atmosphere downwards[§]. The fact that the changes of pressure tend to affect the temperature of the liquid is pointed out in the publications of the Reichsanstalt[‖], and it is apparently recommended that water—preferably near 4° C.—should be used rather than glycerine or mercury[¶].

Observations of the thermometer alternately in the vertical and horizontal positions, at any convenient temperature, supply the means of calculating β_i. The Sèvres-pattern hypsometer is specially adapted for this purpose. The experimental determination of β_i, however, is not so easy as it looks. In a horizontal thermometer the end of the mercury column loses its symmetrical shape—especially in a large bore—and the reading becomes uncertain. There is also apt to be trouble through the capillary effect. Capillarity causes a pressure on the mercury and bulb. Unless this is constant, which ceases

---

[*] *Thermométrie*, pp. 102–103.

[†] *Wiss. Abhandl.* vol. i. p. 70 (1894).

[‡] Schuster and Gannon, Phil. Trans. 1895, p. 436, take 1 cm. of mercury-pressure for the unit instead of 1 mm., giving 154×10^{-6} for the Bureau International's value of $\beta_i - \beta_e$ in *verre dur*. Schuster and Gannon apparently think 141 would be more exact than 154.

[§] See Guillaume's *Thermométrie*, p. 103.

[‖] *Wiss. Abhandl.* vol. ii. 1895, pp. 7–8.

[¶] See Lord Kelvin's 'Mathematical and Physical Papers,' vol. iii. pp. 236–239; also Joule, Phil. Trans. 1859, pp. 133–136.

to be the case when the meniscus changes shape, its influence cannot be neglected.

Regarding the experimental determination of β_i as somewhat uncertain, Guillaume[*] recommends that it be deduced from the observed value of β_e, employing the theoretical connexion between the two coefficients.

The capillarity difficulty is dwelt on in the Reichsanstalt's publications[†]; but, notwithstanding, the plan recommended is to attach equal weights to experimentally determined values β_e', β_i' of the two coefficients, and to calculate values β_e and β_i by means of these and the theoretical value of $\beta_i-\beta_e$. For instance, in a *verre dur* thermometer we should have

$$\beta_e=\tfrac{1}{2}(\beta_e'+\beta_i'-\cdot 0000154),$$

$$\beta_i=\tfrac{1}{2}(\beta_e'+\beta_i'+\cdot 0000154).$$

A table[‡] of observed and calculated values in various thermometers of different patterns shows a pretty close agreement between the *mean* observed difference $\beta_i'-\beta_e'$ and the theoretical; but in individual cases the observed difference varies from 45×10^{-7} to 250×10^{-7}. The explanation favoured is apparently the uncertainty in the capillary effect.

Personally, I am disposed to think that too much reliance is placed on the supposed constancy of $\beta_i-\beta_e$ in all thermometers of the same glass. The supposed relation is exact under two restrictions, absolute homogeneousness in the glass, and uniformity in the distribution of the external and internal pressures over their respective surfaces, inclusive of bulb, stem, and any auxiliary chambers. Under these circumstances a simple proof of the relation has been given by myself[§], and independently by Schuster and Gannon[||]. The supposed uniformity of pressure is, however, non-existent, in the case more especially of the internal pressure, and the means of allowing for this in the theoretical investigation have not yet been devised.

In applying the internal pressure-correction allowance is made at the Reichsanstalt[¶] for the diminution in the density of the mercury-column as the temperature rises. Guillaume[**] omits this, on the ground that the elasticity of the glass also

---

[*] *Loc. cit.* p. 103.

[†] *Wiss. Abhandl.* vol. i. 1894, p. 80.

[‡] *L. c.* p. 79.

[§] Phil. Mag. Oct. 1894, p. 371.

[||] Phil. Trans. A., 1895, p. 435.

[¶] *Wiss. Abhandl.* vol. i. 1894, p. 68.

[**] *Thermométrie*, p. 107, also *Thermomètres Étalons*, p. 18 (Bureau's *Travaux et Mémoires*, vol. x. 1894).

changes with the temperature, and that, at least in the case of *verre dur*, the two influences nearly neutralize one another. The limits of temperature within which this happy arrangement prevails are, I suspect, somewhat uncertain. At temperatures above 100° C., I suspect the uncertainties proceeding from the above sources and from the capillarity are appreciable. Internal pressure-corrections of the order 0°·1 C. are by no means unprecedented, so that when accuracy of the order 0°·001 C. is aimed at, an error of even 2 or 3 per cent. in the value of β_i begins to tell.

§ 28. By adopting the vertical as the standard position, the practice followed at Kew Observatory, the necessity for an internal pressure-correction can usually be avoided, at least for accuracy of the order 0°·01 C. As already pointed out, when the stem is vertical the reduction of the reading, like its two contributory causes expansion of the bulb and compression of the mercury, is proportional, at least as a first approximation, to the length of the mercury-column measured from the middle of the bulb; but in an ordinary thermometer, increment of stem-length is sufficiently nearly proportional to increment of reading. Hence at any temperature t the depression due to internal pressure is, in stem-divisions,

$$q + t/p,$$

where p and q are constants for the thermometer.

Thus to make a Bureau International thermometer register correctly in the vertical position we need only lower the freezing-point mark q divisions below the position answering to a horizontal reading, and shorten each degree-division by $1/p$ of itself. This obviously comes to the same thing as marking the freezing- and boiling-points with the thermometer vertical and subdividing the fundamental interval in the usual way.

This fact has been pointed out by Prof. Schuster and Mr. Gannon*; it was arrived at independently by myself and embodied in a report on thermometry made some years ago to the Kew Observatory Committee.

When an ordinary Kew thermometer is used in the horizontal position a subtractive correction must be applied to obtain the true temperature. Exceptions to this rule exist in the shape of meteorological maximum and minimum thermometers, which it is the custom to employ in a horizontal position. To prevent possible misconception, the certificates issued state explicitly to which of the two positions the table of corrections applies.

---

* Phil. Trans. A., 1895, p. 434.

If the stem of a thermometer were of very unequal bore, and consequently its degree-divisions of very unequal length, the adoption of the vertical as the standard position would not eliminate the internal pressure-correction satisfactorily. It is rare, however, even in the commonest commercial tubes to encounter cases where this need be taken into account for accuracy of the order 0°·01 C.

§ 29. There are, however, thermometers—some of the most approved patterns—for which the ordinary Kew method of avoiding an internal pressure-correction fails. It is becoming increasingly common to have one or more auxiliary chambers blown on stems of thermometers, with the object of including one or both fundamental points in the scale without making the stem inconveniently long. Now the difference between the mercury-pressures in the bulb for two readings, one occurring above, the other below an auxiliary chamber, is no longer proportional to the difference in the temperatures. Thus the determination of the fundamental points in the vertical position, and the subdivision of the stem into equal volumes, each one-hundredth of the fundamental interval, fails to render readings in the vertical position correct measures of temperature. The remedy is easy enough, at least theoretically, as the following investigation shows:—

Let the degree-divisions immediately below and immediately above an auxiliary chamber be respectively t_1 and t_2, while the interval between the two divisions equals the length of τ degree-divisions, instead of t_2-t_1 divisions as it would in an ordinary thermometer. For shortness let

$$t_2-t_1-\tau=\tau' ;$$

in other words let τ' denote the number of degree-divisions by which the stem is shortened through the existence of the chamber.

Let "depression" temporarily signify lowering of reading as the thermometer, its temperature remaining constant, changes from the horizontal to the vertical position; also let Δ represent the depression at the ice-point, and δ the depression due to a column of mercury equal in length to a degree-division.

Above the chamber at a reading t, which may exceed 100° C.,

$$\text{depression}=\Delta+\delta(t-\tau') ;$$

below the chamber at a reading t, which may be below zero,

$$\text{depression}=\Delta+\delta t.$$

The depression, being Δ at the freezing- and $\Delta+\delta(100-\tau')$ at the boiling-point, would require to be given throughout by

$$\Delta+\delta\frac{t}{100}(100-\tau')$$

if the adoption of the vertical position alone were to suffice. Thus above the chamber the depression actually found would be too small by

$$\delta\{(t/100)(100-\tau')-(t-\tau')\}\equiv\delta\tau'(100-t)/100\,;$$

while below the chamber it would be too large by

$$\delta\tau' t/100.$$

Consequently we can accomplish our object by simply shortening each degree-division, whether above or below the chamber, by $\delta\tau'/100$ of its natural length (*i. e.* the length of stem whose volume would be one-hundredth of the fundamental interval). This is equivalent to the following rule:—Determine the freezing- and boiling-points in the vertical position; calculate from a calibration where the divisions t_1 and t_2 immediately below and above the chamber would come in the usual way, but mark the former point lower by $\delta t_1\tau'/100$ and the latter higher by $\delta(100-t_2)\tau'/100$ than according to the calculation; subdivide the stem between the divisions 0 and t_1, t_2 and 100, into equal volumes.

This instruction supposes δ to be known. To determine it observe the difference D in the boiling-point readings obtained with the thermometer vertical and horizontal, then

$$\delta=\mathrm{D}\div(\tau_0+100-\tau'),$$

where τ' has its previous meaning, while τ_0 is the number of degree-divisions between the centre of the bulb and the zero-point. It must be remembered that the correction is a small one, so that we may safely use in our calculation of δ the values obtained for τ_0 and τ' when the correction is neglected.

In practice, for moderate accuracy, the simplest plan would be to mark off two points one slightly below, the other slightly above the chamber, by comparison with an ordinary standard thermometer of the same kind of glass, both thermometers being vertical. Subdividing the two portions of stem included between these marked points and the respective fundamental points into equal volumes, we attain our object.

If a chamber existed above the 100° C. mark, the only change required would be a raising of all the degree-divisions above the chamber—relative to the positions calculated in the ordinary way from calibration—by a constant amount $\delta\tau''$, where δ has its previous meaning, while τ'' is the number of degree-lengths by which the stem is shortened through the existence of the chamber.

In all possible combinations of auxiliary chambers the degree-spaces must be equal in volume throughout the entire stem, if the scale is to be that natural to the glass.

§ 30. In all ordinary mercury-thermometers some trace of air * exists, and if this collects in the stem when the thermometer is exposed to a high temperature the mercury-column is apt to break. Partly to prevent this, and partly to increase the range, limited by the boiling-point of mercury, it is becoming common to introduce nitrogen or carbonic-acid gas above the mercury, sometimes under very considerable pressure. In such thermometers the internal pressure from the gas may much exceed that arising from the weight of the mercury-column. If there were a chamber at the top of the tube similar in size to the thermometer's bulb, and if this chamber and its gaseous contents remained at a constant temperature, the internal pressure contributed by the gas would increase very approximately directly as the length of the mercury-column. Thus, supposing the tube calibrated before the introduction of the gas, the experimental determination of two fixed temperatures and the subdivision of the tube into equal volumes would supply, at least theoretically, an independent standard thermometer. In practice, however, the top chamber is usually of restricted size, and its temperature tends to rise with that of the thermometer. For both reasons the gaseous pressure may become dangerously large when the thermometer is recording near the top of its scale.

In practice it is advisable to regard a gas-filled thermometer as deriving its authority from a direct comparison with an ordinary standard. And the conditions of this comparison should approximate as closely as possible to the circumstances under which the thermometer is to be used.

Under ordinary conditions the external pressure-correction is of trifling importance relative to the internal pressure-correction. Variations of barometric pressure at sea-level may give rise to corrections of a few thousandths of a degree, and similar consequences may follow from immersion in a water-bath. In a heavy liquid, however, such as mercury, a comparatively shallow immersion produces effects which cannot be disregarded even for accuracy to 0°·01 C.

---

* In the Reichsanstalt's *Wiss. Abhandl.* vol. i. p. 71, there is a reference to a supposed influence of the small trace of air or vapour in ordinary thermometers on the internal pressure.

The change of external pressure in balloon ascents or in mountain-climbing merits some consideration. At 20,000 feet above the sea, for instance, neglect of this change might lead to an error of 0°·1 F. in the calculated boiling-point of a thermometer, and thence to an overestimate of some 50 feet in the height.

A more important case arises in the measurement of deep-water temperatures. For this purpose it is customary to use a special pattern of "deep-sea" thermometer, in which complete protection from external pressure is aimed at. Occasionally, however, one meets with instances in which ordinary unprotected thermometers have been used to take readings in deep wells. In one case which came under my notice the consequent error amounted to no less than 15° F. Analogous results would follow the exposure of an ordinary thermometer to high gaseous pressures.

Emergent Column.

§ 31. Elementary theory assumes all the glass and mercury of a thermometer to be at one temperature. Even in well-stirred baths in physical laboratories this is rather an ideal state of matters. In ordinary use there is often an appreciable, sometimes a long, mercury-column exposed to a temperature differing from that of the bulb. This is especially true of thermometers employed to measure the temperature of a liquid, which is largely in excess of the temperature of the surrounding air. In such a case, if a long mercury-column be emergent, the thermometer, if correct, will read considerably below the true temperature of the liquid. A direct mathematical calculation of the correction, supposing even that all the physical data were known, would hardly be feasible, unless one simplified the problem by making jetsom of many of the difficulties. If we suppose the immersed part of the thermometer unaffected by the existence of the emergent part, and assume the whole emergent part, glass and mercury, to be at one temperature, an approximate formula for the correction is easily obtained.

Suppose that the liquid reaches to the division t_1 on the stem, that t is the temperature read and t' the temperature assigned to the emergent column; and use the notation of § 2, x being the correction required. The emergent mercury would at 0° C. have the volume

$$v_0(t-t_1)\,\frac{1+a_1t'+a_2t'^2+\ \dots}{1+\mathrm{A}_1t'+\mathrm{A}_2t'^2+\ \dots},$$

and when it and the corresponding part of the stem are raised in temperature from t' to t the volume increases to

$$v_0(t+x-t_1)(1+a_1t+a_2t^2+\,.\,.).$$

Thus

$$v_0(t-t_1)\,\frac{1+a_1t'+a_2t'^2+\,.\,.}{1+\mathrm{A}_1t'+\mathrm{A}_2t'^2+\,.\,.}\,(1+\mathrm{A}_1t+\mathrm{A}_2t^2+\,.\,.)$$

$$=v_0(t+x-t_1)(1+a_1t+a_2t^2+\,.\,.)\,;$$

whence

$$x=\frac{(t-t_1)(t-t')\{\mathrm{A}_1-a_1+(\mathrm{A}_2-a_2)(t+t')+\,.\,.\}}{(1+a_1t+a_2t^2+\,.\,.)(1+\mathrm{A}_1t'+\,.\,.)}\,.\quad(26)$$

As a first approximation, with the notation of (4),

$$x=e_1(t-t_1)(t-t')^{*}\quad.\quad.\quad.\quad.\quad.\quad(27)$$

As already stated, e_1 is approximately the reciprocal of the number of degree volumes included in the bulb up to the zero mark. It is a constant for any particular kind of glass, and in all ordinary kinds of glass it is a little less than 1/6000. Knowing the glass, we should at once know the correction provided we knew t'.

In reality, of course, the emergent column varies in temperature from base to summit, so that t' must be regarded as its mean temperature. To determine t' directly, the Reichsanstalt employ a "Faden-thermometer," placed apparently with its very elongated bulb closely adjacent to the emergent column. Taking the reading of this auxiliary thermometer for t', and replacing e_1 in (27) by 1/6100, they claim, as already stated, to obtain with thermometers of Jena glass 59^{III} corrected temperature-readings which are consistent to 0°·1 C. even up to 500° C. This practice of the Reichsanstalt seems a development of an idea which originated with Regnault †.

The theoretical aspects of the case are discussed by Guillaume ‡, who refers to the work of Regnault, Mousson, Wüllner, Holzmann, Thorpe, Mills, and Thiesen.

§ 32. In comparing high-range thermometers at the Kew Observatory I have used a formula

$$x=\mathrm{C}(t-t_1)(t-t')\quad.\quad.\quad.\quad.\quad.\quad.\quad(28)$$

---

* This formula supplies a correction to the reading of a maximum thermometer taken when the surrounding temperature is t', if $t-t_1$ represent the number of degrees in the detached column. (*Cf.* Leyst, *Rep. für Meteorologie*, vol. xiv. 1891.)

† *Relation des Expériences*, vol. i. p. 225.

‡ *Thermométrie*, pp. 188–193.

of the same type as (27), but have taken for t' the temperature of the room, and determined C for each thermometer by special experiment. These experiments have been made in two ways:—

1. The lowering in the boiling-point reading caused by increasing the length of mercury-column emergent from the hypsometer has been observed for several lengths of column[*].

2. The thermometer has been attached to a platinum resistance pyrometer, so that the centres of the bulb and resistance-coil are at the same level in a well-stirred bath of fusible metal. Then two series of simultaneous readings have been taken of the two instruments, the immersions in the two series being different. If we assume that the difference of emergence has not affected the readings of the platinum pyrometer, whose coil is completely immersed in both series of experiments, one has obviously the means of calculating C.

The first of the above methods enables one to check the adequacy of the formula for a series of values of t_1 at a specified value of t; the latter enables one to test it readily for a series of values of t. The second method is by no means very difficult in practice when one is content with accuracy of the order $0^\circ{\cdot}1$ C. in the readings, and some promising results have been obtained by means of it. There are several possible pitfalls, however, which want fuller investigation.

When the resistance-coil of a platinum pyrometer is at a high temperature, there can hardly fail to be an appreciable transfer of heat up the tube, both by air convection currents and conduction along the metal leads, tending of course to depress the reading of the pyrometer, and conceivably this depression may vary sensibly when the length of stem immersed above the platinum spiral is altered. Again at high temperatures there is sometimes difficulty in securing a sufficiently slow steady rise of temperature, so that sensible error may arise from a difference in the lag of instruments so different in type as mercury and platinum resistance thermometers.

The apparatus employed in the second method was kindly lent to the Kew Observatory by Mr. C. T. Heycock and Mr. F. H. Neville, who likewise gave me the benefit of their varied experience in comparing thermometers by means of it.

---

[*] *Cf.* Mills, Trans. R. S. E. vol. xxix. 1880, p. 569.

The values obtained for C in formula (28) at Kew Observatory have varied from one thermometer to another. A good deal seems to depend on the fineness of the bore and the thickness of the enveloping glass. Even with a given thermometer so much depends on the conditions to which its stem is exposed, that corrections, whether from a general formula or from direct experiment under one definite set of conditions, are of uncertain value. If corrections are to be deduced from a particular experiment, the conditions ought to be made as similar as possible to those in which the thermometer will normally be employed. The nature of the ventilation over the surface of the hot liquid seems to have a large influence on the temperature of the emergent column and also, one would suspect, on the height of the point where the emergent column has its mean temperature.

Welsh's Method of Graduation.

§ 33. Forty years ago, when Mr. John Welsh, F.R.S., was Superintendent of Kew Observatory, the accurate pointing of thermometers at temperatures within 40° or 50° F. of the normal boiling-point of water presented serious difficulties to makers. At the same time thermometers with such a range as 172° F. to 212° F. were considerably in demand for estimating heights of mountains &c. by hypsometric observations. It was probably this combination of circumstances that led Mr. Welsh to initiate the method to which his name is here attached. The only description of the method published by Welsh, so far as I know, is a brief note entitled "Instructions for the Graduation of Boiling-point Thermometers, intended for the Measurement of Heights," B.A. Report for 1856, Transactions of Sections, p. 49.

In this he gives only numerical results for a particular case. Briefly stated, the method consists in comparing the thermometer with a standard, not in its final state, but in a preliminary state when it contains a larger quantity of mercury.

In the particular case described by Welsh, the excess of mercury in the preliminary state is such that the thermometer reads 212° F. when the real temperature is 82° F., the extra mercury, filling 130 Fahrenheit degree-divisions, being thrown off between the preliminary and final states. Data applicable to the range 172° to 212° F. in the final state are obtained from a comparison in the initial state between the temperatures 42° and 82° F.

As evidence that Welsh did not confine his attention to this particular case, I may mention that he supplied at least one London optician with data applicable to any length of redundant column, and that since his time a considerable variety of thermometers of the type have been sent to Kew Observatory for verification.

The method has gradually extended to thermometers intended for other than hypsometric purposes. Thus before referring further to Welsh's particular case, it is convenient to discuss the general theory.

§ 34. In the *final* state suppose that at 0° C. the internal volume of the thermometer up to the division 0 (or where that division would come if it existed) is V_0, while $100\ v_0$ is then the volume between the divisions 0 and 100. Suppose also that the mercury thrown over when measured at 0° C. occupied τ scale-divisions.

In the preliminary state suppose the thermometer reads $t+\tau+\bar{x}$ when the hydrogen temperature is t, then in the notation of (1) and (2)

$$(V_0+v_0\tau)(1+A_1t+A_2t^2+\ldots)=\\ \{V_0+v_0\tau+v_0(t+\bar{x})\}(1+a_1t+a_2t^2+\ldots),$$

where v_0/V_0 is given by (4).

Eliminating v_0/V_0, and for brevity retaining only constants with suffixes 1 and 2, we get on reduction

$$\bar{x}=x+x', \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (29)$$

where

$$x=t(100-t)(1+a_1t+a_2t^2)^{-1}\left\{\frac{e_1(a_1+100a_2)-e_2}{e_1+100e_2}+a_2t\right\},$$

$$x'=\tau t(e_1+e_2t)/(1+a_1t+a_2t^2). \quad . \quad . \quad . \quad (30)$$

Referring to (8) we see that x is simply the departure at temperature t of the natural scale of the glass of the thermometer from the hydrogen scale. Thus if we compared the thermometer in its preliminary state with an ordinary standard thermometer of the *same* glass, we should find its reading to exceed that of the standard by $\tau+x'$.

The excess of reading thus consists of a constant part τ which should equal the number of degree-lengths (measured at 0° C.) to be thrown over, and of a variable part x' given by (30).

The data already laid down as to the magnitudes of the constants of expansion point to the conclusion that so long as t and τ, in the centigrade scale, are less than 100, it will generally suffice for accuracy of the order 0°·05 C. to replace (30) by

$$x' = e\tau t, \quad . \; . \; . \; . \; . \; . \; . \; . \quad (31)$$

where e is the mean value between 0° and 100° of the coefficient of expansion of the mercury relative to the glass of the thermometer.

Our calculation assumed the thermometer to read correctly in its *final* state, and our conclusion shows that in order that this may be the case it should in the preliminary state read τ degrees too high in ice, and show an error increasing $e\tau$ per degree as we pass up the scale. In other words, in the preliminary state its scale must be too short for the quantity of mercury then present by $100e\tau$ scale-divisions in 100 ; the quantity $100e\tau$ may be conveniently called the *percentage contraction.*

For the mean coefficient of expansion of mercury between 0° and 100° C. Stewart and Gee * give ·0001815. What the mean coefficient of expansion for the glass in English thermometers may be is somewhat uncertain, but it is unlikely to differ much from the value ·000025, given by Stewart and Gee (*l. c.* p. 120) for the mean from nine different kinds of glass.

Taking these values provisionally we have

$$e = \cdot 000156 \dagger,$$
$$100\, e\tau = \cdot 0156 \times \tau.$$

In Welsh's example $\tau = 72\cdot 2$, in centigrade degrees, whence

$$100\, e\tau = 1\cdot 13.$$

The instruction to the optician in this case would thus be : make the scale 1·13 per cent. too contracted for the quantity of mercury originally present ; or, more simply, tell him that the following table of relations applies :—

True temperature (Fahrenheit)	42°	52°	62°	72°	82°
Reading of thermometer in preliminary state	171·55	181·66	191·77	201·89	212.

For simplicity it should be noticed that all we have to consider is the absolute value of $e\tau$ in (31), so long as we measure x' and t on the same scale, whether centigrade or Fahrenheit. In other words the suitable contraction of scale depends only on the amount of mercury thrown off. The mistake, however, of supposing that throwing over

---

* 'Elementary Practical Physics,' vol. i. p. 118.

† For *verre dur* and 59^{III} the values ·000158 and ·0001645 respectively are found at the Reichsanstalt (*Wiss. Abhandl.* vol. ii. pp. 9, 17).

100° C. on a centigrade thermometer is equivalent to throwing over 100° F. on a Fahrenheit thermometer is one against which experience shows me a warning is necessary. The contraction of scale required in the former case is of course 9/5 times that required in the latter.

Welsh's figures, as a matter of fact, differ slightly from those given above, in a systematic way; he has 201·87 for example in place of 201·89, and 171·48 in place of 171·55. The cause of this discrepancy was, I suspect, his neglecting any expansion in the glass, for his figures are deducible from (31) by taking ·000180 as the value of e. The B.A. figures in fact accord with the rule "contract the scale by 1 per cent. for each 100 degrees (Fahrenheit) of mercury to be thrown over," which I know Welsh to have actually laid down.

§ 35. In discussing the theory of Welsh's method we supposed the surplus mercury to be measured at 0° C., as this shows most simply its relation to what remains. In the actual operation it is hardly possible to have mercury and glass at 0° C., and it is usually inconvenient to keep the detached column in the tube and measure it at 0° C. before throwing it off. Welsh's own idea for boiling-point thermometers was simply to put the thermometers in steam and throw off the quantity of mercury required to make them give the correct boiling-point reading. Supposing this not exactly accomplished, he directed that the error observed at 212° F. in the final state should be regarded as a constant zero error, being combined of course with the calibration errors detected by the comparison with the standard thermometer in the preliminary state.

This is certainly satisfactory in short-range thermometers such as Welsh had in view. For example, to introduce an error *varying* by 0°·01 C. throughout a range of 25° C. there would require to be a mistake of about $2\frac{1}{2}$ scale-divisions in the length of the mercury-column thrown over, and this is at least ten times as great as is at all likely in the hands of a competent workman.

§ 36. The modern extension of Welsh's method to high-range thermometers is no doubt exposed to greater uncertainty. Suppose, for instance, we wish the thermometer in its final state to cover the range 200° C. to 350° C. approximately. In the preliminary state the thermometer may cover say from 0° C. to 150° C.—the accuracy of the scale throughout the upper 50° being determined by calibration with a mercury-column whose value is observed at the lower part of the scale, where direct comparison with a standard thermometer is possible. The simplest way to break off the necessary mercury-column is to heat the bulb with a spirit-lamp so as to force a long column into the tube. After it is broken off and measured it must be run into the top chamber, and a second column must be broken off in the same way, so as to get the complete quantity, 200 divisions long, that is required.

The possible cumulation of errors of measurement may be trifling when the operator is skilled: but the very considerable heating of the bulb and mercury renders it appreciably uncertain what is the true length of the mercury-column at 0° C.

For definiteness and generality let us suppose that the mercury thrown off fills τ stem-divisions when mercury and glass are at a temperature of T (on the hydrogen scale), instead of τ divisions at 0° as was intended. Then in its final state the thermometer when at 0° has a volume of mercury too great by

$$v_0 \tau \mathrm{T} \frac{e_1 + e_2 \mathrm{T} + \ldots}{1 + \mathrm{A}_1 \mathrm{T} + \mathrm{A}_2 \mathrm{T}^2 + \ldots}.$$

Consequently at temperature t the thermometer will read y degrees higher than a standard of its own glass, where

$$y = \tau \mathrm{T} \frac{e_1 + e_2 \mathrm{T} + \ldots}{1 + \mathrm{A}_1 \mathrm{T} + \mathrm{A}_2 \mathrm{T}^2 + \ldots} \frac{1 + \mathrm{A}_1 t + \mathrm{A}_2 t^2 + \ldots}{1 + a_1 t + a_2 t^2 + \ldots}. \qquad (32)$$

Suppose, for instance, in the case $\tau = 200$, recently selected, that T is 100° C., corresponding to an error of about $1\frac{1}{2}$ per cent. in the quantity of mercury thrown off. Using the mean values of expansion coefficients between 0° and 100° C. instead of the series—which is quite good enough for our present purpose—we get approximately, in degrees centigrade

$$y = 3{\cdot}10 (1 + {\cdot}000156\, t).$$

This makes the error vary throughout the entire range of 150° C. only between 3°·20 C. and 3°·25 C.

Accuracy to 0°·05 C. is, I suspect, at present a high ideal in mercury thermometers used above 200° C. Thus even in the above case it is doubtful whether we can improve on Welsh's direction to treat the difference observed at some standard point as a constant zero correction.

If, however, the observed error rose to 4° or 5° C. we should certainly expose ourselves to errors of the order 0°·1 C. if we followed this direction.

A direct determination of the error at some one temperature in the final state is all the more necessary because the operation of knocking off the mercury-column, involving severe heating of the bulb, may leave a slight permanent effect.

It is important to notice, in the case of any such high-range thermometer as that just described, that the method assumes the thermometer to be of the same kind of glass as the standard with which it was compared in the preliminary state, and that the readings of the thermometer in its final state—corrected for errors of calibration and of volume of mercury thrown off—are temperatures on the natural scale of a standard thermometer of this particular kind of glass. The readings have no direct relationship to the hydrogen or any other gas-thermometer scale, and temperatures measured near the top of the range must possess an uncertain value unless the natural scale they relate to has been studied.

If the high-range thermometer and the standard it is compared with are of different kinds of glass, and still more if the thermometer has its bulb of one kind of glass and its stem of another, the interpretation of its readings would require further consideration.

§ 37. As related to Welsh's method, I would mention an ingenious way of obtaining e, the mean coefficient of expansion of mercury in glass between 0° and 100° C., described on pp. 102–104 of the Reichsanstalt's *Wiss. Abhandl.* vol. i. It consists in observing the fundamental interval in a thermometer's normal state, and also after a series of threads of mercury of different lengths have been detached and run into a top chamber. For instance, let f, f_1, f_2, f_3 represent the observed fundamental intervals in the original state and after detachment of threads of 50, 100, and 150 degrees respectively. Then, n denoting the number of degree volumes in the bulb up to the zero mark, we have

$$100\,e = f/n = f_1/(n-50) = f_2/(n-100) = f_3/(n-150).$$

Thence we can deduce e and n by least squares or otherwise. Obviously the method need not be restricted to the range 0° to 100° C.

Comparison of Thermometric Methods.

§ 38. It remains for us to consider briefly the relative defects and advantages of the ordinary British methods of thermometry and the more refined methods of the Bureau International. In favour of the latter is the strong argument that when a *verre dur* thermometer verified at the Bureau

International is used in a carefully prescribed way, one can deduce the corresponding temperature on the scale of the hydrogen thermometer (at least throughout the range $-20°$ C. to 100° C.) to a very high degree of accuracy. In a physical laboratory the probable error may be as small as $\pm 0°{\cdot}002$ C., or possibly even $\pm 0°{\cdot}001$ C. It may also be urged that the reduction of the thermometer readings by means of the tables supplied by the Bureau International requires no higher arithmetical powers than every well educated man should possess.

On the other hand, it must be conceded that the existence of lengthy tables is an evil, however necessary, both on account of the very appreciable labour their application entails, and on account of the large increase their calculation makes to the prime cost of the instrument. The fact that for high accuracy an ice reading is desirable after every temperature observation is also a drawback. Very considerable skill is required when the preceding temperature is high, the risk of breaking the thermometer being appreciable, and the frequent preparation of ice is both troublesome and expensive. The conditions under which it is safe to dispense with an ice observation and use the Bureau's table of depressed freezing-points for *verre dur*, are somewhat uncertain. The use of the table is presumably satisfactory when the temperature has been stationary for a long time*, or has been made to rise very slowly. But *how* slow the rise must be, or what is to be done when the temperature falls or oscillates, appears by no means clear. Again, if the thermometer has been at a temperature approaching 100° C. one cannot safely plunge it straight into ice. The time required adequately to immerse it varies with the skill and audacity of the operator. Meantime the thermometer is probably changing its condition, with a rapidity which may depend on the time of its exposure to the high temperature and its previous history. At really high temperatures, 150° C. and upwards, increasing difficulties can hardly fail to oppose the successful use of the depressed zero method, unless the glass employed be extremely little affected by sudden physical changes.

In favour of the customary methods of using English glass thermometers there are certain advantages. The necessity for an internal pressure correction, as we have seen, is avoided

---

* Schuster and Gannon (Phil. Trans. for 1895, p. 429) conclude that $2\frac{1}{2}$ *hours* exposure of a *verre dur* thermometer to a temperature of $22\frac{1}{2}°$ C. was insufficient to produce the full depression of zero. The subsequent ice-reading was, they think, as much as $0°{\cdot}002$ C. too high.

by adopting the vertical as the standard position. The actual readings of a correctly divided thermometer, after allowance is made for any secular change of zero, are likely to give at ordinary atmospheric temperatures—where nearly all very exact absolute measurements are made—results agreeing with those of the hydrogen thermometer to within 0°·1 C. To those physicists and chemists who are accustomed to record temperatures to 0°·001 C., or even 0°·0001 C., this may seem a paltry claim: but it does not seem so to many scientific men whose interests extend beyond the temperature of the thermometer to that of its surroundings. For many purposes, including even some investigations of high scientific importance, the measurement of temperatures to 0°·001 C. is just as futile as the use of 10 figure logarithms in handling data in which the fourth significant figure is uncertain.

On the other hand, there unquestionably exist physical and chemical investigations, tending to increase in number, in which the absolute determination of temperatures with the highest possible precision is of fundamental importance. For these, in the mean time, English glass thermometers and ordinary British methods are not suitable. However good the workmanship, and however accurate the calibration, there exist the following defects:—

1. Ice readings corresponding to infinitely prolonged exposure to 0° C. are not practically obtainable; and ordinary ice readings being affected to some extent by the previous temperature are not strictly comparable.

2. So long as changes of $3\frac{1}{2}$ inches of mercury in barometric pressure are possible, and occasions arise for the immersion of thermometers in heavy liquids, corrections for external pressure cannot always be avoided.

3. Unless frequently subjected to temperature-cycles of considerable range, an ordinary English glass thermometer is apt to be influenced for days, it may be weeks, by exposure to any temperature much over 120° F.

4. For accuracy of an order higher than 0°·1 F. it is certainly unsafe to assume the natural scale of an English glass thermometer identical with that of the hydrogen or nitrogen thermometer, even for the restricted range 22° F. to 212° F.

5. Whether through variety in the constitution of the glass or differences in its treatment, the natural scales of ordinary English glass thermometers do not appear sufficiently accordant to render practicable the use of any general table of reductions to a standard scale of temperature.

For leave to publish particulars of experiments made at

Kew Observatory I am indebted to the Kew Observatory Committee. To one of its members, Mr. W. N. Shaw, F.R.S., I am indebted for a variety of suggestions which have added to the lucidity and completeness of the paper. For any expressions of opinion, and for the accuracy of the facts, I am, however, personally exclusively responsible.

XXXVI. *Radiation Phenomena in the Magnetic Field.*

By Thomas Preston, M.A.*

[Plate XXIII.]

In March 1897, Dr. P. Zeeman announced in the pages of this Magazine the important discovery that the bright lines of the spectra become sensibly modified in appearance and constitution when the source of light is placed in a strong magnetic field. This effect has often been sought for both by men who have published the results of their investigations as well as by many who have not, but it is only now, thanks to the work of Dr. Zeeman, that the scientific world has become convinced of its existence and nature.

Considerable difficulty attends the experimental investigation of this effect owing to its smallness, and it is only when a very strong magnetic field is used in combination with high dispersion that it can be placed distinctly in evidence. Its investigation, however, has been greatly assisted by the present advanced state of electromagnetic theory, which pointed out what the essential features of the phenomenon were which should be looked for. Thus, the theory informed us that each bright line of a line-spectrum should be converted into a doublet, or a triplet, according as the source of light is viewed along, or across, the lines of magnetic force, and further, that each member of a doublet should be circularly polarized, whereas each member of a triplet should be plane polarized, the plane of polarization of the central line being at right angles to that of the two side lines.

This information proved of special importance to the experimental inquiry owing to the fact that a bright spectral line is not a mathematical line, but, on the contrary, possesses a finite width, so that in order to observe the triplet effect the resolution must be great enough to separate the lateral components from each other by a distance greater than the width of the spectral line in question. For resolutions less than this the central member of the triplet overlaps the lateral components, and the effect presented to the eye is

---

* Communicated by the Author.

what appears to be merely a broadening of the bright line by the magnetic field. Once we know, however, that the central line is polarized in a plane at right angles to that of the two side lines we can, by suitably interposing a Nicol's prism, obliterate the central constituent and leave the two side lines of the triplet unextinguished. The overlapping is thus removed and the triplet constitution, as distinguished from a mere broadening, of the affected line becomes to some extent revealed even though the magnetic field has not been strong enough to completely separate the constituents of the triplet from each other.

It is much more satisfactory, however, to push the resolution to such a point that each member of the triplet stands revealed and isolated from the others, so that their general character may be studied separately. In fact, until this has been effected, we cannot assert that a line can be resolved into three others by the magnetic field. The observations mentioned above with the nicol are consistent with tripling, but they do not absolutely prove it. With the object of doing this, if possible, and also with the ulterior object of making a general survey of the spectra of various substances, in order to determine if the spectra of different substances were equally or similarly affected, or if the various spectral lines of a single substance were equally or differently affected, I gladly availed myself of the opportunity afforded me of using the excellent Rowland's concave grating mounted in the physical laboratory of the Royal University of Ireland*. This instrument is of the usual type, having a radius of 21·5 ft., and ruled with about 14,438 lines to the inch. It is fitted with a camera-box, which takes a photographic plate 50 cm. long and 6 cm. broad, so that a length of the spectrum equal to half a metre approximately can be photographed at a single exposure.

As a consequence, I naturally decided to study the phenomena by means of photography as well as by eye observation, for the latter, besides being applicable in the visible portion of the spectrum alone, lends itself somewhat to the personal bias of the observer in the case of small and doubtful effects. The photographic plate, on the other hand, gives a faithful record of the phenomena as they actually exist in the image focussed upon it, and this record can be examined at leisure. Moreover, as a considerable length of the spectrum can be photographed at a single exposure, the

---

* A description of the mounting of this instrument by Dr. W. E. Adeney and Mr. James Carson is published in the Proc. Roy. Dub. Soc. February 1898.

effects produced on many lines, under exactly the same circumstances, can be compared with precision. Still it is to be remembered that the photographic plate is not capable of reproducing detail beyond a certain order of fineness, and, further, that the negative is of the nature of an integral and reproduces the sum total of all the effects produced on it during the time of exposure. For this reason, if the image varies, owing to variations going on in the source of light, or elsewhere, during the time of exposure, the photographic negative will be the muddled integral of these various images, and may not in the end be similar to any one of the various images which produced it. For this reason, the examination of the visible portion of the spectrum by means of the eye cannot be dispensed with, as will appear from what follows.

In the case of a phenomenon which exhibits itself as a very small effect, and one which it is difficult to obtain and observe, it is natural that some doubt should exist at first as to what it is that actually is presented to the observer, and that discrepancies should occur in the statements of different observers regarding the phenomenon now under consideration is not surprising. Thus, while Dr. Zeeman distinctly states that he obtained a tripling of the spectral lines when the source of light is viewed across the lines of magnetic force, and a doubling when it is viewed along the lines of force; yet these effects were obtained only after the theory of the phenomena pointed in this direction, and other reliable observers investigating the same lines have given expression to the opinion that when viewed *across* the lines of force a doubling (like a reversal) or a broadening combined with a doubling occurred. So far as I am aware, all these observers have failed to obtain distinct triplets when the light is viewed across the lines of force, that is, they have not succeeded in separating the constituents sufficiently to enable them to decide what the exact composition of the modified spectral line is. By placing a Nicol's prism in the path of the light it has been determined that the central part of the modified line is plane polarized, and that the edges are also plane polarized, but in a plane at right angles to the plane of polarization of the middle part. Thus with the nicol in one position the middle may be cut out, leaving the two border lines, and with the nicol turned through a right angle the two border lines may be removed, leaving the middle portion alone. It is to be remarked, however, that observations of this nature, although they are in accordance with the supposition that the modified line is a triplet, yet they do not absolutely prove tripling pure and simple. For this purpose it is necessary to obtain high resolving power and a magnetic field strong enough to separate completely the constituent parts of the modified line, so that each can be observed separately, without the complications attending the overlapping of the others.

With the ordinary U-shaped electromagnet at my disposal I had considerable difficulty at first in obtaining any effect whatsoever, even a broadening of the spectral lines. However, by pushing the pole-pieces very close together and working with a very short spark as the source of light, I was able to obtain not only a distinct broadening but ultimately a complete separation of the constituents of the affected line—in the case of certain lines—so that the photographic negative showed them as distinct triplets with each component sharp and clear when viewed through any ordinary hand magnifying-glass. Finally, with a more powerful magnet of the same U-shaped type, kindly lent to me by the Rt. Rev. Monsignor Molloy, I was able to obtain photographs which showed triplets visible to the naked eye without any magnification whatever.

With this apparatus I have made a general survey of the spectra of various substances, from the lowest visible to the highest ultra-violet, and the various effects which I have so far observed I shall now briefly describe.

General Types of Effect observed.

If we take the case of a single substance, say cadmium or zinc, the most casual observation is sufficient to show that the effect produced varies considerably for the different spectral lines; for while some lines are clearly separated into distinct triplets, others are not so resolved, but show on the photographic plate as "quartets" or "doublets," the source being viewed *across* the lines of force, *i. e.* while some lines show as sharp triplets, others (if we wish to express it so) show as triplets in which the middle line is replaced by two fainter lines, while others appear to have lost the middle line completely. Further, some lines on the same plate, that is, photographed under exactly the same conditions, show only a very small broadening; but in no case have I found a line which could with certainty be said to be absolutely unaffected by the magnetic field.

Thus, while some lines are largely affected, others (and these may be of not very different wave-length) are scarcely affected at all. With a stronger magnetic field, however, I hope to resolve these latter, and so obtain the data necessary to a full comparison of the effects produced on all the spectral lines, and so be able to attack the problem of throwing the lines into groups and series, periodic or otherwise, subject to some general law. Although the photographs which I have already obtained throw out some hints as to the direction in which such a law is to be looked for, yet nothing can be sufficiently decided till a careful survey has been made and every line resolved and measured.

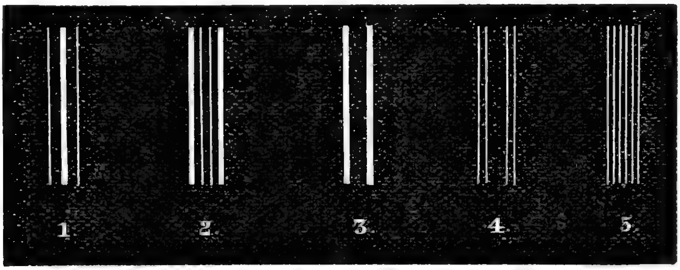
The Plate accompanying this paper (Pl. XXIII.) has been reproduced from a portion of a negative showing the lines of cadmium and zinc taken simultaneously. For this purpose an electric spark from an induction-coil was passed between two electrodes, one of cadmium and one of zinc, so that the lines of the two metals as affected by the magnetic field were photographed under precisely the same conditions. The lines shown are the blue (4800) and the violet (4678) lines of cadmium together with the adjacent lines (4811, 4722, and 4680) of zinc. The Plate shows the various lines unaffected, that is, when the magnet is unexcited, as well as the effects produced by the magnetic field. The bottom row AA shows all the lines converted into doublets when the source of light is viewed *along* the lines of force. The next row BB shows the lines unaffected, *i. e.* with no magnetic field.

The next row CC shows the appearance presented when the source is viewed across the lines of force, and it may be here observed that while the line 4678 of cadmium and the adjacent line 4680 of zinc are both converted into triplets, the line 4722 of zinc and the celebrated blue cadmium line 4800 are not so resolved (in appearance), but show as a species of quartets in which most of the light is concentrated in the side lines. Finally, the line 4811 of zinc shows as nothing very definite, but may in this picture be described as a hazy doublet. It is to be remarked, however, that many lines show as perfectly distinct doublets when viewed across the lines of force, doublets which are scarcely rendered any clearer by the interposition of a nicol.

The two rows at the top of the plate show the polarization of the affected lines. In these a nicol has been inserted in the path of the beam of light. In the top row EE the nicol is placed with its principal plane parallel to the lines of force, and in the other row DD with its principal plane at right angles to the lines of force. The polarization of the constituents is thus shown to be complete and in two rectangular planes.

The foregoing are the appearances presented on the photographic plate. In addition to these there are two other modifications depending on a fineness of detail not as yet recorded on the photographic plate, but which presents itself with great distinctness to the naked eye when the lines are viewed through a well-focussed eyepiece. One of these modifications occurs in the case of lines which photograph as doublets when the source of light is viewed across the lines of force, and may be described by saying that the affected line is a modified triplet in which the central line is absent, and in which at the same time each of the side lines is reversed. Thus the appearance presented to the eye is that of two pairs of lines rather than a doublet or triplet. The other modification of note is that in which each constituent of the triplet shows as a pair of lines, or, in other words, the whole appears not as a triplet, but as a sextet of fine equally spaced well-marked and sharp lines.

All the modifications so far observed, when the source of light is viewed *across* the lines of force, are illustrated by the accompanying woodcut. Thus in fig. 1 we have the normal triplet, such as is presented by the violet line of cadmium 4678 and the vast majority of other lines. Then we have in fig. 2 the weak middled "quartet" in which nearly all the light is concentrated in the two side lines. Next we have in fig. 3 the doublet in which the central line has completely disappeared. Next in fig. 4 we have the double doublet, or two pairs of fine lines, and finally in fig. 5 the sextet or three pairs of equally spaced sharp lines.



The Sodium Lines.

It is interesting to remark that the two D-lines of sodium do not belong to the class which show as triplets in the magnetic field. Owing to the ease with which these lines reverse and to the constant variations going on in the vapour-density in the spark obtained from a solution of the salt, it is not easy to obtain the sodium lines in a steady state for any length of time. But when the sodium salt is contained in small quantity in the solution the lines may be obtained sharp and fairly steady. Under these circumstances one of the sodium lines D_2 shows as a sextet (fig. 5) of fine sharp equally-spaced lines.

of which the two border-lines are somewhat nebulous on their outside edges. The other line D_1 is of the double doublet type (fig. 4), and shows as two pairs of sharp lines. It is thus seen that the sodium lines and the blue cadmium line 4800 do not show as normal triplets; and it is probably for this reason that different observers have differed in their accounts as to the effects observed in the case of these lines*. Difficulties no doubt encumber the observation of the true effect in the case of the sodium lines by reason of the ease with which they become reversed. If the sodium salt is present in any considerable quantity, there is constant sputtering and reversal of the lines which completely masks the true effect.

Explanation of the Various Appearances.

All the modifications of the normal triplet described above may be sufficiently accounted for by the supposition that reversal, or more or less complete absorption, takes place in the case of one or more of the constituents of the triplet. Thus, if the middle line of the triplet (1) becomes reversed, *i. e.* has its middle absorbed, we are furnished with the weak middled quartet (2);, and if the middle line becomes completely absorbed in the layers of colder vapour surrounding the spark, then we have the doublet (3). While if, in addition, the side lines of (3) become reversed, we have the double doublet (4); and, finally, if each line of the triplet (1) becomes reversed, we have the sextet (5).

Although the hypothesis of reversal or absorption accounts easily for the number of the constituents in the modified lines, yet it must be remarked that the appearance of these modifications is not that which is usually associated with reversal. The constituents are not sharp on one side and nebulous on the other, but are sharp and well defined on both sides; nor do the original lines when unaffected by the magnetic field, that is when photographed in the free field, show any sign of reversal. For this reason, in order to test this matter, I have spent much time in taking photographs under various conditions in regard to length of exposure and vapour-density in spark, but so far without any conclusive result. On the whole, however, the weight of evidence lends itself to the supposition that all these variations of the normal triplet type are produced by absorption in the vapour of the spark.

An interesting point to be remembered in this connexion is that the spectral lines may be more or less complex in their

---

* See Fievez, *Bull. de l'Acad. Roy. de Belgique*, 3rd series, vol. ix. p. 381 (1885); Lodge and Davies, 'Nature,' vol. lvi. p. 237 (1897); Zeeman, Phil. Mag. vol. xliv. p. 55 (1897), &c.

own constitution, and an important investigation bearing on this subject has already been made by Prof. A. A. Michelson*, who has shown that the D-lines of sodium and the green and blue lines of cadmium (with others) are really complex in constitution. Now these lines are amongst those which show deviations from the normal triplet type, and it is consequently of importance to determine if this complexity of structure gives rise to the deviations from the normal triplet, or if, on the other hand, the deviations as well as the complexity of structure observed by Michelson are due to a common cause, namely intermittent reversal. For example, if a bright line is not simple but really a close doublet, then in the magnetic field each member of this doublet will give rise to a corresponding triplet. These triplets, however, will overlap and appear as one triplet in the field of view. Yet it is possible to conceive a state of affairs such that, by the action of the magnetic field, one or more of the members of one triplet shall be opposite in phase to the corresponding member of the other triplet. Under such circumstances the overlapping lines will "interfere" and produce doublets. Thus if the central lines alone are opposite in phase they will obliterate each other where they overlap, and the weak-middled quartet will be produced as in the case of the blue cadmium line, and so on.

It is to be remembered, however, that the conditions in the magnetic field are not the same as in the free field, for in the former the spark is violently blown about, and this may lead to a sharpness on both sides of the reversed lines not obtainable in the free field, and thus account for the fact that the modifications described above do not look like ordinary reversals.

Viewed from the theoretical standpoint, however, we have no reason to demand that all, or indeed necessarily any, of the spectral lines should be resolved into sharp triplets when the source of light is viewed across the lines of magnetic force. For, in order that a spectral line should exhibit itself as a characteristic triplet under the influence of the magnetic field, it is necessary that the freedom of vibration should be equal in all directions, and in this case the intensity of each rectangular component will be the same. Hence the middle line of the triplet will contain as much light as the two side lines taken together. If, however, the vibrations are not equally free in all directions the foregoing result will not hold, and it becomes possible to have a triplet with a weak middle line or

---

* A. A. Michelson, 'Brit. Assoc. Report,' 1892, and Phil. Mag. vol. xxxiv. p. 280 (1892). I have to thank Prof. G. F. FitzGerald for attracting my attention to this point.

without a middle line, in which case it shows as a doublet. For example, if the vibration is restricted to one plane, and if this plane sets itself at right angles to the lines of magnetic force under the influence of the magnetic field, then the component of the vibration in the direction of the lines of force will be zero, and the middle line will vanish from the triplet. If, on the other hand, the complete vibration should set itself parallel to the lines of force, then the side lines of the triplet would vanish, or, in other words, the spectral line would be unaffected by the magnetic field. It is clear, therefore, that the study of the way in which the spectral lines are affected by the magnetic field is likely to throw light on the character of the molecular vibrations.

The Magnetic Substances.

The substances which one would expect to present peculiarities in this way are iron and nickel, which, if they retain any of their magnetic properties at the high temperature of the spark produced between the terminals of an induction-coil, should exhibit some characteristic behaviour in the magnetic field. I was led to expect, indeed, that many, if not all, of the iron lines would be resolved into doublets rather than triplets when the spark is viewed *across* the field. I found it no easy matter, however, with the magnetic field at my disposal to resolve the iron lines into anything: in the first place because the effect is much smaller (about one-half) for iron than for the 4678 line of cadmium or zinc; and in the second place, because it is not possible to work with the pole-pieces of the magnet so close together when the spark is passed from a solution of a salt as when it is passed between small metal electrodes. But in the case of a highly-magnetic substance like iron, the metal becomes almost unmanageable in a powerful magnetic field, and so it happened that in my first attempts I was not successful in resolving the iron lines into either doublets or triplets; yet, even at this stage, I observed two or three lines converted into what I considered doublets. Finally, I succeeded in resolving the vast majority of the lines of iron by enclosing pieces of iron wire in small glass tubes, so that the ends of the wire protruded slightly from the glass where the spark occurred. The pole-pieces were then pushed up to touch the glass jackets, and the spark was thus obtained in a very strong field, in fact in a field strong enough to resolve the majority of the iron lines into distinct triplets, showing that these vibrations possess freedom in the magnetic field (at the temperature of the spark); but, in addition to this, other lines are observed as distinct doublets, and the appearance of these doublets is not that usually associated with a reversal. In addition there are other lines in the spectrum of iron which are scarcely affected, if at all, by the field at my disposal.

Similar remarks apply to the magnetic metal nickel, but it is not to be understood that these occasional doublets are in any way characteristic of the magnetic metals, for they occur with considerable frequency in the spectra of many other substances*, such for example as barium, platinum, rhodium, &c., &c.

Gases.

The investigation of the spectra of gases in the magnetic field is a department of this inquiry which would naturally be looked forward to with considerable interest, and it is perhaps disappointing to have to record that, so far, I have not been able to detect any sensational behaviour on their part in the magnetic field.

Observations were made on the spectra of gases under small pressure contained in "vacuum" tubes. The tubes which I had at my disposal, however, showed no lines bright enough for observation except the hydrogen lines, and these were bright and sharp when the magnet was unexcited. When the magnet was excited, however, they became faint and nebulous, showing a scattering of the light and broadening such as might be produced by increase of pressure, both to the eye and on the photographic plate. In addition, the necessity for placing the vacuum-tube between the pole-pieces reduced the strength of the magnetic field, and whether from this or other causes I was unable to observe triplication pure and simple of the lines in any case.

When the electric spark is passed between electrodes in air, the air lines were photographed as well as the lines characteristic of the electrodes, and in all cases attention was paid to the effect produced on the air lines as well as those of the metals. In every case the air lines showed distinct broadening, but in no case were they resolved into triplets. This resolution indeed could hardly be expected except in a very powerful field, owing to the considerable width and nebulous character of the air lines. Nevertheless, by placing a nicol in the path of the light so as to remove the central member of the triplet,

---

* I am indebted to Professor W. N. Hartley who generously placed at my service specimens of several rare substances in a pure state, and to Dr. W. E. Adeney I am further indebted for much friendly assistance in promoting the laboratory work.

distinct doublets have been obtained on the photographic plate, showing that the gases are subject to the influence of the magnetic field, but in no exceptionally marked degree. The photographs which I have already taken, indeed, show a resolution of the violet air lines which is only about two-thirds that produced on the violet lines of zinc and cadmium.

The study of these substances has consequently been postponed till a stronger magnetic field is obtained, which I trust to have soon at my disposal.

Measurements.

Although from time to time measurements have been made on various photographic plates, in order to compare the effects produced on different lines and different substances, yet, so far, no systematic attempt has been made at exact quantitative determination, for it did not appear possible to turn such work to good account until the resolution of all the lines had been effected in a measurable form, or at least until the further resolution (which it is hoped may be soon obtained) has been effected.

In the meantime, however, the following numbers will show the general trend of the effect in the case of a few conspicuous lines. The strength of the magnetic field was not accurately determined, and indeed was of little importance for our present purpose, but it may be taken at something over 20,000 C.G.S. units roughly. One millimetre on the photographic negative corresponds to 2·6 Ångström-units.

In the case of cadmium we have :—

Wave-length.	Distance between side lines of triplet in $\frac{1}{100}$ millim. approx.
5086	29
4800	41
4678	43·5
4413	23
3613	17
3610	17

In the case of zinc we find :—

Wave-length.	Distance between side lines of triplet in $\frac{1}{100}$ millim. approx.
4810·7	29
4722	39
4680	43
4058	18
3683	17

These numbers, although they possess only a rough accurracy, are yet sufficient to show that for both these metals the effect at first increases as the wave-length diminishes, and, in a parallel manner, after rising to a maximum, gradually diminishes. The measurements were made on a photograph taken from a spark passing between a cadmium electrode and a zinc electrode, so that the lines of both substances were taken under exactly the same circumstances. They therefore represent the effects produced on these two substances by the *same* magnetic field.

Other substances were studied in the same way with one electrode of cadmium or zinc, so that the effects produced by the same field could be compared.

For example, in the case of tin and zinc (one electrode of each), when the violet line of zinc 4680 showed a widening of 42, in the units used above, the tin lines 4525 and 3801 showed widenings amounting to 21 and 13 respectively. Thus the resolution for tin is about half as much * as for zinc or cadmium.

Similar results apply to other metals. For example, measurements made on the spectrum of platinum gave a separation ranging from 30 downwards. The magnetic metals iron and nickel gave separations varying from 18 downwards, and rhodium gave similar results.

Many other substances have been examined, but no further novelty is exhibited by them. The above are mentioned merely as typical elements. For splendour and variety of effect throughout the whole range of the spectrum, the substances calcium and barium are noteworthy.

These results do not point to any parallel between the magnetic effect and the pressure-effect investigated by Messrs. Humphreys † and Möhler.

It has not been considered necessary to refer to the theoretical explanation of this magnetic influence on the spectral lines, as Dr. J. Larmor ‡ has already furnished a completely satisfactory explanation of the typical phenomena on the basis

---

* [Dr. Zeeman obtains an effect for the tin line 4585 which is much greater than the greatest obtained for either zinc or cadmium, but this must be either a misprint or a mistake. The line 4525 gives a well-marked sharp triplet, and lends itself to accurate measurement, but the line 4585 is diffuse on both sides and merely becomes more diffuse when subjected to the magnetic field. Hence, although considerably broadened, it does not lend itself to measurement, even when a nicol is interposed. This diffusive broadening occurs in many other cases where the lines have the character of air lines.—*Cf.* Zeeman, Phil. Mag., Feb. 1898.]

† 'Astrophysical Journal,' 1897.

‡ Phil. Mag. xliv. p. 503 (1897).

of the electromagnetic theory. Indeed all the main facts were sufficiently accounted for by Dr. Zeeman [*] as deductions from the theory of Lorenz. The foregoing measurements, however, when taken into consideration with the formula deduced theoretically throw important light on the quantities involved in the theoretical calculation. Thus if T be the periodic time corresponding to any spectral line, and if δT be the difference in period between the side lines of a triplet produced by a magnetic field of strength H, then the formula yielded by theory is, in electromagnetic units,

$$\frac{\delta T}{T^2} = \frac{eH}{2\pi m},$$

where e is the ionic charge and m the inertia. Expressed in terms of the wave-length this becomes

$$\frac{\delta\lambda}{\lambda^2} = \frac{e}{m} \cdot \frac{H}{2\pi v},$$

where v is the velocity of light.

Now the foregoing measurements show that $\delta\lambda$ does not vary as λ^2, nor apparently according to any simple law, so that at present the ratio of e to m has to be determined as a function of λ for each substance, and accordingly we must write

$$\frac{e}{m} = f(\lambda),$$

where the form of the function remains to be determined.

It is possible, however, that the lines of any one substance may be thrown into groups for each of which $\delta\lambda$ varies as λ^2, and each of these groups might be produced by the motion of a single ion. The number of such groups in a given spectrum would then determine the number of different kinds of ions in the atom or molecule.

Homologous relations may also exist between the groups in different spectra, but all this still remains for complete investigation.

APPENDIX.

The following extract [†] cannot fail to be of considerable

---

[*] Zeeman, Phil. Mag. *loc. cit.*

[†] M. Ch. Fievez (Astronome à l'Observatoire Royal de Bruxelles), *Bulletins de l'Académie Royale de Belgique*, 3rd série, tome ix. p. 381 (1885). See also in the same volume, p. 327, the report on this work by M. Stas.

interest as it gives in his own words an account of the experiments made in 1885 by M. Fievez in studying the influence of a strong magnetic field on the spectrum of a source of light placed in it. Dr. Zeeman has already noticed the work of M. Fievez, and he rather inclines to the opinion that the effects observed by the latter, and described in the extract below, were spurious and not the true magnetic effect. Considering the unstable character of the sodium lines, I am strongly of opinion that M. Fievez was dealing with the real magnetic widening, and that he obtained the effect on a tolerably large scale. The effects, indeed, as described by him, are quite true to nature when special precautions are not taken against reversals. The real point of importance is the broadening always observed. No doubt if he had known the theory the whole question would have been settled in 1885:—

"L'installation spectroscopique de l'Observatoire, disposant d'un appareil dispersif de très grande puissance et d'un électro-aimant Faraday, construction Ruhmkorff, pouvant être activé par un courant de 50 ampères d'intensité, a permis d'aborder ce problème.

"La flamme oxyhydrique d'un petit chalumeau était dirigée horizontalement sur un charbon *sodé* placé entre les armatures coniques de l'électro-aimant, distantes l'une de l'autre de 10 millimètres. Une image de la flamme était projetée sur la fente du spectroscope par un objectif double. La quantité d'oxygène introduite dans cette flamme permettait de régler la température de façon à donner aux raies spectrales D_1 et D_2 l'apparence voulue.

"Dans ces conditions, les raies sodiques D_1 et D_2 étant d'abord peu larges et non renversées avant le passage du courant d'aimantation, deviennent immédiatement *plus brillantes, plus longues et plus larges* aussitôt que l'électro-aimant est mis en activité.

"Si les raies brillantes D_1 et D_2 sont déjà élargies, l'électro-aimant étant inactif, elles deviennent plus larges encore et *se renversent* (c'est-à-dire qu' une raie noire paraît au milieu de la raie brillante élargie) pendant le passage du courant d'aimantation.

"Si les raies sont déjà élargies et renversées, l'élargissement de la raie brillante et de la raie noire devient beaucoup plus considérable.

"Ces phénomènes, *qui disparaissent instantanément lors de l'interruption du courant*, peuvent être observés, mais avec moins d'intensité, sur la raie rouge du potassium, du lithium, sur la raie verte du thallium, &c., lorsqu' une minime quantité de ces métaux ou d'un de leurs sels est placée sur le support de charbon.

"Enfin, les armatures coniques de l'électro-aimant étant remplacées par les armatures méplates, de manière que toute la longueur de la flamme sodique soit comprise entre ces armatures, les raies D_1 et D_2, préalablement renversées et élargies, présentent un *double renversement* (c'est-à-dire l'apparition d'une raie brillante au milieu de la raie noire élargie), lorsque l'électro-aimant est en activité."

XXXVII. *Determination of the Frequency of Alternating Currents.* *By* CARL KINSLEY, *M.E., A.M.*[*]

IN using alternating currents experimentally it is necessary to know their periodicity. The most simple and direct methods of measuring self-induction, mutual induction, and capacity require this determination. While working on another subject I found that this could be done with great accuracy by means of a vibrating air-column. If a telephone receiver is used on an alternating current circuit, it will give a musical note whose fundamental—first harmonic—will have the same number of vibrations that the alternating current has periods. When the telephone is placed in front of a tube of the right length this note will be reinforced. If we can compute the velocity of sound in the air in the tube and can determine the wave-length of the sound, the number of vibrations or periods per second follows immediately;

$$n=\frac{V_t}{\lambda}$$

where n=number of periods,
λ=the wave-length of the note,
V_t=the velocity of the sound.

Thus experimentally it is necessary to determine merely the length of the tube for the condition of maximum resonance.

The receiver should be a powerful one and placed at least ·41 diam. from the tube. The tube should be of uniform diameter and the piston fit easily ; then all the adjustments can be made by hand.

[*] Communicated by the Author.

Tubes 3 cm. and larger do not need any tubes leading to the ear. Use tubes with smooth inside surface, such as glass or drawn brass.

In most alternating current work it has been found by numerous observers * that the E.M.F. and current may be considered a simple function of the number of alternations :

$$i = I_1 \sin \omega t$$

where i = instantaneous value of current,
I_1 = maximum value of current,
$\omega = 2\pi n$,
t = time since the current was zero.

This is substantially true under normal working conditions, and may be assumed for alternating current computations.

The actual curve, however, may be strictly considered as the resultant of many pure sine-curves whose periods are multiples of the first harmonic:

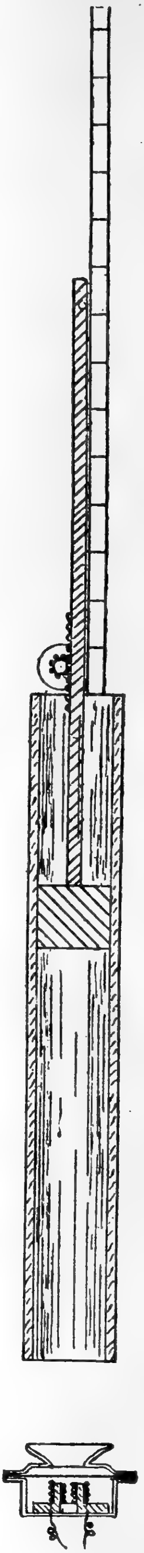
$$i = I_1 \sin \omega t + I_2 \sin 2\omega(t+a) + I_3 \sin 3\omega(t+b) + \&c.$$

where i, ω, and t are as already used,
I_1 is the maximum value of the first harmonic,
I_2 " " " " second "
I_3 " " " " third "
a and b are the differences between the values of t for the fundamental and the corresponding values for the overtones.

As is stated above, all terms but the first term are usually neglected.

The telephone diaphragm will therefore give a composite note containing not only the fundamental but all the higher harmonics that may be present †.

A quantitive determination of the harmonics present has not yet been made, and for the present



Resonance Tube.

---

* Reasons for considering the above equation correct: S. P. Thompson, 'Dynamo Electric Machinery,' 5th edition, p. 553; Rodes, Elect. Rev. vol. xxxix. p. 845; Bedell, 'The Principles of the Transformer,' p. 59. Curves determined from machines—among others: Bedell, Miller, and Wagner, Am. Hist. Elect. Eng. vol. x. p. 500; Fleming, J. A., Elect. Rev. vol. xxxix. pp. 91, 122, &c.

† Dr. Steinmetz, in 'Theory and Calculation of Alternating Current Phenomena,' finds that only the odd harmonics need be considered. Houston and Kennelly, Elect. World, vol. xxiii. p. 35, are of the same opinion.

purpose it is sufficient to state that in the many cases examined *all* of the harmonics are found. This holds up to a periodicity of 1200 per sec., which is in some cases the 36th harmonic. It is therefore plain that any or all of the overtones may be used in the determination of the period of the fundamental. The relation between the self-induction and capacity of the circuit sometimes gives particular prominence to one of the overtones, which may be either an odd or an even harmonic.

This note may then be used most advantageously during the progress of the experiments with the alternating currents. The period of the note can be easily gotten from the accompanying tables.

Use of the Tables.

Open tubes act as though they were $0{\cdot}41\,d$* longer than their real length.

Then the first length of the tube giving maximum resonance will be $l=\rho-0{\cdot}41\,d=\frac{1}{4}\lambda-0{\cdot}41\,d$,

where l = actual length of tube,
ρ = equivalent length,
d = diameter of the tube,
λ = wave-length of the sound.

Table II. is computed for the diameter of tube of 4 cm. and so the correction to be added to l will not be constant.

Add the correction gotten from Table I. to the observed value of l, and in the column for the temperature of the tube find the frequency corresponding to the length. Divide this periodicity by the number of the harmonic, and the periodicity of the fundamental will be the result.

The frequency of the fundamental will be the greatest common divisor of the frequencies of successive harmonics. Having gotten by trial the number of the harmonic most advantageous to use, that harmonic alone may hereafter be used.

As an illustration a typical case will be given:

Let t = temperature of the tube,
c = correction gotten from Table I.
n, n_2, n_3, &c. periodicity of fundamental, second harmonic, third, &c.

$$d=5 \text{ cm.} \qquad t=20^\circ \text{ C.}$$

(1) $l=21{\cdot}68,\quad c=2{\cdot}02,\quad \rho=23{\cdot}70,\quad n_?=360{\cdot}00,$
(2) $l=18{\cdot}30,\quad c=2{\cdot}02,\quad \rho=20{\cdot}32,\quad n_?=420{\cdot}2.$

By inspection No. (1) is found to be the sixth harmonic, and No. (2) the seventh. The fundamental which is the highest common divisor is then 60·0.

---

* Lord Rayleigh, 'Theory of Sound,' vol. ii. p. 295.

A slight difference might be caused by the change in period which is continually occurring under usual conditions. If now No. 1 should be read again it might give

(3) $l=21\cdot72, \quad c=2\cdot02, \quad \rho=23\cdot74, \quad n_6=359\cdot4.$

Then the fundamental would be $n=59\cdot9$.

Table I.

Correction to be added to observed length.

Observed length.	Diameter of tubes in cm.						
	1.	2.	3.	4.	5.	6.	7.
6	·46	·84	1·24	1·64	2·04	2·45	2 86
7	·48	·85	,,	,,	,,	,,	2·85
8	·50	·85	,,	,,	,,	,,	,,
9	·52	·86	,,	,,	,,	2·44	,,
10	·54	86	,,	,,	,,	,,	2·84
11	·56	·87	1·25	,,	,,	,,	,,
12	·58	·87	,,	,,	,,	,,	,,
13	·60	·88	,,	,,	2·03	2·43	2·83
14	·62	·89	,,	,,	,,	,,	,,
15	·64	·89	1·26	,,	,,	,,	,,
16	·66	·90	,,	,,	,,	,,	2·82
17	·68	·91	,,	,,	,,	2·42	,,
18	·71	·92	,,	,,	2·02	,,	,,
19	·73	·93	1·27	,,	,,	,,	2·81
20	·75	·94	,,	,,	,,	,,	,,
21	·78	·94	,,	,,	,,	2·41	,,
22	·81	·95	,,	,,	,,	,,	2·80
23	·84	·96	1·28	,,	2·01	,,	,,
24	·87	·97	,,	,,	,,	,,	,,
25	·90	·98	,,	,,	,,	2·40	2·79
26	·93	·99	1·29	,,	,,	,,	,,
27	·96	1·00	,,	,,	,,	,,	,,
28	·99	1·01	,,	,,	2·00	,,	2·78
29	1·02	1·02	1·30	,,	,,	2·39	,,
30	1·05	1·03	,,	,,	,,	,,	,,
31	1 08	1·04	,,	,,	,,	,,	2·77
32	1·11	1 05	1·31	,,	1·99	2·38	,,
33	1·15	1·06	,,	,,	,,	,,	2·76
34	1·18	1·07	1·32	,,	,,	,,	,,
35	1·21	1·08	,,	,,	,,	2·37	,,

Computation of the Tables.

We have seen that

$$V_t = \lambda n,$$

and so we must know the velocity of sound in air in the tube.

According to Kirchhoff's * equation

$$V_0 = a\left(1 - \frac{y}{d\sqrt{\pi n}}\right),$$

where V_0 = the velocity of sound in the tube at 0° temp. and 0 vapour-pressure.
a = the velocity of sound in free air at 0° temp. and 0 vapour-pressure.
y = a constant depending on the inside surface of the tube.

Use $a = 330{\cdot}9$, $y = {\cdot}00799$.

The equation has been experimentally established and the constants gotten by Dr. Low †.

The velocities for any atmospheric condition can be gotten from the equation

$$V_t = V_0\sqrt{\frac{1+\alpha t}{1-\frac{3}{8}\frac{p}{B}}},$$

where $\alpha = {\cdot}003665$,
p = vapour-pressure,
B = barometric pressure.

The length of the tube, plus correction for open end, will thus be

$$\rho = l + c = \frac{\lambda}{4} = \frac{V_t}{4n} = V_0\sqrt{\frac{1+\alpha t}{1-\frac{3}{8}\frac{p}{B}}} \div 4n$$

$$\rho = \frac{a\left(1 - \frac{y}{d\sqrt{\pi n}}\right)\sqrt{\frac{1+\alpha t}{1-\frac{3}{8}\frac{p}{B}}}}{4n}.$$

In this equation the constants known are

$a = 330{\cdot}9$ metres per sec.,
$y = {\cdot}00799$,
$\alpha = {\cdot}003665$;

---

* Pogg. *Ann.* vol. cxxxiv. p. 177.

† Dr. Low, Phil. Mag. Sept. 1894.

TABLE II.—Frequency of

Observed length of tube plus correction, *ρ*.	Temperature of the tube, *t*.								
	0°.	5°.	10°.	13°.	15°.	17°.	18°.	19°.	20°.
7	1178·1	1189·2	1200·0	1206·5	1211·0	1215·4	1217·5	1219·8	1222·0
8	1030·7	1040·4	1050·0	1055·5	1059·4	1063·2	1065·0	1067·0	1069·0
9	916·0	924·6	933·0	938·0	941·4	944·7	946·5	948·2	950·0
10	824·2	831·9	839·5	839·5	847·1	850·1	851·6	853·2	854·8
11	749·2	756·2	763·1	763·1	770·0	772·7	774·2	775·5	777·0
12	686·6	693·0	699·4	699·4	705·7	708·2	709·5	710·8	712·2
13	633·6	639·6	645·4	645·4	651·3	653·7	654·9	656·1	657·3
14	588·2	593·8	599·2	599·2	604·6	606·8	607·9	609·0	610·2
15	549·0	554·1	559·2	559·2	564·2	566·2	567·2	568·2	569·4
16	514·6	519·4	524·2	524·2	528·9	530·6	531·6	532·6	533·7
17	484·2	488·7	493·2	493·2	497·7	499·4	500·2	501·2	502·2
18	457·2	461·5	465·8	465·8	470·0	471·6	472·5	473·4	474·2
19	433·1	437·1	441·2	441·2	445·2	446·8	447·6	448·4	449·2
20	411·4	415·2	419·0	421·3	422·8	424·4	425·1	425·9	426·7
21	391·7	395·4	399·0	401·2	402·7	404·1	404·8	405·6	406·3
22	374·5	377·4	380·8	382·8	384·3	385·7	386·5	387·1	387·8
23	357·6	361·0	364·3	366·2	367·6	368·7	369·6	370·2	370·9
24	342·7	345·9	349·1	350·9	352·2	353·5	354·1	354·7	355·4
25	328·7	332·1	335·0	336·9	338·1	339·3	340·0	346·0	341·2
26	316·2	319·2	322·1	323·8	325·0	326·2	326·8	327·4	328·0
27	304·5	307·3	310·2	311·9	313·0	314·1	314·7	315·3	315·8
28	293·6	296·3	299·0	300·7	301·8	302·8	303·4	304·0	304·5
29	283·4	286·1	288·7	290·3	291·3	292·4	293·0	293·5	294·0
30	274·0	276·6	279·1	280·6	281·6	282·6	283·1	283·6	284·1
31	265·1	267·6	270·0	271·5	272·5	273·5	274·0	274·5	275·0
32	256·8	259·2	261·6	263·0	263·9	264·9	265·3	265·8	266·3
33	249·0	251·3	253·6	255·0	255·9	256·8	257·3	257·7	258·2
34	241·6	243·9	246·2	247·5	248·4	249·2	249·7	250·1	250·6
35	234·7	236·9	239·1	240·4	241·2	242·1	242·5	243·0	243·4

the Alternating Current.

Temperature of the tube, *t*.								
21°.	22°.	23°.	24°.	25°.	26°.	27°.	28°.	30°.
1224·2	1226·5	1228·7	1231·0	1233·0	1235·4	1237·6	1239·8	1244·1
1071·0	1073·0	1074·8	1076·7	1078·7	1080·7	1082·6	1084·6	1088·5
951·6	953·4	955·1	956·9	958·7	960·4	962·1	964·0	967·5
856·3	858·0	859·5	861·0	862·6	864·2	865·8	867·4	870·5
778·4	779·8	781·2	782·5	784·0	785·5	786·9	788·2	791·0
713·5	714·8	716·0	717·4	718·6	720·0	721·4	722·7	725·3
658·5	659·7	660·9	662·1	663·2	664·5	665·7	666·9	669·4
611·3	612·4	613·5	614·6	615·7	616·9	618·0	619·2	621·4
570·3	571·4	572·4	573·5	574·6	575·5	576·5	577·6	579·8
534·5	535·5	536·5	537·5	538·5	539·5	540·5	541·5	543·5
503·0	504·0	504·9	505·8	506·8	507·7	508·6	509·5	511·5
475·0	475·9	476·8	477·6	478·6	479·5	480·3	481·2	483·0
450·0	450·8	451·6	452·4	453·3	454·0	454·8	455·6	457·4
427·4	428·2	429·0	429·8	430·6	431·3	432·1	432·9	434·4
407·1	407·8	408·5	409·3	410·0	410·7	411·5	412·2	413·8
388·5	389·2	389·9	390·6	391·3	392·0	392·7	393·4	394·9
371·6	372·2	372·9	373·6	374·3	375·0	375·7	376·4	377·7
356·0	356·7	357·4	358·0	358·7	359·4	360·0	360·7	362·0
342·8	342·5	343·1	343·7	344·3	344·9	345·5	346·2	347·4
328·6	329·2	329·8	330·4	331·0	331·6	332·2	332·8	334·0
316·4	317·0	317·6	318·1	318·7	319·3	319·9	320·5	321·6
305·1	305·6	306·2	306·8	307·3	307·9	308·5	309·0	310·2
294·5	295·1	295·6	296·1	296·7	297·2	297·7	298·3	299·3
284·7	285·2	285·7	286·2	286·7	287·2	287·8	288·3	289·3
275·5	276·0	276·5	277·0	277·5	278·0	278·5	279·0	280·0
266·8	267·3	267·8	268·3	268·8	269·3	269·8	270·2	271·2
258·7	259·1	259·6	260·1	260·6	261·1	261·5	262·0	262·9
251·0	251·5	252·0	252·4	252·9	253·4	253·8	254·3	255·2
243·8	244·3	244·8	245·2	245·7	246·1	246·6	247·0	248·6

and the constants assumed are

$p=\frac{1}{3}$ saturation for the different temperatures,
B=760 mm. of mercury,
$d=4$ cm. in Table II.

Since d is assumed to be 4 cm. in Table II. the quantity to be added to l will be $0{\cdot}41\,d=1{\cdot}64$ for every note when a tube of that diameter is used.

For other tubes the correction will vary for different l's due to the changing V_0 for the different tubes. Table I. gives this correction for all tubes apt to be used.

The error in reading l is very small and need be only a fraction of a millimetre in any case.

The greatest possible error due to the assumption of $\frac{1}{3}$ saturation will be, at 20° C., only $+\frac{1}{4}$ per cent. when p is 100 per cent. of saturation and $-\frac{1}{4}$ per cent. for zero vapour in the air.

Consequently for engineering measurements the error will be less than that due to other instruments, and so may be neglected. The adjustment for maximum resonance can be made with greater rapidity than that with which a Siemens dynamometer or a Thomson balance can be used. This makes it possible to use an ordinary alternating current circuit for accurate experimental work.

The tube that will be found most durable, uniform, and convenient will be a drawn brass tube 4 cm. in diameter.

The writer has very carefully compared glass and drawn brass tubes, and finds that y is the same for each. He has also measured the velocities of sound using a chronograph to determine n, and finds that the value for a already given can be used without sensible error.

The uniform internal diameter of the brass tube makes it much more convenient for use, as a dry piston can be employed.

The natural period of the diaphragm in every telephone on the market is too high to affect in any sensible way the harmonics due to the alternating current.

A number of other methods have been proposed * that depend on the resonance of iron wire or strips set into vibration by the alternating current.

Besides the impossibility of making, with either, really accurate adjustments for maximum resonance—which is independent of the accuracy of the gearing—it is necessary to standardize the instrument, and that will be found impossible to do with accuracy except in a very well-equipped laboratory.

---

* Campbell, Phil. Mag., Aug. 1896.

Method for Determining the Velocity of Sound.

The velocity of sound in air has been determined by many observers both in the free air and in air confined in tubes. The early experimenters *, of whom Moll, Von Beek, and Kuytenbrouwer in 1823 did exceptionally careful work, used composite sounds in free air. It was suggested by Le Roux †, that the temperature measurements were incorrect. This view is supported by the observations of Nansen, who found that the temperatures gotten on the surface were at variance with that taken in his "crow's nest." Therefore, the temperature measurements of Parry and Foster ‡ certainly did not give the true temperature of the path of the sound. In 1868 M. V. Regnault § reported measurements taken both in free air and in tubes which cast so much doubt on the earlier measurements that the whole subject was reopened.

Since then, H. Scheebli || and Dr. Low ¶, using pure notes, have found, from the positions of maxima in resonating tubes, the wave-length of sound. Dr. Low's relative results are particularly good ; but his method of standardizing his tuning-forks and of determining the vapour-pressure of the air in the tubes is unfortunately not above criticism. His values for *a* and *y* must, however, be very nearly correct and so they were used in the preceding tables.

The writer would suggest that the use of a telephone as the sounding-body with the periodicity determined directly from the motor generator by means of a chronograph would be a most satisfactory method of producing the notes desired. The telephone may be enclosed if light gases are to be used. Drawn tubing with a dry piston would obviate all difficulty of determining vapour-pressure.

Washington University,
St. Louis, Mo., U.S.A.

---

* See paper by Le Conte, Phil. Mag., Jan. 1864.

† *Comptes Rendus*, p. 392, 1867.

‡ Phil. Trans. p. 97, 1828.

§ *Comptes Rendus*, p. 209, 1868.

|| Pogg. *Ann.* p. 303, 1869.

¶ Phil. Mag. Sept. 1894.

XXXVIII. *Radiation in a Magnetic Field.*

By A. A. Michelson *.

FURTHER analysis of the radiations emitted in a magnetic field shows that the phenomenon is much more complex than was supposed. An examination of the separate components of the "triplet" brings out the fact that in general these are multiple lines.

The laws may be summarized as follows:—

A.

1. All spectral lines are tripled when the radiations emanate in a magnetic field.
2. The separation is proportional to the strength of field and is approximately the same for all colours and for all substances.
3. Viewed in a plane perpendicular to the magnetic field, the outer lines are polarized parallel to the field, and the central line is polarized at right angles with the field.
4. Viewed in a direction parallel with the magnetic field, the central line vanishes, while the outer ones are circularly polarized; the shorter waves in the direction of the magnetizing current, the longer waves in the opposite sense.

To these laws (which were verified by the examination of a dozen or more lines) the following must now be added:—

B.

1. The "middle line" is a symmetrical triple, the distance between the components being one-fourth that of the "outer lines," and hence also proportional to the strength of field.
2. The relative intensity of the components varies for different substances and for different lines of the same substance; and accordingly the group may appear as a single line or a double or a triple.
3. The "outer lines" are unsymmetrical, but are symmetrically placed with respect to the "middle line." The distance between the components is usually one-fourth that between the "outer lines," but is in some cases one-sixth.
4. The intensity of the components varies for different spectral lines, and these variations do not always correspond to those of the "central line." The outer groups may accordingly appear as single or double or triple or multiple lines.

Fig. 1 represents a plan of the arrangement of apparatus

---

* Communicated by the Author.

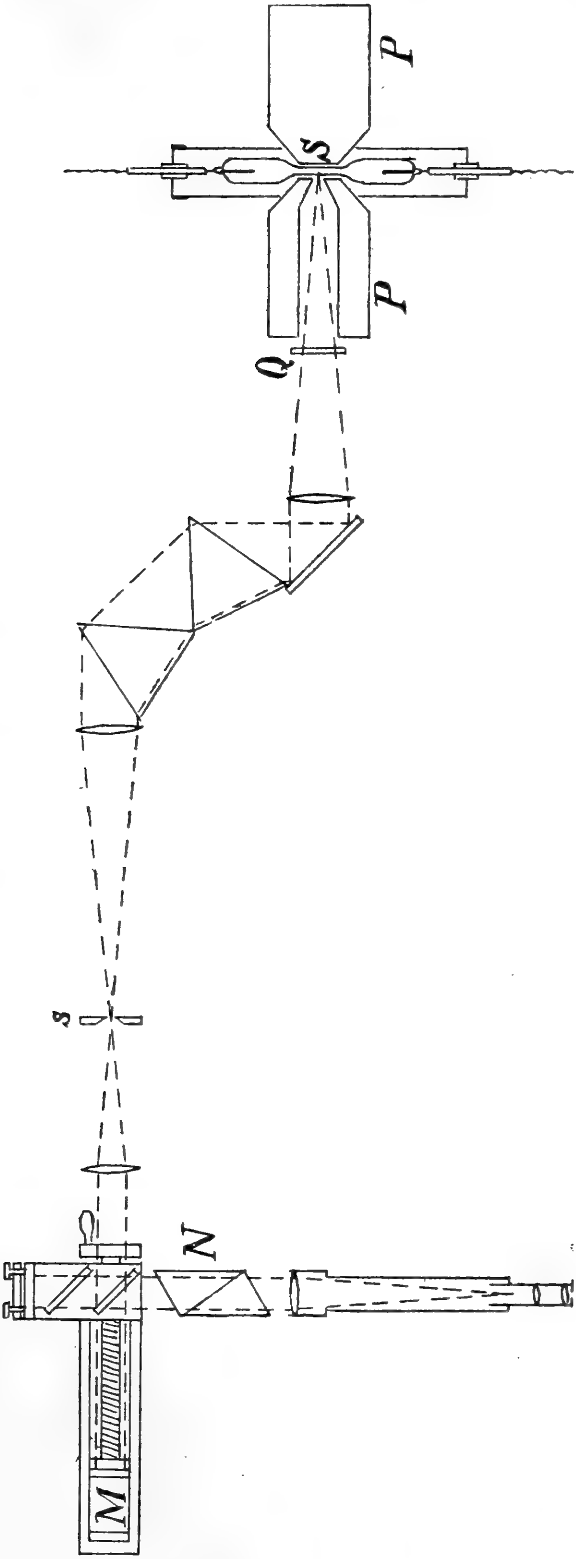


Fig. 1.

employed in the investigation. S is the source of light, either a small hand-blowpipe with a bead of the substance to be examined in the flame, or a vacuum-tube, which is usually placed in a metal box (for heating) of such form as to permit a close approach of the pole-pieces P of an electromagnet. One of these is bored out to permit examination of the axial ray. The light from S undergoes a preliminary analysis by the spectroscopic train (two bisulphide prisms), the radiation to be investigated being isolated by the slit *s*. It then enters the interferometer, one of the mirrors of which, M, is moveable on ways so accurately ground that no readjustment is necessary in any part of its path; that is, the mirror remains so nearly parallel with itself that the interference-fringes (concentric circles) are always as clear as possible. The emergent beam then passes through the analyser N to the observing telescope.

The clearness or "visibility" of the interference-fringes is estimated at positions of the mirror M corresponding to increments of the difference of path of 1, 2, or 5 mm. according to the nature of the curve. This, it must be admitted, leaves much to be desired in the way of precision, and in some cases there may be corrections of as much as 20 per cent., to reduce the observations to the value they should have, namely,

$$V=\frac{I_1-I_2}{I_1+I_2},$$

where I_1 is the maximum intensity and I_2 the minimum for adjacent fringes. Doubtless much more accurate readings could be obtained by the use of a double quartz lens * for comparison; but the process is so much more tedious and troublesome that the form of the curve is liable to alter on account of changes in the source during the observations. The case is somewhat analogous to making eye-estimates of stellar magnitudes, which are but little inferior to photometric determinations and much less troublesome. In any case it is always easy to distinguish ascending and descending slopes, and maxima and minima can be located with very great accuracy, and this is usually quite sufficient to permit a fairly accurate deduction of the distribution of light in the spectrum. It has been shown † that with the definition of visibility just given, if $y=\phi(x)$ is the intensity-curve of the spectrum,

$$PV=\sqrt{C^2+S^2},$$

in which

$$P=\int\phi(x)\,dx,\quad C=\int\phi(x)\cos kx\,dx,$$

* Phil. Mag., Sept. 1892. † Ibid.

and

$$S = \int \phi(x) \sin kx \, dx,$$

the integration extending over the whole spectrum.

But by Fourier's formula

$$\phi(x) = \int_0^\infty C \cos kx \, dk + \int_0^\infty S \sin kx \, dk;$$

so that if C and S are both known, $\phi(x)$ can be determined. In general this is not the case unless another relation between C and S is given. Such a relation is furnished by the "phase curve," which gives the displacement of the fringes from the position they would have occupied had the source been homogeneous. If δ is this displacement and $\theta = 2\pi\delta/\lambda$, then

$$C = V \cos \theta \quad \text{and} \quad S = V \sin \theta.$$

In general the θ curve is troublesome to obtain, on account of the difficulty in securing a sufficiently homogeneous comparison source; but in the present instance this is furnished by the non-magnetized radiations *.

Usually, however, the assumption was made that the spectrum was symmetrical, and in only a few cases was the solution verified by the complete analysis. In this simpler form we have $\theta = 0$, $S = 0$, and $C = V$, whence

$$\phi(x) = \int_0^\infty V \cos kx \, dk.$$

This integral may frequently be calculated when V can be expressed in simple analytical form as a function of k. In general this is not the case, and it was for the solution of such problems that the harmonic analyser † was devised. The curve $V = f(k)$ is "fed" to the machine, which then draws the curve $y = \phi(x)$, the whole operation taking but a few minutes.

It was found on completing the analysis of some fifty or more visibility-curves, that the resulting spectra could be classified under three types; there were some interesting variations which would merit a separate investigation, but most of the cases could be identified at a glance.

The three types of visibility-curve are given in figs. 2, 3,

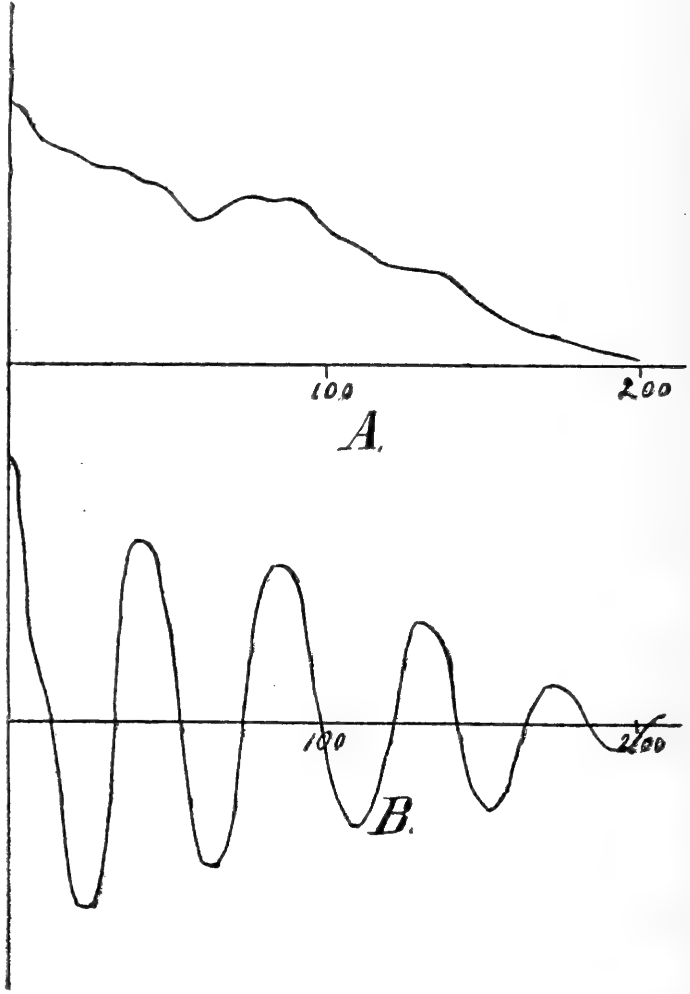
---

* These are not always sufficiently simple as in the case of the green thallium line.

† Phil. Mag., Jan. 1898.

and 4. Those marked A referring to observations made with the line of sight at right angles with the magnetic field and with the plane of polarization perpendicular to the lines of force; while B correspond to observations with the line of sight still normal to the field, but plane of polarization parallel with the lines of force.

Fig. 2.—Type I.



It was found that there was no appreciable difference between these last and the observations taken when the line of sight was parallel with the field; but in this case it was possible to analyse either one of the outer groups separately by the use of the quarter-wave plate, Q, fig. 1. This was done in a few cases, but no new result was obtained.

The abscissæ of the visibility-curves are differences of path in millimetres, reduced to a field-strength of 10,000 as determined by a bismuth spiral.

Fig. 3.—Type II.

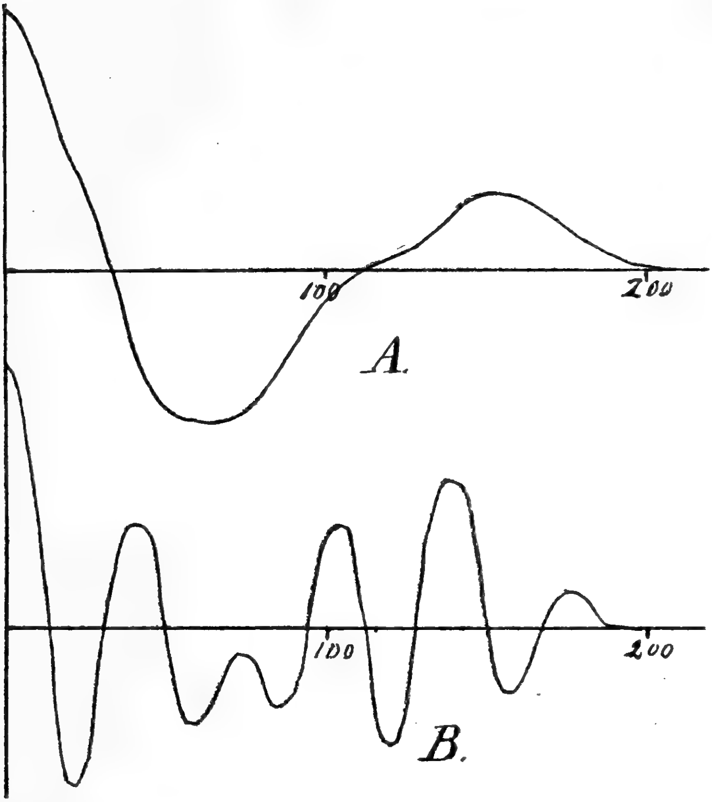


Fig. 4.—Type III.

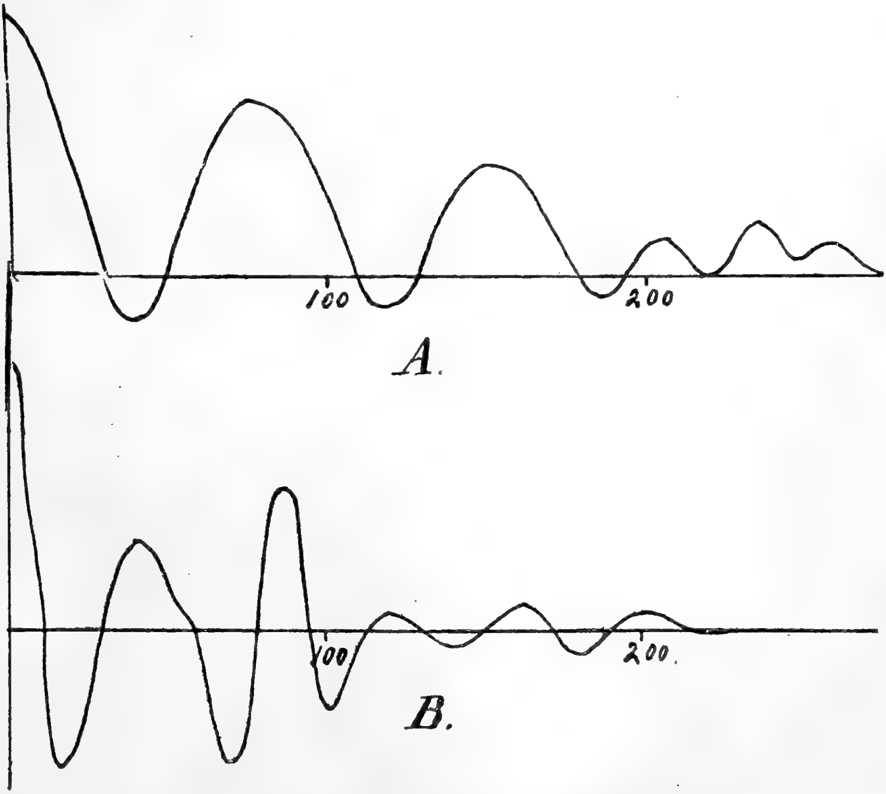


Fig. 5.

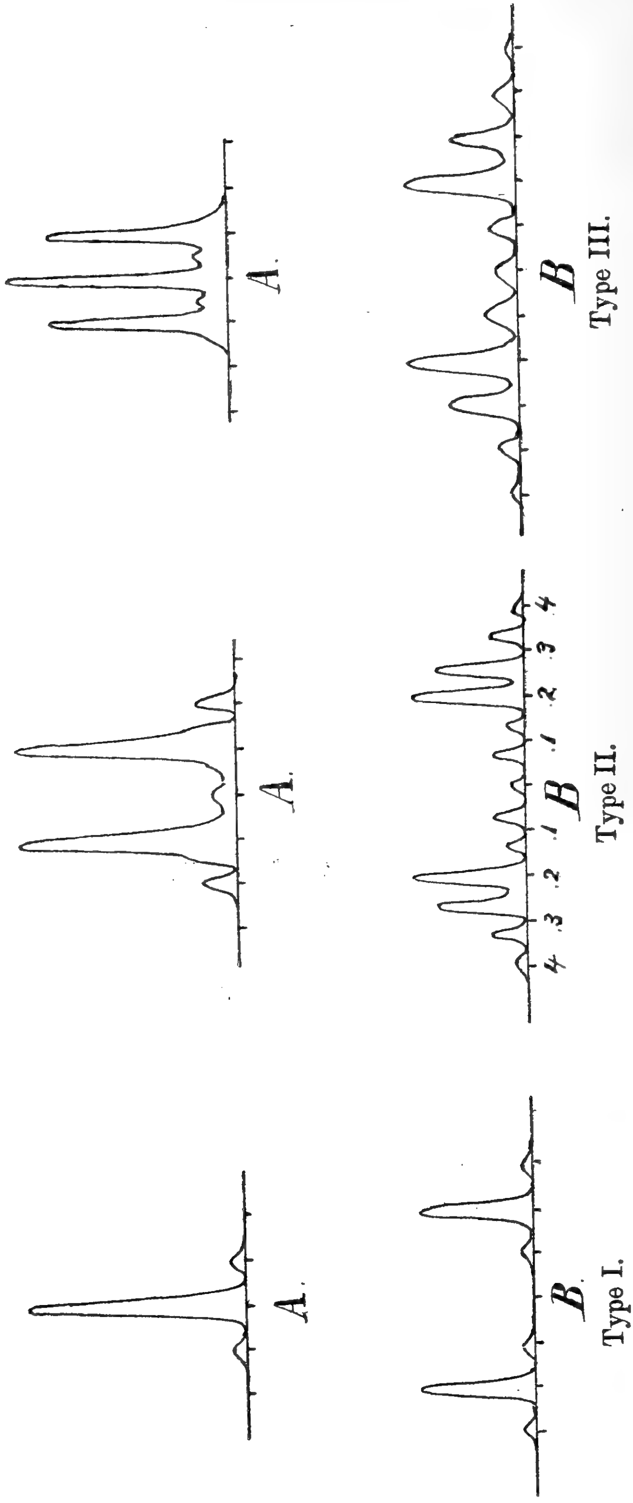


Fig. 5 gives the intensity-curves of the corresponding spectra, the abscissæ being expressed in tenth-metres †.

Following is a list of the radiations examined and their classification :—

Mercury ……	Yellow lines.	Type I.
	Green line.	Type III.
	Violet line.	Type II.
Cadmium……	Red line.	Type I.
	Green line.	Type III.
	Blue line.	Type II.
Zinc ……….	Red line.	Type I.
	Green line.	Type III.
	Blue line.	Type II.
Sodium……..	Yellow lines.	Type II.
Thallium ……	Green line.	Type II. (doubtful).
Lithium ……	Red line.	(Too broad to determine.)
Hydrogen……	Red and blue lines.	(Too broad to determine.)
Helium……..	Yellow and green lines.	(Too broad to determine.)

The following table shows that the law A 2 is only approximately true.

In fact, owing to the complexity of the spectra, there is considerable latitude in the choice of the distance between the outer groups. If this correspond to the brightest components, the law can hardly be said to hold at all ; but if the distance be taken between the centres of gravity of the light areas, a fair agreement is found. The table gives separation in tenth-metres for a field 10,000. The lines marked with an asterisk are less accurate than the others, on account of broadening.

*Hydrogen …………	Red. ……..	0·48
*Lithium …………..	Red. ……..	0·60
Cadmium …………	Red. ……..	0·42
Zinc ……………….	Red. ……..	0·42
Mercury …………..	Yellow. ……..	0·36
*Sodium …………..	Yellow. ……..	0·50
*Helium …………..	Green. ……..	0·37
Mercury …………..	Green. ……..	0·40
Cadmium …………	Green. ……..	0·41
Zinc ……………….	Green. ……..	0·40
*Thallium …………..	Green. ……..	0·36
Cadmium …………	Blue. ……..	0·40
Zinc ……………….	Blue. ……..	0·33
Mercury …………..	Violet. ……..	0·33

Taking into account the uncertainty alluded to, the results show on the whole a fair agreement, from which it may be concluded that the separation is independent of the radiating substance and of the colour.

It is possible that some of the resemblances in the preceding

---

† For this the abscissæ of the curve drawn by the analyser are multiplied by the square of the wave-length.

tables are due to the fact that the substances in question are chemically related; and perhaps it is scarcely justifiable to generalize from such a limited number; and it may well be that a wide range of elements would show other peculiarities.

I desire to express my hearty appreciation of the efficient service rendered in this work by Mr. C. R. Mann, and especially to recognize the patience and skill shown in the tedious and delicate process of preparation of the vacuum-tubes, to which in great measure the success of the investigation is due.

XXXIX. *On Discontinuities connected with the Propagation of Wave-motion along a Periodically Loaded String.* *By* CHARLES GODFREY, *B.A., Scholar of Trinity College, Isaac Newton Student in the University of Cambridge**.

1. THE system described below shows rather remarkable discontinuous properties. The work was suggested by a passage in Sir George Stokes' Read lecture, and formed part of an essay written in December 1896.

A heavy string of density ρ under tension T extends from $-\infty$ to $+\infty$. From $-\infty$ to 0 it is free from loads; from 0 to $+\infty$ it is loaded at equal intervals l with equal particles of mass M. To avoid ambiguity we will suppose that the motion of each mass is retarded by a small viscous force; this will finally be neglected. We will investigate the steady vibration of the system when simple transverse waves are travelling along the string from $-\infty$. These impinge on the system of masses; a reflected wave is generated which travels back along the string; furthermore, the masses are agitated in a certain manner.

2. We will denote by ξ the lateral displacement of a point on the string between $x=-\infty$ and $x=0$. The velocity of propagation along the string is $\sqrt{\frac{\mathrm{T}}{\rho}}=v$. For a motion whose frequency is given by e^{int} we have

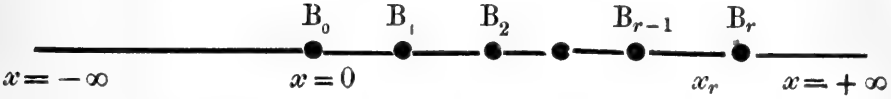
$$\xi=\mathrm{A}e^{in\left(t-\frac{x}{}\right)}+\mathrm{B}e^{in\left(t+\frac{x}{v}\right)} \quad . \quad . \quad . \qquad . \quad . \text{(i.)}$$

Let the displacement of the mass B_r at time t be denoted by y_r. For a point in the rth string $\mathrm{B}_{r-1}\mathrm{B}_r$ let the displacement be ξ_r; and let the distance of such a point from B_{r-1} be x_r.

---

* Communicated by R. T. Glazebrook, F.R.S.

Fig. 1.



For the string $B_{r-1}B_r$

$$\xi_r = \left(P_r\cos\frac{nx_r}{v} + Q_r\sin\frac{nx_r}{v}\right)e^{int} \quad . \quad . \quad . \quad \text{(ii.)}$$

Now at $x_r=0$, $\xi_r=y_{r-1}$, and at $x_r=l$, $\xi_r=y_r$;

$$\left.\begin{aligned} \therefore\ P_re^{int} &= y_{r-1} \\ Q_re^{int} &= \frac{y_r - y_{r-1}\cos\frac{nl}{v}}{\sin\frac{nl}{v}} \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad \text{(iii.)}$$

The equation of motion of B_r, for $r>0$, is

$$M\ddot{y}_r + 2k\dot{y}_r = T\left\{\left(\frac{\partial\xi_{r+1}}{\partial x_{r+1}}\right)_{x_{r+}\ =0} - \left(\frac{\partial\xi_r}{\partial x_r}\right)_{x_r=l}\right\} \quad . \quad . \quad \text{(iv.)}$$

Substituting from (ii.) and (iii.) and remembering that $y_r \propto e^{int}$,

$$y_r(-Mn^2+2kin) = \frac{Tn}{v}\left(Q_{r+1} + P_r\sin\frac{nl}{v} - Q_r\cos\frac{nl}{v}\right)e^{int}$$

$$= \frac{Tn}{v\sin\frac{nl}{v}}\left(y_{r+1} - 2y_r\cos\frac{nl}{v} + y_{r-1}\right),$$

or

$$y_{r+1} + y_{r-1} - 2y_r\left(\cos\frac{nl}{v} + \frac{v\sin\frac{nl}{v}}{2Tn}\,\overline{2kni - Mn^2}\right) = 0. \quad . \quad \text{(v.)}$$

The equation of motion of B_0 is

$$M\ddot{y}_0 + 2k\dot{y}_0 = T\left\{\left(\frac{\partial\xi_1}{\partial x_1}\right)_{x_1=0} - \left(\frac{\partial\xi}{\partial x}\right)_{x=0}\right\} \quad . \quad . \quad . \quad \text{(vi.)}$$

This gives

$$y_0(-Mn^2+2kni) = \left(\frac{Tn}{v}Q_1 + \frac{Tin}{v}(A-B)\right)e^{int}$$

$$= \frac{Tn}{v\sin\frac{nl}{v}}\left(y_1 - y_0\cos\frac{nl}{v}\right) + \frac{Tin}{v}(A-B)e^{int}. \quad . \quad \text{(vii.)}$$

We will now abbreviate by writing

$$\frac{nl}{v} \equiv \psi,$$

$$\frac{Mv^2}{2lT} \equiv \mu;$$

$$\cos\frac{nl}{v} + \frac{v\sin\frac{nl}{v}}{2Tn}(2kni - Mn^2) \equiv \cos(\alpha + i\beta) \equiv \cos\theta, \quad . \qquad \text{(viii.)}$$

where α and β are real.

$$\left.\begin{aligned} \therefore \quad \cos\alpha\cosh\beta &= \cos\frac{nl}{v} - \frac{Mnv}{2T}\sin\frac{nl}{v} = \cos\psi - \mu\psi\sin\psi, \\ \sin\alpha\sinh\beta &= -\frac{vk\sin\frac{nl}{v}}{T} = -\frac{vk}{T}\sin\psi. \end{aligned}\right\} \text{(ix.)}$$

With these substitutions, (v.) will give

$$y_{r+1} - 2y_r\cos\theta + y_{r-1} = 0 \quad . \quad . \quad . \quad . \quad . \qquad \text{(x.)}$$

The general solution of this set of equations is

$$\begin{aligned} y_r &= (Ce^{ir\theta} + De^{-ir\theta})\, e^{int}, \\ &\equiv (Ce^{ir\alpha - r\beta} + De^{-ir\alpha + r\beta})e^{int}. \end{aligned}$$

Now (ix.) will not determine the sign of β; we will always take the positive value. It is then obvious that $D=0$; otherwise the motion would be great for great values of r.

$$\therefore \quad y_r = Ce^{i(r\theta + nt)}. \quad . \quad . \quad . \quad . \quad . \quad . \qquad \text{(xi.)}$$

Again, (vii.) becomes

$$\begin{aligned} B - A &= \frac{e^{-int}}{i\sin\psi}(y_1 + y_0\cos\psi - 2y_0\cos\theta) \\ &= \frac{iC}{\sin\psi}(e^{-i\theta} - \cos\psi). \quad . \quad . \quad . \quad . \quad . \quad . \qquad \text{(xii.)} \end{aligned}$$

Furthermore, at $x=0$,

$$B + A = C. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad \text{(xiii.)}$$

From (xii.) and (xiii.),

$$\left.\begin{aligned} 2A &= \frac{iC}{\sin\psi}(-e^{-i\theta} + e^{-i\psi}), \\ 2B &= \frac{iC}{\sin\psi}(+e^{-i\theta} - e^{i\psi}). \end{aligned}\right\} \quad . \quad . \qquad \text{(xiv.)}$$

These are equivalent to

$$\left.\begin{aligned}\frac{\mathrm{A}}{\mathrm{C}} &= \frac{e^{i\tau}}{2\sin\psi}\left(e^{2\beta}-2e^{\beta}\cos\overline{\alpha-\psi}+1\right)\\ \frac{\mathrm{B}}{\mathrm{C}} &= \frac{e^{i\tau'}}{2\sin\psi}\left(e^{2\beta}-2e^{\beta}\cos\overline{\alpha+\psi}+1\right)^{\frac{1}{2}},\end{aligned}\right\} \quad . \quad \text{(xv.)}$$

where

$$\tan\tau=\frac{e^{\beta}\cos\alpha-\cos\psi}{e^{\beta}\sin\alpha-\sin\psi}, \qquad \tan\tau'=\frac{e^{\beta}\cos\alpha-\cos\psi}{e^{\beta}\sin\alpha+\sin\psi}. \quad \text{(xvi.)}$$

3. We are now provided with a complete solution of the motion ; this we will proceed to interpret.

The quantities α and β are determined by equations (ix.) :

$$\left.\begin{aligned}\cos\alpha\cosh\beta&=\cos\psi-\mu\psi\sin\psi\equiv z \text{ say,}\\ \sin\alpha\sinh\beta&=-\frac{vk}{\mathrm{T}}\sin\psi.\end{aligned}\right\} \quad . \quad \text{(ix.)}$$

We shall clearly perceive the drift of the matter if we neglect the friction and put $k=0$. Then either $\sin\alpha$ or $\sinh\beta$ vanishes ; the former or the latter being the case according as $z^2 \gtrless 1$.

If $z^2<1$, $\beta=0$, and a wave-like motion will be propagated through the masses, for

$$y_r = \mathrm{C}e^{i(r\alpha+nt)}.$$

If $z^2>1$, β is finite, while α is a multiple of π. The equation

$$y_r = \mathrm{C}e^{i(r\alpha+nt)-r\beta}$$

will represent an exponential falling off of motion, consecutive masses being either in the same or in opposite phases. In this case the deeper masses will be practically unaffected by the incident wave.

In order to understand how these phenomena depend upon the frequency of the incident wave, we must trace the changes of z for different values of ψ, or $\frac{nl}{v}$.

4. *Graph of* $z=\cos\psi-\mu\psi\sin\psi$.

This is readily constructed on finding the roots of

$$z=0, \quad z=1, \quad z=-1.$$

For $z=0$ we have $\cot\psi=\mu\psi$. By the usual graphic method, we find that the roots of this equation lie between

$$0 \text{ and } \frac{\pi}{2}, \quad \pi \text{ and } \frac{3\pi}{2}, \quad 2\pi \text{ and } \frac{5\pi}{2}, \text{ \&c.,}$$

approaching closer to the lower limit for the greater values.

For $z=1$ we have

$$\sin\frac{\psi}{2}\left(\sin\frac{\psi}{2}+\mu\psi\cos\frac{\psi}{2}\right)=0.$$

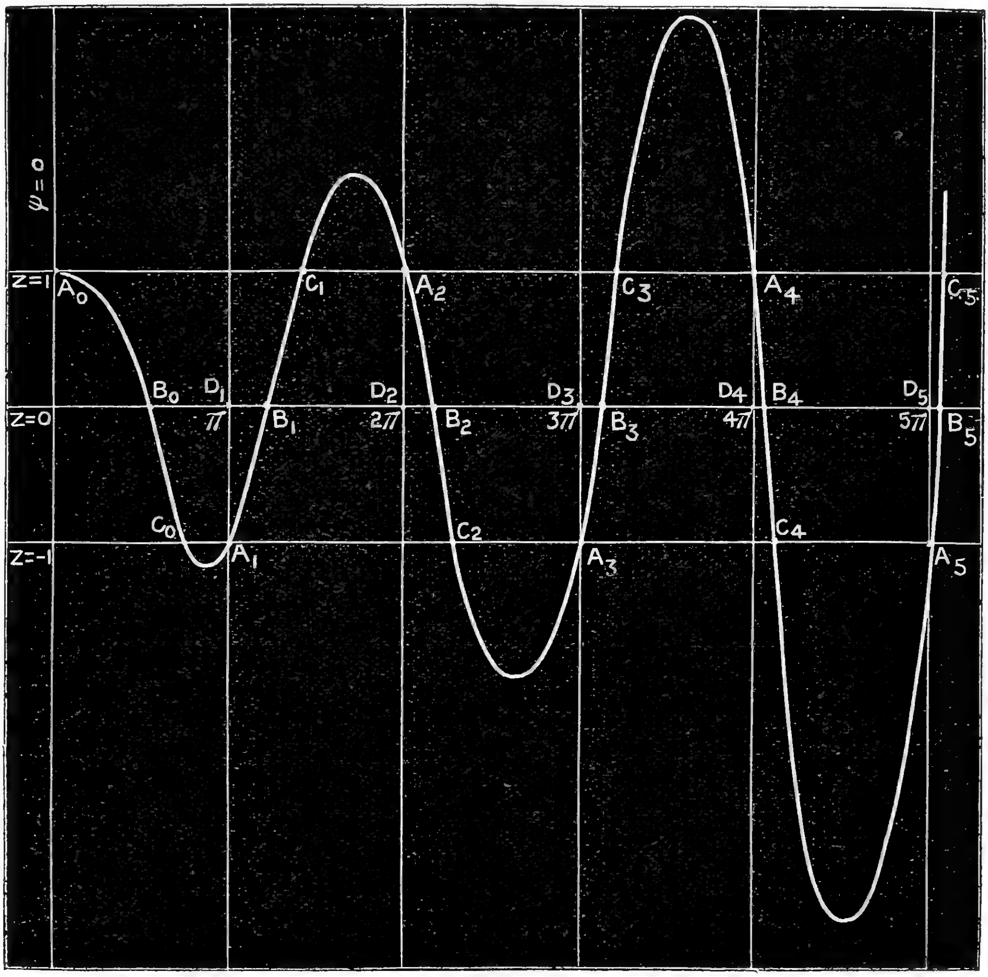
The factor $\sin\frac{\psi}{2}$ gives $\psi=2s\pi$, where s is integral. The other factor gives roots lying between π and 2π, 3π and 4π, &c. It will be seen that they lie beyond the zeros of the corresponding regions.

For $z=-1$ we have

$$\cos\frac{\psi}{2}\left(\cos\frac{\psi}{2}-\mu\psi\sin\frac{\psi}{2}\right)=0.$$

The factor $\cos\frac{\psi}{2}$ gives $\psi=(2s+1)\pi$. The other factor gives roots between 0 and π, 2π and 3π, &c.; and again lying beyond the zeros of the corresponding regions.

Fig. 2.



It is also clear that the maxima and minima become more marked as ψ increases. With these data it is easy to see that the general shape of the curve is as above.

From what has been already said, it is obvious that the wave will penetrate the masses if ψ lies within limits corresponding to the portions A_0C_0, A_1C_1, A_2C_2, &c. of the curve. If, on the other hand, ψ belongs to the regions C_0A_1, C_1A_2, C_2A_3, &c., the motion will only enter to a small distance.

5. It is interesting to look at the magnitude of the different amplitudes rather more closely. We shall lose no generality if we suppose that α lies between 0 and -2π. On considering the signs of $\sin\alpha$ and $\cos\alpha$ as given by (ix.), we have the following table:—

Region.	ψ between	$\sin\alpha$.	z and $\cos\alpha$.	α between
A_0B_0	0 and π	$-$	$+$	0 and $-\frac{\pi}{2}$.
B_0A_1	0 and π	$-$	$-$	$-\frac{\pi}{2}$ and $-\pi$.
A_1B_1	π and 2π	$+$	$-$	$-\pi$ and $-\frac{3\pi}{2}$.
B_1A_2	π and 2π	$+$	$+$	$-\frac{3\pi}{2}$ and -2π.

Similar limits recur for the other reaches, A_2A_4, A_4A_6, . . .

6. *Regions for which $z^2>1$.*

From the above table it appears that α is equal to 0 or $-\pi$ according as ψ lies between $2s\pi$ and $(2s+1)\pi$, or between $(2s-1)\pi$ and $2s\pi$. Now, denoting moduli of complex quantities by straight brackets,

$$\left|\frac{A^2}{C^2}\right| = \left|\frac{B^2}{C^2}\right| = \frac{e^{2\beta}\mp 2e^{\beta}\cos\psi+1}{4\sin^2\psi},$$

the upper or lower sign being taken according as α is 0 or $-\pi$.

This expression is equal to

$$\frac{e^{\beta}}{2}\,\frac{\cosh\beta\mp\cos\psi}{\sin^2\psi}.$$

But $\mp\cosh\beta=\cos\psi-\mu\psi\sin\psi$;

$$\therefore\ \left|\frac{A^2}{C^2}\right| = \left|\frac{B^2}{C^2}\right| = \mp\frac{e^{\beta}}{2}\,\frac{\mu\psi}{\sin\psi}. \quad . \quad . \quad \text{(xvii.)}$$

For the frequencies which are not propagated we thus have $|A|=|B|$, *i. e.* total reflexion, and it is clear from (xvi.) that there is reversal of phase. For the upper limit of such a region of frequency, $\psi=s\pi$ and $C=0$ (xvii.).

7. *Region for which* $z^2<1$.

Here we shall have $\beta=0$.

$$\left|\frac{A^2}{C^2}\right|=\frac{\sin^2\frac{\alpha-\psi}{2}}{\sin^2\psi}, \qquad \left|\frac{B^2}{C^2}\right|=\frac{\sin^2\frac{\alpha+\psi}{2}}{\sin^2\psi}.$$

But
$$\cos\alpha=\cos\psi-\mu\psi\sin\psi,$$

$$\therefore 2\sin\frac{\alpha-\psi}{2}\sin\frac{\alpha+\psi}{2}=\mu\psi\sin\psi,$$

$$\therefore \left|\frac{A^2}{C^2}\right|=\frac{\mu^2\psi^2}{4\sin^2\frac{\alpha+\psi}{2}}, \qquad \left|\frac{B^2}{C^2}\right|=\frac{\mu^2\psi^2}{4\sin^2\frac{\alpha-\psi}{2}}. \quad . \quad (\text{xviii.})$$

Here equation (xvi.) shows that the incident, transmitted, and reflected waves will be in different phases.

For the lower limit of such a region both ψ and α are multiples of π; further, they will be even or odd together. It therefore appears that $\sin\frac{\alpha+\psi}{2}$ and $\sin\frac{\alpha-\psi}{2}$ are zero, and $C=0$. But for these points $n=\frac{s\pi v}{2}$. If then the incident wave is of frequency corresponding to any one of the natural nodes of the intervals of string, the masses will be entirely undisturbed.

The case of ψ small is seen to be exceptional: we easily find that

$$\left|\frac{A^2}{C^2}\right|=\frac{\mu^2}{\{1-\sqrt{1+\mu}\}^2}, \qquad \left|\frac{B^2}{C^2}\right|=\frac{\mu^2}{\{1+\sqrt{1+\mu}\}^2}.$$

8. It may perhaps be allowed that the phenomena here discussed have some mathematical analogy with an ideal case in optics. We may think of the incidence of light from the free æther upon a solid of periodic structure. Without pressing the analogy, we will recapitulate our results in optical phraseology; this presentation will have the advantage of brevity.

Light being incident upon a periodic distribution of molecules, the light is analysed by a spectroscope after transmission through a considerable thickness. We shall find narrow bright bands, their lower edges ranged harmonically; each band will be faint on the lower side and terminate abruptly on the upper. Their width diminishes as we ascend the scale of frequency. Their lower edges correspond to the proper periods of the intermolecular spaces. If we view the light reflected we shall see total reflexion corresponding to the frequencies of the dark bands of the transmission spectrum; for these wave-lengths there will be reversal of phase.

XL. *A Numerical Evaluation of the Absolute Scale of Temperature.* *By* R. A. LEHFELDT[*].

Introduction.

NUMEROUS attempts have been made to reduce the readings of thermometers to the absolute scale, since that scale was first clearly defined by Thomson and Joule; but though the process of calculation has been varied a good deal, the most essential experimental basis of all the reductions is the same, viz. Thomson and Joule's own experiments on the outflow of gases through a porous plug. It is very remarkable, therefore, that no one, so far as I know, has attempted to repeat or extend those experiments, except in one case studied by E. Natanson, and that notwithstanding the great discrepancies in Thomson and Joule's measurements. For hydrogen, the substance which is probably the best for thermometric purposes, there were twelve experiments carried out at about 7° and five at about 90°; but these results varied from +0·9 to −0·1! while the hydrogen was in no case even approximately pure; and although the determination of absolute temperatures depends upon that experiment, it has been left in so unsatisfactory a state for half a century. Of the other data required, some, such as the specific heat and specific volume of the gases used, are known with sufficient accuracy, since they enter only in a small correction term: the most important, however, is the coefficient of pressure in the case of a gas thermometer at constant volume (or the coefficient of expansion in the constant-pressure thermometer). On this point, and this only, a distinct advance has been made since the time of Regnault. This has been accomplished by Chappuis, working at the Bureau International, who has measured the coefficient of pressure between 0° and 100° for hydrogen, nitrogen, and carbon dioxide, with all the care and scrupulousness that modern physical methods can suggest. My object in writing this paper is partly to take advantage of Chappuis's results, and partly to draw attention

---

[*] Communicated by the Author.

to the fact that if the other data required were investigated with equal care and thoroughness, we could now know the absolute scale as accurately as we know the hydrogen or the mercury scale.

The problem consists of two parts: first, taking the interval between the freezing- and boiling-points as 100°, it is necessary to determine the absolute temperature corresponding to one of those fixed points; second, knowing that value, we can proceed to compare the absolute scale with some actual scale, such as that of the constant-volume hydrogen thermometer, throughout its range, so far as experimental information extends. On the first of these points no substantial advance can as yet be made on the result published by Thomson and Joule several decades ago. The freezing-point is approximately 273° absolute, and the uncertainty amounts to one or two tenths of a degree on each side of that number. But I think it is at least certain that the higher values obtained by some later German writers (some as high as 274°·5) are wrong. On the second point very varying results have been found; but I think it is possible to show that the existing data are in good accord with the comparisons between the three gas thermometers made at the Bureau International.

Theory of the Method.

In order to determine the relation between the absolute and an arbitrary scale of temperature, it is not sufficient to know the characteristic equation of a gas, but, as has recently been pointed out afresh by Boltzmann*, calorimetric data are also necessary; consequently any reduction that does not explicitly make use of such data must do so implicitly. The form in which the calorimetric data usually occur is that of the difference between the two specific heats of a gas; consequently the evaluation of the absolute scale is closely bound up with the determination of the difference, or the ratio of the specific heats. But all direct determinations of $K_P - K_V$, by measuring K_P and K_V, or by measuring K_P and the ratio γ, are, I believe, insufficiently accurate. The difference is, however, nearly equal to the principal gas constant ($K_P - K_V = R$), and it is the small outstanding discrepancy between these two quantities which is the subject of Joule and Thomson's experiments on the cooling by free expansion, so that those experiments afford the most satisfactory means of determining the difference between the specific heats.

The argument runs as follows:—Let a gram of hydrogen

---

* Boltzmann, Wied. *Ann.* vol. liii. p. 948 (1894).

be heated at constant volume through the range δt (t being the temperature on the constant volume or "normal" scale). The heat required is $K_V \delta t$. But let it be heated through the same range at constant pressure, and it will expand by an amount

$$\delta v = (\partial_p v / \partial t) \delta t.$$

It will consequently do an amount of external work in the expansion represented by

$$p (\partial_p v / \partial t) \delta t.$$

Further, internal work is done by the gas (*i. e.* its internal energy is increased) by an amount which is a function of the initial and final states of the gas, and which therefore we may find by any method in which the same expansion is considered, whatever the external circumstances may be. We proceed to find it in the following way.

Lemma.—Let one gram of hydrogen in the state specified by p, v, T expand by an amount δv without doing external work and without acquiring any appreciable kinetic energy (as in Joule and Thomson's experiment). In this case it is well known that the relation $\delta U + \delta(pv) = 0$ holds; but as $pv = RT$ nearly, we may put $\delta U + R\delta T = 0$, where δT is the rise in temperature that occurs during the expansion. Next let the gas be cooled at constant volume to its original temperature; then it gives out $K_V \delta T$ of heat. Hence its internal energy is less than at starting by $(K_V + R)\delta T = K_P \delta T$ to the same degree of approximation as before.

Now according to Joule and Thomson, $\delta T = -\epsilon \delta p$, where δp is the change (increase) of pressure involved in the free expansion, and ϵ is a constant, which for hydrogen is positive. Hence the gain of internal energy is $+K_P \epsilon \delta p$. We wish to express this in terms of the change of volume.

But

$$\begin{aligned} \delta v &= \frac{\partial v}{\partial p} \delta p + \frac{\partial v}{\partial T} \delta T \\ &= -\frac{v}{p} \delta p + \frac{R}{p} \delta T \\ &= -\frac{v}{p} \delta p - \frac{R}{p} \epsilon \delta p. \end{aligned}$$

Hence the gain of internal energy may be written

$$= -K_P \epsilon \times \frac{p}{v + R\epsilon} \delta v.$$

But $v + R\epsilon = v(1 + p\epsilon/T)$; so that, as ϵ is very small, unless the pressure be much greater than is actually used in gas thermometers $p\epsilon$ will be less than 1 per cent. of T (for $T = 273°$, say); and as ϵ is not known to 1 per cent., we may safely neglect the second term of the denominator and write the gain of internal energy $= -K_P\epsilon p dv/v$. But as the temperature of the gas is the same at the end as at the beginning of the process, its heat energy is unchanged, and the quantity found is the amount of internal work done by the gas in expansion.

Reverting to the main argument, we find that when a gram of hydrogen is heated at constant pressure, the heat required, being the sum of the increase of heat energy, the external work, and the internal work, amounts to

$$K_P\delta t = K_V\delta t + \left(p - \frac{K_P\epsilon p}{v}\right)\frac{\partial_p v}{\partial t}\delta t,$$

whence

$$K_P - K_V = \left(p - \frac{K_P\epsilon p}{v}\right)\frac{\partial_p v}{\partial t}, \quad . \quad . \quad . \quad . \quad . \quad \text{(i.)}$$

It is in terms of that quantity that we wish to find a relation between the absolute temperature T and the temperature t according to the gas scale. The relation is clearly expressed by Boltzmann in the paper cited; and his argument is substantially the same as the following.

We start with the well-known thermodynamic relation

$$\frac{\partial_p Q}{\partial T} - \frac{\partial_v Q}{\partial T} = -T\left(\frac{\partial_v p}{\partial T}\right)^2\frac{\partial_T v}{\partial p}. \quad . \quad . \quad . \quad \text{(ii.)}$$

But as the specific heats used in the preceding paragraphs are in terms of the normal scale, in introducing them we must multiply the left-hand side of the above equation by δt, and the right-hand side by δT, or

$$(K_P - K_V)\delta t = -T\left(\frac{\partial_v p}{\partial T}\right)^2\frac{\partial_T v}{\partial p}\delta T.$$

Now write $\partial p/\partial T$ as $(\partial p/\partial t).(\partial t/\partial T)$ in this equation, and remembering that $\delta T = \frac{dT}{dt}\delta t$, we get

$$(K_P - K_V)\delta t = -T\left(\frac{\partial_v p}{\partial t}\right)^2\frac{\partial_T v}{\partial p}\frac{dt}{dT}\delta t;$$

or

$$\frac{\delta T}{T} = -\frac{\left(\frac{\partial_V p}{\partial t}\right)^2\frac{\partial_T v}{\partial p}}{K_P - K_V}\delta t,$$

which is directly integrable. Substituting the value of $K_P - K_v$, it becomes

$$\frac{\delta T}{T} = + \frac{\dfrac{\partial_v p}{\partial t}}{p\left(1 - \dfrac{K_P \epsilon}{v}\right)} \delta t, \quad \ldots\ldots\ldots \quad \text{(iii.)}$$

in which $\dfrac{\partial_v p}{\partial t}$ is a constant for any given volume, viz. $p_0 \beta$ where p_0 means the pressure at the freezing-point, and β the coefficient of pressure.

Integrating for constant v, we get

$$\log T = \frac{1}{1 - \dfrac{K_P \epsilon}{v}} \log p + \text{const.} \quad \ldots\ldots \quad \text{(iv.)}$$

on the assumption that $K_P \epsilon$ is independent of the temperature.

Hence to find T_0 we have

$$\log (1 + 100/T_0) = \frac{1}{1 - K_P \epsilon / v} \log (p_{100}/p_0) = \frac{1}{1 - K_P \epsilon / v} \log (1 + 100\beta). \quad \text{(v.)}^*$$

The assumption involved is, of course, not strictly true. ϵ is a function of the temperature, and, according to Joule and Thomson's experiments, it may, for some gases at least, be taken as inversely proportional to the square of the absolute temperature. It would therefore be more exact to express ϵ

---

* To express the equation (v.) in terms of powers of η we have

$$\frac{T_{100}}{T_0} = \left(\frac{p_{100}}{p_0}\right)^{\frac{1}{1-\eta}} = (1+100\beta)^{\frac{1}{1-\eta}} = (1+100\beta)^{1+\eta+\eta^2 \ldots},$$

where η is written for $K_P \epsilon / v$ to save space. Expanding,

$$\begin{aligned} T_{100}/T_0 = (1+100\beta)[1 &+ 100\beta(\eta+\eta^2+\ldots) \\ &+ \frac{100^2\beta^2}{2}(-\eta+0+\ldots) \\ &+ \frac{100^3\beta^3}{6}(+2\eta-\eta^2+\ldots) \\ &+ \frac{100^4\beta^4}{24}(-6\eta+5\eta^2+\ldots)]; \end{aligned}$$

or

$$\begin{aligned} 1+100/T_0 &= (1+100\beta)[1+100\beta(0{\cdot}852\ldots\eta+0{\cdot}99\ldots\eta^2+\ldots)] \\ &= 1+100\beta\,[1+1{\cdot}163\eta+1{\cdot}35\eta^2\ldots], \\ 1/T_0 &= \beta[1+1{\cdot}163\eta+1{\cdot}35\eta^2\ldots]. \end{aligned}$$

The term η^2 is, however, of no consequence, even in the case of carbon dioxide.

in that form in the equation before integrating: but as ϵ is so small, even for carbon dioxide, and is known with so little accuracy, it is not worth while to do so; it is sufficient to find the average value over the range of integration, and take it as constant at that; the error committed is less than the errors in the experimental data. The same is true of K_P; for while it has been measured more accurately than ϵ, its variations are less, indeed practically *nil* for hydrogen, air, and nitrogen.

The logarithmic form of the equation is the most convenient for calculation; but on account of the smallness of $K_P\epsilon/v$ we find approximately that

$$1/T_0=\beta(1+hK_P\epsilon/v), \quad . \quad . \quad . \quad . \quad \text{(vi.)}$$

where h is a numerical constant $=1{\cdot}163\ldots$; this leads to the important conclusion that β, the coefficient of pressure, varies linearly with $1/v$, i. e. with the density. So far as it is true that the cooling effect on expansion is proportional to the change of pressure, so far the result just found holds good; and consequently, so far as the determination of T_0 is concerned, by merely assuming the *form* of Joule and Thomson's result we may dispense with its numerical value if only we know the rate of variation of the pressure-coefficient with the density (*i. e.* $\partial\beta/\partial\rho$). This is analogous to the deduction made by Lord Kelvin with regard to the coefficient of expansion*. We shall have occasion to revert to this below in considering the numerical values of ϵ.

Experimental Data.

Coefficient of Pressure.—Hydrogen, according to Chappuis† has the coefficient 0·00366254 for a pressure of one metre at the freezing-point. The chief earlier measurements are those of Regnault 0·0036678, Magnus 0·0036594, Jolly 0·0036562, all for one atmosphere at the freezing-point. It is impossible to draw any conclusions from these numbers as to the variation of the coefficient with the density of the gas, a variation which in any case must be extremely small. We shall therefore take Chappuis's result simply, or

$$\beta=0{\cdot}00366254.$$

Air.—Jochmann‡ and Weinstein§ in evaluating the ther-

---

* Kelvin, *Encycl. Britt.* art. "Heat."

† P. Chappuis, *Trav. et Mém. de la comité int. des poids et mesures*, vol. vi.

‡ Jochmann, *Schlömilch's Zeits.* v. p. 106.

§ Weinstein, see W. Förster, *Metronomische Beiträge*, no. 3, Berlin, 1881.

modynamic scale both take Regnault's 0·003665 as the coefficient of pressure at the ordinary density; it seems to me there can be little doubt that is too low. Another series of observations by Regnault himself gives 0·0036679, while Magnus finds 0·0036678 (reduced to the boiling-point under 76 cm. pressure), and Jolly 0·0036695, all higher than the first quoted number. Further, Mendeléef[*], on recalculating these three observations and applying certain corrections neglected by the experimenters, concluded that they show a much closer agreement than when uncorrected, thus:—

Regnault	0·0036694
Magnus.........	700
Jolly...........	702

Mendeléef's reduction is regarded probably with justice as the most exact, and we will take, therefore, the mean result as being true for the normal density (corresponding to a pressure of 1 atmo at 0°).

The variation with the density of the air was taken by Jochmann as 0·0000115, the unit of density being, as before, the density at normal temperature and pressure. This represents the set of measurements taken by Regnault for the express purpose; it also agrees moderately well with some observations by Melander, quoted in Landolt and Börnstein's tables, the only others I have been able to find bearing on the point. We may then put

$$\beta = 0{\cdot}0036700 + 0{\cdot}0000115(\rho - 1).$$

Nitrogen (atmospheric, containing argon).—Regnault found 0·0036682 for one atmo pressure at the freezing-point, Chappuis 0·00367466 for 995 mm. pressure. We adopt the latter value, and in the absence of any more definite information take the same density variation as for air.

$$\beta = 0{\cdot}0036710 + 0{\cdot}0000115(\rho - 1)$$

where ρ is the density expressed in terms of that at N.T.P. as unit.

Carbon Dioxide.—This gas has been very much studied, but the results are more discordant than for air. Regnault gives 0·0036871 and 0·0036856 as the result of two experiments, Magnus 0·0036937, Jolly 0·0037060, all at normal density, while Regnault's observations on the density variation are well represented by 0·00004668, the number adopted by Jochmann. The values given here are not corrected in the

---

[*] Mendeléef, *Ber. d. Deutsch. Chem. Ges.* x. p. 81.

manner indicated by Mendeléef for air; but that correction would be too small to bring them into agreement; while only Jolly's number is at all consistent with that recently found at the Bureau International. Chappuis gives these values:—

0·00372477,	with initial pressure	995 mm.
371634,	,,	870 ,,

they may be represented by the equation

$$\beta=0{\cdot}00370893+0{\cdot}00005126(\rho-1),$$

ρ being, as before, expressed in terms of the density at N.T.P. as unit. We may perhaps best make use of the last-mentioned numbers, but it is remarkable that the discrepancies in the measurement of β should be about four times as great as for air. The expressions given above must not, of course, be relied upon either for large densities or for very small.

Specific Heat.—The specific heat and specific volume of the gases need only be known approximately, as they only enter into the expression for the absolute temperature in the small correction term. The well-known experiments of Regnault and E. Wiedemann afford the necessary information on the specific heat at constant pressure. The former used as unit of heat the capacity of water between 12° and 15°, the latter between 16° and 24°. Following the table recently given by Griffiths, the unit used by Regnault is equal to 41,920,000 ergs, that of Wiedemann 41,830,000. The results are, accordingly, for hydrogen

3·409	calories, or	142,900,000 ergs	(Regnault),
3·410	,,	142,640,000 ,,	(E. Wiedemann),

mean 142,770,000 ergs; this quantity is sensibly independent of temperature and pressure over the range considered.

For air

0·23754	cal., or	9,958,000 ergs	(Regnault),
0·2389	,,	9,993,000 ,,	(Wiedemann),

mean 9,975,000 ergs, also sensibly independent of pressure and temperature.

For nitrogen (atmospheric, containing argon)

0·24348 cal., or 10,207,000 ergs (Regnault),

also independent of temperature and pressure.

For carbon dioxide the experiments show the specific heat to be practically independent of the pressure (for such pressures as we are here considering), but to vary considerably with the temperature. Thus at 0°

$$\left.\begin{array}{llll}0{\cdot}1870\text{ cal., or} & 7{,}839{,}000 & \text{ergs} & \text{(Regnault)}\\ 0{\cdot}1948\quad ,, & 8{,}149{,}000 & ,, & \text{(Wiedemann)}\end{array}\right\}\text{ mean }7{,}994{,}000,$$

and at 100°

$$\left.\begin{array}{llll}0{\cdot}2145\text{ cal., or} & 8{,}991{,}000 & \text{ergs} & \text{(Regnault)}\\ 0{\cdot}2177\quad ,, & 9{,}106{,}000 & ,, & \text{(Wiedemann)}\end{array}\right\}\text{ mean }9{,}048{,}000,$$

the average over the range 0° to 100° being 8,520,000 ergs.

Specific Volume.

	Density at 0° and 760 mm.	Specific volume	
		at 0° and 760 mm.	at 0° and 1000 mm.
Hydrogen	0·0000 8987	11 127·0	8457·5
Air	0·0012 93	773·3	587·5
Nitrogen............	0·0012 57	795·5	604·7
Carbon dioxide ...	0·0019 65	508·9	385·9

Cooling on Free Expansion.—This quantity also enters only in the correction term, but it has not been determined with sufficient accuracy even for that. If we take the probable error of β as measured by Chappuis at 2×10^{-7} (or 1/18000 part of its whole value)—an opinion which seems justified by the close agreement of his observations among themselves—then the cooling ϵ resulting from a fall of 1 atmosphere in the pressure should be measured to about $0^\circ{\cdot}004$ to influence the result equally. This is about 1/50 of the value of ϵ for air; that degree of accuracy has certainly not been attained, even for air, much less for the other gases which Joule and Thomson studied.

Hydrogen.—With this gas twelve experiments were made at about 7° and five at about 90°. The mean of the twelve at the low temperature gave $\epsilon=0^\circ{\cdot}301$ per 100 inches of mercury pressure for pure hydrogen; but in only five cases did the analysis of the gas used show as much as 90 per cent. of hydrogen—the rest being air—and these five experiments gave $\epsilon=0^\circ{\cdot}108$, while the one case in which the purity rose to 98·2 per cent. gave $\epsilon=0^\circ{\cdot}075$ for the pure gas. It is obvious that no reliance can be put in deductions made from the mixtures containing much air, and that the whole question is in a very unsatisfactory state. The high temperature measurements gave $\epsilon=0^\circ{\cdot}155$, and the mean between that and 0·108 reduced to the atmosphere as unit of pressure gives $\epsilon=$ **+0°·039** : which is the number used by Lord Kelvin in the *Encyclopædia Brittanica* article. It should be noted, however, that this appears to indicate a greater effect at the higher temperature, contrary to what was observed for the other gases.

Air.—In their earlier memoir, Joule and Thomson* record experiments with air at 17° and at 91°, yielding −0°·259 and −0°·206 per atmosphere respectively. In the later memoir † they paid more attention to the experiments at high temperatures, so probably the results are more trustworthy than that first found for 91°. The following table shows the mean of the observations grouped about four different temperatures, and also the value of ϵ calculated on the assumption that it varies inversely as the square of the absolute temperature :—

Mean temperature.	No. of experiments.	ϵ (obs.).	ϵ (calc.).	
7°	8	−0°·263	−0°·261	per atmo.
26	2	0·229	0·229	
50	6	0·209	0·196	
93	6	0·152	0·153	

The mean over the range 0° to 100° is **−0°·200**. Lord Kelvin, in the *Encycl. Britt.*, gives the value −0°·208, which is practically the same.

Nitrogen.—The single experiment made at 7° with this gas gave a cooling of 1°·034 for 100 inches of mercury; and two experiments at 92° gave 0·576 and 0·691 respectively. These numbers are fairly in agreement with the assumption that the effect varies inversely as the square of the absolute temperature, and lead to

$$\epsilon_0 = -\,0^\circ{\cdot}332 \text{ per atmo at } 0^\circ,$$
$$\epsilon = -\,0^\circ{\cdot}224 \quad \text{,,} \quad \text{mean between } 0^\circ \text{ and } 100^\circ.$$

For carbon dioxide, Joule and Thomson state the same

---

* Phil. Trans. 1854.

† Ibid. 1862.

result as to variation with the temperature; but it is not so well supported by the experimental numbers. The observations recorded in their second memoir, excluding those in which the gas contained more than 10 per cent. of impurity, are as follows:—

Mean temperature.	No. of experiments.	ϵ (obs.).	ϵ (calc.).	
8°	2	−1°·233	−1°·233	per atmo.
36	1	1·022	1·020	
54	1	·885	·911	
96	2	·645	·715	

The numbers in the last column are calculated from the inverse square rule, taking ϵ_0 as −1°·293. The cooling effect seems to fall off more rapidly; I have therefore preferred to make a diagram of the observed results, together with those published in the earlier memoir, which agree with the above, and read off the value of ϵ for each temperature from the curve. With the value of ϵ_0 assumed in the above table and the inverse square rule, the mean over the range 0° and 100° (which is 0·732 of the value at 0°) is −0°·956; whilst the value taken from the curve is **−0°·930**. Lord Kelvin, in the encyclopædia article, gives −1°·105, without explaining the process of reduction used. I have not been able to see how this number is arrived at.

E. Natanson[*] has repeated Thomson and Joule's experiment on carbon dioxide, using the commercial liquefied gas from a cylinder: he made all his experiments at one temperature, −20°, and found that the cooling is not quite proportional to the pressure, but may be represented by

$$-\epsilon = 1{\cdot}18 + 0{\cdot}0126p,$$

p being in atmospheres. This, for low pressures, is in accordance with Joule and Thomson's result at the same temperature.

We have, however, seen that there is an indirect means of estimating the cooling effect, by means of the density-variation of the coefficient of pressure, the relation being

$$\epsilon = \frac{T_0}{hK_T}\,\frac{\delta\beta}{\delta\rho},$$

---

[*] E. Natanson, Wied. *Ann.* vol. xxxi. pp. 502–526 (1887).

in evaluating ϵ in this way we have to remember that it is to be expressed per 1013000 dynes per sq. cm., and that the unit of density (ρ) is the density at the freezing-point and one atmospheric pressure, and consequently for air 773·3 times, and for carbon dioxide 508·9 times less than unity on the C.G.S. system.

The value of $\delta\beta/\delta\rho$ is not known for hydrogen or nitrogen: for air we have

$$\epsilon = \frac{273 \times 1013000 \times 0{\cdot}0000115 \times 773{\cdot}3}{1{\cdot}163 \times 9980000} = 0^\circ{\cdot}221 \text{ per atmo.}$$

This, which represents the mean value of the range 0° to 100°, is in good agreement with the number found experimentally (0°·200). For carbon dioxide

$$\epsilon = \frac{273 \times 1013000 \times 0{\cdot}00005126 \times 508{\cdot}9}{1{\cdot}163 \times 8520000} = 0^\circ{\cdot}728 \text{ per atmo.}$$

This, it will be noticed, is much less than the number derived from Joule and Thomson's measurements, whether by their own reduction or by mine; and if Regnault's value of $\delta\beta/\delta\rho$ be used the result will be 10 per cent. smaller still. This discrepancy wants clearing up.

Results:—(i.) *Value of* T_0.

For the first part of the problem, that of determining the absolute temperature of the freezing-point, we require to know the mean value of $(1-K_p\epsilon/v)$ over the range 0° to 100° for such densities as have actually been used in the gas-thermometers. These values are shown in the following table:—

	Pressure of the gas at 0°.	$1-K_p\epsilon/v$.	Remarks.
Hydrogen	mm. 1000	0·9993	ϵ according to J. and T.
Air	760	1·0025	Ditto.
Nitrogen	995	1·0034	ϵ assumed to be the same as for air.
Carbon dioxide ...	995	1·0203	ϵ from curve of J. and T.'s measurements.
Carbon dioxide ...	995	1·0160	ϵ from $\delta\beta/\delta\rho$ (Chappuis).

Then from equation (v.) we find the following values of T_0:—

	$1/\beta$.	T_0.
Hydrogen	273·035	272·81
Air.........................	272·48	273·27
Nitrogen	272·13	273·20
Carbon dioxide...........	268·47	274·83
Carbon dioxide...........	268·47	273·48

With regard to these results we may remark that in each case the data leave something to be desired. Hydrogen, no doubt, is the best thermometric substance, since its divergence from a perfect gas is so small; and the numbers suggest that perhaps even taking $\epsilon = 0^\circ{\cdot}039$ is an overestimate of the divergence. Moreover, as we shall see below, there appears to be an anomaly in its behaviour which makes the course of the hydrogen scale between 0° and 100° run differently from those of the other gases. For both these reasons, more information on the thermal effect due to free expansion, and its variation with temperature, is especially needed; and it is essential that the experiments should be made with pure hydrogen.

The value for T_0 derived from the air-thermometer is probably entitled to about the same weight as that from hydrogen. It is the only gas for which ϵ appears to be satisfactorily determined; on the other hand, it is probable that β is a little higher than is assumed above; for, according to Jolly, whose measurements seem to be more reliable than Regnault's, air has a coefficient of pressure higher than that of nitrogen by 2×10^{-6}; this is all the more likely because it brings air nearly into the position it would occupy if its constituent gases exercised no influence on one another. Now if this is true, and Chappuis's value for nitrogen is correct, the pressure-coefficient for air would become 0·0036730 and the value of T_0 derived from it $273^\circ{\cdot}05$.

The determination from nitrogen is not of much value on account of the absence of information on the value of ϵ. Of the two values from carbon dioxide, the first is, of course, quite inadmissible; if the theory given in this paper is correct, we must conclude that the measurements of the cooling effect contain some error, and that the variation of the coefficient of pressure is more to be depended on in this instance. It may be remarked too, that if we take Regnault's value of this quantity instead of Chappuis's, T_0 will come out much closer to 273°, and it would probably be right to do so, since the experiments of Regnault covered a wide range of density, whereas only two densities, near together, were used at the Bureau International. The numbers derived from carbon dioxide cannot, however, be allowed much weight in the result, at present; a fresh set of experiments on the cooling effect, carried out with the pure gas, and over a considerable range of temperature, is needed to make the conclusions trustworthy; while determinations of the coefficient of pressure for lower densities, not only for carbon dioxide but for all the gases, including air, would give most valuable information.

The results, allowing for such possible corrections as we have just indicated, seem to point to the conclusion that the freezing-point is within a tenth of a degree of 273° absolute.

Results:—(ii.) *Comparison of the Thermodynamic with Actual Scales.*

To reduce temperatures on a gas scale, we have from (iv.)

$$\log (T/T_0) = \frac{1}{1 - K_P\epsilon/v} \log \frac{t + 1/\beta}{1/\beta}, \quad . \quad . \quad . \quad (vii.)$$

in which K_P and ϵ are to have their mean values over the range 0° to t°. For hydrogen these are assumed to be constant (for want of better information) and equal to their values between 0° and 100°, so that

$$1 - K_P\epsilon/v = 0\cdot9993$$

as before. For nitrogen the values of ϵ are taken as those read off the curve of experimental data for air, and are as follows:—

$$K_P = 10{,}200{,}000\,; \quad v = 608 \text{ c.c.}$$

t.	Mean of ϵ 0° to t°.	$1 - K_P\epsilon/v$.
10	−0·261	1·0043
20	·255	42
30	·249	41
40	·242	40
50	·235	39
60	·228	38
70	·221	37
80	·214	36
90	·207	35
100	·200	34

For carbon dioxide the values of ϵ were read off the curve of Joule and Thomson's results, and then reduced in proportion, so as to give 0·728 over the range 0° to 100°, in accordance with Chappuis's measurements.

$v = 388$ c.c.

t.	Mean of ϵ 0° to t°.	Mean of K_P 0° to t°.	$-K_{P}\epsilon/v$.
10	−0·986	$8·05\times10^6$	1·0206
20	·955	8·10	200
30	·924	8·15	195
40	·893	8·20	190
50	·861	8·25	185
60	·830	8·31	180
70	·798	8·36	175
80	·775	8·42	170
90	·751	8·47	165
100	·728	8·52	160

The results of equation (vii.) are as follows:—

$T-T_0$.	t_H.	t_N.	t_{CO_2}.	t_m.
10	9·999	10·005	10·022	10·051
20	19·998	20·009	20·040	20·083
30	29·998	30·012	30·054	30·101
40	39·997	40·014	40·065	40·107
50	49·997	50·016	50·071	50·105
60	59·997	60·016	60·070	60·094
70	69·998	70·015	70·064	70·078
80	79·998	80·013	80·050	80·056
90	89·999	90·009	90·030	90·031

This table shows the readings of the hydrogen, nitrogen, carbon dioxide, and the Paris mercury thermometer for each ten degrees on the absolute scale: the last column is obtained from the direct comparison of the mercury with the hydrogen and nitrogen thermometers, combined with my reduction of the latter to the absolute scale.

In order to judge of the value of the results, the following table shows, in parallel columns, the difference in reading between each pair of gas thermometers (1) as calculated in this memoir; (2) as observed by direct comparison at Paris.

Differences between Gas Thermometers
(in thousandths of a degree).

t.	$t_N - t_H$.		$t_{CO_2} - t_H$.		$t_{CO_2} - t_N$.	
	calc.	obs.	calc.	obs.	calc.	obs.
10° ...	+ 6	+ 6	+23	25	+17	19
20 ...	11	10	42	43	31	33
30 ...	14	11	56	54	42	43
40 ...	17	11	68	59	51	48
50 ...	19	9	74	59	55	50
60 ...	19	5	73	53	54	48
70 ...	17	1	66	44	49	43
80 ...	15	− 2	52	31	37	33
90 ...	10	− 3	31	16	21	19

The hydrogen and nitrogen scales exhibit a curious reversal at 80° to 90°, which is not reproduced in the calculated numbers; the calculated temperatures are consequently a good deal in error there. It will be noticed that the calculated difference for $t_{CO_2} - t_N$ is much more correct than that for $t_{CO_2} - t_H$, which seems to show that the anomaly is rather in the hydrogen than in the nitrogen scale; this is another point which cannot be cleared up without further experimental data. The greatest difference between the observed and calculated numbers is 0°·022; the mean of all the differences 0°·008.

Conclusions.

In conclusion one may be permitted to repeat that:—

(1) The absolute temperature of the freezing-point is probably within a tenth of a degree of 273°.

(2) The divergences between the different gas scales are fairly represented by the theory; but it is not yet possible to reduce temperatures to the absolute scale with so high a degree of accuracy as it is possible to record them on the scale of the mercury thermometer, or on the "normal scale" of the Comité internationale.

(3) In order to attain to that degree of accuracy, and to settle the value of T_0 to 0°·05—which seems possible—it is necessary:—

(*a*) To measure the coefficient of pressure for air in the same manner as has been done for hydrogen, nitrogen, and carbon dioxide by Chappuis, and to extend the experiments on all four gases to initial pressures lower than one metre of mercury—say for 25, 50, and 75 cm.

(*b*) To redetermine the change of temperature on free expansion for *pure* hydrogen, nitrogen, and carbon dioxide, over as wide a temperature as possible, paying particular attention to the question whether the cooling effect is strictly proportional to the change of pressure, and to its variation with the temperature : the experiments on hydrogen being the most important.

XLI. *Notices respecting New Books.*

Theoretical Mechanics, an Introductory Treatise on the Principles of Dynamics, with Applications and numerous Examples. By A. E. H. LOVE, *M.A., F.R.S., Fellow and Lecturer of St. John's College, Cambridge.* Cambridge, at the University Press, 1897.

THIS well-printed volume issuing from the Cambridge University Press seems intended to meet the wants of candidates for scholarships or places in the Mathematical Tripos. Its contents consist of eighty-four pages of "Preliminary" matter, sixty on "the Principles of Dynamics," and two hundred and twenty-six on "Methods and Applications"; an 'Appendix' of five pages on "Measurement, Units, and Dimensions"; finally, an Index.

The terminology of the treatise involving the frequent use of the vector, ch. ii. is devoted to "the Geometry of Vectors"; while Force is defined (ch. v.), as "a vector localized at a point" or what is represented by a right line drawn from a given point in a determined direction, sense, and length. Bodies have 'mass' and 'act' upon one another; and, in the case of homogeneous bodies, the ratio of mass to volume is the density; the limit at any point of course giving the density at the point, where this is variable. The differential coefficients introduced are usually printed in the fluxional manner with *dots*, as in Thomson and Tait's treatise; axes of coordinates form a 'frame.' 'Gravity' is the vertical action of the particles of the Earth on those of any body "small enough to be handled or moved by machinery"; thus, the neighbourhood of the Earth is a 'field of force,' and the acceleration at any point in the field, *i. e.* the intensity of the field, is at any place vertically downwards and equal to g; weight being "the resultant force on a free falling body in the neighbourhood of the Earth, numerically equal to mg." "At any place on the Earth's surface the mass of a body is proportional to its weight" is a statement which implies an ideal Earth, homogeneous and of spherical form. "The tendency of a body, apart from the action of other bodies, to persist in its state of motion at any instant is known as its *inertia*"; which the mass can be taken to measure.

In an historical note, at the conclusion of ch. v., the idea of acceleration is assigned to Galilei, and that of force as its cause to Newton, of whose *Axiomata sive Leges Motus* a translation is given; they being regarded as postulates of which the third contained what was afterwards known as d'Alembert's Principle.

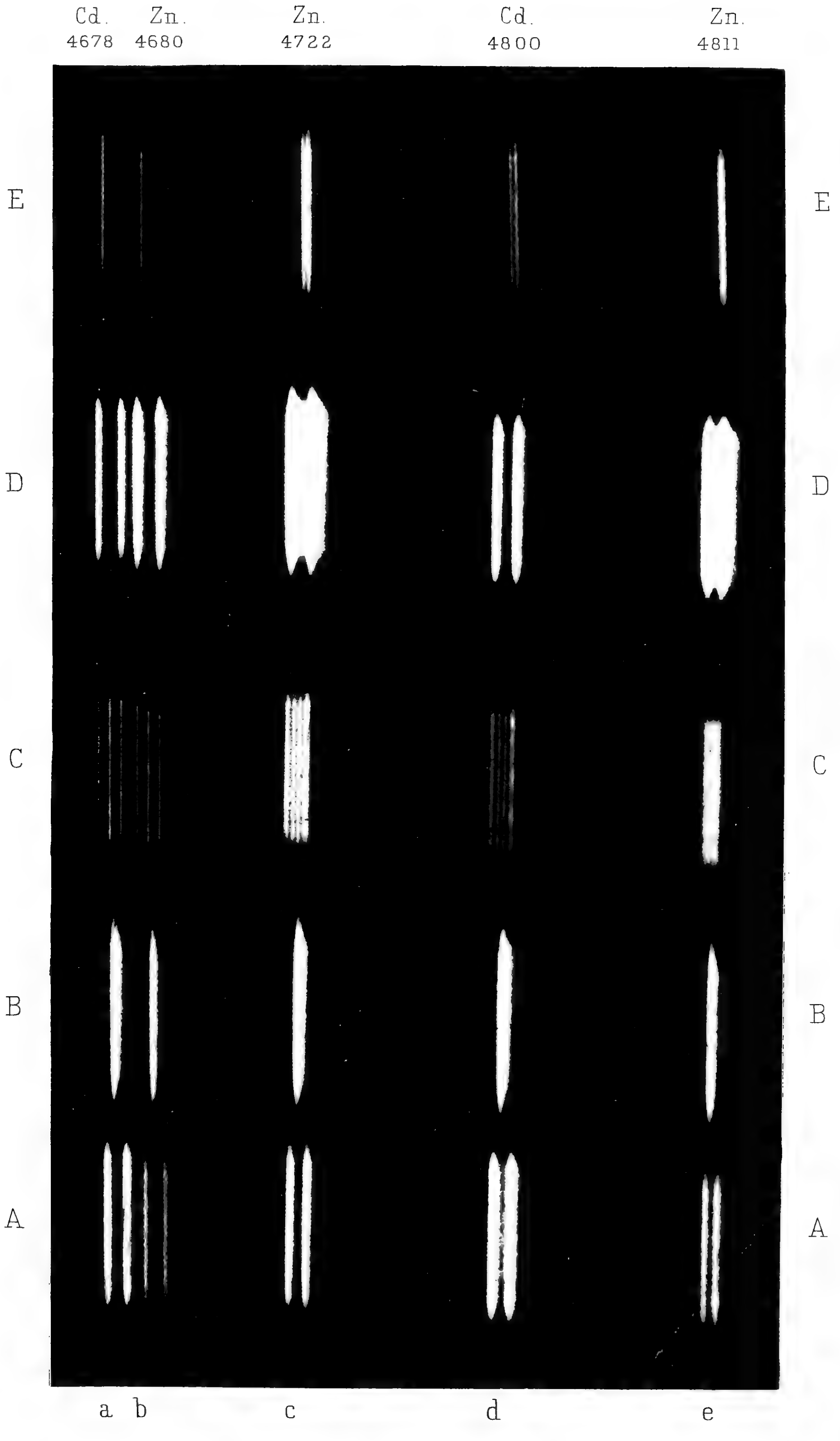
In the Preface Mr. Love refers students for the history of dynamical ideas to Mach's Science of Mechanics.

The term 'kinetic reaction' is used with the definition "a vector localized in the line and with the sense of acceleration of a particle and of magnitude equal to the product of its mass and acceleration."

After a chapter (vi.) of general theorems, in that following the deformability of bodies, regarded as geometrical surfaces filled continuously with particles, strain and stress are introduced; as also 'Bodily forces,' 'Surface tractions,' 'Tensions of strings' (inextensible or extensible) and reactions thereof when in contact with surfaces; finally 'Springs.'

Ch. viii. is devoted to the important subject of 'Work and Energy.' A 'conservative' system, the analytical condition for it, and its 'Potential Energy' are explained. A collection of Examples of the Calculation of Work is appended. The subject being then resumed, a proof of the constancy of the sum of kinetic and potential energy of a conservative system is given; forces are distinguished as 'Positional and Motional,' and it is shown that those of rational mechanics may be conditioned as always conservative as regards positional, but generally non-conservative as regards motional. 'Power' is defined as the rate per unit of time at which work is done by the action of one system on another. The kinetic energy produced by impulses; the principle of virtual work or 'virtual moment of a localized vector,' the 'variation equation of motion,' and a 'critical note'—in which a conclusion is arrived at that "if we are prepared to abandon precise definition and the purely logical deductive method, as unsuited to a science at present incompletely known, we may construct a physical theory, indefinite in parts and incomplete in details, but nevertheless available for co-ordinating the results obtained by physical investigation, and capable of being itself advanced towards perfection thereby"—conclude the first half of the volume. The second half is occupied with 'Methods and Applications,' largely illustrated by a copious collection of examples, some worked out in full, attached to each chapter. 'Motions of Free Particles in given Fields of Force,' and 'under Constraints and Resistances,' form the subjects of chs. ix. and x. devoted to the 'Dynamics of a Particle.' The 'Motion of a Rigid Body in Two Dimensions' occupies ch. xi. Impulse and Collision, as far as Impact of smooth Spheres, necessarily introduces reference to Elasticity,—reminding us of Mr. Love's Treatise on that subject, reviewed in this Magazine some few years since. The Impact of Rigid Bodies in two dimensions, Initial Movement and the Motion of a Chain are the subjects of ch. xii.— one of Examples essentially. A short chapter on 'Relative Motion and Universal Gravitation' forms the last.

From this abstract of its contents, it will be seen that the treatise, within the limits to which it is confined, is quite "up to date"; and maintains the Author's character as a careful and competent teacher of the subjects on which he undertakes to instruct. J. J. W.



Phil. Mag. S. 5. Vol. 45. Pl. XXIII.

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

[FIFTH SERIES.]

MAY 1898.

XLII. *On Electromagnetic Induction in Plane, Cylindrical, and Spherical Current-Sheets, and its Representation by Moving Trails of Images.*—Parts II. & III. *By* G. H. Bryan, *Sc.D., F.R.S.*[*]

Part II. Cylindrical Sheets.

In the Philosophical Magazine for August 1894, I showed how to obtain in a simple way the surface-conditions satisfied by the magnetic potential at a thin sheet of metal of finite conductivity of any form, in which currents are induced by the motion of magnets on either side of the sheet.

In the case of a *cylindrical* sheet of small thickness c, conductivity C, and radius a, the magnetic potentials Ω_1 and Ω_2 inside and outside the sheet were found to be connected by surface-conditions (at $r=a$) which may be written

$$\frac{d}{dt}\left(r\frac{d\Omega_1}{dr}\right)=\frac{d}{dt}\left(r\frac{d\Omega_2}{dr}\right)=\frac{\mathrm{R}}{2}\frac{d}{dr}r\frac{d}{dr}(\Omega_2-\Omega_1), \quad . \quad . \quad (1)$$

involving the condition $d\Omega_1/dr=d\Omega_2/dr$; where

$$\mathrm{R}=\frac{1}{2\pi\mathrm{C}c},$$

so that R is the velocity with which the trails of images in a plane sheet of the same thickness and conductivity would move away from the sheet.

---

[*] Communicated by the Physical Society: read February 11, 1898.

For a *spherical* sheet we found in like manner

$$\frac{d}{dt}\left(r^2\frac{d\Omega_1}{dr}\right)=\frac{d}{dt}\left(r^2\frac{d\Omega_2}{dr}\right)=\frac{R}{2}\frac{d}{dr}r^2\frac{d}{dr}(\Omega_2-\Omega_1), \quad . \quad (2)$$

involving, as before, the condition $d\Omega_1/dr=d\Omega_2/dr$.

Before determining the images which represent the effect of the currents induced by the introduction of a given pole, magnet, or current in the presence of such sheets, it will be convenient to think, for a moment, of the images which occur in hydrodynamical problems relating to the cylinder and sphere. In two-dimensional fluid motion, the image of a line-source outside and parallel to the axis of a cylinder is an equal line-source through the inverse point together with an equal line-sink through the centre (*i. e.* along the axis); similarly the image of a line-source inside the sphere together with an equal line-sink along the axis is an equal line-source through the inverse point. The image of a vortex-filament parallel to the axis of a cylinder, whether inside or outside the cylinder, is an equal and opposite vortex through the inverse point. In the case of a sphere, the image of a doublet whose axis passes through the centre is a certain doublet at the inverse point, whether the original doublet be inside or outside the sphere. The image of a *source* in a sphere, on the other hand, is more complicated, consisting not only of a source but also of a sink extending to the centre if the original source was outside the sphere, and to infinity if inside.

We may therefore expect by analogy that the images in a spherical sheet due to the motion or generation of magnetic *poles* in its neighbourhood will be less simple than those produced by *magnets* whose axes pass through the centre of the sphere; and such we shall find to be actually the case.

Image due to Generation of a Line of Poles parallel to the Axis outside a Cylindrical Sheet.

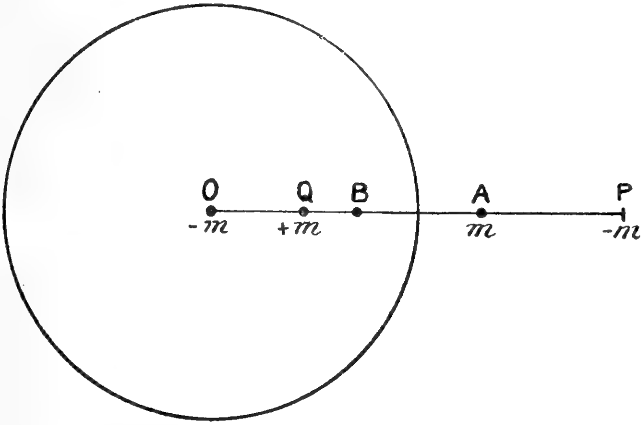
2. We first consider the two-dimensional problem in which the currents are induced by the generation or destruction of a linear distribution of magnetism parallel to the axis of the cylinder at A (fig. 1) in the line whose polar (cylindrical) coordinates are $r=b$, $\theta=0$, where b is greater than a the radius of the cylinder. To cover all cases we commence by assuming at the outset that the line-density of this line of poles, as it may be called, is $f(t)$ a function of the time.

The magnetic potential of this distribution at the point (r, θ) is therefore

$$\Omega_0 \text{ (say) } =\mu f(t)\{\text{const.}-\log\sqrt{(r^2+b^2-2rb\cos\theta)}\},$$

where μ, the magnetic permeability of the dielectric, will be taken to be unity, or included under $f(t)$.

Fig. 1.



The above potential may, for values of r less than b, be expanded in the form

$$\Omega_0 = f(t)\left\{\text{const.} - \log b + \Sigma \frac{1}{n}\frac{r^n}{b^n}\cos n\theta\right\}. \quad . \quad (3)$$

Consistently with the surface condition of continuity of magnetic induction, we can assume for the potentials of the induced currents inside and outside the cylinder expressions of the form

$$\Omega' = -\Sigma\frac{A_n}{n}\frac{r^n}{a^n}\cos n\theta \text{ (inside)}, \quad . \quad . \quad . \quad (4)$$

$$\Omega'' = \Sigma\frac{A_n}{n}\frac{a^n}{r^n}\cos n\theta \text{ (outside)}, \quad . \quad . \quad . \quad (5)$$

since these make $d\Omega'/dr = d\Omega''/dr$ when $r=a$; here the coefficients A_n are functions of the time t.

Substituting the expressions thus obtained, $\Omega_0+\Omega'$ for Ω_1, and $\Omega_0+\Omega''$ for Ω_2 in the surface-conditions (1), we have, on equating coefficients of $\cos n\theta$,

$$\frac{dA_n}{dt} + n\frac{R}{a}A_n = f'(t)\frac{a^n}{b^n};$$

$$\therefore \quad \frac{d}{dt}\left\{A_n e^{ntR/a}\right\} = f'(t)\,e^{ntR/a}\frac{a^n}{b^n};$$

$$\therefore \quad A_n = \int_{-\infty}^{t} f'(\tau)\,e^{-n(t-\tau)R/a}d\tau \,.\, \frac{a^n}{b^n};$$

the limits of integration being t and $-\infty$ if we have to take account of changes that have been taking place from an infinite time back.

To analyse the images, consider now the case in which a line of poles of strength m is suddenly generated at A at the time 0, so that

$$f(t) = 0 \text{ when } t < 0,$$
$$f(t) = m \text{ when } t > 0,$$

$f'(t)$ vanishes except when $t=0$, and the time-integral of $f'(t)$ over a very small interval containing the time 0 is equal to m. We thus obtain

$$\mathrm{A}_n = me^{-nt\mathrm{R}/a}\frac{a^n}{b^n}.$$

Substituting in the expressions for Ω' and Ω'' we find

$$\Omega' = -m\Sigma\frac{1}{n}\frac{r^n}{b^n e^{nt\mathrm{R}/a}}\cos n\theta$$
$$= -m[\log be^{t\mathrm{R}/a} - \log\{r^2 + (be^{t\mathrm{R}/a})^2 - 2rbe^{t\mathrm{R}/a}\cos\theta\}^{\frac{1}{2}}] \quad . \quad (6)$$
$$\Omega'' = m\Sigma\frac{1}{n}\frac{a^{2n}}{b^n e^{nt\mathrm{R}/a}}\cos n\theta$$
$$= m\left[\log r - \log\left\{r^2 + \left(\frac{a^2}{be^{t\mathrm{R}/a}}\right)^2 - 2r\left(\frac{a^2}{be^{t\mathrm{R}/a}}\right)\cos\theta\right\}^{\frac{1}{2}}\right]. \quad (7)$$

Hence we have the following results :—

(i.) At points inside the conductor, the magnetic potential due to the induced currents initially neutralizes that produced by the sudden generation of the line of poles at A, and is equivalent to that of a line of poles (P, fig. 1) of constant strength $-m$, starting at the point A and moving away from the centre so that its distance from the centre at time t is $e^{t\mathrm{R}/a}b$.

(ii.) At points outside the conductor the magnetic potential is equivalent to that of two lines of poles, one of constant strength $-m$ at O the centre, and the other (Q) of strength $+m$, starting at B the inverse point of A $(a^2/b, 0)$, and moving towards the centre so that at time t its distance from the centre is $e^{-t\mathrm{R}/a}a^2/b$. We observe that P and Q, the positions of the inside and outside images, move so as always to remain inverse points.

The velocities with which the images P, Q move away from and towards O respectively are R/a times their distances from O, and their accelerations are R^2/a^2 times these distances respectively.

3. The current function at any point M of the cylinder is given by

$$\phi = \frac{1}{4\pi}(\Omega_2 - \Omega_1) + \text{a constant};$$

therefore ϕ differs by a constant from

$$-\frac{m}{4\pi}(\log \mathrm{PM} + \log \mathrm{QM}).$$

The arbitrary constants in ϕ may be functions of the time, but they do not affect the distribution of currents in the cylinder, and therefore need not be considered. Since P and Q are inverse points

$$\mathrm{PM} : \mathrm{QM} = \mathrm{OP} : a = a : \mathrm{OQ} = be^{\mathrm{R}t/a} : a\,;$$

log PM and log QM therefore differ by a term which may be included in the arbitrary constant of ϕ, so that for determining the currents we may take

$$\phi = -\frac{m}{2\pi}\log \mathrm{PM}, \quad \text{or } \phi = -\frac{m}{2\pi}\log \mathrm{QM},$$

whichever is most convenient.

The current at M is evidently parallel to the axis (since ϕ is independent of the third cylindrical coordinate z), and the intensity of the current is equal to

$$\frac{d\phi}{a d\theta} = -\frac{m}{2\pi}\frac{\mathrm{OP}\sin\theta}{\mathrm{PM}^2} = -\frac{m}{2\pi}\frac{\mathrm{OQ}\sin\theta}{\mathrm{QM}^2}.$$

Images due to Generation of Lines of Poles inside the Cylindrical Sheet.

4. If we were to suppose the currents to be induced by a line of poles of strength $f(t)$ at the point $(b, 0)$ where $b < a$, we should have to expand the potential in descending powers of r in order to obtain a convergent series at the surface of the sheet. We should thus obtain for the inducing potential

$$\Omega_0 = f(t)\left\{\text{const.} - \log r + \Sigma\frac{1}{n}\frac{b^n}{r^n}\cos n\theta\right\}.$$

Now the portion

$$f(t)\{\text{const.} - \log r\}$$

represents the potential of a line of poles of strength $f(t)$ along the axis, for investigating the effects of which the image method is inconvenient. To remove these terms we shall suppose the inducing system to consist of a line of poles through the point $(b, 0)$ together with an equal and opposite line of poles along the axis: in other words of a line of magnets having their positive poles $f(t)$ at $(b, 0)$ and their negative poles $-f(t)$ at the axis. The potential of such a distribution expands where $r>b$ in the form

$$\Omega_0=f(t)\Sigma\frac{1}{n}\frac{b^n}{r^n}\cos n\theta. \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

The analysis is almost identical with that of the preceding case, and passing to the case when

$$f(t)=0 \text{ when } t<0,$$
$$f(t)=m \text{ when } t>0,$$

the following results may be obtained, taking B (fig. 1) to now represent the point $(b, 0)$.

(i.) If lines of poles of strengths m and $-m$ be instantaneously generated at B and the axis respectively, the images representing the magnetic potential of the induced currents outside the cylinder after a time t are lines of poles of strengths $-m$ and $+m$ at Q and the axis, where $OQ=e^{-tR/a}b$. Hence the *total* magnetic potential at outside points due to the poles and the induced currents at time t is that of lines of poles of strengths $+m$ and $-m$ at B and Q respectively.

(ii.) At points inside the cylinder, the magnetic potential due to the induced currents alone is that which would be produced by a line of poles of strength $+m$ at P, where P is the inverse point of Q, so that $OP=e^{tR/a}a^2/b$.

Images due to Generation of Straight Currents Parallel to the Axis of the Cylindrical Sheet.

5. The cases where the currents are induced by the sudden generation of a straight current parallel to the axis of the cylinder probably have a more practical physical interest than those considered in the previous articles. If we suppose all the lines of poles of strengths $\pm m$ of the preceding sections to be replaced by currents of intensities $\pm$I the results regarding the images will be identically the same as before. To satisfy ourselves that such is the case we have only to notice that while the magnetic potential of a line of poles of line-density m at $(b, 0)$ is the real part of the expression

$$m\log(re^{i\theta}-b),$$

the magnetic potential of a current perpendicular to the plane of reference and of intensity $\frac{1}{2}m$ is the imaginary part of the same expression. Where $r<b$, the potential of the current expands therefore in the series

$$m\Sigma\frac{1}{n}\frac{r^n}{b^n}\sin n\theta,$$

and where $r>b$ the potential of a current $\frac{1}{2}m$ at $(b, 0)$

together with a return current $-\frac{1}{2}m$ down the axis expands in the series

$$m\Sigma\frac{1}{n}\frac{b^n}{r^n}\sin n\theta.$$

These expressions differ from those used in the previous articles by the substitution of sines for cosines only, and it follows that when we make similar changes in the assumed expressions for the potentials of the induced currents, the work of substituting in the boundary conditions will be unaltered. For convenience, however, it will be well to again state the final conclusions, which are as follows:—

(i.) If a current of strength I be suddenly generated parallel to the axis of the cylinder at A *outside* the cylinder, the image representing the effect of the induced currents for inside points at time t will be a current of strength $-$I at P, and the images representing their effect for outside points will be a current $+$I at Q and a return current $-$I in the axis, where $\mathrm{OP}=e^{t\mathrm{R}/a}\mathrm{OA}$, and Q is the inverse point of P, so that $\mathrm{OQ}=e^{-t\mathrm{R}/a}a^2/\mathrm{OA}$.

(ii.) If a current of strength I be suddenly generated at B *inside* the cylinder, accompanied by a return current $-$I in the axis, the image representing the effect of the induced currents for outside points will be a current I at P, and the images representing their effect for inside points will be currents $-$I at Q, and I in the axis, where $\mathrm{OQ}=e^{-t\mathrm{R}/a}\mathrm{OB}$, and P is the inverse point of Q, so that $\mathrm{OP}=e^{t\mathrm{R}/a}a^2/\mathrm{OB}$.

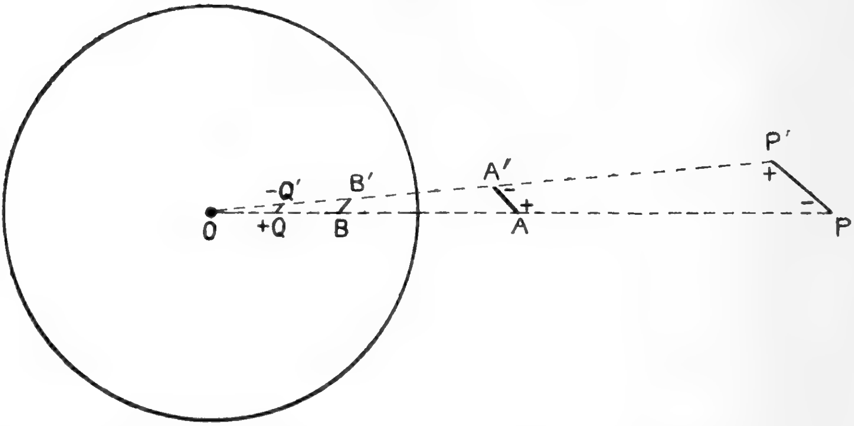
The effect of *breaking* a current of intensity I will of course be the same as that of superposing a current $-$I and the images will be equal and opposite to those above obtained.

Images due to sudden Generation of Lines of Magnets, and Moving Poles.

6. It is now easy to build up the images due to the sudden generation or destruction of lines of magnets parallel to the axis of the cylinder, the motion of lines of poles and so forth. The methods are almost identical with those used for plane sheets, and it will be sufficient here to call attention to one or two salient features of the images in cylindrical sheets. As in the previous articles we shall take A, B to be any two inverse points inside and outside the cylinder, P and Q two other inverse points such that $\mathrm{OP}=e^{t\mathrm{R}/a}\mathrm{OA}$, $\mathrm{OQ}=e^{-t\mathrm{R}/a}\mathrm{OB}$, and similar relations will be supposed to hold when the letters are accented.

(i.) If a line of magnets parallel to the axis at AA′ be generated suddenly, having their positive poles at A and their negative poles at A′ (fig. 2), the image at time t for inside points will be the oppositely magnetized line PP′, and for outside points the line QQ′ having its positive magnetism at Q. From the properties of similar triangles and of inversion, PP′ is parallel and QQ′ is antiparallel to AA′, also, since the strengths of the poles are constant, the magnetic moments of the images are to one another and to that of the inducing system as PP′ : QQ′ : AA′, that is as OP : OQ′ : OA or, in the limiting case (when Q and Q′ are infinitely near together), as the distances OP : OQ : OA.

Fig. 2.



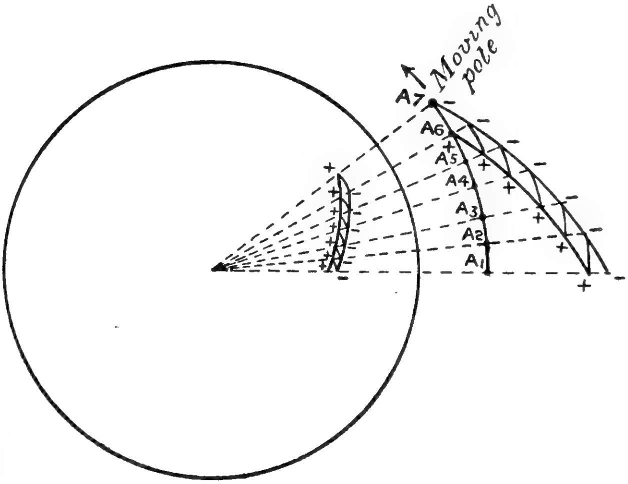
Hence if M be the magnetic moment generated in AA′, those of the images PP′ and QQ′ at time t are $-\mathrm{M}e^{t\mathrm{R}/a}$ and $\mathrm{M}e^{-t\mathrm{R}/a}a^2/b^2$, where $b = \mathrm{OA}$.

(ii.) Similarly, if a line of magnets of moment M be suddenly generated at BB′ *inside* the cylinder, the images at time t for outside points will be the oppositely magnetized line QQ′ of magnetic moment $-\mathrm{M}e^{-t\mathrm{R}/a}$, and for inside points the magnetized line PP′ having its positive poles at P and of magnetic moment $\mathrm{M}e^{t\mathrm{R}/a}a^2/b^2$, b being now taken $= \mathrm{OB}$.

7. If a line of poles of strength m suddenly changes position from A′ to A the effect is equivalent to the superposition of $-m$ at A′ and $+m$ at A, and is thus equivalent to the generation of a line of magnets in the direction AA′, the images of which have just been found.

Fig. 3 shows the application of this method to construct the trails of images of a line of poles revolving uniformly about the centre and passing in succession through the positions A_1, A_2, A_3, A_4, . . ., at equal small intervals of time, the actual trails are of course the limits of the series of moving poles thus plotted out. By supposing the line of poles to remain fixed and the cylinder to rotate, we get a kind of cylindrical two-dimensional Arago's disk, and it is to be noticed that the trails of images lie along equiangular spirals

Fig. 3.



(as may readily be verified), the intensity of magnetization of the trail decreasing toward the centre and increasing outward and being, in fact, proportional to the distance from the centre.

PART III. SPHERICAL SHEETS.

Image due to Generation of a Small Magnet whose Axis is Radial outside a Spherical Sheet.

8. We now consider the problem presented by the currents induced in a spherical sheet of radius a when a magnet is generated at a distance b from the centre, the axis of the magnet passing through the centre of the sphere. Taking this axis as the axis of zonal spherical harmonics, and starting in the first instance with a magnet of variable moment $f(t)$, (the positive pole being furthest from the centre), the magnetic potential Ω_0 at a point (r, θ) is given by

$$\Omega_0 = f(t)\frac{d}{db}\frac{1}{\sqrt{(r^2+b^2-2rb\cos\theta)}}.$$

When $r<b$ this becomes

$$\begin{aligned}\Omega_0 &= \quad f(t)\frac{d}{db}\Sigma_{n=0}^{n=\infty}\frac{r^n}{b^{n+1}}\mathrm{P}_n(\cos\theta)\\ &= -f(t)\,\Sigma_{n=0}^{n=\infty}\frac{(n+1)r^n}{b^{n+2}}\mathrm{P}_n; \quad . \quad . \quad . \quad . \quad (9)\end{aligned}$$

and when $r>b$ we have in like manner

$$\Omega_0 = f(t)\,\frac{d}{db}\sum_{n=0}^{n=\infty}\frac{d}{db}\,\frac{b^n}{r^{n+1}}\,\mathrm{P}_n(\cos\theta)$$

$$= f(t)\sum_{n=1}^{n=\infty}\frac{nb^{n-1}}{r^{n+1}}\,\mathrm{P}_n. \qquad (10)$$

Considering the case where the magnet is outside the sphere, we must take the first expression, since at the surface of the sphere $r<b$.

Let the potentials due to the induced currents at points inside and outside the sphere be assumed of the form

$$\Omega' = \Sigma(n+1)\mathrm{A}\,\frac{r^n}{a^{n+1}}\,\mathrm{P}_n \text{ (inside)}, \qquad (11)$$

$$\Omega'' = -\Sigma n\mathrm{A}_n\,\frac{a^n}{r^{n+1}}\,\mathrm{P}_n \text{ (outside)}, \qquad (12)$$

where the coefficients A_n are functions of the time t; the assumed forms being such as to satisfy the surface-condition $d\Omega'/dr = d\Omega''/dr$.

Substituting the expressions for the potential, $\Omega_0+\Omega'$ and $\Omega_0+\Omega''$ for Ω_1 and Ω_2 in the surface-conditions (2),

$$\frac{d}{dt}\left(r^2\frac{d\Omega_1}{dr}\right) = \frac{d}{dt}\left(r^2\frac{d\Omega_2}{dr}\right) = \frac{\mathrm{R}}{2}\,\frac{d}{dr}\,r^2\,\frac{d}{dr}(\Omega_2-\Omega_1),$$

we find, on equating coefficients of P ,

$$\frac{d\mathrm{A}_n}{dt} + \frac{(2n+1)\mathrm{R}}{2a}\,\mathrm{A}_n = f'(t)\,\frac{a^{n+1}}{b^{n+2}},$$

$$\therefore \quad \frac{d}{dt}\{\mathrm{A}_n e^{(2n+1)t\mathrm{R}/2a}\} = f'(t)\,e^{(2n+1)t\mathrm{R}/2a}\,\frac{a^{n+1}}{b^{n+2}},$$

$$\therefore \quad \mathrm{A}_n = \int_{-\infty}^{t} f'(\tau)\,e^{-(2n+1)(t-\tau)\mathrm{R}/2a}\,d\tau\,.\,\frac{a^{n+1}}{b^{n+2}}.$$

Now let the inducing system be a magnet of moment M suddenly generated at the time $t=0$, so that

$$f(t) = 0 \text{ when } t<0,$$
$$f(t) = \mathrm{M} \text{ when } t>0.$$

Then by a similar artifice to that used for the cylindrical sheet, we have

$$\mathrm{A}_n = \mathrm{M}e^{-(2n+1)t\mathrm{R}/2a}\,\frac{a^{n+1}}{b^{n+2}}.$$

Substituting in the potentials of the induced currents, we have

$$\Omega' = \mathrm{M}\Sigma(n+1)e^{-(2n+1)t\mathrm{R}/2a}\frac{r^n}{b^{n+2}}\mathrm{P}_n$$

$$= \mathrm{M}e^{3t\mathrm{R}/2a}\,\Sigma\frac{(n+1)r^n}{(e^{t\mathrm{R}/a}b)^{n+2}}\mathrm{P}_n; \quad . \quad . \quad . \quad . \quad (13)$$

$$\Omega'' = -\mathrm{M}\Sigma n e^{-(2n+1)t\mathrm{R}/2a}\frac{a^{2n+1}}{b^{n+2}r^{n+1}}\mathrm{P}_n.$$

$$= -\mathrm{M}\frac{a^3}{b^3}e^{3t\mathrm{R}/2a}\Sigma\frac{n(e^{-t\mathrm{R}/a}a^2/b)^{n-1}}{r^{n+1}}\mathrm{P}_n. \quad . \quad (14)$$

Comparing the first of these results with (9) and the second with (10), we have the following conclusions:—

(i.) At time t after the generation of the magnet, the potential due to the induced currents is, at points inside the sphere, equivalent to that produced by a magnet of moment $-\mathrm{M}e^{3t\mathrm{R}/2a}$ at a distance from the centre of $e^{t\mathrm{R}/a}b$.

(ii.) At points outside the sphere, the potential is equivalent to that of a magnet of moment $-\mathrm{M}e^{-3t\mathrm{R}/2a}a^3/b^3$ at a distance from the centre of $e^{-t\mathrm{R}/a}a^2/b$. The positions P, Q occupied by these images at time t are always inverse points.

It is easily seen that the intensities of the images at P and Q are to each other and to that of the inducing magnet as $\mathrm{OP}^{\frac{3}{2}} : \mathrm{OQ}^{\frac{3}{2}} : \mathrm{OA}^{\frac{3}{2}}$, that is in the sesquiplicate ratio of their distances from the centre.

Images due to Generation of a Small Magnet inside a Spherical Sheet.

9. If $b<a$ or the magnet is generated within the sphere, we must expand the potential in the second form (10) in order to substitute in the surface-conditions. When this is done, it will be found that

$$\mathrm{A}_n = \mathrm{M}e^{-(2n+1)t\mathrm{R}/2a}\frac{b^{n-1}}{a^n}$$

and the conclusions finally arrived at are as follows:—

(i.) If a small magnet of moment M be instantaneously generated at a point B inside the sphere at a distance b from the centre, the axis of the magnet being radial; then the image representing the magnetic potential of the induced currents outside the sphere at time t is a magnet of moment $-\mathrm{M}e^{-3t\mathrm{R}/2a}$ at a point Q such that $\mathrm{OQ} = be^{-t\mathrm{R}/a}$.

(ii.) At points inside the cylinder the potential is that which would be produced by a magnet of moment $-\mathrm{M}e^{3t\mathrm{R}/2a}a^3/b^3$ at a distance from the centre of $e^{t\mathrm{R}/a}a^2/b$.

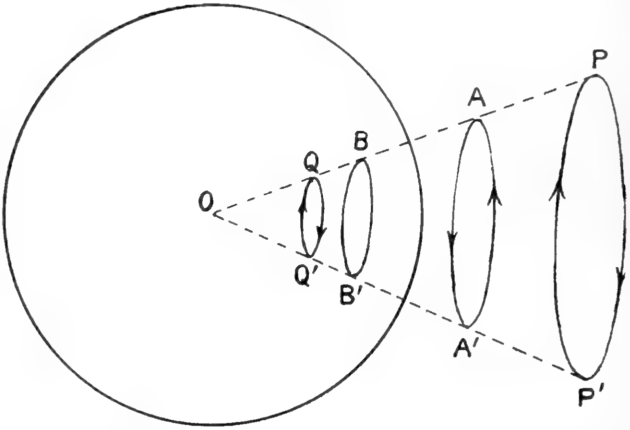
As before, the intensities of the several magnets vary as the $\frac{3}{2}$ power of their distance from the centre.

Images due to Generation of Circular Currents coaxial with a Spherical Sheet, &c.

10. A circular current whose axis passes through the centre of the sphere is equivalent to a magnetic shell forming a spherical cap on the surface of a concentric sphere, this cap being bounded by the current. It is thus evident that the images due to the sudden generation of such a current are also circular currents, which move in the manner described in the preceding sections. There is no need to restrict the results to circular currents as the same arguments are equally applicable when the inducing current flows in any closed curve traced on a spherical surface concentric with the sheet, the images in this case being currents in similar curves. It remains to state the relation between the intensities of the currents in the images and that of the inducing current.

(i.) Let AA′ (fig. 4) be the wire in which an inducing current of intensity I is suddenly generated *outside* the sheet. Then at time t the image for inside points will be a current in PP′ and for outside points a current in QQ′, where $\mathrm{OP}=e^{t\mathrm{R}/a}\mathrm{OA}$ and $\mathrm{OQ}=e^{-t\mathrm{R}/a}a^2/\mathrm{OA}$. Suppose the currents replaced by their equivalent magnetic shells on spherical surfaces centred at O, and let corresponding elements of these shells be taken. The magnetic moments of the elements of PP′, QQ′, and AA′ are respectively as $\mathrm{OP}^{\frac{3}{2}} : \mathrm{OQ}^{\frac{3}{2}} : \mathrm{OA}^{\frac{3}{2}}$; and the areas of these elements are as $\mathrm{OP}^2 : \mathrm{OQ}^2 : \mathrm{OA}^2$; hence the strengths of the shells are as $\mathrm{OP}^{-\frac{1}{2}} : \mathrm{OQ}^{-\frac{1}{2}} : \mathrm{OA}^{-\frac{1}{2}}$, and the current-intensities

Fig. 4.



are in the same ratio. Thus finally, if I′ and I″ be the intensities of the image-currents at time t in PP′ and QQ′, we have

$$\mathrm{I}' = -\left(\frac{\mathrm{OP}}{\mathrm{OA}}\right)^{-\frac{1}{2}}\mathrm{I} = -e^{-t\mathrm{R}/2a}\,\mathrm{I},$$

$$\mathrm{I}'' = -\left(\frac{\mathrm{OQ}}{\mathrm{OA}}\right)^{-\frac{1}{2}}\mathrm{I} = -e^{t\mathrm{R}/2a}\,\frac{b}{a}\,\mathrm{I}.$$

It will be noticed that I″ increases as its circuit QQ′ approaches the centre, while I′ decreases as its circuit PP′ recedes to infinity.

(ii.) Next taking the inducing current I to be suddenly generated in BB′ *inside* the sphere, and taking $\mathrm{OB}=b$, $\mathrm{OQ}=e^{-t\mathrm{R}/a}b$, and $\mathrm{OP}=e^{t\mathrm{R}/a}a^2/b$, we find that at time t, the image representing the potential of the induced currents at points outside the sphere is a current in QQ′ of intensity

$$-e^{t\mathrm{R}/2a}\,\mathrm{I},$$

and at points inside the sphere a current in PP′ of intensity

$$-e^{-t\mathrm{R}/2a}\,\frac{b}{a}\,\mathrm{I}.$$

Images due to Generation of Magnetic Pole outside a Spherical Sheet.

11. A magnetic pole m generated at time 0 outside a sphere at the point $r=b$ where $b>a$ might be regarded as equivalent to a line of elements uniformly magnetized in the direction of the radius-vector extending along that radius from $r=b$ to infinity; and similarly a positive pole m inside the sphere at $r=b$ when $b<a$ together with a negative pole $-m$ at the centre might be regarded as equivalent to a line of elements uniformly magnetized in the line joining these points. In this way the images might be obtained by integration. Practically as short a way of obtaining the result as any is by utilizing the work of §§ 8, 9, taking into account the differences introduced into the coefficients of the various harmonics when for the inducing potential of a magnet there is substituted that of a pole.

When the pole is outside the sphere the inducing potential after its generation becomes

$$\Omega_0 = m\Sigma\frac{r^n}{b^{n+1}}\,\mathrm{P}_n \quad (r<b). \quad . \quad . \quad . \quad . \quad (15)$$

The coefficient of P_n is $-b/(n+1)$ of the corresponding coefficient for a magnet of moment m.

(i.) Hence we find for the potentials of the induced currents at points inside the sphere at time t

$$\Omega' = -m\Sigma e^{-(2n+1)t\mathrm{R}/2a}\frac{r^n}{b^{n+1}}\mathrm{P}_n$$

$$= -me^{t\mathrm{R}/2a}\Sigma\frac{r^n}{(be^{t\mathrm{R}/a})^{n+1}}\,\mathrm{P}_n. \quad . \quad . \quad . \quad (16)$$

This is the potential of a pole of intensity $-me^{t\mathrm{R}/2a}$ at a distance from the centre of $be^{t\mathrm{R}/a}$.

(ii.) For points inside the sphere the corresponding potential is

$$\Omega'' = m\Sigma\frac{n}{n+1}\,e^{-(2n+1)t\mathrm{R}/2a}\frac{a^{2n+1}}{b^{n+1}r^{n+1}}\mathrm{P}_n$$

$$= m\frac{a}{b}\,e^{-t\mathrm{R}/2a}\Sigma\,\frac{(e^{-t\mathrm{R}/a}a^2/b)^n}{r^{n+1}}$$

$$= m\frac{a}{b}\,e^{-t\mathrm{R}/2a}\Sigma\,\frac{n}{n+1}\frac{(e^{t\mathrm{R}/a}a^2/b)^n}{r^{n+1}}\,\mathrm{P}_n.$$

Let $\rho = e^{-t\mathrm{R}/a}a^2/b$. Then

$$\Sigma\frac{n}{n+1}\frac{\rho^n}{r^{n+1}}\,\mathrm{P}_n = \Sigma\frac{\rho^n}{r^{n+1}}\,\mathrm{P}_n - \frac{1}{\rho}\;\Sigma\frac{\rho^{n+1}}{(n+1)r^{n+1}}\mathrm{P}_n$$

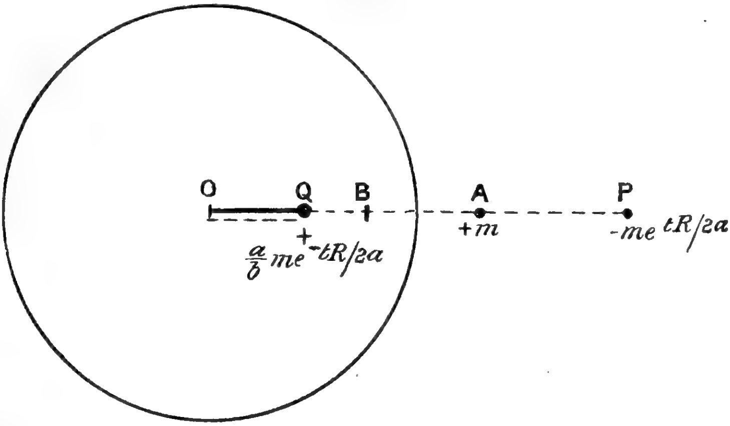
$$= \Sigma\frac{\rho^n}{r^{n+1}}\mathrm{P}_n - \frac{1}{\rho}\int_0^\rho \Sigma\,\frac{\rho'^n}{r^{n+1}}\mathrm{P}_n\,d\rho'$$

$$= \frac{1}{\sqrt{(r^2-2r\rho\cos\theta+\rho^2)}} - \frac{1}{\rho}\int_0^\rho\frac{d\rho'}{\sqrt{(r^2-2r\rho'\cos\theta+\rho'^2)}}$$

$$\therefore\ \Omega'' = \frac{me^{-t\mathrm{R}/2a}a/b}{\sqrt{(r^2-2r\rho\cos\theta+\rho^2)}} - \frac{m}{a}e^{t\mathrm{R}/2a}\int_0^\rho\frac{d\rho'}{\sqrt{(r^2-2r\rho'\cos\theta+\rho'^2)}}. \quad . \quad . \quad . \quad (17)$$

The first part represents the potential of a pole of strength $me^{-t\mathrm{R}/2a}a/b$ at a distance ρ from the centre, *i.e.* at the point Q of the previous sections, and the second represents the potential of a line-distribution of magnetism of uniform linear density $-me^{t\mathrm{R}/2a}/a$ extending from the centre to Q; this being equal to the strength of the pole divided by ρ or OQ, it follows that the total quantity of magnetism on this line OQ is equal and opposite to that in the pole Q (fig. 5). This distribution of images is related to the image on the opposite side in the same manner as the hydrodynamic image of a source in a sphere is related to the source, except in the matter of algebraic sign. The reason for the resemblance is that the two potentials here have to make $d\Omega'/dr = d\Omega''/dr$ at the surface, while in the hydrodynamic problem the portions of the normal velocity components $d\phi/dr$ due to the source and its image have to cancel one another.

Fig. 5.



Images due to Generation of Magnetic Poles inside a Spherical Sheet.

12. If a pole m be generated inside a sphere at the point $r=b$ where $b<a$, together with an equal and opposite pole $-m$ at the centre, the inducing potential takes the form

$$\Omega_0=m\Sigma_{n=1}^{n=\infty}\frac{b^n}{r^{n+1}}\mathrm{P}_n. \quad . \quad . \quad . \quad . \quad . \quad . \quad (18)$$

(i.) The induced potential outside the sphere takes the form

$$\Omega''=-me^{-t\mathrm{R}/2a}\Sigma_{n=1}^{n=\infty}\frac{(be^{-t\mathrm{R}/a})^n}{r^{n+1}}\mathrm{P}_n. \quad . \quad . \quad . \quad . \quad (19)$$

This is the potential of a pole of intensity $-me^{-t\mathrm{R}/2a}$ at a distance from the centre of $be^{-t\mathrm{R}/a}$, together with an equal and opposite pole at the centre.

(ii.) The induced potential inside is

$$\Omega'=m\Sigma\frac{n+1}{n}e^{-(2n+1)t\mathrm{R}/2a}\frac{r^nb^n}{a^{2n+1}}\mathrm{P}_n.$$

If $\rho=e^{t\mathrm{R}/a}a^2/b$, this becomes

$$\Omega'=m\frac{a}{b}e^{t\mathrm{R}/2a}\Sigma\frac{n+1}{n}\frac{r^n}{\rho^{n+1}}\mathrm{P}_n$$

$$=m\frac{a}{b}e^{t\mathrm{R}/2a}\left\{\Sigma\frac{r^n}{\rho^{n+1}}\mathrm{P}_n-\frac{1}{\rho}\int_\rho^\infty\Sigma\frac{r^n}{\rho'^{n+1}}d\rho'\right\}. \quad . \quad (20)$$

The images thus consist of a positive pole of strength $me^{t\mathrm{R}/2a}a/b$ at a distance from the centre ρ equal to $e^{t\mathrm{R}/a}a^2/b$

together with a line-distribution of magnetism of linear density $-me^{-t\mathrm{R}/2a}/a$ extending from distance ρ to infinity. This linear density is equal to the strength of the pole-image divided by its distance ρ.

Other Images in a Spherical Sheet.

13. When the inducing potential is due to a small *magnet whose axis does not pass through the centre* of the sphere we could of course find the images by considering the positive and negative poles separately. If the axis of the magnet makes an angle α with the radius-vector the simplest plan would be to replace it by two magnets of moment $\mathrm{M}\cos\alpha$ and $\mathrm{M}\sin\alpha$ along and perpendicular to the radius.

14. The images due to *a pole of constant strength moving along the radius-vector* and initially at rest will be trails of magnets which can be worked out by employing the simpler images of §§ 10 and 11. If, initially, the pole were *suddenly generated*, we should have to introduce the more complicated images of §§ 11 and 12, but this may be avoided by considering the problem in which the pole is supposed to have remained at rest from an infinite time past (or, at any rate, so long that all currents in the sheet have died out), and investigating only the effects due to its motion. As usual, any small displacement may be represented by putting down a magnet of which one pole cancels the moving pole in its old position, while the other is identical with the moving pole in its new position. If the pole moves in a direction other than radial, the more complicated images of §§ 11 and 12 will of course be introduced.

GENERAL CONCLUSIONS.

1. The phenomena of two-dimensional induction in cylindrical sheets and of induction in spherical sheets due to a sudden disturbance in the magnetic field can be represented by moving trails of images which are generally not much more complicated than those obtained with a plane sheet.

2. The images in every case start from the source of disturbance and its inverse, and move away from the sheet radially with velocities varying directly as the distance, the constant of variation being such as to make the velocity at the surface of the sheet become equal to R, the corresponding velocity for a plane sheet of the same thickness and conductivity. The image which initially coincides with the source of disturbance initially neutralizes its effect on the opposite side of the sheet.

3. Where the source of disturbance is a distribution of

poles or an unclosed current *inside* the sheet it is necessary from physical considerations to place equal and opposite poles at the centre or to close the circuit by axial return currents, and this is also essential for the success of the image method.

4. The relations between the intensity of the source of disturbance and those of the images will readily be found to be in every case expressible in terms of their distances from the centre. The results expressed in this way may be tabulated as follows :—

Sheet.	Inducing disturbance.	Law of variation.
Cylinder.	Line of poles parallel to axis.	Constant.
,,	Current parallel to axis.	,,
,,	Line of magnets.	$\propto$ distance.
Sphere.	Magnet radially magnetized.	$\propto$ (distance)$^{\frac{3}{2}}$.
,,	Concentric magnetic shell. Current on concentric sphere.	$\propto$ (distance)$^{\frac{1}{2}}$.
,,	Magnetic pole (*with exception*).	$\propto$ (distance)$^{\frac{1}{2}}$.

5. The last-named exceptional case is that of a spherical sheet when the disturbance is due to the generation of a pole, and it is required to represent by images the potential of the currents on the same side as the disturbing pole, the images thus being on the opposite side of the sheet. Here, in addition to the single pole whose intensity and position are given by the above statements, the image consists of a line-distribution of magnetism of opposite sign, extending from that pole along the radius away from the sheet, and having its line-density equal to the quantity of magnetism in the pole or image divided by its distance from the centre.

6. When the source of disturbance is in motion, the trails of images can be built up synthetically as in the case of a plane sheet. If the source of disturbance is rotating uniformly about the centre of the cylinder or sphere, the curves described by the trails of images will be equiangular spirals.

XLIII. *On Volta Electricity of Metals.* *By* J. ERSKINE-MURRAY, *D.Sc., F.R.S.E., Heriot-Watt College*[*].

I. *Introductory.*

§. 1. THE experimental investigation described in this communication had as its primary object the elucidation and measurement of the variations of volta contact electricity of a pair of conductors, due to changes in the state of *that portion of the surface of each conductor which was separated from the other conductor by an insulating medium.*

§ 2. The discovery of contact electricity of dry metals in air by Volta at the beginning of the present century, extended a quarter of a century later by Pfaff to dry varnished metals in other gases, has been confirmed by many subsequent experimenters. The reality of the electrostatic force in air near an interface between copper and zinc, inferred as an obvious consequence from it by Lord Kelvin, was experimentally demonstrated by him in 1861[†]. In the next twenty years many investigations were made, the more important being those of Hankel, Gerland, Clifton, Ayrton and Perry, and von Zahn. In 1881 a paper of great importance was published by M. Pellat[‡], and as the present paper is, in some respects, only an amplification and extension of his work, it may be of advantage to give a short *résumé* of it before going further.

M. Pellat's most important results appear to be (1) his demonstration of the influence of the physical condition of the metallic surfaces on their volta-potential ; thus he found that a sharply scratched plate is positive to a more smoothly polished one of the same metal, the metals being washed with alcohol after polishing in both cases and allowed to dry before the measurement of their potential : (2) his measurements of

---

[*] Communicated by Lord Kelvin.

[†] Electrostatics and Magnetism, § 400 *et seq.*

[‡] *Ann. de Chimie et de Physique,* 1881.

the temporary variations of potential due to change of temperature of a copper, iron, or zinc plate: and (3) his experiments which prove the smallness of the changes produced in the potential-difference of copper and zinc by varying either the pressure or the nature of the gas surrounding them.

§ 2*a*. I shall now give a short summary of the results detailed in the present paper:—

(*a*) Metals covered with nonconducting solid films of wax or glass, except at their point of contact, give nearly the same potential as the bare metals in air. The substitution of wax for air next the metal only causes a small change which may be in the same direction and of approximately equal amounts for metals whose potentials in air are very different; *e. g.* the volta-potential-difference between zinc and copper when both are coated with solid paraffin-wax is very nearly the same as that between bare zinc and copper. In this connexion I may mention that I have measured the potential of sodium coated with wax and glass, and find it to be about 3·56 volts positive to a standard gold plate (see Chap. X.).

(*b*) A metal, cleaned by careful polishing and scratching with emery-cloth or glass-paper is less positive when its surface is in a sharply scratched condition than when smoothed or burnished, the difference frequently amounting to ·2 or ·3 of a volt. This result is not in opposition to that of M. Pellat, mentioned above, § 2 (1), for the conditions were different, as his plates were washed with alcohol after polishing while mine were not (see Chap. III. and also §§ 39 *et seq.*).

(*c*) The temperature variations, between 15° C. and 60° C., of the volta-potential of many metals have been determined, both for clean dry metals in air and for metals coated with liquid or nonconducting solid films; and it has been found that they are of considerable magnitude in both cases. The curves representing the variation of potential with temperature appear as if they should meet at a point below −200° C., at a potential about ·4 volt positive to a standard gold plate at 16° C.; this suggests that volta-potential-differences may possibly vanish at a very low temperature (see Chap. IX.).

(*d*) A liquid film, even if of extreme thinness, may cause a considerable change in the potential of a dry polished plate which continues permanent for many hours and even days after the disappearance of the film.

Two films of the same liquid opposed to one another on the surfaces of two plates of different metals do not usually give zero potential-difference, as solid conducting films of one material would do, but give nearly the same potential-difference as the dry metals on which they lie (see Chaps. VI. and VII.).

(*e*) A very thin film of oxide on a metal produces only a very small change in the potential, and every increase in thickness of the film is attended by a further change in potential until a limiting value is reached, which is that of a mass of the oxide (see Chap. IV.).

(*f*) Exposure to the atmosphere at ordinary temperatures does not as a rule produce any rapid change in volta-potential, especially if the air be comparatively dry and free from dust. The ultimate change is usually in the negative direction (see Chap. V.).

(*g*) I have extended Lord Kelvin's experiments on the effect of temporary immersion of a metal in a gas to the cases of copper, zinc, tin, and silver in oxygen, and find that copper, zinc, and silver become temporarily positive, while tin becomes negative in consequence of this treatment (see Chap. VIII.).

The research, suggested by Lord Kelvin, was carried out in the Natural Philosophy Laboratory of Glasgow University during the Sessions 1893–94–95, and during 1895–96 in the Cavendish Laboratory of Cambridge University. My thanks are due to Lord Kelvin for many suggestions and much valuable advice, both in regard to experiments and to the discussion of results; and to Professor J. J. Thomson for similar kindnesses during my work in the Cavendish Laboratory. A small portion of this investigation, on the effect of Röntgen *x*-rays on the contact electricity of metals, was published in the Proceedings of the Royal Society for March 1896.

I have also to thank Professor James Holm, M.A.[*], and Mr. George E. Allan, B.Sc., for the part they took in the earlier portion of the work.

II. *Method of Experiment.*

§ 3. The measurement of the natural potential-difference between any pair of conductors was usually made, in air, by the null method described very briefly by Lord Kelvin in the Report of the British Association for 1880, p. 494. To make clear the exact circumstances in which the potentials were measured it will be advantageous to describe the apparatus and general method of experiment in detail.

§ 4. A circular disk of one of the metals, usually about

---

[*] When this was written last August my friend and former fellow-worker was Professor of Applied Mathematics in the South-African College, Cape Town. He died in October.

9 centim. in diameter and ·2 centim. thickness, is insulated and in permanent connexion with the insulated pair of quadrants of an electrometer. A similar plate of the other metal placed parallel to the first, at a distance of a few millimetres from it, is uninsulated and in connexion with the uninsulated pair of quadrants. While in this position they are temporarily connected with one another through a simple form of potential-divider in which a slope of potential is maintained by a Daniell cell, in the direction opposite to that given by the experimental plates. The temporary connexion is now broken and the plates are separated. In doing so the capacity of the condenser is reduced, hence if there be any electric charge on them it will be indicated by a further deflexion of the electrometer. This operation is repeated with different values of the counterpotential until separation of the plates produces no change in the deflexion of the electrometer. Since it has annulled the charge, the counterpotential must now be equal and opposite to the natural potential-difference.

§ 5. During most of the experiments a piece of apparatus made by Lord Kelvin in or about 1861 was used. In it the plates were surrounded by a cylindrical zinc case. This was made in two parts for convenience in manipulation, the lower fixed to a heavy cast-iron base plate, while the upper stood upon small brackets attached to the top edge of the lower part, its position being regulated by a hole-slot-and-plane arrangement. Openings in the sides of the upper part facilitated the adjustment of the experimental plates, but these were closed by a sliding cover during a measurement of potential.

The lower plate was supported by an insulating glass stem which was kept dry by means of pumice and sulphuric acid. On the top of this stem was a brass cap screwed to fit the sockets which were soldered to the backs of the plates, and from the brass cap a stiff wire passed out through a hole in the case and formed the connexion between the insulated plate and the electrometer.

A piece of platinum foil was soldered round a part of this wire to give a clean contact for the temporary connexion between the plates through the potential-divider, and the wire from the divider had likewise a platinum end piece.

The upper plate hung, by a ring at the centre of its back, on a hook at the lower end of a vertical metal rod which could be drawn up so as to increase the distance between the plates. A small disk fixed on the rod at right angles to it just above the hook had three screws passed through it and pressed against the plate, holding it firmly in the hook; by this means the upper plate can be set parallel to the lower one in a very short time. This apparatus, which is the same as that used by Lord Kelvin in his experiments many years ago, was found to be very convenient, the arrangements enabling us to take out and replace a plate very rapidly, a matter of great importance when observing temporary variations, while at the same time both plates were firmly attached to their supports when in position.

§ 6. The divider, whose total resistance was about 2400 ohms, was made to divide the potential-difference between its outer terminals into one hundred equal parts. The potential applied was usually that given by a Daniell cell of Lord Kelvin's gravity type, and was tested frequently by comparison with a standard Clark cell, or by means of a standard resistance and a current balance.

§ 7. The permanent connexions were as follows:—(1) The lower plate to the insulated pair of quadrants of the electrometer; (2) The terminals of the Daniell cell to the ends of the divider (there was a reversing plug in this circuit); (3) One end of the divider to the upper plate and uninsulated quadrants of the electrometer.

The temporary connexion (§ 5) was from the sliding contact-piece of the divider to the lower plate.

§ 8. Each experiment was as a rule begun by polishing a metal plate on clean glass-paper or emery-cloth. Its contact potential with a standard plate, generally of electrolytically deposited gold washed some hours previously with alcohol, as used by M. Pellat, was then measured. The plate was next subjected to some particular treatment; for instance it was filed, or burnished, or polished on leather or paper, or washed with water, alcohol, or turpentine, or heated in air and oxidized, or not oxidized, or exposed to steam, or oxygen, or fumes of iodine or hydrogen sulphide, or simply left to alter under the influences of the atmosphere and its own molecular forces. Its potential with the same standard plate was again measured, and the change due to the treatment its surface had undergone noted.

§ 9. The metal which requires to be joined to the zinc end of the battery, in order to effect a balance, will be called *positive*. Thus aluminium is positive to zinc and zinc positive to copper. Also, when a plate becomes more positive its potential will be said to *rise*; when more negative, to *fall*. When the "potential" of a plate is mentioned, without other qualification, its contact potential-difference with a standard plate (§ 8) is meant.

§ 10. The results given are not in the order in which they were obtained, but are classified in such a way as to show more clearly their true import, and those of many experiments which are not mentioned in the text will be found in the tables.

III. *Effects of different Methods of Cleansing the Metallic Surfaces.*

§ 11. It seems probable that the very conflicting results obtained by different experimenters for the potential-difference of any given pair of metals, in air, must be due, to a large extent, to differences in the methods by which the surfaces have been prepared. In order to obtain a metallic surface as free as possible from all contamination it is clear that a *hard* polishing agent, such as a clean steel file, or emery-cloth, or glass-paper, must be used, as a softer material leaves more of itself on the plate. A liquid is quite unsuitable, since every particle of the metal probably retains particles of the liquid adhering to it. Thus the smell of a liquid remains long after the plate appears to be quite dry; and it has been found that such a film as must exist to cause the odour is quite sufficient to alter the potential very considerably.

§ 12. In order to obtain uniformity of action the clean glass-paper or emery-cloth used to polish a plate was fixed on a wooden roller made to revolve with a circumferential velocity of about 100 centim. per second. Care was taken to hold the plate so that the scratches, caused by the polisher, should all be parallel. Thus little or no grit could lodge in the surface, which would have occurred had the scratches crossed one another. A record was kept of the nature of the polishing agent in every case. A piece of glass-paper, or other polisher, was seldom used more than once or twice, and was never used for any different metal.

§ 13. M. Pellat found that every change in the smoothness of a surface is accompanied by a change in its contact potential; but as his experiments were limited to metals washed with alcohol, it was of interest to extend them to more general cases. Thus a plate of zinc which had been polished on clean glass-paper, and had therefore a surface sharply scratched in parallel lines, was found to be

·70 volt

positive to the standard gold plate. It was next burnished with a tool of hardened steel, and with the same standard plate it now gave

·94 volt.

Two hours later the same plate gave

·92 volt,

showing that the effect is nearly permanent. By burnishing the plate again a finer polish was obtained. With the same standard the potential is now

1·00 volt,

still further burnishing giving

1·02 volt.

If the zinc be now polished on glass-paper it returns to its original potential. Now steel, of which the burnisher was made, is negative to zinc; hence this rise cannot have been due to particles of steel on the zinc, but must have been caused by some change involved in the smoothing process to which the zinc had been subjected, possibly by a hardening of the surface layer.

§ 14. Intermediate states of polish give intermediate values of potential. For instance, if a zinc plate be filed it will not be so sharply scratched as it would be by glass-paper, and of course not so smooth as if burnished. Its potential is also found to be between those of scratched and burnished zinc. Each polishing, with any given material, was usually sufficiently thorough to efface the effects of all previous polishings; the potential observed thus depended only on the state of surface produced by the polisher used just before the observation. The results of experiments show that the smoother the surface the more positive it becomes.

§ 15. The generalization given in § 14 is supported by the results of over one hundred similar experiments with a number of metals. The actual results are given in Table I. (pp. 406, 407); it will be well, however, to discuss some of these in greater detail than is possible in tabular form.

§ 16. In order to eliminate the use of different polishing agents, I tried the effect of producing different states of surface by rubbing the plate against another of the same metal. Two copper plates which had been polished on glass-paper gave

(*a*) +·01 volt,
(*b*) +·04 volt,

with the standard plate. They were next gently rubbed together until parts of each were shiny, and now gave, with the same standard,

(*a*) +·07 volt,
(*b*) +·07 volt.

In another experiment two coppers which had been polished on medium emery-cloth (Davies's No. $1\frac{1}{2}$) gave, with the standard plate,

(*a*) $-\cdot 11$ v.
(*b*) $-\cdot 06$ v.

When slightly burnished by rubbing them together, they gave

(*a*) $-\cdot 02$ v.
(*b*) $-\cdot 02$ v.

Thus, smoothing by mutual friction made both more positive. The amount of the change is not great, either in potential or in smoothness, as it is very difficult to polish copper on copper, but the direction of change is the same as previously found, namely, a smooth surface is more positive than a sharply scratched one.

§ 17. On account of the difficulty of obtaining a burnished surface by simply rubbing two pieces of the same metal together, I tried another form of experiment in which the possible effect of the material of the polishing agent is eliminated by using the same tool to produce different states of surface. Thus a copper plate was scratched very roughly with a steel tool, and gave

$-\cdot 07$ v.

with the standard plate, then burnished slightly with the same tool it gave

$-\cdot 02$ v.

It had thus risen ·05 v. on account of the alteration produced in smoothing its surface. Thus these results also confirm the conclusion given in § 14.

§ 18. It should be noticed that although individual results occasionally appear to conflict, the average value of the potential of any given metal when polished with a certain agent is almost always in proportion to the smoothness of the surface produced. However much one may try, it is impossible to get *exactly* the same state of surface over again, but by taking the average of a considerable number of experiments with one polishing agent, a general value is got which represents the potential given by the plate in a certain state of polish. It is so arranged that the sharply scratched surfaces come at the head of the table, and each succeeding polishing agent in the list produces a smoother surface.

§ 19. It should be observed that with soft metals such as tin, different polishers produce but little change in the contact potential. This, though at first sight apparently contrary to

TABLE I.

NOTE.—The potentials were taken with a standard gold plate (see § 8), which had been washed with alcohol and allowed to dry in air.

Polishing Agents in order of sharpness of surface produced.	COPPER.						TIN.		ZINC.			ALUMINIUM.		
	Old Hard Plates.			High Conductivity.										
	Potential in Volts.	Average Potential.	Remarks.	Potential in Volts.	Average Potential.	Remarks.	Potential in Volts.	Average Potential.	Potential in Volts.	Average Potential.	Remarks.	Potential in Volts.	Average Potential.	Remarks.
Emery-cloth, 'Fine'	−·19 −·21	−·20												
" 'Medium'	−·19 −·19 −·24 −·20	−·20		+·02 ·00 −·10 −·06 +·02	−·02									
Glass-paper, 'Fine'...	−·24 −·19	−·21		+·11 +·01 +·04	+·05		+·55 +·63 +·59 +·63	+·60	+·66 +·65 +·67 +·69 +·63 +·72 +·75 +·60 +·63 +·72 +·86 +·65	+ ·67 ………… + ·70	Rather smoother than last.	+1·11 +1·04 +1·13 +1·15 +1·21	+1·13	
" 'Medium'	−·14 −·21 −·12 −·11 −·08 −·07 −·11	−·12	Consecutive polishings on same paper.											

Glass-paper, 'Coarse.'	−·06 −·09 −·11	−·08												
File	−·10 −·11 −·12 −·05	−·08		+·05	+·05		+·59	+·59	+ ·76 + ·92 + ·84 + ·81	+ ·80				
									+ ·75	…………	Rather rougher than last.			
									+ ·71	…………	Rather rougher than last.			
Glasspaper which had been worn smooth.	−·00 −·12 +·20	+·02		+·07 +·05 +·08 +·05	+·06							+1·21	+1·21	
Steel burnishing tool.	+·06 +·08 +·08 +·07 +·07 +·11	+·08		+·05 +·06 +·08 +·11 +·11 +·11	+·09	These were consecutive experiments. The plate was burnished before each observation.	+·57	+·57	+ ·90 + ·94 + ·98 +1·02 + ·94	+ ·96	Very bright surface. Finer polish. These four exps. were consecutive.	+1·17 +1·27 +1·25	+1·23	Burnished with glass knob.
Washleather (after glass-paper).	+·15 +·23 +·21 +·06	+·16							+1·00	+1·00		+1·26 +1·22	+1·24	

the results for harder metals, is really in accordance with them, for different polishers do *not* produce appreciably different states of polish on a soft metal. This remark applies equally to the fact that a smaller variation was observed with "high conductivity" copper than with the old copper plate, as the old plate was found to be considerably harder than the purer "high conductivity" copper.

§ 20. It is probable that the variation described in this chapter is not directly due to roughness or smoothness, but rather to alteration, either by change in the outer layer of the metal, or in the film of condensed air which no doubt exists on it, of the interface between metal and air.

IV. *Thin Solid Films of Oxides, Iodides, &c.*

§ 21. Among the earlier experiments were many in which the changes due to films of oxide of different thicknesses were studied ; indeed this was the primary object of investigation suggested by Lord Kelvin. But constant difficulties and ambiguities presented themselves, hindering the interpretation of even the simplest experiments, and it was found necessary to enlarge the scope of the research in order to remove or explain them.

§ 22. The experiments detailed below are among the most definite of those carried out. They show large changes in potential on account of very thin films of oxide, and prove that the amount of change is dependent on the thickness of the film when below a certain limit.

§ 23. Two plates of cast zinc, which had been carefully polished on medium glass-paper the day before, gave with one another

$$+\cdot 02 \text{ v.},$$

the upper plate being positive. The upper plate was now taken out and heated on the back by means of a very small blowpipe flame. Its face became slightly blistered, but not much discoloured, and when cold it gave with the other, which had not been altered in any way,

$$-\cdot 44 \text{ v.},$$

the "minus" indicating that the upper is now negative. This shows a permanent fall of ·46 v. due to heating in air.

§ 24. A plate of rolled zinc, which gave

$$-\cdot 04 \text{ v.}$$

with another plate of the same material, was heated as in the last experiment but to a higher temperature, very nearly to its melting-point. Its face became a streaky yellowish brown, slightly purple towards the centre (the brown parts turn purple if moistened). When cold, compared with the clean plate it now gave

$$-\cdot 79 \text{ v.},$$

a fall of ·75 v. Thus this oxidized zinc plate has practically the same potential as copper. This great fall of potential does not take place by exposure to the atmosphere at ordinary temperatures, unless possibly after the lapse of many years, for the potential of zinc plates which have been left unpolished for many months is only two or three tenths of a volt below that of clean zinc (see § 34).

§ 25. Similar experiments were made with copper. The film of oxide was gradually increased in thickness by repeatedly heating the plate, and the potential measured in each stage of oxidation, the plate being cold. There was a change of about ·03 v. in the negative direction before the oxide-film became visible, and further increase in the thickness of the film was judged by the gradual change of colour, until the well-known dull purplish black of massive copper oxide was attained. The potential, measured each time when the plate was cold, gradually fell until it reached a limit of about ·30 volt negative to a standard gold plate, which value may therefore be taken as the potential of a mass of copper oxide at 15° C. It may be remarked that there are good reasons, which will be given in Chap. IX., for supposing that oxidation does not commence in air unless the temperature of the plate be raised above 80° C.

§ 26. A plate of copper which had been polished on glass-paper and then on wash-leather gave

$$+\cdot 20 \text{ v.}$$

with a standard gold plate, and was next held in iodine vapour for a short time. It looked moist at first, but quickly became dry. In this state it gave

$$-\cdot 34 \text{ v.}$$

with the same standard, a change of ·54 v. in the negative direction. The surface had a dull colour with a whitish "bloom" on it, the tint resembling that of clean copper, though rather darker. Exposure to iodine vapour thus makes the surface of copper nearly as negative as continued exposure to the atmosphere at a high temperature.

§ 27. The potential of a clean zinc plate fell about ·12 v. when it had been exposed to iodine vapour.

§ 28. The iodine compounds formed on the metallic surfaces were not stable, as the appearance and potential of the plates altered considerably during twenty hours' exposure to the atmosphere.

§ 29. The effects of sulphur-fumes, hydrogen sulphide, &c., were tried, but, though in most cases considerable changes were observed, the results obtained were not satisfactory.

V. *Atmospheric or Time Effects.*

§ 30. A small variation of the potential is usually found to take place during a short time after the plate has been polished, and before it has settled down to a more or less permanent value. This is partly due to the fact that the plate has been slightly warmed during polishing, and takes some time to come to the atmospheric temperature (see Chap. IX.). As regards changes which are not due to variation of temperature, I shall give some typical experiments in the following sections of this chapter.

§ 31. Two plates of "high conductivity" copper were polished on fine glass-paper, and several experiments by burnishing them by mutual friction were then made (see § 16). Forty-seven minutes after the original polishing their potentials with the standard plate were

(*a*) +·035 v.
(*b*) +·045 v.

The plate (*a*) was left in the apparatus all night, that is to say, in a position sheltered from dust, and in air partially dried by the sulphuric acid in the lower part of the case; the potential next morning, $20^h\ 3^m$ after the measurements given above, was

+·045 v.

with the same standard plate. It was thus nearly constant. The plate (*b*) was left in a rack in the laboratory without special protection, with the result that next morning the potential, owing to exposure to the air of the room for about 20^h, had fallen to

−·056 v.,

a change of −·101 v. The plate (*b*) was now placed in the apparatus, and remained nearly constant for two hours at least. Plate (*a*) was left in the rack, and its potential fell ·065 v. in about 2 hours. It is thus evident that at ordinary temperatures in a rather dry atmosphere, and in a place protected from dust and light, the rate of variation of the contact-potential of copper is very small; in an exposed place, however, it is by no means negligible.

§ 32. A plate of block tin was polished on glass-paper. Its potential varied with time as follows, the time being counted from the completion of the polishing :—

Hrs.	Min.		Volts.
0	22	. . . .	+·515
0	27	. . . .	+·520
0	46	. . . .	+·535
1	4	. . . .	+·535
1	23	. . . .	+·515
3	18	. . . .	+·495
4	6	. . . .	+·495
4	42	. . . .	+·495
71	12	. . . .	+·465

The plate was kept in the apparatus all the time and its potential taken with a standard gold plate.

§ 33. In another experiment a tin plate polished on the finest glass-paper gave, after $0^h\ 15^m$,

+·525 v.

After $23^h\ 30^m$ it gave

+·535 v.

This experiment was made in June, while the previous one was made in December. Hence probably the greater rate of variation in the former was due to a moister and less pure atmosphere.

§ 34. An old zinc plate which had probably not been polished for many years gave

+·37 v.

with the standard gold plate. It was next polished on glass-paper, and gave

+·75 v.

with the same standard. This shows that the ultimate effect of exposure is to make the potential of zinc fall. I have found that in some cases this fall is preceded by a slight rise, as in the case of tin (§§ 32, 33); but the ultimate effect is in the negative direction.

§ 35. The potential of a silver plate which had been polished on glass-paper remained constant for an hour. The experiment was not continued further.

§ 36. Aluminium becomes gradually negative in air. In one case the potential fell about ·18 v. in a week. The time-change of this metal, mainly on account of its large temperature-variation (see Chap. IX.), is rather difficult to determine.

§ 37. An iron plate which had not been cleansed for two months rose ·05 v. when polished. Hence the effect of exposure had been to make its potential fall slightly.

§ 38. I have included these results, for which, as for many others like them, I can as yet give no definite explanation, in the hope that they may serve as a basis for future experiment and generalization. The great difficulty in all such experiments on contact electricity is to define the circumstances and, with them, the causes of any given variation.

VI. *Very Thin Liquid Films on Metals.*

§ 39. In the earlier experiments it was found that, as a rule, copper polished on glass-paper or emery-cloth was about ·20 v. *negative* to the standard alcohol-washed gold plate. On comparing this value with M. Pellat's results* I found that his value for copper was about ·20 *positive* to the same standard. I at once determined to try his method of cleansing the plate so as to find if the difference (·40 volt) were due to that alone. For this purpose a copper plate was carefully polished on glass-paper. It gave, with the standard,

$$-\cdot 20 \text{ v.}$$

It was then washed with alcohol and allowed to dry in air. It now gave

$$+\cdot 22 \text{ v.}$$

with the same standard. The change due to the treatment is therefore +·42 volt, and continues permanent many hours.

§ 40. A plate of thickly oxidized copper giving, with the standard plate,

$$-\cdot 24 \text{ v.}$$

gave, after it had been washed with alcohol and allowed to dry,

$$\cdot 00 \text{ v.}$$

a rise of ·24 volt.

§ 41. A zinc plate, when cleaned on glass-paper, gave

$$+\cdot 61 \text{ v.}$$

It was then washed with alcohol, and when apparently quite dry gave

$$+\cdot 94 \text{ v.,}$$

remaining constant at this value for many hours. The rise in this case is ·33 volt.

§ 42. The results of many similar experiments made with alcohol and other liquids on gold, silver, zinc, copper, iron, tin, lead, and aluminium will be found in Table II. It is notable that in almost every case the contact-potential of a metal which has been polished on a hard dry material rises at least ·15 volt in consequence of alcohol washing, and that this change is nearly permanent for many hours after the plate appears to be quite dry.

§ 43. It may be remarked that, as M. Pellat appears to have invariably washed the metal with alcohol before making a measurement of its potential, his results do not apply

---

* *Ann. de Chimie et de Physique*, 1881.

directly to metals which have been cleaned by means of a hard dry agent only. Even in the experiment which he gives on pp. 79–80 of his paper as a proof that alcohol-washing does not permanently alter the potential of a clean gold plate, he appears to have used a plate which had already been washed with alcohol; his result, therefore, does not prove that alcohol-washing makes no change in contact-potential of a clean plate, but only that subsequent washings do not alter the state of surface, *i. e.* that the effect is nearly permanent. This agrees with the results given above and in Table II.

§ 44. The alcohol film, whether in combination with the metal or not, must in any case displace the air from the surface of the plate. Other liquids seem to leave films in much the same way, though the change of potential is different for each liquid; in some cases, *e. g.* turpentine on zinc and copper, it is in the positive direction for the positive metal and in the negative for the other.

Table II.— Permanent Changes produced in Contact-potential of Metals by washing with Alcohol and drying in Air.

Metal.	Previous treatment.	Potential with standard gold plate.		
		Before.	After.	Change.
		volt.	volt.	volt.
Copper	Polished on glass-paper	−·20	+·22	+·42
”	” ”	+·09	+·29	+·20
”	Polished on emery-cloth	+ 02	+·14	+·12
”	Filed	−·13	+·14	+·27
”	Oxidized..............................	−·24	·00	+·24
Gold	Polished with dry “ plate-powder ”	......	......	+·05
Silver	Polished on glass-paper	+·10	+·20	+·10
Iron	Dry-polished..............................	+·08	+·16	+·08
”	Polished on emery-cloth	+·17	+·29	+·12
”	” ”	......	......	+·17
”	” ”	......	......	+·14
Lead	Scraped thoroughly with a knife ...	+·61	+·80	+·19
Tin	Polished on glass-paper	+·56	+·66	+·10
Zinc	Polished on glass-paper	+·61	+·94	+·33
Aluminium .	Polished on glass-paper	+1·05	+1·10	+·05
”	” ”	+1·04	+1·07	+·03
”	” ”	+1·13	+1·28	+·15
”	” ”	+1·17	+1·26	+·09

VII. *Thick Liquid Films on Metals.*

§ 45. In experimenting with films of sensible thickness the liquid was usually placed on a flat metal plate, its surface-tension being sufficient to retain it if its depth were not more than ·3 or ·4 centim. In other cases a shallow metal dish was used. The behaviour of water puzzled me for some time, and made me realize the extreme difficulty of obtaining a clean liquid surface. After some preliminary experiments I found that distilled water which has not been exposed to the atmosphere gives fairly constant results when lying to a depth of ·2 centim. on one of the standard gold plates. When water on gold formed the one plate and a dry standard gold plate (see § 8) the other, the value found was ·10 volt, the water being negative. Thus clean water in contact with gold is about ·10 volt negative to it. The value found is quite different if the water has been exposed to the air for some time, and is usually positive instead of negative to the standard plate.

§ 46. Air expelled from the lungs produces a very marked change in the contact-potential of water. By blowing through a fine glass tube which dipped under the surface of a layer of distilled water on a gold plate, the potential was changed from

$$-{\cdot}10 \text{ v.}$$

with the standard plate to

$$+{\cdot}16 \text{ v.},$$

and remained at this latter value for more than half an hour, showing that the change was not due to a rise in temperature. The curious point is that the change seems to be only in the surface of the liquid; for when most of it was shaken off the potential fell to

$$-{\cdot}05 \text{ v.},$$

although the plate was still wet all over. This appears as if the alteration were due to a surface film of oily or dusty matter on the water. It was found that distilled water which had been exposed to the air of the room for some time gave the same value as water which had been breathed through.

§ 47. A lead plate and a flat circular leaden dish were scraped clean with a knife, and next morning, when compared with each other, they gave

$$+{\cdot}10 \text{ v.},$$

the plate being positive to the dish. Water was now poured into the dish until it was nearly full, ·4 centim. deep, the potential remaining the same; *i. e.* the lead plate gave with the water in the leaden dish,

+·10 v.,

water in leaden dish being negative.

§ 48. The plate and dish were again scraped, and this time they gave

+·05 v.,

the plate being positive. Water was next poured into the dish, which gave, with the dry lead plate,

+·09 v.,

the water in the dish being negative as before. The dish was now removed and a standard gold plate put in its place; with this the lead plate gave

+·52 v.,

the lead being positive. Thus water in contact with lead is about ·09 v. negative to dry lead.

§ 49. Turpentine seems to produce opposite effects on zinc and copper, *i. e.* copper becomes more negative and zinc more positive when wet. Thus copper and zinc which had been dry-polished gave with one another

·77 v.,

zinc positive.

With the zinc wet with turpentine,

·96 v.

And with both zinc and copper wet about

1·20 v.

Their potential-difference increased somewhat as the turpentine dried up; and when apparently quite dry its value was still at least

1·20 v.,

the plates, however, smelt strongly of turpentine, showing that an invisible layer remained on their surfaces.

§ 50. A crystal of copper sulphate gave, with a standard gold plate,

+·02 v.,

copper sulphate positive. This result was obtained in connexion with some experiments on the contact-potential of an aqueous solution of copper sulphate. It was found that such a solution in a copper dish gave about +·07 volt with a dry standard gold plate, the solution being positive. Blottingpaper saturated with copper sulphate gave about $+\cdot 10$ volt with the same standard plate. As no special precautions were taken in these experiments to obtain a perfectly pure liquid surface, one cannot, especially in view of the changes described in § 46, be quite certain that they represent the true potential of copper coated with a solution of its sulphate. They agree, however, with Professors Ayrton and Perry's values for saturated and non-saturated solutions.

§ 51. One of the standard gold plates which had been polished with Hollis's plate-powder used dry gave, with a similar plate which had a layer of alcohol on it,

$$-\cdot 13 \text{ v.}$$

When the alcohol had dried up the value was

$$-\cdot 05 \text{ v.},$$

the polished plate being negative as before.

§ 52. The conclusion which I draw from the experiments described in this and the preceding chapter and in the tables is, that a layer of liquid on a metallic surface does not give, with a metal separated from it by air, a definite potential-difference of its own, as in similar circumstances a *solid* conducting film would do, but merely adds a certain amount to that of the plate on which it lies. Thus two different metals coated with layers of the same liquid do not, as a rule in air, give zero potential-difference, but usually give nearly the same potential-difference as the dry metals. For instance, the potential of lead with an alcohol layer ·1 centim. deep on it is about ·13 volt higher than that of dry lead, while that of wet copper is about the same amount higher than the average value for dry copper. There is no tendency shown for a liquid film to take up a definite potential independent of that of the metal on which it lies, with any metal separated from it by a dielectric, as a solid conducting film would do. This seems to be the most important distinction between solid and liquid conductors, and it is in accord with what is known of voltaic cells; for if the potential-differences in the chain copper-water-zinc were equal and opposite to that of zinc-copper, as copper-iron-zinc is to zinc-copper, we should have no electromotive force in the circuit, when the materials are all at one temperature. In most of my experiments the type is copper-water-nonconductor (air)-water-zinc and not copper-water-zinc; the members of the first and last pairs are in contact, while a non-conductor intervenes between the two free water surfaces, these being able to take up their natural contact-potential-differences with the metals they touch; but the sum of the potentials, copper-water and water-zinc, is not equal to copper-zinc, thus proving that the two free surfaces of water are not at the same potential. This is directly demonstrated by the experiments of Professors Ayrton and Perry, and by the results given in Chaps. VI., VII., and X. of this communication.

If the intervening layers of non-conductor be removed by joining the liquid surfaces so that there is but one mass of liquid between the plates, conduction at once tends to reduce the whole liquid to the same potential, leaving the contact-potential-differences, now unbalanced by the removal of the non-conducting medium which was capable of sustaining the stress, to act as external electromotive force. This then shows the connexion between contact-potentials, measured electrostatically by the method described in this paper, and the electromotive force of a voltaic cell.

§ 53. The contact-potential of a liquid with a metal is clearly, if the air-potentials be neglected, the difference between the potential of the dry metal and that of the metal when wet with the liquid; the same standard plate being used as zero of potential in both cases. The results given in the tables must no doubt be in some cases complicated by the formation of solid compounds in the interface between liquid and metal so that the liquid is no longer in contact with clean metal, and the liquid must also displace any film of condensed air which may exist on the metal. The latter influence will be discussed in Chaps. VIII. and X.

TABLE III.—Potential of Metals covered with visible layer of Alcohol.

Metal.	State of Surface.	Potential with standard gold plate.		
		Dry.	Wet.	Change.
		volt.	volt.	volt.
Copper	Filed; wet with alcohol	−·13	+·19	+·32
Gold	Dry-polished; wet with alcohol	......	......	+·13
Silver	Glass-paper polished	+·10		
	Wet, ·05 cm. deep	......	+·18	+·08
	„ thin film	......	+·19	+·09
Lead	Scraped thoroughly	+·61		
	Wet, ·1 cm. deep	......	+·74	+·13
	„ thin film only	......	+·74	+·13

Table IV.—Change in Contact-Potential of Zinc and Copper due to Layers of Turpentine, Rosin-Oil, and Indiarubber Solution.

Liquid.	Metal.	Potential, volts.		Remarks.
			Change.	
Wet with turpentine.	Copper, polished on glass-paper.	……	−·11	*Note.*—The plates were compared with one another, and not with the standard plate. Hence only the *change* of potential is given.
	Zinc, polished on glass-paper.	……	+·20	
Wet with rosin-oil …	Copper, polished on glass-paper.	……	−·12	
	Zinc, polished on glass-paper.	……	−·02	
Wet with indiarubber solution.	Copper, polished on glass-paper.	……	+·02	
	Zinc, polished on glass-paper.	……	+·07	

VIII. *Films formed on Metals by Gases.*

§ 54. The potential of a metal is usually altered by soaking the plate in a gas other than air. If the gas be oxygen, this alteration is as a rule only temporary, and apparently depends on the formation of a surface film, or rather on a change in the film which doubtless already exists. It will be shown that the change of potential of zinc due to soaking in oxygen is nearly equal to that of copper; hence the film of oxygen acts like a liquid film (Chap. VII.), but its effect is less permanent. Previous experimenters, except Lord Kelvin, appear to have neglected the existence of these films, and to have looked only to the nature of the body of gas between the plates. Of course I do not here allude to the "double-layer" which has been offered as an explanation of the phenomenon, but to a layer in mechanical and electrical contact with the metal. If the contact-potential of two metals immersed in a gas were the sum of the potential-differences between each metal and a skin of gas close to it, we should have no slope of potential in the body of the gas between the plates. That a slope does exist, however, is proved by Lord Kelvin's earliest experiments with the divided ring of copper and zinc. In this connexion Dr. Bottomley's research on contact-electricity in high vacua [*] is of great importance as showing that the volta-potential of metals is

---

[*] B. A. Report, 1885.

not sensibly different in different gases so long as the metals are not chemically affected, and is not sensibly altered by a great reduction of pressure. The body of the gas may possibly have some influence, but the variations which have frequently been attributed to it may usually be more satisfactorily explained as being due to change in the film in contact with the metallic surface. In the case of a gas which acts vigorously on the metal at ordinary temperatures the film is permanent, and is probably a solid compound; in other cases it is not permanent, and hence probably not solid.

§ 55. In 'Nature' for 1881, Lord Kelvin describes some very important experiments on this subject. As these appear to be but little known I shall give some extracts from his paper before describing my own results. Under the date November 23, 1880, Lord Kelvin says :—"I have found that a dry platinum disk, kept for some time in dry hydrogen gas, and then put into its position in dry atmospheric air in the Volta-condenser, becomes positive to another platinum disk which had not been so treated, but had simply been left undisturbed in the apparatus. The positive quality thus produced by the hydrogen diminishes gradually, and becomes insensible after two or three days. P.S.—On December 24, 1880, one of the platinum plates in the Volta-condenser was taken out; placed in dried oxygen gas for forty-five minutes; taken out, carried by hand, and replaced in the Volta-condenser at 12.30 on that day. It was then found to be negative to the platinum plate, which had been left undisturbed. The amount of the difference was about ·33 of a volt. The plates were left undisturbed for seventeen minutes in the condenser, and were tested again, and the difference was found to have fallen to ·29 of a volt. At noon on the 25th they were again tested, and the difference found to be ·18. The difference had been tested from time to time since that day, the plates having been left in the condenser undisturbed in the intervals. The following table shows the whole series of these results :—

Time.	Electric difference between surfaces of a platinum plate in natural condition, and a platinum plate after 45 minutes' exposure to dry oxygen gas.
Dec. 24, 12.30 P.M.	·33 of a volt.
„ 24, 12.47 P.M.	·29 „
„ 25, noon	·18 „
„ 27, noon	·116 „
„ 28, 11.20 A.M.	·097 „
„ 31, noon	·047 „
Jan. 4, 11 A.M.	·042 „
„ 11, 11.40 A.M.	·020 „

After detailing some experiments in which the plates were coated with the gases by electrolysis, Lord Kelvin concludes: "Thus in the case of polarization by oxygen, as well as in the case of polarization by hydrogen, the effect of exposure to the dry gas was considerably greater than the effect of electroplating the platinum with the gas by the electromotive force of one volt."

The large effects on contact-potential produced by films formed from gases are clearly shown in these experiments of Lord Kelvin's. It is well known that platinum and other metals have the property of occluding large quantities of gas in their surface layers, and that the condensed gas is possibly in the liquid state, which would account for the similarity between the effects of liquids and of gases on metals.

§ 56. The plates with which my first experiments on this subject were made were of "high conductivity" copper. They had been polished on fine glass-paper five hours before, and their mutual potential had remained constant at

·02 volt

for four hours. The lower plate was then put into a glass vessel, into which oxygen gas was admitted from a cylinder, and the oxygen, which was of Messrs. Brin's manufacture, guaranteed 93–95 per cent. oxygen (nitrogen is usually the only impurity), was allowed to stream through the glass vessel containing the plate for some minutes, and the exit and inlet of the vessel were then closed. Forty-five minutes later the plate was taken out and its potential again measured with the other plate, which had remained in air during the interval. Counting time from the moment at which the plate was taken out of the oxygen, the potential varied as shown below, the oxygenized plate being positive to the other in all cases:—

Time.		Contact-potential.
h.	m.	v.
0	3	·12
0	19	·08
0	30	·06
17	20	·03

Thus the effect of increasing the proportion of oxygen in the surface film was to make the copper more positive.

§ 57. This variation is in the opposite direction to that found by Lord Kelvin for platinum which has been soaked in oxygen. In order to make sure that this difference was not the effect of some impurity in the oxygen, I repeated his experiment, obtaining the same result as he had obtained. This shows that the result given above was not likely to be due to an impurity which had influenced the action of the oxygen.

§ 58. Without any further treatment the same copper plate was placed in the bell-jar and the oxygen admitted. After forty-five minutes it was taken out and its potential again measured by comparison with the other plate. The results are given in the following table, time being counted from the moment the plate was taken out of the oxygen :—

Time.		Contact-potential.
h.	m.	v.
0	2	·13
0	10	·08
0	21	·07
0	29	·06

The experiment was discontinued before the plate had returned to its original value ; but it would no doubt have done so in a few hours, for the amount of change, and its rate, are almost exactly the same as in the former experiment (see § 56).

§ 59. In the following experiment the conditions were somewhat varied. A copper plate was polished on glass-paper. It gave, with a standard copper plate,

—·05 v.

A jet of oxygen was now sent against its surface for two or three minutes, and with the same standard plate it now gave

—·06 v.

It was then left in oxygen for twenty-five minutes, and on being taken out gave

Time.		Contact-potential.
h.	m.	v.
0	4	+·060
0	13	+·035

which shows that its immersion had raised its potential ·12 v I now warmed it with a soldering-bolt applied to its back. When about 47° C. (see Chap. IX.) it gave

Time.		Contact-potential.
h.	m.	v.
0	16	+·020.

It was again warmed slightly, and when at about 30° C. gave

Time. h.	m.	Contact-potential. v.
0	42	—·010.

When 16° C.,

Time. h.	m.	Contact-potential. v.
4	48	—·040;

its potential thus coming back to very nearly the original value.

This experiment shows: (1) that the change requires considerable time; for even a fairly strong jet of oxygen playing on the plate for two or three minutes produces no appreciable effect, while twenty-five minutes in still oxygen causes a rise of ·20 v.: (2) that gentle heating does not produce a rise, as it would do with clean unoxygenized copper (see Chap. IX.), but it must be remembered that the copper was, when heated, already above the potential to which heat alone would have raised it; hence this experiment does not show any connexion between temperature-variation and density of the oxygen film as might at first sight be supposed.

§ 60. A zinc plate which had been polished on glass-paper gave with a standard copper plate

·81 v., zinc positive.

It was then put into oxygen and left for fifteen minutes. After being taken out, its potential was again measured with the same standard plate, and was as follows.—

Time. h.	m.	Contact-potential. v.
0	2	·89
0	10	·87
4	18	·86
4	35	·85

This shows that zinc also is more positive after immersion in oxygen.

§ 61. The same zinc plate was again polished on glass-paper and gave

+·73 v.

with the standard copper. A short time later it gave

+·70 v.

It was now put into oxygen for ten minutes, and after being taken out gave

Time. h.	m.	Contact-potential. v.
0	3	+·80
0	13	+·78
17	1	+·74

§ 62. A tin plate was polished on clean glass-paper, and gave with a copper plate

+·40 v.,

tin being positive. Fifteen minutes later the potential of the plates had not altered. I now put the tin into the bell-jar and turned on the oxygen. After it had soaked for 47 minutes it was taken out and compared with the same copper plate. It gave

Time.		Contact-potential.
h.	m.	v.
0	5	+·32
0	13	+·32
0	35	+·35

Thus the variation of tin appears to be in the negative direction, like that of platinum.

§ 63. A silver plate, polished on glass-paper, gave with a standard copper plate

—·04 v.,

and remained constant during an hour. It was then put into oxygen for 15 minutes, and when taken out its potential was found to be

Time.		Contact-potential.
h.	m.	v.
0	3	+·02
0	19	—·01
0	40	—·02
0	45	—·02
0	59	—·03

Thus by immersion in oxygen for 15 minutes it had risen ·06 v., and had fallen to nearly its original value in an hour in air.

§ 64. Silver polished on clean "fine" glass-paper gave with a standard copper

—·095 v.

After it had been 24 minutes in oxygen it gave in air

+·015 v.,

and 12 minutes later

+·010 v.,

showing that the potential had in 24 minutes in oxygen become ·110 more positive.

§ 65. It is noticeable that the amount of change in these experiments on silver is to some extent proportionate to the time of exposure to oxygen. Thus, in § 63, 15 minutes in oxygen caused a rise of ·06 v.; while, in § 64, 24 minutes in oxygen caused a rise of ·11 v., but there is no doubt a limit to the change.

§ 66. In searching for an explanation of the temperature-variations described in Chapter IX., I compared them with those given above. In the case of copper, the oxygen-film variation is in the same direction as the temperature-variation of copper in air, which suggests the possibility of the latter being caused by an increase of the proportion of oxygen in the film at higher temperatures on account of a greater attraction between the elements. The same reasoning holds as regards zinc and tin, but the results for silver are in direct opposition; while the further experiments described in Chapter X., which show that the temperature-variations exist in cases where air is entirely excluded from the metallic surface, render such an explanation very doubtful. Probably, therefore, the temperature-variation is the more general of the two, it being a change in contact-potential of the metals, which, if they are exposed to a gas, may be complicated by alteration of the surface-film.

IX. *Temperature Variations.*

§ 67. A large number of determinations were made of the variation of contact-electricity with the temperature of the conductor. This was done by heating one plate while the other was kept cool, and their potential-difference was measured from time to time as the warm plate was cooling, their temperatures being observed at the same time. In the diagrams, the abscissæ represent temperature and the ordinates potential; each curve, therefore, shows the temperature-variation of the contact-potential of a particular metal. For instance, the potential represented by the point which corresponds to 16° C. on the gold line is called zero in this and other chapters of the present communication. Thus a standard gold plate (see § 8) at 40° C. is ·04 v. negative to one at 16° C., and an aluminium plate at 40° C. is ·10 v., positive to aluminium at 16° C., or 1·20 v. positive to a gold plate at 16° C.

It must be remembered that unless stated otherwise these variations are for metals in air. In the experiments described in Chapter X., however, the metallic surfaces were protected by solid non-conducting films, and were not in contact with the atmosphere; nevertheless, temperature-variations were found, which in the case of silver were actually larger than those which took place in air.

§ 68. One of the copper plates used was hollow and could be filled with water and a thermometer inserted, but with the other plates other methods of measuring temperature had to be adopted. In some cases the temperature was measured thermo-electrically, while in others a simpler and more rapid mode of measurement was used, which, though not very accurate, is quite reliable within certain limits.

§ 69. By touching the back of the plate I found that its temperature could be judged as "tepid," "slightly warm," "warm," "very warm," and so on. It was found experimentally that these terms correspond to constant temperatures; or rather that each term denotes a small range of temperature, the middle point of which may be taken as corresponding to the term. In determining the values of these terms a plate was used in which a thermometer was inserted. One observer touched the plate with the tips of the first and second fingers and judged its state, naming it by one of the terms, "warm," "tepid," &c., the other observed the thermometer, and the temperatures found to correspond to each term are as follows :—

Cold	16° C.
Quite cool	24
Cool	28
Rather cool	30
Tepid	35
Slightly warm	40
Warm	47
Very warm	50
Hot	53
Very hot	57
Too hot to touch continuously	63
Too hot to touch for more than one second	73

This method of measuring temperature is rough and ready, but since the possible errors are within limits of a very few degrees, one only requires to take the average of a considerable number of results in order to arrive at a very fair approximation to the true values. In experiments on contact electricity in air there are so many possible causes of disturbance that extremely accurate measurement of the temperature is of little use, especially if it require that much time be spent over each reading.

§ 70. As a rule the upper plate was heated, in order that the lower plate might not be affected by draughts of hot air, as would have been the case if the lower had been hot and the upper cold. Sometimes, during the time of cooling, the upper part of the apparatus, including of course the upper plate, was removed after each observation and was replaced only the moment before the next. In other experiments the upper plate was merely drawn up as far as possible (about 10 cm.) to prevent its warming the lower one. The temperature of the lower plate was also observed, but as a rule it varied only a very few degrees.

§ 71. At first I used to apply a hot soldering-bolt to the back of the plate in order to heat it, but latterly I heated two or three small blocks of tinned copper and placed them on the back of the upper plate. By this second method it was possible to observe the variation of the potential during the rise as well as the fall of temperature.

§ 72. In attempting to determine the temperature-coefficient of copper we were long baffled by curious anomalies. Sometimes the plate was positive when hot, other times negative, and occasionally it did not vary at all. The clue to this was found in observing that during one experiment while the copper was cooling it was at first positive, then negative, and then it gradually became positive again, though never quite reaching its original value.

Now it had been found that copper oxide is negative to copper, and that it became temporarily more negative when hot; hence it was guessed that the successively positive and negative variation must be due to hot clean copper being positive to cold copper, but that it had finally become oxidized and therefore negative whether hot or cold, the small permanent change being due to the thin coating of oxide formed.

§ 73. The copper was heated much more gently next time, and gave the expected result that clean copper becomes rapidly more positive as its temperature rises, and that, on cooling, its potential returns to its original value unless the temperature has exceeded a certain limit. If this limit has been exceeded its potential rapidly becomes negative and does not return to its original value.

§ 74. At ordinary atmospheric temperatures the surface of clean copper remains for a long time almost unaltered either visibly or electrically (see § 31), and the film which ultimately forms on the surface cannot be pure copper oxide because the potential of tarnished copper is higher than that of copper oxide obtained by heating in air. If, however, the temperature of the copper is raised to about 80° C. it immediately begins to oxidize, though heating to a temperature below this limit does not rapidly produce any permanent change. Thus there is, as it were, an *ignition point* for copper and oxygen in air; below it, little action takes place; above it, combination proceeds vigorously.

§ 75. I shall now give a specimen experiment. The two standard gold plates gave

$$-\cdot 02 \text{ v.},$$

the minus sign indicating that the upper plate is negative. I now heated the upper; when "tepid," *i. e.,* about 35° C., it gave with the cold plate

$$-\cdot 045 \text{ v.}$$

It was next heated further until "very hot" (57° C.) and gave

$$-\cdot 06 \text{ v.}$$

When it had cooled down to "tepid" (35° C.), it gave

$$-\cdot 04 \text{ v.}$$

When "cool" (28° C.),

$$-\cdot 03 \text{ v.}$$

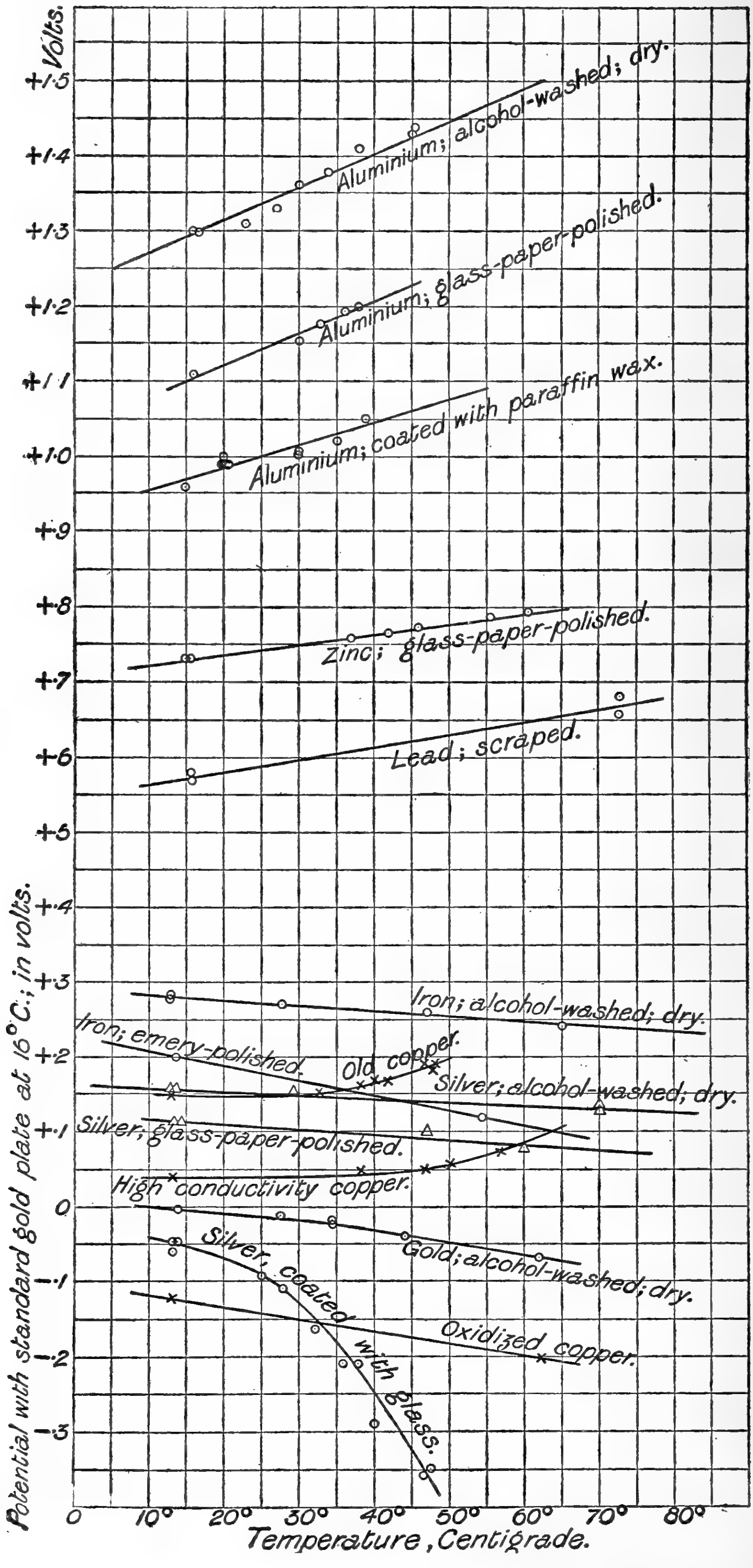
Some hours later, when both plates were cold, they gave as at first

$$-\cdot 02 \text{ v.}$$

Hence the potential of gold which has been washed with alcohol and allowed to dry falls temporarily about ·0016 v. per degree centigrade rise of temperature.

§ 76. Most of the temperature experiments on copper were made with the hollow plate previously mentioned, which was filled with hot water in which the bulb of a thermometer was placed. In many of the experiments on zinc, and also on aluminium, a thermo-electric arrangement was used, and the results obtained with it do not differ materially from those obtained by the above method (§ 69). These and other details are noted on the diagram.

§ 77. The diagram on p. 428 gives the temperature-variations of all the substances studied. The curves in it are plotted by taking the results for each metal of those experiments which are most free from all complication or cause of doubt. If the curves be prolonged in the direction of lower temperature they appear to meet somewhere below −200° C., and probably asymptotically to the line representing ·4 v. positive to standard plate at 16° C. Within their range they show contact-potential-differences diminishing with lowered temperature. The only apparent exceptions are clean copper, and silver coated with glass; but both their curves are distinctly bent between 16° C. and 50° C., so that probably they are directed towards the same point as the others at ower temperatures. Thus it appears that at about −200° C. the contact-potential-differences of metals may vanish, and


+Volts.
+1·5
+1·4
+1·3
+1·2
+1·1
+1·0
+·9
+·8
+·7
+·6
+·5
+·4
+·3
+·2
+·1
0
−·1
−·2
−·3
Potential with standard gold plate at 16° C.; in volts.
Aluminium; alcohol-washed; dry.
Aluminium; glass-paper-polished.
Aluminium; coated with paraffin wax.
Zinc; glass-paper-polished.
Lead; scraped.
Iron; alcohol-washed; dry.
Iron; emery-polished.
Old copper.
Silver; alcohol-washed; dry.
Silver; glass-paper-polished.
High conductivity copper.
Gold; alcohol-washed; dry.
Silver, coated with glass.
Oxidized copper.
0
10°
20°
30°
40°
50°
60°
70°
80°
Temperature, Centigrade.

that a plate of *any* metal at that temperature would be about ·4 volt positive to a standard gold plate at 16° C.

§ 78. The approximate numerical values of the temperature-variations are given in the following table :—

TABLE V.

Metal.	Approximate range of Temperature.	Potential of metal with standard gold plate. Both at 16° C.	Variation of potential per 1° C. Standard plate kept always at 16° C.
	°C.	Volts.	Volts.
Aluminium, polished on glass-paper	16–50	+1·10	+·0043
„ waxed	16–40	+ ·98	+·0032
„ alcohol-washed, *dry*	16–47	+1·30	+·0045
Zinc, polished on glass-paper	15–62	+ ·73	+·0013
Lead, scraped	16–75	+ ·58	+·0016
Tin, polished on glass-paper	16–33	+ ·52	About −·0010
Iron, polished on emery-cloth	16–55	+ ·20	−·0022
„ alcohol-washed, *dry*	16–65	+ ·28	−·0007
Silver, polished on glass-paper	16–60	+ ·12	−·0007
„ alcohol-washed, *dry*	16–70	+ ·16	−·0004
„ coated with glass	16–30	− ·05	About −·0035
„ „ „ „	30–50	......	About −·0110
Copper, polished on emery-cloth	16–30	+ ·04	Very small.
„ „ „ „	30–60	......	About +·0015
Gold, alcohol-washed, *dry*	16–60	·00	−·0016
Oxidized Copper	16–65	− ·11	−·0016

It must be clearly understood that these are *true temperature-variations and not permanent changes in the plate caused by exposure to a high temperature.*

X. *Elimination of Metal-Air Potentials by Solid Non-conducting Films on the Metallic Surfaces.*

§ 79. As very great differences of opinion seemed to exist as to the part played by the layer of air which is close to the metallic surface, I devised a method in which it should be removed and a film of solid non-conducting material of a very different chemical nature put in its place. A copper plate which had been polished on glass-paper was filed with a clean dry file which had not been used for any other metal. Its potential with the standard gold plate being

+·045 v.

A zinc plate was prepared in an exactly similar way, and with the copper plate gave

+·655 v.

The copper plate was now gently heated with a bolt until it was hot enough to melt paraffin-wax; the temperature, about 50° C., required for this is not sufficient to cause sudden permanent change of the copper surface (see § 74). Paraffin-wax was then poured on, and the plate was filed with its own file while covered with molten wax. Thus the fresh surface exposed by the filing came directly into contact with the wax. More wax was poured on and the filings drained off, the plate remaining well covered with wax all the while. It was then allowed to cool, and gave with the bare zinc plate

$$+ \cdot 555 \text{ v.},$$

zinc being positive as before. Thus the change due to substituting paraffin-wax for air next the copper is not more than + ·100 v. I now waxed the zinc in exactly the same way. When it was quite cool it gave with the waxed copper

$$+ \cdot 602 \text{ v.},$$

showing that waxing the zinc had raised its potential

$$+ \cdot 047 \text{ v.}$$

So on the whole, the substitution of wax for air on both copper and zinc had only decreased their mutual potential by

$$+ \cdot 053 \text{ v.}$$

and the potential of the waxed plates remained nearly constant for several hours. The changes due to waxing the plates as given above were confirmed by the independent comparison of each plate with the standard gold plate. It does not follow that even the small changes which did occur were due solely to the substitution of wax for air, for they may have been caused by slight changes in the surface on account of the filing.

§ 80. An aluminium plate was coated with wax in the way described in § 79, a knife being used to scrape the surface under the molten wax. When cold, this waxed plate gave with a bare zinc one

$$+ \cdot 36 \text{ v.},$$

which is about the usual value for *bare* aluminium and zinc. The removal of the air had therefore not appreciably altered the potential.

§ 81. I now warmed the waxed aluminium slightly. Its potential with the zinc varied as follows:—

47° C.	+ ·41 v.
35° C.	+ ·37 v.
28° C.	+ ·35 v.

This gives a variation of about

·0032 v. per degree centigrade,

which is nearly the same as the temperature-variation of bare aluminium in air.

§ 82. A plate of silvered glass used with the glass side facing a standard plate gave almost the same potential as clean silver in air. This plate, which was practically silver coated with glass, gave a temperature-variation larger than that of silver in air. In this case we have glass in contact with the silver surface instead of air, but the change does not alter the potential. It may be mentioned that the back of the silver film was painted black, and not coated with glass; but this is of small consequence, since it has been proved by experiment that the condition of the back of a plate does not sensibly affect the volta contact-potential; or, more generally, that if parts of a plate be in different conditions, the potential observed will be the mean of the potentials of the different parts, the importance of each part being proportional to its capacity.

§ 83. On account of the great attraction of sodium for oxygen, it seemed of interest to measure its potential in circumstances which excluded that gas from the surface of the metal. In order to effect this two pieces of thin sheet-glass, each about 6 cm. square, were put into a dish of melted paraffin-wax together with some clean sodium, and a large drop of the sodium was put between the plates of glass and squeezed out into a small plate of 3 or 4 square cm. area. The glass plates, with sodium between them, were taken out of the melted wax and allowed to cool. Since the glass plates were of much larger diameter than the sodium, the edges of the latter were protected by the wax which filled up the space between the plates not occupied by sodium. The flat faces of the sodium were apparently in contact with the glass. The sodium was connected to the electrometer by a fine copper wire. The greater part of the sodium surface was bright or only slightly tarnished, and it remained in almost the same condition for many days, being protected by the glass plates and by the wax which filled the space between them unoccupied by sodium. The first measurements gave sodium

2·86 v.

positive to a tarnished zinc plate, *i. e.* about

3·56 v.

positive to the standard gold plate. This potential gradually decreased.

§ 84. Experiments were made to make sure that the result was not due to temporary electrification of the glass. For instance I breathed on the glass, causing a conducting layer of impure water to form on its surface. Repeated measurements, made by the usual method, showed that the potential at once fell to a small fraction of a volt, but slowly rose again to nearly its original value as the film evaporated. This shows that the electrification was not a temporary one of the glass surface, for that would not have returned to a definite value. Heating the plate by radiation or washing the glass with benzol caused the potential to rise further, but in no case was the potential quite so high as when the plate was first formed. An even more convincing proof that the potential measured was really that of the sodium was found in the fact that the sensibility of the apparatus was such as would be given by a plate the size of the sodium. If the electrification had been on the whole surface of the glass, the sensibility, on account of the larger surface, would have been at least ten times as great as that observed.

§ 85. The experiments described in this chapter show that (i.) when two metals are coated with the same non-conductor, such as wax or glass, their potential is not sensibly different from that of the bare metals in air; (ii.) that temperature-variation still takes place, though air be excluded. These results seem to prove that gaseous films play no *essential* part in the phenomenon.

XLIV. *On the Susceptibility of Diamagnetic and Weakly Magnetic Substances.* *By* ALBERT P. WILLS*.

FARADAY (1845) showed that all substances, whether solid, liquid, or gaseous, are either diamagnetic or magnetic. Previously, however, Brugmans (1778) and Becquerel (1827) observed certain diamagnetic phenomena.

By way of relative measurement of the coefficient of susceptibility, a great deal has been done by various physicists, among them Plücker and Faraday. For bismuth there have been a number of determinations in absolute measure. It will suffice to mention here four of the best methods used:—1. The method used by Christie† (1858) depends upon the comparison of the inductive action of the magnetized bismuth with that of a solenoid through which a known current is flowing. 2. The method used by Töpler

---

* Communicated by Prof. A. G. Webster.

† Pogg. *Ann.* ciii. p. 577 (1858).

and von Ettingshausen* (1877) depends upon the measurement of the force of repulsion upon a piece of bismuth hung near the end of a solenoid. 3. The method used by Rowland and Jacques† (1879) depends upon the measurement of the influence of the diamagnetic force upon the time of swing of a piece of bismuth suspended in an inhomogeneous magnetic field. 4. The method used by von Ettingshausen‡ (1882) depends upon the measurement directly of diamagnetic action at a distance.

von Ettingshausen has used all the above-mentioned methods, and for an interesting comparison of results obtained with these methods, a table, taken from Winkelmann's *Handbuch der Physik*, is subjoined :—

Values of κ for Bismuth.

Observer.			Extreme Values.	Mean Values.
v. Ettingshausen	1	Method	$13{\cdot}35$ to $13{\cdot}82 \times 10^{-6}$	$13{\cdot}57 \times 10^{-6}$
„	2	„	$13{\cdot}82$ to $14{\cdot}08$ „	$13{\cdot}99$ „
„	2	„	$14{\cdot}41$ to $14{\cdot}80$ „	$14{\cdot}54$ „
„	2	„	$13{\cdot}41$ to $13{\cdot}54$ „	$13{\cdot}48$ „
„	2	„	$14{\cdot}09$ to $14{\cdot}13$ „	$14{\cdot}11$ „
„	3	„	…… „	$15{\cdot}30$ „
„	4	„	$13{\cdot}13$ to $13{\cdot}66$ „	$13{\cdot}35$ „
„	General mean			$14{\cdot}05$ „

von Ettingshausen also made absolute determinations of the susceptibility for antimony and tellurium. There has been a considerable amount of work done by various physicists by way of determination of the susceptibility for liquids and gases. A great number of liquids and gases have been used, and the susceptibility has been determined for them by different methods. The results thus obtained sometimes differ widely.

Concerning the determination of the susceptibility for bodies in which it is exceedingly small it may be said that the methods previously used have sometimes suffered greatly in consequence of the weakness of the action to be observed. No doubt many of the divergent results obtained by different physicists are due in large measure to dissimilarity in the samples of the substance experimented upon. In some cases, however, they seem due to some chronic weakness in the method of experimentation. A fundamental difficulty met with in the elaboration of a method for the study of diamag-

---

* Pogg. *Ann.* clx. p. 1 (1877).

† Amer. Journ. Sci. xviii. p. 360 (1879).

‡ Wien. *Ber.* lxxxv. [2] p. 37 ; Wied. *Ann.* xvii. p. 272.

netic phenomena lies in making the inevitably small action as large as possible.

In the autumn of 1895*, acting upon the suggestion of Prof. A. G. Webster, I attempted to determine the susceptibility of substances by suspending an ellipsoid of the substance in a homogeneous magnetic field, and observing the times of oscillation with a known field and with no field. I failed in this object owing to the fact that I was unable to obtain a field which was uniform to a sufficient degree of approximation, the effect which I wished to observe depending upon the square of the extremely small quantity κ, while the effect depending upon the non-uniformity of the field varies with the first power of κ.

The purpose of the present paper is to describe a method for the determination of κ for bodies in which it is extremely small and either positive or negative, and to give the actual results obtained from experiments upon a large number of substances.

Description of the Apparatus.

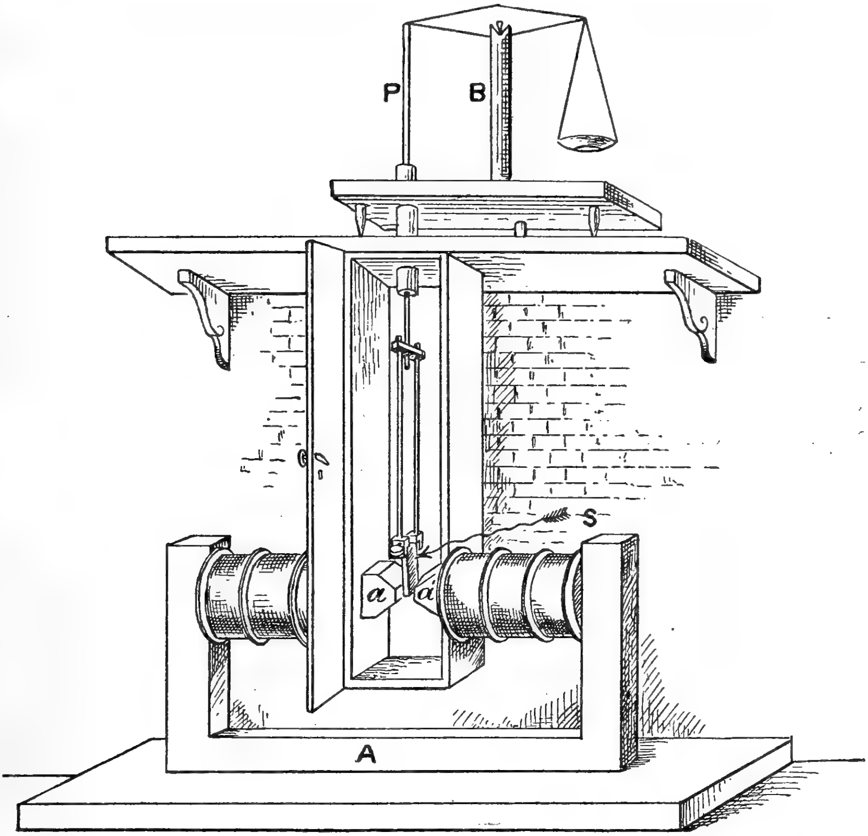
It has long been known that if any substance be introduced into an inhomogeneous magnetic field it experiences a mechanical force which tends to make it enter a stronger or weaker part of the field, according as the substance is magnetic or diamagnetic. To determine this force the amount and distribution of the field in which the substance is placed must be known.

I shall describe the apparatus used in the present experiments, and in the theory of the method shall show that the apparatus as here described is adequate to allow for certain simplifications in the general theory, giving the force acting on a body placed in an inhomogeneous magnetic field. As a result of these simplifications we get an expression for the mechanical force upon the body in a convenient direction, which involves the field-strength at the two limiting surfaces only, in that direction. A suitable form for the experimental body is that of a thin rectangular slab. Fig. 1 will give a general idea of the arrangement of the apparatus.

---

* In connexion with this work I noticed a very remarkable behaviour of non-conductors in a magnetic field, which led me to suspect an action akin to hysteresis in iron. As my direct object at that time was to establish a method for the determination of coefficients of susceptibility I did not push my experiments far enough to warrant my publishing results. Mr. Duane has published (Wied. *Ann.* Bd. lviii. p. 517, 1896) an account of similar experiments performed by him in the physical laboratory at Berlin, which seemed to indicate hysteresis in non-conductors, but in which later research showed the effect to be due to traces of iron (Wied. *Ann.* lxi. p. 436, 1897).

Fig. 1.



A. Large electromagnet.
a, *a'*. Pole-pieces of A.
S. Experimental slab in position.
B. Balance.

The pole-pieces are prismatic, as shown in figs. 2 and 3.

Fig. 2.

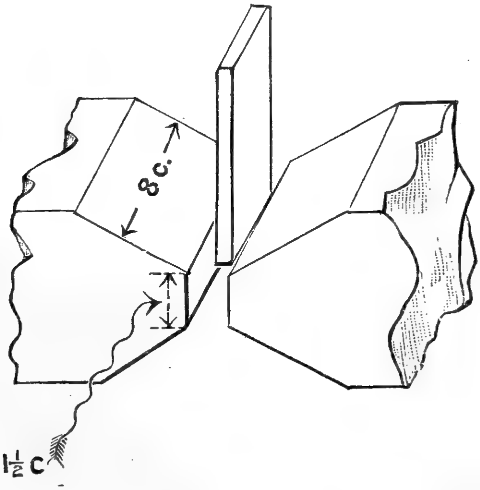
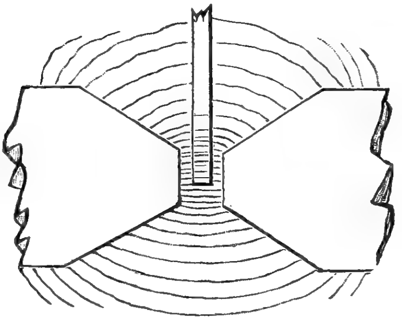
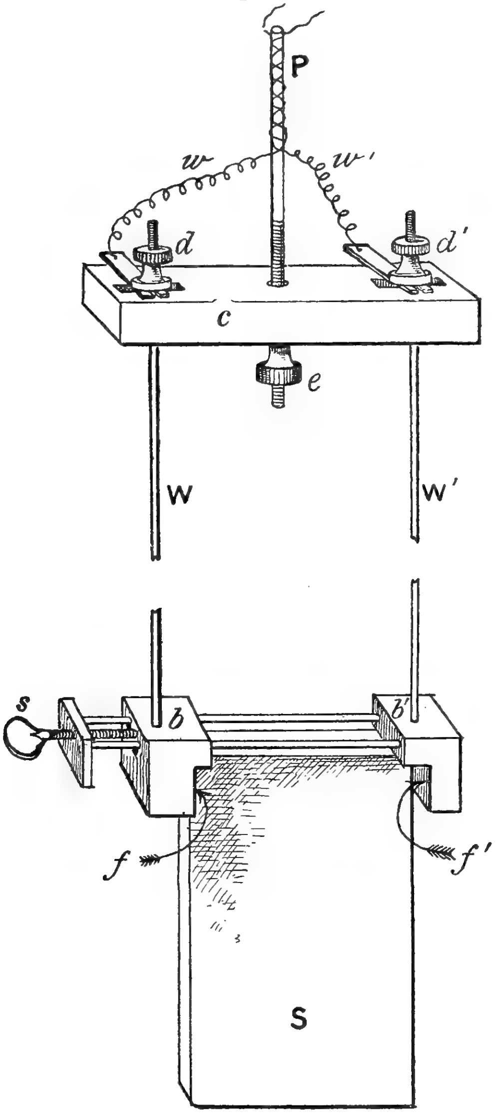


Fig. 3.



The bevelled faces are on angles of 60 degrees. When in position the pole faces are parallel, and the gap between them is about 1·5 centim. The figures given on the diagram represent centimetres.

Fig. 4.



In fig. 4 is shown the suspension arrangement used in holding the slab. P is the suspension-wire from the balance beam passing through the vulcanite cross-arm *c*. W and W′ are rigid brass wires passing easily through slots in *c*, and supported by means of nuts *d* and *d′*, the upper ends of W and W′ being threaded ; *b* and *b′* are small vulcanite blocks to which the rigid wires W and W′ are firmly attached. A part of the surface *f* of block *b* is made conducting and in metallic connexion with W ; likewise a conducting part of surface *f′* is in metallic connexion with W′. For holding the slab S, a clamp operated by the screw *s* is used ; for making the necessary adjustments in the positions of S, the nuts *e*, *d*, *d′* serve. The reason for making the surfaces *f* and *f′* partly conducting and in metallic connexion with W and W′ respectively will be made clear when the method used in making the field determination is described. In circuit with the magnet coils of A there is an ammeter which, with a variable resistance, serves to keep the current constant during an experiment. In the same circuit there is also a commutator which serves to change readily the direction of the current through the magnet coils.

A box, opened in front by means of a door, serves to protect the suspension apparatus from draughts.

An idea of the general distribution of the field is given in fig. 3.

The slabs used in the experiments were rectangular in shape and of approximate dimensions 8 centim. by 4·5 centim. by 0·5 centim. When in position for experimenting the long edges of the slab were vertical. Figs. 1, 2, and 3 show more clearly than could any description how the slabs were suspended.

Theory of the Method.

Considering magnetic forces due to stresses in the media, it can then be shown* that the following equations must in general be satisfied for isotropic media :—

$$X = \frac{1}{8\pi}\{2\mathfrak{L}L - \mathfrak{B}H\},$$

$$Y_y = \frac{1}{8\pi}\{2\mathfrak{M}M - \mathfrak{B}H\},$$

$$Z_z = \frac{1}{8\pi}\{2\mathfrak{N}N - \mathfrak{B}H\},$$

---

* See, for instance, Webster, ‘ Electricity and Magnetism,’ p 387.

$$Y_z = Z_y = \frac{1}{4\pi}\mathfrak{M}N = \frac{1}{4\pi}\mathfrak{N}M,$$

$$Z_x = X_z = \frac{1}{4\pi}\mathfrak{N}L = \frac{1}{4\pi}\mathfrak{L}N,$$

$$X_y = Y_x = \frac{1}{4\pi}\mathfrak{L}M = \frac{1}{4\pi}\mathfrak{M}L.$$

where X_x represents the X-component of the force per unit of area upon a plane element perpendicular to the X-axis (an analogous meaning is to be given Y_y, Z_z, and X_y, X_z, Y_x, Y_z, Z_x, Z_y), and L, M, N, $\mathfrak{L}$, $\mathfrak{M}$, $\mathfrak{N}$ are the components of the magnetic force H and the magnetic induction $\mathfrak{B}$ respectively.

Referring to fig. 3, let the axis of X be in the line of symmetry perpendicular to the pole faces; let the axis of Z be vertical and the axis of Y be perpendicular to the axes of X and Z. We wish to find an expression for the mechanical force acting upon the slab when placed relatively to the pole-pieces as shown in fig. 3. There will be discontinuity in the stress-components X_x, Y_y, Z_z, Y_z, Z_x, X_y at the surface of the slab where there is discontinuity in the permeability μ and the forces per unit area are equal to the amounts of the discontinuities. Let the permeability of the slab be μ_2, and let the permeability of the surrounding medium (air, in the present case) be μ_1. The slab used is so thin that throughout its whole thickness the tubes of induction may be assumed parallel to the X-axis, and consequently perpendicular to the faces perpendicular to the X-axis.

With this assumption we have then

$$Z_z = Y_y = Y_z = Z_x = X_y = 0,$$

and $\iint X_x \, dy \, dz$ taken over the two surfaces of the slab perpendicular to the X-axis equals zero. In a similar way the surface-integral of the forces taken over the two surfaces perpendicular to the Y-axis vanishes. But now consider the forces on the two faces perpendicular to the Z-axis. The conditions of symmetry applicable to the other faces do not hold here. Consider the lower Z-face, that is, the face between the poles. The equations show that all forces vanish at this face save Z_z, and we have then the force Z_z given in medium of permeability μ_1 by

$$Z_z = \frac{1}{8\pi}\mathfrak{B}_1 H_1,$$

and in the slab or medium of permeability μ_2 by

$$Z_z = \frac{1}{8\pi}\mathfrak{B}_2 H_2.$$

The difference in these two values gives us the total force acting upon the surface considered. If P_1 denote its value in grams-weight, we have

$$gP_1 = \frac{1}{8\pi}\{\mathfrak{B}_2H_2 - \mathfrak{B}_1H_1\}.$$

But since H near the surface is the same in both media, we have

$$gP_1 = \frac{1}{8\pi}(\mu_2H_1^2 - \mu_1H_1^2).$$

$$= \frac{\mu_1\kappa_{21}H_1^2}{2}.$$

Since κ_{21} the relative susceptibility is by definition

$$\kappa_{21} = \frac{1}{4\pi}\left(\frac{\mu_2 - \mu_1}{\mu_1}\right).$$

Now a similar treatment of the forces acting at the top surface of the slab will give for the resultant force there

$$gP' = \frac{\mu_1\kappa_{21}H'^2}{2}$$

where H' is the magnetic force at the top surface.

The difference between P_1 and P' times the area of the Z-face will give us the total force acting in the line of suspension. If A is the area of the Z-face, then P, the total vertical force, is given by

$$P = \frac{\kappa_{21}A}{2g}(H_1^2 - H'^2).$$

Now experiment shows that H'^2 is negligible in comparison with H_1^2. If μ_1, the permeability of air, be assumed equal to unity, we have

$$P = \frac{\kappa}{2g}AH_1^2,$$

so that

$$\kappa = \frac{2gP}{AH^2},$$

where H is put for H_1, and κ for κ_{21}.

For the determination of κ it is, accordingly, only necessary to determine P the mechanical force acting on the slab, H the magnetic force at the bottom of the slab, and the area of a cross-section of the slab. In determining the strength of the field the balance mentioned above was used. A known current is sent through a conductor placed in that part of the field the strength of which we wish to measure, and then the vertical force F upon the conductor is determined by means of the balance. The field is given by the equation

$$H = \frac{10gF}{lI},$$

where I is the current in amperes, l is the length of the wire in centimetres, F is the force in grams measured by the balance.

A rectangular slab of plaster-of-paris was made with nearly the dimensions of the experimental slabs. On three edges of this slab (the bottom edge and the two side edges) were pasted thin strips of tinfoil just as wide as the slab was thick. Now when this slab is clamped in the clamp of the suspension apparatus, so that the tinfoil of the side edges is in contact with the metallic part of the surfaces f and f', we shall have metallic connexion between W and W′ by way of the tinfoil along the sides and across the bottom of the slab. When a field determination is to be made the slab is adjusted with reference to the poles in just the same manner as are the experimental slabs. Then the tinfoil strip across the bottom of the slab is a conductor in the field we wish to determine. Since the corners turned by the tinfoil are very sharp the length of this conductor can be determined with great accuracy.

For the determination of the current I a Weston millivoltmeter was used in connexion with a german-silver shunt. The millivoltmeter was carefully calibrated by means of a Rayleigh current-weigher constructed by Mr. S. N. Taylor in connexion with his work on Clark and Weston cells.

Method of Experimentation.

The substance for a series of experiments having been selected, the field magnets were excited with the proper current regulated by a suitable resistance. An assistant kept the needle of an ammeter in circuit with the magnet coils always over a selected line of the scale. The plaster-of-paris slab described above was then introduced into the proper position. Equilibrium was obtained when no current passed through the conductor in the field and a reading of the balance taken. A current measured by the Weston millivoltmeter was then sent through the conductor and a new reading taken when equilibrium had been again secured. The difference in these two readings gave the force in grams F, due to the field, upon the current in the conductor. Then the plaster slab was removed and a slab of one of the substances to be investigated introduced, and a reading of the balance taken when equilibrium obtained. The slab was then reversed, top for bottom, and another reading obtained. This process was then gone through with all the five or six substances selected for a particular series of experiments, another determination of the field being made in the middle of the series and still another at the end.

After the last field determination, and while the field magnets were still excited, a reading of the balance was obtained with no slab in the clamp of the suspension apparatus. This reading was to enable us to make correction for any magnetic action upon the suspension apparatus itself. The current through the magnet coils was then cut off.

The wires *w* and *w′* were hung in long loops from fixed supports in the balance-case to a little rubber support firmly attached to the suspension rod P. From this, being well insulated, they were twined about the rod P and connected with W and W′ at the nuts *d* and *d′*, as shown in fig. 4. These wires would vary slightly in length when a current passed and correction had to be made for this. Both wires *w* and *w′* were connected with one nut (*d*, fig. 4) and the same current used in making the field determination was thus sent over the flexible wires in the balance-case, without going near the field magnets. A reading of the balance was taken when the current had been properly adjusted.

The residual field was then eliminated with great care. This was done by reversing over and over again a small current through the magnet coils until the balance gave no indication of a field when a current was sent through the conductor arranged as when the field was determined.

The slabs were then, each in turn, clamped in the clamp of the suspension apparatus and readings of the balance with the zero field taken. These readings, with those taken with the field on and with the proper corrections mentioned above, are sufficient to determine the mechanical force in the Z direction. This force we have, in the theory of the method, called P.

A scale graduated to read fifths of a millimetre was used to determine the width of the slab, and a Brown and Sharpe micrometer screw-gauge graduated to read thousandths of a centimetre was used to determine the thickness of the slab. The product of this width and thickness gave the area A. In making these width and thickness measurements, many measurements were taken on a given slab and the mean of these used in the calculations.

Results.

The tabular results are, in general, arranged in groups which give the substances experimented upon and the data obtained in a given series of experiments.

A table of abbreviations, which will be found convenient in referring to the tables of data, is given below :—

H.	Unit tubes of force per square centimetre at bottom surface of slab.
p_1.	Balance reading in grams, field on and slab in position.
p_0.	Balance reading in grams, field off and slab in position.
p_1'.	Balance reading in grams, field on, plaster slab in position, and current through conductor.
p_0'.	Balance reading in grams, field on, and plaster slab in position, but no current through conductor.
P.	Mechanical force, in grams, upon the slab.
F.	Mechanical force, in grams, upon conductor in field when current is on.
I.	Current in amperes through conductor when field is being measured.
ϵ.	Correction necessary on account of magnetic attraction upon suspension apparatus without slab.
δ.	Correction necessary on account of extension in wires w and w' when current is sent through them.
l.	Length, in centimetres, of conductor used in measuring field.
w.	Mean width, in centimetres, of experimental slab.
t.	Mean thickness, in centimetres, of experimental slab.
κ.	Coefficient of magnetic susceptibility.

In the first series of experiments, determinations of κ were made for five samples of marble. In the first column of the table are found the names of the different varieties of marble experimented upon. The second column gives p_1, two values being given for each substance, since each slab is reversed in each experiment, the top being put for the bottom ; and the third column gives the mean of these two values. The fourth column gives p_0, the reading of the balance when slab is in position, but no field on. In the fifth column are the values of $P=p_0-p_1+\epsilon$. In the next two columns are found w and t, the factors of A, the area of the slab. The last column gives the values of κ multiplied by 10 to the sixth power, which values are calculated from the data found in the columns and below. Below the columns are found the corrections ϵ and δ, the necessary data for the calculation of H, and the calculated value of H. The same value of H was used throughout a given series of experiments. The same arrangement of data and results is followed in all of the tables given.

TABLE I.—Marble.

	p_1.	Mean p_1.	p_0.	$P = p_0 - p_1 + \epsilon$.	w.	t.	$\kappa \times 10^6$.
Italian ……	103·3462 103·3463	103·3462	103·4244	·0792	5·18	·490	−·940
Italian Statuary (1)	101·2129 101·2146	101·2138	101·2798	·0670	5·13	·474	−·832
Rutland Statuary	102·7416 102·7382	102·7399	102·8070	·0681	5·14	·502	−·795
Gray Knoxville	103·7836 103·7832	103·7834	103·8336	·0512	5·12	·500	−·603
Italian Statuary (2)	102·8600 102·8620	102·8610	102·9279	·0679	5·14	·491	−·811

p_1'.	p_0'.	$F = p_1 - p_0 + \delta$.	H.
74·6630	73·0726	1·593	8,060

$\epsilon = {·}0010$
$\delta = {·}0027$

$l = 4{·}54$
$I = {·}427$

Special attention was thus bestowed upon the study of the magnetic properties of marble since it has been often used as a means of support for coils in standard apparatus. The results obtained seem to indicate that almost any variety of marble can be relied upon to be diamagnetic and free from iron, and to have a magnetic susceptibility of about $-{·}8 \times 10^{-6}$. Therefore it would seem that a free use of marble in magnetic apparatus is allowable.

Table II. gives the data and results for aluminium, glass, and tin.

TABLE II.

	p_1.	Mean p_1.	p_0.	$P = p_0 - p_1 + \epsilon$.	w.	t.	$\kappa \times 10^6$.
Aluminium .	111·5490 111·5565	111·5527	111·3680	−·1837	4·49	·650	+1·88
Glass………	92·5120 92·5120	92·5120	92·5524	+·0414	3·97	·536	− ·578
Tin…………	162·0290 162·0295	162·0293	162·0048	−·0235	4·52	·436	+ ·354

p_1'.	p_0'.	$F = p_1' - p_0' + \delta$.	H.
74·5290	72·9274	1·604	8,120

$\epsilon = {·}0010$
$\delta = {·}0024$

$l = 4{·}54$
$I = {·}427$

It should be noticed that the coefficient of susceptibility for tin is positive, that is, the present experiments would seem to indicate that tin is magnetic. Now previous experiments upon tin have clearly shown it to be diamagnetic, and the conclusion to be drawn in the present case is that the sample used must have contained traces of iron. The glass used was a piece of fine optical glass. The value of κ obtained for this piece of glass is four or five times the value usually given. Aluminium is seen to have a positive value of κ and quite large.

TABLE III.

	p_1.	Mean p_1.	p_0.	P= $p_0-p_1+\epsilon$.	w.	t.	$\kappa\times10^6$.
Antimony ...	173·3254 173·3320	173·3287	173·3830	·0553	4·55	·495	− ·714
Bismuth......	202·5290 202·5970	202·5630	203·3932	·8312	4·58	·430	−12·25
Sulphur	96·3017 96·2963	96·2990	96·3817	·0837	4·94	·644	− ·765
Ebonite	80·9105 80·9232	80·9168	80·7964	−·1194	4·94	·648	+ 1·08
Paraffin	64·4940 64·5054	64·4997	64·5480	·0493	4·44	·559	− ·577
White Wax .	63·7308 63·7298	63·7303	63·7811	·0518	4·94	·544	− ·560
Shellac	74·1848 74·2178	74·2013	74·2422	·0419	5·03	·615	− ·394

p_1'.	p_0'.	F= $p_1'-p_0'+\delta$.	H.
74·6347	73·0140	1·623	6,740

$\epsilon=·0010$ $l=4·54$
$\delta=·0026$ $\mathrm{I}=·427$

The bismuth used in this series was furnished by Eimer and Amend, of New York, and was supposed to be chemically pure. The results for antimony seem to show that the sample used must have contained just a trace of iron, since the value found for κ, while negative, is much smaller than that usually given.

Table IV. gives the results found for several kinds of wood.

TABLE IV.

	p_1.	Mean p_1.	p_0.	$P = p_0 - p_1 + \epsilon$.	w.	t.	$\kappa\times10^6$.
White wood	57·5927 57·5934	57·5930	57·5957	·0029	5·09	·462	−·18
Holly	60·9816 60·9812	60·9814	60·9857	·0045	5·07	·499	−·26
Plain Oak ...	61·6704 61·6704	61·6704	61·6766	·0064	5·04	·449	−·36
Tulip	69 0779 69·0779	69·0779	69 0863	·0086	5·08	·476	−·51
Cedar	58·1218 58·1210	58·1214	58·1239	·0027	5·03	·479	−·16

p_1'.	p_0'.	$F = p_1' - p_0' + \delta$.	H.
73·6900	72·9620	·7314	3,700

$\epsilon =$ ·0002 $\quad l =$ 4·54

$\delta =$ ·0034 $\quad \mathrm{I} =$ ·427

These results show, in general, that the greater the density of the wood the greater is the coefficient of magnetic susceptibility.

Besides these quantitative experiments there were made a large number of qualitative experiments upon a great many different kinds of wood. Only one kind out of about twenty different kinds tried proved to be magnetic, the rest were diamagnetic; the exception was butternut.

There has been in the past a difference of opinion among physicists as to whether diamagnetic substances are or are not less susceptible under the application of large magnetizing forces. Most of the earlier experimenters, among them Tyndall, Joule, and Becquerel, found reason to believe that κ is constant for diamagnetic substances. Plücker, however, believed that in some cases, as the magnetizing force is increased the coefficient of susceptibility κ for diamagnetic bodies decreases in a similar way as in iron. Plücker also thought he observed in some cases an increase in κ with an increase in the magnetizing force up to a certain point, after that a decrease; that is, for a certain value of the magnetizing force κ has a maximum. Silow* (1880) published results which indicate that κ for ferric chloride increases for a while

---

* Wied. *Ann.* ii. p. 324 (1880).

with the magnetizing force, reaches a maximum, and decreases.

The fifth series of experiments was undertaken in the hope of showing definitely whether bismuth, the strongest diamagnetic substance known, does or does not vary in its susceptibility to magnetization as the magnetizing force is made to vary.

Table V., which gives the results of experiments upon bismuth using widely different fields, is arranged in a way similar in all respects to the preceding tables.

TABLE V.—BISMUTH.

	p_1.	Mean p_1.	p_0.	P= $p_0-p_1+\epsilon$.	w.	t.	$\kappa\times10^6$.
Bismuth (1)	203·0078 203·0090	203·0084	203·0414	·0330	4·58	·430	−12·55
Bismuth (2)	202·8664 202·8810	202·8737	203·0400	·1663	,,	,,	−12·25
Bismuth (3)	202·5290 202·5970	202·5630	203·3932	·8302	,,	,,	−12·25
Bismuth (4)	202·0909 202·1010	202·0959	203·0400	·9441	,,	,,	−12·55
Bismuth (5)	201·6156 201·7310	201·6733	203·0400	1·367	,,	,,	−12·45

	p_1'.	p_0'.	F= $p_1'-p_0'+\delta$.	H.
(1)	72·9950	72·6986	·2984	1,620
(2)	73·3670	72·6914	·6764	3,680
(3)	74·6347	73·0140	1 623	8,210
(4)	74·2720	72·6514	1·622	8,800
(5)	74·5500	72·6270	1·925	10,450

Note.—The current used in making the field determinations was in all cases, except the third, ·398 ampere; in the third case it was ·427 ampere. $l=4·54$. δ was approximately equal in all cases to ·0020. ϵ was negligible.

As the table shows, the fields used in the last series of experiments vary between the limits of 1,620 and 10,450 lines per square centimetre. The results show that within these wide limits κ is constant for bismuth.

A similar series of experiments upon white wax indicated that κ for this substance is constant within wide limits of the field-strengths; and in the light of these results it would seem probable that for solids, at any rate, κ is constant.

The method for the study of magnetic and diamagnetic phenomena in solid bodies, described in the present paper, is easily adapted to the study of liquids, and of gases under different pressures.

At various intervals during the present century many different physicists have been interested in the study of diamagnetism. But notwithstanding the fact that many have worked upon the problems which a study of diamagnetism offers, there seem to be some things in connexion with the subject which merit more careful investigation. For instance, the question as to whether or not a diamagnetic body, after being under the influence of a magnetizing force and this force withdrawn, requires, if left mechanically undisturbed, a "coercive force" to bring it to the neutral magnetic state again, is still an open one. Of course the extreme smallness of the action to be measured is the great difficulty encountered in any quantitative work in diamagnetism.

For the illustrations used in this paper I have to thank Mr. Joseph Daniels, of the Colorado State Normal School. I am under great obligations to Dr. A. G. Webster, Professor of Physics at Clark University, for suggestions and advice given while the work was being carried out, also to Clark University for providing the necessary apparatus.

XLV. *On the Function of the Condenser in an Induction-Coil.*

By T. Mizuno, *Riyakushi*[*].

THE efficiency of an induction-coil is greatly increased by the employment of a condenser in its primary circuit, in accordance with Fizeau's suggestion, but the part played by the condenser is not yet clearly known. The most commonly accepted opinion is that the extra current in the primary circuit, instead of being expended in making a spark at the interruptor, darts into the condenser and hastens the decay of the primary current, thereby raising the terminal potential-difference of the secondary circuit. Moreover, according to this view, if a condenser is inserted across the interruptor, then on breaking the primary current it continues to run on into the condenser for a while; but then rebounds, and is reversed in sign, retaining its initial full strength. Hence the electromotive force set up in the secondary circuit is the joint result of a stoppage of a primary current and its immediate reversal in direction. But this simple explanation, however plausible at first sight, is yet an imperfect account of the function of the condenser. In this paper, I propose to

---

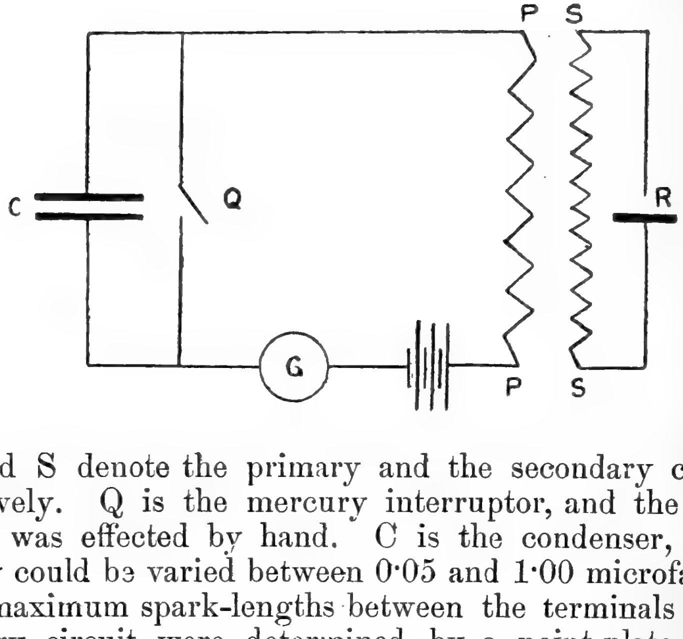
[*] Communicated by the Author.

communicate the results of my investigations on the condenser, which seem to me to throw some new light on its action. The object of my investigation was to examine the effect which variations of the capacity of the condenser inserted in the primary circuit produced upon the maximum spark-lengths of the secondary, when the primary current was kept constant.

Experiments.

An induction-coil 11 centim. in diameter, 21 centim. in length, and having a mercury interruptor was used for experiment. The condenser of the coil was removed, and an adjustable condenser was put in its place. The primary circuit was fed by nine accumulators, the strength of the current being regulated by means of a rheostat.

The arrangement of the experiment is shown in the annexed figure :—



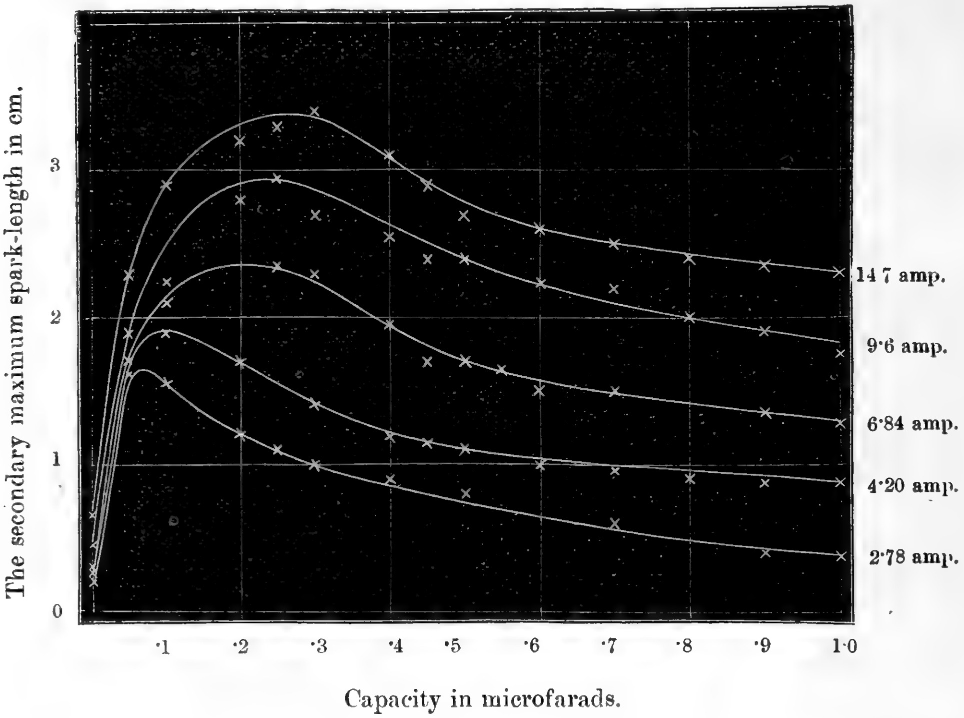
P and S denote the primary and the secondary circuits respectively. Q is the mercury interruptor, and the interruption was effected by hand. C is the condenser, whose capacity could be varied between 0·05 and 1·00 microfarad.

The maximum spark-lengths between the terminals of the secondary circuit were determined by a point-plate spark micrometer R, each measurement being made five times, and the primary current was measured by a Lord Kelvin's graded current-meter G.

Since the spark-length depends, to a large extent, upon the surface conditions, especially on that of the point, the latter was each time thoroughly polished with sand-paper. Even with this precaution the spark-lengths occasionally showed some irregularities. The results of observation are given in the following table :—

Capacity in microfarads.	Secondary maximum spark-length in centim.	Capacity in microfarads.	Secondary maximum spark-length in centim.	Capacity in microfarads.	Secondary maximum spark-length in centim.	Capacity in microfarads.	Secondary maximum spark-length in centim.	Capacity in microfarads.	Secondary maximum spark-length in centim.
0	0·20	0	0·25	0	0·30	0	0·45	0	0·65
0·05	1·62	0·05	1·70	0·05	1·70	0·05	1·90	0·05	2·30
0·10	1·55	0·10	1·90	0·10	2·10	0·10	2·25	0·10	2·90
0·20	1·20	0·20	1·70	0·20	2·35	0·20	2·80	0·20	3·20
0·25	1·10	0·25	1·55	0·25	2·35	0·25	2·95	0·25	3·30
0·30	1·00	0·30	1·40	0·30	2·30	0·30	2·70	0·30	3·40
0·40	0·90	0·40	1·20	0·40	1·95	0·40	2·55	0·40	3·10
0·45	……	0·45	1·15	0·45	1·70	0·45	2·40	0·45	2·90
0·50	0·80	0·50	1·10	0·50	1·70	0·50	2·40	0·50	2·70
0·60	……	0·60	1·00	0·55	1·65	0·60	2·22	0·60	2·60
0·70	0·60	0·70	0·97	0·60	1·50	0·70	2·20	0·70	2·50
0·80	……	0·80	0·90	0·70	1·50	0·80	2·00	0·80	2·40
0·90	0·40	0·90	0·88	0·90	1·35	0·90	1·90	0·90	2·35
1·00	0·38	1·00	0·88	1·00	1·28	1·00	1·75	1·00	2·30
Primary current in amperes. 2·78		Primary current in amperes. 4·20		Primary current in amperes. 6·84		Primary current in amperes. 9·60		Primary current in amperes. 14·70	

Plotting these results we obtain the following curves :—

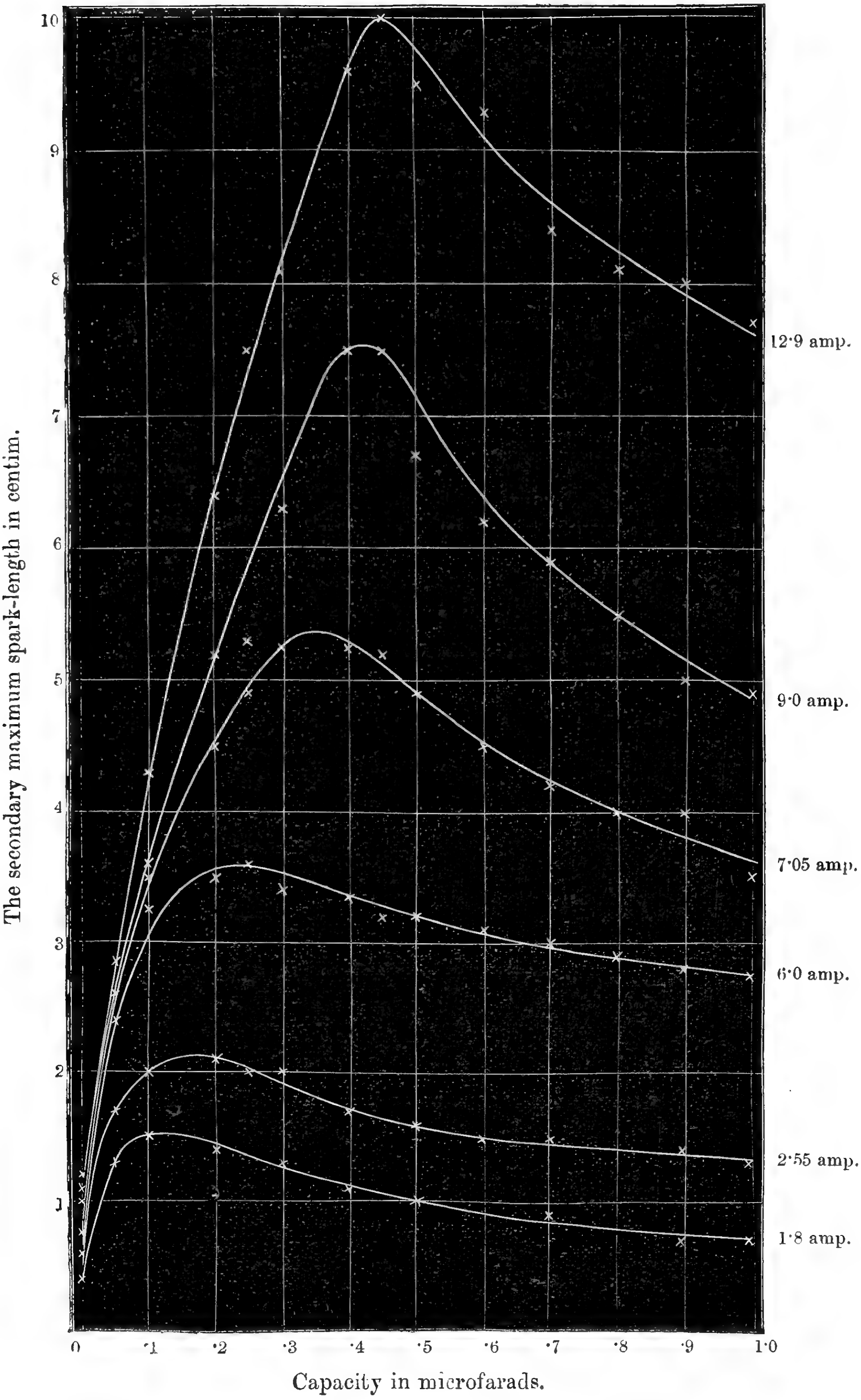


Thus the relation which the capacity of the condenser bears to the maximum spark-length between the terminals of the secondary circuit with various primary currents is made clear.

Next I took another induction-coil, 11·5 centim. in diameter and 21·5 centim. in length, with a platinum interruptor. The condenser of the coil was taken off, and the adjustable condenser was inserted just as in the case of the first experiment. Also the mercury interruptor was substituted for the platinum interruptor, and then experiments were carried on. The results were as follows :—

Capacity in microfrds.	Secondary maximum spark-length in centim.	Capacity in microfrds.	Secondary maximum spark-length in centim.	Capacity in microfrds.	Secondary maximum spark-length in centim	Capacity in microfrds.	Secondary maximum spark-length in centim.	Capacity in microfrds.	Secondary maximum spark-length in centim.	Capacity in microfrds.	Secondary maximum spark-length in centim.
0	0·40	0	0·60	0	0·75	0	1·00	0	1·10	0	1·20
0·05	1·30	0·05	1·70	0·05	2·40	0·05	2·60	0·05	2·60	0·05	2·85
0·10	1·50	0·10	2·00	0·10	3·25	0·10	3·50	0·10	3·60	0·10	4·30
0·20	1·40	0·20	2·10	0·20	3·50	0·20	4·50	0·20	5·20	0·20	6·40
0·25	......	0·25	2·00	0·25	3·60	0·25	4·90	0·25	5·30	0·25	7·50
0·30	1·30	0·30	2·00	0·30	3·40	0·30	5·25	0·30	6·30	0·30	8·10
0·40	1·10	0·40	1·70	0·40	3·35	0·40	5·25	0·40	7·50	0·40	9·60
0·45	......	0·45	......	0·45	3·20	0·45	5·20	0·45	7·50	0·45	10·00
0·50	1·00	0·50	1·60	0·50	3·20	0·50	4·90	0·50	6·70	0·50	9·50
0·60	......	0·60	1·50	0·60	3·10	0·60	4·50	0·60	6·20	0·60	9·30
0·70	0·90	0·70	1·50	0·70	3·00	0·70	4·20	0·70	5·90	0·70	8·40
0·80	......	0·80	......	0·80	2·90	0·80	4·00	0·80	5·50	0·80	8·10
0·90	0·70	0·90	1·40	0·90	2·80	0·90	4·00	0·90	5·00	0·90	8·00
1·00	0·70	1·00	1·30	1·00	2·75	1·00	3·50	1·00	4·90	1·00	7·70
Primary current in amperes. 1·80		Primary current in amperes. 2·55		Primary current in amperes. 6·00		Primary current in amperes. 7·05		Primary current in amperes. 9·00		Primary current in amperes. 12·90	

Graphically representing the observed values of maximum secondary spark-lengths with different primary currents and corresponding capacities, we have the following curves:—



Discussion.

The curves for the two coils both indicate that for a given primary current there is a corresponding special capacity which makes the secondary spark-length a maximum. They also show that the value of the capacity for maximum spark-length becomes larger as the primary current becomes stronger. Again, the character of the curves is such that each of them rises very rapidly at first until a maximum point is reached, and then falls down somewhat gradually.

Hence below this maximum point the effect of capacity is to increase the secondary spark-lengths at a rate much greater than that at which it diminishes them beyond this point. Since the sole object of an induction-coil is to get the greatest possible spark-length, that is to say, the highest attainable potential-difference at the secondary terminals, makers of induction-coils must pay great attention to the above fact.

An insufficient as well as an excessive capacity spoils the action of coils to a large extent; and it is therefore necessary to carefully find out by experiment the most suitable capacity for each coil before construction. From a mere consideration of minimizing a spark at the interruptor of the primary circuit one might be induced to add an extra capacity to the circuit. This addition of capacity, if the condenser belonging to the circuit is already sufficient or too great, would not only be useless but will do harm. What amount of capacity, then, must be inserted in the coil? To settle this question is not a simple matter. As the curves show, the fittest capacity depends upon the strength of the primary current, and consequently it is necessary to choose a capacity giving a maximum resonance-effect, so to speak, for the possible strongest current allowable to the primary circuit. In the two induction-coils examined by me their condensers both had wrong capacities.

For on measurement the capacity of the condenser in the first coil was found to be 0·078, while that of the second was 0·75 microfarad. Judging from the results of my observations and also from actual experiments with those condensers, the first coil had a smaller, the second a much greater capacity than was necessary. One recent valuable research on induction-coils is that of Colley *, who examined both theoretically and experimentally the action of his induction-coil.

A few months ago Walter † also published an interesting paper on the same subject. In fact, he found that there is a

---

* Colley, Wied. *Ann.* xliv. p. 109 (1891).

† Walter, Wied. *Ann.* lxii. p. 300 (1897).

certain capacity giving a maximum spark-length for a given primary current, just as my experiments prove.

But he did not examine the relation of capacities with several primary currents, as I have done. From theoretical consideration he drew the conclusion that a maximum secondary potential-difference E_2 is equal to $J_1 \sqrt{Z_2/C_1}$, where J_1 is the primary current, Z_2 the coefficient of self-induction of the secondary circuit, and C_1 the capacity of the condenser in the primary. This conclusion, however, does not appear to me to be valid.

For a comparison of the above formula with experimentally observed facts brings out a rather wide discrepancy as to the capacity effect. In my view the existing theories of the induction-coil as they stand are yet insufficient to account for all the observed phenomena, especially the effect of capacity on secondary spark-lengths.

This might perhaps arise from the incompleteness of the data in establishing the theories, that is to say, mathematically writing down the differential equations for the primary and secondary circuits. For according to the existing theories the coefficients of self-induction and mutual induction are both regarded as constant, and almost no account is taken of the relation which they bear to the effect of capacity, and also the variable magnetization of iron bundles in the primary circuit.

To sum up, in order to elaborate the theory of the induction-coil to a greater perfection, I believe a considerable number of further experiments on the mutual relations of capacity, self-induction, and the magnetization of iron bundles in the coil must be made.

The Physical Laboratory,
Daiichi Kōtō Pakkō, Tōkyō, Japan,
January 1898.

XLVI. *On the Influence of Dissolved Substances and of Electrification on the Re-formation of Clouds.* By HAROLD A. WILSON, *B.Sc. Lond. & Vict., Trinity College, Cambridge, 1851 Exhibition Scholar* [*].

THE object of the experiments described below was to see whether clouds consisting of dilute solutions of various substances after being destroyed by bubbling through sulphuric acid would reappear on passing into air saturated with moisture, and also to see whether electrifying the cloud had any effect on its power of reappearing.

[*] Communicated by Prof. J. J. Thomson, F.R.S.

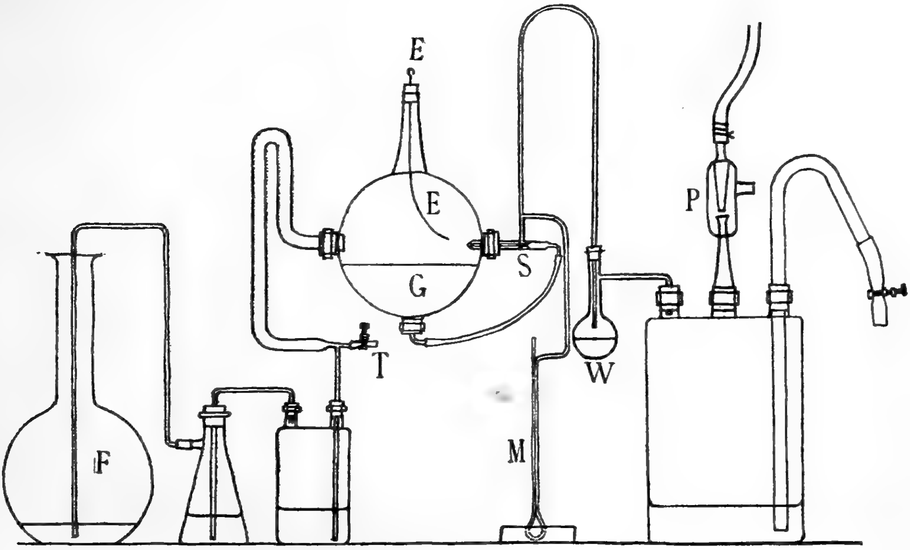
The fact that vapours of various substances, *e. g.* sulphuric acid, form clouds in moist air is well known, and I do not propose to make any attempt to describe previous work connected with this subject.

As J. S. Townsend has shown *, the oxygen and hydrogen evolved by the electrolysis of dilute sulphuric acid or caustic potash solutions are electrified, and also form clouds in presence of moisture. These clouds can be destroyed by bubbling through sulphuric acid and reappear on passing into water without much loss of charge. Gases evolved in certain chemical reactions behave in a similar manner (see Townsend, *loc. cit.* and also Enright, Phil. Mag., 1890).

The gases evolved by electrolysis or by the action of acids on metals carry with them a spray of the solution in which they are formed, and previous experimenters have arrived at different conclusions with regard to the part played by this spray in the above phenomena.

In the papers referred to above, Townsend arrives at the conclusion that the formation of the clouds in the newly-prepared gases is probably an electrical phenomenon, and is not the result of the presence in the gas of spray.

Another view is that the spray of acid condenses moisture on itself, so forming the clouds, and that the electric charge in the gas does not have any important effect on the formation of the clouds.



The apparatus I employed is shown in the accompanying figure. It consisted of a Gouy sprayer S, which was worked by the air supplied by a water-pump P.

---

* "On Electricity in Gases and the Formation of Clouds in Charged Gases," Proc. Camb. Phil. Soc. vol. ix. pts. v. & vii.

The pressure of the air was indicated by the mercury manometer M, and was usually 30 cms. The air could be saturated with moisture if necessary by gently warming water in the flask W. The spray was projected into a four-necked globe G, which also contained the solution being sprayed. The spray given by this sprayer was very abundant and fine, and formed a cloud which did not settle for a considerable time.

The air containing the cloud was bubbled through sulphuric acid in the two wash-bottles, and then through water in the flask F. The rate of bubbling was regulated at the outlet-tube T, where most of the air was allowed to escape.

On spraying distilled or tap-water no cloud could be detected in F usually, though sometimes a very faint cloud appeared. If a dilute solution of sulphuric acid was sprayed, then a cloud appeared over the water in F. If the air from which the dilute-acid cloud had been removed by bubbling through the sulphuric acid was collected in a dry flask, no cloud could be detected in it; but on adding a little water and shaking gently so as to saturate the air in the flask with moisture, a cloud was formed. This occurred even if the dry flask containing the air was allowed to stand for half an hour before the water was put in it. One gram of H_2SO_4 in a litre of water produced a dense cloud.

Dilute solutions of H_3PO_4, KOH, $CaCl_2$, NaCl, K_2CO_3, sugar, and glycerine all gave a similar effect, and the presence of the alkali metals in the cloud could be easily shown by passing it into a bunsen flame. The solutions when made up so as to contain equal numbers of molecules of the dissolved substance per litre gave apparently about equally dense clouds. A considerable change in the strength of a solution was, however, necessary to produce a sensible change in the density of the cloud.

A solution of potassium uranium sulphate gave a similar cloud in F, as also all other solutions of non-volatile substances we tried. Dilute hydrochloric acid gave no cloud in F unless the solution contained about 5 per cent. or more of HCl, when a cloud appeared, no doubt due simply to dry HCl gas entering F. Acetic acid solutions gave no cloud in F.

The explanation of the re-formation of the cloud in F is evidently very simple. The small drops of solution forming the original cloud evaporate to the sulphuric acid, and minute particles of salt or other substance remain which are too small to form a visible cloud. These particles on coming into a moist atmosphere condense water on themselves, re-forming the visible cloud.

A tube containing a plug of glass-wool introduced between the sulphuric acid bottles and F did not prevent the reappearance of the cloud in F, unless it was several inches long and tightly packed.

To try the effect of electrification on the clouds a wire E was fixed opposite the nozzle of the sprayer and connected to one of the poles of a battery of 400 cells giving about 700 to 800 volts. The other pole of the battery and the solution in the sprayer were connected to earth. The original cloud was now found to be strongly charged of opposite sign to the wire. The flask F was insulated and wrapped in tinfoil, which was connected with one pair of quadrants of a quadrant electrometer, the other pair being connected to earth. The air was passed into F through a metal tube, which was connected to earth and did not touch F.

On spraying distilled water a charge on F was indicated by the electrometer. With about six bubbles passing per second the electrometer deflexion increased about 50 divisions per minute (1 volt gave 30 divisions), showing a negative charge on F when E was positive, and *vice versâ*. On stopping the sprayer and blowing unelectrified air into F about half of the electrification could generally be blown out. No cloud appeared in F when the air was bubbled through water, although the air carried with it this charge*.

On putting in a small glass-wool plug between F and the sulphuric acid bottles all the electrification was stopped by the plug unless the bubbling was very rapid.

On spraying dilute sulphuric acid a charge appeared on F as with water, and the usual cloud also appeared. On putting in a glass-wool plug between F and the sulphuric acid bottles, however, the electrification was not stopped as it was when pure water was sprayed, but the electrometer indicated 20 to 30 divisions per minute. On stopping the sprayer and blowing into F practically all this electrification could be blown out, even after some little time had elapsed. Other solutions, including those of sugar and glycerine, gave similar results.

It thus appears that the electrification of these clouds is not able to cause them to reappear in moist air after bubbling through sulphuric acid, although the electrification is not removed from the gas by the passage through the acid. In order that the cloud may reappear it is necessary that the drops forming it should contain some material capable of

---

* When the wire E was unelectrified a small amount of electrification could still be detected on the gas entering F, giving about 4 or 5 divisions per minute.

forming a nucleus on which the drop can be re-formed after it has been evaporated by the acid.

The behaviour of the electrification with a glass-wool plug is interesting. With a solution the particles left after passing through the acid must be larger than those left when water is sprayed. Yet the electrification gets through the plug only in the former case. The fact that the electrification which cannot get through the plug is also not easily blown out, whilst that which goes through the plug is readily almost entirely blown out, shows that the smaller electrified particles, as we should expect, are much more rapidly driven to the sides of the flask and discharged than the larger particles. In the case of the plug, also, the smaller particles, owing to their more rapid motion and greater velocity under electric forces, are much more readily caught by the fibres.

The charged gases obtained by the action of Röntgen rays are unable to carry their charge through a glass-wool plug, whilst the far larger particles which form the nuclei in the above experiments are able to do this. It has been supposed that the fact that the charge of Röntgenized gases is stopped by glass wool shows that the charge is upon large particles, but the above observations lend additional support, if any is needed, to the view that the stoppage of the charge is due to the rapid velocity with which such very small particles move causing them to strike the fibres of the wool plug.

The fact which I have observed, that a charged cloud of water after evaporation over sulphuric acid does not reappear over water although the charge is not stopped, does not seem to favour the view that the formation of clouds in newly prepared gases is an electrical phenomenon. It is, however, possible that in my experiments the number of particles on which the charge was distributed was small compared with the number present in newly prepared gases. Against this we have the fact that nearly all the charge which passed through glass wool could be readily blown out, which seems to show that it was distributed over a large number of particles.

The electrification of hydrogen prepared by the action of 50 per cent. sulphuric acid on iron wire at 80° C., after passing through sulphuric acid and a glass-wool plug, could be almost entirely blown out (Townsend, *loc. cit.* p. 246). In this it exactly resembles the electrification on a cloud of dilute sulphuric acid as I have shown. This similarity between the behaviour of the newly prepared gases and the air containing a cloud consisting of a dilute solution suggests the conclusion that the clouds formed in the newly prepared gases also consist of a dilute solution of some substance, and that their reappearance over water after being bubbled through sulphuric acid is due to small particles of the dissolved substance left when the cloud evaporates over the acid. Townsend's experiments confirm this view, for he finds that the particles left when the cloud is evaporated are large compared with molecular dimensions.

In the case of a gas coming off from more or less dilute sulphuric acid it seems reasonable to suppose that the nuclei which cause the formation of the clouds consist of this substance, but exactly how the gas becomes charged with such an immense number of these particles as is necessary to form a dense cloud is not known.

The most obvious explanation, viz. that the particles are simply spray splashed off, does not seem able to account for many of the facts observed by Townsend. I am not, however, here concerned with the origin of these particles in newly prepared gases, and shall not, therefore, consider the question any further. The chief results obtained in these experiments are :

(1) A cloud of pure water when evaporated by bubbling through H_2SO_4 does not reappear over water.

(2) A cloud of a dilute solution of a nonvolatile substance when evaporated reappears over water.

(3) Electrification of the original cloud in either of the above cases does not affect the reappearance of it over water after evaporation, although the electrification is not stopped by the acid.

In conclusion, I desire to say that my best thanks are due to Prof. Thomson for much information and advice during the course of these experiments.

XLVII. *On the Temperature of Europe.*

By Dr. van Rijckevorsel[*].

WHAT I am going to put before you is not absolutely new. Indeed, I believe that most meteorologists have felt that something like it must be the case; and many have said something amounting to the expression of such a feeling in some of their works. Certainly Roche, Jelinek, Buys-Ballot are among the number, not to speak of others.

But nobody, as far as I know, has followed to the end the path of which many seem to have seen the entrance; and so nobody has as yet perceived how it seems to be a new road to some knowledge about temperature and climate.

What I have done is very simple. I have collected and

---

[*] Communicated by Prof. G. F. FitzGerald, F.R.S., having been read before Section A of the British Association at Toronto.

compared the daily temperatures of a certain number of places mostly in Europe. For the majority of these the figures could be simply taken from the papers published by different institutions or private observers. For some stations, however, I had to compute the daily means from the observations themselves as published periodically by the observers.

However, none of these means could be forthwith used in the state they were in. There is not one series of observations, as yet, covering such a number of years that we may, without more ado, take the means as normal temperatures. Series have been published covering a century and more. (St. Petersburg, Copenhagen, among others.) Unfortunately no series of even nearly that length seems to be extant for a more southern latitude. And for a station in the north of Europe, if the mean temperatures for each day of the year are plotted down, the resulting curve is, even in cases like St. Petersburg and Copenhagen, so hopelessly distorted that not much can be done with it.

Therefore it is necessary to manipulate the figures to a certain degree. I have done so after the simplest possible method. The figures were smoothed down by taking, firstly, the arithmetical means of every set of nine days and assigning this corrected value to the fifth of those nine days. (Why I choose the number nine need not be here related. It has been printed elsewhere*.)

In most cases even this was not enough, and I had to smooth the curves once more. This was done by taking the mean of every set of 3, 5, 7, or even 9 days, and substituting this second mean for the figure of the 2nd, 3rd, 4th, or 5th day of the set.

Of course this may seem to be stretching a useful method to a rather dangerous extent. I think, however, that the results sufficiently show that, for the end I had in view, the expedient was a good one. It is quite possible that by it some peculiarities have been obliterated which may be interesting. But this does not in any way invalidate the results I am going to show you.

Before proceeding any further it is only fair to add that not all my figures must be taken as real normal, or even mean, temperatures. For some stations not nearly enough data were at hand. In some cases each figure is the mean of the readings on three, or even two, chosen hours. In some others it is the mean of the daily maximum and minimum, or even the temperature at noon only. In a few cases also the series

---

* *Annuaire Météorologique* pour 1894, publié par l'Institut Royal Météorologique des Pays-Bas, quarante-sixième année.

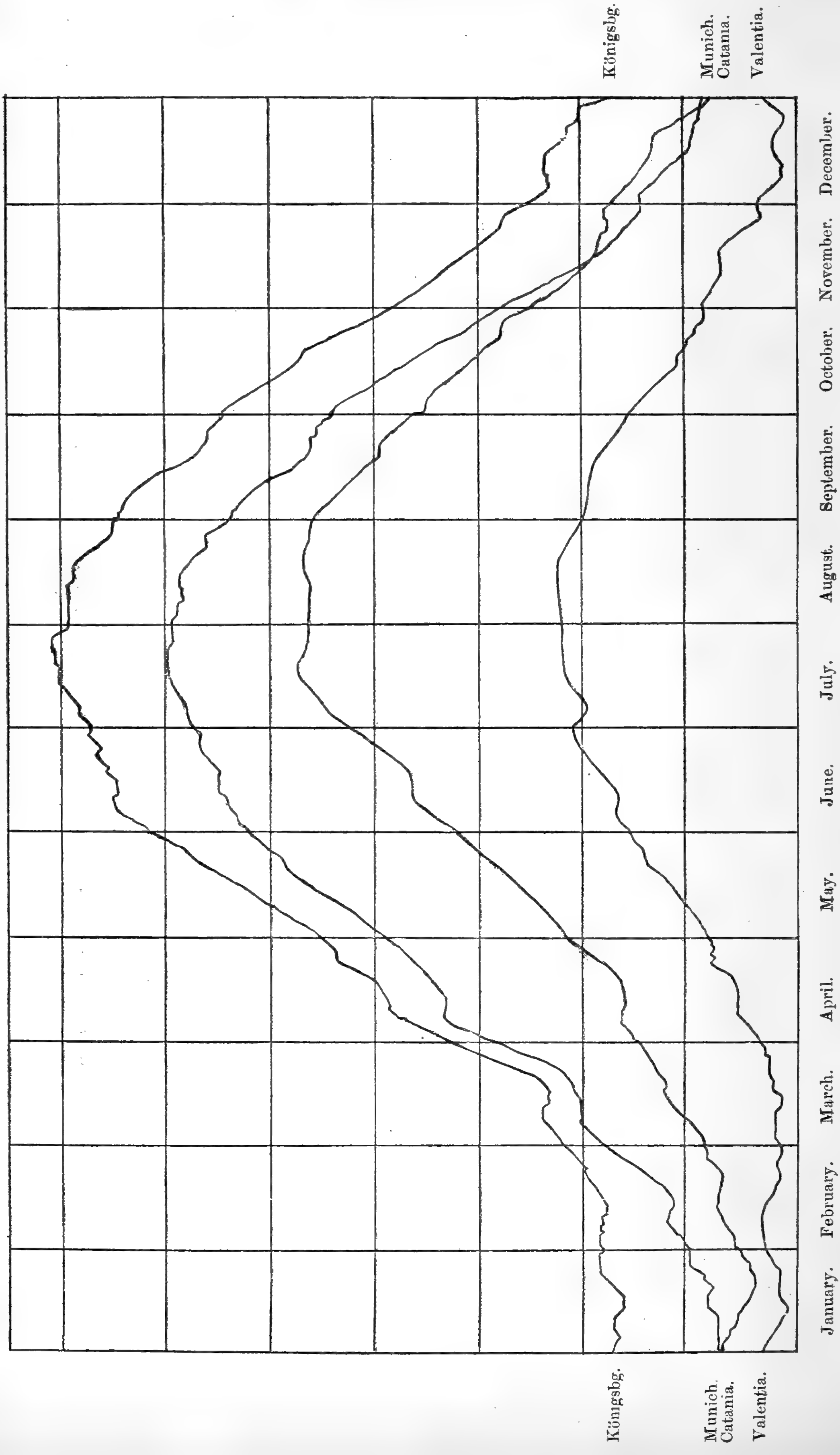
is not at all homogeneous. All this, however, will also be easily understood not to affect my present results.

I will say at once that it is principally the similarity of these temperature-curves which I think interesting. Of more than fifty stations for which I have computed the normal temperature and constructed the lines, I have drawn only four on this diagram in order not to complicate matters. They are Valentia, Catania, Munich, and Königsberg. It is on purpose also that I have left out eastern Europe, which would be an unnecessary complication.

Now I will, later on, speak of these four lines more in detail; but you will at a glance be struck by the extraordinary similarity of lines for stations so far apart and in such different climates. With a few exceptions, every irregularity in one of these curves is exactly repeated in each of the others.

The first inference I draw from this fact is, that the way in which I treated the figures has been adequate.

But, secondly, it struck me that a method, hitherto in great favour, of representing the temperature of a station by a carefully calculated smooth curve cannot be of very great use, because it really misrepresents the true state of things. Of course we may, by introducing a certain number of periodical functions, obtain for any of these stations a fine smooth curve without any irregularity. But does such a curve in reality show us very much more than that it is colder in winter than in summer? I think my curves, by their similarity, plainly show that such a smooth curve can never really represent the temperature as nature makes it for us. Wherever I take a station within or near the triangle Valentia, Königsberg, Catania, whether I take a series of a hundred years or one of twenty, whether I take it in the 19th century or in the 18th, the result always is a similar curve with identically the same irregularities. This proves plainly that these cannot be accidental, cannot be owing to an insufficient number of observations, or to errors of observation. Therefore here again it is not so much the phenomenon as a whole which is the most interesting, but the anomalies, as represented by the divergencies from the smooth curve, by the large amount of secondary maxima and minima. That the temperature rapidly rises in the second half of January and falls again in February, that the same phenomenon is repeated in the end of February, is an apparent fact, and must not be smoothed away; and it will be, I think, the task for meteorologists to investigate what the cause of such anomalies may be. And to a certain extent I think the method here put forward affords some clue to these causes.


Königsbg.
Munich.
Catania.
Valentia.
January.
February.
March.
April.
May.
June.
July.
August.
September.
October.
November.
December.
Königsbg.
Munich.
Catania.
Valentia.

I think also that these curves show that it is not really necessary to have such a very large number of years to calculate the normal temperatures with if they are well manipulated. In this diagram, for instance, the curve for Königsberg is the result of 46 years' observations, that for Catania of only 20 years; and you will see that the result is quite as satisfactory. I have even got curves from only ten years' observations which show exactly the same peculiarities as others. Of course a larger number of years cannot fail to make the normals still better. Suppose we had a series of two or three centuries, it is very probable that minor anomalies would show themselves which might be quite as real, quite as permanent as the larger ones I am able to point out now; we might be able then to take the mean values directly as normal temperatures without any smoothing. But we may safely predict that the grosser features which we can detect now would in no way be altered.

What I have just said is also proved by three curves for the Helder which I have drawn *. In the diagram the lower curve is the result of the observations of the 25 years from 1845–1869, the upper one of the next 25 years from 1870–1894. Now this series is not a homogeneous one. The thermometer was more than once removed from one place to another, and a really good situation seems only to have been found some years ago. Also the mean temperature for the year is several tenths of a degree higher during the first 25 years than during the second series. Still it will be seen that the two curves are strikingly similar. In June and July there is, indeed, a difference. It is evident that during the first period the thermometer cannot have been quite free from some direct influence of insolation. But even here you will remark that all the essential features appear in both curves. Only in the lower one they occur earlier, and seem a little distorted. (Perhaps this might even be a means to detect errors in the location of the thermometer.) And in every other part of the diagram you will see that the two curves are nearly absolutely parallel. This shows plainly, I think, how a rather short series of observations, how even observations of only indifferent quality, can be made use of, if properly handled.

The middle curve shows the mean of the two.

Again, it must even be possible to give normal temperatures for any place in Europe *à priori*, with not much more than a very approximate knowledge of the climate. Suppose

---

* This diagram is not reproduced here as the text seems sufficiently clear without it.

we have some knowledge of the year's mean temperature of some place in Europe, or of its summer temperature, it ought to be possible now to make a very near guess at the mean temperature for every day of the year, to compute normals which ought not to be very far from the true ones.

Now, in order to come to the diagram, I want first to state why I did not give any curves for eastern Europe. It is because the type of the curves in the East is a different one. And I must say I think this is a very interesting fact, because it points to the great advantage of this method. To state things in a concise manner, I will speak of summer only. You will see that practically, for Valentia, the summer lasts, without much variation, from the middle of June to the middle of August. Now as we move eastward you will see that this maximum gradually narrows, and shows a tendency to merge into one single peak, and so it quite does beyond the space covered by these four lines. The maximum there is not for every place in the same part of the curve. Sometimes it is in July, sometimes in August. But the character is the same, and vastly different from that in western Europe, viz., one single maximum, sloping down rather rapidly on both sides. But here again the different characteristics are not by any means confined to a small area. There is for instance a striking similarity between the curves for Constantinople and Archangel, although in two sections they are just as decidedly dissimilar.

In connexion with this fact I must call your attention to one very remarkable concave part of the curve for Valentia. It is the downward movement in the beginning of July. You will see that this peculiarity does not repeat itself in the other three curves. It is only the more interesting for that. It exists very decidedly in all the curves in the United Kingdom, in the Netherlands, is still faintly visible in those for the south of France, but rapidly diminishes in importance and vanishes altogether in an easterly direction. The same remark applies to the next secondary minimum in the end of July. This one, however, is more tenacious: it is very characteristic, I think, for the temperature of the whole of western Europe, and only dies out beyond a line, very crooked indeed, passing somewhere in the neighbourhood of Copenhagen, Königsberg, Vienna, and probably Greece.

The reason why I spoke with a smaller degree of certainty of the peculiar behaviour in the east of Europe is, that possibly there is nothing extraordinary in that. It may be that it is the natural summer which we should also have in western Europe if the two minima just spoken of, whatsoever they may be owing to, did not exist. I have not yet been able to go far enough into the subject, of which I give the first results.

But I think a question like this—and there are others of the same character—points to a more extensive use of our curves. Suppose such curves to be constructed for our whole hemisphere in sufficient number. It is not quite impossible that we should find then that some of the anomalies of my curves are not confined to Europe only. I call your attention here to the fall in the temperature in the first half of June. On this diagram it is very sensible for Valentia, less so for the other stations. This is a most tenacious anomaly, which, although in most cases not at all striking, fails in none of my curves, either in Russia, or in Italy, or England.

Another instance is the rise of temperature about the first days of February, followed by a minimum, which is best to be seen in the curve for Münich. This anomaly is very irregular indeed, appearing for some curves in January, for others in February, and much more apparent in one place than in another. But it is always there.

Now it is not very probable that for both these anomalies I should, by confining myself to European stations, have exactly hit the limits of the area on which they make themselves felt. And should it be that any anomaly of this kind makes itself felt in America and in Asia also, we ought then probably to look for the explanation of it beyond the limits of our globe. I do not mean to imply that I expect such a thing to be the case, but it is not absolutely impossible.

Other anomalies, which appear only on a part of a hemisphere, probably must have a purely terrestrial origin. But if we had a sufficient number of curves I expect that we should be able for most of these anomalies to see at a glance where they originate, and after that, a guess at the cause might in many cases be easier than it is now.

Allow me to give an instance. The three "ice-saints" which have always found such strong belief, and quite as much disbelief, are certainly indicated, faintly, as one would expect, in many curves for central Europe, even as far as Flushing and Königsberg, more strongly, however, for Münich, Bremen, Lyons, Montpellier. But does this prove that their cause lies in the melting of the snow on the Alps, as some meteorologists think? The downward slope of the curve is just as apparent for Rome, Palermo, Biskra in Algeria, especially so for Constantinople and for Arváralja, a station to the north of Vienna. As it is not quite so apparent for many other stations, it seems difficult as yet to give an opinion on the question, still it should seem as if the Alps could hardly be the birthplace of this sudden cooling down in the first half of May, but as if we must rather look for this in a south-eastern or southern direction, beyond the Mediterranean.

Of course, more local anomalies, such as the one last named, also make themselves apparent. Thus the curves for Montpellier and for Lyons—two places so near together, but the second one more inland, more to the north and near to the Alps—are exactly similar for the eight months March–October. This is the more important as I took for Montpellier, and on purpose, a series of observations in the 18th century. But for the four winter months the curves are exactly similar also, only the one for Montpellier is much more contracted. Every feature is repeated in both curves, only in November and December they all occur nearly a fortnight later at Montpellier, in January and February on the other hand as much earlier. A similar instance is given by the curves for Flushing and Brussels. The two are strikingly similar, only that for Brussels shows a far greater range than the other. Here, though the places are so very near together, the influence of the sea-climate of Flushing is immediately shown by the curves.

Thus a minimum in the second half of December, with a very decided rise of temperature towards the end of the year, which is very characteristic for the extreme eastern part of our continent, seems to die out long before it could reach the middle of Russia.

On the other hand it is remarkable how persistent some much smaller features are. Here are the two unimportant maxima, separated by a more or less apparent minimum in April. This feature, slight as it is for most stations, extends over nearly the whole of Europe, and only dies out in the extreme east. This must be another peculiarity of our climate which originates to the west of our continent. The next minimum, just as slight, at the end of the month is still more persistent: it is slightly apparent for all European stations without an exception.

I do not feel justified in taking up more of your time for a subject which I have not at all yet thoroughly investigated. But I think I have said enough to show how interesting temperature-curves may be, and how desirable it is that as many of them should be constructed as can possibly be done. Constructed, and duly compared with others, and on a large scale.

And here is also a reason why I bring the subject before the public before I see the extent even of it myself. Is it temperature alone of which the irregularities are so extremely regular? How does the barometer behave? Do the winds, do the magnetic elements, show something pointing to a common origin? Here are certainly interesting questions, but the field is too large for one man. Therefore my wish is to entice others to walk on this easy and interesting road.

XLVIII. *Notices respecting New Books.*

Science Abstracts: *Physics and Electrical Engineering.* Volume I. Parts 1 and 2. January and February 1898. Published under the direction of The Institution of Electrical Engineers; the Physical Society of London. London: Taylor and Francis.

As our readers are aware, the Physical Society of London commenced, a little more than two years ago, the publication of a series of Abstracts of the most important papers on Physics appearing in foreign scientific journals. The general excellence of these Abstracts was admitted on all hands; and the value of the work thus done by the Physical Society was recognized by grants in aid from the Royal Society and the British Association. It is understood that the expense involved in the publication of the Abstracts was a heavy drain on the resources of the Physical Society, which, notwithstanding the fundamental importance of the subject with which it deals, is, and perhaps will always remain, a comparatively small Society; and it is doubtful whether the publication could have been continued very long without outside help. It is therefore very satisfactory that a large and wealthy Society like the Institution of Electrical Engineers should have united with the Physical Society to put the "Abstracts" on a firm basis. The present publication takes the place of the Physical Society's Abstracts, and also of the extracts from electrical papers of scientific or technical interest that have hitherto appeared in the Journal of the Institution of Electrical Engineers. Speaking very roughly, we may say that Physics occupies, in the two parts under review, about two thirds of the space and Electrical Engineering one third. For the publication generally we have hardly anything but praise. The high standard attained in the Physical Society's Abstracts seems to be maintained, and we are glad to see that the typography and general arrangement remain as before; the only change obvious at first sight is that in the colour of the wrapper from the modest French grey of the Physical Society to a brilliant red; but this, though only superficial, is sufficiently startling,

On the whole, we can heartily congratulate the two Societies concerned in this publication, and hope that it will meet with the success it deserves. We trust it may be found practicable to keep up the genuinely scientific character which characterizes the two Parts before us; but we are a little alarmed by the Editor's announcement, prefixed to a list of fifty-three periodicals from which abstracts are now made, that "this list is only preliminary, and is being daily enlarged." We cannot think that this is good news: to judge by one or two of the abstracts near the end of the February Part, we should be inclined to say that the list is quite large enough already.

The second word of Abstract 230 should surely be "equitable," not "equable."

XLIX. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 284.]

January 5th, 1898.—Dr. Henry Hicks, F.R.S., President, in the Chair.

THE following communication was read:—

1. 'On the Structure of the Davos Valley.' By A. Vaughan Jennings, Esq., F.L.S., F.G.S.

Evidence is brought forward to show that the level area, about 4 miles in length, near Davos is occupied by superficial deposits, and that the lateral talus-fans there have been cut through at a relatively recent date since their accumulation; that the northern end towards Wolfgang is blocked by moraine-material of great thickness, but for which the Davoser See would drain north to the Landquart, carrying with it the waters of the Fluela and Dischma; that the contour-lines suggest the former existence of a far larger lake stretching south towards Frauenkirch, and that in that part there is proof of the previous existence of a great detrital fan sufficient to account for the existence of the lake in question.

It is shown that the former ice-movement was not from the present watershed between the tributaries of the Landwasser and Landquart, but from a spot farther south.

The author concludes that the main valley-systems were marked out in pre-Glacial times, and that at one time there was a watershed somewhere between Davos Platz and Frauenkirch. During the Glacial Period moraine-material was heaped up across the valley below the Hörnli, and held up the waters to the south, forming a great lake of which the present Davoser See is a relic, the outflow being probably over a low saddle near the present Wolfgang; during this time a great moraine and detrital fan existed across the valley to the south, and the lake for a long time was thus prevented from draining in that direction. After the Glacial Period the northern moraine was subjected to little erosion, but the southern one, formed from the first of looser material, was rapidly cut back by the Sertig Bach, and in time the barrier was so weakened as to cause that end of the lake to be tapped, and at that time the terraces opposite Frauenkirch may have been levelled, while the flow over Wolfgang would be stopped, and the Fluela and Dischma streams turned southward; the Landquart would then cut away the margins of the talus-fans which had been accumulating in the lake.

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

[FIFTH SERIES.]

JUNE 1898.

L. *Applications of Diffusion to Conducting Gases.* *By* JOHN S. TOWNSEND, *M.A., Cavendish Laboratory, Cambridge*[*].

THERE are many phenomena connected with charged and conducting gases which can be explained by diffusion. Before proceeding to its application to these gases it is necessary to solve some problems which apply not only to conducting gases, but also to gases in general. The question with which it is proposed to deal may be stated thus:—If there are two gases, A and B, contained inside a vessel the walls of which absorb A, what quantity of A will remain unabsorbed and be left distributed throughout B inside the vessel after a given time has elapsed?

The first section deals with the solution of this problem for the three particular cases where the boundary consists of a pair of parallel planes, a cylinder, and a sphere respectively. It will be supposed that the absorption of the gas A by the sides of the vessel is so complete as to reduce the pressure of A to zero at the surface. In order to obtain solutions that will apply to cases where the pressure at the surface is a small fixed value it will suffice to substitute $p+p'$ for p in the solutions obtained on the assumption that $p=0$ at the surface.

In order that the effect of gravity may not disturb the distribution of the gases it will be supposed that the quantity of A is small compared with that of B.

The second section deals with the application of these results to the cases of charged and conducting gases.

---

[*] Communicated by Prof. J. J. Thomson, F.R.S.

Section I.

1. The conditions to be satisfied by p, the pressure of the gas A, are :—

$$\kappa\left(\frac{d^2}{dx^2}+\frac{d^2}{dy^2}+\frac{d^2}{dz^2}\right)p=\frac{dp}{dt};$$

$p=0$ at the boundary $\phi(x, y, z)=0$, for all values of t;

$p=p_0$ initially throughout the space bounded by $\phi=0$.

Let the gases be contained between two parallel plates. The boundary will then be the two planes $x=0$ and $x=a$.

In this case the differential equation reduces to

$$\kappa\frac{d^2p}{dx^2}=\frac{dp}{dt},$$

the general solution of which is

$$p=\Sigma\text{A}\epsilon^{-\alpha^2\kappa t}\sin(\alpha x+\beta), \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where A, α, and β are to be determined by the conditions

$p=0$ when $x=0$ and $x=a$, for all values of t;
$p=p_0$ when $t=0$ for all values of x between $x=0$ and $x=a$.

The first condition is satisfied by making $\beta=0$ and $\alpha=\frac{n\pi}{a}$.

The coefficients A are determined from the second condition by multiplying both sides of the equation (1) by $\sin\frac{n\pi x}{a}dx$ and integrating from $x=0$ to $x=a$.

Since

$$\int_0^a \sin\frac{n\pi x}{a}\sin\frac{n'\pi x}{a}dx=0,$$

we obtain the following value of A_n, the coefficient of the term

$$\epsilon^{-\left(\frac{n\pi}{a}\right)^2\kappa t}\sin\frac{n\pi x}{a}$$

in the Fourier's series (1),

$$\text{A}_n=-\frac{2p_0}{n\pi}\left[\cos\frac{n\pi x}{a}\right]_{x=0}^{x=a}.$$

Hence, when n is even $\text{A}_n=0$, and when n is odd $\text{A}_n=\frac{4p_0}{n\pi}$.

The equation (1) thus becomes

$$p=\frac{4p_0}{\pi}\sum_{n=1}^{n=\infty}\frac{\epsilon^{\frac{-(2n-1)^2\pi^2\kappa t}{a^2}}}{2n-1}\sin\frac{(2n-1)\pi x}{a}.$$

This value of p is unaltered by changing x into $a-x$, showing, as it should, that at any time the distribution is symmetrical with respect to a plane midway between the two plates.

Let q_t denote the mass of the gas A which remains mixed with B after the gases have been allowed to remain between the two plates for a time t, and q_0 the initial mass of A between the two plates. We have

$$\frac{q_t}{q_0}=\frac{\int_0^a p\,dx}{p_0a}=\frac{8}{\pi^2}\sum_{n=0}^{n=}\frac{\epsilon^{\frac{-(2n-1)^2\pi^2\kappa t}{a^2}}}{(2n-1)^2}.$$

Hence

$$q_t=q_0\frac{8}{\pi^2}\sum_{n=1}^{n=\infty}\frac{\epsilon^{\frac{-(2n-1)^2\pi^2\kappa t}{a^2}}}{(2n-1)^2}.$$

2. Let the gases be contained inside a cylinder of radius a. The differential equation then becomes

$$\frac{\kappa}{r}\frac{d}{dr}r\frac{dp}{dr}=\frac{dp}{dt},$$

where r is the cylindrical coordinate which denotes the distance of any point from the axis.

Let $p=f(r)\epsilon^{-\alpha^2\kappa t}$, and we obtain the equation

$$\frac{1}{r}\frac{d}{dr}r\frac{df}{dr}=-\alpha^2 f$$

to determine f.

The solution of this equation is

$$f=\mathrm{A}\mathrm{J}_0(\alpha r)+\mathrm{B}\mathrm{V}_0(\alpha r),$$

and since the gas has a finite pressure at the centre we must reject the second term, so that we get

$$p=\Sigma\mathrm{A}\mathrm{J}_0(\alpha r)\epsilon^{-\alpha^2\kappa t}.$$

The condition that $p=0$ at the surface is satisfied if α be so determined as to satisfy the equation $\mathrm{J}_0(\alpha r)=0$.

Hence

$$p=\mathrm{A}_1\mathrm{J}_0(\alpha_1 r)\epsilon^{-\alpha_1{}^2\kappa t}+\mathrm{A}_2\mathrm{J}_0(\alpha_2 r)\epsilon^{-\alpha_2{}^2\kappa t}+\&\mathrm{c}.,$$

where $a\alpha_1$, $a\alpha_2$, &c. are the roots of $\mathrm{J}_0(x)=0$.

The coefficients A are determined by using the second condition which p must satisfy: $p=p_0$ when $t=0$.

Hence

$$p_0=A_1J_0(\alpha_1 r)+A_2J_0(\alpha_2 r)+\text{ \&c.}$$

for all values of r.

Multiplying each side of this equation by $rJ_0(\alpha_n r)dr$, and integrating from $r=0$ to $r=a$, we obtain

$$A_n=\frac{-2p_0}{a\alpha_n J_0'(a\alpha_n)}=\frac{2p_0}{a\alpha_n J_1(a\alpha_n)},$$

since

$$\int_0^a rJ_0(\alpha_n r)\,J_0(\alpha_n r)dr=0,\quad \int_0^a rJ_0^2(\alpha_n r)dr=\frac{a^2}{2}J_0'^2(\alpha_n a),$$

and

$$\int_0^a rJ_0(\alpha_n r)dr=\frac{-a}{\alpha_n}J_0'(\alpha_n a)^*.$$

Hence the value of p expressed as a function of r and t is

$$p=\frac{2p_0}{a}\left[\frac{J_0(\alpha_1 r)}{\alpha_1 J_1(\alpha_1 a)}\epsilon^{-\alpha_1^2\kappa t}+\frac{J_0(\alpha_2 r)}{\alpha_2 J_1(\alpha_2 a)}\epsilon^{-\alpha_2^2\kappa t}+\text{\&c.}\right].$$

The ratio of q_t, the mass of the gas A which remains unabsorbed at the time t, to the original mass in the cylinder is

$$\frac{2\int_0^a prdr}{p_0a^2}=4\left[\frac{\epsilon^{-\alpha_1^2\kappa t}}{a^2\alpha_1^2}+\frac{\epsilon^{-\alpha_2^2\kappa t}}{a^2\alpha_2^2}+\text{\&c.}\right].$$

3. When the gases are contained inside a spherical boundary the differential equation becomes

$$\frac{\kappa}{r^2}\frac{d}{dr}\left(r^2\frac{dp}{dr}\right)=\frac{dp}{dt},$$

which can also be written

$$\kappa\frac{d^2}{dr^2}(rp)=\frac{d(rp)}{dt}.$$

The solution of which is

$$rp=\Sigma A\sin(\alpha r+\beta)\epsilon^{-\alpha^2\kappa t}.$$

The condition $p=0$ at the boundary $r=a$ is satisfied if $\beta=0$ and $\alpha=\dfrac{n\pi}{a}$.

---

* Lord Rayleigh: 'Theory of Sound,' sections 203, 204.

So that equation (1) becomes

$$rp = \sum_{n=1}^{n=\infty} A_n \sin \frac{n\pi r}{a} \epsilon^{\frac{-n^2\pi^2}{a^2}\kappa t}.$$

The coefficients A_n are determined, as before, by making $p=p_0$ and $t=0$ simultaneously, and we have

$$\frac{aA_n}{2} = p_0 \int_0^a r \sin \frac{n\pi r}{a} dr$$

$$= p_0 \frac{a^2(-1)^{n-1}}{n\pi}.$$

Hence

$$rp = \frac{2ap_0}{\pi} \sum_{n=1}^{n=\infty} (-1)^{n-1} \frac{\epsilon^{-\frac{n^2\pi^2}{a^2}\kappa t}}{n} \sin \frac{\pi n r}{a};$$

and the ratio $\frac{q_t}{q_0}$ becomes in this case

$$\frac{6}{\pi^2} \sum_{n=1}^{n=\infty} \frac{\epsilon^{-\frac{\pi^2 n^2}{a^2}\kappa t}}{n^2}.$$

By means of this equation a rough estimate could be made of the amount of impurity (A) that would be removed from a gas (B) by bubbling through a liquid which absorbed the gas A. The solution shows that $\frac{q_t}{q_0}$ is constant when $\frac{t}{a^2}$ is constant; or that in order to reduce the quantity of the gas A by a given fraction the time the bubble takes to rise in the liquid must be proportional to the square of its radius. When $\frac{q_t}{q_0}$ is small, account need only be taken of the first term, and the value of $\frac{q_t}{q_0}$ is easily calculated when κ is known.

Section II.

4. The number of charged carriers or ions which are present in a gas and give rise to conductivity is very small compared with the total number of molecules of the gas. These charged carriers we will suppose constitute the gas A, and the rest of the molecules will be denoted as the gas B.

Let m be the mass of a carrier; e its charge; u, v, w the velocities of A parallel to the axes; p the pressure of A; n the number of carriers per cubic centimetre; X, Y, Z the electric forces.

We have

$$nm\frac{du}{dt}=n\mathrm{X}e-\frac{dp}{dx}-\alpha nu,$$

and two similar equations from which the motion of the gas A can be determined. The value of α as given by Maxwell (J. C. Maxwell, "Dynamical Theory of Gases," Phil. Mag. 1868, vol. xxxv.) is $h\mathrm{A}_1\frac{\rho\rho_2}{n}=h\mathrm{A}_1m\rho_2$. In this case ρ_2 is the density of B, which remains constant, so that α is also constant.

Since m is so small compared with the other quantities we may omit the first term, and we obtain

$$\mathrm{X}e=\frac{1}{n}\frac{dp}{dx}+\alpha u. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

If X is large, so that $\frac{1}{n}\frac{dp}{dx}$ is small in comparison with Xe, we have the velocity u proportional to X; and if V is the velocity of the carrier when acted on by a force of 1 volt per centimetre, we have

$$\alpha=\frac{e}{300\mathrm{V}}.$$

Hence when X, Y, and Z are zero, equation (1) reduces to

$$\frac{1}{n}\frac{dp}{dx}+\frac{e}{300\mathrm{V}}u=0. \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Let N= number of molecules in a cubic centimetre of gas at pressure p_0 and temperature 15° C. (being the temperature at which we suppose the conductivity is determined), then

$$\frac{p}{n}=\frac{p_0}{\mathrm{N}}.$$

Hence from equation (2)

$$-pu=\frac{300\mathrm{V}}{e}\frac{p_0}{\mathrm{N}}\frac{dp}{dx}; \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

similarly

$$-pv=\frac{300\mathrm{V}}{e}\frac{p_0}{\mathrm{N}}\frac{dp}{dy}, \quad \text{and} \quad -pw=\frac{300\mathrm{V}}{e}\frac{p_0}{\mathrm{N}}\frac{dp}{dz}.$$

The equation of continuity is

$$\frac{dp}{dt}+\frac{d}{dx}(pu)+\frac{d}{dy}(pv)+\frac{d}{dz}(pw)=0.$$

Substituting for $\frac{d}{dx}(pu)$, $\frac{d}{dy}(pv)$, and $\frac{d}{dz}(pw)$ their values derived by differentiating equations (3), we arrive at the equation

$$\frac{dp}{dt}=\frac{300\mathrm{V}p_0}{\mathrm{N}e}\nabla^2(p),$$

which is the general equation we assumed in Section I.

Thus the value of the constant κ is $\frac{300\mathrm{V}p_0}{\mathrm{N}e}$.

---

5. The loss of conductivity of a gas is due partly to the recombination of some of the positively charged carriers with the negatively charged ones, and partly to the carriers coming into contact with the conductors. It is with this latter phenomenon that we are here chiefly concerned. By substituting the above value of κ in the three solutions obtained in Section I. we obtain expressions which give the loss of conductivity of a gas due to the diffusion of the carriers towards the sides of the vessel which contains it. This loss of conductivity takes place in a closed vessel without any electromotive force acting on the gas.

When a carrier comes into contact with a conductor it either gives up its charge, or remains in contact with the surface. From the way in which the equations in Section I. were solved, it is clear that the solutions apply to the case where the carrier, instead of giving up its charge to the side, induces an opposite charge on the conductor, and is held attracted to the surface by the electric force arising from its image. The solutions apply equally well on the hypothesis that the carrier discharges and comes back into the gas. In this case we have a slight increase in the number of molecules of B, less than one part in 10^{10}; so that the correction to be applied would amount to calculating the difference of the rate of diffusion of A through a gas having a density greater than B in the proportion of $10^{10}+1$ to 10^{10}, which of course can in no way affect the original solution.

If we leave out of consideration the recombination of the ions or charged carriers, we see that the conductivity of a gas will fall from p_0 to p in a time t, where the ratio of p_0 to p is

$$\frac{8}{\pi^2}\sum_{n=1}^{n=a}\frac{\epsilon^{\frac{-(2n-1)^2\pi^2\kappa t}{a^2}}}{(2n-1)^2}$$

when the gas is contained between two parallel plates at a distance a apart;

$$4\left[\frac{\epsilon^{-\alpha_1{}^2\kappa t}}{a^1\alpha_1^2}+\frac{\epsilon^{-\alpha_2{}^2\kappa t}}{a^2\alpha_2^2}+\&c.\right]$$

when the gas is contained inside a cylinder of radius a; and

$$\frac{6}{\pi^2}\sum_{n=1}^{n=\infty}\frac{\epsilon^{-\frac{\pi^2n^2}{a^2}\kappa t}}{n^2}$$

when the gas is contained inside a sphere of radius a, where $\kappa=\dfrac{300p_0\mathrm{V}}{\mathrm{N}e}$.

These equations show how much more rapidly the conductivity is destroyed in smaller vessels than in larger ones.

Let us take the case of oxygen which has been made a conductor by Röntgen rays; let the charge on each carrier be x times the charge that an atom of oxygen carries in electrolysis, which we will denote by E.

One electromagnetic unit of quantity evolves 1·2 cub. cent. of hydrogen and ·6 cub. cent. of oxygen from an electrolytic cell, at ordinary temperature and pressure $p_0=10^6$.

The number of atoms in ·6 cub. cent. is $2\mathrm{N}_x$ (6), and the quantity of electricity that they carry is $\frac{1}{2}$ an electromagnetic unit, or $\frac{3}{2}10^{10}$ electrostatic units.

Hence

$$1{\cdot}2\mathrm{NE}=\frac{3}{2}10^{10} \quad \text{or} \quad \mathrm{NE}=\frac{10^{10}}{\cdot 8},$$

so that

$$\mathrm{N}e=\frac{x10^{10}}{\cdot 8}.$$

The velocity of the carrier under an electromotive force of a volt per centimetre is (E. Rutherford, Phil. Mag. 1897, vol. xliv.) 1·6 centim. per second; so that

$$\kappa=\frac{300\times10^6\times1{\cdot}6\times{\cdot}8}{x10^{10}}=\frac{3{\cdot}84}{x}10^{-2}.$$

6. Let us consider more particularly the second case, which would apply to a conducting gas passing along tubing, and find what the loss of conductivity of Röntgenized oxygen will be in passing along a tube, 10 centim. long and 1 millim. radius, at the rate of 100 centim. a second.

For simplicity we will suppose that the velocity is uniform, so that the time that any portion of the gas will be in the tube will be $\frac{1}{10}$ of a second.

The ratio of the conductivity of the gas entering the tubing to the conductivity of the gas as it escapes will, therefore, be

$$4\left[\frac{\epsilon^{-\alpha_1^2\kappa t}}{a^2\alpha_1^2}+\frac{\epsilon^{-\alpha_2^2\kappa t}}{a^2\alpha_2^2}+\&c.\right]=\mathrm{R}.$$

The values of $a\alpha_1$, $a\alpha_2$, &c. which are the positive roots of $J_0(x)=0$ are 2·404, 5·520, 8·654, &c. (Lord Rayleigh, 'Theory of Sound,' section 206).

Substituting for a, α_1, α_2, &c. their values we obtain

$$\mathrm{R}=4\left[\frac{\epsilon^{\frac{-(2\cdot4)^2kt}{a^2}}}{(2\cdot4)^2}+\frac{\epsilon^{\frac{-(5\cdot5)^2kt}{a^2}}}{(5\cdot5)^2}+\&c.\right],$$

where $a=\frac{1}{10}$, $t=\frac{1}{10}$, $\kappa=\dfrac{3\cdot84\times10^{-2}}{x}$.

So that

$$\mathrm{R}=4\left[\frac{\epsilon^{\frac{-2\cdot21}{x}}}{5\cdot76}+\frac{\epsilon^{\frac{-11\cdot6}{x}}}{30\cdot2}+\&c.\right]=\tfrac{1}{13}\text{ q. p.}$$

If x were unity, in other words if the ion in the oxygen which is conducting under Röntgen rays were to carry the same charge as it does in electrolysis, then the conductivity of the gas would be reduced to $\frac{1}{13}$ of its value by passing it along a tube 10 centimetres long and 1 millimetre radius at the rate of 100 centimetres per second.

It is interesting to find what would be the effect of the attraction, towards the sides of a tube made of conducting material, of each individual carrier by its own image in the conductor. It is quite evident that this effect only comes in when the carrier is near the surface, so that we can regard the radius of curvature of the tube as large in comparison with the distance of the carrier from the surface. When this distance is x, the force on the carrier will be $\dfrac{e^2}{4x^2}$, and under this force it would travel at the rate of $\dfrac{e}{4x^2}\times\dfrac{1\cdot6}{\frac{1}{300}}$, since under a volt a centimetre it travels at the rate of 1·6 centimetres per second.

Hence we have

$$u=\frac{10^{-10}\times120}{x^2}=-\frac{dx}{dt},$$

assuming that the atomic charge on oxygen is 10^{-10}; therefore

$$dt=-\frac{10^9}{12}x^2dx.$$

Let us find the distance of a particle x_0 which in a time t would reach the side: we have

$$t=-\int_{x_0}^{0}\frac{10^9}{12}\,x^2dx=\frac{10^9x_0^3}{36};$$

when $t=\frac{1}{10}$ we have $x_0=\dfrac{\sqrt[3]{3{\cdot}6}}{10^3}$.

So that in the case we are considering a layer of $1{\cdot}5\times10^{-3}$ thickness of the gas would have its conductivity destroyed in $\frac{1}{10}$ of a second owing to the mutual attraction between each ion and its image. The ratio of this volume to the total volume of the tube is $\dfrac{2\pi\times1{\cdot}5\times10^{-3}}{\pi r}$, which becomes 3×10^{-2} when $r={\cdot}1$. Hence in this case the loss of conductivity due to the carriers attracting themselves up to the sides is small compared with the loss of conductivity due to diffusion.

7. When there is an excess of carriers charged with electricity of one kind the gas not only conducts but exhibits the properties of a charged body. The motion of the carriers in such a gas is somewhat complicated, as both the diffusion and the effect of mutual repulsion have to be taken into account. When the charge per c. c. is small we can leave the latter effect out of account and consider only the diffusion. The equations (5) Section II. can then be applied to charged gases, and we can look upon them as particular cases of conducting gases. The properties of these gases vary in many ways in regard to their power of retaining their conductivity; thus some of them can be passed along tubing, bubbled though liquids, or sent through gauze or wool without losing more than from 20 to 50 per cent. of their conductivity, whereas others are made perfect non-conductors when similarly treated. The equations (5) Section II. show that rate of loss of conductivity by coming into contact with conductors increases very rapidly with V the velocity of the carrier under an electromotive force of 1 volt per centimetre. We should, therefore, expect that for those gases which retain their conductivity after bubbling through liquids &c. the value of V is small compared with its value for gases which retain none of their conductivity after similar treatment. In support of this explanation we have the following results:—The conductivity of a gas which has been made a conductor by means of Röntgen rays is destroyed by passing the gas through wool or bubbling through sulphuric acid, and the velocity of the carrier under an electromotive force of a volt per centimetre is (for air)

1·4 centimetres per second (J. J. Thomson and E. Rutherford, Phil. Mag. Nov. 1896; E. Rutherford, Phil. Mag. Nov. 1897). The gases evolved from a sulphuric acid electrolyte retain a large fraction of their charge after passing through wool or bubbling through a liquid, and for the oxygen the velocity of the carrier is only $2{\cdot}2 \times 10^{-4}$ centimetres per second when acted on by the same force (John S. Townsend, Phil. Mag. Feb. 1898).

We have here supposed that e, the charge on the carrier, is the same in each case. This assumption is reasonable from theoretical considerations, but it has not yet been established upon experimental evidence that when an elementary gas conducts the carriers have the same charge as the atoms carry in electrolysis. Information on this point might be gained by testing experimentally the result obtained in § 6.

Many examples of this latter kind are to be found in newly prepared gases. In most of these cases it is easy to account for the growth of the carrier to a large size owing to the presence of gases or vapours which would condense round the charge and thus increase the size of the carrier. The velocity V would thus be greatly diminished.

When newly prepared gases are evolved from a solution it is probable that the electrification is acquired immediately as the gas is generated, so that each little bubble of the gas as it rises in the liquid contains carriers which are charged. Since these bubbles are small it would only require a very short time for carriers which diffused rapidly to be completely discharged by striking the liquid round the bubble, so that in order that an appreciable number of charged carriers should escape with the gas from the liquid it is necessary to assume that they diffuse slowly, or what amounts to the same thing, that they should be large compared with molecules.

8. When the number of carriers charged with positive and negative electricity respectively is unequal, the electrostatic field which is created tends to drive those carriers which are in excess towards the walls of the containing vessel. This effect is easily calculated for the case where the carriers are all charged with electricity of the same sign. Let ρ be the density of electrification, $u\,v\,w$ the velocities parallel to the axes, and ϕ the electric potential.

In order to determine the motion we have three equations of the form

$$\alpha^{u} = -\frac{1}{n}\frac{dp}{dx} - e\frac{d\phi}{dx}.$$

The term $\frac{1}{n}\frac{dp}{dx}$ does not increase with ρ since both p and n are proportional to ρ, so that if we suppose ρ to be above a certain value this term may be omitted in comparison with $-e\frac{d\phi}{dx}$.

Writing the equation of continuity in the form

$$\frac{1}{\rho}\frac{\delta\rho}{\delta t}+\frac{du}{dx}+\frac{dv}{dy}+\frac{dw}{dz}=0,$$

where $\frac{\delta}{\delta t}$ denotes the total differentiation of ρ with respect to t, we obtain on substituting for $u\,v\,w$ the above values

$$\frac{\alpha}{\rho}\frac{\delta\rho}{\delta t}+4\pi e\rho=0.$$

So that

$$\rho=\frac{\rho_0}{1+\frac{4\pi e\rho_0}{\kappa}t},$$

which shows that after a time t the density is a function of t alone, and does not vary from point to point in the gas, if ρ_0 is constant initially throughout the gas. The reduction of the charge due to this effect is usually large compared with the reduction due to diffusion when ρ is greater than 10^{-4}. The two effects can easily be distinguished from one another, since $\frac{q_t}{q_0}$ in this case is a function of ρ_0, the initial density of electrification, and is independent of the form of the vessel. (J. S. Townsend, *loc. cit.*)

It does not appear that the effect of mutual repulsion would be instrumental in increasing the discharging power of a charged gas as it passed through fine gauzes, since the gauzes may be considered part of the boundary, the form or extent of which in no way affects the above value of ρ.

LI. *Microscopic Images and Vision.* By Lewis Wright [*].

1. THE discussion by Lord Rayleigh and Dr. Stoney [†] has thrown considerable further light upon a subject which has been discussed for many years; but there seems still something to be added from the point of view of the microscopist, for whom there is at issue in it a very important

---

[*] Communicated by the Author.

[†] Phil. Mag. Aug., Oct., Nov., Dec. 1896.

practical question not solved by any mere mathematical analysis, and scarcely yet, I think, made clear to him. This question is at the bottom of the term "spectrum theory," happily applied by Lord Rayleigh to Prof. Abbe's view of the matter, to which Dr. Stoney objects as being too limited, but which is strictly correct. Upon whatever general method of mathematical resolution the Abbe theory of microscopic vision ultimately rested, it was itself expounded to microscopists and discussed by them for many years as a matter of fact. It was thus and then confined to the statement that microscopical "resolution," or delineation of detail, was due to the union and interference (in the Fresnel manner) at the focal plane, of the direct dioptric beam and of at least one of the beams "diffracted" by minute periodic structure, in the manner of a grating illuminated by light approximating to the character of plane waves: such diffracted beams with white light becoming spectra. The Abbe theory further affirmed that the *trustworthiness* of the microscopical image solely depended upon, and was in direct proportion to, the *number* of orders of these spectra which were grasped by the aperture of the lens; and it explained all the advantages of greater aperture in greater resolution, upon this basis alone.

This was a definite, limited, and practical theory, easily grasped; and this alone was what came to be known as the Diffraction Theory or Abbe Theory. Since, therefore, Dr. Stoney now desires to apply that term to the wider manner of regarding microscopic vision which he has set forth, in order to keep things clear or even intelligible to any microscopist who has followed the past discussion, there is really no other course than to find a new name for the more limited and already well known Abbe theory, as Lord Rayleigh has so happily done. The truth or error of this "spectrum" theory, or the respective measure of each in it, is a matter of very great practical importance, as will appear. The interest in Dr. Stoney's wider theory is largely speculative; but there are obvious points of contact between it and the other, which also have to be considered, and which throw much light upon it.

2. With the purely theoretical bearings of Dr. Stoney's presentment of the case it is not necessary to deal at length; nor, indeed, am I qualified to do so. Yet it seems desirable to mention very briefly some objections which suggest themselves, and which, if valid, have much bearing on the conclusion he reaches in common with Prof. Abbe, that "diffracted light [defined as "light which advances in other directions than those prescribed by geometrical optics"] is

the machinery by which good definition is brought about." That is, of course, getting back to the "spectrum" theory; and I hope to show, largely from material the discussion has provided, this theory is only true in a conditional and limited sense, while its acceptance in a universal sense is a present cause of positive mischief in microscopy.

3. There appear to me, then, some fundamental physical objections to Dr. Stoney's method of representing what takes place. Putting that most briefly, and merely for the purpose of recollection, it seems to be as follows:—(A) All light emitted by an object may be resolved into undulations consisting of uniform plane waves. (B) We may conceive these reversed in direction (since any dynamical system may be reversed); and when they thus arrive back at the position occupied by the original object, they will there "produce an image the most perfect that the light emitted is capable of producing." This is held to follow because (§ 8, Phil. Mag. Oct. 1896) the plane waves there, as at each step, "reproduce exactly the same state of the æther as had prevailed at the same stations on the outward journey." Hence in general (we need not at present discuss Dr. Stoney's other images, and modifications) "plane waves converging inwards" are capable of producing the most perfect attainable image producible from the rays grasped by the objective. Stating objections to this with similar generality and brevity, it appears to me that such a presentment of the matter must break down as a full and complete explanation, however true in a limited sense, on the ground that "uniform plane waves" such as are spoken of, are not in trustworthy microscopy the actual or veritable dynamical system; and therefore cannot, as it will be shown they do not, produce the supposed most perfect attainable image by reversal.

4. More specifically, it seems evident that we are, *ab initio*, debarred from considering the light from a microscopic object as consisting of uniform plane waves, *except on the condition of plane-wave illumination of the object.* (Here, indeed, we have the secret of Abbe's consistent enforcement of illumination by a small luminous cone or pencil, which gives approximately such illumination.) For what are uniform plane waves? A wave-system is normal to the surface called the wave-surface, over which undulations from the same disturbance are in the same phase. Hence the plane wave arises from the Huygenian spherical wave, as a limiting case, in the manner pointed out by all the standard authorities. Thus Lord Rayleigh says*:—"So long as the radius of curvature

---

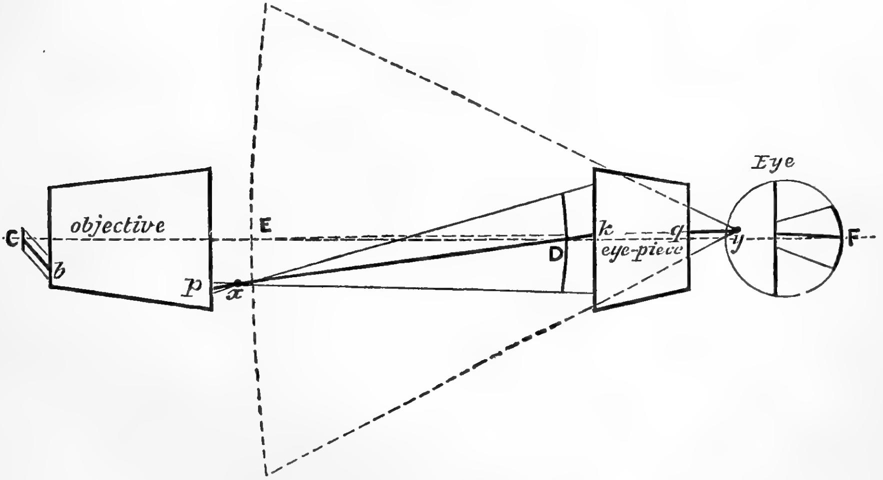
* *Encycl. Brit.*, "Wave-Theory," xxiv. p. 424.

[of the spherical wave] *is very long in comparison with* λ, each small part of a wave-surface propagates itself just as an infinite plane wave coincident with the tangent-plane." Bassett puts it similarly* —" Spherical waves concentric with the source are propagated throughout the medium ; and if the effect which these waves produce at some portion of space whose greatest linear dimension is *small in comparison with its distance from the source*, be observed, the wave may be regarded as approximately plane. We are thus led to study in the first instance plane waves."

The student of physical optics knows that this is so in actual fact. To study plane-wave phenomena, or to verify plane-wave dimensional calculations, he must remove his source of light, itself relatively small, to a considerable distance from his grating or other apparatus ; he must get his beams of rays approximately parallel, that the normal wave-surface may be approximately plane. This necessity belongs to the nature of plane waves.

5. But considering now microscopic objectives, many such have been made as short in focus as $\frac{1}{50}$ of an inch. It is impossible to regard light emitted from an object, as consisting of uniform plane waves on arriving at the surface of such a lens, after a path of, perhaps, $\frac{1}{200}$ of an inch ; except in the case of *plane-wave illumination of the object*, as in the Abbe theory. Even in that case it would seem that considerable modification must be made in Dr. Stoney's presentment of events in his fig. 1 (Nov. 1896, p. 433) here reproduced. His

Fig. 1.



beam C*b* of uniform plane waves is represented as proceeding from the object C ; and supposing only plane waves to

---

* Treatise on Physical Optics, p. 56 (1892).

illuminate the object, no objection need be made. But in this case his first focus of the beam (described as "parallel" rays) at x, can no longer be regarded as a focus of rays emitted from the object C, but contrariwise, of rays proceeding from the much more distant *source of light.* And accordingly, as a simple phenomenon, or fact of observation, it is *not* an image of any point of the object, but of the whole *source of light*, which is focussed at x, and is seen there on removing the eyepiece and looking down the tube.

6. Consider next the supposed dynamical system. This is by hypothesis set up, not by the object alone, or in ordinary method: "We begin by positing repetitions of the objective field" (Prop. 1, § 6). Then it is assumed that all these replicas emit light from their similar points "the same in direction, intensity, phase, and position of transversal." This postulate seems altogether illegitimate in a theory purporting to represent actual phenomena; we know that it is *not* true in physical reality. It, too, depends for the qualified truth which it does possess, upon plane-wave illumination; then it is true, so far as that when approximately plane waves fall upon a grating, the width or number of lines does not affect the image of the ruling, as ruling. But it seems to push the result of certain mathematical expressions to an extent which can hardly be justified. That opinion must be expressed with diffidence, since my very small amount of mathematical knowledge has become so rusty, that it is only with difficulty I can follow (and but too vaguely) even the general drift of the analyses in the discussion which so interests me. I did, however, gather that the ground of the immense postulate here objected to, lay in the fact that resolution into plane waves of æther-disturbances set up by an object, is represented by expressions which equally represent replicas of the disturbances; the nature of circular functions involving this necessity. From a private reply Dr. Stoney was kind enough to give me, it appears this is the case. But mathematical expressions are but tools, and often have the usual defects of tools; in particular that of not being sharp enough. Ask these functions to express a given disturbance and many surrounding replicas, and they will do it. But ask them next to express an actual limited disturbance resolved in this manner, and no more, and they fail; their edge at present is not sharp enough to do that. Such failure, however, is in this case an imperfection; and surely to ground such a physical postulate upon the very imperfection of an imperfect tool, is rather arguing in a circle. It seems to be a case of what was described only the other day in a review of a mathematical work, as "the

special philosophical vice of the mathematicians, the tendency, namely, to mistake the sign for the thing signified."

7. This seems further to appear, when we consider the *reversal* of the supposed dynamical system. This, it is supposed, produces the "best attainable image which the light emitted by the object [and grasped by the objective] is capable of producing." Unquestionably the light-waves emitted may truly be regarded as a dynamical system; and may be conceived as reversed; and the reversal of the whole actual system would produce such an image as described. But it does not seem to follow that mere "coalescence and interference of *uniform plane waves*" involves such a result. Besides what has already been said as to the absence of plane-wave character in rays from any self-luminous object, at the very minute focal distance of a high-power objective, questions as to the longitudinal components in the disturbances, and their disposal and influence, and several other questions, would seem to need further solution than is known at present, before this could be assumed.

In any case, what the reversal of the supposed dynamical system must really reproduce as an image at the place of its origin, must be the postulated operative cause of the system. That, by the hypothesis, is not an actual object and it alone, emitting luminous waves, but *the object surrounded by an indefinite number of identical replicas, emitting identically similar plane waves.* This does not represent any object in reality: and that fact seems to dispose of such a presentment as a full and complete representation of microscopic vision.

The same conclusion follows from Dr. Stoney's directions "how to see the rulings" (Phil. Mag. Dec. 1896, p. 525). We first illuminate the object by a near approximation to plane waves; and then behind the lens further exclude everything but the narrowest pencils of almost exclusively plane waves. Thus we produce a "ruling" extending far beyond the limits of a true image, and which in other respects is as far as possible from being any such. We are really producing, and do produce, easily calculable results of interesting experiments in the interferences of plane waves; and though these results are physically and directly related to the *periodic* structure of the object, considered as an interference-grating, they are no trustworthy representation of it. This truth has always been recognized and insisted upon by Prof. Abbe and his school, resulting in a sort of "counsel of despair" as to any truth or certainty in such microscopical images.

8. This brings us back to the more concrete Abbe "spectrum" theory, as already described. But Prof. Abbe

throughout, as Lord Rayleigh remarks, considers the object to be illuminated by plane waves. In this limited case, what Dr. Stoney advances is more or less true; but Abbe differs from the latter in constantly recognizing that condition and its consequences. Thus, while Dr. Stoney states that a cone of rays from the condenser, as wide as possible, may be used (as in practice it may, for reasons to be seen), Abbe again and again insists that such is not the case, and this at great length[*]. "Strictly similar images," he says, "cannot be expected except with a central illumination with a narrow incident pencil"[†]. This is the condition for securing an approximation to plane-wave illumination, with its diffraction phenomena.

II.

9. We may now consider how far the Abbe theory, which possesses more or less undoubted truth, is an adequate representation of microscopic vision; and the most satisfactory feature about the lengthy discussion from which these remarks originate, is that in several ways additional light is thrown upon that question. Considering the grounds for it afterwards, let me first state the general conclusion at which I have arrived as the result of long and patient experiment, and which is further confirmed by this discussion. Stated briefly as before, it is that *the trustworthiness of a microscopic image is in proportion as the object approximates to a self-luminous condition, and diminishes in proportion as it is or has to be* (for it may have to be) *examined by plane-wave illumination.* Whether this view, or the Abbe view, be true, is of most fundamental and practical importance to microscopy and microscopic optics. Let us see what light we have upon it.

10. Supposing the "spectrum" theory to be true, as a full representation, it was demonstrated that "microscopic vision is *sui generis*"[‡]. Such a statement need not now be criticized; it has vanished in the discussion, and Dr. Stoney expressly dissociates himself from it.

11. Another fundamental objection to the competence of the theory as a general one, is found in the fact mentioned by Lord Rayleigh, that the character of a grating may be such, that its spectra cannot give a proper image.

12. As he also brings out in his article, the object may conceivably be self-luminous; in which case there will be no spectra, and the waves emitted from different points of the

---

[*] Journ. R. Micr. Soc., Dec. 1889.

[†] Dallinger, p. 75.

[‡] Dallinger, p. 62, and many other writers.

object will be quite heterogeneous, and in no permanent phase-relations. Yet an image must be possible, and can in that case be only analysed according to the Airy method. We can only employ a really self-luminous object in experiments with low powers of the microscope—perhaps up to an inch *. But even the results with such a power are decisive of the real question; and with high powers we can more or less approximate to this kind of luminosity in several ways.

Thus, even a wide cone from the condenser approximates to it. Lord Rayleigh has shown (Phil. Mag. Aug. 1896, p. 175) how and why this kind of illumination must introduce a large amount of heterogeneity into the rays proceeding from the object, and concludes "that the function of the condenser in microscopic practice is to cause the object to behave, at any rate in some degree, as if it were self-luminous, and thus to obviate the sharply-marked interference-bands which arise when permanent and definite phase-relations are permitted to exist between the radiations which issue from various points of the object." Since Dr. Stoney, however, seems rather to regard the function of the condenser as being that of providing illumination by plane waves, we had better resort to other methods, which may help us to decide what is a very important practical question. For while, according to Dr. Stoney, the ideal is to get absolutely aplanatic systems of plane waves transmitted through the object, and all conditions short of this (caused by imperfections in the slide or various other details) impair the image (as in one special sense they do impair it, with some objects); according to the view expressed above, irregularities of phase thus produced may add to the *trustworthiness* of the image, though it may impair it in some other features.

Let us take therefore as an object on the stage, a grating of 3000 or 6000 lines to an inch, illuminated by a narrow cone from the condenser, focussing the flat of a rather distant lamp-flame. Place immediately in front of this flame a coarse grating, 50 to 100 lines per inch, either photographed or of wire. The several points of these luminous lines emit light-waves chiefly in the self-luminous manner, indiscriminate in phases and transversals as the points of the flame itself †. Arranging the stage grating so as to cover only half the

---

* Thus, we may employ a grating of platinum wire made incandescent by an electric current.

† If this be questioned, let it be remembered that we might get *pure* self-luminosity by employing in the dark a platinum grating made incandescent as before. Probably no physicist believes that there would be any observable difference in the phenomena, or would even deem it worth while to make the experiment in order to see if there were any.

objective field, a condenser can be selected of such a focal length, and other matters so adjusted, that the focal image of the coarse grating formed by the condenser, corresponds both in intervals and focal plane with the object-grating on the stage, and using the same illuminating cone. Remove now the coarse grating and place the stage grating centrally: then removing the eyepiece and looking down the tube, the dioptric beam and its flanking spectra as so often described will be seen; they are the images of the source of light, formed at x in fig. 1. They interfere and form the image seen by the eye-piece, in the Fresnel and Abbe manner. Removing the stage grating, and replacing the coarse one over the flame, its focal image is now the object. Owing to the heterogeneity of the rays, this aërial image emits no spectra—there neither are nor can be any such. But it is perfectly resolved. Here we have a resolution of 3000 or 6000 lines per inch that has no place at all in the "spectrum" theory; which therefore can be no *complete* theory of microscopic vision, though it has an important place in it [*].

As another expedient, we may place beneath the slide a sheet of finely-ground glass. This ground surface refracts and reflects the light in countless phases and directions through the object, the waves issuing therefrom with similar heterogeneity of character. Here also we must have at least a very considerable degree of approximation to the nature of self-luminosity; nor can we get from such illumination any of the well-marked "spectra" at x (fig. 1), or out-of-focus interference-fringes, familiar to us with the Abbe method. The difference in character of illumination by such methods, and the methods described by Dr. Stoney, is so great, that if the "spectrum" theory be completely true, there should at least be a uniform and vast deterioration in the image of an object thus illuminated. On the contrary, with all good lenses of moderate aperture, and slides with any fair amount of opacity in details, such an image is about *the very best we can get.* The excellence of this method of illuminating was first impressed upon me many years ago by the late Dr. Carpenter; and since I in my turn have recommended it, fresh discovery of it has been made by Mr. G. C. Karop [†]. By its means really good moderate powers can be used up to their full aperture,

---

[*] Using reduced photographs of perforated zinc, I have similarly used their aërial images in comparison with *P. angulatum* on the stage. Only approximately in one respect, because the difficulty of getting sufficiently reduced photographs prevented use of the same illuminating cone in the two cases. But there is no doubt about the results in all important respects.

[†] Journ. Quekett Microscopical Club, Nov. 1896.

rendering the very finest hairs as tapering to a *perfect point*, with entire absence of the diffraction-fringes shown round such details with a narrow pencil. Where and why "resolution" often fails with high powers as regards some objects so illuminated, belongs to the question before us, and is dealt with presently; but the method can be carried much farther than many would suppose. The diatom *P. angulatum* (45,000 to the inch) is resolved by it beautifully with a dry lens; and this self-luminous resolution has the cardinal superiority over Abbe's with a narrow pencil, that by no possibility can any images be produced by it other than the small white disks on dark ground, or black spots on white ground, at different foci, which can be produced in the same way from a sheet of perforated zinc. By grinding the back of the slide itself, even an immersion-lens can be more or less filled with direct rays, and in this way all the spots can be seen (*as* spots, and not falsely as spherules) in *A. Lindheimerii* (69,000 to the inch). With a first-rate apochromatic and one of the slides mounted by Dr. Van Heurck in As_2S_2 referred to by Dr. Stoney [*], I have seen the striæ in *A. pellucida*; though with such objects as these the method comparatively fails, for reasons presently to be seen.

13. We may also compare the results of mathematical analysis with those of experiment. We have two kinds of possible image, for the Abbe or "spectrum" image is a real fact enough under the necessary conditions; our inquiry here is simply what *proportion and value* must be assigned to it in ordinary research. Lord Rayleigh's articles here and elsewhere seem to supply useful *criteria* as regards that question. If I rightly understand him, he shows that according to the "spectrum" theory a square and circular aperture of the same width give the same resolution for points or short lines. On the other hand, respecting the resolution of self-luminous lines of sensible length, another analysis of his [†] led to the conclusion that a circular aperture must exceed a square aperture by say 10 per cent. to give equal resolution. Airy in a slightly different manner calculated that the circular aperture must exceed by about 20 per cent. Of course any analysis must start from certain assumptions, but Lord Rayleigh's appear reasonable.

---

[*] It is an error to suppose no one had succeeded in mounting objects in this medium except or until Dr. Van Heurck. Long ago it was used (I think invented) by Prof. Hamilton Smith in America. Father Thompson was also fond of it, and published a description of his methods of manipulation. Unfortunately the medium is rather apt to slowly crystallize.

[†] See *Enc. Brit.* "Wave Theory," and Phil. Mag. 1879, 1880, for details.

Experimental test was made in conjunction with Mr. R. T. Glazebrook, using a 50 to the inch wire grating in front of a sodium flame, and two different rectangular apertures (with sides parallel to the wires) on the object-glass of a telescope, measuring the distance at which the object-glass (with aperture) resolved the grating. Of circular apertures, four were employed in the same way. The two observers differed very slightly, and the mean for the four circular apertures worked out in the proportions of 1·13, 1·09, 1·09, and 1·09 to 1·0 of rectangular aperture. Here the grating in front of the flame is regarded as self-luminous, just as in the experiment with the microscope above described.

Thus far experiment confirms the analysis; but Dr. Stoney considers (in the previous discussion with me which Lord Rayleigh alludes to) that the same methods cannot be applied to microscopical resolution, on account of the wider angle of the cones of rays concerned, and the physical consequences of that difference. I think he has over-rated such distinction, and it is rather hard to see just where it begins or how far it operates; at all events, the agreement of experiment with analysis as regards both kinds of image*, in the microscope also, is remarkable.

Calculating (as usually done) by the E line for white light, the ultimate limit of resolution for a dry objective of utmost aperture (N.A. 1·0) is 96,410 lines per inch, which we suppose to be attainable according to the "spectrum" theory, although the aperture is circular. In 1888 Mr. E. M. Nelson, whose microscopic vision is phenomenally keen, just "glimpsed" the striæ of *A. pellucida*, mounted in the arsenic medium described by Dr. Stoney, but prepared by Prof. H. L. Smith. Including the double system, or all across the valve, these striæ are about $\frac{1}{2500}$ of an inch in length. He used an oil-immersion condenser of much greater aperture than 1·0, with a single-notched stop, through which sun-rays were sent by a heliostat. The beam through the notch being first so oblique as to be outside or excluded by the 1·0 dry aperture of the objective, a strong green spectrum alone appeared at one side of that aperture, at back of the lens (x in fig. 1). The notch was then gradually deepened until a very small direct or dioptric pencil was just seen on the opposite side of the aperture—

---

* As Lord Rayleigh pointed out to me, and as will be understood, the absolute theoretical limit of resolution must be considered the same in both apertures in each case. But resolution also requires a certain luminous intensity compared with the field of view; and in this matter the circular aperture may be more or less at disadvantage, depending either on the nature of the luminosity or partly on the length of the lines compared with the aperture.

replacing the eyepiece, the striæ were just seen. The diatom was probably something less than 95,000 per inch, and any dry lens must be some little less than 1·0 in N.A. Here then, with *very intense* plane-wave illumination—in fact as nearly as possible in practice Dr. Stoney's "uniform plane waves"—we have also as nearly as possible the theoretical limit attained, or closely approached, with a circular aperture.

Turning now to the more average kind of microscopic image, the extreme closeness with which Lord Rayleigh's 10 per cent. reduction of efficiency in circular apertures represents the facts of observation as found by the most competent observers, will forcibly strike everyone who has studied microscopy for any length of time. But Dr. A. Clifford Mercer, of Syracuse University, U.S., has recently tested the question photographically. It is comparatively easy to prepare circular and square apertures of equal dimensions. He also ruled upon the same glass plate six sets of lines at intervals of 0·42, 0·46, and 0·5 mm. and their doubled intervals of 0·84, 0·92, and 1·0 mm. apart. The apertures were 5·0, 5·5, and 6·0 mm. diameter. It will be seen that both lines and apertures give excesses of about 10 and 20 per cent., representing those calculated by Lord Rayleigh and Airy respectively. An aërial image of these lines focussed by the condenser, was used as the object, and successive photographs taken with all the square and circular apertures. Then only *similarity of resolution* had to be compared, which can be done within very small limits of observational error. The results agreed with Lord Rayleigh's calculation and experiments, not with the Abbe calculation or with Airy's.

14. Dr. Stoney himself, moreover, recognizes essentially what is here maintained in his Proposition 5 (Phil. Mag. Oct. 1896, p. 346). This reads:—"The standard image is the outcome, partly of the features upon the object, and partly of the state of the light by which the object is illuminated. *It may be improved by increasing the degree in which the first of these factors, and by decreasing the degree in which the second, contributes to produce, to modify, or to efface detail in the image.*" So closely does this practically coincide with my proposition, that had it stood alone or as the final conclusion of his exposition, nothing more would have been necessary; and it has the further merit of recognizing the fact (which constitutes the real place and proportion of the "spectrum" theory in microscopy, and the *nexus* between it and the Airy theory) that we have *two distinct elements* to deal with in an image, whose respective preponderance or proportion are highly variable. The present attempt at further treatment is

made chiefly because Dr. Stoney does not seem to recognize the true relative proportions, either in maintaining with Abbe in such a universal sense that "diffracted light is the machinery by which good definition is brought about"; or when he proceeds to illustrate (§ 35) "the great assistance which is rendered to the practical microscopist by Abbe's theory."

15. We therefore next consider that illustration. To begin with, the resolution of *A. pellucida* is no real problem at all: it is not even of the same nature as the problems which do confront the scientific worker. Supposing it were, the latter would regard with consternation the elaborate apparatus described for producing monochromatic light. This diatom, however, has been studied for many years; the dimensions of its structure are known and familiar; and the powers of annular illumination have long since been ascertained. It is no problem, or one in which help is needed, to take what is really a "grating" of this *known* fineness, and already *known* to have this definite periodicity of structure, and arrange matters so as to get the most conspicuous "resolution" of it. The problems in which assistance is really wanted, the microscopic worker's really "difficult" objects, are such as Dr. Dallinger confronted in detecting the spores of a monad, itself only $\frac{1}{6000}$ of an inch in diameter and themselves only $\frac{1}{240000}$ of an inch; or more especially (because here was involved real "resolution" of fine detail) the *process of division* in the nucleus of a cell, itself only $\frac{1}{20000}$ of an inch long. In such cases what will be found and is to be observed is *unknown*; accurate periodicity of structure is probably absent; and mere artificial force or clearness in "resolution," even if obtainable (which it seldom is) is worthless in comparison with known trustworthiness in the image so far as it goes. Taking any such case as this for our test-object, and comparing it with the treatment of the *A. pellucida* as described, we shall be able to appreciate the proportion of both truth and error—for there is truth as well as error—in the "spectrum" theory.

16. We cannot help, in the first place, seeing much error. While the minuteness of structure to be detected by Dr. Dallinger (in an unknown object) was as great, the method of proceeding described for the *A. pellucida* is impracticable, and would be useless even if practicable. Real work has to be done by far different means. The finest lenses, used with a wide and solid aplanatic cone of light, could alone do such work; and moreover, earlier lenses of 1·48 N.A. were surpassed in results by apochromatic lenses of 1·40 N.A., better corrected for spherical aberration—the

meaning of which we shall see. Supposing the microscopist, however, to know or suppose the measure of minuteness in the divisions of the cell-nucleus, he would, on the principles Dr. Stoney explains, have to employ (with doubtless some modification in detail) arrangements for *plane-wave illumination* generically similar to those he describes for the diatom. But he would be wrong, and the results would be *nil* *. Dr. Dallinger had to do such work with a high degree of heterogeneous illumination—as close an approach as is possible with the lens used, to a self-luminous condition of the object.

The image even of the diatom is a false image. It is admittedly so in regard to the "spherules," and competent judges whom I have consulted are very doubtful whether even the breaking up of the striæ so shown, is not due to false diffraction-fringes from the midrib of the valve, the spherules being thus arranged in longitudinal rows far more straight than is really the case. Looking at the matter theoretically, it will be observed that after having laid down how the excellence of the image is in inverse proportion to "the degree in which [the state of the light by which the object is illuminated] contributes to produce, to modify, or to efface detail," he proceeds to obtain this image by almost the greatest specialization of the light which is possible. The effect of this is to replace the actual detail, by other apparent detail which is visually intense, and geometrically symmetrical, to an utterly false degree.

Similar results are traceable in other diatom work by the Abbe school, as may be shown by the most familiar test-valve of all, a much coarser one, the *P. angulatum*. Dr. Van Heurck has photographed this with the celebrated Abbe-Zeiss lens of 1·63 aperture and dense immersion-fluid and medium, by Abbe methods, with an uncorrected condenser; the result is a series of hexagons resembling a honey-comb. Dr. Stoney writes † of the same object, that dry objectives can only image its details "correctly so far as regards their number and position, but any further detail is not correctly represented." Immersions embracing most of the first spectra, "we now see some detail: the dots appear hexagonal, and are separated from one another by walls which are thin,

---

* Narrow pencils and annuli have of course been tried, for the contrast they give. The probable reason of failure is want of sufficiently regular *periodicity* in the detail. Only such periodic detail is shown better by such methods; all else is "blurred," as Dr. Stoney points out in regard to the other features of this diatom itself.

† Modern Microscopy, 2nd ed. p. 109.

and which look like a honey-comb;" and he adds that "this is the first and only step we can take towards learning what the actual detail is," because no objective will embrace the other orders. Examining these several statements, there is every reason to believe that a dry objective with a wide cone of light gives a perfectly truthful image, while it will give the hexagons quite easily if that figure is preferred; Zeiss's well-known large-scale photograph is of a valve so coarse that it is beyond dispute * that a portion of the second-order spectra *were* included by the lens used, with the result of introducing a false *doubled* resolution impossible with first orders alone; and an immense further step can be taken by using a first-rate immersion-lens of 1·40 aperture, with a wide cone. The Zeiss photograph × 4900, and the Van Heurck photograph, are confessedly the highest triumphs of photography by the Abbe method: one has only to compare both with the beautiful photograph × 4900 taken in this other way by Mr. E. M. Nelson, and other similar ones up to a scale of × 6400, to *see*, once for all, which is the truest image †, and the all-importance of a sufficiency of heterogeneous light.

17. We can also, however, see the large amount of *truth* in the Abbe theory, and its important, though not *all*-important, place in microscopic vision, especially for certain classes of objects. Wherever we have a known periodic structure in transparent objects, plane-wave illumination and the consequent interference-lines formed by the beams diffracted by that structure, have an extraordinary effect in *intensifying* into black and white a more or less accurate representation of the periodic detail. How this occurs can be easily seen from two examples, macroscopic and microscopic.

---

* As demonstrated by Mr. E. M. Nelson, Journal of the Quekett Microscopical Club, July 1890.

† The minute detail in some of these photographs could not possibly be shown by Dr. Stoney's method, because, minute as they are, they are *unsymmetrical* and not *periodic*. In regard to the *P. angulatum*, both circular disks and hexagons can be seen, depending upon the precise focus; the sharpest portions show the circles, which, disposed in quincunx arrangement, most diatomists who have worked with English appliances believe to be the true figure. Besides the sharpest image, we have the phenomena of "postage-stamp fracture," and the shape of far coarser markings in other diatoms to guide us. Mr. C. Haughton Gill has demonstrated that the spots are either apertures or depressions, by depositing pigment in them; and the various images can be imitated with perforated zinc. It is the distinct outlines of the *fractures*, and broken-through apertures, which are so magnificently shown in Mr. Nelson's photograph with a wide cone.

Take first quite a coarse striation of 50 to the inch, visible to the naked eye, represented by a grating of platinum wire and by a piece of platinum foil corrugated to the same gauge. Make the wires incandescent, and (checking irradiation by a smoked glass) the striation is easily seen. Make the corrugated foil incandescent (these observations are supposed to be in the dark) and probably the detail will be quite invisible. The eye was quite competent to see structure of this fineness by the Airy self-luminous method, if the detail was in contrast; but there is now *no contrast*, and the detail is more or less invisible. Then let the corrugated foil be cold and illuminated by extraneous light, and the detail is seen again. There is both *shadow* to assist the contrasts, and also there are phase-relations between the tops and bottoms of the striations which come into play.

Let us further imagine a perfectly transparent structure with uniform periodic detail, but the elements of that detail differing in thickness only; and let it be mounted in a medium of nearly the same refractive index. A diatom in balsam nearly represents such a case. It is quite evident that by heterogeneous illumination at all approaching the self-luminous character, it will be difficult to find anything *sufficiently contrasted* in detail to see at all, though the very same illumination of a *black-and-white* photograph of same scale, or of the same diatom in a medium of 2·4 index, might show it easily. But plane-wave illumination might very easily bring about phase-relations more or less approximating to *half-wave discordance*, which we know well would be more effective than black-and-white itself by direct light; in any case these phase-relations will produce conspicuous effect in a Fresnel-fringe image. Thus the Abbe method has a most important function in enabling us to see *contrast* in the details of a large class of objects—especially hyaline or transparent objects—which do not present contrast or opacity sufficient to be seen in any other way. The error has been in giving to it the sole or all-important place, not recognizing that there is quite another kind of image also available, depending upon Airy's theory; and that this latter, while in the case of transparent details often giving images insufficient, or at least far inferior, in black-and-white contrast (what microscopists call "resolution"), is free from the *contour* errors of the Abbe image, and must be used to correct it so far as is possible in the individual cases.

The errors of the "spectrum" image are well known: Prof. Abbe himself has sufficiently insisted upon them. Its very contrast, or "resolution," is in most cases a glaring departure from *truth*, to which (when we can get resolution at all) the more indistinct self-luminous image is in reality a far nearer approach. It tends to make details which should be only geometrically symmetrical to a limited extent, perfectly so. In extreme forms it makes rows of spots into lines, and these lines straight when not really so. It is always liable to false resolutions of double fineness. It fails (as Dr. Stoney explains) to give even a tolerable image of the larger features of the object, thereby showing its failure to be a real "image" at all. All that can really be learnt from it, is that there is probably (for this is subject to possible delusion from the false intercostals above mentioned) *some* periodic difference of structure in the object *similar in dimensional intervals* to "lines" shown: in regard to "spots" this is more uncertain, since these are often produced by false diffraction-fringes from any long line which may cross the true ones. That the lines are lines, or that the "pattern" is so geometrical as appears, is in the highest degree improbable. That the "spectrum" theory and method so long retained exclusive predominance, is because attention has been so concentrated upon either gratings or diatoms of *known periodicity* in structure, but which only represent to a very small extent indeed any serious kind of investigation.

18. We are evidently reaching a very practical conclusion. It appears that in microscopy we have to deal with two characteristics of an image, which often are only to a limited extent compatible; that we have at command two methods of illumination which respectively promote more especially each of such characteristics; and that in most cases our problem is so to combine and balance these two methods as to produce the best result. *Fidelity of contour* will be secured in proportion as we are able to obtain our image by heterogeneous illumination, approximating the object to a self-luminous condition. But this method may prove utterly unable to give us *contrast*, which we may therefore be compelled to increase by using to a greater or less (even to a very large) extent plane-wave illumination, at the expense, however, of some greater or less degree of infidelity in contour. Thus an opaque subject, even of much minuteness, may be best shown by ground-glass illumination, or a very wide cone; while a diatom, unless in a very dense medium, or dry in air, may require narrow pencils of approximately plane waves. It is interesting to observe that there is thus a great degree of practical truth in Prof. Abbe's early contention as to "different origins" of different parts of the image. Many of us have written of this as an "error," now "recanted,"

which strictly is true; but there is this broad practical sense in which it also is true.

And we are unable to use either kind of image or of illumination absolutely pure, if we desired to do so. The narrowest pencil we can practically use will not give us absolutely plane waves alone; there will be some amount of heterogeneity in the pencil, which in some little degree serves to *correct* our image. And the widest cones we can use, or even ground glass, do not prevent greater or less approach to the character of plane waves, as the rays travel farther from the lamp; and these by their interference tend to *intensify* the image. We have to play off and adjust one against the other, and we are helped by a fact which I had not fully seen, till pointed out by Dr. Stoney in our previous discussion *. In so far as we may regard every elementary or excessively small cone or pencil of rays from the condenser as an individual beam of plane waves (which no doubt is the case in some degree), in passing through the object it originates two or more pencils from the same point. These being necessarily in the same phase or phase-relation, so far as they exist must interfere at the focus, and thus *intensify* the image. On the other hand, the numerous such elementary pencils comprising a wide cone, are in many discordant phases and transversals, as Lord Rayleigh has shown: and this very heterogeneity tends to correct the *contours* in the image, as above. We thus understand why, in really critical work, a large cone from a good condenser usually gives us the best result; but why it may be impossible, even with a perfect objective, to use a cone of light which will fill its aperture completely. It may be necessary, to intensify the image, while using as much heterogeneous light as we can, to use only pencils each of which throws out another diffracted pencil grasped by the aperture, so as to intensify, or as Dr. Stoney prefers to say, to "correct" it. But this necessity depends on the nature of the object, and does not exist in all cases.

19. There is a very obvious and simple, yet decisive test as to the correctness of this view. According to the Abbe or spectrum theory (and even according to some of Dr. Stoney's reasoning, I think), the amount of cone or heterogeneous light which can be used will depend upon the *minuteness* of the structure alone. According to the view here maintained (which recognizes the Airy theory as also concerned in the image) the *density or contrast* of the structure is the chief factor in this question. All experience proves that the latter is the case.

---

* English Mechanic, Jan. 31, 1896.

III.

It only remains to show how directly the questions here discussed affect practical microscopy and the work of the microscope optician, and also determine the prospect of further advances in our powers of microscopical research.

20. The Abbe or "spectrum" theory has in its time, confessedly, led to enormous improvement in objectives. Owing to that specialization and ignorance of what physicists had done, which Lord Rayleigh has alluded to, there was amongst microscopists no understanding of the direct function of aperture in resolution; and so the Abbe theory was for years written about, and advanced, as "the first explanation ever given." It thus produced a vivid consciousness of that function which was entirely new, to which we owe our present immersion and other high-aperture lenses. But it is as easy to show that, this work being done, its undue preponderance and acceptance as the *only* theory, especially on the Continent, is now causing distinct prejudicial results, owing chiefly to its connexion in practice with a narrow pencil or cone. Dr. Stoney disclaims this for his more general presentment, as he has of course a right to do; but it is not so with the "spectrum" theory. Abbe himself throughout insisted upon the narrow pencil. Dr. Van Heurck does the same; Dr. Peragallo writes that a cone of more than 0·50 N A. is of no use; and Dr. Dallinger, and authorities like him, who in a general way accept the Abbe theory as *the* "theory," but know from their own experience the vital necessity in difficult research of a wide cone, write expressly of "theory and practice being thus at variance," in some way or other which had to be explained.

It is difficult to estimate the prejudicial effect of this upon microscopy on the Continent. As a quite uncorrected condenser will give a fair cone up to 0·50 N.A., and also by immersion extremely oblique rays from its margin (equivalent to annular marginal illumination), for years no better Continental condenser was made. Prof. Abbe at last was driven to compute an achromatic, but this last production of Continental microscopy only gives an aplanatic cone of 0·65. Except those few who know of English condensers, with their *aplanatic* cones of 1·10 for immersion and 0·90 for dry combinations, Continental workers have thus been condemned to the errors and weaknesses of narrow pencils, which have thence been propagated through our own medical schools; and the results are sufficiently striking. Dr. Koch at last found out for himself, empirically, that wide cones gave much sharper and "finer" images of bacteria, in fact the only images worth having. Prof. Abbe accounted for this observational fact, in an article * expressly contradicting any advantage whatever to the image (as an image) from a wide cone, on the ground that the wide cone, owing to its more sharply defined focal plane (want of "penetration"), makes invisible the transparent tissues in which the bacteria are situate. But he fails to account for the fact that it is just the same with bacteria in invisible culture-media or sputum; and that the advantage really consists in the much greater sharpness or *thinness* of the images of the bacteria themselves; in truth of contour, so that square ends are shown square and not rounded; and in the fact that there are no blurred edges or diffraction-fringes round them, as appear with a narrow cone. In fact, many allied bacteria cannot be distinguished at all by the microscopic methods still too current in our schools, which have taken their methods from Germany.

So also, when Mr. Baugh visited the famed Jena workshop in 1895, he was told by Prof. Zimmermann, one of the scientific staff (who has himself published a work on microscopy), that in photographing they found no difference in results obtained by the chromatic and achromatic condensers; which is equivalent to the statement that they knew of no better results than those from a 0·50 cone. Our English results are quite different. Mr. A. Pringle, whose splendid photographic work on bacteria is well known, often uses the largest aplanatic cones; and, to quote once more our recognized authority on microscopic practice †, "Photomicrography with a small cone is quite easy, as great contrast can be secured [the reason has been shown in foregoing paragraphs]. With a large cone difficulties begin—difficulties of adjustment, difficulties of lens correction, difficulties of exposure, and difficulties of development. If, so far as our experience goes, a good photo-micrograph is required, these difficulties must be mastered."

21. This quotation leads us to the prejudicial effect of the theory (or rather of its undue preponderance) upon microscopic objectives. The mode of illumination directly influences the quality of the objective; because the all-important point of correction for spherical aberration has commanding influence upon the cone of heterogeneous rays which can be used with it. This does not appear under the Abbe method; and accordingly Strahl positively maintains ‡ that "the influence

---

* Journ. R. M. S., Dec. 1889.

† Dallinger, p. 365.

‡ Journ. R. M. S. Dec. 1895.

of spherical aberration has been considerably over-rated in objectives ! " The most eminent firm of Continental opticians states that its lenses, owing to the system of calculation and manufacture, are uniformly free from spherical aberration, so much so that there is no need for any "empirical tests," viz., testing upon the microscope itself. That is not the case when tested by the more perfect English appliances*. Not long ago, having the opportunity of testing and comparing three similar objectives together, I was enabled to see the difference. With the Abbe condenser there was no very obvious distinction ; but tested by English condensers it was quite otherwise. The great firm had no cause to blush for any one of them ; all were good lenses ; but they now revealed as distinct characteristic features as one sees in individual faces. On a graduated series of *Poduras*, one of them now gave most unusually good definition with rather a small cone under the highest (×27) eyepiece ; while a second, scarcely equal in this point, excelled the others in the *wide* cone it was able to use on this object. I had the curiosity to ask another operator to make the same test. He is probably more skilful than myself, and certainly has keener vision : quite independently he reached identical conclusions. Slight variations of pressure in the final polishing of the glasses are quite sufficient to produce such differences as these, in such small lenses as are here in question.

Whether this latter be the cause, or some other, nearly all high-power objectives even of the present day, and of the very best makers, show a very sensible amount of aberration. Drawing a circle to represent the whole aperture, and smaller concentric circles to define zones of its surface, many of the zones have *slightly different foci*. This fact plays all sorts of insidious hanky-panky tricks with small-cone interference images of the Abbe kind ; giving more force to such of the spectra as are correctly focussed than to the others. But in other respects, with small cones, these zonal differences are not obvious, and often escape detection, many portions of the aperture not being utilized at all. There are refined tests familiar to opticians, and some others employed by highly-skilled microscopists ; but not only are these too seldom employed by even the best makers before the lens is sent forth, but we have seen that even their necessity is disputed, and the importance of spherical aberration itself actually chal-

---

* The condenser itself is an English appliance. Ten years ago only one house, I think, made one with wide aplanatic cone. To-day every English house of any standing constructs achromatic combinations with 0·90 of aplanatic cone, and two, I think, construct apochromatics.

lenged, by adherents of the "spectrum" theory as heretofore understood.

When, however, we do employ adequate tests, and at the same time make careful comparisons between one objective and another, we find that the perfect correction of spherical aberrations is just *all-important* in determining how far we can go in using with that lens the heterogeneous illuminating cone which is so important for depicting true contours in our image, still preserving sufficient resolution of minute structure. (We are here postulating sufficient opacity in the details, to dispense with much of the aid we have seen to be often necessary in hyaline subjects.) High-class moderate powers now easily utilize their full aperture, with ground-glass illumination. With high powers, the amount of this, or of aplanatic cone possible, is in almost direct proportion to the perfection of spherical correction. Few lenses over 0·60N.A. will, however, even yet bear more than three-fourths of their aperture as direct light; many very good ones only two-thirds. And objectives differ strangely. In Zeiss's apochromatic series, the half-inch of 0·65 N.A. and the $\frac{1}{8}$ immersion of 1·40 stand out from the rest: some rare specimens of the former will bear their full cone, and occasionally an $\frac{1}{8}$ of 1·40 has been used in photography with a cone of 1·10. Very recently I had sent me for examination by Messrs. Swift a new English $\frac{1}{12}$ apochromatic of 1·40, which I soon found was remarkably well corrected spherically*. It was accordingly next tested upon the object mentioned by Dr. Stoney, *A. pellucida* mounted in arsenic by Dr. Van Heurck. All the transverse striæ in the diatom were most easily resolved with a central, solid, unstopped full aplanatic cone of over 0·90 from a dry condenser. The larger features were of course also quite correctly and sharply imaged, as Dr. Stoney's are not. Need it be asked which is the truest image?

But this is not nearly the limit†. Since that experiment Mr. E. M. Nelson has shown me *A. pellucida* clearly resolved

---

* A rough but very fair idea of the spherical correction may be obtained almost immediately by focussing a *Podura* test-scale with small cone, and then ascertaining how far the iris can be opened without *altering* the image of the exclamation-marks. Using successively larger *annuli* of light, this test becomes far more efficient and severe.

† Owing to some astigmatism and other defects, my own vision is very coarse and imperfect in these matters, and for me to see the striæ means much more for many other observers. The first valve Mr. E. M. Nelson showed me in balsam as "strongly" resolved, was to my sight quite unresolvable, and he had to search for another, which I was able to see. This diatom is one of the most variable in resolvability of the whole list, quite apart from the mere coarseness of striation. *That* is no difficulty at all.

into striæ, *mounted in balsam*, as well as "dry," with a similar cone of over 0·90 from Powell's apochromatic condenser, and a Zeiss $\frac{1}{8}$ apochromatic of 1·40. This latter lens was probably one of the finest ever made—I at least never saw its equal—and the mere striæ were not all it had to tell us, using no arrangements beyond the 0·90 full cone, and Giffard's green light-filter. On a dry valve, it clearly displayed where bits of coarser upper membrane with their blacker lines were overlying the lower, as is more often seen in *A. Lindheimerii.* And on a strong valve in quinidine, carefully adjusting for what may be termed the "white" focus, each of the striæ could be seen clearly *outlined* at both edges, the outlines being a series of small convex curves, scalloping out the stria into partly-defined oval beads. The divisions or narrower necks between these partly-defined ovals did *not* lie in longitudinal rows, but occurred with a considerable degree of irregularity. I think it probable that such resolution, which most closely parallels the coarser *Lindheimerii* valve, may be the truest resolution yet attained.

No doubt the above lens was (for the present) an almost phenomenal one. Every practical microscopist knows that the "similar" objectives, by even the very best makers, are *not* "all alike," whatever the makers may affirm. They differ in features as in a case above mentioned; most of all in the cone they can employ in critical work, and in what such a cone will reveal. Everyone engaged in difficult research has some favourite objective, treasured and spared in work as much as possible; because he knows full well that if parted with or injured, though he can buy a "similar" one at the list price, it may be long ere he finds such another!

22. And this brings us to the last point. The question of how far we may still expect advances in our optical powers of research is important; and it is answered very differently according to the "spectrum" theory, or the qualified views here maintained. It not only follows from the foregoing, but has been over and over again stated expressly by the Abbe school, that we have *no* hope of further advance, except through increase of aperture; and on that ground was constructed the lens of 1·63 N.A. to be used with flint-glass mounts and dense fluid media—conditions under which it is practically useless. So little are other conditions recognized, that Dr. Van Heurck has only used the chromatic condenser in his skilful published diatom photographs; and those results are simply *nil*, not one of them surpassing, or in some respects even equalling, what has been done in England with 1·40 lenses.

It is far different if the Abbe theory be relegated to its proper place and proportion. Then such "lucky" objectives as the above assume a very marked significance, and hold out a world of promise: in them and in what they tell us lies the future of microscopy. Not the best even of them is probably *perfectly* corrected for all its zones; but the best of them reveal a marvellous standard of approach to this; and with that we find ever associated an increase of that practicable cone of heterogeneous light which we have found so all-important to true contours. *And with this we get further revelation.* More *minuteness* we do not indeed get; for that we can look only to the 1·63 lens. But we have a world of structure to learn yet, *within the resolution* of our present lenses; and for that we are only waiting better condensers and better correction. It was only recently that the protoplasm so long written of as "structureless jelly," yielded up some at least of its marvellous and minute structure, which can only be seen by English wide-cone methods, with one of the exceptionally-perfect objectives here referred to; whose significance, however, as we have seen, is not yet recognized on the Continent as it is in England, and even here only by the few. It may be beyond us to-day to discover the minute departures from type which cause the superiority of the few phenomenal lenses: it is no easy thing to ascertain precisely what it is, in a lens one of whose components may not exceed a hemisphere $\frac{1}{16}$ of an inch in diameter. But the superiority is there; it *has* been attained; and we may cherish reasonable hopes of such discovery. We may anticipate that the present rarest excellence may be reached yet as a standard, more generally procurable by the scientific investigator; that the very best of all may even be further improved in correction in some degree. If it be so, such advances will not be barren of results in research. The microscopist may yet hope and take courage.

LII. *Whether the* x-*rays already exist in the Cathodic Beam which produces them.* *By* Prof. A. Roiti[*].

I.

THE question placed at the head of this communication will be answered by most people in the negative; but there are exceptions, and it may therefore be well to do away with all doubts on the subject.

---

[*] From the *Rendiconti della R. Accademia dei Lincei* (Classe di sc. fis., mat. e nat.). vol. vi. pp. 123–129. Communicated by the Author.

The *x*-rays differ from the cathode-rays in that they cannot be deflected by a magnet, and in that they possess a greater penetrating power. Yet this is no hard and fast distinction; for as there are cathodic rays which can be more or less deflected, so there are *x*-rays capable of penetrating to a greater or smaller degree into various substances. And on the border between the two, although we cannot yet assert, we cannot with certainty deny the existence of cathode-rays that cannot be deflected, and of *x*-rays that can to some extent be turned aside by intense magnetic action; while, on the other hand, if the various material mediums are turbid for the cathode-rays, they are not perfectly transparent to the *x*-rays.

Since matters stand thus, one may be allowed to maintain in opposition to the English, who continue to adduce excellent reasons in defence of Crookes's theory of radiating matter, that the two kinds of rays are not essentially different, but that they gradually merge into each other. Röntgen himself, in his third communication*, seems inclined to admit this.

But this is a very different thing from asserting that the *x*-rays are derived from the cathode-rays by a simple process of subtraction.

Among the various attempts at proving this last assertion I must name those of my countrymen †. I refrain, however, from analysing them minutely, as I am anxious to avoid here any appearance of polemic. Besides, a very little thought will be sufficient to confute them if the following considerations be borne in mind.

1. If we attempt to determine dispersion by observing the variations of potential presented by a conductor placed in a discharging-tube (whether subjected or not to magnetic action), we find it impossible to decide whence the dispersive action comes and what way it follows; because the distribution and the fluctuations of the electricity within the tube cannot yet be rightly determined ‡; and because the phenomenon is

---

* *Sitzungsberichte der Akademie zu Berlin,* vom 13 Mai, 1897, vol. xxvi. p. 576.

† A. Battelli, *N. Cimento,* ser. 4, vol. iii. p. 193, and vol. v. p. 386; Phil. Mag. Feb. 1898. A. Battelli and A. Garbasso, *N. Cimento,* vol. iii. p. 289, vol. iv. p. 129, vol. vi. p. 5.

‡ And, in fact, by a slight change in the conditions of the experiments contradictory results are produced. For example, Battelli and Garbasso (*N. Cimento,* vol. iv. p. 129, and vol. vi. p. 5) find that the cathodic rays induce a positive discharge on an insulated conductor (in the last notice *negative* is a printer's error); while before them Perrin (*Compt. Rend.* vol. cxxi. p. 792) had found it negative.

complicated by the fact that the residual gas is decomposed into free ions, as is demonstrated by J. J. Thomson.

2. Every point on a body exposed to cathode-rays becomes a point of emanation for x-rays which go in all directions, both in the tube and in the body exposed, to a considerable depth, so that they can pass through it and emerge. If we maintain, then, that the x-rays already exist in the impinging beam, we must admit that when they meet a body they do not go straight, but are diffused in all directions.

3. Photographic impressions external to the tube and the shadows shown by fluorescent screens move or are modified in accordance with the deviations of the internal cathode-rays; hence we are forced to admit that the points struck by the deflected rays have also become points of emanation for non-deflectible x-rays; and it would be absurd to suppose that these latter already existed in the beam which excited them.

4. As a last defence of the proposition, then, one would have to maintain that, if not all, a great part at any rate of the x-rays were contained in the cathodic beam—that part which does not obey magnetic action.

Later on I shall describe some experiments which show, on the contrary, that if at any rate rays incapable of deflexion may enter into the formation of the cathode-beam, such rays have a degree of intensity incomparably less than the x-rays that issue from the same tube.

5. There remains, therefore, no other explanation than this: that the cathode-rays are transformed when they strike an obstacle; just as occurs in the common phenomena of calorescence and fluorescence produced by the various rays of the solar spectrum.

6. On the other hand, the cathode-rays properly so-called —those that can be deflected by magnetic action—are reflected and are transmitted through thin strata, passing into a vacuum or into the air with all their characteristic properties. This was taught by Lenard and confirmed by recent experiments of photographs taken within the tubes. These photographs, if the wrapper containing the sensitive film is very thin, are at bottom nothing but the repetition under another form of those (now become classic) obtained by Lenard himself.

7. It is natural that no regularity should have been observed in the reflexion of the cathode-rays: in the first place, the surfaces employed have not, as a rule, been smooth and polished enough; in the second place, mirrors optically perfect may prove furrowed for these rays. Nor do I understand how any one can affirm that they do not follow the laws of

diffusion, since these are so various and not yet firmly established. Hence it will be better, until further proof, to assume that they do undergo diffusion.

8. The emission of the *x*-rays is not confined to the surface of the object struck, but takes place within a stratum of perceptible thickness; and this follows as a consequence [*] from the fact that it does not follow Lambert's law of cosines, but is constant in all directions. For this reason, as Hertz and Lenard have shown, very thin strata are transparent for cathode-rays which are there transformed to a certain extent; but when the thickness is increased the intensity of the transmitted rays sinks to zero; while, on the other hand, the intensity of the *x*-rays emanating from these strata increases to a maximum, so that photographs taken in the interior of the tubes upon films protected by not very thin wrappers are due rather to the *x*- than to the cathode-rays.

9. Lenard certainly had under his hands the rays afterwards detected by Röntgen; but they escaped him perhaps because the keton-screen with which he made his observations, although it fluoresces under the action of cathode-rays, does not do so under that of *x*-rays [†], but more probably because his windows of aluminium-leaf were too thin to emit *x*-rays of sufficient intensity; and for this reason all the rays issuing from the tube through these windows appeared to him more or less deflected under the action of the magnet.

10. On the invention of the focus-tubes in which a small plate of platinum inclined at an angle of 45° acts as an anticathode, photographs were obtained with outlines so clear as to indicate that the place from which the *x*-rays emanated was confined to the points of the platinum struck by the cathodic pencil. But it was soon observed that at the discharge the whole of the wall in front of the platinum plate shines with a fluorescent light, while that behind it remains almost dark unless the little plate of platinum be replaced by a leaf. And an object placed within the tube before the anticathode plate throws a shadow both on the wall and on an external fluorescent screen: the first shadow is displaced by magnetic action, but not the second [‡].

There can be, therefore, no doubt that there spring from the platinum anticathode not only *x*-rays, but also rays of a different kind which were called *internal* rays, and were considered different from the cathode-rays because they were

---

[*] Ch.-Ed. Guillaume, "Sur l'Emission des Rayons *x*," *Compt. Rend.* vol. cxxiii. p. 450.

[†] J. Precht, Wied. *Ann.* Bd. lxi. p. 345.

[‡] S. Thompson, 'The Electrician,' Jan. 8, 1897.

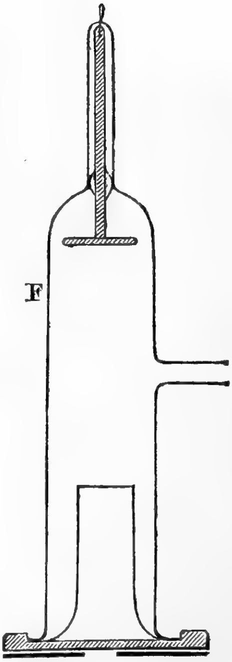
said to be incapable of producing x-rays, and because they did not arise from the impinging pencil either through regular reflexion or through diffusion.

11. It must be remembered, however, that photographs of the tube, taken with a *camera* without lens, show that some x-rays, weak ones it is true, also start from all the surface of the glass rendered fluorescent by the so-called internal rays; and I have already shown that their non-observance of the laws of reflexion does not seem a sufficient reason for making a distinction between these rays and the cathode-rays properly so-called. And let it be remembered that whilst the cathode-rays cause a marked fluorescence in glass, I have assured myself that x-rays cause almost none, even when their point of departure is an anticathode of aluminium serving as a dividing wall, so that they have not already passed through glass.

II.

I pass to the promised experiments, which, as I think, would alone suffice to exclude the idea that the x-rays are derived from the cathodic pencil by a simple process of subtraction.

Fig. 1.



The tube, represented by fig. 1 at one third the natural size, contained the ordinary aluminium cathode, and was closed by a disk also of aluminium held tight against the glass edge, which had been ground. Against the disk was placed a leaden diaphragm pierced by a hole of 1 centim. in diameter, so that only the central rays given out by the disk, which acted as anticathode and as anode, reached the actinometer[*]; and the other face of the actinometer, protected by glasses of cobalt, received the light from an incandescent lamp. A permanent magnet being advanced towards the tube, between cathode and anticathode, the cathodic pencil was directed on to the lateral wall, and the maximum and minimum fluorescence observed by Birkenland[†] were distinctly visible at F.

The deflexion of the internal pencil was accompanied by a great darkening of the face of the actinometer covered with

---

[*] *L'Elettricista,* Year V. p. 197.

[†] "Sur un Spectre des Rayons cathodiques," *Compt. Rend.* vol. cxxiii. p. 492.

platinocyanide of potassium and turned towards the centre of the anticathode disk. Under the influence of moderate rarefaction this face, at first extremely brilliant, became perfectly dark ; but when the rarefaction was great, it still showed 4/100 of its original brightness, showing that the anticathode, notwithstanding the deflexion, was still giving off *x*-rays. This small residuum might, however, have been formed, so to speak, by second intention—by the cathode-rays reflected on the lateral wall F.

To decide this question I introduced into the tube a leaden tube, supported against the aluminium bottom as is seen in the figure ; and by means of this screen the brightness, even with extreme rarefaction, was reduced to 1/1000 under the action of the magnet.

On replacing the permanent magnet by an electromagnet separated from the tube by plates of ebonite to prevent the discharge taking place, at that extreme rarefaction, on the outside, every excitement of the electromagnet was observed to occasion the sudden passage of the actinometer from extreme brilliancy to perfect darkness.

The coil with the interruptor, which I have described in *L'Elettricista* for November 1897, gave a spark of 15 centim. between balls of 2 centim. in diameter, and the magnetizing current of the electromagnet was less than half an ampere.

Hence it is clear that non-deflectible cathodic rays probably do not exist, or if they exist they are not transformable into *x*-rays.

III.

As an appendix to this communication I will speak of one of my former experiments[*], which also shows that *x*-rays cannot be considered as simply diffused cathodic rays. Two tubes, of the form represented in fig. 1, alike in all respects except that in one the disk was of aluminium, in the other of copper, were both applied to the same air-pump and connected with the two poles of a Ruhmkorff coil. On covering the aluminium disk with a copper plate and the copper disk with an aluminium plate in such a manner that the *x*-rays issuing from the two tubes were forced to traverse equal thicknesses of the two metals, I found the power of emission of the second tube considerably greater than that of the first.

Since Prof. Battelli has arrived at a different conclusion[†], finding that glass, aluminium, and magnesium experimented

---

[*] *Rend. della R. Acc. dei Lincei,* vol. vi. p. 355. Experiment of August 26th, 1896.

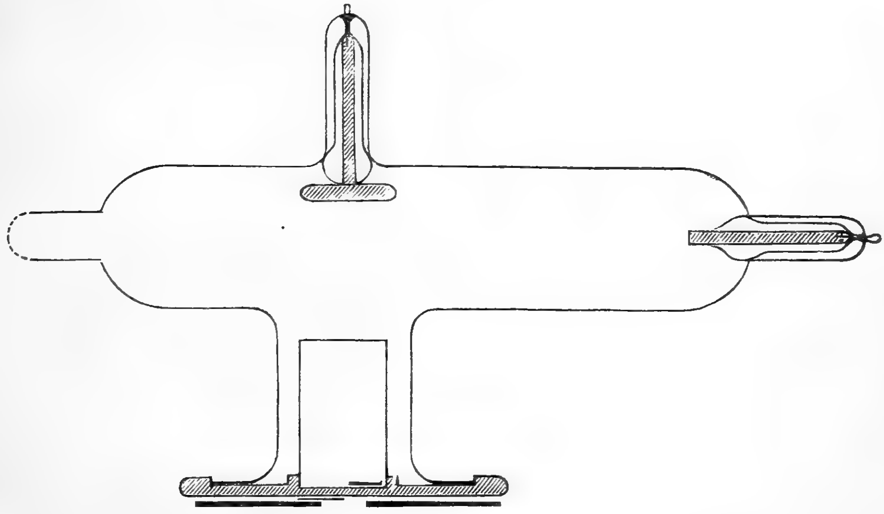
[†] *N. Cimento,* ser. 4, vol. v. May 1897. Experiment 15.

with under like conditions have equal power of emission, I doubted whether the similarity of my tubes might not have been more apparent than real; for the slightest trace of gas contained in the metals or adhering to the walls is sufficient to profoundly modify the results of the experiment; and I repeated the experiment, using one tube only. I covered the aluminium disk, half internally and half externally, with two pieces of the same sheet of magnesium, being careful to fix the internal half, which might otherwise, under the influence of the discharge, have been so much agitated as to be entirely overturned. The disk served as an anode, was in communication with the ground, and was protected externally by a sheet of lead perforated in the centre.

When I applied a cryptoscope I could observe not the slightest difference in brightness on either side of the diameter of separation. This proves nothing if one reflects that, with the concave cathode, the region struck by the cathodic rays must be very limited in extent. But I found it also very limited when I used a convex cathode, on account of the electrostatic action which the cylindrical wall has on the pencil, which would otherwise be divergent.

I was able to obtain a fairly uniform illumination of an extended area by using the tube represented in fig. 2, in which I had placed a slightly convex cathode and had fixed an aluminium tube to the disk. With this, keeping the anodic disk in communication with the filiform aluminium electrode and with the ground, I was able to assure myself that the half internally uncovered and externally covered by the magnesium gave a greater luminosity to the platino-

Fig. 2.



cyanide of barium of the cryptoscope than did the other half. Hence it may be maintained that aluminium has a higher power of emission than magnesium.

It then occurred to me to compare other metals in the same way, by twos; covering the aluminium disk internally half with the metal A and half with the metal B, and placing B externally with regard to A, and A with regard to B.

In the following table I have arranged the various metals on an increasing scale of emissive power. The first column shows the thickness of the plates used; the third and fourth their density and atomic weights.

mm. 0·24	Magnesium	1·74	24·38
1·72	Aluminium	2·67	27·1
0·17	Iron	7·79	56
0·10	Copper	8·95	63
0·10	Zinc	6·92	65·4
0·15	*Tin	7·29	118·1
0·10	Silver	10·47	107·94
0·13	Cadmium	8·67	112
0·012	Platinum	21·3	194·3
0·06	Lead	11·37	206·9

It is evident that the power of emission for x-rays is determined neither by density nor by atomic volume, but that (with the exception of tin, perhaps because I used commercial and therefore impure metal) it increases regularly with the atomic weight.

Should this law prove general it would have a theoretic bearing of no small importance. Meanwhile I restrict myself to observing that there should be an advantage, unless its easy fusibility proves a hindrance, in the use of lead instead of platinum as an anticathode in the Röntgen tubes. Lead placed in sheets on aluminium should serve still better, as I intend proving.

(*Addition during the correction of the proofs*). While commercial tin(*) had to be placed between zinc and silver, that furnished as pure by Merck, of Darmstadt, and reduced to a sheet of 0·11 mm. in thickness, shows a considerably greater power of emission; greater than that of cadmium. We may therefore assert that the *metals of greater atomic weight emit* x-*rays of greater intensity.*

Florence, August 20, 1897.

LIII. *Phase-Reversal Zone-Plates, and Diffraction-Telescopes.*

By R. W. Wood[*].

[Plate XXIV.]

In a paper published in Poggendorff's *Annalen* (1875) Soret showed that if we describe a number of small concentric circles on a glass plate, with radii proportional to the square roots of the natural numbers, and blacken the spaces between the alternate rings, the plate will have the property of bringing parallel rays of light to a focus, like a condensing-lens. The dark rings check the disturbances on the alternate Huygens's zones on the wave-front, which by destructive interference with the disturbances from the other zones ordinarily produce darkness at points within the geometrical shadow.

Soret's method was to draw circles of suitable size on a sheet of paper, blackening the alternate rings, and make a reduced photographic copy of the whole. He showed that such a plate formed real images of luminous objects, and could be used as the objective of a telescope or as the eyepiece. He also showed that in addition to acting as a condensing-lens, the zone-plate acted as a concave or dispersing-lens. Moreover, he pointed out that the plate has multiple foci at distances $\frac{a^2}{\lambda}, \frac{a^2}{3\lambda}, \frac{a^2}{5\lambda}$, where a is the radius of the central circle.

Since the appearance of Soret's paper very little seems to have been done with the subject, though it is alluded to briefly in the textbooks, and it has been pointed out by Lord Rayleigh that if it were possible to provide that the light stopped by alternate zones were replaced by a phase-reversal, a fourfold effect would be produced. After some experimenting I have succeeded in producing such a zone-plate, perfectly transparent over its entire extent, which yields an image at least six or eight times as bright as those produced with the old form of plate. This increase in intensity is greater than one would expect, which is probably accounted for by the lack of perfection of the black zone-plates, the black rings being partly transparent and the clear ones not wholly so. Using one of the new plates as the objective of a telescope in connexion with a low-power eyepiece, I have distinctly seen the lunar craters, and have constructed telescopes in which both objective and eyepiece were zone-plates.

The largest plate made by Soret contained 98 dark circles, and since the scale on which it was drawn was rather small, the outer zones could not have been very accurately placed.

---

[*] Communicated by the Author.

The preliminary drawing which I made contained 115 dark rings, and was drawn on a scale three times as large as that used by Soret. In order to avoid the errors due to shrinkage and expansion of the paper during the progress of the work, the following method was adopted. A sheet of heavy smooth surface drawing-paper, considerably over a metre square, was thoroughly dampened and the edges glued down to a heavy drawing-board. The paper was kept damp until the edges had thoroughly dried to prevent the shrinkage from tearing them loose. On drying, the paper was in close contact with the board and as tight as a drumhead. A copper tack with a small conical dent in its head was driven into the centre of the board to serve as a centre for the beam-compass used in drawing the circles. The pen of the compass was accurately set for each circle by means of a Brown and Sharp steel metre-bar, care being taken to set by the inner and outer edge of the pen alternately, in order that no error due to the width of the line should be present. Two hundred and thirty circles were drawn in this manner, using the greatest care possible to ensure accuracy. The spaces between the alternate rings were then blackened by means of the beam-compass and a broad pen. In spite of every precaution errors crept in which manifest themselves as slight shadings at certain places; their effect, however, is small.

The labour involved in the preparation of such a drawing was so great that it has seemed worth while to publish with this paper a reduced photographic copy, from which other still more reduced copies can be prepared on glass by anyone. I have made photographic negatives of this drawing on lantern-slide plates of various sizes, and find that they act very satisfactorily. These copies were made with a very fine Zeiss lens, and had focal lengths varying from half a metre to ten metres. Smaller plates could not be made with this lens, since the limit in the defining power is about ·02 millim. In order to produce plates of very short focus I made some copies with a Beck microscope-objective of about 2 centim. focus. The focal lengths of these plates varied from 3 to 10 centim., and they were found to act admirably as eyepieces. The central circle of one of these micro-plates had a radius of ·158 millim.; its focus for yellow light is therefore $\frac{(\cdot 158)^2}{\cdot 0006}$, or 4·1 centim. In spite of its small size, the rings were sharp out to the edge, where their width, measured on a small dividing-engine, was found to be only ·005 millim.

A number of experiments were tried with these plates; but as they can be shown to a much better advantage with the transparent plates, I will postpone the discussion of them until a little later in the paper. Before taking up the subject of the phase-reversal plate I may as well call attention to a very pretty method of preparing a zone-plate to illustrate its action.

If a profile-face or other design is cut out of black paper and pinned on to the original drawing somewhere between the centre and the edge, and a photograph of the whole about 3 centim. in diameter be made, on looking at a point of light through the plate, the now perfectly transparent profile will appear jet-black on a brightly illuminated back-ground —that is to say, the space within the design being devoid of black lines does not fill up with light.

To prepare a zone-plate in which the light, instead of being cut off by the alternate rings, shall suffer a phase-reversal, the best method appeared to be to prepare a plate with transparent zones of a thickness sufficient to retard the light one-half wave-length. Sheets of thin plate-glass with the surface carefully cleaned were flowed with a warm solution of gelatine (strong enough to just set into a jelly on cooling), and set on edge to dry. The films were then sensitized by immersing the plates for about five seconds in a weak solution of bichromate of potassium, and dried in the dark. Properly prepared films should exhibit no trace of crystallization, and should have a barely perceptible yellow tint. These plates were placed in contact with the photographic reduction, and printed in direct sunlight; the exposure varying from 20 seconds to 1 minute according to the density of the negative. They were then removed from the frame, washed for a moment in cold water, and then immersed in rather warm water, which dissolved and washed away the gelatine which had been protected by the dark zones, leaving the alternate rings attached to the glass. On drying these plates they were perfectly transparent, but by holding them in certain lights the rings could be seen. The effect produced by them far surpassed my expectations. On viewing a brightly luminous point through them, they fill up with a blaze of light which compares very favourably with a lens. They are not uniformly good, however, everything depending on the thickness of the film, and, moreover, the exposure must have been exactly right. I find that if one of these plates 3 centim. in diameter be set up at a distance of 2½ metres from an arc-light and a paper screen be placed at a suitable distance (3 or 4 metres) behind the plate, an exceedingly bright and sharp, somewhat enlarged image of the arc is projected; the surface irregularities of the heated carbons can be seen, and the blue flame between them, the small incandescent particles that are thrown off being also clearly visible.

If the screen is near the plate, the image of the carbons is orange-red; and as it is moved back the colour changes to yellow, green, and blue, the foci for different colours being far apart. The best definition appears to be in the yellow and green. On setting the plates up at a greater distance from the arc and placing a sheet of paper in the focus, a large part of the light incident on the plate is brought to a very small sharp focus surrounded by a dark area, corresponding to the geometrical projection of that part of the plate which is effective. With some plates this dark area is very small, and the central bright spot correspondingly feeble, showing that what little action there is comes from a few rings in the centre. This forms a very good method of testing the plates; and in general I find that about one out of four can be considered first class. With more care in the preparation of the films greater uniformity in the results could doubtless be secured.

In making these plates it is necessary to use mirror-glass with a ground surface if good definition is to be obtained. I find the German mirror-glass which comes with a deposit of metallic silver backed by varnish gives very good results. The varnish can be removed by alcohol, the silver with nitric acid, and the surface cleaned. It is best to prepare a large number of plates and select the best, either by throwing an image of an arc-light with them on a screen, or by the following method which is perhaps better.

A small hole a millimetre or so in diameter is punched in a black card or a metal plate and set up in front of a bright sodium flame; the zone-plate is mounted at a distance of three metres from this, and the eye brought into the focus. If the plate is good, it lights up brightly and uniformly. Most plates, however, show irregularity in the illumination, and may even have spots that appear quite devoid of light. These dark spots may be caused by the film being so thin as to give no appreciable retardation, or so thick as to give a retardation of a whole wave or a number of whole waves. Plates that do not come up to this test often give very good images, however, and need not be rejected. Those which light up feebly and but a short distance from the centre should be thrown away.

Although a large percentage of the light is wasted by a zone-plate, the focal images produced by these plates were so sharp and brilliant, that I determined to see just what results could be obtained with a telescope constructed with one.

Removing the objective from my 5 in. Clark, I put a 3 centim. gelatine zone-plate in its place. A low-power eyepiece was used, and on viewing terrestrial objects, such as distant chimney-tops and trees against the sky, with this combination, the result seemed to be very unsatisfactory; the amplification was about 60 diameters and the outlines were sharp, but there was very little difference in light value between the sky and dark chimneys. In other words the field was bright. The most suitable objects for a telescope of this description are small, brightly illuminated ones on a black background; under these conditions the general illumination of the field is reduced to a minimum. I tried the telescope on the moon, and although the image was somewhat dim and hazy, as seen through thin clouds, I could distinctly see a number of the larger craters.

The zone-plate used in this case had a focal length of about five feet; but plates of shorter focus show the principle equally well, and make more convenient instruments to handle. If we use an ordinary low-power eyepiece in the principal focus of the zone-plate, we obtain a highly magnified inverted image. If now we push the eyepiece in, the image becomes blurred, vanishes, and a second smaller inverted image appears; and on continuing to diminish the distance, we get images successively smaller and smaller as the eyepiece passes through the different foci of the plate. The best object to view is an incandescent electric lamp at a distance of six or eight metres. With the Soret form of plate, I have never been able to find more than three foci, while the phase-reversal plates easily show six. If instead of the glass eyepiece we use one of the micro-zoneplates, we obtain both erect and inverted images; the inverted when the eyepiece is in the focus, and the erect when it is between the focus and the plate—the former image being due to the action of the micro-plate as a convex lens, the latter to its action as a concave lens, as in the Galilean telescope. In spite of the fineness of the lines in the micro-plates, I find that they can be made to yield very excellent phase-reversal plates by contact printing, so that by making both objective and eyepiece on the new principle, we have a diffraction-telescope wholly without lenses or mirrors, which is fairly efficient for viewing small brightly-illuminated objects.

Starting with the eyepiece close to the objective and moving it slowly back, we get a rapid alternation of erect and inverted images, to the number of eight or ten in all. As the eyepiece moves the colour of the image changes, the foci being differently situated for the different colours.

I have tried making landscape-photographs, using one of these short-focus retardation-plates instead of a lens. The results far surpassed my expectations. There are good qualities in the zone-plate picture that almost offset the deficiencies. Very soft and artistic effects can be produced. Where there is a mass of twigs against the sky the zone-plate rubs in a little extra shade for a background instead of cutting each stem sharp and black against the white sky. The shadows are massed to a great extent, and yet there is definition enough to prevent the picture from appearing out of focus. The landscape reproduced (fig. 1) was made with a plate of 4 millim. aperture and 14 centim. focus, with an exposure of half a second, which is certainly a great improvement on pinhole photography. The picture has lost much in reproduction. The actinic focus of the plate must be found and the image on the ground-glass is never sharp, owing to the chromatic aberration. I have used a blue glass to advantage in finding the focus; and a low sun in thin clouds makes an excellent object to focus upon.

Fig. 1.

A very simple elementary way of explaining the multiple foci, both real and virtual, of the zone-plate which I have not seen given anywhere, probably because of its obvious nature, is the following:—We may regard the zone-plate as a circular grating in which the grating space becomes less and less as we proceed outward from the centre, consequently the bending or deviation of the diffracted ray from the normal becomes greater as we near the edge, and the change in the grating space is such as to bring all the deviated rays of a single colour together in a point. The principal real focus of the plate corresponds to the superimposed spectra of the first order; the second order spectra being bent more come together at a point nearer the plate, forming a second focus, and so on—the different foci corresponding to spectra of different orders. There are also spectra of the first order bent outwards, or away from the centre, and these rays projected backwards behind the plate will meet, forming a virtual focus in a position corresponding to that of the real focus on the opposite side of the plate; the second order spectra are bent out more, consequently the virtual focus of these rays is nearer the plate, and we thus see, that for every real focus on the one side of the plate, there is a corresponding virtual focus on the other.

Plates of very long focus are also useful for demonstration. I have one 7 centim. in diameter, with a focal length of about twelve metres. This plate will project a very good image of the sun, about 11 centim. in diameter, on a screen placed at the focus. The effect is especially fine when the sun is behind the leafless branches of distant trees, for then each twig stands out sharp and distinct on the image. When this plate is set up at a distance of 3 metres from an arc-light and the eye is brought into one of the nearer foci, 3 or 4 metres behind the plate, the effect is very fine; the whole area filling up with a dazzling white light of almost insupportable brilliancy.

The diffraction phenomena produced by openings in thin transparent films were studied by Quincke and published in Poggendorff's *Annalen*, 1867. Quincke, moreover, prepared gratings with narrow strips of a thin transparent lamina instead of the usual dark spaces: these, by retarding the light, threw the waves out of phase with those which passed between them. These gratings he prepared by ruling the silvered surface of a piece of glass on a dividing-engine, forming an ordinary grating in which the dark lines were narrow strips of silver equal in width to the clear spaces. By covering the plate with iodine the silver was changed into transparent iodide. He found that these transparent gratings gave spectra very similar to ordinary gratings, save in one respect, namely, that with films of certain thickness the central direct image disappeared entirely when monochromatic light was used, and passed through a succession of colours when white light was used. By making the films slightly wedge-shaped and ruling the lines at right angles to the thin edge, he obtained a grating in which the thickness increased continually towards the bottom. On looking at an illuminated slit through the top of this grating where the thickness of the film was zero, the central image alone was observed. On moving the grating up, the film began to have an appreciable thickness, and side or diffracted images appeared on either side of the central one. These increased in brightness with increasing thickness of the film, while the central image became fainter, and finally disappeared when the thickness was such as to give to the light passing through it a retardation of one half wave-length on the light passing by it. We thus have a transparent plate which will not allow a ray to pass directly through it, or, in other words, a transparent grating which gives no central image. I find that very satisfactory lamina gratings can be prepared in the same way as the zone-plates; by drawing black lines of uniform width and with uniform spaces between them on white paper, reducing them by photography, and printing on the sensitized gelatine. The same effects can be observed by viewing an illuminated slit through the outer edge of the lamina zone-plate held close to the eye.

Quincke found that if a portion of the hypothenuse surface of a right-angle prism be silvered, a ray reflected from the metal suffers a phase displacement relative to a ray which is totally reflected from the boundary between glass and air. Whether or not this change is a complete phase-reversal or not depends on a number of conditions. If polarized light is used, the change of phase depends on whether the plane of polarization is parallel or perpendicular to the plane of incidence, on the angle of incidence, and on the thickness of the film. It appeared probable that a silver zone-plate, formed on the surface of a reflecting prism, would give excellent results under certain conditions. Such a plate could be ruled on a slowly revolving prism, but the process of advancing the knife-point over the right distances would be tedious in the extreme, and I accordingly hunted about for a photomechanical process. After a little experimenting I devised a method which yielded beautiful results. Plate-glass, carefully cleaned and silvered by the chemical method, is coated with an exceedingly thin film of gelatine: the solution should be just too weak to set into a jelly on cooling, merely thickening a little. It should be poured while warm over the silver surface, and the plate set on edge to dry. This takes place in five or ten minutes, and the surface now shows interference colours. The film is now sensitized by a ten-second immersion in a very dilute solution of bichromate of potash (plate dipped in it should be coloured a very pale yellow only) and dried in the dark *.

An impression is taken by exposure under a negative, the plate is held for a moment in cold water and then washed in a stream of fairly hot water. This is the fussy part of the operation ; for if the stream is too violent or the water too hot it will wash away everything, and if it is too feeble some gelatine will be left between the undissolved zones. I find that pouring from a beaker held at a height of 10 centim. gives a stream of the proper strength, and the temperature should be about what the hand can bear comfortably. A half minute's washing is enough, and the plate must now be flooded with alcohol and then with ether, to prevent the slow solution of gelatine during the drying, which always coats the clear zones with a thin film. The plate will dry in a few seconds, and if it has been properly prepared the odd zones will be clean bright silver, and the even ones covered with a thin film which appears white by reflected light. The plate is now covered with iodine crystals, which convert the exposed silver into iodide, and the process can be watched by holding the plate above the head and looking at the under side. The iodide zones will appear white, and on shaking off the iodine transparent by transmitted light.

A strong solution of sodium thiosulphate is now applied, which dissolves the silver iodide, and the plate is then rubbed with the fingers under warm water until the gelatine is all removed. If everything goes well a most beautiful plate is the result. The silver rings are as sharp as if cut with a knife, and the spaces between them are quite clean, even where there are fifty zones to the millimetre. Such a plate used with transmitted light is much better than the usual photographic type, in that the clear spaces are perfectly clean and transparent, and the dark ones perfectly opaque, which, as I said before, is usually not the case.

It is not necessary to make the zones directly on the prism, though I tried this first. It is essentially the same thing if we make them on a plate of glass and fasten this to the prism face with Canada balsam, the silver side out of course. The brilliancy of the image produced by this arrangement is very great, but since the reflecting surface is at an angle of

---

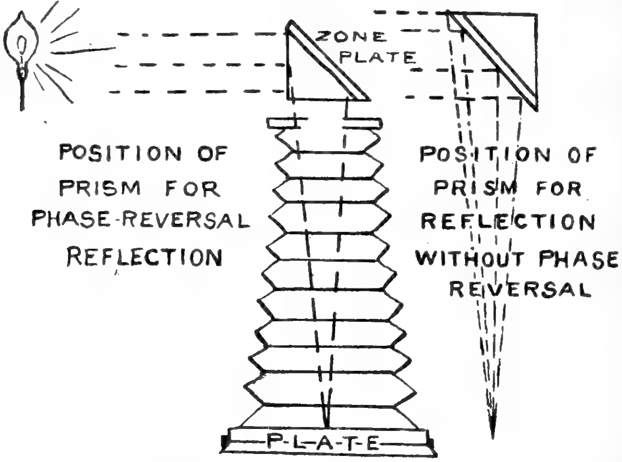
* Lately I have added the bichromate to the gelatine before flowing the plate, and find the process quicker and the results better.

45° the focus instead of being a point is a line. When the screen is near the prism we get a very bright vertical line, which fades away as the screen is moved off, being replaced with a horizontal line. This is exactly what we get when we hold a transmission plate at an angle with the screen, and the greater the obliquity the shorter the focus, the increasing obliquity decreasing the grating space on the sides of the plate.

To get a point-focus from a reflecting plate on a prism the zones must be elliptical, the major axes being double the minor, the whole being so placed that its projection on each of the other two faces of the prism is a circle. To make an elliptical zone-plate we have only to photograph the original drawing in an oblique position. A slight error due to perspective will of course be introduced, but it is insufficient to materially affect the results.

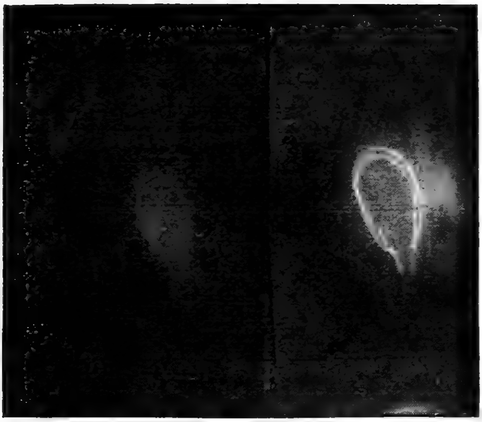
From a negative made from the large drawing set at an angle of 45° I prepared a silver-on-glass elliptical plate, and cemented it onto the prism with balsam. This combination fulfilled every expectation, and yielded a very brilliant and sharp image. It furnished, moreover, a very nice means of comparing the effect of an ordinary plate with a phase-reversal one; for by turning it round a focal image was formed by the reflexion from the outer surface of the silver zones alone, which was many times fainter than that produced by the internal reflexion. In making this comparison I noted the curious circumstance that the focus from internal reflexion is longer than that from external reflexion, but can as yet offer no explanation of the fact.

Fig. 2.



In order to give some idea of the relative intensities of the focal images produced by the plate with and without a phase-reversal, I photographed an incandescent lamp-filament, first from the outside surface of the zones and then from the inside, by the arrangement shown in fig. 2. The time of exposure and development was the same, and both were taken on the same plate. Reproductions of the two images are shown in fig. 3. It will be noted that the brilliant one (or phase-reversal image) is larger than the other. This is due to the increase in the focal length to which I have alluded, and, other things being equal, would make the illumination less. The image obtained from the outer surface of the plate is so faint that it is hardly perceptible.

Fig. 3.



I have made a very cursory examination of these phase-reversal reflecting plates with polarized light, and find that if the light falling on the prism be plane-polarized, rotation of the nicol causes fluctuations in the brilliancy of the image. The effect, however, varies with the angle of incidence; at an angle of 40° with the normal of the reflecting surface no effect is produced by rotating the nicol. When the angle is greater than this, partial extinction of the image is produced when the plane of polarization is perpendicular to the plane of reflexion; when the angle is less than 40°, the reverse is the case. The field about the image grows bright when the central image weakens, and *vice versâ*. This requires a much more complete investigation than I have had time to give it yet, but is doubtless caused by the variations in the phase-change due to variations in the angle of incidence which Quincke observed.

Lord Rayleigh has drawn attention to the fact that if it were possible to construct a lamina-grating in which an arbitrary retardation could be introduced at every part of the aperture all the light could be concentrated in any desired spectrum. This might be accomplished, as Prof. Crew has suggested to me, by shading the original drawing: possibly photographing a wire-grating on a white ground, illuminated by oblique light (the shading being produced by penumbræ). Or a series of wedge-shaped strips of tinted glass or gelatine in transmitted light might yield results. The gelatine films would have to receive the impression through the glass, I imagine, if variations in the thickness were necessary.

If this could be accomplished, the same principle could be applied to the zone-plate, all the light being brought to a single focus.

Experiments in this direction are now in progress.

Physical Laboratory of the University of Wisconsin.
Madison, February 1898.

LIV. *Note on the Pressure of Radiation, showing an Apparent Failure of the usual Electromagnetic Equations.* By Lord RAYLEIGH, *F.R.S.*[*]

FOLLOWING a suggestion of Bartoli, Boltzmann[†] and W. Wien[‡] have arrived at the remarkable conclusion that that part of the energy of radiation from a black body at absolute temperature θ, which lies between wave-lengths λ and $\lambda+d\lambda$, has the expression

$$\theta^5\phi(\theta\lambda)\,d\lambda, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where ϕ is an arbitrary function of the *single* variable $\theta\lambda$. The law of Stefan, according to which the total radiation is as θ^4, is thererein included. The argument employed by these authors is very ingenious, and I think convincing when the postulates are once admitted. The most important of them relates to the *pressure* of radiation, supposed to be operative upon the walls within which the radiation is confined, and estimated at one-third of the *density* of the energy in the case when the radiation is alike in all directions. The argument by which Maxwell originally deduced the pressure of radiation not being clear to me, I was led to look into the question a little more closely, with the result that certain discrepancies have presented themselves which I desire to lay before those who have made a special study of the electric equations. The criticism which appears to be called for extends indeed much beyond the occasion which gave rise to it.

A straightforward calculation of the pressure exercised by plane electric waves incident perpendicularly upon a metallic reflector is given by Prof. J. J. Thomson[§]. The face of the reflector coincides with $x=0$, and in the vibrations under

---

[*] Communicated by the Author.

[†] Wied. *Ann.* vol. xxii. pp. 31, 291 (1884).

[‡] *Berlin. Sitzungsber.* Feb. 1893.

[§] 'Elements of Electricity and Magnetism,' Cambridge, 1895, § 241.

consideration the magnetic force reduces itself to the component (β) parallel to y, and the current to the component (w) parallel to z. The waves which penetrate the conducting mass die out more or less quickly according to the conductivity. If the conductivity is great, most of the energy is reflected, and such part as is propagated into the conductor is limited to a thin skin at $x=0$. According to the usual equations the mechanical force exercised upon unit of area of the slice dx of the conductor is $-wb\,dx$, or altogether

$$\int_0^\infty wb\,dx. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (2)$$

Here b denotes the magnetic induction, and is equal to $\mu\beta$, if μ be the permeability and β the magnetic force. Now

$$4\pi w = d\beta/dx,$$

so that the integral becomes

$$\frac{\mu}{8\pi}\{\beta_0^2-\beta_\infty^2\}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (3)$$

where β_0 is the value of β within the conductor at $x=0$, and $\beta_\infty=0$, if the conducting slab be sufficiently thick. Since there is no discontinuity of magnetic force at $x=0$, β_0 may be taken also to refer to the value at $x=0$ just *outside* the metallic surface.

The expression (3) gives the force at any moment; but we are concerned only with the mean value. Since the mean value of β_0^2 is one-half the maximum value, we have for the pressure

$$p=\frac{\mu}{16\pi}\beta^2_{\text{max.}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (4)$$

It only remains to compare with the density of the energy outside the metal, and we may limit ourselves to the case of complete reflexion. The constant energy of the stationary waves passes alternately between the electric and magnetic forms. If we estimate it at the moment of maximum magnetic force, we have

$$\text{energy} = \frac{1}{8\pi}\iiint \beta^2 dx\,dy\,dz. \quad . \quad . \quad . \quad . \quad . \qquad (5)$$

In (5) β is variable with x. If $\beta_{\text{max.}}$ denote the maximum value which occurs at $x=0$, the mean of $\beta^2=\frac{1}{2}\beta^2_{\text{max.}}$ Thus

$$\text{density of energy} = \frac{\text{energy}}{\text{volume}} = \frac{1}{16\pi}\beta^2_{\text{max.}} \quad . \quad . \quad . \qquad (6)$$

Thus, if the permeability μ of the *metal* be unity, (4) and (6) coincide; and we conclude that in this case the pressure is equal to the density of the energy in the neighbourhood of the metal. This is Maxwell's result. When we consider radiation in all directions, the pressure is expressed as *one-third* of the density of energy.

The difficulty that I have to raise relates to the case where μ is not equal to unity. The conclusion in (4) that the pressure is proportional to μ would make havoc of the theory of Boltzmann and Wien and must, I think, be rejected. So long as the reflexion is complete—and it may be complete independently of μ—the radiation is similarly influenced, and (one would suppose) must exercise a similar force upon the reflector. But if the conclusion is impossible, where is the flaw in the process by which it is arrived at? Being unable to find any fault with the deduction above given (after Prof. J. J. Thomson), I was led to scrutinize more closely the fundamental equation itself; and I will now explain why it appears to me to be incorrect.

For this purpose let us apply it to the very simple case of a wire of circular section, parallel to z, moving in the direction of x across an originally uniform magnetic field (β). The uniformity of the field is disturbed in two ways: (i.) by the operation of the current (w) flowing in the various filaments of the wire, and (ii.) independently of a current, by the magnetic effect of the material composing the wire whose permeability (μ) is supposed to be great. In estimating as in (2) the mechanical force parallel to x operative upon the wire, we should have to integrate wb over the cross-section. In this w is supposed to be constant, and the local value is everywhere to be attributed to b. We may indeed, if we please, omit from b the part due to the currents in the wire, which will in the end contribute nothing to the result; but we are directed to use the actual value of b as disturbed by the presence of the magnetic material. In the particular case supposed, where μ is great, the value of b within the wire is uniform, and just twice as great as at a distance. It follows, when the integration is effected, that the force parallel to x acting upon the wire is greater (in the particular case doubly greater) than it would be if the value of μ were unity.

But this conclusion cannot be accepted. The force depends upon the number of lines of force to be crossed when the wire makes a movement parallel to x. And it is clear that the lines effectively crossed in such a movement are not the condensed lines due to the magnetic quality of the wire, but are to be reckoned from the intensity of the *undisturbed* field.

The mechanical force cannot really depend upon μ, and the formula which leads to such a result must be erroneous.

As regards the problem of the pressure of radiation, I conclude that in this case also, and in spite of the formula, the permeability of the reflector is without effect, and that the consequences deduced by Boltzmann and Wien remain undisturbed.

Another investigation to which perhaps similar considerations will apply is that of the mechanical force between parallel slabs conveying rapidly alternating electric currents. Prof. J. J. Thomson's conclusion [*] is that the electromagnetic repulsion is μ times the electrostatic attraction, so that a balance will occur only when $\mu = 1$. It seems more probable that the factor μ should be omitted, and that balance between the two kinds of force is realized in every case.

LV. *The Reduction to normal Air-Temperatures of the Platinum-Temperatures in the Low-Temperature Researches of* Professors Dewar *and* Fleming. *By* J. D. HAMILTON DICKSON, *M.A., F.R.S.E.*[†]

THE measurement of temperature by means of platinum depends upon the two following propositions:—(1) That for a given piece of pure annealed platinum-wire the temperature is a single-valued function of the electric resistance. This proposition is due to Prof. Callendar (1886), and has been fully verified by many subsequent observers. The second proposition is:—(2) That however different specimens of pure annealed platinum-wire may vary among themselves, nevertheless they agree in giving the same normal air-temperature of any enclosure in which they may be simultaneously placed. This proposition might, at first sight, appear as a logical deduction from the first; but a little consideration will show that the two propositions are equally fundamental, and equally necessarily due to experiment. We are indebted for it to the careful researches of Mr. E. H. Griffiths.

Theory has not yet provided the formula referred to in the first proposition; meanwhile, Prof. Callendar has devised a double formula—or, rather, a formula with a correction—which amounts to the expansion of the electric resistance of the platinum-wire in powers of the temperature, and leads to a somewhat troublesome reduction before finally

---

[*] 'Recent Researches in Electricity and Magnetism,' 1893, § 277.

[†] Communicated by the Author.

obtaining the normal air-temperature required. I have given reasons * to show that another empiric formula amounting to the expansion of the temperature in powers of the electric resistance of the wire, while free from some theoretical faults attached to other formulæ, is at least as good as the compound formulæ of Prof. Callendar, and evades the undesirable idea of "platinum-temperatures."

The end I had in view in getting this formula was the reduction to normal air-temperatures of the platinum-temperatures in which the results were expressed, of the many and important researches of Professors Dewar and Fleming in their varied investigations at low temperature. In furtherance of this end, Messrs. Petavel and Morris, who so ably assisted them in these investigations, told me that the piece of platinum-wire which forms the body of the platinum-thermometer employed by Professors Dewar and Fleming, and referred to as "P_1," is the identical piece of wire whose variations of electric resistance with temperature are given on p. 282 of Phil. Mag. for Sept. 1893, vol. xxxvi.† The experiments on this wire there recorded range over nearly 300°, namely, from about −200° C. to about +100° C. The normal air-temperatures observed are given down to the freezing-point of water, while lower temperatures are merely indicated. Thus, one low temperature which I have employed is indicated as "taken in solid carbonic acid and ether," and the other as "taken in liquid oxygen boiling at 760 millim." I have assumed that the former of these temperatures has most probably Regnault's value, −78°·2 C.; and there seems to be a general convergence of opinion towards −182°·5 C. as the value of the latter.

With these data, and with the details immediately following, the annexed table has been constructed, reducing the "platinum-temperatures" expressed by means of Professors Dewar and Fleming's platinum-thermometer "P_1" to normal air-temperatures on the centigrade scale.

---

* Phil Mag. (1897) xliv. p. 445.

† See also this paper, where it is stated that the same wire is used to measure the temperatures recorded in it, p. 275.

Reduction to degrees centigrade of the platinum-temperatures in Profs. Dewar and Fleming's low-temperature observations as recorded by their thermometer "P_1."

These *pt*-degrees are reduced to resistances R by means of their formula

$$\text{“}R = {\cdot}010975\,(pt+283{\cdot}0)\text{”}$$

on p. 100, Phil. Mag. July 1895; and these resistances are then converted into centigrade degrees by formula (11) on p. 453, Phil. Mag. Dec. 1897, viz.:—

$$\text{“}(R+20{\cdot}529023)^2 = {\cdot}53270015\,(t+1048{\cdot}4396).\text{”}$$

pt°.	*t*° C.	Diff. for 1 *pt*°.	*pt*°.	*t*° C.	Diff. for 1 *pt*°.	*pt*°.	*t*° C.	Diff. for 1 *pt*°.
100	99·85	1·016	− 30	− 28·81	·960	−160	−149·83	·902
95	94·77	1·016	− 35	− 33·61	·956	−165	−154·33	·900
90	89·60	1·014	− 40	− 38·39	·954	−170	−158·82	·898
85	84·62	1·012	− 45	− 43·16	·952	−175	−163·30	·896
80	79·56	1·008	− 50	− 47·92	·952	−180	−167·77	·894
75	74·52	1·006	− 55	− 52·68	·948	−185	−172·22	·890
70	69·49	1·004	− 60	− 57·42	·946	−190	−176·67	·890
65	64·47	1·002	− 65	− 62·15	·942	−195	−181·11	·888
60	59·46	1·000	− 70	− 66·83	·942	−200	−185·53	·884
55	54·46	·998	− 75	− 71·57	·938	−205	−189·94	·882
50	49·47	·996	− 80	− 76·26	·936	*−210*	*−194·34*	·880
45	44·49	·994	− 85	− 80·94	·934	*−215*	*−198·73*	*·878*
40	39·52	·990	− 90	− 85·61	·932	*−220*	*−203·11*	*·876*
35	34·57	·988	− 95	− 90·27	·930	*−225*	*−207·47*	*·872*
30	29·63	·986	−100	− 94·92	·928	*−230*	*−211·82*	*·870*
25	24·70	·984	−105	− 99·56	·924	*−235*	*−216·17*	*·870*
20	19·78	·982	−110	−104·18	·924	*−240*	*−220·51*	*·868*
15	14·87	·980	−115	−108·80	·922	*−245*	*−224·83*	*·864*
10	9·97	·978	−120	−113·41	·918	*−250*	*−229·13*	*·860*
5	5·08	·976	−125	−118·00	·916	*−255*	*−233·43*	*·860*
0	0·20	·972	−130	−122·58	·914	*−260*	*−237·72*	*·858*
− 5	− 4·66	·970	−135	−127·15	·912	*−265*	*−242·00*	*·856*
−10	− 9·51	·968	−140	−131·71	·908	*−270*	*−246·26*	*·852*
−15	−14·35	·966	−145	−136·25	·908	*−275*	*−250·51*	*·850*
−20	−19·18	·964	−150	−140·79	·906	*−280*	*−254·76*	*·850*
−25	−24·00	·962	−155	−145·32	·902	*−283*	*−257·30*	*·847*

The numbers in *italics* are outside the range of experiment.

This table is available for the following papers :—

1. The electrical resistance of metals and alloys at temperatures approaching the absolute zero. Phil. Mag. (1893) xxxvi. p. 271 ; Electrician (1893), xxxi. p. 529.
2. Thermo-electric powers of metals and alloys between the temperatures of the boiling-point of water and the boiling-point of liquid air. Phil. Mag. (1895) xl. p. 95 ; Electrician (1895), xxxv. p. 365.
3. The variation in the electric resistance of bismuth when cooled to the temperature of solid air. Phil. Mag. (1895) xl. p. 303 ; Electrician (1895), xxxv. p. 612.
4. Electric and magnetic research at low temperatures. Electrician (1896), xxxvii. pp. 301, 338.
5. On the electrical reistivity of bismuth at the temperature of liquid air. Proc. Roy. Soc. (1896–97) lx. p. 72.
6. On the electrical resistivity of pure mercury at the temperature of liquid air. Proc. Roy. Soc. (1896–97) lx. p. 76.
7. On the magnetic permeability and hysteresis of iron at low temperatures. Proc. Roy. Soc. (1896-97) lx. p. 81.
8. Note on the dielectric constant of ice and alcohol at very low temperatures. Proc. Roy. Soc. (1896–97) lxi. p. 2 ; Electrician (1897), xxxviii. p. 748.
9. On the dielectric constants of certain frozen electrolytes at and above the temperature of liquid air. Proc. Roy. Soc. (1896–97) lxi. p. 299.
10. On the dielectric constants of pure ice, glycerine, nitrobenzol and ethylene dibromide at and above the temperature of liquid air. Proc. Roy. Soc. (1896–97) lxi. p. 316.
11. On the dielectric constants of certain organic bodies at and below the temperature of liquid air. Proc. Roy. Soc. (1896–97) lxi. p. 358.
12. On the dielectric constants of metallic oxides dissolved or suspended in ice cooled to the temperature of liquid air. Proc. Roy. Soc. (1896–97) lxi. p. 368.
13. Further observations on the dielectric constants of frozen electrolytes at and above the temperature of liquid air. Proc. Roy. Soc. (1896–97) lxi. p. 380.
14. Electric research at low temperatures. Electrician (1897), xxxix. p. 645.
15. A note on some further determinations of the dielectric constants of organic bodies and electrolytes at very low temperatures. Proc. Roy. Soc. (1897–98) lxii. p. 250.

Peterhouse, Cambridge.

LVI. *The Thomson Effect in a Binary Electrolyte.*

By F. G. Donnan, *M.A., Ph.D.*[*]

THE coefficient σ in the equation $de=\sigma dt$ for an unequally heated homogeneous conductor may be calculated for an aqueous solution by means of the theory of electrolytic convection developed by Nernst and Planck. Although the expression for σ is a simple deduction from this theory, the calculation has not yet been explicitly made, so far as I am aware; and it may therefore be of some interest to examine the matter somewhat more closely. For simplicity's sake we shall consider a binary electrolyte, the valency of each ion being ω.

Let e = potential of the free electricity in the system,
t = temperature (absolute),
p = osmotic pressure due to positive plus negative ions,
x = distance measured in direction of maximum temperature-gradient,
c = concentration expressed in gram-molecules of salt per litre,
α = degree of electrolytic dissociation.

The flow of heat is supposed to be rectilinear, and e, x, and p are measured as increasing with t, i. e. from *cold* to *hot*. We have $p=2\alpha c\mathrm{R}t$, and the number of gram-ions in volume element dv equal to $2\alpha c dv$ [†]. The osmotic force on the ionized matter in volume element in the positive direction of x is $-\frac{dp}{dx}dv$. Thus the osmotic force per gram-ion in the element dv is $-\frac{1}{2\alpha c}\cdot\frac{dp}{dx}$. The electrical force in the same direction per positive gram-ion is $-\omega\epsilon\frac{de}{dx}$, where ϵ is the quantity of electricity associated with a monovalent gram-ion. Hence we obtain:—

$$\text{Total force per gram-kation} = -\frac{1}{2\alpha c}\frac{dp}{dx} - \omega\epsilon\frac{de}{dx}.$$

$$\text{Total force per gram-anion} = -\frac{1}{2\alpha c}\frac{dp}{dx} + \omega\epsilon\frac{de}{dx}.$$

If u and v denote the velocities acquired by the kation and anion respectively under unit force, then the number of gram-

---

[*] Communicated by the Author.

[†] Concentration-changes due to expansion of the solution by heat are neglected.

kations which traverse a section ds at which the temperature is t in the time $d\theta$ in positive direction of x is

$$-u\left(\frac{1}{2}\frac{dp}{dx}+\omega\epsilon\alpha c\frac{de}{dx}\right)ds\,d\theta,$$

where α is the dissociation corresponding to t.

For the corresponding number of negative gram-ions in the *opposite* direction we have

$$v\left(\frac{1}{2}\frac{dp}{dx}-\omega\epsilon\alpha c\frac{de}{dx}\right)ds\,d\theta;$$

and hence for the current-density i in the positive direction of x

$$i=-\omega\epsilon\left[\frac{u-v}{2}\frac{dp}{dx}+\omega\epsilon\alpha c(u+v)\frac{de}{dx}\right].$$

Putting $i=0$, we obtain the known equation

$$\frac{de}{dx}=\frac{1}{2\omega\epsilon\alpha c}\frac{v-u}{v+u}\cdot\frac{dp}{dx}, \quad \ldots\ldots \quad \text{(i.)}$$

which holds accordingly in this case. But $\frac{de}{dt}=\frac{de}{dx}\frac{dx}{dt}$, and $\frac{dp}{dt}=\frac{dp}{dx}\frac{dx}{dt}$; so that (i.) becomes

$$\frac{de}{dt}=\frac{1}{2\omega\epsilon\alpha c}\frac{v-u}{v+u}\frac{dp}{dt}. \quad \ldots\ldots \quad \text{(ii.)}$$

Since $p=2\alpha c\mathrm{R}t$,

$$\frac{dp}{dt}=2\alpha c\mathrm{R}+2c\mathrm{R}t\frac{d\alpha}{dt};$$

and therefore

$$\frac{1}{2\alpha c}\frac{dp}{dt}=\mathrm{R}+\frac{\mathrm{R}t}{\alpha}\frac{d\alpha}{dt}.$$

If n denote the migration-constant of the kation, we have furthermore

$$1-2n=\frac{v-u}{v+u}.$$

Hence (ii.) becomes

$$\frac{de}{dt}=\frac{\mathrm{R}}{\omega\epsilon}(1-2n)\left(1+\frac{t}{\alpha}\frac{d\alpha}{dt}\right); \quad \ldots \quad \text{(iii.)}$$

and therefore

$$\sigma=\frac{\mathrm{R}}{\omega\epsilon}(1-2n)\left(1+\frac{t}{\alpha}\frac{d\alpha}{dt}\right). \quad \ldots \quad \text{(iv.)}$$

If $\frac{d\alpha}{dt}$ be small, or if the salt be completely dissociated, equation (iv) reduces to the simple form

$$\sigma = \frac{\mathrm{R}}{\omega\epsilon}(1-2n). \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (\text{v.})$$

From (v.) we see that the electrolyte will have a positive or negative " specific heat of electricity " according as v is $>$ or $< u$, i. e. as $n <$ or $>\frac{1}{2}$.

It is also evident from (v.) that in a completely dissociated binary electrolyte the Thomson effect will be independent of the concentration, as Nernst has already pointed out. As a matter of fact, it is only possible to speak of an initial Thomson effect; and in this sense the foregoing expressions are to be taken. For diffusion sets in at once in the unequally heated solution, and the conductor thus ceases to be homogeneous. The result will be that the P.D. between the ends of the unequally heated conductor will gradually decrease, becoming zero when diffusion-equilibrium is attained if the electrolyte be completely dissociated.

For a completely dissociated electrolyte $p=2c\mathrm{R}t$, and therefore

$$\frac{1}{2\alpha c}\,dp = \mathrm{R}dt + \mathrm{R}t\frac{dc}{c}.$$

Accordingly, integrating from t_1 to t_2, we obtain

$$e_\theta = \frac{\mathrm{R}}{\omega\epsilon}\int_{t_1}^{t_2}\frac{v-u}{v+u}\,dt + \frac{\mathrm{R}}{\omega\epsilon}\int_{t_1}^{t_2}\frac{v-u}{v+u}\,\frac{t}{c}\,dc,$$

as the expression for the total P.D. at any time θ, and for its rate of change

$$\left(\frac{de}{d\theta}\right)_\theta = \frac{\mathrm{R}}{\omega\epsilon}\int_{t_1}^{t_2}\frac{v-u}{v+u}\,\frac{\partial}{\partial\theta}\left(\frac{t\,\frac{\partial\psi}{\partial t}}{\psi}\right)dt,$$

where $c=\psi(t,\theta)$, with the conditions that $\psi=\text{const.}=c_0$ for $\theta=0$, and $\psi=\frac{\text{const.}}{t}$ for $\theta=\infty$.

In order to calculate the initial Thomson effect,

$$e = \frac{\mathrm{R}}{\omega\epsilon}\int_{t_1}^{t_2}(1-2n)dt,$$

it would be necessary to know what function $(1-2n)$ is of the temperature. It is known, however, that n approaches the value ·5 as the temperature increases, so that the numerical value of $1-2n$ decreases. Hence an upper limit may be obtained by putting $1-2n$ equal to its value at the lower temperature limit, and we may write

$$e=\frac{R}{\omega\epsilon}(1-2n)_{t_1}(t_2-t_1).$$

To take a very favourable case, we may suppose the electrolyte to be LiOH. Here we have for 18° C. $1-2n=\cdot 7$, $\omega=1$, and $\frac{R}{\epsilon}=\cdot 0000867$ when e is expressed in volts. Taking $t_2-t_1=100$ as a fairly extreme temperature-interval, we get $e=\cdot 006$ volt.

This rough calculation shows that in the case of salt-solutions the maximum initial potential-difference arising from the Thomson effect would only amount, under ordinary conditions, to a few thousandths of a volt at the most. This result justifies the neglecting of the Thomson P.D. in the measurement of thermo-electric cells, except when the experimental error is less than one-thousandth of a volt.

Holywood, Co. Down, April 1898.

LVII. *Evidence that Röntgen Rays are Ordinary Light.*

By G. JOHNSTONE STONEY, *M.A., D.Sc., F.R.S.*[*]

IN 'Nature' of the 28th of last April, p. 607, Lord Rayleigh calls attention to the fact that under Sir George Stokes's theory of Röntgen rays—that which is known as the hedge-firing theory—we are justified in expecting a large proportion of ordinary light of short wave-length in the radiation from the shield which receives and arrests the "kathode rays." This he infers generally from the properties of Fourier's theorem, without entering into the details which seem necessary; and I understand him accordingly to put forward the conclusion as an opinion we are justified in strongly holding, though not actually proved. It may therefore not be useless to publish a proof which the present writer had worked out for his own satisfaction. An explicit proof seems the more desirable inasmuch as Fourier's theorem, except where it can be applied *throughout* to the actual conditions which exist in nature, furnishes only kinematical resolutions which may differ materially from the actual physical resolutions effected by nature[†].

---

[*] Communicated by the Author.

[†] For example, all the resolutions effected by Fourier's theorem in the paper beginning on p. 281 of the Phil. Mag. for April 1897, are of this illusory kind. The component motions furnished by that investigation do not fulfil the necessary condition that each separately can be propagated by the medinm.

In analysing the motion of a vibrating string, the association of Fourier's theorem with the dynamical conditions of the problem is easy; but it is not so simple a matter when we have to deal with events propagated through a medium in three dimensions. When, however, those events take the comparatively simple form of waves transmitted through a uniform medium, whether double refracting or isotropic, the analysis of the motion in the medium can be made to furnish components that consist *throughout* of physical events, each of which could occur separately under the operation of the forces which exist in the medium.

This essential condition can be secured by dividing the analysis into two parts, in the first of which an advancing spherical wave (such as is propagated from each centre of disturbance in a uniform isotropic medium) is resolved by the Principle of Reversal as used by MacCullagh, into the coexistence of plane wavelets propagated in all the directions towards which the spherical wave travels. In this first part of the inquiry, we are dealing exclusively with events which would spontaneously develop themselves in the medium under the conditions which are assumed in the course of the proof, and each of which could exist by itself in the medium. This preliminary analysis presents the motion of the medium in a form to which a further analysis by Fourier's theorem may be applied without travelling outside actual physical events. We thus render the whole analysis physically legitimate—*i. e.* such that *it represents what really takes place in nature.* By a wavelet is to be understood not a wave which is short, but one of infinitesimal intensity.

The steps of the proof are as follows:—

Sir George Stokes's explanation of Röngten rays amounts to this—That kathode rays consist of negatively charged missiles shot in showers, like hedge-firing, from the negative electrode against the shield which receives and suddenly arrests them; and that the Röngten rays are due to the independent impulses propagated through the æther when the advances of the negative charges are thus abruptly stopped or altered.

Each impulse is a spherical impulsive wave of some complex form propagated through the æther from one of the points of impact. It started on its journey at the instant when the advance of one of the negatively charged missiles was arrested.

Each such spherical wave can, by MacCullagh's method of using the Principle of Reversal, be resolved into the coexistence of innumerable components consisting of plane impulsive wavelets. These wavelets are of unlimited extent laterally, each is uniform throughout, and they propagate themselves forward through the medium without undergoing change. One of them advances perpendicularly to itself in each of the directions towards which the spherical wave advances (see Phil. Mag. for April 1897, p. 273).

Use polar coordinates and consider some one direction $\theta\phi$. One of the plane impulsive wavelets is propagated in that direction from each of the impacts of the hedge-firing on the shield; and those which happen to travel from all the impacts in the direction $\theta\phi$, form an undulation of some kind travelling in that direction. It obviously consists of an irregular succession of uniform plane impulsive wavelets.

The whole of this irregular undulation travelling in any one direction may be represented by the equations

$$\left.\begin{aligned}\tau_1 &= \mathrm{F}_1(r-vt)\\ \tau_2 &= \mathrm{F}_2(r-vt)\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where τ_1 and τ_2 are resolved parts of the transversal in two planes parallel to the radius-vector and at right angles to one another; where v is the velocity of light; and where the forms of the functions F_1 and F_2 vary with the vector $\theta\phi$.

Next let all the events of the Röngten experiment be regarded as repeated at definite equal intervals of time, say at intervals of a day—*i. e.* of 86,400 seconds.

The functions F_1 and F_2 are thereby rendered periodic functions; the period being

$$\mathrm{T} = 86{,}400 \text{ seconds.}$$

These functions then become resolvable by Fourier's theorem into pendulous terms, of which the periods are T and integer submultiples of T.

Equations (1) when thus expanded by Fourier's theorem represent the plane wavelet component in the direction $\theta\phi$ of the whole motion in the æther due to the radiation from the target, along with the same component of previous and subsequent repetitions of this motion in the æther at intervals of a day.

Each *term* of the expansions represents physically a complete undulation consisting of an unlimited train of exactly similar plane pendulous wavelets of some one wave-length, filling the whole of space*, and advancing with the velocity v in the direction $\theta\phi$.

---

* If it fill the whole of space, what is represented by it includes, in addition to the actual radiation, that preceding condition of the æther which would have produced the radiation without external aid.

Accordingly the radiation from the target against which the kathode rays impinge, along with repetitions of the same at intervals of a day, may be resolved into the coexistence of component undulations, each filling the whole of space, and each consisting of an unlimited train of uniform plane pendulous wavelets. The periodic times of these will be T and its integer submultiples.

Next form the series of ascending prime numbers, viz :—

2, 3, 5, 7, 11, 13, 17, 19, &c.,

and call the continued product of the first n of these

$$M_n.$$

Then instead of repeating the Röngten events at intervals of a day, let them be repeated at intervals of M_n days. Thereby the period of the Fourier's series becomes M_nT; and the radiation of the Röngten experiment, repeated at these longer intervals, is represented by the coexistence of series of pendulous terms of which the periods are

M_nT, and its integer submultiples.

If n be changed into $n+1$, these series will include new terms.

The limit of this process, when n is increased without limit, is that the series can contain terms with periodic times of *any* period, whether commensurable *or incommensurable* with T.

And that it then represents the Röngten event *isolated*—*i. e.* without any repetition. It is obvious that one condition which must be fulfilled by a series of the nature of Fourier's series in order that it may be competent to represent an event of limited duration without repetitions of the same, is that it shall contain terms the periodic times of which are incommensurable with one another.

Hence, finally, the impulses which are propagated through the æther, as the consequence of the kathode hedge-firing however irregular, may be resolved into the coexistence of component undulations travelling in the various directions, each of which is a train of perfectly similar plane wavelets, and of which there need be only one of each wave-length in each direction.

It follows from the known properties of Fourier's expansions that the more abrupt and irregular the hedge-firing is, the more prominent a place will terms furnishing undulations of very short wave-length have in the final expansions.

If we choose we can compound these undulations of short wavelets, by working in the inverse direction through the MacCullagh analysis; and thus we learn that the more abrupt and irregular the hedge-firing, the more abundant will be the proportion of rays of ordinary light consisting of spherical waves of short wave-length in the radiation from the target.

If, as appears almost certain, these are what we know under the name of Röngten rays, it follows that anything which increases the intensity, the abruptness, and the irregularity of the hedge-firing will increase the abundance of the Röngten emanations.

Moreover, inasmuch as a certain amount of abruptness and irregularity is present in the emission of light from every visible object or luminous source, it follows that Röngten rays—*i. e.* rays of very short wave-length—are present in all light throughout nature; only the quantity present is usually inconspicuous.

LVIII. *Notices respecting New Books.*

Lehrbuch der Algebra. *Von* HEINRICH WEBER. Zweite Auflage, Erster Band: pp. xvi + 704. (Braunschweig, Vieweg & Sohn, 1898.)

THIS is no hastily composed treatise. The Author tells us in his Preface (p. v) that he had cherished for some years the idea of writing such a work, and the result is the comprehensive one before us. He has traversed the field described several times in his university lectures. That he has not failed in his attempt may be inferred from the fact that the first edition was published so recently as 1895, and then was limited to 654 pages. The book is clearly conceived in plan, and is very thorough in its execution. This first volume contains three books, which are prefaced by an excellent arithmetical introduction which discusses the theory of multiplicities, the theory of rational and irrational numbers, and further gives a proof of the continuity of real numerical magnitude. Here the author refers mainly to Dedekind (‘Stetigkeit und irrationale Zahlen,’ and ‘Was sind und was sollen die Zahlen’) and to G. Cantor.

The first book, which is headed *the Foundations*, in six chapters, treats of Rational functions of one or more Variables, Determinants, the roots of Algebraical Equations, Symmetric functions, Invariants and Covariants, and the Tschirnhausen transformation (the application of which to cubic, biquadratic, and quintic equations is shown in chapter iv., and in chapter vi. Hermite's modification is introduced). The second book, which is devoted to *the Roots*, is also broken up into six chapters, the first four of which deal with the reality of the roots of real equations, with their number in a given interval, with their superior and inferior limits, and with the numerical approximation method. The eighth chapter, on Sturm's theorem, is specially interesting. Continued fractions and their applications to quadratic irrational numbers and to the Pellian equation are discussed in chapter eleven (with some departures, in notation, from Gauss and Dirichlet). In the last chapter the elementary parts of the theory of the roots of unity, and also the subject of quadratic residues and the law of quadratic reciprocity are expounded.

Algebraical quantities, the subject of Book iii., are also handled in six chapters. These last 200 pages constitute the prime portion of the banquet provided by our Author. The *Galois' Theory* is discussed in chapters thirteen and fourteen, as to its fundamental notions, a more advanced treatment being presented in vol. ii. (of the first edition). The account is thoroughly interesting, and, we believe, original. Chapter fifteen is on *Cyclical Equations*, chapter sixteen on *Cyclotomy*, and chapter seventeen contains several propositions connected with the Solution of Equations, carrying forward the Galois' theory, and the last chapter treats of the roots of metacyclic equations of prime degrees. We have given but a bald account of what, possibly, is an epoch-making book. It only remains to say that the printers have done their work well, for the typography is all that one could wish.

Die Mechanik des Weltalls. By Dr. L. ZEHNDER, *Extraordinary Professor of Physics in the University of Freiburg i. B.* (Freiburg i. B., J. C. B. Mohr, 1897.)

IN this treatise Professor Zehnder contributes another to the already numerous theories of æther and matter. He assumes that space is infinite and three-dimensional, and that it is filled with extremely small material particles which exert a gravitational attraction on each other, but move with great velocities; these particles constitute the æther, which therefore behaves as a gas and transmits vibrations exactly as, according to the kinetic theory, ordinary gases do. The density of this æther and consequently the mass of each particle are assumed to be exceedingly small. Electricity consists of a vibrational motion of the æther particles, positive charges corresponding to translational velocities greater than the mean velocity and negative charges to a defect of translational motion. Matter is composed of particles much larger than the æther atoms but of the same material; these, by reason of their greater mass, attract round themselves a shell of condensed æther atoms which serves to communicate the motion arising from the impact of the particles to the surrounding free æther.

The author applies his theory to various physical phenomena in a general non-mathematical fashion. It is obvious, however, that no ultimate theory of æther and matter can lay claim to serious consideration which postulates an attractive force without a medium for its action, and which assumes the existence of elastic spherical atoms without explaining the nature of the elastic forces called into play during their impact. J. L. H.

Text-Book of Physical Chemistry. By CLARENCE L. SPEYERS, *Associate Professor of Chemistry, Rutgers College.* (New York, D. Van Nostrand Company; London, Spon, 1898.)

PHYSICAL chemistry is advancing so rapidly, and has become a subject of such great importance, as to render an English text-book of its more elementary and fundamental principles very desirable. It is fitting that the work should have been undertaken in America, since many researches on the subject, both theoretical and experimental, are due to American chemists. The author's task has, however, been by no means an easy one, because, firstly, it is difficult to decide what portions of the subject are sufficiently permanent to justify their inclusion in a text-book, and, secondly, opinions may differ as to the amount of space which should be devoted to the exposition of physical principles, such as the laws of thermodynamics or the elements of current electricity. Prof. Speyers briefly sketches the subject of thermodynamics, apparently assuming that the student is already familiar with the physical treatment of it, and he recapitulates the definitions of electric units. The chapter on physical changes includes the discussion of fusion, vaporization, solution, and osmotic pressure, and is followed by a chapter on the conditions of chemical equilibrium in the various systems, solid, liquid, gas, or mixtures of these. Chemical kinetics is represented by a brief sketch of the method of determining the rate of decomposition of the substances concerned in chemical reactions; then come chapters on phases and the effects of variable temperature. The chemistry of ionized solutions necessitates a section devoted to electrolysis and properties of the ions, including a discussion of the energy derivable from the chemical action in any given voltaic cell.

The book serves a useful purpose by presenting the facts of the subject in a collected and logical form; it requires some revision, however, especially in the electrical portion. We find, for example, on p. 155, and again on the next page, the statement that a volt is 10^8 *dynes*, which might lead a student to suppose that electromotive force is a mechanical force. By an obvious slip on p. 159, the signs plus and minus are interchanged, making the cathode appear to be the positive electrode, and *vice versâ*. The proposal of the author to use the term absorption of electricity for conductivity is open to the objection that the former term is already used to indicate a well-defined and quite different electrical phenomenon. J. L. H.

LIX. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 468.]

January 5th (*con.*).—Dr. Henry Hicks, F.R.S., President, in the Chair.

2. 'Sections along the Lancashire, Derbyshire, and East Coast Railway between Lincoln and Chesterfield.' By C. Fox-Strangways, Esq., F.G.S.

The portion of the line considered in this paper occupies a distance of about 40 miles, and runs nearly at right angles to the strike of all the beds from the Lias to the Coal Measures.

The lower part of the Lias and the Rhætic beds are entirely concealed; but grey marls overlying red marls occur about a mile east of Clifton Station, and at the station the Red Marl of the Keuper comes on in force. The alluvial deposits of the Trent, pierced to a depth of from 25 to 30 feet, consist principally of loam overlying varying thicknesses of sand and gravel. Horns of red deer were found at a depth of 25 feet. At Dukeries Station white flaggy Keuper sandstones appear from beneath the Red Marl, and probably represent the eastward extension of the Tuxford Stone. A deep well here has been bored to a depth of 644 feet from the present surface, and details of the section are given in the paper. South of Kirton there is a deep cutting in the Waterstones, and after leaving the escarpment the line enters on the great dip-slope of the Bunter Pebble Beds, which are shown at Ollerton and at intervals for 4 miles beyond this. There are no sections in the Lower Red and Mottled Sandstones; and west of Warsop the line crosses the dip-slope of the Magnesian Limestone. Details of the sections in this rock are given.

Between Scarcliff and Bolsover the line crosses the Permian escarpment in a tunnel, the whole of which is in the Coal Measures; these are high up in the series, and contain no coal-seams of value. They are not stained red.

West of Arkwright's Town Station is a very complete section of beds representing the Middle Coal and Ironstone series (the most valuable part of the Derbyshire Coalfield), of which full details are given, most of the important coal-seams being readily recognized; and the author describes some remarkable features in the relationships of some of the sandstones to the other deposits.

The absence of Glacial beds is of much interest; not a trace of genuine Boulder Clay has been seen along the whole line.

January 19th.—Dr. Henry Hicks, F.R.S., President, in the Chair.

The following communications were read :—

1. 'On some Gravels of the Bagshot District.' By Horace W. Monckton, Esq., F.L.S., F.G.S.

The author refers to his papers on Gravels South of the Thames published in the Quart. Journ. Geol. Soc. for 1892 (p. 29) and 1893 (p. 308), and gives some additional details.

He suggests that the occurrence of stones which have been very little rolled or waterworn in gravels at certain localities affords evidence of the presence of ice in the water by which those gravels were deposited, and that the position of some sarsens which he describes is due to the same agency.

He gives details and exhibits photographs of a number of sarsens which he has seen *in situ.*

2. 'On the Occurrence of Chloritoid in Kincardineshire.' By George Barrow, Esq., F.G.S.

The rock containing the chloritoid was first found *in situ* at the entrance to the little gully at the head of Friar Glen Burn, near Drumtochty Castle. It has since been observed at many places along a belt of country extending from the coast north of Stonehaven nearly as far as the North Esk.

The rock is easily recognized by the presence of numerous white spots, which are always present and are larger than the chloritoid. The chloritoid and the spots vary in size, being largest when the rock is most crystalline (a schist), and smallest when it is least crystalline (a slate). The mineral appears as minute glistening scales in the schist, but in the slate it can be recognized only with the aid of the microscope.

The optical characters are described, and shown to be identical with those of the mineral from the Ile de Groix, and with those of the ottrelite from Ottré and Serpont.

An account of the methods adopted to obtain a pure sample is given. Several analyses were made, and it was proved that as the purification increased the analyses approximated more and more closely to the analysis of the mineral from the Ile de Groix.

The final result was as follows :—

SiO_2	26·00
Al_2O_3	40·05
FeO	19·50
Fe_2O_3	5·05
MgO	2·88
Loss on ignition	6·00
Total	99·48

The author discusses some of the published analyses, and suggests that many of the discrepancies may be due to impurities in the material analysed.

February 2nd.—Dr. Henry Hicks, F.R.S., President, in the Chair.

The following communications were read:—

1. 'Contributions to the Glacial Geology of Spitsbergen.' By E. J. Garwood, Esq., M.A., F.G.S., and Dr. J. W. Gregory, F.G.S.

The extent of glaciation of Spitsbergen has been exaggerated, for there is no immense ice-plateau, but normal glaciers with some inland sheets and Piedmont glaciers. These differ from Alpine glaciers, as they are not always formed from snow-fields at the head, and though some of the glaciers (as the Baldhead Glacier) have tapering snouts in front, most have vertical cliffs. Chamberlin's explanation that the latter are due to the low angle of the sun is insufficient, and they seem to be caused by the advance of the ice by a rapid forward movement of its upper layers. The ice of these upper layers falls off and forms talus in front, over which the glacier advances, carrying detritus uphill with it, and producing a series of thrusts. The Booming Glacier illustrates cases of erratics carried in different directions by the same mass of ice.

The deposits of the Spitsbergen glaciers are of four types:—(1) moraines of Swiss type; (2) those formed mainly of intraglacial material; (3) those formed of redeposited beach-material; (4) deposits of glacial rivers, and re-assorted drifts. The materials of the second are subangular and rounded; scratched and polished pebbles and boulders are abundant, and the fine-grained matrix, which is frequently argillaceous, is often well-laminated and false-bedded. Some of these drifts are stratified, others unstratified, and contorted drifts occur. This type of moraine is remarkably like some British Boulder Clay. The third class is sometimes formed by land-ice, at other times beneath the sea; the latter shows stratification. The superglacial and intraglacial streams, so far as seen, were usually clear of drift. Under the fourth head an esker in a tributary of the Sassendal is described.

The direct geological action of the marine ice is of four kinds:—transport of material, contortion of shore-deposits, formation of small ridges of boulder-terraces above sea-level, and striation, rounding, and furrowing of rocks along the sea-shore.

Traces of former glaciation are described in the case of the Hecla Hook beds, and of certain beds of late Mesozoic or early Cainozoic age in Bunting Bluff.

Under the head of general conclusions the authors state that they have discovered no certain test to distinguish between the action of land-ice and marine ice; that there is no evidence to prove that land-ice can advance far across the sea; and that there is evidence, which they regard as conclusive, of the uplift of materials by land-ice. They note that the mechanical processes connected with the advance of the glaciers are of three kinds. All the material seen transported by the glaciers was superglacial or intraglacial, and not subglacial. Some striation of intraglacial material is caused by differential movement of different layers of ice.

The advance and retreat of the Spitsbergen glaciers is very irregular, and apparently due to local changes. The observations of the authors support the views of those who ascribe a limited erosive power to glaciers. Lastly, the theory that glacial periods occurred as a consequence of epeirogenic uplifts receives no support from Spitsbergen.

2. 'On a Quartz-rock in the Carboniferous Limestone of Derbyshire.' By H. H. Arnold-Bemrose, Esq., M.A., F.G.S.

The paper describes the occurrence in the field and the microscopic structure of a rock consisting essentially of quartz, which is found in the Mountain Limestone in several localities. It occurs in irregularly-shaped bosses and veins, and shows no signs of stratification.

Its close association with a quartzose limestone, which in turn passes into an ordinary limestone with few if any quartz-crystals, leads to the inference that it is a silicified limestone.

The microscopical structure of a number of thin slices of these rocks is described. The quartz-rock is seen to be made up of quartz-grains which generally interlock closely, but sometimes possess a crystalline outline and contain zones of calcite. Fluor is occasionally present.

The quartzose limestone is usually a foraminiferal limestone containing a large percentage of quartz, which occurs as separate crystals and as aggregates of crystals. The latter and the small quartz-veins have a structure similar to that of the quartz-rock. The former often contain zones of calcite and penetrate organisms. The residue consists of quartz-crystals.

The author considers that the quartz-rock is not a gritty limestone, altered by the growth of crystalline quartz around the detrital grains, but that it is a limestone replaced by quartz. The gradual passage from the quartz-rock through a quartzose limestone to an ordinary limestone, the presence of chert, of part of a foraminifer, and of pieces of quartzose limestone in it, support the opinion that it is an altered limestone.

February 23rd.—W. Whitaker, B.A., F.R.S., President, in the Chair.

The following communications were read:—

1. 'On some Submerged Rock-Valleys in South Wales, Devon, and Cornwall.' By T. Codrington, Esq., M.Inst.C.E., F.G.S.

The author describes various valleys in which the solid rock is reached at a considerable depth below sea-level, on the sides of Milford Haven and in the Haven itself; beneath the Tivy, Tawe, and Neath, the Wye, the Severn, the Bristol Avon, the Dart, the Laira, the Tavy, the Tamar, and other rivers. In the case of the Dart the rock-bottom has been found at one place at a depth of 110 feet below low-water level, and in the case of other rivers at varying depths less than this. The deposits show that some of the infilling took place after the period of submerged forests, and much before this, for frequent cases of glacial deposits filling the bottoms of these submerged valleys are recorded.

The fact that in the Solent and Thames the Glacial deposits border the sides of the valleys, and do not occur at the bottom as in the case of the valleys described in the paper, indicates that the latter are older than the former, though they present features similar to those of some of the valleys of the North-east and North-west of England.

2. 'Some New Carboniferous Plants, and how they contributed to the Formation of Coal-Seams.' By W. S. Gresley, Esq., F.G.S.

The author, in a paper published in abstract in the Society's Quarterly Journal for May 1897 (vol. liii. p. 245), argued that certain brilliant black laminæ in coal, and similar materials found among some mechanical sediments of the Coal Measures, pointed to the former existence of an aquatic plant. In the present communication he describes structures in the pitch-coal laminæ of bituminous coal and in the glossy black layers of anthracite which he believes to be indications of two other kinds of plants, and states that he has examined structures which may be due to some other kinds of vegetation.

LX. *Intelligence and Miscellaneous Articles.*

LIQUID HYDROGEN.

AT the Meeting of the Royal Society yesterday afternoon Professor Dewar contributed a preliminary note on the liquefaction of hydrogen and helium.

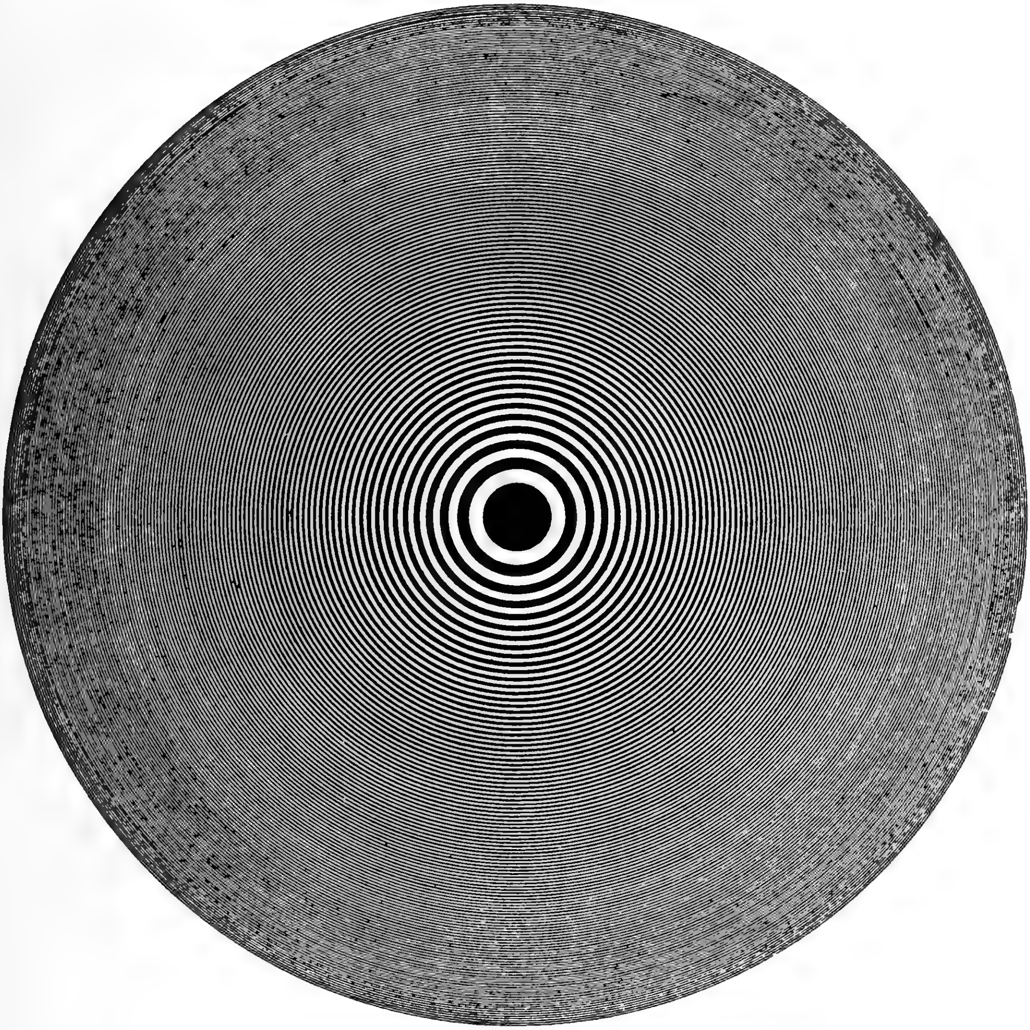
Professor Dewar said that in 1895 he described apparatus for the production of a jet of hydrogen containing liquid, and showed how such a jet could be used to cool bodies below the temperature that could be reached with liquid air, though all attempts to collect the liquid hydrogen failed. So far no investigator had improved on the results described in 1895; and as the type of apparatus employed in those experiments worked well, it was resolved to construct a much larger liquid-air plant, and to combine with it circuits and arrangements for the liquefaction of hydrogen. The apparatus took a year to build up, and many months were occupied in testing and in making preliminary trials. The many failures and defeats need not be detailed. On May 10 an experiment was started with hydrogen cooled to −205° C., and escaping continuously under a pressure of 180 atmospheres from the nozzle of a coil of pipe at the rate of 10 to 15 cubic feet a minute, in a vacuum vessel doubly silvered, and of special construction, surrounded with a space kept below −200° C. With these arrangements liquid hydrogen began to drop from this vacuum vessel into another, doubly isolated by being enclosed within a third, and in five minutes 20 cub. centim. of liquid were collected. The hydrogen jet then froze up from the solidification of air in the pipes of the apparatus. The yield of liquid was about 1 per cent. of the gas. In the liquid condition the hydrogen was clear and colourless, showing no absorption-spectrum, and the meniscus was as well defined as in the case of liquid air. The liquid must have a relatively high refractive index and dispersion, and the density must be in excess of the theoretical values, viz. 0·18 to 0·12, deduced respectively from the atomic volume of organic compounds and the limiting density found by Amagat for hydrogen gas under infinite compression. Professor Dewar's old experiments on the density of hydrogen in palladium gave a value for the combined body of 0·62; and it would be interesting to find the real density of the liquid substance at its boiling-point. No arrangements being at hand to determine the boiling-point, two experiments were made to prove the excessively low temperature of the boiling fluid. In the first place, when a long piece of glass tubing, sealed at one end and open to the air at the other, was cooled by immersing the closed end in the liquid hydrogen, the tube immediately filled, where it was cooled, with solid air. The second experiment was with a tube containing helium. The Cracow Academy 'Bulletin' for 1896 contained a paper by Professor Olzewski entitled "Research on the Liquefaction of Helium," in which he stated that, as far as his experiments went, helium remained a permanent gas, and apparently was much more difficult to liquefy than hydrogen. Professor Dewar, however, suggested that hydrogen and helium would probably be found to have about the same volatility, as was the case with oxygen and fluorine. Having a specimen of purified helium, extracted from Bath gas, sealed up in a bulb with a narrow tube attached, he placed the latter in the liquid hydrogen, whereupon a distinct liquid was seen to condense. From this result it would appear that there could not be any great difference in the boiling-points of hydrogen and helium. In conclusion, Professor Dewar pointed out that all known gases had now been condensed into liquids which could be manipulated at their boiling-points under atmospheric pressure in suitably arranged vacuum vessels, though even so great a man as Clerk-Maxwell had doubts as to the possibility of ever liquefying hydrogen. With liquid hydrogen as the cooling agent, a temperature could be reached within 20° or 30° of the zero of absolute temperature; and its use would open up an entirely new field of scientific inquiry. No one could predict the properties of matter near that zero. Faraday liquefied chlorine in the year 1823. Sixty years afterwards Wroblewski and Olzewski produced liquid air, and now, after an interval of fifteen years, the remaining gases—hydrogen and helium—were obtained as static liquids. Considering the step from the liquefaction of air to that of hydrogen was relatively as great in a thermodynamic sense as that from liquid chlorine to liquid air, the fact that the former had been achieved in one-fourth the time needed to accomplish the latter proved the greatly accelerated rate of scientific progress in the present age. The paper ended with an acknowledgment of the aid rendered by Mr. Robert Lennox, without whose engineering skill, manipulative ability, and loyal perseverance the present successful issue might have been indefinitely delayed.—*The Times*, May 13th, 1898.

INDEX TO VOL. XLV.

END OF THE FORTY-FIFTH VOLUME.

Printed by Taylor and Francis, Red Lion Court, Fleet Street.

See page 511



Mintern Bros. imp.