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

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

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

J. B. Pinelli ad Mazonium.

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PLATES.

ERRATA.

Page 47, lines 6 and 5 from bottom, *for* The roots of (40) are real; and therefore aplanaticism is possible if *read* The roots of (40) are real, and therefore aplanaticism is possible, if
— 193, art. 5, line 12, *after* frame *insert* from
— 196, art 11, line 3, *for* 319 *read* 313
— ,, ,, line 9, *move all the figures one place to the left.*
— 197, line 2, *after* that *insert* after the first stage, *and for* collecting *read* unaccented
— 271, line 37 *for* $U^{kn}N$ *read* $U_{kn}N$
— 271, — 38, *for* U_{kn} is not *read* $U_{kn}N$ is not
— 271, — 41, *for* $U_{kn}=45$ *read* $U_{kn}N=45$
— 272, — 7, *for* observations *read* vibrations
— 282, — 31, *for* curve *read* error.
— 349, penultimate line, *for* Æ *read* Æ^2.
— 351, art. 37, column 5, *for* 2 *read* 2^2.
,, ,, columns 4, 5, and 6, *for* 1 *read* 2.

THE

LONDON, EDINBURGH, AND DUBLIN

PHILOSOPHICAL MAGAZINE

AND

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[FIFTH SERIES.]

JANUARY 1880.

I. *On Torsion.* *By* GUSTAV WIEDEMANN*.

[Plate I.]

1. SINCE M. Weber, in 1835, studied the laws of the temporal course of the permanent elongation remaining after the cessation of a temporary stretching of a thread (the elastic effect)†, and in 1841 advanced as the cause of the elongation "the difference of the position of the axis of elasticity of the smallest particles at any instant from that which corresponds to, and is determined for each tension at, perfect equilibrium," an opportune observation by R. Kohlrausch (in 1847)‡ has been followed by a more extensive series of investigations upon the relations of imperfect elasticity. These have been carried out essentially in two different directions. In the one set the temporary and the permanent alteration of form are in part observed directly, as well as in their temporal course; in the other the diminution of the length of swing of oscillating bodies.

In the experiments of the latter sort (for example, on torsional oscillations) the molecules are immediately transferred from the positions assigned to them by the forces acting at any instant to other positions, before the elastic afteraction has ended; and consequently the phenomena are highly complicated. It therefore appeared to me advisable, before studying

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* Translated from Wiedemann's *Annalen*, 1879, No. 4, vol. vi. pp. 485–520.

† Pogg. *Ann.* xxxiv. p. 247 (1835), liv. p. 1 (1841).

‡ Pogg. *Ann.* lxxii. p. 393 (1847).

relations of this sort, first to investigate the positions of equilibrium of bodies deformed by forces, or the torsions of their molecules which then takes place.

With this intention I have already, in the years 1858–1862[*], published a series of treatises both on torsion and bending considered apart, and also on the analogies between mechanical and magnetic phenomena, as well as the reciprocal actions between the two.

I had endeavoured *inter alia* to determine in general outlines the ratio between the permanent deformations of a body and the temporary deformations occurring during the action of the deforming force, and had shown that, as Wertheim[†] had already observed, the former occur even after the slightest temporary deformations, so that in a body not previously deformed a limit of elasticity does not properly exist, but bodies which have sufficiently often been deformed within certain limits by determinate forces, on the repeated action of the same or less forces in the same direction as that last applied, constantly again receive the same permanent deformation. With this agrees the result subsequently (1865) communicated by Thalén[‡], that by repeated stretching of rods a raising of the limit of their elasticity, increasing with it, takes place[§]. Moreover, with the first deformation the permanent alterations of form increased much more rapidly than the temporary, which is not the case in an equal degree with repeated deformations. I had in like manner shown that, when left longer in a state of rest after repeated deformations, bodies partially return to their previous condition[||], and behave similarly under deformations which exceed the limits of the first. I have also studied the behaviour of bodies under consecutive oppositely directed deformations[¶], &c.

In addition to these experiments I take leave to communicate some older and newer observations by which their results should be extended.

2. *Apparatus.*—The influence upon a wire of repeated tor-

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[*] Pogg. *Ann.* ciii. p. 563 (1858), cvi. p. 161; Basle *Verhandlungen*, ii. Heft 2, p. 169; Pogg. *Ann.* cvii. p. 139 (1859).

[†] *Ann. de Chim. et Phys.* xii. p. 385 (1842); Pogg. *Ann.* Ergbd. ii. p. 70 (1848).

[‡] Pogg. *Ann.* cxxiv. p. 600 (1865).

[§] Similar results also by Tresca, *Compt. Rend.* lxxiii. p. 1153 (1874); Uchatius, *Beibl.* i. p. 273; Bauschinger, *ibid.* p. 380 (1877); and others.

[||] Similarly Bauschinger, *l. c.* (1877).

[¶] A series of the results of torsion and bending obtained by me have, certainly without mention of them, been confirmed by W. Voigt (*Dissertation d. Univ. Königsberg*, 1874?) in the bending of sticks of rock-salt.

sions in the same direction was first more fully investigated. The apparatus employed was the same that had served for the previous experiments. Pl. I. fig. 1 represents it with some more recent modifications. The point which, in the former experiments, held fast in the axis during the torsion the lower extremity of the wire and the circle, bow, and metal rod suspended from it, instead of working as before in an agate bearing, passes now through a small well-polished steel ring fixed on the axially pierced screw h_1. Further, to the frame with roller *r* and weighing-basin *s* (by the load of which the wire is twisted) first a shorter brass wire *v v* is soldered, on which, by means of two silk threads, a longer, very thin and light, smooth horn rod *x x* is suspended beneath the basin, and, on the thread twisting, strikes against the vertical wires *w w* as soon as, the string A A being let run down, the roller is no longer supported by the latter, but by the thread *o*, wound round the circle on the wire. This arrangement is more suitable than the former one, in which the wire *v v* struck direct against the wires *w w*, since thereby the shakings that might take place during the slow lowering of the roller, in consequence of the elastic afteraction in the temporarily twisted wire, and in the thereby occasioned sliding of the wire *v v* on *w w*, are avoided. Finally, the string A A is fastened at *t*, and carried over a pulley at *u* to the observer, where its end is put round an axle Z of 5 millims. thickness, which can be turned by means of a winch. Thus we can still better, when the scale-pan is loaded with the weights, let down and raise the roller very slowly, and twist and untwist the wire without a shock. The wheel *p* turned so readily on its axis that when a string loaded at each end with 200 grams was suspended on it, an excess of less than 1 gram caused it to rotate.

The mirror-reading was now constantly employed for reading off the temporary and the permanent torsion within wider limits. A strip of tin of 1 decim. height, bent to a semicircle of 1 metre radius, and kept in this position by cross bands, was placed concentrically upon the wire and lined inside with a millimetre-scale, which, as usual, was observed in the mirror *d* attached to the clamp *c*, by means of a telescope. A displacement of the image of the scale 34·9 divisions corresponds to 1° of rotation of the mirror.

To the rod *g* a small bow with its edge horizontal was fastened beneath, into which a broad hook was inserted carrying on an iron rod 1 centim. thick, and 30 centims. long, a horizontal circular iron plate of 20 centims. diameter. Upon the latter were laid, very cautiously, in order not to cause any torsion, flat circular iron weights slit radially on one side, which stretched the wire. Before placing them the wire was annealed under feeble tension.

3. *Repeated Torsion in the same Direction.*—First a brass wire of 480 millims. length and 2 millims. thickness, which was loaded beneath with altogether 10460 grams, was repeatedly twisted by increasing weights G, and the first temporary torsion T which took place immediately after the initial complete action of the weights was, so far as possible, constantly observed (the commencement of which was known by the little rod *x* striking against the wires *w w*), as well as the final temporary torsion T_1, which was definitively produced after a long time. Likewise on lifting the torsion-weights the torsion P which at first set in, and the finally constant permanent torsion P_1, were measured. The values of T and P cannot be exactly determined by any method, since the twistings of the wire that take place on lowering and winding up the weights immediately pass over, at the full action and the complete lifting of the weights, into the deformations produced by the aftereffect.

The results of the observations are contained in the following Table, in which Δ denotes the differences between the consecutive values of $T_1 - P_1$:—

I.

W.	0	30	40	50	60	70	80	90	100	110	120	130	140
T	0	312	416	523	628	733	844	956·5	1070	1191	1338	1510	1725
T_1	0	312	416	523	628	734	845	959	1074	1204	1351	1543	1820
P	0	0·6	1·3	2·3	3·8	5·8	9·8	16	26·3	44	73·3	132·8	253
P_1	0	0·6	1·3	2·3	3·8	5·8	9·8	16	26·3	43·3	71·8	130	245
T_1-P_1		311·4	414·7	520·7	624·2	728·2	835	943	1048·7	1160·7	1279·2	1413	1575
Δ		3.1038	103·3	106	103·5	104	104·8	108	105·7	112	118·5	133·8	162

Thereupon the wire was again twisted sixteen times by the weight 140. Then was

XVII.

T.	T_1.	P.	P_1.	T_1-P_1.
1913	1919	373·8	372	1547

If now the same wire was again twisted by increasing weights, its permanent torsion remaining constantly $P_1 = 372$, then was

XVIII.

W.	0	30	40	50	60	70	80	90	100	110	120	130	140
T	372	682	787	892	999·5	1107	1217	1329·5	1442	1555	1673	1793	1909
T_1	372	682	787	892	1001·5	1109	1219	1332·5	1445	1559	1677	1797	1914
T_1-P_1		310	415	520	629·5	737·5	847	960	1073	1187	1305	1425	1542
Δ		3.103	105	105	109·5	107·5	110	113	113	114	118	120	119

Thus the temporary torsions increase, as was previously found, at the first operation of the increasing weights, at first more slowly, and then always more rapidly, up to a determined value corresponding to the maximum loading. This latter value rises on the repeated employment of the maximum loading, at first more rapidly, then more slowly, up to a certain maximum*.

The permanent torsions exhibit the same behaviour; only they rise, on the operation of increasing weights, as already mentioned, more rapidly than the temporary torsions; they are also more increased, on repeated torsion by the same maximum weight, than the latter.

The result previously observed with alternately directed torsions is here confirmed—that the limits up to which oppositely acting forces temporarily twist wires always become narrower with repeated torsions, and gradually approach a constant minimal distance. In the present case those forces are the temporarily twisting maximal weight and the force *nil*, and the distance between the limits is the corresponding value $T_1 - P_1$.

At the same time, with repeated torsions the differences $T_1 - P_1$ that obtain for the various twisting weights become constantly more proportionate to the twisting weights; they rise a little with increasing weights, as the differences Δ of the consecutive values $T_1 - P_1$ show.

4. *Repeated Torsion in opposite Directions.*—In order to pursue this latter relation within wider limits, especially when the wires are not only unilaterally twisted repeatedly in one determined direction, but their molecules are turned aside by alternately acting forces in opposite directions, a wire of the same dimensions as the former ones was loaded with 8175 grams, repeatedly put into oscillation, and thereupon twisted ten times hither and thither by the weights ±114, when its temporary and permanent torsions no longer perceptibly altered; their amounts, in scale-divisions, were:—

W = +80	T 3753	P 1486·2	T − P = 2267
W = −80	T 1122	P 1387·5	T − P = −1265·5

Upon this the wire was twisted by increasing weights (−)W; and then the numbers were:—

W =	44		64		84		104		124
T	943		740		533		331·6		124·5
P	1387·5		1387·5		1387·5		1387·5		1387·5
T − P	444·5		674·5		854·5		1056		1263
Δ		203		207		201·5		207	

Finally the wire was twisted in the opposite direction by

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* See an analogous observation on stretching: Thalén, *l. c.* p. 682.

increasing weights +W, when the following numbers were obtained:—

W=	44	64	84	104	124
T	1857	2077	2295·8	2524·8	2753
P	1408·5	1425·5	1443	1464	1486·2
T−P	448·5	651·5	852·8	1060·8	1267
Δ		203	201·3	208	206·2

Here, as in the previous experiments, after repeated twistings and untwistings the temporary and the permanent final states of the wire were much more quickly reached than at the first action of the twisting forces.

5. *Torsion of a partly untwisted Wire.*—Another, similar wire was again and again twisted to and fro by the weights ±120, till its torsions were no longer altered on the renewed application of the same forces. Again the limits within which the temporary and the permanent torsions were confined continually approached one another. Thus, at the first and the sixth torsion they amounted to:—

	I.		VI.	
W	−120	+120	−120	+120
T	−1343	+1415	−1328	+1401
T_1	−1366	+1431	−1334	+1406
P	−75	+145	−52	+119
P_1	−72	+143	−50	+118
$T_{+120}-T_{-120}$	2758	2797	2729	2740
$P_{+120}-P_{-120}$	220	215	171·5	168

The wire was next twisted once more by increasing weights, when the permanent torsion remained +118; the temporary torsions amounted to:—

W=	+30	+40	+50	+60	+70	+80	+90	+100	+110	+120
T =	430	535	642	749	857	964	1072	1180	1288	1396
T−P	312	417	524	631	739	846	954	1062	1170	1278
Δ		105	107	107	108	107	108	108	108	108

After this the permanent torsion (118) of the wire was reduced to lower values by the weights −20 to −120, and the wire each time again twisted by increasing weights in the positive direction. The operation was afterwards reversed. The results are contained in the following Table, in which T_0 and P_0 denote the temporary and permanent torsions obtained at the reductions.

The results of the first observations (reduction by negatively acting weights) are more exact, because the finally valid action of the weights was longer waited for. They are on that account delineated in the curves in Plate I. fig. 2, of which the ordinates give the permanent torsions $P-P_0$, and the abscissæ the twisting weights. Moreover it follows immediately from the numbers of both Tables that, reckoning from the torsion P which remained at the reduction, the temporary as well as the permanent torsions are substantially the same in both cases.

Reduced by −40. $T_0=-350$, $P_0=+80{\cdot}5$.

	30	60	90	120
T	398	724	1056	1395
P	84·8	93	105	118
$P-P_0$	3·5	12·5	24·5	37·5
$T-P$	314	631·5	951	1277
Δ	317	320	326	

Reduced by +40. $T_0=-409$, $P_0=-8$.

−30	−60	−90	−120
−729	−657	−984	−1338
−12	−20	−33	−54
−4	−12	−25	−46
−317	−637	−951	−1274
320	314	323	

Reduced by −60. $T_0=-587$, $P_0=+54$.

T	375	705	1043	1394
P	61	74	93	118
$P-P_0$	7	20	39	64
$T-P$	314	631	950	1276
Δ	317	319	326	

Reduced by +60. $T_0=-643$, $P_0=14$.

−308	−642	−975	−1334
7	−5	−24	−55·5
−14	−19	−38	−69·5
−315	−637	−951	−1278·5
322	314	327·5	

Reduced by −80. $T_0=-820$, $P_0=27{\cdot}5$.

T	351	687	1034	1394
P	38	55	82·5	118
$P-P_0$	10·5	27·5	55	90·5
$T-P$	313	632	951·5	1276
Δ	319	319·5	325·5	

Reduced by +80. $T_0=884$, $P_0=41{\cdot}6$.

−285	−623	−963	−1333
−32	−14	−13	−54
−9·6	−27·6	−54·6	−95·6
−317	−637	−950	−1279
320	313	329	

Reduced by −100. $T_0=-1068$, $P=-6{\cdot}5$.

T	326	671	1027	1397
P	12	36	72	118
$P-P_0$	18·5	42	78·5	124·5
$T-P$	314	635	955	1279
Δ	321	320	324	

Reduced by +100. $T_0=1130$, $P_0=76$.

−255	−602	−948	−1333
59	32	−2	−57
−79	−44	−78	−127
−314	−634	−946	−1272
320	312	336	

Reduced by +120. $T=-1327$, $P=-51$.

T	290 6	648	1013	1396
P	−21	17	62	115
$P-P_0$	30	68	133	166
$T-P$	312	631	950	1281
Δ	319	320	330	

Reduced by +120. $T_0=1393$, $P_0=116$.

−223	−585	−940	−1332
90	50	8	−47
−26	−66	−124	−161
−313	−636	−948	1284
323	312	336	

6. *Results.*—If, therefore, by determined forces $\pm W_m$ acting in opposite directions, a wire is repeatedly twisted in alternate senses, till the temporary and permanent torsions $T_{\pm m}$ and $P_{\pm m}$ attained by those forces are constant, and if the wire be again twisted by increasing weights in the sense of the last torsion, wherein its residual permanent torsion P_p remains constant, its *transient* torsions T_p-P_p, reckoned from the latter, increase nearly in proportion to the acting weights. A very slight rise in the values of Δ is perceptible with the greater torsions.

If the permanent torsion P_{+m} produced in a wire by the weight W_{+m} be reduced by a counteracting weight to a lower value P_n within the limits $P_{\pm m}$, and then the wire be again twisted by increasing weights $W_{(+)}$, the permanent torsion will gradually increase from P_n to P_{+m}. If this be subtracted from the temporary torsions produced by the above weights W, the differences $T-P$ will again be the same to whatever value P_n within the first limits P_{+m} and P_{-m} the torsion P_{+m} has been reduced. They rise hardly quicker than corresponds with the proportionality to the acting weights, and are equal to the temporary torsions T_m-P_p produced by the same weights in the ascending direction. Within the limits $P_{\pm m}$ and $T_{\pm m}$, therefore, the wire is almost perfectly elastic, if the temporary deformations produced by the forces be reckoned from the actual zero-position, and so the *transient* torsions alone be taken into account. It is consequently only the displacement of the position of permanent torsion toward one side that conditions the deviation of the total (reckoned from the original position of equilibrium) temporary torsions from proportionality with the acting forces. Therewith the permanent torsion P_n increases up to P_m, at first more quickly, and then more slowly, with the rising forces.

From the above-cited experiments it immediately follows, further, that the wire which, after repeated torsions, has acquired a permanent torsion P_{+m} by forces $W_{(-)}$ acting in the opposite direction to that of the last applied twisting force $W_{(+m)}$, receives stronger temporary torsions $T_{(-)}$ than when the forces $W_{(+)}$ act repeatedly in the same direction as the force $W_{(+m)}$. The difference corresponds to the shifting of the position of permanent torsion in the negative direction.

7. *The Permanent Torsion of a Wire that is frequently twisted as far as a determined Limit.*—In other experiments a metal plate directed vertically downwards was soldered in a radial position to the horizontal disk borne by the wire; and horizontal screws were arranged so that at the torsion of the wire the plate struck against them. When in this way a brass wire of 2 millims. thickness, 450 millims. long, and loaded beneath with 9200 grams, was repeatedly twisted by the torsion-weights, the deflection amounting to T scale-divisions, its permanent torsions P_1 were found to be:—

	I.			II.		
	1.	2.	3.	1.	2.	3.
T	1387	1387	1387	2700	2661	2607
P_1......	99·2	106·9	110	1056	1120	1136

If the wire in experiment I. was again temporarily twisted to 1387, struck gently with a flat wooden bar, and untwisted, P rose to 115·2, P_1 to 115.

Thus also with repeated *equal* (or, indeed, somewhat diminishing) temporary torsions of a wire the succeeding permanent torsions increase. The latter also increase after agitation of the temporarily twisted wire.

8. *Influence of the Load.*—I have already, in the year 1858*, shown that, just as the breaking-off of a permanently twisted iron wire by torsion through magnetizing is independent of the weights with which the wire is loaded, so also, within certain limits of loading (in oscillation-experiments), the toughness of iron wires has the same independence of their tension†, and mentioned that this is not the case with silver wires, especially at a higher temperature. A brief statement of similar results respecting the increase of the toughness with the load was published in 1865 by Sir W. Thomson‡. Braun also has recently§ made experiments in the same direction, concerning the action upon bodies of two deforming forces in directions independent of one another‖.

In connexion with those observations, to be communicated later on, I first made some further experiments, on the static conditions of wires which had been subjected to various forces both of tension and torsion. For these experiments the weighted iron rod suspended beneath the rod *g* of the torsion-apparatus was replaced by a zinc beaker 21 centims. deep and 23 in diameter, hanging upon a horizontal bar. The beaker was parted in the middle by a perforated zinc plate, in order to avoid rotatory oscillations of the water with which it was filled. Near its axis a glass tube of 5 millims. diameter, nowhere in contact with the beaker, descended into it to about 2 centims. from the bottom; the glass tube was connected with a vessel full of water by a caoutchouc tube; and by raising and lowering this vessel the level of the water in the beaker could be varied. To the bottom of the tube was cemented a setting which supported, concentric with the end of the tube, a hemisphere of sheet brass about 2 centims. in diameter. This, as well as the lower part of the tube, was bound round with thick flannel; so that the water flowed through the tube into the beaker with very little velocity, and the weight acted upon the wire, as far as possible, without shock —which in experiments of this kind must be particularly attended to, and cannot be secured by screw arrangements, appending the weights by hand, &c. The mirror attached to the torsion-apparatus remained in fact perfectly stationary while, the wire being not twisted and the point *h* hanging freely, water was introduced through the tube into or let out

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* Pogg. *Ann.* ciii. p. 575 (1858). † *Ibid.* cliii. p. 391 (1874).

‡ Phil. Mag. [4] xxx. p. 63 (1865).

§ Pogg. *Ann.* clix. p. 337 (1878).

‖ Here belong indirectly the observations upon the stronger deadening of the vibrations of strings and rods with greater tension and height of tone. Compare A. Helmholtz, *Tonempfindungen*, p. 122; Warburg, Pogg. *Ann.* cxxxix. p. 89 (1870); Braun, *ibid.* cxlvii. p. 64 (1872), "Dissertation," and cli. p. 260 (1874), of the beaker. The total weight of the metal ring hanging to the wire, with its holdfast &c., as well as that of the beaker filled with water up to the end of the glass tube, amounted to 3220 grams.

9. *Loading during Permanent Torsion.*—First, an annealed brass wire, of 2 millims. thickness and 480 millims. length, was twisted ten times, five minutes each time, up to the stroke of 2430 scale-divisions, while it was loaded beneath with 3220 grams. The permanent torsions, five minutes after each detorsion, were :—

I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.	X.
721	733	737	740·8	743·2	745·2	747	748	749	750

6000 grams of water being then admitted into the beaker suspended beneath the wire while the latter *was in the position of permanent torsion*, the permanent torsion was reduced by the shaking to 748·7. If the wire was then again brought to the previous temporary torsion and untwisted, the permanent torsion rose to 764—and on a second torsion and detorsion, to 770. When the water was let out, the permanent torsion was lowered to 762·6 by the shaking, but rose again up to 765 at a single torsion and detorsion.

10. *Loading during Constant Temporary Torsion.*—When in like manner a new wire was temporarily twisted and untwisted to the stroke of 1952 scale-divisions ten times by the lesser loading, the permanent torsions amounted to :—

I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.	X.
351	387·5	401	409	434	447·5	420·2	422·3	424	425·2

The wire was then again twisted up to striking; and *while it was in the position of the temporary torsion*, 6 kilogs. of water was admitted into the beaker with which it was loaded. After the detorsion the permanent torsion had risen to 452 ; after a repeated torsion and detorsion, to 461.

The same experiments were performed inversely ; that is to say, the wire was first twisted and untwisted ten times while loaded, and was then unloaded in the positions both of the permanent and of the temporary torsion. The results were :—

I. Loaded wire temporarily twisted up to 2283 scale-divisions ten times.

	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.	X.
P...	600	644	651	655	658	660·6	662·2	663·5	664·8	665·8

While the wire was in the permanent position the water was let out, when P fell to 662. The wire being again twisted up to 2283 scale-divisions, and untwisted, without loading, P rose to 664—and on a second torsion and detorsion, to 665·6.

II. Loaded wire ten times temporarily twisted to 1839 scale-divisions.

	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.	X.
P ...	350	385	397	304	309	312·5	315	316·8	318	319·5

When the wire in the temporary position was unloaded and untwisted, the permanent torsion was 319·5, and rose to 320 on a second torsion and detorsion of the unloaded wire.

Therefore, if a load acts upon a wire while it is temporarily twisted only up to a determined limit, an increase of the permanent torsion appears, similarly as above, at the shaking of the wire. This takes place both when, after repeated temporary torsions, the weight is hung on the wire in the position of the permanent and when in the position of the temporary torsion. The unloading acts (though scarcely) in the same direction.

11. *Loading during Variable Temporary Torsion.*—Analogous experiments were made on a brass wire of the same dimensions; but the wire was temporarily twisted by a weight (110 grams) freely, *without striking*, and the load was altered *in the position of the permanent torsion.* In order to render the differences of behaviour with different loads more conspicuous, the wire was heated to 100° C. For this purpose it was suspended, with its holdfasts, in a vertical brass tube of 15 millims. diameter, which was surrounded by a somewhat shorter brass tube 6 centims. wide. The ends of the latter were connected with those of the former by conical additions. This entire heating-apparatus was divided lengthwise into two halves, which were soldered together along the surface of the longitudinal section, and were joined to one another by hinges, in order that the wire might be more easily suspended in it. Steam was introduced simultaneously into both halves, through side tubes, from above, and carried off from beneath. The following were the temporary and permanent torsions (P_0 noting the permanent torsion after alteration of the loading weight):—

W.	P_0.	T.	T_1.	P_1.	T_1-P_1.
3220	......	1475	1502	96	1406
„	......	1501	1515	120	1395
„	......	1511	1518	130	1388
„	......	1516	1521	137	1384
„	......	1519	1522	141	1381
„	......	1522	1524	144	1380
„	......	1524	1525	146	1379
„	......	1524·2	1525·2	148	1777
„	......	1524	1525·2	149	1376
9220	140	1570	1627	241	1386
3220	235	1620	1626·5	242	1384
9220	237	1636	1671	282	1389
3220	265	1659	1667	279	1388
9220	272	1673	1685	298	1387
3220	285	1677	1681	293	1388

Consequently, when the state of the wire, after repeated torsions and detorsions, has become constant, an increase of the loading, imparted to it during its detorsion, effects an augmentation of the following temporary and permanent torsions; on the subsequent diminution of the load the final temporary and permanent torsions remain almost unaltered (*conf.* § 14). If, however, we take the differences between the temporary and the permanent torsions after the wire had been once loaded, they remain almost perfectly equal. By the loading, therefore, only the permanent position of equilibrium is displaced (as above, § 5), reckoned from which the transient torsions produced by the torsion-weight are constant. In this sense the wire which has arrived at a constant state possesses equal elasticity with different loads.

12. *Torsion of a Wire alternately loaded with different Weights.*—A similar action of different loading of the wire is shown when it undergoes torsion by increasing weights w at the same time that it is loaded with alternately lighter and heavier weights W. The experiments were made on a brass wire of the same dimensions as the former ones, and at the temperature of 100° C. The results were as follows:—

w.	W.	P_0.	T.	T_1.	P_1.	T_1-P_1.	ΔT_1.	ΔP_1.	$\Delta(T_1-P_1)$.
44	3220	...	470	471	3·2	467·8	113	5·3	107·7
54	9220	3	583	584	8·5	575·5	105	1·8	103·2
64	3220	6·5	689	689	10·3	678·7	117	6·7	110·3
74	9220	9·5	804	806	17	789	111·5	3·5	107
84	3220	16	916·5	917·5	21·5	896	128·5	18·3	110
94	9220	21·3	1041	104·6	39·8	1006·2	115	9·7	111·3
104	3220	38·5	1161	1167	49·5	1117·5	150	38·2	115·8
114	9220	48·5	1311	1323	87·7	1235·3	148	35·3	120·0
124	3220	84	1459	1468	113	1355	220	107	140
134	9220	111	1679	1715	220·1	1495			

The differences Δ show distinctly that the values T_1 and P_1 constantly increase relatively more rapidly at the transition from the unloaded to the loaded wire than in the opposite case, while, on the contrary, the values T_1-P_1 increase with almost perfect regularity.

13. Lastly, we notice the influence of different stretchings with torsions in alternately different directions. A brass wire of the former dimensions was, under alternately lighter and heavier loading, turned both ways up to a determined temporary torsion $\pm T$, and then the permanent torsion $\pm P$ was observed. Thus:—

	T = −1479.		T = +1479.			T = −1479.		T = +1479.	
	W.	P .	W.	P_1.		W.	P_1.	W.	P_1.
1....	3220	−152	3220	+160	8...	3220	−163	3220	+175
2....	,,	−154	,,	+166	9...	,,	−152	,,	+170
3....	,,	−150	,,	+168	10...	,,	−150	,,	+171
4....	,,	−151	,,	+166	11...	9220	−171	9220	+187
5....	9220	−188	9220	+207	12...	,,	−163	,,	+182
6....	,,	−183	,,	+200	13...	3220	−157	3220	+171
7....	,,	−174	,,	+191					

Consequently, if the wire in the alternatingly directed torsions with a lighter load is brought into such a state that it constantly takes up again the same positive and negative permanent torsions, these will be increased with a heavier loading, but will gradually be diminished with repeated temporary torsions, and approach toward those obtained with the lighter loadings.

14. *Effect of very frequently repeated Loadings.*—The hitherto mentioned effects of loading the wire with different weights always manifested themselves before the wire had arrived at the constant state in which it always behaves in the same manner under like conditions. There still remained for investigation the question whether, after the expiration of this variable state of the wire, alterations of the load still exercise any influence upon its torsion.

For this purpose a well-annealed brass wire of 2 millims. diameter and 480 millims. length was put into the apparatus, loaded beneath with the empty beaker, again annealed and then repeatedly temporarily twisted and untwisted by 100 grams, and, as before, the values T and T_1 as well as P and P_1 read off, as follows:—

	I.	II.	III.	IV.	V.	VI.	VII.	VIII.
T	1952	2078	2113	2133	2138	2144	2146	2148
T_1	2106	2123	2141·5	2146	2149	2153	2154·5	2160*
P	409	445	479	489	498	503	507	517
P_1	386	430	472	484	492	498	503	509
T_1-P_1	1720	1693	1669·5	1662	1657	1658	1651·5	1651

	IX.	X.	XI.	XII.	XIII.	XIV.	XV.
T	2154	2155	2158	2157	2158	2165	2161
T_1	2160	2161	2162	2163	2164	2169	2168
P	516·5	517	521	523	523	526·5	
P_1	511	513	516	518	520	522·5	
T_1-P_1	1649	1648	1646	1645	1644	1646·5	

After the wire ceased to be in this way essentially altered, 7 kilograms of water were alternately let into and out of the

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* A slight shaking.

beaker during the temporary torsion of the wire. The temporary torsions T*z* and T*a* were then:—

	I.	II.	III.	IV.	V.	VI.
T*z*	2167	2179 (2188)	2191	2194	2194	2194·5
T*a*	2181	2194	2200	2198	2200	2203

Hereupon the wire was allowed to unturn by lifting the twisting-weights; its permanent torsion was immediately 559, and after a longer time 552. If water was again let into and out of the beaker, the permanent torsions P*z* and P*a* were:—

	I.	II.	III.	IV.
P*z*	542	542	541	541
P*a*	549	549	549	549

In the temporary torsion, therefore, the first loadings and also the unloadings produce first of all an increase; in the permanent torsion the first loading effects a diminution. If this effect has run out, in the present experiments a decrease *apparently* takes place in both cases at the loading, and an increase at the unloading, to the slight amount of 6–9 scale-divisions. In order to ascertain if this trifling effect might not result from the construction of the apparatus notwithstanding the utmost care in its arrangement, the wire was again unloaded. The positions of the mirror before and after the loading corresponded to 584·7 and 579·2 scale-divisions respectively. The difference amounts to +5·5 in favour of the unloading. If, on the other hand, the steel ring was screwed beneath, in which the point that was fastened under the wire and sustained centrically by it was suspended, the position remained perfectly unaltered at 587. If the wire was then temporarily twisted up to 2212 scale-divisions, and then slowly carried over to the permanent torsion-position 494, and after a longer time to 488, and then alternately loaded and unloaded beneath, I obtained the following:—

	I.	II.	III.	IV.	V.	VI.
P*z*	482·5	480·8	480·1	479	478·8	478·8
P*a*	481·8	480·2	480	478·8	478·8	478·8

Here, therefore, after the first effects have run out, which at first produce at each change of tension a diminution of the permanent torsion, the latter remains perfectly invariable. The above-observed small final variations of P*z* and P*a*, as well as of T*z* and T*a*, are consequently to be attributed to slight rollings of the steel point in the ring, which must always occur if at the action of the twisting-weights it is pressed to one side of the steel ring, rotated on it, and then at the change of load caused to approach the centre or another side of the ring.

Accordingly, if the wire has attained its constant state, variations of the loading within the limits of the experiments exercise no sensible influence upon either its temporary or its permanent torsion.

[To be continued.]

II. *On certain Vibrations of Solids.*

By Frederick Guthrie *.

[Plates II. & III.]

§ 1. THE transverse vibrations of rods, prisms, or laths have been examined by Euler, Poisson, Cauchy, Seebeck, and Chladni. These calculators and experimenters have considered the notes and nodes in such bars when both ends are free. The following numerical experimental results may be of use to those studying the vibration of such solids. They to a considerable extent are in accord with the results obtained by previous experimenters.

I propose to consider here :—(I.) The actual position of the transverse nodes in a lath rigidly clamped along one narrow edge and vibrating in segments ; and (II.) the torsional and torsio-transverse vibrations of a lath similarly clamped.

I. The material used was hammered sheet-brass cut into strips or laths, clamped firmly in a heavy horizontal vice. The brass was 2·5 millims. thick. The free ends were always, unless otherwise stated, 280 millims. long. Three such laths were used, which were of various widths. The laths will in the sequel be called A, B, and C.

A was 280 × 2·5 × 9·4 millims.
B „ 280 × 2·5 × 22·0 „
C „ 280 × 2·5 × 32·0 „

The total length of each lath was 305 millims. (say 1 foot). That the effect of the tail of the lath behind the vice's jaws is negligible will appear in § 4. The nodes were determined in the usual way by fine dry sand, the lath being vibrated by a bow. Let us call the line of the vice's jaws the artificial node, and the other nodes the natural ones ; and let us call the piece of lath from the free end to the first node the first segment, and so on, the last segment being the piece bounded at one end by the vice's jaws. We may represent in the cases of segmental vibration the fixed point by a double transverse

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* Communicated by the Author.

line, the nodes by single ones, and the lath itself by a straight line.

§ 2. Fig. 1, Plate II., shows the results of the first series of experiments with A.

In the next series the same lath was used, but its other end was clamped. This comparison tests the uniformity of the lath in density, elasticity, thickness, and width, the latter condition being only negligible when the edges are parallel. The results were:—

A_1	218·5,	61·5.				
A_2	140·5,	102,	37·5.			
A_3	99,	80·5,	74,	26·5.		
A_4	77,	62,	63,	57·5,	20·5.	
A_5	62,	51,	51·5,	52,	47,	16·5.

Similar experiments with B gave, when one end was clamped:—

B_1	218,	62.				
B_2	140·5,	102·5,	37.			
B_3	99,	81,	73,	27.		
B_4	77,	62·5,	63,	57,	20·5.	
B_5	62·3,	51·7,	51·3,	51·7,	46·5,	16·5.

And when the other end was clamped:—

B_1	218,	62.				
B_2	140·5,	102·5,	37.			
B_3	99,	80·5,	74,	26·5.		
B_4	77,	62·5,	63,	57,	20·5.	
B_5	62·3,	51·2,	51·5	51·5,	46·5,	17.

Taking the mean of these closely concordant results with B, we have:—

B_1	218·00,	62·00.				
B_2	140·50,	102·50,	37·00.			
B_3	99·00,	80·75,	73·50,	26·75.		
B_4	77·00,	62·50,	63·00,	57·00,	20·50.	
B_5	62·30,	51·45,	51·40,	51·60,	46·50,	16·75.

Two generalizations are at once manifest:—i. *All the segments are equal except the first, second, and last.* So that if there are four segments, they are all unequal; if there are five, the third and fourth are equal; if there are n segments, the third, fourth, &c. up to $n-1$ are equal. ii. It is seen that *the first and second segments are together equal to the last.* From the latter generalization it of course follows that when there is an odd number of segments, there is a node in the middle of the lath, and that in all cases the equal segments, which we may call normal segments, are grouped symmetrically in regard to the ends and middle of the lath.

§ 3. The conditions of a lath free at both ends were studied by the men above mentioned, and by Strehlke, and are strictly comparable with those of the half-free lath. A quite free lath always vibrates symmetrically about its geometric centre, whether the material centre be at rest or in motion. When the centre is at rest, and therefore forms a node, the free lath might be supposed to have motions similar to those of two half-free laths joined end to end. And so we might expect that the same lengths of segments would be observed in the quite free lath as in the half-free one.

According to Seebeck, the distance from the end of a quite free lath having natural nodes, when $n+1$ is the number of nodes, is got from the expressions:—

$$\text{the distance of first node from end} = \frac{1{\cdot}322}{4n+2}\,l,$$

$$\text{,, \quad ,, \quad second \quad ,, \quad ,,} = \frac{4{\cdot}9820}{4n+2}\,l,$$

$$\text{,, \quad ,, \quad third \quad ,, \quad ,,} = \frac{9{\cdot}0007}{4n+2}\,l,$$

$$\text{,, \quad ,, \quad } m\text{th \quad ,, \quad ,,} = \frac{4m-3}{4n+2}\,l.$$

The last of these, giving the distance from the free end of the quite free lath of the mth node, of course expresses the equality of the distances from one another of all nodes after the third, and so embraces the generalization i. in § 2; but it has no direct bearing upon the lengthening of the last segment in the half-free lath.

From Seebeck's constants and general expression, adopting for the sake of direct comparison my length of 280 millims. for the quite free lath, we should have the numbers of fig. 2.

These numbers agree remarkably well with mine. But what kind of agreement is it? In the quite free lath having two natural nodes, the end segment is equal to the end segment of the half-free lath of the same length having one artificial and one natural node, and so on for more numerous nodes. If, therefore, we have a quite free lath of length l vibrating with a node in the middle—that is, having an even number of segments (say $2n$)—and then clamp it at the central node, we alter its condition of vibration in so far as we lengthen the end segment, which was formerly one of the two middle ones and had the normal length. Or if we have a half-free lath having one artificial and, say, four natural nodes, and then set it quite free and cause it to vibrate, only the last segment will be affected, breaking up into two parts respectively equal to the first and second at the other end.

§ 4. The lengths of segments and consequent vibrating-periods or notes are independent of the length of the part of the lath on the other side of the vice's jaws. This appears from two experiments with B. This lath was shortened by pushing it through till its effective length was 220 millims. The tail from the front edge of the vice was now 85 millims. The lath divided into two segments $s_1=172$ and $s_2=48$. Their ratio is 3·51. The analogous ratios when the length was 280 was 3·51. The next segmentation gave

$$s_1=29{\cdot}5,\quad s_2=80{\cdot}5,\quad s_3=110,\quad \frac{s_2}{s_1}=2{\cdot}73,\quad \frac{s_3}{s_2}=1{\cdot}36.$$

The analogous ratios with the 280-millims. lath were 2·77 and 1·37 respectively.

II. *Torsional and torsio-transverse vibrations.*

§ 5. The laths A, B, and C are, when clamped as before, easily set vibrating torsionally so as to have a single central longitudinal node (fig. 3).

With A and B the nodal line is perfectly straight. With C it was slightly curved, touching the central line near the middle, and bending down at both ends to the amount of ·5 millim. As this was not a condition of bowing (for the line did not shift when the lath was bowed on the other side), it must result either from some slight variation in the quality of the lath or want of perfect symmetry in the clamping. It has no present interest.

§ 6. The simplest combination of the torsional with the segmental as exhibited by all three laths is shown in fig. 4.

The same figures are obtained on whichever side the lath is bowed; so that whether the form of A and C, or that of B is produced must depend upon some indefinitely small difference of condition either of quality or clamping. With B, however, it is possible to obtain a state of vibration by means of very hard clamping, which is perfectly symmetrical about the axis. This is shown in fig. 5. The actual condition of the sand about the cross nodes is interesting.

The next combination is fig. 6, and then fig. 7. Again, Pl. III. fig. 8. Further, fig. 9.

And finally one can with some difficulty produce fig. 10.

From these several examples of torsio-segmental vibration we may gather that with a long narrow lath such as A (that is, one being twenty-eight or thirty and more times as long as it is broad), all the segments excepting the first are approximately equal, and the length of the first is just half that of the others. The segment is here supposed to be the length between the ends of two neighbouring nodal lines measured from where they cut the edge of the lath. The same result is obtained if one measures between the points of intersection of the middle line of the lath and the lines joining the extremities of opposite nodal lines. With the lath B the same generalization is distinctly traced, while with the broad lath C it is considerably obscured. With a lath as wide as C, indeed, an alteration in the position of the nodal lines may be brought about by altering the pressure in the vice. This will be considered in § 9, where the note is considered.

§ 7. A few asymmetrical figures can be obtained as in fig. 11 (*a*) and (*b*). The figure (*b*) is of course derivable from C, fig. 6. The distribution (*b*) gives identically the same note as (*a*), fig. 11.

III. *Notes.*

§ 8. The notes given in several cases were determined by the monochord of No. 25 B. W. G. stretched by a weight of 21 lb. Calling l the length of the monochord in unison with the lath, we find with the lath B in the case of two natural nodes $l' = 337$, with three natural nodes $l'' = 200$, and with four natural nodes $l''' = 119$. Calling the segments s_1', s_2', s_3', and s_1'', s_2'', s_3'', s_4'', and s_1''', s_2''', s_3''', s_4''', s_5''', respectively, as in fig. 12,

$$\frac{l''}{l'} = 0{\cdot}517 \quad \text{and} \quad \frac{l'''}{l''} = 0{\cdot}595$$

$$\left(\frac{s_1''}{s_1}\right)^2 = 0{\cdot}523 \quad \text{and} \quad \left(\frac{s_1'''}{s_1''}\right)^2 = 0{\cdot}587$$

$$\left(\frac{s_2''}{s_2'}\right)^2 = 0{\cdot}514 \quad \text{and} \quad \left(\frac{s_2'''}{s_2''}\right)^2 = 0{\cdot}601$$

$$\left(\frac{s_3'''}{s_3''}\right)^2 = 0{\cdot}608$$

The number of vibrations, therefore, or pitch, varies inversely with the square of the length of the second segment—that is, as though the second segment were free at one end.

§ 9. If the biting of the vice is relaxed, the notes get flatter, and the nodes slightly shift. This appears in fig. 13; the numbers inside the figures give the superficial contents of the segments under the assumption that the nodes are straight. The corresponding lengths of the consonant monochord are:—for C′, $l=628$; for C″, $l=624$; and for C‴, $l=609$. The nodal lines may be as much as 1 millim. curved away from the chord. As such curvature does not appear to be a direct consequence of the slackness of the clamping, but rather of the inequality of clamping almost necessarily accompanying such slackness, I have not further examined it. In all previous experiments the clamping was so hard that no further clamping affected the nodal positions.

§ 10. The numerical relationship of node in the cases of torsional vibrations which I have examined are as follows:—Calling A_l the length of monochord in unison with A, etc., then it was found that when there was simple torsional vibration as in fig. 3,

$A_l = 116$ . . . width of A = 9·4
$B_l = 277$ . . . " B = 22·0
$C_l = 395$ . . . " C = 32·0

$$\frac{B_l}{A_l}=2{\cdot}388 \qquad \frac{\text{width of B}}{\text{width of A}} = 2{\cdot}340$$

$$\frac{C_l}{B_l}=1{\cdot}462 \qquad \frac{\text{width of C}}{\text{width of B}} = 1{\cdot}454$$

Or the pitch of the note varies inversely with the width of the lath.

It was further found that when B had one natural node besides the axial one (b), fig. 4, the unisonal monochord wire was 94 millims. When B had two natural nodes besides the axial one (d), fig. 6, the wire was 57 millims. long. And, finally, the condition of C in (f), fig. 4, required 132 millims., while that of C in (k), fig. 6, required 79 millims. Accordingly

$$\frac{\text{length for }(b)}{\text{length for }(d)} = \frac{\text{pitch for }(d)}{\text{pitch for }(b)} = 1{\cdot}65,$$

$$\frac{\text{length for }(f)}{\text{length for }(k)} = \frac{\text{pitch for }(k)}{\text{pitch for }(f)} = 1{\cdot}67.$$

Which ratios are approximately equal.

III. *On* Newton's *"Regula Tertia Philosophandi."* *By the* Rev. Professor CHALLIS, *M.A., F.R.S., F.R.A.S.*[*]

THE Third Book of Newton's *Principia*, to which exclusively the title "De Mundi Systemate" is attached, contains at its beginning four Rules of Philosophizing, each accompanied by explanations. Of these rules, the first, second, and fourth, with their explanations, have been very generally accepted, and do not require special consideration. The Third Rule, which is enunciated in these terms, "The qualities of bodies which admit neither of increment nor decrement, and which pertain to all bodies on which experiments can be made, are to be considered qualities of universal bodies," is accompanied by special explanatory remarks and definitions respecting the ultimate qualities of bodies. The purpose of this communication is to indicate the necessity of accepting this rule in conducting physical theory, and to discuss the definitions Newton has appended to it.

The object I have in view requires commencing with the premise that all theoretical research for explaining experimental facts is carried on by means of *calculation*, which in essence is *reasoning* conducted by symbols of quantity. The application of reasoning by calculation for acquiring knowledge consists of three distinct processes:—(I.) Making hypotheses (*i. e.* foundations of calculation); (II.) deriving *equations* by means of the hypotheses from the data of proposed questions; (III.) solving the equations, according to previously ascertained rules, for obtaining the answers to the questions. In order to establish the truth and necessity of this method of theoretical inquiry, I shall bring under review the various stages of its application, beginning with the earliest as regards both the mathematics employed and the physical data. As there will be occasion to revert repeatedly to the several parts, I shall designate them respectively as parts (I.), (II.), and (III.).

It is well known that arithmetical calculation, such as that now practised, had its origin in India, where "the device of place," according to which the value of a figure is determined by its place relative to other figures in the same row, was invented. This expedient, which appears to have been unknown to the Greeks and Romans, and without which it was hardly possible that much progress could be made in calculation, was imported into Europe by the Arabians. Whether or not they derived *Algebra* from the same quarter, it is certain that the Arabic name, which properly signifies the oppo-

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[*] Communicated by the Author.

sition of the two sides of an equation, not only indicates advancement in numerical calculation, but shows also that the Arabians put in practice the above-defined processes (I.), (II.), (III.) for acquiring information by calculation. In fact, the formation of the equation implies, first, the making of an hypothesis (putting x for the unknown quantity), thence deriving the equation by arithmetical rules from the data of the question, and, thirdly, the possession of some means of solving the equation. The *rules* of calculation are obtained by means of numerical indications of quantity, it being necessary for that purpose to know the relative magnitudes of the quantities. Letters might thence be put in place of numbers in such manner as to generalize all particular instances of the application of the rules; and the result would be general arithmetic, but not algebra. The essential principle of algebra, as now understood, is to reason according to the arithmetical rules without knowing whether a represents a greater or less quantity than b. The Arabians, in forming and solving equations, do not appear to have put letters for known or given quantities. This step was first taken by Vieta (in the latter half of the sixteenth century), necessitating the use of the signs of operation + and −, and, in logical sequence of the use of such symbols according to ascertained rules, forms of expression which may be called representations of *negative* quantity and *impossible* quantity. From this time the science of general algebra made rapid progress, our countrymen Oughtred, Harris, Wallis, and Newton being conspicuous among its promoters. When by their labours the rules of operating with indices and expanding in series were established, the way was cleared for discovering a new process in the application of calculation for answering questions. This advance was first made by Newton, whether as exhibited in his method of prime and ultimate ratios or that of fluxions. Leibnitz invented the differential calculus, which is the symbolic form of the new calculation which is most suitable for general application. By means of the differential calculus equations are formed the answers to which are no longer values of unknown quantities, but forms of unknown functions of variable quantities.

In the meantime discoveries were made by observation and experiment which called for the application of the new calculus. Kepler ascertained by observational astronomy his three famous laws of the planetary motions without being able to assign any reasons for them. Galileo determined experimentally (in opposition to the Aristotelians) the laws of the acceleration of bodies falling towards the earth by the constant action of gravity, and, what is much more, the parabolic motion of a projectile. The latter fact is indicative of the law that the acceleration in the direction in which gravity acts is the same whatever from other causes may be the movement of the body acted upon. It is here proper to remark that experimental determinations may be classed under two different heads, some being necessary as *foundations* of theoretical research, and others serving to verify and extend theoretically calculated results. Of the former class are Galileo's theorems respecting the acceleration of falling bodies and the parabolic motion of a projectile. All that Newton and Laplace wrote on physical astronomy depends on hypothetically adopting these two theorems. Newton, in his First Book, repeatedly expresses his indebtedness to the second, calling it "Galileo's Theorem." Kepler's observations belong to the other class; and his name occurs in the Third Book, where Newton specially refers to the law of the planetary distances as a *phenomenon* to be accounted for by the theory of gravitation. The laws of the lunar and planetary motions being determinable since Newton's time by theoretical investigation, there was no more occasion to employ for that purpose such observations as those of Kepler, it being the particular province of theory to demonstrate *laws*, while observations are required for ascertaining the numerical values of the constants which the theoretical formulæ involve. The astronomical observer of the present day simply determines as accurately as he can the celestial places of the sun, moon, and planets, and gives his results to the theoretical computer to be dealt with according to his requirements. Flamsteed, from not understanding what he considered to be Newton's crotchets, attempted to discover the laws of the lunar inequalities solely by observation. There is reason to think that something like Flamsteed's misapprehension of the relation between the respective provinces of theory and experiment exists at the present time.

After these preliminaries, I am prepared to enunciate the parts (I.), (II.), (III.) of *Physical Astronomy*. The differential calculus satisfies the demands of physical astronomy so far as regards the operations which calculation has to perform. Part (I.) consists in making these three hypotheses:—(1) that every particle of matter attracts by the force of gravity every other particle; (2) that the action varies with the distance between the particles according to the law of the inverse square; (3) that the action conforms to the law of Galileo's Theorem. Part (II.) consists in deducing from these hypotheses what is equivalent in modern analysis to forming a differential equation involving an unknown expression or function of a variable quantity. This was Newton's most important discovery, which his contemporaries, although, as he says, they were acquainted with the law of the inverse square, in vain attempted to make. Part (III.) is composed of determinations of unknown expressions by virtual solutions of differential equations, together with physical inferences drawn from the solutions, and comparisons of these results with facts of observation. In short, in analytical language Newton effected the solution of a differential equation of the second order between two variables, whereby he not only accounted for Kepler's laws, but also demonstrated, *à posteriori*, by the comparison of calculation with observation, the hypothesis of the inverse square and that of Galileo's Theorem.

The course of inquiry by which the science of physical astronomy has been established forms a paradigm to be followed in the more advanced department of physical research embracing the theories of light, heat, force of gravity, electricity, galvanism, and magnetism. Here also Newton has pointed out the way of proceeding, having stated in his Third Rule of philosophizing, and at the end of the Third Book, the primary *hypotheses* on which such research must be founded. These constitute Part (I.) of this new stage of physical philosophy. They consist of two kinds, namely, hypotheses absolutely true, and hypotheses which admit of being proved to be true by comparison of mathematical results derived from them with facts of observation. To both kinds Newton's dictum, "*hypotheses non fingo*" (I do not arbitrarily make hypotheses), applies. The several hypotheses may be stated as follows. The "least parts" (atoms) of bodies have only sensible qualities, as form, magnitude, mobility, and intrinsic inertia. These are absolute and necessary qualities, inasmuch as their existence is immediately conveyed to us by our senses, and apart from them we cannot conceive of matter. Newton adds the quality of "impenetrability" relative to the least parts, which signifies that they admit of no change of form or magnitude. In fact, they would otherwise not be conformable to that part of the enunciation of the Third Rule, which states that the ultimate qualities of bodies cannot be increased and diminished (*intendi et remitti nequeunt*). Moreover he does not allow the possibility (or conceivableness) of one body acting upon another without intervening substance; and in the scholium at the end of the *Principia* he adverts to the agency of a certain "very subtle" medium (the æther), pervading the grosser bodies. On adopting these Newtonian principles for the purpose of extending the boundaries of physical philosophy, I have further supposed, for the purpose of applying mathematical reasoning, that the atoms are impenetrable *spheres*, that the æther is a perfect fluid of constant intrinsic elasticity, and that it varies in pressure in exact proportion to variations of its density. The æther is thus defined by the equation $p=k\rho$, p being the pressure, ρ the density, and k an absolute constant. These three hypotheses are of the kind which have to be verified by comparisons of deductions from them with experimental facts.

Bearing in mind Newton's monition not to make hypotheses contrary to "the tenor of experience," or to deviate from "the analogy of nature," I have added the above-stated hypotheses on the following grounds. First, since experience has brought to our knowledge no other kind of *masses* of matter than such as are atomically constituted, and since a universal *plenum* under any natural conditions is inconceivable, I suppose the æther to have a uniform atomic constitution—that is, to consist of minute atoms all of the same size and uniformly distributed. Under this constitution it may be conceived to be susceptible of *variations of density* from point to point, and to be capable of pressing in proportion to the density. Again, in the formula $p=k\rho$ as applied to the æther, there can be no variation of the factor k, because, as the existence of the æther is supposed to be the primary condition of physical force, there are no ulterior forces to which a variation of its elasticity can be referred. This view makes it necessary to demonstrate that all the forms of physical force are modes of pressure of the ætherial medium. To show this has been the express object of physical researches (subsequently referred to in this communication) on which I have been engaged during many years. Further, it is to be said that a system of philosophy which rests wholly on the indications of sensation and experience can admit of no other kind of force than *pressure*, because of this force we have immediate *sensible* cognition (as when we press with the hand against any substance); and the same assertion cannot be made respecting any other kind of force. In support of the hypothesis of the spherical form of the atom I might cite another of Newton's sayings, "*natura solet esse simplex*," the form of the sphere being defined by a single constant. Also the fact that a mass of water remains the same in magnitude and quality after any amount of change of the relative positions of its parts, implies that each ultimate part has the same relation to surrounding parts after as before the disturbance, and that the relation is the same in all directions from the ultimate part. These views may be regarded as justifying the hypothesis of the spherical form of the atom, but only as a ground for mathematical reasoning whereby the truth of the hypothesis may be tested.

Before proceeding to Part (II.) of the stage of physical inquiry now under consideration, a certain mathematical difficulty has to be stated and cleared up. In physical astronomy, as we have seen, Newton overcame the difficulty of discovering the calculation proper for determining the motion of a single particle acted upon by given forces. Since, according to the foregoing hypotheses, the pressure of the ætherial medium performs an essential function, we have now to discover the *hydrodynamical* principles and calculations applicable to the determination of the motion and pressure of a fluid particle in juxtaposition with other particles, which is a preliminary difficulty of the same kind as that just mentioned. The former process required the formation of a differential equation containing two variables; this requires differential equations containing at least three variables, and is therefore of greater complexity. The discovery and treatment of two such equations, one expressing the principle of constancy of mass, and the other derived from an application of D'Alembert's Principle, were effected by the researches of Euler, Lagrange, and Poisson. On attempting to advance to Part (II.) by employing these equations, I was stopped by finding that Poisson had deduced from them, in the case of a fluid the pressure of which varies proportionally to its density, an equation from which I could strictly infer that the same fluid particle might be at rest and have a maximum velocity at the same moment of time (see Phil. Mag. for June 1848, p. 496, and my 'Principles of Pure and Applied Calculation,' p. 195). This was evidently a *reductio ad absurdum* absolutely demanding a reconsideration of the hydrodynamical principles. As those principles on which the two equations are founded are unquestionably true, the only logical inference is that they are *insufficient*. It consequently occurred to me that, besides the equation expressing constancy of mass, one was required for expressing *continuity of the motion*. Accordingly I have proposed to found such an equation on the following principles:—(1) that the course of a given particle is so far continuous that the directions of its motion in successive instants cannot make with each other a finite angle; (2) that the directions of the lines of motion in each elementary particle are at each instant normals to a surface of continuous curvature, which is either of finite or infinitely small extent, the total surface of displacement being conceived to be such that, however it might be composed of discontinuous parts, no two of its contiguous elements are inclined to each other by a finite angle.

In proof of principle (1) it suffices to say that a finite change of direction of the motion of an element in an infinitely small interval could only be produced by infinite force, which, by the nature of the inquiry, is excluded. The principle (2) of geometrical continuity, being supposed independent of change of time and place, requires to be expressed as such, like that of constancy of mass, by a differential equation, the mathematical investigation of which I proceed now to give.

If u, v, w be the velocities at the point xyz at the time t in the directions of the axes of x, y, z, it is known that in the case supposed of surfaces of displacement of continuous curvature, $udx+vdy+wdz$ is either integrable of itself or by a factor. Hence if $\frac{1}{\lambda}$ be the factor, when one is required, and $(d\psi)$ be put for the exact differential $\frac{u}{\lambda}dx+\frac{v}{\lambda}dy+\frac{w}{\lambda}dz$, we shall have $(d\psi)=0$ for a surface of displacement. The above stated principle of continuity requires that such an equation should apply to each element of the fluid at successive instants. This condition is expressed by the formula $(d\psi)+\delta(d\psi)=0$, the sign of variation δ having reference to change of position of the element in space and time. Hence as $(d\psi)=0$, we shall have also $\delta(d\psi)=0$, and, on account of the independence of the signs of operation δ and d, $(d\,.\,\delta\psi)=0$, whence by integration $\delta\psi=\phi(t)\delta t$. The complete variation $\delta\psi$ of the function ψ in the time δt is, by the Calculus of Variations,

$$\frac{d\psi}{dt}\delta t+\frac{d\psi}{dx}\delta x+\frac{d\psi}{dy}\delta y+\frac{d\psi}{dz}\delta z,$$

ψ being a function of x, y, z, and t. Since the variations δx, δy, δz apply to the change of position of the given particle in the small interval δt, $\delta x=u\delta t$, $\delta y=v\delta t$, and $\delta z=w\delta t$. Also we have $\frac{u}{\lambda}=\frac{d\psi}{dx}$, $\frac{v}{\lambda}=\frac{d\psi}{dy}$, $\frac{w}{\lambda}=\frac{d\psi}{dz}$. Hence by substitution, and, after rejecting the common factor δt, supposing $\phi(t)$ to be included in $\frac{d\psi}{dt}$, we obtain

$$\frac{d\psi}{dt}+\lambda\left(\frac{d\psi^2}{dx^2}+\frac{d\psi^2}{dy^2}+\frac{d\psi^2}{dz^2}\right)=0. \quad . \quad . \quad . \quad (A)$$

This is the required equation of continuity. By this equation the mathematical theory of the motion of a fluid is completed, inasmuch as the three differential equations, together with the equations $u=\lambda\frac{d\psi}{dx}$, $v=\lambda\frac{d\psi}{dy}$, $w=\lambda\frac{d\psi}{dz}$, and a given relation between the pressure p and the density ρ, furnish seven equations, which suffice for determining the seven unknown quantities ψ, λ, u, v, w, p, ρ as functions of x, y, z, and t. (See another investigation of the same equation in pp. 174 and 175 of the work already cited.)

As the equation (A) serves for determining λ as a function of x, y, z, and t, and as I have not been able to discover any fault in the reasoning by which it was reached, I regard it as giving proof of the reality of motion for which $udx+vdy+wdz$ is integrable by a factor. In short, reasons may be adduced for concluding that the factor $\frac{1}{\lambda}$ is applicable to cases in which the motion is not distinctively that of a fluid, but such as a fluid is capable of if conceived to be composed of infinitely small parts that are solid—such, for instance, as uniform rectilinear motions parallel to a given plane and varying as some function of the distance from the plane, or uniform motions of revolution of infinitely thin cylindrical shells with velocities varying as some function of the distance from a common axis, or steady spiral motions composed of these two kinds. So far as the motion partakes of that which pertains to a solid, it must be determinable from the data of the problem by separate treatment, and has to be eliminated. For the remaining motion $udx+vdy+wdz$ is integrable of itself, because this analytical circumstance specially indicates that the motion is such as pertains to a fluid. Hence the reasoning (in the Phil. Mag. for March 1851, p. 232, and June 1873, p. 436, and in the 'Principles of Applied Calculation,' p. 186) from which I have inferred that the motion is rectilinear when $udx+vdy+wdz$ is an exact differential, and have thence attempted to get rid of the difficulty resulting from the before-mentioned *reductio ad absurdum*, cannot be maintained. I have recently ascertained that the origin of the difficulty admits of being simply stated as follows. In the reasoning by which the rate of propagation in a fluid defined by the equation $p=a^2\rho$ can be shown to be such as to lead to the above-mentioned absurdity for a particular form of the arbitrary function, it is assumed that the lines of motion are all *parallel* to a fixed plane, or that the surfaces of displacement are planes. (See Phil. Mag. for Jan. 1851, example I., p. 34, and 'Principles &c.' example I., p. 193.) Now in that case, as is evident, the motion is not such as is peculiar to a fluid, but of the kind for which, as above said, $udx+vdy+wdz$ is integrable by a factor, whereas in the reasoning that differential expression is assumed to be integrable of itself. This contradiction accounts for the *reductio ad absurdum*.

In order to clear up the difficulty, I have supposed that *rec-*

tilinear propagation takes place along an *axis*, and that the contiguous motion is such as only a fluid is capable of. To express these conditions, it suffices, after taking the straight line of propagation for the axis of z, to assume that

$$(d\,.f\phi)=udx+vdy+wdz,$$

f being a function of x and y only, and ϕ a function of z and t only. For, on these suppositions,

$$u=\phi\frac{df}{dx},\quad v=\phi\frac{df}{dy},\quad w=f\frac{d\phi}{dz};$$

so that if the function f be such that $f=1$, $\frac{df}{dx}=0$, and $\frac{df}{dy}=0$, where $x=0$, and $y=0$, the axis of z will evidently be an axis of the motion. On reasoning from these antecedents no contradiction is met with like that which occurred in the previous method; and the reasoning is proved to be legitimate by actually conducting to a definite form, expressed in series, of the function ϕ, and also to a series of definite form for f in case the motion be a function of the distance from the axis for any given value of z. The series for ϕ is the harmonic series *assumed* by Helmholtz and other physicists in the mathematical theory of music. As the motion indicated by this series is not supposed to be dependent on a particular mode of disturbance, it evidently should be derived mathematically from the initial hydrodynamical definitions. This is what is done by the above-stated course of reasoning. The other factor f differs very little from unity in aerial vibrations; but in a medium of great elasticity, such as we have supposed the æther to be, it is of special significance, serving to account for *transverse* vibrations in the undulatory theory of light, and the distinction between common and polarized light. As experience shows that polarization is a quality of light depending on the constitution of the medium by which the luminous vibrations are transmitted, being producible under a great variety of extraneous conditions, it is an important confirmation of the present reasoning that it is capable of deducing transverse vibrations from the original definition of the æther.

By the same course of reasoning, the velocity of propagation is found to be κa, κ being a determinable numerical constant. Having concluded that my attempt to calculate the value of κ, contained in the Phil. Mag. for February 1853, was erroneous, I made another in the Phil. Mag. for May 1865, p. 329, which I have introduced into the 'Principles' &c. prop. xiv. pp. 214–224. The theoretical value of the velocity of sound thence derived is 1109·3 feet per second, which exceeds by 17·5 feet the value obtained experimentally by Dr. Schröder (Phil. Mag. for July 1865, p. 47).

It will here be proper to make some remarks on methods that had been previously employed for determining theoretically the velocity of sound. The reasoning adopted by Newton, and afterwards by Laplace, fails to give the true theoretical value, because it is vitiated by involving the contradiction which I have indicated above. Laplace, assuming that from hydrodynamical considerations no value different from the quantity a was deducible, proposed to account for the difference between observation and theory by the effect of development of heat and cold produced by the condensations and rarefactions of the aerial vibrations. He accordingly sought, quite logically, to establish this explanation on a theory of heat. I do not suppose that any physicist now accepts that theory, inasmuch as the explanation of the above-mentioned difference is made to rest on some gratuitous hypothesis which is not supported by any reference to an antecedent theory of heat. For instance, when it is said that the acceleration of the rate of propagation is due to slow dispersion of the developed heat and cold, no evidence is adduced to show that this is a *vera causa* in unconfined air. From a theory of heat which I have founded on Newton's *à priori* hypotheses, I have inferred that the dispersion is so rapid as to produce little or no effect on the rate of propagation; and this result accords with the value of κ deduced mathematically, as said above, from the properties of a fluid defined by the equation $p=a^2\rho$. (See the discussion of this question, under the head of "Mathematical Principles of Physics," in pp. 472–474 of the before-cited work.)

After the foregoing rectification of the principles of hydrodynamics, the way is clear for proceeding from Part (I.) of general physics to the consideration of Parts (II.) and (III.). The former of these two parts consists of formations of equations applicable to the several theories of light, heat, gravitation, electricity, galvanism, and magnetism; and the latter comprises deductions from the solutions of the equations and comparisons of the results with experiment. Respecting the investigations belonging to these two divisions, I can only refer to my own mathematico-physical productions, because, as my contemporaries have in no instance adopted the Newtonian hypotheses of Part (I.), they are logically debarred from entering upon Parts (II.) and (III.). In a communication like the present it would not be possible to advert in detail to the many problems I have undertaken to discuss under these two heads; so that I can do no more than direct the reader's attention to the investigations contained in my 'Principles of Pure and Applied Mathematics,' published in 1869, and in two smaller works, one entitled 'Essay on the Mathematical Principles of Physics,' and the other 'Remarks on the Cambridge Mathematical Studies and their Relation to Modern Physical Science,' published respectively in 1873 and 1875. These investigations are founded, for the most part, on numerous communications made to the Philosophical Magazine. I am well aware that, as was likely to happen in so extensive an undertaking, there are many imperfections and errors in these productions (some of which I have rectified in later communications to the Phil. Mag); but since the course which they follow rests on Newton's authority, and, as I think I have shown in this communication, is pointed out by the antecedents of physical philosophy, I cannot but be of opinion that these works, or one embodying like views, will eventually have to be regarded as holding the same place relative to general physics as Newton's *Principia* holds with respect to physical astronomy.

I shall here introduce a few results obtained under Parts (II.) and (III.), selecting such as may serve to justify the opinion above expressed as to the necessity of the course of philosophy I have been advocating.

(1) The distinction between common light and polarized light is indicated by means of the function f already defined, which for common light is a function of the distance r from the axis of propagation independently of any special mode of disturbing the æther, and for polarized light a function of x and y depending on arbitrary disturbance. These results agree with experimental facts.

(2) The dynamical action of ætherial vibrations on a small sphere, when terms of the second order are taken into account, is capable of producing, according to differences of circumstances, attraction or repulsion of the sphere, and thus of accounting for atomic repulsion, molecular attraction, and the attraction of gravity.

(3) It may be demonstrated that no particle of the æther, supposed to be of unlimited dimensions, can be transferred across a plane fixed in space so as permanently to alter the quantities of fluid on the two sides of the plane. Consequently the motions are either vibratory or in re-entering currents. To the former motions the phenomena of light, heat, molecular adhesion, and gravitation are referable, and to the latter the phenomena of electricity, galvanism, and magnetism.

(4) On the same principles, the existence of a mechanical equivalent of heat is accounted for (see 'Principles &c.' pp. 469 and 481).

(5) It may plainly be demanded of this advanced department of physical philosophy to give reasons for the qualities of the three hypotheses on which, as before stated, the science of physical astronomy rests. It affords, in fact, the following reasons:—(1) The universality of gravitation is a direct consequence of its being a mode of action of a universal medium, the æther. (2) The gravitation law of the inverse square is mathematically derived from the dynamical action of ætherial undulations of large magnitude on an atom taken to be a small sphere. The most complete investigation of this law that I have succeeded in giving on hydrodynamical principles, I consider to be that contained in arts. 31–38 of a communication on Attractive and Repulsive Forces, in the Phil. Mag. of September 1872. (3) In art. 39 of the same communication, an argument embracing the *squares* of the velocities of the æther is adduced, from which the *coexistence* of the translatory effects of all forces referable to the action of ætherial vibrations is inferred. Hence, after accounting for universal gravitation and the law of the inverse square, Galileo's Theorem, in its most general acceptation, is an immediate consequence of the above result.

If it should be inquired whether the hypotheses of general physics, like those of physical astronomy, admit of being derived from ulterior conditions, I should decidedly say they are not so derivable. For as the philosophy of general physics consists of fundamental principles perfectly intelligible from sensation and experience, and of mathematical deductions therefrom, no further research is either needed or possible, the results being reached by means whereby alone complete human knowledge of nature is attainable. These physical principles must therefore be referred to causes that are "not mechanical" (to use Newton's expression), and may consequently exist by the immediate Will of the Author of the Universe. It may further be remarked that in this scheme of philosophy no place is left for the exercise of the *imagination*; and accordingly Newton has laid down the rule (not enough attended to by some modern physicists), "somnia temere confingenda non sunt."

It is right that in connexion with the foregoing views I should advert to the many excellent treatises on physics which have been published in recent times both in England and on the Continent. These, one and all, are devoted to establishing physical facts, and expressing the laws by which they are governed by mathematical formulæ. This necessary preliminary department of physical philosophy, which has been handled with admirable skill and exactness in those treatises, evidently differs in its character from Newton's mathematical *Principles* of Natural Philosophy. French writers, with their usual attention to logical accuracy, name that department *La Physique* (Physics), and carefully distinguish between ascertaining laws by such means, and referring them to *à priori* principles. The deduction of laws from experiment and observation, and expressing them by mathematical formulæ, is often called *theory*; and such it is in a subordinate sense. But theory in its most exact and complete sense consists in giving *reasons* for experimental laws by means of mathematical reasoning founded on intelligible primordial *principles*. This is the department I have taken up, in accordance with Newton's example and anticipations, being of opinion that his *Principia* has established the presumption that laws, just because they are *laws*, are proper subjects of human research and demonstration.

My physical investigations may be classed under two heads:—those relating to the principles and processes of Hydrodynamics, which I commenced in the Philosophical Magazine as far back as the year 1829, and have carried on, not always successfully, up to the present time; and those which treat theoretically of the laws of the physical forces on the basis of the Newtonian hypotheses, beginning with a Mathematical Theory of Heat contained in the Philosophical Magazine for March 1859. Among this class of researches I beg to call attention more particularly to the article on Newton's “Foundation of all Philosophy,” in the Philosophical Magazine for October 1863, p. 280. As I intend the present communication to be a kind of *résumé* of the portions of my productions which I consider to be most conducive to the progress of theoretical physics, and as probably I may be unable to make any further efforts of the same kind, I take this opportunity for expressing the opinion, which I have long entertained, that injustice has been done to Newton's scientific fame by the persistent neglect, and even opposition, with which modern physicists have treated the parts of his Book III. which have reference to the future of theoretical philosophy, and may be considered to justify his naming that Book *Mundi Systema*, those parts being, according to my judgment, among the most remarkable proofs of the greatness of his genius.

Cambridge, November 17, 1879.

Postscript, November 26, 1879.—The theoretical rate of propagation of sound, which, as stated in the foregoing communication, was obtained in the Philosophical Magazine for May 1865 (p. 329), and introduced into the 'Principles, &c.,' prop. xiv., pp. 214–224, was derived from an exact integral of the equation

$$\frac{d^2f}{dr^2}+\frac{df}{r\,dr}+4ef=\frac{f}{4r^2},$$

namely,

$$f=(4\pi r\sqrt{e})^{-\frac{1}{2}}\cos\left(2r\sqrt{e}-\frac{\pi}{4}\right).$$

I argued that the term $\frac{f}{4r^2}$ might be omitted for very large values of r, as being incomparably less than the other terms by reason of the denominator $4r^2$. It has, however, very recently occurred to me that under the same circumstances the term $\frac{df}{r\,dr}$ is incomparably less than either $\frac{d^2f}{dr^2}$ or $4ef$, as might readily be inferred from the given expression for f. The integral I employed was therefore an approximation, for large values of r, to that of $\frac{d^2f}{dr^2}+4ef=0$, but not, as I supposed, to that of

$$\frac{d^2f}{dr^2}+\frac{df}{r\,dr}+4ef=0.$$

In fact it may readily be shown that the latter equation is *exactly* satisfied by the solution,

$$f=\frac{c}{r}\cos\left(\pi r\sqrt{e}-\frac{\pi}{4}\right),$$

for the special values of r which make the cosine vanish, and for all other values with increasing approximation as r is greater, and that these special values are separated by the constant interval $\frac{1}{\sqrt{e}}$. These results confirm those obtained by the very different investigation I gave in the Number of the Philosophical Magazine for February 1853, which I was induced to relinquish only under the above-stated misapprehension as to the applicability of the former expression for f. After full consideration I have not been able to discover any fault in the reasoning of that investigation. The rate of propagation it gives is $a\left(1+\frac{4}{\pi^2}\right)^{\frac{1}{2}}$, which, if we take the value of a to be 916,322 feet, as adopted by Sir John Herschel, we obtain for the velocity of sound 1086·25 feet, which is only

3·17 feet less than the experimental value 1089·42 feet deduced by the same author from a large number of observations. For these reasons I consider the theoretical value of the velocity of sound to be

$$a\left(1+\frac{4}{\pi^2}\right)^{\frac{1}{2}},$$

as deduced exclusively from hydrodynamical principles, such as I have defined them to be in the foregoing communication.

J. Challis.

IV. *On a Suggestion as to the Constitution of Chlorine, offered by the Dynamical Theory of Gases.* By A. W. Rücker, *M.A., Professor of Physics in the Yorkshire College, Leeds*[*].

If a gas of density δ consists of molecules each of which possesses m degrees of freedom, and if also the intermolecular forces are negligible, the specific heats at constant pressure (c_p) and at constant volume (c_v) are connected by the two well-known equations

$$(c_p-c_v)\delta=\cdot0694, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

$$\frac{c_p}{c_v}=1+\frac{2}{m+e}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

where e is a quantity which depends upon the potential energy of a molecule. Hence, if c_p is given by experiment, c_v can be calculated from the first of these equations; and then $m+e$ is known from the second.

The accuracy of the value of $m+e$ thus deduced will depend upon that of c_p, and on the legitimacy of the application of the two equations to the gas or vapour under consideration.

With respect to the first of these points, it may be remarked that E. Wiedemann has recently (Pogg. *Ann.* Bd. clvii. p. 1, 1876, and Wied. *Ann.* Bd. ii. p. 195, 1877) determined the specific heats at constant pressure of 14 out of the 35 gases and vapours studied by Regnault. The difference between the results of the two investigators amounts in two cases only (ethylene and ammonia) to 6 per cent.; in three cases it is about 5 per cent., and in all the others less. Thus even on the assumption that the later experiments are absolutely correct, it follows that Regnault's numbers may be trusted to 6 per cent. His results, however, can only be taken as true for the particular temperatures at which the experiments were made, as Wiedemann shows that in all but the most perfect

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[*] Communicated by the Physical Society, having been read on November 22nd.

gases the specific heat at constant pressure varies considerably with the temperature.

More recently still, Wüllner (Wied. *Ann.* Bd. iv. p. 321, 1878), using Kundt and Warburg's method, has determined the ratio of the specific heats of air, carbonic oxide, carbonic acid, nitrous acid, ethylene, and ammonia at 0° and 100° C. He finds that for gases which obey Boyle's law, c_p and c_v are constant, but that in the case of less perfectly gaseous bodies they increase with the temperature. The difference between them, however, is always very approximately constant and equal to the theoretical number—thus justifying the application of the first of the above equations to imperfect gases, and proving that the observed increase in the specific heats is due to work done within the molecules, and not against the intermolecular forces, which must therefore be negligible.

On the whole, then, the result of these researches is to show that $m+e$ can be calculated very approximately from the above equations if c_p is given, and that Regnault's values of this quantity are probably trustworthy to 6 per cent.

One of the chief difficulties of the thermodynamic theory of gases has been to attribute to m and e values which would at once lead to the observed ratios of c_p to c_v, and satisfy any rational supposition as to the interior mechanism of a molecule. Kundt and Warburg proved that for mercury vapour $\frac{c_p}{c_v}=1{\cdot}666$, which is consistent only with the supposition that the atoms of that substance are smooth rigid spheres. Boltzmann (Pogg. *Ann.* Bd. clx. p.175, 1877) and Bosanquet (Phil. Mag. April 1877) have since drawn attention to the fact that, for a smooth rigid surface of revolution, $m=5$ and $e=0$, which would make $\frac{c_p}{c_v}=1{\cdot}4$. The fact, therefore, that this number agrees very closely with those given by experiment for a large number of gases (air, O, N, H, CO, and NO) would be accounted for by supposing their molecules to be surfaces of revolution. This condition would be fulfilled, as is pointed out by Mr. Bosanquet, by two spheres rigidly united, and would thus accord well with our conception of the atomic constitution of the above gases. It would perhaps be better to regard the spheres not as rigidly united, but as bound together by forces which prevent the separation of their surfaces, while leaving them otherwise free to move. The required five degrees of freedom would thus be obtained, and the hypothesis would better coincide with the supposition of the union of two *smooth* spheres to form the ultimate particles of the gases.

For a discussion of the difficulties offered to any such theory by the spectroscope I must refer to the above papers; my present purpose is to point out an interesting fact connected with its application to chlorine and its compounds.

The maximum number of degrees of freedom which a molecule composed of n smooth rigid spheres could possess would be $3n$; but the forces in play between the spheres might, as in the case of air and the other gases above referred to, reduce the number to much below this amount. Thus the value of $m+e$ could not exceed, but might be less than $3n+e$. For gases in which the molecule consists of two atoms, $e=0$; but in the cases of more complex combinations we can say only that, *cæteris paribus*, we should expect that its value would increase with the number of atoms in the molecule. Bearing these facts in mind, the following Tables lead to a curious result. In the first column of each are placed the symbols of the substances referred to. In the second columns are the ratios of the specific heats, deduced (except in the case of mercury) from Regnault's experiments by the use of equation (1). In the third are the values of $m+e$ (omitting fractions) deduced by equation (2). In the fourth are the values of $3n$, where n is the number of atoms of which the molecule is composed.

Table I.

I.	II.	III.	IV.
	$\frac{c_p}{c_v}$.	$m+e$.	$3n$.
Hg	1·666	3	3
Air	1·413	5	6
O_2	1·403	5	6
N_2	1·409	5	6
H_2	1·417	5	6
NO	1·403	5	6
CO	1·416	5	6
N_2O	1·243	8	9
H_2O	1·302	7	9
H_2S	1·335	6	9
CO_2	1·265	8	9
SO_2	1·276	7	9
CS_2	1·198	10	9
NH_3	1·300	7	12
CH_4	1·290	7	15
C_2H_4	1·144	14	18
C_2H_6O	1·110	18	27
$C_4H_{10}O$	1·059	34	45
$C_4H_{10}S$	1·059	34	45
C_3H_5N	1·283	7	27
$C_4H_8O_2$	1·060	33	42
C_3H_6O	1·091	22	30
C_6H_6	1·072	28	36
$C_{10}H_{16}$	1·031	64	78

TABLE II.

I.	II.	III.	IV.	V.
	$\frac{c_p}{c_v}$.	$m+e$.	$3n$.	
Cl_2	1·286	7	6	18
HCl	1·408	5	6	12
C_2H_5Cl......	1·131	15	24	30
$CHCl_3$	1·118	17	15	33
$C_2H_4Cl_2$ ...	1·097	21	24	36
PCl_3	1·122	16	12	30
$AsCl_3$	1·111	18	12	30
$SiCl_4$	1·098	20	15	39
$SnCl_4$	1·092	22	15	39
$TiCl_4$	1·089	22	15	39

The first Table contains a number of simple and more or less complex compound gases and vapours; the second is confined to chlorine and its compounds alone. The difference between the two is most marked. In the first the value of $m+e$ is for every substance (with one exception) less than $3n$, or than the maximum possible value of m. In the second the reverse statement holds good in more than two thirds of the whole number of cases.

This difference can hardly be accidental; nor can it be explained by an error of 6 per cent. in Regnault's experiments. It might be accounted for by supposing that in the case of chlorine e is abnormally large—and that this gas differs from others in which the molecule is built up of two spheres, in that the spheres are not necessarily in contact, and are probably therefore less firmly united.

Another supposition, however, would meet the case equally well, viz. that n has been taken too small, that the symbol Cl_2 is incorrect, and that the atoms of chlorine, and therefore the molecules of its compounds, contain a larger number of subatoms or atoms than has been supposed. It need hardly be added that this supposition fits in most satisfactorily with the results of the recent researches of Prof. Victor Meyer on the vapour-density of chlorine; and in the fifth column of Table II. are given the values of $3n$ calculated on the assumption that throughout the first column we ought to write Cl_3 for Cl. Hydrochloric acid now offers a difficulty, as a body composed of four spheres could only possess so small a number of degrees of freedom as five if the spheres were rigidly connected with their centres in one straight line. With this exception, however, columns III. and V. of Table II. now present differences of the same sign and order as those in the corresponding columns in Table I.

The number of degrees of freedom attributed to each substance may perhaps be wrong by one in some of the simpler bodies, and by rather larger numbers in some of the more complex; but the general character of the Tables is probably beyond the reach of any such changes. An error *e. g.* of 6 per cent. in the specific heat at constant pressure of the tetrachlorides would only reduce the number of their degrees of freedom by one. Much greater alterations would be introduced by taking the specific heats at other temperatures than those at which they were determined by Regnault. Chloroform is the only compound of chlorine of which the law of the variation of the specific heat with the temperature was studied by Wiedemann; and using his results, I find that at 0° C. the number of degrees of freedom of this substance would be one less instead of two greater than 15.

In spite, however, of the uncertainty thus introduced, the comparison of the two Tables is sufficiently suggestive to induce me to lay them before the Physical Society. The fact that the application of the theory of gases to the specific heats of a large number of substances, determined as far as might be under similar circumstances, leads to the alternatives that the atoms of which a molecule of free chlorine is composed are either less strongly united or are more numerous than in the case of other elements, is not unimportant.

It remains to add that the ratios of the specific heats of bromine and of the only one of its compounds which has been studied agree with those of chlorine and the corresponding chlorine compound.

TABLE III.

I.	II.	III.	IV.	V.
	$\frac{c_p}{c_v}$.	$m+e$.	$3n$.	
Br_2	1·302	7	6	18
C_2H_5Br ...	1·114	18	24	30

Note.—Since the above was written, the conclusions at first drawn from Prof. V. Meyer's research have been questioned (Chem. News, Nov. 21, 1879, p. 244) on the ground that experiments made by Seelheim, of Utrecht, indicate the possibility of the formation of a volatile chloride of platinumat high temperatures. However this may be, the cause of the anomalous specific heats of chlorine and its compounds remains to be explained; and the above statement of the alternative suppositions to which the theory discussed leads may not be uninteresting, even if it should be proved that one of them lacks the support which, at the time of writing, Prof. V. Meyer's research was supposed to afford it.

V. *Investigations in Optics, with special reference to the Spectroscope.* By Lord Rayleigh, *F.R.S.*

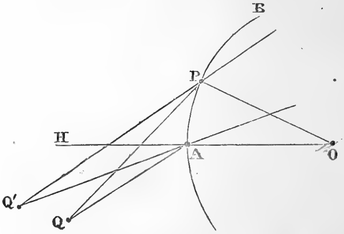
[Continued from vol. viii. p. 486.]

§ 7. *Aberration of Lenses and Prisms.*

THE following investigation refers to the aberration of rays in the primary plane. Let Q be a radiant point in air, from which rays fall upon the spherical surface of glass A P B, of radius r. We require an approximate expression for the length of any ray Q P referred to that of a standard ray Q A.

$POA=\omega$, $QAH=\phi$, $QA=u$. $PA=2r\sin(\tfrac{1}{2}\omega)$. $QAP=\tfrac{1}{2}\pi+\phi+\tfrac{1}{2}\omega$.

Fig. 17.



$$QP^2=u^2+4r^2\sin^2\tfrac{1}{2}\omega+4ru\sin\tfrac{1}{2}\omega\sin(\phi+\tfrac{1}{2}\omega),$$

so that as far as the cube of ω,

$$QP-u=r\sin\phi\,.\,\omega+\frac{r}{2}\left(\cos\phi+\frac{r\cos^2\phi}{u}\right)\omega^2$$
$$-\frac{r\sin\phi}{2}\left(\frac{1}{3}+\frac{r\cos\phi}{u}+\frac{r^2\cos^2\phi}{u^2}\right)\omega^3. \quad . \quad (1)$$

Similarly if Q′ be any other point, and $Q'A=u'$, $Q'AH=\phi'$,

$$Q'P-u'=r\sin\phi'.\,\omega+\frac{r}{2}\left(\cos\phi'+\frac{r\cos^2\phi'}{u'}\right)\omega^2$$
$$-\frac{r\sin\phi'}{2}\left(\frac{1}{3}+\frac{r\cos\phi'}{u'}+\frac{r^2\cos^2\phi'}{u'^2}\right)\omega^3. \quad (2)$$

If QP, Q′P be incident and refracted rays corresponding to $\omega=\theta$, the condition must be satisfied that $\delta(QP)=\mu\,\delta(Q'P)$ as ω passes from the value θ to $\theta+\delta\theta$. Thus

$$\sin\phi+\left(\cos\phi+\frac{r\cos^2\phi}{u}\right)\theta$$
$$-\frac{\sin\phi}{2}\left(1+\frac{3r\cos\phi}{u}+\frac{3r^2\cos^2\phi}{u^2}\right)\theta^2$$
$$=\mu\sin\phi'+\mu\left(\cos\phi'+\frac{r\cos^2\phi'}{u'}\right)\theta$$
$$-\mu\frac{\sin\phi'}{2}\left(1+\frac{3r\cos\phi'}{u'}+\frac{3r^2\cos^2\phi'}{u'^2}\right)\theta^2. \quad (3)$$

If QA, Q′A be also corresponding rays, $\sin\phi-\mu\sin\phi'=0$, as appears by supposing $\theta=0$ in (3), which then becomes

$$\left(\cos\phi+\frac{r\cos^2\phi}{u}\right)\left(1-\frac{3r\theta\sin\phi}{2u}\right)$$
$$=\mu\left(\cos\phi'+\frac{r\cos^2\phi'}{u'}\right)\left(1-\frac{3r\theta\sin\phi'}{2u'}\right).\quad .\quad (4)$$

If we make $\theta=0$ in (4),

$$\cos\phi+\frac{r\cos^2\phi}{u}=\mu\left(\cos\phi'+\frac{r\cos^2\phi'}{u'}\right),\quad .\quad .\quad (5)$$

the usual formula for the primary focus. For our present purpose θ is small, but is not zero.

We will now apply the fundamental formula (4) to the case of a lens whose thickness can be neglected in comparison with the radii of curvature and the distances of the foci. The pencil is supposed to fall centrically, so that the angle of incidence at the second surface is equal to the angle of refraction at the first surface. The distance corresponding to PA is the same for both surfaces, and will be denoted by y. Thus, for the first refraction,

$$\left(\frac{\cos\phi}{r}+\frac{\cos^2\phi}{u}\right)\left(1-\frac{3y\sin\phi}{2u}\right)$$
$$=\mu\left(\frac{\cos\phi'}{r}+\frac{\cos^2\phi'}{u'}\right)\left(1-\frac{3y\sin\phi'}{2u'}\right).\quad .\quad (6)$$

For the second refraction we have to interchange ϕ and ϕ', writing s for r, u' for u, μ^{-1} for μ, and v for u'. Thus

$$\mu\left(\frac{\cos\phi'}{s}+\frac{\cos^2\phi'}{u'}\right)\left(1-\frac{3y\sin\phi'}{2u'}\right)$$
$$=\left(\frac{\cos\phi}{s}+\frac{\cos^2\phi}{v}\right)\left(1-\frac{3y\sin\phi}{2v}\right).\quad .\quad .\quad (7)$$

By addition of (6) and (7), and writing for brevity c for $\cos\phi$ and c' for $\cos\phi'$, we get

$$\left(\frac{c}{r}+\frac{c^2}{u}\right)\left(1-\frac{3y\sin\phi}{2u}\right)-\left(\frac{c}{s}+\frac{c^2}{v}\right)\left(1-\frac{3y\sin\phi}{2v}\right)$$
$$=\mu\left(\frac{c'}{r}+\frac{c'^2}{u'}\right)\left(1-\frac{3y\sin\phi'}{2u'}\right)-\mu\left(\frac{c'}{s}+\frac{c'^2}{u'}\right)\left(1-\frac{3y\sin\phi'}{2u'}\right)$$
$$=\mu c'\left(\frac{1}{r}-\frac{1}{s}\right)-\frac{3y\sin\phi}{2u'}\left(\frac{c'}{r}-\frac{c'}{s}\right),$$

or

$$\frac{c^2}{u}-\frac{c^2}{v}=(\mu c'-c)\left(\frac{1}{r}-\frac{1}{s}\right)$$
$$-\frac{3y\sin\phi}{2}\left\{\frac{c'}{u'}\left(\frac{1}{r}-\frac{1}{s}\right)-\frac{1}{u}\left(\frac{c}{r}+\frac{c^2}{u}\right)+\frac{1}{v}\left(\frac{c}{s}+\frac{c^2}{v}\right)\right\}. \quad . \quad (8)$$

The quantity within brackets in (8) may be written

$$\frac{c'}{u'}\left(\frac{1}{r}-\frac{1}{s}\right)-\frac{c}{u}\left(\frac{1}{r}-\frac{1}{s}\right)+\left(\frac{c^2}{v}-\frac{c^2}{u}\right)\left(\frac{1}{v}+\frac{1}{u}+\frac{1}{cs}\right).$$

On substitution for $\frac{c^2}{v}-\frac{c^2}{u}$ of its approximate value from (8), $\frac{1}{r}-\frac{1}{s}$ becomes a factor of the whole expression, and we get

$$\left(\frac{1}{r}-\frac{1}{s}\right)\left\{\frac{c'}{u'}-\frac{c}{u}-(\mu c'-c)\left(\frac{1}{v}+\frac{1}{u}+\frac{1}{cs}\right)\right\}.$$

Again, from (5) with sufficient approximation for our purpose,

$$\frac{c'}{u'}-\frac{c}{u}=\frac{c'}{u'}-\frac{\mu}{c}\left(\frac{c'}{r}+\frac{c'^2}{u'}\right)+\frac{1}{r}=-(\mu c'-c)\frac{c'}{cu'}-(\mu c'-c)\frac{1}{cr};$$

so that the bracket in (8) assumes the form

$$-(\mu c'-c)\left(\frac{1}{r}-\frac{1}{s}\right)\left\{\frac{c'}{cu'}+\frac{1}{v}+\frac{1}{u}+\frac{1}{cr}+\frac{1}{cs}\right\}. \quad . \quad (9)$$

From (6) and (7),

$$2\frac{\mu c'^2}{u'}=c^2\left(\frac{1}{u}+\frac{1}{v}\right)-(\mu c'-c)\left(\frac{1}{r}+\frac{1}{s}\right). \quad . \quad . \quad (10)$$

Using this in (9), we get

$$-\frac{\mu c'-c}{2\mu cc'}\left(\frac{1}{r}-\frac{1}{s}\right)\left\{c(2\mu c'+c)\left(\frac{1}{u}+\frac{1}{v}\right)+(\mu c'+c)\left(\frac{1}{r}+\frac{1}{s}\right)\right\},$$

so that

$$\frac{c^2}{u}-\frac{c^2}{v}=(\mu c'-c)\left(\frac{1}{r}-\frac{1}{s}\right)\left[1+\frac{3y\sin\phi}{4cc'\mu}\left\{c(2\mu c'+c)\left(\frac{1}{u}+\frac{1}{v}\right)\right.\right.$$
$$\left.\left.+(\mu c'+c)\left(\frac{1}{r}+\frac{1}{s}\right)\right\}\right]. \quad . \quad (11)$$

From this we see at once that in the case of an equiconvex or equiconcave lens, for which $\frac{1}{r}+\frac{1}{s}=0$, the aberration vanishes if $\frac{1}{u}+\frac{1}{v}=0$, *i. e.* if the primary focus be at the same distance on one side of the lens as the radiant point is on the other.

For some purposes a more convenient expression may be obtained by substituting for v its approximate value. Writing μ' for $\frac{\mu c'}{c}$, we get

$$\frac{c^2}{u}-\frac{c^2}{v}=(\mu c'-c)\left(\frac{1}{r}-\frac{1}{s}\right)\left[1+\frac{3y\sin\phi}{2\mu c'}\left\{\frac{(2\mu'+1)c}{u}+\frac{\mu'^2}{s}\right.\right.$$
$$\left.\left.+\frac{\mu'-\mu'^2+1}{r}\right\}\right]. \quad . \quad (12)$$

If the incident rays be parallel, $u=\infty$, and the aberration vanishes when

$$\frac{s}{r}=-\frac{\mu'^2}{\mu'-\mu'^2+1}=-\frac{\mu^2\cos^2\phi'}{\mu\cos\phi'\cos\phi-\mu^2\cos^2\phi'+\cos^2\phi}. \quad (13)$$

If the second surface be plane ($s=\infty$), this condition is

$$\mu'-\mu'^2+1=0,$$

or

$$\mu'=\tfrac{1}{2}(1+\sqrt{5})=1{\cdot}62.$$

For a refractive index $\mu=1{\cdot}5$, the value of ϕ which makes $\mu'=1{\cdot}62$ is about 29°. This is the obliquity at which a plano-convex lens of plate glass must be held in order to give a thin primary image. A more refractive lens must be held at a less obliquity. If the refractive index exceed 1·62, there is no position of the lens for which the image is free from aberration.

From the above formulæ there is no difficulty in calculating the aberration due to any combination of lenses. As an example I will take the case of two lenses of equal focal length, inclined at equal angles but in opposite directions to the axis of the pencil. Denoting the radii of the second lens by r' and s', the final focal length v' is to be found from (11) in combination with

$$\frac{c^2}{v}-\frac{c^2}{v'}=(\mu c'-c)\left(\frac{1}{r}-\frac{1}{s}\right)\left[1-\frac{3y\sin\phi}{4\mu c'c}\left\{c(2\mu c'+c)\left(\frac{1}{v}+\frac{1}{v'}\right)\right.\right.$$
$$\left.\left.+(\mu c'+c)\left(\frac{1}{r'}+\frac{1}{s'}\right)\right\}\right].$$

By addition,

$$\frac{c^2}{u}-\frac{c^2}{v'}=(\mu c'-v)\left(\frac{1}{r}-\frac{1}{s}\right)\left[2+\frac{3y\sin\phi}{4\mu cc'}\left\{c(2\mu c'+c)\left(\frac{1}{u}-\frac{1}{v'}\right)\right.\right.$$
$$\left.\left.+(\mu c'+c)\left(\frac{1}{r}+\frac{1}{s}-\frac{1}{r'}-\frac{1}{s'}\right)\right\}\right]. \quad . \quad (14)$$

If the lenses be of the same curvatures and be similarly turned,

$r'=r$, $s'=s$. The aberration then varies as $\frac{1}{u}-\frac{1}{v'}$, and cannot vanish.

If the lenses be of the same curvature and be turned opposite ways (fig. 18), $r'=-s$, $s'=-r$. The aberration is proportional to

Fig. 18.

$$c(2\mu c'+c)\left(\frac{1}{u}-\frac{1}{v'}\right)+2(\mu c'+c)\left(\frac{1}{r}+\frac{1}{s}\right);$$

or, on substitution of the approximate value of $\frac{1}{u}-\frac{1}{v'}$,

$$4c\left\{\frac{\mu'^2}{r}+\frac{1-\mu'^2+\mu'}{s}\right\}, \quad . \quad . \quad . \quad . \quad (15)$$

where, as before,

$$\mu'=\frac{\mu\cos\phi'}{\cos\phi}.$$

Thus the combination is aplanatic, independently of the value of u, if

$$\frac{s}{r}=-\frac{1-\mu'^2+\mu'}{\mu'^2}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (16)$$

By supposing the light to be parallel between the lenses, we obtain (13) as a particular case of (16).

We will next investigate the aberration in the case of a prism with flat faces on which divergent or convergent light falls at the angle of minimum deviation.

From (4), with notation similar to that employed above,

$$\frac{c^2}{u}\left(1-\frac{3y\sin\phi}{2u}\right)=\mu\frac{c'^2}{u'}\left(1-\frac{3y\sin\phi'}{2u'}\right), \quad . \quad . \quad . \quad (17)$$

$$\mu\frac{c'^2}{u'+l}\left(1+\frac{3y'\sin\phi'}{2(u'+l)}\right)=\frac{c^2}{v}\left(1+\frac{3y'\sin\phi}{2v}\right), \quad . \quad (18)$$

and

$$y':y=u'+l:u', \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (19)$$

l denoting the length of the path of the axis of the pencil within the prism.

If y be neglected,

$$\frac{c^2}{u}=\frac{\mu c'^2}{u'}, \qquad \frac{\mu c'^2}{u'+l}=\frac{c^2}{v}, \quad . \quad . \quad . \quad . \quad . \quad (20)$$

whence

$$\frac{v}{u}=\frac{u'+l}{u'}; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (21)$$

so that

$$\frac{y'}{v}=\frac{u'+l}{u'}\frac{y}{v}=\frac{y}{u}. \qquad (22)$$

Accordingly (18) becomes

$$\mu\frac{c'^2}{u'+l}\left(1+\frac{3y\sin\phi}{2u'}\right)=\frac{c^2}{v}\left(1+\frac{3y\sin\phi}{2u}\right). \qquad (23)$$

From (17) and (23), by multiplication,

$$\frac{\mu^2c'^4}{u'(u'+l)}=\frac{c^4}{uv},$$

or

$$v=\frac{c^4u'(u'+l)}{\mu^2c'^4u}. \qquad (24)$$

Again, from (17),

$$u'=\frac{\mu c'^2u}{c^2}\left\{1+\frac{3y\sin\phi\,(\mu^2c'^2-c^2)}{2\mu^2c'^2u}\right\}$$

$$=\frac{\mu c'^2u}{c^2}\left\{1+\frac{3y\sin\phi\,(\mu^2-1)}{2\mu^2c'^2u}\right\}. \qquad (25)$$

From (24) and (25),

$$v=u+\frac{c^2l}{\mu c'^2}+\frac{3y\sin\phi\,(\mu^2-1)}{\mu^2c'^2}\left(1+\frac{c^2l}{2\mu c'^2u}\right). \qquad (26)$$

In most cases $l\div u$ is negligible in the small term, and the longitudinal aberration is simply

$$\delta v=\frac{3y\sin\phi\,(\mu^2-1)}{\mu^2c'^2}. \qquad (27)$$

The linear width of the image is $\delta v\,.\,\theta$, where θ denotes the angular semi-aperture; and the angular width of the image is $\frac{\delta v}{u}\,\theta$, or, since $\frac{y\cos\phi}{u}=\theta$,

$$\frac{3(\mu^2-1)\sin\phi\,\theta^2}{\mu^2cc'^2}. \qquad (28)$$

To apply this result to the spectroscope, it is convenient to compare (28) with the angular deviation due to an alteration of refractive index from μ to $\mu+\delta\mu$. This is equal to

$$\frac{2\delta\mu\sin\phi'}{\cos\phi}=\frac{2\delta\mu\sin\phi}{\mu\cos\phi}. \qquad (29)$$

If (28) and (29) be equal,

$$\delta\mu=\frac{3\theta^2(\mu-\mu^{-1})}{2\cos^2\phi'}. \qquad (30)$$

which determines the degree of confusion of refrangibilities produced by the aberration.

For a glass prism, $\mu-\mu^{-1}$ differs very little from unity. If the refracting angle be 60°, $\cos^2\phi'=\frac{3}{4}$, and thus

$$\delta\mu=2\theta^2. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (31)$$

By (8), (10), § 4, the value of $\delta\mu$ for the soda-lines in the case of a prism of "extra-dense glass" is $5{\cdot}8\times10^{-5}$. Hence, if the image of one soda-line just touch the image of the other, $\theta={\cdot}0054$, or the distance u must be about 100 times the width of the beam where it falls upon the prism. With small instruments, whose whole resolving-power is only a small multiple of that necessary to resolve the soda-lines, it is possible to work without a collimator by allowing a distance of 10 or 20 feet between the slit and the prisms; but in the case of powerful instruments a pretty accurate adjustment of the collimator is indispensable.

The next thing to consider is the effect of curvature in the faces of prisms. We shall neglect l, the length of path within the prism, as small in comparison with the other distances concerned; its influence is doubtless no more important than when the faces are flat. From (4),

$$\left(\frac{c}{r}+\frac{c^2}{u}\right)\left(1-\frac{3y\sin\phi}{2u}\right)=\mu\left(\frac{c'}{r}+\frac{c'^2}{u'}\right)\left(1-\frac{3y\sin\phi'}{2u'}\right), \quad . \quad (32)$$

$$\mu\left(\frac{c'}{s}+\frac{c'^2}{u'}\right)\left(1+\frac{3y\sin\phi'}{2u'}\right)=\left(\frac{c}{s}+\frac{c^2}{v}\right)\left(1+\frac{3y\sin\phi}{2v}\right), \quad . \quad (33)$$

giving by subtraction

$$\left(\frac{c}{r}+\frac{c^2}{u}\right)\left(1-\frac{3y\sin\phi}{2u}\right)-\left(\frac{c}{s}+\frac{c^2}{v}\right)\left(1+\frac{3y\sin\phi}{2v}\right)$$
$$=\mu c'\left(\frac{1}{r}-\frac{1}{s}\right)-\frac{3y\sin\phi}{2u'}\left(\frac{c'}{r}+\frac{c'}{s}+\frac{2c'^2}{u'}\right).$$

Hence

$$\frac{c^2}{u}-\frac{c^2}{v}=(\mu c'-c)\left(\frac{1}{r}-\frac{1}{s}\right)-\frac{3y\sin\phi}{2}\left\{\frac{1}{r}\left(\frac{c'}{u'}-\frac{c}{u}\right)\right.$$
$$\left.+\frac{1}{s}\left(\frac{c'}{u'}-\frac{c}{v}\right)+\frac{2c'^2}{u'^2}-\frac{c^2}{u^2}-\frac{c^2}{v^2}\right\}$$
$$=(\mu c'-c)\left(\frac{1}{r}-\frac{1}{s}\right)-\frac{3y\sin\phi}{2}\left\{\left(\frac{c'}{u'}-\frac{c}{u}\right)\left(\frac{1}{r}+\frac{c'}{u'}+\frac{c}{u}\right)\right.$$
$$\left.+\left(\frac{c'}{u'}-\frac{c}{v}\right)\left(\frac{1}{s}+\frac{c'}{u'}+\frac{c}{v}\right)\right\}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (34$$

The condition that there shall be no aberration requires that the quantity within brackets vanish. In order to discuss it further it will be convenient to express u and v in terms of u'. By the original equations, y being neglected,

$$\frac{c}{u}=\frac{\mu c'^2}{cu'}+\frac{\mu c'-c}{cr}=\frac{\mu' c'}{u'}+\frac{\mu'-1}{r}, \quad . \quad . \quad . \quad (35)$$

$$\frac{c}{v}=\frac{\mu c'^2}{cu'}+\frac{\mu c'-c}{cs}=\frac{\mu' c'}{u'}+\frac{\mu'-1}{s}, \quad . \quad . \quad . \quad (36)$$

μ' being written as before for $\frac{\mu c'}{c}$.

By substitution of these values in (4) the condition becomes

$$\left(\frac{c'}{u'}+\frac{1}{r}\right)\left\{(\mu'+1)\frac{c'}{u'}+\frac{\mu'}{r}\right\}$$
$$+\left(\frac{c'}{u'}+\frac{1}{s}\right)\left\{(\mu'+1)\frac{c'}{u'}+\frac{\mu'}{s}\right\}=0. \quad . \quad (37)$$

This condition assumes a simplified form when $r=s$, i. e. when one face is as much convex as the other face is concave; it is satisfied either by

$$\frac{c'}{u'}+\frac{1}{r}=0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (38)$$

or by

$$(\mu'+1)\frac{c'}{u'}+\frac{\mu'}{r}=0. \quad . \quad . \quad . \quad . \quad (39)$$

In the first case, by (35) $u=-cr$; so that if the first face be convex the incident rays must be convergent. In the second case,

$$u=-c(\mu'+1)r=-(\mu\cos\phi'+\cos\phi)r.$$

With general values of r and s, (37) may be written

$$2(\mu'+1)\frac{c'^2}{u'^2}+(2\mu'+1)\left(\frac{1}{r}+\frac{1}{s}\right)\frac{c'}{u'}+\mu'\left(\frac{1}{r^2}+\frac{1}{s^2}\right)=0, \quad . \quad (40)$$

which determines the values of u' (if any) for which the aberration vanishes. The roots of (40) are real; and therefore aplanaticism is possible if

$$\frac{2(2\mu'+1)^2}{rs}-(4\mu'^2+4\mu'-1)\left(\frac{1}{r^2}+\frac{1}{s^2}\right) \quad . \quad . \quad (41)$$

be positive. Unless both faces are flat, one must be convex and the other concave. The limits within which a suitable value of u' will secure aplanaticism are found by equating (41) to zero. The roots are reciprocals, and are given by

$$\frac{s}{r}=\frac{(2\mu'+1)^2\pm4\sqrt{(\mu'^2+\mu')}}{4\mu'^2+4\mu'-1}. \quad . \quad . \quad . \quad . \qquad (42)$$

If the actual value of $s:r$ be nearer unity than the values determined by (42), the aberration may be destroyed by selecting for u' one of the roots of (40).

If, for example, $\mu=1{\cdot}62$, and the refracting angle be 60°, $\mu'=2{\cdot}392$,

$$\frac{s}{r}=1{\cdot}43 \text{ or } (1{\cdot}43)^{-1}.$$

Unless, therefore, the curvatures are opposite and pretty nearly equal, no adjustment of the focus of the collimator can destroy the aberration.

In any optical instrument whatever, the aberration in the primary plane may be eliminated by sloping one of the lenses to a suitable angle; but, as was shown in the preceding section, we have also in many cases to contend with the aberration of the rays in the secondary plane. The latter aberration is more difficult to calculate than the former. Among lenses, the only case that I have investigated is that of a plano-convex lens on the curved side of which parallel light falls. It appears that the aberration of the rays in the secondary plane vanishes when the following relation obtains between the refractive index and the obliquity:

$$\sin^2\phi=\frac{3\mu^2-\mu^4-1}{3-\mu^2}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (43)$$

The obliquity is zero when $\mu=1{\cdot}62$—the same value of μ that gives no aberration in the primary plane for rays of small obliquity. Neither kind of aberration is important for a glass lens of this sort moderately sloped. If $\mu=1{\cdot}5$, (43) gives $\phi=73°$. If μ exceed 1·62, (43) cannot be satisfied.

Another case of considerable interest can be investigated more easily. It is that of a prism with flat sides, through which convergent or divergent light passes. The prism is supposed to be adjusted until the symmetrical (horizontal) plane contains the radiant point; but it need not be in the position of minimum deviation. The problem depends upon the same principles as are applied by Professor Stokes* to investigate the curvature of the image of a straight line when seen through a prism. If θ denote the altitude of a ray before it falls upon the prism, θ' the altitude of the ray within the prism, the horizontal projection of the ray follows a course

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* Royal-Society Proceedings, April 30, 1874.

which would be that of an actual ray if the refractive index were changed from μ to $\mu\frac{\cos\theta'}{\cos\theta}$. Since $\sin\theta'=\mu^{-1}\sin\theta$, and θ, θ' are supposed to be small, the virtual change in the refractive index is from μ to $\mu+\frac{1}{2}(\mu-\mu^{-1})\theta^2$. Thus, if $\delta\mu$ represent the range of refrangibilities confused in the resulting spectrum of an infinitely thin slit,

$$\delta\mu=\tfrac{1}{2}\left(\mu-\frac{1}{\mu}\right)\theta^2. \quad . \quad . \quad . \quad . \quad . \quad . \qquad (44)$$

The factor $\mu-\mu^{-1}$ is equal to unity when $\mu=1{\cdot}62$, and for glass will never differ much from unity. By comparison with (31), it would appear that in the case of a 60° prism in the position of minimum deviation, the admissible vertical angular aperture is twice as great as the admissible horizontal angular aperture; but, on account of the variable distribution of light in the image, this conclusion probably requires modification in favour of the horizontal aperture.

If there is to be no confusion of the two soda-lines when seen with a prism of extra-dense glass one inch high, the distance of the radiant points must exceed four feet.

Equation (44) is applicable without change to a spectroscope of any number of properly adjusted prisms of similar material.

§ 8. *The Design of Spectroscopes.*

The circumstances under which spectroscopes are used are so various that it is probably impossible to lay down any one construction as absolutely the best; but the principles of the foregoing sections allow us to impose certain limitations within which the choice of the designer must be confined. The first point to be considered is the resolving-power. This, in the case of prismatic instruments of one given material, carries with it the total thickness traversed; and the question is simply in what form it is most advantageous to employ this thickness. The other points to be attended to are principally illumination, quantity of material, ready applicability to various parts of the spectrum, simplicity and ease of construction.

To a certain extent the requirements of illumination and resolving-power are antagonistic. If, indeed, the eye were a perfect instrument, a beam of diameter equal to that of the pupil would present the full degree of brightness, and a resolving-power corresponding to the thickness employed. But, as was explained in § 3 (vol. viii. p. 272), in order to obtain the full value of the thickness it is necessary further to narrow the beam; and then illumination suffers. If full illumination be required, we must employ a thickness three or four times as great as that defined by (5), § 3.

Apart from the question of the area of the pupil occupied by the beam, illumination suffers from the effects of absorption and reflection. The first depends simply upon the thickness traversed, and is therefore an invariable quantity when the material and resolving-power are given. Some years since it was laid down by Pickering* that in spectroscopes composed of prisms of the same material and in the position of minimum deviation (which disperse equally and admit the same amount of light) the loss by absorption will be the same. In accordance with what has just been proved, we are now able to dispense with the restriction to minimum deviation.

In powerful spectroscopes the transparency of the material of which the prisms are made is a point of great importance. Some specimens even of well-made flint and crown glass examined by Christie † stopped as much as half the light in a thickness of 4 inches. Such a degree of absorption renders the glass unsuited for instruments of more than moderate power. From measurements by Robinson and Grubb ‡, however, it would seem that absorption need not stand in the way of much more powerful instruments than any yet attempted. One specimen of Chance's glass was of such transparency that 111 inches would be necessary to reduce the transmitted light in the ratio 2·7 to 1.

The loss of light by reflection depends upon the number of surfaces and upon the angles at which the rays are incident. It might be thought that a great multiplication of surfaces was necessarily very unfavourable to brightness; but, as has been pointed out by Pickering in the paper referred to, this difficulty may be overcome by using prisms of such angle that the reflected light is perfectly polarized. Under these circumstances *half* the light at least escapes reflection; and the necessary angles (64° for ordinary flint glass) are not otherwise objectionable. The least loss of light is incurred when the whole thickness is thrown into one prism of moderate angle; but the gain in brightness would rarely compensate for the other disadvantages of such a construction.

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* "On the Comparative Efficiency of Different Forms of the Spectroscope," Phil. Mag. vol. xxxvi. p. 41, 1868.

† "On the Magnifying-power of the Half-prism as a Means of obtaining great Dispersion, and on the general Theory of the Half-prism Spectroscope," Proceedings of the Royal Society, March 1, 1877.

‡ "Description of the Great Melbourne Telescope," Phil. Trans. 1869, p. 160.

With regard to the material for prisms, the choice lies principally between various kinds of glass and fluids. It is not without difficulty that glass is prepared free from striæ and well annealed; but solid prisms have the great advantage that, when once good ones are obtained, there is no further trouble. In consequence probably of the practice of using in all cases a standard angle of 60°, an exaggerated idea is often entertained of the advantage of great density. According to Hopkinson, the difference of indices relative to the lines D and B for dense flint, extra-dense flint, and double-extra-dense flint are respectively ·0067, ·0075, and ·0091, which numbers are inversely as the equivalent thicknesses.

Of fluids, bisulphide of carbon has the merits of cheapness and very high dispersive power, the difference of indices for D and B being ·0126. When pure, it is also in a high degree transparent. On the other hand, its sensitiveness to variations of temperature is so great that 1° C. makes about nine times as great a change of refrangibility as a passage from D_1 to D_2. Great precautions are therefore required to prevent inequalities of temperature from destroying definition; and frequent shaking is generally necessary. Some observers have thought that, apart from inequalities of temperature, ordinary bisulphide of carbon tends to arrange itself in strata of different refracting-power; but this does not seem to be established satisfactorily. In some recent experiments with a rotating stirrer, introduced with the view of promoting uniformity of temperature, I obtained evidence of a thin layer of moisture floating on the surface. Under the action of the stirrer this layer was broken up and the liquid rendered very irregular. A few lumps of chloride of calcium introduced into the liquid absorbed most of the layer; but an arrangement such that the free surface remained undisturbed would have been preferable.

Within the last few months Prof. Liveing[*] has proposed the use of a solution of iodide of mercury, which is considerably more dispersive than even bisulphide of carbon, the difference of indices between D and B amounting to ·017. This liquid is of a yellowish colour, and is hardly sufficiently transparent, even at the lower end of the spectrum, to make its use advisable in very powerful instruments. But for some purposes its great dispersion is an important recommendation. Using a single prism of 60° with an available thickness of $1\frac{1}{2}$ inch, I have obtained results which many pretentious instruments could not surpass.

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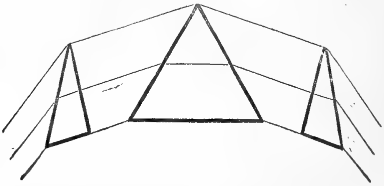
[*] "On the Dispersion of a Solution of Mercuric Iodide," Cambridge Proceedings, May 19, 1879.

Experience has not yet decided the question as to the relative advantages of large and small prisms. As generally used, large prisms have the disadvantages of requiring a greater quantity of material for a given thickness and of involving cumbrous and more expensive telescopes. The first might be avoided partially, and the second wholly, by the use of higher refracting angles, or (perhaps preferably) by the addition of half-prisms to the ends of the train (fig. 19). That prisms may act as cylindrical telescopes was observed many years ago by Brewster*; and recently Christie has constructed half-prism spectroscopes in which this property is taken advantage of. In these instruments, however, the total thickness of glass is too small for high resolving-power.

Fig. 19.



In the arrangement of fig. 19 the rays from the collimator are received on *diminishing* half-prisms, by which the width of the beam is increased up to the point suitable for the big prisms. Afterwards the rays are concentrated by *magnifying* half-prisms until the width of the beam is the same as at first. On this plan the larger prisms need be no *higher* than small ones; and the quantity of glass corresponding to a given total thickness varies as the first power, instead of as the square, of the linear dimension.

I have experimented with a pair of 60° prisms, 3 inches thick and only $\frac{3}{4}$ inch high. The glass is of rather low density; so that when the position of minimum deviation is adopted, the emergent beam is inconveniently wide. With this material a larger angle would have been preferable; but much the same result may be arrived at by turning the prisms a little, so as to increase the angle of incidence on the first surface and the angle of emergence from the last surface†. In this way the incident and emergent beams are so far narrowed that they can be received on small telescopes; and the combination is very economical in comparison with one in which the position of minimum deviation is adopted,

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* 'Optics' (London, 1853), p. 513.

† A pair of prisms thus arranged is called by Thollon a *couple*.

involving, as it would, larger telescopes, and, unless a loss of brightness were accepted, higher prisms.

Another point requiring attention in the discussion of the question of large *versus* small prisms is the relative difficulty of securing the necessary accuracy of surfaces in the two cases; but it can hardly be treated satisfactorily *à priori.* It would, however, seem that small prisms have the advantage in this respect also.

So far I have supposed the prisms to be simple. Of late years compound prisms of flint and crown have come largely into fashion; and quite recently M. Thollon has revived the use of compound prisms of bisulphide of carbon. Mr. Christie considers that compound prisms are, on the whole, to be preferred; but I cannot help thinking that, except where direct vision is important, their advantages have been overrated. The difference of indices in the case of crown glass for the rays D and B is ·0035, which is quite sufficiently high to come into comparison with the corresponding numbers for flint and bisulphide of carbon. In a direct-vision prism the thickness of the crown is greater than that of the flint; so that the loss of efficiency is even more serious than a mere comparison of the dispersions would suggest. See (6) § 4.

The principle of the compound prism is carried to its limit by employing media of *equal* refracting-power for the part of the spectrum under examination. For this purpose I chose bisulphide of carbon and flint glass. With Chance's "dense flint" the refractions are the same, and the difference of dispersions relative to D and B is ·0056, *i. e.* about as great as the difference for "double-extra-dense flint" and crown. A dozen glass prisms of 90° were cemented in a straight line on a strip of glass and immersed in bisulphide of carbon. The liquid is contained in a tube closed at its ends with glass plates. The "thickness" of each prism is 1 inch and the height about $\frac{3}{4}$ inch, so that the total thickness is 12 inches. The character of the glass is such that at ordinary temperatures the red rays pass without deviation. To observe other parts of the spectrum with advantage, it is necessary to mix a little ether with the bisulphide of carbon in order to lower its power. A similar result could be obtained by slightly warming the tube; but this course is not to be recommended.

The instrument, which was made by Hilger, gave excellent results at times, but showed the usual caprice of bisulphide of carbon. With the refracting edges vertical, the definition was usually good for twenty or thirty seconds after shaking up, but would often rapidly deteriorate afterwards. Although care was taken not to touch the tube with the hands, this effect was sometimes so persistent that I began to think I had evidence of a tendency to separate into distinct layers. When the edges of the prisms (and slit) were horizontal, the tube being also horizontal as before, the loss of definition after shaking still occurred, but could be remedied in great measure by a change of focus. Further experience, however, led me to attribute these effects to temperature-differences, caused perhaps by the ceiling of the laboratory being warmer than the floor. At any rate, they were greatly mitigated by wrapping round the tube strips of copper and numerous folds of cloth; and they could be produced with considerable persistency by touching the top of the naked tube for a few seconds with the hands. A difference of even $\frac{1}{100}$ of a degree Cent. between the upper and lower halves of the prisms might be expected to make itself apparent when the edges are vertical. The advantages of this construction are the elimination of reflection and the almost absolute immunity from defects due to errors in the separating surfaces; but they are rather dearly purchased. As might be expected, the best results as to definition are obtained when the tube is vertical; but such an arrangement is inconvenient, as it involves the aid of reflectors. Spectroscopes on this plan may perhaps be useful for special purposes; but the want of ready adaptability to different parts of the spectrum is a serious objection.

The general result of this discussion would seem to be in favour of a spectroscope with simple glass prisms of such angle that the reflected light is wholly polarized, the number of prisms being increased up to the point at which mechanical difficulties begin to interfere. With the aid of reflection, at least six prisms may be used twice over. When it becomes necessary to increase the size of the prisms, considerable economy may be effected by the introduction of half-prisms at the ends of the train, as already explained.

When the surfaces are not quite flat, or when the focus of the collimator is imperfectly adjusted, it becomes important to secure a rather exact perpendicularity between the edges of the prisms and a plane passing through their middle points; otherwise the linear image of a point may not be parallel to the length of the slit. Even when the plane of symmetry is obtained, there may remain considerable errors, due to *curvature* of the image, dependent upon the quantity denoted by γ in § 6. Much information as to the cause of bad definition may often be obtained by replacing the slit by a simple hole and examining the character of the resultant image. In many instruments a great improvement is effected by narrowing the vertical aperture whenever the light will bear diminution. Such a result may be anticipated when with full aperture the top and bottom of the spectrum appear better defined than the central parts. The principal cause of error is probably a deviation of the extreme rays from horizontality in their passage through the prisms, due either to imperfect action of the collimator or to the curved faces of preceding prisms.

It is not easy to decide whether the highest resolving-power is more likely to be obtained by gratings or by prisms. Up to a certain point the resolving-power of gratings is principally a question of the *accuracy* with which the lines can be ruled. If the deviations of the lines from their proper places do not exceed (say) one fourth of the interval between consecutive lines, the definition in the first spectrum will not be materially injured. To obtain corresponding results in the 2nd, 3rd, 4th, . . . spectrum, the errors must not exceed $\frac{1}{8}$, $\frac{1}{12}$, $\frac{1}{16}$, . . . respectively of the same interval. Every effort should also be made to rule as great a number of lines as possible, even if it be necessary for this purpose to reduce their length.

I have lately had the opportunity of experimenting with two Rutherfurd gratings, one on glass, with nearly 12,000 lines (a present from Mr. Rutherfurd himself), the other on speculum-metal, with nearly 14,000 lines (kindly lent me by Mr. Spottiswoode). The lines are at the rate of 17,296 to the inch. Both these gratings give admirable results in the second spectrum, where, according to (14) § 4, the resolving-power in the orange exceeds that obtainable from 25 centims. of extra-dense flint. In the third spectrum the gain of resolving-power is still apparent, but illumination is rather deficient.

It is much to be wished that spectroscopists in possession of powerful instruments would compare their actual performances with those of which they are theoretically capable. A carefully arranged succession of tests of gradually increasing difficulty, like those applied to telescopes, would be of especial value. In my own observations I have usually attended principally to the definition of the fine lines bordering b_2 and to the double line b_3.

For experiments on dark heat, to which in some respects gratings are well adapted, resolving-power is secondary to illumination. In order to avoid confusion of spectra, it would be well to eliminate the second spectrum altogether, which could be effected by ruling equally strong lines at alternate intervals in the ratio of one to three.

Terling Place, Witham,
Nov. 8, 1879.

VI. *Note on a Method of Checking Calculations.*

By W. H. Walenn[*].

THE method of checking calculations by casting out the nines has only been utilized to a limited extent—principally in schools, to check multiplications and divisions of considerable length. The fact does not seem to have been realized by teachers, although it has long been known to mathematicians, that every operation in natural numbers has a corresponding operation in the remainders to any divisor of those natural numbers, which may be used to check a calculation in many cases from beginning to end without appeal to the original numbers, except at the beginning and the end.

Moreover the application of these remainders is never made, by teachers, to decimal calculations. This is easily understood, because the change in the condition of the remainder (when so used) is not realized by them. When employed to check calculations, the remainder is no longer in the condition implied in the equation $\frac{a}{b}=c+\frac{r}{b}$, but it is simply the number of units by which the number is in excess of being exactly divisible by nine. Therefore the checking process is as applicable to all terminable decimals as it is to ordinary numbers.

If, for instance, it be required to ascertain the weight of water in a tank which is 2·375 feet high, 3·25 feet wide, and 3·75 feet broad, the formula may be written $w=62{\cdot}32\,abc$, and the state of the question is $w=62{\cdot}32\,.\,2{\cdot}375\,.\,3{\cdot}25\,.\,3{\cdot}75$ in pounds, the result being $w=1803{\cdot}871875$ lbs. The definition of a unitate being the number of units by which the corresponding number is in excess of being exactly divisible by a given number (nine in this instance), the operation of checking the calculation by which w was found may be symbolized as follows:—$\mathrm{U}_9w=\mathrm{U}_9(4\,.\,8\,.\,1\,.\,6)=3$. That the above value of w is correct appears because $\mathrm{U}_9 1803{\cdot}871875=3$. It will be found that the formulation of the process to be effected by unitation, as a check upon a main calculation, is of great service.

Not only is it possible to check calculations involving direct operations, but it is also possible to check inverse operations by unitation. In checking subtraction, it must be remembered that a remainder is not really altered by the addition of the divisor, for instance $\mathrm{U}_9 3=\mathrm{U}_9 12$; subtraction may then take

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[*] Communicated by the Author, having been read before the British Association, Section A, August 23, 1879.

place in the ordinary manner, even when the unitate of the subtrahend is greater than that of the minuend. The dealing with fractions is more difficult if our unitates are always to be whole numbers; but this need not be the case if we take care to preserve the fractional form throughout. The unitates of the form $U_9\frac{1}{x}$, however, have $U_9\frac{1}{2}=5$, $U_9\frac{1}{4}=7$, $U_9\frac{1}{5}=2$, $U_9\frac{1}{7}=4$, and $U_9\frac{1}{8}=8$: these may be formed into the following Table:—

$\frac{1}{x}$.	$U_9\frac{1}{x}$.
$\frac{1}{2}$	5
$\frac{1}{4}$	7
$\frac{1}{5}$	2
$\frac{1}{7}$	4
$\frac{1}{8}$	8

This leaves $U_9\frac{1}{3}$, $U_9\frac{1}{6}$, and $U_9\frac{1}{9}$ still fractional because irreducible; and the cases in which they occur must be allowed to retain their fractional form throughout.

The probability of correctness in the checking by U_9N (N being the number that furnishes the unitate or remainder), is interfered with by the following peculiarities:—1. There is no check upon the number of digits in N. 2. Nor upon the place of any particular figure in N. 3. Nor upon the figures themselves, if they be either 9 or 0, or if their sum $=9$ or any multiple of 9.

Moreover, as evident above, the number of reciprocals or fractions which can be expressed in whole numbers by U_9N is limited to $66{\cdot}\dot{6}$ per cent. In 100 there are 33 fractional unitates to reciprocals.

If the function $U_{11}N$ be made to do duty for U_9N (that is, if we cast out the elevens instead of the nines), we gain the following advantages:—1. There is a check upon the number of digits in N; for they must be either odd or even, and the addition of a digit to, or its elision from, N, alters the value of $U_{11}N$. This results from the method of determining $U_{11}N$ by unitation; for the formula is

$$U_{11}N = \ldots\ldots + a_5 - a_4 + a_3 - a_2 + a,$$

$a_1, a_2, a_3, \ldots\ldots$ being the digits in N. 2. There is a check upon the place of any figure, so far as to its being in an even or odd place. 3. There is a check upon the figures themselves, except when their sum (in the odd places or in the even places) $=11$ or any multiple of 11.

One eleventh, only, of reciprocals or fractions are unable to be expressed by whole-number unitates. In 100 there are 9 fractional unitates to reciprocals.

Therefore the method of checking calculations that this paper advocates is the combination of the formulation of work and of results with casting out the elevens, in preference to casting out the nines.

The shortest way to cast out the elevens is to add together all the digits in the odd places of the given number to form one sum total, and all the digits in the even places to form another sum total, then to subtract the latter sum from the former. In the process of obtaining each sum total, as soon as any number higher than 11 is obtained its unitate should be substituted. In obtaining the unitate to a decimal, it must be remembered to take all the alternate digits from the unit's digit as additive.

In checking additions, the one operation only need be used, and that without putting pen to paper. All the digits in the odd places may be added together, reducing from time to time, from which sum the sum of the digits in the even places, similarly obtained, is subtracted.

In checking subtractions, it is only necessary to be sure that the unitate of the subtrahend is less than that of the minuend; this is easily done, if necessary, by the addition of eleven to the unitate of the minuend.

Multiplications only require the reduction of the unitate of the product to a number less than 11. In multiplications into which decimals enter, the operation must be without contraction.

In divisions, the division may be finished at any predetermined place of decimals, taking into account the fraction at the end which includes the remainder. The fractions are dealt with (as if of the form $a \cdot \frac{1}{b}$) by the table of unitates of $\mathrm{U}_{11}\mathrm{N}^{-1}$, which is

N^{-1}.	$U_{11}N^{-1}$.
$\frac{1}{1}$	1
$\frac{1}{2}$	6
$\frac{1}{3}$	4
$\frac{1}{4}$	3
$\frac{1}{5}$	9
$\frac{1}{6}$	2
$\frac{1}{7}$	8
$\frac{1}{8}$	7
$\frac{1}{9}$	5
$\frac{1}{10}$	10
$\frac{1}{11}$	$\frac{1}{11}$

The table of powers of $U_{11}N$ repeats after every ten powers ; values of the form a^m must therefore have a unitated to the base 11 and m to the base 10 as a preliminary operation to $U_{11}N$. The unitation to the base 10 is accomplished simply by striking off the last digit of m, except when it is 0, when $U_{10}m=10$.

By the above means all calculations not involving surd roots can be checked with much greater ease and certainty than by casting out the nines.

VII. *On the Graduation of the Sonometer. By* J. H. POYNTING, *M.A., Fellow of Trinity College, Cambridge*[*].

IT seems likely that such valuable results will be obtained by means of Professor Hughes's sonometer, that it is desirable that some method should be employed to turn its at present arbitrary readings into absolute measure, so that, for instance, the induced currents caused by different metals in the induction-balance may be measured and compared with each other.

In Maxwell's 'Electricity,' vol. ii. chap. xiv., the general formula is given for the coefficient of induction of one circular circuit on another. Adapting this to the case where two equal

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[*] Communicated by the Physical Society, having been read on December 13, 1879.

circular circuits are on the same axis at a distance apart greater than the radius of the coils, the following formula is obtained.

Let a = distance between centres,
b = radius of either circle,
c = distance of either circumference from centre of other,
M = coefficient of induction.

Then

$$\mathrm{M} = -\frac{4\pi^2 b^4}{c^3}\left\{\frac{1}{2} - \frac{3}{4}\frac{b^2}{c^2} + \frac{15}{8}\frac{b^4}{c^4} - \frac{35}{8}\frac{b^6}{c^6} + \frac{2835}{256}\frac{b^8}{c^8} - \&\text{c.}\right. \quad (1)$$

or

$$= -\frac{4\pi^2 b^4}{a^3}\left\{\frac{1}{2} - \frac{3}{2}\frac{b^2}{a^2} + \frac{75}{16}\frac{b^4}{a^4} - \frac{490}{32}\frac{b^6}{a^6} + \frac{24570}{256}\frac{b^8}{a^8} - \&\text{c.}\right. \quad (2)$$

Of these the latter uses directly the distance between the centres, the observed quantity—but is not nearly so convergent as the former, in which c may be at once deduced from $c = \sqrt{a^2+b^2}$.

To obtain formulæ which might be strictly applied to the sonometer, we should have to consider the more general case of two coils of unequal radii b and β, for which I have found the formula corresponding to (2), viz.

$$\mathrm{M} = \frac{4\pi b^2\beta^2}{a^3}\left(\frac{1}{2} - \frac{3}{4}\frac{b^2+\beta^2}{a^2} + \frac{15}{16}\frac{b^4+3b^2\beta^2+\beta^4}{a^4}\right.$$
$$\left. - \frac{35}{32}\frac{b^6+6b^4\beta^2+6b^2\beta^4+\beta^6}{a^6} + \&\text{c.}\right) \quad (3)$$

We should then have to take the finite integrals of each term between the limiting values of b and β. But this would be exceedingly complicated and would require a knowledge of all the details of construction; and we may at least get a first approximation to the true result by replacing the coils by a single one of a radius intermediate between the greatest and least radii.

In Prof. Hughes's paper (Phil. Mag. July 1879) he gives the internal and external radii of his coils as 15 millims. and 27·5 millims. respectively. I have considered, then, that 25 millims. will give results not very far from the truth; and as it makes the calculations considerably easier, I have taken that as the value of b and applied the formulæ to the numbers given in the paper. The resultant current in the middle coil was zero when it was distant 47 millims. from one end and 200 from the other. This enables us to find the ratio between the number of turns in the two ends at least sufficiently nearly to apply to some of the results.

Let M_1 be the coefficient of induction of the larger coil on the movable one, M_2 that of the smaller, the former having m turns, the latter n. When the movable coil was 200 millims. from the large and 47 millims. from the small coil, since there was no induced current,

$$mM_1 = nM_2.$$

Applying formula (1), we have c for the larger coil

$$=\sqrt{200^2+25^2}=201{\cdot}5,$$

and for the smaller coil

$$c=\sqrt{47^2+25^2}=53{\cdot}2,$$

b being the same for both. Then

$$\frac{m}{(201{\cdot}5)^3}\left\{\frac{1}{2}-\frac{3}{4}\left(\frac{25}{201{\cdot}5}\right)^2+\frac{15}{8}\left(\frac{25}{201{\cdot}5}\right)^4-\&c.\right\}$$

$$=\frac{n}{(53{\cdot}2)^3}\left\{\frac{1}{2}-\frac{3}{4}\left(\frac{25}{53{\cdot}2}\right)^2+\frac{15}{8}\left(\frac{25}{53{\cdot}2}\right)^4-\frac{35}{8}\left(\frac{25}{53{\cdot}2}\right)^6\right.$$

$$\left.+\frac{2835}{256}\left(\frac{25}{53{\cdot}2}\right)^8-\&c.\right.$$

Multiplying each side by 2 and finding the successive terms,

$$m\times\frac{122}{10^9}\{1-{\cdot}02308+{\cdot}00088-\&c.\}$$

$$=n\times\frac{6645}{10^9}\{1-{\cdot}33123+{\cdot}18286-{\cdot}09422+{\cdot}02633,$$

or

$$\frac{m}{n}=43{\cdot}6.$$

I have applied the formula to the results for various metals given by Prof. Hughes in a table in his paper. In the table below, in the first column are Prof. Hughes's numbers, *i. e.* distances from the point of no induction. In the second are numbers proportional to mM_1-nM_2; where M_1, M_2 are the coefficients of induction of two simple coils calculated on the above hypothesis, m and n the number of turns in the two respectively. In the third column are the resistances for bars of the metal 100 millims. long and 1 millim. in diameter

(Jenkin, p. 249). In the last column are the products of the numbers in the two preceding columns.

Metal.	Distance from point of no induction.	mM_1-nM_2, proportional to	R.	$(mM_1-nM_2)R$.
Silver............	125	178	·21	37·4
Gold	117	135	·27	36·5
Aluminium...	112	116	·375	43·5
Copper	100	84	·21	17·6
Zinc	80	50·1	·72	36·1
Tin...............	74	44·6	1·70	75·8
Iron	45	22·46	1·25	28·1
Lead	38	18·87	2·5	47·2
Antimony	35	17·35	4·5	78·1
Bismuth	10	5·75	16·8	96·6

Mercury has been omitted, as it gives a very much higher value than any of the others. Were the induced currents in the induction-balance proportional to the resistances given in the table, the numbers in the last column would of course be all the same. The deviations from equality are far greater than could be accounted for by errors in the approximations I have adopted, especially for the metals not at the beginning or end of the list. Hence we are driven to conclude, either that the resistances of the metals given in the tables are not the same as the resistances of the metals used by Prof. Hughes, or that the induced current is not proportional to the conductivity of the metal.

It should be noticed that the method of measuring currents by the sonometer assumes that the telephone integrates, as it were, the current; *i. e.* the loudness of the sound depends only on the total current, not on the time during which the current is passing, provided that the time be very short. I do not know whether this point has been investigated; but if not, it would probably be easy to examine it by means of the sonometer. It would be advisable to modify the instrument in such a way that the formulæ might be more easily employed, and that the approximations might be nearer to the truth.

The formulæ used in this paper may be obtained as follows, the method being adapted from that given in Maxwell.

The potential of a circular unit current at any point is the same as that of a magnetic shell of unit strength bounded by the circuit. This, again, is the same as the attraction of a thin plate of matter of unit surface-density in a direction perpendicular to the plane of the plate. If ω be the attraction of a plate of radius b, at a point distant b from the plate along its axis,

$$\omega = 2\pi\left(1 - \frac{1}{\sqrt{1+\frac{b^2}{c^3}}}\right)$$

$$= 2\pi\left\{\frac{1}{2}\frac{b^2}{c^2} - \frac{1\,.\,3}{2\,.\,4}\frac{b}{c^4} + \frac{1\,.\,3\,.\,5}{2\,.\,4\,.\,6}\frac{b^6}{c^6} - \&c.\right\}.$$

If we introduce zonal harmonics as coefficients, this becomes

$$\omega = 2\pi\left\{\frac{1}{2}\frac{b^2}{c^2}\mathrm{P}_1 - \frac{1\,.\,3}{2\,.\,4}\frac{b^4}{c^4}\mathrm{P}_3 + \frac{1\,.\,3\,.\,5}{2\,.\,4\,.\,6}\frac{b^6}{c^6}\mathrm{P}_5 - \&c.\right\}.$$

This is now the potential at any point in space where $b < c$.

If there be a second circular circuit of radius β on the same axis, we may suppose it replaced by a magnetic shell bounded by the current and lying on the sphere, with centre at the centre of the first current, the radius of the sphere being c.

This shell may be considered to consist of two layers of matter of equal and opposite densities, μ and $-\mu$, at distances c and $c+dc$ from the centre. The potential on the second layer is

$$\iint \mu\omega d\mathrm{S},$$

where the integration is taken over the shell. The potential on the second layer is

$$-\iint \mu\left(\omega + \frac{d\omega}{dc}dc\right)d\mathrm{S},$$

the sum being

$$-\iint \mu\frac{d\omega}{dc}dc\, d\mathrm{S}\,;$$

but since the strength $=1$, $\mu dc = 1$, and we have the mutual potential

$$\mathrm{M} = -\iint \frac{d\omega}{dc}d\mathrm{S}.$$

Replacing the element $d\mathrm{S}$ by $c^2 d\mu\, d\phi$, the limits will be for ϕ from 0 to 2π, and for μ from 1 to μ.

Integrating with respect to ϕ, and remembering that c is constant in integrating for μ, we have

$$\mathrm{M} = 2\pi c^2\int_\mu^1 \frac{d\omega}{dc}d\mu$$

$$= -4\pi^2c^2\left\{\frac{b^2}{c^3}\int_\mu^1 \mathrm{P}_1 d\mu - \frac{1\,.\,3}{2\,.\,4}\,.\,4\frac{b^4}{c^5}\int_\mu^1 \mathrm{P}_3 d\mu + \&c.\right\}.$$

But we have the relation for zonal harmonics,

$$\int_\mu^1 \mathrm{P}_n d\mu = \frac{1-\mu^2}{n(n+1)} \frac{d\mathrm{P}_n}{d\mu}.$$

Substituting, we obtain

$$\mathrm{M} = -4\pi^2(1-\mu^2)\left\{ \frac{b^2}{2c}\frac{d\mathrm{P}_1}{d\mu} - \frac{1.3}{2.4}\frac{b^4}{3c^3}\frac{d\mathrm{P}_3}{d\mu} + \frac{1.3.5}{2.4.6}\frac{b^6}{5c^5}\frac{d\mathrm{P}_5}{d\mu} - \&c. \right\}.$$

The following are the values for the coefficients (Ferrer's 'Spherical Harmonics,' p. 23), both in terms of μ and when we substitute $\mu^2 = 1 - \frac{\beta^2}{c^2}$:—

$$\frac{d\mathrm{P}_1}{d\mu} = 1,$$

$$\frac{d\mathrm{P}_3}{d\mu} = \frac{3}{2}(5\mu^2 - 1) = \frac{3}{2}\left(4 - 5\frac{\beta^2}{c^2}\right),$$

$$\frac{d\mathrm{P}_5}{d\mu} = \frac{15}{8}(21\mu^4 - 14\mu^2 + 1) = \frac{15}{8}\left(8 - 28\frac{\beta^2}{c^2} + 21\frac{b^4}{c^4}\right),$$

$$\frac{d\mathrm{P}_7}{d\mu} = \frac{3003\mu^6 - 3465\mu^4 + 945\mu^2 - 35}{16} = \frac{448 - 3024\frac{\beta^2}{c^2} + \&c.}{16},$$

$$\frac{d\mathrm{P}_9}{d\mu} = \frac{109395\mu^8 - 180180\mu^6 + 90090\mu^4 - 13860\mu^3 + 315}{128}$$

$$= \frac{5760 + \&c.}{128}.$$

Substituting these values and putting $c^2 = a^2 + \beta^2$,

$$\therefore \mathrm{M} = -4\pi\frac{\beta^2 b^2}{a^3}\left\{ \frac{1}{2} - \frac{3}{4}\frac{b^2+\beta^2}{a^2} + \frac{15}{16}\frac{b^4 + 3b^2\beta^2 + \beta^4}{a^4} - \frac{35}{32}\frac{b^6 + 6b^4\beta^2 + 6b^2\beta^4 + \beta^2}{a^6} + \&c. \right\},$$

The more useful form is obtained by retaining c. If we take the two circles of equal radius (i. e. $b = \beta$), we obtain

$$\mathrm{M} = -4\pi^2\frac{b^4}{c^3}\left\{ \frac{1}{2} - \frac{3}{4}\frac{b^2}{c^2} + \frac{15}{8}\frac{b^4}{c^4} - \frac{35}{8}\frac{b^6}{c^6} + \frac{2835}{256}\frac{b^8}{c^8} - \&c. \right\}.$$

VIII. *Notices respecting New Books.*

Auroræ; their Characters and Spectra. By J. RAND CAPRON, *F.R.A.S.* E. and F. Spon, London (46 Charing Cross) and New York (446 Brown Street): 1879.

THOUGH of all visible things, both above and below, the most pure, ethereal, peaceful, yet the sometimes-seen changeful, fitful shining of the Aurora Polaris in the sky is most frequently likened by men in general, on any of its grander displays, to conflagrations of cities and war's alarms!—as when, on one of those rarer occasions on which the usually very Northern lights, extending far southwards, have been seen superbly even in Palestine, it is recorded in the second book of Maccabees (ch. 5, vv. 2 & 3) that

"There were seen horsemen running in the air, in cloth of gold, and armed with lances like a band of soldiers, and troops of horsemen in array, encountering and running one against another, with shaking of shields, and multitude of pikes, and drawing of swords, and casting of darts, and glittering of golden ornaments, and harness of all sorts."

When the delicate, innocent Aurora can so often be described in this warlike guise, we may be excused for alluding to the present state of science and scientists towards it in somewhat of military phrase, while yet describing a state of things seldom occurring in earthly warfare. For, behold, the campaign of the last eleven-year cycle of Solar activity *cum* Auroral brilliancy has been duly fought out; yet who can say with what results achieved? Most creditably to themselves, observers in every civilized country, and more especially in America and Western Europe, have discharged at the strange phenomenon, the spearmen of the sky, their biggest guns in volleys, in platoons, and as single isolated sharp-shooters' pieces, from rock and tower as well as from populous cities and large observatories. They have also exploded mines enough to have hoisted the very auroral testudo or corona right above their heads. But now that the cycle is over, the battles ended, where are the prizes? what is the spoil? The lively opponents have vanished with all their flashing sheen of golden armour for full three years past; but the slain are still nowhere to be found, and we know not when, where, or how the next encounter may take place.

The present, then, while it is certainly no occasion for claiming the triumphs of victory, is happily not one for deploring any very ruinous defeat; it is merely that every one is nonplussed with the past, uncertain of the present, and dark on the future—a time, therefore, this above all others, for gathering up our scattered forces, comparing notes, collecting information, and preparing to present a new front to the foe at present invisible, but certain to return before long. In fact this is the very time for a book on the Aurora, for chronicling in black and white every thing that has yet been done without fear and without favour, yet with practical ability, and for publishing it in such a form that every observer shall be both able and found willing to read deeply therein before entering into the coming Solar Cycle, the auroral displays of which who can attempt at present to describe or predict?

This, then, evidently is even the pressing time for such a book; and now verily here it is before us—a handsome quarto volume of above two hundred pages, well printed, bound, cut round the edges in the American manner, so as to be ready for immediate deep perusal or rapid reference—decorated and additionally explained by many fine engravings, both chromo- and monolithographs, while by its size and shape it is peculiarly adapted to lie open on the table for consultation with other similar-sized astronomer's books. And to whom do we owe such an addition to our scientific literature, this book of the time, this probable foundation and help for future discovery?

Not to any large Society or public observatory or Government office of any kind or degree. It is all the voluntary, spirited, generous work of a private gentleman, J. Rand Capron, Esq., already favourably known in the ranks of modern physicists by his volume of 'Photographed Spectra,' prepared in his country-house at Guildford. And any one who has ever once met with him knows that he has not only a love for the present subject, but an untiring energy in working at it himself, unflagging spirits in keeping up the enthusiasm of others, an admirable battery of telescopes (both refracting and reflecting), spectroscopes, electric tubes, and photographic apparatus, the ability to use them all, and the generosity of soul to desire that any one and every one else shall profit by all that he knows and almost all that he possesses, so long as something more shall thereby be discovered touching the Aurora. For *vive* aurora-observation! *vive* aurora-research! and especially *vive* aurora-spectroscopings! form the animating spirit which vivifies every page of Mr. Capron's most timely publication.

The work is divided into three parts, whereof the *First* is historical chiefly, and descriptive of the phenomena of the Aurora as appreciated by humanity in general, age after age. Collective is it also most extensively of all the theories which the busy, various, inquiring mind of man has formed concerning the nature and causes of Aurora, its why and wherefore, together with the phenomena which influence it and which it may influence in turn. In this part, therefore, figure frequently the honoured names of Lemström, Becquerel, Loomis, De la Rive, and Planté, amidst an encyclopædic collection of other authors of more or less note; and there are many pictorial illustrations.

Foremost amongst these pictures we are certainly not inclined to put one of the most ambitious of them, a chromolithographic copy of an oil-painting from Norway; for it is to us crude, shallow, without atmosphere, and betraying very limited observation in the painter. But as corrective of that picture, we can fortunately point attention to the author's own view of the Aurora as seen in Kyle Akin, Isle of Skye, in 1874. The quiet of the scene there, the grand symmetry of the chief arrangements in luminous arch behind arch of impalpable light, the mysterious extra darkness below, and the black clouds in front, are what, we well know, do often characterize the Aurora in that part of the world, and speak so solemnly of one of the greater mysteries of nature, extending from here into starry space, and thence presenting to man a symbol of the still unknown, unsolved enigmas of the universe of earth and heaven.

Part the *Second* deals with the spectroscoping of the Aurora, and is very full with all that has been, or may be, done in the way of spectroscopic observation, and the best instrumental means for accomplishing it.

Herein it may just happen that some persons will be moved to complain of the rather fragmentary character of much of the reading; repeated and intensified, too, as that defect is in some of the plates, where spectra appear longways, crossways, on scales of various length, and in bits and corners sometimes of plates already occupied with spectroscopes, gas-vacuum tubes, micrometers, &c. Yet that is really one of the merits of the book when fully considered; for in the present backward and uncertain state of Aurora-theory it would never do to have too much organizing, cutting, trimming, and reducing to some one man's Procrustean ideas alone, to gain mere symmetry of literary work, seeing that future progress may show some day that, in so doing, certain of the most important points of observation have been left out.

Mr. Capron therefore wisely gives us now every thing he can lay hands on, packing it always into the most practicable shape for printer and engraver, author and reader combined. And to judge how important it was that he should do so, we have only to refer back to seven years since, when some few spectroscopists of Aurora, or what they thought was such, had arrived at a most simple explanation of the whole affair—grand certainly, but baldly insipid,—and had very nearly induced the world to rest from its exertions, under the idea that there was nothing more to be discovered. At that period, so soon after the brilliant discoveries of the greater men, when a single glance almost in a spectroscope had settled the gaseous nature of nebula *versus* the solid or fluid state of stars, and had detected by a line whether, say, hydrogen, or iron, or calcium, or any other particular chemical element existed in sun, or star, or any burning flame anywhere upon this earth far or near, it was certainly *expected* that the auroral light, so close to us denizens of the earth as to be generally within ninety miles of its surface, would soon fall an easy prize to the wonder-working modern power of prismatic vision through a narrow slit.

Moreover one distinguishing and all-important bright line had been found in the citron or yellow-green region of the Aurora's spectrum. It had, too, in every auroral display a constancy and a power which rendered almost needless the notation of any thing else in the spectrum; and every thing there did accordingly pale its insignificant head to most observers; while reported discovery after discovery extended through the whole Solar system the glory, unity, and rule of that one chief dominant citron line. It was said, for instance, that it also characterized the whole of that mysterious and believed solar phenomenon of the night-sky, the zodiacal light—then that it was the chief external feature in the spectrum of the solar corona as seen in total eclipses, and proved a perpetual aurora to be always environing the sun; and then, coming down to earth, that wondrous citron-line was found to be an iron-line; wherefore, behold, iron can give out a rarer, lighter, more ascensional vapour than any gas known.

All this *would* have been, if the spectrum-place of the observed line in each case had been really the same; for spectrum-place, if well measured, is just as absolute as stellar position by right ascension and polar distance. But, as time went on, the introduction by successive observers of more of that marvellously discriminating power "dispersion" (or the parent of all spectrum-place) soon told another tale—declaring *firstly* that without a terrestrial aurora in the sky there was no citron or any other bright line in the spectrum of the zodiacal light, *secondly* that the solar corona-line did not pretend to correspond with the aurora's grand citron line, but with some faint subsidiary line suspected by some one and confirmed by none; and, *thirdly*, the same coronal line, and still more the true auroral line, did not either of them correspond in place with the expected iron-line, and were essentially different in physiognomy as well—this being the particular discovery and clearing-up of Prof. Young, of Princeton, U. S., who does all things, spectroscopic, well.

Then, in that case, what does the all-important aurora citron-coloured line correspond with?

That is exactly the question which Mr. Capron asks you and me and every one to join him in trying, by night and by day, and by every manner of means, to discover if we may; for, far as he, and they, and all of us have searched yet, there is nothing else in heaven above or earth below which gives a line of that comparative intensity in that particular spectrum-place. Every pure chemical element known to science, and not a few compounds also, have been burned either in flame, or furnace, or electric spark, whether ordinarily presenting themselves as solids, fluids, or gases; but the line is not there; neither is it in the sun, nor in any star, nor in any nebula whatever.

The last work of the late eminent Professor Ångström, who himself had, years before, discovered this puzzling citron aurora-line, was to compare it with the spectrum of the light of the negative pole in an air-vacuum tube illuminated by electric spark; and he thought that he had at last therein and thereby attained the aurora-spectrum of nature. But the necessary absolute coincidence was subsequently found to be, if at all, only with some infinitesimally faint and hazy band in the blue; so that the whole expected great discovery was, after all, a most extreme case of the play of 'Hamlet' not only without the part of Hamlet (the princely citron line), but without the part also of the red-handed old King—or the "red line," which is the next most important feature of the aurora-spectrum (a line, too, that is very like the well-known red hydrogen-line, yet most decidedly not it): and nothing was left further to claim identity of spectrum-place with, except an uncertain faint shimmering of mere continuous spectrum in the sky-blue, which might be any thing or nothing, and so caused Prof. Alexander Herschel to exclaim, with Shakespearian pity, and full rounding off of the chief characters in the play, "Alas! poor ghost!"

Was there ever, then, such a call upon every one with a telescope and every one with a spectroscope to join in the next campaign, and endeavour to retrieve the future of observation and the credit of spectroscopic science? Never certainly in all the written history of Aurora; wherefore to aid in that good cause the *Third* part of Mr. Capron's book goes largely into magneto-electric experiments touching all sorts of possibilities affecting the anxious mystery of the coming foe.

Every one, we suppose, now looks on the Aurora as having something electric in its nature—as possibly a slow discharge of some kind even of atmospheric electricity, but which, in place of the discontinuous discharge of lightning common to warmer regions of the earth, travels from closely abutting particle to particle of semi-frozen vapour nearly uniformly and most extensively distributed through the atmosphere of not exactly the Polar, but rather only the proximately Polar regions of the earth.

Such slow electric discharge, after once being set in action by electricity, is also believed to be influenced in position and shape by terrestrial magnetism. But no known artificial production of any kind of electric light, with or without any magnetism that can be put into it, will produce in the spectrum that wonder of the present age, the auroral citron line.

Nevertheless Mr. Capron has researched into this matter largely, having fitted up a very powerful electromagnet and tried its effects on the illuminations already electrically excited of various gas-vacuum tubes, and with the effect often of changing the intensity, colour, and shape of the light therein (as detailed through forty well-filled pages of his book), but never of producing the auroral citron line.

Some grand experiments of a further kind have been imagined by Mr. Henry R. Procter, of Clemensthorpe, North Shields, the author of the excellent article on "Aurora" in the presently publishing edition of the 'Encyclopædia Britannica;' and though they are hardly likely to be tried soon, because they will inevitably prove expensive, yet we do hope that an intelligent Government may relieve him of that difficulty before long.

Meanwhile, however, we are practically left in the cold comfort, of almost as weak an hypothesis as the one famous medieval idea that "nature abhors a vacuum within the limits of 32 feet of water"—seeing that the final outcome of every thing auroral seems to resolve itself into this rebuke to mankind, that the electric discharge of that phenomenon takes place in a medium which we do not know any thing about, and that is why we cannot reproduce the citron spectral line.

But that by no means satisfies so untiring a genius as Mr. J. Rand Capron; wherefore up he starts to his feet again in an effective *Fourth* part to his book, or a series of five Appendices giving additional information, references to, and reprints from, divers works, essays, and codes of instruction issued to polar voyagers. He does all this; and now comes the one black blot with which to bring this review to a too effective end.

Yet Mr. Capron himself is not to blame in it. He comes out brighter, more the benefactor of the public, and the pure-souled prosecutor of science for science's own sake, than ever. In his ardent desire to omit nothing that could contribute to the progress of auroral knowledge, he thought he would give a niche in his book, and thereby wider publicity as well as renewed circulation, to the *modicum* of auroral work performed by the Government's late Polar expedition. With this view, he both printed the auroral instructions prepared for the officers of the Arctic ships by the Royal Society, and the subsequent reports of what the said officers had observed accordingly, just as they appear in the Government's Arctic papers.

That was easy enough so far; but finding that the mere written descriptions were hardly clear without the assistance of certain lithographic plates which, having already answered all their intended purpose for the Government's report, were about to be wiped off, Mr. Capron applied to My Lords of the Treasury for leave to have copies printed from those stones at his own expense. And what did My Lords of the Treasury answer to this hero, *sans peur et sans reproche*, of auroral research for advancing the science of our country and of the age in which we live? They informed him "that he could not be permitted so to do, unless he paid also one third of the cost of the original execution of the drawings"!—an additional burthen to the other expenses of his scientific book, which it was of course quite out of the question for him to undertake.

Oh! that the effects of the now universal education which is spreading throughout the country might be to produce in the upper Government offices, as well as the country at large, genius, largeness of soul, and hearty love of science; and then such an example as Mr. Capron has shown in his book on 'Auroræ,' though still perhaps rare, would be appreciated more nearly as it well deserves to be.

Recherches sur l'Electricité. Par Gaston Planté. Paris: 1879. Pp. 271.

M. Gaston Planté has republished in a convenient collected form the experimental researches in Electricity that have occupied him for nearly twenty years. The chief points in the work are:—(1) a research on the secondary currents produced by electrolytic polarization; (2) the behaviour of different metals used as electrodes in respect of the secondary currents they yield; (3) the construction of secondary batteries of large size, and the mechanical contrivances for charging them in compound circuit and discharging in simple circuit; (4) the various effects—chemical, thermic, magnetic, and mechanical—produced by the powerful discharges from such batteries; (5) the analogies presented by these phenomena with certain natural phenomena; (6) the construction of an instrument called a Rheostatic Machine, for rapidly charging and discharging a large accumulator consisting of mica plates, to be used in conjunction with large secondary batteries.

The interest of the greater part of the work is in the experimental detail, which, however, adds little to electrical theory. Some of the more important conclusions are as follows:—

The falling-off of the current observed when a saline or acidulated solution is electrolyzed between metallic electrodes, is due to various causes, of which the chief are:—(1) the insolubility of the oxide formed at the positive electrode; (2) the low conductivity of the same; (3) the resistance of the layer of saline liquid which surrounds the pole if the oxide is soluble; (4) the electromotive force of the separated ions, tending to produce an inverse secondary current. This current M. Planté finds to be due, in the case of most metals, to the reduction of the metallic oxide formed at the positive electrode, and the oxidation of the metal of the negative electrode or of the hydrogen that has been liberated upon its surface. In the case of the difficultly oxidizable metals, however, such as gold or platinum, the secondary current is chiefly due to the action of the hydrogen which has been more or less occluded by the negative electrode, and also possibly to the gases dissolved in the liquid in the neighbourhood of the two electrodes respectively. In the case of the metal lead, when used as electrodes in dilute sulphuric acid, the action is very marked, the surfaces of the electrodes becoming porous with repeated oxidation and reduction, and so holding the liberated gases in loose combination to a high degree. M. Planté therefore uses sheets of lead in constructing the cells of his secondary batteries, which unite the double advantages of an E.M.F. as high as 2·718 volts, and an internal resistance as low as 0·12, or even 0·048 ohm. The inability of De la Rive to obtain a secondary current from lead was due to his use of dilute hydrochloric acid; for the chloride of lead formed at the positive electrode is practically insoluble.

Some of the phenomena of discharge studied by M. Planté are of great interest, and suggestive by their analogies with natural phenomena.

IX. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from vol. viii. p. 570.]

November 5, 1879.—Henry Clifton Sorby, Esq., F.R.S., President, in the Chair.

THE following communications were read:—

1. "On the probable Temperature of the Primordial Ocean of our Globe." By Robert Mallet, Esq., F.R.S., F.G.S.

According to the latest hypotheses as to the quantity of water on the globe, its pressure, if evenly distributed, would be equal to a barometric pressure of 204·74 atmospheres. Accordingly water, when first it began to condense on the surface of the globe, would condense at a much higher temperature than the present boiling-point under ordinary circumstances. The first drops of water formed on the cooling surface of the globe may not impossibly have been at the temperature of molten iron. As the water was precipitated, condensation of the remaining vapour took place at a lower temperature. The primordial atmosphere would be more oblate and less penetrable by solar heat than the present, and the difference of temperature between polar and equatorial regions would be greater; so that, in the later geologic times, ice may have formed in the one, while the other was too hot for animal or vegetable life. Thus, formerly the ocean would be a more powerful disintegrant and solvent of rocks, mineral changes would be more rapid, and meteoric agencies would produce greater effects in a given time.

2. "On the Fish-remains found in the Cannel Coal in the Middle Coal-measures of the West Riding of Yorkshire, with the description of some new Species." By James W. Davis, Esq., F.G.S., &c.

3. "On the Skull of *Argillornis longipennis*, Owen." By Prof. R. Owen, C.B., F.R.S., F.G.S., &c.

November 19.—Henry Clifton Sorby, Esq., F.R.S., President, in the Chair.

The following communications were read:—

1. "Supplementary Note on the Vertebræ of *Ornithopsis*, Seeley (=*Eucamerotus*, Hulke)." By J. W. Hulke, Esq., F.R.S., F.G.S.

2. "On the Concretionary Patches and Fragments of other Rocks sometimes contained in Granite." By J. Arthur Phillips, Esq., F.G.S.

Patches resembling fragments of other rocks frequently occur in granite, sometimes angular, sometimes rounded, sometimes with clearly defined boundaries, sometimes melting away into the surrounding mass, generally finer in grain than the latter. After a sketch of the literature of the subject, the author described the results of chemical and microscopic investigations of these patches in the granites of Cornwall, Shap Fell, Aberdeen, Peterhead, Fort William, and North-eastern Ireland. There are two classes of inclusions:—(1) the result of the abnormal aggregation of the minerals constituting the granite itself, containing generally more plagioclastic felspar, mica, or hornblende than it, with some other distinctions, most probably concretions formed contemporaneously with the solidification of the mass; (2) fragments of included schistose or slaty rock, often not very highly altered, caught up from the rock-masses through which the granite has forced its way.

3. "Certain Geological Facts witnessed in Natal and the Border Countries during nineteen years' residence." By the Rev. George Blencowe. Communicated by the Rev. H. Griffith, F.G.S.

Shales and sandstones are the prevalent rocks from the coast for about twenty-four miles inland. Here is a protrusion of granite; beyond the sandstones come ferruginous shales, with scattered boulders of trap on the surface. The northern third of Natal is white sandstone, formed into hills and ridges by denudation, with a long trap-capped plateau near Helpmakaar. Coal-seams occur in the sandstones. There are frequent vertical pipes in these sandstones, which, the author thinks, mark the site of trunks of trees, round which the sandbeds had accumulated. Rorke's House and Isandhlwana are near the above plateau. Near the former is an extinct mud-volcano. A remarkable "vitreous shale" is found near the Buffalo; isolated pinnacles of it occur at the spot where the few survivors of the fight crossed that river. A range of mountains, with mural escarpments, remnants of an ancient plateau, rising to a height of some 2000 feet above another plateau which is 5000 to 6000 feet above the sea, extends for about 500 miles from the north of Natal to near Cradock in the Cape colony; they are sandstone horizontally stratified, capped by trap. Some other geological features are described. The Transvaal consists of undulating hills of soft limestone, a sandstone range, and a country rich in metals; iron-ore, cobalt, nickel, copper, and gold occur, as well as plumbago.

December 3.—Henry Clifton Sorby, Esq., F.R.S., President, in the Chair.

The following communications were read:—

1. "The Gneissic and Granitoid Rocks of Anglesey and the Malvern Hills." By C. Callaway, M.A., DSc., F.G.S.; with an Appendix on the Microscopic Structure of some of the Rocks, by Prof. T. G. Bonney, M.A., F.R.S., Sec.G.S.

The author described the results of his investigations into the stratigraphy and petrology of the above districts, which have led him to the following conclusions:—(1) The granitoid (Dimetian) rocks of Anglesey pass down into an anticlinal of dark gneiss (above) and grey gneiss (below). (2) Associated with the granitoid series are bands of felsite, hälleflintas, and felspathic breccias. (3) The succession of gneissic and granitoid rocks in Anglesey resembles so closely the metamorphic series of Malvern as to justify the correlation of the two groups. (3) The Pre-Cambrian rocks of Anglesey and the Malverns, from the highest known member down to the base of the gneiss, may be thus classified:—A. Pebidian (to be described hereafter); B. Malvernian—(*a*) Dimetian, with associated quartz-felsites and hälleflintas (Arvonian) passing down into (*b*) Lewisian.

2. "Petrological Notes on the Neighbourhood of Loch Maree." By Prof. T. G. Bonney, M.A., F.R.S., Sec.G.S.

The author had visited the upper end of Loch Maree and its vicinity with the view of seeing whether microscopic examination threw any light on the vexed questions as to the age of the newer gneiss &c. He described the microscopic structure of a typical series of the Hebridean gneiss, and gave reasons for considering the mass of rock on the right bank of Glen Laggan to be not an intrusive "syenite," as has usually been supposed, but a mass of the Hebridean gneiss faulted against the *newer* series. The microscopic structure of the Torridon Sandstones was described: it proves that, as previously asserted, they are made up of the *débris* of the Hebridean series; from this also probably came the materials of the quartzites. By examination of specimens, collected both in Glen Laggan and at other points along the northern escarpment of the *newer* series, the author showed that its rocks have been rightly called metamorphic; and then, by comparison of these with specimens collected in Glen Docherty, he concluded that the latter belonged to the *newer* series, and that no part of the Hebridean series reappeared here (which would require a most unusual unconformability). On Ben Fyn also he could find no trace of the older series, the rocks there, where not igneous, resembling the *newer* series, though more highly altered than it is further north. The paper concluded with some remarks upon the bearing of the evidence obtained from these studies upon questions of metamorphism, especially as regards its "selective" action.

3. "On some undescribed *Comatulæ* from the British Secondary Rocks." By P. Herbert Carpenter, M.A., Assistant Master at Eton College.

X. *Intelligence and Miscellaneous Articles.*

LECTURE EXPERIMENTS IN ACOUSTICS.
BY PROFESSOR SILVANUS P. THOMPSON*.

1. *Propagation of a Longitudinal Wave.*

A PIECE of apparatus for illustrating the propagation of a longitudinal disturbance more effectively than the customary rows of suspended ivory balls, or of glass balls laid in a groove upon a table, is made by hanging to a wooden rod a row of the balls of thin caoutchouc distended with air, which are sold coloured for children's toys. There is enough tangential friction, if properly arranged, to allow of the propagation of a transversal disturbance being also shown.

2. *Demonstration of Vowel Qualities.*

The usual demonstration of the part played by the various cavities and positions of the throat and mouth in producing vowel qualities of tone is to set the air in them vibrating by resonance to the tones of appropriately chosen tuning-forks, corresponding to a harmonic series, held in front of the open mouth. The jew's harp, with its simple reed set vibrating by the finger, affords a capital adjunct to the experiments with tuning-forks. The entire set of vowel sounds, and even such simple phrases as "who are you?" (minus the consonantal sounds) may be pronounced by the jew's harp in the following manner:—The instrument is held against the slightly opened teeth in the usual manner for playing. The operator then adjusts his throat and mouth as if to pronounce the desired vowel or vowels, and, breathing softly to sustain the vibrations of the reed, strikes the turned-up end, or tongue, with his forefinger. An audience of one or two hundred people can hear the sounds without difficulty.

3. *Illustration of compounding a Rectilinear Vibration with a Simple Translation at right angles to it.*

Let a straight piece of stout clock-spring or of flat steel "crinoline-wire" be fastened to a suitable handle, and a heavy silvered bead be attached to the other end. If this be set vibrating, the bright point produced by viewing a light by reflexion in the spherical surface of the bead appears drawn out into a line of light. If the spring be held in a horizontal plane, this line is of course vertical. Let the spring be then moved in the hand with a swift horizontal motion of translation; the line of light will then appear drawn out into a luminous sinusoidal curve.

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* Communicated by the Author.

ON A NEW STANDARD OF LIGHT.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN, New York, Dec. 4, 1879.

In a recent Number of the Philosophical Magazine (Nov. 1879, p. 392 &c.) Mr. Louis Schwendler communicates a memoir "On a New Standard of Light." It consists of a strip of platinum raised to a standard temperature by an electric current. He remarks of this method, that it appeared to him so natural and simple that he could scarcely understand why it had not been proposed and acted upon before. Subsequently he found that Dr. Draper had suggested the same previously. Mr. Schwendler's experiments were instituted on behalf of the Board of Directors of the East Indian Railway Company, under orders of the Secretary of State for India, to inquire into the feasibility and practicability of lighting up Indian railway-stations by the electric light.

In view of this, will you do me the favour to reprint a passage in the Philosophical Magazine. It is in the Number for May 1847, in a memoir by me, "On the Production of Light by Heat," p. 359.

"Among writers on Optics it has been a desideratum to obtain an artificial light of standard brilliancy. The preceding experiments furnish an easy means of supplying that want, and give us what might be termed a unit lamp. A surface of platinum of standard dimensions, raised to a standard temperature by a voltaic current, will always emit a constant light. A strip of that metal, 1 inch long and $\frac{1}{20}$ of an inch wide, connected with a lever by which its expansion might be measured, would yield at 2000° a light suitable for most purposes. Moreover it would be very easy to form from it an available photometer, by screening portions of the shining surface. An ingenious artist would have very little difficulty, by taking advantage of the movements of the lever, in making a self-acting apparatus in which the platinum should be maintained at a uniform temperature notwithstanding any change taking place in the voltaic currents."

I am, Gentlemen,
Yours respectfully,
JOHN W. DRAPER.

ON THE PASSIVITY OF IRON. BY L. VARENNE.

When a piece of iron is put into the ordinary commercial nitric acid a reaction is immediately set up and becomes intensely vigorous. Concentrated nitric acid, that which in the laboratory mostly bears the name of *fuming nitric acid*, does not react upon iron; moreover the metal acquires, by contact with this acid, the singular property of not being attacked by the dilute acid: the fuming acid is said to render the iron *passive*. The experiments which I have the honour to submit to the Academy were instituted with the aim of casting some light on this interesting question.

I. (1) Iron which has been rendered passive is immersed in the ordinary acid and caused to strike against the sides of the vessel containing it, the necessary energy of the shock being the less the more dilute the acid; solution takes place instantaneously. The same result can be attained either by rubbing the metal with a glass rod, or by projecting a jet of water upon it while immersed, or by setting it in rapid gyration.

(2) Vibrations even of extremely slight amplitude suffice to make the passivity cease. M. Ducretet has been so good as to construct, according to my indications, a very simple apparatus for the study of this novel influence. A strip of soft iron is interrupted in its middle by a strip of caoutchouc. One of the parts is fixed vertically in a pincer; the lower part is completely immersed in the fuming acid and thus rendered passive. The monohydrated is replaced by commercial acid, in which the iron remains intact. Vibrations are then induced by bringing near to the upper part an electromagnet similar to those employed for electric bell signals. Scarcely has the vibratory motion commenced before the reaction appears.

II. On carefully examining with a powerful lens a fragment of rugous metal immersed in the fuming acid, I saw a gaseous sheath form about the iron, which persists when it is put into the dilute acid. If the fragment is very smooth and compact, this sheath is more difficult to perceive, but its formation is nevertheless apparent. I was led by this observation to study the part which might be played by the gaseous envelope in the phenomenon with which we are occupied. The following experiments were made for that purpose.

(1) Iron rendered passive is entirely immersed in the dilute acid, then cautiously taken out so as to be completely covered with the acid liquid, and suspended in the air. After a few moments (almost immediately in a rather brisk current of air) the attack commences, and continues with energy. It is preceded by the sudden appearance of gas-bubbles coming to the surface and bursting.

(2) Iron rendered passive is plunged into dilute nitric acid; some air-bubbles are caused to arrive in the vicinity of the metallic fragment; the reaction commences, being set up the more quickly the more uneven the surface of the metal.

(3) Under the same conditions, the metal being *suspended* in the liquid, some pieces of a carbonate (chalk, for example) are deposited at the bottom of the vessel; carbonic acid being liberated, the metal is attacked.

(4) A vessel containing dilute nitric acid is prepared beforehand, on the bottom of which a little spongy platinum or platinum-black is placed (substances eminently porous and liberating on immersion numerous gas-bubbles); a piece of iron rendered passive being sus-

pended in the liquid so that it does not touch the platinum, the attack is induced at the end of a few seconds.

(5) At the bottom of a cylindrical vessel a small fragment of iron is deposited (a Paris tack, for instance), and covered with fuming nitric acid; after a few moments of contact, passivity being induced, the monohydrated acid is decanted and replaced by the dilute acid; an exactly similar fragment, but not passive, is then cautiously introduced by means of a pincer consisting of two pieces of glass, and arranged parallel to the first and at a distance from it rather considerable in comparison with their diameters. The new fragment is attacked, gas being liberated; the other is unaltered. If they be brought nearer together, at a given moment, when the distance is reduced to a certain amount, the passive iron enters into solution. All other things being equal, the greater the diameters of the fragments the greater is this maximum distance.

(6) Some iron which has been rendered passive being plunged into the dilute acid is rapidly attacked if a strip of metal attackable by the acid is placed near it but not in contact (a metal electropositive in relation to iron, such as zinc). I have observed that the contact of the immersed part of the iron with a copper wire was sufficient to induce the reaction of the acid upon the iron; but if the contact took place upon the part which was out of the liquid, no such reaction was produced. Is not the cessation of passivity to be attributed to the liberation of gas produced under the former conditions?

III. It follows from all the experiments above referred to, that any shaking produced in the vicinity of the passive metal, either by a shock or a vibration, or by a current of gas (sometimes very slight, as in the case of spongy platinum), is sufficient to cause the passivity to disappear. On the other hand, the monohydrated acid exerts an action upon the metal; but that action immediately ceases, the phenomenon being *interpreted* by the disposition around the metallic fragment of a gaseous enveloping sheath. We are thence led to conclude from these results of experiment that this gaseous sheath is the sole obstacle to further attack, that it is more adherent to a smooth surface and to a sample possessing great molecular condensation than to one that is uneven and less compact, and that mechanical shakings and gaseous currents either weak or strong (the latter perhaps adding in certain cases a chemical action to their displacing influence) more or less rapidly determine its dislocation.

Experience has confirmed these previsions to which the preceding trials necessarily led. If the passivity of the metal is the consequence of the formation of the gaseous sheath, this will disappear in a vacuum, and the passivity with it. A fragment of iron rendered passive was placed *in vacuo* by means of peculiar arrangements, so as to avoid any shaking. A vacuum having been produced ($h=15$ millims.), the piece of iron was cautiously withdrawn without directly touching it, and immersed in the dilute acid, where it was immediately attacked.

The nature of the enveloping gas may, besides, be very approximately determined. In fact, if a few air-bubbles are let into the vacuum-apparatus at the moment when the exhaustion is finished, the orange-red coloration characteristic of hyponitric vapour appears in the receiver: the gaseous sheath is therefore formed principally of binoxide of nitrogen.

What are the determining causes of the formation of the gaseons sheath around the metal? What is the nature of the influences which afterwards keep up its adherence? I am undertaking investigations bearing upon these questions*.—*Comptes Rendus de l'Académie des Sciences,* Nov. 10, 1879, t. lxxxix. pp. 783–786.

AN EXPERIMENT ON SULPHUR. BY DR. THEODOR GROSS.

In the following I take leave to make a brief communication on an experiment with sulphur, by which I have obtained a substance which, according to my judgment, is new, and cannot be further analyzed by several of the most powerful chemical reagents.

Some purest commercial milk of sulphur was mixed with linseed-oil, in the proportion of one part of sulphur to not quite one part of oil, and then gently heated in a very wide bowl. The mixture swells up strongly, emitting disagreeable fumes, and leaves a black porous mass behind in large pieces which will not ignite under conditions under which sulphur by itself would burn briskly. The black mass was then finely pulverized and, with pure concentrated sulphuric acid, heated gently during several hours till it boiled. After a brisk liberation of sulphurous acid, I obtained a liquid which in its most concentrated state has the appearance of thick fluid sulphur. More attenuated, it appears clear and pure yellowish red; still more diluted, brownish yellow. Into this diluted acid liquid, sulphuretted hydrogen was passed, and thereby, at least in part, a bright-brown precipitate thrown down. This is soluble in sulphide of ammonium, and also in hot potash-lye. On the other hand, even a hot mixture of hydrochloric acid and chlorate of potass dissolves but little of it.

When the precipitate is heated in air, the sulphur burns up, and the residue is a black substance likewise very indifferent to inorganic acids. This, on being heated in the combustion-boat in a tube of difficultly fusible glass, with a current of oxygen passing through it, till the glass was bright-red-hot, proved fire-proof. Similarly heated in chlorine it became first brown, and after longer action white; and part of it was deposited as a sublimate immediately behind the hottest place of the glass tube. By heating it in hydrogen the original black substance was then again obtained.

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* This memoir will be inserted *in extenso* in the *Annales de Chimie et de Physique.*

Since the black substance is precipitable from its combination with or solution in sulphuric acid by sulphuretted hydrogen as a precitate soluble in sulphide of ammonium, it belongs to a certain well known group of elements, from all of which it is essentially distinguished by the above-mentioned characters. Hence I feel called upon to regard it as new. It is, moreover, either not at all altered, or undergoes no further resolution, by strong acids, oxygen, chlorine, and hydrogen. Since such a substance as the one described is not contained in linseed-oil, it can only be derived from the sulphur; and the question immediately arises how it is present in the latter. If sulphur is an element, our substance can only have been mixed with it. But as a trifling mixture of something with an element that occurs in abundance is out of the question (for 10–20 grams of sulphur give an amply sufficient quantity of the above-described substance for the chemical reactions and experiments), one would suppose that, since it is not mixed as such with the sulphur, it occurs therein in a combination which could not be resolved by distillation &c., goes with sulphur into its combination with alkali or alkaline earth, is likewise precipitated by acid, and, finally, could only be detected by the experiment with linseed-oil. Yet such a supposition would not be a probable one. Sulphur separated from chemically pure gases with the utmost precaution does not behave otherwise to linseed-oil than milk of sulphur, and, as I in March last communicated to the Physical Society of this place, can by itself alone be heated so that, employed even in minute quantities (such as about half a gram), it leaves a black residue in which carbon as carbonic acid can be identified. Further, I have observed (*inter alia*) that sulphur, distilled under certain conditions, yields peculiar gaseous products which could not be brought into accordance with any known inorganic sulphur combination. From all these reasons it has become very doubtful to me that sulphur is an element. This question, however, shall not be further pursued here, since I hope shortly to publish further experiments for its discussion, and since the purpose of this communication was chiefly to make known the above-described substance, which I hold to be new.

Permit me here to mention that, besides sulphur, I am also investigating other bodies, and have observed with phosphorus especially something similar to what I have observed in the case of sulphur.—*Communicated to the Berlin Academy of Sciences by Prof. Helmholtz. Monatsbericht der k. p. Akademie der Wissenschaften zu Berlin*, August 1879, pp. 788–790.

THE
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PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[FIFTH SERIES.]

FEBRUARY 1880.

XI. *The Dilatation of Crystals on Change of Temperature. By* L. FLETCHER, *M.A., Fellow of University College, Oxford, Assistant in the Mineralogical Department, British Museum*[*].

DURING the last fifty years a considerable amount of energy has been devoted by mathematicians and crystallographers to the investigation of the nature of the alteration of a crystal due to change of temperature, and it cannot even yet be said that the state of our knowledge is very satisfactory. It must be hoped that this short paper may serve to once more direct attention to this important and interesting question. Mitscherlich was the first to observe that the angles of crystals may vary with the temperature (Pogg. *Ann.* vol. i. 1824, vol. x. 1827); and he concluded from his experiments that the alteration is probably related to the axes of optical elasticity.

F. E. Neumann was the next to take up the question (Pogg. *Ann.* vol. xxvii. 1833), and assuming, as a deduction from experiment, that the luminiferous æther is in every crystal symmetrical to three perpendicular planes, and that the symmetry of the æther is superinduced by the symmetry of the arrangement of the crystal-molecules, decided that the matter of the crystal must also be symmetrical to these three planes, and that on change of temperature the perpendiculars to these planes must remain permanently fixed in direction in space: to these lines he applied the term *thermic axes*, and gave formulæ by the help of which their position could be calculated in the case of an oblique crystal from goniometrical observations at any two temperatures. Employing for gypsum the data supplied by Mitscherlich, he found that, within the limits

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[*] Communicated by the Crystallological Society.

of errors of experiment, these thermic axes did actually coincide with the axes of optical elasticity; and he therefore concluded that there really exist for all crystals what may be called rectangular "morphological axes." If this be true, it follows that the oblique and anorthic systems are probably mere hemihedral developments of the rhombic. About the time of the publication of this paper the crossed dispersion of the optic axes of borax was discovered by Herschel and Nörrenberg; and thus considerable doubt may be felt as to the truth of Neumann's assumption that the luminiferous æther and therefore also the matter of every crystal are symmetrical to three rectangular planes. Ångström (Pogg. *Ann.* vol. lxxxvi. 1852) next made a series of experiments on gypsum and felspar, and enunciated as result that the axes of optical elasticity, of cohesion, of ordinary elasticity, and of heat expansion did not all coincide, and that therefore the idea of rectangular morphological axes must be given up. Grailich and Lang (*Sitzungsber. d. Akad. in Wien*, vol. xxxiii. 1859), in their celebrated memoir on the physical relations of crystals, treat of this subject. They come to the conclusion that, for all changes of temperature, the parametral ratios are permanently either rational or irrational, and that in an oblique or anorthic crystal rectangular thermic axes do not necessarily, and in fact in gypsum do not, exist—thermic axes being defined to be "such directions in crystals of the non-tesseral systems as at every temperature are equally inclined to each other and present the same unaltered relation to the crystallographic elements." It was, however, soon pointed out that an expression which they had found on substitution of the data for gypsum to become imaginary, could in the most general case be expressed as the square root of the sum of the squares of two real quanities, and therefore was necessarily real. It is clear that some numerical error had been made in their calculation, and that their formulæ really prove that in the symmetry-plane of an oblique crystal two lines can be found, at any one temperature, which are at right angles at a second. It must be remarked that it is not proved that the lines are at right angles at the intermediate temperatures. It may be here stated that the relations given by Grailich and Lang, as connecting the angles between any five planes, are only proved for the case where two of the planes are crystalloid-planes and the other three are zone-planes, and so can scarcely be called general relations between five crystalloid-planes. If, however, we remark, as will be shown later, that the constancy of the indices, on which the proof depends, still holds even if they be irrational, it follows that the relations mentioned above are true in any case, it being a matter of no moment whether the planes be crystalloid-planes, zone-planes, or neither. C. Neumann (Pogg. *Ann.* vol. cxiv. 1861) shows that, in an anorthic crystal, for any two temperatures three lines can be found which are at right angles at both, and shows how their positions may be calculated from the observed angles of the crystal at the two temperatures. In 1868 (Pogg. *Ann.* vol. cxxxv.) appeared a paper by C. Pape, on the "thermic and chemical axes" of gypsum and chalcanthite, in which he (somewhat inaccurately) states that, in the preceding article, Neumann has proved the existence of axes which remain at right angles at all temperatures. From his experiments he infers the coincidence of the thermic and chemical axes in both gypsum and chalcanthite, whence he rapidly passes to the belief that he has established the existence of rectangular morphological axes for all crystals.

It is difficult to see how the dispersion of the planes of the optic axes, met with in many crystals, can be simply explained by the theory of the existence of these rectangular morphological axes. In the same volume of the *Annalen* is published one of Fizeau's papers on the dilatation of crystals as determined by experiment; and he finds, by his admirable method, that the coefficients of dilatation of felspar along two directions equally inclined to an optic mean line, and on opposite sides of it, in the plane of symmetry, are almost in the ratio of 5 : 1; and thus the belief in the existence of any simple relation between the positions of the axes of dilatation and the axes of optical elasticity ought to be finally discarded. In his treatment of the theoretical part of the question, Fizeau assumes that the directions of maximum, mean, and minimum dilatation are permanent for all temperatures; and in this assumption he is followed by Groth (*Physikalische Krystallographie*, 1876, p. 140). Summing up, we may say that up to the present it has merely been proved, and that in a laborious manner, that at any two temperatures there are three crystal-lines at right angles at both: it has not been proved that these lines are at right angles at other temperatures or are fixed in space; but still Pape, Fizeau, and Groth seem to think they may legitimately assume it.

We shall assume that the physical and geometrical properties along all parallel lines of a crystal are the same. Hence it immediately follows that any set of particles of the crystal in a right line at any one temperature will be so at any other, although the position in space and the length of the line may vary; equal parallel lines will remain equal and parallel; parallel planes will remain parallel planes; parallelograms will remain parallelograms; and parallelepipeds will remain parallelepipeds. We may remark, in the first place, that since all the edges of tautozonal planes are parallel, and lines parallel at one temperature are parallel at every other, the property of tautozonality is a permanent one; and, further, since the condition for the tautozonality of planes, whether crystalloid or not, depends only on their indices, the latter, whether rational or irrational, must also be unaltered by change of temperature. This may also be proved as follows:—Imagine the crystal freely suspended in space: the forces due to change of temperature being all internal and in the nature of actions and reactions, the centre of mass must remain unaltered in position, and may be taken as a fixed origin. At the initial temperature let OX . OY . OZ be any three crystal-lines passing through the centre of mass O, and let ABC . HKL be any two planes cutting them in A, B, C, H, K, L respectively; if ABC be the parametral plane, the indices of HKL will be

$$\frac{\mathrm{OA}}{\mathrm{OH}},\quad \frac{\mathrm{OB}}{\mathrm{OK}},\quad \frac{\mathrm{OC}}{\mathrm{OL}}.$$

At any other temperature let A′, B′, C′, H′, K′, L′ be the new positions of the crystal-points A, B, C, H, K, L; the indices of H′, K′, L′, if A′ B′ C′ be the parametral plane, will be

$$\frac{\mathrm{OA}'}{\mathrm{OH}'},\quad \frac{\mathrm{OB}'}{\mathrm{OK}'},\quad \frac{\mathrm{OC}'}{\mathrm{OL}'}.$$

But points which are in a right line and equidistant from each other at one temperature will possess these properties at any other, and thus parts of a crystal-line which have any ratio at one temperature will have the same ratio permanently; we therefore must have

$$\frac{\mathrm{OA}'}{\mathrm{OH}'}=\frac{\mathrm{OA}}{\mathrm{OH}},\quad \frac{\mathrm{OB}'}{\mathrm{OK}'}=\frac{\mathrm{OB}}{\mathrm{OK}},\quad \frac{\mathrm{OC}'}{\mathrm{OL}'}=\frac{\mathrm{OC}}{\mathrm{OL}};$$

and the indices, whether rational or irrational, are invariable. Since parallelograms remain parallelograms, it follows, from the known properties of homographic figures, that a circle will in general become an ellipse, and a sphere an ellipsoid. This may, however, be shown very simply as follows:—O being, as before, the centre of mass, let OA . OB . OC be three perpendicular crystal-lines at the first temperature; construct a parallelepiped having OA . OB . OC for adjacent edges, and let P be the other extremity of the diagonal which passes through O; at a second temperature the lines OA . OB . OC will in general not only have taken new directions in space, but have varied in mutual inclination and in length. Let OA′, OB′, OC′ be these new directions and lengths; on these lines as adjacent edges construct a parallelepiped, and let P′ be the other extremity of the diagonal which passes through O: since parallelepipeds at one temperature remain so at any other, it follows that P′ is the new position of P, and its coordinates $x' \; y' \; z'$ referred to the oblique axes will be OA′. OB′. OC′ respectively. Let the unit lengths in the directions OA . OB . OC at the first temperature have increased to the lengths $\alpha \,.\, \beta \,.\, \gamma$ in the directions OA′. OB′. OC′ at the second; we must have

$$\mathrm{OA}'=\alpha\,.\,\mathrm{OA}, \quad \mathrm{OB}'=\beta\,.\,\mathrm{OB}, \quad \mathrm{OC}'=\gamma\,.\,\mathrm{OC},$$

and

$$\left(\frac{x'}{\alpha}\right)^2+\left(\frac{y'}{\beta}\right)^2+\left(\frac{z'}{\gamma}\right)^2=\mathrm{OA}^2+\mathrm{OB}^2+\mathrm{OC}^2=\mathrm{OP}^2.$$

If the locus of P at the first temperature be a sphere of unit radius, the equation to the surface at the second temperature and referred to oblique axes becomes

$$\frac{x'^2}{\alpha^2}+\frac{y'^2}{\beta^2}+\frac{z'^2}{\gamma^2}=1.$$

But this is the well-known equation of an ellipsoid referred to three conjugate diameters as axes: thus a sphere at any temperature will in general become an ellipsoid at any other, and any triad of perpendicular lines in the sphere will become a triad of conjugate diameters of the ellipsoid; conversely, any triad of conjugate diameters of the ellipsoid must have been at right angles in the sphere. But one and only one triad of conjugate diameters of an ellipsoid, namely the principal axes, is rectangular; and thus there must always exist one and only one triad of lines which is rectangular at two temperatures. If, however, the sphere remains a sphere or becomes a spheroid, it is clear that it will be possible to find an infinity of triads which are rectangular at two temperatures. The axes of the ellipsoid will evidently be the triad of lines which have experienced respectively a maximum, mean, and minimum dilatation. It may also be remarked that in general two, and only two, great circles of the sphere will remain circles—those, in fact, which become at the second temperature the circular sections of the ellipsoid. The above is true whatever the magnitude of the change, and whatever the cause which produces it; the only requisite is that all parallel equal lines should remain parallel and equal. The state of the crystal in the interval has not yet been considered.

It will be convenient now to enter into a short digression as to the permanency of a plane of symmetry at all temperatures, and thus to consider the question of the permanency of the crystallographic systems. It has been shown above that the indices of planes remain constant, and that for this reason the change of position of any plane of the system of given indices will be known if we are given the changes of the elements ξ, η, ζ; $a:b:c$. Now it may be proved, by help of the rationality of the anharmonic ratios of any four of its planes, that any crystalloid system must present one or other of six types of symmetry, according to the values of ξ, η, ζ and the rationality or irrationality of the ratios $a:b:c$. For instance, if $\xi=\eta=\zeta=\frac{\pi}{2}$ and $\frac{a}{b}, \frac{b}{c}, \frac{c}{a}$ be all irrational, the crystalloid system of planes will present symmetry to three and only three perpendicular planes; while if one and only one of these ratios, say $\frac{a}{b}$, is irrational and the other elements are unaltered, the crystalloid system will present symmetry to the above three planes and also to two additional planes perpendicular to each other and bisecting the angles between two of the former. It becomes an interesting question as to whether or not, in the case of a crystal presenting symmetry to only three perpendicular planes, $\frac{a}{b}$ can ever be so altered by change of temperature as to become rational. In the above memoir Grailich and Lang answer this question in the negative; for, they argue, if such an event were possible, a crystalloid system of planes might pass, on change of temperature, from rhombic to tetragonal symmetry: and this they think is disproved by the statement that no crystal has been observed to pass on change of temperature from one type of symmetry to the other, as shown by the fact that the optical properties of a crystal at various temperatures point to a permanent type of symmetry of the luminiferous æther, and therefore of the mass of the crystal. In the first place, we may remark that the difference between an irrational number and the nearest rational one is not very large, being somewhere near the infinitieth decimal figure: if, then, $\frac{a}{b}$ experience any finite change, it must necessarily pass through a large number of rational values, for every one of which the crystalloid system of planes must possess tetragonal symmetry. The fallacy of the argument is this: it assumes that the symmetry of the crystal, as shown in all its physical properties, is the same as the symmetry of disposition of the crystalloid planes. But it is easy to see that this need not be always the case.

Suppose the centres of mass of the molecules to be arranged in rectangular-parallelepipedal order, the edges of an elementary parallelepiped having absolute lengths a, b, c respectively; and, as usual, suppose that all the planes through molecular mass-centres are crystalloid planes: it follows from the above that such a set of *planes* will possess the symmetry characteristic of the rhombic system so long as the ratios $\frac{a}{b}, \frac{b}{c}, \frac{c}{a}$ are all irrational, but the symmetry of the tetragonal if one and only one of them (say $\frac{a}{b}$) become rational; but it is clear that unless a is not only in a rational ratio to b, but actually equal to it, the symmetry of disposition of the *mass-centres* themselves, and therefore the symmetry of the physical properties, will still be no higher than rhombic. But we may go still further, and assert that even if a actually become equal to b, the physical properties of the crystal need only be symmetrical to three perpendicular planes. For if the molecules themselves "have sides," or different properties in different directions (and no one has ventured to assume that they have not), it is possible to imagine that each molecule has an internal symmetry, upon which, as well as upon the arrangement itself of the molecules, the symmetry of the physical properties must ultimately depend. If, then, in the case we are considering, the internal symmetry of the molecule be only symmetry to the above three planes, even though the symmetry of disposition both of the molecular mass-centres and of the molecular planes be tetragonal, those physical properties (among which we must include dilatation on change of temperature) which depend on the internal symmetry of the molecules as well as upon their grouping will still present only that symmetry which characterizes the rhombic system. And we may further observe that, as the dilatations along the directions perpendicular to these planes are independent, $\frac{a}{b}$ will again become irrational, and the crystalloid system itself once more pass to rhombic symmetry.

It might appear at first sight that, from the measurement of the angles of a crystal at only one temperature, no safe conclusion could be arrived at as to the crystal-system to which it must be referred, and that either the crystal must be measured at a second temperature or some physical tests must be applied. We must, however, remember that the faces of a crystal will be in general called into existence in such a way as to accord in their symmetry of disposition with that which is common to the internal arrangement of the molecules and to their grouping, even if the crystal is formed at some temperature at which the latter possesses a symmetry corresponding to a higher type.

In what follows, it will be assumed that in a crystal there is an extremely large number of points arranged in parallelepipedal order, and that at each of these points there is a centre of mass of a molecule or molecular group—all these groups being equal and parallel in orientation. For brevity, such a molecular group will be spoken of as a molecule.

We infer from the above, that the only symmetry-planes which are permanent on change of temperature are those which are at once planes of symmetry to the molecules themselves and to the molecular grouping, and also that it would be possible for a single crystal to determine at each of six different temperatures a crystalloid system presenting symmetry corresponding to a different crystal-system.

Now nothing in the above would conflict with the possibility of new planes of symmetry common to the molecules and their arrangement starting into existence. All we can say is that, if they do come into existence, they must do so symmetrically to the preexisting common symmetry-planes; but once in existence, they cannot be made to disappear again by any cause which would produce internal reactions symmetrical to these common symmetry-planes. Even if the molecules explode, they must do so simultaneously and symmetrically to these planes, and the products of these explosions must therefore also be symmetrical to them. As a crude illustration of the way in which such planes might come into existence, take the previous example of a rhombic arrangement of molecules themselves possessing rhombic symmetry, and let the temperature change to that at which the arrangement of the molecules is tetragonal. It is quite conceivable that at this temperature the internal equilibrium of the molecules might become unstable, and that each molecule might rearrange itself in such a way as to be still symmetrical to the old planes of symmetry, but also at the same time to the two additional planes requisite for tetragonal symmetry. From this instant the whole system, including the arrangement and the internal constitution of the molecules, would present a symmetry corresponding to the tetragonal type; in fact, the crystal would henceforth be tetragonal.

But this symmetrical state could only be so permanent in a space where no disturbing force could enter. We know from experiment that the cohesion of a body diminishes as the temperature increases, and that in general the body may come to find itself in a fluid condition, a state in which the slightest exertion of force will disturb the general arrangement of the molecules. If the cohesion has diminished to this extent in any single direction in the æolotropic body we are considering, the body has reached a condition at any rate analogous to the fluid state; and although, under the sole action of change of temperature, it would for ever retain its symmetrical crystalline arrangement, still, if any attempt were made to determine its hardness while in this state, the result must inevitably be the total collapse of the parallelepipedal nature of the system. If the crystal is within reach of the force of gravity, it will not be necessary even to try the hardness; the parallelepipedal system on arriving at the above state will instantly be destroyed.

It would thus appear that, although by the action of change of temperature the introduction of new planes of symmetry common to the molecules and their grouping may be possible, the reverse operation cannot be achieved until after a serious interference with the parallelepipedal grouping, and not at all in regions far away from disturbing forces. This seems to point to the greater stability of the systems of higher symmetry, and to the fact that we may entertain the hope of being able to transfer most crystals under favourable circumstances into the cubic system—hexagonal crystals, however, being denied this privilege by virtue of their possessing an equatorial plane of symmetry. The reverse operation might, however, be effected by the action of some force, such as magnetism, which would not necessarily induce internal reactions symmetrical to the common planes of symmetry. As remarkable instances of this kind of transformation from a lower to a higher type of symmetry, we may mention the transition of:—(1) amorphous bodies to the crystalline state, as in wrought iron, barley sugar, and the fusible alloys of Rose and Newton; (2) an oblique to a rhombic crystal, as in the case of artificial sulphur; (3) a rhombic to tetragonal, as in that of iodide of mercury; (4) a rhombic to rhombohedral, as in the change of arragonite to calcite. And although the red tetragonal iodide of mercury does become yellow, and therefore presumably rhombic, on heating, still the general rule seems to be that if a crystal is to pass, under the action of the above kind of forces, from a type of symmetry in which there is any number of symmetry-planes common to the molecules and their grouping, to a type in which any of these planes has disappeared, the crystal must first pass through the fluid condition; and in all cases we may say that the parallelepipedal nature of the system will first be destroyed.

Neglecting cases where this destruction of the parallelepipedal nature of the system takes place, we shall henceforth assume the permanency for all temperatures of any of these common symmetry-planes.

Returning to the so-called thermic axes, we have seen that their rectangularity and permanency has been generally assumed for all temperatures. But on consideration we must decide that this is an unfair assumption; for just as Neumann was led by his theory of the symmetrical nature of every crystal with respect to three perpendicular planes to conclude that these perpendicular planes would remain fixed in space for all temperatures, so we might in the same way almost conclude the converse of this proposition, and say that if these planes are permanent in position in space for all temperatures, they are planes of symmetry to the molecules and their grouping. It is true that if these planes only retain their mutual inclination whilst altering their directions in space, we cannot come to the above conclusion; but in that case we should probably say that thermic axes which revolve with the temperature do not bear any very intimate relation to the structure of the crystal—that, in fact, they are mere geometrical inventions, and something akin to the axes of that hypothetical ellipse which the moon describes round a certain point as focus.

For simplicity we shall first consider the alteration of a crystal presenting at least one plane of symmetry common to the molecules and their grouping, and this at all temperatures.

Imagine a circular cylinder cut at any temperature from such a crystal, with its axis perpendicular to the plane of symmetry. From the above, if the cylinder be freely suspended in space and its temperature changed, the axis will remain permanent in direction. In general, the planes perpendicular to the axis will be translated parallel to it; but one plane, that passing through the centre of mass of the crystal or the middle point of the axis, will remain permanently fixed in space; the crystal-lines, however, which lie in this plane will in general change their directions in space and likewise their mutual inclination. To investigate the nature of these changes, we shall take in this plane rectangular axes OX . OZ fixed in space. At any instant determined by the time t, let the crystal-line of unit length coinciding with OX be rotating in space with the angular velocity $\frac{d\theta}{dt}$ or $\dot{\theta}$, and be increasing in length at the rate $\frac{d\alpha}{dt}$ or $\dot{\alpha}$; and let $\frac{d\phi}{dt}$ or $\dot{\phi}$, $\frac{d\gamma}{dt}$ or $\dot{\gamma}$, be the corresponding values for the crystal-line of unit length coinciding at the same instant with the direction OZ. If x z be the coordinates of any point P at this instant, its resolved velocities in the directions OX . OZ will be $x\dot{\alpha}-z\dot{\phi}$, $x\dot{\theta}+z\dot{\gamma}$ respectively. If the change of OP at this instant be one of simple elongation, we must have

$$\frac{x\dot{\alpha}-z\dot{\phi}}{x}=\frac{x\dot{\theta}+z\dot{\gamma}}{z},$$

or

$$\dot{\theta}x^2-(\dot{\alpha}-\dot{\gamma})xz+\dot{\phi}z^2=0. \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

We may first remark that there is either a single pair of such lines, whether real or imaginary, or an infinity of them; for so long as the coefficients do not all vanish, this equation represents only two lines; while if all the coefficients do vanish, and thus the equation reduce to any identity, we must have $\dot{\theta}=\dot{\phi}=0$ and $\dot{\alpha}=\dot{\gamma}$, in which case the change is one of simple linear dilatation in all directions. If the crystal present the common symmetry-planes characteristic of the cubic, tetragonal, or hexagonal systems, it follows from the permanency of direction of those planes that there will be more than two lines in certain of these symmetry-planes likewise permanent in direction in space: in these cases, therefore, the change of every line of the system lying in the common symmetry-plane containing these lines of fixed direction will be one of simple elongation without rotation. In the case of the orthorhombic system, the two lines in a symmetry-plane which remain unchanged in direction in space will be the same for all temperatures, and, moreover, will be at right angles.

We shall now show that these lines, in the case of an oblique crystal, are real, though it is clear that in general they are not at right angles. From dynamical considerations it is known that, so long as the forces acting on a material system are internal, and therefore of the nature of actions and reactions, there can be no change of the moment of momentum of the system about any line. As the molecular system under consideration starts from rest, the moment of momentum of the system about any line must therefore be zero throughout the motion. Further, the moment of momentum of a material system about any straight line is known to be equal to the moment of momentum of the system, supposed collected at its centre of mass and moving with it, plus the moment of momentum of the system relative to its centre of mass about a straight line drawn parallel to the given straight line through the centre of mass. In the case of a molecular group, the latter of these terms will be either absolute zero, or, at any rate, of an order of magnitude which may safely be neglected in comparison with the former term. If, then, r be the distance of the centre of mass of a molecular group of mass m from the above axis, ω the angular velocity at this instant of the radius vector perpendicular to the axis and passing through the centre of mass of the group, the moment of momentum of the molecule about the axis is $mr^2\omega$, and for the whole system $\Sigma mr^2\omega$. From the above this must be zero; and thus at any instant the values of ω for some molecules must be positive and for others negative, and there must be at least one intermediate position for which ω must be zero. From the above equation, or from more simple considerations, it follows that there is a second real line at this instant fixed in space. For these lines Professor Maskelyne (to whom I may here express my hearty thanks for his many valuable suggestions) proposes the convenient term *atropic*. The change of configuration of the system at this instant may be treated as a simple linear dilatation along these two directions. Expressed in rectangular coordinates, the above condition $\Sigma mr^2\omega=0$ may be written as

$$\dot{\theta}\Sigma mx^2-(\dot{\alpha}-\dot{\gamma})\Sigma mxz-\dot{\phi}\Sigma mz^2=0. \quad . \quad . \quad . \quad (2)$$

As the atropic lines are in general not at right angles, and yet one pair of crystal-lines rectangular at one temperature has been shown to be also rectangular at a second, it is seen that these latter lines will in general have changed their direction in space. The position of this pair of lines may be found in the following way:—Let $r\ \psi$ be the polar coordinates of any point P, OX being the initial line, and let ω be the angular velocity of OP at any instant, while $\frac{1}{r}\frac{dr}{dt}$, the rate of elongation of the unit crystal-line in the direction OP, may be denoted by κ. We have immediately, by resolution perpendicularly to the radius vector OP,

$$\omega=[\dot{\theta}\cos\psi+\dot{\gamma}\sin\psi]\cos\psi-[\dot{\alpha}\cos\psi-\dot{\phi}\sin\psi]\sin\psi,$$

or

$$2\omega=\dot{\theta}+\dot{\phi}+[\dot{\theta}-\dot{\phi}]\cos 2\psi-[\dot{\alpha}-\dot{\gamma}]\sin 2\psi. \quad . \quad . \quad . \quad . \quad (3)$$

Similarly for the line perpendicular to OP, and by hypothesis having the same angular velocity,

$$2\omega=\dot{\theta}+\dot{\phi}-[\dot{\theta}-\dot{\phi}]\cos 2\psi+[\dot{\alpha}-\dot{\gamma}]\sin 2\psi.$$

Equating these values of ω, we deduce

$$\omega=\tfrac{1}{2}[\dot{\theta}+\dot{\phi}], \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

$$\tan 2\psi=\frac{\dot{\theta}-\dot{\phi}}{\dot{\alpha}-\dot{\gamma}}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

So long as the atropic lines are fixed in space, the ratios of the coefficients of x^2, xz, and z^2 in equation (1) must be constant, and just so long will ψ remain constant. If, then, the lines given by equation (1) are atropic for more than one instant, the directions of the crystal-lines which are at right angles and are rotating in the same direction with equal velocities will also be fixed in space for more than one instant. But it must be noticed that, by reason of the angular velocity of such lines, different pairs of crystal-lines will coincide at different instants with these fixed directions in space: in other words, if two lines are permanently atropic and not at right angles, not only will the lines which at any instant are retaining their perpendicularity have an angular velocity, but at different instants different crystal-lines will possess the property. We shall now proceed a step further, and show that this retention of the mutual inclination for any instant is possessed by an infinity of pairs of lines. If χ be the angle any crystal-line makes with the axis OX, it follows from (3) that, if ω be the angular velocity of this line,

$$2\omega=\dot{\theta}+\dot{\phi}+(\dot{\theta}-\dot{\phi})\cos 2\chi-(\dot{\alpha}-\dot{\gamma})\sin 2\chi. \quad . \quad (6)$$

If ψ, $\psi+\frac{\pi}{2}$ be the angular coordinates of those lines which at this instant are retaining their perpendicularity, we may write from (5),

$$\dot{\theta}-\dot{\phi}=a\sin 2\psi, \quad \dot{\alpha}-\dot{\gamma}=a\cos 2\psi,$$

and thus

$$2\omega=\dot{\theta}+\dot{\phi}+a\sin 2(\psi-\chi).$$

Similarly, if ω be also the angular velocity of a line whose angular coordinate is χ',

$$2\omega=\dot{\theta}+\dot{\phi}+a\sin 2(\psi-\chi').$$

Equating these values of ω, we deduce

$$(\chi'-\psi)+(\chi-\psi)=\frac{\pi}{2}.$$

Thus, at any instant, any two lines will be retaining their mutual inclination if the sum of the angles which they make with one of the lines of that pair which is retaining its perpendicularity be equal to $\frac{\pi}{2}$—in other words, if they are equally inclined to and on opposite sides of a bisector of the angle between the lines of this latter pair.

The following relations between the angular velocities and between the rates of elongation of perpendicular lines are interesting.

If ω' be the angular velocity of the crystal-line whose angular coordinate is $\chi+\frac{\pi}{2}$, it follows, as above, that

$$2\omega'=\dot{\theta}+\dot{\phi}-(\dot{\theta}-\dot{\phi})\cos 2\chi+(\dot{\alpha}-\dot{\gamma})\sin 2\chi; \quad . \quad . \quad (7)$$

and by addition of (6) and (7),

$$\omega+\omega'=\dot{\theta}+\dot{\phi}.$$

Thus at any instant the sum of the angular velocities of any two perpendicular lines in the plane of symmetry is constant. In the case of an orthorhombic crystal, two lines in a plane of symmetry have been shown to be perpendicular and without angular velocity; thus at any instant in such a crystal, any two perpendicular lines in a symmetry-plane will be rotating with equal velocities in opposite directions. Again, resolving the velocities $x\dot{\alpha}-z\dot{\phi}$, $x\dot{\theta}+z\dot{\gamma}$ along the radius vector OP, we get

$$\kappa=\frac{1}{r}\frac{dr}{dt}=(\dot{\theta}\cos\chi+\dot{\gamma}\sin\chi)\sin\chi+(\dot{\alpha}\cos\chi-\dot{\phi}\sin\chi)\cos\chi$$
$$=\dot{\alpha}\cos^2\chi-(\dot{\phi}-\dot{\theta})\sin\chi\cos\chi+\dot{\gamma}\sin^2\chi,$$

and similarly, for the perpendicular line,

$$\kappa'=\dot{\alpha}\sin^2\chi+(\dot{\phi}-\dot{\theta})\cos\chi\sin\chi+\dot{\gamma}\cos^2\chi$$

and

$$\kappa+\kappa'=\dot{\alpha}+\dot{\gamma};$$

or at any instant the sum of the rates of elongation along perpendicular lines in the plane of symmetry of a crystal is constant for all pairs.

To sum up, we may say that at any instant the change of configuration of the molecular distribution in a plane of symmetry may be treated as (1) a simple linear dilatation along two straight lines, in general not at right angles, (2) a linear dilatation along any pair of an infinity of pairs of lines, accompanied by a rotation of the system as a rigid body: the lines of one of these pairs are at right angles; and the dilatation along them is in one case a maximum and in the other a minimum.

It may be shown in a somewhat similar way that, whatever be the magnitude of the changes, if the crystal-lines of unit length which at a temperature τ coincide with the fixed rectangular axes OX . OZ have become at a second temperature τ' of lengths α and γ respectively, and have revolved through angles of θ and ϕ, the coordinates ξ ζ of the point P′ (the position at τ' of the point P, of which the coordinates at τ were x z),

$$\xi=\alpha x\cos\theta-\gamma z\sin\phi,$$
$$\zeta=\alpha x\sin\theta+\gamma z\cos\phi.$$

Whence it follows that one pair of lines, whether real or imaginary, will have the same directions at the two temperatures, their equation being

$$\alpha \sin \theta x^2 - (\alpha \cos \theta - \gamma \cos \phi) xz + \gamma \sin \phi z^2 = 0. \quad . \quad (8)$$

We have seen above that in the limiting case these lines are real. Also one pair of crystal-lines, inclined at angles of ψ and $\psi + \frac{\pi}{2}$ to the axis OX at the initial temperature τ, will be at right angles at the temperature τ', the angle ψ being given by the very simple relation

$$\tan 2\psi = \frac{2 \sin (\theta - \phi)}{\frac{\alpha}{\gamma} - \frac{\gamma}{\alpha}}.$$

If the lines given by (8) have been atropic *during* the interval from τ to τ', the same crystal-lines cannot have been at right angles during the whole of this interval; for if the ratios $\alpha \sin \theta : \alpha \cos \theta - \gamma \cos \phi : \gamma \sin \phi$ be constant during the interval, ψ can only be constant when θ and ϕ are zero and $\alpha = \gamma$. Further, it may be shown that if any crystal-line OP at the temperature τ makes an angle ψ with the axis OX, a second crystal-line OQ making an angle $\psi + \epsilon$ with the axis OX can be found which has the same inclination to the crystal-line OP at the two temperatures. If $\alpha \sin \theta = a$, $\gamma \cos \phi = b$, $\alpha \cos \theta = c$, $\gamma \sin \phi = d$, and ψ_1 be the inclination of the crystal-line OP to the axis OX at the temperature τ',

$$\tan \psi_1 = \frac{a + b \tan \psi}{c - d \tan \psi},$$

$$\tan (\psi_1 + \epsilon) = \frac{a + b \tan (\psi + \epsilon)}{c - d \tan (\psi + \epsilon)}.$$

Eliminating ψ_1 between these two equations and dividing through by $\tan \epsilon$, we obtain a linear relation connecting $\tan \psi$ and $\tan \epsilon$, and thus for any value of ψ we get a real value of ϵ. Thus, considering only the molecular distribution in a common plane of symmetry, the system may be brought from the configuration at one temperature to the configuration at any other by (1) a simple linear dilatation along two directions in general not at right angles, or (2) a linear dilatation parallel to any one of an infinity of pairs of lines followed by a rotation of the system through some angle round the perpendicular to the symmetry-plane: the lines of one of these pairs are rectangular; and in this case they are the crystal-lines which experience the maximum and minimum elongation.

In exactly the same way it may be proved that, in the case of an anorthic crystal, if the crystal-lines of unit length coinciding at a temperature τ with fixed rectangular axes in space have at a second temperature τ' increased to α, β, and γ respectively, and have taken new directions defined respectively by the direction-cosines

$$\lambda_1\mu_1\nu_1, \quad \lambda_2\mu_2\nu_2, \quad \lambda_3\mu_3\nu_3,—$$

(1) One triad of lines not at right angles will have the same directions in space at both temperatures, each line being determined by the three equations

$$\begin{aligned}
(\lambda_1\alpha-\delta)x+\lambda_2\beta y+\lambda_3\gamma z &=0,\\
\mu_1\alpha x+(\mu_2\beta-\delta)y+\mu_3\gamma z &=0,\\
\nu_1\alpha x+\nu_2\beta y+(\nu_3\gamma-\delta)z &=0,
\end{aligned}$$

where δ is any one of the roots of the cubic

$$\begin{vmatrix} \lambda_1\alpha-\delta & \lambda_2\beta & \lambda_3\gamma \\ \mu_1\alpha & \mu_2\beta-\delta & \mu_3\gamma \\ \nu_1\alpha & \nu_2\beta & \nu_3\gamma-\delta \end{vmatrix}=0.$$

In any case one of these lines must be real; and by the principle of the "conservation of areas" it may be shown, as above, that in the limit all three must be real.

(2) An infinity of triads are equiangular at the two temperatures; one only of these triads is rectangular. This property of two homographic point-systems is virtually proved in a paper by Prof. H. J. S. Smith, "On the Focal Properties of Homographic Figures," published by the London Mathematical Society. The dilatations along the lines which are at right angles at the two temperatures will be maximum, mean, and minimum respectively.

Thus we may pass from the configuration at the first to that at the second temperature by (1) simple linear dilatations along three lines in general not at right angles, or (2) linear dilatations along equiangular triads followed by a rotation of the system as a rigid body.

It would appear that the term thermic axes, if applied at all, ought really to be devoted to those which have been here called atropic.

The following problems may be suggested for solution:—

(1) What is the relation between these atropic lines and the directions of the edges of an elementary parallelepiped?

(2) Are the same lines atropic for all temperatures?

XII. *On Torsion.* *By* Gustav Wiedemann.

[Concluded from p. 15.]

15. *Rotation of the Molecules.*—I have already, in my memoirs from the year 1858 to 1862, endeavoured, by the investigation of the mutual relations between the mechanical and the magnetic behaviour of bodies when they undergo alterations of form, to demonstrate experimentally, besides the displacement of their molecules on one another, at the same time also the alterations of direction of their axes, or their rotations about their centre of gravity, and to show that these displacements are constantly accompanied by the last-mentioned rotations which are rendered evident by alterations in the magnetism of bodies (at torsions of temporary and permanent magnets), and also that, conversely, a rotation of the molecules may have for its consequence a displacement of them (when a current is passed through a magnet).

If the existence of discrete molecules in bodies be admitted at all, those experiments might not only prove in general the existence of preformed molecules capable of rotation about their centre of gravity, of displacement, and, finally, magnetic, but also determine, at least in general, the nature of the rotations they undergo in individual cases of external alteration of form (for example, in the torsion of iron wires through which a current passes, &c.). Thus also it can be proved, as already follows from Wertheim's observations, that magnetic steel wires which have first been twisted and then passed into the position of permanent torsion or of absolute *nil* exhibit polarity externally, consequently that their molecules still retain a sensible rotation. The hypothesis advanced in the appendix to W. Weber and Kohlrausch's paper* (1866), that the elastic afteraction proceeds from a rotation of the molecules producing a change in the molecular forces without the centres of the molecules necessarily altering their relative position, would have found from the beginning a very positive support by reference to these results.

The above results, and especially the indication of the magnetism of a wire which, having been first transversally magnetized by a current passing through it, and undergone torsion, is fully restored to the zero position, in conjunction with the observations on the peculiar behaviour of bent and then unbent, or twisted and then untwisted bodies under changes

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* Pogg. *Ann.* cxxviii. p. 414 (1866); also Braun, Pogg. *Ann.* clix. p. 337 (1876).

of temperature or slight shakings, already contain directly likewise another proposition, which F. Kohlrausch (1876)* deduced from the interesting temporal course of the torsion of a wire repeatedly turned in opposite directions. According to this proposition, "Different arrangements (of the molecules) may coexist with one and the same configuration; and there are forces of elasticity which can temporarily remove the shape of a body from the state of equilibrium." Here even different positions of equilibrium of the molecules come in, which are at once manifested by the magnetic behaviour, and out of which the molecules may be brought, by various causes and in the course of time, into more stable positions.

From all the foregoing experiments and many similar ones, the investigation of the magnetism of deformed bodies appears especially suitable for the purpose of gaining intelligence respecting the molecular processes within them, a knowledge of which it is absolutely necessary to presuppose for a complete theory of deformations. Only therein a complication presents itself, inasmuch as with the purely mechanical relations the reciprocal magnetic actions of the molecules are secondarily combined. Apart from this, a force acting from without upon a magnetizable body can in this relation be conceived precisely like any other mechanically acting force, except only that it moves the molecules in other paths.

16. *Agitation-Effect.*—I have now shown, *inter alia*, that the temporory torsion (or bending) of a body is augmented by slight shakings as well as by magnetizing (*i. e.* by the motion of the molecules in essentially different paths, as in the first deformation), and likewise by changes of temperature, and the permanent torsion (or bending) diminished, and that in like manner the first slight alterations of figure, shakings, and heatings act upon both the temporary and the permanent magnetism. These actions have not directly any component coinciding with the original direction of displacement of the molecules, and are at first independent of the direction of the force that produces the first deformation. I had, on that account, designated the action of this influence as "agitation-effect," by which the molecules become movable, and, more and more in correspondence with the forces acting from without, place themselves in new positions of equilibrium. I had separated this agitation-effect from the regular deformations which indeed go on together with it, but do not emerge undisturbed by it until after its close†.

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* Pogg. *Ann.* clviii. p. 375 (1876).

† Streintz has since (in 1874) analogously designated by the name "accommodation" that property of wires in consequence of which their

In the subsequent first-observed state of equilibrium the agitation-effect may show itself in various ways. Sometimes it consists merely of a rotation of the molecules about their centre of gravity—as, for instance, on the diminution of the permanent and the increase of the temporary magnetism by the first agitations, heatings, &c.

Further, the agitations may produce a turning of the molecules about their centre of gravity without any directly perceptible gliding of them over one another, which may only afterwards have for its consequence an alteration of their displacements on one another—as, for example, at the first alterations (increase or decrease) of the load of a wire fixed in an invariable position of temporary torsion (§10), by which, after detorsion, the permanent torsion of the wire is increased in precisely the same manner as if the wire had been frequently turned to the extent of its position of temporary torsion.

Finally, agitation may produce at the same time a rotation of the molecules and their displacement on one another. This appears, *e. g.*, when a permanently twisted wire is shaken or loaded, heated or magnetized, on which its permanent torsion first diminishes (*conf.* my observations of 1858, and § 9 of this paper). Herein is integrally included the result found by Braun in 1876 *, that permanent alterations in the shape of a body in a determined direction are lessened by alterations in a direction substantially different from and independent of the former.

The corresponding effect also results when a temporarily twisted wire, which can freely yield to the pull of the torsion-weights, undergoes changes of load, and thereby has both its temporary and afterwards its permanent torsion increased.

17. The effects of agitation (or accommodation actions) might be conceived as similar to the diminution of the friction of rough surfaces by rubbing them together. When deformation takes place in any wire, the groups of molecules present are displaced in relation to one another, and their cohesion is more or less impaired, so that they become movable and follow further the forces acting from without than before. In the "cracking" of tin when bent in one and afterwards in another direction, the commotion of the entire mass corresponding to these purely mechanical shiftings of the groups

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resistance to distortions, within the limits of their elasticity (?), become less the oftener such distortions take place (Pogg. *Ann.* cliii. p. 406; *Wien. Ber.* lxix. März 1874, p. 38 of separate impression).

* Pogg. *Ann.* clix. p. 337.

of crystals can be directly heard. The molecules then gradually take up new positions of stable equilibrium with respect to both their relative situation and the orientation of their supposed axes.

If the deformations take place successively (or even simultaneously) in different, independent directions—*e. g.* in two directions perpendicular the one to the other, the molecules may be rendered still more movable by this double motion than if the deformation took place in only one direction, because now the rotations of the molecules are conditioned by both influences. Thus, *e. g.*, we know that by torsion, as well as by stretching, the molecules of magnetic wires are in fact variously rotated.

The phenomenon that a loaded wire under certain circumstances, *e. g.* at higher temperatures, receives through torsion-forces greater temporary and permanent torsion than without loading (§ 12), and that even when temporarily twisted as far as a fixed limit the same holds good for the residual permanent torsion (§ 10), may be in part conditioned hereby. Stretching, however, may also act, in this way: by it the molecules are more forcibly pressed together and are consequently rotated further round their centres of gravity during the torsion, which rotation may then have for its result a further displacement of the permanent positions of equilibrium of the consequently differently oriented molecules with respect to one another, beyond which the displacements as far as the temporary torsion (T_1-P_1) are entirely independent of the load. With repeated torsions the differences of behaviour of the wire with and without a load become less and less as the mobility of the molecules is enhanced; substantially, therefore, they fall into the accommodation-period (*cf.* the experiments on oscillation, § 24), and by no means universally prevail.

The proposition advanced by Braun[*], that a body which simultaneously undergoes two temporary deformations in two independent directions A and B receives a greater permanent deformation in the direction A than if only the temporary deformation in the direction A had operated alone, may be quite in harmony with the above experiences, and would probably also hold only for the period of accommodation.

If a body has lost unhomogeneousness within the limits conditioned by the repeatedly applied, at first unilateral deformations, and if its molecules have "accommodated" themselves, the regular phenomena come out more and more. At the same time the displacements of the molecules occasioned

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[*] Pogg. *Ann.* clix. p. 312 *et seq.* (1876).

by various forces in the direction of the deformations are directly proportional to the forces, provided that they do not approach too near to the maximum of the first deforming force. The deviations from this in the extreme positions, beyond which the wire behaves more or less like a yet unaltered one (see my memoir of 1858), may arise from the molecules not yet possessing as much mobility in those positions as beyond them.

If thereupon the deformations are effected in the opposite direction, the molecules are rotated and displaced within new limits, between which homogeneity is not yet restored; the body behaves more or less like a fresh one; a displacement of the permanent positions of equilibrium, in the direction of the new temporary deformations, comes in, until, after repetition of these, the wire behaves in this direction as it did in the other.

18. *Torsion of different Parts.*—When a wire is deformed alternately in opposite directions, it is by no means absolutely inevitable, especially before the body has accommodated itself, that constantly the same molecular groups will be moved and turned. Just as at the demagnetizing of a magnet succession-points may arise (to which I shall recur in another place), they may also appear, for instance, in a rod which is first twisted and then turned in the opposite direction. With slight torsions this is not immediately perceptible; with greater ones it is very distinctly so. In order to prove this, one end of a rod of soft copper 15 millims. thick, and 160 millims. long, was fixed in a screw vice, while the other end was twisted 720° and then turned as far back again. In the wave-like coils of the fibres it was distinctly seen that the detorsion was most considerable at the extremities of the rod, and was much slighter towards the middle. Since the molecular groups of such a rod, removed unequal distances from their final positions of equilibrium, in consequence of the elastic after-action simultaneously tend towards them with different and variable velocities, and these motions accumulate, an alternatingly directed rotation of the free end of the rod may take place, as was observed by Kohlrausch[*]. Also the more superficial and more central layers of the wire, which in the torsion are unequally stretched and compressed, may be affected in different measure by the detorsion, and may therefore condition this interesting phenomenon[†].

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[*] Compare also experiments by Neesen (Pogg. *Ann.* clvii. p. 584, 1876) and O. E. Meyer (Pogg. *Ann.* cliv. p. 357, 1875; Wied. *Ann.* iv. p. 252, 1878).

[†] Compare also Tresca, *Comptes Rendus*, lxxiii. pp. 1104, 1153 (1871).

19. *Permanent Deformations.*—There is, finally, the variously answered question to be discussed, whether the deformation (torsion) which remains long after the removal of the deforming force is to be attributed to a new position of stable equilibrium of the molecules, corresponding to the state of the body at the time, or whether it is merely a transitional stage through which the body tends, though extremely slowly, to the position of absolute zero which it possessed before that deformation. The former of these two views, however, is most probably the true one; for, first, otherwise a body could not receive any lasting alteration of shape at all without being submitted to change of temperature, &c., but all bodies would inevitably return gradually to their original form. A second ground for the view that the permanent torsion reached by a wire when repeatedly twisted in one direction corresponds to a permanent position of equilibrium is, that transient torsions in the same direction, produced by weaker forces, reckoned from that position onwards are proportional to the forces. Lastly, one must remember that at the opening of a current temporarily magnetizing a steel rod the molecular magnets, which have been deflected from their position of absolute rest, suddenly spring back with great velocity into new permanent positions of equilibrium, corresponding to the permanent magnetization, without the latter becoming, either at once or in the course of time, perceptibly less.

20. *Elastic Afteraction.*—It was to be foreseen, and is confirmed by the experiments, that with repeated deformations the molecules always reach their temporary as well as permanent position of equilibrium the more quickly the oftener the body is deformed in the respective direction. The elastic afteraction declines therefore with different rapidity in the different periods of the accommodation of a body. Further, if a body, *e.g.* a wire, has been frequently deformed (twisted) in alternately opposite directions, it is not unimportant for the course of the elastic afteraction whether the wire be once more twisted in the direction of the last torsion and then again returns to its previous permanent torsion, or whether it be twisted in the opposite direction and its permanent position of equilibrium shifted. Consequently a wire cannot be at once brought up to a determined elastic state, for the observation of the elastic afteraction, by frequently twisting it hither and thither, but there is also the direction of the new torsion to be considered. In this sense, probably, to the previous careful and particular investigations on the elastic afteraction further observations will have to be added, which will

also be of especial importance for the theory of torsion oscillations &c.*

21. *Torsion Oscillations.*—Only after the study of the static conditions into which a body is brought by alternately directed forces, does it appear possible to discuss with more exactitude what takes place in its elastic oscillations, since evidently precisely the same phenomena must prevail in these as in the former—only with this difference, that in them the temporal course of the elastic afteraction is not attended to.

As according to the preceding investigations a wire twisted hither and thither behaves quite otherwise on being twisted toward one side of the existing position of equilibrium than on being twisted toward the other, the hypothesis according to which the diminution of the amplitudes of oscillation in torsion oscillations, for example, is to be referred to an internal friction which, for the entire course of the oscillations, is a function of the velocity, can no longer be maintained. Just as little can the elastic afteraction alone suffice for the explanation if, for example, it be assumed that the force impelling the wire which undergoes an elongation x, namely $\frac{d^2x}{dt^2}$, does not depend simply on x (for instance is not $=ax$), but, since the wire at the time t has not yet attained the position of equilibrium, is equal to $a(x-p)$, p being a function of x and t (for example, $\frac{dp}{dt}=-\alpha p$)†.

The chief cause of the diminution of the vibration-amplitudes lies rather, according to the above observations, in the shifting of the position of permanent torsion at the end of each oscillation, beside which the slow alteration of it in the course of time, the elastic afteraction, plays a secondary part. Let us first neglect the latter, and consider a wire "accommodated" by frequent oscillations hither and thither within certain limits, the procedure at the alternating oscillations may be figured in the simplest form in the following manner:—

In the absolute first position of rest of the wire, before any

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* In reference to the theories on the elastic afteraction compare especially, besides those cited in other places in this memoir, the following:—O. E. Meyer's papers in Pogg. *Ann.* cli. p. 168 (1874), and Borchardt's Journ. lxxvii. p. 130, lxxx. p. 315; Neesen, Pogg. *Ann.* cliii. p. 498 (1874); Warburg, Wied. *Ann.* iv. p. 232 (1878); Boltzmann, *Wiener Ber.* lxx. (Oct. 1874); Wied. *Ann.* v. p. 430 (1878); and the reflections of Butcher (based upon the views of Clerk Maxwell), Proc. Lond. Math. Soc. viii. pp. 110-112 (1878), Wied. *Beibl.* ii. p. 625. The former papers at the same time treat the causes of the diminution of the amplitudes of vibration of bodies, *e. g.* in torsion oscillations.

† Compare Neesen, *Berl. Monatsber.* 1874, Feb. 12.

torsion in its molecules, we will draw axes parallel with the axis of the wire. Let the wire next be "accommodated" by frequent rotations to and fro, and therein at last be temporarily twisted in the positive direction describing an angle $+a$, while the molecules may be rotated so that the lower ends of their axes, looked at from the axis of the wire, describe an angle $+\alpha$, say to the left. The direction of this rotation follows even from my magnetic experiments *. If the wire be slowly brought back into the permanent torsion-position $+b$, the axes of the molecules will retain a portion $+\beta$ of their rotation to the left. If the wire now receives an impulse in the positive direction, which again elongates it up to $+a$, according to the laws of perfect elasticity it will swing back again to the position $+b$. If it now swings beyond this position further to the right, and if the molecules in their rotation had no friction on one another at all to overcome, it would arrive at the elongation $-a$, since, according to § 6, the same force that twists it from $+b$ to $+a$ twists it in the contrary direction from $+b$ to $-a$, while the axes of the molecules would be rotated just as far $(-\alpha)$ to the right as previously to the left. Again, with perfect elasticity the wire would go back to the position $-b$, in which the molecules would be rotated $-\beta$, and so forth.

The to-and-fro motions of the wire, between $\pm a$ and $\pm b$, are perfectly elastic; therefore the performances of work in the swingings outwards and the swingings back again must, within these limits, completely compensate one another. In fact, however, there results a diminution of the amplitudes of oscillation; hence the loss of *vis viva* therein can only correspond to the work which is expended for the alteration of the positions of equilibrium, or the rotation of the molecules from $+\beta$ to $-\beta$ which determines it. The above diminution may therefore be taken as a measure for this work.

Since the shifting of the permanent torsion-position to accommodation is, within certain narrow limits, proportional to the temporary torsion, and therefore the rotation of the molecules therewith is likewise approximately so, the oscillation-amplitudes must, within those limits, diminish according to the law of a geometrical series.

22. *Oscillation-Experiments.*—That, in proportion to the displacement itself of the permanent position of equilibrium, the resulting loss of *vis viva* may be relatively very little, follows also directly from oscillation-experiments.

The wire which had been made use of for the experiments

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* Pogg. *Ann.* cxvii. p. 203 (1862).

mentioned in § 4, and loaded in all with 8175 grams, was set in torsion-oscillations. The elongation A_1 and A_{50}, of its first and its fiftieth swing in the same direction, amounted, in scale-divisions as follows:—

A_1	520−390=130	500−372=158
A_{50}	516−394=122	525−378=147

A_1	570−334=236	516−387 =129
A_{50}	559−346=213	512−390·5=121·5

In order that the influence of the resistance of the air might be estimated, a light pasteboard box closely similar in form to the weights with which the wire was loaded was placed upon them, which at the same time bore a pasteboard ring similar to the brass ring, and some pasteboard appendages copying the eccentric parts of the apparatus. The weight of them was only 68 grams; so that the alteration thereby conditioned could not exert any particular action; the oscillation-period, too, was not greatly changed; the resistance of the air, however, was probably about doubled.

The oscillation-amplitudes A_0 and A_{50} were:—

A_0	534−372=152	552−350=202	529−376=153
A_{50}	526−379=147	543−362=181	523−382=141

From this, according to the formula $A_{50}=A_1x^{49}$, follows the ratio x of two successive elongations (in percentages):—

Without the pasteboard addition	99·95	99·98	99·94	99·97
With „ „	99·82	99·92	99·94	

With the thick wire here employed, the influence of the friction of the air is therefore not considerable. The diminution of the oscillation-amplitude does not amount on the average to 0·1 per cent., while the shifting of the permanent position of equilibrium in the same wire from $+b$ to $-b$ with the alteration of the temporary torsion from $+a$ to $-a$ amounts to about 3·8 per cent. of the latter.

23. *Accommodation.*—As the same frictional relations that condition the static displacement of the position of equilibrium occur also in a perfectly analogous manner in the oscillations, the analogous general relations must also result in the latter as in the former.

Just as in alternatingly directed torsions by temporarily acting weights, we observe correspondingly in the oscillations also first a period of accommodation. As during it with the former the permanent position of equilibrium is first much shifted, so at first the elongations of the swingings diminish greatly. Since in this period the permanent torsions rise very considerably in proportion to the increments of the temporary, the decrease of the oscillation-amplitudes during the same period must, as has been observed, with greater elongations be relatively more considerable. If before the oscillations the wire (as, for example, a drawn wire) already possesses a permanent torsion, this also will be lessened by the action of agitation during the oscillations; a corresponding displacement of the zero-position accompanies the diminution of the elongations; and the latter can be greater in the direction of the displacement than in the contrary direction *. If after the accommodation the wire be again permanently deformed (for instance, more twisted), it is self-evident that the wire in swinging makes a new period of accommodation, with enhanced diminution of the elongations by the action of agitation, &c.†

24. *Influence of the Load.*—As in the above-mentioned torsion-experiments (§§ 12 and 13) in relatively soft wires the static displacements are initially greater with a heavier than with a lighter load, so also must the decrements attendant upon oscillations be greater in the former case, but, like those, gradually approach the magnitude which is observed with lighter loading. This is proved by experiments which I made as long ago as the year 1860.

A soft silver wire of about 0·5 millim. thickness and 75 centims. length was suspended in a double-walled vertical brass tube, through the annular space of which aqueous vapour could be conducted. The wire, like those used above (§ 2 *et seqq.*), carried a brass clamp provided with a mirror, which in like manner supported on a hook a cylindrical brass box 14 centims. in height and 6 centims. wide, into which closely-fitting leaden weights (B) could be put. The amplitudes of the torsion-oscillations of the wire, previously annealed and then set oscillating, were observed by means of a telescope and scale. The following Table contains under T the excursions in both directions directly observed on the scale, under E the total elongations, under N the mean of each pair of excursions in the two directions, under v the percentage of diminution from the first to the tenth elongation. The numbers placed before the observations are the ordinal numbers of successive oscillations. In series II., under Z are given the times which had elapsed between the observations. The wire was kept incessantly oscillating during the whole series.

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* Compare, for instance, Neesen, Pogg. *Ann.* cliii. p. 54 (1874).

† Compare also P. M. Schmidt, Wied. *Ann.* ii. p. 63 (1877).

Series I.

Temp. 15°.

B.		T.		E.	*v.*	N.
grms. 770	1	375	274	101	......	324·5
	10	367	281	86	14·9	324
	1	365·5	284	81·5	......	324·7
	10	359	290	69	15·3	324·5
	1	379	269	110	......	324
	10	371	277·5	93·5	15·0	324·3
1370	1	380	258	122	......	319
	10	370	268	102	16·7	319
	1	365	273·5	91·5	......	319·2
	10	357·3	240·2	77·1	15·8	318·6
	1	371·2	267	104·2	......	319·1
	10	363·8	275	88·8	14·8	319·4
1970	1	560	372·5	187·5	......	466·2
	10	545·6	386·7	158·9	15·2	466·1
	1	542·5	389·5	153	......	466
	10	530·3	401·4	129·9	15	465·8
	1	527	406	121	......	466·5
	10	518	415	103	14·9	466·5

Temp. 100°.

B.		T.		E.	*v.*	N.
grms. 770	1	385	283	102	......	334
	10	372·2	296·7	75·5	26·0	334·5
	1	395·5	292·3	103·2	......	343·9
	10	380·6	305·6	75	26·2	343
	1	392·2	290	102·2	......	341·1
	10	378·6	304	74·6	27·0	341·3
1370	1	380·5	265·5	115	......	323
	10	360·3	284·6	75·7	34·2	322·5
	1	398·3	248·8	149·5	......	323·5
	10	374	273·8	100·2	32·9	223·9
	1	372	276	96	......	324
	10	356	292·2	64	33·3	324·2

B.		T.		E.	*v.*	N.
grms. 1670	1	450·6	245·5	205·1	......	348·6
	10	409·9	286·3	123·6	39·8	348·1
	1	406·6	289·9	116·7	......	348·2
	10	383·2	312·6	70·6	39·5	347·9
	1	410·4	284·6	125·8	......	347·5
	10	386·3	308·6	77·7	38·2	347·4
1820	1	433·5	242	191·5	......	337·7
	10	388·9	287·5	101·4	47·1	338·2
	1	452·5	225·6	226·9	......	339·0
	10	401·3	277·6	123·7	45·5	339·4
	1	397·8	282	115·8	......	339·9
	10	371·1	308·8	62·4	46·1	340
1970	1	497·2	199·8	297·4	......	348·5
	10	418	287·6	130·4	56·0	352·8
	1	414·3	295·2	119·1	......	354·7
	10	381·2	330·2	51·0	57·1	355·7
	1	419·6	295·6	124	......	357·6
	10	384·2	330·2	54	56·4	357·2
2120	1	375·5	245·6	130	......	310·5
	10	325	292·8	32·2	75·3	308·9
	1	429	182	247	......	305·5
	10	343	275·4	67·6	72·7	309·9
	1	444	190	254	......	317
	10	352	281·6	70·7	71·3	316·8
1370	1	358·8	276	82·8	......	317·4
	10	343·4	291·5	51·9	37·2	317·4
	1	366·2	271	95·2	......	318·6
	10	349·2	287·7	61·5	35·4	318·5
	1	364·5	273	91·5	......	318·8
	10	349·3	287·7	61·5	32·8	318·5

Series II.

Temp. 100°.

B.		T.		E.	v.	N.	Z.	B.		T.		E.	v.	N.	Z.
grms.							min.	grms.							min
831	1	263	607	344	......	435		1831	1	667	327	340	......	497	
	10	289	582	293	14·8	435·5			10	517	455·5	61·5	81·9	486	
	1	299	572	273	......	435·5			1	312	608	296	......	460	
	10	318	553	235	13·9	435·5			10	407	513·5	106·5	64·0	460·2	
	1	334	537	203	......	435·5			1	600	315	285	......	457	
	10	348	524	176	13·3	436			10	555·5	392	123·5	54·4	453·2	
	1	515	360	155	......	437·5	20		1	619	270	349	......	444·5	
	10	505	370	135	12·9	437·5			10	522·5	368·5	154	57·2	444·5	
	1	502	372	130	......	437			1	598	165	433	......	380·5	30
	10	494	381	113	13·1	437·5			10	526	236	290	33·0	381	
	1	546	329	217	......	436·5	50		1	660	245	415	......	452·5	
	10	532	343	189	12·9	437·5			10	602·5	313	289·5	29·6	457·5	
	1	529·5	346	183·5	......	437·8			1	592	323	269	......	457·5	
	10	518	357	161	12·3	437·5			10	552·5	363	189·5	29·6	457·5	
	1	511·8	363·8	148	......	437·8			1	656	253	403	......	454·5	15
	10	502·5	372·5	130	12·2	437·5			10	602	308	294	27·0	455	
1831	1	657	356	301	......	506·5			1	327	577	250	......	452	
	10	514	475	39	87·0	494·5			10	362	542	180	28·0	452	

Therefore, while at ordinary temperature the diminution of the oscillation-amplitudes is but very little affected by the load, at the temperature of 100° C. it rises with it very rapidly, so that on the load being increased from 770 to 2120 grams it becomes three times as much as before, rising with every ten oscillations from 27 up to 73 per cent. of the initial amplitude. When after a heavier load a lighter one is applied, the decrement immediately falls to the previous value corresponding to this lighter load. But if with the heavier load the wires are caused to oscillate for a very long time, the decrement continually approaches nearer to that which they possess with the lighter load*. The action of the heavier loading falls, therefore, really within the period of accommodation.

25. *Influence of Unequal Torsion at different Places.*—The unequal torsion of different parts of the same wire (§ 18) may also have an influence upon the torsion-oscillations. This might account for the fact observed by P. M. Schmidt†, that the decrements of the oscillations of wires of different lengths are not equal, but diminish as the length increases. Otherwise there would be no reason why the different parts

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* Compare also Thomson, *loc. cit.* § 8.

† Wied. *Ann.* ii. pp. 48 & 241 (1877).

of a wire should behave differently; for the slightly heavier loading of its upper parts could hardly, according to the above investigation, explain this anomaly, at least after the state of the wire has become constant.

The present experiments show that the experimental portion of this field of research still needed extension ere it would be possible to arrive at an exhaustive theory of the phenomena of imperfect elasticity, in which, after determining the static (*i. e.* the mean) positions of equilibrium of the molecules executing their thermal vibrations, one might also enter upon the direction of the latter.

I hope shortly to be able to make further communications upon the subject here treated, and especially on its relations to magnetism.

Leipzig, January 1879.

XIII. *On a new Form of Resistance-Balance adapted for comparing Standard Coils. By* J. A. Fleming, *D.Sc. (Univ. Lond.), Scholar of St. John's College, Cambridge*[*].

[Plate IV.]

1. THE British-Association Committee on electrical standards concluded their valuable labours on the unit of resistance by constructing copies of the selected standard. Certain of these coils, some fourteen in number, are at present preserved in the Cavendish Laboratory, Cambridge. It is important that these coils, which consist of wires of various alloys, should be from time to time carefully compared together in order to determine whether the ratio of their resistances at definite temperatures remains the same[†]. Observations ought also at the same time to be made of the temperatures at which they agree, and also of their coefficients of variation of resistance with the temperature. In using for the purpose the ordinary form of divided-metre bridge, several objections present themselves which render it a tedious process to determine accurately the difference in the resistance of two coils at different temperatures, and hence to deduce their variation-coefficients. It seemed, on consideration, that

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[*] Communicated by the Physical Society, having been read December 13th, 1879.

[†] A detailed and most careful comparison of these coils was made by Prof. G. Chrystal and Mr. S. A. Saunder in 1875; and their Report is printed *in extenso* in the Report of the British Association at Glasgow in 1876. This is the most recent occasion on which these coils have been examined.

a modification of the usual form of Wheatstone's bridge would render these processes more expeditious and at the same time more accurate. It is the object of the present paper to describe a form of resistance-balance which has been recently constructed for the Cavendish Laboratory, and which experience shows to have several decided advantages over the old form.

2. *Description of the Resistance-balance.*—A circular disk of mahogany 18 inches in diameter and about 1 inch thick (f) (Plate IV. figs. 1 and 2) stands upon three short feet L. Upon this, and concentric with it, is screwed down a disk of ebonite 14 inches in diameter and $\frac{3}{4}$ of an inch thick (e). This ebonite disk has a semicircular groove turned in its circumference. The circular wooden base extends on one side into a narrow rectangle (j) 4 inches wide and of the same thickness as the disk. To this are connected two other rectangular pieces (h, i,) which are joined together by slotted brass bars (y, see fig. 2) underneath, in such a manner as to permit the two intervals to be made wider or narrower at pleasure. This promontory is of wood, of the same material and thickness as the disk f, and is supported and levelled by three levelling-screws n, n', n''. Through the centre of the ebonite disk passes a brass centre-pin D D′ (fig. 2), on which is centred a brass arm, H H′, capable of revolving round just clear of the disk. Beneath the arm, and soldered to it, is a short brass spring x, which depends vertically downwards. This spring carries at its extremity a small prism of platinum-iridium with one edge vertical and turned inwards. In the groove turned in the disk e is stretched a platinum-iridium wire about $\frac{3}{32}$ of an inch in diameter. The wire extends round about $\frac{25}{27}$ of the circumference, and is about 39 inches long; and the groove is of such a size that the wire lies with exactly half its thickness imbedded in it. This wire is represented by the thick black line A C A′ in fig. 1. The ends of this wire are soldered to copper strips k, k. On the wood rectangles j, h, i is fastened an arrangement of longitudinal copper strips, k, k, which connect together eight transverse square copper bars in the manner shown in fig. 1. On the ends of these transverse bars are fixed vertical copper pins $\frac{5}{16}$ of an inch in diameter and $\frac{3}{4}$ of an inch high. On these pins are slipped short lengths of india-rubber tube which extend beyond the pins, so that they form small cups about 1 inch deep, p' (see fig. 3). The top of the copper pin is well amalgamated with mercury, and forms the bottom of the cup. These cups are filled about a quarter full of mercury. On the longitudinal strips of copper are fixed three binding-screws B, B′, G; and a fourth (G′) simply goes through

the wood, and is connected by a wire *t* underneath the base-board with the centre-pin D, and is therefore in metallic connexion with the spring *x*. The battery is connected with the terminals B, B′, and the galvanometer with the terminals G, G′. To the arm H H′ is adapted a trigger, T, of such shape that when the button *w*, which is of ebonite, is pressed down, the spring *x*, carrying the platinum-iridium knife-edge, is bent inwards until it touches the wire strained round the circumference of *e*. The arm carries a vernier N, which travels round sunk in a shallow groove in the face of the ebonite disk; and the ebonite is graduated on the face on the margin of the groove. The graduations are cut into the ebonite, and then rubbed over with powdered chalk mixed with gum and water. This gives a graduation very legible and pleasant to look at. The length of the wire is just one thousand divisions; and the vernier enables these to be divided into tenths. The zero of graduation is so placed that, when the pointer of the vernier reads zero, the knife-edge on the spring *x* is exactly opposite the extremity of the platinum-iridium wire.

It is thus clear that the revolving arm carrying its knife-edge can be moved round so that, on pressing the trigger-button *w*, the knife-edge makes contact at any point of this wire, and thus connects this point with the terminal G′.

This part of the arrangement answers to the sliding block and piston-contact piece of the ordinary divided-metre bridge.

3. *Method of using the Balance.*—Let now two resistance-coils of about equal resistance be provided, and let the coil-terminals of one coil be placed in the mercury-cups *p* and *r*, and those of the other be placed in *q′* and *s′*. And let two more coils be taken of not very unequal resistance which it is desired to compare with each other, let the terminals of one be placed in the mercury-cups *a* and *c*, and those of the other in *b′* and *d′*. It will then be seen that if a battery be connected with B B′, and a galvanometer with G G′, that we have the usual Wheatstone's bridge arrangements (see fig. 5 on page 113, which gives a diagram of the connexions). Two quart Leclanché cells are best suited for ordinary use. If a more powerful battery is used, there is danger of heating the platinum-iridium wire, and so expanding it that it may slip down out of its groove.

The coils in the intervals between the cups *p* and *r* and *q′* and *s′* form two branches; and the coil in the interval between *a* and *c*, together with the resistance of the platinum-iridium wire round to the place where the spring *x* touches it, forms the third branch, whilst the coil in the interval *b′* *d′*, together with the remainder of the wire, forms the fourth. The "bridge"-wire consists of the arm H H′ and the wire under the base-board together with the galvanometer inserted between G and G′. By moving round the arm H H′ and pressing the button *w*, we can find a position where there is no current through the galvanometer. The copper strips *k k* are made of copper so thick that their resistance is practically nothing. Having established a balance between the conductors and read the vernier, the next operation is to lift up the legs of the coil which were inserted in the cups *a* and *c* and drop them into the cups *b* and *d*. Likewise a similar change is effected on the other side; the terminals of the coil inserted in *b*′ and *d*′ are changed to *a*′ and *c*′. An examination of the connexions as shown in fig. 1 will show that the result of the operation is as if the coils had *changed places* whilst preserving their former connexion. Now let the arm be moved round and a fresh position of equilibrium found by pressing the trigger and reading the vernier. A little consideration will show that the difference of these readings gives the difference between the resistances of the coils in terms of a length of the bridge-wire; for the amount by which one coil exceeds the other in resistance is equal to the resistance of that part of the bridge-wire included between the two readings*. In order to render this method of determining the difference of the two coils practicable, the platinum-iridium wire must be exceedingly uniform in resistance, or else a Table of calibration will have to be made. Great pains were taken to procure a length of wire as uniform in size and resistance as possible; and considerable care was taken, in laying the wire in its groove, not to strain it in any way. It lies evenly in its groove, just sufficient tension being put upon it to keep it in its place. The whole resistance of the wire from end to end is not far from $\frac{1}{20}$ of an ohm at about 15° C.

The wire was carefully calibrated by measuring the difference in the resistance of two pieces of thick brass wire of such lengths that the difference of their resistances was about equal to that of thirty divisions of the bridge-wire; and this difference was measured at about a hundred different equidistant

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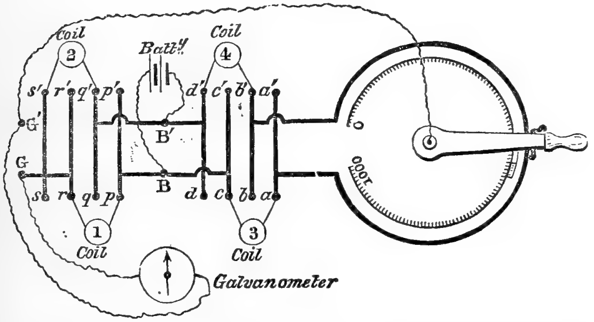
* This method of obtaining the difference of two resistances in terms of a length of the calibrated bridge wire was suggested by Prof. G. C. Foster, F.R.S., in a paper read before the Society of Telegraphic Engineers, May 8, 1872. In this paper is given an account of the method of calibrating a wire. It is obvious, without any further proof, that if the coil placed in *a* and *c* exceeds in resistance that placed in *b* and *d*, then on exchanging them, since the united resistance of coils and bridge-wire remains the same, that the contact knife-edge must be moved back along the bridge-wire by a length exactly equal in resistance to the excess of one coil over the other.

positions all along the bridge-wire, and found to be so nearly the same that no Table of calibration was deemed requisite. To protect the bridge-wire from injuries, as well as to preserve it from being heated by radiation from surrounding bodies, a wooden ring, *v v*, is fastened down on the base-board. The ring is 1¼ inch wide and ¾ inch deep; and its internal diameter is 1 inch greater than that of the ebonite disk. The wire, therefore, lies hidden away on the side of a square-sectioned circular tube; and, furthermore, a shield of cardboard faced with tinfoil lies upon the face of the disk *e*, extending just beyond the ring. An aperture is cut in this shield to permit the passage of the trigger, as well as to allow the vernier to be read. By this means the wire is not only out of sight, but out of reach of all radiation as well as mechanical injury.

Fig. 5.



4. *Method of determining the Variation-coefficients of Coils.*—To determine the variation-coefficient of any given coil we proceed as follows:—Three other coils are provided, two of them nearly equal in resistance, which we will call 1 and 2. A third coil, 3, must be taken whose resistance is nearly equal to that of 4, the coil whose variation-coefficient is desired (see fig. 5). The terminals of 3 are inserted in the mercury-cups *a* and *c*, those of 4 in *b′* and *d′*, those of 1 in *p* and *r*, and those of 2 in *q′* and *s′*. Now the operation to be conducted is to keep the coils 1, 2, and 3 at a fixed temperature, and to keep 4 successively at two known temperatures, differing by about 15° Cent., and to obtain the difference of the resistances of 3 and 4 at these two temperatures. The difference of these differences, divided by the difference of the temperatures, is the mean coefficient of variation of resistance between these temperatures. The chief difficulty to be contended with is that of keeping the temperature of the coils constant during the operation, and of ascertaining what that temperature is;

for, as Prof. Chrystal has remarked in his report (Brit. Assoc. Report, 1876), it is not easy to tell whether the temperature of the water in which the coil rests is identically the same as that of the wire, since the latter is imbedded in a mass of slowly conducting paraffin. To reduce as far as possible the difficulty of keeping the coils at a constant temperature, they are placed in water-vessels made of zinc (see fig. 3, Pl. IV.). These water-boxes are composed of two cylindrical vessels—an outer case 9 inches high and 8 inches in diameter, and an inner one of lesser size; the two are connected at the top, so that they form a sort of jar with hollow sides and double bottom. This interspace forms an air-jacket. Around the inside vessel near the top is a row of small holes; and two tubes communicate at the bottom—one with the inner vessel, and the other with the annular interspace. The top is closed by a wooden lid with apertures for thermometer and stirrer. Water can be made to flow from the supply-pipes into the inner vessel; it rises up and overflows through the holes, and drains away down the interspace and out by the other pipe. The bodies of the four coils are placed in four water-boxes of this description; and water from the town mains being sent in a continuous stream through all four water-boxes, the coils are rapidly brought to and maintained at a known temperature. Any desired temperature can be given to one coil by leading warm water from a cistern into its vessel. The annular air-filled space renders the rate of cooling very slow. Hence the coils, once at the desired temperature, can easily be kept there. Fig. 4, Pl. IV., gives a sketch of the arrangement, two of the water-boxes being removed to show the connexions.

The advantage of the somewhat complicated arrangement of copper bars will now be seen. We can, without withdrawing the coils 3 and 4 from their water-boxes, and without in any way disturbing the other arrangements, *reverse* the position of the coils 3 and 4 on the bridge, by simply lifting up the legs half an inch and changing the mercury-cups into which they dip. Thus the legs of coil 3 are changed from cups a and c to b and d, and those of coil 4 from b' and d' to a' and c'. This exchange does not occupy more than a few seconds; and hence we can obtain the two readings necessary to give the difference of the resistance of the coils 3 and 4 when they are at different temperatures in a very short time. During this short time the temperatures of the two coils will not change perceptibly, protected as they are by an air-jacket.

In the ordinary form of straight bridge there is considerable trouble in exchanging the coils, because the water-vessels have to be moved and the mercury-cups readjusted; and all this time the coils are cooling; so that the two readings are never made under the same circumstances as regards temperature. Beginning, then, with all four coils at the same temperature, we take the difference between 3 and 4. To get them all at the same temperature, water from the town mains is allowed to circulate through the system for half an hour. At the end of this time the difference of 3 and 4 is taken; and several readings are taken at small intervals of time to see if the temperatures are constant. This being done, the temperature of coil 4 is raised by the introduction of warm water until it is about 15° above that of coil 3. It is best to raise the temperature about 20° above the other at first, and keep it there for 20 minutes, and then let it fall very slowly. In this way coil and water cool together, and an equilibrium of temperature is established between them. The difference between 3 and 4 is again taken; and from these two readings we have, as seen above, the mean variation-coefficient between the two temperatures. Another method, which would probably be a more accurate one, for obtaining the mean coefficient of variation between 0° C. and 15° C. would be to wait until the temperature of the water in the town mains was about 15° C., and then to keep three of the coils at that temperature, and to cool the fourth by means of ice to zero. If then all four were kept at 15° and the observations repeated, we should have the means of finding the variation-coefficient of the fourth coil between 0° and 15°. Prof. Chrystal, in his report, threw out the suggestion that resistance-coils should have a thermoelectric couple attached to them, one junction being buried in the heart of the paraffin surrounding the wire, and the other outside. This has been tried in some coils recently made, and proves a satisfactory method of ascertaining the equilibrium of temperature between the wire and the water.

Another source of error in the ordinary methods arises from uncertain or variable resistances at the mercury-cups. It is important that the copper legs of the coil-terminals should press very firmly against the tops of the copper pins on which the india-rubber-tube cups are fixed. To ensure this, the plan adopted is to fasten on the coil-legs an ebonite clamp. Along the edge of the wooden promontory, *j h i* (fig. 1), are put brass pins *m*; and by means of steel spiral springs fixed to these and attached to the clamps the coil-legs are pressed down very firmly (see fig. 3). The ends of the pins which carry the india-rubber cups and the ends of the coil-legs being well amalgamated, we get, when they are thus firmly pressed in contact, a very good joint, and one whose resistance is small and constant. If the clamps are not used, then one leg may get lifted up a little, and thus a short length of mercury interposed, which leads to an error in a reading.

5. *Example of a determination of the Variation-coefficient of a Coil.*—The whole resistance of the platinum-iridium wire is very nearly 0·0512 of an ohm, or not far from $\frac{1}{20}$ of an ohm, at about 15° Cent. As the whole length can be divided by the vernier into 10,000 parts, this gives as the value of $\frac{1}{10}$ of a division $\frac{1}{200000}$ of an ohm.

The unit in the following example is $\frac{1}{10}$ of a division. To secure the greatest accuracy of measurements a low-resistance galvanometer must be used. I am in the habit of using one having a resistance of about half an ohm. The image of a wire strained across a slit is reflected on a scale in the usual way, and read at a distance by means of a telescope. This galvanometer will give an indication, when used with precautions, due to a difference of one tenth of a division when comparing two ohm coils. But as the temperature can hardly be measured with certainty to within less than $\frac{1}{20}$ of a degree, this alone renders such refinement of reading nugatory, in the absence of better methods of ascertaining with certainty the real temperature of the wire.

Two coils were compared. Call them F and K. K is the coil whose variation-coefficient is required.

I. *Difference of Resistance of Coils* F *and* K *at* 11° *Cent.*

	Bridge-readings.		Difference.
Exp. i.	5000	4955	45
Exp. ii.	5000	4954	46
Exp. iii.......	5000	4955	45

The first column gives the number of experiment, the second the reading with the coils F and K in one position on the bridge, the third when F and K are reversed or have exchanged places on the balance; and the fourth gives the difference of their resistances at 11° C. in units of the bridge-wire.

II. *Difference of Resistance of Coils* F *and* K *at* 28°·2 *Cent.*

	Bridge-readings.		Difference.
Exp. i.	5439	4492	947
Exp. ii.	5442	4497	945
Exp. iii.......	5440	4490	950

As before, the fourth column gives the difference of F and K at 28°·2 C. Taking the mean difference at 28°·2 C. to be 947 units, and that at 11° C. to be 45 units, we have

$$\frac{947-45}{28{\cdot}2-11}=52{\cdot}4 \text{ units}$$

as the mean variation-coefficients between 11° C. and 28° C. in units of bridge-wire. Since the coils F and K are approximately ohm coils, this gives as the variation-coefficient of the coil K ·0262 per cent. This coil is of platinum-silver wire. These three determinations occupied about an hour and a half, during which time many more readings were taken, all closely agreeing with the above. The actual measurement of the differences requires but a few moments to effect, the principal expenditure of time being that required to bring the coils to the same temperature as the water.

In conclusion, I may state that this resistance-balance has been constructed in the workshops of the School of Mechanical Engineering at Cambridge, under the direction of Prof. Stuart. Great care was taken in laying on the wire so as to avoid straining it in any way; and the performance of the instrument is consequently very satisfactory. I should also express the fact that this excellent performance is due to the supervising care of Prof. Stuart, who not only supplied several of the details of the construction, but aided, by his valuable suggestions generally, during the process of carrying out my rough designs into a practical form.

Cavendish Laboratory, Cambridge,
December 1879.

XIV. *A Dispersion-Photometer.*

By JOHN PERRY *and* W. E. AYRTON [*].

IN measuring what is usually termed the power of a light, it is common to have a screen placed at such a distance from the light that its illumination is equal to that which it or another screen receives from a standard candle. Now, if a standard candle is, say, one foot from a screen, an electric light of, say, 6400 candle-power must be placed at the distance of 80 feet from a screen to give the same illumination. That a great distance like this should be necessary, and in a chamber whose walls are supposed to be blackened, in the laboratories of works where electric lights are usually examined, has placed

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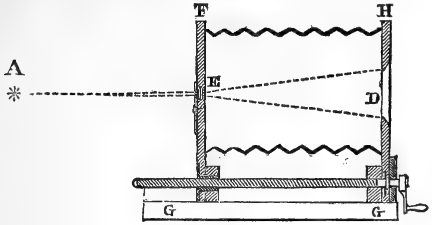
[*] Communicated by the Physical Society, having been read at the meeting held on December 13, 1879.

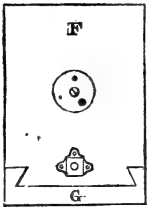
great difficulties in the way of exact determinations of the powers of lights. Our experience in experimenting with electric lights leads us to believe that, but for this difficulty, we should have a vast body of information on the subject of electric lighting, instead of the vague and conflicting statements which fill the scientific journals. Perhaps only those who have made experiments will appreciate fully the great advantages of our having some compact form of photometer. The members of the Society will no doubt see many applications of a compact photometer, such as measuring the light from various parts of the sky for instance, in which ordinary photometric methods are unavailable. Now our instrument will resemble a camera; and it may be turned in all directions.

We were delighted to hear from Dr. Guthrie that his principle of a "retention-image photometer," recently communicated to the Society, has proved to be correct quantitatively. The success of his test must be as interesting to physiologists as to physicists. Besides testing his principle by using powerful lights, he will have experiments to make concerning the loss of light in reflection, as we shall have concerning loss in refraction. Probably, however, he does not intend to apply his instrument to powerful lights, on account of the very great difficulty he would meet with in measuring the breadth of the fine slot which would be needed.

In ordinary photometric methods, the rays of light illuminating unit area of screen, if coming from a powerful source, are contained in a very small solid angle. We use the very simple expedient of causing these rays to fill a much greater solid angle by passing them through a thin concave lens, and in this way obtain the same amount of illumination as before, but on a screen placed at a short distance behind the lens. Thus we not only save space, but prevent a great deal of the absorption which occurs when light passes through air. This absorption is sometimes very great in London. We have, however, absorption in the lens as a disadvantage; but with a very thin lens this may probably be reduced to an almost inappreciable amount.

A is the light to be measured ; D is a paper screen illuminated by light passing through the concave lens E. The frame F, which carries the lens, slides on a stand G, which has marked divisions, so that a pointer tells the distance from the focus of the lens to the screen D. The sides of the space F D are of black cloth, like the sides of a folding camera; and the inside is all blackened except the screen D. A circular plate with three round holes of different sizes is in front of the lens. A diaphragm of this kind is not necessary, if we can be assured of there being no reflection from the inside of the box. Beside the instrument, and containing the standard candle, is a box blackened inside, in one end of which is a screen similar to D. If D is the distance from the light to the lens and d is the distance from the principal focus of the lens to the screen; if δ is the focal length of the lens, then, roughly, a bundle of rays of unit solid angle gets to have, after refraction, an angle





$$\frac{D^2}{\delta^2},$$

as D is always great in comparison with δ. If L is the total light, then the unit angle of incident rays and the unit angle of refracted rays have amounts of light

$$\frac{L}{4\pi} \text{ and } \frac{L}{4\pi}\cdot\frac{\delta^2}{D^2};$$

so that a screen at the distance d from the focus of the lens has illumination of the intensity

$$\frac{L}{4\pi}\cdot\frac{\delta^2}{D^2}\cdot\frac{1}{d^2}.$$

If, now, another screen has the same illumination from a candle whose total light is unity, at the distance D_1 this illumination is

$$\frac{1}{4\pi D_1^2}=\frac{L}{4\pi}\cdot\frac{\delta^2}{D^2}\cdot\frac{1}{d^2};$$

and hence

$$L=\frac{D^2}{D_1^2}\cdot\frac{d^2}{\delta^2}.$$

A double-concave lens, of focal length 1 centimetre, being employed, and d being capable of variation from 40 to 10 centimetres, and D being as much as five times D_1, we can measure in a very small space a light which is from 40,000 to 100 times the standard candle.

We propose to make careful experiments on the absorption in passing through the lens; and by interposing thin plates of glass between the standard candle and its screen to produce there a similar absorption, we have no doubt that measurements may be made with this instrument with much greater accuracy than is possible by the ordinary method.

The power of the light is practically, therefore, proportional to the square of the product of the measurable distances. It will be, of course, unnecessary to measure the variations both in D and in d, as the scale can be so graduated as to give both distances at one reading.

XV. *The Electric Light.* *By* P. E. Nipher[*].

In the Philosophical Magazine for January 1879, p. 30, Mr. W. H. Preece gives a discussion, in which he shows the condition to be supplied in electric lighting, in order to obtain a maximum effect. In equation (2), p. 31, he gives, for the heat distributed to the incandescent material,

$$H=\frac{E^2 l}{(\rho+r+l)^2},$$

where ρ represents the battery-resistance, and r and l represent the resistances of the connecting wires and an incandescent lamp respectively.

For n lamps joined up in series, we must substitute nl for l; while if joined in multiple arc, we must put $\frac{l}{n}$ for l. In either case the value of H is found to be a maximum when the resistance of the lamp system is equal to that of the rest of the circuit.

Mr. Preece then proceeds on the assumption that this condition cannot be complied with if n is large, reaching the conclusion that the amount of heat liberated in each lamp varies inversely as the square of the number of lamps. This is true in either of the two cases discussed by him.

If, however, we have n lamps arranged in n' parallel circuits, in each of which we have n'' lamps, the previous equation becomes

$$H'''=\frac{E^2 \frac{n''}{n'} l}{\left(\rho+r+\frac{n''}{n'} l\right)^2}.$$

With this arrangement it is *always* possible to supply the condition which makes H''' a maximum, entirely irrespective

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[*] Communicated by the Author.

of the value of n. If

$$\rho + r = \frac{n''}{n'} l,$$

we shall have

$$H''' = \frac{E^2}{4(\rho + r)};$$

or the total heat in n lamps is independent of the number of lamps.

The heat generated in each lamp will then vary inversely as the number of lamps.

St. Louis, Dec. 30, 1879.

XVI. *On Unitation.—IX. Practical Remarks thereon, together with Examples.* By W. H. Walenn, *Mem. Phys. Soc.*

[Continued from vol. v. p. 218.]

37. THE distinction between a unitate and the remainder to a division is shown in articles 24, 26, and 27; and in articles 31, 33, 34, and 36 the unitation-formula

$$U_\delta N = a_n(r-\delta)^{n-1} + a_{n-1}(r-\delta)^{n-2} + \ldots + a_3(r-\delta)^2 + a_2(r-\delta) + a_1$$

is compared with the ordinary formula expressing the scale of notation of a given number N. In articles 29 and 30, the absence of a_0, as a symbol, from the formula

$$U_\delta N = a_n(r-\delta)^{n-1} + a_{n-1}(r-\delta)^{n-2} + \ldots + a_2(r-\delta)^1 + a_1(r-\delta)^0 + a_{-1}(r-\delta)^{-1} + a_{-2}(r-\delta)^{-2} + \ldots + a_{-(n-1)}(r-\delta)^{-(n-1)} + a_{-n}(r-\delta)^{-n}$$

is explained, a_0 being the decimal point; a_0 may, in general, be taken to be the origin from which the order of the digits of N is to be reckoned in either direction.

The use of these results and comparisons is manifest throughout the whole subject. The first step in their application is to obtain easily the unitate to a given number. Although the general unitation-formula will, in all cases, furnish this value by substitution, there are short methods which point out different processes according to the algebraical form of δ in $U_\delta N$. The practical work in some of these methods has been illustrated in the papers I., III., and V.; but an analytical treatment of the subject is possible in certain cases. Some of these cases will now be examined.

38. The algebraic form of $U_\delta N$, which may be written

$U_{999\ldots}$N, and in which $\delta =$ any number which has only nines for its digits, there being n digits, includes the particular case U_9N. U_9N may be styled the normal set of unitates, just as the method of expressing an angle in terms of the angle subtended by the arc equal to the radius may be called the normal method, or the Napierian system of logarithms the normal system.

In the general unitation-formula (art. 31), if $r=10^n$, n being taken equal to the number of nines in δ $(=999\ldots)$, the formula has all its coefficients (of the form $(r-\delta)^m$)$=1$, and it resolves itself into adding every set of n digits in N, and repeating the operation until a number less than δ is obtained. The examples at the end of this article will make this short method quite plain. Any reader wishing to verify examples of $U_{999\ldots}$N by actual division will be assisted by knowing that all the multiples of 999... are of the form $a_na_{n-1}\ldots a_3a_2a_1$, writing the digits a_1, a_2, a_3, &c. as the number is expressed decimally (for instance in $1760=a_4a_3a_2a_1$, $a_4=1$, $a_3=7$, $a_2=6$, $a_1=0$), and in which $a_n+a_1=9$, and

$$a_{n-1}\,a_{n-2}\ldots a_3\,a_2=999\ldots,$$

there being $(n-1)$ nines. For instance, $9999\,.\,7=69993$; $999999\,.\,6=5999994$; and so on. Moreover the unit's digit of the product $=10-m$, m being the multiplier. The whole form of these multiples is therefore $(m-1)999\ldots(10-m)$.

Examples:—I. Find U_{99}N, when $\text{N}=31415926536$.

$$U_{99}\text{N}=U_{99}(36+65+92+15+14+3)=U_{99}225$$
$$=U_{99}(25+2)=27.$$

II. Find U_{999}N, N being 10004.

$$U_{999}(4+10)=14.$$

III. Find U_{9999}N, when $\text{N}=31415926536$.

$$U_{9999}\text{N}=U_{9999}(6536+1592+314)=8442.$$

IV. Find U_{999}N, when $\text{N}=3{\cdot}1415926536$.

$$U_{999}\text{N}=U_{999}(536+926+415+31)=U_{999}1908=909.$$

39. The form $U_{1111\ldots}$N, includes U_{11}N, which is a very useful system for practical work (11 being a prime number); it is treated of in No. III., also in articles 4, 6, 8, 14, 16, and 27. If $r=10^n$, n being equal to the number of ones in δ, all the coefficients are equal to unity, and the process of unitation resolves itself into an exactly similar process to that used in $U_{999\ldots}$N, excepting at the last step, when the next less product of an integer and δ must be subtracted from the amount to obtain the required unitate.

Examples:—I. Find $U_{11}N$, N being $= 625$.

$$U_{11}N = U_{11}(25+6) = U_{11}31 = 31-22 = 9.$$

II. Find $U_{111}N$, when $N = 31415926536$.

$$U_{111}N = U_{111}(536+926+415+31) = U_{111}1908 = U_{111}909 = 909-888 = 21.$$

III. Find $U_{1111}N$, when $N = 1004008016$.

$$U_{1111}N = U_{1111}(8016+400+10) = U_{1111}8426 = 8426-7777 = 649.$$

IV. Find $U_{11111}N$, when $N = 3{\cdot}1415926536$.

$$U_{11111}N = U_{11111}(3+14159+26536) = U_{11111}40698 = 40698-33333 = 7365.$$

V. Find $U_{11111}N$, when $N = 3{\cdot}141592654$.

$$U_{11111}N = U_{11111}(31415+92654) = U_{11111}124069 = U_{11111}24070 = 24070-22222 = 1848.$$

74 Brecknock Road, N.,
December 1879.

XVII. *On Intermittent Currents and the Theory of the Induction-balance.* By Oliver J. Lodge, *D.Sc.*[*]

1. The telephone, considered as a scientific instrument, seems destined to play an important part as a detector of minute currents of rapidly changing intensity; and the general theory of intermittent currents is being brought into prominence by its use.

The equations to which most attention has been hitherto directed have been those relating to the steady flow of a current after the initial inductive or inertia-like effects have subsided; and in arrangements such as the Wheatstone bridge, a double key is commonly used, in order to allow the introductory stage to subside before any observation is taken. The galvanometer is essentially an instrument for measuring steady currents, or for giving the algebraically integrated expression for the total quantity of electricity which has passed in the case of transient currents; that is, $\int_0^\tau i\,dt$, τ being small compared with the period of swing of the galvanometer-needle.

Again, the electrodynamometer has an important use as an integrator of the current without paying attention to sign;

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[*] Communicated by the Physical Society, a portion having been read at the Meeting on the 24th of January, 1880.

that is, its indications give the value $\frac{1}{\tau}\int_0^\tau i^2\,dt$, where again τ, the total duration of the current, is small.

But the telephone-plate has such a very small period of swing that the same τ, which is vanishing compared with the time of oscillation of a needle, may be many times greater than that of the telephone-plate. Moreover the plate is not limited to one mode of vibration, but can have minor vibrations superposed on the fundamental; so that it can enter into the changes going on, and render minute fluctuations audibly apparent which would in more slowly moving detectors be simply merged in the total effect.

Thus a rapidly alternating current (such as the telephone itself produces), which is totally unfelt by a galvanometer, is appreciated by a telephone-plate in its variability—the pitch of the note indicating the number of vibrations, even if they are so rapid as to produce a shrill whistle. The telephone, in fact, does not integrate the current, but gives all its fluctuations within certain limits.

The complete theory of the telephone, setting forth precisely on what the loudness of its indications depends, would be a most interesting and important investigation; but if it has been attacked, I am ignorant of it. It seems probable that the loudness of the sounds will be found to depend upon the amplitude of the vibrations of the plate and upon their velocity conjointly—in other words, both on the total change of the current and on the rate at which the change takes place; *i. e.*, in the symbols hereafter to be used, that the loudness is a direct function of $j\,\frac{dj}{dt}$. I shall not, however, assume anything of this sort, but shall content myself with simply finding the value of the current j as a function of the time, leaving the rest to be done subsequently.

It is quite true that the telephone is only an indicator and not a measuring-instrument; but so many null methods can be devised which permit measurements to be made with a simple detector, that it is probable that it will have important applications in this capacity also. And hence I think the general theory of intermittent currents, or of currents in the variable stage, will come into more prominence than hitherto.

The induction-balance furnishes an illustration. The thing itself is old and well known: Dove made experiments with it; and Felici established the laws of current induction by its aid: but its power as an instrument of research was never appreciated till Prof. Hughes applied to it an intermittent current and a telephone.

Faraday interposed blocks of copper between a primary coil and a secondary connected with a galvanometer, and was surprised to find that the effect at make and break was precisely the same with the copper as without it*. It was, however, afterwards found that the physiological effects were very different, being much less when the copper was present—thus proving that though the copper did not affect the integral flow of electricity, yet it greatly affected the time during which that flow took place.

Dove repeated Faraday's experiment more perfectly by means of an induction-balance, and showed that no non-magnetic media produced any effect appreciable by a galvanometer.

Thus may numerous phenomena be overlooked with a galvanometer which with a telephone become obtrusively evident.

2. My attention was more particularly directed to the subject by an observation which was made by Mr. W. Grant, assistant in the Physical Laboratory, University College, and which I have his permission to describe.

The intermittent current from a mouth-telephone, or the induced current from a clock-ticking microphone and coil, is sent through a long helix of wire wound upon a bobbin, with another similar but quite disconnected wire wound alongside it. A telephone and ear are also arranged in the circuit of the first wire, and the loudness of the sound observed. The disconnected wire, which is wound on the same reel as the first, now has its two ends joined up so that it itself forms a closed circuit: the loudness of the sound is thereby considerably increased. The secondary circuit is broken again, and the sound again becomes faint. The strengthening on closing the secondary circuit is so great, indeed, that short-circuiting the primary so as to shunt out all its resistance, scarcely produces any additional strengthening effect. In other words, a moderate resistance (several ohms) may be thrown into a telephonic circuit without the slightest appreciable weakening of the effect, provided a second wire coiled alongside the first be arranged so as to form a closed circuit. If a second telephone be put in circuit with this second wire, it will give about the same indications as the other; and this may sometimes be a good way of working two telephones.

The rough general explanation is, of course, not far to seek. The extra currents, which at first oppose the primary, are able to form in the secondary when its circuit is closed; and hence the changes in strength of the primary are more rapid, and therefore more complete.

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* Exp. Res. arts. 1721, 1735.

An obvious modification is, to arrange both speaking and hearing telephone in circuit with both the parallel coiled wires in series, first, so that the currents flow the same way in both wires, and then with one wire reversed; so that in the first case the self-induction is a maximum, and in the other almost zero.

Any one who tries this experiment will at once appreciate the very decided difference in the telephonic indications. In fact, resistance is of much less consequence in a telephone-circuit than is self-induction. This fact obviously explains the discrepancy which has been observed between experiments on resistance-bobbins thrown into a telephonic circuit in a laboratory, and the same resistance when forming an actual line. The resistance-coils are wound so as to have no self-induction; and they accordingly weaken the effect very little. It is surprising how many thousand ohms can be thrown into a talking circuit without stopping a conversation. But if the bobbins were wound all in one direction, they would be still worse than a line-resistance.

3. It is plain from this that the only right method of arranging a telephone-line is to have two wires running close together—one for the direct, the other for the return current. This arrangement has, I believe, been used in order to avoid induction-effects from battery-currents in neighbouring wires; but it has also the great advantage of diminishing the self-induction of the wire itself. It is, however, a question how far the increase in electrostatic capacity would militate against this advantage in a very long land-line. It is certain to be injurious as far as it goes; but if the wires are very thin, their electrostatic capacity will be but small.

In the case of a cable the double wire would have decidedly less capacity than a single wire; and the capacity diminishes rapidly with the size of the wire. Hence it appears that a telephone-cable should have a pair of exceedingly fine insulated wires of the very highest conductivity pretty close together in a large insulating sheath.

I need hardly say, perhaps, that no advantage will be gained by attempting to do away with the self-induction of the telephone-bobbin itself by means of a second wire wound alongside the first and forming a closed circuit—because the intermittent currents induced in this would be nearly as strong as those in the direct circuit, and, being always in an opposite direction, will nearly neutralize their intermittent effect on external coils or on the telephone-plate.

I propose now to work out the theory of the induction-balance, and of one or two other cases of intermittent currents, as completely as possible without taking into account the electrostatic capacity of the wires. It would be very interesting to obtain the perfectly general equations; but they are very complex, and appear to be beyond me at present.

General Theory of Current-induction.

4. The general theory of the establishment of a current in a number of circuits of known resistance, in one or all of which a given electromotive force begins to act, was begun by Helmholtz and Thomson, who independently showed that the fact of the existence of induced currents, discovered experimentally by Faraday, as well as the laws of the same, could be deduced mathematically from the electromagnetic attractions of Oersted and Ampère by means of the principle of the conservation of energy. The complete theory, it is needless to say, is to be found in Maxwell's 'Electricity,' and also in that admirable digest of the whole subject, Professor Chrystal's article in the *Encyclopædia Britannica.*

Suppose there are n circuits of resistances $r_1, r_2, \ldots,$ in each of which "impressed" electromotive forces $E_1, E_2 \ldots$ begin to act at the same instant; then the strengths of the currents $i_1, i_2 \ldots$ in the several circuits at any time t afterwards are to be found by the following set of n equations—

$$\left.\begin{aligned} E_1 - r_1 i_1 &= \frac{d}{dt}(M_{11}i_1 + M_{12}i_2 + \ldots + M_{1n}i_n), \\ E_2 - r_2 i_2 &= \frac{d}{dt}(M_{21}i_1 + M_{22}i_2 + \ldots + M_{2n}i_n), \\ &\cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \\ E_n - r_n i_n &= \frac{d}{dt}(M_{n1}i_1 + M_{n2}i_2 + \ldots + M_{nn}i_n), \end{aligned}\right\} \quad \ldots \quad (1)$$

where M_{lk} is a double integral, called the coefficient of mutual induction between the circuit l and the circuit k.

Induction-balance.

5. Now in the induction-balance as ordinarily arranged there are three such circuits, with the coefficients M all constant, and two of them without any electromotive force. The first, or battery-circuit, consists of a battery and two equal coils, and it also contains some kind of intermittent break or microphone; the second is the telephone-circuit, and consists of two equal coils and a telephone; and the third is complete within the coin or other piece of conducting matter in the neighbourhood of one pair of the coils of the other circuits. The general value of the telephonic current at any instant after a make has occurred in the primary would therefore be

obtained by the solution of a linear differential equation of the third order with constant coefficients.

But the problem may be simplified by observing that the "coin" is not introduced until a perfect "balance" has been obtained between the first and second circuits—that is, until they are conjugate to one another so that $M_{12}=0$. Moreover the coefficients of induction between the two coiled circuits and the coin are ordinarily sufficiently small for their squares to be neglected; in which case, as will be shown more fully later (sections 12 and 13), the above set of equations reduces to the following—

$$\begin{aligned} M_{11}\frac{di_1}{dt} + 0 + 0 + r_1 i_1 &= E, \\ 0 + M_{22}\frac{di_2}{dt} + M_{23}\frac{di_3}{dt} + r_2 i_2 &= 0, \\ M_{31}\frac{di_1}{dt} + 0 + M_{33}\frac{di_3}{dt} + r_3 i_3 &= 0, \end{aligned}$$

or, as I shall for greater convenience write them,

$$\left.\begin{aligned} L\frac{di}{dt} + Ri &= E, \\ l\frac{dj}{dt} + m\frac{dk}{dt} + rj &= 0, \\ \lambda\frac{dk}{dt} + \mu\frac{di}{dt} + \rho k &= 0, \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (2)$$

where the three coefficients of self-induction are written L, l, and λ (λ being very small); and where the battery-current is i the telephone-current is j and is small, and the current in the coin is k; m signifies the effect of the coin-current on the telephone-circuit, and μ the effect of the primary circuit on the coin. The effect of the primary on the secondary (M) is, as already explained, supposed to be 0; that is, balance is supposed to have been obtained before the insertion of the coin. ρ, or the resistance which the current circulating in the coin experiences, is a quantity which is likely to be several times larger than the specific resistance of the material (*i. e.* the resistance of a centimetre cube); but for ordinary coins it is a number of the same order of magnitude. It must be remembered that R and L stand for the resistance and self-induction of the *whole* of the battery-circuit, and, similarly, r and l include the telephone-coil as well as the other two coils of the telephone-circuit.

Now the only difficulty in solving these equations consists in the varying resistance of the battery-circuit, in which a contact-breaker, or clock-ticking microphone, or some other arrangement for producing an intermittent current is inserted. I shall therefore leave the consideration of the induction-balance for the present, and examine the case of a primary circuit in space by itself and having a break and make in some part of it. Then we will consider the case of an intermittent primary in the neighbourhood of a closed secondary; and after this it will be easy to apply our results to the induction-balance.

On the Law of Variation of a Battery-current in a solitary Circuit.

6. Let a battery of constant electromotive force E act in a circuit whose total resistance may be *suddenly* changed from the value R to the value S, and let L be the coefficient of self-induction of this circuit. Then the current i at any time t after the change of resistance has occurred is to be found from the equation

$$L\frac{di}{dt}+Si=E, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

with the initial condition $i=\frac{E}{R}$ when $t=0$. This gives us in the integral form

$$i=\frac{E}{S}\left(1+\frac{S-R}{R}e^{-\frac{S}{L}t}\right). \quad . \quad . \quad . \quad . \quad (4)$$

Putting $R=\infty$, we get the well-known expression for the current at "make,"

$$i=\frac{E}{S}(1-e^{-\frac{S}{L}t}). \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

Putting S very large, we get an expression for the current at partial break, provided there is no extra-current spark at the surface of separation,

$$i=\frac{E}{R}e^{-\frac{S}{L}t}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

It will not do to put $S=\infty$, because it is impossible to make the resistance *suddenly* infinite, *i. e.* to stop the current instantaneously.

7. It is difficult to get a reasonably correct expression for the value of the current at break. We may suppose that, by separation of two portions of the circuit, the resistance is suddenly changed from R to a quantity which would be S if the current instantaneously ceased, but which is much less than S for a very short time, owing to the heat generated by the

current itself. The temperature of the spark at any time t after the break is to be found, we may suppose, from such an equation as this,

$$mc\theta=\int_0^t (ri^2-\mathrm{H}\theta)dt,$$

where H is the cooling-constant. The mode in which the resistance of air changes with temperature is unknown; so we may assume any simple and not improbable law, such as that the decrement of resistance is proportional to the increment of temperature, or

$$dr=-k\,d\theta,$$

where k is a constant. From these two equations r has to be obtained as a function of the current i and of the time t; and then its value has to be substituted for S in equation (3). As a first approximation, we may imagine the time too short for cooling, or make H=0. Then we get

$$r=\mathrm{S}e^{-k'\int_0^t i^2dt};$$

an expression which, as a coefficient in equation (3), to me seems quite unmanageable. I will therefore assume for the future that the change in the battery-circuit resistance is always made suddenly from one finite value to another, no spark or current across air-spaces being produced.

8. *Suppose the resistance of a battery-circuit oscillates rapidly between the values* R *and* S, *each change being made suddenly and lasting for the short time* τ; *what is the strength of the current after a few seconds?*

This may be assumed to be the sort of thing that happens in a microphone or other arrangement for producing an intermittent current.

The strength of the current before the vibration begins is

$$i_0=\frac{\mathrm{E}}{\mathrm{R}};$$

at the end of the first period τ the strength is, by equation (4),

$$i_1=\frac{\mathrm{E}}{\mathrm{S}}+\frac{\mathrm{E(S-R)}}{\mathrm{SR}}e^{-\frac{\mathrm{S}}{\mathrm{L}}t};$$

at the end of the second period τ the current is

$$i_2=\frac{\mathrm{E}}{\mathrm{R}}-\frac{\mathrm{E(S-R)}}{\mathrm{SR}}\left(e^{-\frac{\mathrm{R}}{\mathrm{L}}\tau}-e^{-\frac{\mathrm{R+S}}{\mathrm{L}}\tau}\right),$$

and so on—

i_4 being $\frac{\mathrm{E}}{\mathrm{R}}-\frac{\mathrm{E(S-R)}}{\mathrm{SR}}(\text{exp.})\{(\mathrm{R})-(\mathrm{R+S})+(2\mathrm{R+S})$
$-(2\mathrm{R}+2\mathrm{S})\}\left(-\frac{\tau}{\mathrm{L}}\right)$

and

i_5 being $\frac{\mathrm{E}}{\mathrm{S}}+\frac{\mathrm{E(S-R)}}{\mathrm{SR}}(\text{exp.})\{(\mathrm{S})-(\mathrm{S+R})+(2\mathrm{S}+\mathrm{R})$
$-(2\mathrm{S}+2\mathrm{R})+(3\mathrm{S}+2\mathrm{R})\}\left(-\frac{\tau}{\mathrm{L}}\right)$

Accordingly, after a very large number of vibrations (which will be accomplished in a few seconds) the current is either

$$i=\frac{\mathrm{E}}{\mathrm{S}}+\frac{\mathrm{E(S-R)}}{\mathrm{SR}}\left(\frac{1-e^{-\frac{\mathrm{R}}{\mathrm{L}}\tau}}{1-e^{-\frac{\mathrm{S+R}}{\mathrm{L}}\tau}}\right)e^{-\frac{\mathrm{S}}{\mathrm{L}}t}, \quad . \quad . \quad . \quad (7)$$

or else the same expression with R and S everywhere interchanged, according as the last change of resistance was from R to S or from S to R, *t* being the time which has elapsed since the last change.

The law of variation of such a current is therefore just the same as for a simple make and break (4); but the part depending on time is multiplied by a constant fraction always less than unity, and which diminishes rapidly as τ (the period of a semi-vibration of the intermittence) diminishes.

If τ be infinitesimal compared with $\frac{\mathrm{L}}{\mathrm{R+S}}$, the fraction is $\frac{\mathrm{R}}{\mathrm{S+R}}$, and hence the current is

$$i=\frac{\mathrm{E}}{\mathrm{S}}\left(1+\frac{\mathrm{S-R}}{\mathrm{S+R}}e^{-\frac{\mathrm{S}}{\mathrm{L}}t}\right),$$

or, as of course *t* (being less than τ) is itself vanishing,

$$i=\frac{2\mathrm{E}}{\mathrm{S+R}}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

Hence the current tends to approach this constant value if the vibrations are too rapid. Such a current as this is of course inappreciable by a telephone; but the above would be its galvanometric indication.

This must be something like the state of things in a coil with a too rapid break.

Query, whether any thing of the same sort happens when a battery-current is passed through a vacuum-tube, the intermittence being almost too rapid to be heard by the telephone. Drs. De la Rue and Hugo Müller have put a telephone in circuit with a battery and vacuum-tube, and heard only a faint rustling when the stratifications were steady.

Equation to a continuously Intermittent Current.

9. It would be interesting to obtain an expression for the current when the resistance of the circuit varies *continuously* from some finite value to infinity and back again in a short regular period $\frac{\pi}{\omega}$. Thus R might be supposed to be $\rho \sec^2 \omega t$; and the equation would be

$$L\frac{di}{dt}+\rho i \sec^2 \omega t=E, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

of which the solution is

$$i=Ce^{-\frac{\rho}{L}\tan \omega t}+\frac{E}{L}e^{+\frac{\rho}{L}\tan \omega t}\int e^{-\frac{\rho}{L}\tan \omega t}dt; \quad . \quad . \quad (10)$$

but the integral $\int e^{\tan x}dx$ does not appear to be evaluable except by an unmanageable lot of series.

Equation to an Alternating Current.

10. Instead of varying the resistance, we may produce an alternating current by a periodic electromotive force, as, for instance, when the circuit contains an electromagnetic machine of constant resistance, such as a telephone itself. The electromotive force is then representable by a simple harmonic function or by a sum of a number of these. Take the simplest case,

$$L\frac{di}{dt}+Ri=E \sin 2\pi nt, \quad . \quad . \quad . \quad . \quad (11)$$

where the electromotive force oscillates from E to $-E$ and back again n times a second. The solution of this is

$$i=Ce^{-\frac{R}{L}t}+\frac{\frac{R}{L}\sin 2\pi nt-2\pi n \cos 2\pi nt}{\frac{R^2}{L^2}+4\pi^2 n^2}\cdot\frac{E}{L}. \quad . \quad (12)$$

The first term rapidly dies out; and so the permanent value is

$$i=\frac{E}{R}\cdot\frac{\sin \omega t-\lambda\omega \cos \omega t}{1+\lambda^2\omega^2}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (13)$$

writing ω for $2\pi n$ and λ for the time-constant $\frac{L}{R}$.

On the Law of Variation of a Battery-current in a Polarizable Circuit.

11. In the previous sections we have considered the electromotive force E of the battery to be constant. Now this

is never accurately true, as the electromotive force begins to fall off the instant the circuit is closed, and some discrepancies between theory and experiment may arise from this cause. Let us see what happens when the battery is not constant, or when a polarizable voltameter forms part of the circuit. According to Kohlrausch, the electromotive force of polarization is proportional to the amount of decomposition, and therefore a short time t after the current has been established it is

$$p\int_0^t i dt,$$

where p is the electromotive force produced by the passage of a unit of electricity. The equation to the current is therefore

$$\mathrm{L}\frac{di}{dt}+\mathrm{R}i=\mathrm{E}-p\int_0^t i dt,$$

or

$$\mathrm{L}\frac{d^2i}{dt^2}+\mathrm{R}\frac{di}{dt}+pi=0\,; \quad . \quad . \quad . \quad . \quad . \quad (14)$$

and the integral of this, remembering that $i=0$ when $t=0$, is

$$i=\frac{2\mathrm{E}}{\mathrm{R}}e^{-\alpha t}\sinh\beta t, \quad . \quad . \quad . \quad . \quad . \quad (15)$$

where

$$\alpha=\frac{\mathrm{R}}{2\mathrm{L}} \text{ and } \beta=\sqrt{\left(\frac{\mathrm{R}^2}{4\mathrm{L}^2}-\frac{p}{\mathrm{L}}\right)}.$$

If $p=\frac{\mathrm{R}^2}{4\mathrm{L}}$, there could never be any current; and it is impossible for p to be greater than $\frac{\mathrm{R}^2}{4\mathrm{L}}$. Practically, however, p would always be very small compared with this quantity; and so we may write

$$\beta \bumpeq \frac{\mathrm{R}}{2\mathrm{L}}-\frac{p}{\mathrm{R}}.$$

Hence in any ordinary polarizable circuit the strength of the current at any time after "make" is

$$i=\frac{\mathrm{E}}{\mathrm{R}}\left(e^{-\frac{p}{\mathrm{R}}t}-e^{\left(\frac{p}{\mathrm{R}}-\frac{\mathrm{R}}{\mathrm{L}}\right)t}\right). \quad . \quad . \quad . \quad . \quad (16)$$

On the Law of Variation of a Battery-current when a closed secondary circuit is stationary in its neighbourhood.

12. So far we have considered a primary circuit in space by itself; but now we will arrange near it a secondary coil with a resistance r and a coefficient of self-induction l, and

we will suppose M to be the coefficient of mutual induction between the primary and secondary. The coils shall be fixed in position so that M is constant; then a current j will be developed in the secondary circuit whenever the primary changes from R to S; and this will react on the primary current i, the equations for determining both being

$$\mathrm{L}\frac{di}{dt}+\mathrm{M}\frac{dj}{dt}+\mathrm{S}i=\mathrm{E} \quad . \quad . \quad . \quad . \quad . \quad (17)$$

and

$$l\frac{dj}{dt}+\mathrm{M}\frac{di}{dt}+rj=0, \quad . \quad . \quad . \quad . \quad . \quad (18)$$

with the conditions $i=\frac{\mathrm{E}}{\mathrm{R}}$ and $j=0$ when $t=0$.

Differentiating these (E being supposed constant), we easily separate the variables and obtain

$$(\mathrm{L}l-\mathrm{M}^2)\frac{d^2i}{dt^2}+(\mathrm{L}r+\mathrm{S}l)\frac{di}{dt}+\mathrm{S}ri=\mathrm{E}, \quad . \quad . \quad (19)$$

and

$$(\mathrm{L}l-\mathrm{M}^2)\frac{d^2j}{dt^2}+(\mathrm{L}r+\mathrm{S}l)\frac{dj}{dt}+\mathrm{S}rj=0. \quad . \quad . \quad (20)$$

Now the solution of (20), remembering the initial condition, is

$$j=\mathrm{J}e^{-\alpha t}\sinh\beta t \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (21)$$

or

$$=\tfrac{1}{2}\mathrm{J}\{e^{-(\alpha-\beta)t}-e^{-(\alpha+\beta)t}\},$$

where

$$\alpha=\frac{\mathrm{L}r+l\mathrm{S}}{2(\mathrm{L}l-\mathrm{M}^2)} \text{ and } \beta=\frac{\sqrt{\{(\mathrm{L}r-l\mathrm{S})^2+4r\mathrm{S}\mathrm{M}^2}}{2(\mathrm{L}l-\mathrm{M}^2)} \quad . \quad (22)$$

$$\left[\text{i. e. } \alpha^2-\beta^2=\frac{r\mathrm{S}}{\mathrm{L}l-\mathrm{M}^2}\right],$$

and where J is a constant which has yet to be determined. It is well to notice that neither α nor β can vanish or become imaginary; but they can become infinite simultaneously.

To obtain the value of the primary current i, we can either write down the solution of (19), viz.

$$i=\frac{\mathrm{E}}{\mathrm{S}}+e^{-\alpha t}(\mathrm{C}_1e^{\beta t}+\mathrm{C}_2e^{-\beta t}); \quad . \quad . \quad . \quad . \quad . \quad (23)$$

or, what is practically more convenient as determining all arbitrary constants directly, we can combine equations (17) and

(18) so as to eliminate $\frac{di}{dt}$, and then put in the values of j and $\frac{dj}{dt}$ from (21). We thus get

$$M(Si-E)=\tfrac{1}{2}Je^{-\alpha t}\{\sqrt{(Lr-Sl)^2+4rSM^2}.\cosh\beta t + (Lr-lS)\sinh\beta t\},$$

which will give us the value of J, since $i=\frac{E}{R}$ when $t=0$. We find then that

$$J=\frac{2ME(S-R)}{R\sqrt{\{(Lr-lS)^2+4rSM^2\}}}; \quad . \quad . \quad . \quad . \quad (24)$$

and this is the value to be substituted in equation (21).

Hence, finally, we can write out explicitly the values of i and j, though, as the constants are very long, we will make an abbreviation by writing

$$\frac{4rSM^2}{(Lr-lS)^2}=\lambda^2; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (25)$$

then the strength of the battery-current at any time after the resistance of its circuit has suddenly jumped from R to S is

$$i=\frac{E}{S}+\frac{E(S-R)}{SR}e^{-\alpha t}\left\{\cosh\beta t+\frac{\sinh\beta t}{\sqrt{(1+\lambda^2)}}\right\}; \quad . \quad (26)$$

and the strength of the induced current at the same instant is

$$j=\frac{2ME(S-R)}{R(Lr-lS)\sqrt{(1+\lambda^2)}}e^{-\alpha t}\sinh\beta t. \quad . \quad . \quad . \quad (27)$$

We may notice that the expression for i contains only the *square* of M; that is, the square of M expresses the reaction of the secondary on the primary; hence when M is small this may be neglected.

Special Cases.

13. It will be interesting now to consider the special cases in which these equations (26) and (27) may be expected to assume simple forms.

Case 1. When the coils are so far apart that M^2 is negligible compared with Ll.

In this case $\lambda=0$, $\alpha+\beta=\frac{r}{l}$, $\alpha-\beta=\frac{S}{L}$; and it will be found that the expression for the primary current i as given in equation (26) reduces to the expression (4), which we found for the current in a solitary primary circuit, as it evidently ought to do; and there is nothing new to be said about it.

But the value of j, the current induced in the secondary circuit, becomes

$$j=\frac{\mathrm{ME(S-R)}}{\mathrm{R(L}r-l\mathrm{S)}}\left(e^{-\frac{\mathrm{S}}{\mathrm{L}}t}-e^{-\frac{r}{l}t}\right). \quad . \quad . \quad . \quad (28)$$

To find the induced current at instantaneous break in the special case now being considered, we must put $\mathrm{S}=\infty$, and we get

$$j=\frac{\mathrm{ME}}{l\mathrm{R}}e^{-\frac{r}{l}t}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (29)$$

which is the recognized value for it (see Chrystal, 'Encyc. Brit.' equation (41)). Its initial or maximum value is $\frac{\mathrm{M}}{l}\cdot\frac{\mathrm{E}}{\mathrm{R}}$; and it rapidly dies away.

To find the induced current at "make" we must put $\mathrm{R}=\infty$; and then (28) becomes

$$j=-\frac{\mathrm{ME}}{(\mathrm{L}r-l\mathrm{S})}\left(e^{-\frac{\mathrm{S}}{\mathrm{L}}t}-e^{-\frac{r}{l}t}\right). \quad . \quad . \quad . \quad (30)$$

This, therefore, begins at zero, rises to a maximum after the lapse of time

$$t=\frac{\mathrm{L}l}{\mathrm{L}r-l\mathrm{S}}\log\frac{\mathrm{L}r}{l\mathrm{S}},$$

and then dies away.

If the primary and secondary circuits are *similar*, so that

$$\frac{\mathrm{S}}{\mathrm{L}}=\frac{r}{l},$$

the expression for the induced current at make simplifies, becoming

$$j=-\frac{\mathrm{ME}}{\mathrm{L}l}te^{-\frac{r}{l}t}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (31)$$

14. *Case* 2.—When the primary and secondary circuits are wound side by side, so that the coefficient of mutual induction nearly equals the coefficient of self-induction of either—in other words, so that $\mathrm{L}=l=\mathrm{M}$.

For this case (see equations 22 and 25),

$$\lambda=\frac{4r\mathrm{S}}{(r-\mathrm{S})^2}, \text{ and } \alpha=\beta=\infty;$$

but if we put $\mathrm{L-M}=$ a small quantity z, say, and then proceed to the limit, we shall find a finite value for the difference of α and β, viz.

$$\alpha-\beta=\frac{rS}{L(r+S)},$$

while

$$\alpha+\beta=\frac{r+S}{2z};$$

and accordingly we get as the value of the primary current (26),

$$i=\frac{E}{S}\left\{1+\frac{S-R}{R(r+S)}\left\{re^{-\frac{rS}{L(r+S)}t}+Se^{-\frac{r+S}{2z}t}\right\}\right\}. \quad (32)$$

The last term in these brackets decreases at a nearly infinite rate; hence the primary current jumps almost suddenly from the value $\frac{E}{R}$ to the value

$$\frac{E}{R}\cdot\frac{r+R}{r+S}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (33)$$

and then increases (or decreases, as the case may be) at a more moderate pace, its subsequent values being given by the equation

$$i=\frac{E}{S}\left(1+\frac{r(S-R)}{R(r+S)}e^{-\frac{rS}{L(r+S)}t}\right). \quad . \quad . \quad . \quad (34)$$

The form of expression (32) when S is made very great is noticeable, as it shows that the primary current in a wire coiled up with a closed secondary is able to stop nearly as dead as if the primary were doubled upon itself (6). It is

$$i=\frac{E}{R}e^{-\frac{S}{2z}t}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (35)$$

The value of the induced current under the same circumstances (viz. when the two circuits are coiled close together) is easily obtained from (27), and is

$$j=\frac{E(S-R)}{R(r+S)}e^{-\frac{rS}{l(r+S)}t}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (36)$$

If $S=\infty$, this is the current at instantaneous break, viz.

$$j=\frac{E}{R}e^{-\frac{r}{l}t}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (37)$$

the same as (29) would have given; but if $R=\infty$, it is the current at make, viz.

$$j=-\frac{E}{r+S}e^{-\frac{S}{r+S}\cdot\frac{r}{l}t}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (38)$$

The induced current at *make* in this case therefore has an instantaneous maximum value $\frac{E}{r+S}$ and it then dies away at about half the rate of the current at break.

All this is very instructive. It must be the state of things approximated to in many arrangements.

15. We can now see what happens in Mr. Grant's case of a primary wound alongside a secondary, whose circuit can be closed or unclosed; though the state of things considered in the last section is only roughly attained, because the telephone-coil forms part of the battery-circuit, and adds materially to the self-induction of that circuit without contributing to the mutual induction. However, neglecting this and taking the circuits similar, we find the primary current at "make" ($R=\infty$, $S=r$), by equation (32), to be

$$\frac{E}{S}\left(1-\tfrac{1}{2}e^{-\frac{S}{2L}t}-\tfrac{1}{2}e^{-\frac{S}{z}t}\right),$$

whereas if the secondary circuit had been unclosed it would have been (5)

$$\frac{E}{S}\left(1-e^{-\frac{S}{L}t}\right).$$

Similarly, at the break ($S=$ large, $R=r$), the primary current, when the secondary is closed, is

$$\frac{E}{R}e^{-\frac{S}{2z}t}$$

instead of

$$\frac{E}{R}e^{-\frac{S}{L}t}$$

when it was open.

Hence the initial or maximum rate of variation of the battery-current either at make or break when the secondary current is closed, is to its initial rate of variation when the secondary is unclosed as L is to $2(L-M)$,—a ratio which, in the case supposed, is nearly infinite.

The Theory of Tertiary Currents as produced in an induction-balance—that is, in a coil which is arranged so as to be conjugate to the primary coil, but which is in the neighbourhood of a third coil or conductor affected inductively by the primary current.

16. This is the case of the induction-balance disturbed by a coin. The primary and secondary circuits are arranged in some conjugate position so that they have no effect on each

other—this being done practically by splitting each into two equal parts, and arranging the two parts so as to oppose each other; and the third circuit is represented by the piece of metal put near or inside the primary and secondary coils. The primary circuit contains as part of itself a battery and an intermitter; the secondary contains a telephone.

Neglecting electrostatic capacity and leakage (which latter, by the way, must be carefully avoided in a good balance), the current in either primary is the same, and the current in each secondary is the same, not only on the whole, but at every instant of time. In fact, the separation of the two halves of the primary and secondary is immaterial to the theory. The current which is induced in the secondary circuit, and which is heard in the telephone, is a tertiary current produced by induction from the third coil or piece of conducting matter.

The currents in the battery-circuit at any instant being called i, that induced in the metal being k, and that induced by this in the secondary circuit being called j, and being the value appreciated by the telephone, the equations for determining these three quantities are given in § 5, on the supposition that the coefficients of mutual induction between the metal and the two coils respectively (m and μ) are small numbers, which is always true in practice.

The battery-circuit is then practically undisturbed by the presence of the other parts of the instrument, and may be regarded as in space by itself (see end of §§ 12 and 13); so that all we have said about the intermittence of currents in such a circuit in sects. 6–11 applies to the primary of an induction-balance: we will, however, only take the case when the resistance jumps suddenly from any finite value R to another finite value S. The value of i is then given by equation (4); and its rate of variation is

$$\frac{di}{dt}=-\frac{\mathrm{E(S-R)}}{\mathrm{RL}}e^{-\frac{\mathrm{S}}{\mathrm{L}}t};$$

but as (equation 2)

$$\lambda\frac{dk}{dt}+\rho k+\mu\frac{di}{dt}=0,$$

it is easy to determine k; viz.

$$k=\frac{\mu\mathrm{E(S-R)}}{\mathrm{R}(\mathrm{L}\rho-\mathrm{S}\lambda)}\left\{e^{-\frac{\mathrm{S}}{\mathrm{L}}t}-e^{-\frac{\rho}{\lambda}t}\right\}. \quad . \quad . \qquad (39)$$

This value agrees exactly with that obtained in the general case (§ 12) when the condition that M^2 might be neglected was put in (see eq. 28), and therefore confirms the correctness of our working. All that was said about the special cases of j in

§ 13 is therefore now true of k. What we want to find, however, is not k but j, the current induced in the telephonic circuit; hence, differentiating the above expression for k and substituting it in

$$l\frac{dj}{dt}+rj+m\frac{dk}{dt}=0,$$

we get, remembering the initial condition that $j=0$ when $t=0$,

$$j=-\frac{m\mu\mathrm{E}(\mathrm{S}-\mathrm{R})}{\mathrm{R}}\left\{\frac{r}{(\mathrm{L}r-l\mathrm{S})(l\rho-\lambda r)}e^{-\frac{r}{l}t}\right.$$

$$\left.+\frac{\rho}{(l\rho-\lambda r)(\lambda\mathrm{S}-\mathrm{L}\rho)}e^{-\frac{\rho}{\lambda}t}+\frac{\mathrm{S}}{(\lambda\mathrm{S}-\mathrm{L}\rho)(\mathrm{L}r-l\mathrm{S})}e^{-\frac{\mathrm{S}}{\mathrm{L}}t}\right\}. \quad (40)$$

And this is the current heard in the telephone.

The expression $\int_0^\infty jdt$ identically vanishes; hence this current cannot affect a galvanometer (see § 1).

At "make" $\mathrm{R}=\infty$, and the three terms in the brackets are all present, the factor outside reducing to $+m\mu\mathrm{E}$. But at "break" $\mathrm{S}=\infty$, and the third term disappears, leaving

$$j=\frac{m\mu\mathrm{E}}{\mathrm{R}(l\rho-\lambda r)}\left\{\frac{r}{l}e^{-\frac{r}{l}t}-\frac{\rho}{\lambda}e^{-\frac{\rho}{\lambda}t}\right\}; \quad . \quad . \quad (41)$$

which is the tertiary current at break, and has some of the characteristics of the secondary current at make, see equation (28).

17. If the battery and telephone circuits are *similar*, so that $\frac{\mathrm{S}}{\mathrm{L}}=\frac{r}{l}$, the value of j at the make simplifies considerably, becoming

$$\frac{m\mu\mathrm{ES}}{\mathrm{L}l(\lambda\mathrm{S}-\mathrm{L}\rho)}\cdot te^{-\frac{r}{l}t}\Big\}, \quad . \quad . \quad . \quad . \quad (42)$$

which in this case therefore bears a constant ratio to the secondary current at make under the same circumstances (see equation 31).

Theory of the Induction-balance continued.

18. Having now integrated equations (2) and obtained an expression (40) for the current passing through the telephone at any instant, several things may be noticed about it. First, the current produced by the piece of metal is in general different in character to that produced by a slight shift of the secondary coil (that is, by M being made not quite zero); and consequently it is impossible in general to balance the effect

of metal completely by moving the other pair of coils a little further apart, though it may be done partially. Both effects would then be superposed, and the two sounds would be separately heard and might differ in pitch, because there would be more oscillations in the tertiary current than in the secondary. It is quite possible that a musical ear would perceive that the quasi-note produced by the insertion of a piece of metal contains a trace of a tone an octave above that produced by a shift of the coils. Equation (42), however, appears to show that when the battery- and telephone-circuits are *similar* the secondary and tertiary currents are expressible by the same function of the time, and that, therefore, in this case the effect of introducing metal *may* be counteracted by a shifting of the coils.

Next, we see that the effect is independent of M (that is, of the mutual induction between the primary and secondary coils); and therefore it may occasionally be well to place them in some nearly conjugate position, so as more easily to obtain a balance. In fact the four coils are unnecessary; two coils placed so as not to act on each other will do; and a piece of metal placed near them will produce disturbance; only it will not be so easy to bring the metal close or parallel to both coils.

Next the effect is proportional to the produced $m\mu$; of which μ is the potential of the primary, and m that of the secondary, on the coin. Hence, if the two coils are equal in size, every thing is symmetrical with respect to them, so that it does not matter whether the coin is nearest the primary or the secondary; moreover the middle point halfway between the two coils must be either a maximum or a minimum position of the coin.

Prof. Hughes long ago stated, from experimental observation, that the position of the coin which gave the loudest sound was half way between primary and secondary, the two coils being pretty close together and being equal in all respects. Before investigation I had felt inclined to doubt whether this were more than approximately true (as it was by no means evident *à priori* that every thing was symmetrical with respect to both coils, and the middle of the primary coil seemed a not improbable place for the maximum). Accordingly I arranged two pairs of coils as an induction-balance, the components of each pair being 4 or 5 inches apart, but with their planes parallel as usual; and I then moved a coin about along the common axis of one pair to see what happened. The maximum was *not* in the middle; but there were two maxima, one on each side the middle, and every thing appeared symmetrical with respect to both coils. The preceding investigation throws light upon this, and reconciles the two observations. It turns out (as might have been anticipated) that Professor Hughes's statement is quite correct when the coils are near together; but if they are separated by a distance greater than the diameter of either (the coin being supposed small), the middle point becomes a *minimum* with a maximum on each side of it (see next section).

On the Law according to which the disturbance produced by a small coin on the common axis of a pair of coils in an induction-balance depends on its position.

19. The general expression for the induction-coefficient between two circular coils in any position whatever is given in 'Maxwell,' art. 696. We can specialize this to the case required, viz. a coil of n turns and a "coin" or coil of one turn, both on the same axis. Let the mean radius of the "coil" be a, and let the distance of its circumference from some point O on the axis be c. Also let the mean radius of the "coin" be b, and let d be the distance of its centre from the point O. Then the mutual induction-coefficient between the coil and the coin is (art. 699)

$$2n\pi^2\frac{a^2b^2}{c^3}\left\{1+3\frac{\sqrt{(c^2-a^2)}}{c^2}d+6\frac{c^2-\frac{5}{4}a^2}{c^4}(d^2-\tfrac{1}{4}b^2)+\&c.\right\}.$$

Now d we can make zero at once by taking the point O at the middle of the coin, so that c is the distance from the mean circumference of the coil to the middle of the coin; and there remain inside the brackets of the above expression 1 + terms involving the square and higher powers of $b:c$, which is a small quantity. Thus a very good approximation to the induction-coefficient between the primary and a small coin is

$$\mu=\frac{2\pi^2a^2b^2n}{c^3}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (43)$$

Similarly the induction-coefficient between the secondary and the coin is the same expression with dashed letters; hence the product

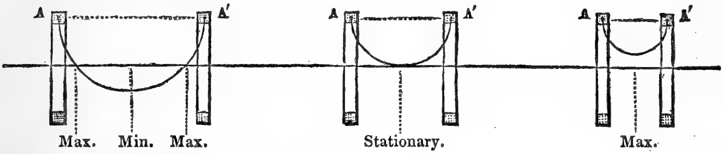
$$m\mu=\frac{nn'}{(cc')^3}(2\pi^2b^2aa')^2. \quad . \quad . \quad . \quad . \quad . \quad . \quad (44)$$

And this is a maximum when cc' is a minimum, a thing which is very easily represented geometrically.

For let A and A′ be points on the mean circumferences of the coils from which c and c' are measured; draw the system of lemniscates cc' = various constants, and then draw through the system any straight line at a distance a from A and a' from A′.

This line will represent the common axis of the two coils, and points on it represent possible positions of the coin. Now the line cuts every lemniscate in four points: hence there are four points on the axis at which the coin produces the same disturbance. These can run together two and two at points where the line is touched by the lemniscate; and these are the maximum or minimum points. They are maxima if the line is touched on the side of A A′, but minima if the line is touched on the other side.

If the coils are the same size (*i. e.* if $a=a'$), and if they are at a proper distance apart (viz. $=2a$), all four points can run together in the middle, which is therefore then a stationary point, so that moving the coin a little either way will produce very little diference. But if the coils are closer together than this, two of the points of section become imaginary, and hence there are now only two positions of the coin which give the same noise; and these two can run together in the middle, which is now a maximum.



Analytically the problem might be stated thus:—Find the max. and min. values of $\sin\theta\sin\theta'$, given the condition $a\cot\theta+a'\cot\theta'=$ const., θ and θ' being the angles subtended by mean radii of the primary and secondary coils at the coin. If the coils are equal ($a=a'$), the solutions are $\theta=\theta'$ and $\theta+\theta'=\frac{1}{2}\pi$; hence another way of stating the result is the following:—Draw a circle with A A′ (the mean points of the equal coils) as diameter: then if this circle cuts the common axis of the coils, the points of section are maxima, and the middle point is a minimum; but if the circle does not cut the axis, the middle point is a maximum, and the only one (see figure).

The law of decrease with distance is interesting: equation (44) shows us that when the two coils are close together and the coin is moved along the axis away from them, the effect which it is able to produce in the telephone varies pretty nearly as the inverse *sixth* power of the distance as soon as it has got a little way—a tremendously rapid rate of decrease.

It also shows that the effect varies directly with the fourth power of the diameter of the coin.

The Connexion between Induction-balance Effect and Conductivity.

20. The way in which the telephone-current depends on the conductivity $\frac{1}{\rho}$ of the coin is apparent in equation (40): it is evidently not simply proportional to it in general. But the self-induction λ of a circuit like that in a solid disk of metal must always be a very small quantity; and, except perhaps for highly conducting metals, it must probably be almost negligible compared with ρ. Assuming, then, that λ is infinitesimal, the expression simplifies, and ρ becomes a factor of one part of it. We get, in fact,

$$j=\frac{m\mu \mathrm{E}(\mathrm{S}-\mathrm{R})}{\rho \mathrm{R}(\mathrm{L}r-l\mathrm{S})}\left\{\frac{\mathrm{S}}{\mathrm{L}}e^{-\frac{\mathrm{S}}{\mathrm{L}}t}-\frac{r}{l}e^{-\frac{r}{l}t}\right\}$$

$$+\frac{m\mu \mathrm{E}(\mathrm{S}-\mathrm{R})}{\mathrm{R}(\mathrm{L}-l)}e^{-\frac{\rho}{\lambda}t} \quad . \quad . \quad . \quad . \quad (45)$$

The term written by itself does not contain ρ except as an exponential; but it has a very great rate of variation depending directly on ρ, and it soon ceases to exist. The rest of the expression for the current is simply proportional to the conductivity, both as regards its own value and its rate of variation. What the precise meaning of all this is depends on what the loudness of the telephone-indication definitely depends on (§ 1); so for the present we will leave it in this state.

On Measuring with the Induction-balance.

21. Two methods have been employed both by Professor Hughes and by Mr Roberts, with the view of obtaining quantitative readings from the balance. The first consisted in estimating the loudness of the sound produced by the insertion of a coin, and then imitating it by an arrangement of primary and secondary coils called the "sonometer"—a key being used to transfer the connexions of the telephone quickly from one instrument to the other. Mr. Poynting has adapted a formula from Maxwell which suffices to graduate the sonometer when the secondary coil is some distance from either primary (Phil. Mag. January), though it is rather an unwieldy one.

But there must always be some objection to readings taken in this way, because of the difficulty of estimating precisely when two sounds have the same strength—especially if, as appears probable from § 18, they differ at all in quality, the sound produced by the coin in the balance having a tone mixed up with it which is shriller than any produced by the sonometer. (I do not know whether this has ever been noticed experimentally. One often notices a change of pitch in the telephone-rustle; but it appears to depend in some cases on the direction of the current, *i. e.* on whether it strengthens or weakens the magnet.)

If the sonometer is to be used, I would suggest that the second primary coil be done away with, and that the secondary coil be of small diameter. It may have as much wire on it as is wanted for long range; but the further it is away from the primary, compared with its own diameter, the better. The diameter of the primary does not much matter. The induction would now simply vary as the inverse cube of the distance from the mean circumference of the primary; hence, if the sonometer were graduated so as to give equal distances from this line, its readings would only have to be turned upside down and cubed to give comparative results. If both primary and secondary coils are small, the sonometer-arm may be graduated like an ordinary millimetre-scale.

If an absolute zero were wanted at any time, it might be obtained by an arrangement for rotating one of the coils till it was at right angles to the other.

22. Prof. Hughes's second method of measuring, that of the graduated zinc wedge, is a much better one, because it is a null method and gives true readings, though they are not easily interpretable.

A third method has, I believe, been tried, viz. a copper damper rotating above one pair of coils; and this seems also pretty good; but I think it might possibly be better to modify it by having a uniform disk (or ring) of high-conductivity copper capable of being moved along the axis of one pair of coils with its plane always parallel to theirs. The distance of the disk from the middle point of the coils should be read, and the distance apart of the coils should be constant. Given all particulars, I have no doubt that a mathematician could readily interpret results so obtained.

Another modification would be to use a coil of wire forming a circuit closed within itself instead of the copper disk. It would have just about the same effect as a solid disk of the same size; but the data connected with it might be more definite.

The Effect of Magnetic Bodies.

23. If a thin piece of iron wire be held in the balance, it powerfully disturbs it by a concentration of the lines of force in the iron, so that M (the mutual induction between the

battery- and telephone-circuits) is no longer zero. If the iron were instantaneously magnetizable, one ought to be able to balance its effect by moving the other pair of coils a little nearer together, so as to make M zero again. But as the magnetization of iron always takes time, this is not completely possible, because, though M may be made zero at any one instant, it will not remain so during the period of variation of the current. As regards pitch, the rustle produced by iron wire should agree with that produced by a shift of the coils, both being duller than that produced by a piece of copper. Of course if a *mass* of iron be inserted, the effect will be composite, secondary currents being induced in it which give rise to tertiary currents in the telephone-circuit. But this effect is feeble as compared with the magnetic effect.

In measuring the conductivity of a metal or of an electrolytic solution by means of the induction-balance, care must be taken that it is not magnetic; or erroneous results will be obtained. The conductivity effect and the magnetic effect will tend partially to destroy each other; for though the effect of a piece of copper cannot be completely balanced by introducing a scrap of iron into the same pair of coils, it can partially, just as it might partially by shifting the coils (§ 18).

It would be very interesting to observe an opposite effect with a diamagnetic body—say a large bundle of thin bismuth wires insulated from each other; but it would not be easy to make sure of the correctness of the observation.

XVIII. *Examination of two new Amalgams, and a Specimen of Native Gold. By* WALTER FLIGHT, *D.Sc., of the Mineral Department of the British Museum*[*].

A SPECIMEN of "native silver" from Kongsberg proved, on analysis, to be an amalgam, and, as it appears to me, to be an amalgam new to science. The composition of the specimen in question was found to be :—

Silver . .	75·900	$\frac{75·900}{108}=0·70.$
Mercury .	23·065	$\frac{23·065}{100}=0·23.$
Insol. part .	0·490	
	99·455	

These numbers indicate the formula $Ag_3\,Hg$ as that of the amalgam, or a composition the converse of that of the amal-

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[*] Communicated by the Crystallological Society.

gam from Moschelladsberg, $AgHg_3$. It is worth recording how firmly and tenaciously mercury is retained by silver in the molten state. In one experiment, a fragment of the above amalgam was fused in a glass tube and kept at a bright red heat for more than ten minutes. The bead of silver, when cold, was beaten flat, cut into strips, and again heated, when what proved to be almost an additional one per cent. of mercury came off.

Another specimen of supposed "native silver" from Kongsberg was found to possess a somewhat different composition. The analysis gave the following numbers:—

	I.	II.	Mean.
Silver . . .	=92·454		
Mercury . .	= 7·022	7·369	7·195
Iron oxide . .	= 0·033		
Lime . . .	= 0·055		
Silver chloride	= 0·088		
Insol. part . .	= 1·328		
	100·980		

Calculated.

$Ag_{12}Hg$.	
Silver	=92·84
Mercury	= 7·16
	100·00

This is an amalgam having the formula $Ag_{12}Hg$, and is new to science. These two amalgams are the only ones which I have examined; and they both yield ratios which appear to indicate the existence of actual chemical compounds.

Travellers journeying through the Straits of Magellan stop at Punta Arenas. At this station the natives offer for sale washed gold in laminated grains and scales against its weight in sovereigns. A specimen of this gold, presented to the Mineral Department by my friend Mr. C. L. Claude, of Valparaiso, proved to have the following composition:—

Gold	=91·760
Silver	= 7·466
Copper	= 0·248
Iron oxide . . .	= 1·224
	100·698

XIX. *Notices respecting New Books.*

A Theoretical and Practical Treatise on the Manufacture of Sulphuric Acid and Alkali, with the collateral Branches. By GEORGE LUNGE, *Ph.D., F.C.S., Professor in the Polytechnic School in Zurich, formerly Manager of the Tyne Chemical Works.* London: John Van Voorst, Paternoster Row. 1879. (8vo, pp. 685, and 309 woodcuts.)

THE first volume of Professor Lunge's exhaustive monograph on our leading chemical industry must be ranked with a well-known work on metallurgy, of which, as metal-workers, we may be justly proud.

As Professor Lunge says in his Preface, the treatise is intended to supply various wants, and appeals to various classes of readers, and, we hope, careful studiers; for in hardly any other text-book or manufacturing treatise have we found such a mass of well-told general information and accuracy of minutiæ and detail as in this work of Professor Lunge's, who also may be deservedly complimented on his vigorous style and use of an acquired language.

In a brief and pithy introduction the Professor says "the manufacture of sulphuric acid and alkali is the foundation of the whole Chemical Industry of our time," a statement with which we fully agree.

The earlier chapters of the book treat on Oxides of Sulphur, on Chemical and Physical Properties, Sulphuric Acid, and working methods of analysis, passing to a long chapter on raw materials of manufacture, well supplied with Tables and illustrations.

But it is the portion of the work treating on the production of Sulphurous Acid from Pyrites, the recovery of Nitrogen compounds, and concentration of Sulphuric Acid, which will be more especially valuable to manufacturers and practical men—very large and carefully executed drawings of plant and apparatus, mostly to scale, and working formulæ being very plentiful.

Under "The Denitration of Vitriol" the Glover tower is fully described, and its drawbacks and merits somewhat more exhaustively discussed than is perhaps now necessary.

Under "By-products of Manufacture" we are pleased to see that, in addition to Silver, the recovery of Selenium and Thallium, and various processes for the making of fuming acid, receive a good share of attention.

In the presence of so much correct and useful matter in this first volume, from which errors of all kinds seem to have been most thoroughly sifted, we wonder what the second may contain; but we can safely prophesy the same success that cannot fail to attend the present one.

The Mode of Formation of the Granitic Effusions of Neudeck and Carlsbad, and the History of the Stanniferous Rock-formation in the Erzgebirge. A Study by Ed. Reyer. (Tektonik der Granitergüsse von Neudeck und Karlsbad, und Geschichte des Zinnbergbaues im Erzgebirge. Studie von Ed. Reyer.) Reprint from the 'Jahrbuch d. k.-k. geol. Reichsanstalt,' vol. xxix. pt. 3, pp. 405 *et seq*. Vienna, 1879.

This memoir divides itself into:—1. A study of the relations of the granite and schists; 2. A discrimination of the various local kinds of granite, as being necessary for a knowledge of the nature and origin of the granitic masses—with both earlier descriptions of the several granites and the author's own researches; 3. The history of the most important tin-bearing rocks and tin-works of the district. These chapters are illustrated with several woodcuts, among which is a plan of the country between Carlsbad, Ebenstock, and Schwarzenberg. The literature relating to the subjects above mentioned is fully indicated in each division.

As to the origin and subsequent conditions of such granite-systems as these of North-western Bohemia, the author considers that, in deep water, the erupted rock-material issued from a series of parallel fissures, and generally became crystalline on account of the pressure. These eruptive masses formed themselves into one great whole. The fissures were enlarged, and gave out more and more eruptions, which formed broadly obtuse cones; and these have their main axis coincident with the strike of the fissures. Fine-grained mud-like laminæ also were intercalated on the sides of the cones among the erupted matter. Here and there the consolidated crust burst, and the still liquid rock below was injected into the new fissures, thus giving rise to *apophyses*, which vary in appearance from fine-grained rock below to veins of segregation above. Ultimately the whole was covered with tuff and deep-sea mud.

The massif thus formed, having become dry land in course of time, was subjected to erosion. The soft shaly coating was gradually removed; and the upper crust of the ejected rock-mass was then worn away, until the cone-like injections appeared. Lastly the granitic mass, having been furrowed throughout, was finally removed, leaving a system of granite veins, more or less linear and concentric, in the old schists.

On the Structure of the Volcanos of Bohemia. By Ed. Reyer. (Ueber die Tektonik der Vulcane von Böhmen, &c.) Reprint from the 'Jahrbuch d. k.-k. geol. Reichsanstalt,' vol. xxix. pt. 3, pp. 464 *et seq*. Vienna, 1879.

The relation of the North-Bohemian series of volcanos to the Erzgebirge is indicated; and then the concentric, onion-like, and bulbous arrangement of the phonolitic material in the Schlossberg of Teplitz, previously noticed by Reuss, is dwelt upon. The volcano of Priesen is especially described and illustrated, as belonging to the same kind of concentrically laminated mass, formed by the successive ejection of mass within mass of tough or viscid molten felspathic rock from a central vent below.

Reyer notices its radiating veins or dykes, their nature and direction as shown by the vesicles in the magma having been dragged out by the stream or current, also its central tuff basin as the relic of the crater, the groin-like ridges radiating from the tuff centre, and the radiating lines of erosion on the surface.

He also points out that the two most simple of the typical kinds of volcanos are :—1. The tuff cone ; 2. The effused or effluvial volcanic hill (Quellkuppe), more or less dome-like. In the first case a highly hydrous magma, issuing from a fissure, and resolving itself into dust, has assumed the well-known conical form. In the other case a magma with less water in it has welled out, and, according to its consistency, and the levelness or slope of the ground, has either formed itself into a dome-like and bulbous hill, or has spread over the surface as a stream.

These types, however, are rarely met with in the simple form; most volcanos have something of each kind. The Kammerbühl, near Egra, is one of the least complicated instances of the association of the two types. Dana and Von Seebach divide volcanos into Ejection-volcanos and Tuff-volcanos, with reason.

The type represented by the Kammerbühl may be theoretically varied in several ways :—1. Had the eruptive fissure on which the Lesser Kammerbühl stands been larger, repeated eruptions would have taken place, giving rise to a larger isolated volcano, with a central tuff-cone, radiating veins, and peripheral streams. 2. Had the fissure reached the surface at more than one point, a *volcanic series* would have resulted. 3. Had the points of eruption been very near one another, they would have given rise to a *volcanic ridge* (a series united by fusion, or an elongated cone). The Bohemian forms of volcanos can be readily ranked under the above-mentioned types. As to their local distribution, M. Jokely mentions one case of *serial* arrangement along a line striking N.E. Most probably there are more than one linear series of fissures (as in other countries), and these all parallel to the Erzgebirge.

XX. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 74.]

January 7, 1880.—Henry Clifton Sorby, Esq., F.R.S., President, in the Chair.

THE following communications were read:—

1. "On the Portland Rocks of England." By the Rev. J. F. Blake, M.A., F.G.S.

The author gave a general account of the relation of the several Portland rocks in the areas of their development to each other, and hence deduced the history of the Portland "episode." The name is used on the continent in a wider sense than in England; and this use was shown to be unjustifiable. After giving an account of his observations on the rocks at Portland itself, and dividing the limestones into the building-stone and flinty series, the author showed that the so-called "Upper Portlandian" of Boulogne corresponds to the latter, and the upper part of the "Middle Portlandian" to the Portland Sand. He then endeavoured to prove by the proportionate thickness, the indications of change in the lithology, and the distribution of some of the fossils, that the rest of the so-called "Middle" and the "Lower Portlandian" are represented by integral portions of the Upper Kimmeridge, which are thus the "normal" form corresponding to what the author calls the "Boulognian episode." The series in the Vale of Wardour has been made out pretty completely. The Purbeck is separated by a band of clay from the Portland, and is not amalgamated with it. The building-stones and flinty series are here seen again; and a fine freestone occurs at the base of the latter. The representatives of the Portland Sand were considered to be older than those of other districts.

The relations of the Purbeck to the Portland rocks at Swindon were very carefully traced; and it was shown that, while the upper beds of the latter put on here some peculiar characters, the former lie on their worn edges. The upper beds of the Portland, which have been referred to the sand, correspond to the freestone and the base of the flinty series of the Vale of Wardour; hence the Purbecks of Swindon may be coeval with the upper beds of the Portland to the south. At the base of the great quarry and elsewhere in the neighbourhood are the "*Trigonia*-beds;" beneath which is clay, hitherto mistaken for the Kimmeridge Clay; and beneath this are the true Portland Sands, with an abundant fauna new to England. The limestones of Oxfordshire and Bucks were considered to represent the "*Trigonia*-beds" only; and as the Purbecks here lie for the most part conformably, it was suggested that they were formed in a lake at an earlier period than those at Swindon, which are of a more fluviatile character.

Hence the Portland episode, considered as marine, was at an end in the north before it was half completed in the south.

2. "On the Correlation of the Drift-deposits of the N.W. of England with those of the Midland and Eastern Counties." By D. Mackintosh, Esq., F.G.S.

The object of the author was not to dogmatize, but to present the subject in a concise form so as to stimulate to further research. His scheme of correlation was founded on the horizontal continuity of the deposits and their included erratics. He gave an account of his discovery of the continuous extension of the upper Boulder-clay of Cheshire, above a great thickness of sand and gravel, as far as Berrington, south of Shrewsbury, and its appearance at intervals along the Severn valley to below Worcester, where it was probably represented by a bed with Malvern-hill boulders above shelly sand and gravel. He traced the great boulder-bearing clay and gravel around Wolverhampton eastward through Central England, to where it graduated into the chalky clay of Lincolnshire; and laid great stress on the commingling, at Wolverhampton, in this deposit, of erratics (chiefly granite and felstone) from the north with erratics (chiefly chalk-flints and gryphites) from the east. He described the clay and sand around Gainsborough, Retford, &c. He correlated the "carrion," or lower Boulder-clay of the Vale of York (containing Carboniferous, Jurassic, and granitic erratics), with the lower yellowish-brown clay of the Aire and Wharfe valleys and the plain of Craven. He likewise correlated patches of upper clay in the latter areas with the upper Boulder-clay of the Lancashire plain, but was not certain that they were of Hessle age. The solution of the main question depended chiefly on the relative age of the Wolverhampton and Stafford clay-and-gravel, which he was disposed to regard as the equivalent of the lower brown Boulder-clay of the N.W. and likewise of the chalky clay of Lincolnshire. He concluded by considering facts which might be regarded as opposed to this view, and by giving his reasons for regarding the palæontological evidence of the relative age of deposits as not, in all cases, reliable.

January 21.—Henry Clifton Sorby, Esq., F.R.S., President, in the Chair.

The following communications were read:—

1. "On the Genus *Pleuracanthus*, Agass., including the Genera *Orthacanthus*, Agass. & Goldf., *Diplodus*, Agass., and *Xenacanthus*, Beyr." By J. W. Davis, Esq., F.G.S.

2. "On the Schistose Volcanic Rocks occurring on the west of Dartmoor, with some Notes on the Structure of the Brent-Tor Volcano." By Frank Rutley, Esq., F.G.S.

Among the ashy beds of this district are certain amygdaloidal schistose rocks, which the author is of opinion are really lava-flows, which have probably been crushed or infiltrated, and have so assumed a foliated structure owing to pressure from superincumbent beds acting on rocks thus constituted. They are much altered, but were probably once basalts. The author considered it very probable that these schistose beds and Brent Tor, considered to be of Carboniferous age, are identical with beds near Tavistock and in the Saltash district, which are of Upper Devonian age.

In the concluding part of the paper the author described the beds of alternating ashes and lava, now much disturbed by faults, which constitute all that remains of the ancient Brent-Tor Volcano, and endeavoured, from the evidence which can be thus obtained, to give a probable reconstruction of the former cone.

3. "On Mammalian Remains and Tree-trunks in Quaternary Sands at Reading." By E. B. Poulton, Esq., F.G.S.

XXI. *Intelligence and Miscellaneous Articles.*

ON MAGNETS. BY O. A. S. PIHL[*].

THE author, in his comprehensive treatise, seeks to trace the attraction of armature and magnet to the distribution of the magnetism in the two. He first describes his multifarious and careful experiments on the distribution of the magnetic moments in a straight magnet when both poles are free or when iron rods are placed at the ends, or an iron rod laid between two like or unlike poles of magnets, and likewise in these iron rods and in one with its extremities placed between the arms of a horse-shoe magnet. The determinations were made by means of induction-spirals after the method of van Rees. In like manner were the attractions of the magnets for their armatures at various distances determined by forcible separation. It would carry us too far to specify the particular results, which, for the first determinations, fall in with those obtained by Erman, van Rees, Weihrich, and others, as they also are to be deduced from the putting into position of the molecular magnets (gradually becoming feebler from the operating force onwards). Beside the graphically represented distribution of the moments, the above-sketched distributions of the free magnetisms are deduced.

Accordingly, if we imagine the magnets and armatures made up of slices which contain each the calculated amount of free magnetism, we can calculate the attraction exerted by each slice of the magnet upon each slice of the armature in juxtaposition with it, assuming that it is distributed uniformly over the slices. The sum of all the attractions should give the attraction of magnet and armature. The calculated and the observed values, however, are not at all equal: if the armature be brought nearer to the magnet, the ratio of the observed to the calculated attraction diminishes, until at a distance of about $\frac{5}{8}$ of the radius it reaches its minimum and then with lessening distances increases. The deviation, at all events, lies in the well-known more copious distribution of free magnetism at the places more remote from the axis of the magnet-slices, which can be observed by removing them by corrosion &c., but cannot be accurately determined, since herewith the distribution changes. On the approximately true assumption that one portion of the magnetism is distributed uniformly over the cross section, another only on the periphery of the slices, at greater distances results are obtained agreeing better with the observations.

If the diameters of armature and magnet are unequal, the attraction becomes different, and, correspondingly, the magnetisms distributed in it are otherwise arranged, and of another amount, than with an armature of equal thickness, as the curves constructed by the author show.

On placing two permanent steel magnets in juxtaposition the distributian in the two is likewise altered; only the direct action is

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[*] Christiania, 1878: pp. i–xiv, and 1–135; 6 plates.

more powerful, but the propagation of the rotation of the molecules feebler, than with an armature of soft iron and a magnet.

In regard to the numerous details and further results of this painstaking investigation we must refer our readers to the original work.—*Beiblätter zu den Annalen der Physik und Chemie*, 1880, No. 1, pp. 73, 74.

ON THE THERMAL ABSORBENT AND EMISSIVE POWERS OF FLAMES, AND ON THE TEMPERATURE OF THE VOLTAIC ARC. EXTRACT OF A LETTER FROM FR. ROSETTI TO A. CORNU.

Conclusions.—1. The transparency of flames is very great; consequently flames absorb very little of the thermal radiation which traverses them. If the radiation proceeds from a flame of the same nature, and if the flame through which it passes has a thickness of 1 centimetre, the coefficients of transparency and absorption are represented by the numbers 0·865 and 0·135 respectively, for the white flames produced by illuminating-gas, as well as for the pale blue flames of Bunsen burners.

2. The transparency diminishes and the absorption increases proportionally, if the thickness of the flames is augmented. If the flame has an infinite thickness, it is athermanous; that is to say, it absorbs all the heat-rays proceeding from a flame of the same nature: the transparency is *nil*, the absorption equal to unity. But these limits are nearly reached with finite and not very great thicknesses; 1 metre thickness is sufficient to render a flame almost completely athermanous to rays coming from another flame of the same nature.

3. The formula $y = a\,\dfrac{1-k^{e}}{-\log \mathrm{h}\,k}$ very well represents the intensity of the thermal radiation emitted by flames of any thickness ϵ whatever, the transparency-coefficient being $k = 0{\cdot}865$, the thickness ϵ being expressed in centimetres, and a being a constant the value of which depends on the nature of the flame.

4. The *absolute* thermal emissive power of the *white* flames produced by illuminating-gas (that is to say, the intensity of the radiation from such a flame possessing an infinite thickness, compared with the intensity of the radiation emitted by lampblack at a temperature equal to the mean temperature of the flame) is equal to *unity*.

The *absolute* thermal emissive power of the *pale blue* flames produced by Bunsen burners is represented by the fraction 0·3219; that is, it is nearly *one third* of the emissive power of the white gas-flames.

5. The *relative* emissive power of a flame of a determined thickness can be obtained by multiplying the ratio between the intensity of its radiation and the *maximum* intensity (the intensity of the radiation from the same flame if its thickness were infinite) by the number which represents the absolute thermal emissive power of that species of flames.

A pale blue Bunsen-flame with a thickness of 4 millims. has its emissive power expressed by the number 0·01744; that is to say, lampblack raised to the same temperature sends out a thermal radiation whose intensity is $\frac{1}{0{\cdot}01744}=57{\cdot}73$, compared with that of the flame.

6. The electric light includes, as is known, two sorts of radiations —viz. the rays emitted by the incandescent carbons, and those emitted by the voltaic arc which springs between the polar extremities of the carbons. The former give a white, the others a purple-blue light; the resulting light is bluish white.

7. The two polar extremities have temperatures very different the one from the other. The number of degrees which expresses their temperature can be deduced from the formula

$$y=m\mathrm{T}^2(\mathrm{T}-\theta)-n(\mathrm{T}-\theta),$$

supposing that the carbons have the maximum emissive power.

8. The voltaic arc has very little thermal emissive power, comparable to that of the pale blue flames of Bunsen burners. Its temperature can also be obtained with the aid of the preceding formula, provided that the value of the thermal emissive power of the voltaic arc relative to its thickness be introduced.

9. A great number of experiments have given the maximum temperature of about 3900° C. for the positive, the temperature of about 3150° for the negative polar extremity of the carbon. For the voltaic arc which springs between these two extremities the temperature was always about 4800°, whatever the thickness of the arc and the intensity of the current.—*Comptes Rendus de l'Académie des Sciences*, Nov. 10, 1879, t. lxxxix. pp. 781–783.

ON SOME NEW LUMINOUS TUBES. BY M. TRÈVE.

If the induced current of a Ruhmkorff coil is passed into a Fizeau's condenser, the vibratory motion of the coil is returned, amplified, by the condenser in the form of a well-known kind of humming sound. If pressure, at first slight, and then gradually increasing, be exerted upon the condenser, the humming successively diminishes in intensity and at last dies away altogether.

The cessation of the sound will be coincident with a pressure capable of expelling from the condenser the air which was interposed in the intervals between its tinfoil sheets. The condenser is then as in a vacuum; and no sound can issue from it. This observation led me to make the following experiment.

Into a large Geissler tube I introduce a Fizeau's condenser. The two poles of the induced current of the Ruhmkorff end at the condenser through the intermediation of the ordinary electrodes of this kind of tubes, which, fused into the glass, are fixed to the eleventh and twelfth sheets of tinfoil*.

When the induced current passes into the condenser, the tube still containing air at the atmospheric pressure, the usual humming is heard. If vacuum be gradually produced in the tube, the hum-

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* I experimented on a condenser of twelve sheets, carefully prepared by MM. Alvergniat.

ming grows more and more feeble. At last, if air be withdrawn until only a pressure of 3 or 4 millims. is left in the tube, the ear can no longer perceive any thing; but a brilliant white light appears, springing in the form of beads from the tinfoil of the condenser, and absolutely distinct from the pale and indistinct phosphorescent light of Geissler tubes.—*Comptes Rendus de l'Académie des Sciences,* Jan. 5, 1880, p. 36.

ON THOMSON'S GALVANOMETER. BY A. GAIFFE.

When electric currents are measured with the aid of Thomson's reflecting galvanometer, the indications read on its graduated scale are found not to be proportional to the values of the currents measured, and to be exaggerated in proportion as those values increase.

This cause of error arises from the fact that the angles of deflection of the magnetized needle are doubled by the reflection of the mirror, and consequently it is not the tangents of the real angles of deflection that one reads on the scale, but the tangents of the doubled angles. If it can be assumed without inconvenience that the tangent of the double of a very small angle is twice that of the simple angle, the case is not the same for large angles; and although the deflections of the needles of this class of apparatus are limited, with the aid either of a directing magnet or of derivations, to about 8°, the indications are already sensibly exaggerated at the extremities of the scale which the 8° include.

I essayed to remedy this defect by substituting a very fine platinum wire ($\frac{18}{1000}$ in diameter) for the untwisted thread of silk by which the system of needles of the galvanometer are suspended; but the want of proportionality was produced in the opposite sense, and was more considerable.

I then had recourse to a bifilar suspension composed of two cocoon-threads very close to one another. The results were almost completely satisfactory. By this second means the errors are not entirely corrected; but they are reduced to less than one hundredth of the value measured.

The verification-experiments were made, using the whole extent of the graduated scale, with the aid of three condensers charged from one and the same constant source of electricity. The condensers were first charged and measured separately, and afterwards charged and measured in combination.—*Comptes Rendus de l'Académie des Sciences,* Jan. 12, 1880, t. xc. p. 94.

TIN-MINING IN BANKA AND BILITONG.

In the *Oesterreichische Zeitschrift für Berg- und Hüttenwesen,* vol. xxvii. Jahrgang 1879, Dr. Ed. Reyer has given a concise, though well detailed, notice of the nature and condition of the tin-mining in Banka and Bilitong, with a geological sketch map of the northeast district of the former island, and full references to former observers and writers on these works and the structure of the islands.

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

[FIFTH SERIES.]

MARCH 1880.

XXII. *Measurements and Law in Electro-optics. By* JOHN KERR, *LL.D., Mathematical Lecturer, Free Church Training College, Glasgow*[*].

MY last communication to the Philosophical Magazine contains an account of some electro-optic measurements in connexion with carbon disulphide, cumol, carbon dichloride, and several other liquid dielectrics. That set of measurements was very imperfect, and of little interest except as a first trial. In the leisure of my last summer holidays I resumed the inquiry with better means, and carried it forward for some weeks with great care. I had intended to examine several dielectrics in succession, but found time only for the first of them, which was carbon disulphide. These new experiments form the subject of the following paper.

1. The dioptric actions of dielectrics are generally of one kind—pure double refractions with reference to line of electric force as axis; but they vary largely in intensity, and vary even in sign, from one dielectric to another. Dielectrified bodies are therefore optically equivalent to uniaxal crystals, and exhibit like variations, both from strong to weak, and from positive to negative. Our present dielectric (carbon disulphide) is of the positive class; and in regard to strength, it holds, as yet, a place among dielectrics like that of Iceland spar among crystals. For the particular facts and the proofs I must refer to my papers already published[†].

I consider it probable in the highest degree that these optical

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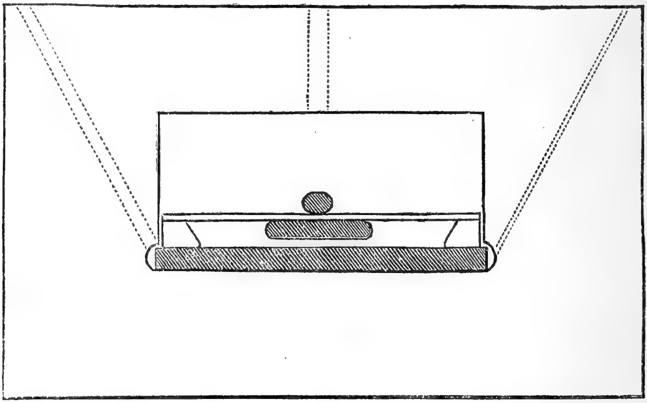
[*] Communicated by the Author.

[†] Philosophical Magazine, Nov. and Dec. 1875; also Aug. and Sept. 1879.

actions are subject to uniform laws in media of the one class as of the other, in dielectrics as in crystals. I venture therefore to accept the principal result of my present experiments as a general law, and to enunciate accordingly, although the actual measurements were limited to one dielectric.

LAW.—*The intensity of electro-optic action of a given dielectric—that is, the quantity of optical effect (or the difference of retardations of the ordinary and extraordinary rays) per unit of thickness of the dielectric—varies directly as the square of the resultant electric force.*

2. To prepare the way for an account of the experiments, I must begin with a description of the cell which contains the dielectric. The cell-block is composed of three thick slabs of plate-glass, which stand on the experimental table, with their plate-faces vertical and in close contact.



The diagram gives an end view of the block. The two dimensions exhibited in the diagram are about 10 and 6 inches; the third dimension of the block, or joint thickness of the slabs, is exactly $3\frac{15}{16}$ inches. The inner rectangle in the diagram represents a tunnel, which passes pretty uniformly through the block, leaving wide margins of polished surface on all the faces of the slabs. As in the case of the old plate cell, the tunnel is closed by two panes of thin and carefully selected plate glass, which lie in contact with the ends of the block. Outside of the panes are two thick plates of india-rubber cloth; and outside of the cloths are two strong planks of mahogany, which are a little wider horizontally than the slabs of glass. The planks are connected by a strong screw-bolt at each end. The tunnel is continued fairly through the cloths and planks.

When the cell is charged with carbon disulphide, and well closed by screw-pressure on the planks, it is almost perfectly tight, the trifling leakage being due to a flaw in the workmanship. The 4-inch plate of powerfully refractive and dispersive liquid thus obtained is perfectly transparent; and a beam of light, incident normally on either of the limiting panes, passes through the plate without sensible traces of dispersion, deviation, or depolarization. The absence of all cement in the construction of the cell is, I think, a most important condition. The liquid is in contact with nothing more than compact masses of metal and the glass walls of the cell; and accordingly, with a fair start, it may be kept sufficiently clean for any length of time.

The shaded pieces in the diagram represent the two conductors. The lower conductor is a thick plate of solid brass, which rests on the floor of the cell. The upper is also a plate of solid brass, of the same length and thickness as the lower, but much narrower: it is supported by a small rafter of thin plate glass at each end, rafter and conductor being connected by a bullet-headed screw, which passes freely through a fine hole drilled in the rafter, and works deeply into the conductor. Each of the rafters rests on the two trapezoidal pieces shown in the diagram. These are prisms of glass taken out of one thick plate, the horizontal surfaces (upper and lower) being primitive plate-faces kept intact. These lateral supports rest simply on the brass floor, and extend from front to back of the cell.

The opposed faces of the two conductors are perfectly flat and smooth, just as they left the planing-machine: they are also sensibly parallel, and generally distant from each other about one twelfth of an inch. All edges and corners on the upper conductor have been carefully rounded away, as well as the two adjacent edges on the lower conductor. The length of the conductors (at right angles to the plane of the diagram) is such as to leave the brass barely short of contact with the limiting panes.

The dotted lines in the diagram represent three borings in the central slab. The vertical boring admits a glass tube, within which passes a fine shaft of brass, resting below in a small cavity made in the upper conductor, and terminating outwards in a perforated knob and connecting-screw. The smaller boring on the right gives passage to a wire from the lower conductor; the larger boring on the left is used for charging the cell with liquid, and for emptying it, the latter operation being effected by means of a small siphon.

3. *Working of the Cell.*—The slabs, the closing-panes, and

all the pieces that lie within the cell are cleaned with the greatest care. The lower conductor is inserted in the central slab, and has its connecting-wire drawn up through the proper boring; and the cell-block is completed by the addition of the outer slabs. The lateral supports of glass are then put in position, also the upper conductor and the external knob connected with it. The conductors are adjusted stably, with their opposed faces exactly parallel. All the pieces are now cleared of dust as thoroughly as possible, and the cell is closed.

The liquid is introduced rapidly into the large boring, through filtering-paper and funnel properly placed. To prevent the formation of air-bubbles between the two conductors during the charging of the cell, it is necessary to have the plane faces of the conductors inclined a little to the horizon; and this is done by the temporary insertion of a thin slip of wood under one end of the cell-block. The charging is continued until the liquid stands well above the balls of the upper conductor; but the cell is never quite filled, as that might result in a very inconvenient explosion. When the borings are now stoppered and the cell firmly closed by screw-pressure, there is very little loss of the liquid by evaporation and leakage.

Sometimes, in spite of all precautions, the liquid is found to be not sufficiently clean, a quantity of solid particles gathering in chains between the electrified conductors, and masking the optical effect in some degree, or even discharging the upper conductor at comparatively low potentials. In such a case the charge is drawn off (as much of it as is possible) by the siphon; the cell is opened, and the faces of the conductors are swept clear of dust, which is done well by friction with narrow slips of clean writing-paper. The cell is then closed and charged anew.

These operations in preparation for experiment are sometimes very troublesome and tedious, a great deal more so than in the case of the old plate cell. Partly on this account, and partly because of the troublesome and expensive character of the workmanship, I consider the present cell very imperfect; but these and other faults are forgotten in the presence of the following phenomena.

4. *Electro-optic Experiment in Chromatic Polarization.*—Here, as in all the following experiments, the cell is charged with rather less than a pint of clean carbon disulphide. No other optical pieces are employed at present than the charged cell and a couple of Nicol's prisms.

A beam of light from a bright cloud is reflected horizontally into the room through a port in the window-shutter, passes through the first Nicol, then perpendicularly through the plate of liquid, then through the ocular Nicol. The pieces are so levelled and directed, that the observer at the polariscope looks fairly through the deep slit that separates the two conductors in the cell. I may mention again that the dimensions of the slit are about *one twelfth* of an inch, *one* inch, and *four* inches—the first dimension lying vertically as the lines of force, and the last lying horizontally along the line of sight. The two Nicols are fixed, the first with its principal section at 45° to the vertical, and the second at extinction, which is here quite pure. Wires are led permanently from the two conductors—from the lower to earth, and from the external knob of the upper to the prime conductor. To give greater steadiness and distinctness to the progress of the optical effect, the wire from prime conductor to cell is put in permanent contact with the knob of a Leyden jar, whose outer coating is uninsulated.

When the machine is set in motion at a moderate rate, the potential of the upper conductor rises slowly, and the black space between the two conductors is illuminated, the light passing gradually through impure black, faintly bluish grey, faint white, and so forward, up to a sensibly pure and brilliant white. Thus far there is nothing new, except that the highest potential yet reached is comparatively low, while the optical effect is very large, and already far beyond neutralization by the action of any hand compensator of strained glass.

As the potential of the prime conductor still rises, the polariscope gives a fine progression of chromatic effects, which descend regularly and continuously through a certain range of Newton's scale. The luminous band between the conductors passes first from white to a bright straw-colour, which deepens gradually to a rich yellow, then passes through orange to a deep brown, then to a pure and dense red, then to purple and very deep violet, then to a rich and full blue, then to green. All the colours are beautifully dense and pure, certainly as fine as any that I have ever seen in experiments with crystals in the polariscope.

Generally about the point last named of the scale of colours, at or near the green of the second order, the process terminates in spark-discharge through the liquid. Sometimes, but not frequently in my observations, it terminates at an earlier stage, to run its regular course at the next trial. The irregularity appears to be due to an accidental precipitation of discharge by the action of solid particles, impurities in the liquid.

Through this whole range of effect, from the pale blue or impure black of the first order to the green of the second order, the plate of electrically-charged liquid acts regularly as a uniaxal crystal, as a plate of quartz with optic axis parallel to the lines of force, the plate increasing in thickness continuously and very rapidly as the potential rises.

I may mention here, that with the old plate cell, under favourable conditions, I obtain one step of the scale of colours, that from white to yellow, but not easily nor very distinctly.

5. By simple observation in connexion with this experiment, the electro-optic field is seen to be sensibly uniform through the greater part of the space between the two conductors, and for potentials high or low.

Beginning at very low potentials, for which the optical effect lies below intense white, and applying a hand compensator of glass, I obtain, as the first effect of stress in the glass against electric force in the liquid, a couple of extinction-bands in the outer borders of the field, just as in my old experiments with the plate cell. As the stress of the glass, horizontal tension or vertical compression, is increased, the bands move inwards, till at last, as they pass through the borders of the field, and enter the space between the parallel faces of the conductors, they widen inwards and meet each other, coalescing in one wide and uniformly black band.

At higher potentials, and using only the two Nicols, I obtain equally distinct results. Thus, at one stage of the experiment the band of light between the two conductors is of a deep red, and sensibly uniform from end to end; but at each end, in the borders of the field, where the upper conductor is rounded off, the band is tipped with a narrow fringe of brilliant yellow. Similarly, another stage of the experiment gives a violet or blue band, sensibly of the same tint from end almost to end, and tipped at each end with a narrow fringe of dense red.

As an important practical effect of the characteristics of the new electro-optic field, its *intensity* and *uniformity*, I have been able to discard the old and imperfect compensator of strained glass. One such compensator that had been elaborately prepared was found quite inadequate, the action of a fine slip of glass (not an inch wide), under a compressing weight of 50 pounds, being neutralized and far exceeded by the action of the plate of CS_2 at low potentials.

6. *Rate of Disappearance or Degradation of the optical effect after the stoppage of the machine.*—Some observations were made upon this subject at different times; and the results are worthy of brief notice in passing. The arrangements were the same as in the preceding experiment.

The light is restored from extinction up to one of the tints; and the effect is kept pretty constant for several seconds.

When the machine is stopped suddenly, the tints disappear at once and the optical effect falls very rapidly to faint white. The subsequent rate of degradation, through faint white to pure extinction, varies very much with circumstances. In damp weather the light disappears in one or two seconds, or less; but on several occasions, the weather being favourable, and the external conductors very well insulated, I have seen the light sustained clearly for five and even for six minutes. This continuously sustained effect was certainly due to the present action of electric force at each instant; for it disappeared sharply and totally at the instant of spark-discharge of the prime conductor, and then, a few seconds after the spark, it was restored quite distinctly by the action of the residual charge of the jar.

The Leyden jar is now withdrawn, and the prime conductor is connected simply by a wire with the upper conductor in the cell, the procedure and circumstances of the observation being otherwise unaltered. There is nothing new observed in this case, except a more rapid rate of degradation of the optical effect. In favourable circumstances I have seen the effect still sustained clearly for a good many seconds—40 to 60.

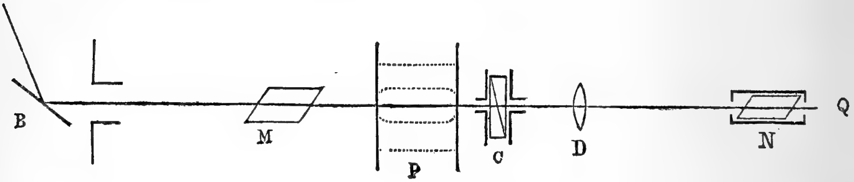
The permanent wire from cell to prime conductor is now detached from the cell, and its extremity is fixed in a small ball of brass, which is held by an insulating handle of glass. The polariscope being at extinction, and the machine in motion, the ball connected with the prime conductor is brought up towards the knob of the upper conductor in the cell, till one small spark passes. At the instant of the spark there is a strong restoration in the polariscope. The effect degrades at first very rapidly like a flash, but afterwards more slowly, so that it is still seen distinctly, though at last very faintly, for 12 or 15 seconds. And here, as in the former cases, the sustained effect is certainly due to the present action of electric force at each instant; for it vanishes instantaneously when the knob of the upper conductor is touched by the finger.

There are several points proved or illustrated by these observations:—that the birefringent action of the liquid is due to electrostatic force, and not to electric discharge in any form; that the dissipation of the charge of electricity in damp weather is principally from the external parts of the apparatus, as was to be expected; that within the cell there is always a sensible discharge of electricity as long as the two conductors are at different potentials; and that the rate of this discharge or dissipation through the liquid is very great at high tensions. I proceed now to the measurements.

7. *Instruments of Measurement.*—To measure potential and

optical effect respectively, I use a Thomson's Long-Range Electrometer and a Jamin's Compensator. These admirable instruments are now so widely known that any description of them here would be superfluous. Besides, they are described fully by the inventors themselves in books which are generally accessible*.

8. *Arrangements and Method.*—The diagram shows all the optical pieces, in horizontal section through the observer's eye, Q. Several of the pieces are placed exactly as in the first experiment (4). These are :—the reflector, B, in the window-shutter; the polarizing Nicol, M; the charged cell, P; and the ocular Nicol, N. The Nicols are fixed, here as formerly, with their principal sections at 90° to each other, and at 45° to the vertical. After the cell, and close to it, comes the compensator C, which is placed with great care and very stably, with the optic axes of its quartz prisms properly directed, one vertical and the other therefore horizontal, and both perpendicular to the ray. The narrow beam transmitted by the compensator, and received at Q, passes of course through the central parts of the electro-optic field. After the compensator comes a convergent lens D, which is fixed by trial in such a position as to give the observer at Q a distinct view of the black band and reference-wires in the compensator. Some changes were made afterwards in the optical arrangements; but I will describe these in the proper place.



Two wires are led permanently from the conductors within the cell—from the lower to earth, and from the upper to prime conductor. Wires are also led permanently from prime conductor to inductric or upper plate of the electrometer, and to the inner coating of a good Leyden jar whose outer coating is connected permanently with earth.

The index of the compensator being at the zero-point, the observer at Q sees a strong black band exactly midway between the two parallel reference-wires. When the machine is set in

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* For the Electrometer, see Sir W. Thomson's Reprint of Papers on Electrostatics and Magnetism, § 383; and for the Compensator, see Jamin's *Cours de Physique*, tome iii. pp. 622, 639.

motion, the band is displaced slowly (here to the right), and can be driven easily out of the field of the instrument.

Each measurement requires the conjoint and repeated action of two observers—one at the electrometer, the other at the second Nicol. The lower plate of the electrometer is fixed at a chosen point of the scale ; and the screw of the compensator is worked so as to displace the black band some distance to the left. When the machine is set in motion, the potential of the prime conductor rises slowly, and the observer at the electrometer gives a continued set of sharp audible signals, which are intended partly to inform the observer at the polariscope, and partly to guide the worker at the machine, whose task is to keep the index of the electrometer as steadily as possible in or near the sighted position. The present relation of *actual* to *chosen* potential, whether of defect, or equality, or excess, is indicated incessantly by the signals.

Meanwhile the observer at the polariscope has a distinct view of the black band, and sees whether it is midway between the wires, or to the right or left, at the time of the critical signal. The screw of the compensator is now worked in the proper direction, and the observation is repeated. The screw is again worked and the observation again repeated, and so forward, until at last the black band lies exactly midway between the reference-wires when the index of the electrometer is in the sighted position—that is, when the potential of the upper conductor has the value indicated on the scale of the electrometer. Two other observations are always made at the same time, and the results recorded—one giving a superior limit of the optical effect at the chosen potential, and the other an inferior limit ; and the two limits are always drawn as close as possible.

9. *Preliminary Work.*—In mastering this method, as also in applying it afterwards, I was able to keep the manipulations, readings, and records all in my own hands; but for the observations I had to depend partly on my two assistants. The preliminary drill was found useful in respects not at first contemplated; for in the course of it I discovered several sources of error, and saw how to avoid them : I found also that, when proper care was taken, the observer's error with the compensator could be certainly limited within a definite and small range of the scale.

After more than a week of incessant exercise of myself and assistants in this way in the practical management of the instruments and method, I felt confident that good results could be obtained in one day's work at least—one mounting of the cell, and one setting of all the optical pieces, to be kept unmoved from morning till night. I determined therefore to make a new start, and to devote one day to a small number of independent and very careful measurements, through a pretty long range of potential.

10. *First Appearance of the Law.*—Accordingly on one day, Tuesday, the 12th of August, I effected eight measurements of potential and corresponding optical effect. Every precaution was adopted, every check and test that I could think of as likely to be of use. All the pieces were placed and fixed with scrupulous care. No guidance towards the results was accepted in any case, except from the actual indications of the instruments. The limits of error were drawn always as close to each other as possible; and every observation made by an assistant at either of the instruments was repeated and verified by myself before the result was recorded. The work was very tedious, some of the single measurements taking more than an hour of time; but the results were understood to be proportionately sure.

When the day's work was done and the laboratory closed, I collected the results, and reduced the readings of the compensator, subtracting the scale-reading of the true zero-point, and expressing the remainders in terms of a convenient unit, which was the twentieth part of one screw-step of the instrument*. The final results were as follows. The numbers in the first line are the scale-readings of the electrometer, or the potentials of the upper conductor; and those in the second line are the corresponding quantities of optical effect, each probably true to ±1.

30	40	50	60	80	90	100	110
$12\frac{1}{2}$	22	36	50	88	115	144	170

Observe the effects corresponding to potentials 30 and 60, exactly as 1 to 4; the effects again for potentials 40 and 80; those also for potentials 50 and 100. It was from these numbers, exactly as they stand, that I obtained my first view of what I was looking for—a definite relation between potential and optical effect. It will be apparent immediately, that the exactness of equality of those three ratios to each other and to $\frac{1}{4}$ is partly accidental; but we shall see no reason to question the truth of the law that is here so very clearly indicated.

11. It appears therefore that, in the circumstances of the present experiments, the dielectric (CS_2) being given, and the electro-optic field also given in form and dimensions, the quantity of optical effect is proportional to the square of the difference of potentials of the two conductors that limit the field.

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* The unit of potential, here and afterwards, is one screw-step of the electrometer, the 200th part of the range of the instrument.

12. Dividing the square of each of the preceding potentials by the corresponding optical effect, and keeping the quotients true to half a unit by excess or defect, we find the eight numbers

72 73 69½ 72 73 70½ 69½ 71.

There is here a very fair approximation to equality, though it is hardly so close as might have been expected.

13. *Later Measurements.*—In the next three sets of measurements the work was still of the same empirical and tedious kind as formerly (8, 9, 10). The weather was exceptionally favourable both for electrical and optical work; and the range of potential in the second set was almost as large as the instruments could reach in any circumstances.

In the actual work, I considered that each of the observed values of optical effect was very probably true to less than ±1, and that none of them was possibly in error by so much as ±2.

There is no equality to be expected between the results of one set of measurements and those of another set; for the constancy of the electro-optic field was not carefully attended to at present, except in each day's work by itself.

14. On the day after the decisive experiments of (10) I obtained the following set of results (potentials are given as formerly in the first line, and optical effects in the second):—

60	70	90	120	130	150
50	67	113	197	234	310

Dividing the squares of the successive potentials by the corresponding optical effects, and keeping each quotient true to half a unit, we find the six numbers

72 73 71½ 73 72 72½.

Applying the number 72½ as divisor to the squares of the successive potentials, we find the following set of calculated values of optical effect, each written under the corresponding potential. For comparison, the observed values of optical effect are repeated in the third line, and the differences between observed and calculated values are written in the fourth line.

60	70	90	120	130	150
49·6	67·6	111·7	198·6	233·1	310·3
50	67	113	197	234	310
·4	−·6	1·3	−1·6	·9	−·3

The differences in the third and fourth columns are rather large (13).

15. Next day I obtained the following set of results. The

first and second lines contain potentials and optical effects as formerly; the third line contains a set of numbers which are as the squares of the first five integers 1, 2, &c.—that is, as the squares of the successive potentials.

30	60	90	120	150
13	53	117	206	321
13	52	117	208	325

Dividing the squares of the potentials by the observed optical effects, and keeping the quotients true to quarter units, we obtain the five numbers

$$69\tfrac{1}{4} \qquad 67\tfrac{3}{4} \qquad 69\tfrac{1}{4} \qquad 69\tfrac{1}{2} \qquad 70.$$

Taking now the number 70 as divisor of the squares of successive potentials, we find the following table of results (they are arranged, line for line, as those given in the end of the preceding article):—

30	60	90	120	150
$12\frac{6}{7}$	$51\frac{3}{7}$	$115\frac{5}{7}$	$205\frac{5}{7}$	$321\frac{3}{7}$
13	53	117	206	321
$\frac{1}{7}$	$1\frac{4}{7}$	$1\frac{2}{7}$	$\frac{2}{7}$	$-\frac{3}{7}$

The differences in the second and third columns are still rather large (13).

16. The two following measurements were made as one set, in new circumstances, and with great care. The light employed was that of a paraffin-lamp; and the distance of the two conductors was about one eighth of an inch, which was a good deal larger than in the former experiments. The potentials were 90 and 120; and the quotients of square of potential by optical effect were as follows:—

$$\frac{90^2}{38}=213, \qquad \frac{120^2}{67}=215.$$

These numbers are satisfactorily near equality. Otherwise, the calculated value of optical effect at potential 120

$$=\frac{4^2}{3^2}\times 38=67\tfrac{5}{9},$$

which differs from the observed value by half a unit.

17. Is there any *regular* difference, however small, between observed and calculated values of the optical effect? Such was the question that I specially attempted to solve in some of my latest experiments on this part of the subject. Some changes of method were adopted. The lens D of (8) was withdrawn, and its place was supplied by an ocular lunette; the second Nicol, deprived of its diaphragms, was moved close up to the compensator; the room also was well darkened, so

that all other light beside that from the port was excluded. These alterations added greatly to the purity and steadiness of the optical effect, and made the observations both easier and surer.

The electrometer was set at a convenient potential; and the corresponding optical effect was measured as accurately as possible. A new potential was chosen; the corresponding optical effect was calculated; and both electrometer and compensator were set *initially* as they ought, by the law of squares, to stand *finally* in this new case. The machine was then set in motion; and the observers had to determine, without touching the instruments, whether the black band lay exactly midway between the wires of the compensator, or perceptibly to the right or left, when the index of the electrometer was exactly in the sighted position. Some careful trials were made in this way, through various ranges of potential. Differences did present themselves at first in one or two cases, but they disappeared on closer examination. Upon the whole, there was no deviation from the law of squares made out clearly in any case, in either direction, or in any degree (great or small). I must therefore leave the statement already made in (11) as the net and undoubted result of all that has been done upon the subject up to this point.

18. *Second Part of the Proof.*—Let D be the distance between the opposed faces of the two conductors, let V be the difference of potentials of the conductors, and let Q be the corresponding quantity of optical effect; also let R be the resultant electric force at any point. Comparing the present system of dielectric and conductors with one of the simplest forms of electric field imagined in the Mathematical Theory of Electricity (that, namely, which is limited by two parallel conducting planes of infinite extent, and maintained at different potentials), we obtain the following principle as one that we can safely proceed upon.

For any small given value of D, for any given value of V, and for all points lying between the opposed parallel faces of the conductors, and not very near the borders of the field, the force R is constant in direction and magnitude, its direction being perpendicular to the faces of the conductors, and its intensity proportional to the quotient of V by D.

The optical action that we are concerned with has been already characterized as a case of double refraction with reference to direction of the force R as axis. The quantity Q of the birefringent action, or the difference of retardations of the ordinary and extraordinary rays, is proportional to, and may therefore be measured by, the reduced reading of the compensator. The quantity V is measured, with sufficient accuracy, by the scale-reading of the electrometer.

It is already proved that, when D is given,

$$Q \propto V^2;$$

and from this it follows that

$$Q \propto R^2.$$

To anticipate the effect of a variation of D, we may simply extend this last statement, in the form

$$Q \propto R^2 \propto \frac{V^2}{D^2};$$

and what remains to be proved is that Q varies inversely as D^2.

Generally, we may connect the experimentally measured quantities D, V, Q by the equation

$$Q = \frac{V^2}{E \,.\, D^2};$$

and then, to complete the proof of the Law of Squares as already stated in (1), we must find that the quantity E, or the equivalent quantity

$$\frac{V^2}{Q \,.\, D^2},$$

preserves a constant value when D varies as well as V.

19. *Method of Experiment.*—It was necessary here to obtain a set of exactly measured values of D. The following method was adopted, as well within reach and at the same time sufficiently accurate. I assume that the reader remembers what has been said about the upper conductor and its adjuncts, the ball-headed screws, rafters of glass, and lateral supports of thick plate (2). A sheet of thin plate glass, chosen for its apparently perfect uniformity, was cut up into a number of equal slips, of about the same length and breadth as the upper conductor. The joint thickness of sixteen of the slips was exactly one inch. When the slips were laid side by side on a plane table, and tested optically and otherwise, they were found to differ sensibly, though very little, in thickness. Four of them were selected that showed no such difference; and these were reserved as *standard* slips, the thickness of each of them being taken as unit value of D.

The pieces being all in final position for experiment, the cell empty and open, and the upper conductor screwed close up to the rafters, the distance between the two conductors was found to be rather greater than 1. A standard slip was laid on the floor of the cell, between the two conductors; and the screws were worked through small distances till the upper conductor was in close and uniform contact with the slip. The final adjustment was obtained when the standard slip was barely movable through the whole space between the conductors, sliding with tight friction, but without lifting or dragging the upper conductor. The cell was now closed and charged, the electro-optic measurements were made; the cell was again emptied and opened, the standard slip was again applied, and the value of D was found to be absolutely unaltered.

All the pieces being in position as they were left by the preceding experiment, an unreserved slip was thrust in permanently between the brass floor of the cell and each of the lateral supports; two of the standard slips, held togther as one, were laid between the two conductors; and the screws were worked backwards and forwards through very small distances, till the double slip could slide without lifting or dragging the upper conductor, but with tight friction as formerly. The cell was then closed and charged, and the electro-optic measurements were made for the new value 2 of D, as formerly for the value 1. It was also seen here, as in the former case, that the faces of the conductors were parallel, and that D had exactly the same value at the end of the experiment as at the beginning.

Distances 3 and 4 were managed in the same way. The necessary time and pains were taken to make all the operations as correct as possible. No further explanation of the method is required, the electrometer and compensator being worked here in the same way exactly as before. The improvements mentioned in (17) were retained here of course.

20. *Measurements.*—As my time was now running short, I thought it best to restrict the work to one very careful measurement, or to two measurements at most, for each value of D. The limits of error of observation in the measurement of Q were drawn as close to each other as possible in every case; and, as far as I could judge, the error lay always well within the range ±1; no observed value of Q could possibly be in error by so much as ±2.

(1) D=1. One measurement was made; and the result was as follows:—

$$E=\frac{V^2}{Q\,.\,D^2}=\frac{60^2}{63\times 1^2}=57.$$

This experiment was made in very favourable circumstances; and the result was verified again and again.

(2) D=2. Two measurements were made and verified; and

the values of E were as follows:—

$$\frac{90^2}{36\times 2^2}=56, \qquad \frac{120^2}{64\times 2^2}=56.$$

The agreement with the first case is more than satisfactory; for if we suppose the true values of Q to be less than the observed values by *half* a unit each, (a difference which lies well within the range of probable error of observation,) the corrected values of E rise to 57 and 56·7.

(3) D=3. Two measurements were made; and the values of E were as follows:—

$$\frac{90^2}{16\times 3^2}=56, \qquad \frac{120^2}{27\times 3^2}=59.$$

The agreement of these results with each other and with the first case is pretty close, and hardly unsatisfactory; for if we suppose the true value of Q at potential 120 to be greater than the observed value by 1, the corrected value of E falls to 57.

(4) D=4. Two measurements were made and verified, with the following results:—

$$\frac{120^2}{15\times 4^2}=60, \qquad \frac{150^2}{24\times 4^2}=58\tfrac{1}{2}.$$

The agreement with the first case is close, and, indeed, satisfactory; for if we suppose the true values of Q to be greater each by 1 than the observed values, the corrected values of E fall to about 56.

The uniformity of the electro-optic field was not sensibly perfect in this fourth case, and the increase of D could not safely be carried further.

21. These numerical results are as trustworthy as any that have yet been obtained on the subject. Taken by themselves also, they afford a sketch of a full proof of the Law of Squares in the actual case of CS_2. I think it worth while, therefore, to exhibit the contents of the last article in another form, as in the following Table. The calculated values of Q are obtained from the equation

$$Q=\frac{V^2}{57\,.\,D^2}.$$

D measured	1	2	2	3	3	4	4
V measured	60	90	120	90	120	120	150
Q measured	63	36	64	16	27	15	24
Q calculated	63·1	35·5	63·1	15·8	28	15·8	24·6

It will be noticed that the differences between observed and calculated values of Q do not extend, in any case, beyond the range ±1 of probable error of observation. With this Table I close the experimental proof of the Law of Squares in the actual case of dielectric CS_2.

In proceeding now to my last experiment, I may mention that the rubber of my machine is insulated on pillars of glass, as well as the prime conductor. The object in view is to compare contrary electric forces.

22. *Experiment.*—The arrangements are as described in (8), all the optical pieces being placed as in the diagram.

(1) As in (8), the prime conductor is connected by wires with the cell, the electrometer, and the Leyden jar, while the rubber is connected by a wire with earth. A convenient potential is chosen; the electrometer is set accordingly; and the screw of the compensator is worked as in the former experiments, till the optical action of the dielectric is exactly neutralized at the instant of the critical signal. In the final state of the instruments, the black band lies exactly midway between the reference-wires of the compensator when the index of the electrometer is exactly in the sighted position. From now till the end of the experiment, the electrometer and the compensator are left untouched.

(2) The wires connecting prime conductor with cell and electrometer and Leyden jar are detached from the prime conductor, and fixed in the rubber; and the wire connecting rubber with earth is detached from the rubber, and inserted in the prime conductor. The electro-optic observation is repeated, and the effects are found to be the same in kind and quantity as before. As the electric force increases, the black band moves gradually to the right; and when the index of the electrometer is exactly in the sighted position, the band lies exactly midway between the wires.

It appears thus, as might have been confidently expected, that the optical effect of electric force is independent of the sense of the force, being of exactly the same kind after reversal of the force as before, equal also in amount for equal intensities of the force.

23. In conclusion, I observe that the principal result of the experiments, what I have called the Law of Squares, may be correctly stated in several very different forms. The quantity of optical effect, per unit of thickness of the dielectric, varies either

(1) Directly as the square of the resultant electric force, or

(2) Directly as the energy of the electric field per unit of volume, or

(3) Directly as the mutual attraction of the two conductors that limit the field, or

(4) Directly as the *electric tension* of the dielectric, a quantity that was conceived long ago very clearly by Faraday, and introduced afterwards definitely into the Mathematical Theory of Electricity by Professor Clerk Maxwell.

Faraday's and Clerk Maxwell's views as to the action of the dielectric in the transmission of electrostatic force, and as to the state of molecular constraint that is associated with and essential to that action, are very strongly confirmed by the new facts of electro-optics. The dioptric action of an electrically-charged medium is closely related to the electric stress of the medium, the axis of double refraction coinciding in every case with the line of electric tension, and the intensity of double refraction varying, certainly in CS_2 and probably in all other dielectrics, directly and simply as the intensity of the tension.

Glasgow, January 22, 1880.

XXIII. *Note on the Measure of the Intensity of Sound.* *By* R. H. M. Bosanquet, *Fellow of St. John's College, Oxford.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

I AM happy to say that my proposal for the establishment of an acoustic laboratory* is in a fair way to be realized, and I hope shortly to get to work. But I reserve all account of this for the present.

The object of this note is to present certain considerations as to the expression of the measure of sound-intensities, with a deduction of Fechner's law in a manner which I do not remember to have seen before. The point was suggested to me by a criticism, in which it appeared to be supposed that the measurement of differences of sensation arising from differences of intensity by the ratio of the mechanical intensities was inconsistent with Fechner's law.

The first and simplest hypothesis concerning the difference of the sensations due to two mechanical intensities is, that the difference of the numbers which express the measure of the sensation is proportional to the difference of the mechanical intensities. (The variations due to difference of pitch are throughout disregarded, as that question forms a distinct

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* Phil. Mag. Oct. 1879.

branch of the subject.) It is easy to prove, from facts of ordinary knowledge, that this hypothesis is impossible. For instance, a single diapason stop of soft quality, in the organ, may be taken to represent a definite mechanical intensity. If this be added to a mechanical intensity of less, or not very much greater amount, the addition produces a finite difference of sensation. If, however, the full organ is being used, so that the existing mechanical intensity is very much greater than that added by the single soft stop, the addition of the latter does not affect the sensation at all. The first hypothesis therefore falls to the ground.

Similar conditions may be applied to any definite loud and soft sounds; but the above is sufficient to establish the point.

The second hypothesis in point of simplicity would appear to be, that the difference of the numbers which express the measure of the sensation is proportional to the ratio of the mechanical intensities. This is an hypothesis often practically employed in a vague sort of way. I shall endeavour to trace it to its consequences, and show that it is necessary that the measure of sensation thus defined should be connected with another measure of sensation. The second measure introduced is that in which small differences of mechanical intensity *correspond* to small differences of sensation; and this latter measure is related to the mechanical intensity, according to Fechner's law.

Let λ be the measure of sensation, differences of which are proportional to the ratio of the mechanical intensities, so that

$$\lambda_1 - \lambda_0 = k\frac{I_1}{I_0}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (i)$$

Let

$$\lambda_1 = \lambda + \Delta\lambda, \qquad I_1 = I + \Delta I,$$

$$\lambda_0 = \lambda, \qquad I_0 = I.$$

Then (i) becomes

$$\begin{aligned}\Delta\lambda &= k\frac{I+\Delta I}{I}\\ &= k\left(1+\frac{\Delta I}{I}\right). \quad . \quad . \quad . \quad . \quad . \quad (ii)\end{aligned}$$

If we diminish ΔI indefinitely, Δλ tends to the value k, which cannot vanish consistently with the fundamental assumption. λ cannot, therefore, be such a measure of sensation as we ordinarily recognize, since differences of sensation must ultimately diminish indefinitely with differences of the mechanical intensity.

Taking logarithms in (ii),

$$\log_e \Delta\lambda = \log_e k + \log_e\left(1 + \frac{\Delta \mathrm{I}}{\mathrm{I}}\right)$$

$$= \log_e k + \frac{\Delta \mathrm{I}}{\mathrm{I}}$$

ultimately, when $\frac{\Delta \mathrm{I}}{\mathrm{I}}$ is small. If then we put

$$\Delta\sigma = \log_e \Delta\lambda \text{ and } k = 1,$$

$$\Delta\sigma = \frac{\Delta \mathrm{I}}{\mathrm{I}}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(iii)}$$

and the differences of the new measure of sensation (σ) and the mechanical intensity vanish together. This equation (iii) is the basis of Fechner's law.

Resuming the argument. The hypothesis that differences of a measure of sensation are proportional to the ratios of mechanical intensity is impossible, *totidem verbis*; but the differences of the measure in question are the logarithms of the differences of another measure, which satisfies the conditions of observation, and leads to Fechner's law.

A third assumption, which may perhaps be stated separately, is that the ratio of the sensations is proportional to the ratio of the intensities—that is,

$$\frac{\lambda_1}{\lambda_0} = k\frac{\mathrm{I}_1}{\mathrm{I}_0},$$

and, with the same notation as before,

$$1 + \frac{\Delta\lambda}{\lambda} = k\left(1 + \frac{\Delta \mathrm{I}}{\mathrm{I}}\right).$$

If the differences are to vanish simultaneously,

$$k = 1,$$

and

$$\lambda = k'\mathrm{I},$$

where

$$k' = \frac{\lambda_0}{\mathrm{I}_0}.$$

Whence it follows that the differences of sensation ($\Delta\lambda$) are proportional to the differences of mechanical intensity (ΔI); but this was shown to be untrue in connexion with the first hypothesis. This hypothesis is therefore really identical with that first made, and falls with it.

Of these, the simplest hypotheses that can be made on this subject, the only one which satisfies the elementary conditions, is that which leads to Fechner's law. It is possible that this law may not be accurately and rigorously true; that would only be to say that our fundamental hypotheses err in the same manner, which is not at all improbable. But until experiments of a far more careful and extensive character than any yet made come to decide the question, we may take Fechner's law to be highly probable.

This law will always probably be best led up to by the physiological reasoning commonly employed. But I have thought it not useless to examine the consequences of the various elementary assumptions as to the measure of the sensation of sound, from a different point of view.

P.S.—Since the above was written, my attention has been drawn to an experimental verification of Fechner's law, for the sounds produced by falling weights (Carl Nörr, *Zeitschrift für Biologie*, 1879, p. 297).

XXIV. *On the Mean free Path of the Molecules.*
By N. D. C. Hodges[*].

THE free path of a molecule is dependent on the amount of obstruction it meets with, or the density of the medium. O. E. Meyer gives for the mean free path (on page 308 of his *Kinetische Theorie der Gase*), $L=\frac{1}{\pi\sqrt{2}N\zeta^2}$. Here N is the number of molecules in the unit volume.

I consider the length of path in a medium of variable density. At the surface of a liquid, if there is no sharp transition from the liquid to the gaseous state, we shall have a succession of layers of less and less dense vapours, from where there is liquid to the surrounding atmosphere. The layers V are what I refer to. The depth of these vapours is, of course, much magnified.

I propose to find the pressure upon the particle p, when the surface of the liquid is plane and when it is spherical. Taking the molecules moving with any definite velocity, they will reach p and give it an impulse when they are at a distance from p less than their mean free path. Now the molecules from below come from denser layers than those from above. A greater number will come from below than from above; there will be a tendency to drive p upward. To find this tendency, we must find how much denser the lower layers, from

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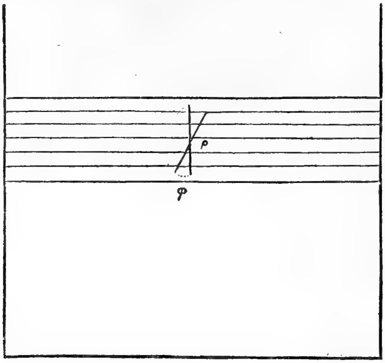
[*] Communicated by the Author.

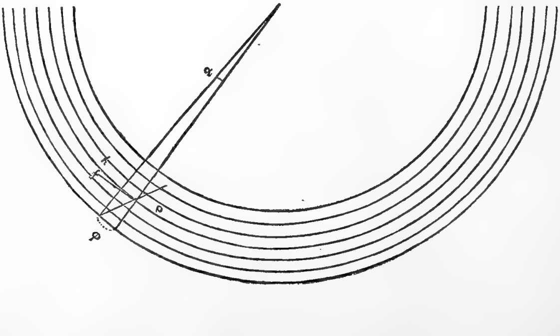
which molecules impinging on p come, are than those above, from which particles come to p.



If $\delta\rho$ is the obstruction met by a molecule in passing vertically upward through a single layer, $\frac{\delta\rho}{\cos\phi}$ will be that met in a direction at an angle ϕ with the vertical. The integral $\int_{\rho_0}^{\rho_1}\frac{d\rho}{\cos\phi}$ gives the obstruction met with by the molecule from one end to the other of its path. This integral must be constant; for the length of path is independent of the direction. As the differential of the obstruction is the same as the differential of the density,

$$\frac{1}{\cos\phi}\int_{\rho_0}^{\rho_1}d\rho=\frac{\rho_1-\rho_0}{\cos\phi};$$

where ρ_0 is the density at the point p, and ρ_1 that at the other end of the path. As $\frac{\rho_1-\rho_0}{\cos\phi}=k$, the numerator $\rho_1-\rho_0$ must be proportional to $\cos\phi$. The pressure on p is proportional to this difference, and the resultant component in the upward direction to $\cos^2\phi$.



When the surface is spherical, each element of the path offers an obstruction expressed by $\frac{\delta\rho}{\cos(\phi-\frac{1}{2}\alpha)}$ for the parts below p, and by $\frac{\delta\phi}{\cos(\phi+\frac{1}{2}\alpha)}$ for those above. α is the angle between the radius of curvature at the point p and that to the other end of the path; $\phi-\frac{1}{2}\alpha$ is the mean value of the angle between the direction of path and the normals to the surfaces of equal density for the parts below p, and $\phi+\frac{1}{2}\alpha$ the corresponding angle for those above p.

Integrating,

$$\frac{\rho'_1-\rho'_0}{\cos(\phi-\frac{1}{2}\alpha)}=k=\frac{\rho'_0-\rho'_2}{\cos(\phi+\frac{1}{2}\alpha)}.$$

This shows that, whereas the difference in density above and below was the same for a plane surface, in the case of a spherical surface the pressure from below is greater, and that upward less. Or the pressure from below is greater and that from above greater. This must cause a greater density at p.

The tendency of a particle to move from the liquid into the surrounding atmosphere is due to the difference in density of the liquid and of its vapour. For small changes in the density, the change in this tendency may be assumed as proportional to the change of density. It must be found what change of density takes place at p. As the change of density is due to an increase of pressure on p, this increase must be equal in all directions. So it is only necessary to consider one direction. Take the direction tangent to the curved surface at p. The increase in pressure is therefore proportional to the difference in density of the layer through f and that through h, or to the length fh. It is evident that

$$\frac{fh}{h}=\frac{\frac{1}{2}h}{r},$$

where h is the free path, and r the radius of curvature of the surface. Hence we have

$$\frac{\text{the change in tension}}{\text{the tension of vapour at plane surface}}=\frac{\frac{1}{2}h}{r}.$$

Sir William Thomson has shown that the change in tension at a curved surface is equal to the pressure of a column of the vapour of the height to which the liquid would rise in a capillary tube of a diameter of twice the radius of curvature of the surface.

In a tube of diameter 1·294 millim. water rises to a height of 23·379 millim. The data for the calculation are:—

Weight of a litre of water-vapour at 100° C.	·80357 grm.
" " " mercury	13·579 grms.
Tension of vapour of water at 20° C. . . .	18·495 millims.

The height of a column of mercury equivalent to the column of water-vapour of height 23·379 millims., is

$$\frac{{\cdot}80357}{13{\cdot}579} \cdot 100 \cdot \frac{18{\cdot}495}{760} \cdot \frac{23{\cdot}379}{100}.$$

The first factor is the fraction of a litre of mercury which a litre of water-vapour at 100° C. equals. The second factor reduces the height of this to millimetres. The third gives the result at 20° C., supposing the vapour to follow Boyle's Law. The fourth is the part of a litre there was to be considered.

The expression for the mean free path in these surface-vapours is then

$${}_2\,L = {\cdot}647 \cdot \frac{{\cdot}80357}{13{\cdot}579} \cdot 100 \cdot \frac{18{\cdot}495}{760} \cdot \frac{23{\cdot}379}{100} \cdot \frac{1}{18{\cdot}495},$$

$r = {\cdot}647$.

This gives $L = {\cdot}0000024$ millim.

If the law, according to which the density of the vapours varies with the depth, were known, the free path of a molecule in a gas at the ordinary pressure could be found.

Physical Laboratory, Harvard College,
Cambridge, U. S. A., Jan. 27, 1880.

XXV. *Crystallographic Notes. By* L. Fletcher, *M.A., Fellow of University College, Oxford, Assistant in the Mineralogical Department, British Museum**.

[Plate V.]

I. *Copper.*

THE following forms have already been described as occurring on native copper:—

(100) (110) (111) (210) (520) (310)
(311) (412) (18 10 5).

The forms (311), (412), (18 10 5) have not been observed on any of the crystals in the collection of the British Museum. The form (311) was described by Rose† as occurring on some rude crystals from Nijni-Tagilsk: the crystals

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* Communicated by the Crystallological Society.

† *Reise nach dem Ural,* von G. Rose, vol. i. p. 313 (1837).

were always twinned, and their faces dull and not very plane. Schrauf* has observed the same form on some artificial crystals of this metal. The same mineralogist† describes crystals from Wallaroo in South Australia which exhibit the hemihedral form $\pi(210)$. Zerrenner‡ has observed an imperfect development of this hemihedral form on a crystal from Bolivia, whilst a second crystal on the same specimen presented the combination of (2 1 0) with slight (4 1 2) and (1 1 0). The form (18 10 5) was observed by Vom Rath § on crystals of a specimen from Lake Superior. The form (3 1 0) was observed (1877) by Jeremejew ‖ on some crystals from Syryanow in the Altai district; and he incidentally remarks that, of all the native metals or alloys, it has only been met with on amalgam. We shall see later that it also occurs on both native silver and native gold.

To the list of observed forms must now be added

(4 1 0) (5 3 0) (7 3 0) (7 4 0) (4 1 1) (5 1 1) (5 3 1).

The form (3 1 0) has also been observed on crystals from other localities than the above.

Where not stated to the contrary, the instrument employed for the determination of the angles was a reflecting goniometer. As images often could not be obtained, the position of a face was in general determined by its maximum illumination. For more accurate determination of the angles, in some cases the faces were slightly oiled, while in others thin reflecting plates were fixed upon them. A few of the observed and calculated angles are given in each instance.

In the case of some of the above new forms the indices have been marked on the crystals for some years: they are confirmed by the measurements given below.

A crystal removed from a specimen from Lake Superior presented a symmetrical development of the combination (1 0 0) (1 1 0) (3 1 0):—

	Observed.	Calculated.
$310 . 3\bar{1}0$ . .	36° 30′–37° 20′	36° 52′
310 . 110 . .	25° 15′	26° 34′

A specimen, from the Bank Mines near Ekaterinburg is a fine ramose mass of crystals, in a matrix of grey crystalline limestone. Some of the crystals present the combination (1 1 1) (1 0 0) (1 1 0) (3 1 0), much shortened along a trigonal axis:—

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* Tschermak, *Mineral. Mittheil.* 1873, p. 290.

† *Ibid.* 1872, p. 53.

‡ *Ibid.* 1874, p. 94.

§ Groth's *Zeitschrift für Krystallographie,* 1878, p. 169.

‖ *Ibid.* 1877, p. 398.

	Observed.	Calculated.
110.310 . .	26° 5′	26° 34′
100.310 . .	18 50	18 26
101.013 . .	47 40	47 52

Other crystals on this specimen have quite a different habit, and present a symmetrical development of the combination (100) (110) (111) (411), the faces of the cube being predominant, as shown in fig. 1:—

	Observed.	Calculated.
111.411 . .	34° 45′	35° 16′
110.411 . .	33 5	33 33

Several specimens from Lake Superior present forms previously unobserved. The first we shall mention is a parallel aggregation of cubes: on some of the crystals the faces of a tetrakishexahedron are developed. The indices of this form were found to be (410):—

	Observed.	Calculated.
100.410 . .	14° 1′	14° 2′

In this case images were obtained.

The second is a fine tetrakishexahedron, almost an inch in diameter and very symmetrically developed; the form proved to be (530):—

	Observed.	Calculated.
$530\cdot5\bar{3}0$ . .	62° 45′	61° 56′
530.350 . .	27 15	28 4
350.053 . .	42 45	42 40

Blurred images were obtained with this crystal.

The same simple form is shown by a crystal projecting from some massive copper and associated with laumontite and calcite. The angles were measured with the hand-goniometer:—

	Observed.	Calculated.
$530.5\bar{3}0$ . . .	60°	61° 56′
350.053 . . .	44	42 40

The same form (530), but in combination with the cube, is shown by a fourth specimen from Lake Superior. The angles were measured with the hand-goniometer:—

	Observed.	Calculated.
$530.5\bar{3}0$ . .	63½°	61° 56′
530.350 . .	27	28 4

A fifth specimen, from the same locality, presents some very sharply defined crystals associated with massive copper, native silver, and calcite. The crystals present a symmetrical development of the combination (5 3 0) (5 3 1) (1 0 0), as shown in fig. 2. The angles were measured with the hand-goniometer:—

	Observed.	Calculated.
	°	° ′
035.053 . . .	$27\frac{1}{2}$	28 4
$351.35\bar{1}$ . . .	$19\frac{3}{4}$	19 28
351.350 . . .	$10\frac{1}{4}$	9 44

The remaining specimens to be mentioned are all spinel-like twins, and are much shortened along that trigonal axis which is perpendicular to the twin-plane.

A specimen from Lake Superior presents some large crystals drused with quartz. One of the smaller and very sharply defined crystals was shown by help of the reflecting goniometer to be a spinel-like twin of the combination (1 0 0) (1 1 0) (7 3 0).

	Observed.	Calculated.
	° ′	° ′
073.037 . .	43 14	43 36
037.001 . .	23 5	23 12
037.307 . .	32 12 32 17	32 21

The remaining specimens are from the Relistian Mines, Cornwall.

The first specimen is a dendritic aggregation of crystals, mostly indistinct. One crystal, however, from which good measurements could be obtained, proved to be a spinel-like twin of the combination (1 1 1), (7 4 0), having the habit shown in fig. 3; in which the lines formed by the recurrence of dots and dashes denote reentrant edges:—

	Observed.	Calculated.
	° ′	° ′
$740.\bar{7}40$ . .	60 2	59 30
740.470 . .	31 0	30 30
111.740 . .	38 3	38 $1\frac{1}{2}$
740·704 . .	41 22 41 52	41 4

It will be seen later that this form (7 4 0) is also presented by crystals of native silver from Chili.

The second and third specimens present crystals which are spinel-like twins of the combination (1 1 1) (2 1 0) (5 1 1) and have the habit shown in fig. 4. The faces of the form

(5 1 1) lying in any octant are always striated perpendicularly, or approximately so, to the edges they form with the octahedron-face in the same octant:—

	Observed.	Calculated.
111.210 . .	39° 37′ 39 53	39° 14′
201.210 . .	36 9 36 18	36 52
111.511 . .	38 35	38 56
$\bar{1}$11.115 . .	55 about	56 15

The fourth and fifth specimens present crystals very similar to the last, having the habit shown in fig. 5. The observed angles, however, accord better with those of the combinations (1 1 1) (5 2 0) (5 1 1):—

		Observed.	Calculated.
Fourth specimen	111.151	38° 50′	38° 56′
	$\bar{1}$11.151	57 28	56 15
Fifth specimen	520.502	29 30	30 27
	520.111	41 30	41 22

whilst on the latter specimen four different angles from an octahedron-face to a face of the form (5 1 1) in the same octant gave measurements 37° 0′, 38° 40′, 39° 10′, 39° 15′ respectively, the calculated angle being 38° 56′.

II. *Silver.*

The following forms have already been described as occurring on native silver:—

(1 0 0) (1 1 0) (1 1 1) (2 1 0) (5 2 0) (4 1 0) (2 1 1) (3 1 1).

The form (7 5 1) has been observed by Dauber* on an artificial crystal. The forms (2 1 0), (5 2 0), (4 1 0), (2 1 1) have not been observed on any of the crystals in this collection. The form (2 1 1) is given by Groth in his Catalogue of the Strassburg collection, but is possibly a misprint for (3 1 1): (5 2 0) was observed by Sadebeck†; the forms (2 1 0) (4 1 0) have been confirmed by the same crystallographer. To the list of observed forms must now be added (3 1 0) and (7 4 0).

A specimen, originally labelled "Arquerite, Chili," but from which analysis proves the entire absence of mercury, consists of a group of beautiful crystals of native silver associated with cerargyrite and crystallized calcite in a limestone cemented

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* *Annalen der Chemie und Pharmacie,* vol. lvii. 1851.

† Tschermak, *Mineral. Mittheil.* 1878, p. 293.

by talc. A crystal presenting the habit shown in fig. 6, was found to be a spinel-like twin of the combination (1 0 0) (3 1 0) (7 4 0) shortened along that trigonal axis which is at right angles to the twin-plane. The following are some of the angles obtained:—

	Observed.	Calculated.
1 0 0 . 3 1 0 . .	18° 24′ : 18° 33′ : 18° 16′ 19° 27′ : 18° 51′	18° 26′
7 4 0 . 4 7 0 . .	29° 59′ : 30° 14′	30° 30′
7 4 0 . 3 1 0 . .	11° 48′ : 11° 30′ : 11° 0′	11° 19′
3 0 1 . 3 1 0 . .	26° 4′	25° 50½
0 7 4 . 3 0 1 . .	81° 10′ about	80° 58′

We have seen above that both these new forms also occur on native copper.

III. *Gold.*

The forms already described as occurring on native gold are:—

(1 0 0) (1 1 0) (1 1 1) (2 1 0) (4 1 0) (2 1 1)
(3 1 1) (8 1 1) (4 2 1) (19 11 1).

The form (3 2 1) has been observed by Lang* on an artificial crystal. The forms (4 1 0) (8 1 1) were first observed by Lewis†. The forms (2 1 1), (4 2 1), (19 11 1)‡ have not been observed on any crystals in this collection. To the list of observed forms must now be added (3 1 0).

A specimen from Beresowsk in the Urals presents the habit shown in fig. 7, and proves to be a spinel-like twin of the combination (3 1 0) (1 0 0) having no reentrant angles. Eight different angles between a cube-face and an adjacent face of the tetrakishexahedron gave values varying between 16° and 19°, and having 18° for mean, the calculated angle being 18° 26′; 3 1 0 . 3 1 0, observed 24¾°, calculated 25° 50½′.

Some crystal-sprays from various localities in the Philippines proved to be composed of small crystals presenting the known form (3 1 1) and occasionally twinned about the octahedron-plane.

IV. *Bismuth.*

The crystals of this mineral are generally very indistinct. The following forms are given by Miller:—(1 1 1) cleavage,

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* Philosophical Magazine, 1863, vol. xxv. p. 435.

† Ibid. 1877, vol. iii. p. 456.

‡ See a paper by Helmhacker in Tschermak, *Mineral. Mittheil.* 1877, p. 12.

(011) cleavage, $(100):(\bar{1}11)$ cleavage; and it is stated that the occurring combinations are (111) $(\bar{1}11)$, and (111) (011) $(\bar{1}11)$. Dana gives the following forms:—(111) cleavage, $(5\bar{1}\bar{1})$ cleavage, (100) $(\bar{1}11)$ cleavage, $(\bar{1}22)$: perhaps 2R $(5\bar{1}\bar{1})$ is a misprint for $-\frac{1}{2}$R (011). The mineral crystallizes from fusion in the form (100).

In this collection crystals from Wittichen and Johanngeorgenstadt presented one of the combinations mentioned by Miller, namely (111) $(\bar{1}11)$.

To the list of observed forms must now be added the form $(\bar{1}33$. A specimen from Siebenschlehn near Schneeberg, in Saxony, shows crystals of bismuth associated with quartz. Of two crystals, one presented the new form $(\bar{1}33)$ in combination with a largely developed $(\bar{1}11)$ and a slight (100); while the other presented the combination of $(\bar{1}33)$ $(\bar{1}11)$ with slight $(1\bar{1}1)$, the latter face being probably due to cleavage: the habit of the former crystal is seen in figure 8.

First Crystal.

	Observed.	Calculated.
$\bar{1}11.\bar{1}33$ . .	21° 46′: 21° 21′: 22° 20′	21° 19′
$\bar{1}33.100$ . .	106° 44′: 105° 18′	106° 42′
$\bar{1}33.3\bar{1}3$ . .	82° 15′: 82° 8′: 81° 31′	83° 34′
$\bar{1}11.1\bar{1}1$ . .	109° 7′: 110° 42′	110° 32′

Second Crystal.

$\bar{1}11.\bar{1}33$ . .	21° 22′	21° 19′

A third crystal from the same specimen presented the combination $(\bar{1}11)$ (100) (111), and a cleavage face (011).

V. *Sulphur.*

Forms already described as occurring on native sulphur:—
(100) (010) (001) (110) (210) (310) (011) (013) (101) (203) (103) (111) (112) (113) (115) (119) (313) (315); and also (117) (311), (131) (434).

In Dana's 'Mineralogy,' (014) is given in mistake for (013), owing to a misprint in a paper of Hessenberg. The last four forms were observed by Brezina* on some crystals

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* *Berichte d. Akad. d. Wissensch. Wien,* vol. lx. p. 539, 1869.

from Oker: (1 3 1) (4 3 4) were observed on only two of these crystals.

The form (1 3 1) has since been observed by Zepharovich* slightly developed on a crystal from near Miss in Carinthia.

The forms (3 1 0), (2 0 3), (1 1 9), (4 3 4) have not been met with on any crystals in this collection.

Some crystals were removed from a specimen of unknown locality: the colour of the crystals resembles very much that of some specimens from the Lipari Isles, while the accompanying rock is a highly altered scoriaceous lava. One crystal, about 3 millimetres in diameter, presented the combination

(1 0 0) (1 0 1) (1 1 0) (1 1 1) (1 1 2) (1 1 3) (3 1 1)
(3 1 3) (3 1 5),

of which (3 1 1) is one of the rare forms due to Brezina: the form (1 1 2) has been long known, but did not occur on the crystals from Oker. Another fragment from the same specimen, and about 4 millimetres in diameter, was found to consist of two interpenetrant crystals, one individual presenting the combination

(1 0 0) (0 1 0) (0 0 1) (0 1 1) (0 1 3) (1 0 1) (1 1 0)
(1 1 1) (1 1 3) (1 1 5) (1 1 7) (3 1 1) (3 1 3) (3 1 5),

of which both of the forms (3 1 1), (1 1 7) are due to Brezina, and in this crystal are largely developed; the old form (0 1 1) presented by this individual was not present on Brezina's crystals. The second individual presented the combination

(0 0 1) (1 0 3) (1 1 1) (3 1 1) (3 1 3) (3 1 5).

Some small crystals from the Wheatley Mines, Phœnixville, Chester Co., Pennsylvania, were found to be exceeding rich in the development of planes, and likewise showed some of the rare forms mentioned above.

A crystal of about 1 millim. diameter, from a specimen in which the sulphur was associated with anglesite, galena, and copper pyrites, was found to present the following combination:—

(0 1 1) (0 1 0) (1 1 0) (1 1 5) (1 1 3) (1 1 2) (1 1 1)
(1 0 1) (3 1 3) (1 1 7) (3 1 1).

A second specimen from the same mine showed the association of sulphur with cerussite and quartz; a crystal of about 1 millim. diameter presented the following forms, the magni-

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* *Jahresberichte des Vereines "Lotos" in Prag,* 1878.

tude of the faces being approximately in descending order:—

(1 0 0) (2 1 0) (0 1 1) (1 1 5) (1 0 3) (3 1 5) (1 1 0) (3 1 1) (1 1 2) (0 0 1) (1 0 1) (0 1 0) (1 1 1) (1 1 3) (1 1 4) (1 1 7) (1 3 1) (3 1 3).

Of these forms, (1 3 1), (3 1 1), (1 1 7) were first observed on the crystals from Oker; (2 1 0), (0 1 1), (1 1 2) are old forms, which did not occur on those crystals; while (1 1 4) has not been previously described: the faces of the latter form were about the same size as those of the forms (1 1 3), (1 1 7). The following measurements were obtained by the method of maximum illumination:—

	Observed.	Calculated.
115.114 . .	6° 0′	5° 55′
$\bar{1}13.\bar{1}14$ . .	8 0	8 8

VI. *Nagyagite.*

This mineral, which never seems to present very perfect crystals, has been long assumed to belong to the tetragonal system. Lately Schrauf* has pointed out that the crystals he has examined present a rhombic rather than a tetragonal development of planes; while from the measurement of angles of the crystals, and of the striations upon the basal plane, he thinks that the crystals really belong to even a lower type of symmetry than the rhombic. Assuming, however, rhombic axes, he gives the following indices of observed planes:—

(0 1 0) (1 6 0) (1 3 0) (1 2 0) (0 5 1) (0 3 1) (0 1 1) (2 5 2) (1 2 1) (3 4 3) (1 1 1).

All the planes, with the exception of the basal plane (0 1 0) are almost linear. He measured five crystals, and found that in three (1 2 0) was associated with (0 1 1), whilst tetragonal symmetry would have required (0 2 1); in a fourth (1 2 0) is associated with (0 3 1), while the fifth presented no measurable planes in these zones. In one, and only one, of these crystals (1 3 0) and (0 5 1) occur; (1 2 1) occurs on three crystals, (1 1 1) on two, (2 5 2) and (3 4 3) each on a single crystal. From the above, Schrauf hopes that it may be possible to distinguish between the two domes.

To the above forms may be added (1 1 0), (1 3 1), (1 4 1).

Six crystals were measured; and of these five were removed from the same specimen; and all came from the old locality, viz. Nagyag in Transylvania. The following angles were

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* Groth's *Zeitschrift für Krystallographie*, 1878, p. 239.

obtained by the method of maximum illumination:—

	Observed.	Calculated.
010.131 . .	59° 0′	59° 24′
010.141 . .	51 45	51 45

In four crystals (0 2 1) occurs at the same time as (0 1 1), whilst in two crystals (1 1 0) and (0 1 1) occur together: (2 5 2) was met with on three crystals, (3 4 3) on two, (1 2 1), (1 1 1) on one and the same, (1 3 1), (1 4 1) each on a single crystal. We therefore must agree with Schrauf that the domes appear to show a different development of planes; but still we can give no rule by which one dome may be distinguished from the other.

VII. *Realgar.*

Forms already described as occurring on realgar:—

(1 0 0) (0 1 0) (0 0 1) (1 0 1) (2 0 1) (0 1 2) (0 1 1)
(0 3 2) (2 3 0) (1 1 0) (4 3 0) (2 1 0) (5 2 0) (2 1 2)
(1 1 1) (2 3 2) (4 1 2) (6 1 2) (4 2 1) ($\bar{2}$ 1 2).

To these Scacchi adds* (1 2 0), (4 1 0), ($\bar{2}$ 1 4), (2 1 4). Hessenberg† confirms the forms (1 2 0), (4 1 0), and adds further the forms ($\bar{1}$ 0 1), (6 1 0), (2 1 1), (6 7 0), (2 2 1). To the above Dana, in his list of observed forms, adds (4 1 4), while he omits (6 7 0). Groth, in his Catalogue of the Strassburg collection, confirms the existence of the form (6 7 0), and adds (4 3 2), considering that Hessenberg's (2 2 1) was in reality this same form.

To these may be added (3 2 0), (3 1 0), (6 5 0), (0 3 4), (1 1 2), (6 1 6), (15 1 15): the plane (4 1 4) given by Dana has also been observed. The unconfirmed form ($\bar{2}$ 1 4) of Scacchi has been met with largely developed. The forms (5 2 0), (2 1 4), ($\bar{1}$ 0 1), (6 1 0), (2 1 1), (6 7 0), (2 2 1), (4 3 2) have not been met with on crystals of this collection.

A specimen from the Solfatara near Naples presents some small crystals of realgar in a scoriaceous mass. Three crystals were measured: the first presented the above-mentioned forms ($\bar{2}$ 1 4), (4 1 4) in combination with old forms; the second showed the rich combination

(0 1 0) (2 3 0) (1 1 0) (6 5 0) (4 3 0) (3 2 0) (2 1 0)
(4 1 0) (1 0 0) (0 3 2) (0 1 1) (0 1 2) (0 0 1) (2 3 2)
(1 1 1) (2 1 2) (1 0 1) (4 1 2) (2 0 1) ($\bar{2}$ 1 2).

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* Liebig and Kopp's *Jahresberichte*, 1852, p. 844.

† Hessenberg, *Min. Notiz.* i. and iii.

The forms (3 2 0), (6 5 0) have not been heretofore described.

	Observed.	Calculated.
	° ′	° ′
0 1 0 . 3 2 0 . .	48 11	48 43
0 1 0 . 6 5 0 . .	42 24	42 21

The third crystal presented the combination

(0 1 0) (1 2 0) (2 3 0) (1 1 0) (4 3 0) (2 1 0) (3 1 0) (4 1 0) (1 0 0) (0 3 2) (0 1 1) (0 1 2) (0 0 1) (1 0 1) (2 0 1) (2 3 2) (1 1 1) (2 1 2) (4 1 2).

	Observed.	Calculated.
0 1 0 . 3 1 0 . .	66° 27′	66° 18′.

A specimen from Moldawa presents some crystals with prism-development; they are associated with spongy quartz containing disseminated iron pyrites. A crystal having the habit shown in fig. 9 presented the following combination:—

(0 1 0) (1 2 0) (2 3 0) (1 1 0) (2 1 0) (1 0 0) (0 3 2) (0 1 1) (0 3 4) (0 1 2) (0 0 1) (1 1 1) (2 1 2) (4 1 2) (2 0 1) (1 1 2).

The planes (0 3 4), (1 1 2) have not been previously observed.

	Observed.	Calculated.
0 1 1 . 0 3 4 . .	8° 15′	7° 57′

The plane (1 1 2), which is somewhat largely developed on this crystal, is that assumed by Naumann and Hessenberg as parametral plane; it was found to be in the zones [1 1 1 . 0 0 1], [1 0 0 . 2 1 2].

	Observed.	Calculated.
	° ′	° ′
$1\,\bar{1}\,2\,.\,2\,1\,2$ . .	54 20	54 41
1 1 2 . 1 1 1 . .	23 20	24 3½
1 1 2 . 2 1 2 . .	18 12	18 41

An isolated crystal from Felsobanya, of rich colour and very clear, having the habit shown in fig. 10, was found to present the following combination:—

(0 0 1) (2 1 0) (1 1 0) (4 3 0) (3 2 0) (1 0 0) (0 1 0) (4 1 0) (2 1 2) (1 1 1) (0 1 2) (0 1 1).

We have seen above that this form (3 2 0) was also observed on a Solfatara crystal:—

	Observed.	Calculated.
4 3 0 . 3 2 0 . .	3° 30′	3° 23′.

A specimen from one of the Transylvanian localities presents some beautiful crystals, associated with stibnite and native arsenic in barytes: on a large crystal still on the specimen there is a plane between (2 1 2) and (1 0 1), developed as in fig. 11. From measurement of a smaller crystal which was removed from the specimen, the indices of this plane would appear to be (4 1 4), the plane given by Dana. On this same small crystal are two other minute planes, affording measurements which accord with (6 1 6), (15 1 15):—

	Observed.	Calculated.
010.212	64° 54′	64° 59′
212.414	11 52	11 53
414.616	4 21	4 18
616.15 1 15 . .	5 8	5 16
15 1 15.101 . .	3 13	3 34

XXVI. *On a Calculating Apparatus based on* Napier's *Rods.* *By* JOHN BRIDGE, *M.A.*[*]

[Plate VI.]

THE ingenious contrivance known as Napier's rods or bones is perhaps usually regarded as of some historical interest, but as no longer of practical use. For half a century, however, after its description in *Rabdologia*[†] it received much attention; and it has often been revived in various forms[‡]. I propose here to show that, by an improvement in manipulation, it may still be made to give valuable aid by diminishing labour and increasing safety and speed in calculation.

Fig. 1 (Pl. VI.) represents the apparatus, omitting some non-essential points, diminished linearly as 4 to 1. S S B B is a board on which, along its middle, a series of Napierian tablets (I have discarded the form of rods) are to be arranged for use. These have a notch at each end, so that they may be set in their places by means of pairs of pins (figs. 1 and 2) fixed in the board, which is about ·1 inch higher between the pins than elsewhere.

The tablets, which carry on one side multiples of 0, 1, 2, 3, 4, carry on the other side multiples of 9, 8, 7, 6, 5, respectively. They are kept arranged and instantly accessible in a magazine which, when not wanted, is deposited in a cavity (M) in the board.

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[*] Communicated by the Author.

[†] The full title is, *Rabdologiæ seu Numerationis per Virgulas Libri duo; cum Appendice de expeditissimo Multiplicationis Promptuario, Quibus accessit et Arithmeticæ Localis Liber unus.* Authore et inventore Johanne Nepero. 12mo. Edinb. 1617.

[‡] See *Bulletin de la Société de l'Encouragement*, 1851, p. 415.

F G (fig. 3) is a frame which is movable along the board. It carries a number of glass slides, *a*, *b*, *c*, *d*, &c. (figs. 1 and 2), movable on it in the direction of their length. These are made opaque except at the middle, where an aperture is left (fig. 4) large enough to show two adjacent digits on the tablets.

g (fig. 1) is a fixed ridge of hard wood with notches at equal intervals; and *g′* (fig. 3) is a corresponding groove in the frame. *p*, *p* are pins going down through the frame so that their lower ends, which are rounded, fall into the notches of *g*. By the help of a little pressure from the finger on one of them they serve to fix the frame at any of the positions it ought to take—namely, so that each slide is half over one tablet and half over the next (fig. 4).

2. *Multiplication.*—Suppose 74628394 to be the multiplicand and 4736 the multiplier. Fig. 2 shows the arrangement required. Tablets headed with the digits of the multiplicand are set up in *direct* order on the board, the first filling the space 0 to 1 of the scale S S; and the slides *a*, *b*, *c*, *d* are set to the lines corresponding to the digits 4, 7, 3, 6 respectively of the multiplier.

The frame is then moved step by step from right to left. At each step the sum of all the digits seen through the apertures is taken, its units' figure recorded and its tens' figure carried. When F arrives opposite 0 on the scale the whole product will have been recorded.

3. This will be seen without explanation; but for the sake of what is to follow, the process may be compared with the arranged details of the product.

	0	1	2	3	4	5	6	7	8
1	2	81	62	40	83	21	23	61	6
2	4	92	84	21	45	62	16	32	8
3	2	11	21	80	62	40	92	71	2
4	4	22	43	61	24	81	85	42	4

Let the sum of each pair of adjacent digits be called an *element*, each vertical series of elements a *column*, and each horizontal series a *row*. The columns will then correspond to the places on the scale S S, or of the digits of the multiplicand; and the rows will correspond to the *places* of the digits of the multiplier, row 1 to 1st digit, and so on. The position of an element in the product may be described by indicating its column and its row, and its *rank* is the sum of the numbers indicating its column and its row. Thus the element (6, 3) is that which is found in column 6 and row 3, and its rank is

6 + 3. In the process the elements of the product came out in the following order:—

With F at 12 appeared (8, 4),
” ” 11 ” (8, 3), (7, 4),
” ” 10 ” (8, 2), (7, 3), (6, 4),
” ” 9 ” (8, 1), (7, 2), (6, 3), (5, 4),
” ” 8 ” (7, 1), (6, 2), (5, 3), (4, 4),

and so on; at each step all the elements of the product which have the rank indicated by F.

4. It is evident how the apparatus may be made to work $ap + bq + \ldots$, so as to require no writing except to record the answer, the number of slides on the frame and places on the board being supposed great enough. For example, with an apparatus having the range shown in fig. 1, we can write down the sum of the squares of two numbers of 5 figures, to do which in the ordinary way would require about 75 figures more.

5. *Division.*—Let the multiplicand (divisor) be set up, as before described, on the board, and the product (dividend) written on paper, which it would be convenient to have ruled to correspond with the scale S S. Then the question is—how to get the slides *a*, *b*, *c*, &c. set to the successive digits of the multiplier (quotient).

The apparatus may be made to do this by following the ordinary process of division; and it is obvious what advantages that plan would have*. But it is much better to follow a method analogous to Horner's synthetic division in Algebra †, for which the apparatus is very well adapted.

In this plan the slides are carried by the frame left to right, and each takes its proper setting when it arrives at a column agreed on, usually column 1 or column 2. Until it has *passed* this column let it be called a *trial* slide, afterwards a *collecting* slide.

6. *Division, using three trial slides.*

First stage, fixing *a*.—Advance F to 3, so that *c*, *b*, *a* come over columns 0, 1, 2. Bring *c*, *b*, *a* down *together*, seeking somewhat less than the first three figures of the dividend, and subtract the number found‡. Put back *c*, *b*; move F on to 4, and append a new figure from the dividend.

Second stage, fixing *b*.—Subtract the element shown by *a*

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* It would be this:—Having found by inspection (at any stage of the process) what row is to be subtracted, set a slide to that, and move the frame towards the left, subtracting each element as it presents itself.

† Todhunter's 'Algebra,' p. 805.

‡ Not the sum of the elements; for the elements shown by the *trial* slides are of different ranks.

in column 3. Bring down *d*, *c*, *b* together, seeking somewhat less than the remainder, and subtract the number found. Put back *d*, *c*; move F on to 5, and append a new figure from the dividend.

The plan for any stage is—

(α) Subtract the sum of all the elements shown by the collecting slides.

(β) Seek somewhat less than the remainder with the trial* slides, and subtract the number found.

(γ) Put back all the trial slides but the one nearest F, move F on a step, and append a new figure from the dividend.

At the end the quotient is to be read off backward from the slides.

If the complete remainder at any stage is required, transfer the frame to the right-hand side of the board and move it up towards the left, subtracting at each step the sum of the elements shown.

7. It will be seen that the elements of the product or dividend are taken up at successive stages in the following order (compare art. 3):—

	0	1	2	3	4	5	6	7	8
First stage .	1	1	1	F					
Second stage .	2	2	2	1	F				
Third stage .	3	3	3	2	1	F			
Fourth stage .	4	4	4	3	2	1	F		

and so on.

The ranks being found by adding the numbers indicating the column and the row, are—

At the first stage . .	1, 2, 3
„ second stage .	2, 3, 4, 4
„ third stage . .	3, 4, 5, 5, 5

and so on.

In the above, each slide after leaving column 2 retains the position on the frame which it got there; the partial dividends have (usually) 3 figures, and the remainders to be recorded at the end of each stage 2 figures.

If it is agreed that each slide shall get its setting in column 1, two trial slides must be used, and F for the first stage must be placed at 2. This will be found the more convenient plan, unless the divisor have a small leading figure. The estimation

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* The element shown by the one nearest F is of the same rank as those shown by the collecting slides.

of the coming digit is sometimes less certain; but there is usually only one figure of remainder to record, and the result of the first subtraction need not be recorded at all.

8. If a doubt occurs at any stage, it may be solved in several ways:—

(1) By pushing forward the frame for a moment and so ascertaining what would be carried from the next rank.

(2) By increasing the number of trial slides. Thus, supposing that we have made the first subtraction, and trying for the new digit we are in doubt. Put back the trial slides, append another figure from the dividend, push on the frame a step, and subtract the sum of the elements shown. Then try again, using one more trial slide than before.

For example,

	0	1	2	3	4	5	6	7	8
Fourth stage . .	4	4	3	2	1	F			
			4	3	2	1	F		
Fifth stage . .	5	5	5	4	3	2	1	F	

shows the plan of changing from two to three trial slides after 4 figures of quotient have been obtained.

(3) By finding the complete remainder, recording the digits of the quotients already obtained, and beginning the division again with this complete remainder as dividend.

With three trial slides it is very unlikely that any difficulty will occur before 12 figures of quotient have been obtained, and then only if the divisor have more (say) than 7 figures. The method of computing should in all cases be exactly that given in Sonnenschein and Nesbitt's 'Arithmetic,' part i. p. 105.

9. *Examples of Division.*

Ex. 1. In these examples the divisor is 74628394, so that the process may with patience be followed on fig. 2.

Two trial Slides.

Dividend . . .	3249683142966
Written work . .	345599775883
Quotient	435448623344

The partial dividends are to be read obliquely 34, 49, 56, &c., and in the following example, 260, 310, &c.; so that in the above only one figure was recorded at each step: and the result was very quickly verified by reversing the motion of the frame for the purpose of multiplying divisor and quotient.

Ex. 2. The reciprocal of ·74628394.

Three trial Slides.

Dividend . . . 10000000000000
Written work . . 617594672085
„ „ 237752446776
Quotient . . . 1·33997255789

10. *Square Root.*—In the process for square root a new tablet as well as a new slide is introduced at each stage. Hence in the diagram it will be necessary to represent what intervals are at each stage occupied by tablets. This is done by inserting an asterisk. Also some of the elements must be doubled; these are indicated by accents.

I avoid length of description by giving only the diagram, hoping that it will be sufficient explanation of the process. The first digit is obtained in the usual way.

Plan for finding Square Root, using two trial Slides.

0		1		2		3		4		5		6		7		8
1	*	1		F												
2′	*	2	*	1		F										
3′	*	3	*	2	*	1		F								

&c.

Plan for changing from two to three trial Slides.

0		1		2		3		4		5		6		7		8
1	*	1		F												
2′	*	2	*	1		F										
	*		*	2		1		F								
3′	*	3′	*	3	*	2		1		F						
4′	*	4′	*	4	*	3	*	2		1		F				

&c.

After the first two stages the process is as easy as that for division.

11. *Example of process for Square Root. Two trial Slides.*

Given number . . 12·34567890000
Written work . . 3 19533940908
. . . . 1 1
Square root . . 3·51364182864

The same, using *three trial slides* after the second stage:—

Given number . . 12·34567890000
Written work . . 3 19551504526
. . . . 243162653

A simplification worth noticing in this process arises from the fact that the digits on the collecting slides, the number of which is always odd, are equal at equal distances from the middle.

12. Since writing the above I have found analogies to the processes described. Lieut.-Col. Oakes, in the Journal of the Institute of Actuaries, Jan. 1863, gives "*A method of multiplication which may be practised mentally*," the knowledge of which from some other source led me to construct the apparatus. The other processes came, as it were, by the spontaneous action of the machine. Methods having a partial correspondence with them are given by Dr. Peacock in his "Arithmetic" in the *Encyc. Met.* as Arabian or Persian methods; also in his 'Algebra,' vol. i. pp. 100–105.

Napier's *Promptuarium*, described in the Appendix to his *Rabdologia*, multiplies by a process *arithmetically* identical with that given; but the construction and manipulation of his apparatus are very troublesome.

Hampstead, Jan. 2, 1880.

XXVII. *On the Forms of the Vibrations of twitched and stroked Strings.* By F. LINDEMANN[*].

1. DURING my lectures on Fourier's series and their applications it struck me that various objections might be raised against the theories of twitched and of stroked strings as they are given by Helmholtz[†]. This occasioned the following investigations, which are intended to establish those theories more rigorously.

In regard to the twitched string, the series

$$y=\frac{2bL^2}{\pi^2a(L-a)}\Sigma\frac{1}{n^2}\sin\frac{n\pi a}{L}\sin\frac{n\pi x}{L}\operatorname{cosin}\frac{2n\pi t}{T}, \quad . \quad (1)$$

derived in the well-known manner from the general theory, cannot be regarded as the integral of the fundamental differential equation

$$\frac{\partial^2 y}{\partial t^2}=\alpha^2\frac{\partial^2 y}{\partial x^2}, \text{ where } \alpha=\frac{2L}{T}, \quad . \quad . \quad . \quad . \quad (2)$$

merely because the latter is satisfied by every single term of the series. In the present case the admissibility of this view

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[*] Translated from a separate impression, communicated by the Author, from the *Berichte über die Verhandlungen der naturforschenden Gesellschaft zu Freiburg i. B.* vol. vii. no. 4, pp. 500–532.

[†] *Die Lehre von den Tonempfindungen*, Brunswick, 1863, Beilage II. and V. These supplements remain unaltered in the subsequent editions.

is much rather to be specially established*; for the twice repeated differentiation of each term of (1) leads to a series which is not convergent. The desired proof is carried out in nos. 2–6. In no. 7 it is shown that the conditions laid down by Christoffel† are fulfilled, under which the occurrence of an acute angle is compatible with the subsistence of the differential equation (2).

The remaining part of this essay is occupied with the stroked string (violin-string). In nos. 8–14, therefore, the form of vibration of the violin-string is discussed without making use of those series. Christoffel's above-mentioned conditions of discontinuity present the necessary means for that purpose. The formulæ found do not fully agree with those of Helmholtz, as is set forth at the end in no. 19; nevertheless the motion of the string proceeds, on the whole, as described by Helmholtz.

As is known, the results thus obtained do not accord with the observations; on this account we have endeavoured, in nos. 15–18, to settle the theory of the stroked string in a more general manner. The results therein gained correspond very well with the observations for the case that the stroking-place is a point marking an aliquot division of the string and lies pretty near one end (distant, at the most, but little more than one fourth of the length of the string from the end, as otherwise node-points are readily formed).

The twitched String.

2. We will suppose the string to execute transversal vibrations in one plane. In the position of equilibrium let it extend along the positive X axis from $x=0$ to $x=\mathrm{L}$. Then L is its length; let M denote its mass, and P the force stretching it; and the form of the string at the time t is determined by the differential equation (2), if $\mathrm{M}\,.\,\alpha^2=\mathrm{L}\,.\,\mathrm{P}$.

In order to treat the motion of the "twitched string," let us assume that the given string is drawn on one side from its position of rest $y=0$ by means of a pointed peg, while the point of contact of the peg with the string, $x=a$, has moved in the positive direction to the distance b from its resting-place.

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[*] Schlömilch, in his *Compendium der höheren Analysis*, lays down conditions for the possibility of differentiating geometrical series term by term (2nd edition, vol. ii. p. 139). In the present case those conditions are fulfilled but not applicable; for they imply the presupposition that the differential quotient of the series in question can likewise be represented by a trigonometrical series. But the second differential quotient of (1) is 0; and 0 can only be represented by a trigonometric series whose coefficients are collectively $=0$.

[†] *Annali di Matematica*, serie 2, t. viii. p. 81.

At the time $t=0$ the peg is suddenly withdrawn. The initial velocity for every point of the string is then $=0$; and at the time $t=0$ the string has the form of an angularly bent line, whose vertex lies in the point $x=a$, $y=b$. We have therefore, for $t=0$:—

$$\left.\begin{aligned} y&=b\frac{x}{a} && \text{for } 0\leqq x\leqq a;\\ y&=b\frac{\mathrm{L}-x}{\mathrm{L}-a} && \text{,,}\quad a\leqq x\leqq \mathrm{L}. \end{aligned}\right\}\quad\ldots\ldots\quad(3)$$

If it be admitted that y can be developed as a function of x in a Fourier series of the form

$$\Sigma\left(\mathrm{A}_n\sin\frac{2n\pi t}{\mathrm{T}}+\mathrm{B}_n\operatorname{cosin}\frac{2n\pi t}{\mathrm{T}}\right)\sin\frac{n\pi x}{\mathrm{L}},$$

we get, in the known manner, series (1), found also by Helmholtz. For the reason mentioned in no. 1, the admissibility of the latter must, in the present case, be more closely investigated.

3. From D'Alembert's solution of equation (2) it is easily proved that the form of the twitched string consists, at any moment, of three straight lines, apart from single moments in which it is composed of two or only of one line*. Consequently, for all points of the string, $\frac{\partial^2 y}{\partial x^2}=0$; and, consequently also, $\frac{\partial^2 y}{\partial t^2}=0$, by which (2) is fulfilled. We must therefore inquire whether the function (1) is a corresponding linear function of x and t.

Of the three right lines, one passes through the initial point, another through the point $x=\mathrm{L}$, $y=0$. Let these two be cut by the third in the points γ, δ and γ', δ' respectively; then is

$$\left.\begin{aligned} y&=\delta\frac{x}{\gamma} && \text{for } 0\leqq x\leqq \gamma,\\ y&=\frac{\gamma\delta'-\delta\gamma'+(\delta-\delta')x}{\gamma-\gamma'} && \text{,,}\quad \gamma\leqq x\leqq \gamma',\\ y&=\delta'\frac{\mathrm{L}-x}{\mathrm{L}-\gamma'} && \text{,,}\quad \gamma'\leqq x\leqq \mathrm{L}. \end{aligned}\right\}\quad\cdot\quad(4)$$

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* Compare Thomas Young's "Experiments and Inquiries respecting Sound and Light," Phil. Trans. 1800, part i. p. 135; Monge, *Journal de l'Ecole Polytechnique*, t. viii. p. 118 (1809); and Helmholtz *l. c.*

From this results

$$\int_0^L y \sin\frac{n\pi x}{L}\,dx = \frac{L^2}{n^2\pi^2}\left[\left(\frac{\delta}{\gamma}-\frac{\delta-\delta'}{\gamma-\gamma'}\right)\sin\frac{n\pi\lambda}{L}\right.$$

$$\left.+\left(\frac{\delta-\delta'}{\gamma-\gamma'}+\frac{\delta'}{L-\gamma'}\sin\frac{n\pi\gamma'}{L}\right]\right..$$

The function y, defined by equations (4) in the interval from $x=0$ to $x=L$, is therefore represented by the series

$$\frac{2L}{\pi^2}\Sigma\frac{1}{n^2}\left[\frac{\gamma\delta'-\gamma'\delta}{\gamma(\gamma-\gamma')}\sin\frac{n\pi\gamma}{L}\right.$$

$$\left.+\frac{L(\delta-\delta')+\gamma\delta'-\gamma'\delta}{(L-\gamma')(\gamma-\gamma')}\sin\frac{n\pi\gamma'}{L}\right]\sin\frac{n\pi x}{L}.$$

The quantities γ, δ, γ', δ' must now be so determined as functions of t that this series shall become identical with that defined by (1). This latter is equal to

$$\frac{bL^2}{\pi^2 a(L-a)}\Sigma\frac{1}{n^2}\left[\sin n\pi\left(\frac{a}{L}-\frac{2t}{T}\right)+\sin\pi a\left(\frac{a}{L}+\frac{2t}{T}\right)\right]\sin\frac{n\pi x}{L}.$$

If, therefore, μ, ν are undetermined positive or negative whole numbers, we obtain the equations:—

$$\left.\begin{aligned}\frac{\gamma}{L}&=\pm\left(\frac{a}{L}-\frac{2t}{T}+2\mu\right),\\ \frac{\gamma'}{L}&=\pm\left(\frac{a}{L}+\frac{2t}{T}+2\nu\right);\end{aligned}\right\}\quad\dots\dots\dots\dots\quad(5)$$

$$\left.\begin{aligned}\gamma\delta'-\gamma'\delta&=\pm\frac{bL\gamma(\gamma-\gamma')}{2a(L-a)},\\ L(\delta-\delta')+\gamma\delta'-\gamma'\delta&=\pm\frac{bL(L-\gamma')(\gamma-\gamma')}{2a(L-a)}.\end{aligned}\right\}\quad\dots\quad(6)$$

The numbers μ, ν are to be determined, and their signs chosen, so as to make γ and γ' constantly positive for all values of t between 0 and T, and $\gamma \leqq \gamma' \leqq L$. The same sign must be chosen for the first of equations (5) and the first of equations (6), as well as, on the other hand, for the second equation (5) and the second equation (6). These conditions oblige us to distinguish whether $a > L-a$ or $a < L-a$. *We assume the former*; the other case can be decided in a precisely corresponding manner, and therefore need not be specially treated.

4. First, let $\mu=\nu=0$ and

$$\frac{\gamma}{L}=\frac{a}{L}-\frac{2t}{T},\quad \frac{\gamma'}{L}=\frac{a}{L}+\frac{2t}{T}.$$

These equations can only exist for

$$0 \leqq t < \frac{L-a}{L} \cdot \frac{T}{2}; \qquad (7)$$

for if t exceeded the given upper limit, γ' would become $>L$. From (6) result then

$$\delta = \frac{b\gamma}{2a(L-a)}(2L-\gamma-\gamma') = b\frac{\gamma}{a};$$
$$\delta' = \frac{b(L-\gamma')}{2a(L-a)}(\gamma+\gamma') = b\frac{L-\gamma'}{L-\gamma}.$$

Consequently, so long as the inequalities (7) subsist, according to (4) we have:—

$$\left.\begin{aligned} &y = b\frac{x}{a} \text{ for } 0 \leqq \frac{x}{L} \leqq \frac{a}{L} - \frac{2t}{T}; \\ &y = b\frac{L(aT-2Lt)+(L-2a)x}{2a(L-a)T} \\ &\qquad \text{for } \frac{a}{L} - \frac{2t}{T} \leqq \frac{x}{L} \leqq \frac{a}{L} + \frac{2t}{T}; \\ &y = b\frac{L-x}{L-a} \text{ for } \frac{a}{L} + \frac{2t}{T} \leqq \frac{x}{L} \leqq 1. \end{aligned}\right\} \qquad (8)$$

Of the three straight lines, therefore, which make up the figure of the string in the period under consideration, one only changes with the time, and, indeed, that which does not pass through an extremity of the string; it always remains parallel to the straight line

$$y = \frac{b(L-2a)x}{2a(L-a)}. \qquad (9)$$

At the close of the time-interval (7), *i. e.* for

$$t = \frac{L-a}{L} \cdot \frac{T}{2}, \qquad (10)$$

γ' becomes $=L$, $\delta'=0$; the string now consists of two straight lines, namely

$$\left.\begin{aligned} &y = b\frac{x}{a} &&\text{for } 0 \leqq x \leqq 2a-L, \\ &\gamma = \frac{b(2a-L)(L-x)}{2a(L-a)} &&\text{,, } 2a-L \leqq x \leqq L. \end{aligned}\right\} \qquad (11)$$

The second straight line is here again parallel to (9).

5. In the following section of time, defined by

$$\frac{L-a}{L} \cdot \frac{T}{2} < t < \frac{aT}{2L}, \qquad (12)$$

we have to choose $\mu=0$, $\nu=-1$, and to put

$$\frac{\gamma}{\mathrm{L}}=\frac{a}{\mathrm{L}}-\frac{2t}{\mathrm{T}},\quad \frac{\gamma'}{\mathrm{L}}=2-\frac{a}{\mathrm{L}}-\frac{2t}{\mathrm{T}}.$$

Calculating now δ and δ' from (6)—that is, from

$$\gamma\delta'-\gamma'\delta=\frac{b\mathrm{L}\gamma(\gamma-\gamma')}{2a(\mathrm{L}-a)},$$

$$\mathrm{L}(\delta-\delta')+\gamma\delta'-\gamma'\delta=-\frac{b\mathrm{L}(\mathrm{L}-\gamma')(\gamma-\gamma')}{2a(\mathrm{L}-a)},$$

we get for the interval (12):—

$$\left.\begin{aligned}
&y=b\frac{x}{a}\text{ for }0\leqq\frac{x}{\mathrm{L}}\leqq\frac{a}{\mathrm{L}}-\frac{2t}{\mathrm{T}};\\
&y=b\frac{\mathrm{L}(a\mathrm{T}-2\mathrm{L}t)+\mathrm{T}(\mathrm{L}-2a)x}{2a(\mathrm{L}-a)\mathrm{T}}\\
&\qquad\text{for }\frac{a}{\mathrm{L}}-\frac{2t}{\mathrm{T}}\leqq\frac{x}{\mathrm{L}}\leqq2-\frac{a}{\mathrm{L}}-\frac{2t}{\mathrm{T}};\\
&y=-\frac{b}{a}(\mathrm{L}-x)\text{ for }2-\frac{a}{\mathrm{L}}-\frac{2t}{\mathrm{T}}\leqq\frac{x}{\mathrm{L}}\leqq1.
\end{aligned}\right\}\quad .\ (13)$$

Again only the middle line depends on t; it remains always parallel to (9); the point at which it cuts the X axis now lies between the points $x=0$ and $x=\mathrm{L}$, while it previously lay outside this interval, towards the positive side. The other two segments of the string are parallel to one another, and lie on different sides of the x axis; the segment adjacent to the point $x=0$ has its direction unchanged.

At the time

$$t=\frac{a\mathrm{T}}{2\mathrm{L}}\quad .\ .\ .\ .\ .\ .\ .\ .\ (14)$$

the string again consists of only two straight lines, given by

$$\left.\begin{aligned}
&y=\frac{b(\mathrm{L}-2a)}{2a(\mathrm{L}-a)}x\text{ for }0\leqq x\leqq2(\mathrm{L}-a),\\
&y=-\frac{b}{a}(\mathrm{L}-x)\quad,,\quad 2(\mathrm{L}-a)\leqq x\leqq\mathrm{L}.
\end{aligned}\right\}\quad .\ (15)$$

The former of these two lines now coincides with (9). The string lies entirely on the negative side of the X axis.

6. The latter holds also for the entire following time-interval, in which

$$\frac{a\mathrm{T}}{2\mathrm{L}}<t<\frac{\mathrm{T}}{2}.$$

Again $\mu=0$, $\nu=-1$; and on the right sides of (5) the negative sign must both times be chosen, so that

$$\frac{\gamma}{\mathrm{L}}=-\frac{a}{\mathrm{L}}+\frac{2t}{\mathrm{T}},\quad \frac{\gamma'}{\mathrm{L}}=2-\frac{a}{\mathrm{L}}-\frac{2t}{\mathrm{T}}.$$

Equations (6) therefore become

$$\gamma\delta'-\gamma'\delta=-\frac{b\mathrm{L}\gamma(\gamma-\gamma')}{a(\mathrm{L}-a)},$$

$$\mathrm{L}(\delta-\delta')+\gamma\delta'-\gamma'\delta=-\frac{b\mathrm{L}(\mathrm{L}-\gamma')(\gamma-\gamma')}{a(\mathrm{L}-a)}.$$

Therefore the string now consists of the three straight lines

$$\left.\begin{array}{l} y=-\dfrac{b}{\mathrm{L}-a}x \text{ for } 0\leqq\dfrac{x}{\mathrm{L}}\leqq\dfrac{2t}{\mathrm{T}}-\dfrac{a}{\mathrm{L}}, \\ y=b\dfrac{(a\mathrm{T}-2\mathrm{L}t)+\mathrm{T}(\mathrm{L}-2a)x}{2a(\mathrm{L}-a)\mathrm{T}} \\ \qquad\qquad \text{for } \dfrac{2t}{\mathrm{T}}-\dfrac{a}{\mathrm{L}}\leqq\dfrac{x}{\mathrm{L}}\leqq 2-\dfrac{a}{\mathrm{L}}-\dfrac{2t}{\mathrm{T}}, \\ y=-\dfrac{b}{a}(\mathrm{L}-x) \text{ for } 2-\dfrac{a}{\mathrm{L}}-\dfrac{2t}{\mathrm{T}}\leqq\dfrac{x}{\mathrm{L}}\leqq 1. \end{array}\right\} \quad . \quad (16)$$

Here also only the middle line is dependent on t; it again remains parallel to (9). The straight line passing through $x=0$ is parallel with the third; and that passing through $x=\mathrm{L}$ is parallel with the first line in (8).

At the time $t=\frac{\mathrm{T}}{2}$, γ becomes $=\gamma'$, and the string consists of the two straight lines

$$\left.\begin{array}{l} y=-\dfrac{b}{\mathrm{L}-a}x \text{ for } 0\leqq x\leqq \mathrm{L}-a, \\ y=-\dfrac{b}{a}(\mathrm{L}-x) \ \text{,,} \ \ \mathrm{L}-a\leqq x\leqq \mathrm{L}. \end{array}\right\} \quad . \quad . \quad . \quad (17)$$

Consequently it now lies symmetrical with its initial position, as is always the case at the end of half the period of a vibration if the initial velocity of the several points was $=0$. From this point the string returns in a perfectly analogous manner to its initial position: this need not be further enlarged upon. The motion thus presented corresponds with the delineations given by Thomas Young*.

---

* *Conf. loc. cit. suprà*, p. 199, and 'The Theory of Sound,' by Lord Rayleigh, vol. i. (London, 1877) p. 185.

By Ch. Delagrave, Libraire-Éditeur, a model (first, constructed by Monge) is sold, of the surfaces which are generated by the twitched string when it is moved with uniform velocity perpendicularly to its plane.

Donkin* and Lord Rayleigh enunciated the proposition that the inclination of the string to the position of rest takes two constant values alternately at the two ends. But this is not wholly correct; *e. g.* for $x=0$ we have, according to the above:—

$$\frac{dy}{dx}=\frac{b}{a} \qquad \text{for } 0 \leqq t < \frac{a\mathrm{T}}{2\mathrm{L}};$$

$$\frac{dy}{dx}=\frac{b(\mathrm{L}-2a)}{2a(\mathrm{L}-a)} \quad \text{,,} \quad t=\frac{a\mathrm{T}}{2\mathrm{L}};$$

$$\frac{dy}{dx}=-\frac{b}{\mathrm{L}-a} \quad \text{,,} \quad \frac{a\mathrm{T}}{2\mathrm{L}}<t\leqq\frac{\mathrm{T}}{2};$$

&c. Between each two leaps of the function $\frac{dy}{dx}$ there is therefore a moment when the inclination takes a third value, as at that moment the line (9) itself forms a part of the string. This is in accordance with the circumstance that at a place of discontinuity a trigonometric series represents the mean value.

7. From the foregoing it follows that the series (1) for $0\leqq t\leqq\frac{\mathrm{T}}{2}$ is constantly a complete linear function of x and t, and, indeed, according to the quantity of t, like the functions occurring in the equations (8), (11), (13), (15), (16), (17). *Therefore series* (1) *in fact satisfies the differential equation* (2), since simultaneously

$$\frac{\partial^2 y}{dt^2}=0, \quad \frac{\partial^2 y}{dx^2}=0.$$

That the conditions for the possibility of an angle in transversely vibrating strings are fulfilled in the solution by means of a Fourier series was already remarked by Christoffel *l. c.* It is easy to verify this in the present case. Here the expressions

$$\frac{\partial\gamma}{\partial t}\cdot\frac{\partial y}{\partial t}+\alpha^2\frac{\partial y}{\partial x},$$

where α^2 signifies the quantity oocurring in (2), and

$$\frac{\partial\gamma}{\partial t}\cdot\frac{\partial y}{\partial x}+\frac{\partial y}{\partial t}$$

have each the same value on both sides of the angle γ, δ. We have to form these expressions for the several time-intervals above distinguished; an easy calculation then shows that they in fact give identical values on both sides of the angle γ, δ. The like holds good for the angle γ', δ'.

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* Acoustics, Part I. p. 83: Oxford, 1870.

Hence the validity of the differential equation (2) *is not impaired by the discontinuities of the differential quotients, according to x, of series* (1), *which occur at the angles of the string.*

The stroked String.

8. The notation introduced in no. 2 shall stand also for the violin-string, to be treated of in the following. We base the mathematical treatment of this upon the following proposition, to which direct observations have conducted*, and which represents with approximate accuracy the motion of the several points of the string (*conf. infrà*, no. 16):—

Each point in a stroked string vibrates hither and thither with constant velocity; this velocity is different for the different points of the string.

It will be shown that by this the motion of the string is completely determined, if, in addition, the amplitude of any single point of the string is given, and excluding the occurrence of nodes.

We shall find that the velocity of a point of the string (with the exception of the central point) when ascending is necessarily different from the velocity when descending. At first we make no hypothesis about this; we will only suppose the velocity in each direction separately to be constant.

Let us consider a point x of the string, which at the time $t=0$ commences its ascending motion. The total period of a vibration may be denoted by T, the time of the ascent by $\mathfrak{T}$, therefore that of the descent by $\mathrm{T}-\mathfrak{T}$. We can then put

$$\left.\begin{aligned} y&=at+h \text{ for } 0\leqq t\leqq\mathfrak{T},\\ y&=bt=k \ \ ,, \ \ \mathfrak{T}\leqq t\leqq\mathrm{T}.\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (18)$$

Since for all values of t the second differential quotient according to t of y is $=0$, this must also be the case with the second differential equation according to x of y, by which equation (2) is satisfied; a, b, h, k are consequently complete linear functions of x. They are subsequently to be more precisely determined. $\mathfrak{T}$ is also a function of x, to be determined, while T is a constant.

In the determination of these functions the occurrence of node-points shall be excluded.

9. As velocities of opposite motions, a and b are quantities different from one another when they are not both $=0$.

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* *Conf.* Helmholtz *l. c.* and Clem. Neumann, "Beobachtungen über die Schwingungen gestrichener Saiten," *Sitzungsberichte der Wiener Akademie* 1870, *math.-naturw. Classe*, Band lxi. Abth. 2, p. 89.

Therefore $\frac{\partial y}{\partial t}$ is discontinuous for $t=\mathfrak{T}$; that is, at the time $t=\mathfrak{T}$ the point x receives an impulse. Accordingly the formulæ of Christoffel already mentioned in no. 7 become applicable, viz.

$$\left[c\frac{\partial y}{\partial x}+\frac{\partial y}{\partial t}\right]_{+}=\left[c\frac{\partial y}{\partial x}+\frac{\partial y}{\partial t}\right]_{-},$$

$$\left[c\frac{\partial y}{\partial t}+\alpha^2\frac{\partial y}{\partial x}\right]_{+}=\left[c\frac{\partial y}{\partial t}+\alpha^2\frac{\partial y}{\partial x}\right]_{-},$$

in which α^2 has its previous meaning, and where the two values of the bracketed quantities on the two sides of the place of discontinuity $t=\mathfrak{T}$ are distinguished by the signs + and − used as indices. We have

$$c=\frac{dx}{dt} \text{ for } t=\mathfrak{T};$$

therefore

$$\frac{1}{c}=\frac{d\mathfrak{T}}{dx}.$$

Now a, b, and $\mathfrak{T}$ must be continuous functions of x, unless we suppose the connexion of the string broken. In like manner h and k are continuous; for otherwise $\frac{\partial y}{\partial x}$, and consequently also $\frac{\partial y}{\partial t}$, would be discontinuous at other times as well. *Even* $\frac{d\mathfrak{T}}{dx}$ *is a continuous function of* x; for since all the points will not simultaneously be at the greatest distance from the position of equilibrium, for a variable point x equations (18) are to be replaced by

$$\left.\begin{array}{ll} y=at+h & \text{for } \mathfrak{T}_0\leqq t\leqq\mathfrak{T}_0+\mathfrak{T}, \\ y=bt+k & \quad ,, \quad \mathfrak{T}_0+\mathfrak{T}\leqq t\leqq\mathfrak{T}_0+\mathrm{T}, \end{array}\right\} \quad . \quad . \quad . \quad (18^*)$$

when the point x arrives at its extreme position at the time $t=\mathfrak{T}_0$, where $\mathfrak{T}_0$ is a continuous function of x. As the motion is to have the period T,

$$a\mathfrak{T}_0+h=b(\mathfrak{T}_0+\mathrm{T})+k.$$

This holds good for every point x of the string. Forming the corresponding equation for $x+dx$ and for $x-dx$, and distinguishing the differentials taken in the positive and negative directions by the indices + and −, there follows, by subtraction of the two resulting equations:—

$$a\left[\left(\frac{d\mathfrak{T}_0}{dx}\right)_{+}-\left(\frac{d\mathfrak{T}_0}{dx}\right)_{-}\right]=b\left[\left(\frac{d\mathfrak{T}_0}{dx}\right)_{+}-\left(\frac{d\mathfrak{T}_0}{dx}\right)_{-}\right].$$

Therefore either $a=b$, or $\frac{d\mathfrak{T}_0}{dx}$ is continuous.

Further, in order to preserve the continuity of the string, the condition

$$a(\mathfrak{T}_0+\mathfrak{T})+h=b(\mathfrak{T}_0+\mathfrak{T})+k$$

must be fulfilled. From this, in an analogous manner, it follows that $\frac{d(\mathfrak{T}_0+\mathfrak{T})}{dx}$, and consequently also $\frac{\delta\mathfrak{T}}{dx}$, is continuous as soon as a is different from b. But if we assume that $a=b$, the last condition gives $h=k$, and the one before made use of gives $h=a\mathrm{T}+k$; therefore must $a=b=0$, as we have already remarked (at the commencement of this no.). *Hence* $\frac{d\mathfrak{T}}{dx}$ *can become discontinuous only at a node-point**; but we have excluded the occurrence of nodes. By this the above assertion is proved. It was necessary to use this demonstration, since in Christoffel's investigations the continuous progression of the place of discontinuity is presupposed.

In consequence of these discussions the above conditions of discontinuity can be applied, and, taking (18*) into consideration, can be written in the following form:—

$$\left(\frac{\partial y}{\partial x}\right)_+ + a\frac{d(\mathfrak{T}_0+\mathfrak{T})}{dx} = \left(\frac{\partial y}{\partial x}\right)_- + b\frac{d(\mathfrak{T}_0+\mathfrak{T})}{dx}; \quad . \quad . \quad (19)$$

$$a+\alpha^2\left(\frac{\partial y}{\partial x}\right)_+ \frac{d(\mathfrak{T}_0=\mathfrak{T})}{dx} = b+\alpha^2\left(\frac{\partial y}{\partial x}\right)_- \frac{d(\mathfrak{T}_0+\mathfrak{T})}{dx}. \quad . \quad (20)$$

Hence it follows that at no place in the string can $\frac{d(\mathfrak{T}_0+\mathfrak{T})}{dx}$ vanish; for otherwise a would necessarily be $=b$, which is impossible (*conf. suprà*). Therefore

The function $\mathfrak{T}_0+\mathfrak{T}$ *has in the interval from* $x=0$ *to* $x=\mathrm{L}$ *neither a maximum nor a minimum; it increases continuously, or it decreases continuously.*

10. Hence in the interval mentioned there is only one value of x which satisfies the equation $t=\mathfrak{T}_0+\mathfrak{T}$. That is, at any time t the string has only *one* angle. Or, since y, according to no. 8, is a complete linear function of x,

At any time whatever the string consists of two straight lines.

From this immediately follows:—

All the points of the string pass simultaneously through the position of equilibrium.

On account of the latter circumstance it is advisable to select as the point of time $t=0$ that in which all the points x

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* In a node such discontinuities actually occur: *conf. infrà*, no. 15.

pass through the position of equilibrium; for the moment $t=0$ selected in equations (18) appears essentially dependent on x. In consequence of this new determination equations (18) change into:—

$$\left.\begin{aligned} y&=at \quad \text{for } 0\leqq t\leqq \frac{\mathfrak{T}}{2};\\ y&=bt+f \quad \text{,,} \quad \frac{\mathfrak{T}}{2}\leqq t\leqq \mathrm{T}-\frac{\mathfrak{T}}{2};\\ y&=at+g \quad \text{,,} \quad \mathrm{T}-\frac{\mathfrak{T}}{2}\leqq t\leqq \mathrm{T}.\end{aligned}\right\} \quad . \quad . \quad . \quad (21)$$

a, b, f, g are complete linear functions, to be determined, of x. Between them and the function $\mathfrak{T}$ a series of relations subsist.

At the time T, namely, every point must again be in the position of equilibrium; therefore

$$0=a\mathrm{T}+g. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (22)$$

Further, we have

$$a\mathfrak{T}=b\mathfrak{T}+2f, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (23)$$

$$b(2\mathrm{T}-\mathfrak{T})+2f=a(2\mathrm{T}-\mathfrak{T})+2g, \quad . \quad . \quad . \quad (24)$$

and thence

$$a\mathrm{T}=-b(\mathrm{T}-\mathfrak{T}). \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (25)$$

This last equation affirms that the path travelled in ascending differs from that travelled in descending only in its sign, as must be the case in a periodic vibration.

11. Respecting $\mathfrak{T}$, according to no. 9 two different assumptions can be made: either $\mathfrak{T}$ increases when x increases; or $\mathfrak{T}$ diminishes as x increases. In both cases for $x=0$ and $x=\mathrm{L}$, *i. e.* for the extremities of the string, must

$$\mathfrak{T}\equiv 0 \text{ mod. T.}$$

In the former case, therefore, we can take $\mathfrak{T}=0$ for $x=0$, and consequently (since no nodes are supposed to be present, and since $\mathfrak{T}$ increases simultaneously with x increasing) $\mathfrak{T}=\mathrm{T}$ for $x=\mathrm{L}$; that is, the points in the vicinity of $x=0$ rise very quickly and descend very slowly, while the neighbouring points to $x=\mathrm{L}$ descend rapidly and ascend very slowly. In the other case the behaviour of the two ends of the string is exactly the reverse. *In the following we make the former assumption; the other would be decided simply by the exchange of* x *with* L−x.

According to this, x can always be chosen so small that, for a given value of t,

$$\frac{\mathfrak{T}}{2} \leqq t \leqq \mathrm{T} - \frac{\mathfrak{T}}{2}.$$

For points of the string which are situated sufficiently near the end $x=0$, therefore, the middle equation of (21) always holds good; so that for them $y=bt+f$. For $x=0$ must $y=0$; consequently we can put

$$b=\mathrm{B}x, \quad f=\mathrm{F}x,$$

where B and F denote constants.

The function $\mathfrak{T}$ reaches its greatest value for $x=\mathrm{L}$. Therefore, if $2t<\mathrm{T}$, then the first of the equations (21) holds good in the vicinity of the end $x=\mathrm{L}$; it must give $y=0$ for $x=\mathrm{L}$; we thus get

$$a=\mathrm{A}(\mathrm{L}-x),$$

where A denotes a constant.

It then follows, further, from (22), that

$$g=-\mathrm{AT}(\mathrm{L}-x).$$

In fact, g must contain the factor $\mathrm{L}-x$, since for $x=\mathrm{L}$ and $2t>\mathrm{T}$ the third of the equations (21) is valid.

The function $\mathfrak{T}$ is now determined from (25); we get

$$\mathfrak{T}=\frac{-b\mathrm{T}}{a-b}=\frac{-\mathrm{BT}x}{\mathrm{A}(\mathrm{L}-x)-\mathrm{B}x};$$

lastly, f is found from (23)—namely,

$$f=\mathrm{F}x=\frac{\mathfrak{T}}{2}(a-b)=-\tfrac{1}{2}\mathrm{BT}x.$$

12. The constants A and B, by which all is expressed, can be reduced to a single constant by means of the discontinuity-conditions (19) and (20). In correspondence with the replacing of equations (18) by (21), $\mathfrak{T}$ must previously be replaced in these relations by $\frac{1}{2}\mathfrak{T}$ or by $\mathrm{T}-\frac{1}{2}\mathfrak{T}$.

For the place of discontinuity $t=\frac{1}{2}\mathfrak{T}$ both sides of equation (19) become, by virtue of the values just found for a, b, f, g, and $\mathfrak{T}$, equal to

$$-\mathrm{ABT}\frac{\mathrm{A}(\mathrm{L}-x)^2+\mathrm{B}x^2}{2[\mathrm{A}(\mathrm{L}-x)-\mathrm{B}x]^2}.$$

This relation is therefore spontaneously fulfilled.

Equation (20), on the contrary, gives for the same place of discontinuity:—

$$
\begin{aligned}
&4A(A+B)^3x^4-4AL(A+B)^2(4A+B)x^3\\
&\qquad\qquad\qquad\qquad+12A^2L^2(A+B)(2A+B)x^2\\
&-A^2L[12AL^2(A+B)+4A^2L^2+\alpha^2B^2T^2]x+4A^4L^4\\
&\qquad\qquad\qquad=-4B(A+B)^3x^4+12ABL(A+B)^2x^3\\
&-12A^2BL^2(A+B)x^2+A^2BL(4AL^2-\alpha^2BT^2)x\\
&\qquad\qquad\qquad\qquad\qquad+\alpha^2A^2B^2L^2T^2.
\end{aligned}
$$

This equation has to subsist for all values of x, which can only be the case if

$$A+B=0 \text{ and } \alpha^2=\frac{4A^2L^2}{B^2T^2}=\frac{4L^2}{T^2}.$$

The second of these conditions shows us the dependence between the vibration-period, on the one hand, and the length, mass, and tension, of the string, on the other (*conf.* no. 2); it is the same relation which is otherwise obtained from the theory of Fourrier's series for transversal vibrations. Here it was to be demonstrated in another way; the same could also have been done in the well-known manner with the aid of d'Alembert's solution.

The first of the relations found expresses B in terms of A. The functions a, b, f, g, $\mathfrak{T}$, determined in no. 11, now become

$$
\begin{aligned}
&b=-Ax, \qquad f=\tfrac{1}{2}ATx,\\
&a=A(L-x), \quad g=AT(L-x),\\
&\mathfrak{T}=\frac{T}{L}x^{*}. \quad \ldots\ldots\ldots\ldots \quad (26)
\end{aligned}
$$

Herewith the relations subsisting for the discontinuity-place $t=T-\frac{1}{2}\mathfrak{T}$ are spontaneously fulfilled.

Accordingly equations (21) *change into*

$$
\left.
\begin{array}{ll}
y=A(L-x)t & \text{for } 0\leqq\dfrac{t}{T}\leqq\dfrac{x}{2L},\\[2ex]
y=Ax\left(\dfrac{T}{2}-t\right) & \ ,, \ \dfrac{x}{2L}\leqq\dfrac{t}{T}\leqq\dfrac{2L-x}{2L},\\[2ex]
y=-A(L-x)(T-t)\ ,, & \dfrac{2L-x}{2L}\leqq\dfrac{t}{T}\leqq 1.
\end{array}
\right\} \quad . \quad (27)
$$

By these the motion of the string is represented completely.

---

* In the determination of $\mathfrak{T}$ we might have started from the relation $a^2\left(\frac{1}{2}\frac{d\mathfrak{T}}{dx}\right)^2=1$, which, according to Christoffel, must be fulfilled, and by virtue of which one of the conditions of discontinuity results from the other.

The constant A is determined by the amplitude of any point of the string which is equal to $\frac{AT}{2L}(L-x)x$. Therefore, if the amplitude of the central point of the string is denoted by P, we have $P=\frac{ALT}{8}$, and consequently

$$A=\frac{8P}{LT}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (28)$$

According to Neumann's experiments (*conf.* no. 8) P depends on the place at which the string is stroked, and on the velocity with which the bow is drawn across; that is to say, the velocity of the stroked point is, in ascending, equal to that of the bow. Let V be this velocity, and $a=\lambda$ the stroking-place; then, according to (27) and (28),

$$V=A(L-\lambda),\ P=\frac{VLT}{8(L-\lambda)}.$$

This, however, holds good only when the stroking-place lies pretty near one end of the string (*conf.* the end of no. 1).

13. *In detail, the motion of the string takes place in the following manner:—*

At the time $t=0$ it is in the position of equilibrium.

If $0<t<\frac{T}{2}$, there is one point of the string for which $T-\frac{xT}{2L}$ would be $<t$; hence it consists of the two straight lines

$$y=A(L-x)t,\ y=Ax\left(\frac{T}{2}-t\right).$$

To their point of intersection corresponds the abscissa

$$x=2L\frac{t}{T}.$$

It moves upon the parabola

$$y=\frac{AT}{2L}(L-x)x \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (29)$$

from $x=0$ to $x=L$ while t increases from 0 to $\frac{T}{2}$.

For $t=\frac{T}{2}$, only the middle equation of (27) holds good; we have therefore $y=0$; that is, at the expiration of the half vibration-period all the points of the string pass through the position of equilibrium.

For $t>\frac{T}{2}$ the two equations

$$y = \mathrm{A}x\left(\frac{\mathrm{T}}{2} - t\right), \quad y = -\mathrm{A}(\mathrm{L}-x)(\mathrm{T}-t)$$

hold good.

The string is deflected from the position of equilibrium in the opposite direction, y becoming negative, while previously (when $\mathrm{A}>0$) it was positive. It consists of two straight lines whose point of intersection passes along the parabola symmetrical to (29)

$$y = -\frac{\mathrm{AT}}{2\mathrm{L}}(\mathrm{L}=x)x, \quad \ldots\ldots \quad (30)$$

in the direction from $x=\mathrm{L}$ to $x=0$.

Lastly, for $t=\mathrm{T}$ the last of the equations (27) subsists; the string has returned to its initial position.

The parabolas (29) and (30) would be travelled in the opposite direction by the angle of the string on the second of the two suppositions respecting $\mathfrak{T}$ possible according to no. 11 and, in accordance therewith, the substitution of $\mathrm{L}-x$ for x in the foregoing formulæ.

14. It is of importance for acoustics that the function y, defined by (27), be expanded into a Fourrier series. Since $y=0$ for $t=0$ and $t=\frac{\mathrm{T}}{2}$, a series of sines will be chosen so that

$$y = \Sigma \mathrm{B}_n \sin\frac{2n\pi t}{\mathrm{T}},$$

$$\mathrm{B}_n = \frac{2}{\mathrm{T}}\int_0^{\mathrm{T}} y \sin\frac{2n\pi t}{\mathrm{T}}\,dt = \frac{\mathrm{ALT}}{n^2\pi^2}\sin\frac{n\pi a}{\mathrm{L}}.$$

If, in addition, by means of (28) the amplitude P of the central point of the string be introduced, we shall get for y the following expansion into a trigonometrical series:—

$$y = \frac{8\mathrm{P}}{n^2}\Sigma\frac{1}{n^2}\sin\frac{n\pi x}{\mathrm{L}}\sin\frac{2n\pi t}{\mathrm{T}}. \quad \ldots\ldots \quad (31)$$

This is of the same form as the series which otherwise occur with vibrating strings.

According to formula (31), besides the fundamental tone all the upper tones are contained in the sound of the string. But if the stroking-place happens to be at a point marking an aliquot part, those upper tones are wanting (according to the observations of Helmholtz and Neumann) which have a node-point at the stroking-place. This fact does not obtain expression in formula (31). Hence, although the above-described motion of the violin-string agrees very well with Neumann's stroboscopic observations, the hypothesis enunciated in no. 8

can only be designated as approximately accordant. On this account we will now give another treatment of the motion of the violin-string, more closely agreeing with the facts, for the case that the bow acts at a node-point of an upper tone.

15. For our purpose it is expedient at first to let the supposition made in no. 8 stand, but now to take into account the occurrence of node-points, which has hitherto been excluded.

The occurrence of node-points is usually settled with the aid of Fourrier's series. This, however, is not necessary; for d'Alembert's solution conducts to similar results. The latter can be applied in our case also; we can therefore assume as known that the points

$$x = \frac{2}{m}\mathrm{L}, \quad x = \frac{3}{m}\mathrm{L}, \;\ldots\ldots\; x = \frac{m-1}{m}\mathrm{L}$$

must be node-points as soon as the point $x = \frac{\mathrm{L}}{m}$ is a node, and that every portion of the string vibrates separately as an independent string, each two adjacent parts vibrating symmetrically to one another. If, then, A is a constant and T the vibration-period of a division of the string, we have, according to (27),

$$\text{for } 0 \leqq x \leqq \frac{\mathrm{L}}{m}:\!-$$

$$\left.\begin{array}{lll} y = \mathrm{A}\left(\frac{\mathrm{L}}{m} - x\right)t & \text{for} & 0 \leqq \frac{t}{\mathrm{T}} \leqq \frac{mx}{2\mathrm{L}}, \\ y = \mathrm{A}x\left(\frac{\mathrm{T}}{2} - t\right) & \;\;\text{"} & \frac{mx}{2\mathrm{L}} \leqq \frac{t}{\mathrm{T}} \leqq \frac{2\mathrm{L} - mx}{2\mathrm{L}}, \\ y = -\mathrm{A}\left(\frac{\mathrm{L}}{m} - x\right)(\mathrm{T} - t) & \;\;\text{"} & \frac{2\mathrm{L} - mx}{2\mathrm{L}} \leqq \frac{t}{\mathrm{T}} \leqq 1, \end{array}\right\} \quad (32)$$

where now $\frac{2\mathrm{L}}{m\mathrm{T}} = \alpha$, if α denotes the fundamental constant of the differential equation (2).

Further, for $\frac{\mathrm{L}}{m} \leqq x \leqq \frac{\mathrm{L}}{m}$:—

$$\left.\begin{array}{lll} y = \mathrm{A}\left(\frac{\mathrm{L}}{m} - x\right)t & \text{for} & 0 \leqq \frac{t}{\mathrm{T}} \leqq \frac{2\mathrm{L} - mx}{2\mathrm{L}}; \\ y = -\mathrm{A}\left(\frac{2\mathrm{L}}{m} - x\right)\left(\frac{\mathrm{T}}{2} - t\right) & \;\;\text{"} & \frac{2\mathrm{L} - mx}{2\mathrm{L}} \leqq \frac{t}{\mathrm{T}} \leqq \frac{mx}{2\mathrm{L}}; \\ y = -\mathrm{A}\left(\frac{\mathrm{L}}{m} - x\right)(\mathrm{T} - t) & \;\;\text{"} & \frac{mx}{2\mathrm{L}} \leqq \frac{t}{\mathrm{T}} \leqq 1. \end{array}\right\} \quad (33)$$

In the next interval, where $\frac{2L}{m} \leqq x \leqq \frac{3L}{m}$, we should have:—

$$y = A\left(\frac{3L}{m} - x\right)t \qquad \text{for } 0 \leqq \frac{t}{T} \leqq \frac{mx-2L}{2L};$$

$$y = A\left(x - \frac{2L}{m}\right)\left(\frac{T}{2} - t\right)t \quad ,, \quad \frac{mx-2L}{2L} \leqq \frac{t}{T} \leqq \frac{4L-mx}{2L};$$

$$y = -A\left(\frac{3L}{m} - x\right)(T-t) \quad ,, \quad \frac{4L-mx}{2L} \leqq \frac{t}{T} \leqq 1.$$

Lastly, we find generally, when

$$\frac{2i}{m}L \leqq x \leqq \frac{2i+1}{m}L:—$$

$$y = A\left(\frac{2i+1}{m}L - x\right)t \qquad \text{for } 0 \leqq \frac{t}{T} \leqq \frac{mx-2iL}{2L};$$

$$y = A\left(x - \frac{2i}{m}L\right)\left(\frac{T}{2} - t\right) \quad ,, \quad \frac{mx-2iL}{2L} \leqq \frac{t}{T} \leqq \frac{2(i+1)L-mx}{2L};$$

$$y = -A\left(\frac{2i+1}{m}L - x\right)(T-t) \text{ for } \frac{2(i+1)L-mx}{2L} \leqq \frac{t}{T} \leqq 1.$$

And when $\frac{2i-1}{m}L \leqq x \leqq \frac{2i}{m}L$:—

$$y = A\left(\frac{2i-1}{m}L - x\right)t \qquad \text{for } 0 \leqq \frac{t}{T} \leqq \frac{2iL-mx}{2L};$$

$$y = -A\left(\frac{2i}{m}L - x\right)\left(\frac{T}{2} - t\right) \quad ,, \quad \frac{2iL-mx}{2L} \leqq \frac{t}{T} \leqq \frac{mx-2(i-1)L}{2L};$$

$$y = -A\left(\frac{2i-1}{m}L - x\right)(T-t) \text{ for } \frac{mx-2(i-1)L}{2L} \leqq \frac{t}{T} \leqq 1.$$

Since y is a function $\phi(x)$ of x satisfying the conditions

$$\phi\left(x + \frac{2L}{m}\right) = \phi(x),$$

$$\phi\left(x + \frac{L}{m}\right) = -\phi\left(\frac{2L}{m} - x\right),$$

in expanding it into a series of sines we need only make use of equations (32). We find (analogously to no. 14)

$$y = \frac{ALT}{m\pi^2}\Sigma\frac{1}{n^2}\sin\frac{nm\pi x}{L}\sin\frac{2n\pi t}{T}. \quad . \quad . \quad . \quad (34)$$

16. It shall now be assumed, further, that the several portions of the string vibrate by themselves, as in the preceding

case, but that the points

$$x=\frac{L}{m},\quad x=\frac{2L}{m},\ \ldots\ x=\frac{m-1}{m}L,$$

which just now as node-points remained fixed, themselves execute vibrations in accordance with the law enunciated in (27). Evidently we have then

$$y=\eta+\Delta\eta,$$

if η signifies the function denoted in (27) by y, and if $\Delta\eta$ signifies the function denoted in no. 15 by y; only we must in the latter substitute for A another constant B, and replace T by $\frac{T}{m}$.

We thus find, if $0\leqq x\leqq\frac{L}{m}$,

$$\left.\begin{array}{l}
y=\Big[A(L-x)+B\Big(\frac{L}{m}-x\Big)\Big]t \quad \text{for } 0\leqq\frac{t}{T}\leqq\frac{x}{2L},\\
y=\Big[A\Big(\frac{T}{2}-t\Big)+B\Big(\frac{T}{2m}-t\Big)\Big]x \quad ,, \quad \frac{x}{2L}\leqq\frac{t}{T}\leqq\frac{2L-mx}{2mL},\\
y=Ax\Big(\frac{T}{2}-t\Big)-B\Big(\frac{L}{m}-x\Big)\Big(\frac{T}{m}-t\Big)\\
\qquad\qquad\qquad\qquad \text{for } \frac{2L-mx}{2mL}\leqq\frac{t}{T}\leqq\frac{2L+mx}{2mL},\\
y=\Big[A\Big(\frac{T}{2}-t\Big)+B\Big(\frac{3T}{2m}-t\Big)\Big]x\\
\qquad\qquad\qquad\qquad \text{for } \frac{2L+mx}{2mL}\leqq\frac{t}{T}\leqq\frac{4L-mx}{2mL},\ \text{\&c.}
\end{array}\right\}\quad(35)$$

We have generally:—

$$y=\Big[A\Big(\frac{T}{2}-t\Big)+B\Big(\frac{2i-1}{2m}T-t\Big)\Big]x$$

$$\text{for } \frac{2(i-1)L-mx}{2mL}\leqq\frac{t}{T}\leqq\frac{2iL-mx}{2mL};$$

$$y=Ax\Big(\frac{T}{2}-t\Big)-B\Big(\frac{L}{m}-x\Big)\Big(\frac{iT}{m}-t\Big)$$

$$\text{for } \frac{2iL-mx}{2mL}\leqq\frac{t}{T}\leqq\frac{2iL+mx}{2mL};$$

$$y=\Big[A\frac{T}{2}-t\Big)+B\Big(\frac{2i+1}{2m}T-t\Big)\Big]x$$

$$\text{for } \frac{2iL+mx}{2mL}\leqq\frac{t}{T}\leqq\frac{2(i+1)L-mx}{2mL}.$$

This system of three equations is to be formed successively

for $i=1, 3, 5, \ldots m-1$ when m is even,
for $i=1, 3, 5, \ldots m-2$ when m is odd.

In the former case the operation finishes with the last of these three equations, formed for $i=m-1$; in the other case, with the first, formed for $i=m$. Therefore, when m is even, the last equation reads

$$y=\left[A\left(\frac{T}{2}-t\right)+B\left(\frac{2m-1}{2m}T-t\right)\right]x$$

$$\text{for } \frac{2(m-1)L+mx}{2mL} \leqq \frac{t}{T} \leqq \frac{2L-x}{2L};$$

and when m is odd,

$$y=\left[A\left(\frac{T}{2}-t\right)+B\left(\frac{2m-1}{2m}T-t\right)\right]x$$

$$\text{for } \frac{2(m-1)L-mx}{2mL} \leqq \frac{t}{T} \leqq \frac{2L-x}{2L}.$$

The equation

$$y=-A(L-x)(T-t)-B\left(\frac{L}{m}-x\right)\left(\frac{T}{m}-t\right)$$

$$\text{for } \frac{2L-x}{2L} \leqq \frac{t}{T} \leqq 1 \quad . \quad (36)$$

must be superadded, in order to have the motion in the interval of time from $t=0$ to $t=T$ represented for all the points between $x=0$ and $x=\frac{L}{m}$. From (35) and (36) it follows that the ascending motion, for the part of the string considered, proceeds with the constant velocity

$$A(L-x)+B\left(\frac{L}{m}-x\right);$$

while the velocity of the descending motion fluctuates between the values

$$-(A+B)x, \quad -Ax+B\left(\frac{L}{m}-x\right), \quad -(A+B)x, \ldots$$

17. The general formula can be constructed in an analogous manner. We have, when

$$\frac{2i}{m}L \leqq x \leqq \frac{2i+1}{m}L:—$$

$$y=A(L-x)t+B\left(\frac{2i+1}{m}L-x\right)\left(t-\frac{j-1}{m}T\right)$$

$$\text{for } \frac{j-1}{m} \leqq \frac{t}{T} \leqq \frac{mx-2(i-j+1)L}{2mL};$$

$$y=\mathrm{A}(\mathrm{L}-x)t+\mathrm{B}\left(x-\frac{2i}{m}\mathrm{L}\right)\left(\frac{j+1}{2m}\mathrm{T}-t\right)$$

$$\text{for } \frac{mx-2(i-j+1)\mathrm{L}}{2m\mathrm{L}} \leq \frac{t}{\mathrm{T}} \leq \frac{2(i+j)\mathrm{L}-mx}{2m\mathrm{L}};$$

$$y=\mathrm{A}(\mathrm{L}-x)t-\mathrm{B}\left(\frac{2i+1}{m}\mathrm{L}-x\right)\left(\frac{j}{m}\mathrm{T}-t\right)$$

$$\text{for } \frac{2(i+j)\mathrm{L}-mx}{2m\mathrm{L}} \leq \frac{t}{\mathrm{T}} \leq \frac{j}{m}.$$

These equations are to be formed for $j=1, 2, 3, \ldots . i$; and then the first of them, formed for $j=i+1$, is to be superadded —namely,

$$y=\mathrm{A}(\mathrm{L}+x)t+\mathrm{B}\left(\frac{2i+1}{m}\mathrm{L}-x\right)\left(t-\frac{i}{m}\mathrm{T}\right)$$

$$\text{for } \frac{i}{m} \leq \frac{t}{\mathrm{T}} \leq \frac{x}{2\mathrm{L}}.$$

Thereby the first half of the *ascending* motion is determined; its velocity fluctuates between the values

$$\mathrm{A}(\mathrm{L}-x)+\mathrm{B}\left(\frac{2i+1}{m}\mathrm{L}-x\right)$$

and

$$\mathrm{A}(\mathrm{L}-x)+\mathrm{B}\left(\frac{2i}{m}\mathrm{L}-x\right).$$

For the *descending* motion we find:—

$$y=\mathrm{A}x\left(\frac{\mathrm{T}}{2}-t\right)+\mathrm{B}\left(x-\frac{2i}{m}\mathrm{L}\right)\left(\frac{i+j+1}{m}\mathrm{T}-t\right)$$

$$\text{for } \frac{mx+2(j-1)\mathrm{L}}{2m\mathrm{L}} \leq \frac{t}{\mathrm{T}} \leq \frac{2(2i+j)\mathrm{L}-mx}{2m\mathrm{L}};$$

$$y=\mathrm{A}x\left(\frac{\mathrm{T}}{2}-t\right)-\mathrm{B}\left(\frac{2i+1}{m}\mathrm{L}-x\right)\left(\frac{i+j}{m}\mathrm{T}-t\right)$$

$$\text{for } \frac{2(2i+j)\mathrm{L}-mx}{2m\mathrm{L}} \leq t \leq \frac{mx+2j\mathrm{L}}{2m\mathrm{L}}.$$

These equations are to be formed for

$$j=1, 2, \ldots . m-2i-1,$$

and then the first of them, formed for $j=m-2i$ is to be superadded—namely

$$y=\mathrm{A}x\left(\frac{\mathrm{T}}{2}-t\right)+\mathrm{B}\left(x-\frac{2i\mathrm{L}}{m}\right)\left(\frac{m-i+1}{m}\mathrm{T}-t\right)$$

$$\text{for } \frac{mx+2(m-2i-1)\mathrm{L}}{2m\mathrm{L}}\leqq\frac{t}{\mathrm{T}}\leqq\frac{2\mathrm{L}-x}{2\mathrm{L}}.$$

Accordingly the velocity of the descending motion fluctuates between the values

$$-(\mathrm{A}+\mathrm{B})x+\frac{2i}{m}\mathrm{BL} \text{ and } -(\mathrm{A}+\mathrm{B})x+\frac{2i+1}{m}\mathrm{BL}.$$

The *other half of the ascending motion* now remains to be considered. It is represented by the equations

$$y=-\mathrm{A}(\mathrm{L}-x)(\mathrm{T}-t)-\mathrm{B}\left(\frac{2i+1}{m}\mathrm{L}-x\right)\left(\frac{m-i-1+j}{m}\mathrm{T}-t\right)$$

$$\text{for } \frac{2(m+j-1)\mathrm{L}-mx}{2m\mathrm{L}}\leqq\frac{t}{\mathrm{T}}\leqq\frac{2(m-2i-1+j)\mathrm{L}+mx}{2m\mathrm{L}},$$

$$y=-\mathrm{A}(\mathrm{L}-x)(\mathrm{T}-t)+\mathrm{B}\left(x-\frac{2i}{m}\mathrm{L}\right)\left(\frac{m-i+j+1}{m}\mathrm{T}-t\right)$$

$$\text{for } \frac{2(m+j-2i-1)\mathrm{L}+mx}{2m\mathrm{L}}\leqq\frac{t}{\mathrm{T}}\leqq\frac{2(m+j)\mathrm{L}-mx}{2m\mathrm{L}}.$$

These equations are to be formed for $j=1, 2, \ldots . 2i-1$. The first of them holds good also for $j=2i$; if the latter has been superadded, the entire system concludes with the equation

$$y=-\mathrm{A}(\mathrm{L}-x)(\mathrm{T}-t)+\mathrm{B}\left(x-\frac{2i}{m}\mathrm{L}\right)\left(\frac{m+i+1}{m}\mathrm{T}-t\right)$$

$$\text{for } \frac{2(m-1)\mathrm{L}+mx}{2m\mathrm{L}}\leqq\frac{t}{\mathrm{T}}\leqq 1.$$

Analogous formulæ can be constructed for those parts of the string which are limited by the points $x=\frac{2i-1}{m}\mathrm{L}$ and $x=\frac{2i}{m}\mathrm{L}$. This, however, shall not here be pursued further.

18. The function y defined by the foregoing conducts to a very simple Fourrier series. We find, namely,

$$y=\Sigma\,\mathrm{S}_n\sin\frac{2n\pi t}{\mathrm{T}}=\eta+\Delta\eta,$$

if η and $\Delta\eta$ have the signification stated in no. 16, so that η is equal to the series given in (31), while, according to (34),

$$\Delta\eta=\frac{\mathrm{BLT}}{m^2\pi^2}\Sigma\frac{1}{n^2}\sin\frac{nm\pi x}{\mathrm{L}}\cdot\sin\frac{2nm\pi t}{\mathrm{T}}.$$

Both series can be integrated term by term; hence we have

$$S_n=\frac{2}{T}\int_0^T(\eta+\Delta\eta)dt=\frac{(A+B)LT}{n^2\pi^2}\sin\frac{n\pi x}{L},$$

or

$$=\frac{ALT}{n^2\pi^2}\sin\frac{n\pi x}{L},$$

according as n is or is not divisible by m.

The representation of y by a Fourrier series gives therefore

$$y=\frac{LT}{\pi^2}\sum\frac{A_n}{n^2}\sin\frac{n\pi x}{L}\sin\frac{2n\pi t}{T},$$

in which, when n is not divisible by m,

$$A_n=A,$$

and, when n is divisible by m,

$$A_n=A+B.$$

Christoffel's conditions of discontinuity are spontaneously fulfilled by the equations formed in no. 17; hence the quantities A and B are at first arbitrary. Observations have nevertheless shown that the upper tones corresponding to the places $x=\frac{L}{m},\frac{2L}{m},\ldots$ are not present in the sound of the string. B must therefore be $=-A$.

Consequently according to no. 16, *for a point between* $x=0$ *and* $x-\frac{L}{m}$, *the velocity of the ascending motion is equal to* $A\frac{m-1}{m}$, *therefore independent of* x; *but that of the descending motion fluctuates between the values*

$$0,\ -A\frac{L}{m},\ 0,\ -A\frac{L}{m},\ldots.$$

The curve of the velocities will therefore consist of a rectilinear ascending and a scalariform descending portion.*

This is in accordance with Helmholtz's figures (*l. c.* p. 144). In the declinations published by Neumann (*l. c.*) the descending portion is not scalariform, but slightly rippled, and thus the equation $A+B=0$ is not perfectly fulfilled. Indeed all the upper tones are absent only when the bow is drawn very uniformly across the string, and touches it very accurately at the wished-for node-point. The number of the ripples which

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* This is confirmed also by Clifton's observations: *cf.* Donkin, *l. c.* p. 136.

appear in Neumann's drawings agrees with the formulæ given in no. 16*.

For other points of the string both the descending and the ascending portion of the velocity-curve are rippled†. For $\frac{2i}{m}\mathrm{L} \leqq x \leqq \frac{2i+1}{m}\mathrm{L}$ for example, the velocity of the ascending motion fluctuates, according to no. 17, when $\mathrm{B}=-\mathrm{A}$, between the values

$$\mathrm{AL}\frac{m-2i-1}{m} \text{ and } \mathrm{AL}\frac{m-2i}{m},$$

that of the descending motion between

$$-\mathrm{AL}\frac{2i}{m} \text{ and } -\mathrm{AL}\frac{2i+1}{m}.$$

If the vibration-form considered be resolved into two elementary vibrations η and $\Delta\eta$ (*conf.* no. 16), and the amplitude of the central point of the string for the motion η be denoted by P, while the amplitude of the central point of any one part of the string for the component $\Delta\eta$ is denoted by P′, we shall have, according to (28),

$$\mathrm{A}=\frac{8\mathrm{P}}{\mathrm{LT}}, \quad \mathrm{B}=\frac{8m^2\mathrm{P}'}{\mathrm{LT}},$$

and therefore, when $\mathrm{B}=-\mathrm{A}$,

$$\mathrm{P}'=-\frac{\mathrm{P}}{m^2}.$$

The form of the string at a given time will be approximately the same as in the case described in no. 13, except that for the straight lines there occurring slightly rippled lines are to be substituted.

19. We may remark that Helmholtz gives the following two equations in place of the three equations (27):—

$$y=\frac{8\mathrm{P}}{\mathrm{LT}}(\mathrm{L}-x)t \text{ for } 0 \leqq t \leqq \frac{x\mathrm{T}}{\mathrm{L}};$$

$$y=\frac{8\mathrm{P}}{\mathrm{LT}}x(\mathrm{T}-t) \quad ,, \quad \frac{x\mathrm{T}}{\mathrm{L}} \leqq t \leqq \mathrm{T}.$$

They only agree with the first two of the equations (27) if

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[*] Disregarding isolated cases in which *larger* secondary ripples appear, and thus the motion is still more complicated.

[†] These ripplings are recognized also in the following manner:—If a bright violin-string vibrates over a dark ground, the field of vibration appears to the naked eye uniformly bright; but if observed by means of a lens, there appear in it a number of brighter lines running parallel with the string.

$\frac{T}{2}$ is substituted for T and $\frac{P}{2}$ for P; and then they represent only one half of the motion, while T signifies the entire duration of a vibration. On the other hand, the trigonometric series given by (31) agrees in substance with the series found by Helmholtz, if, in the latter, $\mathfrak{X}$ is understood as denoting the time of the ascending motion of a single determined point of the string. How this series in Helmholtz's work is obtained does not appear to me to be clearly shown. For, apparently, the letters x and $\mathfrak{X}$ are at one time used for a variable, and then again for a fixed point of the string; and it does not distinctly appear whether the mathematical development rests upon the assumption of the law enunciated in no. 8 as valid for *all* points of the string, or whether it is presupposed for a *single* point only. The latter is done by Donkin and Lord Rayleigh. But if the problem is to be determined, not only must y but also $\frac{\partial y}{\partial x}$ be given as a function of t for the one point of the string. This uncertainty asserts itself in the works mentioned by rendering it necessary, in the determination of $\mathfrak{X}$ as a function of x, to go back to the observations.

Freiburg i. B., July 12, 1879.

XXVIII. *Cyclones of the Bay of Bengal*[*]. *By* W. R. BIRT.

It is well known that the Bay of Bengal is a very dangerous locality for our East-Indian shipping, especially the head of the Bay in the neighbourhood of Calcutta. The danger principally arises from the cyclones which sweep over the Bay, wrecking ships and raising immense storm-waves exceedingly destructive of human life. Thus in the year 1737, October 7, *three hundred thousand* people are stated to have been drowned; more than one hundred years later, in 1876, October 29 to November 1, *one hundred thousand* people were drowned in Backergunge. In June 1822, *fifty thousand* people perished by the storm-wave. In the year 1864, October 2 to 5, *forty-eight thousand* people were drowned in the Hooghly by the storm-wave; and in 1831, October 31, *ten thousand* people were drowned by the storm-wave. In the following year, 1832, May 21, between *eight* and *ten thousand* people perished by the same agency. The total loss of human life, as recorded in connexion with severe cyclones that swept over the Bay on the dates given above, is no less

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[*] Report of the Madras Cyclone of May 1877, by J. Eliot, Esq, M.A., Meteorological Reporter to the Government of Bengal. Calcutta, 1879.

than *five hundred and seventeen thousand*; and this great number is small compared with the immense number that must have perished in the aggregate number of cyclones that have occurred; consequently any addition to our knowledge of these destructive meteors cannot fail of being interesting to our readers, as well as to navigators and seamen.

Antecedents of Powerful Cyclones.

The importance of a knowledge of the antecedents of cyclones, especially of such as are attended with great loss of life, cannot be overrated. For an increase of our knowledge in this respect we are indebted to J. Eliot, Esq., M.A., Meteorological Reporter to the Government of Bengal, in his Report on the Madras Cyclone of May 1877. In order clearly to connect the antecedents with the cyclones it is necessary to take into consideration the two transition-periods:—one, in April or near the latter end of April, between the breaking-up of the north-east monsoon and the establishment of the south-west; the other, in October, succeeding the south-west monsoon and ushering in the north-east monsoon. Mr. Eliot finds that cyclones or revolving storms of great extent and intensity occur only at these periods, while cyclonic disturbances of small intensity are of frequent occurrence during the rainy season or south-west monsoon. During the continuance of each monsoon certain meteorological relations are established over the Bay and the adjoining land-areas; and when a change from one monsoon to the other occurs—in other words, when a transition-period sets in, it implies a complete reversal as regards the preceding monsoon of baric gradient, wind-direction, and other atmospheric conditions over the large area including the Arabian Sea, the Bay of Bengal, India, and Further India.

These reversals of meteorological elements occurring gradually tend to an approximate equality and uniformity of barometric pressure over a large area. This approximate uniformity of pressure over and round the coast of the Bay of Bengal, accompanied by light variable winds and calms over a considerable portion of the Bay with little or no rainfall, Mr. Eliot regards as invariable antecedents of a powerful cyclone.

Connexion of Rainfall with Cyclones.

In consequence of the small rainfall about the time of the commencements of the transition-periods, the amount of aqueous vapour in the atmosphere accumulates and gives rise to some peculiar sky-effects. When the point of saturation is attained, precipitation follows, and a heavy rainfall is concentrated over a portion of the Bay, accompanied by a strong in-draught most marked from the Indian Ocean at the entrance of the Bay. Mr. Eliot regards this heavy rainfall as an immediate antecedent of a cyclone. For some years, we believe, an opinion has existed that cyclones are connected with rainfall. If our memory serves us correctly, we remember that some time in the early part of the decade 1850–60, a work on storms was published in which the author laid some stress on the fact that cyclones mostly originate in regions characterized by active precipitation of aqueous vapour. A later writer, the Rev. Clement Ley, speaks of extensive precipitation contributing to cyclonic generation as follows:—"Extensive precipitation occurring in a region of atmosphere previously approaching a condition of tranquillity is the primary factor of every system of baric depression with its resulting atmospheric circulation, retrograde in the northern and direct in the southern hemisphere." Mr. Meldrum's Mauritius Meteorological Results for the year 1875 furnish some evidence which appears to indicate that the connexion between rainfall and cyclones is of a much stronger kind than he had before anticipated. He says:—"The oft-observed fact that the dry weather in Mauritius from December to April is accompanied by an absence of hurricanes in the Indian Ocean, has been repeated in 1875. It is remarkable that, while February is generally the wettest month in Mauritius and the stormiest month in the Indian Ocean, it was in 1875 the reverse in both these respects. This seems to point to an intimate connexion between rainfall and cyclones."

Mr. Eliot, in discussing the principal facts of the Midnapore cyclone of October 1874, found that the rainfall on that occasion was conterminous with the entire duration of the cyclone. "It is," he says, "in this case, as in others, the one meteorological factor, the introduction of which into the previously existing conditions determined the commencement of the cyclone; and the intensity and situation of the rainfall ran parallel with the intensity and position of the cyclone. The evidence which the Midnapore cyclone thus affords of the intimate connexion between rainfall and cyclone-generation is very strong, and is almost equally conclusive in favour of the condensation theory of the origin of cyclones."

Connexion between Sun-spots and Cyclones.

Mr. Eliot, in commenting on the passage above quoted from Mr. Meldrum, remarks:—"This statement of Mr. Meldrum's, when it is remembered that he has upheld the parallel-wind hypothesis for the explanation of the cyclones of the southern Indian Ocean, is a very remarkable one. It may also be added, in further confirmation of it, that it affords some explanation of the connexion between cyclone-frequency, rainfall, and sun-spot-frequency. Mr. Meldrum's investigations have established that the frequency and energy of cyclones in the Indian Ocean and the rainfall of the globe vary directly with the number of sun-spots, or are subject to the same law of periodicity. This relation is almost self-evident, if it be admitted that massive terrestrial atmospheric phenomena are due solely or primarily to the action of solar radiant heat, and that any marked periodicity in the cause (solar heat and action) must impress a corresponding periodicity on the more important phenomena of the earth's atmosphere. . . . . . If it be granted that rainfall varies with change of the sun's photosphere, as indicated by the number of sun-spots, and that cyclones are due to rainfall, it is probable that cyclone-frequency will be subject to the same law of periodicity which affects the distribution of rainfall, and, consequently, that the occurrence of cyclones will vary with the number of sun-spots. This probable mutual connexion between phenomena apparently unconnected appears to give additional weight and force to the condensation theory."

In calling attention to severe cyclones Mr. Eliot remarks, "That severe cyclones at the head of the Bay during the month of May have generally occurred about the period of minimum sun-spots." He also finds the same connexion characteristic of the opposite transition-period, viz. October:—"The three cyclones which have occurred in the month of October during the present century, happened at or near minimum sun-spot years." He embodies the results in two general propositions as follows:—

"First. That severe cyclones, *i. e.* cyclonic storms extending over a large area and of considerable barometric depression, and therefore attended by a destructive storm-wave, occur at or near the minimum sun-spot period, and that the year of minimum solar spots, or the preceding year, appears to be the most probable.

"Second. That, during the months of November and December, only ten severe cyclones have occurred during the present century; or, taking every eleventh year as defining the minimum solar-spot period, they occurred as follows:—1822, three storms, viz. in 1822, 23, and 24; 1833, three storms, viz. in 1831, 32, and 33; 1844, one storm in 1842; 1855, one storm in 1852; 1866, one storm in 1864; 1877, one storm in 1876." "Hence," Mr. Eliot says, "there is a strong probability of at least one severe cyclone at each minimum solar-spot period, and of its occurring in the same year, or in the first or second year before the year of minimum spots."

[It is to be remarked that the three storms at the minimum period of 1822 occurred in that year and the two years succeeding it.]

Mr. Eliot proceeds:—"It will thus be seen that the conclusions to which I have arrived are quite different from those established by Mr. Meldrum for the southern Indian Ocean. His conclusions are that cyclones in that region are most frequent and most intense at or near the maximum solar-spot periods." Mr. Eliot considers that the data are too imperfect to attempt to assign any reason for this opposite character of cyclone distribution in the Bay of Bengal and the southern Indian Ocean as referred to sun-spot periods.

For the above views, we regard Mr. Eliot as an official authority. We have read his memoir on the Madras Cyclone, May 1877, with great interest, and consider that it embodies very valuable information on cyclones generally.

XXIX. *On a new Action of the Magnet on Electric Currents.* *By* E. H. Hall, *Fellow of the Johns Hopkins University*[*].

SOMETIME during the last University year, while I was reading Maxwell's 'Electricity and Magnetism' in connexion with Professor Rowland's lectures, my attention was particularly attracted by the following passage in vol. ii. p. 144:—

"It must be carefully remembered, that the mechanical force which urges a conductor carrying a current across the lines of magnetic force, acts, not on the electric current, but on the conductor which carries it. If the conductor be a rotating disk or a fluid, it will move in obedience to this force; and this motion may or may not be accompanied with a change of position of the electric current which it carries. But if the current itself be free to choose any path through a fixed solid conductor or a network of wires, then, when a constant magnetic force is made to act on the system, the path of the current through the conductors is not permanently altered, but after certain transient phenomena, called induction-currents, have subsided, the distribution of the current will be found to be the same as if no magnetic force were in action. The only force which acts on electric

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[*] From a separate impression from the 'American Journal of Mathematics,' 1879, communicated by the Author.

currents is electromotive force, which must be distinguished from the mechanical force which is the subject of this chapter."

This statement seemed to me to be contrary to the most natural supposition in the case considered, taking into account the fact that a wire not bearing a current is in general not affected by a magnet, and that a wire bearing a current is affected exactly in proportion to the strength of the current, while the size and, in general, the material of the wire are matters of indifference. Moreover, in explaining the phenomena of statical electricity, it is customary to say that charged bodies are attracted toward each other or the contrary solely by the attraction or repulsion of the charges for each other.

Soon after reading the above statement in Maxwell I read an article by Prof. Edlund, entitled "Unipolar Induction" (Phil. Mag. Oct. 1878, or *Annales de Chimie et de Physique*, Jan. 1879), in which the author evidently assumes that a magnet acts upon a current in a fixed conductor just as it acts upon the conductor itself when free to move.

Finding these two authorities at variance, I brought the question to Prof. Rowland. He told me he doubted the truth of Maxwell's statement, and had some time before made a hasty experiment for the purpose of detecting, if possible, some action of the magnet on the current itself, though without success. Being very busy with other matters however, he had no immediate intention of carrying the investigation further.

I now began to give the matter more attention, and hit upon a method that seemed to promise a solution of the problem. I laid my plan before Prof. Rowland, and asked whether he had any objection to my making the experiment. He approved of my method in the main, though suggesting some very important changes in the proposed form and arrangement of the apparatus. The experiment proposed was suggested by the following reflection:—If the current of electricity in a fixed conductor is itself attracted by a magnet, the current should be drawn to one side of the wire, and therefore the resistance experienced should be increased.

To test this theory, a flat spiral of German-silver wire was enclosed between two thin disks of hard rubber, and the whole placed between the poles of an electromagnet in such a position that the lines of magnetic force would pass through the spiral at right angles to the current of electricity. The wire of the spiral was about $\frac{1}{2}$ millim. in diameter, and the resistance of the spiral was about two ohms. The magnet was worked by a battery of twenty Bunsen cells joined four in series and five abreast. The strength of the magnetic field in which the coil was placed was probably fifteen or twenty thousand times *H*, the horizontal intensity of the earth's magnetism.

Making the spiral one arm of a Wheatstone's bridge, and using a low-resistance Thomson galvanometer, so delicately adjusted as to betray a change of about one part in a million in the resistance of the spiral, I made, from October 7th to October 11th inclusive, thirteen series of observations, each of forty readings. A reading would first be made with the magnet active in a certain direction, then a reading with the magnet inactive, then one with the magnet active in the direction opposite to the first, then with the magnet inactive, and so on till the series of forty readings was completed.

Some of the series seemed to show a slight increase of resistance due to the action of the magnet, some a slight decrease, the greatest change indicated by any complete series being a decrease of about one part in a hundred and fifty thousand. Nearly all the other series indicated a very much smaller change, the average change shown by the thirteen series being a decrease of about one part in five millions.

Apparently, then, the magnet's action caused no change in the resistance of the coil.

But though conclusive, apparently, in respect to any change of resistance, the above experiments are not sufficient to prove that a magnet cannot affect an electric current. If electricity is assumed to be an incompressible fluid, as some suspect it to be, we may conceive that the current of electricity flowing in a wire cannot be forced into one side of the wire or made to flow in any but a symmetrical manner. The magnet may *tend* to deflect the current without being able to do so. It is evident, however, that in this case there would exist a state of stress in the conductor, the electricity pressing, as it were, toward one side of the wire. Reasoning thus, I thought it necessary, in order to make a thorough investigation of the matter, to test for a difference of potential between points on opposite sides of the conductor.

This could be done by repeating the experiment formerly made by Prof. Rowland, and which was the following:—A disk or strip of metal, forming part of an electric circuit, was placed between the poles of an electromagnet, the disk cutting across the lines of force. The two poles of a sensitive galvanometer were then placed in connexion with different parts of the disk, through which an electric current was passing, until two nearly equipotential points were found. The magnet-current was then turned on and the galvanometer was observed, in order to detect any indication of a change in the relative potential of the two poles.

Owing probably to the fact that the metal disk used had considerable thickness, the experiment at that time failed to give any positive result. Prof. Rowland now advised me, in repeating this experiment, to use gold-leaf mounted on a plate of glass as my metal strip. I did so, and, experimenting as indicated above, succeeded on the 28th of October in obtaining, as the effect of the magnet's action, a decided deflexion of the galvanometer-needle.

This deflexion was much too large to be attributed to the direct action of the magnet on the galvanometer-needle, or to any similar cause. It was, moreover, a permanent deflexion, and therefore not to be accounted for by induction. The effect was reversed when the magnet was reversed. It was not reversed by transferring the poles of the galvanometer from one end of the strip to the other. In short, the phenomena observed were just such as we should expect to see if the electric current were pressed, but not moved, toward one side of the conductor.

In regard to the direction of this pressure or tendency, as dependent on the direction of the current in the gold-leaf and the direction of the lines of magnetic force, the following statement may be made:—If we regard an electric current as a single stream flowing from the positive to the negative pole, *i. e.* from the carbon pole of the battery through the circuit to the zinc pole, in this case the phenomena observed indicate that two *currents*, parallel and in the same direction, tend to repel each other. If, on the other hand, we regard the electric current as a stream flowing from the negative to the positive pole, in this case the phenomena observed indicate that two *currents* parallel and in the same direction tend to attract each other.

It is, of course, perfectly well known that two *conductors*, bearing currents parallel and in the same direction, are drawn toward each other. Whether this fact, taken in connexion with what has been said above, has any bearing upon the question of the absolute direction of the electric current, it is perhaps too early to decide.

In order to make some rough quantitative experiments, a new plate was prepared, consisting of a strip of gold-leaf about 2 centims. wide and 9 centims. long mounted on plate-glass. Good contact was ensured by pressing firmly down on each end of the strip of gold-leaf a thick piece of brass polished on the underside. To these pieces of brass the wires from a single Bunsen cell were soldered. The portion of the gold-leaf strip not covered by the pieces of brass was about $5\frac{1}{2}$ centims. in length, and had a resistance of about 2 ohms. The poles of a high-resistance Thomson galvanometer were placed in connexion with points opposite each other on the edges of the strip of gold-leaf, and midway between the pieces of brass. The glass plate bearing the gold-leaf was fastened, as the first one had been, by a soft cement to the flat end of one pole of the magnet, the other pole of the magnet being brought to within about 6 millims. of the strip of gold-leaf.

The apparatus being arranged as above described, on the 12th of November a series of observations was made for the purpose of determining the variations of the observed effect with known variations of the magnetic force and the strength of current through the gold-leaf.

The experiments were hastily and roughly made, but are sufficiently accurate, it is thought, to determine the law of variation above mentioned as well as the order of magnitude of the current through the Thomson galvanometer compared with the current through the gold-leaf and the intensity of the magnetic field.

The results obtained are as follows:—

Current through gold-leaf strip. C.	Strength of magnetic field. M.	Current through Thomson galvanometer. c.	$\frac{C \times M}{c}$.
·0616	11420 H	·00000000232	303000000000.
·0249	11240 „	· 085	329 .
·0389	11060 „	· 135	319 .
·0598	7670 „	· 147	312 ,
·0595	5700 „	· 104	326 .

H is the horizontal intensity of the earth's magnetism $=\cdot 19$ approximately.

Though the greatest difference in the last column above amounts to about 8 per cent. of the mean quotient, yet it seems safe to conclude that, with a given form and arrangement of apparatus, the action on the Thomson galvanometer is proportional to the product of the magnetic force by the current through the gold-leaf. This is not the same as saying that the effect on the Thomson galvanometer is under all circumstances proportional to the current which is passing between the poles of the magnet. If a strip of copper of the same length and breadth as the gold-leaf, but $\frac{1}{4}$ millim. in thickness, is substituted for the latter, the galvanometer fails to detect any current arising from the action of the magnet, except an induction-current at the moment of making or breaking the magnet circuit.

It has been stated above that in the experiments thus far tried the current apparently tends to move, without actually moving, toward the side of the conductor. I have in mind a form of apparatus which will, I think, allow the current to follow this tendency, and move across the lines of magnetic force. If this experiment succeeds, one or two others immediately suggest themselves.

To make a more complete and accurate study of the phenomenon described in the preceding pages, availing myself of the advice and assistance of Prof. Rowland, will probably occupy me for some months to come.

Baltimore, Nov. 19th, 1879.

It is perhaps allowable to speak of the action of the magnet as setting up in the strip of gold-leaf a new electromotive force at right angles to the primary electromotive force.

This new electromotive force cannot under ordinary conditions manifest itself, the circuit in which it might work being incomplete. When the circuit is completed by means of the Thomson galvanometer, a current flows.

The actual current through this galvanometer depends of course upon the resistance of the galvanometer and its connexions, as well as upon the distance between the two points of the gold-leaf at which the ends of the wires from the galvanometer are applied. We cannot, therefore, take the ratio of C and c above as the ratio of the primary and the transverse electromotive forces just mentioned.

If we represent by E' the difference of potential of two points a centimetre apart on the transverse diameter of the strip of gold-leaf, and by E the difference of potential of two points a centimetre apart on the longitudinal diameter of the same, a rough and hasty calculation for the experiments already made shows the ratio $\frac{E}{E'}$ to have varied from about 3000 to about 6500.

The transverse electromotive force E' seems to be, under ordinary circumstances, proportional to Mv, where M is the intensity of the magnetic field and v is the *velocity* of the electricity in the gold-leaf. Writing for v the equivalent expression $\frac{C}{s}$, where C is the primary current through a strip of the gold-leaf 1 centim. wide, and s the area of section of the same, we have $E' \propto \frac{MC}{s}$.

November 22nd, 1879.

XXX. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 152.]

February 4, 1880.—Henry Clifton Sorby, Esq., LL.D., F.R.S., President, in the Chair.

THE following communication was read:—

"On the Oligocene Strata of the Hampshire Basin." By Prof. John W. Judd, F.R.S., Sec.G.S.

The study of the succession of strata in the fluvio-marine series of the Isle of Wight and the New Forest is attended with considerable difficulties, partly on account of the inconstant character of the beds composing estuarine formations, and partly because of the thick superficial deposits which everywhere cover them. By Webster a Lower Freshwater Series, a Middle Marine, and an Upper Freshwater Series were recognized; but Mr. Prestwich showed, in the year 1846, that at Hamstead Cliff we have both freshwater and marine strata lying above all these; and in 1853 Edward Forbes proved that the marine and freshwater strata seen at Bembridge Ledge were not, as had previously been supposed, the equivalent of those of Headon Hill, but occupy a distinct and higher horizon. Hitherto, however (in spite of some suggestions to the contrary which were made by Dr. Wright and Prof. Hébert), the strata exposed at the base of Headon Hill have been believed to be a repetition, through an anticlinal fold, of those seen at Colwell and Totland Bays.

In the present memoir it is shown, both by stratigraphical and palæontological evidence, that the Colwell- and Totland-Bay beds are distinct from and overlie those at the base of Headon Hill. The distinctness and importance of the purely marine series exposed at Whitecliff Bay, Colwell Bay, and several localities in the New Forest is pointed out; and it is shown that, among the 200 forms of Mollusca which they contain, only one fifth are found in the Barton Clay below. For this important division of the strata the name of the *Brockenhurst Series* is proposed.

In consequence of the detection of an error in the accepted order of succession of the strata, a rectification of the classification of the fluvio-marine series is rendered necessary; and it is proposed to divide them as follows:—

1. The Hempstead Series (marine and estuarine), 100 feet.
2. The Bembridge Group (estuarine), 300 feet.
3. The Brockenhurst Series (marine), 25 to 100 feet.
4. The Headon Group (estuarine), 400 feet.

By this new grouping the strata of the Hampshire Basin are brought into exact correlation with those of France, Belgium, North Germany, and Switzerland; and the whole series of fluvio-marine beds in the Isle of Wight, which are shown to have a thickness of between 800 and 900 feet, are proved to be the representatives of the Lower and Middle Oligocene of those countries. The use of the term Oligocene in this country is advocated on the ground that by its adoption only can we avoid the inconvenient course of dividing the fluvio-marine series between the Eocene and the Miocene.

XXXI. *Intelligence and Miscellaneous Articles.*

ON INTERMITTENT CURRENTS AND THE INDUCTION-BALANCE.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

WITH reference to my communication on the subject of the Induction-Balance in your last issue, will you permit me to add a word in order to avoid any possible misunderstanding with reference to section 1.

I have there rather unguardedly spoken of the induction-balance as if I considered that all Prof. Hughes had done was to apply a telephone to it. I by no means intended to convey this impression. Of course it is perfectly true, and Prof. Hughes himself points it out (Phil. Mag. July 1879), that induction-currents had been balanced by a double pair of coils by both Dove and Felici some time ago; but, nevertheless, the induction-balance as devised by Prof. Hughes must be regarded as an essentially new instrument; for no one could have obtained the same effects from the old "differential inductor" of Dove without enormous modification.

Soon after hearing of Prof. Hughes's discovery, I tried to obtain his effects with numerous coils which happened to be accessible in University College Laboratory, varying the conditions in several ways; and I experienced the most uniform failure in getting a good "balance" (that is, perfect silence in the telephone) until I abandoned all of them and imitated Prof. Hughes's arrangement and dimensions very closely. A "balance," in fact, is a very difficult thing to obtain with such a detector in the circuit as a telephone; for it is influenced not only by the least want of equality, but also by the least want of synchronism between the two opposing electromotive forces.

Few persons, therefore, can appreciate more highly than I do the unremitting toil which Prof. Hughes must have devoted to his experiments, before contriving the induction-balance and sonometer in their present form.

I am, Gentlemen,
Your obedient servant,
OLIVER J. LODGE.

ON THE CARRYING-POWER OF MAGNETS. BY PROF. J. STEFAN.

In the calculation of the carrying-power of magnets three forces are to be distinguished:—1, the attraction of the magnetic masses on the contact-surfaces of the magnet and armature; 2, the actions at distance which those masses undergo from the free magnetic portions of the magnet and armature; 3, the actions at distance of the free portions of the magnet upon those of the armature.

The calculation can be accomplished if the distribution of the magnetism in the magnet and armature is known. This, however, can only be ascertained in few cases. Two such are treated in the present memoir: in the first the magnet and armature constitute by their combination a ring magnet; in the second, a uniformly magnetized sphere.

In the first case no free magnetic portions occur, the carrying-power is determined by the attraction of the masses present at the contact-surfaces alone. With this case is connected the solution of three problems.

The first problem has respect to the determination of the maximum of carrying-power which can be imparted to a ring magnet. This can be calculated from the greatest value the magnetic moment of the unit volume of iron is capable of taking, and amounts to 12450 grams for each square centimetre of contact-surface.

The second problem refers to the representation of the connexion between the carrying-power and the intensity of the current by which the ring is magnetized. This connexion cannot at present be expressed by a valid formula; but probably the nature of it can be ascertained with tolerable accuracy. The carrying-power rises at first in a much greater proportion than the intensity of the current, until it attains a fourth part of its maximal value; thenceforward the carrying-power rises more slowly than the current-intensity. In the vicinity of the value above given the carrying-power remains during a considerable interval nearly proportional to the intensity of the current.

The third problem concerns the determination of the dependence of the carrying-power on the situation of the plane which divides the ring into magnet and armature. When these latter form unequal segments of the ring, the carrying-power is less, notwithstanding the greater surface of contact; it diminishes in nearly the same proportion as the surface of contact increases when the plane of contact moves away from the axis of the ring.

If the magnet and armature form a spherical magnet, all three sorts of forces come in to be calculated. The influence of the free magnetic masses which are upon the surface of the sphere diminishes the carrying-power. The relative carrying-power (*i. e.* that which falls to the unit of surface of contact) is greatest when magnet and armature are hemispheres; but even in this case it is only half as great as that of a ring magnet. The further the plane dividing the sphere into magnet and armature is from the centre, the less is the relative carrying-power; so that the total capacity of carrying diminishes with the fourth power of the radius of the surface of contact.—*Sitzung der mathematisch-naturwissenschaftlichen Classe der kaiserl. Akad. d. Wissensch. in Wien vom* **15.** *Jänner* 1880, pp. 14, 15.

ON A METHOD OF STUDYING THE REFLEXION OF SOUND-WAVES.

BY O. N. ROOD, PROFESSOR OF PHYSICS IN COLUMBIA COLLEGE.

It has been the custom for several years to introduce in certain forms of the melodeon a revolving fan for the purpose of obtaining rapid alternations in the intensity of the notes. This arrangement is called a "tremolo;" and its action was considered by its inventor and by those interested in it to depend on the currents of air produced by the motion of the fan. An examination of the apparatus soon convinced me that this idea was erroneous, and that the alternations in the loudness of the sound was due to reflexion or non-reflexion from the face of the revolving fan; for I found that the same effects could be produced by the aid of a circular disk consisting of open and closed sectors and revolving in its own plane. A disk of this kind constitutes a useful piece of apparatus for studying the reflexion of sound-waves; and some results obtained with it were communicated by me to the National Academy of Sciences as long ago as October, 1876.

As no account of these experiments has ever been published, a short description of them may not be without interest to those engaged in experimental researches on sound, as with their aid the following facts may be easily demonstrated.

1st. *At a perpendicular incidence the short sound-waves are more copiously reflected than those that are longer; and the regular reflexion is more copious from large than from small surfaces.*

The diameter of the zinc disk was, in the first set of experiments, 21 inches $=53{\cdot}3$ centimetres; alternate quadrants were removed, and the rate of rotation varied from two to four turns in a second. The tuning-forks were mounted on their resonance-boxes, and gradually removed away from the revolving disk till the alternations could no longer be distinguished by the ear placed near the fork. The results are given in the table below, in which "distance" indicates that of the open end of the tuning-fork from the disk:—

	inches.
Diameter of disk	21
Ut$_3$ fork, alternations heard at a distance of	13
Ut$_4$ „ „ „ „	20
Ut$_5$ „ „ „ „	96

When the same experiments were made with a disk having a diameter of only 8½ inches or 21·5 centimetres, it was found necessary to bring the forks much nearer to the disk before the alternations could be perceived.

	inches.
Diameter of disk	8½
Ut$_3$ fork, alternations heard at a distance of	2
Ut$_4$ „ „ „ „	3
Ut$_5$ „ „ „ „	6

2nd. *When the sound-waves fall upon small flat surfaces at an acute angle, the reflexion is most copious in the same direction as with light, but the reflected and inflected waves can be traced all around the semicircle.*

Experiments on this point were made in the open air, the larger disk being used with angles of 60° and 70° (from the perpendicular); the Ut_4 and Ut_5 forks were employed.

The regularly reflected waves could be heard at a distance of ten or twenty feet from the disk, the fork being held a foot or two from it; inflected waves were easily distinguishable all around the disk and even a few feet *behind* the fork.

When the forks were placed in the plane of the disk the alternations of loudness were reduced to a minimum, but, in the open air and in a room, never wholly disappeared. This I suppose to be owing to the fact that the source of sound is not a point but a surface. Even under these circumstances, feeble alternations were heard all around the disk, the inflected waves actually returning to their source. With a plain disk alternations were not perceived.

3rd. *Qualitative comparisons between the power of different substances to reflect sound can easily be made.*

For example, a disk of card-board in which filter-paper is fastened over the open sectors gives alternations, owing to the difference of the reflective powers of the two substances.

4th. *If a composite sound-wave falls on the rotating disk, the shorter waves will undergo regular reflexion more copiously than the other components.*

This experiment is most easily made with a reed without its pipe. Ut_3, Ut_4, Ut_5 reeds give alternations, but mainly in their high overtones; the alternations consequently have a ringing metallic sound.

5th. *The reflexion of sound from very small surfaces is easily demonstrated.*

If an Ut_3 or Ut_4 reed without its pipe be employed, alternations are easily obtained by moving a visiting card properly near the reed. By substituting a gas-flame for the card the reflexion from the flame can be demonstrated. The gas-burner should be attached to a long slender rod.

Almost all of these experiments are so easily performed as to be suitable for lecture-room purposes.—Silliman's *American Journal*, February 1880.

EXPERIMENTS ON THE COMPRESSION OF GASEOUS MIXTURES.

BY L. CAILLETET.

When a mixture of air and carbonic acid is enclosed in the apparatus which served me for the liquefaction of gases, the liquefaction of the carbonic acid undergoes (as already observed by Andrews and several other physicists) a retardation often very great: it is even possible to compress at zero C. to more than 400 atmospheres 1 volume of air and 1 vol. of carbonic acid mixed, without obtaining any alteration of aspect in the tube.

By compressing in the apparatus 5 vols. of carbonic acid and 1 vol. of air, the carbonic acid is readily liquefied. If then the pressure be carried to 150 or 200 atmospheres, the meniscus of the liquefied acid, which till then was concave and of perfect sharpness, becomes plane, loses its sharpness, is progressively effaced; at last the liquid entirely disappears. The tube then appears filled with a homogenous body which henceforth resists all pressure as a liquid would do.

If now the pressure be slowly diminished, at a pressure which is constant for determinate temperatures the liquid suddenly reappears; a thick mist is produced, which expands, vanishes in a moment, and marks the level of the liquid which has just reappeared.

The following numbers indicate the course of the phenomenon:—The mixture operated on being formed of approximately 5 vols. of carbonic acid and 1 vol. of air, the liquid carbonic acid reappears at

atm.		°
132	at the temperature of	+ 5·5
124	” ”	10
120	” ”	13
113	” ”	18
110	” ”	19
The carbonic gas compressed above 350 atm. is no longer liquefied at		21

This phenomenon of the disappearance of the liquid cannot be explained by the heat which the compression evolves; for in this experiment the tube dips in water, which keeps it at a constant temperature, and the compression takes place with sufficient slowness for the cooling to be always complete.

In reality every thing goes on as if at a certain degree of compression the carbonic acid spread into the gas which surmounts it, producing a homogeneous body without sensible alteration of volume. There is therefore nothing to prevent us from assuming that the gas and the liquid have dissolved in one another. I have essayed to verify this hypothesis by colouring the liquefied carbonic acid. Of all the substances tried, iodine alone could be dissolved in the acid; but, unfortunately, in this experiment the mercury is rapidly attacked, and the phenomenon is immediately masked by the iodide of mercury which is deposited on the wall of the tube.

It might, however, be supposed that the disappearance of the liquid is only apparent, that, the index of refraction of the compressed air increasing more quickly than that of the liquid carbonic acid, a moment arrives when, the two indices becoming equal, the surface of separation between the liquid and the gas ceases to be visible. But if the pressure of the system were then augmented by some hundreds of atmospheres, the surface separating the gas from the liquid would become again visible, the index of refraction of the gas continuing, by hypothesis, to increase more rapidly than that of the liquid.

Now the experiment, tried up to 450 atmospheres, gave only negative results. We may, then, suppose that under high pressures a gas and a liquid can be dissolved in one another so as to form a homogeneous whole.—*Comptes Rendus de l'Académie des Sciences,* Feb. 2, 1880, t. xc. pp. 210, 211.

THE

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PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[FIFTH SERIES.]

APRIL 1880.

XXXII. *On the Determination of Chemical Affinity in terms of Electromotive Force.*—Part I. *By* C. R. ALDER WRIGHT, *D.Sc. London, Lecturer on Chemistry and Physics in St. Mary's Hospital Medical School*[*].

1. BETWEEN the years 1841 and 1846 Joule made a series of experiments on the development of heat during electrolysis, which led him to the conclusions:—that when a current is passed through a fluid conductor, heat is developed in accordance with the same law as that which he found to obtain with a solid metallic wire, viz. that the heat produced in a given time is proportionate to the square of the current and to the resistance of the conductor—usually known as Joule's law; that if electrolysis is produced, the actual heat developed in the electrolyte is less than that deducible from this law by a quantity precisely equal to the amount that would be given out in re-forming from its products of decomposition the compound decomposed; and consequently that it is possible to determine the "heat of formation" of a compound by the inverse method of determining the heat absorbed during its electrolysis. The actual results thus obtained, as regards the "heat of formation" of copper sulphate, zinc sulphate, and water, were communicated to the French Academy in 1846, but were not published in detail until 1852 (Phil. Mag. [4] vol. iii. p. 481). Some few months before they appeared, Dr. Thomas Woods published (ibid. vol. ii. p. 268) the results of experiments showing that in the decomposition of water by electrolysis a quantity of heat is taken up approximately equal

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[*] Communicated by the Physical Society.

to that evolved by the recombination of the hydrogen and oxygen evolved.

2. Joule's method of operating is described by him in the following words:—"I take a glass vessel filled with the solution of an electrolyte and properly furnished with electrodes; I place the electrolytic cell in the voltaic circuit for a given length of time, and carefully observe the quantity of decomposition and the heat evolved. By the law of Ohm I then ascertain the resistance of a wire capable of obstructing the current equally with the electrolytic cell. Then by the law we have proved [*i. e.* 'Joule's law'] I determine the quantity of heat which would have been evolved had a wire of such resistance been placed in the current instead of the electrolytic cell: this theoretical quantity, being compared with the heat actually evolved in the electrolytic cell, is always found to exceed the latter considerably. The difference between the results evidently gives the quantity of heat absorbed during the electrolysis, and is therefore equivalent to the heat which is due to the reverse chemical combination by combustion or other means" (*loc. cit.* pp. 493, 494).

Put into symbols this may be thus expressed:—Let h_1 be the heat actually developed in the electrolyte by a current C_1 in a given time t during which w_1 grammes of electrolyte are decomposed. Let h_2 be the heat that would be developed in a wire of the same resistance (*i. e.* capable of equally "obstructing the current") in the same time t by the same current C_1. Let h_3 be the heat actually developed in another wire by a different current C_2 in the same time t; and let the resistance of the electrolytic cell be a times the resistance of this second wire. Then, by Joule's law,

$$\frac{h_2}{h_3}=\frac{C_1^2}{C_2^2}a,$$

whence the heat absorbed in decomposing w grammes of electrolyte is

$$h_2-h_1 \text{ or } =h_3\frac{C_1^2}{C_2^2}a-h_1,$$

and the heat absorbed in decomposing 1 gramme of electrolyte is

$$\frac{h_3\frac{C_1^2}{C_2^2}a-h_1}{w}.$$

In this way Joule obtained in three sets of observations the following mean values for the heat absorbed in the decomposition of a gramme-equivalent of water:—

	34101
	34212
	32356
Average . .	33557

the numbers found by direct determination of the heat of combustion of hydrogen in oxygen being close to 34100 gramme-degrees, according to the most trustworthy experiments (*vide* § 31).

3. Joule further applied this principle to the electrolysis of zinc and copper sulphates, and found that the heat actually developed in the electrolytic cell was in each case less than that which would be developed in a wire "obstructing the current" equally with the cell by a quantity sensibly equal to the heat that would be generated by the oxidation of the metal liberated during the electrolysis by the oxygen simultaneously set free, together with that produced by the solution in the dilute sulphuric acid also formed of the metallic oxide thus produced. That this should be so is an evident deduction from the principle of conservation of energy; for if the total work that could be done in the passage of a current C through a resistance R in a time t be expressed by the formula C^2Rt (Joule's law), and some of that work be performed not as heat but in undoing chemical combination, it results that the amount of work actually done as sensible heat must be just so much less than C^2Rt as corresponds to the chemical work performed.

4. The experiments of Woods were of a far less accurate kind than those of Joule, but were based on much the same principles. A current being passed through an electrolytic cell for a given time, the heat developed was roughly measured and the amount of decomposition; a wire was then substituted for this electrolytic cell of such resistance that the current passing was the same as before (the electromotor remaining the same). The heat developed in this wire being determined, was found to exceed that produced in the electrolytic cell in the same time by a quantity sensibly equal to that which would be generated by the recombination of the products of electrolysis.

The formula just deduced for Joule's method of experimenting then becomes

$$\frac{h_3-h_1}{w};$$

for $C_1=C_2$ and $a=1$.

5. In 1865 Joule's results were confirmed by Raoult (*Ann.*

de Chim. et de Phys. 4th series, ii. p. 317, and iv. p. 392) by a method which will be discussed later on (§ 23). The amount of heat absorbed per gramme-equivalent of water decomposed was found by Raoult to be 34028, or sensibly the same as that deduced by direct combustion; whilst similar results were obtained with copper sulphate. In 1869 Kiechl published experiments (*Wien. Akad. Ber.* lx. [2] 121) in principle the same as Joule's, but differing considerably in the details, notably in that the wire was made into a rheostat, so that its resistance could be regulated throughout the observations. As a mean of nine accordant experiments, 33653 was found to be the heat absorbed per gramme-equivalent of water decomposed. Shortly before this, Favre demonstrated in a very simple way (*Comptes Rendus*, lxvi. p. 252; Pogg. *Ann.* cxxxv. p. 300) that as much heat is absorbed during the decomposition of an electrolyte as is produced by the recombination of the products of decomposition. A small battery was enclosed in a calorimeter, and the heat produced by the solution of a given quantity of zinc determined, the current produced being made to circulate through a coil of wire also contained in the calorimeter; so that the total heat generated was determined. A voltameter was then substituted for the coil of wire, when the quantity of heat was found to be lessened by an amount equal to 34,204 gramme-degrees per gramme-equivalent of zinc dissolved, *i. e.* of water decomposed. Similarly when copper sulphate was decomposed instead of water, the heat-evolution was lessened by an amount sensibly equal to the heat evolved in the synthesis of the salt.

6. In 1851 Sir William Thomson carried the ideas suggested by the experiments of Joule and others a considerable way further. In a remarkable paper on the mechanical theory of electrolysis (Phil. Mag. 1851, vol. ii. p. 429), he demonstrated that the force of chemical affinity is expressible in terms of electromotive force, and calculated, from data largely furnished by Joule, that the work done in decomposing water corresponds to an E.M.F. of 3,305,400 foot-grain-second electromagnetic units (equal to $1{\cdot}415 \times 10^8$ C.G.S. units, or 1·415 volt), that representing the sum of the chemical actions in a Daniell cell corresponding to 2,507,100 (equal to $1{\cdot}074 \times 10^8$ C.G.S. units); so that the "affinity" between oxygen and hydrogen in water is $\frac{1{\cdot}415}{1{\cdot}074} = 1{\cdot}318$ times as strong as the sum of the affinities exerted in the chemical changes going on in a Daniell cell. Similar calculations, based on more recent determinations of the values involved, lead to somewhat higher numbers, approximating to $1{\cdot}502 \times 10^8$ C.G.S. units for water

and $1{\cdot}154 \times 10^8$ for the Daniell cell (*vide* § 16), giving the ratio $\frac{1{\cdot}502}{1{\cdot}154} = 1{\cdot}302$.

7. The general proposition that the force of chemical affinity is expressible in terms of E.M.F. may be simply demonstrated as follows:—Let the strength of chemical combination, or the chemical work done by the force of chemical affinity, be defined as the absolute amount of work spent or gained in the synthesis from its constituents of a given weight (say 1 gramme) of the compound considered. This work may be expressed in terms of heat, and is ordinarily measured as such: thus in the synthesis of a gramme of water from gaseous oxygen and hydrogen at the ordinary pressure and temperature, an amount of work is done representing an evolution of a quantity of heat amounting, according to the most accurate determinations, to close upon 3800 gramme-degrees (§ 31); whence, taking the mechanical equivalent of heat as 42 megalergs per gramme-degree*, the work done in this synthesis is close to 16 erg-tens $= 16 \times 10^{10}$ C.G.S. units.

The experiments of Faraday first demonstrated the two following laws, which have since been fully confirmed by many other experimenters:—

I. When a compound is electrolyzed, the weight of substance decomposed is proportionate to the quantity of electricity passing. [A low degree of conductivity is supposed by some experimenters to be possessed by electrolytes, of such a nature that very feeble currents can be passed through them without causing any chemical decomposition; this, however, even if admitted to be the case, would not appreciably interfere with the correctness of the foregoing proposition.]

II. When a current is passed for a given time successively through several compounds so as to electrolyze them all, it decomposes quantities of them in the ratio of simple functions of their "molecular weights," evolving the products of decomposition, when elementary, in the ratio of simple functions of their combining numbers or "atomic weights." The quantities of compounds decomposed and of elements evolved by a current which evolves 1 part of hydrogen from water under these circumstances are called their "equivalent values," or, more briefly, their *equivalents*, in reference to the particular compounds decomposed.

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* The values obtained by Joule by friction of water lie somewhat below this figure (near to $41{\cdot}5 \times 10^6$); whilst that deduced by him from the development of heat by electric currents in a wire of known resistance lies somewhat above it ($42{\cdot}10 \times 10^6$). Numbers lying somewhat above 42×10^6 have been obtained by Hirn, Violle, and Regnault (*vide* §§ 33 & 34).

Law I. may be thus expressed:—Let n be the number of grammes of compound decomposed, and q the quantity of electricity passing; then

$$n \propto q.$$

Law II. states that, if a be the "equivalent" of a given compound,

$$n \propto a.$$

Hence

$$n \propto aq;$$

and consequently

$$n = aq \times \chi,$$

where χ is a multiplier independent of the quantity of electricity passing, and of the value of the "equivalent" of the compound.

When $n=a$, $\chi=\frac{1}{q}$; that is, χ is the reciprocal of the quantity of electricity requisite to decompose a gramme-equivalent of the electrolyte employed. For convenience of reference, χ may be termed the *electrochemical constant.*

8. The term "electrochemical equivalent of a substance" is employed to mean the quantity of substance decomposed by a unit quantity of electricity, *i. e.* the value of $\frac{n}{q}$ (which $=a\chi$); and the term "electrochemical equivalent of an element," in reference to such a compound, is used to indicate the quantity of that element evolved during electrolysis of that compound during the passage of a unit of electricity. If b be the equivalent of the element in reference to the compound employed, the equivalent of the compound being a, the electrochemical equivalent of the element in reference to the compound is

$$\frac{n}{q} \cdot \frac{b}{a},$$

which $=b\chi$.

n is of necessity a constant for any given compound: and so is b in many cases; that is, in such cases, no matter what compound of a particular element be electrolyzed, the same quantity of that element is set free by the passage of a unit of electricity. In other cases, however, b varies with the character of the compound employed; that is, the same quantity of element is not necessarily always evolved by the passage of a unit of electricity. For example, the value of b for hydrogen, chlorine, and silver appears to be constantly 1, 35·5, and 108 respectively; whilst for copper the value is 31·75 for compounds of the class known as cupric salts, and 63·5 for compounds of the class known as cuprous salts. When b is thus variable, its different values are always simple integral multiples of a common divisor.

9. Sir William Thomson calculated in 1851 (*loc. cit. supra*) the value of $a\chi$ for water on the foot-grain-second electromagnetic system to be as follows:—

From data furnished by experiments of	Weber . .	0·02034
" " "	Bunsen . .	0·02011
" " "	" . .	0·01995
" " "	Casselmann	0·02033
" " "	"	0·02021
" " "	Joule . .	0·01981
" " "	" . .	0·02030
" " "	" . .	0·02002
	Average . . .	0·02013

On the C.G.S. system (electromagnetic measure) this average value becomes 0·0009282. Stas found that 1 part of hydrogen combines with 7·98 of oxygen to form water, whence $a=8{\cdot}98$ for water, and consequently

$$\chi=\frac{0{\cdot}0009282}{8{\cdot}98}=0{\cdot}00010336.$$

More recent experiments of Kohlrausch (Pogg. *Ann.* cxlix. p. 170) indicate a perceptibly higher value, viz. 0·00010527; for he found that one metre-gramme-second unit of quantity of electricity causes the deposition of 0·11363 gramme of silver, or 1 C.G.S. unit of electricity causes the deposition of 0·011363 gramme of metallic silver; *i. e.* $b\chi=0{\cdot}011363$, whence

$$\chi=\frac{0{\cdot}011363}{107{\cdot}93}=0{\cdot}00010527,$$

since Stas found the equivalent of silver to be 107·93.

In the same paper Kohlrausch gives the following values for the electrochemical equivalent of water deduced from the experiments of Weber, Bunsen, Casselmann, and Joule, allowance being made for probable errors in the values assigned to the horizontal component of terrestrial magnetism (the numbers are given by him in the metre-gramme-second system):—

Weber	0·009396
Casselmann	0·009391
Bunsen	0·009624
Joule	0·009222
Mean . . .	0·009406

On the C.G.S. system this value becomes 0·0009406, whence

$$\chi=\frac{0{\cdot}0009406}{8{\cdot}98}=0{\cdot}00010474.$$

Hence the mean of this value and that deduced by Kohlrausch from his own observations (viz. $\chi=0{\cdot}00010527$) may be taken as probably the nearest approximation to the truth; *i. e.* $\chi=0{\cdot}000105005$, or practically $\chi=0{\cdot}000105$.

10. If it were possible to obtain a current between the two electrodes of a voltameter or other similar electrolytic cell which would decompose the electrolyte without any disturbance of the thermal equilibrium, then the difference of potential between the electrodes would represent the force of chemical affinity exerted between the products of decomposition in the compound examined; for if e be the potential-difference (E.M.F. existing between the electrodes) during the passage of a quantity of electricity q, eq units of work will be done. If n grammes of substance are decomposed, and f is the work done by the force of chemical affinity in the synthesis of a gramme of substance from the products of decomposition, the total work done is also expressed by nf, which, by the definition of electrochemical equivalents (§ 8), $=a\chi qf$. Hence

$$af=\frac{e}{\chi}.$$

Or "affinity," reckoned per gramme-equivalent, is measured by electromotive force divided by a quantity which is constant; *i. e.* the work done in the synthesis of a gramme-equivalent of substance is expressible by a value indicated in electromotive-force units, just as it is expressible by a value indicated in heat units—the one valuation involving the constant χ, the other J.

11. In practice it is impossible to realize this supposition; for experiment shows that, in the passage of electricity between two electrodes, work is done not only as chemical decomposition but also as heat; by determining the amount of the latter, however, it is easy to calculate how much of the total potential-difference existent between the electrodes is due to the chemical affinity of the constituents: thus, if h gramme-degrees are evolved during the decomposition of n grammes of substance of electrochemical equivalent $a\chi$ by the transfer of a quantity of electricity q, and if E represent the total potential-difference existent between the electrodes, the total work done in the electrolytic cell is $\mathrm{E}q$, of which $h\mathrm{J}$ is done as heat. Hence $\mathrm{E}q-h\mathrm{J}$ is done as chemical decomposition. Let this be equal to eq, where e is the E.M.F. representing the chemical affinity to be measured; then

$$e=\mathrm{E}-\frac{h\mathrm{J}}{q}=\mathrm{E}-h\mathrm{J}\,\frac{a\chi}{n},$$

since, by definition of electrochemical equivalents (§ 8),

$$q=\frac{n}{a\chi}.$$

Finally, therefore,

$$e=\mathrm{E}-\mathrm{H}\chi\mathrm{J},$$

where

$$\mathrm{H}=\frac{ha}{n},$$

i. e. where H is the heat evolved per gramme-equivalent of substance decomposed.

Experiments made in this way (detailed in Part II., *vide* also § 32) have given numbers for the value of e in the case of water closely corresponding with the value of e deducible from direct combustion-experiments by the formula arrived at above (§ 10), viz.

$$af=\frac{e}{\chi},$$

which becomes

$$e=af\chi=ah'\mathrm{J}\chi=\mathrm{H}'\chi\mathrm{J},$$

where h' is the heat liberated per gramme, and H′ that per gramme-equivalent of water formed by combustion of hydrogen and oxygen.

12. On attentively considering the phenomena attendant on electrolysis, it is evident that it by no means follows that the direct action of the current causes the entire change taking place; physical changes of state and chemical actions secondary to those due to the action of this current alone may occur; so that the value of e thus deduced, although representing the E.M.F. corresponding to the *nett work done* in the sum of the total changes taking place, and consequently representing the "affinity" between the ultimate products in the original compound as above defined, *is not necessarily a measure of the tendency to combine together of the substances primarily formed by the electrolytic decomposition.* For instance, suppose that in the electrolysis of water the primary products are ordinary gaseous hydrogen and ozone, the latter becoming subsequently transformed (by a secondary chemical action) into ordinary gaseous oxygen: then, since heat is evolved in the conversion of ozone into oxygen to the extent of 14,800 gramme-degrees per 24 grammes of oxygen (Berthelot, *Bull. Soc. Chim. Paris*, 1876, vol. xxvi. p. 56), or 4933 gramme-degrees per 8 grammes of oxygen (corresponding to 9 grammes of water), the value of e deduced as just indicated will be below the true value by

an amount equal to $\frac{4933}{34100} = 14{\cdot}5$ per cent.; for the negative term in the expression $E - H\chi J$ will have been overestimated by that quantity.

Again, suppose that the water is brought by a secondary physical action into a quasi-vaporous state before it is decomposed by the current into gaseous hydrogen and oxygen (*e. g.* on the molecular theory of the constitution of matter, suppose that only detached molecules of water are operated on by the current, and not the water *en masse* in the liquid state); since about 600 gramme-degrees are required to volatilize a gramme of water at the ordinary temperature (the latent heat of steam at near 15° being about 600), or 5400 per gramme-equivalent, the value of e deduced as above will be too large by about $\frac{5400}{34100} = 15{\cdot}8$ per cent, because in this case the negative term $-H\chi J$ will be underestimated to that extent.

Consequently, if both secondary actions be supposed to take place (*i. e.* if the electrolysis proper takes place between detached molecules of water converting them into gaseous hydrogen and ozone), the value of e will be $15{\cdot}8 - 14{\cdot}5$, or $1{\cdot}3$ per cent. too large.

Suppose, again, that the primary electrolytic action is to convert liquid water into *liquid* oxygen and hydrogen, and that the conversion of these liquefied elements into the ordinary gaseous condition is due to secondary physical changes; it is evident that the value of e found as above is too large by $\frac{h_1}{34100} \times 100$ per cent., where h_1 represents the heat that is absorbed in the vaporization of a gramme-equivalent of each of the liquefied gases taken together.

Yet again, in the electrolysis of copper sulphate, suppose that whilst the ultimate chemical change is expressed by the equation

$$CuSO_4 + H_2O = Cu + O + H_2SO_4,$$

this change is actually the sum of two changes, of which the first is secondary:—

$$\begin{aligned} &(1)\quad CuSO_4 + H_2O = CuO + H_2SO_4,\\ &(2)\quad CuO \qquad\qquad\;\; = Cu + O; \end{aligned}$$

it is evident that the value of e deduced as above will be greater than that corresponding to the second change only, by an amount equivalent to the heat absorbed during the first change.

13. During the last three or four years the writer has made various experiments with the view of determining the E.M.F. measuring the "affinity" between various constituents in dif-

ferent compounds (especially oxygen and hydrogen in water), not only when the substances developed by the electrolysis are in their ordinary conditions, but also when they are "nascent" —*i. e.* with the view of determining, first, the E.M.F. corresponding to the "heat of formation" of (for instance) water from gaseous oxygen and hydrogen, and, secondly, that requisite to produce electrolysis proper *apart from secondary physical and chemical changes.* It is evident that, if such determinations are practicable, the comparison of the two sets of values might lead to interesting conclusions as to the nature of these secondary actions, and might throw light on the internal mechanism of the chemical changes taking place, and especially on the phenomena connected with the "nascent state." Results in this direction have already been obtained by Favre, with this difference, that the values are expressed in gramme-degrees, being obtained entirely by calorimetric observations. The discussion of the observations of this kind already made by previous experimenters, and of the writer's results (obtained chiefly by electrical measurements), will be resumed in a future paper, the present essay having chiefly reference to the determination of the E.M.F. representing the sum of the physical and chemical actions taking place during electrolysis. It should, however, be noticed in connexion with this subject, that since Sir William Thomson first sketched out the Mechanical Theory of Electrolysis (*loc. cit. supra*), more exact measurements have shown that the E.M.F. actually capable of being generated by a given voltaic combination is by no means necessarily equal to the sum of the physical and chemical changes taking place in the pile, and that it is not necessarily constant for the same ultimate chemical change—varying with the temperature, the state of dilution of the solutions used, &c.

14. For example, when zinc is dissolved in dilute sulphuric acid, the total ultimate change is expressed by the equation

$$H_2SO_4 + Zn = H_2 + ZnSO_4,$$

which corresponds to an evolution of somewhat more than 18,000 gramme-degrees per gramme-equivalent of zinc dissolved or of hydrogen liberated, and representing therefore about $18000 \times 0{\cdot}000105 \times 42 \times 10^6 = 0{\cdot}794 \times 10^8$ C.G.S. units of E.M.F., or about 0·794 volt. Similarly, when zinc or any analogous metal is dissolved in any other acid (*e. g.* hydrochloric) with evolution of hydrogen by a parallel reaction, a heat-evolution of h gramme-degrees results per gramme-equivalent, corresponding to $h\chi J$ C.G.S. units of E.M.F. But when this change takes place in an electromotor with a second metal as the other plate, the E.M.F. actually set up by the tendency to produce this ultimate change varies with the nature of the second metal; and whilst the E.M.F. produced is rarely, if ever, precisely equal to $h\chi J$ even at first, when a current has been made to circulate for some short time the E.M.F. is found to have diminished to an amount depending on the rate of evolution of hydrogen on the surface of the other plate, the character of its surface, the temperature, &c. This is usually expressed by saying that the plates become "polarized," or that a "counter electromotive force" x is set up, diminishing the effective E.M.F. of the arrangement from E, which it was at first, to $E-x$, the value of x varying with circumstances.

From the principle of the conservation of energy it results that, when the current circulates, the difference between the work $h\chi J \times Q$ which would be performed during the passage of a quantity of electricity Q sufficient to decompose a gramme-equivalent of substance, and $(E-x)Q$ the work which the current can actually perform, must make its appearance as sensible heat. If, therefore, $E-x<h\chi J$, the electromotor must rise in temperature from this cause—irrespective of the rise due to the fact that, as the current traverses the electromotive equally with the rest of the circuit, heat is developed therein in proportion to its resistance in accordance with Joule's law (§ 1). If, however, $E-x>h\chi J$, the battery is of necessity proportionately cooled; and if the experiment is so carried out that the resistance of the battery is small compared with that of the rest of the circuit, so that the heat developed in the battery in accordance with Joule's law is but small, the cooling effect may be made to preponderate over the heating effect, so that an absorption of heat on the whole takes place in the battery.

15. This effect has been experimentally studied by Favre (*Comptes Rendus*, lxvi. p. 252, lxvii. p. 1012, lxviii. p. 1300, and lxix. p. 34), who has shown that under the last-named conditions an absorption of heat takes place during the solution of zinc or cadmium in hydrochloric acid, whilst heat-evolution takes place if sulphuric acid be substituted for hydrochloric. In other words, the secondary actions set up, as a whole, absorb heat in the first case and evolve it in the second. Similarly in two-fluid batteries, such as Daniell's and Grove's, analogous results were obtained, although in these cases "polarization" by evolution of hydrogen could not possibly be produced. With cells after Daniell's construction heat was found to be evolved by the secondary actions, though not to a very large extent (about 4·3 per cent. of the heat due to the

consumption of zinc and precipitation of copper); whilst with a Grove's battery a considerably larger heat-absorption took place, amounting to about 11·9 per cent., of the heat due to solution of zinc and reduction of nitric acid.

16. This result with a Daniell cell is borne out by experiments of a different kind. The E.M.F. which a Daniell cell is practically capable of giving has been variously estimated by different observers, at from 1·05 to 1·14 volt, 1·10 to 1·12 being a usual average. The E.M.F. corresponding to the heat evolved in the reaction

$$CuSO_4 + Zn = Cu + ZnSO_4$$

(which expresses the ultimate change in a Daniell cell, apart from local action) is several per cent. higher. Thus Favre estimated the heat thus evolved per gramme-equivalent to be 27,388 (*Comptes Rendus*, lxiii. p. 369), and subsequently as 25,060 (*ibid.* lxix. p. 35): the mean of these two numbers is 26,224, corresponding to an E.M.F. of $26{,}224 \times 0{\cdot}000105 \times 42 \times 10^6 = 1{\cdot}156 \times 10^8$ C.G.S. units, or 1·156 volt. Julius Thomsen has calculated (*Berichte deut. chem. Ges.* 1872, vol. v. p. 170) from his own determinations, together with those of Andrews, Dulong, Hess, and Favre and Silbermann, that the heat-evolutions taking place during the formation of zinc sulphate and copper sulphate are respectively 108,460 and 56,216 per "gramme-molecule" (these formations being expressed in Thomsen's symbols thus—Zn, O, SO_3, aq, and Cu, O, SO_3, aq). Hence the heats are 54,230 and 28,108 per gramme-equivalent; whence the displacement of copper from copper sulphate by metallic zinc, forming copper and zinc sulphate, must give rise to an evolution of $54{,}230 - 28{,}108 = 26{,}122$ gramme-degrees per gramme-equivalent, corresponding to $26{,}122 \times \chi J = 1{\cdot}152$ volt.

17. Results analogous to those of Favre have also been obtained by Raoult (*Ann. de Chim. et de Phys.* 4th series, ii. p. 317, and iv. p. 392)—some forms of voltaic combinations giving rise to the production of more heat outside the battery than corresponds to the heat evolved by the ultimate chemical and physical changes taking place therein, and others to less. Raoult found that the current from the Daniell cell used by him was capable of doing total work equal to 23,900 gramme-degrees per gramme-equivalent of zinc dissolved in each cell: this represents only $23{,}900 \times \chi J = 1{\cdot}054$ volt, a quantity less by 8·5 per cent. than the value calculated above from Thomsen's figures as the E.M.F. corresponding to the sum of the chemical and physical actions taking place in the cell.

To express the difference between the heat actually producible by the sum of the chemical and physical actions taking place in an electromotor and that corresponding to the current actually obtainable from the electromotor, Raoult calls the first the *chemical heat* of the pile, and the latter the *voltaic heat* thereof; whilst the terms *chemical heat* and *galvanic heat* have been employed by Edlund to indicate these quantities respectively (Phil. Mag. 1877, vol. iii. pp. 428 & 501); so that the above results may be indicated by saying that the chemical heat of a Daniell battery is 26,122 gramme-degrees (per gramme-equivalent of zinc dissolved), whilst the voltaic or galvanic heat is 23,900, and so on. Several points touched upon in these papers are of interest as bearing on the problem of the determination of the amount of E.M.F. corresponding to the primary effects of electrolysis; their discussion, like that of several analogous memoirs by previous observers, is postponed till a later occasion.

18. In order to apply the general method above indicated (§ 11) for the determination of the E.M.F. corresponding to the sum of the chemical and physical actions taking place during electrolysis, it is requisite to determine:—first, the difference of potential subsisting between the electrodes; secondly, the quantity of electricity passing in a given time (known from the weight of substance decomposed); and, thirdly, the heat evolved as such in the electrolytic cell in that time. The first quantity may be found approximately by the aid of a sine galvanometer of very high resistance. A method which is in principle substantially this has already been used by Raoult (*loc. cit.*); this will be more fully discussed further on (§ 23).

Another way of determining it may be based upon observations with a voltameter, thus:—The amount of decomposition produced in a given time t by passing a current from the battery employed (of E.M.F. = E') through the voltameter is noticed $=n_1$; the electrolytic cell is then placed also in circuit, and the amount of decomposition in the voltameter in the same time t again determined $=n_2$, and also the heat produced in the cell $=h$. R being the resistance of the battery, voltameter, and connexions, and r being the resistance of a wire "obstructing the current equally with the electrolytic cell," the currents flowing in the two cases are respectively

$$C_1=n_1\times k=\frac{E'}{R} \text{ and } C_2=n_2\times k=\frac{E'}{R+r},$$

where k is a constant—whence

$$\frac{R}{R+r}=\frac{n_2}{n_1}, \text{ and } \frac{r}{R+r}=\frac{n_1-n_2}{n_1}.$$

The difference of potential set up between two points connected by a medium of resistance r traversed by a current C_2 is

$$rC_2 = \frac{r}{R+r}E' = \frac{n_1-n_2}{n_1}E';$$

and hence the expression for the E.M.F. equivalent to the affinity to be found, $E-H\chi J$, becomes

$$\frac{n_1-n_2}{n_1}E' - \frac{ha\chi J}{n_2},$$

where a is the equivalent of water $=8\cdot98$.

19. Experiments on the electrolysis of water have been made by the writer in accordance with this method, and have furnished moderately concordant results approximating to the value of the E.M.F. representing the affinity to be measured, calculated from the heat of combustion of hydrogen (§ 32); but as the experimental errors attendant on the method are considerably greater than those involved when the value of $\frac{r}{R+r}E'$ is directly determined by means of a quadrant electrometer, this latter mode of operating has been adopted in preference: the details of observations thus made in the case of water are given in Part II. Meanwhile it is of interest to notice that if a tangent galvanometer be substituted for a voltameter (the quantity of electricity passing being determined by observing the quantity of electrolyte w decomposed whilst a quantity of heat h is evolved), the value of the affinity to be determined is given by the analogous formula

$$e = \frac{\tan\alpha - \tan\beta}{\tan\alpha}E' - \frac{ha\chi J}{w},$$

where α and β are the deflections observed with the currents C_1 and C_2 respectively. The experiments of Joule above referred to (Phil. Mag. vol. iii. p. 481), although made for the purpose of calculating the heat of formation of water, copper sulphate, and zinc sulphate in a wholly different way, contain the data requisite for the application of this mode of observation, based on the use of the tangent galvanometer; it being taken for granted that E', the value of the E.M.F. of the Daniell battery used, was $m\times1\cdot10$ volt, where m is the number of cells used in the experiment recalculated. Thus, Joule found the following numbers in a series of four observations on the electrolysis of water in which six Daniell cells were used:—

	Mean tan α.	Mean tan β.	Gramme of H evolved.	Rise of thermometer.
	3·4170	1·6234	0·03978	40·6 scale divisions.
	3·9429	1·7780	0·04212	37·8 " "
	4·3412	1·8374	0·04372	41·7 " "
	4·0508	1·8011	0·04411	38·8 " "
Average	3·9380	1·7600	0·04243	39·725
		Correction for radiation...		+ 0·656
				40·381

The water-equivalent of the calorimeter was found to be 1155 grammes, whilst 23·38 scale-divisions of the thermometer represented 1°; hence

$$h=1994{\cdot}9.$$

The E.M.F. of affinity to be found, then, is

$$\frac{3{\cdot}9380-1{\cdot}7600}{3{\cdot}9380}\times 6\times 1{\cdot}10\times 10^{8}-\frac{1994{\cdot}9\times 1\times 0{\cdot}000105\times 42\times 10^{6}}{0{\cdot}04243},$$

or

$$1{\cdot}577\times 10^{8}\text{ C.G.S. units }=1{\cdot}577\text{ volt.}$$

The value deduced from Joule's own figures by his mode of experimenting is 34,101 gramme-degrees per gramme-equivalent of water decomposed, representing $34101\times\chi J=1{\cdot}504$ volt; whilst the value deducible from the best determinations of the heat evolved in the combustion of hydrogen is 34,100, representing 1·504 volt (§ 32). In two other similar sets of observations, Joule obtained the values 34,212 gramme-degrees corresponding to 1·509 volt, and 32,356 gramme-degrees corresponding to 1·427 volt. Unfortunately the data for recalculating these values, as has just been done with the first set, are not given in his paper; still the difference between the values of the E.M.F. of affinity, calculated as above and from Joule's heat-values, is apparently no greater than the differences observed in these latter on repetition. It is noticeable that a small error in the value of the E.M.F. of the battery produces a much larger error in that of the calculated result: thus, if in the set of observations recalculated as above the E.M.F. of a Daniell cell be taken as 1·08 instead of 1·10 volt, the calculated E.M.F. of affinity comes out 1·511 instead of 1·577 volt; *i. e.* a diminution of less than 2 per cent. in the value of the Daniell cell makes a diminution of more than 4 per cent. in the E.M.F. of affinity.

20. In just the same way Joule's experiments on copper

sulphate and zinc sulphate may be applied by means of the above formula. Thus with copper sulphate the following numbers were obtained, a battery of 4 Daniell cells being used:—

	Mean tan α.	Mean tan β.	Gramme of copper deposited.	Rise of thermometer.
	3·4006	0·7275	0·5686	20·4
	3·7510	0·7535	0·5777	19·4
	3·8538	0·7650	0·5881	19·45
	3·8084	0·7968	0·6153	17·4
Average	3·7035	0·7607	0·5874	19·162
		Correction for radiation ...		−0·049
				19·113

In these experiments the water-equivalent of the calorimeter was 1179·2, whence

$$h=963·99,$$

and the E.M.F. representing the total chemical and physical changes is

$$\frac{3·7035-0·7607}{3·7035}\times 4\times 1·10\times 10^8-\frac{963·99\times 31·75\times \chi J}{0·5874},$$

or

$$1·198\times 10^8+10^8 \text{ C.G.S. units or } 1·198 \text{ volt.}$$

With zinc sulphate the following numbers were obtained, a battery of 7 Daniell cells being used:—

	Mean tan α.	Mean tan β.	Gramme of zinc deposited.	Rise of thermometer.
	3·5500	0·7167	0·5797	32·2
	4·0133	0·7092	0·5647	30·0
	3·8356	0·7573	0·6010	31·3
	3·9025	0·7548	0·5991	30·5
Average	3·8254	0·7345	0·5861	31·0
		Correction for radiation ...		− 0·016
				30·984

In these experiments the water-equivalent of the calorimeter was 1180·9, whence

$$h=1565,$$

and the E.M.F. to be found is

$$\frac{3·8254-0·7345}{3·8254}\times 7\times 1·10\times 10^8-\frac{1565\times 32·5\times \chi J}{0·5861},$$

or

$$2·395\times 10^8 \text{ C.G.S. units, }=2·395 \text{ volts.}$$

21. These numbers 1·198 and 2·395 volts lie close to those calculated from Joule's figures by his own method of experimenting, and also to those calculated from the heat evolved in the synthesis of copper and zinc sulphates from the metal, oxygen, and dilute sulphuric acid; these latter quantities were found by Thomsen (*Deut. chem. Ges. Berichte*, 1872, vol. v. p. 170) to be respectively 56,216 and 108,460 per "gramme-molecule," or 28,108 and 54,230 per gramme-equivalent. A somewhat smaller number than the former of these (viz. 26,568) was found by Favre for the "heat of formation" of copper sulphate from copper, oxygen, and dilute sulphuric acid by the method referred to in § 5. These various results may be thus contrasted:—

Copper Sulphate.

Observers furnishing the principal data for calculation.	Nature of method of calculation.	Heat evolved.		E.M.F. of affinity in volts (1 volt $=10^8$ C.G.S. units).
		Per gramme of metal deposited.	Per grm.-equivalent of metal deposited.	
Joule	Joule's own method, described in §§ 2 & 3.	836·4	26556	1·171
"	"	856	27178	1·199
"	"	796·5	25289	1·115
		829·6	26341	1·162
Joule	From formula $e=E-H\chi J$ (§ 11).	......	......	1·198
Julius Thomsen	From formula $e=H'\chi J$ (§§ 10 & 11).	......	28108	1·240
Favre	"	......	26568	1·172
Zinc Sulphate.				
Joule	Joule's own method (§§ 2 & 3).	1523·1	49501	2·183
"	"	1547	50277	2·217
"	"	1619	52617	2·320
		1563·0	50798	2·240
Joule	From formula $e=E-H\chi J$ (§ 11).	......	......	2·395
Julius Thomsen	From formula $e=H'\chi J$ (§§ 10 & 11).	......	54230	2·392

22. In reference to Julius Thomsen's values, it is noticeable that some slight uncertainty in the precise values of the heat-developments in the syntheses Cu, O, SO_3 aq., and Zn, O, SO_3 aq.

is introduced by the circumstance that these values are calculated from determinations of the heat evolved by the direct combustion of metal to oxide, and that produced by solution of the metallic oxide in dilute sulphuric acid—these determinations being made by different observers necessarily working with different specimens of metallic oxides, and their results being averaged. Ditte has shown, however (*Comptes Rendus*, lxxii. pp. 762 & 878, and lxxiii. p. 108), that the heat developed by dissolving a metallic oxide in an acid varies with the degree of heat to which the oxide has previously been exposed. Thus, with zinc oxide and dilute sulphuric acid, the following values were obtained per gramme-equivalent:—

	Gramme-degrees.
Zinc oxide previously heated to 350°	9,890
„ „ „ dark red heat .	11,016
„ „ „ bright „ .	12,138

The value of this "heat of solution" was given by Favre as 10,455, and by Julius Thomsen as 11,705, the temperature to which the zinc oxide had been previously heated not being stated.

23. The experiments of Raoult above adverted to (§ 18) were conducted as follows:—A current from a Daniell battery was passed through a galvanometer and a voltameter placed in a calorimeter, a derived circuit containing a sine galvanometer being also applied to the voltameter-plates; so that the current in this derived circuit virtually measured the difference of potential subsisting between the voltameter-plates. Calling f the "intensity of the derived current," and d that due to a Daniell cell working through the same resistance, and taking 23,900 as the "galvanic heat" (§ 17) of each Daniell cell used, the total work done in the voltameter per gramme-equivalent will be represented by $\frac{f}{d} \times 23{,}900$ gramme-degrees: subtracting this from the heat actually developed as such, a value in gramme-degrees is obtained representing the work done as chemical decomposition. In this way Raoult obtained the following values for the chemical decomposition of water and copper sulphate:—

	Water.	Copper sulphate.
	33,858 gramme-degrees.	30,407 gramme-degrees.
	34,229 „ „	29,765 „ „
	33,998 „ „	29,683 „ „
Mean .	34,028 „ „	29,951 „ „

These mean values correspond respectively to 34,028 $\chi J=$

1·501 volt and 29,951 χJ=1·321 volt. The former number closely agrees with the above-cited results of Joule, Kiechl, and Favre, and with the number deduced by direct combustion of hydrogen (§ 32); the latter is somewhat larger than the values deducible from the observations of Joule, Julius Thomsen, and Favre, given in § 21.

The mode of calculation employed by Raoult, however, was far less simple than would appear from this brief account of his results. He complicated the work by attempts to measure the amount of polarization set up in the voltameter; and from the numbers thus obtained, together with the strengths of the derived currents and the amounts of heat actually generated as such, he calculated certain amounts of heat, to which he applied the terms "local heat" and "voltaic heat of the voltameter," the difference between which represented the heat equivalent to the chemical decomposition—*i. e.* the above numbers. On revising his formulæ, however, it becomes evident that the determination of the amount of polarization of the voltameter-plates (a determination the accuracy of which is open to some criticism) is in no way involved in the determination of the heat equivalent to the chemical decomposition, inasmuch as the terms involving the amount of polarization in the expressions for the "local heat" and the "voltaic heat of the voltameter" are identical, and consequently become both eliminated in the expressions representing the difference between these two quantities—*i. e.* the chemical decomposition.

24. It is evident that any method that will give a direct valuation by electrical measurements of the work done in the formation of a given compound in terms of E.M.F. will, when compared with the direct determination of the "heat of formation" of the same compound by means of the calorimeter, enable a new valuation to be deduced for J, the mechanical equivalent of heat; for the work done in the formation of a gramme-equivalent of the compound being found by the one method to be measured by e C.G.S. units, and by the other by h gramme-degrees, the relationship

$$hJ=eQ=\frac{e}{\chi}$$

holds, where Q is the quantity of electricity requisite to decompose a gramme-equivalent of substance, *i. e.* where $Q=\frac{1}{\chi}$ (§ 7); whence

$$J=\frac{e}{h\chi}.$$

If, however, the value of J is included in the value of e, as is necessarily the case when e is calculated by the formula $e = \mathrm{E} - \mathrm{H}\chi\mathrm{J}$ (§ 11), this formula becomes

$$h\mathrm{J} = \frac{\mathrm{E} - \mathrm{H}\chi\mathrm{J}}{\chi},$$

whence

$$\mathrm{J} = \frac{\mathrm{E}}{(\mathrm{H}+h)\chi}.$$

25. In the case of water, a number of determinations of h (the heat evolved by the union of oxygen and hydrogen by combustion) have been made by various observers, but for the most part with slightly different values for the heat unit. Leaving out of sight the first approximate determinations of Dulong, Hess, and Grassi (respectively 34,743, 34,792, and 34,666 gramme-degrees per 9 grammes of water formed), the following comparisons of these different determinations may be made, reducing them all, as far as possible, to the same conditions—viz. supposing the oxygen and hydrogen originally used per gramme-equivalent of water (respectively 7·98 and 1 gramme) and the water produced (8·98 grammes) to be all at 0° and 760 millims., the value of a gramme-degree being defined as the heat required to raise 1 gramme of water from 0° to 1° C.

The most recent of these determinations is that of Schuller and Wartha. By introducing hydrogen and oxygen at 0° into a slightly modified Bunsen's ice-calorimeter, burning them, and weighing accurately the water formed, these observers found (Pogg. *Ann.* [2] ii. p. 359), as the mean of four closely concordant valuations, that when 8·98 grammes of water are produced from 1 gramme of hydrogen and 7·98 of oxygen, the materials and products formed being all at 0° and 760 millims., the heat produced is sufficient to raise 341·261 grammes of water from 0° to 100°; *i. e.* defining what they term a "mean calory" to be $\frac{1}{100}$ of the heat required to raise a gramme of water from 0° to 100°, the heat developed is 34126·1 "mean calories." According to Regnault (*Mémoires Acad. Sciences*, 1847, vol. xxi.), the specific heat of water at various temperatures is given by the formulæ

$$\mathrm{Q} = t + 0{\cdot}00002\,t^2 + 0{\cdot}0000003\,t^3,$$

$$\mathrm{S} = 1 + 0{\cdot}00004\,t + 0{\cdot}0000009\,t^2;$$

where Q is the quantity of heat required to raise a unit of weight of water from 0 to t, and S is the specific heat at t°; so that the mean specific heat between 0° and 100° (*i. e.* the value of a "mean calory") is 1·005. On the other hand, Bosscha has revised the data on which Regnault founded his formulæ (Pogg. *Ann.* Jubelbd. p. 549), and gives the two following expressions for the quantities Q and S:—

$$Q=t+0\cdot00011\,t^2,$$
$$S=1+0\cdot00022\,t;$$

whence the mean specific heat between 0° and 100° is 1·011, a value largely exceeding Regnault's. Employing Regnault's value, the heat-evolution observed by Schuller and Wartha becomes $34126\cdot1\times1\cdot005=34,297$ gramme-degrees; whilst if Bosscha's formula be employed (as was done by Schuller and Wartha in comparing their results with those of other observers), the heat evolution becomes $34126\cdot1\times1\cdot011=$ 34,501 gramme-degrees.

26. Shortly before these experiments were published, von Than found, by nearly the same mode of operating, that 1 gramme of hydrogen and 7·98 of oxygen, at 0° and 760 millims., liberate, in forming water, 33,928 gramme-degrees (*Deut. chem. Ges. Ber.* 1877, vol. x. p. 947). The chief difference between his experiments and those of Schuller and Wartha was that, whilst the latter weighed the mercury sucked into the calorimeter through the melting of the ice, von Than measured it in a capillary tube according to Bunsen's plan, using Bunsen's value for a gramme-degree as above defined in terms of mercury sucked in. Schuller and Wartha calculate, from Bunsen's data, that the quantity of mercury corresponding to a "mean calory" is 0·01541 gramme, whilst their own determinations give 0·015442; von Than used values corresponding to the former number, whilst Schuller and Wartha employed the latter. The differences between each of the values used and the true value are included in the experimental errors of determination in the two heat-evolutions—viz. 34,297 (Regnault) or 34,501 (Bosscha), found by Schuller and Wartha, and 33,928 observed by von Than.

27. A few years previously Julius Thomsen found (Pogg. *Ann.* 1873, vol. cxlviii. p. 368), from three accordant observations, that the formation of 17·96 grammes of water from dry oxygen and hydrogen, all materials and products being at near to 18°, produces an evolution of heat sufficient to raise the temperature of 68,207 grammes of water 1°, the water being to start with at close to 18°; the heat required to raise the temperature of the water produced being allowed for in the determination. From Regnault's formula (§ 25) the specific heat of water at 18° is 1·00101, and from Bosscha's

1·00396; so that this observed heat-evolution corresponds to $34103{\cdot}5 \times 1{\cdot}00101 = 34138$, or to $34103{\cdot}5 \times 1{\cdot}00396 = 34239$ gramme-degrees per 8·98 grammes of water, according as the specific heat of water is calculated from the one or the other formula. If, however, the combination of the oxygen and hydrogen took place at 0° (producing water at 0°) instead of at 18° throughout, the amount of heat-evolution would be increased by $a-b$; where a is the heat given out by 8·98 grammes of water in cooling from 18° to 0°, and b the amount of heat required to raise 1 gramme of hydrogen together with 7·98 grammes of oxygen from 0° to 18°. Since the specific heats for equal weights under constant pressure of oxygen and hydrogen are respectively 0·218 and 3·405, the value of $a-b$ is sensibly 69 gramme-degrees; so that Thomsen's value becomes 34,207 if Regnault's formula is used, and 34,308 if Bosscha's be employed.

28. Favre and Silbermann found (*Annales de Chimie* [3], xxxiv. p. 357) that when 9 grammes of water were formed from oxygen and hydrogen, the gases being at a mean temperature of 9° (at 6° to commence with and 12° to end with), and the water produced being at 6° (the heat required to raise the temperature of the water produced being taken into account in determining the water-equivalent of the calorimeter), the average heat-evolution sufficed to raise 34,462 grammes of water 1°, the water being at the average temperature of 9°. According to Regnault's formula the specific heat of water at 9° is 1·00043, and according to Bosscha's 1·00198; whence the heat-evolution per 8·98 grammes becomes

$$\frac{8{\cdot}98}{9} \times 34462 \times 1{\cdot}00043 = 34400,$$

or

$$\frac{8{\cdot}98}{9} \times 33462 \times 1{\cdot}00198 = 34453,$$

according as the one or the other formula is used. To this must be added a quantity $c-d$ to reduce the value to that which would have been produced had the gases and water produced been all at 0°: c is the heat given out by 8·98 grammes of water in cooling from 6° to 0°, whilst d is the heat required to raise the hydrogen and oxygen from 0° to 9°; whence $c-d$ is sensibly 8 gramme-degrees. So that the heat-evolution becomes 34,408 by Regnault's formula, and 34,461 by Bosscha's formula.

29. Andrews found (Phil. Mag. 1848, vol. xxxii. p. 321) that a litre of dry hydrogen gas at 0° and 760 millims., in combining with oxygen, the gases being at the average tem-

perature of 19°·7, produced (on an average of four accordant experiments) enough heat to raise 3036 grammes of water 1°, the water being at an average temperature of 20°·7, and the calculation not including the heat required to raise the water produced from the average initial temperature of 19°·7 to the average final temperature 21°·8. Since a litre of hydrogen at 0° and 760 millims. weighs 0·089578 gramme (Regnault), the corrected heat-evolution per gramme of hydrogen burnt (*i. e.* per 8·98 grammes of water produced) would suffice to heat $\frac{3036}{0·089578}+19=33,911$ grammes of water 1°, the water being at 20°·7. According to Regnault's formula the specific heat of water at 20°·7 is 1·00122; according to Bosscha's formula, 1·00455. Hence the heat-evolution is $33911\times 1·00122=33952$ (Regnault's formula), or $33911\times 1·00455=34065$ (Bosscha's formula). To this must be added a quantity $e-f$ to reduce the value to that which would have been produced had the gases and water formed been all at 0°, e being the heat given out by 8·98 grammes of water in cooling from 19°·7 to 0°, and f that required to heat up the constituent hydrogen and oxygen from 0° to 19°·7; so that $e-f$ is sensibly 76 gramme-degrees; whence finally the heat-evolution becomes 34,028 by Regnault's formula, and 34,141 by Bosscha's formula.

30. These reduced values may therefore be thus contrasted:—

	By Regnault's formula.	By Bosscha's formula.
Schuller and Wartha . .	34297	34501
Von Than	33928	33928
Julius Thomsen	34207	34308
Favre and Silbermann . .	34408	34461
Andrews	34028	34141
Mean . . .	34174	34268

That is, the average of these different determinations indicates that by the combustion of 1 gramme of hydrogen and 7·98 grammes of oxygen to water, the gases and water produced being at 0° and 760 millims. throughout, the heat produced would raise the temperature of 34,174 or 34,268 grammes of water from 0° to 1° C., according as Regnault's or Bosscha's formula for the specific heat of water is used. It is noticeable that the latter formula makes Schuller and Wartha's most carefully made experiments appear abnormal, by showing a much greater divergence, both from von Than's (made by essentially the same method) and from the general average, than is the case when Regnault's formula is employed; and hence an additional reason is derived for viewing Bosscha's alleged corrections of Regnault's numbers as being more open to objection than these numbers themselves.

If the combination of the oxygen and hydrogen be supposed to take place at 20° instead of 0° (*i. e.* if the gases used and water produced be all at 20°), the heat-evolution will be less than that calculated above by $g-h$ gramme-degrees, where g is the heat required to raise 8·98 grammes of water from 0° to 20°, and h that requisite to raise the constituent gases through the same range, *i. e.* where $g-h=$ sensibly 77 gramme-degrees. Whence, if water be decomposed at 20° and 760 millims. into oxygen and hydrogen at the same temperature and pressure, the heat-absorption is 34,097, using Regnault's formula, and 34,191 using Bosscha's formula. Similarly at 15°, the correction $g-h$ would be 58 gramme-degrees, giving the values 34,116 (Regnault's formula), and 34,210 (Bosscha's formula).

31. On the other hand, experiments have been cited above (§§ 2 & 5), made by Joule, Kiechl, Raoult, and Favre, giving the following values for the heat-absorption during the electrolysis of water at the ordinary temperature:—

Joule	33557
Kiechl	33653
Raoult	34028
Favre	34204
Mean . .	33860

As these values are mostly calculated per 9 grammes of water, this average value should be reduced in about the proportion of $\frac{8·98}{9}$; but since the increase of temperature of the calorimeter observed represents a slightly higher number of gramme-degrees than that stated, on account of the increase of the specific heat of water with the temperature, the average value requires slightly raising from this cause. After making all probable allowances for these corrections, however, the mean value still remains perceptibly lower than that calculated as above from the results of Schuller and Wartha, von Than, Julius Thomsen, Andrews, Favre, and Silbermann.

On the whole, the data at present extant may be taken to show that *when* 8·98 *grammes of water are formed from gaseous oxygen and hydrogen at the ordinary temperature* (15° to 20°) *throughout, the evolution of heat is sensibly that required to raise the temperature of* 34,100 *grammes of water from* 0° *to* 1° C; or when 1 gramme is formed, the heat-evolution suffices to raise 3797 (or practically 3800) grammes from 0° to 1° C., this value being taken from the reductions by Regnault's formula as being probably more accurate than those made by Bosscha's alleged corrected formula.

32. A number of observations, the details of which are given in Part II., show that, admitting the accuracy of Latimer Clark's valuation of the E.M.F. of his standard cell (Proc. Roy. Soc. 1872, vol. xx. p. 444) as being $1{\cdot}457 \times 10^8$ C.G.S. units at 15°, the value of e in the expression $e = \mathrm{E} - \mathrm{H}\chi\mathrm{J}$ (§ 11) is $1{\cdot}5003 \times 10^8$ C.G.S. units when water is electrolyzed at a temperature of 15°–20°, the value of J being taken as 42×10^6—the probable error of the mean result being $\pm 0{\cdot}0048$, or $\pm 0{\cdot}32$ per cent. Owing, however, to certain causes detailed in Part II. (§ 39), this value is slightly too low—this result being esentially due to the impracticability of collecting every trace of gas evolved, so that the value of H (the heat evolved per gramme-equivalent of water decomposed) is slightly overvalued, and hence e slightly undervalued.

From the combustion experiments of previous investigators, summarized in the preceding eight paragraphs, it results that the value of e in the expression $e = \mathrm{H}'\chi\mathrm{J}$ (§ 11) is $1{\cdot}5038 \times 10^8$ (since $\mathrm{H}' = 34{,}100$), J being, as before, taken as 42×10^6. These numbers, $1{\cdot}5003 \times 10^8$ and $1{\cdot}5038 \times 10^8$, accord so closely as to show that (so far as these experiments can decide) the value of J cannot be far from 42×10^6. Applying the formula

$$\mathrm{J} = \frac{\mathrm{E}}{(\mathrm{H} + \mathrm{H}')\chi}$$

(§ 24) to the experimental data given in Part II., the average value deduced for J is $41{\cdot}96 \times 10^6$, this value for J giving the following values for e:—

From formula $e = \mathrm{E} - \mathrm{H}\chi\mathrm{J}$ . . . $e = 1{\cdot}5023 \times 10^8$
„ „ $e = \mathrm{H}'\chi\mathrm{J}$ . . . $e = 1{\cdot}5024 \times 10^8$.

Since, however, the mean value of H is slightly overvalued, it results that the value of J finally deduced is slightly *above* $41{\cdot}96 \times 10^6$, with a probable error of less than $\pm 0{\cdot}4$ per cent., *i. e.* of less than $\pm 0{\cdot}016 \times 10^6$, due to the electrolytic experiments, the probable error in the determination of H′ not being taken into account.

33. It is evident that, considering the nature of the data from which this conclusion is drawn, not much dependence can be placed on the final value of J deduced except as an approximation. It is noteworthy, however, that this value of J very closely agrees with that deduced by Joule in 1867 B. A. Reports, 1867), by determining the heat producible in a wire of known resistance by a known current, and that

both are perceptibly higher than the value obtained by Joule by means of water-friction &c. in 1850 (Phil. Trans. 1850, p. 61); whilst this older value is completely corroborated by his recently made experiments on water-friction &c. (Phil. Trans. 1878, p. 365). In 1850 Joule found that the heat required to raise 1 lb. of water (weighed *in vacuo*) from t to $t+1$ (where t lies between 55° and 60° F.) corresponded to the following number of foot-pounds at Manchester:—

By friction of water . . .	772·692
„ „ mercury . .	774·083
„ „ cast iron . .	774·987

Of these values the first was considered probably the most accurate; and as a little vibration and sound was unavoidable in the experiments, the round number 772 was adopted as the most probable value. The 1878 value finally arrived at is that the heat required to raise 1 lb. of water (weighed *in vacuo*) from 60° to 61° F. corresponds to 772·55 foot-pounds, and is hence sensibly identical with the former result. The 1867 values, however, were as follows, expressed in foot-grain-second units:—

	Per grain degree F. (water weighed in air).
1st series. Mean of 10 experiments . .	25335
2nd „ „ 15 „ . .	25366
3rd „ „ 30 „ . .	25217

Of these the last value was taken as most exact; when reduced to water weighed *in vacuo* it becomes 25187. As in this series the average temperature of the calorimeter was 18°·63 C. = 65°·5 F., the final result is that, to raise the temperature of 1 gramme of water from 65° to 66° F., requires 25,187 foot-grain-second units of work.

34. When reduced to C.G.S. measure and degrees Centigrade the three values, 772·692 foot-pounds, 772·55 foot-pounds, and 25,187 foot-grain-second work units, become respectively 41·60, 41·59, and 42·14 megalergs per gramme-degree, the value of g at Manchester being taken at 981·3. (Compare illustrations of the C.G.S. system of units, by Prof. Everett, p. 51.) Since, however, the specific heat of the water at the three assigned temperature-limits, 55°–60°, 60°–61°, and 65°–66° F., is not quite the same, on reduction to the gramme-degree taken above as unit (the heat requisite to raise 1 gramme of water from 0° to 1° C.), these values become

$$\frac{41{\cdot}60}{1{\cdot}0008}=41{\cdot}57,\quad \frac{41{\cdot}59}{1{\cdot}0009}=41{\cdot}54,\ \text{and}\ \frac{42{\cdot}14}{1{\cdot}0010}=42{\cdot}10$$

megalergs respectively when Regnault's formula for the specific heat of water (§ 25) is taken; whence the mean value of J is 41·555 megalergs from the two sets of friction experiments (1850 and 1878), and 42·10 by the electric-current method (1867)—the latter consequently being $\frac{42\cdot10}{41\cdot555}=$ 1·0131 times the former, or 1·31 per cent. in excess. The value for J deduced above from the writer's experiments (viz. 41·96 megalergs) is $\frac{41\cdot96}{41\cdot555}=1\cdot0098$ times that got from Joule's friction experiments, showing an excess of 0·98 per cent. Hence, on the whole, the writer's numbers and Joule's 1867 values concord much more exactly than might have been anticipated, each showing an excess over Joule's water-friction value (1850 and 1878) of about 1 per cent., Regnault's formula being employed for reducing the observed rise of temperature to gramme-degrees at 0°–1° C.

35. Reasons have already been given (§ 32, *vide* also Part II. § 39) for supposing that the value 1·5003 found for the E.M.F. representing the affinity for oxygen and hydrogen in water is slightly understated. The effect of an error of this kind would be to raise the value of J deduced by the formula

$$J=\frac{E}{(H+h)\chi} \quad (\S\ 24),$$

and hence to make the excess of the value of J thus deduced over Joule's water-friction value to be somewhat more than the 1 per cent. or thereabouts just calculated. It is worthy of notice that there is also reason for supposing that Joule's 1867 value is understated. Experiments are described in Part II. the result of which is to show that an unobserved source of error existed in Joule's 1867 experiments, the effect of which was to diminish the value of J thence deduced by an amount probably equal to some tenths per cent.; so that on the whole Joule's 1867 value, when corrected, is probably from 1·5 to 2·0 per cent. higher than his water-friction value (1850 and 1878).

This difference between the two values is precisely that which would subsist did an error to an equal amount exist in the B.A. resistance-unit valuation: *i. e.* if the B.A. unit were 1·015 to 1·020 earth-quadrant per second instead of being exactly 1 earth-quadrant per second, the value of J deduced from Joule's 1867 experiments would be 1·015 to 1·020 times the true value; for it is calculated by the formula

$$J=\frac{C^2Rt}{H},$$

where C is the current, R the resistance, t the time, and H the heat evolved. Moreover the existence of such an error in the B.A. unit would cause a precisely corresponding error in Latimer Clark's valuation of the E.M.F. of his standard cell (Proc. Roy. Soc. 1872, vol. xx. p. 444), this valuation being based on the determination of the resistance R between two points traversed by a known current C, the potential difference E between the points being equal to the E.M.F. of the cell, so that by Ohm's law $E = CR$; and the effect of this would be to cause a precisely similar error in the value of J deduced by the formula $J = \frac{E}{(H+h)\chi}$, as in the writer's experiments.

36. In confirmation of the supposition that the B.A. unit of resistance is too large, Kohlrausch's experiments may be cited (Phil. Mag. 1874, vol. xlvii. pp. 294 & 342). A careful comparison of several copies of the B.A. unit with coils standardized by himself in absolute measure led Kohlrausch to the conclusion that the B.A. unit is really 1·0196 earth-quadrant per second, or 1·96 per cent. too large; for he found that the resistance of certain coils graduated in Siemens mercury units averaged $0·9717 \times 10^9$ C.G.S. units, whilst the B.A.-unit coils averaged 1·0493 times the resistance of the Siemens coils, giving therefore for the B.A. unit the value

$$0·9717 \times 1·0493 \times 10^9 = 1·0196 \times 10^9.$$

On the other hand, it is to be observed that Lorenz has made a determination of the value of a Siemens mercury unit with a lower result (Pogg. *Ann.* 1873, vol. cxlix. p. 251), viz. 0·9337 earth-quadrant per second. Lorenz's mercury-tubes were made by himself; and it does not seem from his paper that any evidence was obtained of the purity of the mercury used other than the fact that its specific gravity at 5°·3 C. was 13·586 after digesting for a long time with nitric acid. These mercury-tubes do not seem to have been compared directly with any B.A.-unit coil so as to give the value of the B.A. unit in absolute measure; on comparing them with a reputed Siemens unit, Lorenz found a difference of nearly 1 per cent. ($0·943 \times 10^9$ as against $0·9337 \times 10^9$)—the difference between Lorenz's mercury-tube value of a Siemens unit and the value of the latter in B.A. units, as deduced by Kohlrausch, being nearly exactly double this amount, or nearly 2 per cent., $\frac{1}{1·0493} \times 10^9 = 0·9530 \times 10^9$ as against $0·9337 \times 10^9$.

37. Yet, again, values for J have been obtained by other observers considerably higher than that deduced from Joule's

water-friction experiments. The following Table (mainly taken from Prof. G. C. Foster's article "Heat" in Watts's 'Dictionary of Chemistry,' 1st Supplement, p. 687) gives the chief of these results expressed in gravitation measure, no corrections being made for differences in the value of g at the various localities where the experiments were made, or in the specific heat of water at different temperatures. The last column gives the approximate ratio of the value found to that deduced by Joule from water friction, this latter being taken as 423·5 metre-grammes per gramme-degree Centigrade :—

Observer.	Method employed.	Value of J found.	Ratio to Joule's water-friction value.
Hirn	Crushing of lead	425·2	1·004
"	Friction of water and brass...........................	432	1·020
"	Escape of water under pressure	433	1·023
"	Specific heats of air	441·6	1·043
Joule	Heat produced by electric currents	429·3	1·014
Violle......	" " induced currents (aluminium)	434·9	1·027
"	" " " " " (copper) ...	435·2	1·028
"	" " " " " (tin)	435·8	1·029
"	" " " " " (lead)	437·4	1·033
Regnault...	Velocity of sound	437	1·032

On the whole, it is evident that further experiments are desirable as to the development of heat in circuits of resistance measured in terms of the B.A. unit by known currents, so as to establish as accurately as possible a connexion between the values of J and of the B.A. unit, so that if the first is assumed to be correctly given by Joule's water-friction value the error (if any) in the B.A. unit can be calculated, or so that the value of J can be determined afresh when the precise value of the B.A. unit is known from further experiments. The writer hopes shortly to be enabled to communicate the results of experiments in this direction made by a method differing from that employed by Joule in certain important points, so as to avoid the sources of error existent in Joule's 1867 experiments (Part II.).

XXXIII. *A Suggestion in regard to Crystallization, on the Hypothesis that Molecules are not infinitely Hard. By* S. TOLVER PRESTON*.

IN what follows, it is assumed (1) that molecules are *elastic.* This is not contradicted by any evidence, and is powerfully supported by spectroscopic observations. (2) It is assumed that molecules possess an *open* structure. This inference is equally not opposed to any observed facts, and is indeed strongly upheld by many. (3) The basis of the constitution of the æther, developed by the present writer in the Philosophical Magazine (Sept. and Nov. 1877, and Feb. 1878) is taken as a groundwork. Briefly stated, according to this the æther is to be regarded as a gas whose atoms are so small that their mean length of path is far greater than any planetary distances, the atoms automatically adjusting their motions (according to principles investigated in connexion with the kinetic theory of gases) so as to be capable of producing gravity under the sheltering principle of Le Sage.

Since the elementary *ring*† is evidently the *simplest* form of open structure for a molecule, this form is taken (in the cases to be considered). The rings (relatively very large) constituting the molecules of gross matter are, accordingly, to be regarded as pervaded through and through (in their open structure) by streams of minute æther atoms, which automatically and inevitably adjust their motions so as to move equally in all directions. If we conceive (for the purpose of illustration) a number of rings [molecules] to lie in one plane with their peripheries touching, or very close, then, owing to their adjacent parts being now sheltered from the streams of æther atoms, which impinge with full energy elsewhere, the peripheries of the rings will be impelled towards each other (according to Le Sage's principle). As the ring molecules are (by assumption) elastic, they will naturally bend, and the contiguous surfaces will flatten (somewhat) under the powerful impulsive action of the streams of æther atoms, which tend to bring as much as possible of the mutually sheltered peripheries

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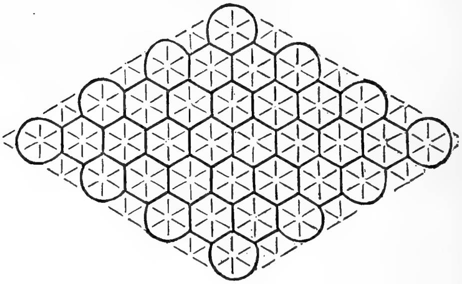
* Communicated by the Author.

† I have before had occasion to remark that the common tacit assumption of molecules being *solid throughout* involves a waste of material that violates one of the first principles of large-scale architecture, where superfluity of material is recognized to be one of the worst faults, detrimental alike to the elasticity and stability of the structure. Dynamical principles are admittedly independent of *scale.* The elasticity of molecules (demonstrated by the spectroscope) would also sufficiently indicate an *open* structure, so as to allow a relative mobility of parts consistently with inseparability.

into contact. If we suppose the impulsive influence to be powerful enough to bring (sensibly) the entire peripheries of all the rings into mutual contact, the rings bending (from their elasticity) for this purpose, then it would appear that the whole must form a system of hexagons, as the annexed diagram (fig. 1) may serve roughly to illustrate. For the conditions of equilibrium would not allow any thing else. It is true that squares might conceivably be formed if the bending were sufficient for that purpose (and by a somewhat different arrangement of the rows), but it must evidently in any case (from the conditions of equilibrium) be some regular or geometric figure. Might not this possibly be capable of throwing some light on the phenomena of crystallization? It appears evident that the ring molecules, under these conditions, could also form hollow solid figures of geometric shape, if the rings were to unite at their peripheries or edges in a regular manner. But it will be seen that the tendency for the rings to unite at their edges or boundaries in a regular manner is *automatic*, since at their edges alone shelter exists, the streams of æther atoms passing freely through the open parts of the rings, so that there exist virtually "lines of impulsive action" directed symmetrically from edge to edge, producing automatically a *guiding* action, or (crystalline?) "building."

Fig. 1.

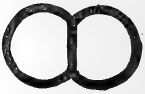


If there were any foundation for these data in reality, it would follow as an *à priori* conclusion, that leaving the ring molecules to themselves in an unhampered manner (as, for example, occurs in the process of melting or solution) would be most favourable to crystalline building. Also it would appear, equally *à priori*, that if the ring molecules were artificially interfered with (as by hammering for instance) they might (especially from their elasticity) be crossed or jostled up together irregularly, and so not unite symmetrically at their edges, but sideways (*i. e.* in all conceivable modes); so that in this case a certain amount of sliding or play of the ring molecules over each other would be possible before they finally separate (which occurs when the substance is broken into parts). On the other hand, it appears equally evident that this sliding or play would be impossible in that case where the molecules are united symmetrically at their extreme boundaries or edges, so as to build up the crystalline structure. The above inference would at least be consistent with the observed fact that non-crystalline masses (such as hammered iron, for instance) are found to allow a certain amount of play or sliding of their ultimate parts over each other, before they finally separate. Crystalline masses, on the other hand (cast iron, for example), are found to break with any attempt to alter the position of their parts. These conditions of ring molecules (capable of bending into various forms within certain limits) might also possibly throw some light on the fact that the same substance may have occasionally various crystalline forms and sometimes even no crystalline form at all. Sulphur, for instance, may be brought (as is known) into a non-crystalline (amorphous) state, when it can be stretched like india-rubber. Other examples might be given.

It appears evident that, in addition to the bending of the peripheries of the elastic rings, the transverse sections (where in contact) would also flatten somewhat under the pressure due to the streams of æther atoms, which tend energetically to cause a closer approach. Under this flattening of the contiguous surfaces the lines of contact would come to have a measurable (though small) breadth, thereby increasing the area of shelter, and consequently the energy with which the ring molecules are made to cohere. This flattening of the contiguous surfaces would also have the effect of increasing the limits of elasticity of the whole structure. When the ring molecules are gradually pulled apart, there would first naturally be a gradual recovery of the circular form of periphery and diminution of the flattening (where contact exists), the area of shelter thereby becoming less and less, until finally, when the peripheries of the ring molecules are nearly separate, their energetic natural tendency to recover their normal (circular) form would no doubt cause them to spring apart, provided the tendency to recovery of form predominated over the residual impulsive tendency to approach (produced by the streams of atoms). Might not this possibly be capable of throwing some light on the observed mutual repulsion of the parts of substances when first brought into proximity, and their (contrary) impulsion towards each other (union) when brought into closer proximity, as, for example, india-rubber, glass, plates of metal, and numerous substances may be made to unite on their parts being pushed into sufficient nearness by an adequate pressure. Fig. 2 may serve to represent two ring molecules, pushed into such proximity that their adjacent parts are slightly bent and flattened, but the surface of contact is not yet sufficient to afford shelter enough to satisfy the condition that the impulsive tendency of approach can predominate over the (contrary) repellent action due to elasticity of form (*i. e.* the tendency of the rings to preserve their normal circular shape). The rings accordingly bound back (repel) directly the artificial pressure is removed*. Fig. 3 may serve to illustrate the case where the ring molecules have been previously pushed into such proximity, that the bending and flattening of the contiguous surfaces has gone far enough to increase the shelter to such an extent that the impulsive tendency to approach can predominate over the residual elasticity of form, and so the molecules are impelled closer together (the shelter increasing with the contiguity of parts). The sudden flying of some substances into fragments (such as badly annealed glass for instance), under certain conditions, where the elastic ring molecules would be in a state of strain, and perhaps nearly at the point at which they would naturally spring apart in virtue of their elasticity of form, may perhaps serve as rather a confirming illustration of this view.

Fig. 2.

Fig. 3.



The writer at least ventures to think that the points dealt with may not be without interest or unworthy of attention, on account of the simplicity of the premises, by which some degree of insight would seem to be afforded into certain interesting but obscure phenomena, and under physical conditions that could not of themselves be said to be independently improbable, and which are even supported by some inductions and observations of modern science. Any light whatever that these premises might be capable of throwing on obscure facts might then legitimately be regarded as some additional confirmation of the validity of the premises, which themselves have been inferred on independent grounds.

The conception of *elastic*† molecules (rendered necessary by

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* No doubt (in addition) the vibration of the ring molecules also plays a part here, as molecules are known to become completely dissociated by excessive increase of vibrating energy (at extreme temperatures).

† The conception of molecules of *open* structure (itself probable) would be fundamentally the only other hypothesis required to suit the above conditions.

the principle of the conservation of energy and by spectroscopic observations) would seem to be a highly practical one, much needed in physics. By it all idea of the inconceivable jar of the collision of *infinitely* hard molecules (or atoms) is avoided. On account of the perfect elasticity, all motions take place with complete smoothness, so that a perfect state of mobile equilibrium is rendered possible in nature, which (without due precautions) may well be competent to deceive the senses into the idea that in what we call "space" all is in a state of rest.

London, 1880.

XXXIV. *On Unitation.—X. Practical Remarks thereon, together with Examples. By* W. H. Walenn, *Mem. Phys. Soc.*

[Continued from p. 123.]

40. THE next algebraic forms of δ that claim attention are those comprised under the formula $\mathrm{U}_{kn}\mathrm{N}$, k being a fundamental base such as 9 or 11, and n a whole number as a multiplier. This set of unitates may be said to be the entry to the domain which enables unitation to furnish easily the remainders to divisors in general.

41. In dissecting the expression $\mathrm{U}_{kn}\mathrm{N}$, it is observable that the next less number exactly divisible by k is $\mathrm{N}-\mathrm{U}_k\mathrm{N}$, and that $\mathrm{U}_{kn}\mathrm{N}=\mathrm{U}_k\mathrm{N}+km$, in which m is a function of $\mathrm{U}_n(\mathrm{N}-\mathrm{U}_k\mathrm{N})$. Each of the multiples of k up to kn may give a separate value when unitated to the base n. Let the multiples of k be a_1, a_2, $a_3, \ldots a_n$ (so that $k=a_1$, $2k=a_2$, $3k=a_3$, &c.); then the series $\mathrm{U}_n(a_1, a_2, a_3, \ldots a_n)$ may be $1, 2, 3, 4, \ldots n$, or it may be other values. If $\mathrm{U}_n(a_1, a_2, a_3, \ldots a_n)=1, 2, 3, 4, \ldots n$, this is the same sequence in which the multiples of k enter into $\mathrm{U}_{kn}\mathrm{N}$, and $m=\mathrm{U}_n(\mathrm{N}-\mathrm{U}_k\mathrm{N})$; if $\mathrm{U}_n(a_1, a_2, a_3, \ldots a_n)$ gives other values, means must be found to reduce them to the set of values $1, 2, 3, 4, \ldots n$, if $\mathrm{U}_{kn}\mathrm{N}$ is to be used for obtaining the remainders to the divisor under consideration.

42. If $k=9$, then $a_1, a_2, a_3, \ldots n=9, 18, 27, 36, \ldots n$; and to test whether $\mathrm{U}_{kn}\mathrm{N}$ is applicable when $kn=72$, for instance, it is necessary to obtain $\mathrm{U}_8(9, 18, 27, 36, 45, 54, 63, 72)$. This is found to be $=1, 2, 3, 4, 5, 6, 7, 0$; therefore $\mathrm{U}^{kn}\mathrm{N}$ is applicable in this case in a direct manner. U_{kn} is not applicable when $kn=27$; for then $\mathrm{U}_3(9, 18, 27)=0, 0, 0$: this result is also evident from the simple consideration that 3 is a submultiple of 9. If $\mathrm{U}_{kn}=45$, $\mathrm{U}_5(9,18,27,36,45)=4,3,2,1,0$; this can be reduced to the required sequence 1, 2, 3, 4, 0 by taking $m=-\mathrm{U}_5(\mathrm{N}-\mathrm{U}_9\mathrm{N})$, or obtaining the complement to

5 of the result given by the operation $U_5(N-U_9N)$. For instance, to obtain $U_{45}\,671$,

$$U_9\,671=5,$$

$$U_5(N-U_9N)=U_5(671-5)=U_5 666=1,$$

and

$$m=-1 \text{ or } 4.$$

Therefore

$$U_{45}671=5+9\,.\,4=41.$$

43. In the set of unitates $U_{63}N$,

$$U_7(a_1,\,a_2,\,a_3,\ldots a_n)=2,\,4,\,6,\,8,\,10,\,12,\,0,$$

and $m=\frac{1}{2}\{U_7(N-U_9N)\}$. For instance, to find $U_{63}1002$,

$$U_9N=3,\quad U_7(N-U_9N)=U_7 999=5,\quad m=\tfrac{1}{2}(5+7)=6,$$

and $U_{63}N=3+9\,.\,6=57$.

44. The operations for obtaining m, in the formula

$$U_{kn}N=U_kN+km,$$

when $k=9$, may be tabulated as follows:—

kn.	*m.*	*kn.*	*m.*
18	$U_2(N-U_9N)$.	90	$-U_{10}(N-U_9N)$.
27		99	
36	$U_4(N-U_9N)$.	108	
45	$-U_5(N-U_9N)$.	117	$\frac{1}{9}\{U_{13}(N-U_9N)\}$.
54		126	$\frac{1}{9}\{U_{14}(N-U_9N)\}$.
63	$\frac{1}{2}\{U_7(N-U_9N)\}$.	135	
72	$U_8(N-U_9N)$.	144	$\frac{1}{9}\{U_{16}(N-U_9N)\}$.
81			

The blanks indicate that the operation is impracticable for the corresponding value of *kn*, as shown in paragraph 42.

45. It should be observed that, when m is a fractional part of $U_n(N-U_kN)$, as in $U_{117}N=U_9N+9\,.\,\frac{1}{9}\{U_{13}(N-U_9N)\}$, $U_n(N-U_kN)$ must be made an exact multiple of the denominator of the fraction by the addition to it of one of the multiples of n. Thus, in finding $U_{117}25763$,

$$U_{9\,.\,13}N=5+9\,.\,\tfrac{1}{9}\{U_{13}25758\}=5+9\,.\,\tfrac{1}{9}(U_{13}5)$$
$$=5+9\,.\,\tfrac{1}{9}(5+13)=5+9\,.\,2=23.$$

46. When $k=11$, the operations for obtaining m take a

different tone, and the only values of n that present impracticable cases are those in which n is an exact multiple of 11. The table of values for m up till $n=16$ is as follows:—

kn or δ.	m.	kn or δ.	m.
22	$U_2(N-U_{11}N)$	110	$U_{10}(N-U_{11}N)$
33	$-U_3(N-U_{11}N)$	121	
44	$-U_4(N-U_{11}N)$	132	$-\ U_{12}(N-U_{11}N)$
55	$U_5(N-U_{11}N)$	143	$-\frac{1}{2}\{U_{13}(N-U_{11}N)\}$
66	$-U_6(N-U_{11}N)$	154	$-\frac{1}{3}\{U_{14}(N-U_{11}N)\}$
77	$\frac{1}{4}\{U_7(N-U_{11}N)\}$	165	$-\frac{1}{4}\{U_{15}(N-U_{11}N)\}$
88	$\frac{1}{3}\{U_8(N-U_{11}N)\}$	176	$-\frac{1}{5}\{U_{16}(N-U_{11}N)\}$
99	$\frac{1}{2}\{U_9(N-U_{11}N)\}$		

47. Examples of $U_{11n}N$:—

I. Find $U_{55}1257$.

$$U_{55}N=3+11\{U_5(1257-3)\}=3+11(U_5 1254)$$
$$=3+11.4=47.$$

II. Find $U_{66}83253$.

$$U_{66}N=5+11(-U_6 83248)=5+11.U_6(-4)=5+11.2=27.$$

III. Find $U_{99}75316$.

$$U_{99}N=10+11(\tfrac{1}{2}U_9 75306)=10+11.U_9\tfrac{3}{2}=10+11.6=76.$$

IV. Find $U_{165}6782$.

$$U_{165}N=6+11(-\tfrac{1}{4}U_{15}6776)=6+11.U_{15}(-\tfrac{11}{4})$$
$$=6+11.1=17.$$

48. When k is a prime number, as in $U_{7n}N$, the function behaves itself like $U_{11n}N$ in respect to its general shape and in its comparative freedom from impracticable cases; at the same time other properties are brought out and a fresh series of divisors are arranged under the formula $U_{kn}N$.

49. The examples of $U_{kn}N$ are devised to show the principle involved by the aid of only a few figures. With a little practice, the work may be performed mentally or with only a sparse use of writing-materials.

520 Caledonian Road, N,
February 1880.

XXXV. *On the Motion of Glaciers.*

By R. H. Koch *and* Fr. Klocke[*].

THE measurements hitherto made upon glacier-motion have either been only approximative determinations of the annual or daily *mean* velocity, or have been executed for the purpose of showing that the motion of glaciers corresponds with that of liquids; while investigations upon the actual course of this downward motion are wanting—whether, namely, it proceeds continuously in the same direction, or whether it is discontinuous, stoppages or even reversals of the motion taking place. From the observations made by M. Pfaff on the névé[†] the latter is probable even for the motion of glaciers.

Our investigations were limited to the observation of a point of the surface in a vertical plane parallel to the longitudinal direction of the glacier; the measuring of the lateral motion, and the motion of the deeper layers of the glacier, as well as observations during the night, were for the time excluded. Nevertheless, from the facts which have up to the present time been observed, it can already be concluded that the motion of a glacier is not at all uniform; for one and the same point may now move toward the mountain, now toward the valley, now ascend, now descend. It was further found that two points of the surface, distant from 50 to 60 metres from one another, can move in different, nay, in opposite directions.

The method chosen for the observations was that employed by M. Pfaff, and already used for other purposes. Two scales, one perpendicular to the other, were fixed upon the glacier so that the one was vertical and the other horizontal; their movements were observed on the cross-threads of a firmly placed telescope. The number of scale-divisions travelled over by the cross-threads gave then directly the horizontal and vertical components of the motion[‡].

The examinations were made in the days from the 28th of August to the 6th of September, on the west side of the Morteratsch glacier. The observing-station was about 1½ kilometre distant from the lower principal extremity of the glacier. This situation was selected on account of the firm ground

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[*] Translated from a separate impression, communicated by the authors, from Wiedemann's *Annalen*, 1879, vol. viii. pp. 661–666.

[†] *Abh. d. math.-phys. Classe der k. bayer. Akademie der Wissenschaften*, xii. Abth. 2, p. 105 *seqq.* 1876.

[‡] The telescopes and tripod stands were kindly intrusted to us by the Director of the Freiburg Mathematical Cabinet, Prof. Lindemann, and by the Director of the Physical Institute, Prof. Warburg.

which it presented for the setting-up of the instrument (a condition not everywhere fulfilled), and also because at this spot the glacier formed a tolerably continuous mass, without any longitudinal, transverse, or marginal fissures worth mentioning; here, too, was the best prospect of being free from accidental anomalies of the motion.

In setting up the scales we proceeded in the following manner. The stake on which the scales were fixed was sunk into the ice to the depth of half a metre, and a mound of ice 30 centims. in height raised around it, in order as much as possible to prevent melting away of the ice at this spot. The mound was covered with earth and stones; so that the whole resembled one of the well-known earth-mounds. A stake thus planted was perfectly stable and could be employed for the observations for about four days. The reading-telescopes were set up firmly on the bank, and sufficiently sheltered from the action of the sun. The readings were nearly always executed by both of us, each independently of the other. The scales were graduated in half-centims.; millimetres were estimated. The uncertainty of the estimation amounted, with unfavourable illumination or with trembling of the image in consequence of unequal heating of the intervening air, to about 0·1 centim.; but it sometimes rose to 0·2 centim.; in normal circumstances, however, the readings of both observers were perfectly concordant.

In order to convince ourselves that the motions observed on the scales were not motions of the scales themselves, but really indicated the motion of the point in question of the glacier, near one of the signals a second was fixed in the ice, and so close to the first that both appeared in the field of the telescope. With this arrangement the observations showed that both pairs of scales moved in close conformity to one another.

We select as evidence for our above-expressed assertion the observations of the 3rd and 4th of September, because on those days the motions of the glacier took place under different conditions: the 3rd was all bright and cloudless, while the sky during the forenoon of the 4th (the time of the greatest motion) was covered with cumulus clouds and the glacier was only at times exposed to the sunshine.

The times in the following Table are reckoned from midday to midday. The numbers give the horizontal and vertical motions in centimetres. + denotes a downward motion, − a motion upward (uphill). The readings took place halfhourly during the period from 6 A.M. to 6 P.M.

	Scale I. Distance from the margin of the glacier 35 m. Distance from the telescope 93 m.		Scale II. Distance from the margin of the glacier 90 m. Distance from the telescope 148 m.		State of the sky.
From Sept. 2, 23^{h} 30^{m} to	Horizontal motion.	Vertical motion.	Horizontal motion.	Vertical motion.	
Sept 3 0^{h} ……	±0·0	+0·2	+1·0	−0·1	Sept. 3 clear.
0^{h} $—^{m}$ to 0^{h} 30^{m}	+1·0	+0·6	+0·6	+0·2	
0 30 „ 1 —	−0·2	±0·0	+0·5	+0·3	
1 — „ 1 30	+0·1	±0·0	+0·5	+0·2	
1 30 „ 2 —	+0·2	+0·3	+0·6	+0·3	
2 — „ 3 —	+0·1	+0·5	+0·9	+0·6	
3 — „ 4 —*	+0·1	−0·1	+0·3	+0·0	
4 — „ 5 —	−0·4	+0·2	±0·0	±0·0	
5 — „ 6 —	+0·8	+0·1	−0·3	±0·0	
6 — „ 18 —	+2·3	−1·0	−6·2	−1·0	
18 — „ 19 —†	+1·1	−0·1	−0·1	+0·3	
19 — „ 19 30	+0·6	+0·7	−2·1	+0·2	
19 30 „ 20 —	−0·7	−0·3	+1·3	+0·8	
20 — „ 20 30	+2·3	+0·2	+2·1	−3·3	
20 30 „ 21 —	+0·7	−0·3	+1·7	+3·5	
21 — „ 21 30	−1·6	+1·2	+0·4	+0·8	
21 30 „ 22 —	−0·1	+1·1	+1·6	+1·0	
22 — „ 22 30	+0·7	+0·8	+2·3	+0·3	
22 30 „ 23 —	+1·8	−0·4	−0·9	−1·0	Cumuli, occasional glimpses of sunshine.
23 — „ 23 30	−0·8	−0·2	+0·6	−0·2	
23 30 „ 24 —	−0·5	+0·2	+0·8	+0·4	
Sept. 4					
0 — „ 0 30	−0·3	±0·0	+0·2	±0·0	Sept. 4 clear.
0 30 „ 1 —	−0·2	±0·0	+0·3	±0·0	
1 — „ 1 30	±0·0	+0·1	+0·8	+0·2	
1 30 „ 2 —	±0·0	−0·1	±0·0	−0·1	
2 — „ 3 —‡	±0·0	+0·4	+0·7	+0·4	
3 — „ 4 —	−0·2	+0·3	+0·2	+0·1	
4 — „ 5 —	……	……	−0·2	±0·0	
5 — „ 6 —	……	……	−0·2	±0·0	
6 — „ 18 —	+2·1	−2·2	−6·9	+0·3	
18 — „ 18 30§	+0·6	+0·2	−0·3	+0·1	
18 30 „ 19 —	+0·8	−0·3	−0·5	−0·1	
19 — „ 19 30	+0·9	−0·1	+3·3	+0·7	
19 30 „ 20 —	+0·6	−0·2	−2·5	+0·5	
20 — „ 20 30	+0·4	+0·4	+0·9	+0·5	
20 30 „ 21 —	+0·2	+0·8	+0·6	+0·2	
21 — „ 21 30	+0·1	+0·3	+0·8	+0·1	
21 30 „ 22 —	−0·1	+0·4	+1·1	+0·7	
22 — „ 22 30	−0·3	+1·3	+0·6	+0·4	
22 30 „ 23 —	−0·2	−0·2	+0·8	+0·2	
23 — „ 23 30	−0·6	−0·1	+0·4	−0·3	
23 30 „ 24 —	±0·0	+0·2	+0·6	−0·2	

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* Shortly before 4 h. the scales enter the shadow of the mountains; about 4 h. 30 m. the entire glacier lies in the shadow.

† Shortly before 19 h. the irradiation of the glacier by the sun commences.

‡ Shortly before 4 h. the scales enter the shadow of the mountains; about 4 h. 30 m. the whole of the glacier lies in the shadow.

§ Shortly before 19 h. the irradiation of the glacier by the sun commences.

Let us first consider the motion of scale I. (distant about 35 metres from the margin of the glacier). During the afternoon the point moves but little, horizontally as well as vertically, and that in the positive direction. In the later hours P.M. this motion constantly diminishes, and at last entirely ceases. After this, during the night (6^h–18^h), the point moved vertically upwards, and horizontally towards the valley. As soon as the sun's irradiation commences (about 19^h), absolutely irregular motions take place, which last till noon; then (from 0^h onward) again commence the slow downward movements, and all is repeated in like manner.

With Scale II. (more towards the middle of the glacier) the course of the movements is similar. During the afternoon a tolerably uniform motion takes place both horizontally and vertically, in the positive direction; at about 4^h the glacier comes to a stand. But in the night, in the horizontal direction a considerable retrograde movement came in (the opposite of what took place with Scale I.), possibly amounting to 9·9 centims., while the vertical motion almost always remained but feeble. Then, as in the case of Scale I., with the commencement of the solar irradiation begin the irregular movements, which last on till midday, and thereupon pass over again into the regular downward motion.

Even at a point situated about 1 kilometre further up the glacier similar movements of the ice were ascertained. Briefly, at all points observed by us the maximum of the variations of the motions falls in the forenoon, commencing with the irradiation of the glacier by the sun. Thus, everywhere (at least on the west side of the glacier) the sum of the motions during the night was a going back toward the mountain.

The observations of the 4th of September, finally, show that with a less intense irradiation the motions of the glacier become correspondingly less considerable. The motion of the glacier was similar on the other days of observation.

From the observations which have at present been made it is not yet possible to perceive which of the more or less probable hypotheses would explain the actual motion of the ice. We intend to continue these labours on a larger scale at the commencement of the coming summer.

Freiburg im Breisgau, Sept. 17, 1879.

XXXVI. *Acoustical Observations.* III. *By* Lord Rayleigh, *F.R.S., Professor of Experimental Physics in the University of Cambridge*[*].

Intermittent Sounds.

IN the Philosophical Magazine for May 1875, Prof. A. M. Mayer describes some experiments on this subject, made by rotating a perforated cardboard disk between a resonator and a vibrating fork. "When the disk is stationary with one of its openings opposite the mouth of the resonator, it is evident that the ear will experience a simple sonorous sensation when a tuning-fork is brought near the mouth of the resonator. On revolving the perforated disk, two additional or secondary sounds appear—one slightly above, the other slightly below the pitch of the fork. An increasing velocity of rotation causes the two secondary sounds to diverge yet further from the note of the beating fork, until, on reaching a certain velocity, the two secondary sounds become separated from each other by a major sixth, while at the same moment a resultant sound appears, formed by the union of the sound of the fork with the upper and lower of the secondary sounds. This resultant is the lower second octave of the note given by the fork. On further increasing the velocity of rotation of the disk, the two secondary sounds and the resultant disappear, and the ear experiences only the sensation of the simple sound produced by the fork, whose beats at this stage of the experiment have blended into a smooth continuous sensation."

In concluding his paper Prof. Mayer calls attention "to the evident difference existing between the dynamic constitution of the sonorous waves belonging to beating pulses produced by the action of a perforated rotating disk on a continuous stream of sonorous vibrations, and those waves which cause beats, and which are formed by the joint action of sonorous vibrations differing in pitch. That these two kinds of beats are alike in their effects when following in the same rapidity I have assumed to be the fact in this paper."

At the time when Prof. Mayer's paper first appeared, I examined this question more closely; and some of my results were referred to in a discussion before the Musical Association. The difference between the two kinds of beats is considerable. If there are two vibrations of equal amplitude and slightly differing frequencies, represented by $\cos 2\pi n_1 t$ and $\cos 2\pi n_2 t$, the resultant may be expressed by

$$2 \cos \pi (n_1 - n_2)t \quad \cos \pi (n_1 + n_2)t,$$

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[*] Communicated by the Author.

and may be regarded as a vibration of frequency $\frac{1}{2}(n_1+n_2)$, and of amplitude $2\cos\pi(n_1-n_2)t$. Hence, in passing through zero, the amplitude changes sign, which is equivalent to a *change of phase of* 180°, if the amplitude be regarded as always positive. This change of phase is readily detected by measurement in drawings traced by machines for compounding observations. If a force of the above character act upon a system whose natural frequency is $\frac{1}{2}(n_1+n_2)$, the effect produced is comparatively small. If the system start from rest, the successive impulses cooperate at first, but after a time the later impulses begin to destroy the effect of former ones. The greatest response is given to forces of frequency n_1 and n_2, and not to a force of frequency $\frac{1}{2}(n_1+n_2)$.

On the other hand, when a single vibration is rendered intermittent by the periodic interposition of an obstacle, there is no such change of phase in consecutive revivals. If a force of this character act upon an isochronous system, the effect is indeed less than if there were no intermittence; but as all the impulses operate in the same sense without any antagonism, the response is powerful. An intermittent vibration or force may be represented by

$$2(1+\cos 2\pi mt)\cos 2\pi nt,$$

in which n is the frequency of the vibration, and m the frequency of intermittence. The amplitude is always positive, and varies between the values 0 and 4. By ordinary trigonometrical transformation the above expression may be put into the form

$$2\cos 2\pi nt+\cos 2\pi(n+m)t+\cos 2\pi(n-m)t;$$

which shows that the intermittent vibration is equivalent to *three* simple vibrations of frequencies n, $n+m$, and $n-m$. This is the explanation of the secondary sounds observed by Mayer. When m is equal to $\frac{1}{4}n$, $n+m:n-m=5:3$, or the interval between the secondary sounds is a major sixth. The frequency of the resultant sound is m (that is, $\frac{1}{4}n$): and its pitch is two octaves below that of the original vibration.

In the Edinburgh Proceedings for June 1878, an experiment similar to Mayer's is described by Professors Crum Brown and Tait, and is explained in the above manner.

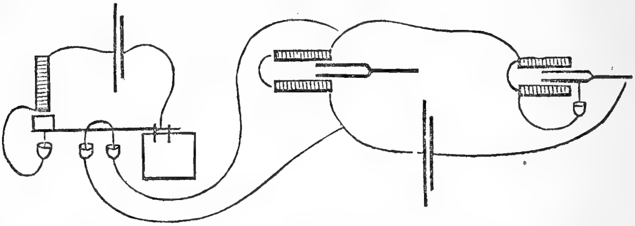
If the intensity of the intermittent sound rise more suddenly to its maximum, we may take $4\cos^4\pi mt\cos 2\pi nt$; and this may be transformed into

$$\tfrac{3}{2}\cos 2\pi nt+\cos 2\pi(n+m)t+\cos 2\pi(n-m)t$$
$$+\tfrac{1}{4}\cos 2\pi(n+2m)t+\tfrac{1}{4}\cos 2\pi(n-2m)t.$$

There are now *four* secondary sounds, the frequencies of the two new ones differing twice as much as before from that of the primary sound.

Other cases might be treated in the same way; but my object at present is to describe a modified form of the experiment which I planned some years ago, but first carried out last summer.

Fig. 1.



The desired forces were obtained electromagnetically. A fork interrupter of frequency 128 gave a periodic current, by the passage of which through an electromagnet a second fork of like pitch could be excited. The action of this current on the second fork could be rendered intermittent by short-circuiting the electromagnet. This was effected by another interrupter of frequency 4, worked by an *independent* current from a Smee cell. To excite the main current a Grove cell was employed. When the contact of the second interrupter was permanently broken, so that the main current passed continuously through the electromagnet, the fork was, of course, most powerfully affected when tuned to 128. Scarcely any response was observable when the pitch was changed to 124 or 132. But if the second interrupter was allowed to operate, so as to render the periodic current through the electromagnet intermittent, then the fork would respond powerfully when tuned to 124 or 132 as well as when tuned to 128, but not when tuned to intermediate pitches, such as 126 or 130.

The operation of the intermittence in producing a sensitiveness which would not otherwise exist, is easily understood. When a fork of frequency 124 starts from rest under the influence of a force of frequency 128, the impulses cooperate at first, but after $\frac{1}{8}$ of a second the new impulses begin to oppose the earlier ones. After $\frac{1}{4}$ of a second, another series of impulses begins whose effect agrees with that of the first, and so on. Thus if all these impulses are allowed to act, the resultant effect is trifling; but if every alternate series is stopped off, a large vibration accumulates.

A new form of Siren.

Some years ago[*], I observed that a light pivoted blade is

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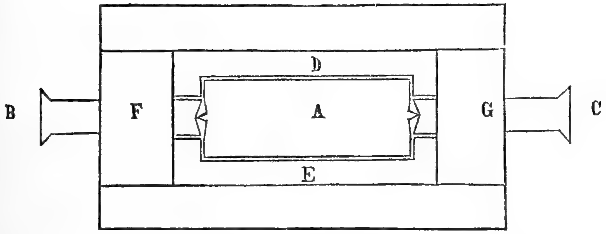
[*] "On the Resistance of Fluids," Phil. Mag. Dec. 1876.

set into rapid rotation when exposed to wind. The phenomenon is of the same character as the rotation of a slip of paper falling freely in air, which was discussed a long while since by Prof. Maxwell*. In both cases the rotation may occur in *either direction*, proving that its cause is not to be looked for in any want of symmetry. But the view expressed by Maxwell does not appear to apply to the pivoted blade; and I think that the real explanation is yet to be discovered. At present, however, I am concerned merely with an application.

Fig. 2.



In fig. 2, A is a blade cut out of sheet brass and provided with sharp projecting points, which bear in hollows at the ends of the screws B, C. These screws pass through a small wooden frame F G, and are adjusted until the blade can turn freely but without perceptible shake. D and E are pieces of cardboard or sheet metal, fitting pretty closely to the blade when in the same plane with them, so that in this position of the blade the passage through the frame is almost closed. As the blade turns, it acts the part of a revolving stopcock.

In the summer of 1878 I made several sirens on this plan, which performed well. One of them is represented about full size in the figure. If the wind from the bellows is admitted symmetrically, they will revolve in either direction, and soon acquire sufficient speed to give a note of moderate pitch. The position of maximum obstruction is for small displacements a position of stable equilibrium. If a larger displacement is given, the vibration tends of itself to increase up to a certain point, or even to pass into continuous rotation; but the precise behaviour in this respect probably depends upon the details of construction.

The Acoustical Shadow of a Circular Disk.

In a well-known experiment, suggested by Poisson, a bright point is observed in the centre of the shadow of a circular disk on which waves of light are directly incident. It is some years since I first attempted to obtain the acoustical analogue

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* Cambridge and Dublin Math. Journal, 1854.

of this beautiful phenomenon; but my efforts were without success until a few months since. The difficulties to be overcome are entirely different in optics and in acoustics, on account of the immense disproportion of wave-lengths. In the former case the disk must be small and accurately shaped, and the source of light must be of very small angular magnitude—in practice, an image of the sun formed by a distant lens of short focus. In the latter case the difficulty is to arrange the experiment on a scale that shall be adequate in comparison with the wave-length of the sound.

The best way of considering the subject theoretically is with the use of Huyghens's zones. The plane of the disk is divided into zones by its intersection with spheres whose centres are at the point under consideration, and whose radii form an arithmetical progression with common difference $\frac{1}{2}\lambda$. The vibrations due to these zones are at first nearly equal, but gradually diminish to nothing, unless the outer boundary of the aperture is circular; and thus the aggregate effect is represented by a series of which the terms are of opposite sign and of slowly diminishing magnitude. Now the sum of such a series is equal approximately to half its first term; so that the whole effect of the aperture outside the disk is independent of the disk's diameter—that is to say, is the same as if no obstacle at all were present.

This way of regarding the matter shows at once what degree of accuracy is required in the figure of the circumference, which must not sensibly encroach upon the first exterior zone. If x be the radius of a circle in the plane of the disk, b the distance of the point under consideration, and $r^2=x^2+b^2$, $dx=rdr \mid x$; so that if $dr=\frac{1}{2}\lambda$, $dx=r\lambda \mid 2x$. If, therefore, x be the radius of the disk, the radial curve should be a small fraction of $r\lambda \mid 2x$.

In like manner, we may form an estimate of the *size* of the bright spot, a subject which has been treated analytically by Airy*. If the disk be moved laterally through the width of one zone, it is clear that the effect at the old point will be materially changed. Hence the diameter of the bright spot is comparable with $r\lambda \mid x$, and its apparent magnitude as seen from the disk is comparable with $\lambda \mid x$. For the full success of the experiment, the apparent magnitude of the luminous source should be of the same order.

When we pass to the analogous experiment in acoustics, it is of course impossible to retain any approximation to optical conditions. Instead of a ratio of $\lambda : x$, equal to, say, $\frac{1}{10000}$, we must be satisfied with some such value as $\frac{1}{20}$. In order to

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* Phil. Mag. May 1841.

diminish λ as far as possible, it is advisable to use sounds of very high pitch, which have the additional advantage of readily exciting sensitive flames. I have found it best to work indoors, in which case a disk of 15 inches diameter is suitable; with a much larger disk and in an ordinary room there would hardly be sufficient free space on all sides. I have tried a considerable variety of sources of sound, including electric sparks, a small electric bell, and a Galton's whistle; but the best results were obtained with a bird-call and with a squeaky toy-reed.

On November 20 the source was a bird-call blown with a pressure of 4″ water, and was placed about 20″ from the 15″ disk. The observation was made at a distance of 24″ on the further side of the disk, and was successful both with the ear and with the sensitive flame. In the former case I employed a plate of wood bored with a hole about $\frac{1}{4}''$ in diameter, and held against the side of the head in such a position that the hole was opposite the ear-passage. The head was moved about until the position of maximum sound was determined, and was then withdrawn, leaving the plate *in situ*. In order to verify conveniently that the position of maximum sound was really at the centre of the shadow, a hole was bored through the centre of the disk, which could be closed with a cork during the adjustment of the ear-plate. When the adjustment was complete, the cork was removed; and then the eye placed behind the ear-plate would see the source of sound through the two holes. With a little practice the central point could be picked out almost as well by ear as by eye.

With a sensitive flame the observation was even easier. The most suitable is that from a pin-hole burner brought near the flaring-point by a gas-pressure of about 10″ of water. To get the best result, the pressure must be carefully adjusted; and in order to avoid disturbance, it is advisable to move the source rather than the flame. When the place of maximum effect has been determined, the cork is removed from the central hole of the disk, and the gas is lowered. By looking just over the burner it is then easy to see whether or not the source of sound occupies the central position.

On November 24 the toy reed was substituted for the bird-call, the disk and distances being the same as before. In the case of this source the experiment succeeded better with the flame than with the ear.

On a subsequent occasion a larger disk, of 30″ diameter, was tried; but the results were not so good, probably in consequence of the increased effect of reflection from the floor and walls of the room.

XXXVII. *On* Maxwell's *Theory of Light*. *By* J. J. Thomson, *B.A., Scholar of Trinity College, Cambridge*[*].

THE theory that light is an electromagnetic disturbance propagated in the medium which transmits ordinary electromagnetic action, and the supposition on which the theory depends (viz. that the variation of the electric displacement in a dielectric produces the same electromagnetic effect as an ordinary current), seem first to have been stated by Maxwell, in his paper on the "Electromagnetic Field" (Phil. Trans. 1848). In the 'Treatise on Electricity and Magnetism' two chapters are devoted to the theory, one of them being entirely on the rotation of the plane of polarization. These appear to constitute the literature of the subject. The object of the present paper is, by taking into account the motion of the medium through which light is passing, to obtain equations a little more general than those used by Prof. Maxwell, and to develop some of the consequences of the theory.

Let (F, G, H), (a, b, c), (P, Q, R) be the x, y, z components of the vector potential of electric induction, of magnetic induction, and of electromotive force respectively at the point (x, y, z), in a medium whose specific inductive capacity is K, and whose coefficient of magnetic induction is μ; then (Maxwell's 'Electricity and Magnetism,' vol. ii. § 598)

$$\mathrm{P}=c\frac{dy}{dt}-b\frac{dz}{dt}-\frac{d\mathrm{F}}{dt}-\frac{d\psi}{dx},$$

$$\mathrm{Q}=a\frac{dz}{dt}-c\frac{dx}{dt}-\frac{d\mathrm{G}}{dt}-\frac{d\psi}{dy},$$

$$\mathrm{R}=b\frac{dx}{dt}-a\frac{dy}{dt}-\frac{d\mathrm{H}}{dt}-\frac{d\psi}{dz};$$

$$a=\frac{d\mathrm{H}}{dy}-\frac{d\mathrm{G}}{dz},$$

$$b=\frac{d\mathrm{F}}{dz}-\frac{d\mathrm{H}}{dx},$$

$$c=\frac{d\mathrm{G}}{dx}-\frac{d\mathrm{F}}{dy};$$

$$\frac{da}{dx}+\frac{db}{dy}+\frac{dc}{dz}=0.$$

Now the electromotive force (P, Q, R) acting on the dielectric will polarize it. Let ϕ be the potential due to the polariza-

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[*] Communicated by the Author.

tion of the dielectric; let f, g, h be the electric displacements parallel to the axes of x, y, z respectively; then

$$f=\frac{K}{4\pi}\left\{P-\frac{d\phi}{dx}\right\}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

$$g=\frac{K}{4\pi}\left\{Q-\frac{d\phi}{dy}\right\}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

$$h=\frac{K}{4\pi}\left\{R-\frac{d\phi}{dz}\right\}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

and

$$\frac{df}{dx}+\frac{dg}{dy}+\frac{dh}{dz}=0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

where there is no free electricity. Differentiating (1) with respect to y, (2) with respect to x, and subtracting, writing p, q, r, θ for $\frac{dx}{dt}, \frac{dy}{dt}, \frac{dz}{dt}, \frac{dp}{dx}+\frac{dq}{dy}+\frac{dr}{dz}$ respectively, we get

$$\frac{4\pi}{K}\left(\frac{df}{dy}-\frac{dg}{dx}\right)=p\frac{dc}{dx}+q\frac{dc}{dy}+r\frac{dc}{dz}+c\theta-a\frac{dr}{dx}-b\frac{dr}{dy}$$
$$-c\frac{dr}{dz}+\frac{dc}{dt}. \quad . \quad . \quad . \quad (5)$$

Similarly,

$$\frac{4\pi}{K}\left(\frac{dh}{dx}-\frac{df}{dz}\right)=p\frac{db}{dx}+q\frac{db}{dy}+r\frac{db}{dz}+b\theta-a\frac{dq}{dx}-b\frac{dq}{dy}$$
$$-c\frac{dq}{dz}+\frac{db}{dt}, \quad . \quad . \quad . \quad (6)$$

$$\frac{4\pi}{K}\left(\frac{dg}{dz}-\frac{dh}{dy}\right)=p\frac{da}{dx}+q\frac{da}{dy}+r\frac{da}{dz}+a\theta-a\frac{dp}{dx}-b\frac{dp}{dy}$$
$$-c\frac{dp}{dz}+\frac{da}{dt}. \quad . \quad . \quad . \quad (7)$$

If u, v, w be the x, y, z components of the electric current respectively,

$$4\pi\mu u=\frac{dc}{dy}-\frac{db}{dz},$$

$$4\pi\mu v=\frac{da}{dz}-\frac{dc}{dx},$$

$$4\pi\mu w=\frac{db}{dx}-\frac{da}{dy},$$

$$u=\frac{df}{dt}+\frac{\mathrm{P}}{\rho},$$

$$v=\frac{dg}{dt}+\frac{\mathrm{Q}}{\rho},$$

$$w=\frac{dh}{dt}+\frac{\mathrm{R}}{\rho},$$

where ρ is the resistance of the medium.

We shall suppose ρ to be infinite, so that

$$u=\frac{df}{dt}, \quad v=\frac{dg}{dt}, \quad w=\frac{dh}{dt}.$$

Differentiating (5) with respect to y, (6) with respect to z, and subtracting, we get, remembering

$$\frac{df}{dx}+\frac{dg}{dy}+\frac{dh}{dz}=0,$$

$$\begin{aligned}\frac{4\pi}{\mathrm{K}}\left(\frac{d^2f}{dx^2}+\frac{d^2f}{dy^2}+\frac{d^2f}{dz^2}\right)\\ =4\pi\mu\left(\frac{d^2f}{dt^2}+\left(p\frac{d}{dx}+q\frac{d}{dy}+r\frac{d}{dz}\right)\frac{df}{dt}+\theta\frac{df}{dt}\right)\\ -\left(a\frac{d}{dx}+b\frac{d}{dy}+c\frac{d}{dz}\right)\left(\frac{dr}{dy}-\frac{dq}{dy}\right)+c\frac{d\theta}{dy}-b\frac{d\theta}{dz}\\ +\left(\frac{dq}{dy}-\frac{dr}{dz}\right)\left(\frac{dc}{dy}+\frac{db}{dz}\right)+\left(\frac{dr}{dy}+\frac{dq}{dz}\right)\left(\frac{dc}{dz}-\frac{db}{dy}\right)\\ +\frac{dp}{dy}\frac{dc}{dx}-\frac{dr}{dx}\frac{da}{dy}+\frac{da}{dz}\frac{dq}{dx}-\frac{dp}{dz}\frac{db}{dx}, \quad . \quad . \quad (8)\end{aligned}$$

with corresponding equations for g and h.

Again, differentiating (8) with respect to y, (9) with respect to x, subtracting, and remembering

$$\frac{da}{dx}+\frac{db}{dy}+\frac{dc}{dz}=0,$$

we get

$$4\pi\mu\left\{\frac{du}{dy}-\frac{dv}{dx}\right\}=\frac{d^2c}{dx^2}+\frac{d^2c}{dy^2}+\frac{d^2c}{dz^2};$$

but

$$u=\frac{df}{dt}, \quad v=\frac{dg}{dt},$$

$$\therefore\ 4\pi\mu\frac{d}{dt}\left\{\frac{df}{dy}-\frac{dg}{dx}\right\}=\frac{d^2c}{dx^2}+\frac{d^2c}{dy^2}+\frac{d^2c}{dz^2},$$

or, by equation (5),

$$\mu\mathrm{K}\frac{d^2c}{dt^2}+\frac{d}{dt}\left(p\frac{d}{dx}+q\frac{d}{dy}+r\frac{d}{dz}\right)c-\frac{d}{dt}\left(a\frac{dr}{dx}+b\frac{dr}{dy}+c\frac{dr}{dz}\right)$$

$$=\frac{d^2c}{dx^2}+\frac{d^2c}{dy^2}+\frac{d^2c}{dz^2}, \quad . \quad . \quad . \qquad (9)$$

with similar equations for a and b.

So long as the dielectric moves as a rigid body with uniform velocity and without rotation, the equations for a, b, c are exactly the same as those for f, g, h, and are of the type

$$\frac{1}{\mu\mathrm{K}}\left(\frac{d^2f}{dx^2}+\frac{d^2f}{dy^2}+\frac{d^2f}{dz^2}\right)=\frac{d^2f}{dt^2}+\left(p\frac{d}{dx}+q\frac{d}{dy}+r\frac{d}{dz}\right)\frac{df}{dt}. \qquad (10)$$

The boundary conditions at the surface of separation of two media are:—

1. That both the electric and magnetic displacement resolved along the normal to the surface of separation should be the same in both media.

2. That both the electric and magnetic forces resolved parallel to the surface of separation should be the same on both media.

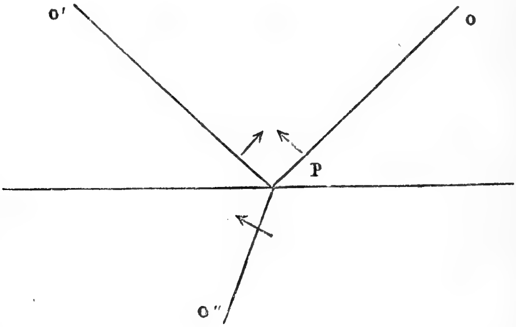
If the medium is at rest, the equations reduce to the form

$$\frac{1}{\mu\mathrm{K}}\left(\frac{d^2f}{dx^2}+\frac{d^2f}{dy^2}+\frac{d^2f}{dz^2}\right)=\frac{d^2f}{dt^2},$$

which corresponds to a wave-motion propagated with a velocity $\frac{1}{\sqrt{\mu\mathrm{K}}}$. In the media we are concerned with, μ differs from unity only by a very small fraction; so that the velocity of propagation is very nearly $\frac{1}{\sqrt{\mathrm{K}}}$. Now, if the specific inductive capacity of a dielectric depended upon the number of times in a second the electric displacement was reversed, the velocity of the different coloured rays would be different; *i. e.* there would be dispersion. There seems to be evidence that the specific inductive capacity does depend on the rate at which the electric displacements are reversed; for Schiller and Gordon, reversing the displacements about 2000 times in a second, found the specific inductive capacity of glass was much smaller than when it was found in the usual way (Electrical Researches of the Hon. H. Cavendish, Appendix). To explain dispersion, the specific inductive capacity ought to increase with the rapidity of reversal; and as in light the displacement is reversed billions of times in a second, it is no argument against its doing so that the specific inductive capacity is less for 2000 reversals a second than when the displacement is not reversed. It would be interesting, however, to see whether, with the rate of reversal obtainable in a laboratory, the specific inductive capacity altered more quickly with the rate of reversal the greater the refractive index of the medium.

The boundary conditions will enable us to determine the intensities of the refracted and reflected rays produced by a ray of plane-polarized light incident on a reflecting surface. According to Maxwell's theory, the electric displacement is perpendicular to the plane of polarization, the magnetic displacement is in the plane of polarization, and both these displacements are perpendicular to the direction of propagation of the ray.

Let us calculate, first, the intensities for the reflected and refracted ray for light polarized perpendicular to the plane of incidence.



Let

$$a_1 \sin\frac{2\pi}{\lambda}(vt-e), \quad a_2 \sin\frac{2\pi}{\lambda}(vt-e), \quad a_3 \sin\frac{2\pi}{\lambda'}(v't-e)$$

be the electric displacements perpendicular to the incident ray O P, the reflected ray P O′, and the refracted ray P O″, respectively; then since we suppose p, q, r all zero, by equation (5) the magnetic displacements perpendicular to the plane of incidence will be respectively

$$-\frac{4\pi}{K_1 v'} a_1 \sin\frac{2\pi}{\lambda}(vt-e), \quad -\frac{4\pi}{K_1 v} a_2 \sin\frac{2\pi}{\lambda}(vt-e),$$

$$-\frac{4\pi}{K_2 v} a_3 \sin\frac{2\pi}{\lambda'}(v't-e)$$

for the incident, reflected, and refracted rays, where K_1, K_2 are the specific inductive capacities of the first and second media respectively. Let i be the angle of incidence, r the angle of refraction; and let μ_1, μ_2 be the coefficients of magnetic induction in the two media.

The electric displacement perpendicular to the surface of separation must be the same in both media;

$$\therefore\ (a_1+a_2)\sin\iota=a_3\sin r$$

(it should be noticed that this makes the magnetic force parallel to the surface of separation the same in both media); and the electromotive force along the surface is the same in both media. Now electromotive force $=\frac{4\pi}{\mathrm{K}}$ (electric displacement);

$$\therefore\ (a_1-a_2)\frac{\cos\iota}{\mathrm{K}_1}=\frac{a_3}{\mathrm{K}_2}\cos r;$$

$$\therefore\ \frac{a_1+a_2}{a_1-a_2}=\frac{\mathrm{K}_2\tan r}{\mathrm{K}_1\tan\iota};$$

$$\therefore\ a_2=a_1\frac{(\mathrm{K}_2\tan r-\mathrm{K}_1\tan\iota)}{\mathrm{K}_2\tan r+\mathrm{K}_1\tan\iota}$$

$$a_3=\frac{2\mathrm{K}_2a_1\sin\iota}{(\mathrm{K}_2\tan r+\mathrm{K}_1\tan\iota)\cos r}.$$

Now the energy is half electric and half magnetic, the energy due to an electric displacement $f=\frac{1}{2}\frac{f^2}{\mathrm{K}}$; therefore if α_1^2, α_2^2, α_3^2 be the intensities of the incident, reflected, and refracted rays respectively; since α_1^2, α_2^2, α_3^2 are respectively proportional to the energy in unit volume along these rays,

$$\alpha_1:\alpha_2:\alpha_3=\frac{a_1}{\sqrt{\mathrm{K}_1}}:\frac{a_2}{\sqrt{\mathrm{K}_1}}:\frac{a_3}{\sqrt{\mathrm{K}_2}};$$

$$\therefore\ \alpha_2=\alpha_1\frac{(\mathrm{K}_2\tan r-\mathrm{K}_1\tan\iota)}{\mathrm{K}_2\tan r+\mathrm{K}_1\tan\iota},$$

$$\alpha_3=2\alpha_1\frac{\sqrt{(\mathrm{K}_1\mathrm{K}_2)}\sin\iota}{\{\mathrm{K}_2\tan r+\mathrm{K}_1\tan\iota\}\cos r}.$$

Now

$$\frac{\sin\iota}{\sin r}=\frac{\sqrt{\mathrm{K}_2\mu_2}}{\sqrt{\mathrm{K}_1\mu_1}};$$

and if, as is very nearly the case for transparent dielectrics $\mu_1=\mu_2$, we may put

$$\frac{\sin\iota}{\sin r}=\sqrt{\frac{\mathrm{K}_2}{\mathrm{K}_1}},$$

substituting for $\frac{K_2}{K_1}$, we get

$$\alpha_2 = \frac{\alpha_1 \tan(\iota - r)}{\tan(\iota + r)},$$

$$\alpha_3 = \frac{\alpha_1 \sin 2\iota}{\sin(\iota + r)\cos(\iota - r)}.$$

For light polarized in the plane of incidence, if we consider magnetic disturbances instead of electrical, the formulæ for the intensities will be got by exactly the same process as before; hence writing μ_1, μ_2 in the place of K_1, K_2, and using the same notation as before

$$\alpha'_2 = \alpha'_1 \frac{(\mu_2 \tan r - \mu_1 \tan \iota)}{\mu_2 \tan r + \mu_1 \tan \iota},$$

$$\alpha'_3 = \alpha'_1 \frac{2\sqrt{\mu_1\mu_2}\sin \iota}{(\mu_2 \tan r + \mu_1 \tan \iota)\cos r}.$$

If $\mu_1 = \mu_2$, we get

$$\alpha'_2 = -\alpha'_1 \frac{\sin(\iota - r)}{\sin(\iota + r)},$$

$$\alpha'_3 = \alpha'_1 \frac{\sin 2\iota}{\sin(\iota + r)}.$$

The first is the same as that given by Fresnel; but the second is not quite the same: Fresnel's formula is

$$\alpha'_3 = \alpha'_1 \frac{2 \sin \iota \cos r}{\sin(\iota + r)}.$$

According to Maxwell's theory, the refracted ray is more intense in the ratio $\frac{\cos \iota}{\cos r}$[*].

Equation (10) will enable us to find the effect produced by the motion of the dielectric on the velocity of light. To take the simplest case, let us suppose the dielectric moving with velocity u in the direction of propagation of the light, which we shall take as the axis of x.

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[*] Since the above was written, I have found that the problem of the reflexion and refraction of light, according to Maxwell's theory, has been very fully treated by Lorentz, in Schlömilch, *Zeitschrift*, vol. xxii.

Let V be the velocity of propagation of light in the dielectric when at rest. The equation

$$V^2\left(\frac{d^2f}{dx^2}+\frac{d^2f}{dy^2}+\frac{d^2f}{dz^2}\right)=\frac{d^2f}{dt^2}+\left(p\frac{d}{dx}+q\frac{d}{dy}+r\frac{d}{dz}\right)\frac{df}{dt}$$

reduces in this case to

$$V^2\frac{d^2f}{dx^2}=\frac{d^2f}{dt^2}+u\frac{d^2f}{dxdt}.$$

Let the solution of this equation be $f=A\cos(qt-px)$; substituting in the equation, we get

$$V^2p^2=q^2-upq;$$

$$\therefore\ \left(\frac{q}{p}\right)^2-u\frac{q}{p}=V^2;$$

$$\therefore\ \frac{q}{p}=\frac{u}{2}\pm\sqrt{V^2-\frac{u^2}{4}}.$$

Taking the upper sign (corresponding to a wave propagated in the positive direction of x), we have

$$\frac{q}{p}=\frac{u}{2}+\sqrt{\left(V^2-\frac{u^2}{4}\right)}$$

$$=\frac{u}{2}+V\text{, approximately.}$$

Now $\frac{q}{p}$ is the velocity of propagation of light in the medium; hence we see that the velocity of the light is increased by one half the velocity of the dielectric. This result is confirmed in a very remarkable way by some experiments made by Fizeau, and of which an account is published in the *Comptes Rendus*, t. xxxiii. He found the difference in the velocity of light passing through a tube filled with water when the water in the tube was still and when it was moving; he was able to do this very accurately by measuring the displacement produced in some interference-fringes; and he found that the velocity of light was increased by half the velocity of the water.

When the dielectric does not move uniformly as a rigid body without rotation, the differential equations to find the electric and magnetic displacements are no longer the same.

XXXVIII. *Determination of the Acceleration of Gravity for Tokio, Japan. By* W. E. AYRTON *and* JOHN PERRY[*].

As no experiments had, as far as we were aware, been made to determine the value of g in Japan, it appeared to us desirable that the value should be accurately measured, at any rate for our own college in the capital, Tokio. Consequently in 1877 an elaborate series of experiments was carried out by some of the students under our supervision. The method first employed consisted in experimentally finding two parallel axes in a pendulum on opposite sides of the centre of gravity, and in a plane with it, such that the times of oscillation about either axis would be the same. The distance, then, between these axes experimentally found would, as is well known, be the length of the equivalent simple pendulum, from which g could be calculated by the formula

$$t=\pi\sqrt{\frac{l}{g}}.$$

Two Kater's pendulums were employed—one made by Messrs. Elliott, and the other by Messrs. Negretti and Zambra. Borda's method of coincidence was employed; that is, one of the Kater's pendulums was suspended exactly in front of the pendulum of a clock, consisting of a wooden rod carrying a brass bob and beating approximately seconds. One observer watched the two pendulums, vibrating one in front of the other, through a telescope some ten feet away; and at the instant a pointer attached to one of the pendulums exactly coincided with a line drawn on the other (the coincidence taking place in the axis of the telescope as observed by the cross-wires), a signal was given, and the time of the clock noted by two independent observers. A very large number of successive coincidences was in this way observed, then the Kater's pendulum inverted, swung on the other knife-edge, and the same thing repeated. From these experiments the exact time of vibration of the Kater's pendulum about either knife-edge was ascertained, and one or both knife-edges moved to diminish the difference in the time of vibration, and the whole experiment repeated.

But although these observations, first with one of the Kater's pendulums and then with the other, were continued for some months, many thousands of vibrations being observed by the students, who worked at these experiments in their usual most praiseworthy way, the results were always unsatisfactory. For an approximate value of g for any part of the

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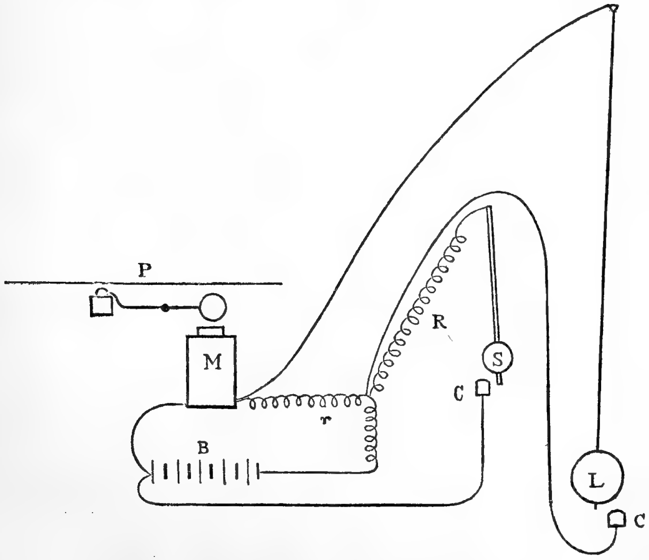
[*] Communicated by the Physical Society.

earth's surface can be calculated by a formula developed by Clairault, who from pendulum-experiments made at a variety of latitudes on the earth's surface has shown that, approximately, for any latitude λ and any height h centimetres above the level of the sea,

$$g = 980{\cdot}6056 - 2{\cdot}5028 \cos\lambda - 0{\cdot}000003\,h.$$

As the latitude of the Imperial College of Engineering, Tokio, is about 35° 39′, Clairault's formula gives for g the value 979·7 centimetres per second per second. But the value obtained by the series of experiments made with the Kater's pendulums differed too much from this and from one another to allow of our trusting them.

We therefore decided on employing a totally different method and a much less laborious one:—A brass ball 2352·2 grammes in weight was suspended by a long steel wire 0·45 millimetre in thickness, and in the earlier experiments 978·7 centimetres in length. The wire was supported from a steel knife-edge resting on a brass plate. Both the brass ball and the bob of the seconds-pendulum of the standard clock were fitted with fine pieces of platinum wire, either of which dipped into a small cup of mercury when the pendulum to which it was attached was vertical. The mercury-cups &c. were then joined up with a battery and resistance-coils to a quick-running Morse instrument, as seen in the figure, in which M is



the Morse instrument, B the battery, P the paper, S the seconds-pendulum, L the long pendulum, C, C the mercury-cups, R a resistance small compared with that of M plus that of the coil r, but large compared with that of M alone. The whole constituted what is known as a "break-circuit chronograph;" that is, a continuous ink mark was made on the paper run out by clockwork, broken by a very small gap each time the wire attached to the bob of the seconds-pendulum passed through the mercury. These breaks, then, in the ink line indicated seconds; if, however, both pendulums were simultaneously in the vertical line, no break was made. Hence the absence of a break in the line at the end of any special second indicated coincidence of the two pendulums; and in this way the times of a large number of coincidences could be automatically registered.

During this set of experiments we could not measure the length of the long fine steel wire with as much accuracy as was desired, since, although we had two or three brass scales, the makers had omitted to record on them at what temperatures they were correct. However, assuming that one of them was accurate at 0° C., then a rather large number of experiments gave, as the value of g, 978·8 centimetres per second as a first rough approximation.

Subsequently we obtained from the Finance Department of Japan the loan of two very beautiful standard brass scales, by Deleuil of Paris, and guaranteed correct at 0° C. One was graduated in millimetres; the other consisted of a brass rod with two pieces at its ends at right angles to the rod, and the distance between the two planes of the inner surfaces of the pieces was exactly a metre at 0° C. We now had, then, the means of making a far more complete series of experiments than before; but as our trial pendulum was nearly ten times as long as the seconds-pendulum of our clock, the method of coincidences was an inconvenient one; and so we merely adopted the following:—The long pendulum alone controlled the "break-circuit chronograph;" so that the number of breaks in the line during any time indicated the number of vibrations of the long pendulum in that time. At the commencement of the experiment, after the pendulum had been set swinging and the paper was running out at a fairly uniform speed, a mark was made on it by tapping sharply the armature up with the finger when a chronometer, lying beside the Morse instrument, indicated a certain time; and after an hour or so, the paper being kept running all the time, a second mark was sharply made on the paper when the chronometer indicated a certain other noted time. So much paper had then run out in the interval of time shown by the chronometer; and the breaks in the line, counted carefully afterwards by two independent students, gave the whole number of vibrations of the pendulum in that time. The fraction of a vibration could also, of course, be ascertained by comparing with the length of the lines in the neighbourhood the length of the first line made after the first break had been produced, on tapping the armature, and repeating the same process at the end of the paper. The experiment is, of course, independent of the rate at which the paper runs out, provided, of course, it is never allowed to run so slowly that there is any difficulty in distinguishing the different breaks electrically made by the long vibrating pendulum. The mean temperature of the wire was carefully taken at each experiment. A sample of one or two of the many experiments recorded in the students' laboratory notebook follows.

Number of Experiment 26.—25*th of January*, 1878.

Number of vibrations obtained from counting on the paper . . . . $1015\frac{3}{4}$
Time taken—observed on the chronometer . 0 h. 52 m. 0 s.
Mean temperature at the beginning . . . . 10°·5 C.
" " " end . . . . 11°·25 C.
Time of vibration = 3·0744 seconds.

Number of Experiment 45.—15*th of February*, 1878.

Number of vibrations obtained from counting on the paper . . . . 1385·5
Time taken—observed on the chronometer . 1 h. 11 m. 0 s.
Mean temperature at the beginning . . . . 9°·25 C.
" " " end . . . . 12°·25 C.
Time of a vibration = 3·0745 seconds.

Number of Experiment 53.—21*st of February*, 1878.

Number of vibrations obtained from counting on the paper . . . . 1288·5
Time taken—observed on the chronometer . 1 h. 6 m. 0 s.
Mean temperature at the beginning . . . . 8°·5 C.
" " " end . . . . 12°·25 C.
Time of a vibration = 3·0741 seconds.

Frequent sun-observations were made to check the rate of the chronometer, which is comparatively easy in Japan, as during the winter there the sun is seen almost daily from sunrise to sunset.

The next point was to measure accurately the length of the wire. As it was impossible to do this satisfactorily with the wire hanging up, it was taken down without disconnecting either the knife-edge carrying it or the ball at the other end. The knife-edge was then fixed at one end of a horizontal rail, and the other end of the wire close to the ball hung over a wheel with very little friction. By this arrangement the wire in a horizontal position was, of course, stretched as much as it was in the vertical position, as far as the effect of the weight of the ball was concerned. A correction had, however, to be made for the weight of the wire itself, which of course caused the tension to be a little less at the bottom than at the top when the pendulum was hanging up vertically. A few centimetres of similar fine steel wire being weighed, a simple integration gave the small additional weight necessary to be added. This being done, the final result obtained was that the length of the pendulum equalled 939·09 centimetres at 0° C.; and the consequent value of g in air for Tokio, Japan, calculated from the result of about eighty thousand vibrations of the long pendulum, would be 980·06 centimetres per second per second, if the pendulum could be regarded as a simple mathematical pendulum.

Correcting Factors.

1. The two most obvious corrections to apply to this result are the corrections for infinitely small arcs and for the air-friction—neither of which were found of any practical consequence, on account of the very small angle through which the pendulum usually swung, and that the decrement of the amplitude of the vibrations was imperceptible even after many swings. Although, however, such a pendulum as we were using approaches very nearly a perfect simple pendulum, there are certain causes of possible error arising from its flexibility and slight elasticity which would not affect a rigid compound pendulum. To estimate the practical effect of these possible errors, it is necessary to solve generally the complete problem of a heavy ball supported by an elastic wire, one end of which is soldered to the ball and the other end to a steel knife-edge. When a suspended ball is swinging in the arc of a circle, we know that near the end of a swing the attachments of the ball have to resist a tendency for the ball to turn. For since the ball has been turned in passing from its lowest to its highest position, it would continue to turn were it not stopped by the wire itself. At the end of every swing, then, there must be a slight kick; so that in fact the ball will make minor swings about its point of attachment all the time of the motion. To make this kick less perceptible, we must make the fastening of the wire to the ball capable of resisting the tendency of the ball to continue its turning motion. If we do this by soldering the wire, a smaller kick will result, and will be due to the bending-moment of the wire resisting the turning action. If there were no difficulty of construction, it might be better to get rid of this kick difficulty by making the bob capable of rotating in the plane of swinging about an axis through its centre of gravity.

The investigation of the general problem of the swinging of a heavy ball soldered to an elastic wire, the upper end of which is attached to a knife-edge, may take somewhat the following form:—Let A be a ball, of mass m and radius a, suspended from a free hinge at B by a chain of $n-1$ links, each of length a and hinged to one another, the last hinge being on the surface of the ball. Suppose at any hinge where two adjacent links make an angle θ with one another, equal and opposite couples act in them of moment $c\theta$ tending to bring them into the same straight line. As n is made greater and greater, we approximate more and more nearly to our actual case of an elastic wire. Let n be very great, and $\phi_1, \phi_2, \ldots \phi_{n-1}$ the inclinations of the 1st, 2nd, ... $(n-1)$th link to the vertical, the nth link being the radius of the ball up to the hinge, and its inclination ϕ_n. If now we know the mass of the links per unit of length, it is easy to state the values of $\psi_1, \psi_2, \ldots \psi_s, \ldots \psi_n$, the couples acting on each respective link: thus

$$\psi_n = mga \sin \phi_n + c(\phi_n - \phi_{n-1}),$$

care being taken to remember that the form of ψ_s is different from that of ψ_1 or of ψ_n.

If the inclination ϕ is everywhere very small, we find, if v_s is the velocity of the end of link s, that

$$v_s^2 = a^2 \{(\dot{\phi}_1)^2 + 2\dot{\phi}_1\dot{\phi}_s \cos(\phi_s - \phi_1) + (2s-3)(\dot{\phi}_s)^2\},$$

where $\dot{\phi}_s$ means $\frac{d\phi_s}{dt}$.

So that the kinetic energy T of the whole system may at once be written out in terms of the coordinates $\phi_1, \phi_2, \ldots$ &c., $\dot{\phi}_1, \dot{\phi}_2, \ldots$ &c.

We can therefore find the partial differential coefficients

$$\frac{d\mathrm{T}}{d\dot{\phi}_1}, \ \frac{d\mathrm{T}}{d\dot{\phi}_2}, \text{ \&c., and } \frac{d\mathrm{T}}{d\phi_1}, \ \frac{d\mathrm{T}}{d\phi_2}, \ldots \text{ \&c.,}$$

so as to use Lagrange's equation

$$\frac{d}{dt}\left(\frac{d\mathrm{T}}{d\dot{\phi}}\right) - \frac{d\mathrm{T}}{d\phi} = -\psi.$$

In this way we have obtained the n differential equations connecting $\phi_1, \phi_2, \ldots \phi_n$, and their first and second differential coefficients with respect to t. As, however, these equations can only be regarded as true when n is infinite, and as the labour of solution is very great when n is great, it seems useless proceeding further with the solution.

If we regard the motion of the ball as a harmonic motion of period P, determined by assuming the connexions as rigid, combined with motions of much shorter periods P_1, P_2, P_3, ... &c., there will be some little difficulty in finding the motion of shortest period P_1, namely the *kick* above mentioned; but we know that when the wire is, as in our experiments, very thin, the kick cannot be much less than the time of a complete vibration of the ball when freely suspended by a point on its surface, or

$$2\pi\sqrt{\frac{12a}{5g}},$$

where a is the radius of the ball. But this periodic time is 0·528 seconds, or about one twelfth of that of the pendulum moving as a whole, which is about 6 seconds.

Since the tendency of the ball to add this quick vibration to its motion is due to its rotational energy, it may be diminished by lessening the moment of inertia of the ball (that is, by making the ball small), or by diminishing the angular velocity of the pendulum (that is, by making the pendulum as long and its swing as small as possible). We may regard, then, the motion of the ball as compounded of a pure harmonic motion with an amplitude of about 30 centimetres and a periodic time of 6 seconds, with another motion having a very small amplitude and with a period of about half a second. But we have proved in the paper on our seismograph*, that in such a case the compound motion would differ very slightly from that of a pure harmonic motion, even if there were no internal friction in the substance of the wire (supposing the pendulum started without shock); but as internal friction, of course, exists in the wire, this error becomes exceedingly small.

2. Next, with regard to the stretching of the wire arising from variations in the centrifugal force of the ball while swinging. Since the time of a complete vibration of our pendulum was nearly 6 seconds and the arc about 30 centimetres,

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* "On a Neglected Principle that may be employed in Earthquake Measurements," Trans. Asiatic Soc. of Japan, vol. v. part 1, p. 181; reprinted in Phil. Mag. July 1879.

the velocity at the middle of its path was

$$\frac{30\times\pi}{6}, \text{ or } 15{\cdot}7 \text{ centimetres per second;}$$

hence the pull on the wire, which at the end of the swing was equal to the weight of the wire, or 2352·2 grammes, was increased by

$$\frac{2352{\cdot}2\times(15{\cdot}7)^2}{939\times979{\cdot}7} \text{ grammes}$$

at the middle of the swing. But this is less than a gramme, so that no practical extension of the wire arose from centrifugal force.

3. Shortening of the length of the wire, due to its curvature, arising from the resistance of the air making it concave in the direction of motion. It is easy to see that the shortening of the pendulum due to this cause is excessively small, and is of the same order as the lengthening arising from the centrifugal force; so that these two very small errors may be regarded as balancing one another.

Also, since it may be calculated that the period of transverse vibration of the wire is less than one fortieth of the periodic time of the pendulum, the resistance of the air cannot tend to cause amplification of the lateral vibrations in the wire itself.

We may therefore assume that our pendulum vibrated like a rigid body, consisting of a ball of brass, a straight steel wire, and a triangular steel prism of which the edge was the fixed axis.

Calculation of g.

The complete formula is, of course,

$$t=\pi\sqrt{\frac{\Sigma(mr^2)}{l\,.\,mg}},$$

l being the distance from the axis of rotation to the centre of gravity of the pendulum.

The steel knife-edge had a length of about 4 centimetres, a breadth of about 1 centimetre, and a depth of $\frac{1}{2}$ a centimetre; hence its weight was about 7·8 grammes, its moment of inertia about the axis of rotation 0·98 (gramme, centimetre), and the distance of its centre of gravity from the axis of rotation 0·33 centimetre. The weight of the wire was 11·6 grammes, and its length 934·99 centimetres at 0° C. Its moment of inertia was therefore $3{\cdot}3803 \times 10^6$ (gramme, centimetre), and the distance of its centre of gravity from the axis of rotation 467·49 centimetres. The weight of the brass ball was 2352·2 grammes, its moment of inertia about the axis of rotation $2{\cdot}0744 \times 10^9$, and the distance of its centre of gravity 939·09 centimetres at 0° C. Of the whole system, then, the weight was 2371·6 grammes, the moment of inertia about the axis of rotation $2{\cdot}0778 \times 10^9$ (gramme, centimetre), and the distance of its centre of gravity $2{\cdot}2144 \times 10^6$. Consequently

$$g=\left(\frac{\pi}{t}\right)^2 \frac{2{\cdot}0778 \times 10^9}{2{\cdot}2144 \times 10^6},$$

and

$$t=3{\cdot}0748 \text{ seconds};$$

or

$$g=979{\cdot}58 \text{ centimetres per second per second in air,}$$

or

$g=979{\cdot}74$ *centimetres per second per second* in vacuo *for the Imperial College of Engineering, Japan*—

a result agreeing extremely closely with the number 979·7 obtained above from Clairault's formula.

In beginning this series of observations we expected to find g to be greater than what Clairault's formula gives it. Our reason was this:—Clairault's formula assumes a circular equator; Capt. Clarke has found that the equator is elliptical, one extremity of its major axis being in 15° 34′ E. longitude; and therefore Tokio is in longitude nearer a minor axis than a major one. We find, however, a reason why g satisfies so well Clairault's formula, in spite of this excentricity of the equator. The greatest depression of the earth's surface is only a few hundred miles to the east of Japan; and probably the diminution in g produced by this cause just counterbalances the increase of g produced by ellipticity of the equator. As for local perturbations, it is to be remarked that Tokio is situated on a very large plane, there being no hills of any magnitude within eighty miles. We think that the geodesy of Japan is of special interest on account of the great Pacific depression, and on account of the very gradual slope of the earth's surface from Japan to China, which causes Japan to be a sort of ridge.

Probably the best method of determining the value of g

would be to use a *rigid* homogeneous pendulum, of such a shape that its moment of inertia could be easily calculated with accuracy from its linear dimensions measured at any temperature—for example, a sphere at the end of a cylindrical rod with a knife-edge, all cast in one piece and turned true in the lathe, or a cylindrical bob cast at the end of the cylindrical rod. The only possible objection to this method would be the possible want of homogeneity in the metal. This might be allowed for in the following way :—Instead of casting the cylindrical bob in one piece, make it consist of a number of concentric tightly fitting cylindrical shells accurately turned. Experiments would then be made first with the cylinders all in one position, then with some of them twisted slightly round, and so on until in the mean result the errors of eccentricity of mass would probably be eliminated.

Another, and perhaps the best of all methods, would be for rigid compound pendulums to be accurately timed experimentally at Greenwich at a number of different temperatures, and sold with a scale of temperature-corrections for the time of vibration attached.

One set of experiments, then, with one of these pendulums anywhere would at once give the value of g; and such a pendulum would undoubtedly be the most suitable for surveys and expeditions in foreign countries. The fact is, the mathematical beauty of the principle involved in the Kater's pendulum has, in our opinion, caused far too much importance to be attached to it as a practical instrument for determining experimentally the value of the acceleration of gravity.

We have to thank several of our late students, and especially Messrs. Honda, Kikkawa, A. Kasai, J. Nakahara, and H. Nobechi for assistance rendered us during this investigation. And it may here be mentioned that this investigation, like the many others we have been enabled to carry out during the last few years, has resulted from the plan we have followed of teaching the laboratory students not, as is customary in Colleges, to repeat well-known experiments, but to endeavour in their investigations to advance, in some small degree at any rate, the bounds of existing knowledge. And this system of enlisting the assistance of even quite young students in original research we have found to create an enthusiasm in experimental work otherwise unproducible.

XXXIX. *Notices respecting New Books.*

Geology of Wisconsin. Survey of 1873–77. Vol. II. 8vo, 768 pp., with numerous Plates and other Illustrations; accompanied by an Atlas of Maps. Published under the Direction of the Chief Geologist, by the Commissioners of Public Printing, in accordance with Legislative Enactment. [Madison.] 1877.

THOUGH entitled the "second," this volume of the Geological Survey of Wisconsin is the first in order of publication, the initial or "first" volume being delayed until the Survey shall have been completed and the general conclusions and economical applications required by the law establishing the Survey to be given in vol. i. shall have been arrived at. By far the greater part of the State has been distributed among the Geological Surveyors; but in this volume the results of the examination of only about half of the area (namely the eastern, south-central, and south-western Counties) are given; and this alone comprises more than 30,000 square miles. Fourteen large topographical and geological maps illustrate the region.

The "Historical" division of this volume includes the "Annual Reports of progress and results" for the years 1873 and 1874, by the late Dr. I. A. Lapham, who lived to be Chief Geologist only two years, and for the year 1875 by Dr. O. W. Wight, who was on duty for one year. These Annual Reports had not been previously published. That for 1876, by the present Chief Geologist, T. C. Chamberlin, was published; and, together with the separate and local Reports from the individual Surveyors, Assistant Geologists, Chemists, Naturalists, and Palæontologists, it has been incorporated in the elaborated chapters of the finished volume, as far as concerns those portions of Wisconsin now described.

Part II. of this volume treats of the Geology of Eastern Wisconsin, more than 12,000 square miles, by T. C. Chamberlin (pp. 93–405), freely illustrated with sections, plans, and maps. A bibliographic list of previous observers and writers is first given; and then the Topography of the district forms Chapter I., pointing out:—(1) the features due to geological agencies in Pre-Glacial, Glacial, and Post-Glacial times; (2) the valleys (especially of Fox River, Rock River, and Lake Michigan), their peculiarities and commercial importance; (3) the dividing ranges; and (4) elevations, in a long alphabetical list. Chapter II., on the Hydrology, is well occupied by:—(1) the Drainage, the Mississippi and St. Lawrence being the great recipients; (2) the origin and geological relations of the Lakes of Eastern Wisconsin. Lake Michigan, it is pointed out, "has been the theatre of powerful glacial action; and to this cause its present regular outline and great depth and breadth are undoubtedly due" (p. 137). Many of the smaller lakes have been similarly formed; whilst some have been dammed up by moraines, and others confined between drift-ridges. Many lake-areas have been limited, or quite filled up, by sedimentary deposits, now forming valuable land. (3) Water-supply by springs and wells, also local water-power, and changes of drainage, especially by the felling of timber, are duly noticed.

Chapter III. treats of the Native Vegetation, dividing itself into:—A. Upland Vegetation; B. Marsh Vegetation; C. Intermediate groups. The relation of rocks and soils to the local vegetation is carefully kept in sight. Soils and their origin form the interesting matter of Chapter IV.; and Chapter V. has a full treatment of the Quaternary formations, or Drift, of the district under notice. In the first place, (1) morainic ridges, and (2) boulder-clay or till, are described as "Primary Drift;" secondly, the sands and gravels of lake-beaches and lacustrine deposits of clay are described as "Secondary or modified Drift;" thirdly, many local deposits due to streams and small lakes are referred to. Glacial movements and the results of ice-action come within the subject-matter of these pages; and the Kettle Range, with its "Pot-holes" or "Sinks" and its mounds and ridges of sand and gravel, is particularly described. The Kettle Range proper is parallel with Lake Michigan, but continues south, west, and north, in an elliptical curve, as the boundary line of a great radiating ice-drift from the north-east. In its eastern portion it has been accumulated between the above mentioned and another, similar, parallel, south-west driftage, which came along the Lake Michigan area. The economic uses of the Drift-deposits for bricks, pottery, &c., and the shell-marl and peat of obliterated lakes, are also treated of.

Four chapters are next occupied with a description of the geological formations which constitute the substructure of Eastern Wisconsin, namely:—(1) *Devonian*: the Hamilton group. (2) *Upper Silurian*: the Lower Helderberg and the Niagara. (3) *Lower Silurian*: the Cincinnati, Galena, Trenton, St.-Peters, Lower Magnesian, and Potsdam formations. (4) *Archæan*: the Huronian and Laurentian.

In Part III. of the volume we have the "Geology of Central Wisconsin," by Roland D. Irving (pp. 407–636). This tract comprises an area of about 10,000 square miles, and exhibits Laurentian, Huronian, Primordial (namely, 1. Lower or Potsdam Sandstone, and 2. Beds of Passage, as the Mendota Limestone and Madison Sandstone), Canadian (namely, 1. Lower Magnesian Limestone, and 2. St.-Peter's Limestone), and Trenton (namely, 1. Trenton Limestone, 2. Galena Limestone) groups (see Table, with details, at p. 460). Above these Lower Silurian rocks are Quaternary deposits of the *Glacial*, *Champlain*, and *Recent* formations. This report is illustrated with numerous and especially neat sections and plans, with the proportional scales systematically given. Among the many interesting details supplied, the definition and description of the western portion of the Kettle Range, consisting of morainic drift (pp. 615 &c.), alluded to above, are especially noteworthy. To this Report is an Appendix, by Charles E. Wright, on the Microscopic Lithology of a portion of the rock-material collected by the Survey in Central Wisconsin (pp. 637–642).

Part IV. (pp. 643–752) gives an account of the geology and topography of the Lead Region, by Moses Strong, with many illustrations. This is the south-western corner of Wisconsin, in the angle between the Wisconsin river on the north and the Mississippi on the west, with Illinois on the south. The plan of surveying, list of elevations, topography, including drainage and surface-features, are noted and described. The diminution of river-water is noticed as due probably to the removal of forests. Many old water-mills now stand neglected; but at the same time some mines have been reopened and profitably worked, owing to the lessened quantity of underground water. The surface-geology shows soils, subsoils, and drift, which are duly noticed; and the deeper geological formations are described as fully as space permits. They are Potsdam Sandstone, Lower Magnesian Limestone, St.-Peter's Sandstone, Trenton (buff and blue) Limestones, with ores of zinc and lead, Galena Limestone (dolomite), with ores in nests, sheets, &c., and the Cincinnati group. The "Lead Region" proper is then described (pp. 689 &c.), namely the area of the exposure of the Galena Limestone, about 1776 square miles. The local mining terms are explained; and the mineralogy of the district is shown by a list of classified local minerals. "The minerals appear to have been deposited in the following general order:—

Galenite.
|
Sphalerite.
|
Dolomite. Calcite.
|
Pyrite. Marcasite. Chalcopyrite.
|
Barite.
|
Calcite.
|
Cerussite. Smithsonite. Malachite. Azurite."

Exceptions, however, and obscure conditions are far from being uncommon.

The present condition of the mines and diggings in the various districts (pp. 694 &c.) is described in some detail; and the statistics of all the mineral produce are given.

A full, clear Index completes this volume, which is well worthy of every geologist's consideration and study. Being a highly praiseworthy production, at the hands of the geologists, artists, printers, and all concerned, it brings honour to the statesmen of Wisconsin, who are wise enough to have a good Geological Survey, and liberal enough to get it done in a creditable manner.

Clarendon Press Series. A Treatise on Statics, containing the fundamental Principles of Electrostatics and Elasticity. By GEORGE M. MINCHIN, *M.A. Dublin, Professor of Applied Mathematics in the Royal Indian Engineering College, Cooper's Hill.* Second Edition, Corrected and Enlarged. Oxford, at the Clarendon Press, 1880. (8vo, pp. x and 518.)

We noticed the first edition of this work at the time of its publication (p. 386, vol. iv. fifth series), and expressed our sense of its great merits as a comprehensive treatise on Statics. We need not, therefore, do more on the present occasion than mention the leading points in which this second edition differs from its predecessors. There are, of course, numerous minor alterations and corrections, such as those which are almost invariably required in the second edition of a mathematical work abounding in examples, or as the division of chapter 6 of the first edition into chapters 6 and 7 of the new edition. Besides these alterations there are additions of importance—such as the articles in chapter 5, on the force polygon and the funicular polygon, and on the construction derived from them of the resultant of a given system of forces in one plane; and those on "Astatic Equilibrium," a subject on which an interesting paper was published not long ago by the author in the Proceedings of the London Mathematical Society. The most important addition, however, is chapter 16, on what we pointed out as an omitted subject in our notice of the first edition. It treats of the Equilibrium of an Elastic Solid, and consists of three sections, viz. the "analysis of small strains," the "analysis of stresses," and the "expression of stress in terms of strain." Although the chapter runs to the length of sixty-two pages, it may be surmised that the author felt himself rather in want of space, or he would hardly have given so important a theorem as "the equation of three moments" in the form of an unworked example. However, the chapter is a most valuable addition to the work, and one that will require and repay the student's closest attention.

The present edition is issued as a volume of the Clarendon Press series, and is printed in the ordinary octavo form. The latter circumstance has greatly improved its appearance; it has also given it the air of having been greatly enlarged; but so far as we have compared the two editions this does not seem to have been the case, except so far as chapter 16 is concerned.

XL. *Intelligence and Miscellaneous Articles.*

ON THE TEMPERATURE OF FROZEN LAKES. BY F.-A. FOREL.

IN 1879 some interesting thermometric soundings beneath the ice of the Scotch lakes were published in 'Nature,' vol. xix. p. 421, by Mr. J. Y. Buchanan, which have considerably modified the accepted ideas upon the limit of the vertical propagation of cold in fresh water. Instead of finding at the bottom, as was expected, a layer of water with the temperature of 4° C. (that of the maximum density of water), Mr. Buchanan ascertained that, in Loch Lomond, the temperature of the water gradually rose from 0° at the surface to 2°·4 at 20 metres depth, but did not rise higher. Must we infer from this the incorrectness of the classic theory of the congelation of lakes? According to that theory, water, after cooling throughout its mass down to 4° under the action of thermal convection currents, becomes colder only at the surface, being stratified in layers the colder the more superficial they are, conformably to the order of their densities. The following observations prove that the depths reached by Mr. Buchanan were not sufficient to show the limit of the superficial cooling, which descends to a much greater depth than has ever been supposed.

I have repeated these researches in some Swiss lakes which are deeper than those of Scotland. My soundings, executed with a Negretti-and-Zambra thermometer, were made in the Lake of Morat on December 23, 1879, and the Lake of Zurich on January 25, 1880.

I. *Lake of Morat.*

(Superficies 27·4 kilometres; greatest depth 45 metres. The lake began to freeze on the 18th December. Thickness of the ice on the 23rd December, 11 centims.; on the 1st of February, 36 centims.)

Depth.	December 23.	February 1.	Difference.
metres.	°	°	°
0	0·36	0·35	−0·01
5	1·60	1·90	+0·30
10	2·00	2·00	0·00
15	2·23	2·45	+0·22
20	2·46	2·50	+0·04
25	2·60	2·50	−0·10
30	2·66	2·40	−0·26
35	2·75	2·55	−0·20
40	2·70	2·70	0·00
Means..	2·15	2·15	

II. *Lake of Zurich.*

(Superficies 87·8 kilometres; greatest depth 141 metres. The lake froze during two days at the end of December, and then again and definitively on the 21st of January. Thickness of the ice on the 25th of January, 10 centims.)

Depth.	Temperature.	Depth.	Temperature.
metres.	°	metres.	°
0	0·2	70	3·7
10	2·6	80	3·8
20	2·9	90	3·8
30	3·2	100	3·9
40	3·5	110	3·9
50	3·6	120	4·0
60	3·7	133	4·0

From these numbers I draw the following conclusions:—

1. The old theory of the freezing of lakes, which assumes a progressive cooling of the whole mass down to 4°, and then a further lowering of the temperature of the superficial layers stratified from 0° to 4° according to the order of their densities, is perfectly correct.

2. The penetration of the cold into the upper layers may descend to a depth of 110 metres (Zurich).

3. It was in consequence of its comparative shallowness that Loch Lomond did not exhibit to Mr. Buchanan the temperature of 4° in its bottom layers.

4. The penetration of cold into the upper layers takes place progressively and very gradually. The curve which can be drawn from my numbers from the Lake of Zurich presents neither leaps nor jerks; it is altogther analogous to the curves of superficial heating of a lake in summer. This, it seems to me, is quite sufficient to put aside the supposition that the cooling, which penetrates so deeply, takes place either by way of thermal convection or by mechanical mixture under the action of waves and currents—with the exception, perhaps, of the upper layer of from 5 to 10 metres thickness.

Must this penetration of cold be attributed to phenomena of conductivity, or to phenomena of radiation, either of the water itself or of the soil through the water? The experiments have not supplied me with any elements for answering this question.

5. On comparing the two series of soundings made in the Lake of Morat, separated by an interval of forty days, I find that the mean temperature remained exactly the same. Therefore the layer of ice absolutely stopped the cooling of the water, and the action of the exterior cold was all expended in increasing the thickness of the ice.

6. During these forty days the water of the Lake of Morat beneath the ice underwent an equalization of temperature; the colder upper layers were warmed a little, while the warmer bottom layers were cooled; but we are still very far from finding complete uniformity of temperature in the entire depth of the lake on the 1st of February.—*Comptes Rendus de l'Acad. des Sciences*, Feb. 16, 1880.

ON GAS-FRICTION, AND ON THE VELOCITY OF ELECTRICITY IN THE ELECTRIC CURRENT. BY PROF. LUDWIG BOLTZMANN.

In a memoir entitled "To the Theory of Gas-friction," the relative velocity and its longitude and latitude with respect to the velocity of one of the molecules as axis, and to the plane of the velocities of both molecules as first meridian, are introduced as integration-variables; and thereby the equation for the distribution of velocities in a moved gas is simplified. Here the velocities meant are everywhere those before impact. The determination of the coefficient of friction by the development of a series, exact account being taken of the distribution of velocities, is then encountered by no difficulty.

In addition, M. Boltzmann remarks that the magnetizing of a ring can be most easily calculated by introducing Carl Neumann's coordinates, and that the absolute value of the velocity of the electricity in a current can be calculated from E. H. Hall's recently published uncommonly interesting experiments. If the gold leaf employed by Hall, of the length l and breadth b, is in a homogeneous magnetic field of the intensity m (measured in absolute Gauss measure), the electromagnetic force which tends to impel it perpendicular to the lines of magnetic force has the intensity

$$k = ml\mathrm{J}_m = \frac{mt\mathrm{J}_e}{v},$$

in which J_m is the intensity of the current passing through the gold leaf in the direction of its length (in magnetic measure), J_e is the same current-intensity measured in Weber's electrostatic or mechanical measure, v is equal to $31 \cdot 10^7 \frac{\text{metres}}{\text{second}}$. If in the time t the quantity of electricity e passes through the cross section of the gold leaf with the velocity e, then is $\mathrm{J}_e = \frac{e}{t} = \frac{ec}{l}$, and hence $k = \frac{mec}{v}$. If now at two places in a conductor, separated by the distance b, the difference of potential p prevails, in its interior the force $\frac{p}{b}$ acts upon the unit amount of electricity, upon the amount e the force $\frac{pe}{b}$. Hence, if the force above denoted by k itself acts upon the movable electricities in the gold leaf, and the potential-difference thereby produced between the two extremities of the gold leaf be denoted by p, then is $k = \frac{pe}{b}$, $p = \frac{kb}{e} = \frac{mbc}{v}$. Now let the two extremities of the gold leaf be connected with a galvanometer. The total resistance of this circuit (gold leaf, galvanometer, and conducting-wires) shall be denoted by w, and i shall denote the current-intensity produced therein by the magnet, while again the index m signifies magnetic, the index e mechanical measure of the current. Then is $i_e = \frac{p}{w_e} = \frac{mbc}{vw_e}$, $i_m = \frac{mbc}{w_m}$, from which it follows that $c = \frac{i_m w_m}{mb}$. From this formula the absolute velocity c of the electricity in the current J can be determined. It is exactly equal to the velocity with which a wire of length b must be moved, perpendicular to itself, through the magnetic field, in order that it may generate the current i in a circuit of resistance w. In this the wire is supposed to be parallel to the length, the direction of its motion parallel to the width, of the gold leaf. If we put $i_m w_m = e_m$, then e_m is the electromotive force, in magnetic measure, which would produce in the same circuit the same current i_m. Its measurement suffices for the calculation of c. In order to obtain the general theory of Hall's phenomenon, the equations constructed by Kirchhoff, Weber, Helmholtz, Maxwell, Stefan, &c. for the motion of electricity in material conductors must be extended by adding to the electrostatic potential and the action of induction a term expressing the electrodynamic action, and easily calculated when the current in the volume-element that acts and is acted on is resolved into three components at right angles to one another and their reciprocal action calculated according to an electrodynamic law (Ampère's for instance).—*Kaiserliche Akademie der Wissenschaften in Wien, math.-naturw. Classe*, Jan. 15, 1880, pp. 11–13.

RESEARCHES ON THE RELATIVE INTENSITY OF THE SPECTRAL LINES OF HYDROGEN AND NITROGEN IN REGARD TO THE CONSTITUTION OF NEBULÆ. BY CH. FIEVEZ, ASSISTANT ASTRONOMER AT THE ROYAL OBSERVATORY AT BRUSSELS.

Huggins's observations[*] having shown that the spectrum of the nebulæ 37 H IV. Draconis, 73 H IV. Cygni, and some others consisted of a few bright lines, one of which, brighter than the others, coincides with a nitrogen-line, and another, finer, corresponds with the line F of hydrogen, he was led to investigate whether the complicated spectrum of nitrogen might not be simplified by extinction[†].

He remarked that, by causing the induction-spark to spring between platinum electrodes placed before the objective of a telescope, only one line of nitrogen was visible in the spectroscope applied to the ocular extremity.

Afterwards, examining the spectrum of nitrogen with a spectroscope having a prism of 60°, and placing near the eye a neutral-tint prism corrected for refraction and possessing equal absorptive power for all parts of the spectrum, he saw that the two groups of nitrogen-lines in the orange were nearly extinguished, while the lines in the green were very bright. And on employing a small direct-vision spectroscope, Huggins found it possible, by increasing the distance between the instrument and the spark, to obtain a position in which the double line in the green (Plücker's group IV.)[‡] coincident with that of the nebula was alone visible. The spectrum of the spark in nitrogen then resembled that of the nebula.

Huggins's researches do not appear to have been carried further in this direction; nevertheless he believes that, if the spectrum of hydrogen were reduced in intensity, the line F, which corresponds with that of the nebula, would alone remain visible after the line C and the other more refrangible ones had become too faint to affect the eye.

The verification of this hypothesis is connected with a very important question, viz. "Do the lines of the spectrum of a nebula represent the total light emitted by that body? or are they merely

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[*] "On the Spectra of some of the Nebulæ," Philosophical Transactions, 1864, p. 437.

[†] Phil. Trans. 1868, p. 542.

[‡] "On the Spectra of Ignited Gases," Phil. Trans. 1865, plate ii.

the most marked, the others having been extinguished by the effect of distance?"

Moreover, if it were proved that the other lines of the spectra of nitrogen and hydrogen are stopped in their passage, we might regard this as an indication of the extinctive power of cosmic spaces. For this reason we have endeavoured, in imitation of Huggins, to ascertain by experiment if an alteration in the luminous intensity of a gas, *without modification of the temperature and pressure of the gas*, would correspond with the disappearance of one or more lines in the spectrum.

The method we have employed consists in projecting, by means of a lens, a real image of the luminous body upon the slit of the spectroscope, and then altering the intensity of that image, either by reducing the aperture of the projecting lens, or by shifting a diaphragm pierced with a circular aperture between the lens and the projected image. All the observations took place in a dark room, the apparatus being arranged as follows:—

(1) A Melloni optical bench supporting the projection-objective and the diaphragm. The objective has a clear opening of 8 centims.; the diaphragm is that of Melloni's apparatus.

(2) An automatic reversing-spectroscope of Young, with a variable dispersion of from two to ten prisms, placed behind the optical bench.

The observing-telescope is of the same focal length, parallel, and twinned with the collimator. The two objectives are thus shifted simultaneously, and to the same extent, at the time of adjusting. The slit of the spectroscope is horizontal; and the optic axis of the collimator coincides with that of the projection-objective.

(3) A Plücker's tube coated all over with lampblack, with the exception of a small portion of the straight part, is placed vertically (upon a movable foot, permitting it to be fixed at a suitable height) in front of the projection-objective.

The distances from the tube to this objective, and from this to the slit, are so combined that the width of the projected image is less than the length of the slit, and that the whole of the luminous pencil passing through the slit is received upon the objective of the collimator. This last arrangement, devised by Lockyer, constitutes a most important advance in the improvement of the methods of spectral observation: it permits us to recognize in a spectrum the long lines and the short lines, which were first discovered by that accomplished English physicist.

In our experiments the Plücker tube was placed at 40 centims. from the front lens of the objective, and about 1·3 metre from the slit of the spectroscope.

The induction-current was furnished by a Gaiffe induction-coil of large dimensions, set in action by a battery of eight couples with bichromate of potass, and capable of giving a spark of 50 centims. in free air. The intensity of the current was regulated by the greater or less immersion of the polar plates in the acid solution.

A condenser, formed of ten fulminating squares having a total surface of 5 square metres, and arranged either in tension or in quantity, could be put in communication with the two poles of the bobbin.

A small coil, giving a spark of a few millims. in free air, was also used when a very feeble current was required. The experiments were repeated a great number of times; so that they present a fair degree of certainty.

Hydrogen Spectrum.

Experiment 1.—The large coil was employed, with the condenser arranged in tension, the dispersion of the spectroscope consisting of six prisms, and the three lines C, F, and H being distinctly visible under these conditions. The luminous pencil being then progressively narrowed by means of a diaphragm pierced with an aperture of 7 millims. diameter, on moving this diaphragm further from the slit, and consequently bringing it nearer to the objective, the length of the lines is observed to diminish, and at last the line H becomes invisible, while C and F are still bright.

Exp. 2.—Conditions the same; dispersion of four prisms; aperture of the diaphragm 2 millims. By operating as before, the line H is first extinguished, then the line C; and on placing the diaphragm close to the projection-objective, the line F only is visible.

Exp. 3.—Conditions the same, the same dispersion, condenser arranged in quantity, aperture of the diaphragm 2 millims. The same results are obtained; but it is not necessary to move the diaphragm so far from the slit to cause C to disappear.

Exp. 4.—With the small coil, without a condenser, and with a dispersion of six prisms, the lines C, F, and H are well shown in the spectrum. By then interposing the diaphragm of 2 millims. aperture, the lines are observed at first to grow fainter and shorter; then the line H disappears, and afterwards the line C, the line F remaining then alone visible. By employing a dispersion of six prisms the same effect is produced more rapidly.

Nitrogen Spectrum.

It is known that the line spectrum of nitrogen, called also the spectrum of the second order, is formed of several groups of lines, indicated by Plücker * by the numerals I., II., III., IV., and V.

Exp. 1.—With the large coil, the condenser in quantity, and a dispersion of six prisms, group I. was faintly visible; the others were very marked. By interposing the diaphragm of 7 millims. aperture, groups I., III., V., II., and the lines of group IV. with the exception of the double line coinciding with that of the nebulæ 37 H IV. and 73 H IV., are successively extinguished.

Exp. 2.—Same conditions, dispersion of two prisms, and diaphragm of 2 millims. aperture. The same results were obtained; the groups successively disappear, and the double line alone remains visible.

If in any of the preceding experiments, at the instant that extinction of a line is produced *the slit of the spectroscope be opened wider*, the line immediately reappears—which demonstrates that the disappearance is caused by the weakening of the luminous in-

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* "On the Spectra of Ignited Gases," Phil. Trans. 1865, plate ii.

tensity. It seems, therefore, well established that a gas, although possessing several spectral lines, may manifest itself in the spectroscope by the presence of a single line, the others remaining invisible on account of the want of brightness of the luminous body. There is, then, a great probability that a known element exists in a celestial body when a line belonging to that element has been proved to exist in the spectrum. And as the spectra of the nebulæ 37 H IV. &c. show those lines of nitrogen and hydrogen which longest resist extinction, we may, with Huggins, regard those nebulæ as including nitrogen and hydrogen among their constituent substances, and attribute the *relative* invisibility of the other lines to absorption by space, acting equally upon rays of every degree of refrangibility. We say "*relative* invisibility," because it is probable that the lines now invisible could be perceived with more powerful telescopes than those which we possess at present.—*Bull. Soc. de l'Acad. royale de Belgique*, sér. 2, t. lxix. no. 2, 1880.

ON THE ELECTROMOTIVE FORCES WHICH APPEAR IN FREE JETS OF WATER.

We have been favoured by the author, M. J. Elster, with a copy of his Inaugural Dissertation* on the above subject, which appears to have been prepared under the guidance of Prof. Quincke. It is considered as proved by these investigations :—

1. That if an electromotive force is to arise in a free jet of water, it is necessary that the water particles be in contact with a solid body ;

2. That the evolution of electricity takes place only where the particles of the liquid undergo friction, so that only a relatively small portion of the water-jet contributes any thing to the development of electricity ;

3. That when the velocity of a jet issuing constantly from the same orifice is varied, the electromotive forces are proportional to the *vires vivæ* of the liquid particles—a relation the reason of which is found in the law of the conservation of energy ;

4. And, lastly, that the electromotive force is dependent on the nature of the body which is in contact with the liquid.

From these results the author draws the following conclusions :—

1. The motion of a fluid does not by itself produce any electromotive force. Consequently the inference deduced by Edlund from his unitary theory of electricity has not been confirmed.

2. Capillary electric currents are conditioned solely by the friction of the particles of the fluid in motion—in nonhumectant fluids by their friction against the particles of the solid wall, in humectant fluids by friction on the particles of a layer of the same fluid condensed upon the surface of the solid body, which behaves towards the less dense layer like a heterogeneous substance.

3. The capillary-electric currents discovered by Quincke are identical with the friction-currents, first observed by Zöllner, which make their appearance in the rubber of an electrical machine.

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* *Ueber die in freien Wasserstrahlen auftretenden electromotorischen Kräfte.* Von Julius Elster. Leipzig, 1879. 8vo, pp. 40 and 1 plate.

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XLI. *On the Diffusion of Liquids.*

By John H. Long, *of Lawrence, Kansas, U. S. A.*[*]

[Plate VII.]

Historical.

Among the many subjects of physical inquiry which have claimed the attention of investigators during the past thirty years, one of the most interesting and at the same time not the least important has been that of the diffusion of liquids. Leaving out of consideration the experiments of Jolly[†], Ludwig[‡], Cloetta[§], and others, who observed the progress of diffusion between two media separated by a membrane, and whose results, besides being somewhat contradictory among themselves, have a physiological rather than a physical or chemical interest, we come first, in the consideration of the literature, to the work of Graham[‖], which, as has been remarked, must be regarded as the first and only general investigation of this subject in our possession.

The method employed by Graham is too well known to require a minute description here. I will simply say that he allowed the diffusion to take place between salt-solutions or other liquids, contained in small phials, and a much larger

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[*] Communicated by the Author, being a Dissertation presented to the Faculty of Science of the University of Tübingen for the attainment of the degree of Doctor of Science.

[†] *Zeitschrift für rationelle Medicin.* Also Pogg. *Ann.* lxxviii. p. 261.

[‡] Pogg. *Ann.* lxxviii. p. 307.

[§] *Diffusionsversuche durch Membranen mit zwei Salzen.* Zürich, 1851.

[‖] Phil. Trans. 1850, p. 1. *Ann. Pharm.* lxxvii. pp. 56 & 129.

quantity of water in vessels upon whose bottoms the phials stood. By carefully removing these from time to time and analyzing the contents of the vessels, the amount of substance diffused was found. In these experiments Graham dispensed with a membrane entirely, and thus avoided a source of error which had robbed the investigations of his predecessors of much of their scientific value. In the two papers read before the Royal Society in 1850, he established among others the following facts :—

1. The velocity of diffusion is in general different for each substance in solution.
2. The amounts of salt diffused in the same time from solutions of the same substance, but of different concentrations, are very nearly proportional to these concentrations.
3. The amount of salt diffused from a given solution increases with the temperature of the solution.

From his investigations Graham was led also to believe that a relation exists between the boiling-points of solutions and their rates of diffusion, and that no simple relation seems to exist between the molecular weight of a salt and its rate of diffusion when dissolved. These and other points of Graham's work will be referred to again later, as will also the results he obtained by means of the method of jar-diffusion, as he termed it*.

The chief characteristic of the earlier Graham experiments (those of 1850 I mean) is that they were concerned, not so much with an investigation of the *theory* of diffusion (that is, with the purely physical side of the question), as with the relation of various substances to each other as regards diffusion, an inquiry more chemical than physical in its nature. This is not the case, however, with an investigation published by Fick in 1855†. He regarded the work of Graham as possessing a qualitative rather than a quantitative value, and remarked that it gave no clue to the fundamental law of the phenomenon. He showed that liquid diffusion may be compared to the conduction of heat—that is, that the spread of salt-particles through water is in many respects analogous to the spread of heat in a conducting body, and that similar formulas to those established by Fourier for the latter case may be applied in the former.

Fick described several methods by means of which the law of diffusion may be determined experimentally, one of which is the following. A vertical glass cylinder open at both ends was fitted tightly below into a vessel containing saturated salt-

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* Phil. Trans. 1861, p. 138.

† Pogg. *Ann.* xciv. p. 59. [Also Phil. Mag. [4] vol. x. p. 30.]

solution, and then carefully filled with water and placed in a large reservoir likewise containing water. Here it stood for several weeks, the water in the outer vessel being removed from time to time and fresh added. The concentration of the liquid in the cylinder was then determined at different heights, and from these data the amounts of salt diffused to the same heights reckoned.

It was found that these amounts decreased from below upwards, and indeed in such a manner as to satisfy the requirements of this previously assumed hypothesis :—"If in a cylindrical vessel dynamic equilibrium exists, the concentration-differences of any two layers must be related to each other as the distances separating them ; in other words, the densities must decrease from below upward as the ordinates of a right line."

In another experiment, Fick arranged three tubes of equal diameters but of unequal lengths in the same manner, and allowed the salt to diffuse through them into large vessels of water. He found in this case that the amounts of diffused salt were inversely proportional to the lengths of the tubes, the concentrations in the reservoirs below being the same.

These results can be expressed in the following equation,

$$S = kqt\frac{u_1 - u_2}{l};$$

in which S is the amount of salt passing the horizontal section q in the time t; u_1 and u_2, the concentrations of two layers ; and l, the distance separating them, supposing that a condition of constancy (*Beharrungszustand*) has been reached ; k is the so-called constant of diffusion, depending on the nature of the salt in question.

Beilstein* was the next who published on the subject. He, as Graham, considered the chemical rather than the physical side of the problem; and the method employed by him was in substance as follows. A glass tube about 3 inches long was bent at one end, and the shorter arm ground off until it was not more than a millimetre in length ; the other end was drawn out and fitted with a ground glass stopper. This diffusion-cell, resembling somewhat a very short siphon-barometer, was filled with salt-solution of known strength and suspended vertically in a large vessel of pure water. Diffusion now took place between the solution in the short arm of the glass and the medium above. After allowing this to continue a few days, the small vessel was removed and the salt still remaining in it determined ; subtracting this from the original

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* *Ann. Chem. Pharm.* xcix. p. 165.

amount, the portion diffused was known. The amounts diffused from similar solutions of ten different substances are given by Beilstein; but, owing to the fact that he failed to state the exact dimensions of his diffusion-glass (he mentioned that it contained about 5·5 cubic centims.), it is impossible to compare these results with those of Graham. The determination of Fick's diffusion-constant from Beilstein's work is, of course, impossible.

In a paper published in 1857, Simmler and Wild* have shown that in certain cases it is possible to determine the diffusion-constant from a condition of the solution not yet stationary, as they claim was the case in the experiments of their predecessors.

They also suggested several new methods of observation. One of these, a so-called optical method, was some twenty years later applied by Johannisjanz†; and in the meanwhile another optical property of certain solutions was made use of by Hoppe-Seyler‡ and Voit§ in similar investigations.

Hoppe-Seyler and Voit experimented with substances possessing the property of rotatory polarization—a solution of grape-sugar for instance. Such a solution was placed in a glass vessel with parallel sides; a layer of water was then brought over this without mixing with it. The sugar, diffusing into the water above, imparted to this latter the rotatory power. By observing the amount of rotation at different heights in the glass vessel, data were obtained from which the diffused sugar at these points could be calculated.

Johannisjanz used a hollow glass prism as diffusion-cell. The lower half of this was filled with salt-solution, the upper with water. As the salt diffused upwards the refracting-power of the latter was altered; and upon this was based the method of determining the amount of diffused salt. For the details of these processes I refer to the original articles mentioned above. Although these three investigations were evidently conducted with great care, it was found, however, that the results obtained by Hoppe-Seyler and Voit did not agree with each other, and that the value of k, as determined by Johannisjanz for NaCl, was quite different from that obtained by Fick.

The cause of these discrepancies remained unknown until very recently, when Stefan‖ showed that the optical methods are based on a false assumption, and that they can give as a consequence only false results. He shows that a liquid

* Pogg. *Ann.* c. p. 217.

† Wiedemann's *Annalen,* ii. p. 24.

‡ *Medicinisch-chemische Untersuchungen*: Berlin, 1876.

§ Pogg. *Ann.* cxxx. p. 227.

‖ *Wiener Sitzungsber.* Dec. 1878.

whose density varies from below upward, as is the case in a diffusing salt or sugar solution, must act as a prism whose refracting angle is turned upward, and hence that a ray of light which enters such a medium in a horizontal direction cannot pass through it in the same straight line, but must experience a refraction downwards. This conclusion of Stefan is strengthened by the fact that the values found by means of the optical methods are much smaller than those required by Fick's theory. Any further discussion of these methods is unnecessary in this place.

Marignac[*] published the next investigation on diffusion. He followed exactly the process of Graham; and his work differed from that of the latter only in this, that he observed the simultaneous diffusion of two salts, instead of that of solutions of single salts, as did Graham in most cases. We shall have occasion to refer to his work again.

About a year ago H. F. Weber published a very interesting paper, under the title "Untersuchungen über das Elementargesetz der Hydrodiffusion"[†]. For a description of the unique method employed by him I refer to his paper, and will mention here only some of the results. He experimented with solutions of $ZnSO_4$, and found the constant k for this substance corresponding to several different concentrations and temperatures of the solution. From his results it appears that k very slowly decreases in value as the concentration increases, and that, on the other hand, it increases very rapidly with the temperature, as shown by the following figures:—

$$k=0{\cdot}1252 \text{ when } T=1^\circ{\cdot}2 \text{ C.}$$
$$k=0{\cdot}2421 \text{ when } T=18^\circ{\cdot}5$$
$$k=0{\cdot}4146 \text{ when } T=44^\circ{\cdot}7,$$

which results can be expressed in the equation

$$k=0{\cdot}1187[1+0{\cdot}0557\,T].$$

Weber was led to conclude from his experiments that Fick's law is approximately correct.

Since Weber's still another investigation of the subject has appeared. In the Philosophical Transactions for 1861[‡] is an article by Graham, "On Liquid Diffusion applied to Analysis," which seems to have escaped the notice of succeeding investigators.

Stefan[§], however, has quite recently reviewed this work, and has deduced therefrom the values of k for a number of

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[*] *Arch. Phys. et Nat.* l. p. 79. *Compt. Rend.* lxxviii. (1874).

[†] *Vierteljahrsschrift der Züricher naturf. Gesell.* Nov. 1878. [Phil. Mag. December 1879.]

[‡] Also Liebig's *Annalen*, cxxi. p. 1.

[§] *Wiener Sitzungsber.* Jan. 1879. [Phil. Mag April 1879.]

substances. A short description of Graham's method will not be out of place here. Into a cylindrical glass vessel 87 millims. wide and 152 millims. high was poured 0·7 litre of water; and under this, by means of a small pipette, 0·1 litre of salt-solution was carefully brought. The water was displaced upward by the heavier solution; and the surface separating the two media remained sharply defined. The total length of the liquid column was 127 millims.

The vessel was now allowed to remain at rest a number of days; and at the end of that time the liquid was carefully drawn off, layer by layer, in sixteen equal portions, of 50 cubic centims. each. The amount of diffused salt was determined in each one of these portions separately, with the exception of the two lower ones, which were analyzed together.

As an example of these determinations the following may be given:—

Diffusion of a 10-per-cent. solution of NaCl in pure water for 14 days at 10° C.

No. of layer.	Grams of NaCl diffused.
1.	0·104
2.	0·129
3.	0·162
4.	0·198
5.	0·267
6.	0·340
7.	0·429
8.	0·535
9.	0·654
10.	0·766
11.	0·881
12.	0·991
13.	1·096
14.	1·187
15. 16.	2·266

Graham experimented with a large number of substances according to this method, and arranged the results in tables similar to the above. These results Stefan has made the subject of an analytical investigation, whose object was to ascertain whether or not they stand in harmony with the theoretical requirements of Fick's law. This he has shown to be not always the case. But in many instances, however, the agreement is very close, as illustrated in the following Table, found on page 4 of Stefan's paper:—

Diffusion of NaCl.

No. of layer.	Observed value.	Calculated value.
1. 2.	3·284	3·328
3.	1·527	1·482
4.	1·317	1·290
5.	1·057	1·073
6.	0·850	0·853
7.	0·640	0·648
8.	0·460	0·469
9.	0·318	0·325
10.	0·211	0·215
11.	0·134	0·135
12.	0·081	0·082
13.	0·051	0·047
14.	0·028	0·026
15.	0·017	0·015
16.	0·013	0·011

A better correspondence than the above could scarcely be expected, considering the number of unavoidable sources of error there are in connexion with the experimental method.

I have thus far mentioned briefly the chief results of the work done on diffusion. The several investigations may be divided into two classes:—(1) Those which are concerned with the physical side of the question—that is, the determination of the constant k for a single substance: to this class belong the investigations of Fick, Simmler and Wild, Voit, Hoppe-Seyler, Johannisjanz, Weber, and Stefan. (2) Those which are concerned with the chemical phase of the subject—that is, the comparison of many different substances as to their rates of diffusion: to this class belong the investigations of Graham, Beilstein, and Marignac. In regard to the first class, it may be said that there a very satisfactory end has been attained. The proof of Fick's law by Weber and Stefan, and the determination of the influence of temperature and concentration of solution by the former, leave little to be desired in connexion with this part of the subject. The same cannot be said, however, of the other—the chemical phase of the question. The experiments of Beilstein are too few to establish any thing with certainty; and those of Graham and Marignac, while rather numerous, and agreeing very well among themselves, seem to establish nothing as regards the dependence of diffusion on the molecular weight or other physical property of the substance employed. Their accuracy has also been questioned more than once*.

The importance of the subject seemed to make another investigation necessary; and this, at the suggestion of Professor Lothar Meyer, I have undertaken. The method described in the following pages was likewise proposed by him; and part of the experiments were conducted in his laboratory at Tübingen. Another part of the work was performed in the physical laboratory of Professor O. E. Meyer in Breslau. I would take this opportunity of expressing to both these gentlemen my sincere appreciation of the many kindnesses shown me by them during all the time spent in their laboratories.

The New Method.

There are several serious objections to the methods of Graham, Beilstein, and Marignac. In applying them, one is unable to follow the progress of the diffusion; that is, one cannot tell with any accuracy how much salt diffuses from hour to hour, or from day to day even, nor when the condition of constancy has been reached and how long it lasts.

These methods give only the average velocity of diffusion for the duration of the experiments. During the first days it may be relatively large, and during the last relatively small. Another objection to the methods is, that in them the diffusion takes place into a medium of constantly increasing density. This increase, being different for different substances, must exert in each case a different disturbing force, whose amount we have no means of determining. Hence it seemed desirable that a method should be employed in which these and other sources of error would be avoided. Such a method is the one proposed by Professor Meyer; and its chief features are (1) that it allows a determination of the rate of diffusion from hour to hour, or even at shorter intervals if necessary, and (2) that the diffusion takes place into a medium whose concentration is always zero.

How these ends are attained will be best understood from a consideration of the diagram and its explanation.

Fig. 1 (Plate VII.) represents the profile of the apparatus in natural size. *m n o p* is a beaker-glass into which fits the bent tube *a r s t b*, whose internal diameter is about one and a half millimetre. At *e f* this tube is joined to the larger tube *e f g h*, whose internal diameter is fifteen millims. This latter tube is open below; and the appearance of the whole as seen from above is represented in plan in fig. 2.

The tube *a r s t* and the beaker-glass are filled to the level

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* For instance, see Voit, *loc. cit.*

a b with the solution to be investigated. Distilled water is then allowed to drop very slowly from a Mariotte's bottle into the receiving-tube *w*. This water gradually displaces the solution in the whole length of *a r s t* and flows out at *b*. In *i* it stands at the level *e f*, though this is not independent of certain other influences to be mentioned shortly. Diffusion takes place between the solution below *e f* and the water above, the diffused particles being carried away and discharged at *b*. The concentration in *i* soon becomes less, and fresh portions of salt then enter from below and around *g h*, at which level the concentration changes, but very slowly, as above this is the whole of the solution in the beaker-glass. This is equivalent to saying that the diffusion takes place between a level of constant concentration at *g h* and a level of concentration zero at *e f*.

As a matter of course, the first portions of water which flow through the apparatus become nearly as saturated as the original solution; and some time must elapse before this mechanical mixing gives place to the real diffusion. The first experiments tried were on this point. The glass was filled with solution of NaCl at 8 h. 30 m. A.M. and the flow of the water started. The portion carried over until 9 h. was thrown away; that flowing after this time was saved and titrated with $\frac{n}{40}$ $AgNO_3$ solution*. The results for the first twenty-four hours were as follows, the velocity of the flowing water being 40 cubic centims. per hour:—

Time.		Cub. cent. $\frac{n}{40}$ $AgNO_3$.
9–10	required	80·0
10–11	”	22·0
11–12	”	11·0
12–1	”	8·1
1–2	”	6·4
2–3	”	6·1
3–4	”	5·9
4–5	”	5·7
5–9 A.M.	”	4·0 per hour.
9–10	”	2·2
4–5	”	2·0

And this rate continued for some length of time, as will be shown further on. It is seen that, although the progress of the diffusion is obscured at first by an excess of mechanically

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* Containing $\frac{1}{40}$ equivalent in grams of silver in a litre.

removed salt, this excess rapidly diminishes, until at the end of about thirty hours the titrations show a constant quantity of salt diffused per hour. The time required for this constant rate to be reached is not always the same, varying between twenty-four and forty-eight hours. Another experiment, made under exactly the same conditions as the above, with a similar tube, gave results as follows:—

Time.		Cub. cent. $\frac{n}{40}$ $AgNO_3$.
9–10	required	64·0
10–11	,,	17·0
11–12	,,	9·6
12–1	,,	7·6
1–2	,,	6·4
2–3	,,	6·0
3–4	,,	5·6
4–9 A.M.	,,	3·4 per hour
9–11	,,	1·9 ,, ,,
11–4	,,	1·9 ,, ,,

The constant rate was reached here in about twenty-four hours. Many other experiments gave similar results; but in no case was the time required less than twenty-four hours.

As was stated, the velocity of the flowing water in the above experiments was 40 cubic centims. per hour. Thinking it possible that this might have some influence on the rate of diffusion, experiments were tried in which this velocity was very variable. The beaker was first filled with normal $BaCl_2$ solution, and after the constant state had been reached the velocity of the water was varied, as in the following Table.

For convenience, the amounts of diffused salt will be given here and in the following in cubic centims. $AgNO_3$ used per hour in titration.

H_2O in cub. cent. per hour.	Cub. cent. $\frac{n}{20}$ $AgNO_3$ per hour.
42	1·17
45	1·17
47	1·22
53	1·18
60	1·15
88	1·05

The change of velocity seemed to have no disturbing influence in this case. To decide the matter fully, other solutions were operated upon in a similar way; and the results are given below. But perhaps it would be well to explain first how this changing the velocity of the H_2O can be readily accomplished. Into the caoutchouc stopper of the lower tubulure of a Mariotte's bottle was fitted a tube bent twice in planes at right angles to each other, and having the outward end drawn to a fine point. Now, turning the tube in the stopper has the effect of elevating or lowering the orifice—that is, of diminishing or increasing the distance between this and the lower end of the vertical air-tube in the flask. Upon this distance depends the velocity of efflux, that being proportional to its square root; and hence to attain the desired velocity it is simply necessary to move the orifice up or down, as the case may be. By this means it is easy to obtain a velocity varying not more than a cubic centimetre per hour for a week at a time.

n* KCl solution.
Duration of experiment
24 hours.

H_2O in cub. cent. per hour.	Cub. cent. $\frac{n}{40}$ $AgNO_3$ per hour.
62	1·69
58	1·60
57	1·54
47	1·69
37	1·69
30	1·62

$2n$ NaCl solution.
Duration of experiment
48 hours.

H_2O in cub. cent. per hour.	Cub. cent. $\frac{n}{40}$ $AgNO_3$ per hour.
28	2·74
30	2·80
30	2·73
64	2·62
65	2·73
66	2·71
70	2·64

NaCl solution, nearly $2n$.
Duration of experiment
26 hours.

Cub. cent. H_2O per hour.	Cub. cent. $\frac{n}{40}$ $AgNO_3$ per hour.
50	2·45
49	2·41
48	2·38
45	2·47
44	2·50

$2n$ NaCl solution.
Duration of experiment
5 hours.

Cub. cent. H_2O per hour.	Cub. cent. $\frac{n}{40}$ $AgNO_3$ per hour.
80	1·84
88	1·70
100	1·20

From the first four of the above Tables it is seen that, for velocities varying between 30 and 70 cubic centims. per hour, the movement of the water exerts no disturbing influence. In the fifth experiment the velocity of the water had been constant

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* *n* signifies "normal," *i. e.* 1 eq. in grams in a litre.

at 50 cubic centims. per hour, and when suddenly changed to 80 cubic centims. gave results as in the Table. This anomalous case will be explained when I come to speak of the influence of concentration.

From the Tables given above it is seen that the rate of diffusion is constant for 30 hours at least. To ascertain how much longer this constancy might continue was the object of the next experiments. Two tubes of the same size were filled, one with *n* and the other with 2*n* NaCl solution. After waiting until the rate of diffusion had become uniform, the titrations were commenced with the following results. The velocity of the flowing water was 40 cubic centims. per hour.

Duration of experiment.	Cub. cent. $\frac{n}{40}$ $AgNO_3$ solution required for	
	n sol.	2*n* sol.
4 P.M.– 8 A.M. = 16 hours.	12·8	26·0
8 –12 = 4 "	3·7	7·5
12 – 6 = 6 "	6·1	11·2
6 –12 = 18 "	17·7	34·6
12 – 4 = 4 "	3·5	7·8
4 – 8 = 16 "	15·8	31·2
8 –12 = 4 "	3·8	7·8
12 – 6 = 6 "	5·9	11·6
6 – 8 = 14 "	14·3	26·0
8 – 4 = 8 "	8·2	16·4
4 –12 = 20 "	20·5	40·5
12 – 6 = 6 "	6·5	11·8
6 –10 = 16 "	16·0	30·0
Total =138 "	134·8 c.c.	262·4 c.c.

As the results for the last days are no smaller than those for the first, it must be concluded that the duration of constant diffusion is rather long, for NaCl at any rate.

Indeed this seems to be true of the chlorides in general. In the case of the weaker of the above solutions the glass contained at the outset 14·5 grams salt, and the whole amount diffused was only 0·2 gram in the 138 hours. It is probable that the same rate would have been conserved for a much longer period.

The above Table also shows something regarding the effect of concentration. As stated, one solution was twice as strong as the other; and the titrations show that, of the stronger, nearly twice as much has diffused as of the weaker solution. This corresponds with the observation of Weber on $ZnSO_4$

solution, as well as with those of Graham on NaCl and other solutions. Other experiments on the influence of concentration were also tried. Three KCl solutions, *n*, 2*n*, and 3*n*, were prepared and diffused one after the other from the same tube. I titrated with $\frac{n}{40}$ $AgNO_3$ as usual, with the following results:—

n solution.		2*n* solution.		3*n* solution.	
Time.	Cub. cent. $\frac{n}{40}$ $AgNO_3$ per hour.	Time.	Cub. cent. $\frac{n}{40}$ $AgNO_3$ per hour.	Time.	Cub. cent. $\frac{n}{40}$ $AgNO_3$ per hour.
12–3	1·80	5– 8 A.M.	3·60	6–12 A.M.	5·83
3–5	1·75	8–10	3·65	12– 5	5·70
5–8 A.M.	1·66	10– 6	3·51	5– 9 A.M.	5·60
8–6	1·93	6– 9 A.M.	3·53		
6–9 A.M.	1·73	9– 1	3·62		
9–2	1·90	1– 3	3·55		

The results obtained from the *n* and 2*n* solutions agree very well with those obtained from the two NaCl solutions. In both cases the amount diffused from the 2*n* solution is slightly less than twice the amount from the *n* solution. But the results obtained from the 3*n* solution are somewhat anomalous and require an explanation. This will lead me to speak of the chief sources of error in connexion with the method.

In some of the earlier experiments of the investigation, solutions of the greatest possible concentration were used, in order to make the determinations of the diffused salt easy because of its greater quantity. But it was soon noticed that in such cases this amount was much larger than was expected. Five solutions of NaCl for instance, *n*, 2*n*, 3*n*, 4*n*, and 5*n*, diffused in the same time quantities of salt proportional to 1, 2·1, 3·8, 6·4, and 8.

Something similar was observed in the case of strong solutions of $CaCl_2$ and $SrCl_2$. These unexpected results seemed to point to the correctness of the opinion expressed by Beilstein*, that from different solutions of the same substance the amounts diffused increase at a greater rate than the concentrations. About this time, however, in the hope of clearing up the matter, another little experiment was tried, the results of which were so important for the understanding of the whole method that I will describe it fully here. To the water in the supply-bottle a small amount of indigo solution was added. The blue liquid flowing down through the tube *a r s t* displaced

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* *Loc. cit.*

the solution, which at the time being was rather concentrated, from the tube *i*. The amount of this displacement was much smaller than was expected. Instead of sinking to *e f* or lower, the blue layer remained stationary at *c d*; and the surface separating this from the solution was very sharply defined and perfectly reflecting. This remained the case during the day, and, left to itself several days longer, was but little changed; a sharply-defined surface was still to be seen.

This allowed of but one explanation, viz. that with a solution of this concentration the position of equilibrium between it and the water was such that their surface of separation could only be found quite high in the tube *i*. In such a case, not only diffusion but also a mechanical carrying-over of salt was observed. An NaCl solution of much smaller concentration (1·07) was next taken, and quite a different result obtained. At first the sharply-marked surface was seen as before, but lower in the tube, and in the position *e f* (fig. 1). Left until the next day the surface of demarcation was still very plain, but its reflecting-power was gone; and on the third day the surface itself had become indistinct, disturbed by the diffusion from below. The experiment was repeated several times with different solutions, and always with the same result; from which it was seen that in general for solutions of sp. gr. 1·10 and over, and for velocities of the flowing water not exceeding 70 cubic centims. per hour, the position of equilibrium of the liquids in the tubes was such that their surface of separation was above the line *e f*, and the progress of the diffusion remained for a great length of time obscured more or less by an excess of mechanically removed salt.

On the contrary, if the solutions be weaker, the surface of separation is found at the outset in the position *e f* or thereabouts. A real diffusion soon commences in *i*, and gradually extends itself to *g h*, below which point the concentration can decrease but very slowly, as explained above. It is now easy to account for the anomalous action of the 3 *n* KCl solution. This has a density of 1·13—that is, somewhat greater than that which was experimentally found to stand at the level *e f*. While in the case of solutions of medium density the diffusion may be considered as taking place between *g h* and *e f*, in this case it may be considered as taking place between the same lower level and an upper level *c d*, which is above *e f*, and more directly in the path of the flowing water.

One would expect in this case that more particles would be carried over than are really diffused; and this the results show.

By means of the blue water the influence of velocity can be nicely illustrated. The experiments undertaken to decide this point showed that for velocities up to 70 cubic centims. per hour the amount diffused was independent of this factor, but for velocities above this it rapidly decreased. It was not until the indigo-water was resorted to that this action was explained. It was then found to be as follows:—For ordinary velocities, the reflecting surface occupies in the beginning the position *ef* nearly, as shown above; and this is true whether the velocity be 30 or 70 cubic centims. per hour. However, if it be increased to 80 cubic centims., a change takes place: the reflecting surface no longer remains stationary at *ef*, but is pushed downward in *i* until finally the salt-solution is entirely expelled from this part of the apparatus, and blue particles ascend outside of *i* to the free surface above. The cause of this is evident. The water now flows in at *w* faster than it can be discharged at *b*, and it must make a place for itself by passing through *i* into the solution outside. It thus appears that the narrow opening at *b* exerts a rather great capillary resistance, which makes itself manifest as soon as the velocity of the water reaches a certain limit. The salt being expelled from *i*, allows the blue water to stream in visible currents outward and upward; and these currents, instead of permitting the diffusing particles below *g h* to pass into *i*, carry them along upwards on its outside. This continues, of course, until the increasing height of the liquid column in the beaker is capable of holding the water in equilibrium at the level *g h*. An undisturbed diffusion can then commence again; but some time must elapse before a condition of constancy is attained in *i*; and until this is the case the amount diffused must appear too small.

Something similar is observed when, instead of increasing the velocity of the flowing water, the capillary resistance at *b* is increased. Indeed, if this increase be but slight, the action is exactly the same as in the above case; but if the increase of resistance be greater, as when, for instance, the inside of the tube *b* is covered for a short distance with a thin film of wax, the action is as follows:—At first no liquid at all drops from *b*, as a convex meniscus is formed which exerts a pressure in an opposite direction. As a consequence the solution is expelled from *i*, the level moves upward in *w* and in the beaker, until the excess of pressure thus produced is sufficient to overcome the resistance at *b*. As soon as this limit is reached the action is entirely reversed: the water now commences to flow, and drops out quite rapidly; the level sinks in *w* and in the beaker; and the surface of separation rises in *i*. This continues until the excess of pressure has entirely disappeared, when the flow suddenly stops at *b*, and does not recommence until again a head of water accumulates as before. There seems to be no limit to this cycle of operations: on one occasion they were seen to take place during the whole of a day; and at the same time the amount of salt diffused was about one fourth of what was expected. The same phenomena were several times observed when, before filling the apparatus, the thorough cleansing of the tube *b* was neglected. A small amount of greasy substance at this point seemed to exert a very great disturbing force, and if not guarded against could become a source of serious error, as was the case in many of the earlier experiments, which were thus rendered quite worthless. I have described at some length the influence of excessive concentration of solution and the influence of capillarity at *b*, but at no greater length than the importance of this part of the subject demands.

It was only by guarding most carefully against these sources of error that constant results, and results comparable among themselves when different tubes were used, could be obtained. It has been seen that the height *g e* at which the solution stands in *i* at the beginning of the experiment, and upon which depends the amount of salt diffused, depends in turn, not only on the concentration, but also to a certain extent on the velocity of the water, and on the amount of the resistance at *b*. By keeping these elements within certain limits constant results can be obtained, even when different tubes are used, as shown below.

Thus far nothing has been said of the influence of temperature on the progress of diffusion. During the continuance of the above experiments, most of which were conducted in the cellar of the laboratory at Tübingen, the temperature remained quite constant at 14°–15° C. Experiments undertaken to decide this point of the influence of temperature were not very successful, and chiefly for the reason that it was found very difficult to keep any other than the ordinary temperature constant for the necessary length of time. The experiments were arranged as follows:—In a wooden box with double walls, used as an ice-chest, were placed two diffusion-cells with the necessary water-flasks. The remaining space on the bottom of the box was occupied by vessels containing pounded ice. Two thermometers were hung in different parts of the box. It was possible by this means to obtain a temperature of 10° C. inside the box when that of the outside was 15° C. But the difficulty lay in keeping this 10° constant long enough to complete an experiment—that is, for four or five days. The chest was not large enough to contain a great deal of ice; and replenishing this from day to day disturbed the glasses always more or less. Refilling the water-flasks had also a disturbing effect. Taking these facts into consideration, it cannot be wondered at that at this temperature the results were rather inconstant. The tubes gave, when filled with *n* NaCl solution, at 15° an amount of salt represented by 1·1 cubic centim. $_{40}$AgNO$_3$ per hour. But usually, when in the ice-chest, the salt diffused from similar solutions required for its titration 1·5–2 cubic centims. $\frac{n}{40}$AgNO$_3$ per hour, indicating that the progress of the diffusion was disturbed. On one occasion, however, for a period of about 14 hours, 6 P. M. to 8 A.M., the diffused amount was equivalent to 0·87 cubic centim. per hour. This rate did not continue during the next day; opening the box to renew the ice and water disturbed it as usual. The experiments at a higher temperature were somewhat more successful. Instead of the ice a Bunsen burner was placed in the box, and a tolerably constant temperature of 25° was obtained. The amount of AgNO$_3$ solution required now per hour varied between 1·3 and 1·4 cubic centim. Assuming that for the lower temperature the result 0·87 cubic centim. was correct, the amounts diffused from solutions of the same strength at three different temperatures may be thus tabulated:—

At 10°, 0·87 cub. cent. $\frac{n}{40}$ AgNO$_3$ required per hour.
„ 15°, 1·10 „ „ „ „
„ 25°, 1·35 „ „ „ „

But these results can only be considered approximately correct.

[To be continued.]

XLII. *Relative Intensity of the Spectral Lines of Gases.*
By J. Rand Capron, *F.R.A.S.*[*]

In reference to M. Ch. Fievez's "Researches on the Relative Intensity of the Spectral Lines of Hydrogen and Nitrogen in regard to the Constitution of Nebulæ," which appears in your present month's Number, and a copy of which the author has obligingly sent me, it may perhaps be useful to refer to a few experiments detailed in my 'Auroræ and their Spectra,' chapter xi. p. 108 *et seq.*

These were made for the purpose of testing how far it was possible or probable that the bright green and red lines in the aurora-spectrum might be the brighter and only visible rays

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[*] Communicated by the Author.

of a more complicated spectrum, concealed partly by want of brightness in the aurora itself, and partly by the medium, more or less dense, in which that phenomenon probably occurs. As Geissler's tubes were at the time under examination, they were found convenient for this purpose. Nitrogen- (in the shape of "air"-) tubes were also used; but I do not find they were specially tested; at least they are not recorded. I have, however, a recollection that the bright double line in the green survived all others. The other results are taken from notes made at the time.

The spectroscope was a direct-vision one, by Browning, giving a dispersion equal to about two white glass prisms of 60°. Collimator and telescope 6 inches long respectively, and carrying 1-inch aperture lenses. Slit about $\frac{1}{300}$ of an inch wide. Source of light Geissler tubes excited by a $\frac{1}{2}$-inch-spark coil.

Hydrogen tube.—Tube showing ordinary hydrogen-lines, with some others probably due to impurity. At 6 inches from slit, α (solar F) was very bright; the lines β, γ, δ, ϵ, and ζ also seen, but faint. At 12 inches from the slit, F and γ were alone seen; and at 24 inches, F stood by itself alone upon a dark ground.

Carbonic-acid tube.—At 18 inches from the slit the continuous spectrum and fainter lines disappeared, while the four principal lines still shone out, that in the green being the strongest. At 24 inches same lines visible, but faintly.

Coal-gas tube.—At 24 inches whole spectrum quite brilliant, the four principal lines very bright, and preserving their distinctive colours.

Oxygen tube (impure).—At 12 inches distance from the slit the spectrum lost nearly all its light. F hydrogen and three oxygen lines, α, β, and γ, alone remained. At 24 inches no spectrum at all seen.

It was noticed in the foregoing experiments that, on withdrawing the tube from the slit, the colours of the spectrum disappeared in the following consecutive order—red, yellow, violet, and, lastly, green. It thus seems pretty clearly proved that the brighter line or lines of a spectrum may be seen singly as a matter of intensity.

This, however, must be treated as independent of the exaltation or suppression of individual lines of a spectrum by such causes as temperature, pressure, magnetism, &c.

It is easy to conceive that the celestial bodies may be liable to these influences to a very considerable extent, and that modifications of their spectra may thus arise which we do not produce in the laboratory and which are not dependent on the question of intensity alone.

Guildown, April 2, 1880.

XLIII. *On the Determination of Chemical Affinity in terms of Electromotive Force.*—Part II.* *By* C. R. Alder Wright, *D.Sc.Lond., Lecturer on Chemistry and Physics, and* E. H. Rennie, *M.A. (Syd.), B.Sc. (Lond.), Demonstrator of Chemistry in St. Mary's Hospital Medical School.*

Experimental Determination of the Electromotive Force corresponding to the Work done in the Decomposition of Water into Oxygen and Hydrogen at the ordinary temperature.

38. In order to apply the principles described in Part I. (§ 11, 18, 32), the current from a Daniell battery was passed through a voltameter placed in a calorimeter, and the average difference of potential between the voltameter-plates determined by connecting them with a quadrant electrometer standardized by a Clark cell (verified for us by Dr. Alexander Muirhead). The amount of decomposition being determined ($=n$ grammes), and the quantity of heat, h, developed in the voltameter observed, the data were obtained for the calculation of the E.M.F. representing the nett work corresponding to the sum of the physical and chemical changes taking place by the formula

$$e=\mathrm{E}-\frac{ha\chi\mathrm{J}}{n},$$

a being the equivalent of the electrolyte.

In the case of water acidulated with sulphuric acid, the following results were obtained. The voltameter consisted of a wide test-tube of about 30 millimetres diameter, into the neck of which was fixed an india-rubber cork perforated with three holes: through the centre one passed a delivery tube of small bore for the collection of the evolved gases; through the other two, copper rods 5–6 millims. in diameter. To the lower ends of these were soldered equally thick platinum rods, the free ends of which were previously forged into spade-like plates which were arranged parallel to one another; the solderings and the portions of the copper rods inside the test-tube were imbedded in a thick mass of gutta-percha, with the threefold object of keeping the plates at an invariable distance from one another (the voltameter being also intended for some other experiments in which this was essential), of protecting the copper and soldering from corrosion by the acid and the consequent introduction of metals into the solution electrolyzed, and of filling up the upper space in the tube, so that any error due to alteration in temperature of the voltameter and conse-

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* Part I., *suprà*, pp. 237–266.

quent retention therein of varying quantities of gas should be rendered negligible. The projecting ends of the rods were passed through small india-rubber corks and amalgamated; by then fixing short pieces of wide glass tubing over these smaller corks and pouring mercury into the cups thus formed, connexion by mercury-cups could readily be established between the battery and the voltameter-plates.

This arrangement of mercury-cups was also applied to the copper Daniell-battery cells, a thick wire bent downwards and amalgamated at the end being also soldered to each zinc plate so as to dip into the mercury-cup of the next cell. Much trouble in brightening connexions &c. was thus saved; the current could readily be broken instantaneously by simply lifting one of the zinc plates an inch or so, so that the wire no longer dipped into the mercury; whilst an easy means was afforded of introducing more cells or shutting some out from the circuit when required without actually interrupting the current.

39. A number of attempts were made to utilize Bunsen's ice-calorimeter for the measurement of the heat evolved; the construction of apparatus sufficiently large to enable considerable amounts of heat to be measured accurately, however, was found to present some difficulties; whilst with smaller apparatus the errors of measurement of the amount of gas evolved became considerable, even when the voltameter was connected with the gas-measuring apparatus used for Frankland and Armstrong's water-analysis process; for the occlusion of the evolved gases by the electrodes, the absorption of oxygen by the acidulated water of the voltameter, the production of ozone or of hydrogen dioxide, and the removal of hydrogen whilst still nascent from the one electrode by combination with dissolved oxygen, and of oxygen from the other by combination with dissolved hydrogen, present sources of diminution in the amount of gas actually evolved, which mostly become relatively greater with weak currents, such as would have to be employed with a small ice-calorimeter. Although it did not appear to be impracticable to overcome these difficulties, yet it was found that to do so would require a considerable amount of time and trouble; and therefore we reverted to the use of an ordinary water-calorimeter, employing currents of sufficient magnitude to furnish upwards of 500 cub. centims. of mixed gases during the time which the experiment lasted (from ten to forty-five minutes, according to the battery power used). Even with the strongest currents used, representing about 0·65 C.G.S. current-unit, the amount of substances formed capable of liberating iodine from potassium iodide corresponded to no more than ·001 to ·002 gramme of

iodine per 500 cub. centims. of gas produced, representing a diminution in the volume of gas evolved practically quite inappreciable. The gas-measuring apparatus in our possession being incapable of measuring so large a quantity as 500 cub. centims., we employed the method used by Joule for determining the volume of gas produced, viz. collection over water in a bottle, the weight of which was known when filled with water and closed by a stopper. To determine the gas collected, the bottle was immersed for some minutes in a large bucket of water, the temperature of which was known; when the gas had attained the temperature of the water, the bottle was raised until the level of the fluid inside and out was the same (*i. e.* until the pressure was atmospheric); the stopper was then inserted and the bottle removed, wiped dry, and weighed. In this way a close approximation to the quantity of gas produced was obtained, the error being one of defect, owing to the sources above mentioned and the absorption of oxygen by the water during the process of collection. From the number of cub. centims. of mixed gases thus obtained (reduced to dryness, 0° C., and 760 millims.) $=v$, the weight of water decomposed, w, was calculated by the formula

$$w=v\times 0\cdot0005363,$$

the coefficient 0·0005363 being deduced from Regnault's observations that 1 litre of dry hydrogen and oxygen at 0° and 760 millims. weigh respectively 0·089578 and 1·429802 gramme,

whence 2 grammes of hydrogen occupy 22·327 litres,
and 15·96 ,, oxygen ,, 11·162 ,,

so that 17·96 grammes of detonating gas occupy 33·489 ,,

or 1 cub. centim. of detonating gas weighs $\frac{17\cdot96}{33489}=0\cdot0005363$ gramme.

40. The calorimeter employed in these experiments consisted of a glass beaker capable of holding about 1500 cub. centims., fitted closely inside a polished tin cylinder supported (on a wooden block and several folds of wadding) concentrically inside a similar tin cylinder, so that about an inch of air-space everywhere intervened between the two tin surfaces, the two tin cylinders being kept in the same relative positions by cork wedges. This outer cylinder was weighted with lead and sunk inside a third similar larger cylinder filled with water; so that the calorimeter itself was surrounded on all sides, saving the top, by a water jacket. Polished tin lids for the calorimeter and the outermost vessel were provided, perforated with holes for the passage of the voltameter (in the axis of the cylinders),

of the thermometers employed, and of the stirrers; these latter consisted of annular horizontal metallic plates, with stout vertical wires attached, so that by moving them up and down a very effective agitation could be accomplished. The thermometers were graduated in millimetres, and were carefully calibrated and checked against one another and against a Kew-standardized thermometer. The one used for the inner vessel was found to be sensibly uniform in calibre at the part where the observations were made (chiefly that corresponding to 12°–20° C.); and at this part the value of 1° C. was 11·28 millims. Several careful determinations of the water-equivalent of the calorimeter (including the stirrer, thermometer-bulb, and voltameter, which always contained 20 grammes of acidulated water containing 22 per cent. of H_2SO_4,) gave numbers varying from 1092 to 1097, and averaging 1094·5 grammes when 1000 grammes of water (weighed in a vacuum) were placed therein.

41. In order to obtain with as great a degree of accuracy as possible the corrections for radiation with this instrument, a large number of observations were made of the rate of alteration of the reading of the calorimeter-thermometer under various conditions, viz. when the water jacket was hotter or colder than the calorimeter, and when the calorimeter was warmer or colder than a thermometer-bulb placed a few inches above the outer lid, so as to indicate the temperature of the air together with the effect produced by radiation from the hand during stirring. It was found that all the observations could be expressed with a fair degree of accuracy by the formula

$$x = 0{\cdot}16\,(\mathrm{M} - m) + 0{\cdot}0225\mathrm{N},$$

where x is the alteration of the calorimeter-temperature (*loss* of heat) in millimetres per hour, M the difference between the average readings of the calorimeter and water-jacket thermometers during the period of observation, m a small correction (derived from a specially constructed table) to reduce the scale-reading of the second of these thermometers to that of the first, and N the difference between the average temperature of the calorimeter and that of the air, as indicated by the third outer thermometer and expressed in terms of the scale of the calorimeter-thermometer. Thus, for example, the following table illustrates the observed and calculated values of x. It is noticeable, *en passant*, that whilst the heating or cooling effect of the air on the calorimeter exerted through the double lid is, for equal differences of temperature, much less than that of the water jacket, it is still not negligible, amounting to about $\frac{1}{7}$ of that of the jacket.

Water jacket warmer than room. Calorimeter colder than jacket and warmer than room.		Water jacket colder than room. Calorimeter colder than jacket and colder than room.		Water jacket colder than room. Calorimeter slightly warmer than jacket and colder than room.		Water jacket colder than room. Calorimeter warmer than jacket and slightly colder than room.	
Obs.	Calc.	Obs.	Calc.	Obs.	Calc.	Obs.	Calc.
−4·8	−4·9	−8·0	−8·2	+0·2	+0.1	+2·3	+2·3
−4·0	−4·1	−7·4	−7·3	−0·2	−0·2	+1·9	+1·9
−3·6	−3·6	−7·0	−6·7	−0·7	−0·6	+1·2	+1·4
−3·2	−2·7	−5·1	−5·4	−1·0	−1·0	+1·0	+1·3
−2·4	−2·0	−4·8	−4·8	−1·5	−1·2	+0·8	+0·8
−1·5	−1·3	−4·3	−4·3	−1·4	−1·2	+0·3	+0·3
−0·9	−0·7	−3·8	−3·9	−1·0	−1·1	+0·2	0
−0·1	0	−3·6	−3·5				
+0·9	+0·7						
+1·2	+1·3						
+1·5	+1·8						
+1·8	+2·1						

42. It is evident from these numbers that the difference between the observed and calculated rates of alteration per hour rarely exceeds 0·2 millimetre, and averages much less; so that the difference between the results deduced by the formula and those attained by direct observation is practically negligible for all experiments lasting for only half an hour or less. In order, however, to diminish any error in the radiation-correction caused by the use of the formula, a threefold determination was made, as follows, the average of the three observations being taken. First, the correction was estimated from the observed average values of M and N throughout the experiment. Secondly, the rate of alteration of temperature of the calorimeter (rising) before the experiment was commenced was determined by observations made for 30–60 minutes before commencing; and, similarly, the rate of alteration (falling) was determined by observations made for 30–60 minutes after concluding, and the mean of the two rates taken. Thirdly, the rate of alteration at the commencement of the experiment was calculated from the values of M and N then observed; and similarly the rate of alteration at the close was calculated from the then values of M and N, and the mean taken as before. The three values rarely differed from their mean by so much as 0·1 millimetre per hour. Thus the following values were obtained in an experiment lasting eighteen

minutes, the + value meaning loss of heat, and the − sign gain*.

			Millims. per hour.
Value of x obtained by first method (formula applied to mean values of M and N)			−1·00
Value of x obtained by second method.			
Observed rate of alteration before experiment commenced	−4·1	Mean	=−1·05
Observed rate of alteration after experiment concluded	+2·0		
Value of x obtained by third method.			
Calculated rate of alteration at commencement	−4·0	Mean	=−1·02
Ditto at close	+1·95		
		Average	−1·02

Hence the radiation-correction for the eighteen minutes during which the experiment lasted is $-\frac{18}{60}\times 1{\cdot}02=-0{\cdot}31$.

43. The above correction, however, is not the only one to be applied; for the water in the voltameter gets heated more quickly than the calorimeter, and after the end of the experiment some time is required for the heat retained in the voltameter to become uniformly diffused throughout the calorimeter by passage through the glass wall of the voltameter. It was found that 6 or 7 minutes sufficed to bring about perfect equalization of temperature; so that during the first 5 minutes or so the temperature of the calorimeter slightly rose, and then began to fall again from the cooling effect of the radiation. Accordingly, to determine the true amount of rise in temperature during the experiment, the thermometer was read 8, 10, and 12 minutes after the conclusion—the loss by radiation during these several periods of 8, 10, and 12 minutes being added on to the observed temperature at the ends of these periods severally; the mean of the three numbers thus obtained was taken as the true temperature that would have been observed at the end of the experiment had the heat retained in the voltameter been diffused throughout the calorimeter at that moment. The loss during these three periods is calculated from the data in the latter two methods just

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* This particular experiment is selected as an illustration because the temperature-corrections detailed in this and the subsequent paragraphs are *larger* than those in almost any other of the 18 experiments made in all (§ 46).

described for the determination of the radiation-correction during the experiment: thus, in the illustration given,

	Millims. per hour.
Observed rate of alteration after experiment concluded	+2·0
Calculated ” ” ”	+1·95
Mean	+1·97

Hence the losses by radiation during 8, 10, and 12 minutes are respectively, +0·26, +0·33, and +0·39; and the true rise in temperature of the calorimeter is found thus :—

	$t=8$.	$t=10$.	$t=12$.
Reading of calorimeter-thermometer t minutes after conclusion of experiment	201·05	201·0	200·9
Radiation-loss during t minutes	+0·26	+0·33	+0·39
Calculated temperature of calorimeter at close of experiment	201·31	201·33	201·29

Average	201·31	millims.
Reading of calorimeter-thermometer at commencement of experiment . . .	166·7	”
Calculated rise of thermometer	34·61	”
Correction for radiation during experiment	− 0·31	”
Corrected rise of thermometer	34·30	”

Since the water-equivalent of the calorimeter &c. is 1094·5, and 11·28 millims. of the thermometer-scale represent 1° C., the amount of heat actually produced is

$$\frac{34\cdot30}{11\cdot28}\times 1094\cdot5=3328 \text{ gramme-degrees.}$$

44. Two other small corrections also have to be applied to the heat-evolution thus determined. In the first place, the escaping gases pass out saturated with moisture, the evaporation of which absorbs a certain amount of heat. In each experiment the average temperature was close to 15°, at which temperature the tension of aqueous vapour is 1·27 centim. or $\frac{1\cdot27}{76}$ atmosphere, =1·67 per cent. Hence every 1000 cub. centims. of evolved gas would contain 16·7 cub. centims. of water-vapour (at 0° and 760), weighing 0·0134 gramme; the

latent heat of water-vapour at 15° being nearly 600, this would represent very nearly 8 gramme-degrees per litre of evolved gases. Inasmuch as the gases were evolved not from pure water but from water containing 22 per cent. of sulphuric acid, it is likely that the gases actually evolved contained slightly less moisture than the normal saturating amount; but as in most of the experiments only about 500 to 550 cub. centims. of gases were evolved, the total correction is only about 4 gramme-degrees, and a trifling error in its estimation is negligible.

In the next place a minute amount of heat is absorbed in separating from sulphuric acid the water electrolyzed. From the experiments of Favre and Quaillard (*Comptes Rendus*, l. p. 1150), 98 grammes of sulphuric acid already mixed with 216 of water (forming a dilute acid of composition $H_2SO_4+12H_2O$) evolve 483 gramme-degrees when 72 grammes more of water are added (forming $H_2SO_4+16H_2O$). The further addition of 72 grammes more of water evolves 222 gramme-degrees, whilst the addition of a third 72 grammes (forming $H_2SO_4+24H_2O$) evolves 141 gramme-degrees. From these numbers, by interpolation, it results that acid of strength $H_2SO_4+20H_2O$ (containing 22 per cent. of H_2SO_4) will evolve close upon 2·5 gramme-degrees when 1 gramme of water is added to such a quantity of acid as not sensibly to alter its composition by dilution; *i. e.* that for every gramme of water decomposed by electrolysis 2·5 gramme-degrees are absorbed.

In the experiment the calorimetric details of which have just been given, the corrected heat-evolution finally becomes as follows:—

	gramme-degrees.
Heat-evolution calculated from rise of thermometer corrected for radiation &c. . . .	3328
Correction for heat absorbed by moisture in 0·4967 gramme of mixed oxygen and hydrogen evolved during experiment . . .	9·2
Correction for heat absorbed in separating 0·4967 gramme of water from dilute sulphuric acid	1·2
	3338·4

45. The value of E, the average difference of potential between the voltameter plates, was found thus. Before the experiment commenced the scale of the electrometer was standardized by means of a Clarke cell, several readings being taken by reversal so as to obtain total readings each equal to double the

deflection; after the experiment was concluded the scale was again similarly standardized. The difference between the two standardizings was always very small or nil, but frequently was just perceptible, owing to leakage of electrometer charge; the mean of the two standardizings was taken to represent a potential diference of

$$1{\cdot}457\{1-(t-15)\times 0{\cdot}0006\} \text{ volt},$$

where t is the Centigrade temperature, and 0·0006 the alteration in E.M.F. per 1° C. (equal to 0·06 per cent. per 1°). As in the whole series of experiments taken together the average temperature of the cell was very close to 15°, the cell was assumed to have the constant average value 1·457 volt throughout. Throughout the experiment the potential difference between the electrodes was determined at as nearly as possible equal intervals of time by reversal, the quadrants being connected with the mercury-cups of the voltameter through the reversing-gear. As a general rule, the indicated potential-difference rose steadily throughout the experiment to the extent of from 3 to 6 per cent. of its initial value (owing to polarization of the electrodes); but in some cases this effect was just about compensated by a gradual diminution in current-strength, so that the potential difference remained nearly the same or slightly fell*. In the experiment the calorimetric details of which have just been given, the following values were obtained:—

	Scale-divisions.
Value of Clark cell before experiment . .	80·65
,, ,, after ,, . .	80·55
Mean	80·60

Average of 23 pairs of readings of potential difference be-

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* The fluctuations in the current and in the potential difference set up between the electrodes necessarily cause corresponding fluctuations in the amount of decomposition per second and the quantity of heat developed per second, and hence are allowed for, and (if not too great in extent) produce no effect at all on the end result. In just the same way, it is immaterial whether the electrodes are perfectly parallel and equal in size, whether the current passes more between their edges than between their centres, of what material they are made, and so on; for what is actually done is to measure the difference of potential between the mercury-cups at the ends of the thick copper rods connected with the electrodes, and also the quantity of electricity passing, and hence the total work done between the cups. The resistance of the cups and thick copper rods being inappreciable, all this work must be done inside the voltameter between the electrodes, either as heat or chemical decomposition; the former being measured directly, the amount of the work representing the latter is known by difference.

tween voltameter-plates (commencing with 237·5 and slowly sinking to 231·0) = 232·40.

Average potential-difference in volts,

$$\frac{232{\cdot}40}{80{\cdot}60}\times 1{\cdot}457=4{\cdot}201.$$

Hence, taking $a=8{\cdot}98$, $\chi=0{\cdot}000105$, and $J=42\times10^6$, the value of e in the expression

$$e=E-\frac{ha\chi J}{n}$$

becomes $1{\cdot}540\times10^8$, since $E=4{\cdot}201\times10^8$, $h=3338$, and $n=0{\cdot}4967$.

46. In precisely the same way the following numbers were obtained in seventeen other experiments.

Battery-power employed.	Time in minutes.	E. Average difference of potential between electrodes in volts.	*n*. Grammes of water decomposed.	*h*. Heat evolved		Value of $\frac{h}{n}a\chi J$.	Value of $E-\frac{h}{n}a\chi J$.
				Corrected for radiation only.	Further corrected for the other two sources of heat-absorption. (§ 44)		
4 Daniell cells.	44	2·995	·2821	1091	1096	1·539 $\times10^8$	1·456 $\times10^8$
4 "	41	3·065	·2740	1094	1099	1·589 "	1·476 "
4 "	34	3·134	·2764	1126	1131	1·621 "	1·513 "
5 "	24	3·336	·2687	1122	1227	1·808 "	1·528 "
6 "	23	3·380	·2695	1289	1294	1·901 "	1·479 "
6 "	22	3·369	·2676	1262	1267	1·875 "	1·494 "
8 "	16	3·714	·2785	1543	1548	2·200 "	1·514 "
8 "	15	3·685	·2794	1567	1572	2·228 "	1·457 "
8 "	14	3·746	·2765	1552	1557	2·230 "	1·516 "
10 "	14	3·777	·2708	1553	1558	2·278 "	1·499 "
10 "	13·5	3·845	·2815	1659	1664	2·341 "	1·504 "
12 "	13	3·805	·2708	1534	1539	2·250 "	1·555 "
12 "	13	3·830	·2729	1606	1611	2·337 "	1·493 "
12 "	12	3·979	·2877	1833	1838	2·530 "	1·449 "
12 Daniells and 6 Groves in weak action.	18	4·201	·4967	3328	3338	2·661 "	1·540 "
Ditto	17	4·242	·4934	3368	3378	2·711 "	1·531 "
12 Daniells and 6 Groves in full action	9·25	4·393	·3379	2479	2484	2·912 "	1·481 "
Ditto	8	4·442	·2853	2100	2105	2·921 "	1·521 "
						Average ...	1·5003 $\times10^8$

Maximum deviation
from mean.

On side of excess . $\frac{1{\cdot}555-1{\cdot}5003}{1{\cdot}5003}=3{\cdot}65$ per cent.

On side of deficiency $\frac{1{\cdot}5003-1{\cdot}449}{1{\cdot}5003}=3{\cdot}42$ „

		per cent.
"Probable error" of one determination	$\pm 0{\cdot}0202=$	$\pm 1{\cdot}34$
„ „ result . . .	$\pm 0{\cdot}0048=$	$\pm 0{\cdot}32$

Experiments on a possible Source of Error in Joule's *Valuation of* J *made in* 1867 (Brit. Assoc. Reports).

47. The method used by Joule in these experiments consisted essentially in the comparison of the resistances of a platinum-silver wire (made into a coil and immersed in a calorimeter) and of copies of the B.A. unit, and then passing a current, measured by a tangent-galvanometer, through the coil and determining the amount of heat produced therein in a given time. The mean temperature of the calorimeter throughout a series of observations being determined, the resistance which the wire would possess at that temperature was calculated from the observed resistance at some other temperature, the rate of variation in resistance of the wire with temperature used being known. Calling this calculated resistance R, the average current being C, and the time t seconds, the value of J was deduced by the formula $J=\frac{C^2Rt}{H}$, where H is the heat evolved.

It is evident that any error in the valuation of R must produce an error to exactly the same extent in the value of J thus deduced; and that such a source of error must have existed is evident from the circumstance that inasmuch as heat was continuously developed in the interior of the wire, and as an interval of time must necessarily elapse before heat could pass from the interior of the wire into the water by conduction, the temperature of the interior of the wire, and consequently the mean temperature of the wire as a whole, must have been somewhat higher than the mean temperature of the calorimeter. Apart from this, too, the wire must have been heated to a small extent above the whole mass of water in the calorimeter, owing to the adhesion to the wire of a film of warmed water which could not possibly be displaced by stirring absolutely *pari passu* with its becoming warmed. The very efficient automatic continuous stirring-arrangement adopted by Joule in the third series of investigations detailed in the paper referred to must necessarily have reduced this source of superheating of the wire to a minimum; but it is doubtful if it would wholly remove the additional tendency to superheating from this cause.

48. In order to form some idea of the possible extent to which the wire might thus become superheated by currents of the magnitude used by Joule*, a platinum wire about 50 centims. long and 1·15 B.A. unit resistance was twisted into a spiral, the ends of which were soldered to thick copper rods surmounted with mercury-cups and passing through an india-rubber cork to keep them together: this spiral was immersed in a beaker containing about a litre of distilled water, the temperature of which was read off to 0°·1 degree by a thermometer placed with its bulb almost touching the centre of the spiral; by means of a broad horizontal ring of metal attached to a stout wire the water could be briskly agitated, to a much greater extent indeed than would be safe with a calorimeter in which splashing must be avoided. Currents of different strengths were then passed through the coil for periods of time varying from 20 minutes to an hour, a voltameter being also included in the circuit, and the total gas evolved collected as previously described. During this time the water was kept continually stirred, and its temperature observed at equal intervals of time, whilst the difference of potential existing between the ends of the wire was observed as frequently as possible by connecting the mercury-cups with a quadrant-electrometer standardized by a Clark cell before and after the experiment, precisely as above described. During the first two or three minutes the electrometer-readings always diminished through diminution of current by polarization of the voltameter-plates; afterwards they remained nearly constant, gradually rising through the increasing mean temperature of the wire, or slightly sinking if the current-strength diminished through continued use of the battery. During the first few minutes the readings were accordingly taken every 10 or 15 seconds without reversal, so as to obtain a large number of observations giving the mean potential difference during this period; subsequently the readings were taken by reversal at the rate of one pair per minute, the readings being divided into a

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* In Joule's experiments the galvanometer made one turn only of 0·62723 foot radius, the angular deflection varying from 26° to 32°; hence the currents must have lain between $\frac{0·62723\times30·48}{2\pi}.\mathrm{I}.\tan 26°$ and $\frac{0·62723\times30·48}{2\pi}.\mathrm{I}.\tan 32°$, where 30·48 is the number of centims. in a foot and I the horizontal terrestrial magnetic force. Taking $\mathrm{I}=0·18$, the currents must therefore have averaged about 0·3 C.G.S. unit. The resistance of the platinum-silver wire used was nearly equal to 1 B.A. unit $=10^9$ C.G.S. units.

number of groups corresponding to equal intervals of time (3 or 5 minutes), the average readings for each interval being again averaged to give the mean reading for the whole period; so that for an hour's observation more than 120 total readings were taken.

49. In this way the following numbers were obtained, the amounts of gas collected being translated into C.G.S. current-units by the formula

$$C = \frac{V \times 0{\cdot}0005363}{T \times 60 \times 8{\cdot}98 \times 0{\cdot}000105} = \frac{V}{T} \times 0{\cdot}09479,$$

where C is the C.G.S. current, V the volume of gas (dry, at 0° and 760 millims.), T the time in minutes, and 0·0005363 the weight in grammes of a cubic centim. of mixed gases at 0° and 760 (§ 39); *i. e.* where $\frac{V}{T}$ is the volume of gas evolved per minute. The resistance of the wire is calculated from Ohm's law $\left(C = \frac{E}{R}, \text{ whence } R = \frac{E}{C}\right)$ by dividing the average potential difference by the current. Every experiment was made as nearly as possible in the same way, so as to diminish sources of error and make the results comparable, the only noteworthy difference being that with the stronger currents shorter times were given, and the beaker of water in which the wire was immersed was slightly cooled at intervals during the experiment to avoid the temperature rising too high and having too wide a range.

No. of cells in battery.	Time in minutes.	Gas evolved in cub. cent., dry at 0° and 760 mm.	Average G.C.S. current.	Temperature of water.			Average potential-difference in C.G.S. units.	Resistance in C.G.S. units.
				Initial.	Final.	Average.		
3	60	256·6	0·0406	8·5	10·0	9·24	0·4655 $\times 10^8$	1·147 $\times 10^9$
3	60	234·5	0·0371	7·5	9·5	8·36	0·4275 „	1·152 „
		Average	0·03885	……	……	8·80	………	1·1495 „
4	30	244·3	0·0772	7·9	9·2	8·50	0·8931 „	1·157
6	30	461·7	0·1459	7·4	9·0	8·20	1·697 „	1·163 „
9	30	777·9	0·2458	7·2	11·0	9·16	2·881 „	1·171 „
9	20	493·4	0·2339	7·4	9·7	8·58	2·732 „	1·168 „
		Average	0·2398	……	……	8·87	………	1·1695 „
12	20	624·7	0·2961	7·1	10·1	8·64	3·469 „	1·172 „
12	20	638·6	0·3027	7·6	10·8	9·18	3·549 „	1·172 „
		Average	0·2994	……	……	8·91	………	1·172 „

Since the average temperature of the water surrounding the wire was nearly the same throughout, not differing by more than a few tenths of a degree from the mean 8°·75, the values in the last column are fairly comparable with one another; whence it is evident that a notable rise in resistance of the wire is produced by each successive increase in current-strength. The resistance of the wire for a current of very small value would clearly lie under $1\cdot1495 \times 10^9$; so that the increase in resistance caused by currents of from 0·24 to 0·30 C.G.S. unit is *at least* $\frac{1\cdot1695-1\cdot1495}{1\cdot1495}$ to $\frac{1\cdot172-1\cdot1495}{1\cdot1495}$, or 1·7 to 2·0 per cent.

50. In order to see what increase in mean temperature of the wire over that of the surrounding water this would represent, the fourth experiment was repeated, using warm water (at about 27°) to surround the coil: in this way the following numbers were obtained:—

	C.G.S. current.	Average temperature.	Potential-difference.	Resistance.
Water warmed .	0·1419	27·25°	$1\cdot716 \times 10^8$	$1\cdot209 \times 10^9$
,, cold . .	0·1459	8·20	$1\cdot697 \times 10^8$	$1\cdot163 \times 10^9$
	Difference .	19·05		$0\cdot046 \times 10^9$

Since an increase in resistance of 0·046 corresponds to 19·05 degrees, an increase of 0·0200 to 0·0225 would correspond to 8·3 to 9·3 degrees; *i. e.* currents of 0·24 to 0·30 C.G.S. unit caused a superheating of the wire above the temperature of the surrounding water to an extent averaging 8°·3 to 9°·3*. If it be admitted that in Joule's experiments a similar superheating to the extent of 8° to 9° may have taken place, the effect on the calculated resistance of the platinum-silver used would be to cause the amount to be underestimated by about $8 \times 0\cdot031 = 0\cdot25$ per cent. to $9 \times 0\cdot031 = 0\cdot28$ per cent., 0·031 being the percentage increase in resistance per degree of the alloy used by Joule (this alloy was purposely not used by the authors, in order to make the alteration in resistance, if any, more perceptible); that is, the value of J ultimately deduced would be about 0·25 to 0·28 per cent. too low.

51. In order to see whether an increased amount of superheating would be produced by only stirring the water surrounding the wire at intervals of one or two minutes instead of continuously, some of the above observations were repeated

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* This increase in resistance with temperature amounts to about 0·21 per cent. per 1° C. Matthiessen found the average increase of pure metals to be about 0·37 per cent. per 1° C., that of an alloy of platinum and iridium containing 33 per cent. of the latter being only 0·06 per cent. per 1°; probably, therefore, this platinum wire was somewhat impure, very likely containing iridium.

under these conditions with the following results, clearly showing that whilst no very material influence is exerted by discontinuous stirring with weak currents, the effect is marked with currents of 0·23 to 0·29 C.G.S. unit.

Cells in battery.	Average temperature.	Average current.	Potential-difference, in volts.	Resistance	
				at average temperature of experiment.	corrected to 8°·75.
3	7·75	0·03155	0·3628	$1·150\times10^9$	$1·152\times10^9$
6	7·25	0·1350	1·562	1·157 "	1·160 "
9	8·30	0·2285	2·717	1·189 "	1·190 "
12	7·70	0·2880	3·441	1·195 "	1·197 "

52. In order to see whether any analogous effect would be produced by varnishing the wire, as was done in Joule's experiments (owing to possible diminution in rate of passage of heat from wire to water through diminished conducting-power of the varnish), a thin coat of clear filtered shell-lac varnish was lightly applied to the wire; and after two days drying, some of the observations were repeated, with the result of showing that whilst the effect of the varnishing was inappreciable with a weak current, it became noticeable with currents of 0·14 to 0·24 C.G.S. unit. To make sure that the resistance of the wire itself had not been altered permanently by strain &c. during varnishing, the varnish was dissolved off by immersion in alcohol, after which the resistance-value again diminished to its former amount. Thus the following numbers were obtained:—

	Current.	Cells used.	Mean temperature.	Potential-difference.	Resistance.
			°		
After varnishing	0·0808	4	8·85	$0·9347\times10^8$	$1·157\times10^9$
Before " (*suprà*)	0·0772	4	8·50	0·8931 "	1·157 "
			Increase in	resistance ...	Nil.
After varnishing	0·1426	6	8·79	1·675 "	1·174 "
Before "	0·1459	6	8·20	1·697 "	1·163 "
			Increase in	resistance ...	0·011 "
After varnishing	0·2273	9	8·93	2·676 "	1·178 ,
Before "	0·2398	9	8·87	2·806 "	1·169 "
			Increase in	resistance ...	0·009 "
After varnish again dissolved off...... } ...	0·2426	9	8·75	2·835 "	1·168 "

Apparently, therefore, with the stronger currents the varnish produced an increase in resistance about equal to that due to an increase in temperature of from 4° to 5°; so that if the varnish in Joule's experiments produced a similar effect, the total superheating must have been near 12° to 14°, representing 0·37 to 0·43 per cent. of error in deficiency in the estimation of R, and consequently of J.

53. Yet another source of error, and in the same direction, in these experiments of Joule's lies in the fact that, in comparing the resistances of the B.A. unit and the experimental wire employed, the method adopted consisted in determining the angular deflections α, β, and γ produced in a tangent-galvanometer on passing the current from a given electromotor (1) through the galvanometer only, (2) through the galvanometer and B.A.-unit coil, and (3) through the galvanometer and experimental wire—the resistance of the experimental wire being called x, and that of the unit coil $=1$,

$$x=\frac{(\tan\alpha-\tan\gamma)\tan\beta}{(\tan\alpha-\tan\beta)\tan\gamma}.$$

Now, were the currents used so feeble that no appreciable amount of heat was developed by them in either wire, or were the two wires compared similarly sized and situated so as to be heated to equal extents, the value of x thus deduced would be subject to no other errors than the instrumental and observational ones; but as the B.A.-unit-coil wires are imbedded in solid paraffin whilst the experimental wire was immersed naked in water (save for the film of varnish), it is evident that, if any heat at all had been generated by the currents employed, the B.A.-unit coil must have been more heated than the experimental wire, as the heat developed could not pass away readily through the badly conducting solid paraffin. The currents actually employed were sufficient to cause deflections of 36° to 37° with a galvanometer of nine turns and 17 inches diameter, and of 34° to 50° with a galvanometer of eighteen turns and 17 inches diameter; so that the currents were of magnitude equal to from

$$\frac{17\times 2{\cdot}54}{4\pi\times 18}\,.\,\mathrm{I}\tan 34 \text{ to } \frac{17\times 2{\cdot}54}{4\pi\times 9}\times\mathrm{I}\tan 37$$

(where I is the horizontal magnetic terrestrial force $=0{\cdot}18$, and 2·54 is the number of centims. in an inch), or averaged about 0·04 C.G.S. unit. In a wire of 1 B.A. unit resistance a current of this magnitude would produce per minute $\frac{0{\cdot}04\times 0{\cdot}04\times 10^9\times 60}{42\times 10^6}=2{\cdot}3$ gramme-degrees, which, assuming the wire to weigh 10 grammes and to have the specific heat 0·04 (approximately that of an alloy of one part silver to two of platinum), would raise its temperature $\frac{2\cdot3}{10\times0\cdot04}$, or nearly 6° per minute.

It would therefore seem from this rough estimate that the heat-development produced in the B.A.-unit coil by the currents used may have been sufficient to raise its temperature perceptibly above that attributed to it; while the same result would not be produced to any thing like the same extent with the experimental wire, owing to the latter being immersed in water instead of solid paraffin. If it be assumed that this heat-development sufficed to raise the average temperature of the B.A.-unit coil during the observations 5° above that of the experimental wire (an amount of heating not at all unlikely to have occurred), the ultimate effect of this would be to cause the underestimation of R, and consequently of J, by $5\times0\cdot031=0\cdot15$ per cent.

54. Hence, finally, putting together this source of error and that due to the superheating of the wire (increased by varnishing), there is reason for supposing that the value of J obtained by Joule in 1867 by the electric-current method may be at least 0·5 per cent. too small; that is, instead of giving a value 1·31 per cent. larger than that deduced from Joule's water-friction experiments of 1850 and 1878 (Part I. § 34), the corrected value would probably be at least 1·8 per cent. larger than the water-friction value.

The practical conclusion to be drawn from these experiments, then, is that, in any determination in which it is necessary to pass a current through a wire for any length of time, an appreciable error, through increase in the resistance of the wire, will be brought about by its becoming superheated above the temperature of the medium in which it is placed when the current exceeds a certain limit, the exact value of which necessarily depends on the conditions. This source of error will affect determinations of E.M.F. made by such methods as those used by Latimer Clark in the valuation of his standard cell (Proc. Roy. Soc. xx. p. 444), and will similarly affect any experiments on the determination of J by methods based on the same principles as those involved in Joule's 1867 observations, unless the mode of experimenting be modified in such a way as to eliminate this source of error, or at least to render it negligible. We propose to examine certain such modifications which have occurred to us as being likely to reduce this source of error to an inconsiderable amount.

XLIV. *Supplementary Paper on Primary Forms. By* Sir James Cockle, *M.A., F.R.S., F.R.A.S., F.C.P.S., Mem. Lond. Math. Soc., Corr. Mem. Lit. and Phil. Soc. Manchester, Hon. Mem. Roy. Soc. New South Wales**.

16. In my original paper (printed in the Number† for February 1875) certain forms of binomial biordinals, regarded by Boole as distinct, were shown to be allied. Boole's four constants, α_1, α_2, β_1, β_2, were replaced by three, A, Æ, and E. The systems are connected, through a and e, by

$$1-\mathrm{A}-a,\quad \mathrm{A}-a=\beta_1,\quad \beta_2;$$
$$1+\mathrm{E}-e,\quad -(\mathrm{E}+e)=\alpha_1-2,\quad \alpha_2-2;$$
$$\alpha_1+\alpha_2-\beta_1-\beta_2=2(a-e)+4=2(\text{Æ}+1);$$

wherein A and E are two-valued and Æ may take either sign.

17. These relations give

$$\beta_1-\beta_2=\pm(1-2\mathrm{A}),\quad \alpha_1-\alpha_2=\pm(1+2\mathrm{E});$$

and when A and E are integers we have the case of art. 2, and Boole's 1st case (*op. cit.* pp. 430, 431).

18. For $\alpha-\beta$ the relations give the four expressions

$$\text{Æ}\pm(\mathrm{A}+\mathrm{E})+1;\quad \text{Æ}-1\pm(\mathrm{A}-\mathrm{E}+1);$$

whereof I denote the first two by K and Q, and the last two by S and U, respectively, reading the upper signs first. When any one of the four is an even integer we have the cases of arts. 3 and 4, and Boole's 2nd case (*ib.* p. 431), wherein a factor disappears.

19. When E is an integer and 2Æ an odd integer we have Boole's 3rd case (*ib.*), a so-called primary form.

20. When A is an integer and 2Æ an odd integer we have Boole's 4th case (*ib.*), a so-called primary form.

21. The distinction between primary and other, say regular, forms may (see arts. 13 and 14) be obliterated by transformation. I shall now show that from a given system A, Æ, E we may derive five others by the following rule, viz.:—Vary at pleasure the order of the symbols A, Æ, E, subtracting from a symbol half a unit at each direct step made by it, and adding to a symbol half a unit at each retrograde step which it makes.

22. These transformations can be effected by algebraical changes of the independent variable x, the change of x into ψx being accompanied by that of the dependent variable y into $(\psi' x)^{\frac{1}{2}}y$. Differentiations with respect to x I shall denote by accents.

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* Communicated by the Author.

† See also the end of the footnote to my paper "On a Differential Criticoid," in the Number for December 1875.

23. Put $1+x^2=-t^2$, and suppose (12) to be given under the form

$$y''+(\mathrm{L},\ \mathrm{M},\ \mathrm{N})y=0, \quad . \quad . \quad . \quad . \quad . \quad (27)$$

wherein

$$(\mathrm{L},\ \mathrm{M},\ \mathrm{N})=x^{-2}t^{-4}(\mathrm{L}+\mathrm{M}x^2+\mathrm{N}x^4),$$

and L, M, and N are, or are taken to be, one-valued.

24. A change of the independent variable from x to x^{-1} changes t^2 into $x^{-2}t^2$, and (27) into

$$y''+\frac{2}{x}y'+(\mathrm{N},\ \mathrm{M},\ \mathrm{L})y=0\,;$$

and a change of the dependent variable from y to $x^{-1}y$ changes the last equation to

$$y''+(\mathrm{N},\ \mathrm{M},\ \mathrm{L})y=0\,; \quad . \quad . \quad . \quad . \quad . \quad (28)$$

so that we have interchanged L and N.

25. The expression

$$\mathrm{L}+\mathrm{M}x^2+\mathrm{N}x^4$$

being identical, save in form, with

$$1-\text{Æ}^2-(\mathrm{M}-2\mathrm{N})t^2+\mathrm{N}t^4,$$

the change from x to t changes (27) into

$$\frac{d^2y}{dt^2}-\frac{dt}{dx}\,\frac{d^2x}{dt^2}\,\frac{dy}{dt}+\mathrm{T}\frac{dx^2}{dt^2}\,y=0,$$

wherein

$$\mathrm{T}=x^{-2}t^{-4}\{1-\text{Æ}+(2\mathrm{N}-\mathrm{M})t^2+\mathrm{N}t^4\}\,;$$

but

$$\frac{dx}{dt}=-\frac{t}{x},\quad \frac{d^2x}{dt^2}=-\frac{1}{x}\,\frac{1}{1+t^2}\,;$$

consequently we have

$$0=\frac{d^2y}{dt^2}-\frac{1}{t(1+t^2)}\,\frac{dy}{dt}+$$
$$\{1-\text{Æ}^2+(2\mathrm{N}-\mathrm{M})t^2+\mathrm{N}t^4\}\,t^{-2}(1+t^2)^{-2}y;$$

and if we change from y to $t^{\frac{1}{2}}(1+t^2)^{-\frac{1}{4}}y$, and then replace t by x, we shall have (27) transformed into

$$y''+(\tfrac{1}{4}-\text{Æ}^2,\ 2\mathrm{N}-\mathrm{M}-\tfrac{3}{2},\ \mathrm{N})y=0. \quad . \quad . \quad (29)$$

26. Denote these transformations by f and ϕ respectively, and let V mean (L, M, N); then we have

$$\mathrm{V}=(\mathrm{L},\ \mathrm{M},\ \mathrm{N}),$$
$$f\mathrm{V}=(\mathrm{N},\ \mathrm{M},\ \mathrm{L}),$$
$$\phi\mathrm{V}=(\tfrac{1}{4}-\text{Æ}^2,\ 2\mathrm{N}-\mathrm{M}-\tfrac{3}{2},\ \mathrm{N}),$$
$$f\phi\mathrm{V}=(\mathrm{N},\ 2\mathrm{N}-\mathrm{M}-\tfrac{3}{2},\ \tfrac{1}{4}-\text{Æ}^2),$$
$$\phi f\mathrm{V}=(\tfrac{1}{4}-\text{Æ},\ 2\mathrm{L}-\mathrm{M}-\tfrac{3}{2},\ \mathrm{L}),$$
$$f\phi f\mathrm{V}=(\mathrm{L},\ 2\mathrm{L}-\mathrm{M}-\tfrac{3}{2},\ \tfrac{1}{4}-\text{Æ}^2).$$

27. Since our transformations operate differently on x and on y, we may, ψ being any one of them, put

$$\psi\{y, x\} = \{(\psi' x)^{\frac{1}{2}} y, \psi x\}.$$

The transformations f and ϕ, so far as they affect x only, are of the second order; and although $f\phi x$ and $\phi f x$ are of the third order, yet $(f\phi)^2 x = \phi f x$ and $(\phi f)^2 x = f\phi x$. Moreover

$$\phi f \phi x = f \phi f x.$$

28. Since

$$\begin{array}{llll} f \text{ changes} & x^2 & \text{into} & x^{-2}, \\ \phi \quad " & x^2 & " & -(1+x^2), \\ f\phi \quad " & x^2 & " & -(1+x^{-2}), \\ \phi f \quad " & x^2 & " & -(1+x^2)^{-1}, \\ f\phi f \quad " & x^2 & " & -(1+x^{-2})^{-1}, \end{array}$$

we may pass directly from the first of these forms to any other by making the appropriate changes of variables and then taking the criticoid. And any one of these forms indifferently may be made the first form.

29. It remains to be indicated what changes in A, Æ, and E correspond with those made in L, M, N.

30. Since the system

$$A^2 - A = -L, \quad E^2 + E = -N,$$

is, save in form, the same as

$$(A-1)^2 + (A-1) = -L, \quad (E+1)^2 - (E+1) = -N,$$

an interchange which sends N to the extreme left and L to the extreme right will not disarrange our formulæ, provided that we replace A and E by E+1 and A−1 respectively.

31. In order to see that the following system, viz.

$$\begin{array}{rl} V, & (A, Æ, E), \\ fV, & (E+1, Æ, A-1), \\ \phi V, & (Æ+\frac{1}{2}, A-\frac{1}{2}, E), \\ f\phi V, & (E+1, A-\frac{1}{2}, Æ-\frac{1}{2}), \\ \phi f V, & (Æ+\frac{1}{2}, E+\frac{1}{2}, A-1). \\ f\phi f V, & (A, E+\frac{1}{2}, Æ-\frac{1}{2}), \end{array}$$

corresponds, line for line, with that of art. 26, it will be sufficient to point out how two of these lines are constructed from the corresponding line of art. 26.

32. Take the last lines. To L corresponds A. To $\frac{1}{4} - Æ^2$ corresponds $Æ - \frac{1}{2}$, because

$$(Æ - \tfrac{1}{2})(Æ + \tfrac{1}{2}) = -(\tfrac{1}{4} - Æ^2);$$

while to $2\mathrm{L}-\mathrm{M}-\frac{3}{2}$ corresponds

$$(2\mathrm{L}-\mathrm{M}-\tfrac{3}{2})-\mathrm{L}-(\tfrac{1}{4}-\text{Æ}^2)+1=-\mathrm{N}+\tfrac{1}{4}, \quad =(\mathrm{E}+\tfrac{1}{2})^2,$$

a quantity formed from the bracketed constituents in the last line of art. 26 in the same way that Æ is formed from L, M, and N.

33. Take the third lines. To $\frac{1}{4}-\text{Æ}^2$ corresponds $\text{Æ}+\frac{1}{2}$, because

$$(\text{Æ}+\tfrac{1}{2})(\text{Æ}-\tfrac{1}{2})=-(\tfrac{1}{4}-\text{Æ}^2)\,;$$

while to $2\mathrm{N}-\mathrm{M}-\frac{3}{2}$ corresponds

$$\begin{aligned}&(2\mathrm{N}-\mathrm{M}-\tfrac{3}{2})-(\tfrac{1}{4}-\text{Æ}^2)-\mathrm{N}+1,\\ &=-\mathrm{L}+\tfrac{1}{4}, \quad =(\mathrm{A}-\tfrac{1}{2})^2.\end{aligned}$$

34. The roots of the squares are inserted instead of the squares themselves; and the middle quantities in the system of art. 31 might take the double sign.

35. From the system of art. 31 a second might be formed by substituting $1-\mathrm{A}$ for A, a third by substituting $-(1+\mathrm{E})$ for E, and a fourth by making both substitutions simultaneously. With art. 10 the concluding sentence of art. 11 should be incorporated.

36. Now let us consider cases in which the conditions of arts. 17–20 are satisfied. To this end I symbolize the regular case, that of art. 17, by R; the four cases of art. 18 by K, Q, U, and S respectively; the first primary form, that of art. 19, by P, and the second, that of art. 20, by *p*. Then we have seven cases, viz. R, K, Q, S, U, P, and *p*.

37. If we arrange these symbols horizontally, and denote the primitive arrangement by 1, the five transformations will enable us to form the following table, viz:—

1;	R,	K,	Q ,	S ,	U ,	P,	*p*,
$f1$;	R,	K,	Q ,	$\mathrm{U}+2$,	$\mathrm{S}-2$,	*p*,	P,
$\phi 1$;	P,	K,	$-(\mathrm{U}+1)$,	S ,	$-(\mathrm{Q}+1)$,	R,	*p*,
$f\phi 1$;	*p*,	K,	$-(\mathrm{U}+1)$,	$1-\mathrm{Q}$,	$\mathrm{S}-2$,	R,	P,
$\phi f1$;	P,	K,	$1-\mathrm{S}$, ,	$\mathrm{U}+2$,	$-(\mathrm{Q}+1)$,	*p*,	R,
$f\phi f1$;	*p*,	K,	$1-\mathrm{S}$, ,	$1-\mathrm{Q}$,	U ,	P,	R,

38. This table discloses the convertibility of regular and primary forms, and leads to another result on which I shall not now dwell.

2 Sandringham Gardens, Ealing,
April 13, 1880.

XLV. *On Induction in Telephonic Circuits. By* W. Grant *.

THE apparatus used in the following experiments was such as was ready to hand in the laboratory. The particulars given are intended merely to show the conditions under which the results described were obtained; there is no reason to suppose that moderate, or perhaps even considerable alterations in the dimensions of the apparatus would have caused the results to be essentially different.

One of the appliances used is a coil which consists of two similar No. 20 copper wires of equal length wound side by side throughout, forming a coil of 2·3 centimetres internal, and 12·7 centimetres external diameter, and about 12·5 centimetres in length. The resistance of the two wires connected end to end is 28 ohms. This is afterwards referred to as the double helix.

Two other coils, afterwards spoken of as "flat" coils, consist of approximately equal lengths of No. 19 copper wire, wound in rectangular grooves in two flat wooden reels. The inside and outside diameters of these coils are about 10 centimetres and 15 centimetres respectively. The resistance of each is roughly 1 ohm.

Two rough coils of copper wire, in the state in which they came from the manufacturer, were also employed in the first experiment described; but as they are not afterwards referred to, their dimensions need not be given.

If four separate circuits are so arranged that the primary one includes a battery, a microphone which is actuated by a watch, a telephone, and one of the wires of the double helix—the secondary includes the other wire of the double helix, a telephone, and a rough coil—the tertiary includes another rough coil in proximity to that in the secondary circuit, a telephone, and a flat coil—the quaternary includes another flat coil in proximity to that in the tertiary circuit, and also a telephone,—a current of a different order circulates in each of the separate circuits, and the following effects are observed.

The loudness of the sounds in the telephones in the primary and secondary circuits is as nearly as possible the same, in fact no difference can be detected between them; but in the tertiary and quaternary circuits the sounds are not so loud, partly no doubt because the mutual induction between the secondary and tertiary circuits, and between the tertiary and quaternary, was less than that between the primary and secondary, seeing that the coils by whose mutual induction the effects were transferred from the secondary to the tertiary

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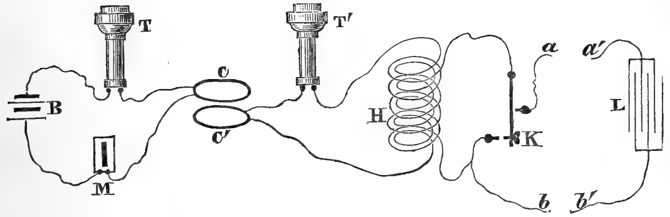
* Read before the Physical Society, January 24th, 1880.

circuit, and from the tertiary to the quaternary, were wound separately, and consisted of a smaller length of wire than the two which were wound side by side to form the double helix used to transfer the induction from the primary to the secondary circuit.

The pitch of the sound, however, is found to be the same in all the four circuits. Now, if the rate of the pulsations of induced currents is greater as the order of the current is higher, and if the telephone is capable of vibrating in unison with these pulsations, it follows that the pitch of the sound should be higher in some of the telephones than it is in others. But as the pitch of the sound is the same in all, either the telephones do not vibrate in unison with the rapid pulsations of the currents of high order, or the sameness of pitch must be due to some other cause.

The arrangement of apparatus in the next experiment is shown in fig. 1.

Fig. 1.



There are three separate circuits. The primary includes a battery B, a telephone T, a flat coil C, and a microphone M; the secondary includes a flat coil C′, a telephone T′, and one of the wires of the double helix H; the tertiary includes the other wire of the double helix and a contact-key K. L is a condenser.

The coils C and C′ are placed with their faces in contact and their axes coincident; and the microphone is actuated by a watch.

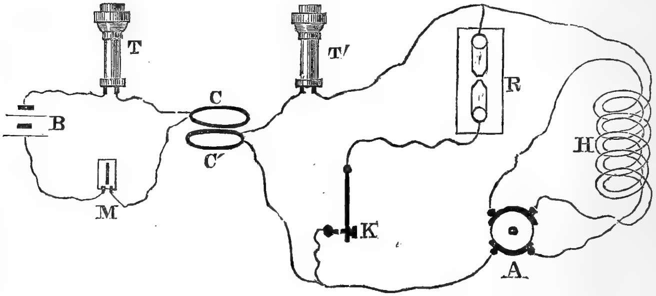
When a battery-current circulates in the primary circuit under these circumstances, currents are induced in the secondary and tertiary circuits, and it is observed, when the ear is applied to the telephone in the secondary circuit, that the loudness of the sound is increased when the key is depressed and the tertiary circuit is closed, and that it again becomes weak when the key is raised and the tertiary circuit is opened. The increase in the loudness is most readily observed when the sound is rather faint when the tertiary circuit is open; for if the sound is already loud the increase is not so marked.

The increased loudness seems to be due to an increase in the strength of the secondary current, caused by the reaction upon it of the currents induced in the tertiary circuit when the key is depressed. When the tertiary circuit is open, the self-induction of the secondary circuit opposes the variations of the secondary current; but when the tertiary circuit is closed, the mutual induction between it and the secondary circuit neutralizes to a great extent the effect of the self-induction of the secondary current, the strength of which is increased or augmented by the inverse currents in the secondary circuit, due to the reaction of the tertiary current upon it.

If the condenser L, fig. 1, is connected to the tertiary circuit by joining the wires at *a a′*, and *b b′*, it will be brought into action when the key is raised, and short-circuited when the key is depressed. If its capacity is three quarters or half a microfarad, the sound in the telephone T′ retains its normal pitch when the key is depressed; but when the key is raised and the condenser is brought into action the pitch of the sound rises; and the rise is greater with half than with three quarters of a microfarad. With a capacity of from about one to five microfarads, the sound retains its normal pitch.

The arrangement for the following experiment will be intelligible from fig. 2.

Fig. 2.



The primary circuit is the same as that in fig. 1, where B, T, C, and M, represent the battery, telephone, coil, and microphone respectively. The secondary circuit includes the coil C′, the telephone T′, the double helix H, and a commutator A, by means of which the direction of the current can be reversed in one of the wires of the helix. K is a key which, when it is depressed, short-circuits the double helix; R is a box of resistance-coils.

If the commutator is so placed as to cause the current to pass through both wires of the double helix in the same direction, the self-induction is strong, the current is weakened by it, and the sound in the telephone T′ is weak in consequence. If the current passes in opposite directions through the two wires of the helix (or returns on itself as in a resistance-coil), the self-induction is eliminated, and the current, having only to overcome the resistance of the circuit, is stronger than before, and the sound in the telephone T′ is considerably louder in consequence.

If the self-induction has been eliminated and the sound is already loud, no further increase in the loudness is observed when the key is depressed and the double helix is short-circuited. If the self-induction has not been eliminated, and the sound is weak, it becomes loud when the key is depressed and the double helix is short-circuited, and it requires 3000 ohms to be introduced at R in order that the loudness may be the same whether the current passes through the resistance-coils and key or through the double helix. With a larger pair of coils substituted for the flat coils C and C′, it requires 5000 ohms to be introduced at R in order to produce a similar result. As the resistance of the double helix is only 28 ohms, these results show, on the one hand, the deleterious effects which are produced by the self-induction of even a small coil, and, on the other, the importance of eliminating induction in conducting-circuits.

In the following experiments speech was transmitted by Bell's telephone, and two observers were employed, one at either end of the line.

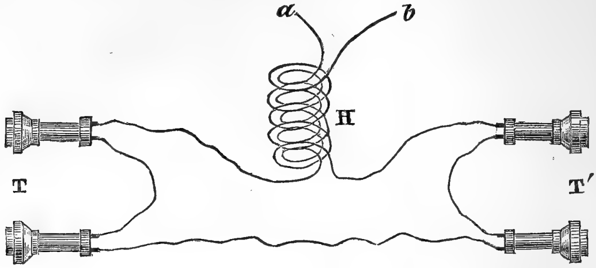
The coils, keys, &c. were placed at one end of the line, and were under the control of one observer, whose chief duty was to read a paragraph of a newspaper in a monotone the loudness of which was kept as nearly as possible constant, so that the inflections of the voice might not be mistaken by the observer at the other end of the line for an increase or diminution in the loudness of the tone resulting from the manipulation of the apparatus while the reading was progressing.

When a single circuit was employed which included one wire of the double helix, the loudness of the sound increased when the other wire of the helix was joined up as a separate circuit—a result similar to that obtained (as already mentioned) by joining up the tertiary circuit fig. 1.

When two separate circuits were employed, each of which included one of the wires of the double helix, the helix acted as a "relay," and messages were transferred or translated from one circuit to the other with perfect facility from either end of the line, and conversation was carried on with the same distinctness as when a single circuit only was employed.

A single circuit was arranged as shown in fig. 3.

Fig. 3.



T and T′ are the telephones, H is the double helix. The helix was joined up as shown in the figure, one end of each of its wires, *a* and *b*, being left unconnected and insulated. Thus arranged, there was a break in the continuity of the circuit, but nevertheless speech was transmitted with perfect distinctness, and conversation carried on with the same facility as in the previous experiments, the helix acting not as a conductor, but as a condenser, its two wires replacing the two sets of plates; and it made no difference which of the ends of the wires of the helix were connected to the line-wires, or whether the currents entered the wires of the helix in the same or in opposite directions.

The helix in the last case having been replaced by a condenser, the other arrangements remaining unaltered, speech was again transmitted with as much facility and clearness as if the circuit had been complete—the capacity of the condenser, which was one microfarad, being amply sufficient to allow conversation to be carried on.

XLVI. *On Method in Causal Research.*

By S. Tolver Preston[*].

A POINTED reflection as to the "danger of forgetting that mathematical reasoning can only lead to useful results when founded upon definite physical conceptions"[†], deserves, no doubt, to be fully indorsed; and nowhere perhaps could this remark be said to be more justly applicable than to the problem of the constitution of the æther, which may be regarded as underlying the basis of the science of optics. No one, of course, would for one moment underrate

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[*] Communicated by the Author.

[†] 'Nature,' vol. xxi. p. 370 (letter of correspondent).

the value of mathematics in physical inquiries. Its achievements, when resting on definite physical conceptions, may be illustrated by the splendid results of the kinetic theory of gases; its complete powerlessness, in the absence of such conceptions, may be equally well exemplified by the inquiry as to the constitution of the æther. Nowhere, perhaps, have the spiritualistic assumptions about "force"* been so indiscriminately applied as here, to the complete exclusion of all that can be called clear physical conceptions. This has naturally resulted from the fatal step taken at the time of Newton, of putting forward that notion of "action at a distance," which he characterized as a "great absurdity," and out of which inevitably sprang the endless train of "forces;" of which it is aptly remarked by the writer of the article on "the Atomic Theory of Lucretius" (North British Review, March 1868, page 239):—"This idea of the constitution of matter was perhaps the worst of all. . . . . No attempt was made to show how or why the forces acted; but gravitation being taken as due to a mere 'force,' speculators thought themselves at liberty to imagine any number of forces, attractive or repulsive, or alternating, varying as the distance, or the square, cube, fifth power of the distance, &c."

Sir W. Thomson observes on the waste of mathematical skill due to the speculations about "force," much in the following terms†:—"The eighteenth century forms a scientific school of its own, where in place of the not unnatural assertion of the ancient schoolmen—'*a body cannot act where it is not*,' the most extraordinary of all paradoxes was set up—'*contact does not exist*.' This strange notion took deep root; and out of it sprang a barren tree, which exhausted the ground and overshadowed the whole field of molecular physics, and upon which so much useless work of the great mathe-

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[*] It will perhaps be said that the notion of "force" (in the sense of an action across space without the intervention of matter) is now almost abandoned, at least in this country. But then there is a great difference between the mere tacit relinquishment of an assumption or theory and that practical indication of its complete rejection which is afforded by the stirring-up of the mind to active inquiry and realization of the physical conditions that can alone replace the assumption. That this result is far from being generally attained yet, more especially in regard to that which must be expected as to the functions of the æther, where the physical deductions inevitably following on the rejection of "force" would be of immense practical interest, I think I shall sufficiently prove in the sequel. Indeed it will become apparent that the former vast influence of the notion of "force" still remains almost undiminished in relation to the æther.

[†] I translate conveniently from a German quotation (that I happen to have by me) from the work 'Reprint of Papers on Electrostatics and Magnetism,' 1872.

maticians of the beginning of the nineteenth century has been expended."

Faraday, Davy, and Rumford are known to have opposed the notion of "action at a distance." It is thus refreshing to see that the greatest leaders of physical science, those who have added most to knowledge and contributed most to real progress, have always been averse to this kind of spiritualistic doctrine.

In the Report of the British Association for 1862 is a paper by Prof. Stokes, entitled "Report on Double Refraction"[*], to which I refer here, as this paper gives an account serving well to illustrate some of the different theories of the æther. I would particularly beg of those who are singly interested in the advancement of knowledge, and who would desire to take an unbiassed and unprejudiced view of the case, to give a share of attention to the following points which I shall develop. I need not add that I do not enter on this subject lightly, having already devoted some years of study to the question. First the theory of Fresnel is mentioned; and the Report states of it (p. 253) that the æther "is regarded as consisting of distinct material points, symmetrically arranged, and acting on one another with forces depending, for a given pair, only on the distance."

Here, it will be observed, we have the arbitrary assumption about "force" (or "action at a distance"). It is stated further in the Report that "Fresnel distinctly makes the supposition that the æther is incompressible, or at least is sensibly so under the action of forces comparable with those with which we are concerned in the propagation of light." Prof. Stokes then raises the probably not unnatural question whether this supposition of "incompressibility is not inconsistent with the assumed constitution of the æther" (given above); and he adds (page 253), "I have mentioned these points, because sometimes they are slurred over, and Fresnel's theory spoken of as if it had been rigorous throughout."

The theories of Cauchy and Neumann are then referred to. It is stated that, according to their view (page 254), "the æther is supposed to consist of distinct particles, regarded as material points, acting on one another by forces in the line joining them which vary as some function of the distances; and the arrangement of these particles is supposed to be dif-

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[*] My attention was directed to this Report by a reference to it by the late Prof. Clerk Maxwell, in the article "Ether" in the new edition of the *Encyc. Brit.*, where it is also remarked—"The undulatory theory, in the form which treats the phenomena of light as the motion of an elastic solid, is still encumbered with several difficulties."

ferent in different directions. The medium is further supposed to possess three rectangular planes of symmetry &c."

We see that the same arbitrary speculations about "force" are applied here. Alluding further on to a particular result deduced by Cauchy, Prof. Stokes observes (page 259):—"This result is, however, in the present case only attained by the aid of *two* sets of forced relations; that is, relations which there is nothing *à priori* to indicate, and which are not the expression of any simple physical idea, but are obtained by *forcing* the theory, which in its original state is of a highly plastic nature, from the number of arbitrary constants which it contains." (The number of these arbitrary constants is further on stated to have been *twenty-one*!) Probably this may be sufficient to illustrate the waste of the highest mathematical skill on this subject. It is said that these speculations were inserted in the *Exercices de Mathématiques*; so that possibly as an exclusively mathematical study (apart from any physical applications) the labour may not have been altogether lost.

Prof. Stokes makes the remark (on page 262):—"The arguments in favour of the existence of ultimate molecules in the case of ponderable matter appear to rest chiefly on the chemical law of definite proportions, and on the laws of crystallography, neither of which of course can be assumed to apply to the mysterious æther."

I would venture to put the question here, whether the best argument for the *molecular* constitution of the æther (as of gross matter) is not to be found in the fact that the æther is observed to have certain properties, and that these properties would be totally inexplicable unless the æther were assumed to be molecular. This was the argument of Lucretius, adopted and expounded by Newton in regard to gross matter (and applies equally to the æther). We can only have recourse to arbitrary assumptions about "force," or to dogmatic affirmations, which exclude all possibility of an explanation, unless we regard the æther as molecular. In a paper "On the Dynamical Theory of Gases" (Phil. Trans. 1867, page 49), Prof. Maxwell has made a remark which I have before had occasion to quote, viz.:—"The properties of a body supposed to be a uniform *plenum* [*i. e.* not molecular] may be affirmed dogmatically, but cannot be explained mathematically." It would follow from this that, unless the æther be inferred to be *molecular*, its properties can only be affirmed dogmatically, or no rational account can be given of properties that can exist solely in virtue of the explanation that underlies them.

In fact, if the spiritualistic notion about "force" had never been invented, it is difficult to see how it could have occurred to any one to imagine that the æther could be in any respect different from gross matter, excepting as to the *scale* or the degree of motion of its parts. For let it be noticed that the endless assumptions that have been made about the æther—as to its being essentially different from gross matter (as if there were *two* kinds of matter in existence), as to its being imponderable (as an occult quality)—and all the mysterious attributes that have been ascribed to it, rest in their totality upon the assumption of "force," and are therefore completely gratuitous, or of the invention of those who invented "force." If this notion of the existence of "force" had never been put forward, it could not fail to have been seen that the only conceivable way in which matter can be physically affected is by *motion*; and the supposed "laws" of nature (in the sense of mere arbitrary fiats, which are based upon no dynamical conditions, but upon the notion of "force") can in reality only be self-made "laws," grounded upon nothing else than upon the phantom "force"*. The distaste that at present exists for the study of that magnificent physical agent the æther, may be no doubt largely attributed to the spurious mystery thrown over the subject by the occult notion of "force;" and this dislike for the study would probably disappear with this notion; progress would be rendered possible. The one fact of the æther forming the physical groundwork of the science of optics should surely be enough to exite interest in this agent.

The splendid results pictured by modern inductive reasoning, whereby the æther must be inevitably regarded, by the rejection of the occult notion of "action at a distance" (and by the light of the principle of the Conservation of Energy), as

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* It would no doubt be unjust if the blame for these vagaries in regard to "force" were entirely attached to the present generation, without keeping fully in view the fact that it was inoculated by some of its precursors, who (in spite of the protests of Newton) imagined that without any addition to knowledge, but by the mere invention of a phrase ("force," "attraction," &c.), they could render unnecessary the search for the cause of gravitation. Rumford's words of protest against this were, "Nobody surely in his sober senses has ever pretended to understand the mechanism of gravitation." Huyghens, Hobbes, Leibnitz, Descartes, (Newton himself), and all the leading men of that period, as Lange relates in his notable historical work *Geschichte des Materialismus* (vol. i. p. 264), were of the same view as Rumford. But not only (as justly remarked in this work) did those who invented "force" (or "action at a distance") bring forward nothing themselves, but (becoming numerically the stronger party) they checked progress by throwing cold water on the efforts of those who attempted to account in a rational manner for phenomena; and it has hence been a matter of considerable difficulty to attract a reasonable amount of attention to any explanation whatever of the effects of gravitation.

the general source of the motions developed in gross matter* (in the coal of the steam-engine, the explosion of gunpowder, chemical action generally, gravity, &c.), would come to be contemplated and talked of as worthy of the intense interest that such problems must excite in minds capable of appreciating their rational worth. Under the reign of spiritualism the simple physics of Newton and his contemporaries run the risk of being forgotten; and for the method and strict system that existed then, which was the very keystone to progress, we have the absence of method †, resting upon the abortive and endless speculations about "force," whereby every thing is involved in mystery and the rational inquirer discouraged. How shall the magnificent proofs of a concealed store of motion exhibited by nature in the endless phases of motion developed in gross matter on every hand, ever assert themselves in the face of a spiritualistic theory (based on "action at a distance")‡, viz. that energy can exist *without motion* (*i. e.* in some ghostly form), that energy can in fact have a *duplex* nature (kinetic and *not* kinetic) and exist in matter *at rest*? It is so far per-

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* There would seem to be some who profess to reject "action at a distance," and yet do not recognize the only logical alternative that could replace it, viz. the *fact* that motions developed in gross matter must be derived from a material agent in space. Where is the motion to come from consistently with the Conservation of Energy, unless it come from an external material agent that encloses a store of motion? For it is needless to add that we can have no recourse to "force" or "potential" energy by the rejection of "action at a distance." The recognition of the *fact* of the derivation of the motion from the material agent is obviously the necessary preliminary towards an inquiry after the *process*. How shall progress ever be possible unless these facts are first debated and realized? Shall a truth wait for appreciation until it becomes a truism or platitude. The time may come when the present superficial theories, whereby motions are referred *en bloc* to spiritualistic stores of energy (represented by the high-sounding phrase "potential" energy) will be looked back upon with something like contempt. It only needs that attention should be thoroughly roused to this matter to ensure this end.

† The grand fatality of the error of "action at a distance" consisted in its departure from a *method*; and errors of *principle* are always the most disastrous.

‡ The prevalence, even now, of the theory of "potential" energy (in the sense of an energy which is *not* kinetic), or the idea that energy is of *two* kinds, is an additional proof of the fact before stated, viz. that although "action at a distance" may be professedly rejected, the logical *consequences* of its rejection are not adequately realized. There would not be such an objection to the term "potential energy" if it were not inevitably contrasted with *kinetic* energy as something *different*, thereby serving as a continued support to the fiction of "action at a distance." Indeed the prefix "kinetic" itself (applied to energy) acts as a perpetual cover to this fiction; for where would be the use of the distinctive prefix "kinetic," unless it were thereby implied that *some other* energy than "kinetic" energy (or the energy of *motion*) existed?

fectly certain (as has been proved again and again in science) that no truth is safe from being disguised by spiritualistic theories of this character, in the face of which reason loses its influence, and legitimate proof its power of appeal.

It does not seem to be adequately realized that there can be nothing mysterious in the mere *scale* of the parts of the matter that forms the æther, that it is as easy to reason of matter of one scale as of another, that *smallness* of moving particles (so as to allow *number* in unit of volume) is the essential condition for the concentration of the greatest possible amount of energy in a given volume of the æther, that it may be proved definitely beforehand that this energy, if it existed, would certainly be concealed in its normal state. All this should excite interest in the case. But what do we find in fact? The subject is scarcely discussed; and even care would not seem to be taken to inquire what we should expect to find if these things were true*. The mystery due to the notion of "force," which precludes all hope of any addition to knowledge, has rendered the subject repulsive.

May we not at least attempt what preliminary steps can be made towards an advance by dropping the spiritualistic assumption about "force." In the first place, as before remarked, we must regard the æther as of *molecular* constitution, or all means of accounting for its properties would be excluded. The most important observed fact in regard to the æther is perhaps the entire absence of any measurable resistance opposed by it to the passage of bodies (such as the

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* No doubt the preconceived idea would naturally be that, in order for a motive agent to contain a store of motion adapted to produce such an effect as an explosion of gunpowder (for example), something boisterous and appealing to the senses would be required, not the thin impalpable æther whose very presence eludes direct detection by the senses. But on the question being examined into, exactly the *reverse* of all this found to be true. For it may be proved, with mathematical certainty, that the greater the intensity of a store of motion in the material agent, the *less* would it appeal to the senses. For the greater the *velocity* of the particles of a material agent, the less is the resistance opposed by the agent to the passage of masses through it, or the more impalpable does the agent become. This one fact, as a definite mechanical truth (independently of all questions as to its possible application in nature), ought surely to be enough to excite inquiry and arouse a rational interest. The vague and utterly unpractical way in which this great physical agent (the æther) is commonly treated would indicate an absence of realization of the physical qualities that we should expect to find in the æther if it were a great dynamic agent of enormous practical importance in the everyday operations of nature. If it were merely attempted (as an ordinary engineering problem) to reason out beforehand what qualities a material agent ought to possess in order to be adapted as a general source of motion, then the qualities the æther appears to have would be seen to be precisely of the kind required.

planets for instance) through its substance. A molecular medium with its constituent molecules at rest, or in fixed positions (like a solid), could not conceivably be capable of satisfying this condition of free passage. Hence we must infer that the molecules (or atoms) of æther in their normal state are perfectly free to move past each other without obstruction; for on no other condition could the æther yield freely to a passing body. Next, we must infer that the atoms of æther are *in motion* in their normal state. For, first, this inference is necessary on the ground that a medium with atoms *at rest*, and with no power of acting on each other at a distance, would have no positive properties at all. Secondly, another independent reason for this inference is that, if the atoms were at rest, they would unavoidably be put in motion by a body passing through the æther, which would entail some appreciable resistance—which is contrary to observation. The only means of avoiding the resistance to passage is for the atoms of æther to be already in motion, whereby the transference of motion by the moving body to the æther atoms in front may be (sensibly) compensated for by the transference of motion (conversely) from the æther atoms in rear, to the body. If on the other hand, the æther atoms in rear of the body were not already *in motion*, they could not of course transfer any motion to the passing body. Indeed it has been proved mathematically with perfect definiteness, that the greater the velocity of motion of a system of atoms, the *less* is the resistance opposed by the medium (formed of these atoms) to a body passing through it; so that by an adequate velocity for the atoms, the resistance will be rendered inappreciable. This, therefore, affords a complete means of explaining the observed absence of appreciable resistance to bodies passing through the æther. Next, considering therefore the æther as consisting of atoms in free motion among each other, the question arises as to what is the character of the motion. It will be evident that atoms in free motion, and incapable of acting on each other at a distance, can only move in straight lines. Hence the inference that the æther is in principle a gas (constituted according to the kinetic theory) would seem to suggest itself at the very outset as an apparently inevitable deduction. No postulates whatever need be resorted to as to the general directions of the motions of the æther atoms among themselves, as that is all automatic, and dependent on principles already investigated in connexion with the kinetic theory of gases. The store of enclosed energy, the pressure, the density &c., are then all connected by rigid dynamical relations, not in any way permitting of arbitrary speculation. As regards a possible insight that this constitution of the æther might afford into the transverse vibrations of light, and the effects of gravity (under Le Sage's sheltering principle), by admitting that the atoms have an adequately long mean path—I must refer to a previous article * in 'Nature' vol. xxi. page 256, and also Phil. Mag. Sept. & Nov. 1877.

At least it appears to the writer that to explain the observed absence of resistance to the passage of bodies through the æther is very important. It is difficult to imagine what is supposed to take place in these "statical" theories, where the æther is supposed to consist of material particles in fixed positions and assumed to be acting on each other by "forces" at a distance, or the constitution of the æther is regarded as resembling that of a "solid." What becomes of the delicate structure of particles and "forces" in the "solid" with (say) the immense mass of the earth flying through the "solid" at about 18 miles per second? Can even the fertile resources of these plastic theories, when taxed to their utmost extent, be of avail here? The objection is not the less cogent because elementary.

There would seem to be sometimes a certain looseness of reasoning or carelessness in regard to the treatment of the æther, that would be considered out of place in ordinary mechanical problems. Sir John Herschel, whose expositions are known in general to have been distinguished for their clearness and lucidity, seems to have made an exception in the case of the æther. In referring to the speculation as to the æther being a "solid," he ('Popular Lectures,' page 285) defines the solidity of the æther in the sense, *that none of its elementary molecules are to be supposed capable of interchanging places* †, or of bodily transfer to any measurable distance from their own special and assigned localities in the universe." What (one might ask) becomes of the molecules of æther incapable "of bodily transfer from their own assigned localities," with a planet careering through them, or the solar system in its proper motion of several miles per second through space?

The vague and loose reasoning occasionally applied to the æther, may perhaps be partly accounted for from the feeling of utter hopelessness as to making any advance when all efforts are paralyzed by the boundless labyrinth of speculation involved in the assumption of "force." Can it be seriously believed

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* The proposed mode of accounting for the transverse vibrations of light, given in the article in 'Nature,' was based on a suggestion or hint thrown out by Prof. Clerk Maxwell, under subject "Ether" (*Encyc. Brit.* new edition).

† The italics are Sir John Herschel's.

for one moment that this complete helplessness is in truth a necessary thing, or that it is any thing more than the natural consequence of a single departure from the strict and defined path of reason, by making an assumption without a definite conception to base it upon? A single false step may well be fatal if it involves the departure from a system (or the overthrow of a guiding principle).

No doubt the above theory, suggested as a groundwork for the constitution of the æther, will, from its very definiteness, be specially liable to attack, since there will not be the power of "saving appearances," by introducing first one hypothetical "force" and then another, that belongs to the plastic statical theories. My object, however, has not been so much to suggest a theory as to point to the necessity for a system in order to find one. Difficulties may naturally be expected to present themselves at first. If, however, the theory suggested were only capable of explaining a single fact, such for instance as the absence of resistance of the æther to the passage of bodies, then even this might fairly be regarded as more satisfactory than those dogmatic assumptions that exclude all possible explanation so long as they are adhered to. The theory at least shows the æther to be a body necessarily uniform as to density and pressure, and enclosing a store of concealed energy of probably enormous value in a small volume of space, this latter quality being precisely that required to render the æther fitted to be a powerful motive agent, capable of playing a most important part in the everyday operations of nature. Who shall estimate the practical (I might say the industrial) value of a study that deals with the source of the motions developed in gross matter generally? and, since knowledge itself is power, who can positively say beforehand how far a clear realization of the existence of these exhaustless stores of motion may go towards their possible practical utilization under more advantageous conditions?

But the universal recognition of the immense power of a method gained by the complete renunciation of "force," would probably be of far more value than the discovery of any concrete facts, since the appreciation of the worth of a method may afford the indispensable key to a science of discovery. That no method or system exists, and therefore that the essential condition for a successful research into the hidden nature of physical causes is entirely wanting, so long as the spiritualistic notion of "force" is adhered to, is at least an absolute certainty, of which any one can convince himself without much thought, and that independently of taking the trouble to inquire into the legitimacy of the assumption of "force." For where, indeed, would be the gain of inquiry into the legitimacy of that which, when put forward as a *cause*, serves only to make phenomena darker than before, by attributing occult qualities to matter, and, in addition to this, leaves one without a scientific method, entailing a fruitless waste of intellectual energy. To those who might say that I have assumed the idea of "force" to have more influence than it actually has, I reply:—Recognize, then (for the sake of consistency), the sole logical alternative that can replace it, viz. the fact that the æther (or material agent in space) is the source of the endless phases of motion developed in gross matter on all sides—since the recognition of this fact is the only proof that can be afforded that "force" (with its attendant *spiritualistic* stores of energy) is rejected, not in profession, but in practice. *After* the recognition of this fact, then the inquiry into the mode or *process involved* in the derivation of the motion from the material agent, becomes the next step, which presents itself as an engineering problem of great practical interest.

I venture to think that the critical remarks made in this essay will not be considered inopportune. May it not well be conceivable that the immense success of experiment in this age has caused the value of a strict and pure system of theoretic reasoning to be less appreciated than would otherwise have been the fact? or that the danger of wandering from such a system is greater (because its effects are less felt) when we have the resource of experiment to fall back upon? The ancient Greeks, who had not recognized the worth of experimental research, were consequently thrown back upon theoretic reasoning as a sole guide; and the value of a strictly rational system was every thing to them; and the remarkable progress they made in just theoretic views as to natural facts is admitted (some of their fundamental deductions remaining unshaken to this day). One may well imagine the confusion that would have resulted, had the notion of "force" and the attendant wandering into a maze of spiritualism, occurred to them, without the backstay of experiment to put a curb on these vagaries.

Nevertheless, although the worth of experiment can scarcely be overestimated, it is no doubt none the less true that the general agreement upon a uniform and strict theoretic system would be of immense practical value as an accompaniment to this. Indeed, when one compares the enormous advantage we possess in the present day in the extensive knowledge of facts accumulated through years of experiment, with the disadvantage the ancients laboured under in their dearth of knowledge in this respect, one may be almost led to surmise that the progress made in the present day in obtaining an insight into the hidden nature of physical causes has not kept pace with this increased knowledge of facts; and this would hint again at the evil attendant on the want of a method. Also the lull of any great discoveries in the field of pure physics during the last few years would seem to make the direction of attention to any fresh pathway all the more opportune.

London, April 1880.

XLVII. *On* Berthelot's *Thermo-Chemistry.*

By J. P. Cooke, *Jun.*[*]

THE new work of M. Berthelot, entitled *Essai de Mécanique Chimique fondée sur la Thermo-chimie,*' presents for the first time in a systematic form the results accumulated during the past ten years from one of the most fruitful fields of investigation ever opened to the chemist. The book supplies a most important want; for the details of the work, published in numerous separate papers rapidly following each other in the chemical journals, have been almost unintelligible, except to those who have followed the investigation from the beginning, and no connected statement of the general principles involved was accessible to the student. The work in this new field has been done almost wholly by two investigators—Berthelot, of Paris, and Thomsen, of Copenhagen. Guided by different theoretical views, these skilful experimenters have gone over very nearly the same ground; and their united testimony, concurrent as it is in most cases, gives a certainty to the results obtained which is as fortunate as it is unusual when the field explored is so extensive as the one we are considering. These two men alone could write authoritatively on the subject; and it is, perhaps, fortunate that the first presentation should come from M. Berthelot, who has the usual skill of his nation in exposition and generalization.

In his introduction, Berthelot enunciates the fundamental principles of thermo-chemistry under the three following heads:—

Principle of Molecular Work.

I. The quantity of heat evolved is the measure of the sum of the chemical and physical work accomplished in any reaction.

Principle of Conservation of Energy.

II. When a system of bodies, simple or compound, starting from a given condition undergoes either physical or chemical

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[*] From Silliman's American Journal, April 1880.

changes, which bring it into a new condition without producing any mechanical effect on external bodies, the amount of heat evolved or absorbed, as the total result of these changes, depends solely on the initial and final states of the system, and is the same, whatever may be the nature or order of the intermediate states.

Principle of Maximum Work.

III. In any chemical reaction between a system of bodies not acted on by external forces, the tendency is toward that condition and those products which will result in the greatest evolution of heat.

The first two of these principles are direct deductions from the mechanical theory of heat; but the third is a generalization which Berthelot claims as original; and if so, it is his greatest contribution to this department of the science. In the work before us the first two principles are discussed in the first volume, and this discussion, together with a description of the methods of experimenting and an enumeration of the numerical data thus far obtained, fill nearly 600 large octavo pages; while the discussion of the third principle occupies a second volume which is still larger. We will follow the same order in the few remarks which the limits of a short notice permit.

As the announcement of an almost axiomatic principle of thermodynamics, which every investigation of thermo-chemistry necessarily assumes, the first of these general principles has an appropriate place at the opening of a discussion of the subject. In regard to heat, as in regard to other manifestations of energy, the total effect is equal to the sum of all the partial effects. But Berthelot adds to his statement of the Principle of Molecular Work the remark, "This principle furnishes the measure of chemical affinities." When, however, we come to his discussion of this general principle, we are disappointed to find that the whole subject is summarily dismissed without giving the reader any clear conception of the distinction between the two modes of change whose results are inextricably blended in all chemical processes.

Were we able to distinguish between chemical and physical change, the first principle would undoubtedly give us a measure of what we might then clearly define as chemical affinity or chemism. Not only, however, is it at present impossible to eliminate from our results the effects of physical changes, but, moreover, when we study the details of the chemical processes with which we are most familiar, we are surprised to find to what a large extent the thermal effect depends on the changes in the obviously physical condition which the process involves. For example, in the formation of hydrogen gas from diluted sulphuric acid and zinc, the passing of a solid into a liquid, on the one hand, and the development of a gas from a liquid, on the other, involve physical changes which very largely control the amount of heat developed in the process, and therefore also, according to the third principle, control the process. Indeed, as is well known, all chemical action ceases as soon as the water becomes saturated with zinc sulphate, although a large excess, both of sulphuric acid and zinc, may be present. But, after making all allowances for the potency of physical conditions, it would undoubtedly appear, from the present standpoint of Chemistry, that there must be certain differences of qualities inherent in the atoms which correspond to differences of chemical affinity, and which are important factors in determining chemical changes; and it is certainly legitimate to seek to measure what we may call the relative potential of the atoms when in a state of indefinite expansion. It is obvious, however, from Berthelot's discussion of the subject, that we are as yet far from realizing such a result. In fact, the only case in which he claims that we measure directly the heat of chemical action independently of physical changes is in the well-known reaction $H_2 + Cl_2 = 2\,HCl$, which is attended with the evolution of 22 units of heat for every 36·5 grams of hydrochloric acid gas formed. Since in this case the volume of the aeriform compound is equal to the sum of the volumes of the two elementary gases from which the compound has been formed, and since, moreover, there has been no essential change in the specific heat, we may reasonably infer that the heat evolved results from chemical action only. But, according to the theory which is accepted by the great majority of chemists, this chemical action is by no means so simple as the direct union of two gas-volumes would seem to indicate; for, as our symbols show, the process implies the parting of the similar atoms which are united in the molecules, both of hydrogen and of chlorine gases; and, unless we misinterpret a very large number of facts, this separation implies the expenditure of a not inconsiderable amount of mechanical work, and may imply a change of physical condition as well. Berthelot, in common with a school of French chemists, rejects the modern theory based on the assumption of the equal molecular volumes of all substances when in the state of gas, and uses throughout his work the chemical equivalents of the older chemistry in place of the atomic weights of the new. At the same time he accepts fully the mechanical theory of heat, and the conceptions of molecular work which this theory implies.

To those who consider that Avogadro's law, and therefore the modern theory of chemistry, are direct deductions from the mechanical theory of heat, this course seems inconsistent; and this inconsistency deprives the work of a very considerable degree of simplicity which might otherwise have been secured. The remarkable progress made in organic chemistry during the last twenty years has resulted almost wholly from the circumstance that the investigators have worked back from the elementary substances to the elementary atoms, and discussed the various modes in which these atoms might be grouped in the molecules. In the same way, in thermo-chemistry we shall find no satisfactory basis until we go back likewise to the atoms, and discuss the thermal effects which attend their union or their separation. One generalization we can already make in regard to atomic work with a great degree of certainty—that the union of atoms is attended with the evolution of heat, and the parting of the same atoms in the same associations with an equal absorption of heat: and it must be remembered that, as defined by modern chemistry, atoms are definite masses of matter; so that in enunciating this general principle we refer to a palpable effect as resulting from a well-defined process, entirely independently of the theoretical views we may have in regard to the nature of the chemical atoms or of the modes by which they are united and grouped together. The general principle just stated explains a great many facts of thermo-chemistry which are otherwise anomalous and obscure. It, moreover, gives us the basis for a clear theoretical distinction between a chemical and a physical process—the first consisting in the separation or union of atoms, the last in the separation or drawing together of molecules. It is true that the distinction here drawn is, as yet, theoretical; but the theory involved gives us a basis from which to work, and this is enough for the present. The problem of finding what we have called the thermal potential of the atoms is not more remote than many problems which have been successfully solved in organic synthesis; and it is in this direction, as it seems to us, that we can alone expect to reach a measure of chemical affinity. It may, indeed, be found that the problem cannot be solved; and attempts to solve it may lead to results which will modify or supplant our present theories. It may appear that the difference between a chemical and a physical process is one of degree, and not of kind; but, whatever the result may be, there can be no doubt that the investigation will lead to larger knowledge and clearer conceptions.

As it seems to us, the principle of molecular work should be supplemented by the principle of atomic work; and it is certain that neither clearness of conception nor definiteness of statement has been gained by the obvious attempt to avoid the recognition of the modern theory of chemistry.

We readily accept Berthelot's second fundamental principle of thermo-chemistry when enunciated as above, because it so obviously falls under the general law of the conservation of energy; but it is obvious that this principle could not have been assumed prior to its experimental verification, any more than could the principle of the conservation of mass, prior to the experiments of Lavoisier; and as Lavoisier worked out this last great principle with the balance, so Berthelot and Thomsen have demonstrated with the calorimeter the corresponding fundamental principle of thermo-chemistry, which must be regarded as a generalization from the results of their work. Moreover, although in cases of simple direct combination the principle under discussion is almost self-evident, and has been long admitted, yet, before the investigations of Berthelot and Thomsen, no chemist conceived of its application in the very complex and indirect reactions by which the greater part of the thermo-chemical data have been obtained. It must be remembered that very few processes of direct chemical combination fulfil the conditions which an accurate measure of the accompanying thermal change involves; and a vast amount of chemical knowledge and ingenuity has been shown in devising indirect methods by which the results could be reached. The general theory of these indirect methods may be stated thus:—We arrange two systems of reactions, both of which begin with the same factors in the same conditions, and end with the same product in the same conditions. In one of these series of reactions there must be no process whose thermal result, if not already known, cannot be measured with the calorimeter. In the other series the chemical combination or decomposition, whose thermal effect we are investigating, enters as an unknown term, the effect of the other chemical changes involved being known or capable of measurement, as in the first series. It follows now, from the principle we are discussing, that if we subtract the sum of the quantities measured in the second series from the sum of those measured in the first series, we shall have the value of the unknown quantity. An example will make the method more intelligible.

It is required to determine the heat evolved when aluminium combines with bromine to form Al_2Br_6; and in the following scheme we assume, as is usual in this subject, that the chemical symbols stand for a number of grams corresponding to the atomic weight, and that the amount of heat is expressed in gram units. Two series of reactions may now be arranged so as to fulfil the conditions we have assumed:—

First Series.

$K_6 + Br_6$ (gas) $+ Aq = (6KBr + Aq)$	570,000 units.
$Al_2 + Cl_6 = Al_2Cl_6$	321,800 "
Al_2Cl_6 dissolved in $(6KBr + Aq)$	152,000 "
	1,043,800 "

Second Series.

$K_6 + Cl_6 + Aq = (6KCl + Aq)$	604,800 units.
$Al_2 + Br_6 = Al_2Br_6$	x "
Al_2Br_6 dissolved in $(6KCl + Aq)$	173,800 "
	778,600 "

$$x + 778{,}600 = 1{,}043{,}803. \qquad x = 265{,}200.$$

In studying these two series of reactions, it will be evident that we begin in each case with the same amounts of the same elementary substances, namely K_6, Al_2, Br_6, Cl_6, and that we end with aqueous solutions in the same condition. Hence the total amount of heat evolved in each of the two series must be the same, and we can at once deduce the value of the only unknown quantity. In this determination, the only quantities which had to be measueed at the time were the heat of solution of aluminic chloride in an aqueous solution of potassic bromide on the one hand, and the heat of solution of aluminic bromide in an aqueous solution of potassic chloride on the other, using, of course, equivalent quantities in each case. The other values given had previously been determined by indirect methods; and it can easily be seen that the investigation displays, not only a great command of knowledge, but also a great fertility of invention; and yet this is a comparatively simple case.

As deductions from the general principle of the conservation of energy, Berthelot gives a large number of theorems which serve to illustrate the extent and variety of its application to the study of the thermal changes which accompany chemical reaction. We give two as examples:—

THEOREM III.—*In two series of reactions starting from different initial conditions, but ending in the same final state, the difference in the quantities of heat evolved is equal to that which would be evolved in passing from one of the initial states to the other.*

This theorem enables us to determine very simply the amount of heat evolved in the formation of the definite hydrates, although it would be very difficult, if not impossible, to form these substances with a definite composition in the calorimeter. Thus, to determine the heat evolved in the reaction $SO_3 + H_2O = H_2SO_4$, we have only to dissolve in one experiment SO_3 and in another H_2SO_4 in a comparatively large amount of water, when the difference in the heat evolved in the two cases will be the quantity required. So also the heat of formation of a hydrocarbon may be determined, by comparing the heat of combustion of the compound with the heat of combustion of the hydrogen and carbon of which it consists. Thus the heat of combustion of acetylene, C_2H_2 (26 grams), has been directly measured, and is equal to 321,000 units, while the heat of combustion of C_2 (24 grams), plus the heat of combustion of H_2 (2 grams), only amounts to 257,000 units. Hence it is evident that, in the formation of 26 grams of acetylene, 64,000 units are absorbed. Acetylene, indeed, belongs to a class of compounds whose formation is attended with the absorption of heat. This class of compounds, which have a special interest in thermo-chemistry, are said to be endothermous; while by far the larger class of compounds, whose formation is attended with an evolution of heat, are said to be exothermous.

THEOREM VI.—*When a compound gives up one of its elements to another body, the heat evolved in the reaction is the difference between the heat of formation of the first compound and that of the resulting product.*

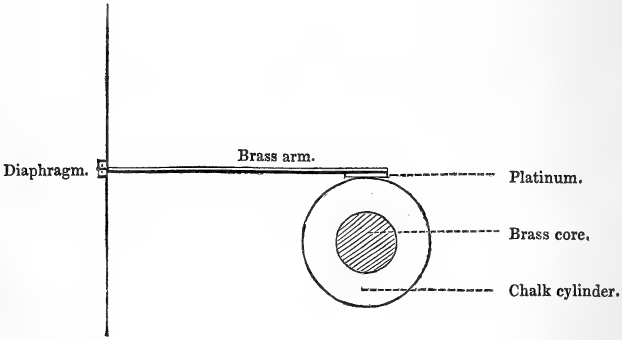
Thus, when an aqueous solution of chlorine is used as an oxidizing agent, for every 18 grams of water decomposed 9600 units of heat are evolved; and this amount is the difference between the heat of formation of H_2O and $2HCl$. As can easily be seen, the same theorem applies to the problem presented by explosive agents of various kinds, and simplifies the solution to a remarkable extent.

These few illustrations will serve to give a general idea of the mode of investigation in this new field of thermo-chemistry; but they are wholly inadequate to show either the extent of the field or the great skill with which it has been cultivated. We must reserve for another number a notice of some very interesting relations which, under his third fundamental principle, Berthelot discusses in the second volume of his great work.

XLVIII. *The Influence of Friction upon the Generation of a Voltaic Current.* *By* SHELFORD BIDWELL, *M.A.**

IN 'Nature' of March 4th Prof. Barrett calls attention to the fact (which I believe had been observed before) that the electro-motograph, or receiving-instrument of Edison's loud-speaking telephone, is capable of generating an electric current. He considers that we have here a new source of electricity, the current generated being due to the motion of a chalk cylinder under a metallic surface. His chalk, he adds in a note, had been impregnated some months before with a solution of phosphate of soda, but was, when used, practically dry.

The construction of the motograph is essentially as follows:—One end of an arm of brass is joined to the diaphragm of the instrument; the other end, which is faced with platinum, is pressed by a spring against the upper surface of a horizontal chalk cylinder. This cylinder is mounted on a thick brass core or spindle, and can be made to rotate and rub against the platinum on the arm by turning a winch-handle. The cylinder is impregnated with certain chemical substances, of which I believe caustic potash is always one, though it is not mentioned by Prof. Barrett.



It will be seen that there are here two metals, brass and platinum, connected by a medium containing caustic potash; and the arrangement looks so much like a voltaic couple that I was not surprised, on connecting the brass and platinum to a galvanometer, to find a small + current flowing from the platinum-faced arm. The cylinder was not rotated, and had not been moistened for some months. On turning the handle the galvanometer at once indicated a much stronger + cur-

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* Read before the Physical Society, March 13, 1880.

rent passing from the platinum. The effect of the friction was to greatly increase the strength of the current.

I then faced the brass arm with zinc instead of platinum. On connecting the instrument to the galvanometer a small deflection was again observed; but this time the + current flowed, as I expected, not from the zinc-faced arm, but from the brass core of the cylinder. The substitution of the zinc for the platinum had the effect of reversing the current, brass being positive to platinum and negative to zinc. In this case, too, rotation of the cylinder largely increased the current.

I now laid aside the motograph, and connected to the two wires of the galvanometer a sheet of brass and a sheet of platinum. Between these metallic plates I placed a thin slice of dry chalk; the galvanometer indicated nothing. I rubbed the metals successively against the chalk; still the galvanometer remained motionless. For the pure chalk I substituted a chalk plate which had been soaked in a saturated solution of phosphate of soda and thoroughly dried; again there was no result. Neither was there when I used dry blotting-paper which had been saturated with phosphate of soda. But with a piece of blotting-paper which had been saturated with a solution of caustic potash and made as dry as possible, the results were just the same as with the electro-motograph: brass and platinum gave a + current from the platinum; brass and zinc gave a + current from the brass; and in both cases the current was much increased by rubbing.

This experiment was repeated with the following pairs of metals—brass and platinum, zinc and lead, zinc and copper, lead and copper, tin and copper, zinc and tin—covering one of every pair of metals successively with a wet cloth and rubbing the one so covered with the other. In every case the friction seemed to considerably increase the current which was generated on mere contact. If, for instance, a piece of lead is covered with a wet cloth and a piece of copper is pressed upon it, a + current will of course flow from the copper. On rubbing, this current is very greatly increased. If the copper be the metal covered, and the lead rubbed against it, the current from the copper will again be greatly increased, but apparently not quite so much so as in the former case. And I believe that this difference in the effect produced according to the metal covered, occurs in the case of all the other pairs of metals which I tried. In some cases it is very notable; in others it is small, and extremely difficult to detect without a machine for producing uniform friction. But, as far as I can judge, the effect on the current is always greatest when the positive element is covered and the negative element exposed to friction.

It seems, therefore, that Prof. Barrett's experiment is only an illustration of the effect of friction on one of the elements of a voltaic couple in increasing the current. This effect is, however, so very remarkable that I was induced to make further experiments.

The most curious result at which I arrived is this:—Take two plates of the same metal, cut from the same sheet, and connect them with a galvanometer; cover one of them with a wet cloth and bring the other down upon it. If the two pieces of metal are in the same physical condition there will be no material deflection of the galvanometer. Now rub the covered metal with the bare metal. As long as the rubbing continues, the galvanometer indicates a current of electricity, which ceases as soon as the rubbing is stopped. And this current invariably flows from the plate which is covered by the wet cloth.

I have made the experiment with plates of tin, lead, copper, brass, and zinc, with the same result in every case.

I do not suggest an explanation of the above-mentioned phenomena. When the two plates of a single metal are used, it appears that friction renders the one rubbed relatively electro-positive. But in the case of two metals, friction seems generally to have a greater effect upon the negative than the positive element, and it makes the negative element not more positive but more negative. Any possible explanation that occurs to me of the one case is inconsistent with the other.

Certain speculations led me to try the effect of passing a battery-current through a pair of metallic plates, separated by a piece of wet rag or paper, while they were being rubbed together; and I found that when the current passed from the covered to the uncovered plate, a remarkable diminution in the friction occurred. A current in the opposite direction produced no such result. I exhibit a little apparatus for rendering this effect visible to a large audience.

This experiment seems to show conclusively that the generally received theory of electrolytic action is sufficient to explain the phenomena presented by the electro-motograph; for it is difficult to conceive any other possible effect of the current than the liberation of hydrogen on the surface of the plate which is connected with the negative pole of the battery, the layer of hydrogen having the effect of diminishing the friction.

Riverstone Lodge, Wandsworth, S.W.

XLIX. *Notices respecting New Books.*

Report on the Administration of the Meteorological Department of the Government of India in 1878–79. *By* H. F. Blanford, *Meteorological Reporter to the Government of India.*

Indian Meteorological Memoirs. Vol. I. *part* III., *containing* VII. *Variations of Rainfall in Northern India, by* S. A. Hill, *Esq., B.Sc., Meteorological Reporter to the Government of the North-western Provinces and Oudh.* VIII. *Meteorological and Hypsometrical Observations in Western Tibet, recorded by* Dr. Scully, *with a Discussion by* H. F. Blanford, *Meteorological Reporter to the Government of India.* Calcutta: Office of the Superintendent of Government Printing. 1879.

THESE publications constitute the latest instalments of the results of meteorological inquiry so energetically pursued in India. The first testifies to the unwearied perseverance of the Reporters to the different Governments in India; and the second treats of the Rainfall, with its variations, in the northern parts of India.

One of the most striking features, we may say, of both publications is the connexion of the condition of the sun's surface with the state of the earth's atmosphere as manifested by its meteorological phenomena; and we are disposed to consider, from a careful perusal of the first, that our Indian meteorologists are in a fair way, if not of fully solving the problem of solar influence on terrestrial phenomena, at least of throwing much light upon it. In this connexion we shall extract some of the most important passages bearing on it.

"A branch of physical enquiry, the close connexion of which with Meteorology has been fully recognized only within the last few years, is that of the physical condition of the Sun. So long ago as the beginning of the present century, Sir William Herschel speculated on the probable variation of the sun's heat, and the consequences of such variation on terrestrial meteorology, and secondarily on agricultural produce. But it was not until the British Association meeting of 1872, that attention was prominently drawn to this subject, by the late Colonel Strange, since which time the study of Solar Physics has more and more been recognized as inseparable from that of the meteorology of our planet. Hitherto such coincidences as have been traced out, with more or less distinctness or vagueness, between the variations of the sun's condition and the atmosphere and other physical phenomena of our planet and its inhabitants, have been purely empirical. To take, for instance, the case which is perhaps the most firmly established of all, *viz.* the variation of terrestrial magnetism,—all that can be said is, that the various changes in the sun's condition and position with reference to the earth, such as the diurnal and annual periods of the earth's revolution, cyclical variation of sun-spots, the sun's periodical revolution on his axis, &c., are faithfully reflected in the variations of the terrestrial magnetism, in the varying intensity and direction of the force. But as to the chain of physical causes through which the solar variations thus react upon the earth—whether, for instance the influence is such as one magnet exerts upon another, or whether the terrestrial magnetism is affected mediately, owing to a variation in the intensity of the solar radiation, are points on which various opinions are held."

"The coincidences between solar and meteorological phenomena are less certainly determined; but such as they are, they are of the same character, *viz.* simple empirical coincidences, the physical reason of which is still obscure; and this obscurity cannot be removed until we have answered beyond all cavil the fundamental question, "Does the intensity of solar radiation vary *pari passu* with the visible changes of his surface, and if so, what is the law of that variation?" The only attempts that have been made to answer this question, by direct observation, are two—*viz.*, a discussion by Mr. Baxendell of the recorded temperatures of sun-thermometers at the Radcliffe observatory at Oxford, and at Eccles near Manchester; and a discussion of the recorded radiation-temperatures at ten stations in different parts of India. In both cases the conclusion was the same, and distinctly indicated. The sun emits the more heat the greater the visible agitation of the photosphere as indicated by the greater number of spots, of red flames and faculæ. But the sun-thermometer is justly regarded as an instrument inadequate to decide a question so important as that of the variations of the sun's heat." [In consequence of this inadequacy] "some persons have drawn the conclusion that the variation of the solar heat is the opposite of that indicated by the sun-thermometer. Others, again, have arrived at a similar inference, reasoning from the ascertained fact that sun-spots emit less heat than the general solar surface; and forgetting apparently that sun-spots and faculæ increase and decrease together. The question, therefore, still remains for final decision; and there is no more important inquiry in the whole range of the science."

In the introductory remarks to Mr. Hill's paper on the Variation of Rainfall in Northern India, we have the following passage:—"Early in 1877 Dr. W. W. Hunter published a pamphlet entitled 'The Cycle of Drought and Famine in Southern India,' in which it was shown that there is a remarkable tendency to a deficiency in the rainfall of the city of Madras, at times when the surface of the sun is free from spots—a phenomenon that had previously been pointed out, though perhaps upon somewhat insufficient evidence, by Mr. J. Norman Lockyer. Dr. Hunter also stated, upon the authority of Sir W. Robinson of the Madras Council, that severe famines and scarcities in various parts of the Southern Presidency also tend to recur at intervals of about eleven years, this being the approximate length of the cycle of the solar maculation."

It having appeared from discussions by Mr. Blanford, Mr. Hill, and Mr. E. D. Archibald, that although the registers examined were not sufficient to establish a relation between sun-spot area and rainfall, yet they seemed to lend some support to the theory that, underlying very great irregular and non-periodic variations, there is a fluctuation of the total annual rainfall coinciding approximately with that of sun-spot frequency. Mr. Hill, in order to test the soundness of this conclusion, and also of inquiring into the truth of the supposed cyclical variation of the annual rainfall, examined all the available rainfall registers for the North-west Provinces and Oudh, as well as many others placed at his disposal by the courtesy of the Meteorological Reporter to the Government of India, and of the Secretary to the Financial Commissioner of the Punjab, from which Mr. Hill selected the registers of twenty representative stations, several extending over a period of twenty or more consecutive years. The country included by the discussion extended over more than eleven degrees of latitude and twenty-four degrees of longitude.

In carrying out this discussion Mr. Hill divided his material into two series, viz. summer and winter rainfall. The summer rains afford a result fairly accordant with the assumption that there is an eleven-year cycle of rainfall similar to that obtained by Mr. Blanford; but he has this remark:—"Neither Mr. Blanford's result nor this can therefore be said to lend unequivocal support to Mr. Meldrum's hypothesis; for the character of the oscillation in the case of the rainfall is very different from that of the sun-spots—the fall from maximum to minimum occurring in three years and the rise from minimum to maximum occupying eight, whereas in the average sun-spot cycle the fall occupies seven years and the rise only four."

The discussion of the winter rainfall furnishes a considerable degeee of uniformity as regards the epoch of maximum and minimum of four cycles of eleven years each, from 1834 to 1877, and is illutrated by a series of curves, of which that of Wolf's sun-spot numbers (inverted) for the same cycles, is compared with that of the winter rains, with the following result:—"The maximum of winter rainfall would therefore appear to be reached on the average rather more than a year *before* the minimum of sun-spots; and the minimum rainfall appears either to coincide with, or to follow the maximum of sun-spots at about an equal interval. The two phenomena cannot, therefore, be directly related to each other as cause and effect, though they may both be the effects of a common cause."

Since the establishment by General Sir Edward Sabine of a connexion between the variation of sun-spots and that of terrestrial magnetism, to which allusion is made in Mr. Blanford's Report, several other phenomena have been considered to be associated in a somewhat similar manner with variations in the sun's photosphere. We have been able to ascertain from various sources the following:—

Sun-spot prevalence accordant with	Authority.
1. Position of inferior planets	Dr. De La Rue.
2. Terrestrial magnetism	Gen. Sabine.
3. Auroræ.	
4. Barometric variations	A. Schuster. F. Chambers.
5. Rainfall, storms, cyclones, &c. . . .	L.Trouvelot. C. Meldrum. J. N. Lockyer.

Sun-spot prevalence accordant with	Authority.
6. Underground temperature*	Piazzi Smyth. E. J. Horne.
7. Depth of rivers in northern Europe.	
8. Fluctuation of level of American lakes .	G. M. Dawson.
9. Severe famines and scarcities in India . .	Sir W. Robinson.
10. The vine-crop	A. Schuster.
11. The sweating sickness	'Nature.' Balfour Stewart.

There are several other points in Mr. Blanford's Report and in the memoirs which are well worthy notice; but our space forbids. Indeed both publications are so full of interest that we cannot do better than refer the reader to them for the latest information relative to Indian meteorology, which is likely to attain a prominent place in meteorological literature.

L. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 232.]

February 25, 1880.—Robert Etheridge, Esq., F.R.S., President, in the Chair.

THE following communications were read:—

1. "On the Geology of Anglesey." By Prof. T. McKenny Hughes, M.A.

The author brought forward evidence to show that, resting on the central gneissic axis of Anglesey, there was a series of conglomerates which he referred to the base of the Cambrian; that the Lingula-flags had not yet been recognized; that the conglomerates were followed by the brown sandstones hitherto referred to Caradoc, but which he identified by the included fossils with Tremadoc; that the lower part of the Black-Shale Group was Arenig, as shown by the Graptolites; while he thought that the higher parts of the Black-Shale Group might turn out to be Lower Bala; that the Black Shales pass under the Gnarled Schists. He then adduced evidence to show that these Gnarled Schists were not foliated or in any way true metamorphic rocks, but only crumpled laminated beds in which all the alteration that had taken place was of the nature of vein-structure, and a kind of universal slickenside, consequent upon the crushing of a rock consisting of thin laminæ of different texture; and suggested that the whole might be, like the green slates &c. of Chapel-le-dale, in Yorkshire, the water-sorted outlying equivalents of volcanic rocks elsewhere, and be contemporaneous with the Snowdon volcanic series.

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* "A Committee, of which General R. Strachy is a member, is now sitting in London, to consider the best means of investigating the whole question of Solar Physics; and I have lately been informed that the Astronomer Royal, who is also a member of the Committee, considers that the regular observation of earth-temperature by means of buried thermometers, somewhat similar to those employed by Professor Piazzi Smyth, at Edinburgh, would be of much service to this end."—H. F. BLANFORD'S *Report*.

2. "Notes on the Strata exposed in laying out the Oxford Sewage-Farm at Sandford-on-Thames." By E. S. Cobbold, Esq., F.G.S., Assoc.M.Inst.C.E.

The beds noticed in this paper belong to the Kimmeridge Clay and the upper and middle part of the Oxford Oolite. They were exposed (over an area of about 1½ mile by 1 mile) in making deep trenches for a sewage-farm, about 4 miles south of Oxford. The author described variations in the Coralline Oolite and Coral Rag, which become, in places, a marl without corals. The Calcareous Grit also is variable in character. On either side of this tract, at Headington and Cumnor, are coralliferous strata. Thus it appears to indicate a gap in the reef. The clay may indicate the proximity of some river, which thus caused an interruption. The author gave reasons for thinking that it came from the S.E., and suggested that it may have descended from the Palæozoic ridge beneath London. The paper also included some notes on the Kimmeridge Clay and a peat deposit.

3. "A Review and Description of the various Species of British Upper-Silurian Fenestellidæ." By G. W. Shrubsole, Esq., F.G.S.

In this paper the author passed in review the forms of Fenestellidæ which have been described from British Upper Silurian rocks. These amount to nine in all—six described by Lonsdale, two by M'Coy, and one by Portlock. From the imperfect condition of the specimens at the command of these authors, and especially from their characters being masked by an incrusting growth of corals &c. and by subsequent matting, it is in most cases impossible to identify them, only one of the nine species, namely *F. rigidula*, M'Coy, being recognizable. The author discusses the peculiarities of these supposed species, of which he adopts only *F. rigidula*; and describes the other forms known to him under the following new names:—*Fenestella reteporata*, *F. lineata*, and *F. intermedia*. The author further discussed the relations of the Silurian Fenestellidæ to those of the Carboniferous and to other organisms.

March 10, 1880.—Robert Etheridge, Esq., F.R.S.,
President, in the Chair.

The following communication was read:—

1. "On the Geological Relations of the Rocks of the South of Ireland to those of North Devon and other British and Continental Districts." By Professor Edward Hull, M.A., LL.D., F.R.S., F.G.S., Director of the Geological Survey of Ireland.

In this paper the author, after referring to his previous paper on the geological age of the Glengariff beds (Quart. Journ. Geol. Soc. vol. xxxv. p. 699), in which he showed that between them and the succeeding Old Red Sandstone in the south of Ireland there existed a very great hiatus, proceeded to compare the sections of the rocks of the south of Ireland with those of North Devon, and to show that the hiatus in question is represented in the latter locality by the whole of the Middle and Lower Devonian rocks. He then discussed the relations of the Devonshire rocks to those occurring north of the Severn, in Scotland, and in Belgium; and from this review of the whole question he arrived at the following conclusions:—first, that there is only one Old Red Sandstone properly so called—represented in Devonshire by the Pickwell-Down Sandstone, in Ireland by the so-called Upper Old Red Sandstone (including the Kiltorcan beds), in Scotland by the so-called Upper Old Red Sandstone, and in Belgium by the "Psammites du Condroz;" secondly, that the so-called Old Red Sandstone of Herefordshire is the estuarine representative of the Middle and Lower Devonians of Devonshire, and that the so-called Lower Old Red Sandstone of Scotland, with its fish-remains, is the lacustrine representative of the Upper Silurian rocks. In conclusion the author discussed the physical conditions under which these various formations were deposited.

March 24, 1880.—Robert Etheridge, Esq., F.R.S.,
President, in the Chair.

The following communication was read:—

1. "The Newer Pliocene Period in England.—Part I. Comprising the Red and Fluvio-marine Crag and Glacial formations." By Searles V. Wood, Esq., Jun., F.G.S.

The author divided this part of his subject into five stages, commencing with

Stage I. The Red Crag and its partially fluvio-marine equivalent. The Red Crag he regards as having been a formation of banks and foreshores mostly accumulated between tide-marks, as shown by the character of its bedding. The southern or Walton extremity of this formation, which contains a molluscan fauna more nearly allied to that of the Coralline Crag than does the rest of it, became (as did also the rest of the Red Crag south of Chillesford and Butley) converted into land during the progress of the formation; while at its northern or Butley extremity the sea encroached, and an estuary extending into East Norfolk was also formed; during which geographical changes a change took place in the molluscan fauna, so that the latest part of the Red Crag proper and the earliest part of the fluvio-marine (both containing the northern species of mollusca and those peculiar forms only which occur in older glacial beds) alike pass up without break into the Chillesford sand and laminated clay, which form the uppermost member of the formation. He also regards the principal river of this estuary as flowing into it from North Britain, through the shallow preglacial valley of chalk in which stands the town of Cromer, and in which the earlier beds of Stage II. accumulated in greatest thickness. The forest and freshwater beds, which in this valley underlie the beds of Stage II., he regards as terrestrial equivalents of the Red Crag; and having observed rolled chalk interstratified with the base of the Chillesford clay in Easton-Bavent cliff, he considers this to show that so early as the commencement of this clay some tributary of the Crag river was entered by a glacier in the Chalk country, from which river-ice could raft away this material into the estuary. He also regards the copious mica which this clay contains as evidence of ice-degradation in Scotland having contributed to the mud of this river.

In *Stage* II. he traced the conversion of some of this laminated clay, occupying sheet 49 and the north-east of sheet 50 of the Ordnance map, into land, the accumulation against the shore of this land of thick shingle-beaches at Halesworth and Henham, and the outspread of this in the form of seams and beds of shingle in a sand originally (from its yielding shells in that region) called by him the Bure-valley bed, and which Prof. Prestwich recognized under the term "Westleton Shingle." As the valley of the Crag river subsided northwards as the conversion of this part of the Chillesford clay into land occurred, there was let in from the direction of the Baltic the shell *Tellina balthica*, which is not present in the beds of Stage I. The formation thus beginning he traced southwards nearly to the limit in that direction of the Chillesford clay about Chillesford and Aldboro'. The Cromer Till he regards as the modification of this formation by the advance of the Crag glaciers into the sea or estuary where it was accumulated, such advance having been due partly to this northerly subsidence, but mainly to the increase of cold. Then, after describing a persistent unconformity between this Till and the Contorted Drift, from the eastern extremity of the Cromer cliff (but which does not appear in the western) to its furthest southern limit, he showed how the great submergence set in with this drift, increasing much southwards, but still more westward towards Wales. The effect of this was to submerge the area of Red Crag converted into land during Stage I., so that the Contorted Drift lies upon it 50 feet thick, and to cause the retreat of the ice which had given rise to the Till to the slopes of the Chalk Wold; whence masses of reconstructed chalk were brought by bergs that broke off from it and were imbedded by their grounding in this drift, contorting it (and in those parts only) by the process. He then traced, in the form of gravels at great elevations, the evidences of this submergence southwards and westwards, showing it to have increased greatly in both directions, but mostly in the western; and he connects these gravels with the Contorted Drift by the additional evidence of one of these marl masses, in which he found a pit excavated near the foot of Danbury Hill, in the London-clay country of South Essex, which hill is covered from base to top by this gravel. The gravel which thus covers Danbury Hill, of which the summit has an elevation of 367 feet, rises in North Kent to upwards of 500 feet; to between 400 and 500 feet on the Neocomian within the Weald; to 600 feet in North Hants (where it overlooks the Weald), and also in Wilts, Berks, and the adjoining parts of Bucks; to 420 feet in South Hants; to 540 feet in Oxfordshire; to 400 feet in Cornwall; to upwards of 700 (and perhaps 1000 and more) in the Cotteswolds; to 1200 feet in Lancashire, and to 1340 feet in North Wales. Eastwards, through Kent towards France, the elevation falls, and in the north of France appears to be about 130 feet, whence northwards the evidences of the submergence are furnished by the Campinian sands and the diluvium of North Germany and Holland.

In *Stage* III. the author traced the rise from this depression, the increase of the ice from the greater snow-interception caused by it on the Pennine chain, and the consequent advance of the glacier- or land-ice. This advance gave rise to the Chalky Clay, which was the morainic mud-bank which preceded this glacier, and was pushed by it as it advanced, and the land rose partly into the shallow sea (where it covered and protected for a time the gravel which was synchronously forming there), and partly onto the land; and by the aid of maps he showed the islands that were overwhelmed by it. He then showed, by a line on a map, the limit up to which this ice, as it thickened, cut through and destroyed this first deposited moraine and the gravel which it had covered, as well as such beds of Stage II. as were formed there, all this material being pushed on to add to later deposited moraine. Outside this line the gravel, for the most part, remains undestroyed, its contents, particularly in the uppermost layers, showing that it was fed by the approaching moraine. By the level at which the junction of this gravel with the moraine clay occurs he traces the position of the sea-line at this time (towards the end of the formation), and finds it to rise along the south-eastern edge of the clay, from 40 feet in N.E. Suffolk to 160 feet in South Essex, and from that along the south-western edge to upwards of 350 feet in North Warwickshire and the parts of Northamptonshire adjoining, all this agreeing with the original increment of submergence in Stage II. He then showed, from evidence afforded by the Yare and Gipping valleys, that this ice, ceasing to advance in East Anglia, shrunk into the valleys of that district, exposing the moraine it had previously laid down to the growth of vegetation, and issued only through these valleys to the sea. The Hoxne palæolithic brick-earth he regards as the deposit of a lagoon produced from the interception of the drainage of this surface by the glacier-tongue thus passing through the Waveney valley. The Brandon palæolithic brick-earth he regards as connected with the same state of things.

In *Stage* IV. he described the plateau and cannon-shot gravels of Norfolk as resulting from the washing-out of the morainic clay by the melting of this ice, which, though shrunken into the valleys of the East of Norfolk, still lay high and in mass in West Norfolk; and showed that, by having regard to the different inclination of the land thus traced, the position of this gravel is reconcilable in no other way. The cannon-shot part of it he attributed to the torrents pouring from this high-lying ice over the west side of the Wensum valley; and the plateau gravels to the deposition of other parts of the same spoil carried into East Norfolk at the commencement of the process and while the ice had not thawed out of the valleys, this gravel afterwards, as the valley-ice thawed, being deposited in them. He also traced the excavation of the trough occupied by the Bain and Steeping rivers in Lincolnshire to the same cause. The finer or sandy part of this material has an extensive spread in South-west Norfolk, forming thick beds, and in a thinner form spreads over North-west Suffolk, where it wraps the denuded edges of the Hoxne and Brandon palæolithic brick-earths.

In *Stage* V. he traced the line of gravels that overlie the Chalky Clay where this clay entered the sea. This entry to the sea over the Severn drainage-system took place by way of the watershed between the Welland and Avon, and by the valley of the latter. Its entry into the sea over the Thames system was by way of the watershed between this system and that of the great Ouse in South Bucks, as well as by the valley of the Colne, Lea, and Roding, and over the lower part of the watershed in South-east Essex. Its entry into the North Sea was by the valleys of the Blackwater, Gipping, and other Essex and Suffolk valleys, the entry by the Yare and Waveney being far out beyond the present coast-line. He also traced, by similar evidence, the extent to which the sea entered the Trent system after the ice vacated it. This line of gravel (after allowing for the case that the elevation of the junction of the gravel beneath the clay represents that of the sea-bottom, while that over the clay more nearly represents that of the sea-top), he showed to correspond with that of the junction of the gravel beneath the clay so far as this is not destroyed in the parts where the ice did not shrink into the valleys; and it also agrees with this line, supplemented by the amount of rise in the interval where the ice did so shrink. Along the south-western edge of the clay this line of gravel, subsequent to the clay, falls from near 400 feet in Bucks to 150 feet in South Essex; whence northwards along the south-eastern edge it falls uniformly to Ordnance datum in central East Suffolk, and probably continued to fall to 100 feet or so further to the extreme point where the ice from the Yare valley entered the North Sea, far beyond the present coast. Along the north-western edge of the formation this line falls northwards in a corresponding way to that on the south-eastern edge, save that, starting there from near 350 feet, it does not fall below, if even quite down to, Ordnance datum near the Wash. He then traced the extent to which the sea on the west, deepening in that direction in accordance with the original depression of Stage II., entered the valleys of the area covered by the ice of the Chalky Clay as this vacated it; the carrying out through the Welland and Avon valleys of the red and white chalk spoil of the Bain-Steeping trough, and its deposition in the Cotteswold gravel up to a high level, coming from the Avon system over the Gloucestershire water-parting into the valley of the Evenlode, a part of the Thames system.

All river-gravels north of the point where the line of gravel over the clay sinks below Ordnance datum, he regards as concealed below the alluvium, and at depths proportional to the fall of that line. Examining in detail the grounds for the contrary opinion heretofore held by himself and by geologists in general, that the great submergence succeeded the principal glaciation of England, he rejected that opinion; and no longer regarding the basement clay of Holderness (with its ancient molluscan facies) as identical with the Chalky Clay, but as moraine synchronous with the Till of Cromer, he considered the gravels with shells at extreme elevations in Lancashire to have preceded all glacial clays but these, and to have escaped destruction by the advance of the ice during the rise only at the south end of the western slope of the Pennine chain, those on the eastern having been wholly swept away, but that gravels were deposited on the east side of the Pennine after the dissolution of the Chalky-clay ice up to the reduced height of the sea-level at that time, and so far as the ice of the purple clay allowed the sea to come. He then relinquished the opinion formerly held by him that the passage of the Shap blocks was due to floating ice, and referred this to the land-ice crossing the Pennine chain consequent upon greater snow-interception from the progress of the rise; and to the same cause he referred the drift which rises high on the eastern slope of the Pennine ridge north of the Aire. To this crossing of the ice having diverted first a part and then the whole of the ice-supply of the Chalky-clay glacier, he attributed first the shrinking of that glacier into the valleys in East Anglia, and afterwards its dissolution by the agencies always rife in the Greenland ice (but which are there balanced by continual reinforcement), when by this diversion its reinforcement by ice from the Pennine chain ceased. The purple clay of Holderness, being thus in its lowest part in Holderness coeval with the valley-formed portion of the Chalky Clay of Norfolk and Suffolk (or "third Boulder-clay" of Harmer), was the moraine of this invading ice, which, after crossing at Stainmoor, divided against the eastern moorlands of Yorkshire; and one branch going north of these moorlands through the valley of the Tees, sent off an arm down their eastern flank, the moraine from which is the narrow belt of purple clay which skirts the Yorkshire coast north of Holderness, and spreads out wider in Holderness. This arm from the Chalky-clay ice not having, in consequence of the westerly increment of depression, descended the eastern slope of the Wolds, found sea then covering the basement clay of Holderness, in which sea it stopped between the Humber and the Wash, by means of which the lower part of the purple clay up to the level of about 150 feet, contains intercalated in it beds of sand and gravel, and contains shells and shell-fragments, as does the Lancashire clay similarly extruded beneath the sea. The other branch came south along the western flank of the east moorlands and through the Vale of York, where it ended, and became stationary in the sea as this entered the Trent system on the final dissolution of the chalky-clay glacier.

The author discovers no trace of any thing like the intercalation of warm periods up to the stage with which he concludes this part of his memoir; and leaves the description of the later beds, as well as an examination how far arboreal vegetation and the coexistence of Pachyderms and Proboscideans can be reconciled with the contiguity of extensive land-ice, for the concluding part of it.

LI. *Intelligence and Miscellaneous Articles.*

ON THE SPECIFIC HEAT AND CONDUCTIVITY OF BODIES.

BY M. MORISOT.

I HAVE the honour to present to the Academy the general method and the principal experimental characters of a study which I have commenced upon specific heat and conductivity.

Suppose in the centre of an enclosure kept at 0° a body which is a sufficiently good conductor to be admitted as *homothermous*—that is to say, of equal temperature in all its parts. I call that temperature x; F is the thermal capacity of the body, ρ the amount of heat which it loses in a second for an excess of 1° above the surrounding temperature on account of radiation, contact of the air, and of the supports.

Into this first body I introduce a second, which I suppose also homothermous. Let y be its temperature, C its thermal capacity, ϵ its loss of heat per second for 1° of excess at all the points where it is not in contact with the first body, λ its loss at all points where it is in contact with the first body (this will be the external conductivity).

The immersion of the second body in the first will cause the temperatures x and y to vary according to the following differential equations:—

$$-\mathrm{C}\frac{dy}{dt}=\lambda(y-x)+\epsilon y;$$

$$+\mathrm{F}\frac{dx}{dt}=\lambda(y-x)\rho x.$$

Integration gives for x and y the functions:—

$$x=\mathrm{M}e^{-mt}+\mathrm{N}e^{-nt};$$

$$y=\mathrm{P}e^{-mt}+\mathrm{Q}e^{-nt}.$$

Observation of the course of the temperatures will give the quantities M, N, P, Q, m, n—with which we obtain

$$\frac{\mathrm{C}}{\mathrm{F}}=\frac{-\mathrm{MN}}{\mathrm{PQ}}, \qquad (1)$$

$$\frac{\lambda}{\mathrm{F}}=\frac{-(n-m)\mathrm{MN}}{\mathrm{MQ}-\mathrm{NP}}, \qquad (2)$$

$$\frac{\rho}{\mathrm{F}}=\frac{m\mathrm{M}(\mathrm{Q}-\mathrm{N})-n(\mathrm{P}-\mathrm{M})}{\mathrm{MQ}-\mathrm{NP}}, \qquad (3)$$

$$\frac{\epsilon}{\mathrm{C}}=\frac{m\mathrm{P}(\mathrm{Q}-\mathrm{N})-n\mathrm{Q}(\mathrm{P}-\mathrm{M})}{\mathrm{MQ}-\mathrm{NP}}. \qquad (4)$$

A second experiment, in which, F remaining constant, C will have been augmented by a known quantity k, will give

$$\frac{\mathrm{C}+k}{\mathrm{F}}=\frac{-\mathrm{M}_{,}\mathrm{N}_{,}}{\mathrm{P}_{,}\mathrm{Q}_{,}}, \qquad (5)$$

and, with (1), permit the calculation of F and C.

If the immersed body is not homothermous, I suppose it (for simplicity) of a cylindrical form, and replaced by two concentric bodies:—the one exterior, having in all its points the temperature z of the surface of the real body; the other interior, its temperature y suitably chosen in order that the sum of these bodies may replace the real body in its effects upon the exterior body. This second case, which I will call *dithermy*, will give three equations instead of two. The calculations will be longer, but not more complicated; and they will furnish, besides the physical unknown quantities of the preceding problem, the measure of the internal conductivity of the real body.

In order to approximate to the conditions assumed in this method, certain experimental arrangements are necessary:—

(1) Instead of noting and utilizing merely the initial temperatures and that called *final* or *stationary* (*i. e.* the maximum of x), I had, by observations sufficiently close and sufficiently long-continued, to follow the course of the temperatures in the body which was receiving and in that which was losing heat.

(2) Instead of observing only, as in the method called the *cooling* method, the temperature of the central parts of the hot body, I was obliged to add, near the surface, a second thermometer, giving the temperature of the outer zone.

I employ as the body receiving heat (as calorimeter) a brass cylinder admitting the immersed body into a cavity passing along its axis. Another small cavity, made in the pretty thick wall of the calorimeter, receives, together with a very inconsiderable known weight of water, the reservoir of the thermometer which will give the temperature x. The calorimeter rests, by three pointed feet, on the cork bottom of a larger metallic cylinder blackened inside and surrounded by melting ice.

The body under experiment, liquid or pulverulent, is placed in a tube of thin glass with a flat bottom, fitting with very gentle friction into the central cavity of the calorimeter. The two thermometrical reservoirs are entirely surrounded by the body, the upper surface of which scarcely exceeds them and is flush with the brim of the calorimeter. This body being some degrees hotter than the calorimeter, I effect the immersion, and continue the readings, alternating them always in the same order, from minute to minute for each thermometer.

Before any numerical determination I wished to control the method by a few results in some known cases. I took, for example, as hot bodies some determined weights of distilled water, retaining always the same calorimeter. Twelve experiments gave as the mean result:—

	grams.
Calorimeter and its thermometer, reduced to water	19·527
Immersed portion of the tube, with its two thermometers	1·836

I have also drawn up the following comparative Table:—

Weight of water employed.	Calculated value of $\frac{C}{F}$.	Experimental value of $\frac{MN}{PQ}$ for each case.
grams. 12·0	0·70853	0·70713 0·70870 0·70604
13·0	0·75975	0·75878 0·75987 0·76467
13·5	0·78536	0·78700
14·0	0·81098	0·81168 0·81248
15·0	0·86220	0·86446 0·85476 0·86356

In another series of experiments, different weights of mercury gave for its specific heat numbers presenting an error of at most 0·00061.

I shall continue the investigation, applying the method to chemically pure substances, and determining their conductivity.—*Comptes Rendus de l'Académie des Sciences,* April 5, 1880, t. xc. pp. 814–817.

NOTE ON SUPERNUMERARY RAINBOWS. BY CH. MONTIGNY, MEMBER OF THE ROYAL ACADEMY OF BELGIUM.

"When a rainbow is very brilliant, there are often seen inside of the interior and outside of the exterior bow coloured bands, which are designated *secondary*, *supplementary*, or *supernumerary* bows: immediately after the violet, red is distinguished, then green and violet. Sometimes these colours are repeated several times in the same order along the inner margin of the principal bow; they appear more rarely outside of the second bow." M. Daguin, from whom I borrow these statements, adds that the supernumerary bows are only seen in the culminating portions of ordinary rainbows, and only when the altitude of the latter is considerable; no traces are seen of them near the horizon*.

Kaemtz, in his *Traité de Météorologie*, states the same particulars, from Langwith and other observers, who have never noticed supplementary rainbows near the parts of the principal bow in the vicinity of the earth.

Contrarily to these facts, I have recently had an opportunity of observing some supernumerary rainbows which were visible only at the lower extremities of the principal bow. The circumstances were, perhaps, exceptional; but it is important to make them known.

On the 30th of August last, after a rainy day, I was with some other persons in the vicinity of Rochefort a little before sunset. A few moments after a fresh shower of short duration we remarked in the east, in which direction the rain continued, two ordinary rainbows. The colours of the principal bow lost some of their brightness in the lower regions of the air. These were sensibly darkened up to a certain height by a misty tinge, forming a broad zone perfectly distinct from the rain-cloud entirely above it, upon which the two bows appeared brighter. Moreover, at the inner side of each lower extremity of the inner bow, and consequently *near the ground*, *four* supernumerary rainbows could be distinguished, which appeared to rise on each side into the misty zone of the air to about one third of the altitude of the inner bow. The colours of these eight supernumerary portions possessed but little brightness: the red and a dark violet were alone distinct. I observed no supplementary bow outside of the large bow.

In this observation, which took place twenty minutes before the disappearance of the sun below the horizon, the production of supernumerary bows at the extremities, and not at the culminating portion, of the principal bow, constitutes a manifest exception to the facts hitherto observed. This exception can be explained by the circumstances which accompanied the observation, as we shall see.

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* *Traité de Physique*, 3e édition, t. iv. pp. 284, 445.

We know that Young, Arago, and Babinet attributed supernumerary rainbows to effects of diffraction. Airy has developed this theory by mathematical analysis. The results which he obtained have been verified by Miller with respect to bows of this kind produced by the sun's rays meeting water-drops from $\frac{1}{2}$ to $\frac{1}{3}$ of a millimetre in diameter.

According to this theory, the necessary condition for supernumerary rainbows to be separated so as to be distinct is that the effects of diffraction be produced by a great number of very small and sensibly equal rain-drops. The larger the drops become, the more do the fringes diminish in breadth and the less distinct are they. Thus is explained how it is that in ordinary cases supernumerary bows are seen only in the upper regions, where the rain-drops are usually smallest—while they do not appear near the horizon, where the drops have become larger (Daguin).

At the time of the observation of Aug. 30 the rain-clouds, moving away from us towards the east, formed two distinct layers:—the one, higher, on which appeared the upper portion of the principal bow, destitute of supernumerary bows; and the other, appearing nearer to us, extending between the former and the misty zone above mentioned, in front of which the four supernumerary bows that bordered interiorly each extremity of the principal bow seemed to be delineated. Those cloud-arrangements are sometimes exhibited during days of almost continual rain in hilly regions such as that in which I happened to be.

If it be admitted that the drops of rain proceeding from the second, relatively lower layer of clouds, when near the ground presented still the minuteness necessary to the production of supernumerary bows, their having been seen at the two extremities of the principal bow, near the ground, will be intelligible. If, contrarily to the facts hitherto observed, these bows were not distinct at the upper part of the ordinary bow, it is because the rain-drops which traversed the upper regions of the air, proceeding from clouds much more elevated than those of the second layer, were doubtless large enough to render impossible the effects of diffraction which produce the supernumerary bows, the appearance of which is, moreover, often enough an exception. Under these conditions the effects of the phenomena of refraction and dispersion which produce ordinary rainbows were alone visible in the higher regions of the air.—*Extrait des Bulletins de l'Académie Royale de Belgique*, série 2, t. xlviii. nos. 9 & 10, 1879.

ON THE DETERMINATION OF HIGH TEMPERATURES.

BY H. SAINTE-CLAIRE DEVILLE AND L. TROOST.

A certain number of chemists are at present engaged in the more or less precise determination of elevated temperatures. At the meeting of the Academy on the 23rd September, 1878, one of us in conjunction with M. Debray announced, in a Note upon the dissociation of the oxides of the platinum group, that the measurement of those temperatures had been accomplished by means of the apparatus employed by us in 1863 (for determining the coefficient of dilatation of porcelain), subsequently simplified by the employment of a Sprengel pump, which permits us, as often as we wish it, to take out and measure the thermometric material (nitrogen) contained in the reservoir and to calculate the temperature.

We have now to describe this method, which is manifestly more expeditious than that (V. Regnault's manometric method) which we formerly employed.

A cylindrical vessel of Bayeux porcelain, having its two ends spherical, and with a capacity of at least 50 cubic centims., serves as thermometric reservoir. To this a capillary tube is cemented, likewise of porcelain, about 30 centims. in length. This apparatus is cemented to a glass threeway cock, which puts it in communication alternately with the air and, by means of an almost capillary leaden tube, with a Sprengel pump. The thermometer being placed in a partitioned earthen tube with a mixture of asbestus and fireclay, is heated in an oven fed with paraffine oil, the flow of which is regulated by very sensitive cocks.

The temperature is varied in a very regular manner by a greater or less supply of oil; it can, at will, be rendered stationary or be raised more than sufficiently to soften or even to fuse the porcelain*.

The desired temperature being attained, the flow of the oil is determined so that the nitrogen neither dilates nor contracts any more at that temperature. The fulfilment of this condition is easily ascertained—because the cock carries a drying-apparatus with chloride of calcium, and is terminated by a tube which can be made to dip in the water in order to verify the constancy of the temperature.

The threeway cock is then turned so as to close all communication between the outer air, the Sprengel pump, and the reservoir. At that instant the leaden tube which joins the threeway cock to the pump is completely emptied; turning the cock again, we exhaust the nitrogen contained in the thermometer. This gas is collected in a graduated tube placed upon the mercury-vat of the pump; and we only stop when this no longer gives passage to the least bubble of nitrogen. The graduated tube is surrounded with water, the temperature of which is taken. On it is read the volume of the nitrogen aspirated; the height of the mercury above the level of the vat, as well as the height of the barometer, is determined; and by calculation the volume observed is reduced to that which it would be at 0° and 760 millims. If by a preliminary experiment we have determined the volume of nitrogen contained in the reservoir at 0° and 760 millims., we have the principal element of the calculation of the temperature attained.

But it will be noticed that in this apparatus there is a space which is prejudicial; it is the capacity of the stem of the thermometer.

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* This way of heating is so advantageous that we cannot recommend it too highly to those who are occupied with these delicate questions.

In order to make with accuracy the correction due to this space, we join to the capillary tube another tube of exactly the same length and the same diameter as the thermometer-stem. This tube, which we have named *compensator*, is closed at one end; it is attached by means of mastic to a threeway cock, permitting it to be put in communication successively with the outer air and with the Sprengel pump. We close the cock of the compensator at the same time as that of the reservoir of the thermometer; and after the exhaustion of the nitrogen contained in the latter, following the same order of operations we determine the volume, reduced to 0° and 760 millims., of the gas contained in the compensator, and consequently the volume of the prejudicial space. Deducting this volume from that of the gas extracted from the thermometric reservoir, we obtain exactly the quantity of nitrogen which remained in the thermometer at the temperature reached. With all these numbers, introduced into a very simple formula,

$$V=\frac{V_0(1+kx)}{1+\alpha x}\times\frac{H}{760}*,$$

we have all that is necessary for calculating, with a high degree of accuracy, the temperature to be determined.

We will only remark that the determination of the temperature and pressure of the gases successively collected in the graduated tube must be made with the greatest exactness. All the precautions necessary for determining these numbers should be taken with minute precision, following the most exact methods indicated by Regnault. A mode of connexion must also be adopted, between the thermometer and the compensator on the one hand, and the Sprengel pump on the other, such that the volume of the prejudicial space in the thermometer shall be precisely equal to the capacity of the compensator. There are many means of satisfying this condition; and it is needless to describe them here.

A drawing of the apparatus, together with the arrangements we have adopted, will be given in a memoir which we shall publish in the *Annales de l'École Normale supérieure*†.—*Comptes Rendus de l'Académie des Sciences*, March 29, 1880, t. xc. pp. 727–730.

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* V_0 is the capacity of the reservoir—that is, the volume of the air pumped into the entire apparatus, minus the air pumped into the compensator—the whole supposed at 0° and 760 millims.; V is the volume of the air left in the reservoir at the temperature x and at the exterior pressure H—that is to say, the volume of the air pumped into the reservoir at the temperature x, minus the volume drawn into the compensator—the whole reduced to 0° and 760 millims.; k is the sum of the coefficients of the normal dilatation and the permanent dilatation which we have established (*Comptes Rendus*, t. lix. p. 169).

† We have merely to add that the Sprengel pump ought to be constructed without the employment of caoutchouc, and that the vertical tube which serves as pump should, in the part where the impact of mercury against mercury takes place, be furnished with a tube of steel or platinum in order to protect the glass from the effect of the water-hammer. The absolute necessity of this last arrangement will be explained in a memoir by one of us and M. Mascart.

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[FIFTH SERIES.]

JUNE 1880.

LII. *On the Behaviour of Carbonic Acid in relation to Pressure, Volume, and Temperature.* *By* Prof. R. CLAUSIUS[*].

IN relation to pressure, volume, and temperature, gases follow, as is well known, with a certain degree of approximation, the laws of Mariotte and Gay-Lussac, which can in common be expressed by the following equation—

$$pv = \text{RT}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

wherein p represents the pressure, v the volume, and T the absolute temperature, while R is a constant dependent on the nature of the gas. The further the gas in question is removed from its condensation-point the closer is the approximation to these laws. With those gases which under ordinary circumstances are so far from their condensation-point that, until recently, their condensation could not be effected, and which were therefore named *permanent* gases, the approximation is so close that for a long time it was believed that they followed these laws exactly, until Regnault, in his distinguished investigations[†], first demonstrated some slight deviations. Somewhat later, Natterer[‡] showed that on the application of very powerful pressure very considerable deviations from Mariotte's law are obtained; and the deviations observed by him were

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[*] Translated from a separate impression, communicated by the Author, from Wiedemann's *Annalen*, 1880, vol. ix. pp. 337–357.

[†] Regnault, *Mém. de l'Acad. des Sciences*, xxi. 1847.

[‡] Natterer, *Wiener Ber.* v. (1850) p. 351, vi. (1851) p. 557, and xii. (1854) p. 199.

of a different kind from those found by Regnault. That is to say, while Regnault had found that, with all the gases examined by him, the pressure increases more slowly than the density, in Natterer's experiments it turned out that with very great pressure the case is reversed, and the pressure increases more quickly than the density: with air, nitrogen, and carbonic oxide gas the pressure already reached an amount of about 3000 atmospheres when the density had only become from 700 to 800 times that which exists under the pressure of one atmosphere.

Upon the causes on which these deviations of the gases from Mariotte's and Gay-Lussac's law depend, in my treatise "Ueber die Art der Bewegung, welche wir Wärme nennen"*, I expressed myself as follows:—

"In order that the law of Mariotte and Gay-Lussac, and the laws in connexion with it, may strictly hold good, the gas must, as regards its molecular state, satisfy the following conditions:—

"(1) The space actually filled by the molecules of the gas must be vanishingly little in comparison with the entire space taken up by the gas.

"(2) The time of a collision (*i. e.* the time required by a molecule, when it strikes against another molecule or a solid obstacle, in order to alter its motion in the manner in which it is altered by the collision) must, in comparison with the time which elapses between two collisions, be vanishingly short.

"(3) The influence of the molecular forces must be vanishingly little. This implies two things. In the first place, it is required that the force with which all the molecules at their mean distances attract each other vanish in comparison with the expansive force arising from the motion. But the molecules are not always at their mean distances from one another; often in the course of the motion one molecule comes into the immediate vicinity of another or of a solid obstacle likewise consisting of operative molecules; and at such moments the molecular forces of course come into action. Hence the second requirement is that those parts of the path described by a molecule on which the molecular forces have an influence, perceptibly altering the motion of the molecule in direction and velocity, vanish when compared with the portions of the path on which the forces can be regarded as inoperative.

"If these conditions are not fulfilled, deviations in various directions from the simple law of the gases take place, which

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[*] Clausius, Pogg. *Ann.* c. p. 358 (1857); and *Abhandlungensammlung*, ii. p. 235.

become so much the more considerable as the molecular state of the gas corresponds less to these conditions."

The deviations here mentioned must make themselves manifest, so far as the pressure is considered at a given temperature and a given volume, in this being either greater or less than it should be, according to Mariotte and Gay-Lussac's law, taking as the starting-point the highly rarefied state of the gas. The space above-mentioned (under 1) actually filled by the molecules conditions an augmentation of the pressure, since by it, at a given volume of the gas, the free space for the motion of the molecules is lessened, and accordingly the number of the collisions is increased. The molecular forces mentioned under 3 effect, when attraction prevails, a diminution of the pressure. The circumstance mentioned under 2, namely the time that passes during a collision, has a more complicated effect, since, when two molecules rush against one another, first an acceleration, and only after this the retardation and reversal of the motion take place; hence, in considering the total effect, that of this circumstance can be joined partly to that of the first, partly to that of the last circumstance.

Of the two opposite effects of augmentation and diminution of pressure, the one or the other may preponderate, according to circumstances. The above-mentioned experiments of Regnault and Natterer show in hydrogen a universal preponderance of the increase of pressure, while they make known that in the other gases diminution of pressure predominates when the densities are less, and increase of pressure when the densities are greater.

The behaviour of a substance becomes still more complicated when on condensation it does not still remain gaseous, but changes its aggregate-state by becoming liquid. On the connexion of this process with those previously discussed some very fine experiments have recently been made by Andrews*. Submitting carbonic acid at various temperatures to powerful compressions, and observing the increases of pressure that took place, he found that there is an essential difference in its behaviour, according as the temperature is above or below 31°. Above 31°, only the above-mentioned deviations from Mariotte and Gay-Lussac's law are shown, while below 31° at a certain pressure condensation takes place.

Andrews represents the relations between pressure and volume observed by him by curves which have pressure and volume as abscissæ and ordinates. The curves referring to temperatures above 31° exhibit a continuous course; while those referring to temperatures below 31° are broken, as with diminishing volume the pressure at first increases, but from a

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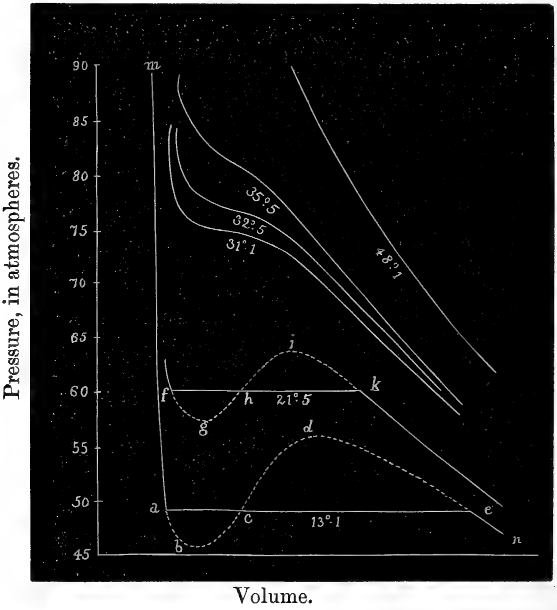
* Andrews, Phil. Trans. 1869, p. 575.

certain volume onwards, at which condensation begins, subsequent diminution of volume takes place without increase of pressure, and only when the volume has become much smaller and the entire mass is liquid, does the pressure again commence to increase as the volume is diminished, the increase then being rapid. The portion of the curve corresponding to the occurrence of condensation is a straight line, met at both ends by the continuously curved portions of the curve. A slight curvature delineated by Andrews at one extremity of the straight line appears to depend on a slight mixture of air, and hence may here be left out of consideration.

Two years afterwards James Thomson*, whose ingenious speculations have already so much contributed to the enlargement of mechanical and physical science, supplemented Andrews's curves by adding, at the places where in them a straight line is found, a curved line which joins on to the two curved portions of the curve given by Andrews in a continuous manner and represents a gradual transition from the gaseous to the liquid state, in which the entire quantity of the substance is found continuously in a similar state—a kind of transition which is theoretically thinkable, but cannot actually occur, because it contains intermediate states in which no stable, but only unstable, equilibrium exists. In fig. 1

Fig. 1.



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* Proc. Roy. Soc. November 1871.

Andrews's figure is reproduced with Thomson's supplementary curves added, drawn in dotted lines; only the external arrangement of the figure is altered, in a way first brought into use by Maxwell. Namely, Andrews represented in his figure the pressures by the abscissæ, and the volumes by the ordinates; but it is now customary, in the mechanical theory of heat, to represent the volumes by the abscissæ and the pressures by the ordinates, and the figure is redrawn in accordance therewith.

In the curves belonging to the temperatures 13°·1 and 21°·5, *a e* and *f k* are the above-mentioned straight lines corresponding to the occurrence of condensation, which J. Thomson has replaced by the dotted curved lines *a b c d e* and *f g h i k*.

J. Thomson drew his conclusion respecting the form of these lines merely from the form of Andrews's curves belonging to the higher temperatures—tracing how these latter gradually change as they approach toward the temperature 31°, and then continuing the same kind of change below 31°. Upon an investigation of the reasons for this peculiar conformation of the pressure-curves, and the formation of a mathematical expression corresponding to them, he did not enter.

As regards the latter point (the mathematical treatment of the subject), attempts have been made by various authors, some before and some since Andrews's experimental investigation, to express the deviations of the gases from Mariotte and Gay-Lussac's law by an equation.

Rankine* constructed an equation in place of (1), with which also an equation derived by Sir W. Thomson and Joule† from their experiments on the changes of temperature that take place during the expansion of gases very closely agrees, and which in its simplest form can be written thus,

$$pv = \mathrm{RT} - \frac{c}{\mathrm{T}v}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

in which c, as well as R, denotes a constant.

Hirn‡ effected upon equation (1) a transformation, in which the two above-mentioned circumstances that preeminently occasion deviation by gases from the law of Mariotte and Gay-Lussac, namely the volume of the molecules and their reciprocal attraction, are taken into account by the introduction of special quantities. The equation formed by him, which he says is applicable not merely to gases, but also to

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* Phil. Trans. 1854, p. 336.

† Ibid. 1862, p. 579.

‡ *Théorie mécanique de la Chaleur*, 2e éd. i. p. 195; 3e éd. ii. p. 211.

bodies in other states of aggregation, is

$$(p+r)(v-\psi)=\mathrm{RT}. \quad \ldots\ldots \quad (3)$$

Herein ψ denotes "la somme de volumes des atomes," and r "la somme des actions internes;" the latter quantity he also names "la pression interne." In his further treatment of this equation, by which he seeks to determine the quantities ψ and r, he makes inferences which seem to me not to be justified, and the result of which therefore, in my opinion, does not correspond with the reality.

Recknagel, in a memoir* which appeared in 1871, and which he supplemented in 1872, formed instead of (1) an equation agreeing externally with that set up by Rankine. It has the form

$$pv=\mathrm{RT}\left(1-\frac{\mathrm{B}_t}{v}\right), \quad \ldots\ldots \quad (4)$$

in which R is a constant, and B_t a function of the temperature, which latter Recknagel determines differently from Rankine, since according to him it is directly proportional to the absolute temperature, and inversely proportional to the corresponding pressure of the saturated vapour of the given substance.

J. D. van der Waals, in a very interesting work† published since Andrews's above-cited experiments, has substituted for (1) an equation in which, as in Hirn's, the two circumstances, the space filled by the molecules and the reciprocal attraction of the molecules, are taken into account: it has the form

$$\left(p+\frac{a}{v^2}\right)(v-b)=\mathrm{RT}, \quad \ldots\ldots \quad (5)$$

or, resolved with respect to p,

$$p=\mathrm{R}\frac{\mathrm{T}}{v-b}-\frac{a}{v^2}, \quad \ldots\ldots\ldots \quad (5\,\mathrm{A})$$

where R, a, and b are constants. The values of these constants for carbonic acid Van der Waals determined‡ as follows, taking as unit of pressure one atmosphere, and as unit of volume that which the carbonic acid occupies under the pressure of one atmosphere at the freezing-point; the quantity T_0 standing as denominator in the value of the first constant represents the absolute temperature corresponding to the freezing-point, consequently approximately the number 273:—

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* Pogg. *Ann.* Ergbd. v. p. 563, & cxlv. p. 469.

† *Over de Continuiteit van den Gas- en Vloeistoftoestand* (Leiden, 1873), p. 56.

‡ *Op. cit.* p. 76.

$$\left.\begin{aligned} R&=\frac{1{\cdot}00646}{T_0},\\ a&=0{\cdot}00874,\\ b&=0{\cdot}0023. \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (6)$$

This equation, extraordinarily simple in form, gives pressure-curves corresponding well with those constructed by Andrews and supplemented by J. Thomson, and exhibiting likewise the characteristic difference between the forms which belong to temperatures above and below 31°.

As to the more precise numerical accordance of the values of p calculated from this equation with those observed by Andrews, Van der Waals himself made the remark that with volumes less than 0·0046 the value of p can no longer be regarded as constant, but must become less as the volume decreases. But by what function of the volume one has to represent b he had not yet succeeded in discovering*.

In addition to these there are other deviations, which could only reveal themselves later; for after the publication of Van der Waals's work the stock of observations suitable for comparison with the calculated values received a great and important enrichment, as Andrews continued his investigation, and published in 1876 three new series of observations for the temperatures 6°·5, 64°, and 100°†, which far surpass those previously published in extent and possess an enhanced degree of exactness. On comparison with these experiments, the equation set up by Van der Waals is found not to agree with experience, and cannot be brought into accordance even by altering the values attributed to the constants, but needs for that purpose a more essential modification.

The principal reason of these deviations appears to me to be the following. Van der Waals assumed it as self-evident that the mutual attraction of the molecules is independent of the temperature, and therefore can only be a function of the volume. According to that, when a gas is heated at constant volume the molecular attraction must remain unaltered. This would doubtless be true if the motion of the molecules of a gas at a lower temperature differed from that at a higher temperature only by the different quantity of the mean *vis viva* of the motion, but in all other respects took place in precisely the same manner, the paths of all the molecules and the ratios of the velocities in the different stages of a path remaining the same. I also believe that such an assumption respecting the

* *Op. cit.* pp. 78 & 52.

† Phil. Trans. 1876, p. 421.

homogeneity of the motion would be admissible in considering only the ideal state, which we call the *perfect* state of a gas; but when we have to investigate the deviations of a gas from the perfect state, that assumption appears to me to be inadmissible.

I will not here advance any definite theory about the way in which the motion changes when a gas leaves the perfect state; but I will take leave to adduce a mode of alteration as at least a possible one. For the perfect gaseous state it may be assumed that every two molecules that rush together separate again after the collision. On the contrary, when the gas is condensed to liquidity, a quite different behaviour takes place: namely, the molecules are in general held together by their mutual attraction; and only exceptionally, on a specially favourable coincidence of the phases of motion, do individual molecules separate themselves from the rest of the mass. Now, between these two extreme cases one can well imagine an intermediate state of this kind:—As a rule, indeed, the molecules separate again after the collision; but it sometimes happens that two molecules after meeting do not again separate, but only oscillate about one another while carrying out the progressive motion in common, until, by the change that takes place in the motion on further collision, the separation is again occasioned. The number of such pairs of attached molecules would then become so much the greater the lower the temperature (and hence the less the mean *vis viva* of the motion) became; and on a further fall of the temperature there might supervene instances of not merely two, but several molecules holding together and executing as groups the progressive motion in common.

If such a behaviour occurred, the mean strength of the mutual attraction of the molecules would be thereby increased, since the molecules remaining united would of course, on account of the greater nearness, attract one another more strongly; and, *accordingly, it would not be allowable to regard the quantity which represents in the formula the mutual attraction of the molecules as independent of the temperature, but one would be obliged to admit that it becomes greater with falling temperature.*

Van der Waals has, further, from theoretical considerations, drawn the conclusion, also already expressed by others in their formulæ, that the decrease of pressure conditioned by the mutual attraction of the molecules is inversely proportional to the square of the volume. It may be granted that for larger volumes this conclusion is approximately correct; and yet no universal and rigorous validity need be ascribed to it, but one may assume that from it also a deviation takes place which becomes the greater the smaller the volume becomes.

Now I have tried to construct for p a formula which retains what appears to me to be correct in previous formulæ, but at the same time makes allowance for the above-mentioned modifying circumstances, and, while as simple as possible, is in satisfactory accordance with both the older and the newer observations of Andrews, as well as with the other extant observations. This, on account of a peculiar circumstance, is beset with great difficulties. The formula to be formed for p has, as may be seen even from the equations (2), (3), (4), and (5) when solved with respect to p, the peculiarity that it is the difference of two quantities which may both have much higher values than p. The effect of this is, that inaccuracies which in proportion to the two single quantities are but little may yet in p produce deviations from experiment considerable in comparison with its value, and therefore each single quantity must be so much the more precisely determined.

The formula which I have constructed has the following form:—

$$p=\mathrm{R}\frac{\mathrm{T}}{v-\alpha}-\frac{c}{\mathrm{T}(v+\beta)^2}, \quad \dots\dots \quad (7)$$

wherein R, c, α, and β are constants.

For carbonic acid these constants, if (as before) the chosen unit of pressure be one atmosphere, and the unit volume that which is occupied by the carbonic acid under the pressure of one atmosphere and at the temperature of the freezing-point, are to have the following values:—

$$\left.\begin{aligned}\mathrm{R}&=\frac{1\cdot00682}{\mathrm{T}_0}=0\cdot003688,\\ c&=2\cdot0935,\\ \alpha&=0\cdot000843,\\ \beta&=0\cdot000977.\end{aligned}\right\} \quad \dots\dots \quad (8)$$

If, on the other hand, as pressure-unit the pressure of a kilogram upon a square metre, and as volume-unit a cubic metre be chosen, it being presupposed that the quantity of carbonic acid is a kilogram, we have to attribute to the constants the following values:—

$$\left.\begin{aligned}\mathrm{R}&=19\cdot273,\\ c&=5533,\\ \alpha&=\ 0\cdot000426,\\ \beta&=\ 0\cdot000494.\end{aligned}\right\} \quad \dots\dots\dots \quad (9)$$

In order to test the accordance of this formula with experiment, I selected for comparison the three newer series of observations made by Andrews which relate to the temperatures 6°·5, 64°, and 100°, and three of the older series, referring to the temperatures 13°·1, 31°·1, and 48°·1. In each of these series, I selected for consideration, out of the volumes therein occurring, a number which are distributed as equally as possible over the entire interval of observation. I have, however, to remark that Andrews has not so expressed the volumes that all the numbers have one and the same unit for a base, but with each temperature he has taken as unit that volume which the carbonic acid, under the pressure of one atmosphere, occupies *at that temperature.* The volume thus expressed he designates by ϵ. In order to calculate from this the volume we designate by v (which has for its fundamental unit the volume occupied by the carbonic acid *at the freezing-point* under the pressure of one atmosphere), we must know the coefficient of expansion of carbonic acid under atmospheric pressure. For this the value found by Regnault, 0·00371, and which Andrews used in his calculations, is employed.

The values thus obtained of v are, together with the respective temperatures, inserted in the formula, and then from these the values of p belonging to them calculated. These values are designated in the following Tables by "p (calc.);" and below them are the corresponding observed values, designated by "p (obs.)." Δ denotes the differences between the observed and the calculated values of p. At each series it is stated whether it belongs to the older or the newer series of Andrews's observations.

TABLE I.—Temp. 6°·5 (newer series).

ϵ.	$\frac{1}{16\cdot13}$.	$\frac{1}{29\cdot62}$.	$\frac{1}{45\cdot80}$.
v	0·06349	0·03458	0·02236
p (calc.)......	14·65	24·63	34·15
p (obs.)	14·68	24·81	34·49
Δ	− 0·03	− 0·18	− 0·34

TABLE II.—Temp. 13°·1 (older series).

ϵ.	$\frac{1}{76\cdot16}$.	$\frac{1}{80\cdot43}$.	$\frac{1}{480\cdot4}$.	$\frac{1}{510\cdot7}$.
v	0·013768	0·013037	0·0021828	0·0020532
p (calc.) ...	47·98	49·27	54·66	74·96
p (obs.) ...	47·50	48·76	54·56	90·43
Δ...........	+ 0·48	+ 0·51	+ 0·10	−15·47

TABLE III.—Temp. 31°·1 (older series).

ϵ.	$\frac{1}{80{\cdot}55}$.	$\frac{1}{124{\cdot}4}$.	$\frac{1}{174{\cdot}4}$.	$\frac{1}{311{\cdot}1}$.	$\frac{1}{405{\cdot}5}$.
v	0·013847	0·008966	0·006395	0·003585	0·002751
p (calc.) ...	54·92	68·44	75·33	78·22	92·47
p (obs.) ...	54·79	67·60	73·83	75·40	85·19
Δ...........	+ 0·13	+ 0·84	+ 1·50	+ 2·82	+ 7·28

TABLE IV.—Temp. 48°·1 (older series).

ϵ.	$\frac{1}{86{\cdot}45}$.	$\frac{1}{146{\cdot}8}$.	$\frac{1}{298{\cdot}4}$.
v	0·013631	0·008028	0·003949
p (calc.)	62·05	84·42	112·6
p (obs.)	62·60	84·35	109·4
Δ	− 0·55	+ 0·07	+ 3·2

TABLE V.—Temp. 64° (newer series).

ϵ.	$\frac{1}{24{\cdot}18}$.	$\frac{1}{46{\cdot}34}$.	$\frac{1}{83{\cdot}44}$.	$\frac{1}{185{\cdot}5}$.	$\frac{1}{446{\cdot}4}$.
v	0·05118	0·02670	0·01483	0·006671	0·002772
p (calc.) ...	22·41	39·95	63·99	107·06	202·30
p (obs.) ...	22·56	40·54	64·96	106·88	222·92
Δ	− 0·15	− 0·59	− 0·97	+ 0·18	− 20·62

TABLE VI.—Temp. 100° (newer series).

ϵ.	$\frac{1}{26{\cdot}09}$.	$\frac{1}{50{\cdot}63}$.	$\frac{1}{96{\cdot}65}$.	$\frac{1}{218{\cdot}0}$.	$\frac{1}{379{\cdot}3}$.
v	0·05255	0·02708	0·014185	0·006289	0·003615
p (calc.) ...	24·65	45·30	78·69	146·29	230·09
p (obs.) ...	24·85	45·99	80·25	145·44	223·57
Δ...........	− 0·20	− 0·69	− 1·56	+ 0·85	+ 6·52

From these Tables it is evident that there is in general a satisfactory, and in part a strikingly good accordance between the values of p calculated from the formula and those observed; nevertheless, at the greatest densities of carbonic acid reached in the experiments, amounting to four or five hundred times the densities occurring under the pressure of one atmosphere, considerable differences are met with. These at first induced me to undertake another modification of the formula, by introducing into the second term a temperature-function as a factor in order to balance the differences. Through this, however, the formula lost its simplicity; and the question arose whether these differences were really of so much importance as to justify such an alteration of the formula. This question, upon closer consideration of the matter, I believed must be answered in the negative.

For the differences change their signs in quite a striking manner: at 13°·1 the greatest difference is negative, at 31°·1 positive, at 64° negative, and at 100° again positive. So frequent a change of sign does not make for the supposition that the cause of the differences lies in the formula, but rather that it is to be sought in errors of observation; and in the case in question such errors are very well conceivable, even with the most careful observation. The volume of the highly condensed carbonic acid was measured in capillary tubes. Now, when it had become so small as to amount to only one four- or five-hundredth part of its original magnitude, errors might easily occur in the reading, which, though absolutely very small, were relatively great enough to cause, in the formula for p, of which the value very rapidly changes with v when the values of v are small, differences from the quantities in the Table.

Besides, the air-manometer used for the determination of the pressure consisted of a capillary tube, in which the air, when the greatest pressures occurred, occupied volumes so small that a slight error of observation must have exerted a very great influence upon the pressure deduced from the observation.

Further, it is to be remarked that Andrews, in deducing the pressure from the data supplied by the air-manometer, started from the hypothesis that air follows Mariotte's law up to the greatest pressures employed in the experiments, which reached more than 200 atmospheres. But this, it is well known, is not the case; at such pressures considerable deviations take place. I at first tried to take advantage of the observations of Cailletet and Amagat on the compression of nitrogen, in order to correct the pressure-quantities deduced from the manometer-indications; but I found that the results of their observations are not sufficiently accordant with each other to be employed with safety for such a correction. Hence I have simply quoted in the Tables Andrews's values of p.

Lastly, I must call attention to a peculiar distinction which appears in the differences between the observed and the calculated values of p: namely, in the older series the differences are almost all positive, and in the newer almost all negative. This also makes for the hypothesis that the differences originate rather in the circumstances affecting the experiments than in the formula.

For all these reasons it must be concluded that some uncertainty still attaches to the highest observed values of p; and consequently accordance between the calculated and the observed values must not be too strictly insisted on. I therefore abandoned the above-mentioned complicating alteration of the formula, and reverted to its original and most simple form, which seems to me, not only in respect of practice, but of theory also, to deserve preference.

By rather laborious calculations I so determined the constants of the formula that the resulting values of p agreed as well as possible with both the newer and the older results of observation obtained by Andrews, and that, of the differences remaining at high densities of the carbonic acid, about as many are positive as negative. With these values of the constants there is also a satisfactory accordance with Regnault's results of observation, which in regard to the condensation of carbonic acid do not extend so far, by a long way, as those of Andrews. I therefore think that these values of the constants correspond with sufficient accuracy to the stock of observations at present existing, which is more complete as regards carbonic acid than with respect to any other gas.

To the other gases the general equation (7) can, in my opinion, be applied; but of course the constants must be determined for each gas.

In connexion with the foregoing another question must be discussed, which forces itself upon us in the consideration of the curves drawn by Andrews and completed by James Thomson.

When a gas, *e. g.* carbonic acid, is compressed at a temperature below the critical temperature, at a certain volume condensation begins; and therewith a state enters in which one portion of the substance is liquid and the other gaseous. As long as this state continues, with the further diminution of the volume the pressure remains constant, and the corresponding part of the isothermal pressure-curve is consequently a horizontal straight line. Beside this straight line, one can imagine, as was discussed above, according to James Thomson, another isothermal pressure-curve, representing that pressure which would take place at the same change of volume if this proceeded in such wise that constantly the entire quantity of the substance was in the same state. Although this latter kind of change of volume does not really take place, because the states of equilibrium occurring in it are in part unstable, yet it must be regarded as theoretically possible; and, in fact, the latter pressure-curve represents the pressure determined by our formula.

But now comes the question, In what position do this theoretical pressure-curve and the horizontal straight line corresponding to the actual process stand to each other?

James Thomson has expressed no opinion upon it, but has only, in Andrews's figure (reproduced in fig. 1), added to the pressurve-curves referring to the temperatures 13°·1 and 21°·5 the curve-portions there drawn in dotted lines. These were probably only intended to give a rough idea of the possible form and position of the portions of the curves. In that form they cannot be accepted as really correct.

Maxwell, in the first edition of his 'Theory of Heat,' p. 125, goes into the matter more closely. If one of the theoretical continuous pressure-curves, *e. g.* that belonging to the temperature 13°·1, be imagined to be given, and if the horizontal straight line be drawn at different heights, two points *a* and *e* are always obtained as the extreme points of the straight line. The difference between the two values possessed by the energy of the substance in the states corresponding to those two points differs in magnitude for the different positions of the straight line. Now, says Maxwell, that position of the straight line at which this difference is a maximum is the correct one. In the fourth edition, however, the passage is altered and the position of the straight line left undetermined. It must therefore be assumed that Maxwell afterwards relinquished his previous view on this point.

Van der Waals says (in p. 121 *loc. cit.*), "I have not succeeded in finding in any of the properties of saturated vapour a characteristic by which it could be determined where the [straight] line must be drawn through the isotherms."

After this the question what position the horizontal straight line giving the pressure of saturated vapour has in the isothermal pressure-curve may well be regarded as still an open one; and I wish to be permitted to communicate here the answer to this question which has presented itself to me on consideration of the subject.

When the pressure-curve drawn by Andrews and completed by James Thomson for the temperature of 13°·1, is examined, it is seen to be single from *m* to *a* and again from *e* to *n*, while it is double between *a* and *e*. Between the two states of the substance corresponding to the points *a* and *e* (and which we will briefly name states *a* and *e*) there are consequently two ways in which the substance can pass out of the one into the other. The transition can take place on both of these paths, under perfectly similar circumstances, in the direction from *a* to *e* and also in the direction from *e* to *a*; the respective changes are therefore both to be designated as reversible.

Now, if we imagine the substance making the transition from *a* to *e* along the path represented by the curve *a b c d e*, and returning from *e* to *a* along the path represented by the straight line *e a*, we have a reversible cyclical process. Hence, for the positive or negative quantity of heat communicated from without to the changing substance in the course of this process, an element of which may be called $d\mathrm{Q}$, the well-known equation

$$\int\frac{d\mathrm{Q}}{\mathrm{T}}=0$$

must hold good. And since in the present case the temperature T is constant, and the lines which graphically represent the cyclical process are only isothermal lines for one and the same temperature, the equation simplifies itself into

$$\int d\mathrm{Q}=0.$$

Therefore the positive and negative quantities of heat communicated to the substance eliminate one another.

It hence follows, further, that the positive and negative parts of the external work done during the cyclical process must also counterbalance one another. The excesses of the positive work over the negative belonging to the two sections of the cyclical process are represented by the areas occurring in the figure: the area *c d e c* above the straight line represents a positive, and the area *a b c a* beneath the same line a negative excess. Consequently these two areas must, in order to give 0 for the value of the total work, be *equal the one to the other.* Hereby, when the theoretical pressure-curve corresponding to the homogeneous state is given, the position of the horizontal straight line answering to the actual processes of vaporization and condensation is also determined.

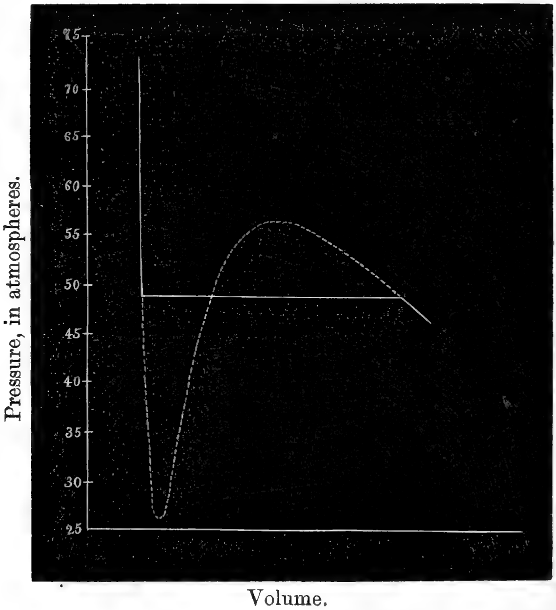
The foregoing condition can be expressed as a proposition thus:—*The pressure of the saturated vapour is so great that the external work performed at the vaporization is equal to that which would be performed if, the increase of volume being the same, the substance remained homogeneous.* This can also be put still more briefly thus:—*The pressure of the saturated vapour is equal to the mean pressure of the substance remaining homogeneous while receiving an increase of volume corresponding to complete vaporization.*

The curve drawn between *a* and *e* by J. Thomson for the temperature 13°·1 does not correspond to this condition; for the areas of the figures *a b c a* and *c d e c* formed by it and the horizontal straight line are visibly unequal. Now, in order to see what is the form taken when equation (7) is employed for the determination of the pressure, I have calculated the corresponding values of p for so many values of v lying between a and e that from them may be understood the course of the curve between a and e. The values therein obtained, together with the values of p already given above in Table II. referring to some values of v outside of the interval $a\,e$, are collected in the following Table:—

v	0·013768	0·013037	0·012	0·011	0·010	0·009	0·008	0·007	0·006	0·005	0·004	0·00350	0·00325	0·00300	0·00285	0·00275	0·00250	0·0021828	0·0020532
p	47·98	49·27	51·12	52·87	54·50	55·84	56·63	56·38	54·28	49·00	38·83	32·05	28·83	26·53	26·11	26·52	31·52	54·66	74·96

On the employment of these values a curve is obtained of the form given in fig. 2. This curve corresponds with satisfactory accuracy to the above condition.

Fig. 2.



LIII. *The use of Silver Films in improved Instruments of the Camera-Lucida Class.* By JOHN C. DOUGLAS*.

THESE instruments are divisible into two classes, viz. :—that in which a reflected image of the object is seen while the tracing-point is seen direct; and the other class, in which the object or tracing is seen by reflection, but the tracing-point or object is seen by light transmitted through a plate which acts at the same time as a reflector.

The forms in most common use are the camera lucida and the steel disk or Sœmmering's mirror (of the first class), and the parallel plate or tinted glass reflector (of the second class). There are other forms, less common, but each referable to one of the two classes described above. Instruments of the first class give a brilliant and well-defined reflected image ; but they are fatiguing to use, and some persons experience great difficulty in using them. Instruments on the other principle are far more easily used, they cause less fatigue ; but the reflected image is not so brilliant. In the case of the plane glass reflector the definition cannot be so good, as both surfaces of the glass reflect, and there are therefore two superimposed images which do not exactly coincide ; the second reflection, however, is weakened by using tinted glass ; and this colouring also serves to reduce the transmitted light, which would otherwise flood out the weak reflected image.

What is required in an instrument of this kind is the brilliancy and clear definition of the camera lucida, combined with the simplicity and ease in use and the cheapness of the tinted plane glass reflector, with facility, where desired, for using two reflections in order that the reflected image may not be reversed.

I believe these requirements are attainable by the use of silver films on glass. Silver films are so highly reflective that two or more successive reflections may be used if desired ; by transmitted light the colour of the film is suitable for tinting the glass. The thickness of the film may be regulated according to requirements—a thick film being used when reflection only is required, and a thinner one according to the ratio desired between the reflected and transmitted light. The reflective power of the thinnest film is greatly superior to that of glass. The silver film is applicable to most forms in use, and it may be used not only on plane but on curved surfaces : *e. g.*

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* Communicated by the Author, having been read at the Meeting of the Asiatic Society of Bengal, Calcutta, on the 7th of April, 1880.

a plane concave lens silvered on the plane side might be used by a shorted-sighted person instead of the common plane reflector used in sketching microscopic objects; a slight curvature of the first or second reflecting surface in the camera lucida might be used to render it unnecessary to employ a lens to equalize the sensibly different distances of the images of the object and plane of delineation. The cost of silver films on glass is very trifling; and if taken care of they last for years. A number might be made at intervals; or they might be supplied for a trifling sum by the opticians.

For many purposes the films might be deposited on thin glass and varnished, or protected by glass, when they would be very durable and would bear handling. For some purposes the film might be thickened by electro-deposition and removed from the glass. As the films are so cheap, a number of graduated thicknesses might be kept, and a suitable one selected in each case to adjust the relative brilliancies of the reflected and transmitted light; or the films might be applied as the dark glasses usually supplied with the camera lucida: but this seems less simple and convenient than the use of a thicker or thinner film as transmitting reflector. A silver surface may reflect upwards of 90 per cent. of the incident light; a total reflecting prism has been found to reflect only about 75 per cent. or less, the loss being due to reflection at the first surface and absorption; the superiority of the silver surface is evident, particularly where several successive reflections are required. Even if the highest attainable brilliancy be not generally required, still the higher this is the greater the range of adjustment without alteration of the source of light. The strictest regularity in the film not being essential, suitable films are very readily obtained. With strict cleanliness, pure chemicals, care that the glass is wetted equally in every part by water or alcohol at the moment of immersion in the silvering solution, and care that the solution is properly mixed (*i. e.* homogeneous), success is readily attained.

I find* that Professor Govi, of Rome, has devised a form of camera lucida in which a metallic film is used. He simply gilds the reflecting surface of the camera-lucida prism with a thin film of gold, and cements to this surface with Canada balsam another similar prism. M. Nachet has adopted this improvement in the construction of various forms of camera lucida.

The greater advantages of the silver film are obvious. By the use of silvered glass, instruments of various forms and of large size may be readily constructed for a trifling sum

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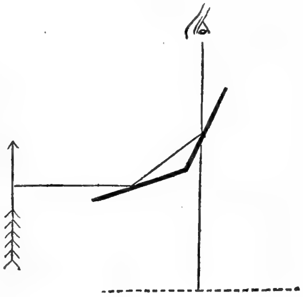
* Annual Record of Science and Industry, 1875, p. 144.

by any ingenious person; thus an instrument may be devised and readily constructed for any special purpose. The following is a description of the instruments exhibited at the meeting:—

(1) An ordinary tinted glass reflector for use with the microscope. The tinted glass usually used was replaced by a piece of glass covered with a thin film of silver. The silvered side is turned towards the eyepiece and reflects the magnified image. In this form several reflectors, differing in the thickness of the silver film, should be available for regulating the ratio between the transmitted and reflected light; but a certain thickness of film will be found which is applicable to most purposes; so that change of reflector is seldom necessary.

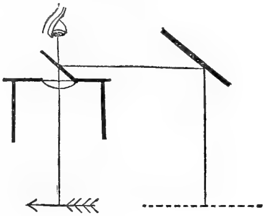
(2) Camera lucida with double reflection (fig. 1). The first reflection is from a thick film of silver; the second is from a thinner film. The thickness of the second film may be adjusted as described above. It will be seen that the plane of delineation is seen *through* the second reflector, not past it as in the ordinary instrument. In the diagrams the thick oblique lines are the silver films, the thin lines the directions of the light; the arrows the object, and the dotted line the paper on which the object is to be drawn.

Fig. 1.



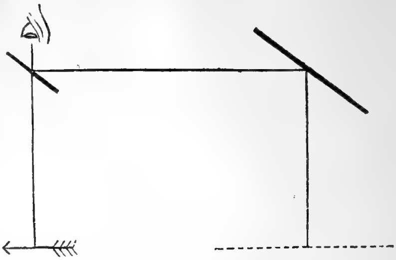
(3) A form of reflecting camera for sketching microscopic objects (fig. 2). This instrument being fitted to the eyepiece of the microscope, the paper and pencil-point under the larger reflector appear in the field of the microscope. The object is seen direct. The second mirror in the instrument exhibited was an inch square. This instrument may be used with the body of the microscope at any angle, it being merely necessary to place the drawing-paper in a plane parallel with that of the microscope-stage. In the figures 2 and 3 the mirrors are represented as parallel; they should usually be inclined to each other, to increase the distance between the plane of delineation and the object.

Fig. 2.



(4) Another reflecting camera for sketching small objects is

Fig. 3.



represented in fig. 3. In the instrument exhibited, the larger reflector was $1\frac{1}{2}'' \times 1\frac{3}{4}''$ and placed 10″ from the paper; the field was about $4\frac{1}{2}$ in. square. This instrument may be used horizontal or inclined; and it is admirably adapted for drawing such objects as insects, leaves, shells, &c. If the vertical distances between the mirrors and the object and paper respectively be constant, in instruments of this form the relative magnitudes of object and drawing will obviously vary with the distance between the reflectors. It is evident that by the use of reflectors in instruments of this class, the reflecting surfaces may be larger and the distance between them greater than if a prism were used. The above are only examples of the application of silver films to a particular class of instruments; it is evident that they offer great facility for giving this class of instruments its maximum development. It is obvious also that silver films are applicable with advantage in many other cases where prisms are used at present, particularly where it is desired to divide a beam of light into two; *e. g.* if fig. 3 be turned upside down and the two eyes of the observer be in the place of the arrow and the dotted line, the diagram represents an arrangement suitable for a non-stereoscopic binocular microscope, the inclination between the mirrors being varied to suit the distance between the eyes. The loss of light in such an arrangement would be very little; and the brilliancy of the two images might be rendered very nearly equal. To illustrate how cheaply such instruments may be made, the mirrors in the instruments exhibited were mounted in tubes of thin sheet zinc, which is readily cut with ordinary scissors and bent into shape with pliers; a coat of asphalt varnish used for making shallow cells was applied for the sake of appearance.

The instrument, fig. 2, was fastened to the eyepiece by a piece of zinc bent half round the eyepiece-tube, and held against it by a small elastic rubber band. The tinted reflector was supported by a bent plate of zinc, hung on the milled edge of the eyepiece by a groove passing almost half round the eyepiece: this is a most convenient method of attaching the reflector or camera to the eyepiece, as it is quite firm enough, and yet removable in an instant without disturbing the microscope.

Note.—Silver films might possibly be found useful in the construction of illuminating apparatus for the microscope. If well varnished, they might well replace the usual mercurial reflecting surface used in illuminating-mirrors, as the mercurial layer is very perishable, particularly in a damp climate, as in India during the rainy season.

LIV. *On the Diffusion of Liquids.*

By JOHN H. LONG, *of Lawrence, Kansas, U. S. A.*

[Concluded from p. 329.]

IN the above I have explained the general workings of the method and discussed its chief sources of error. It remains now to mention several other causes of error. From the tables given it is seen that the amounts diffused undergo slight fluctuations from time to time. Various causes can be assigned for this. The temperature undergoing slight changes has its effect, of course, in producing currents in the solutions, besides changing for the time being their rates of diffusion. Other disturbing currents are produced when, by any cause, the diffusion-vessels are jarred, as when, for instance, a door in their vicinity is rapidly closed. In order to avoid another source of error, it is to be observed, of course, that the table upon which the vessels stand be horizontal, in order that $g\,h$ be parallel to $e\,f$.

I turn now to the experiments relating to the comparison of different salts as to their rates of diffusion—the real object of the investigation. This necessitated the performance of a great number of single experiments; and in order that the results of these be comparable, it was necessary that the conditions be in all cases the same. This end could be attained, of course, by diffusing all the salts, one after the other, from the same tube, observing the above-mentioned points regarding concentration &c. But to carry the investigation out in this way would require a very long time. The only alternative was to make use of a larger number of diffusion-tubes; and this I did. Twelve tubes and beakers, having as nearly as possible the same dimensions and of the form represented in fig. 1, were obtained, and compared by filling them with $2n$ NaCl solution and observing the amounts diffused in equal times, the velocity of the water being in all cases 40 cubic centims. Some of these tubes were almost immediately rejected: in one the section of i had been left elliptical instead of circular by the glassblower; in another the point b was too small and did not allow the water to pass through fast enough;

and a third was rejected for still another cause. After a number of trials six were found which gave constant results, and diffused amounts of salt in equal times proportional to 17, 18, 17, 17, 17, 17. Of course these rates were subject to slight fluctuations; but the averages for any length of time were constant. In searching for the cause of the larger amount diffused from the tube No. 2, it was found that in it *i* was slightly shorter than in the others. A normal NaCl solution stood in the six tubes at the height *ef* when the velocity of the water was 40 cubic centims. The exact dimensions of the tubes are as follows—the internal diameter of *a r s t* 1·5 millim., the diameter at *b* 1 millim., the diameter of *i* 14·5 millims., the distance *g e* 11 millims., and the distance *g k* 15 millims. The beaker-glasses, when filled to the level *a b*, contained 240 cubic centims. All the experiments described below were performed with these six tubes, and those with the coloured solutions above also. The other experiments cited above were performed with other tubes having slightly different dimensions. In these the part *i* had a slightly greater diameter (about 16 millims.) and was somewhat shorter.

At the beginning of the series of experiments given below each tube was filled with a different salt-solution; but this plan was soon abandoned, and two salts were then diffused simultaneously—that is, in three of the glasses a solution of one salt, and in the other three a solution of the other. In some cases the amounts diffused from each of the three glasses of each set were determined separately; and in other cases (with the nitrates and sulphates always) the amounts from the three tubes of each set were determined together. The values given below are in all cases the averages reduced for one cell.

The amount given by tube No. 2 was first reduced to the standard of the other five.

The velocity of the water was maintained constant at 40 cubic centims.; and the temperature of the room was 14°–16° C. As explained above, after filling the tubes, about 48 hours were allowed to elapse before the determinations of the diffused salt were commenced. The appearance of the diffusion-cells as they stood when in operation is shown in fig. 3 (Pl. VII.).

The Mariotte bottles held in some cases 8 litres, and in others 4. The latter are here represented.

The salts made use of were in most cases obtained quite pure; in other cases it was necessary to purify them myself (LiCl, $LiNO_3$ for instance). Where possible, the solutions were made by weighing out the required number of grammes of dry or well-crystallized salt and dissolving this in a litre of water. The strength of this solution was then afterwards determined as a control. In the case of some few salts ($CaCl_2$ for instance) only the latter method could be well applied.

The first experiments were on solutions of the chlorides; and the diffused amounts were determined by means of $\frac{n}{40}$ $AgNO_3$ with K_2CrO_4 as indicator. The burettes were furnished with Erdmann's floats, and their accuracy tested by comparing them with a standard.

The results of the experiments are as follows:—

Diffusion of $2n$ NaCl.

Time.	Cub. cent. $\frac{n}{40}$ $AgNO_3$.	Cub. cent. per hour.
8–9	2·1	2·10
9–11	4·0	2·00
11–2	5·7	1·90
2–4	3·7	1·85
4–8 A.M.	28·8	1·80
8–9	2·0	2·00
9–2	10·1	2·02
2–4	4·2	2·10
4–8 A.M.	33·6	2·10
8–9	2·1	2·10
9–1	8·4	2·10
53 h.	104·7	1·97

Diffusion of $2n$ NH_4Cl.

Time.	Cub. cent. $\frac{n}{40}$ $AgNO_3$.	Cub. cent. per hour.
4–8 A.M.	32·5	2·03
8–9	2·4	2·40
9–10	2·3	2·30
10–11	2·4	2·40
11–2	7·2	2·40
2–4	4·9	2·45
4–8 A.M.	38·4	2·40
8–1	10·5	2·10
45 h.	100·6	2·23

In another experiment with NH_4Cl solution, 44·5 cubic centims. were required in 19 hours—that is, 2·34 cubic centims. per hour. The average of these is 2·28 cubic centims. per hour; and this for *n* solution is equivalent to about 1·15 cubic centim. per hour, or

0·0369 gram NH_4Cl in 24 hours.

The rate of the NaCl solution is almost exactly equivalent to 1 cubic centim. per hour for *n* solution, or

0·0351 gram NaCl in 24 hours.

Diffusion of n KCl.

Time.	Cub. cent. $\frac{n}{40}$ $AgNO_3$.	Cub. cent. per hour.
8–9	1·40	1·40
9–11	2·90	1·45
11–2	4·35	1·45
2–4	2·64	1·32
4–8 A.M.	20·85	1·30
8–10	2·70	1·35
26 h.	34·84	1·34

Diffusion of $2n$ LiCl.

Time.	Cub. cent. $\frac{n}{40}$ $AgNO_3$.	Cub. cent. per hour.
3–4	2·1	2·10
4–8 A.M.	28·8	1·80
8–11	5·1	1·70
11–2	5·7	1·90
2–4	3·8	1·90
4–8 A.M.	30·4	1·90
41 h.	75·9	1·85

This is equivalent to

0·0599 gram KCl in 24 hours;

and for *n* LiCl 0·93 cubic centim. per hour, or

0·0237 gram LiCl in 24 hours.

Another series of experiments gave

0·0230 gram LiCl in 24 hours.

The LiCl was not absolutely free from NaCl, although treated a number of times with alcohol in the usual manner. The spectroscope still showed the yellow line. Other metals were absent.

Diffusion of *n* $CaCl_2$.

Time.	Cub. cent. $\frac{n}{40}$ $AgNO_3$.	Cub. cent. per hour.
9–12	3·9	1·30
12–3	3·9	1·30
3–5	2·7	1·35
5–9 A.M.	24·0	1·50
9–10	1·3	1·30
25 h.	35·8	1·43

Diffusion of *n* $MgCl_2$.

Time.	Cub. cent. $\frac{n}{40}$ $AgNO_3$.	Cub. cent. per hour.
11–1	2·8	1·40
1–3	3·3	1·65
3–5	2·9	1·45
5–9 A.M.	20·8	1·30
9–5	8·8	1·10
30 h.	38·6	1·29

The first of these corresponds to

0·0476 gram $CaCl_2$ in 24 hours.

In another series of experiments with $MgCl_2$, 21·6 cubic centims. were required in 16 hours. The average of these corresponds to

0·0373 gram $MgCl_2$ in 24 hours.

Diffusion of $\frac{n}{2}$ $BaCl_2$.

Time.	Cub. cent. $\frac{n}{40}$ $AgNO_3$.	Cub. cent. per hour.
4–8 A.M.	11·40	0·71
8–9	0·65	0·65
9–11	1·50	0·75
11–2	2·40	0·80
2–4	1·60	0·80
4–8 A.M.	12·90	0·81
8–10	1·60	0·80
10–12	1·30	0·65
44 h.	33·35	0·76

Diffusion of $\frac{n}{2}$ $SrCl_2$.

Time.	Cub. cent. $\frac{n}{40}$ $AgNO_3$.	Cub. cent. per hour.
10–11	0·8	0·80
11–1	1·7	0·85
1–2	1·0	1·00
2–4	1·5	0·75
4–8 A.M.	11·7	0·73
8–10	1·5	0·75
10–12	1·4	0·70
12–7	4·6	0·67
33 h.	24·2	0·73

Reduced to normal solutions, these figures correspond to

0·0936 gram $BaCl_2$ in 24 hours,

and

0·0686 gram $SrCl_2$ in 24 hours.

Diffusion of $NiCl_2$ and $CoCl_2$.

Experiments with *n* solutions of these salts required for $CoCl_2$, 1·02 cubic centim. $\frac{n}{40}$ $AgNO_3$ per hour, corresponding to

0·0397 gram $CoCl_2$ in 24 hours;

and for $NiCl_2$, 1·01 cubic centim. $\frac{n}{40}$ $AgNO_3$ per hour, corresponding to

0·0394 gram $NiCl_2$ in 24 hours.

An attempt was made to determine the rate of diffusion of $Fe_2 Cl_6$; but the rapid dissociation of this salt in solution made the results of no value.

Diffusion of Bromides and Iodides.

These salts were diffused as were the chlorides, and determined in the same manner.

Diffusion of *n* KBr.

Time.	Cub. cent. $\frac{n}{40}$ $AgNO_3$.	Cub. cent. per hour.
3–5	2·74	1·37
5–8 A.M.	19·70	1·31
8–1	6·55	1·31
1–5	5·22	1·30
5–8 A.M.	18·75	1·25
8–12	6·40	1·60
12–4	6·04	1·51
4–8 A.M.	21·64	1·35
8–12	6·05	1·51
69 h.	93·09	1·35

Diffusion of *n* NH_4 Br.

Time.	Cub. cent. $\frac{n}{40}$ $AgNO_3$.	Cub. cent. per hour.
8–12	4·1	1·02
12–4	4·0	1·00
4–8 A.M.	15·6	0·97
8–12	4·5	1·12
12–4	4·7	1·17
4–9 A.M.	18·5	1·09
9–1	4·5	1·12
53 h.	55·9	1·05

These values correspond to

0·0965 gram KBr in 24 hours,

and

0·0617 gram NH_4 Br in 24 hours.

Experiments with *n* solutions of KI, NaI, NaBr, and KCy gave the following results:—

nKI	required	1·37 c.c.	$\frac{n}{40}$AgNO$_3$	per hour	=0·1364	gr.	KI	in 24 hours.
nNaI	„	1·12	„	„	=0·1008	„	NaI	„
nNaBr	„	0·85	„	„	=0·0525	„	NaBr	„
nKCy	„	0·64	„	„	=0·0499	„	KCy	„.

The last value can be considered only an approximation to the truth, as the cyanide contained a little cyanate, and, besides, in solution it suffers always more or less decomposition.

Diffusion of Nitrates.

The first difficulty in connexion with these salts was the accurate determination of the small amounts diffused. This was finally accomplished, however, by means of the "Marx" method, which, as is well known, depends on the decoloration of a solution of indigo by free HNO_3.

An indigo solution was prepared as recommended by Marx*, and was of such strength that 8 cubic centims. corresponded to 1 milligram NO_3. The liquid containing diffused salt was evaporated to a small volume and 25 cubic centims. taken. To this were added 50 cubic centims. concentrated $H_2 SO_4$, and the indigo was run in immediately from a burette. If more than 30 cubic centims. of this were required, the operation was repeated with a weaker liquid. By means of this preliminary titration it was possible to determine approximately the amount of NO_3 in the solution to be examined. This should not contain more than 4 or 5 milligrams in 25 cubic centims. The first test was soon found to be unnecessary, however, as a few titrations showed about how much salt was to be expected for a certain number of hours. Then, by making two titrations, the first a near approximation, it was possible to obtain pretty accurate results. Enough solution was always taken to require from 30 to 50 cubic centims. indigo solution. Observing these precautions, the following experiments were made:—

Diffusion of $\frac{n}{4}$ $BaN_2 O_6$.

Time.	Amount diff.
8–12	·0068 gram.
12–6	·0110 „
6–12 A.M.	·0315 „
12–6	·0107 „
6–12 A.M.	·0330 „
52 hours.	·0930

Diffusion of $\frac{n}{4}$ $SrN_2 O_6$.

Time.	Amount diff.
6–10 A.M.	·0204 gram.
10–6	·0090 „
6–12 A.M.	·0211 „
12–6	·0076 „
6–12 A.M.	·0229 „
66 hours.	·0810 „

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* Fresenius' *Zeitschrift*, vii. p. 412.

These values correspond to $\left(\frac{n}{4}\text{ solutions}\right)$

0·0429 gram. BaN_2O_6 in 24 hours,

and

0·0294 gram SrN_2O_6 in 24 hours.

Diffusion of *n* KNO_3.

Time.	Amount diff.
9–12	·0077 gram.
12–3	·0071 "
3–8 A.M.	·0450 "
8–4	·0199 "
4–10 A.M.	·0494 "
10–3	·0124 "
54 hours.	·1415

Diffusion of *n* NH_4NO_3.

Time.	Amount diff.
4–8 A.M.	·0360 gram.
8–2	·0144 "
2–5	·0065 "
5–9 A.M.	·0358 "
9–4	·0161 "
48 hours.	·1088 "

The first corresponds to ·0026 gram per hour; another series of experiments gave ·0025 gram per hour. The mean of these two corresponds to

0·0614 gram KNO_3 in 24 hours.

The second corresponds to ·00227 gram NH_4NO_3 per hour, or to

0·0544 gram NH_3NO_4 in 24 hours.

Normal solutions of $NaNO_3$ and $LiNO_3$ gave quite constant results at the rate of

0·0446 gram $NaNO_3$ in 24 hours,

and

0·0353 gram $LiNO_3$ in 24 hours.

This $LiNO_3$ was prepared from Mg-free Li_2CO_3. This was digested a number of times with hot water, and after filtering and drying was found to contain but the merest traces of Na_2CO_3, as shown by the spectroscope. The Li_2CO_3 was then converted into nitrate.

Diffusion of Sulphates.

Six different salts of this class, each in $\frac{n}{2}$ solution, were investigated as to their rates of diffusion. An accurate titration of the diffused salt was, of course, impossible in this case; and weight analyses were resorted to.

At first the liquid obtained in three or four hours from the tubes was evaporated in glass vessels to a small bulk and then transferred to a platinum crucible and evaporated to dryness. But this method was soon found to be inconvenient and very inaccurate, as the weight of substance dissolved from the glass vessels during the evaporation was about equal to that of the diffused salt. For a similar reason, it was found unsafe to evaporate in glass and precipitate with $BaCl_2$. These sources of error were finally avoided by evaporating the whole of the liquid obtained in 24 hours in platinum dishes, then precipitating with $BaCl_2$, and weighing as $BaSO_4$. The results of the experiments are as follows, the values being reduced for one cell:—

Diffusion of $\frac{n}{2}(NH_4)_2SO_4$.

1st	day diffused	0·0491 gr.
2nd	,, ,,	0·0492 ,,
3rd	,, ,,	0·0463 ,,

Mean 0·0482 gr. $(NH_4)_2SO_4$ in 24 hours.

Diffusion of $\frac{n}{2}Na_2SO_4$.

1st	day diffused	0·0502 gr.
2nd	,, ,,	0·0481 ,,
3rd	,, ,,	0·0452 ,,

Mean 0·0478 gr. Na_2SO_4 in 24 hours.

Diffusion of $\frac{n}{2}ZnSO_4$.

1st	day diffused	0·0298 gr.
2nd	,, ,,	0·0266 ,,
3rd	,, ,,	0·0243 ,,

Mean 0·0269 gr. $ZnSO_4$ in 24 hours.

Diffusion of $\frac{n}{2}CuSO_4$.

1st	day diffused	0·0271 gr.
2nd	,, ,,	0·0234 ,,
3rd	,, ,,	0·0250 ,,

Mean 0·0252 gr. $CuSO_4$ in 24 hours.

Diffusion of $\frac{n}{2}MgSO_4$.

1st	day diffused	0·0243 gr.
2nd	,, ,,	0·0209 ,,
3rd	,, ,,	0·0184 ,,
4th	,, ,,	0·0183 ,,

Mean 0·0209 gr. $MgSO_4$ in 24 hours.

Diffusion of $\frac{n}{2}MnSO_4$.

1st	day diffused	0·0230 gr.
2nd	,, ,,	0·0231 ,,
3rd	,, ,,	0·0213 ,,
4th	,, ,,	0·0231 ,,

Mean 0·0226 gr. $MnSO_4$ in 24 hours.

What are here spoken of as the 1st, 2nd, and 3rd days are in reality the 3rd, 4th, and 5th days of the experiment, two days being allowed to pass before the determinations were commenced.

Considering all the experiments given above in detail, it will be seen that, as a rule, the progress of the diffusion from day to day was pretty regular, the fluctuations not being greater than one might expect. The sulphates show the greatest irregularities; and it would seem from the results found that the two days allowed, in which the diffusion should reach a condition of constancy, was not a period long enough for these salts. It is also possible that this is the normal action of these substances.

But before speaking any further of the probable cause of the above discrepancies, it would be well to compare my results with those of previous experimenters. Of all the results obtained by others only those of Graham can be readily compared. Beilstein neglected to give the dimensions of his diffusion-cell with sufficient accuracy to allow a fair comparison to be made; and Marignac's experiments were conducted in such a way that a direct comparison is impossible. His results can be considered only with reference to each other; and, besides, he has shown that the relative rates of two salts, when simultaneously diffused, are often quite different from their relative rates when diffused alone.

But, fortunately, Graham's work was so performed and his results given in such a form that their reduction and comparison with my own can be more readily made. Graham's diffusion-cells, mentioned at the beginning of this paper, consisted of small phials, 9·64 centims. high, holding 134·8 cubic centims. to the base of the neck. The neck itself was 12·7 millims. high and slightly conical, being on the upper surface 31·49 millims., and on the lower 30·48 millims. in diameter. These phials were filled to the base of the neck with the solution to be examined; and the neck was filled with pure water in such a manner as not to mix the two liquids. The phials were placed in large vessels containing 567 cubic centims. water, and here allowed to remain a number of days. At the end of this time the water was drawn off and the amount of diffused substance analytically determined. The results of these determinations are given in tables, of which the following may serve as a sample. It is from Graham's second paper on the subject*:—

Diffusion of Chloride of Barium in 8·57 days at 63°: two cells.

	grs.	Ratio.
From 1-per-cent. solution . .	6·32	1·047
„ 2 „ „ . . .	12·07	2·000
„ 4 „ „ . . .	23·96	3·970
„ 8 „ „ . . .	45·92	7·608

The temperature is given in degrees Fahr. "The quantities 1, 2, 4, and 8 per cent. indicate the parts of salt in a con-

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* Phil. Trans. 1850, p. 817.

stant volume of liquid—10, 20, 40, and 80 grains of salt in 1000 watergrain measures of the solution." In Graham's first paper*, the percentages referred to parts by weight of the solution. These values I have reduced to grams per litre, and the amounts of diffused salt likewise to grams. Then constructing a curve from these data, I have taken out the value corresponding to the amount of salt used per litre in my experiments. For instance, I used an *n* solution of $BaCl_2$—that is, a solution of 104 grams per litre, or, corresponding to the above, a solution of 10·4 per cent. On the curve I have found the amount which should be diffused from a solution of this concentration in the Graham method and compared this with the value found by my own. These amounts depend on the dimensions of the diffusion-vessels; that is, in the general formula established by Fick,

$$\frac{q}{l} : \frac{q'}{l'} :: S : S'.$$

But l may be taken as equal to l', as the distances between the levels of concentration-zero and those of the strong solutions are about the same in both cases. Then

$$q : q' :: S : S',$$

or

$$S' = \frac{Sq'}{q},$$

in which q is the section of the neck of Graham's cells, q' a section equal to those of the cells used by me, S the amount of salt diffused in a given time in a Graham cell, and S′ finally the amount which should be diffused in a Graham cell of the same dimensions as those of the cells used by me. This latter is the value sought. S, q, and q' are known; and if we consider the lower section of the neck of Graham's cells as determining the amounts diffused, we may write

$$S' = \frac{\pi 7{\cdot}25^2}{\pi 15{\cdot}24^2} S = 0{\cdot}226\, S.$$

If S represents the amount of salt diffused in one day taken as the unit of time, no further reduction is necessary; if not, it simply remains to divide by the number of days of the experiment. This gives, of course, only the mean value for the rate of diffusion during the whole time; but that is the best which can be done under the circumstances.

In this way I have reduced Graham's results; and the values will be found below side by side with my own. No reduction

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* Phil. Trans. 1850, p. 1.

has been made for temperature, as the exact coefficients for this factor are not yet known. Besides, the temperature was nearly the same in all cases of both series of experiments, being with Graham about 60° to 65° F., and with me 14° to 16° C.

Salt diffused from *n* solutions in 24 hours.

Formula.	From my tubes.	From Graham's.	Formula.	From my tubes.	From Graham's.
	gr.	gr.		gr.	gr.
KCl	0·0599	0·0638	NaI	0·1008	0·0959
$NaCl$	·0351	·0373	KCy	·0499	
NH_4Cl	·0369	·0378	KNO_3	·0614	·0715
$LiCl$	·0233		NH_4NO_3	·0544	·0596
$CaCl_2$	·0476	·0547	$NaNO_3$	·0446	·0531
$SrCl_2$	·0686	·0766	$LiNO_3$	·0353	
$BaCl_2$	·0936	·0974	$BaN_2O_6 \ldots \frac{n}{4}$	·0429	·0296
$MgCl_2$	·0373	·0444	$SrN_2O_6 \ldots \frac{n}{4}$	·0294	·0241
$CoCl_2$	·0397		$CuSO_4 \ldots \frac{n}{2}$	·0252	·0235
$NiCl_2$	·0394		$ZnSO_4 \ldots \frac{n}{2}$	·0269	·0194
KBr	·0965	·1037	$MnSO_4 \ldots \frac{n}{2}$	·0226	
$NaBr$	·0525	·0645	$MgSO_4 \ldots \frac{n}{2}$	·0209	·0150
NH_4Br	·0617		$Na_2SO_4 \ldots \frac{n}{2}$	·0478	·0300
KI	·1364	·1416	$(NH)_2SO_4 \frac{n}{2}$	·0482	·0369

It will be seen from the above that the correspondence in the results for the chlorides, bromides, iodides, and some of the nitrates is as close as could reasonably be expected from methods so different as were the two here employed. But the differences in the cases of BaN_2O_6, SrN_2O_6, and the five sulphates are far greater than could be allowed for errors of observation or accidental disturbances. Most of Graham's values represent the mean rates of diffusion for periods varying from 5 to 8 days. The values for BaN_2O_6 and SrN_2O_6, however, are the mean rates for 11·43 days; and those given for $MgSO_4$ and $ZnSO_4$ are the mean rates for the period of 16·16 days. The other sulphates were diffused for 8 days. The discrepancies might be accounted for, in some cases, by assuming that the periods chosen by Graham were too long to admit of a fair comparison. Indeed it would seem from my results obtained with the sulphates, that had they been diffused longer and the mean values taken, they would have been equally as small as those found by Graham. It will be of course recognized that a chief advantage in the method employed by me rests in the fact that it allows any variation in the rate of diffusion, as in the above case, to be followed from day to day. For the present I must leave the cause of these variations unexplained.

The question now presents itself, Do the phenomena of diffusion bear any relation to other known physical phenomena? or, in other words, are the relations which molecules of different substances bear to each other as regards diffusion analogous to those depending on other molecular actions? From the Tables given above no simple relation is recognizable; but if instead of giving the results in grams of substance diffused, they be given in number of molecules diffused, several very interesting relations will appear. Of course we have no means of knowing the absolute number of molecules which undergo a change of place; but it is quite easy to express the relative numbers diffused in any given length of time.

In the Tables above the salt diffused is expressed in grams per 24 hours; now if each one of these values be divided by the molecular weight of the salt, the quotients thus obtained will be proportional to the numbers of molecules diffused from the solutions in question in equal times.

It will be remembered that the solutions submitted to diffusion were normal, half-normal, and quarter-normal—that is, were made to contain a definite number of molecules per constant volume; and it will be interesting to see how many molecules diffused in equal times from solutions which contained originally the same number. This is presented in the following Table. In order to have these values in the form of whole numbers, the quotients as obtained above have been simply multiplied by 1,000,000, which of course does not alter their relative values. The results obtained from salts diffused in half- or quarter-normal solution have been multiplied by two or four to make the comparison easier. This is not absolutely accurate, as the amounts diffused are not quite proportional to the concentrations; but the error will not be large.

KCl ...	803	$CaCl_2$ ...	429	KI ...	823	SrN_2O_6 ...	552
NH_4Cl ...	689	$MgCl_2$ ...	392	NaI ...	672	$(NH_4)_2SO_4$	724
NaCl ...	600	$CoCl_2$ ...	306	NH_4NO_3	680	Na_2SO_4 ...	678
LiCl ...	541	$NiCl_2$ ...	304	KNO_3 ...	607	$MgSO_4$ ...	348
KCy ...	767	KBr ...	811	$NaNO_3$ ...	524	$ZnSO_4$ ...	332
$SrCl_2$ ...	432	NH_4Br ...	629	$LiNO_3$ ...	512	$CuSO_4$ ...	316
$BaCl_2$ ...	450	NaBr ...	509	BaN_2O_6 ..	656	$MnSO_4$ ...	298

This arrangement of the results shows several very interesting peculiarities. For instance, the chlorides, bromides, and iodides of the alkali metals form a series in which NH_4 stands between K and Na. KCl, KBr, KI, and KCy have nearly the same rate. The same can be said of the corresponding NH_4 and Na salts as far as investigated.

The chlorides of the dyad metals Ba, Sr, Ca, and Mg are also seen to form a series as to their rates of diffusion. In the case of the alkaline nitrates something similar is seen to exist; but these and other analogies will be better understood when presented in tabular form below (p. 426).

If we compare the results in the above Table with those obtained by Kohlrausch[*], on the electrical conducting-power of liquids, several other interesting relations will be observed. It seems to be true in almost every case that those salts which offer the least resistance to the passage of the galvanic current when in solution are the ones which have the fastest rate of diffusion. These analogies will also be found below.

Another relation will be found when we compare the rates of diffusion with the molecular weights and the molecular volumes of the salts dissolved. In most cases it will be seen that those salts having the greatest molecular volumes diffuse the best. Another very interesting relation will be found when the rates of diffusion of the anhydrous salts are compared with their thermal action on being dissolved. Here it appears that those salts which absorb the greatest amount of heat on going into solution—that is, those upon which the greatest amount of work has been expended and which are, as a conquence, in the finest state of division—are the ones which diffuse the most rapidly.

As these several phenomena have in all probability the same or similar causes, it will be well to present the several series of constants, molecular volume, electrical conducting-power, heat-absorbing power, and rate of diffusion of the various substances in a single Table. The specific gravities of the salts from which the molecular volumes are computed were taken from F. W. Clarke's collection of Tables[†], and are the mean values of those there given for the substances in question.

The values for the electrical conductivity are from Kohlrausch's papers[‡] cited above. They are referred to that of Hg as unit, and are multiplied by 10^7. The solutions were also normal—that is, similar to those used by me.

The calorimetric values are those of Thomsen, Berthelot and

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[*] Wied. *Ann.* 1879, Nos. 1 & 2, pp. 1 & 145.

[†] "Constants of Nature," 'Smithsonian Contributions to Knowledge.' Washington, 1873.

[‡] Wied. *Ann.* Band vi. pp. 1 & 145.

Favre and Valson as in Gmelin-Kraut*, and represent the heat absorbed, expressed in the customary units, when the molecular weight in grams of the salt in question is dissolved in excess of water.

Formula of salt.	Molecular weight.	Molecular volume.	Molecules diffused.	Molecular conductivity.	Heat of solution.
KCl	74·6	38·4	803	97	− 4440
NH_4Cl	53·5	35·2	689	95	− 3880
$NaCl$	58·5	26·2	600	81	− 1180
$LiCl$	42·5	20·9	541	70	
KBr	119·1	45·9	811	104	− 5080
NH_4Br	98·0	42·4	629	103	− 4380
$NaBr$	103·0	34·2	509	81	− 150
KI	166·0	55·5	823	103	− 5110
NaI	150·0	43·4	672	84	+ 1220
KCy	65·1	42·1	767	101	− 2860
NH_4NO_3	80·0	47·2	680	93	− 6320
KNO_3	101·1	48·6	607	92	− 8520
$NaNO_3$	85·0	38·4	524	76	− 5060
$LiNO_3$	69·0	28·9	512	...	
				For ½ molec.	
BaN_2O_6	261·0	81·6	656	69	− 4640
SrN_2O_6	211·5	76·1	552	...	− 2540
$BaCl_2$	208·1	54·1	450	79	+ 2344 ($BaCl_2.2H_2O$) − 4836
$SrCl_2$	158·5	55·0	432	77	+10966 ($SrCl_2.6H_2O$) − 7172
$CaCl_2$	111·0	49·7	429	75	+18106 ($CaCl_2.6H_2O$) − 3258
$MgCl_2$	95·0	43·3	392	72	
$CoCl_2$	129·8	44·3	306		
$NiCl_2$	129·8	50·7	304		
$N_2H_8SO_4$	132·0	75·8	724	76	− 1950
Na_2SO_4	142·0	53·7	678	63	+ 708
$MgSO_4$	120·0	45·4	348	37	+20304 ($MgSO_4.7H_2O$) − 3720
$ZnSO_4$	161·2	46·2	332	34	+18548 ($ZnSO_4.7H_2O$) − 4148
$CuSO_4$	159·5	44·5	316	33	+16298 ($CuSO_4.5H_2O$) − 2432
$MnSO_4$	151·1	48·8	298	...	+14170 ($MnSO_4.5H_2O$) + 460

A consideration of the Table shows the completeness of the analogies referred to above. Taking the alkaline chlorides first, it will be seen that they stand in the same order as regards molecular volume, rate of diffusion, conducting-power, and latent heat of solution. Exactly the same is true of the corresponding bromides and iodides; and the cyanide of potassium has very nearly the same rate of diffusion and electrical conducting-power as the chloride, bromide, and

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* Gmelin-Kraut, erster Band, pp. 512–544.

iodide of the same metal. It is probable that the rate I have found for the bromide of sodium is somewhat too small, being considerably smaller than that found by Graham for the same.

In the next series it is seen that the order of the salts is the same as regards rate of diffusion and electrical conductivity, and the same in respect to molecular volume and latent heat of solution. Ammonium nitrate precedes potassium nitrate in regard to rate of diffusion and conducting-power, and follows it in regard to molecular volume and heat-absorption.

Barium nitrate precedes strontium nitrate in every known respect; to my knowledge, its conducting-power has not yet been determined.

In the series of the dyad chlorides the order is the same in respect to rate of diffusion, conductivity, and latent heat of solution as far as is known. The molecular volume of strontium chloride seems to be slightly greater than that of barium chloride; but as the values of the specific gravities from which the volumes were calculated differed considerably among themselves, it is possible that the mean taken does not represent the true value.

In the case of the sulphates the order is the same respecting rate of diffusion and electrical conductivity. The sulphates of magnesium, copper, and zinc agree with each other very well in both respects; and it is interesting to observe that the rates of ammonium and sodium sulphates, while also agreeing pretty well in both respects, are much larger than those of the dyad sulphates. The molecular volumes and heats of solution are here quite irregular; indeed the anhydrous sulphates of magnesium, zinc, copper, and manganese stand in exactly reversed orders in respect to their rates of diffusion and latent heats of solution. But a correspondence in this case could hardly be expected, as these substances, on going into solution, combine with large quantities of water and in variable proportions.

It will be seen also from the Table that Graham was scarcely correct in stating that no relation exists between molecular weight and rate of diffusion.

Before seeking for an explanation of these several analogies, it might be well to cite a few more data bearing on the subject. Kohlrausch has shown, in his paper referred to, that the conducting-powers of the acids HCl, HBr, HI, and HNO_3 are nearly the same. It would be interesting to know how these substances act as regards diffusion. I have not attempted an investigation of them; but, fortunately, Graham has experimented upon all four; and his results, reduced to the same standard as above (that is, relative number of molecules diffusod from *n* solutions in 24 hours), will be found below. The heats of solution for one equivalent of each acid are also given.

Formula.	Molecular weight.	Molecules diffused.	Molecular conductivity.	Latent heat of solution.
HCl . .	36·5	989	323	17500
HBr . .	81·0	965	311	20000
HI . .	128·0	994	328	19500
HNO_3 .	63·0	977	334	7500

It is seen that the analogy is still preserved; the four acids are as closely related to each other in respect to rate of diffusion as to electrical conductivity. The heats of solution of the three similar acids are also nearly the same; that of HNO_3, however, is less.

Of all the above relations, that which seems the most natural and most readily explained is the analogy between rates of diffusion of similarly constituted salts and the amounts of heat they absorb on going into solution. The rendering latent of a large amount of heat indicates the performance of a correspondingly large amount of work. The work in the present case is probably the division of the molecules into smaller aggregates; and these, as is quite plain, are able to move through the solution with greater facility than those which are not so finely divided. Opposed to this, however, we should not expect from a salt having a large molecular volume a rapid rate of diffusion. Now it is possible that the large latent heat corresponds to, and compensates for, the large volume, and the rate of diffusion may not depend upon either. What the true explanation of these facts is I cannot at present say, but must content myself with the mere indication of the several data and their analogies.

On an entirely different footing stand the analogies between rate of diffusion and electrical conducting-power. In both cases a molecular motion is concerned; and although at first sight an explanation may seem as difficult as before, we have a means of bridging the difficulty. In the first place, it will be well to call to mind the theory of the constitution of solutions as advanced by Clausius *, Williamson †, and others. Clausius believes that in a solution of a salt, as NaCl for instance, the two atoms cannot be assumed as remaining attached closely together, or even as vibrating within limits near each other; but, on the contrary, they fly about in all directions, forming or dissolving connexion with other atoms just according to the

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* Pogg. *Ann.* ci. p. 338.

† See his "Theory of Etherifications," Chem. Soc. Quart. Journ. iv. p. 229; also *Ann. Chem. Pharm.* lxxvii. p. 37.

intensity of the attractions acting upon them at any particular instant of their course. Moreover the velocities with which these two atoms move may be quite different, and what may be spoken of as the velocity of the molecule NaCl in solution is compounded of the velocities of its two atoms Na and Cl.

The action of the galvanic current on such a system would then be as follows:—The atoms would still continue to fly about, but in directions more nearly parallel to that of a line joining the two electrodes; and the tendency of these latter to draw to the one side the negative atoms, and to the other the positive, would be facilitated by their continually recurring states of liberty. If a partition be imagined in the vessel in which electrolysis is taking place, say halfway between the two electrodes and perpendicular to the line joining them, it is clear that more positive than negative atoms pass this partition in a positive direction, and more negative than positive atoms pass it in a negative direction in a given time. But positive atoms moving in a negative direction, and negative atoms moving in a positive direction, compensate for each other, and there remains only the excess of motion in each direction; which amounts to saying that a certain number of positive atoms cross the partition in a positive direction, and a certain number of negative atoms cross it in a negative direction, in a given time. "These two numbers need not be the same, as they depend not only upon the impulsive force which for both is the same, but also on the degree of mobility, which for several reasons can be different for different substances"[*].

Now the ratio of the number of negative atoms which cross the partition to the number of molecules which may be considered as decomposed in a given time is designated by Hittorf, in his well-known papers on the "Wanderung der Ionen"[†], with the letter n, and called by him the "*Ueberführungszahl.*" He shows further that this n is equal to the ratio of the velocity of the negative atoms to the sum of the velocities of both negative and positive atoms[‡]. Kohlrausch, in his papers referred to above, expresses this relation thus,

$$n = \frac{v}{u+v},$$

where v is the velocity of the negative and u that of the positive atoms; and from the numerous values of n given by Hittorf for each substance he has deduced those which correspond

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[*] Clausius, *loc. cit.*

[†] Pogg. *Ann.* lxxxix. p. 176; xcvii. p. 1; ciii. p. 1; cvi. p. 337.

[‡] See Pogg. *Ann.* ciii. p. 20, and cvi. p. 353

to dilute solutions. He has also shown that v, the velocity (*Beweglichkeit*, mobility) of a negative atom, as Cl, is the same in all its compounds from which it may be electrolyzed, in HCl, KCl, $BaCl_2$, and so on. The same is also true for any positive atom, as K in KCl, KOH, KNO_3, &c.

If the values of n for any series of salts as found by Hittorf and reduced by Kohlrausch be compared with the rates of diffusion of the same, it will be found that in the cases where n is large the rate of diffusion is small; and, indeed, in each series the product of n and the latter, which I shall call d, is nearly a constant quantity. Considering, for instance, the dyad chlorides, we have

	$\frac{n}{2}$.	d.	$\frac{n}{2}d$.
$BaCl_2$	·618	450	277
$SrCl_2$	·655	432	283
$CaCl_2$	·673	429	288
$MgCl_2$	·682	392	267

In the case of the salts KCl, KBr, and KI we have

	n.	d.	nd.
KCl	·515	803	413
KBr	·514	811	416
KI	·505	823	415

and in the case of the sulphates,

	$\frac{n}{2}$.	d.	$\frac{n}{2}d$.
$MgSO_4$	·630	348	219
$ZnSO_4$	·640	332	212
$CuSO_4$	·645	316	204

From these tables it appears, comparing any two substances in each series, that

$$n : n' : : d' : d;$$

that is,

$$\frac{v}{v+u} : \frac{v'}{v'+u'} : : d' : d.$$

But, as remarked above, v has been shown to be equal to v'; hence

$$d : d' : : v+u : v+u';$$

that is, the rate of diffusion of a salt is proportional to the sum of the velocities with which its component atoms move during electrolysis.

We shall now be able to understand the analogy between my results and those of Kohlrausch; for the latter has shown that the molecular conductivity of a salt in solution depends also on the velocities of its component parts. From this standpoint he has calculated the conducting-powers of the substances for which *n* is known; and the results thus obtained agree very well with those obtained by experiment.

It appears, then, that the law of the independent migrations of the ions is as applicable before as during the electrolysis of salt in solution; otherwise what is the meaning of the above analogies?

There are also several other phenomena showing a close relation between diffusion and conductivity. Both increase with the temperature, as one might expect; and from Graham's experiments it appears that the increase in the case of diffusion is about the same as Kohlrausch found in the case of conductivity. The influence of temperature on diffusion has been as yet, however, determined but for few substances; and there is of course more or less uncertainty in these determinations.

Another point of analogy is this. Graham has shown that, in the case of nearly every substance investigated by him, the increase in the rate of diffusion is less as the concentration increases. This is also true of the conductivity of the same solutions; and the explanation is the same in both cases. As remarked by Kohlrausch, in concentrated solutions the molecules interfere with each other's motions; and this interference is manifested in the one case by a decrease in the rate of diffusion, and in the other by a decrease in the conducting-power, both relative of course.

Indeed, considering all the above data, it will be seen that the most perfect analogy exists. In the one case the number of particles, in the other the force transmitted by the same is the element observed; and that a correlation should be found is not at all singular.

Whether the velocity of the salt-molecules is the same during diffusion as during electrolysis I cannot say; but it seems to be established by the above that, if any increase take place in the latter case, the fraction representing this increase must be the same for all substances of each series.

But before proceeding any further with this subject, there are several other points which must be investigated. For instance, the rates of diffusion of chlorine and bromine in water must be more exactly determined; and, if possible, more correct determinations of the influence of temperature should be made.

The alkaline hydrates present also a very interesting field of investigation; and if the above analogies be general, these bodies must be characterized by a rapid rate of diffusion as well as by good conductivity. There are several phosphates and carbonates which likewise deserve investigation; and these points I expect to make the subject of future examination.

Tübingen, 1879.

LV. *Preliminary Notes on* Mr. Hall's *Recent Discovery.* *By* H. A. ROWLAND*.

THE recent discovery by Mr. Hall† of a new action of magnetism on electric currents opens a wide field for the mathematician, seeing that we must now regard most of the equations which we have hitherto used in electromagnetism as only approximate, and as applying only to some ideal substance which may or may not exist in nature, but which certainly does not include the ordinary metals. But as the effect is very small, probably it will always be treated as a correction to the ordinary equations.

The facts of the case seem to be as follows, as nearly as they have yet been determined:—Whenever a substance transmitting an electric current is placed in a magnetic field, besides the ordinary electromotive force in the medium, we now have another acting at right angles to the current and to the magnetic lines of force. Whether there may not be also an electromotive force in the direction of the current has not yet been determined with accuracy; but it has been proved, within the limits of accuracy of the experiment, that no electromotive force exists in the direction of the lines of magnetic force. This electromotive force in a given medium is proportional to the strength of the current and to the magnetic intensity, and is reversed when either the primary current or the magnetism is reversed. It has also been lately found that the direction is different in iron from what it is in gold or silver.

To analyze the phenomenon in gold, let us suppose that the line A B represents the original current at the point A, and that B C is the new effect. The magnetic pole is supposed to be either above or below the paper, as the case may be. The line A C will represent the final resultant electromotive force at the point A. The circle with arrow represents the direction in which the current is rotated by the magnetism.

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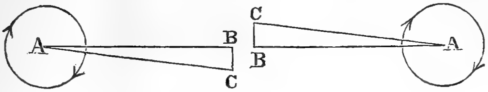
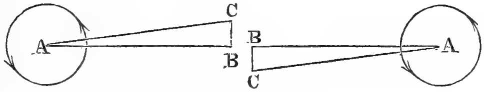
* From the American Journal of Mathematics. Communicated by the Physical Society.

† Phil. Mag. [5] vol. ix. p. 225.

North Pole above.



North Pole below.



It is seen that all these effects are such as would happen were the electric current to be rotated in a fixed direction with respect to the lines of magnetic force, and to an amount depending only on the magnetic force and not on the current. This fact seems to point immediately to that other very important case of rotation, namely the rotation of the plane of polarization of light. For, by Maxwell's theory, light is an electrical phenomenon, and consists of waves of electrical displacement, the currents of displacement being at right angles to the direction of propagation of the light. If the action we are now considering takes place in dielectrics, which point Mr. Hall is now investigating, the rotation of the plane of polarization of light is explained.

I give the following very imperfect theory at this stage of the paper, hoping to finally give a more perfect one either in this paper or a later one.

Let $\mathfrak{H}$ be the intensity of the magnetic field, and let E be the original electromotive force at any point, and let $\mathfrak{c}$ be a constant for the given medium. Then the new electromotive force E' will be

$$E' = \mathfrak{c}\mathfrak{H}E,$$

and the final electromotive force will be rotated through an angle which will be very nearly equal to $\mathfrak{c}\mathfrak{H}$. As the wave progresses through the medium, each time it (the electromotive force) is reversed it will be rotated through this angle; so that the total rotation will be this quantity multiplied by the number of waves. If λ is the wave-length in air, and i is the index of refraction, and c is the length of medium, then the number of waves will be $\frac{ci}{\lambda}$, and the total rotation

$$\theta = c\mathfrak{c}\mathfrak{H}\frac{i}{\lambda}.$$

The direction of rotation is the same in diamagnetic and ferromagnetic bodies as we find by experiment, being different in the two; for it is well known that the rotation of the plane of polarization is opposite in the two media, and Mr. Hall now finds *his* effect to be opposite in the two media. This result I anticipated from this theory of the magnetic rotation of light.

But the formula makes the rotation inversely proportional to the wave-length, whereas we find it more nearly as the square or cube. This I consider to be a defect due to the imperfect theory; and it would possibly disappear from the complete dynamical theory. But the formula at least makes the rotation increase as the wave-length decreases, which is according to experiment. Should an exact formula be finally obtained, it seems to me that it would constitute a very important link in the proof of Maxwell's theory of light, and, together with a very exact measure of the ratio of the electromagnetic to the electrostatic units of electricity which we made here last year, will raise the theory almost to a demonstrated fact. The determination of the ratio will be published shortly; but I may say here that the final result will not vary much, when all the corrections have been applied, from 299,700,000 metres per second; and this is almost exactly the velocity of light. We cannot but lament that the great author of this modern theory of light is not now here to work up this new confirmation of his theory, and that it is left for so much weaker hands.

But before we can say definitely that this action explains the rotation of the plane of polarization of light, the action must be extended to dielectrics, and it must be proved that the lines of electrostatic action are rotated around the lines of force as well as the electric currents. Mr. Hall is about to try an experiment of this nature.

I am now writing the full mathematical theory of the new action, and hope to there consider the full consequences of the new discovery.

Addition.—I have now worked out the complete theory of the rotation of the plane of polarization of light, on the assumption that the displacement currents are rotated as well as the conducted currents. The result is very satisfactory, and makes the rotation proportional to $\frac{i^5}{\lambda^2}$, which agrees very perfectly with observation. The amount of rotation calculated for gold is also very nearly what is found in some of the substances which rotate the light the least. Hence it seems to me that we have very strong ground for supposing the two phenomena to be the same.

LVI. *On some Effects of Vibratory Motion in Fluids; on the Attraction due to the Flow of Liquids from an Expanded Orifice; and Laboratory Notes.* *By* R. H. RIDOUT*.

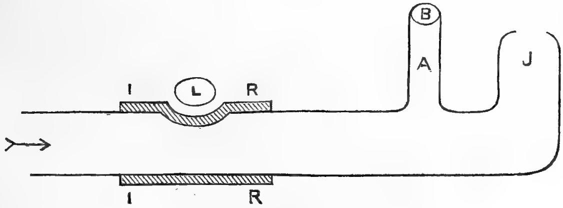
On some Effects of Vibratory Motion in Fluids.

ON causing a stream of coal-gas to bubble through a liquid contained in a flask, it may by careful adjustment be made to issue from a suitable jet in an oscillatory manner, in the plane which would contain a sensitive flame issuing from the same jet. Thus a flame may be sensitive when the disturbance is imparted from within.

An analogous experiment with water was attempted as follows:—An electromagnetic engine was made to oscillate a lever, pressing against an india-rubber tube, conveying water from a height of 14 feet to a partially closed tube, such as would emit a sensitive flame under a moderate pressure. The water issued in a pulsatory manner, but showed no new form.

On comparing the two experiments, it will be seen that the bubble of gas, in passing through the liquid, is gradually increasing in volume, and when it reaches the surface, probably expands to a size it could not permanently retain. Such is not possible with water; for though its elasticity is greater than that of air, the range through which it acts (which may be called its amplitution) is much less. To provide for this amplitution, an air-chamber was introduced between the lever and the jet (fig. 1). The water then issued in exactly the same manner as an excited sensitive flame, breaking into two streams lying in *one* plane. The motion of the lever upon the flowing water produces in the latter a sinuous wave; and as the troughs and crests reach opposite sides of the jet, they

Fig. 1 (section).



L = Lever.
I R = India-rubber tube.
A = Air-chamber.
B = Air-bubble.
J = Jet.

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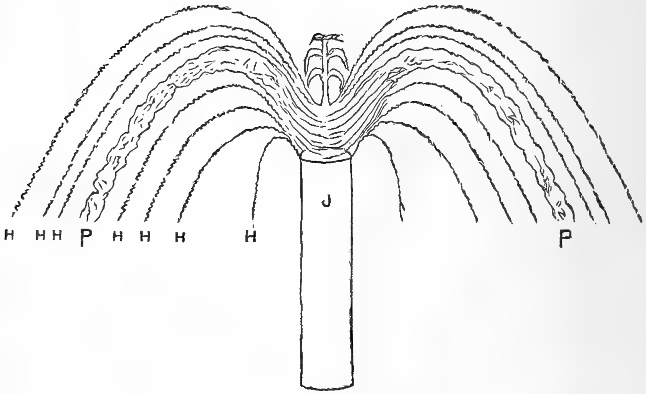
* Read before the Physical Society.

take alternatively opposite directions, forming two complete streams with about 400 vibrations per second. On substituting a straight tube for the jet, figures of great beauty were obtained.

Fig. 2 shows a simple wave free from harmonics. The phase of the wave relatively to the lever's motion is so strongly marked, that it is not destroyed by passing through tortuous tubes 2 feet long. When the lever presses the tube at any but a right angle, the figures are more complex and show a tendency to rotation.

A wave with harmonics produces minor streams (fig. 3) in addition to the primary one. I have shown in 'Nature' (Oct. 3, 1878) that a flame is capable of producing harmonics of its primary note; and this makes the analogy complete between gaseous and liquid columns, in their behaviour to sensible vibratory motion.

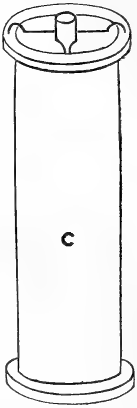
Figs. 2 and 3.



P P. Figure resulting from primary wave.
H H &c. Harmonics.
J. Straight tube.

The experiment analogous to the ordinary sensitive flame was attempted by immersing a sensitive jet in a vessel of water and passing a coloured liquid. The jet always "roared" when a higher pressure than $\frac{1}{2}$ in. water was used. Near the critical point I could not produce any marked effect by applying to the water a 4-inch tuning-fork. Possibly larger apparatus would give better results. While working with this, I was struck with the great depth to which a stream of the liquid will descend under a very small pressure from a non-sensitive jet, made by contracting a $\frac{5}{8}$-inch test-tube to about $\frac{1}{16}$ inch, and cutting off some distance below the contraction. When a liquid flows through this with a fall of $\frac{1}{4}$ inch into a tall cylinder of the same liquid, it continues in an unbroken line for 8 inches or more, having the diameter of the smallest part. Recalling a deduction of Froude's, that a liquid flowing through a tortuous tube without friction has no tendency to straighten it, I mounted the jet on pivots, and, by giving it an oscillatory motion, found that the stream travelled the liquid as a perfect wave, showing the deduction to be experimentally true. This apparatus (fig. 4), if mounted on the board carrying the vibrating lever and water-jet, gives a graphic representation of the wave which is producing the figures (figs. 2 and 3), and shows the presence or absence of harmonics.

Fig. 4.

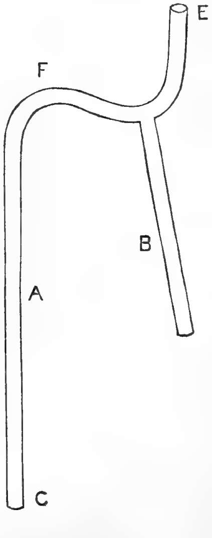


By using in the glass cylinder a weak solution of oxalic and sulphuric acids, and supplying the jets with a solution of sulphuric acid and permanganate of potash of exactly the same density, the waves are extinguished after a few seconds, while the liquid remains quite clear.

It will be seen that the streams (figs. 2 and 3) soon break into beads. On receiving these in water, they manifest the spheroidal state in a high degree.

When the bubble of air in the air-chamber (fig. 1) is above a certain size it splits into several lesser ones; and these on entering the current have a less velocity than the water towards the jet. Fig. 5 shows how this effect may be magnified. In a glass tube of $\frac{1}{4}$-inch bore, the limb A is about 1 foot long and closed at C. An india-rubber tube is attached at E, and bent back at a small angle in order to vibrate, as in the Bunsen pump. B is the waste-pipe. A bubble of air, the size of a rape-seed, is placed in the bend at F, A being full of water. When the current of water through E causes the whole to vibrate, the air-bubble detaches itself from the tube, takes the shape of a string of four or five beads, and sinks through the water to C, where it remains as long as the vibration continues. Attempts were made to substitute sphericles of glass, shellac, wax, &c., each made a little lighter than water, but without success. If the result were due to the

Fig. 5.



passage of vibration being more rapid in water than air, then globules of mercury, bromine, and bisulphide of carbon, all of which have a greater velocity for sound than water, should either float or show some tendency to do so. A great number of experiments, under varying conditions, failed to show that they were in any way affected.

As an air-bubble sinks as rapidly as a grain of sand would do, I tried hydrogen in place of air, and mercury in place of water in the tube A. When the hydrogen reaches the mercury it rests a little, and then suddenly sinks with the same apparent velocity as the air in water.

If in these experiments the bubble is large it does not descend; and this points to the close analogy with the experiments of Guthrie (Proc. Roy. Soc. 1870, pp. 35–41), who found that the vibration of a solid body caused the "approach" of a piece of cardboard from a considerable distance. We might say, conventionally, that the attraction was less between the air and the sounding body than the cardboard and the latter. Now in the tube A, C being a node, any particle near C is relatively at rest to one nearer E, and the attraction between them is greater than either for the bubble. Hence the particle at C tends to move towards E, or, what is the same thing, the bubble moves to C. When the bubble is large it is still repelled, but it leaves no way for the approach of the attracted water. A great number of small bubbles may be successively sunk till a large one results at C, showing that the conditions of the experiment only proscribe the motion of the large one. By inverting fig. 5 it may be used to produce the water-figures; but they can seldom be obtained free from harmonics by such means.

On the Attraction due to the Flow of Liquids from an Expanded Orifice.

M. Lacoutre has shown in *Les Mondes* for May 10, 1866, that when a liquid flows perpendicularly from a cylindrical tube upon a flat disk, its motion is arrested till the disk is brought to within half a diameter of the tube (but varying with pressure), when the outflow is greater than if the disk be entirely removed. I find that if the tube terminate in a similar disk, attraction ensues at the position of greatest outflow.

A glass filter funnel containing a light cone with a weight dependent from its apex, or a tube terminating in a hemispherical cup with a light ball, have more stability than the disks, and answer equally well. In all cases the apparatus may be immersed in liquid and the attraction continued. The explanation usually accepted of the corresponding pneumatic experiment of M. Clément Desormes, is based on the assumption that the velocity of the fluid gradually decreases as it approaches the edges of the disks. To determine how far this was generally true, I took an outer cone A of 4 inches diameter at base with angle of 60° at apex, and an inner cone B upon same base with angle of 120°. For the velocity of the liquid to diminish between the apex of B and base of A, the cones must be separated nearly $\frac{1}{4}$ inch. Using water at varying pressure, in no case did attraction take place at more than $\frac{1}{10}$ in., where the velocity must *increase* towards the outlet. I find also that the presence of the attracted cone, ball, &c. diminishes the pressure in the supply-pipe, as shown by a water-guage attached to the latter. Hence the presence of the cone &c. facilitates the efflux.

Laboratory Notes.

Apparatus for showing Electrolysis of Water.

A glass bolthead of 30 or 40 oz. capacity is stopped with an india-rubber cork carrying two glass tubes, which contain hermetically sealed platinum wires, projecting an inch at the inner end and terminating in binding-screws at the other. The vessel is filled one fifth full of acidulated water, boiled, and the stopper inserted to cause a vacuum when cold. On connecting with two "Grove" cells, the bubbles of gas so expand as to make the whole liquid appear to boil. With either a single "Grove," "Bunsen," "bichromate," or "Leclanché" cell continuous decomposition may be obtained. When sufficient gas has collected to impair the vacuum it may be restored by boiling.

Experiment showing Cohesion in Liquids.

A shallow tray 6″ × 2″, open at one end and lipped, is supported on three levelling-screws, the lipped end being *slightly* higher than the other. A quantity of mercury placed in the tray falls to the lower end; but if now a little more be added to make it flow over the lip, the cohesion is such as to enable the descending stream to drag the remainder up the inclined plane. Water gives similar results; but, from the difficulty of getting a surface which will long remain unwetted, the results are not so satisfactory.

Production of a Musical Note in a Continuous Tube.

In most wind instruments the sound results either from the movement of a solid body, or the air has the choice of two directions, which it alternately takes. I find, however, that it is possible to produce a good note from a tube $\frac{1}{4}$ to $\frac{5}{8}$ inch in diameter, and from 6 inches to a foot long, and having a part of it contracted smoothly and evenly to about a fourth of its diameter, by blowing through it. If the tube be bent upon itself at the point of contraction, the sounds are more readily obtained, though not of greater intensity.

Apparatus for showing Absorption of Heat on Liquefaction of Solids.

In a differential air-thermometer, the usual flasks are replaced by others which have had their bottoms softened, and then introverted to form a cup or basin. In this latter water is placed, and the solid then added. Any change in the liquid's temperature is at once communicated to the air-space round the cup.

Experiment showing the Expansion of Glass by Heat, and its low Conducting-power.

A glass tube of $\frac{1}{8}$-inch bore and 18 inches long is bent into the form of a violet-leaf—its free ends forming the apex, and leaving an interval in which a coin may be held by the elasticity of the glass. When heat is applied to the convex side of the middle bend, the limbs open and the coin falls.

LVII. *On a Simple Method of identifying a submerged Telegraph-Cable without cutting it. By* W. P. Johnston, *Officiating Electrician, Indian Government Telegraphs*[*].

CIRCUMSTANCES having arisen which rendered it desirable, and at times absolutely necessary, to be in possession of a method for identifying a submerged telegraph-cable by applying certain tests to the protecting guards, the following experiments were made for the purpose.

The particular case referred to me was this:—

"How with two cables (one good and one faulty) across a river, both cables having similar guards, can one be distinguished from the other without cutting, when either one of them has been raised from the bottom onto the repairing-boat? The boat being a small one, and the river running rapidly, it becomes difficult and dangerous to cut the cable in the first instance; and to make a joint simply for the purpose of identification is above all things to be avoided."

My first idea was to attach a very sensitive instrument to the guards where they were raised out of the water, to signal

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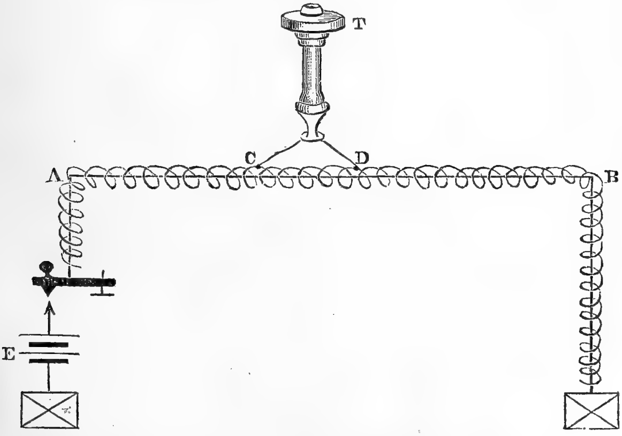
[*] Communicated by Mr. Louis Schwendler.

from the bank through the guards, and to observe the effects on this instrument, first, when the battery and instrument were joined to the guards of the same cable, and, secondly, when the battery was joined to the guards of one and the instrument to the guards of the other cable. This, however, did not answer.

My next idea was to join an instrument to the guards, and to observe whether it was sufficiently sensitive to show any sign of the current that would be induced in the guards of the cable when a strong current was sent through the conductor; and on mentioning this to Mr. J. J. Allen, Assistant Superintendent of Indian Government Telegraphs, he suggested that in this case a telephone attached to the guards might be influenced. The value of this suggestion was clear to me; for I have reason to know how exceedingly sensitive an instrument the telephone is, having about a year ago made for Mr. Schwendler the experiment to convey messages from one bank of a river to the other without any conductor between the two banks but the water, which experiment was for a certain distance quite successful.

I may now be allowed to detail the experiments which I made with the telephone.



A B is a submerged telegraph-cable about 7300 yards long, and having an absolute translation-resistance of 580 megohms;

———— is the conductor of copper of 23 ohms resistance;

/OOOO) the guards, 12 galvanized iron wires each weighing 900 lb. per mile;

C D the portion of the cable raised to the repairing-boat;

E the signalling battery, joined to the conductor;

T the telephone, of 2·5 ohms resistance.

The distance C D being 12 yards, every signal that was sent through the conductor was distinctly audible on the telephone (T); reducing the distance gradually, the signals were still readable so long as that distance was not less than 6 feet. At a less distance the signals became faint; and at 3 inches only an exceedingly feeble sound could just be heard, and not sufficient for reading.

On pressing the key a sound was heard from the telephone; and on releasing the key a sound was also heard.

The explanation, of course, is simple:—"For every current made in the conductor, a current is induced in the iron guards in the opposite direction to the primary current; and for every interruption made, a current is induced in the iron guards in the same direction as the primary current. The telephone therefore receives a succession of opposite currents; and being such an exceedingly sensitive instrument, notwithstanding the very minute fraction of these induced currents which passes through the shunt formed by the telephone, still it gives out audible sound."

As stated before, the guards were 12 galvanized iron wires each weighing 900 lb. per mile, and

the resistance per mile of one 900-lb. wire $= 7$ ohms,
the resistance per mile of twelve 900-lb. wires $= \frac{7}{12}$ ohm,
and the resistance of 6 feet of twelve 900-lb. wires $= 0{\cdot}66$ milliohm.

In this case the resistance of 0·66 milliohm offered by the 6 feet of the guards was shunted by a telephone offering with its connecting wires a resistance of 2·5 ohms. Therefore, of the currents induced in the guards by the primary current in the conductor, only $\frac{1}{3800}$ passed through the telephone.

The method which I suggest for the identification of a submerged telegraph-cable is therefore as follows:—Attach a telephone of low resistance to the guards of the raised portion of the cable (which portion may be either in or out of the water), as shown in the diagram, and send signals from the bank through the conductor of one of the cables; and then, if the telephone is connected to the guards of the cable through the conductor of which the signals are being sent, those signals will be audible in the telephone.

There are doubtless many other cases in which this method may be found of practical service.

The telephone in its short history resembles very much the development of a boy whose father had brought him up to be a priest, but who, contrary to all expectations, turned out to be a general; for the application in practical telegraphy for which the telephone was invented has not turned out to be so successful as the inventor, Prof. Bell, and others anticipated. But instead of this, the instrument will undoubtedly become of the greatest assistance to physical research. In addition to signalling across rivers already referred to, Mr. Schwendler had already used the telephone for indicating the speed of the induction cylinder of a dynamoelectric machine (see *précis* of his Electric Light Report); and Professor Hughes has used it with great success in his induction-balance. It seems, in fact, the telephone is too sensitive and too quick working for practical use.

Calcutta, April 27th, 1880.

LVIII. *Complete Theory of the Bifilar Magnetometer and new Methods for the Determination of the Absolute Horizontal Intensity of the Earth's Magnetism, as well as of the Temperature and Induction-coefficients of Magnets. By* H. WILD [*].

THE above is the title of a communication made to the Imperial Academy of Sciences of St. Petersburg on the 15th (27th) January, 1880 (*Bull. Acad. Impér. Sci. St. Pétersburg, Mélanges Phys. et Chim.* xi.), as an abstract of a more detailed paper to be shortly laid before the Academy.

The starting-point of the paper is the observation that the indications of the bifilar magnetometer, even when all causes of disturbance are excluded, differ from the results of direct measures of horizontal intensity to a far greater extent than can be accounted for by errors of observation. The author traces the cause of these discrepancies in the twisting of the suspending fibres, of which no account is taken in the ordinary theory of the bifilar magnetometer, according to which the moment of the directive couple of the suspension is ascribed simply to the weight of the magnet and its stirrup. The effect which this cause is capable of producing may either be calculated from the dimensions and coefficient of torsion-elasticity of the fibres, or it may be determined experimentally upon the bifilar instrument itself by observing the period of vibration before and after a known change has been made in the moment of inertia. Concordant determinations by both methods indicated that in the case of a bifilar suspended by brass wires 4 metres long, the couple due to torsion amounted to 1 per cent. of that due to the suspended weight. In the bifilar magnetometer of the St.-Petersburg magnetograph, which was suspended by steel wires only 0·3 metre long, the effect of torsion was as much as about 7 per cent. of the gra-

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[*] Communicated by the Physical Society.

vitation-couple. On the other hand, it amounted to only 0·3 per cent. in a bifilar of the Pawlowsk Observatory, this instrument having cocoon fibres 1·2 metre long. From the consideration of the conditions which determine the magnitude of the torsion and gravitation-couples respectively, Prof. Wild concludes that it is not probable that the proportion between them can be reduced much below that which exists in the last-mentioned instrument, namely 0·3 per cent., and hence that the correction for torsion cannot be of negligible amount in accurate observations.

When this correction is applied, as well as that for induction, the author finds that the differences between the results of calculation and experiment disappear, and that very nearly identical values of the characteristic magnitude of the bifilar magnetometer, namely the so-called "torsion-angle" z, are obtained by both methods. The reader may be reminded that this angle z is the angle which a vertical plane passing through the upper ends of the suspending fibres makes with a vertical plane through their lower ends when the magnet is perpendicular to the magnetic meridian. If D be the moment of the directive couple due to the suspension, H the earth's horizontal intensity, and M the moment of the magnet, the angle z is defined by the equation

$$\mathrm{D} \sin z = \mathrm{HM}.$$

Its magnitude may be found by direct experiment; or it may be calculated from the periods of vibration of the magnet observed when its position of equilibrium is in the magnetic meridian and its north-seeking pole is towards magnetic north and its south-seeking towards magnetic south respectively. If t_1 and t_2 are the periods in the two cases,

$$\sin z = \frac{t_2^2 - t_1^2}{t_2^2 + t_1^2}.$$

The author observes, in the next place, that the direct determination of z, together with the observation of the periods t_1, t_2, t_3 (the last being the period of vibration when the position of equilibrium of the magnet is perpendicular to the meridian), affords an excellent method for determining separately the two induction-coefficients of the magnet, namely the coefficient of increase and that of decrease of the magnetic moment. The advantages of this method are chiefly that it does not depend upon the mutual action of two magnets, which can be only approximately calculated, and that it requires the use of only one instrument. He also remarks that these advantages apply also to the method indicated by him in a previous publication for the determination of the temperature-coefficients of magnets, and that he has assured himself by actual trial of the precision of this method.

The remainder of the paper is devoted to the description of a process for determining in absolute measure the earth's horizontal magnetic intensity by means of the bifilar magnetometer, and to the statement of the formulæ required for the complete reduction of the results so as to take account of all corrections. The principles involved in this process may be thus indicated:—It is divisible, like Gauss's process, into a method for determining (*a*) the product of the horizontal intensity H into the moment M of a particular magnet, and (*b*) the ratio of the same two quantities. To determine the *product*, the magnet is suspended in the bifilar instrument, and the torsion-angle z is observed which is required to set the axis of the magnet perpendicular to the meridian. This operation gives the equation

$$\mathrm{D} \sin z = \mathrm{HM}.$$

The *ratio* of H to M is found by suspending another magnet of moment M′ in the bifilar, placing the first magnet with its axis in the magnetic meridian through the centre of suspension of the instrument, and determining the torsion-angle z_1, needed to set the suspended magnet perpendicular to the meridian when the north-seeking pole of the first magnet (moment M) is towards magnetic north, and also the corresponding angle z_2, when the north-seeking pole of the first magnet is towards magnetic south. These observations give, subject to the proper corrections, the equations

$$\mathrm{D}' \sin z_1 = \left(\mathrm{H} + \frac{2\mathrm{M}}{r^3}\right) \mathrm{M}',$$

$$\mathrm{D}' \sin z_2 = \left(\mathrm{H} - \frac{2\mathrm{M}}{r^3}\right) \mathrm{M}';$$

from which the ratio in question is easily obtained in terms of z_1, z_2, and the distance r between the centres of the magnets. The horizontal intensity is then given by an expression which, when the corrections (for which we must refer to the original paper) are omitted, becomes

$$\mathrm{H}^2 = \frac{2\mathrm{D} \sin z}{r^3} \cdot \frac{\sin z_1 + \sin z_2}{\sin z_1 - \sin z_2}.$$

LIX. *On the Determination of the Acceleration of Gravity for Tokio, Japan.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

My attention has been directed to a paper in your Journal for April, on the "Determination of the Acceleration of Gravity for Tokio, Japan," which I am sorry to see contains a considerable proportion of questionable teaching. As the subject is attracting a good deal of attention abroad, I am unwilling to allow this to pass muster as a fair representation of English acquaintance with it. Neither is it pleasant to think that the youth of Japan are being enlightened in this way regarding a pursuit in which England was once foremost.

To any one acquainted with the history and literature of pendulum experimentation, it is patent that the teachers in this case are either entirely unacquainted with both, or hold the opinion that the first canon of "original research" is to avoid all study of previous work. As it is more pardonable to hold a heterodox opinion than to be ignorant, I am willing to attribute the originality in this paper to such a source—the more so as the concluding sentences seem to point to such a a doctrine. The writers say, "that this investigation . . . . . . has resulted from the plan we have followed of teaching the laboratory students not, as is customary in colleges, to repeat well-known experiments, but to endeavour in their investigations to advance, in some small degree at any rate, the bounds of existing knowledge."

The doctrine is as erroneous as the design is laudable. But I must say that the teaching as here in evidence has followed the lines laid down only too well; and if I could believe that the absence of all indication of a knowledge of previous experiments is feigned—if the mistakes are not genuine—if the oversights are intentional—then the authors of this account have done themselves great injustice in publishing this instance of their method of instigating to original research.

"To repeat well-known experiments" slavishly and uncomprehendingly is no doubt undesirable; but it is far more undesirable to neglect the commonest and most essential principles of exact experimentation, in favour of abstruse investigation of conditions which, almost obviously, are unimportant. I will give a single extract to show that I am not making a random charge:—

"The two most obvious corrections to apply to this result [the time of vibration of a pendulum in air] are the corrections for infinitely small arcs and for the air-friction—neither of which were found of any practical consequence, on account of the very small angle through which the pendulum usually swung, and that the decrement of the amplitude of the vibrations was imperceptible even after many swings. Although, however, such a pendulum as we were using approaches very nearly a perfect simple pendulum, there are certain causes of possible error arising from its flexibility and slight elasticity which would not affect a rigid compound pendulum."

Perhaps I ought to regard this as dust thrown into the pupils' eyes, to prevent their attending to such well-known reductions as those for arc, buoyancy, resistance, &c. But is it possible not to suspect another explanation, when we have just before heard that long-continued experiments with two of "Kater's pendulums" (the convertible form is meant) were at last abandoned as having proved "always unsatisfactory"? How could they possibly be otherwise, in the absence of any knowledge of, or care about, the instrumental constants? Also we have been told that these results differed too much from the value "calculated by a formula developed by Clairault, who, from pendulum-experiments made at a variety of latitudes on the earth's surface, has shown that, approximately, for any latitude λ, and any height h centimetres above the level of the sea,

$$g = 980{\cdot}6056 - 2{\cdot}5028 \cos\lambda - 0{\cdot}000003\, h.\text{"}$$

To say nothing of the misprint (of $\cos\lambda$ for $\cos 2\lambda$) and the anachronism of assigning to Clairaut a formula *metrically* expressed, I cannot find words to represent adequately the state of dazed astonishment created by such an appeal. Not only is the formula wrong, numerically, but it no more belongs to Clairaut than to Copernicus. And even admitting that, in demonstrating or elucidating some point in connexion with the law of increase of gravity, Clairaut may have had recourse to numerical exposition analogous to the above, the idea of testing a modern experiment thereby is perfectly astounding, when regarded as part of academical instruction.

The force of gravity may be expressed, with all the precision at present justifiable, in metres, thus:—

$$g = 9{\cdot}8063 - 0{\cdot}02553 \cos 2\lambda.$$

If Clairaut is here credited with what he would not claim, Colonel Clarke is scarcely more fortunate:—"Clairault's formula assumes a circular equator; Captain Clarke has found that the equator is elliptical." Colonel Clarke will not thank the authors for that raw statement. What he has taught is in effect this: *Assuming an equator more or less elliptical, the* *ellipticity seems to be such-and-such; but this is not to be taken as any proof of such ellipticity.* The idea was not his, either.

One remark more, and I have done. Kater is recognized, and Borda—the one in speaking of "Kater's pendulum," and the other in alluding to "Borda's method of coincidences"—which, by the way, was not any more peculiarly Borda's than the formula (as given) was Clairaut's; Clairaut's name also is mentioned, as we have seen; not a single other author or experimenter in this field is mentioned from the first line to the last. Surely this is carrying "originality in experimental research" too far—even to the very verge of decency. I would suggest to the students of the Japanese college that they return to the discarded convertible pendulums, and demand to know, as a matter of scientific interest, WHY their results were so unsatisfactory. They may rest assured that it was because they had neglected some of those precautions and reductions which "well-known experiments" are especially useful in inculcating.

No experimental *result* is worth publishing which does not carry with it direct or indirect evidence of reliability—*or* the means of correcting its errors. In the present case we have neither the one nor the other. The force of gravity at Tokio in Japan may be known more certainly from the above formula than from the experiments recorded in the paper under review.

I am, Gentlemen,
Yours truly,
J. HERSCHEL.

LX. *Supplement to Researches on the Hydrodynamical Theory of the Physical Forces, including a Theory of the Microphone.* *By* Professor CHALLIS, *M.A., F.R.S., F.R.A.S.*[*]

WHEN I wrote the article on Newton's "Regula Tertia Philosophandi," inserted in the Philosophical Magazine for January 1880, I supposed that I should not have occasion to say more relative to the hydrodynamical theory of physical force. As, however, I have since discovered that some points of considerable importance have been either insufficiently explained or altogether omitted, I propose to supplement the previous researches by the subjoined corrections and additions.

(1) In the first place, the postscript to the above-mentioned article, which was written and despatched somewhat hastily, requires to be corrected by the following considerations.

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[*] Communicated by the Author.

According to principles and processes therein indicated, it appeared that the determination, purely by analytical reasoning, of the rate of propagation in a homogeneous medium the pressure of which varies proportionally to its density requires the integration, at least approximately, of the equation

$$\frac{d^2f}{dr^2}+\frac{1}{r}\frac{df}{dr}+4ef=0. \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(B)}$$

To effect this integration, the form of f was assumed to be such that we might have

$$f=cr^{-\frac{1}{2}}\cos(mr+c'),$$

c, m, and c' being certain constants. By substituting this value of f in the equation, a result is obtained differing from zero by the residual quantity

$$\left(\frac{1}{4r^2}-m^2+4e\right)f.$$

Hence it follows that, whatever be m, the equation is exactly satisfied by all the special values of r for which $f=0$, which are the special values that cause $\cos(mr+c')$ to vanish. Now it is evident that the value of the residual quantity is *cæteris paribus* least when $m^2=4e$, and hence that the approximation to the solution of the equation (B) is closest when $m=2\sqrt{e}$. This, in fact, is the case in the approximate solution which I referred to as having been obtained by Sir W. Hamilton and Professor Stokes, whence I deduced a velocity of sound exceeding by 17·5 feet the experimental determination. In the above-mentioned postscript I made the supposition that $m=\pi$, and consequently found a velocity of sound only 3·17 feet less than the observed value. But although this difference is less than the other, that supposition must be rejected, because the equality $m^2=\pi^2e$ does not give so close an approximation to the solution of the equation (B) as $m^2=4e$. Hence I have finally come to the conclusion that the principles and processes of Hydrodynamics which I have adopted conduct, by pure reasoning, to a velocity of sound exceeding by 17·5 feet the latest determination by experiment and observation. Possibly the excess may be attributable to the fact that the air, instead of being homogeneous, as the theory supposes, is composite, and in its lower strata charged with vapour.

(2) The assumption at the top of page 29 of the January Number, that $(d\psi)$ has the form $(d\,.f\phi)$, f being a function of x and y only and ϕ a function of z and t only, may appear to be arbitrary, because I omitted to infer from the equation (A), p. 27, that $(d\psi)=0$ if $\lambda=1$, and the propagation be

along a rectilinear *axis*. The assumption was required for satisfying that condition.

(3) In the statement of the fundamental principles of the general theory of the physical forces, one of the adopted hypotheses is that atoms are *spheres* admitting of no change of form or magnitude. At the same time it was asserted that this hypothesis is one of those which require to be verified by comparisons of deductions from them with experimental facts. I did not, however, refer to certain theoretical results I had obtained which actually afford such verification. These results are given in a dissertation entitled "The Hydrodynamical Theory of the action of a Galvanic Coil on an external small Magnet," which is divided into three parts, contained respectively in the Numbers of the Philosophical Magazine for September, November, and December 1874. The hypotheses on which the theory rests are stated in art. 1 of the dissertation, and are identical with the fundamental principles referred to above. In a postscript to the article in the November Number two sets of numerical comparisons of the theory with observation are exhibited; and at the end of the dissertation in the December Number (art. 76) a summary is given of the grounds on which the whole theory may be regarded as satisfactory. What I am now concerned with is to state that this theory depends essentially on the hypothesis that atoms are spherical in form and of constant magnitude, and that the satisfactory conclusions to which it leads are confirmatory of this hypothesis.

(4) Since in the above-mentioned dissertation there are some remarks which might be supposed to imply that the hydrodynamical theory is not in complete accordance with the formula obtained experimentally by Ampère for the mutual action between a galvanic element and a magnetic element, I take occasion to say that I have since seen reason to conclude that the accordance is quite satisfactory. I have adverted to this point because in Clerk Maxwell's 'Treatise on Electricity and Magnetism' (vol. ii. p. 16) that mutual action is expressed in four different forms, and the formula of Ampère is considered to be the *best*. It is evident that on such a question there should be no uncertainty, and that a good theory ought to be capable of deciding whether or not a proposed formula is true. According to the hydrodynamical theory which I have advocated, Ampère's is the true formula.

(5) To the "few results" obtained under Parts II. and III. of the general hydrodynamical theory, which are cited in the January Number (p. 31), and considered to be confirmatory of that theory, I might have added the explanations it is

capable of giving of the phenomena of the radiometer, plane and cup-shaped (Phil. Mag. for May and November 1876 and April 1877), and also of the actions of the otheoscope and telephone (Phil. Mag. for June 1878). I have adverted to these productions for the purpose of making a remark relative to certain inferences which experimenters have drawn from this novel class of phenomena, and which they designate by the terms "the viscosity of residual gas," "the viscosity of vacuum," "a fourth state of matter" in addition to the solid, fluid, and gaseous. The explanations above cited appear to me to point to the conclusion that these terms, so far as they are expressions of real phenomena, are referable to modes of operation of the ætherial medium in which light and heat are generated and transmitted, and are to be accounted for by mathematical deductions from the hydrodynamical theory of physical force.

(6) The Theory of the Microphone I am about to propose is a corollary from that of the Telephone given in the Philosophical Magazine for June 1878. The following mode of producing the phenomena of the microphone, which was conducted in my presence, is convenient for describing the theoretical explanation. An oblong rectangular plate of charcoal of moderate breadth and small thickness was approximately balanced about its middle transverse section, and so placed that the end of its heavier half rested slightly on another plate of charcoal of greater breadth. An arrangement was made by which the electric current pertaining to a telephone could be passed through the two pieces of charcoal. Although my hearing is not good, words transmitted from a distant room were heard with sufficient loudness and distinctness by employing the telephone in the ordinary way, and applying a receiving-cup to my ear. But as soon as the current was made to traverse the pieces of charcoal, the loudness and distinctness were marvellously increased, and I had no longer need to apply the cup. This increment of sound may, I think, be accounted for, on the principles of the hydrodynamical theory of the telephone, in the following manner. The current, in passing out of the narrower into the broader plate, enters into a larger channel, and consequently, according to the hydrodynamics of steady currents, its velocity is there diminished and its density increased. The increment of pressure thence resulting might suffice to raise the narrow plate; but if so, a very slight separation would so far intercept the current as to cause a diminution of the lifting force, and the plate would immediately be brought back by the force of gravity. Thus a quick succession of partial interruptions would

be produced, whereby the steady motion might be partially converted into *vibratory* motion. This effect would be precisely analogous to the generation by the siren of Cagniard de la Tour of musical sounds by means of a succession of interruptions of a steady current of air, the pitch of the sound increasing with the velocity of the current. These vibratory motions, being an accession to those produced by the speaker at the transmitting end of the telephone, and having a constant relation to them, are attended by an accession of current, and therefore an accession of effect at the receiving-end. This follows from the analytical formula in the second paragraph of the article in the Phil. Mag. for June 1878, the effects under consideration being all referable to pressures depending upon the *squares* of the velocity of a steady current of the ætherial medium.

I have now completed all that I am able to say relative to the department of physical science the principles of which were inaugurated by Newton, and which for distinction may be named Theoretical Physics. I must now leave to younger mathematicians the task of correcting and extending, if they think good, the efforts I have made in this direction, and shall only urge the important consideration that *Theoretical Physics* are as necessary for constituting a complete system of Physical Science as *Experimental and Empirical Physics.*

Cambridge, May 15, 1880.

LXI. *Intelligence and Miscellaneous Articles.*

ON THE THEORY OF INDUCTION-CURRENTS. BY M. MASCART.

M. HELMHOLTZ has shown that, taking as starting-point the laws of Ohm and Joule, the induction-phenomena produced by the displacement of a magnetic system in the vicinity of a current might have been foreseen. It has seemed to me that a natural generalization of the results obtained in this particular case permits us to establish the theory of electrodynamic induction-currents so as to connect them simply with a common principle.

It is known that, according to Ampère's theory, the work necessary for displacing a magnetic mass m in the vicinity of a current is equal to the product of that mass by the intensity I of the current and by the increment of the angle under which the circuit is seen from two points occupied successively by the magnetic mass. The energy of the mass m with respect to the current may therefore be represented by $\mathrm{I}m\omega$. It will be useful to enunciate this property in another form by employing the notion of Faraday's lines of force.

If the force exerted by a magnetic system upon the unit of mass situated in a point be considered, and any surface-element whatever be carried through that point, the name *stream of force* (or number of lines of force) passing through that element may designate the product of its surface by the normal component of the force. It is readily seen that the stream of force of a mass m in a cone of angular opening ω is equal to $m\omega$.

The energy of a magnetic mass with respect to a current is therefore equal to the product of the intensity by the stream of force emanating from that mass and passing through the circuit. Let us designate this stream of force by ϕ, and agree to consider it positive when the forces enter the circuit through the negative surface of the current—that is, through the negative side of the equivalent magnetic plate; the energy of the mass m will have for its expression

$$\mathrm{I}m\omega=-\mathrm{I}\phi.$$

Similarly, the energy with respect to a current of any magnetic system whatever is equal to the sum of the energies of all the masses —that is to say, to the product of the intensity of the current by the total stream of force of the system which passes through the circuit. This energy diminishes when the system is left to the action of the current; and for a slight displacement the work of the electromagnetic forces is equal to $+\mathrm{I}d\phi$.

If the displacement is effected during the time dt and a liquid-pile is employed, the energy derived from the chemical actions must heat the circuit and furnish the electrodynamic work $\mathrm{I}d\phi$ corresponding to the increment $d\phi$ of the stream of force, which gives, calling the electromotive force E and the total resistance R,

$$\mathrm{EI}dt=\mathrm{I}^2\mathrm{R}dt+\mathrm{I}d\phi, \quad \ldots\ldots\ldots\ldots\ldots \quad (1)$$

from which is deduced

$$\mathrm{IR}=\mathrm{E}-\frac{d\phi}{dt}. \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (2)$$

The intensity of the current is the same as if there existed in the circuit a new electromotive force (induction) having the value

$$e=-\frac{d\phi}{dt}. \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (3)$$

The electromotive force of induction is therefore equal and of contrary sign to the derivate with respect to the time from the stream of force which emanates from the magnetic system and passes through the circuit. This expression, here deduced from the principle of the conservation of energy, is equivalent to that which Neumann obtained by starting from Lenz's law.

We shall assume as a general rule that the electromotive force of induction in a circuit is always expressed by equation (3) as a function of the stream of force which passes through it, whatever may be the causes which make the forces vary, such as the displacement or modification of a magnetic system, change of form, intensity, or position of an exterior current, deformation of the circuit itself, or variation of the current already passing through it.

Such a generalization can be justified in each particular case when it is admitted that the effect produced by a change in the magnets and currents is equivalent to that which would be obtained by bringing from an infinite distance a magnet or current equal to the given variation. The consideration of the streams of force only will appear especially legitimate if it be conceived that electric and magnetic actions are not really exerted at a distance, but are due to a modification of the intervening medium, characterized at every point by the direction and magnitude of the force.

Let us first consider the effect of a current upon itself. The stream of force which traverses the circuit, supposed invariable, is proportional to the intensity of the current, and can be represented by UI, the factor U designating the stream of force corresponding to the unit of current. If the variation of the current be dI, the variation of the stream of force is UdI, and the energy derived from the pile

$$IUdI = d\left(\frac{UI^2}{2}\right).$$

As long as the current has not become constant a part of the energy of the pile is therefore employed to augment the term $\frac{UI^2}{2}$, called the *potential energy of the current.* Suppose that there is in the vicinity a second current, of intensity I′, in a circuit of constant form. The stream of force from the second current which traverses the circuit of the first is proportional to the intensity; it may be represented by VI′. The factor V denotes the stream of force that emanates from either of the two circuits and traverses the other when the two currents have the same intensity equal to unity. The product II′V is called the *relative potential energy of the two currents.*

When the product I′V, in consequence of a change of intensity or a displacement, varies by $d(I'V)$, the energy derived from the pile of the first current is $Id(I'V)$. Therefore, if the two modifications are simultaneous and are produced during the time dt, we have for the first circuit

$$EIdt = I^2Rdt + d\left(\frac{UI^2}{2}\right) + Id(I'V)\,; \quad \ldots\ldots \quad (4)$$

and, in like manner, for the second,

$$E'I'dt = I'^2R'dt + d\left(\frac{U'I'^2}{2}\right) + I'd(IV). \quad \ldots\ldots \quad (5)$$

By addition we get

$$(EI + E'I')dt = (I^2R + I'^2R')dt + d\left(\frac{UI^2}{2} + \frac{U'I'^2}{2} + II'V\right) + II'dV. \; (6)$$

This equation expresses that the energy furnished by the two piles during the time dt is employed in heating the conductors, increasing the potential energy of each of the currents as well as their relative potential energy, and, lastly, in furnishing the electrodynamic work corresponding to the relative displacement. If the form of the circuits be altered, the variation of the stream of force emanating from one of the currents in the direction of the other is comprised in the terms $d(\text{I}'\text{V})$ and $d(\text{IV})$; and in order to value the action of a circuit upon itself it suffices to regard the factor U as variable. We thus find again the known equations of electrodynamic induction-currents.

The same mode of reasoning permits us to take account of the changes which have happened in exterior magnets, and of the magnetization produced by currents on masses of soft iron.—*Comptes Rendus de l'Académie des Sciences*, April 27, 1880, t. xc. pp. 981–984.

EXPERIMENTAL RESEARCHES ON THE DECOMPOSITION OF SOME EXPLOSIVES IN A CLOSED VESSEL; COMPOSITION OF THE GASES FORMED. BY MM. SARRAU AND VIEILLE.

These researches were undertaken for the purpose of determining the conditions of the employment of gun-cotton in mines.

Since the important improvements introduced by Mr. Abel into the manufacture of gun-cotton, this explosive is prepared in homogeneous masses of determined form and density, it is kept without danger in the wet state, and its explosive force, comparable with that of dynamite, is much greater than that of gun-powder; consequently its use in mines affords great advantages. It presents, however, an inconvenience in that its explosion produces mephitic gases injurious to the workmen in the galleries. In fact its decomposition gives rise to the formation of carbonic oxide. This can be obviated by adding to the gun-cotton an oxidizer, such as a nitrate.

In this memoir we make a comparative study of the products formed, the heat evolved, and the pressure developed by the explosion in a closed vessel,—1st, of pure gun-cotton*; 2ndly, of a mixture of equal parts of gun-cotton and nitrate of potass; 3rdly, of a mixture of 40 parts of gun-cotton and 60 of nitrate of ammonia; 4thly, of nitroglycerine; and, 5thly, of ordinary mining-powder†. We exhibit today the results of our study of the gases formed during the explosion in a closed vessel.

Some previous experiments having shown that the nature and the composition of the gases depend on the conditions of their production, and particularly on the pressure under which they are formed, we operated upon gases collected in a closed test-tube,

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* The composition, by weight, of gun-cotton, deduced from organic analysis, and verified by elementary analysis of the products of decomposition, has led us to regard the products of the manufacture current at the Moulin-Blanc works as a mixture of 3 eq. of trinitrated and 1 eq. of binitrated cellulose—

$$3[C_{12}H_7(NO_4)_3O_{10}]+C_{12}H_8(NO_4)_2O_{10},$$

corresponding to the rough formula

$$C_{48}H_{29}N_{11}O_{84}.$$

† This powder consists, from its manufacture, of 62 parts saltpetre, 20 parts sulphur, and 18 charcoal.

measuring the maximum pressure in the tube. The volumetric analysis of the gaseous products was completed by the absolute measurement of the volume occupied, at 0° temperature and normal pressure, by the gases from a determined weight of explosive. These are the principal results obtained:—

(1) *Gun-cotton.*—The composition and volume of the gases were determined under four conditions, differing by the mean density of the products. It follows from the numbers of experiment that, when the density augments, the proportion of carbonic oxide progressively diminishes, that of carbonic acid increasing.

The following formulæ, which very well represent the composition of the gases produced under the four conditions of our experiments, show what is the law according to which the decomposition of the substance is modified by the pressure; they correspond to the decomposition of 1 equivalent of gun-cotton ($C_{48}H_{29}N_{11}O_{84}$):

Density.	Formula of decomposition.
0·010	$33CO+15CO_2+8H+11N+21HO$
0·023	$30CO+18CO_2+11H+11N+18HO$
0·200	$27CO+21CO_2+14H+11N+15HO$
0·300	$26CO+22CO_2+15H+11N+14HO$

They indicate that, for increasing values of the density of loading, the carbonic oxide and carbonic acid tend to be produced in equal volumes, according to the formula

$$C_{48}H_{29}N_{11}O_{84}=24CO+24CO_2+17H+11N+12HO;$$

and it may be assumed that this formula represents sensibly the mode of decomposition realized under ordinary conditions in practice, in which gun-cotton is generally used in very dense charges.

(2) *Gun-cotton mith Nitrate of Potass.*—The analysis of the gases shows that the oxidation of the explosive is incomplete under low densities of charge, although the proportion of the oxidant may be considerably higher than that which theoretically corresponds to complete combustion; but at 0·3 and higher densities carbonic oxide disappears, and the decomposition of the mixture takes place regularly according to the formula

$$C_{48}H_{29}N_{11}O_{84}+12(NO_5KO)=36CO_2+23N+19O+29HO \\ +12CO_2KO.$$

(3) *Gun-cotton with Nitrate of Ammonia.*—The combustion is complete, even under low densities of charge. The results of analysis show that the reaction corresponds to the equation

$$C_{48}H_{29}N_{11}O_{84}+22(NO_6H, NH_3)=48CO_2+55N+117HO+3O,$$

the first member of which represents exactly the composition of the mixture (deducting the humidity and the saline residue contained in the gun-cotton); while the second member is established on the supposition that the decomposition of the nitrate generates, in one of the modes studied by M. Berthelot, oxygen, nitrogen, and water.

(4) *Nitroglycerine.*—According to a remark by M. Berthelot, the composition of the products of an explosion can be foreseen when the explosive contains sufficient oxygen to transform the elements

into more highly oxidized stable compounds. This condition is fulfilled by nitroglycerine; in fact, experiment proves that its decomposition in a closed vessel is represented by the theoretic equation

$$C_6H_5N_3O_{18} = 6CO_2 + 3N + O + 5HO.$$

(5) *Mining-powder.*—The composition of the gases was found sensibly the same for densities equal to 0·3 and 0·6; it is given below.

In a recapitulation we infer from these results the qualitative and quantitative composition of the gases furnished by each explosive under the normal conditions of its employment. The following Table gives the volume (in litres) of each of the gases, per kilogram of the substance, under those conditions:—

	CO.	CO_2.	H.	N.	O.	C_2H_4.	HS.	Total.
Pure gun-cotton	234	234	166	107	..	..	..	741
Gun-cotton with nitrate of potass	..	171	..	109	45	..	..	325
Gun-cotton with nitrate of ammonia	..	184	..	211	6	..	..	401
Nitroglycerine	..	295	..	147	25	..	..	467
Ordinary mining-powder	64	150	4	65	..	4	17	304

—*Comptes Rendus de l'Académie des Sciences*, May 3, 1880, t. xc. pp. 1058–1060.

SUMMARY OF THE LAWS WHICH GOVERN MATTER IN THE SPHEROIDAL STATE. BY P.-H. BOUTIGNY.

I. *Temperature.*—The temperature of bodies in the spheroidal state is always below that of their ebullition: it is +97° for water.

II. *Non-equilibrium of Temperature.*—The substance in the spheroidal state never places itself in equilibrium of temperature with the vessel which contains it; its temperature is always in a state of stable equilibrium, whether it be in a capsule with free access of air or in the muffle of a cupel furnace. But if the body in the spheroidal state does not place itself in equilibrium of temperature, the vapour emanating from it always does so. These two phenomena are manifested very clearly when the operation is performed in a hollow sphere arranged *ad hoc.*

III. *Reflection of Radiant Heat.*—Matter in the spheroidal state reflects radiant heat.

IV. *Volume and Mass of the Spheroids.*—The volumes of matter in the spheroidal state are in the inverse ratio of their density; and their masses are equal among themselves.

V. *Repellent Force at a sensible Distance.*—This law is the most important of all, the richest in deductions; for we regard it as the antagonist of universal attraction.

On the floor of the Panthéon, in the axis of the cupola, upon a good charcoal fire a large platinum capsule is placed, and its temperature raised to the utmost possible. This being arranged, water is poured down from the top of the Panthéon (about 70 metres high), falls into the capsule without wetting it, and instantly passes into the spheroidal state.

The experiment is recommenced in the open air when it rains or when hail is falling; and the results are the same as in the Panthéon experiment. Can it be said that in these experiments the water and the hail are sustained in the capsule by the vapour which envelops them? Certainly not; they are repelled instantaneously by the repellent force to which the heat in the capsule gives rise.

Let us now operate with non-volatile bodies, which cannot be distilled, but are decomposed by heat.

The capsule is heated as before, and small fragments of wax, tallow, stearic or margaric acid are then thrown into it, or else some drops of oleic acid or a fixed oil; and this is what takes place:—As the molecular motions are not transmitted with very great velocity, the body experimented on remains suspended over the capsule *without vapour and without gas* proceeding from its decomposition; afterwards the gases resulting from its decomposition are liberated, not from its surface, but from its interior: they take fire, and the spheroid vanishes.

The body under experiment not being volatile, giving off no vapour, and the gases arising from its decomposition not being yet produced, evidently the body can only be sustained beyond the radius of the physico-chemical activity of the capsule by the repulsion of the latter.—*Comptes Rendus de l'Académie des Sciences*, May 3, 1880, t. xc. pp. 1074–1075.

ON A GENERAL THEOREM ADVANCED BY PROF. CLAUSIUS IN REFERENCE TO ELECTRICAL INFLUENCE. BY G. J. LEGEBEKE.

In vol. i. of Wiedemann's *Annalen*[*] Prof. Clausius communicated a theorem on the connexion between the electrical charges of an arbitrary number of conductors which act by influence on one another. That author deems this theorem very general and new, and also assumes it in Part II. of his *Mechanische Wärmetheorie*, p. 33. I wish to point out that Clausius's is a special case of a more general principle, which, again, can itself be regarded as an extension of a well-known equation of Gauss's.

Over each of the closed surfaces C_1, C_2, &c., named generally C, a layer of a certain agent is spread, and acts by attraction or repulsion according to the usual laws. The density of the agent in points of

$$C_1,\quad C_2,\quad \&c.,\quad C,$$

is represented by

$$h_1,\quad h_2,\quad \&c.,\quad h,$$

and the potential of all these layers in the same points by

$$V_1,\quad V_2,\quad \&c.,\quad V.$$

Secondly, the layers upon C_1, C_2, &c., C, are replaced by layers

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[*] See Phil. Mag. [5] iv. pp. 454 *et seqq.*

with the densities $\mathfrak{h}_1$, $\mathfrak{h}_2$, &c., $\mathfrak{h}$, the potentials of which are

$$\mathfrak{V}_1,\quad \mathfrak{V}_2,\ \text{\&c.},\quad \mathfrak{V}.$$

Now, if dw_1, dw_2, &c., dw represent the surface-elements of C_1, C_2, &c., C,

$$\int V_1\mathfrak{h}_1 dw_1+\int V_2\mathfrak{h}_2 dw_2+\text{\&c.}=\int\mathfrak{V}_1 h_1 dw_1+\int\mathfrak{V}_2 h_2 dw_2+\text{\&c.},$$

or even

$$\Sigma\int V\mathfrak{h}\,dw=\Sigma\int\mathfrak{V}h\,dw \quad\ldots\ldots\ldots\ldots\ldots\quad (1)$$

holds good universally.

Before demonstrating this theorem, I remark that it can be applied directly to a number of conducting bodies C_1, C_2, &c. charged with electricity in two different ways and acting by influence upon each other; for then the potentials V and $\mathfrak{V}$ are constants expressing the potential-levels of the conductors, and equation (1) changes into the equation of Clausius.

In order to prove the above equality, we start, as Clausius did, from Green's well-known equation. This equation is employed for the space enclosed by the surface C; and for the two functions occurring therein I take V_i and $\mathfrak{V}_i$, of which the first represents the potential of all the layers with densities h, and the second the potential of the layers with densities $\mathfrak{h}$, both potentials taken in points within C. We then have

$$\int V_i\frac{d\mathfrak{V}_i}{dn}dw=\int\mathfrak{V}_i\frac{dV_i}{dn}dw.$$

The operation $\frac{d}{dn}$ is the differentiation with respect to the normal drawn within C. By successive applications of Green's theorem to the spaces enclosed by all the surfaces, we get, after summation,

$$\Sigma\int V_i\frac{d\mathfrak{V}_i}{dn}dw=\Sigma\int\mathfrak{V}_i\frac{dV_i}{dn}dw'. \quad\ldots\ldots\ldots\quad (2)$$

Green's proposition is further applied to the space outside the surface C, which space was first bounded by the surface of a sphere; the centre and radius were taken so that all the surfaces C lay within the sphere. The potentials of the layers h and $\mathfrak{h}$ in points outside the surfaces C are represented respectively by V_a and $\mathfrak{V}_a$, and these functions substituted in Green's equation. We then find, in the usual way, if the radius of the sphere be infinite,

$$\Sigma\int V_a\frac{d\mathfrak{V}_a}{dN}dw=\Sigma\int\mathfrak{V}_a\frac{dV_a}{dN}dw. \quad\ldots\ldots\ldots\quad (3)$$

The operation $\frac{d}{dN}$ is the differentiation with respect to the normal drawn outside of the surface C. The functions V_i and V_a in equations (2) and (3) have of course the same value V for the same point of one of the surfaces C; likewise the functions $\mathfrak{V}_i$ and $\mathfrak{V}_a$, which both change into $\mathfrak{V}$. It is not so with the differential quotients along the normal.

The densities being h, let the potential of the layer on the surface C, in points within C, be represented by P_i, and the potential of the other layers by H_a; then is

$$\frac{dV_i}{dn}=\frac{dP_i}{dn}+\frac{dH_i}{dn},$$

and

$$\frac{dV_a}{dN}=\frac{dP_a}{dN}+\frac{dH_a}{dN}.$$

Manifestly, for points of C,

$$\frac{dH_i}{dn}=-\frac{dH_a}{dN};$$

and according to a known property $\frac{dP_i}{dn}+\frac{dP_a}{dN}=-4\pi\epsilon h$, where ϵ represents a constant. Hence we find

$$\frac{dV_i}{dn}+\frac{dV_a}{dN}=-4\pi\epsilon h,$$

and in precisely the same way

$$\frac{d\mathfrak{V}_i}{dn}+\frac{d\mathfrak{V}_a}{dN}=-4\pi\epsilon\mathfrak{h}.$$

Therefore, by the summation of both members of equations (2) and (3), we get

$$\Sigma\int V\mathfrak{h}\,dw=\Sigma\int\mathfrak{V}h\,dw.$$

Lastly, I remark that this general equation is to be regarded as an extension of a property given by Gauss. Let V represent the potential of a system of masses m_1, m_2, &c. situated in the points p_1, p_2, &c., and $\mathfrak{V}$ the potential of masses $\mathfrak{m}_1$, $\mathfrak{m}_2$, &c. in the points $\mathfrak{p}_1$, $\mathfrak{p}_2$, &c.; further, V_1, V_2, &c. the values of V in the latter points, and $\mathfrak{V}_1$, $\mathfrak{V}_2$, &c. the values of $\mathfrak{V}$ in the points p_1, p_2, &c.; then, according to Gauss,

$$V_1\mathfrak{m}_1+V_2\mathfrak{m}_2\quad\&c.=\mathfrak{V}_1m_1\mathfrak{V}_2m_2+\&c.$$

$$\Sigma V\mathfrak{m}=\Sigma\mathfrak{V}m.$$

This equation is identical, since both members represent aggregates of the same combinations. That the theorem still remains valid when first the masses m are spread out upon a surface C and afterwards the masses $\mathfrak{m}$ upon the same surface, is not completely demonstrated by Gauss*, but "the way in which this extension of the theorem can be rigorously justified with respect to its principal moments only" pointed out.

In the foregoing this theorem is demonstrated for an arbitrary number of surfaces as a simple deduction from Green's equation.—Wiedemann's *Annalen*, 1880, No. 5, x. pp. 154–158.

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* Gauss, *Allgemeine Lehrsätze in Beziehung auf die im verkehrten Verhältnisse des Quadrats der Entfernung wirkenden Kräfte*, § 19.

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END OF THE NINTH VOLUME.

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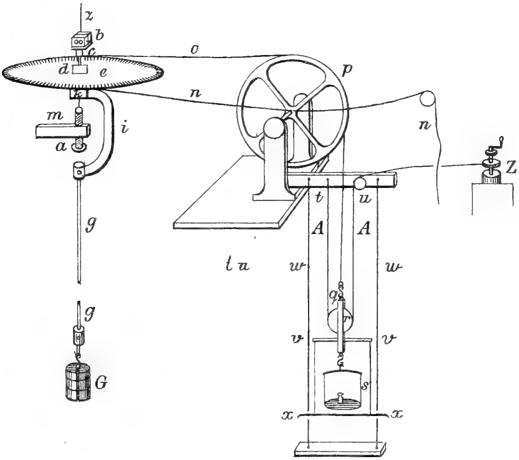

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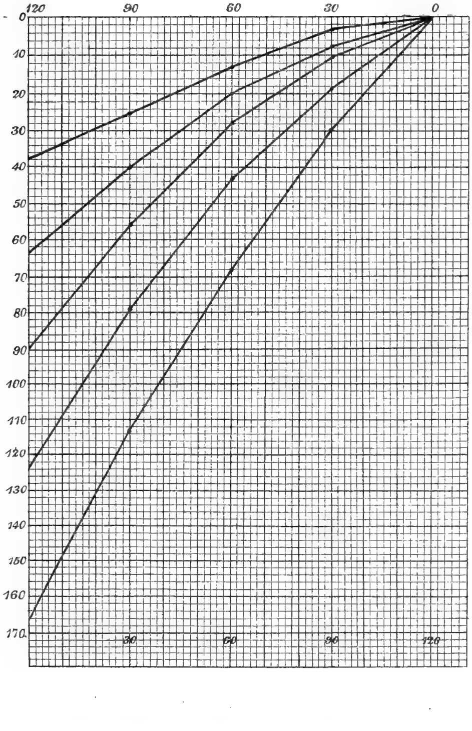

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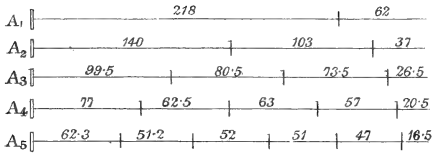

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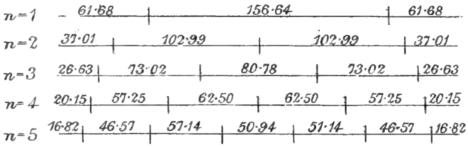

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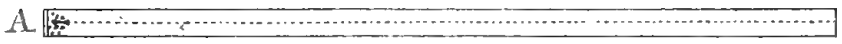

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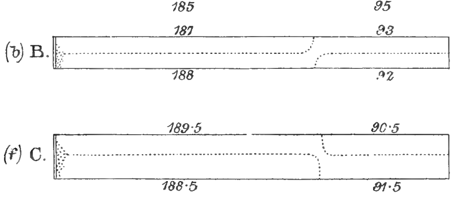

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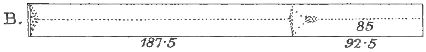

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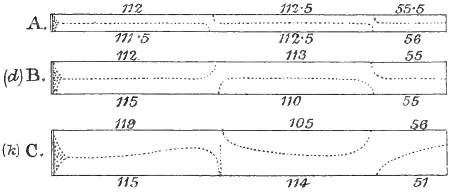

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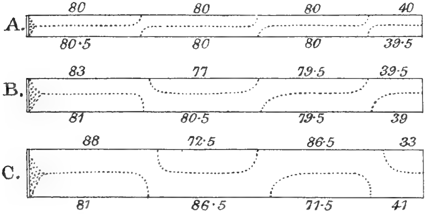

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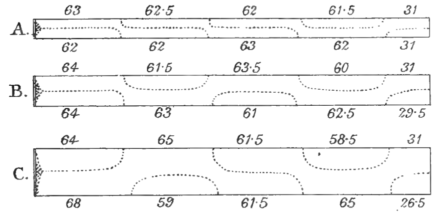

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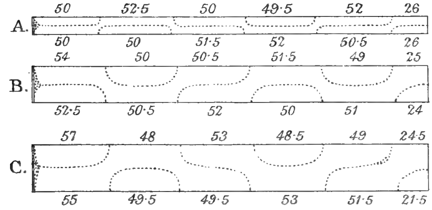

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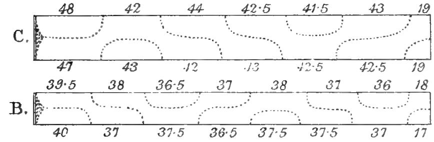

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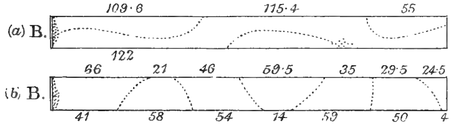

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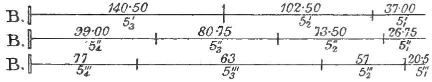

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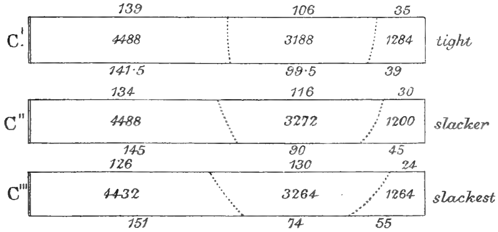

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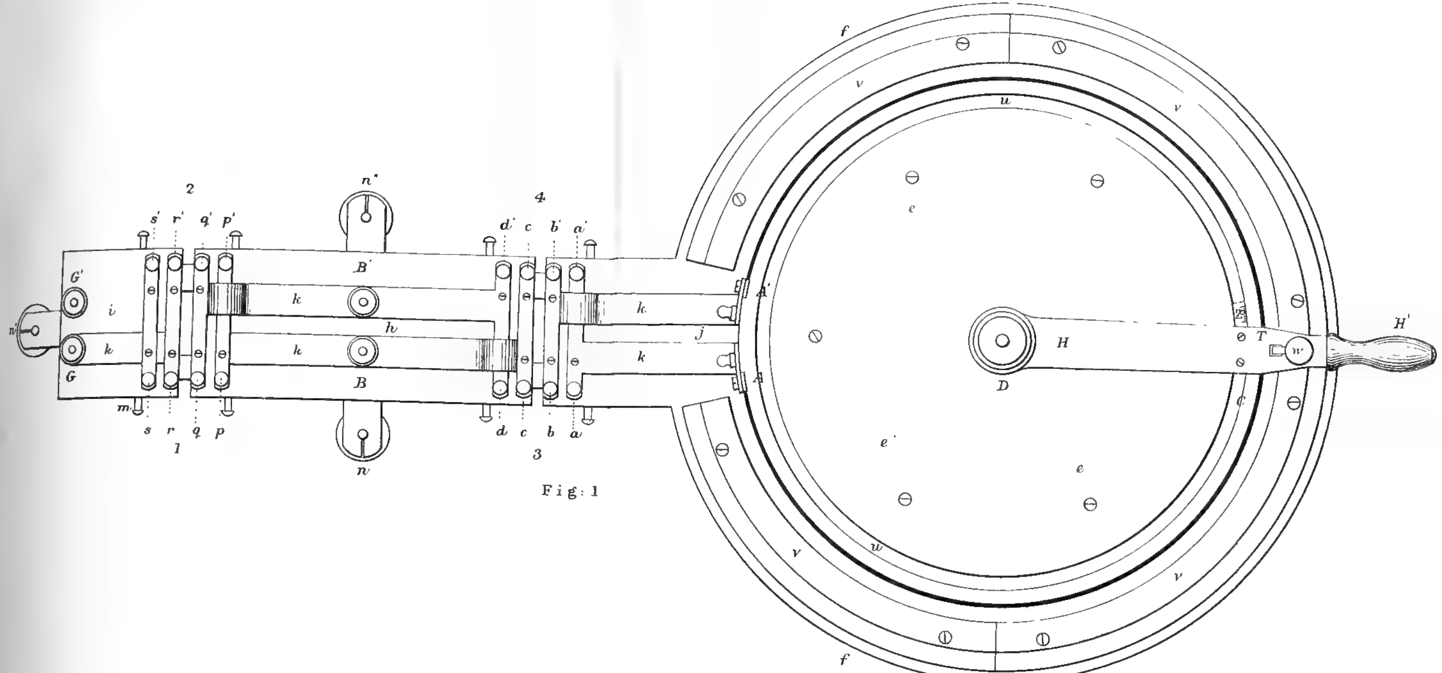

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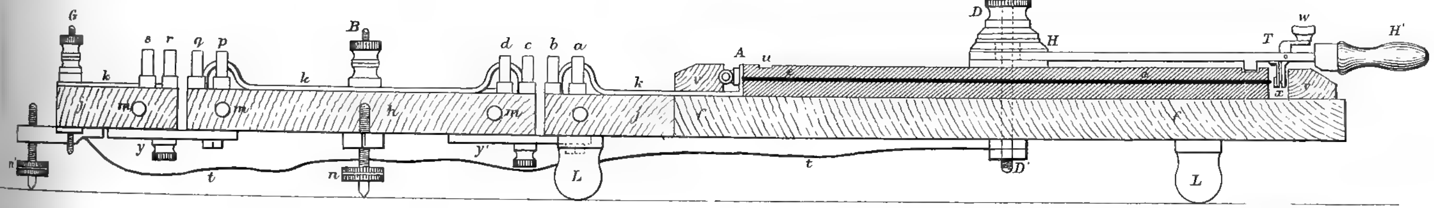

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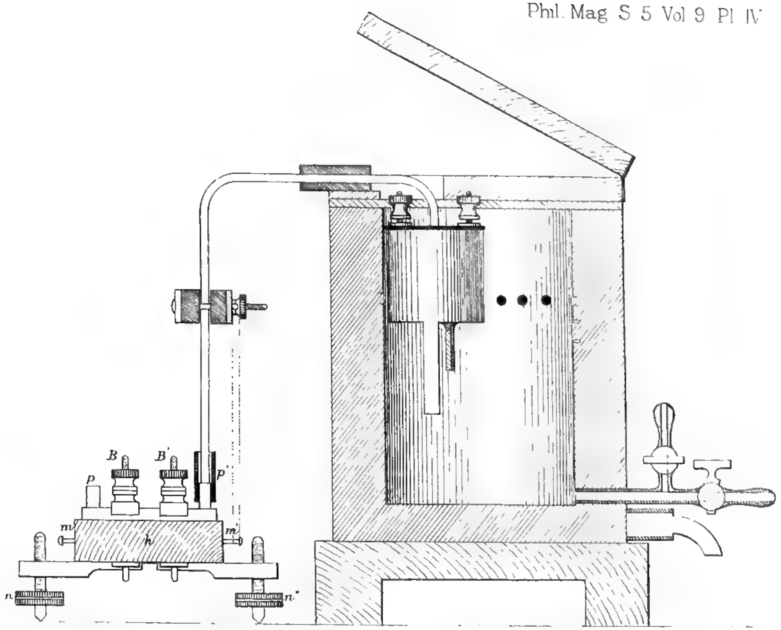

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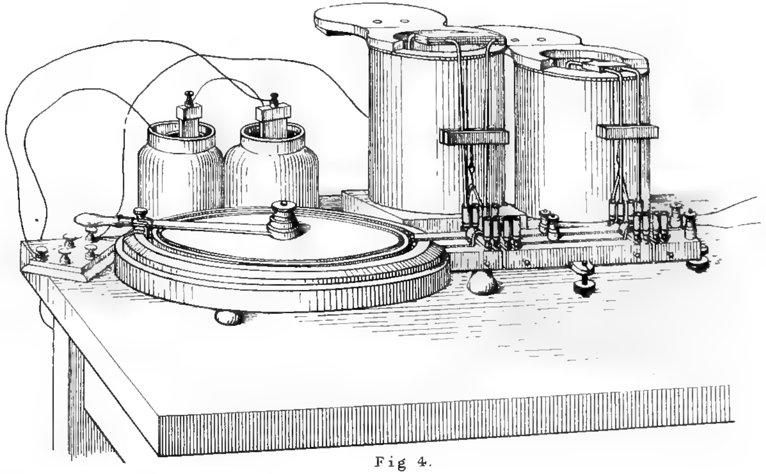

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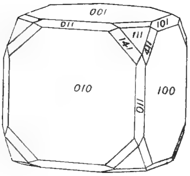

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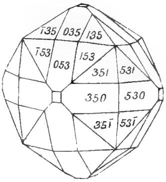

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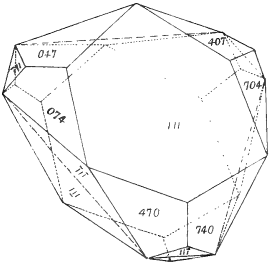

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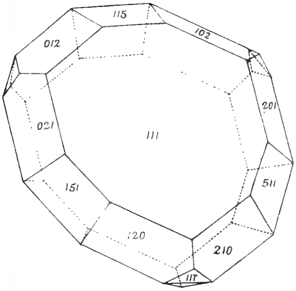

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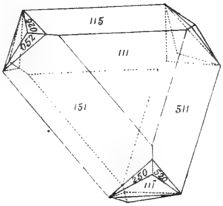

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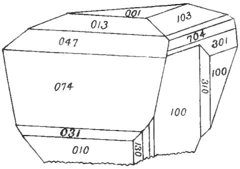

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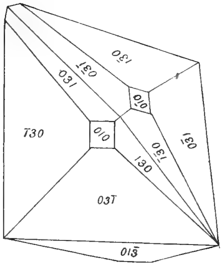

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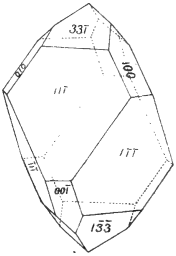

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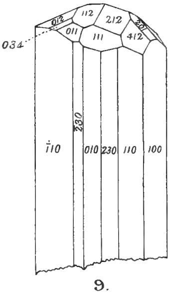

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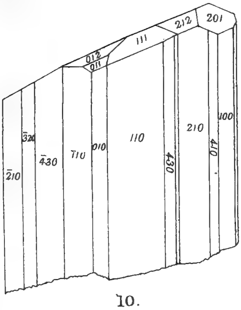

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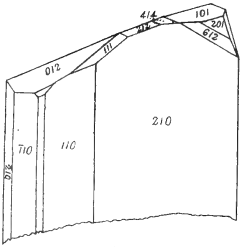

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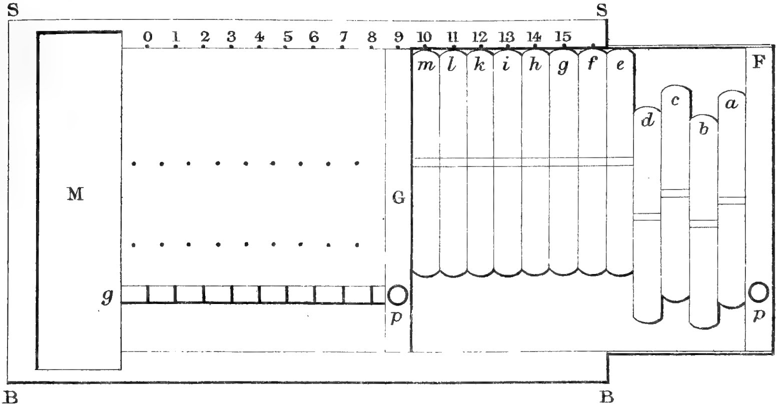

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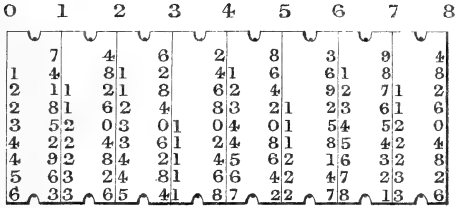

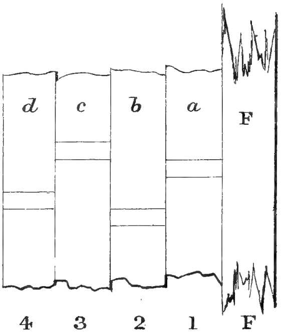

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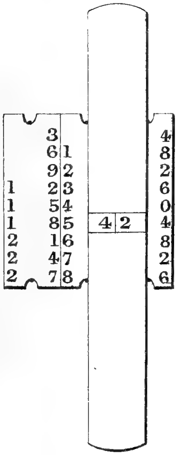

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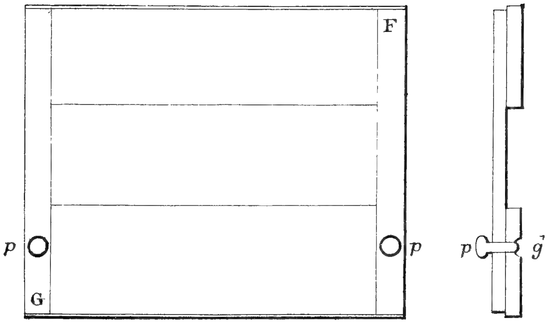

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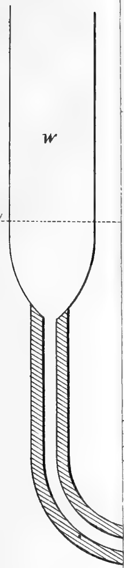

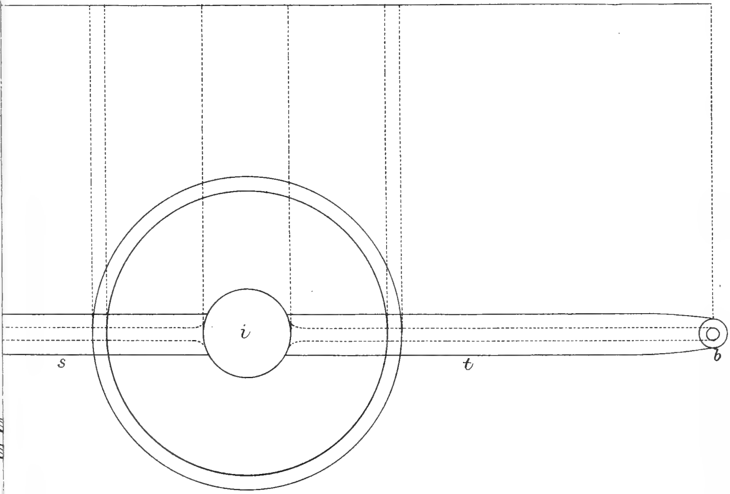

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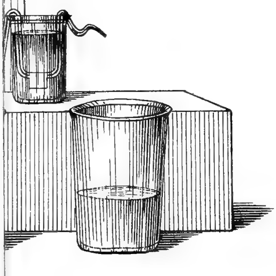

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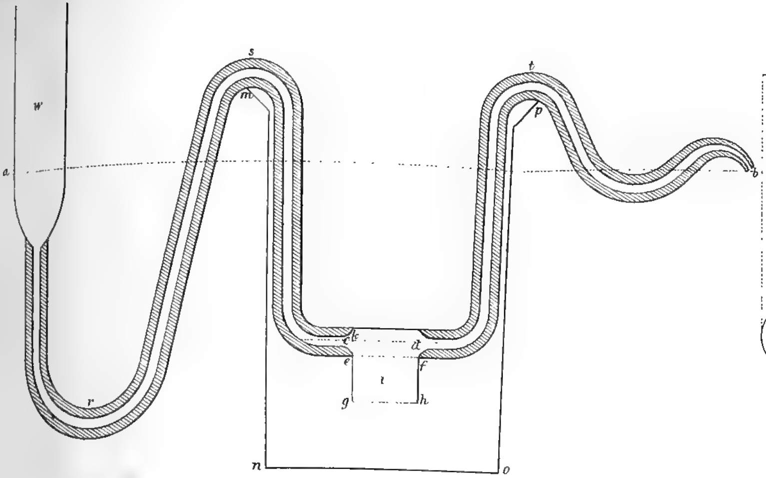

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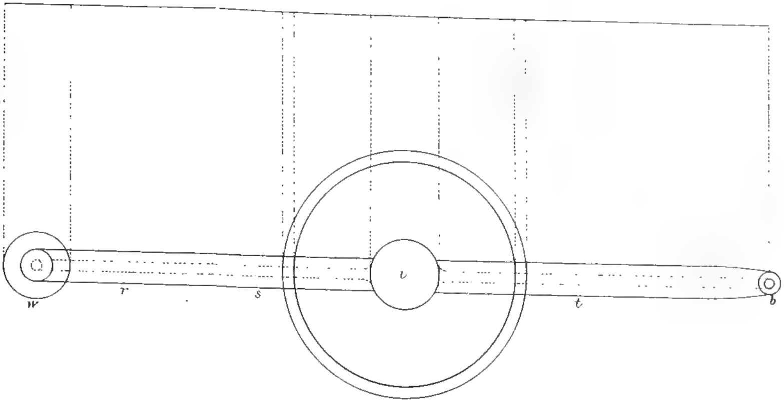

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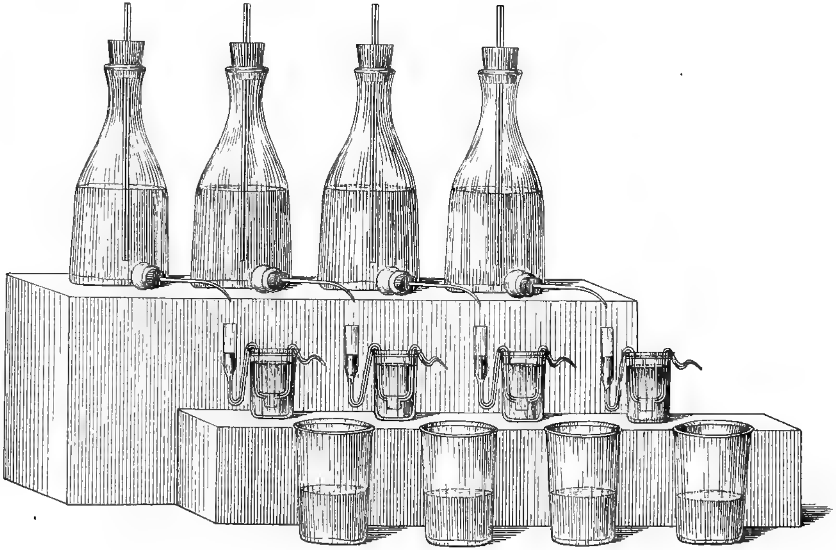

Fig. 3.

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Erratum in No. 53 (January 1880).

Page 47, lines 6 and 5 from bottom, *for* The roots of (40) are real; and therefore aplanaticism is possible if *read* The roots of (40) are real, and therefore aplanaticism is possible, if

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Errata in No. 55 (March 1880).

Page 193, art. 5, line 12, *after* frame *insert* from
— 196, art 11, line 3, *for* 319 *read* 313
— „ „ line 9, *move all the figures one place to the left.*
— 197, line 2, *after* that *insert* after the first stage, *and for* collecting *read* unaccented

Errata in No. 56 (April 1880).

Page 271, line 37 *for* $\mathrm{U}^{kn}\mathrm{N}$ *read* $\mathrm{U}_{kn}\mathrm{N}$
— 271, — 38, *for* U_{kn} is not *read* $\mathrm{U}_{kn}\mathrm{N}$ is not
— 271, — 41, *for* $\mathrm{U}_{kn}=45$ *read* $\mathrm{U}_{kn}\mathrm{N}=45$
— 272, — 7, *for* observations *read* vibrations
— 282, — 31, *for* curve *read* error.

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