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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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THE
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[FOURTH SERIES.]

JULY 1863.

I. *On the Relation between the Temperature and the Tension of Vapours.* By H. Buff, *Professor of Physics in the University of Giessen*[*].

If a volume of air V, measured under a pressure p, be suddenly compressed to the volume V′, its temperature gradually rises τ° above the original temperature t°.

Suppose this compressed air has, without further change of volume, cooled down to the original temperature t°, it will then have the tension $p' = \frac{V.p}{V'}$. Let P be the tension of the volume V′ at the temperature $t+\tau$, then

$$P : \frac{V}{V'}p = 273+t+\tau : 273+t,$$

and therefore

$$\frac{V}{V'} = \frac{P}{p} \cdot \frac{273+t}{273+t+\tau}. \quad \ldots\ldots\ldots \quad (1)$$

If the amount of heat liberated by the compression of the variable volume V′ by $\frac{V'}{273+t+\tau}$ be the fraction α of that amount which the volume V′ requires to raise its temperature 1° C. without a change of volume, we may make

$$\frac{V'}{273+t+\tau} : \alpha = -dV' : d\tau,$$

whence it follows that, when it is remembered that for the value $V' = V$ the rise of temperature $\tau = 0$,

$$\alpha \log \frac{V}{V'} = \log \frac{273+t+\tau}{273+t},$$

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[*] Communicated by Prof. Hofmann, F.R.S.

and likewise

$$\frac{V}{V'}=10^{\frac{1}{\alpha}\log\frac{273+t+\tau}{273+t}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (2)$$

By combining equations (1) and (2), it follows that

$$\alpha=\frac{\log\frac{273+t+\tau}{273+t}}{\log\left(\frac{P}{p}\cdot\frac{273+t}{273+t+\tau}\right)}, \quad . \quad . \quad . \quad . \quad . \qquad (3)$$

and

$$P=p\,\frac{273+t+\tau}{273+t}\,10^{\frac{1}{\alpha}\log\frac{273+t+\tau}{273+t}} \quad . \quad . \quad . \quad . \quad . \qquad (4)$$

If the difference $V'-V$ indicate an expansion of the original volume, τ becomes negative; and the fraction α, the cooling consequent to the expansion of the volume by $\frac{V'}{273+t+\tau}$, is negative likewise; in other respects the same expressions as before hold good.

It is known that the value of α is constant for air, that is, it is independent of temperature and degree of density; it is also assumed to be true for all permanent gases. We can determine the extent to which liquefiable gases obey this law when we measure the changes that such gases undergo in temperature and tension by different degrees of compression, the latter never being sufficiently great to cause the gases to become liquid.

We know with great accuracy the temperatures corresponding to the maximum tensions of vapours, those of water especially, for widely varying densities. The question therefore whether α, for aqueous vapour for instance, is a constant or a changeable magnitude may be decided by help of equation (3). We must of course make the supposition, which though probably not strictly exact is still very nearly correct, that the coefficient of expansion of vapour agrees with that of air.

Assuming this, the following question might be asked: If saturated aqueous vapour at t° and under p millims. pressure obtain at $(t^\circ+\tau^\circ)$ and under P millims. pressure a maximum density again, what is the mean value of α between these limiting temperatures? Or, to put the question in another form: How great is the quantity of heat, expressed in fractional parts of a thermometric degree, which becomes latent or is set free by an expansion or contraction amounting to $\frac{1}{273+t+\tau}$ of a changeable volume V', which volume at t° and under the pressure p occupies the space V?

If α be here, as in the case of the gases, a constant number, we should, by starting with a given tension p and temperature t, obtain by means of equation (3),

$$\alpha = \frac{\log(273+t+\tau)-\log(273+t)}{\log P(273+t)-\log p(273+t+\tau)},$$

and, by substituting various values for P and τ, always find the same value for α. This, however, is not the case; for it furnishes with increasing temperatures an appreciable though very slowly augmenting value for α.

In order to obtain an expression as generally applicable as possible for the calculation of these numbers, I have made use of five of the determinations directly resulting from the observations which Regnault has especially selected in his work, *Relation des Expériences, &c.*, première partie, p. 608, as data for the calculation of a general formula for the determination of the tensions of saturated vapours. They are as follow:—

Temperatures of the air-thermometer in degrees C.	Tensions in millims. of mercury.
− 20	0·91
+ 40	54·91
100	760·00
160	4647·00
220	17390·00

Of these tensions, the one at 100° C. and under a pressure of 760 millims. is determined with most accuracy. In the foregoing equation, therefore, we put

$$t=100, \quad p=760;$$

and as an example, $t+\tau=-20$, and P accordingly corresponds to 0·91, &c.

The values of α thus found may be calculated very exactly by the equation

$$\alpha = 0{\cdot}06479 + 0{\cdot}0001722\,T - 0{\cdot}0000001\,T^2, \quad . \quad . \quad (5)$$

wherein T stands for $t+\tau$.

The value of α, as calculated from this equation, for any chosen temperature T signifies a mean value between T and 100°.

It is evident that the heat which saturated aqueous vapour sets free by the compression to a very small but an equal fractional part of its volume at 100° C. certainly increases with the temperature, but in a somewhat smaller ratio than the latter. If we transform equation (3), which was formerly employed for the determination of α, and give it the following form—

$$\log P = \log\frac{760}{373} + \log(273+T) + \frac{\log(273+T)-\log 373}{\alpha}, \quad (4)$$

we can use it with the help of the now known values of α for the calculation of the tensions of saturated vapours. This formula differs from all others hitherto employed for the same purpose, because it is deduced purely from theory, and because the number α, the only one occurring therein which is derived from experiment, has a definite scientific signification.

How far the numbers calculated by means of this formula agree with experiment is perceived by comparison with some examples which have been taken indiscriminately from Regnault's work, and which, with the numbers of the pages whereon they occur, are contained in the following Table:—

Page in the work.	Temperature of the air-thermometer.		Tension.		Difference.
	T° C.	α.	Calculated.	Observed.	
599	− 32	0·05918	0·25	0·32	−0·07
509	− 30·89	0·05936	0·34	0·32	+0·02
	− 23·00	0·06078	0·70	0·66	+0·04
599	− 16·00	0·06201	1·29	1·29	0·00
509	− 10·30	0·06301	2·01	1·99	+0·02
	− 5·47	0·06385	2·99	2·95	+0·04
508	− 2·43	0·06436	3·76	3·81	−0·05
	0	0·06478	4·50	4·50 to 4·69	
504	+ 20·16	0·06821	17·48	17·64	−0·16
606	40·00	0·07152	54·90	54·91	−0·01
526	89·83	0·07945	522·00	522·02	−0·02
	92·20	0·07982	571·00	569·83	+1·17
568	116·30	0·08346	1324·21	1324·62	−0·41
	116·36	0·08347	1326·77	1326·67	+0·10
	151·37	0·08856	3711·88	3708·96	+2·92
	151·38	0·08856	3712·95	3711·19	+1·76
	160·28	0·08982	4679·60	4677·43	+2·17
	160·28			4679·04	+0·56
570	201·91	0·09548	12145·10	12147·94	−2·84
	201·92	0·09548	12148·07	12155·63	−7·56
572	230·50	0·099172	21129·65	21127·00	+2·66
	230·52	0·099175	21136·89	21136·72	+0·17
	230·54	0·099177	21144·37	21139·30	+5·07
	230·56	0·099180	21152·03	21144·02	+8·01

The differences between the calculated and the observed tensions have, it will be seen, sometimes a positive and sometimes a negative sign, and in no case exceed the limits of unavoidable errors of observation. Between 90° and 100° an error of $\frac{1}{10}$th of a degree in temperature makes one of more than 2 millimetres in the tension, and at temperatures above 150° a difference of temperature of only $\frac{1}{20}$th of a degree corresponds to differences in the tension of from 6 to 18 millimetres.

As it has thus been proved that the values obtained from the formula agree with very great accuracy with the results directly obtained by experiment throughout the whole range of measurements that have hitherto been made, I have devised the following Table, which contains the tensions corresponding to temperatures from −30° to +230° C. of the air-thermometer, together with the values pertaining to α for increments of 10°. The differences between these values diminish so slowly, that the value of α corresponding to any chosen temperature may be derived with sufficient accuracy from the proportional parts relating to one degree of temperature of those numbers within which it is situated. The calculation of the corresponding tension P is consequently greatly facilitated by the use of the formula

$$\log P = 0{\cdot}3091048 + \log(273+T) + \frac{\log(273+T) - 2{\cdot}5717088}{\alpha}.$$

The numbers contained under R in the Table are obtained from Regnault's work, page 608. They are the mean values obtained from the graphic representation of his observations.

The last column of the Table contains the tensions of aqueous vapour from −20° to +110°, as determined by Magnus, which, it will be seen, accord well with the preceding values.

Tension of saturated Aqueous Vapour, in millims. of mercury, for temperatures between −30° and +230° C. of the air-thermometer.

T°.	α.	*pp.*	P.	R.	M.
− 30	0·05953	17·8	0·37	0·39	
− 20	0·06131	17·5	0·92	0·91	0·92
− 10	0·06306	17·3	2·10	2·08	2·11
0	0·06479	17·1	4·50	4·60	4·53
+ 10	0·06650	16·9	9·06	9·16	9·13
20	0·06819	16·8	17·31	17·39	17·39
30	0·06987	16·5	31·51	31·55	31·60
40	0·07152	16·3	54·91	54·91	54·97
50	0·07315	16·1	91·94	91·98	91·97
60	0·07476	15·9	148·77	148·79	148·58
70	0·07635	15·8	233·03	233·09	232·61
80	0·07793	15·5	354·54	354·64	353·93
90	0·07948	15·3	525·46	525·45	524·78
100	0·08101	15·1	760·00	760·00	760·00
110	0·08252	14·9	1075·10	1073·70	1077·26
120	0·08401	14·8	1491·07	1489·00	
130	0·08549	14·5	2029·18	2029·00	
140	0·08694	14·3	2716·05	2713·00	
150	0·08837	14·1	3578·15	3572·00	
160	0·08978	13·9	4646·30	4647·00	
170	0·09117	13·8	5953·40	5960·00	
180	0·09255	13·5	7534·72	7545·00	
190	0·09390	13·3	9429·20	9428·00	
200	0·09523	13·1	11672·30	11660·00	
210	0·09654	12·9	14309·90	14308·00	
220	0·09783	12·8	17386·00	17390·00	
230	0·09911		20937·40	20915·00	

If we give the general formula (4)

$$\log P = \log p + \log\frac{273+T}{273+t} + \frac{1}{\alpha}\log\frac{273+T}{273+t}$$

the following form—

$$\log P = \log p + \frac{1+\alpha}{\alpha}\log\frac{273+T}{273+t}, \quad \ldots\ldots \quad (6)$$

or

$$P = p10^{\frac{1+\alpha}{\alpha}\log\frac{273+T}{273+t}}, \quad \ldots\ldots\ldots\ldots \quad (7)$$

or, finally, as the first approximation,

$$P = p10^{\frac{1+\alpha}{\alpha}\cdot\frac{0\cdot4343\tau}{273+t}}, \quad \ldots\ldots\ldots\ldots \quad (8)$$

it assumes a great similarity to the empirical formula

$$e = 4^{\text{mm}}\cdot525 \times 10^{\frac{7\cdot4475\times t}{234\cdot69+t}},$$

on which Magnus* founded his calculations, as well as to the partly theoretical formulæ of Roche†, von Wrede‡, and Holzmann§, which collectively have the form $e = a\,.\,\alpha^{\frac{t}{m+nt}}$.

An essential peculiarity of formula (8) is, however, the separate introduction of the coefficient α, the variation of which with the temperature was unknown to the former authors.

The formula $P = p10^{\frac{1+\alpha}{\alpha}\log\frac{273+T}{273+t}}$ may be arranged more conveniently for use if we unite the numerator and the denominator of the expression $\frac{1+\alpha}{\alpha}$ in one coefficient a. We then have

$$\log P = \log p + a\log\frac{273+T}{273+t};$$

or, if the tension is to be determined in terms of atmospheric pressure, $p = 760$ millims. and

$$\log\frac{P}{p} = \log n = a\log\frac{273+T}{373}. \quad \ldots\ldots\ldots \quad (9)$$

The following Table gives the values of a between $-30°$ and $+230°$ C. by increments of 5°, supposing $p = 760$, and $t = 100$, and T to be counted from 0°:—

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* Poggendorff's *Annalen*, vol. lxi. p. 225. [Translated in Taylor's Scientific Memoirs, part xiv. p. 218.]

† *Ann. de Chim. et de Phys.* Jan. 1830.

‡ Poggendorff's *Annalen*, vol. liii. p. 225.

§ Poggendorff's *Annalen*, Supplementary Volume ii. p. 183. [Taylor's Scientific Memoirs, part xiv. p. 189.]

T°.	a.	pp.	T°.	a.	pp.
− 30	17·7983	495	100	13·3442	230
− 25	17·5508	481	105	13·2294	222
− 20	17·3105	462	110	13·1182	218
− 15	17·0797	444	115	13·0091	212
− 10	16·8580	432	120	12·9033	208
− 5	16·6421	415	125	12·7994	204
0	16·4345	404	130	12·6972	198
+ 5	16·2323	389	135	12·5982	192
10	16·0376	379	140	12·5022	189
15	15·8478	366	145	12·4077	186
20	15·6649	357	150	12·3150	178
25	15·4864	348	155	12·2259	175
30	15·3123	333	160	12·1383	172
35	15·1457	327	165	12·0521	167
40	14·9821	317	170	11·9685	165
45	14·8236	306	175	11·8861	162
50	14·6705	299	180	11·8050	158
55	14·5208	290	185	11·7260	154
60	14·3761	283	190	11·6492	151
65	14·2345	274	195	11·5739	147
70	14·0975	268	200	11·5005	144
75	13·9634	263	205	11·4285	141
80	13·8317	254	210	11·3580	138
85	13·7048	246	215	11·2891	135
90	13·5818	242	220	11·2218	133
95	13·4610	234	225	11·1554	131
100	13·3442		230	11·0898	

The numbers in the third column (*pp*) are proportional parts of one degree of the thermometer. Suppose, for example, we wish to calculate the tension at 6°·74; we multiply 389, the proportional number situated between 5° and 10°, by 1·74, the excess of 6°·74 over 5°. The product 677, being subtracted from 16·2323, gives us the desired value of $a=16·1646$. The corresponding tension is found =7·26 millims. Regnault obtained by observation 7·25, and by calculation from the formula which he adopted 7·36.

By means of an equation of the form

$$a=\mathrm{A}+\mathrm{B}\log n+\mathrm{C}(\log n)^2, \quad . \quad . \quad . \quad . \quad . \qquad (10)$$

wherein n expresses the tension in atmospheres, the values of a can be represented as functions of the pressure of the vapour. If we make

$$\mathrm{A}=13·3442,$$
$$\mathrm{B}=-1·493037, \qquad \log \mathrm{B}=0·1740705,$$
$$\mathrm{C}=-0·0513, \qquad \log \mathrm{C}=0·7101174-2,$$

the values of a between 100° and 230° are obtained with great accuracy. We thus possess ourselves of a tolerably simple mode of calculation for the determination of the temperature corresponding to any chosen tension of vapour, the formula (9), on account of the variable value of a, not being directly fitted for this purpose. Without employing formula (9), however, the temperature can now be derived from the tension by means of the values of a given in the Table.

Suppose we find n in equation (10) to be 1·5 atmosphere, the value $a=13{\cdot}0797$. The next higher number in the Table, 13·1182, corresponds to a temperature of 110°. The proportional number pertaining to it, in other words, the decrease of a for 1° C., is 218, and the difference is

$$13{\cdot}1182-13{\cdot}0797=0{\cdot}0385.$$

The ratio therefore is

$$218:385=1^\circ:1^\circ{\cdot}76.$$

The desired temperature is 111°·76, or 1°·76 higher than 110°.

The numbers representing the magnitudes of the tensions of aqueous vapour which Dulong and Arago* obtained from their observations in the year 1828 are in terms of atmospheric pressure. In the Table given by Regnault the tension is expressed in millims. of mercury. We are consequently unable to judge of the degree of agreement which exists between the old and the new measures. The following Table, wherein some of the numbers calculated from Regnault's observations are placed by the side of the corresponding ones of Dulong and Arago, may therefore be acceptable:—

Pressures in atmospheres of 0·76 metre of mercury.	a.	T° C.		
		R.	D. and A.	E.
1	13·3442	100·	100	100
2	12·8901	120·63	121·4	120·69
3	12·6202	133·93	135·1	134·18
4	12·4267	144·02	145·4	144·40
5	12·2755	152·25	153·1	152·76
6	12·1513	159·26	160·2	159·88
7	12·0458	165·39	166·5	166·10
8	11·9540	170·87	172·1	171·66
9	11·8728	175·83	177·1	176·68
10	11·7999	180·37	181·6	181·27
15	11·5173	198·87	200·5	199·90
20	11·3149	213·06	214·7	214·08
25	11·1568	224·75	226·3	225·65
30	11·0269	234·77	236·2	235·50

Column E. in the above Table is extracted from Dove's *Repertorium der Physik*†. The numbers contained therein were computed by means of an empirical formula which Egen ‡ developed,

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* Poggendorff's *Annalen*, vol. xviii. p. 437.

† Vol. i. p. 47.

‡ Poggendorff's *Annalen*, vol. xxvii. p. 9.

founding it on Dulong and Arago's original observations at temperatures above 100°. It corresponds with the experimental results of the above-named French academicians better than the formula which they themselves have used, and it may therefore be regarded as the best general expression of their experiments.

It will be seen that Dulong and Arago throughout their determinations found somewhat higher temperatures than Regnault; and this difference, constantly but very slowly increasing, amounts in the case of the greater tensions to even 1° C. This disparity, however, immediately appears far less considerable when it is remembered that Regnault's results refer to the air-thermometer, and those of Dulong and Arago, on the other hand, to the mercurial thermometer; moreover the results contained in the column to which D. and A. are affixed were made with a thermometer the bulb of which dipped into the boiling liquid, whilst the thermometer employed for the observations under E. was only surrounded by the vapour.

It is accordingly self-evident that the numbers in both columns must have been somewhat greater than the corresponding results referred to the air-thermometer, and indeed in about the ratio in which they have actually been found. Regnault, during his very numerous experiments on the tension of aqueous vapour at high temperatures, took the precaution to employ the air-thermometer side by side with the mercurial thermometer.

Some of his observations allow of a good comparison with similar ones of Dulong and Arago, made at nearly the same temperatures, and taken from their original papers. They are as follow:—

T°.	D. and A. P millims.	T°.	R. P millims.	Millims.
149·6	3475·9	149·46	3479·31	9·5
168·4	5605·4	168·01	5554·83	14·0
169·5	5773·7	169·36	5757·34	14·2
180·7	7500·1	180·50	7459·12	17·5
193·7	9998·9	193·88	10001·64	21·8
		193·79	9979·79	
209·1	13769·0	209·61	13789·33	28·2
218·4	16381·4	218·67	16364·59	32·2
220·6	17182·6	220·21	16886·00	33·2

The numbers in the last column indicate the effect of a change in temperature of 0°·1 on the height of the column of pressure. It will be perceived that the differences in these columns of figures do not exceed the limits of unavoidable errors of observation with the mercurial thermometer. Regnault's observations not only, in consequence of their great number, control and confirm one another, but are also, from their very close agreement with the determinations of the older academicians, of even greater value.

If we were entitled to employ the equation

$$\alpha = 0{\cdot}06479 + 0{\cdot}0001722\,T - 0{\cdot}0000001\,T^2$$

beyond the limits of the experiments from which it was derived, it would follow that α has a maximum at 861° which corresponds to the value $\alpha = 0{\cdot}13892$. This maximum, however, is not in the least degree probable. The increase of temperature resulting from $\frac{1}{273+t}$ compression of a volume of aqueous vapour in a completely gaseous condition, measured at $t°$, amounts to 0·277°*; and it is to be presumed that with increasing temperatures α approaches this limiting value in the same degree as the influence of cohesion decreases. If observations were made on the tension of aqueous vapour at temperatures far above 230°, it would probably be found that a fourth positive term would enter the above equation, wherein T would be raised to the third power.

The numbers calculated for α are the mean values between 100°, and the chosen temperatures T above or below 100°.

If the melting-point of ice instead of the boiling-point of water were to be taken as the starting-point for comparison, it would then be necessary to make t equal to 0°, and $p = 4{\cdot}5$ millims. in the equation

$$\log \alpha = \frac{\log (273+T) \log (273+t)}{\log (273+t)P - \log (273+T)p}.$$

The values thus found can be represented between 0° and 100° by the equation

$$\alpha' = 0{\cdot}05279 + 0{\cdot}000131\,T - 0{\cdot}00000011\,T^2.$$

If we neglect that term in the above equation which contains the second power of T, as of trifling importance within the limits indicated, the formula then signifies that aqueous vapour, saturated at 0°, cannot be converted into such at higher temperatures by compression, unless we impart heat to it from without in the ratio 0·000131 T to 0·05279, or of 0·248 T to 100. In order, for example, to produce saturated aqueous vapour at 100°, besides the 100 parts of heat set free by compression, 24·8 parts of heat must be supplied from without.

This variation in the quantity of heat which it contains at different temperatures, explains why the relations between the temperature and the tension of aqueous vapour at different degrees of maximum density cannot be represented in so simple a manner as the same relations in the case of the permanent gases.

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* *Ann. der Chem. und Pharm.* vol. cxv. p. 312.

The great exactness with which the tension of aqueous vapour has been experimentally determined throughout a wide range of temperature allows of our submitting the applicability of the formula

$$P=p\,.\,10^{\frac{1+\alpha}{\alpha}\log\frac{273+T}{273+t}}$$

to a rigorous test. Its employment, however, is not limited to this case; for the theoretical considerations which led to the use of this formula for the determination of the tension of aqueous vapour apply with equal force to the vapours of other liquids. I have only investigated the comportment of saturated vapours of alcohol, ether, sulphurous acid, and ammonia in this respect.

The Tables given by Regnault in *Comptes Rendus*, vol. l. p. 1663, furnished me with the data for the determination of the values of α.

The tension of saturated alcohol-vapour is given in these Tables from $-20°$ to $+155°$, increasing in each case by $5°$. Using these numbers of Regnault's, and taking the temperature (τ) at 100° C., the mean values of α were calculated from the formula

$$\alpha=0{\cdot}0843+0{\cdot}00019\,\tau+0{\cdot}00000032\,\tau^2,$$

or, on counting the temperature from 0°, from the formula

$$\alpha=0{\cdot}0685+0{\cdot}000126\,T+0{\cdot}00000032\,T^2.$$

At 100°, P is assumed $=1694{\cdot}92$ millims.; for other tensions, therefore,

$$P^{mm}=1694{\cdot}92\times 10^{\frac{1+\alpha}{\alpha}\log\frac{273+T}{373}}.$$

The results of these calculations are embodied in the following Table, the column R. of which contains the tensions that Regnault calculated from his experiments.

T°.	α.	*pp.*	P.	R.	*dd.*
− 20	0·06611	11·6	3·24	3·34	− 0·10
− 10	0·06727	12·3	6·63	6·58	+ 0·05
0	0·06850	12·9	13·02	12·83	+ 0·19
+ 10	0·06979	13·6	24·60	24·30	+ 0·30
20	0·07115	14·2	44·75	44·48	+ 0·27
30	0·07257	14·8	78·61	78·49	+ 0·12
40	0·07405	15·5	133·12	133·64	− 0·52
50	0·07560	16·1	218·82	219·88	− 1·06
60	0·07721	16·8	348·38	350·26	− 1·88
70	0·07889	17·4	538·73	541·21	− 2·48
80	0·08063	18·0	809·66	812·76	− 3·10
90	0·08243	18·7	1187·13	1188·43	− 1·30
100	0·08430	19·3	1694·92	1694·92	0·00
110	0·08623	20·0	2366·95	2361·63	+ 5·32
120	0·08823	20·6	3227·60	3219·68	+ 7·92
130	0·09029	21·2	4314·10	4301·04	+13·06
140	0·09241	21·9	5651·10	5634·00	+17·10
150	0·09460		7265·40	7258·73	+ 6·67
155	0·09572		8168·60	8185·02	−16·42

The differences are not more considerable than would be expected, and in no case exceed those which exist between these recently obtained numbers and those deduced from the earlier observations of Regnault*. A difference of 0°·1 in temperature signifies a variation of 0·2 millim. in the tension of alcohol-vapour at temperatures between 10° and 20°, 3 millims. between 70° and 80°, and as much as 15 millims. at a temperature of about 140°. With the exception of a few cases, the differences between the two columns of numbers are not greater than that which corresponds to 0°·1 of temperature. A still greater accordance would have been obtained if I had not assumed that the tension at 100° was determined with the greatest accuracy, and that the boiling-point of alcohol must be somewhat higher than it is according to Regnault's numbers. According to the latter it is below 78°·3, whilst the most accurate determinations have given 78°·4.

The preceding formulæ furnish the following numbers:—

T°.	P millims.
78·4	759·968
78·401	760·000
78·5	763·019

From the numbers given for the tensions of ether-vapour I obtain the following simple expression for α:

$$\alpha = 0{\cdot}09243 + 0{\cdot}000215\,T;$$

wherein T is counted from 0°, and the tension corresponding to a temperature of 35° is assumed to be 763·27 millims. It will be seen by the following Table that the tensions which have been computed from the above accord very well at low temperatures. At high temperatures they show a somewhat greater disparity; with the exception of a single case, however, it does not exceed that which corresponds to a difference of 0°·1 of temperature. In what manner the difference which appears at 120°, and which corresponds to an error of 0°·3 of observation, will render necessary another determination of the value of α, can only be decided by comparison with the original observations. A comparison of the new Table with the earlier one communicated by Regnault, only justifies the conclusion that there were difficulties to be overcome in the experiments which prevented the determination of the higher tensions of ether-vapour with the same accuracy as those of lower temperatures.

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* Poggendorff's *Annalen*, vol. xliii. p. 540.

The following Table contains the tensions which have been observed by Regnault; those under R′. being his earlier, and those under R″. his later determinations:—

T°.	α.	P.	R′.	R″.
− 20	0·08813	67·27	67·49	69·2
− 10	0·09028	113·33	113·35	113·2
0	0·09243	183·43	183·34	182·3
+ 10	0·09458	286·56	286·40	286·5
20	0·09673	433·38	433·26	434·8
30	0·09888	636·30	636·33	637·0
40	0·10103	909·62	909·59	913·6
50	0·10318	1269·03	1271·12	1268·0
60	0·10533	1731·17	1728·52	1730·3
70	0·10748	2313·84	2307·81	2309·5
80	0·10963	3034·79	3024·41	2947·2
90	0·11178	3912·06	3898·05	3899·0
100	0·11393	4963·13	4950·81	4920·4
110	0·11608	6204·54	6208·37	6249·0
120	0·11823	7652·65	7702·20	

According to calculation, 34°·8 corresponds to a tension of 757·831 millims., and 34°·9 to one of 760·547 millims. The boiling-point of ether is consequently 34°·88 C.

Regnault determined the tension of liquid sulphurous acid between −25° and +65° C. In accordance with his Table we take the tension at 0°=1165·06, and calculate from this,

$$\alpha = 0{\cdot}099180 + 0{\cdot}000267\,T - 0{\cdot}00000034\,T^2,$$

$$P = 0{\cdot}63019 + \log(273+T) + \frac{\log(273+T) - 2{\cdot}43616}{\alpha}.$$

T°.	α.	*pp.*	P.	R.	*dd.*
−25	0·09229	28·2	373·85	373·79	+0·06
−20	0·09370	27·8	479·41	479·46	−0·05
−10	0·09648	27·0	762·49	762·49	0·00
0	0·09918	26·4	1165·06	1165·06	0·00
+10	0·10182	25·6	1719·84	1719·55	+0·29
20	0·10438	25·2	2461·90	2462·05	−0·15
25	0·10564	24·8	2915·30	2915·97	−0·67
30	0·10688	24·4	3429·80	3431·80	−2·00
40	0·10932	23·6	4665·80	4670·23	−4·43
50	0·11168	23·0	6214·30	6220·01	−5·71
60	0·11398	22·4	8122·30	8123·80	−1·50
65	0·11510		9227·20	9221·40	+5·80

The tension at −10° is 762·49 millims.; at −10°·1 we find it to be 759·02 millims.; the boiling-point of liquid sulphurous acid is −10°·072.

The determination of the value of α for the vapour of liquid ammonia led to less concordant results; this would be expected after what Regnault* has stated in the extract from his memoir about the difficulty which attends the accurate determination of the temperature of boiling ammonia.

I made use of the formulæ

$$\alpha = 0{\cdot}10605 + 0{\cdot}0002287\,T + 0{\cdot}0000003\,T^2$$

and

$$\log P = 1{\cdot}06392 + \log\,(273+T) + \frac{\log\,(273+T) - 2{\cdot}43616}{\alpha}.$$

The tensions calculated by means of the above equation differ considerably from Regnault's numbers; it will be easily seen, however, by the following Table that these differences are in no case so great as that which corresponds to 0°·3 C. in the determination of temperature:—

T°.	α.	P.	R.	*dd.*
− 40	0·09738	530·60	528·61	+ 1·99
− 30	0·09946	873·52	876·58	− 3·06
− 20	0·10159	1387·30	1397·74	− 10·44
− 10·1	......	2118·10		
− 10	0·10379	2127·20	2149·52	− 22·32
− 5	0·10491	2603·26	2632·25	− 28·99
0	0·10605	3162·87	3162·87	00·00
+ 5	0·10720	3815·00	3854·47	− 39·47
5·1	......	3829·08		
10	0·10836	4570·30	4612·19	− 41·89
20	0·11074	6428·40	6467·00	− 38·60
30	0·11318	8820·20	8832·20	− 12·00
40	0·11567	11825·90	11776·42	+ 49·48
40·1	......	11859·00		

The foregoing examples may suffice to prove the superiority of the formula

$$P = p\,10^{\frac{1+\alpha}{\alpha}\log\frac{273+T}{273+t}}$$

to the numerous empirical formulæ which have hitherto been employed for the calculation of the tensions of vapours. We are not, however, always in a position to directly determine by means of this equation the maximum tension of a vapour at different temperatures. Certain constants must still be determined empirically. But these determinations, since they refer to very small differences, are much more easily made after it has been esta-

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* Poggendorff's *Annalen*, vol. cxi. pp. 405 and 415.

blished that the deviations in the relations between the temperature and the tension of saturated vapours and the analogous relations of permanent gases rest essentially on the fact that the quantity of heat contained in vapours increases with the temperature.

The similarity of the formulæ which have been constructed for the calculation of the tensions of vapours of different liquids approaches identity, if we, in order to ascertain the coefficient $a=\frac{1+\alpha}{\alpha}$, start with the boiling-point $t°$ of each liquid, and the corresponding tension of 760 millims. We thus obtain the equation

$$P^{mm}=760\times10^{a\log\frac{273+t+\tau}{273+t}},$$

wherein, for equal distances τ above or below the boiling-point of different liquids, the logarithmic factor remains, it is true, not exactly the same, but, with decreasing boiling-points, very gradually increases in the first case and decreases in the other. The corresponding values of a generally change in the opposite direction, whereby the logarithm $a\log\frac{273+t+\tau}{273+t}$ remains constant for small differences of τ.

If this were exactly the case, different liquids at temperatures equidistant above or below from their respective boiling-points would have the same tension of vapour,—an experimental conclusion which was first expressed by Dalton.

II. *On a new Gas-Furnace for Experimental, Analytical, and other purposes.* By G. Gore, *Esq.*[*]

BEING engaged some time ago in an investigation upon certain properties of fused substances, I required a furnace by means of which I could obtain a temperature equal to the fusing-point of cast iron, and have the fused substances perfectly accessible to be manipulated upon for a continuous and lengthened period of time without contact with impurities or with the atmosphere, and without lowering their temperature sufficiently to cause them to solidify. I wished also to obtain these conditions by means of ordinary coal-gas and atmospheric air, without the use of a bellows, blowing apparatus, or tall chimney, or the aid of valves requiring frequent attention.

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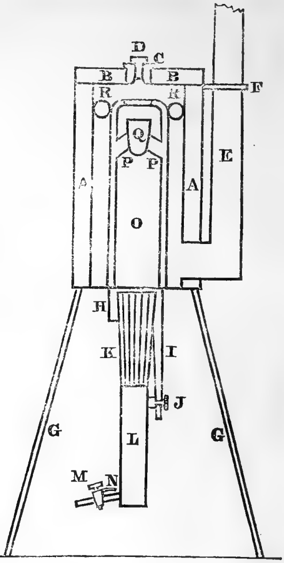
[*] Communicated by the Author.



After making a considerable number of furnaces of different kinds, I at length found an arrangement possessing almost completely all the qualifications desired. It is as follows:—A is a cylinder of fireclay about 9 inches high and 6 inches diameter, open at both ends, and with a hole in its side near the bottom to lead into the chimney; it is covered by a moveable plate of fireclay, B, with a hole in its centre for introduction of the crucible or of substances to be melted: this hole is closed by a perforated plug of fireclay, C, for access to the contents of the crucible; and that again is closed by another stopper of clay, D. E is a chimney of sheet iron about 5 or 6 feet high, kept upright by a ring of iron, F, attached to the top of the furnace.

The fireclay cylinder is enclosed in a sheet-iron casing with a bottom of iron, to which are fixed three iron legs, G. An iron tube, H, with a prolongation, I, supports by means of the screw J, the burner K and its tube L, which is open at both ends. Gas is supplied to the burner by means of the tap, M, which has a small index, N, attached to it for assistance in adjusting the gas.

Inside the larger cylinder is another fireclay cylinder or cupola, O, with open ends, and with three projections of fireclay, P, for supporting the crucible Q; it is kept steady by means of three clay marbles, R.

The gas-burner is a thin metal cylinder deeply corrugated at its upper end, with the corrugations diminishing to nothing at its lower end.

The action of this furnace is as follows:—Gas is admitted to the open tube L by the tap M; it there mixes with air to form a nearly explosive mixture, which ascends through the burner and burns in the clay cylinder O, being supplied with the remainder of air necessary to complete combustion through the tube H to the outer surface of the flame by means of the spaces between the corrugations. The flame and products of combustion pass up through the cylinder O, and then downwards outside it to the chimney, the point of greatest heat being at Q. It is important in using this furnace that the burner is placed quite in the centre of the bottom of the tube O; also that a crucible of not too large nor too small dimensions be selected. The most suitable way of supporting a smaller crucible is by placing it in a larger one that has had its upper parts broken off. If desirable, a little clay luting may be placed round the top edge of the iron casing to exclude air entering between it and the cylinder; also a little thin clay luting upon the part of the bottom of the furnace where the inner cylinder rests.

In lighting the furnace, the plugs C and D are removed, a light held within the opening, and the gas turned on full; should the flame blow down to the bottom of the tube L on lighting (which, however, rarely occurs unless the furnace is already hot), the gas must be turned off and the bottom end of L momentarily closed whilst lighting the gas as before. Should the flame not burn down to the burner, but only burn at the orifice in the clay plate B, it must *at once* be extinguished and relighted, otherwise some of the gaseous mixture will pass into the chimney unburned, and subsequently ignite and cause an explosion. A large flame now issues from the top orifice, and is white if too much gas is on, and violet or red with the proper quantity. This flame generally diminishes and nearly disappears into the body of the furnace in the course of one minute: whether it does or not, the annular plug C should now be inserted, which will compel it to pass downwards to the chimney; and as soon as the small remaining flame disappears or nearly disappears, as it will in a few seconds, the small stopper D should also be inserted; instead of this the large flame may be deflected against the chimney by means of a shoot or bent piece of sheet iron until it withdraws inwards; the plugs C and D may then be reinserted and the gas-tap partly adjusted. The crucible may be placed in the furnace either before or some time after the act of lighting, but not *immediately* after (if the furnace is cold), or explosions may occur by unburned gaseous mixture passing the crucible into the chimney and igniting afterwards.

After about five minutes the gas should be adjusted until a sound is heard inside like a series of small explosions. This sound is sometimes not very distinct, especially at high temperatures, and therefore requires a little experience in the use of the furnace in order to be detected; it is, however, a chief guide in determining the proper amount of gas, and should therefore be carefully studied. To assist in adjusting the gas, it will be found very useful to place a small piece of looking-glass beneath the tube L, and to adjust the gas-tap until the flame between the burner and crucible appears wholly violet or slightly white; but this test is liable to fallacy if employed when the gas is just lighted, because the coldness of the parts makes the flame much whiter than it otherwise would be; it is also fallacious when the crucible is very hot, the flame appearing whiter than it really is; it is, however, of great assistance at intermediate temperatures. A rough deposit upon the outer edge of the crucible indicates an excess of gas; the deposit is carbon.

Less gas is required with a crucible in the furnace than without one; also less is necessary when the small hole at the top of the furnace is open than when it is closed; and less is also required when the furnace is cold than after it has been lighted some time, because the draught gradually increases and draws in more air. After having accurately adjusted the gas, no further attention to the furnace is requisite.

Having once found the proper adjustment of gas under certain known circumstances, it is well to notice the position of the index N, in order to be able at once to adjust it to the right point on other occasions. Under ordinary circumstances, during daylight I have found it best to set the gas nearly fully on at first, and fully on at about five minutes afterwards when the draught has become more powerful; but during twilight, when the supply of gas from the gas-works is more free, I have set the index-pointer at the numbers 2½ or 3. The gas should be supplied by a pipe of not less than ⅜ths of an inch bore, with a main pipe of half an inch; but all depends upon the pressure of gas at the particular locality, which is very variable. The consumption of gas varies from 30 to 40 cubic feet per hour, the cost of which is about twopence.

The top of the chimney should be placed in a position where the products of combustion can pass freely away; if it is placed in an opening or pipe leading to another chimney, care must be taken not to have the draught too powerful, otherwise the heat will be drawn more into the chimney, and the supply of gas in the daytime may be found rather deficient. The furnace will act satisfactorily, though less powerfully, with the chimney standing in an open room without any special outlet for the products of combustion, provided the full height (6 feet) of chimney is employed; under other circumstances I have generally used a chimney 4½ or 5 feet in height.

This furnace will readily melt half a pound of copper or six ounces of cast iron. I have melted those quantities in it: it will melt as large a quantity of those substances as the largest-sized crucible that can be introduced into it will contain, sufficient space being allowed around the crucible for draught. It requires from 20 to 30 minutes to acquire its highest temperature; *and then the entrance part of the chimney exhibits a faint red heat in day-*

light; if it exhibits much more than this, the draught is too powerful.

With one ounce of copper put into the cold furnace, and the gas lighted and properly adjusted, the copper generally begins to melt at about the tenth or twelfth minute, and is completely melted by the fifteenth. With the heat well up, one ounce of copper has been melted in it in 2¼ minutes, one ounce of cast iron in 3 minutes, five ounces of copper in 4¼ minutes, and three ounces of cast iron in 5 minutes. With the smaller hole in the top of the furnace open, one ounce of copper has been melted in 3½ minutes; and several ounces of copper have been kept in fusion for upwards of half an hour, and may be kept so for any length of time: cast iron has also been fused and kept melted under the same conditions. These various effects have also been obtained in a somewhat diminished degree with the chimney standing in an open room.

When the small hole D is open, some air is drawn in that way, and less air passes up with the gas through the tube O; but this cold air does not much diminish the temperature of the crucible, because it combines with the excess of gas now passing over the edge of the inner cylinder; it however renders the flame round the crucible white by deficiency of air, and this should be partly corrected by lessening the amount of gas. An excess either of air or gas renders the surface of melted copper dull.

When it is desirable to perfectly avoid the contact of air with the fused substance during manipulation, a thin and narrow ring of fireclay should be placed upon the top of the tube O to contract its opening; the flame then completely closes over the top of the crucible and prevents access of air. A proper adjustment of gas, together with exclusion of air in this manner, enables a perfectly bright surface of melted copper, or even tin, to be continuously maintained, from which the images of the parts above are clearly reflected. The clay ring may be withdrawn by lifting the plate B. A less perfect exclusion of air may be obtained by employing a narrow crucible placed rather low down in its support. A small iron dish should be placed beneath the tube L, to receive any melted substance that may fall. The furnace is protected by letters patent, and may be obtained of the maker, E. W. Ball, 11 Islington, Birmingham. The *chief* conditions of success in the use of this furnace are, *sufficient gas, proper regulation of gas to air, and a suitable degree of draught.*

Note.—I beg to acknowledge the assistance I have received from Mr. Thomas Simons, crucible-maker, Northwood Street, Birmingham, in the production of suitable clay cylinders, &c. for this furnace.

III. *Note on the Problem of Pedal Curves.*

By A. Cayley, *Esq.**

IT is not, so far as I am aware, generally known that the problem of pedal curves (Steiner's *Fusspuncten-Curve*) was considered by Maclaurin in the *Geometria Organica*, 1720. He appears to have been led to it through an idea such as Sir W. R. Hamilton's *Hodograph*, or at any rate with a view to a dynamical application, for he remarks, p. 95, "Cum vero geometria quæ curvas ad datum centrum relatas contemplatur in philosophia naturali ad motus corporum et vires evolvendas facilius applicari possit, . . . hac sectione considerabimus curvas tanquam ad punctum quodvis datum relatas ex quo ad omnia circumferentiæ puncta radii undique educuntur, et simul perpendicula in illorum punctorum tangentes demittuntur, et rationem radii ad perpendiculum tanquam curvæ characterem usurpabimus." And accordingly, Props. IX. to XII., he considers the problem, viz., Given a point S in the plane of a given curve, to find the locus of the intersection of a tangent of the curve by the perpendicular let fall upon it from the point S; with some special cases, and deductions from it. In particular if the given curve be a circle, the locus in question or pedal curve is a curve of the fourth order having a double point at S; viz. if S be inside the circle, this is a conjugate or isolated point; but if outside, a double point with two real branches; if S be on the circle, then instead of the double point we have a cusp: it is shown that in each case the pedal curve is in fact an epicycloid. If the given curve be a parabola, then the locus or pedal curve is a curve of the third order, viz. a defective hyperbola having a double point at S, and with its single asymptote perpendicular to the axis of the parabola: some particular cases are specially noticed. If the curve be an ellipse or hyperbola, then, as in the case of the circle, the locus or pedal curve is a curve of the fourth order having a double point at S. And it is moreover shown, Prop. XII., that for any given curve whatever the locus or pedal curve is, in a generalized sense of the term, an epicycloid. This is in fact seen very easily by a mere inspection of the figure. Imagine the curve $O'P'$, rigidly connected with and carrying along with it the point S', to roll on the similar and equal fixed curve OP symmetrically situate on the other side of the common tangent OM or OM'; then when P' coincides with P, the point S' is brought to S'', where $SNN'S''$ is the perpendicular from S on the tangent PN or PN', and $SN=N'S''$, that is, $SS''=2SN$; and the curve generated by S'' (that is S'), or say the epicycloid

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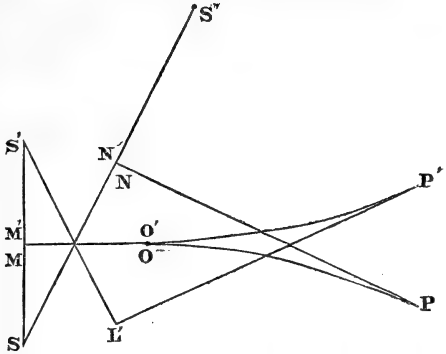
* **Communicated by the Author.**

the locus of S', is a curve similar to and similarly situate with the pedal curve the locus of N, but of twice the linear magnitude of the pedal curve. Or, what is the same thing, if instead of the given curve we consider a similar and similarly situated curve of twice the linear magnitude (the point S being the centre of similitude), then the epicycloid the locus of S' is the pedal curve of the substituted curve in relation to the point S. It may be added that, in accordance with a theorem of Dandelin's, if rays proceeding from the point S are reflected at the given curve, then the epicycloid or pedal in question is the *secondary caustic*, or an orthogonal trajectory of the reflected rays.



2 Stone Buildings, W.C.,
June 3, 1863.

IV. *On the Diathermancy of Dry and Moist Air.*
By Professor MAGNUS*.

[With a Plate.]

IT is with regret that I find myself obliged to return to a subject the importance of which is not sufficient to warrant a renewed treatment of it. But inasmuch as the method which I have employed for determining the transmission of heat through gaseous bodies has been attacked, and since it has been asserted that it is incapable of yielding trustworthy results, I felt it my duty to submit it to still further examination; especially since, during my stay in London last autumn, Prof. Tyndall had the kindness to show me some experiments, made according to his method, which were apparently at variance with my own.

It is known that a difference exists between the results which

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* Translated from the *Monatsber. der königl. preuss. Akad. der Wissensch. zu Berlin* (Sitzung vom 19 März, 1863), with additions from Poggendorff's *Annalen*, vol. cxviii. p. 575.

Prof. Tyndall obtained for the absorption of radiant heat and those which I found myself. It is true that both of us, independently of each other, and following perfectly distinct methods, obtained for nearly all gases values which are as concordant as can be expected from measurements of this kind, although I obtained a greater value than Prof Tyndall for the absorption by dry air compared with a vacuum. Between our observations for air saturated with moisture at the common temperature there is, however, a great disagreement. For while I have observed only a very small difference between the power of transmitting heat possessed by dry air and that possessed by moist air, Prof. Tyndall* finds the absorption by moist air so great that, the absorption of dry air being taken as unity, that by the undried air of the laboratory was—

On the 23rd of October, 1861 . . . . =63
„ 24th „ . . . . . =62
„ 29th „ . . . . . =65
„ 31st „ . . . . . =56
„ 1st of November . . . . . =50
„ 4th „ . . . . . =58
„ 8th „ . . . . . =49
„ 12th „ . . . . . =62

Prof. Tyndall employs in his method two sources of heat which are placed opposite the two faces of the thermo-pile furnished with its conical reflectors, and they are adjusted until both faces of the pile receive an equal amount of heat, and the galvanometer is consequently not deflected. For this purpose there is placed in front of one of the sources of heat, which we may call the compensator, and which consists of a cube filled with boiling water, a screen made of two parallel tin plates, by moving which the quantity of heat which falls upon one face of the pile can be so regulated as to be always equal to that which reaches the opposite face.

Between the thermo-pile and the principal source of heat, which consists of a plate of copper heated to about 300° C. by a gas-flame, is placed the experimental tube, which can be closed at both ends with plates of rock-salt, and can then be employed either vacuous or filled with the gases or vapours to be examined. It has, however, also been used by Prof. Tyndall for comparing the absorption by dry and moist air without using the closing plates, the two kinds of air having been allowed to flow into the tube at one end, while the other end was connected with an air-pump. By this latter method Prof. Tyndall obtained a deflection of 30° of his galvanometer when he allowed moist air to flow

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* Phil. Trans. 1862, p. 89. [Phil. Mag. vol. xxiv. p. 426.]

through the tube; and, on the other hand, dry air brought the needle back to zero*.

This experiment appeared to me more decisive than any of those in which the rock-salt plates were used, and I therefore owe Prof. Tyndall my best thanks for his courtesy in showing it to me. The needle swung through 30° or 40°; but whether on leading dry or moist air through the tube I do not remember; and I further omitted to notice which of the two sources of heat had the preponderance. The result of this experiment was so surprising, and so little in accordance with what I had found by other processes, that when I reached home I determined to repeat it. In my earlier experiments I had used an astatic needle-galvanometer, because the reflecting galvanometer, as commonly constructed, has too great a directive force to be deflected sufficiently by a weak current like that of the thermo-pile. The needle-galvanometer, however, has the defect that its indications are proportional to the strength of the current only when the deflections do not exceed a few degrees; I have therefore increased the sensibility of the reflecting galvanometer by adding to it a second magnet, exactly similar to the first, and connected with it on the principle of astatic needles, so as to diminish the directive force of the instrument.

Fig. 1, Plate I. shows the galvanometer thus constructed. It consists of two parallel coils *g g*, each containing 94 turns of wire; *a* is a circular piece of steel placed inside the coils, and serving at once as mirror and as magnet. Between it and the coils there is a copper ring *k k*, 60 millims. broad and 30 millims. thick, which acts as a damper. This ring carries a small tube *c s*, which passes up between the coils, and upon which is fastened the cylindrical brass vessel *q q*, containing a second circular magnet *b*, of exactly the same size as the mirror *a*, and firmly connected with it by the wire *a b*, so that both are in the same plane, and have their magnetic axes horizontal but in opposite directions. They hang by a cocoon-fibre R R, 0·3 metre in length. In order to be able to put them in their places, after they have been rendered astatic to the proper degree outside the galvanometer, the copper ring *k k* is cut through vertically to the extent of half its thickness. With so thick a ring this cutting does not injure its action as a damper. It is plain that this system of mirrors ought not to be quite astatic; it is, however, only necessary to leave it just so much directive force that it shall always come back to the direction of the magnetic meridian. Its position of equilibrium does not remain constant. Independently of other disturbing causes, such as the torsion of the fibre, &c., the daily variations of the earth's magnetism occasion alterations

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* Phil. Trans. 1862, p. 92. [Phil. Mag. vol. xxiv. p. 430.]

of its position. For if the telescope, with the scale fixed below it, is placed at 2 metres distance from the mirror, every alteration of one millimetre in the position of the image corresponds to a change of 51 seconds of arc in the position of the mirror; consequently the daily variations in the intensity of the earth's magnetism, amounting as they do to several minutes, cause a very perceptible variation in the direction of the mirror. No harm, however, can arise from this, so long as no observations are taken as belonging to the same series except such as are made in close sequence, and if it is also borne in mind that the observed deflections have not an absolute, but only a relative value. Nevertheless they are always proportional to the intensities of the current, even when they amount to one or two hundred millimetres. This proportionality, together with the certainty ensured by reading off through a telescope, make this instrument specially fitted for thermo-electrical experiments.

A galvanometer of this construction was used in repeating Prof. Tyndall's experiments. As sources of heat I used two blackened vessels filled with boiling water, which, in order to avoid the disturbing effect of flames, was kept boiling by means of steam. In order to hinder the accidental cooling of the vessels, each of them was surrounded by a pasteboard box, in which the only opening, besides that through which the rays of heat escaped, was a small one in the cover. The screen of the compensating source was placed inside its box. The boiler for producing the steam was outside the boxes. The rays from both sources passed through tubes, open at both ends, to the thermopile furnished on both sides with its conical reflectors.

The experimental tube was 0·66 metre long, and had an opening in the side near each end. One of these openings was connected with an air-pump; and through the other, air was pressed into the tube by means of a pair of bellows. As in Prof. Tyndall's experiments, this air could be made to pass through several tubes containing chloride of calcium and broken glass moistened with sulphuric acid, or through tubes containing broken glass moistened with water, before it entered the experimental tube, and could in this way be used either dry or saturated with moisture.

With this arrangement I got, on allowing dry or moist air to flow through the tube, deflections of the galvanometer which corresponded to those described by Prof. Tyndall. But I did not always get them; and what particularly surprised me was, that the deflection of the needle did not correspond to an absorption of heat by its passage through moist air, but that, on the contrary, when moist air was passed through the tube, the face of the pile which was turned towards the tube was found to be most heated. In order to clear up the already mentioned uncertainty of the experiment, I have repeated the blowing in of dry and moist air alternately many hundred times; but in no single case was the deflection such as to indicate a greater absorption by moist air.

It would be out of place to relate the numerous experiments which were undertaken, partly in order to make myself master of the phenomena, and partly in order to explain the surprising contradiction between my results and the conclusions which Prof. Tyndall has drawn from his experiments. I found, in the first place, that the deflection took place only when the air was driven in with a certain amount of force. It was found, further, that when the air was pressed in continuously, the deflection of the galvanometer was not maintained constant, but that the instrument gradually returned to its position of equilibrium. Hence it resulted that the air did not cause the deflection by absorption. I suspected that possibly moisture might be condensed on the internal surface of the tube, and that a heating effect might be thus produced; but this supposition was likewise found to be erroneous. It appears, on the other hand, that the phenomenon is occasioned by an absorption which takes place at the surface of the pile itself.

In fact, when air is blown in at the side of the tube, the greater part of it escapes at the end nearest the hole through which it enters, but the air also escapes from the tube at the opposite end. This can be seen very distinctly by bringing a small flame in front of the open ends of the tube. If the air-pump is connected with the side of the tube near the other end, it causes the air to escape with less force, or not quite continuously; nevertheless, if the pressure under which the air enters the tube is sufficient to cause any of it to escape at the other end, the air-pump is incapable of altogether counteracting the motion, its action being neither sufficiently continuous nor sufficiently powerful. The air which issues from the tube continues its motion in the same direction, and thus reaches, first, the conical reflector, and then the pile itself, even when the latter is at a considerable distance.

Now, if the air is saturated with moisture, it appears that water is condensed on the surface of the pile; a heating effect is thus produced, and the galvanometer is deflected. If, however, the air is dry, it takes up from the pile the moisture previously condensed upon it, and thus an evaporation and consequent cooling is occasioned.

This explanation at once shows why the galvanometer gradually returns to its position of equilibrium when an unbroken current of saturated air is kept up. For when so much aqueous vapour has been condensed that no further condensation can take place, there can then be no further production of heat, and the heat which was developed at the beginning of the absorption is now gradually given up again. In like manner, when dry air is blown into the tube, evaporation ceases as soon as all the moisture is removed; and since there is no further cooling, the pile gradually acquires the temperature of the surrounding medium, even when dry air is forced into the tube in a continuous stream.

When the current of either kind of air is stopped, the galvanometer is deflected in the opposite direction, because, supposing moist air to have been used, the water now evaporates again, or if dry air has been blown in, the water is again absorbed.

It is doubtless scarcely necessary to say that the evaporation and absorption take place more rapidly, and that hence the deflections of the galvanometer are greater, in proportion as the pressure is increased under which the air is blown into the experimental tube, and also that the deflections are greater when the air is blown into the end of the tube nearest to the thermo-pile.

If the deflections of the galvanometer are due to the condensation and evaporation of watery vapour, they ought to occur when the source of heat is altogether absent. This was found to be really the case, and it was thus proved that absorption of the rays of heat has nothing to do with the phenomena.

The deflections were, however, much greater when the tube was taken away altogether and the air was blown directly into the reflector of the thermo-pile, or upon the pile itself. The mirror of the galvanometer mentioned above was moved so far that the scale was no longer visible, and, with the astatic needle galvanometer which I formerly used, the needle was so violently deflected as to be sent quite up to the stop.

It is unnecessary to say that the air must of course have the same temperature as the pile.

On removing the lampblack from the face of the pile, perfectly analogous but less violent deflections were obtained. In like manner, similar effects were observed when the pile was coated with substances of different kinds.

We see by these results how little fitted air is, while in motion, for experiments as to its power of absorption.

I would gladly pass over the objections which Prof. Tyndall has urged against my method of determining the absorption of radiant heat by gaseous substances*, did I not fear that my silence would be interpreted as an admission that the objections in question are well founded—an admission I am by no means prepared to make.

In particular, it has been objected to this method, that the

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* Poggendorff's *Annalen*, vol. cxii. p. 516. [Phil. Mag. vol. xxii. p. 1.]

gas to be examined is brought into immediate contact with the source of heat. The latter, according to Prof. Tyndall, is thereby cooled; and this cooling is said to take place at different rates with the various gases and with a vacuum. In support of this assertion, Prof. Tyndall quotes an experiment* in which, his "front chamber" being full of air, it produced a much smaller effect on the thermo-pile than when exhausted of air, the state in which it was commonly employed.

This front chamber, however, consists of a horizontal cylinder which is heated at one end, while the entire cylinder is surrounded with water in order to keep it cold. Under such circumstances it is plain that no one could doubt that a circulation of air must arise, and that this would produce a cooling effect. In my apparatus the heat is applied from above—an arrangement which precludes the formation of internal air-currents, unless indeed a lateral cooling or heating should also take place. The essential thing about the apparatus is the heating from above, and accordingly this ought not to be lost sight of in estimating its merits.

I have modified somewhat the arrangement of the apparatus which I formerly employed, and which I described in Poggendorff's *Annalen*, vol. cxii. p. 516. By reference to fig. 2, Plate I. it will be easily understood. I am thus enabled to construct the tube R R, which serves to contain the gas under examination, out of the several glass tubes R A, B C, D R, and to divide it into sections of various lengths by interposing plates of rock-salt, or of any other diathermanous material. Each section can be pumped out separately and filled with whatever gas may be desired. Moreover, such gases as would injure the air-pump can be passed through any particular section so as to displace the air contained in it. The lower part, E F, of the tube is somewhat wider than the rest, and contains the thermo-pile *a c* provided with its conical reflector *a b*. The conducting wires of the thermo-pile pass insulated through the brass plate F F, which closes the bottom of the tube. The portions which project from this plate are surrounded with caoutchouc, as is also the whole length of the wires which pass through the water M N whereby the lower part of the apparatus is surrounded, and which is kept constantly at 15° C.

In order to be able to unite easily the separate tubes, they are provided at each end with a brass mounting carrying a projecting rim. These rims being ground perfectly true and smeared with a very little grease, the tubes are placed one on the top of another; a strong piece of brass shaped like a horse-shoe is then pushed over each mounting so as to press against the projecting rim, and by now screwing together each pair of these horse-shoe pieces

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* Phil. Trans. 1862, p. 93. [Phil. Mag. vol. xxiv. p. 430.]

by means of three screws, the rims of the brass mountings are held firmly together. When it is required to interpose a plate of rock-salt, it is placed between two tubes the rims of which have been very slightly covered with grease. It is of course also needful that the plate should have been ground quite true and parallel. In order to be able to apply a screen inside the tube, the four-cornered brass box P P is interposed. It contains two flaps, made of double tin-plate, and moveable about axles which pass air-tight through the sides of the box and project on the outside. By means of these axles, the flaps can be opened from without and placed vertically so that they let all the rays of heat pass. They can be closed in the same way—that is, brought to the horizontal position, in which case no heat can pass through.

The upper section, S S, was 0·15 metre high. When this section was cut off from the rest of the tube by the interposition of a plate of rock-salt and was used alternately full of air and vacuous, the rest of the tube, B F, below the rock-salt plate being all the time filled with dry air, the effect on the thermo-pile was perfectly identical in the two cases. I have made this experiment repeatedly. It refutes, as it seems to me, Prof. Tyndall's assertion that the air to be examined must not be allowed to come in contact with the source of heat. When the air is below the heated surface, the latter can only be cooled by conduction; but, as I have shown, the conducting-power of all the gases except hydrogen is so small that less heat traverses a space filled with gas than would traverse a vacuum; and hence it follows that the effect of conduction in the case of the gases is so small as to be inappreciable in comparison with the effect of athermancy. The only possible exception to this would be in the case of hydrogen. But even here, the cooling which results from conduction is so trifling as to be of no consequence whatever when the experiments are made with a vessel kept constantly at the temperature of boiling water by means of steam led into it.

Prof. Tyndall* disputes the conduction of heat by hydrogen. The grounds upon which he does so are not quite intelligible to me; but the fact which is alone conclusive as to the conducting-power of this gas, he has left entirely unnoticed. This fact is, that, as already stated, heat applied from above traverses a space filled with any of the other gases less easily than it traverses a vacuum: hydrogen alone transmits more heat than is transmitted by a vacuum. Since, however, hydrogen does not allow more rays of heat to pass than atmospheric air, and since, moreover, even when its motion is hindered by means of eider-down, heat is propagated through it more easily than through a vacuum or

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* Phil. Trans. 1862, p. 96.

through the other gases, this superior propagation of heat can only be a consequence of conduction.

Another objection which Prof. Tyndall urges against my apparatus is, that the pile is applied inside the experimental tube*. He quotes an experiment in which he had cemented a thermopile into the side of a tube, so that one side of it was inside the tube and the other outside. When the air was now pumped out, a very considerable current was produced. This result was what might have been foreseen. But I may add that even when the pile is entirely within the tube a current is produced on pumping out the air, because the two sides of the pile and the neighbouring parts of the apparatus are not cooled to the same extent. The current, however, disappears entirely after a little time, especially if the part of the tube containing the pile is surrounded with water of constant temperature, as was always the case in the apparatus that I used.

When the pile is situated entirely outside the experimental tube, the latter is cooled on being pumped out, just as though the pile were in it; this cooling, however, exerts a scarcely perceptible effect on the pile applied externally, which proves that the delicacy of the apparatus is now much less than it is when the pile is placed entirely inside the tube. This inferior degree of delicacy arises from the fact that the rock-salt plate, whereby the tube is closed, absorbs, in proportion to its thickness and transparency, a not inconsiderable amount of heat. When two rock-salt plates are used, as in Prof. Tyndall's apparatus, the delicacy of the apparatus becomes still less.

By using two plates of rock-salt I have, even with my own apparatus, found scarcely a perceptible difference in the amount of heat transmitted across a vacuum and across a space filled with dry air, a result which quite accords with Prof. Tyndall's statements; when, however, the experiment was made without rock-salt plates, the difference was quite decided. By the use of the galvanometer mentioned above, I have convinced myself that it is not quite so great as I had previously stated†; but it amounts at any rate to several per cent.

For all gases, except atmospheric air, in the dry and moist states, Prof. Tyndall's values agree with my own as nearly as measurements of this kind could be expected to do. Hence the method I have employed is at all events not so faulty as Prof. Tyndall reproaches it with being; for were his objections well founded, the determinations of all the gases must have been wrong. There must consequently be some other cause which specially affects the determination of the absorptive power of

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* Phil. Mag. S. 4. vol. xxiii. p. 263.

† Poggendorff's *Annalen*, vol. cxii. p. 524. [Phil. Mag. vol. xxii. p. 93.]

moist air. There seems to be no doubt that this cause is to be sought in the employment of the rock-salt plates.

When the absorption by dry air was compared with that by air saturated with moisture without using rock-salt plates, a small difference was found between them, but always less than 1 per cent. When rock-salt plates were employed, the difference was considerably greater, and when moist air was passed through the tube for a longer time, it attained a value similar to what I had obtained in previous experiments*.

Besides the defect arising from the hygroscopic character of the rock-salt plates, Prof. Tyndall's method labours under another difficulty. The absorptive powers of the various gases were measured by him by first adjusting the compensating source of heat so that the radiation through the exhausted experimental tube produced no deflection of the galvanometer, and then determining the deflection caused by dry air. The value thus obtained for dry air formed the unit for the determination of the other gases, all of which were compared in the same way with the vacuum. Therefore the smaller the observed difference between dry atmospheric air and the vacuum, the greater the apparent absorptive power of the other gases. Hence, if this difference were to be equal to nothing, the absorption by the other gases would come out infinitely great.

In the method which I have employed, the determinations are altogether independent of any comparison between an exhausted space and a space filled with air. For the deflection caused by atmospheric air is observed first each time, and then that caused by the gas under examination or by the exhausted tube; so that each time the values obtained for both under precisely similar circumstances are compared together.

V. *On the Relation of Radiant Heat to Aqueous Vapour.*

By JOHN TYNDALL, *F.R.S. &c.*†

I HAVE already placed before the Royal Society an account of some experiments which brought to light the remarkable fact that the body of our atmosphere, that is to say the mixture of oxygen and nitrogen of which it is composed, is a comparative vacuum to the calorific rays, its main absorbent constituent being the aqueous vapour which it contains. It is very important that the minds of meteorologists should be set at rest on this subject —that they should be able to apply, without misgiving, this newly revealed physical property of aqueous vapour; for it is

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* Poggendorff's *Annalen*, vol. cxiv. p. 635. [Phil. Mag. vol. xxiii. p. 249.]

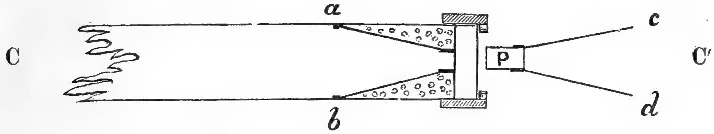
† From the Philosophical Transactions, Part I. for 1863, having been read at the Royal Society December 18, 1862.

certain to have numerous and important applications. I therefore thought it right to commence my investigations this year with a fresh series of experiments upon atmospheric vapour, and I now have the honour to lay the results of these experiments before the Royal Society.

Rock-salt is a hygroscopic substance. If we breathe on a polished surface of rock-salt, the affinity of the substance for the moisture of the breath causes the latter to spread over it in a film which exhibits brilliantly the colours of thin plates. The zones of colour shrink and finally disappear as the moisture evaporates. Visitors to the International Exhibition may have witnessed how moist were the pieces of rock-salt exhibited in the Austrian and Hungarian Courts. This property of the substance has been referred to by Professor Magnus as a possible cause of error in my researches on aqueous vapour; a film of brine deposited on the surface of the salt would produce the effect which I had ascribed to the aqueous vapour. I will, in the first place, describe a method of experiment by which even an inexperienced operator may avoid all inconvenience of this kind.

In the Plate which accompanies my former paper, the thermo-electric pile is figured with two conical reflectors, both outside the experimental tube; in my present experiments the reflector which faced the experimental tube is placed *within the latter*, its narrow aperture, which usually embraces the pile, abutting against the plate of rock-salt which stops the tube. Fig. 1 is a sketch of this end of the experimental tube. The edge of the inner reflector fits tightly against the interior surface of the tube at $a\,b$; $c\,d$ is the diameter of the wide end of the outer reflector, supposed to be turned towards the "compensating cube" situated at C′*. The naked face of the pile P is turned towards the plate of salt, being separated from the latter by an interval of about $\frac{1}{20}$th of an inch. The space between the outer surface of the interior reflector and the inner surface of the experimental tube is filled with fragments of freshly-fused chloride of calcium,

Fig. 1.



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* I here assume an acquaintance with my two last contributions to the Philosophical Transactions, in which the method of compensation is described. [These memoirs will be found in vols. xxii. and xxiv. of the Philosophical Magazine.]

intended to keep the circumferential portions of the plate of salt perfectly dry. The flux of heat coming from the source C being converged upon the central portion of the salt, completely chases every trace of humidity from the surface on which it falls.

With this arrangement I repeated all my former experiments on humid and dry air. The result was the same as before. *On a day of average humidity the quantity of vapour diffused in London air produced upwards of* 60 *times the absorption of the air itself.*

It has been suggested to me that the air of our laboratory might be impure; the suspended carbon particles in a London atmosphere have also been mentioned to me as a possible cause of the absorption which I had ascribed to aqueous vapour. With regard to the first objection, I may say that the same results were obtained when the apparatus was removed to a large room at a distance from the laboratory; and with regard to the second cause of doubt, I met it by procuring air from the following places:—

1. Hyde Park.
2. Primrose Hill.
3. Hampstead Heath.
4. Epsom race-course.
5. A field near Newport, Isle of Wight.
6. St. Catharine's Down, Isle of Wight.
7. The sea-beach near Black Gang Chine.

The aqueous vapour of the air from these localities exerted absorptions from 60 *to* 70 *times that of the air in which the vapour was diffused.*

I then purposely experimented with smoke, by carrying air through a receiver in which ignited brown paper had been permitted to smoulder for a time, and drying it afterwards. It was easy, of course, in this way to intercept the calorific rays; but, confining myself to the lengths of air actually experimented on, I convinced myself that, even when the east wind blows, and pours the carbon of the city upon the west end of London, the heat intercepted by the suspended carbon particles is but a minute fraction of that absorbed by the aqueous vapour.

Further, I purified the air of the laboratory so well that its absorption was less than unity; the purified air was then conducted through two U-tubes filled with fragments of clean glass moistened with distilled water. Its neutrality when dry proved that all prejudicial substances had been removed from the air; and in passing through the U-tubes it could have contracted nothing save the pure vapour of water. *The vapour thus carried into the experimental tube exerted an absorption* 90 *times as great as that of the air which carried it.*

I have had the pleasure of showing the experiments on atmospheric aqueous vapour to several distinguished men, and among others to Professor Magnus. After operating with common undried air, which showed its usual absorption, and while the undried air remained in the experimental tube, I removed the plates of rock-salt from the tube and submitted them to the inspection of my friend. They were as dry as polished rock-crystal or polished glass; their polish was undimmed by humidity; and a dry handkerchief placed over the finger and drawn across the plates left no trace behind it*.

I would make one additional remark on the above experiments. A reference to the plate which accompanies my two last papers will show the thermo-electric pile standing, with its two conical reflectors, at some little distance from the end of the experimental tube. Hence, to reach the pile after it had quitted the tube, the heat had to pass through a length of air somewhat greater than the depth of the reflector. It has been suggested to me that the calorific rays may be entirely sifted in this interval—that all rays capable of being absorbed by air may be absorbed in the space of air intervening between the experimental tube and the adjacent face of the pile. If this were the case, then the filling of the experimental tube itself with dry air would produce no sensible absorption. Thus, it was imagined, the neutrality of dry air which my experiments revealed might be accounted for, and the difference between myself and Professor Magnus, who obtained an absorption of 12 per cent. for dry air, explained. But I think the hypothesis is disposed of by the foregoing experiments; for here the reflector which separated the pile from the tube no longer intervenes, and it cannot be supposed that in an interval of $\frac{1}{20}$th of an inch of air an absorption of 12 per cent. has taken place. If, however, a doubt on this point should exist, I can state that I have purposely sent radiant heat through an interval of 24 inches of dry air previous to permitting it to enter my experimental tube, and found the effects to be the same as when the beam had traversed 24 inches of a vacuum.

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* The present Number of the *Monatsbericht* of the Academy of Berlin contains an account of some experiments executed with plates of rock-salt by Professor Magnus. The plates which stopped the ends of a tube were so far wetted by humid air that the moisture trickled from them in drops. As might be expected, the plates thus wetted cut off a large amount of heat. The experiments are quite correct, but they have no bearing on my results. In the earlier portions of my journal many similar cases are described. In fact, it is by making myself, in the first place, acquainted with the anomalies adduced by Professor Magnus, that I have been able to render my results secure. I may add that the communication above referred to was made to the Academy of Berlin before my friend had an opportunity of examining my rock-salt plates. I do not think he would now urge this objection against my mode of experiment.

In confirmation of the results obtained when my tube was stopped by plates of rock-salt, I have recently made the following experiments with a tube in which no plates were used. S is the source of heat, and S T the front chamber which is usually kept exhausted, being connected with the experimental tube at T. This chamber is now left open. A B is the experimental tube, with both its ends also open. P is the thermo-electric pile, the anterior face of which receives rays from the source S, while its posterior surface is warmed by the rays from the compensating cube C′. At *c* and *d* are two stopcocks—that at *c* being connected with an india-rubber bag containing air, while that at *d* is connected with an air-pump.

My aim in this arrangement was to introduce at pleasure, into the portion of the tube between *c* and *d*, dry air, the common laboratory air, or air artificially moistened. The point *c*, at which the air entered, was 18 inches from the source S; the point *d*, at which the air was withdrawn, was 12 inches from the face of the pile. By adopting these dimensions, and thus isolating the central portion of the tube, one kind of air may with ease and certainty be displaced by another without producing any agitation either at the source on the one hand, or at the pile on the other.

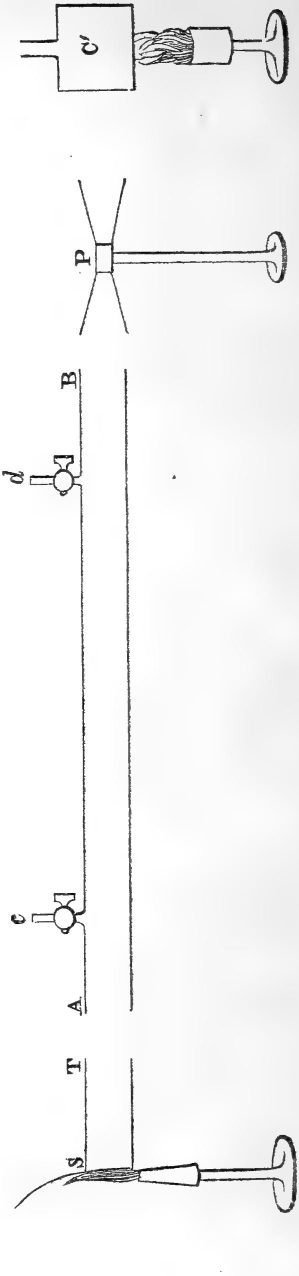

Fig. 2.

The tube A B being filled by the common air of the laboratory, and the needle of the galvanometer pointing steadily to zero, dry air was forced gently from the india-rubber bag through the cock *c*; the pump was gently worked at the same time, the dry air being thus gradually drawn towards *d*. On the entrance of the dry air, the needle commenced to move in a direction which showed that a greater quantity of heat was now passing through the tube than before. The dry air proved more transparent than the common air, and the final deflection thus obtained was 41 degrees. Here the needle stopped, and beyond this point it could not be moved by the further entrance of dry air.

Shutting off the india-rubber bag and stopping the action of the pump, the apparatus was abandoned to itself; the needle returned with great slowness to zero, thus indicating a correspondingly slow diffusion of the aqueous moisture through the dry air within the tube. By working the pump the descent of the needle was hastened, and it finally came to rest at zero.

Dry air was again admitted; the needle moved as before, and reached a final limit of 41 degrees; common air was again substituted, and the needle descended to zero.

The tube being filled with the common air of the laboratory, which was not quite saturated, and the needle pointing to zero, air from the india-rubber bag was now forced through two U-tubes filled with fragments of glass wetted with distilled water. The common air was thus displaced by air more fully charged with vapour. The needle moved in a direction which indicated augmented absorption; the deflection obtained in this way was 15 degrees.

I have repeated these experiments hundreds of times, and on days widely distant from each other. I have also subjected them to the criticism of various eminent men, and altered the conditions in accordance with their suggestions. The result has been invariable. The entrance of each kind of air is always accompanied by its characteristic action. The needle is under the most complete control, its motions are steady and uniform. In short, no experiments hitherto made with solids and liquids are more free from caprice, or more certain in their execution, than are the foregoing experiments with dry and humid air.

The quantity of heat absorbed in the above experiments, expressed in hundredths of the total radiation, was found by screening off one of the sources of heat, and determining the full deflection produced by the other and equal source.

By a careful calibration, repeatedly verified, this deflection was proved to correspond to 1200 units of heat,—the unit being, as before, the quantity of heat necessary to move the needle of the galvanometer from 0° to 1°. According to the same standard, a deflection of 41° corresponds to an absorption of 50 units. From these data we immediately calculate the number of rays per hundred absorbed by the aqueous vapour,

$$1200 : 100 = 50 : 4{\cdot}2.$$

An absorption of 4·2 per cent. was therefore effected by the atmospheric vapour which occupied the tube between the points *c* and *d*. Air *perfectly saturated* on the day in question gave an absorption of $5\frac{1}{2}$ per cent.

These results were obtained in the month of September, and on the 27th of October I determined the absorption of aqueous vapour with the above tube when stopped with plates of rock-salt. Three successive experiments gave the deflections produced by the aqueous vapour as $46^\circ{\cdot}6$, $46^\circ{\cdot}4$, $46^\circ{\cdot}8$. Of this concurrent character are all the experiments on the aqueous vapour of the air. The absorption corresponding to the mean deflection here is 66. The total radiation through the exhausted tube was on this day 1085; hence we have

$$1085 : 100 = 66 : 6{\cdot}1;$$

that is to say, the absorption of the aqueous vapour of the air contained in a tube 4 feet long, was on this day 6 per cent. of the total radiation.

The tube with which these experiments were made was of brass, polished within; and it was suggested to me that the vapour of the moist air might have precipitated itself on the interior surface of the tube, thus diminishing its reflective power, and producing an effect apparently the same as absorption. In reply to this objection, I would remark that the air on many of the days on which my experiments were made was at least 25 per cent. under its point of saturation. It can hardly be supposed that air in this condition would deposit its vapour upon a polished metallic surface, against which, moreover, the rays from our source of heat were impinging. More than this, the absorption was exerted even when only a small fraction of an atmospere was made use of, and found to be proportional to the quantity of atmospheric vapour present in the tube. The following Table shows the absorptions of humid air at tensions varying from 5 to 30 inches:—

Tensions in inches.	Absorption.	
	Observed.	Calculated.
5	16	16
10	32	32
15	49	48
20	64	64
25	82	80
30	98	96

The third column here is calculated on the assumption that the absorption, within the limits of the experiment, is sensibly proportional to the quantity of matter in the tube. The agreement with observation is almost perfect. It cannot be supposed that results so regular as these, agreeing so completely with those obtained with small quantities of other vapours, and even with small quantities of the permanent gases, can be due to the condensation of vapour on the surface of the tube. When 5 inches were in the tube it had less than one-sixth of the quantity of vapour necessary to saturate the space. Condensation under these circumstances is not to be assumed, and more especially a condensation which should produce such regular effects as those above recorded.

The subject, however, is so important that I thought it worth while to make the following additional experiments:—

C (fig. 3) is a cube of boiling water, intended for our source of heat; Y is a hollow brass cylinder, 3·5 inches in diameter and 7·5 inches in depth; P is the thermo-electric pile, and C′ the compensating cube; S is an adjusting screen, used to regulate the amount of heat falling on the posterior surface of the pile. The apparatus was entirely surrounded by boards, the space within being divided by tin screens into compartments which were loosely stuffed with paper or horsehair. The formation of air-currents near the cubes or the pile was thus prevented, and irregular motions of the external air were intercepted. A roof, moreover, was bent over the pile, and this was flanked by sheets of tin. The action here sought I knew must be small, and hence the necessity of excluding every disturbing influence.

The cylinder Y was first filled with fragments of quartz moistened with distilled water. A rose burner *r* was placed at the bottom of the cylinder, and from it the tube *t* led to a bag containing air. The bag being subjected to gentle pressure, the air passed upwards amid the fragments of quartz, imbibing moisture from them, and finally discharged itself in the open space between the cube C and the pile. The needle moved and assumed a permanent deflection of 5 degrees, indicating that the opacity of the intervening space to the rays of heat was augmented by the discharge of the saturated air.

The moist quartz fragments were now removed, and the vessel Y was filled with fragments of the chloride of calcium. The rose burner being, as before, connected with the india-rubber bag, air was gently forced up among the calcium fragments and discharged in front of the pile. The needle moved and assumed a permanent deflection of 10 degrees, indicating that the transparency of the space between the pile and source was augmented by the presence of the dry air. By timing the discharges the swing of the needle could be augmented to 20 degrees. Repetition showed no deviation from this result: the saturated air always augmented the opacity, and the dry air always augmented the transparency of the space between the source and the pile.

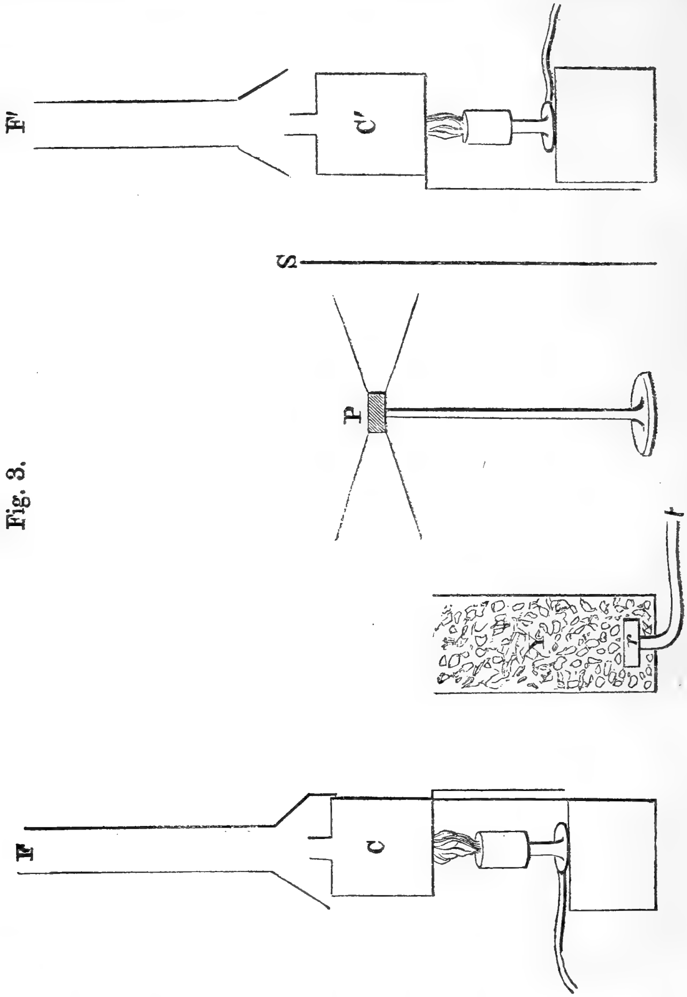

Fig. 3.

Not only, therefore, have the plates of rock-salt been abandoned, but also the experimental tube itself, the displacement between dry and humid air being effected in the open atmosphere. The experi-

ments are all perfectly concurrent as regards the action of the aqueous vapour upon radiant heat.

The power of aqueous vapour being thus established, meteorologists may, I think, apply the result without fear. That 10 per cent. of the entire terrestrial radiation is absorbed by the aqueous vapour which exists within ten feet of the earth's surface on a day of average humidity, is a moderate estimate. In warm weather and air approaching to saturation, the absorption would probably be considerably greater. This single fact at once suggests the importance of the established action as regards meteorology. I am persuaded that by means of it many difficulties will be solved, and many familiar effects, which we pass over without sufficient scrutiny because they are familiar, will have a novel interest attached to them by their connexion with the action of aqueous vapour on radiant heat. While leaving these applications to be made in all their fullness by meteorologists, I would refer, by way of illustration, to one or two points on which I think the experiments bear.

And first it is to be remarked that the vapour which absorbs heat thus greedily radiates it very copiously. This fact must, I think, come powerfully into play in the tropical region of calms, where enormous quantities of vapour are raised by the sun, and discharged in deluges upon the earth. This has been assigned to the chilling consequent on the rarefaction of the ascending air. But if we consider the amount of heat liberated in the formation of those falling torrents, the chilling due to rarefaction will hardly account for the entire precipitation. The substance quits the earth as vapour, it returns to it as water; how has the latent heat of the vapour been disposed of? It has in great part, I think, been radiated into space. But the radiation which disposes of such enormous quantities of heat subsequent to condensation, is competent, in some measure at least, to dispose of the heat possessed prior to condensation, and must therefore hasten the act of condensation itself. Saturated air near the surface of the sea is in circumstances totally different from those in which it finds itself in the higher atmospheric regions. Aqueous vapour is a powerful radiant, but it is an equally powerful absorbent, and its absorbent power is a maximum when the body which radiates into it is vapour like itself. Hence, when the vapour first quits the equatorial ocean and ascends, it finds, for a time, a mass of vapour above it, into which it pours its heat, and by which that heat is intercepted and in part returned. Condensation in the lower regions of the atmosphere is thereby prevented. But as the mass ascends it passes through successive vapour-strata which diminish far more speedily in density than the associated strata of air, until finally our ascending body of vapour finds itself lifted above the screen which for a time protected it. It now radiates freely into space, and condensation is the necessary consequence. The heat liberated by condensation is, in its turn, spent in space, and the mass thus deprived of its potential energy returns to the earth as water. To what precise extent this power of aqueous vapour as a radiant comes into play as a promoter of condensation, I will not now inquire; but it must be influential in producing the torrents which are so characteristic of the tropics.

The same remarks apply to the formation of cumuli in our own latitudes. They are the heads of columnar bodies of vapour which rise from the earth's surface and are condensed to cloud at a certain elevation. Thus the visible cloud forms the capital of an invisible pillar of saturated air. Certainly the top of the column, piercing the sea of vapour which hugs the earth, and offering itself to space, must lose heat by the radiation from its vapour, and in this act alone we should have the necessity for condensation. The "vapour plane" must also depend, to a greater or less extent, on the chilling effects of radiation.

The action of mountains as condensers must, I think, be connected with these considerations. When a moist wind encounters a mountain-range it is tilted upwards, and condensation is no doubt to some extent due to the work performed by the expanding air; but the other cause cannot be neglected; for the air not only performs work, but it is lifted to a region where its vapour can freely lose its heat by radiation into space. During the absence of wet winds the mountains themselves also lose their heat by radiation, and are thus prepared for actual surface condensation. We must indeed take into account the fact that this radiant quality of water is persistent throughout its three states of aggregation. As vapour it loses its heat and promotes condensation; as water it loses its heat and promotes congelation; as solid it loses its heat and renders the surfaces on which it rests more powerful refrigerators than they would otherwise be. The formation of a cloud before the air which contains it *touches* a cold mountain, and indeed the formation of a cloud anywhere over a cold tract of land, where the cloud is caused by the cold of the tract, is due to the radiation from the aqueous vapour. The uniformly diffused fogs which sometimes fill the atmosphere in still weather may be due to cold generated by uniform radiation throughout the mass, and not to the mixture of currents of different temperatures. The cloud by which the tract of the Nile and Ganges (and sometimes the rivers of our own country) may be followed on a clear morning is, I believe, due to the chilling of the saturated air above the river by radiation from its vapour.

Observation proves the radiation to augment as we ascend a mountain. Martins and Bravais, for example, found the lowering of a radiation-thermometer 5°·7 Cent. at Chamouni; while on the Grand Plateau, under the same conditions, it was 13°·4 Cent. The following remarkable passage from Hooker's Himalayan Journals, 1st edit. vol. ii. p. 407, bears directly upon this point:—"From a multitude of desultory observations I conclude that, at 7400 feet, 125°·7 or 67° above the temperature of the air, is the average maximum effect of the sun's rays on a black-bulb thermometer. . . . . . These results, though greatly above those obtained at Calcutta, are not much, if at all, above what may be observed on the plains of India [because of the dryness of the air.—J. T.]. The effect is much increased with the elevation. At 10,000 feet, in December, at 9 A.M. I saw the mercury mount to 132° [in the sun], with a difference [above the shaded air] of 94°, while the temperature of shaded snow hard by was 22°. At 13,100 feet, in January, at 9 A.M. it has stood at 98°, with a difference of 68°·2, and at 10 A.M. at 114°, with a difference of 81°·4, *whilst the radiating thermometer on the snow had fallen at sunrise to* 0°·7." This enormous chilling is fully accounted for by the absence of aqueous vapour overhead. I never under any circumstances suffered so much from heat as in descending on a sunny day from the so-called Corridor to the Grand Plateau of Mont Blanc. The air was perfectly still, and the sun literally blazed against my companion and myself. We were hip deep in snow; still the heat was unendurable. Immersion in the shadow of the Dôme du Goûté soon restored our powers, though the *air* of the shade was not sensibly colder than that through which the sunbeams passed. Notwithstanding the enormous daily accession of heat from the sun, terrestrial radiation at these altitudes preserves an extremely low temperature at the earth's surface.

Without quitting Europe we find places where, even when the day temperature is high, the hour before sunrise is intensely cold. I have often experienced this even in Germany; and the Hungarian peasants, if exposed at night, take care, even in hot weather, to prepare for the nocturnal chill. The *range* of temperature augments with the dryness, and an "excessive climate" is certainly in part caused by the absence of aqueous vapour.

Regarding Central Australia, Mr. Mitchell publishes extremely valuable tables of observations, from which we learn that, when the days are at the same time calm and clear, the daily thermometric range is exceedingly large. The temperature at noon being 68° on the 2nd of March 1835, that at sunrise next morning was 20°, showing a difference of 48°. The 7th and 8th were also clear and calm; the difference between noon and sunrise on the former day was 38°, while on the latter it was 41°. Indeed between April and September a range of 40° in clear weather was quite common—or more than double the amount which it is in London at the corresponding season of the year.

A freedom of escape similar to that from bodies at great elevations would occur at any other level, were the vapour removed from the air above it. Hence the withdrawal of the sun from any region over which the atmosphere is dry must be followed by quick refrigeration. This is simply an *à priori* conclusion from the facts established by experiment; but, I believe, all the experience of meteorology confirms it. The winters in Tibet are almost unendurable from this cause. The isothermals dip deeply from the north into Central Asia during the winter, the earth's heat being wasted without impediment in space, and no sun existing sufficiently powerful to make good the loss. I believe the fact is well established that the desert of Sahara, which during the day is burning hot, is often extremely cold at night. This effect has been hitherto referred in a general way to the "purity of the air;" but purity, as judged by the eye, is a very imperfect test of radiation, for the existence of large quantities of vapour is consistent with a transparent atmosphere. The purity really consists in the absence of aqueous vapour from those so-called rainless districts, which, when the sun is withdrawn, enables the hot surface of the earth to run speedily down to a freezing temperature.

On the most serene days the atmosphere may be charged with vapour; in the Alps, for example, it often happens that skies of extraordinary clearness are the harbingers of rain. On such days, no matter how pure the air may seem to the eye, terrestrial radiation is arrested. And here we have the simple explanation of an interesting fact noticed by Sir John Leslie, which has remained without explanation up to the present time. This eminent experimenter devised a modification of his differential thermometer, which he called an *Æthrioscope.* The instrument consisted of two bulbs united by a vertical tube, of a bore small enough to retain a little liquid index by its own adhesion. The lower bulb was protected by a metallic coating; the upper or sentient bulb was blackened, and was placed in the concavity of a polished metal cup, which protected it completely from terrestrial radiation. "This instrument," says its inventor, "will at all times during the day and night indicate an impression of cold shot downwards from the higher regions. . . . . But the cause of its variations does not always appear so obvious. Under a fine blue sky the *Æthrioscope* will sometimes indicate a cold of 50 millesimal degrees; yet on other days, *when the air is equally bright*, the effect is hardly 30°." It is, I think, certain that these anomalies were due to differences in the amount of aqueous vapour in the air, which escaped the sense of vision. Leslie himself connects the effect with aqueous vapour by the following remark :—"The pressure [apparently a misprint for *presence*] of hygrometric moisture in the air probably affects the indications of the instrument." In fact, the absence or presence of moisture opened or closed an invisible door for radiation from the "sentient bulb" of the instrument into space. The following observation in reference to radiation-experiments with Pouillet's pyrheliometer, now receives its explanation. "In making such experiments," says M. Schlagintweit, "deviations in the transparency are often recognized which are totally inappreciable to the telescope or the naked eyes, but which afterwards announce themselves in the presence of thin clouds," &c.

In his beautiful essay on Dew, Wells gives the true explanation of the formation of ice in India, by ascribing the effect to radiation. I think, however, his theory needs supplementing. Given the same day-temperature here as at Benares, could we, even in clear weather, obtain a sufficient fall of temperature to produce ice ? I think not. The interception of the calorific rays by our humid air would too much retard the chill. It is apparent, from the descriptions given of the process, that a dry still air was the most favourable for the formation of the ice. The nights when it was formed in greatest abundance were those during which the dew was not copious. The flat pans used in the process were placed on dry straw, and if the straw became wetted it was necessary to have it removed. Wells accounts for this by saying that the wetted straw is more dense than the dry, and hence more competent to transfer heat from the earth to the basins. This may be to some extent true; but it is also certain that the evaporation from the moist straw, by throwing over the pans an atmosphere of aqueous vapour, would check the radiation and thus tend to diminish the cold.

Melloni, in his excellent paper "On the Nocturnal Radiation of Bodies," gives a theory of the *serein*, or excessively fine rain which sometimes falls in a clear sky a few moments after sunset. Several authors, he says, attribute this effect to the cold resulting from radiation of the air, during the fine season, immediately on the departure of the sun. "But," writes Melloni, "as no fact is yet known which distinctly proves the emissive power of pure transparent elastic fluids, it appears to me more conformable to the principles of natural philosophy to attribute this species of rain to the radiation and subsequent condensation of a thin veil of vesicular vapour distributed through the higher strata of the atmosphere"*. Now, however, that the power of aqueous vapour

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* Taylor's Scientific Memoirs, vol. v. p. 551.

as a radiant is known, the difficulty experienced by Melloni disappears. The former hypothesis, however, though probably correct in ascribing the effect to radiation, was incorrect in ascribing it to the radiation of "*the air.*"

Dr. Hooker encourages me to hope that this newly discovered action may throw some light on the formation of hail. The wildest and vaguest theories are afloat upon this subject. But the same action which produces *serein* must, if augmented, freeze the minute rain, and the aggregation of the small particles thus frozen would form hail. I cannot think the hail that I have had an opportunity of examining to be due to the freezing of drops of water, each hailstone being merely the ice of the drop. The "stones" are granular aggregates, the components of which may, I think, be produced by the chill of radiation. I will not, however, dwell further on this subject, but will now commit the entire question to those who are more specially qualified for its investigation.

VI. *On the Passage of Radiant Heat through Dry and Humid Air.* *By* John Tyndall, *F.R.S., &c.*[*]

It is known to the readers of the Philosophical Magazine, that Professor Magnus and myself have arrived at different conclusions regarding the action of dry air, and of the aqueous vapour diffused throughout our atmosphere, on radiant heat. Last autumn I had the pleasure of meeting my eminent friend in London; and soon after his arrival it was agreed upon between us to subject the points on which we differed to a searching examination. We accordingly met on several occasions in the laboratory of the Royal Institution, where every result that I had previously announced was reproduced in the presence of Professor Magnus. Facts were placed before him which he professed his inability to explain; but, like a cautious philosopher, he reserved his opinion. It was, however, proved that the results observed by us in common could not be ascribed to any defect of method or error of observation which it was then possible to point out. I wished very much to subject the most recent experiments of Professor Magnus to a similar examination, and he evinced an equal desire to show them to me. He began his arrangements, but it was not my good fortune to see them accomplished. In fact, coming to London as a visitor to the International Exhibition, the numerous other claims upon his time and attention were amply sufficient to prevent him from carrying out his own wishes and gratifying mine.

In the latest Number of Poggendorff's *Annalen,* Professor Magnus has published a paper "On the Diathermancy of Dry

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[*] Communicated by the Author.

and Moist Air," a translation of which is printed in this Number of the Philosophical Magazine. From it I learn that the experiments on atmospheric vapour which struck him most were those performed with a tube the ends of which were not stopped by plates of rock-salt. The results obtained with this tube were so opposed to those obtained in another way by himself, that he returned to Berlin resolved to repeat my experiments. The paper just referred to contains an account of his researches, and an explanation of my results.

Operating with an open tube, he displaced by means of a pair of bellows dry air by moist and moist air by dry, and obtained, though not always, deflections corresponding to mine. But he was particularly surprised to find that the direction in which the needle moved when moist air was blown into the tube, indicated, not a withdrawal of heat from the thermo-electric pile, but an augmentation of heat. When dry air was forced into the tube, the deflection observed did not indicate that a greater amount of heat fell upon the pile, but, on the contrary, that the pile was chilled. He explains these effects by reference to the absorption of aqueous vapour by the lampblack which coated the face of his pile. This absorption, when moist air was blown against the instrument, rendered heat free; when dry air, on the other hand, was forced against it, the evaporation of the condensed vapour chilled the pile, and the deflection due to cold was observed. From all this it is to be inferred that in my experiments I have mistaken cold for heat, and heat for cold, and have ascribed to absorption effects which are really due to the condensation and evaporation of aqueous moisture at the surface of my thermo-electric pile.

To commit such an error, and to persist in it so long, would, I fear, leave me little claim to confidence as an experimenter. But the truth is that some years have elapsed since I became acquainted with the facts now urged against me by Professor Magnus. Experimenting years ago on dry and moist air with tubes which had been coated inside by lampblack or lined with blackened paper, I found, when moist air was introduced, the radiation from the interior surface so energetic as to compel me to abandon the coating. The promptness and energy with which these effects of condensation and evaporation are produced are remarkable. Dry air urged against the face of my pile on a day of average humidity drives the needle of my galvanometer through an arc of 196 degrees, and keeps it for a time pointing to nearly 90°, from which, while the air-current continues, it gradually sinks to zero. On simply stopping the current of dry air, the needle springs quickly to the other side of zero and swings through an arc of 120 degrees, this large deflection being produced by the sudden reabsorption of the atmospheric vapour when the dry air is intercepted. Air artificially moistened produces still larger deflections than the above.

Such effects were well known and duly guarded against. Indeed, it is to me interesting to notice my own experience in this inquiry reproduced years subsequently in the experience of Professor Magnus. I never had the least doubt of the correctness of his results; but, for the most part, they have absolutely nothing to do with mine. We are equally successful in our efforts. His object, for example, is to bring the hygroscopic character of rock-salt into strong relief, and he succeeds in wetting the plates; my object is to avoid this source of disturbance, and I am equally successful. He, by blowing vigorously into his tube, urges the air against the face of his pile, and obtains the effects due to condensation and evaporation; I, by operating cautiously and permitting the air to enter the tube so slowly and at such distances from the source and from the pile that neither of them is affected by it, obtain the effects due to absorption. One great feature of the case, however, is, that while the results of Professor Magnus have been known to me for years, and while I can produce them on a large scale at any moment, he has not yet succeeded in reproducing mine. "Never," he writes, "in a single instance has the deflection indicated a greater absorption by the humid air."

As soon as I had read the last paper of Professor Magnus, I felt that it would be useless on my part to reiterate what I had already so often affirmed, and I therefore wished to subject my experiments to the scrutiny of an independent observer. Mr. Faraday had already seen those experiments, and it is purely my reluctance to give him trouble that prevented me from asking him to witness them again. Next to him I could hardly find a man whose testimony on such a subject will have greater weight than that of my colleague, Dr. Frankland; and he, at my request, kindly undertook to satisfy himself upon the points at issue. I mounted the apparatus, and left it entirely in his hands, and he has favoured me with the following account of his observations:—

"My dear Tyndall,—At your request I have made a number of experiments on the comparative transcalency of common air, and of air deprived of its moisture by contact with monohydrated sulphuric acid. The apparatus which I used was that described by you in the Philosophical Transactions for December 1862*. It was exclusively under my own control; and I arranged the details of manipulation in such a manner as

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* [And also in the present Number of the Philosophical Magazine.—J. T.]

appeared to me best calculated to eliminate all sources of error. My mode of operating was as follows:—The brass tube (A B, fig. 2 [see antè, p. 34]), open at both ends, formed the conduit for a portion of the thermal radiation from the source (S). These heat-rays, after passing through the tube, traversed several inches of intervening air-space before entering the cone of the thermo-electric pile, where they produced their effect, in opposition to that arising from another constant source of heat affecting the opposite face of the pile (the compensating cube). The differential action was indicated as usual by a delicate galvanometer. These arrangements being once for all made, I was able by means of an air-pump to introduce at pleasure into the tube either the ordinary air of the laboratory, dry air, or air rendered moist by passage over extensive surfaces of wet glass. At the commencement of the experiments the tube was of course full of the common air of the laboratory; the needle of the galvanometer marked 42°, and remained steady for a quarter of an hour within a degree of that point. I now interposed in the path of the rays entering the brass tube a sheet of tin-plate; the needle at once bounded from 42° up to 90°. It was thus evident that any obstruction to the passage of the rays of heat through the tube, or, in other words, any *cooling* of that face of the pile which was turned towards the tube, would be indicated by an increased deflection of the needle on the same side of zero, which I will call the — side, whilst a *heating* of the same face of the pile would be attended by a diminished deflection, or even by a passage of the needle to the opposite or + side of zero. The following are the results which I observed:—

Tube filled with	Permanent deflection.
Common air	−42°
Air dried by contact with monohydrated sulphuric acid and introduced gently into the open tube.	+13
Common air which had spontaneously displaced the foregoing dry air	−43
Common air gently drawn in by air-pump . .	−43·3
Common air gently blown in from caoutchouc bag.	−45
Same air gently blown in from caoutchouc bag, but dried by passage over sulphuric acid .	+ 6
Air from same bag but not dried	−46·5
Air of laboratory	−42
Air dried and introduced as before	+14
Air dried as before with sulphuric acid, but afterwards passed over fragments of glass moistened with water	−46
Common air gently drawn in by air-pump . .	−42·5

"At the conclusion of the experiments I found that the deflection due to the total radiation was 86°·2.

"I also saw the following experiments made by yourself when the ends of the brass tube were closed by plates of rock-salt:—

	Permanent deflection of needle.
	°
Tube filled with dry air	+ 7
After exhaustion of tube	— 3
After admission of laboratory air .	—42

"Rain was falling at the time these last determinations were made, and the air was very moist. On removing the plates of rock-salt from the tube they appeared to be quite dry; and after being breathed upon, the film of moisture soon disappeared and they recovered their previous lustre. I ought perhaps to mention that these experiments are not selected, they are the only ones I have made upon the subject, and they were performed in the sequence given above: after a very careful scrutiny I have been unable to detect any source of fallacy in them, and they therefore appear to me to prove conclusively that obscure radiant heat passes much more readily through dry than through moist air.

"In conclusion, I cannot but express my surprise and admiration at the precision and sharpness of the indications of your apparatus. Without having actually worked with it I should not have thought it possible to obtain these qualities in so high a degree in determinations of such extreme delicacy, and which are so well known to be exposed to numerous sources of derangement.

"Believe me,
"Yours very truly,
"E. Frankland."

"Royal Institution,
June 19, 1863."

"P.S. Since writing the foregoing letter, I have repeated the experiments there recorded without any source of heat at either end of the pile, in order to ascertain whether the introduction and withdrawal of dry air at all affected the galvanometer. The tube was first full of the common air of the laboratory, and the needle remained steadily at +12·5 for a quarter of an hour. A current of moistened air was now drawn through the tube for ten minutes in precisely the same manner as when the two sources of heat were employed, the needle being closely watched during the whole time. It oscillated between +12 and +13, but never passed these limits on either side. The current was now interrupted and the needle closely watched for five minutes: it remained perfectly steady at 12·5. A current of dried air was now conveyed through the tube for ten minutes; the needle oscillated as before between 12° and 13°.

"Thus far I operated on the air exactly as in the experiments recorded in the foregoing letter. I then quadrupled the velocity of the current through the tube, introducing in the first place dry air: the needle in a first experiment moved 6 degrees in the direction of cold; but on repeating the experiment with both dry and moist air no effect whatever was produced. I now removed the tube and delivered a gentle current of dry air into the cone of the pile, immediately the needle moved 90° in the direction of *cold;* the current was continued uninterruptedly for ten minutes, during which time the needle gradually returned to nearly its original position. The current of dry air being now stopped, the needle moved 40° in the direction of *heat,* returning again gradually and slowly to its normal position. The same temporary deflection for heat was also produced in an exalted degree when the dry current was immediately succeeded by a moist one.

"These supplementary experiments lead me to the following conclusions:—

"1st. The gentle currents of air which were caused to flow through the tube in the experiments detailed in my letter did not in any way disturb the results of those experiments, neither would they have done so in any material degree even had their velocity been quadrupled.

"2nd. The impact of air drier than that previously in contact with the pile cools that face of the instrument with which it comes in contact, whilst the like impact of moister air produces the opposite effect.

"3rd. It is, however, impossible to confound the effects obtained in the above experiments on transcalency with those produced by the impact of dry and moist air upon the face of the pile, because in the first place the former are *permanent,* whilst the latter are essentially *transitory;* and in the second place the deflections due to the impact of dry or moist air against the face of the pile are always *in the opposite direction* to those obtained by the interposition of the same kind of air in the path of radiant heat. Thus, if the heat-rays falling upon one face of the pile be made to traverse dry air, the needle will move in the direction of heat, but if the apparatus be so arranged as to cause the dry air to impinge upon the face of the pile, the effect due to the greater transcalency of the dry air would be at first more or less neutralized, or even altogether overborne, by the cooling influence due to evaporation at the surface of the pile so brought into contact with dry air.—E. F."

"Royal Institution,
June 20, 1863."

In my remarks on the experiments of Prof. Magnus, I had pointed out two probable sources of error in the method which he employed. One of these was the bringing of the cold gas to be examined into direct contact with his source of heat; and the other was the bringing of the same gas into direct contact with the face of his thermo-electric pile. In his last paper he urges, in reference to the first point, that my objections do not apply to his apparatus, because in it the column of air is heated at the top. This argument would be strictly valid if the heat could be applied with perfect uniformity to a perfectly horizontal plane, but in practice such perfection is not attainable. The top of Prof. Magnus's recipient is dome-shaped, even where it is in perfect contact with the source of heat, while beyond the limits of this contact, that is to say down the sides of the recipient, it is propagated more or less by conduction. Indeed Prof. Magnus himself states that a portion of the heat effective in his experiments is derived from the glass thus warmed. "The heating of the thermometer," he writes, "although due only to conduction and radiation, involves a very complicated process. Besides the direct heating through conduction and radiation, reflexion also takes place at the inner surface of the vessel. Further, *the portions of the surface adjacent to the vessel of boiling water are heated by conduction, and also radiate heat against the thermometer*"*. I have italicised the most important part of the passage. Now air in contact with such a surface is substantially in the same condition as in my front chamber, and such air must more or less diminish the temperature of the surface exposed to it. If Professor Magnus fails to detect this, it can, I think, only be that his new apparatus lacks the requisite delicacy. Without the actual numbers no safe opinion can be formed upon this point; the probability is that his total heat is so small that the lowering of the temperature of his source by the admission of air into contact with it becomes infinitesimal.

An important difference between Prof. Magnus and myself consists in the high absorptive power which he ascribes to air. He makes this absorption more than 140 times what I make it. I would here bespeak the reader's attention while I examine the conditions in which Prof. Magnus places his instruments. From his last figure, and also from a passage of his paper, I infer that in his recent experiments the air has free access to the two faces of his pile, the axis of which is vertical. The upper face is furnished with a conical reflector, while the lower one is provided with one of the cylindrical tubes which usually accompany the instrument. Let us reflect for a moment on the processes involved in this arrangement. Professor Magnus keeps the space which contains his pile at a constant temperature of 15° C. Let us first suppose the two faces of his pile to be at the same

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* Poggendorff's *Annalen*, vol. cxii. p. 544.

temperature, the radiation being suspended, and the space around the pile a vacuum. Suppose, in the first instance, the temperature of the air outside to be lower than that of the pile, that the pile, in other words, is a warm body in comparison with the air; what will be the effect of admitting the air into the vessel? The reader will more easily understand me if he refers to the Plate which accompanies the paper of Prof. Magnus in the present Number of the Philosophical Magazine. On the upper face of the pile will rest a column of air, which is heated at its bottom by the surface on which it rests; convection will immediately set in, and heat will be continually abstracted from the face of the pile. At the lower face, on the contrary, an equal abstraction does not take place; the air once warmed remains in contact with the face of the pile, convection here being almost *nil.* Thus a less amount of heat is abstracted from the lower than from the upper face of the pile, and hence the instrument, which before the entrance of the cool air produced no current, will, in virtue of the different action of this air on its two opposite faces, generate a current similar to what would be produced by the direct heating of the lower face of the pile.

A moment's reflection suffices to prove that precisely the same deflection is obtained when the external air is *hotter* than the pile. Supposing, as before, the temperature of both faces to be the same at the commencement, the needle of the associated galvanometer being at zero. When the warm air enters it is chilled by the upper face of the pile, contracts, and remains in contact with that face, forming in fact a pool of heavy air at the bottom of the reflector. The air chilled by the opposite face of the pile falls by its weight; its place is supplied by fresh warm air, which again falls and is replaced. Thus it is evident that the lower face of the pile will in this case be more heated by the air than the upper one; and hence we infer that whether the external air be colder than the pile, or hotter than the pile, the same galvanometric effect follows its introduction into the vessel.

Instead of supposing the pile to be in the first place of uniform temperature, let us imagine it exposed to the radiation from the source. This makes the upper face warmer than the under one, and produces a deflection commensurate with the difference of temperature of the two faces. Let air now enter: it is manifest from the foregoing analysis that, whether this air is colder than the pile or hotter than it, its effect will be to render the lower face *relatively* warmer, and thus to diminish the deflection. If, moreover, the air be of the exact temperature of the upper face, it will warm the under one, if of the exact temperature of the under face it will chill the upper one. If its temperature be the mean temperature of the mass of the pile, it will chill the upper face and warm the lower one at the same time. No matter, then, what the temperature of the air may be when it enters the vessel of Prof. Magnus, the effect of its contact with the pile is to diminish the deflection due to the radiation from the source, and thus produce the same galvanometric effect as a true absorption would produce, the effect, however, being one of pure contact, in which absorption has absolutely nothing to do. How Prof. Magnus releases his pile from this apparently inevitable action he does not inform us; and how he can distinguish between this effect and one of absorption I am at a loss to imagine.

His apparatus will enable him to make this experiment in a far more unexceptionable manner. Let him place a second plate of salt across his tube at E E, and thus isolate his pile from the air which he intends to examine. He will then obtain the almost pure effect of radiation. Prof. Magnus has actually made this experiment, and the result, expressed in his own words, is "a hardly perceptible difference between dry air and vacuum."

It is scarcely necessary to repeat what I have already stated regarding the heating of the pile when the air enters, in virtue of the collision of the air-particles against the face of the pile. Curiously enough Prof. Magnus never once refers to this effect, though he does refer, for the first time, in his last paper to the chilling consequent on pumping out. I think it is almost certain that, had his apparatus been sufficiently delicate, the striking effect to which I refer must have long ago attracted his attention. Some conception of its magnitude may be formed from the following quotation from a paper laid before the Royal Society on the 18th of this month:—

"A brass tube 3 feet long and very slightly tarnished within was used for dynamic radiation. Dry air on entering the tube produced a deflection of 12 degrees. The tube was then polished within and the experiment repeated: the deflection by dry air was instantly reduced to 7·5 degrees.

"The rock-salt plate at the end of this tube was removed, and a lining of black paper 2 feet long was introduced within it. The tube was again closed, and the experiment of allowing dry air to enter it was repeated. The deflections in three successive experiments rose from 7°·5 to

80°, 81°, 80°,

and this result might be obtained as long as the lining was permitted to remain within the tube.

"The plate of rock-salt was again removed, and the length of the lining was reduced to a foot; the dynamic radiation on the entrance of dry air produced in three successive experiments the deflections

76°, 74°, 75°.

"The plate was again removed and the lining reduced to three inches in length; the deflections obtained in two successive experiments were

66°, 65°.

"Finally, the lining was reduced to a ring only 1½ inch in width; the dynamic radiation from this small surface gave, in three successive trials, the deflections

60°, 56°, 56°·5.

"The lining was then entirely removed; and the deflection instantly fell to

7°·5.

"In the foregoing experiments the lining was first heated by the collision of the air, and it then radiated its heat through a thick plate of rock-salt against the pile. The effect of the heat was enfeebled by distance, by reflexion from the surface of the salt, and by partial absorption. Still we see the radiation thus weakened competent to drive the needle almost through the quadrant of a circle. Suppose, instead of being thus separated from the lining, the face of the pile itself to form part of the inner surface of the tube, receiving there the direct impact of the particles of air; of course the deflections then obtained would be far greater than the highest of those above recorded. I do not doubt the possibility of causing the needle of my galvanometer, subjected to such an action, to swing through an arc of 1000 degrees; and it is my reluctance to derange the magnetism of my needle that prevents me from making the experiment"*.

Professor Magnus refers to the agreement which subsists between his results and mine in the case of the more powerfully acting gases, in proof of the correctness of his mode of experiment. The agreement, however, is not such as to warrant the conclusion drawn from it. The case may be illustrated by reference to a delicate chemists' balance as compared with one of those used in common life. Weighing *pounds*, both balances would roughly agree, but in weighing *milligrammes* the coarser balance would infallibly fail. I think it vain to expect a correct determination in any case requiring great delicacy with the apparatus which Prof. Magnus employs.

Prof. Magnus again refers to the hygroscopic character of

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* When the pile was placed entirely within the tube (as Prof. Magnus places it), a single stroke of the pump in exhausting drove the needle through an arc of 115 degrees.

rock-salt. This is admitted. His experiments on this substance are quite correct; but they have no bearing upon mine. During our joint experiments, and while the humid air, whose absorption produced a deflection of 43 degrees of my galvanometer, was still in the experimental tube, the rock-salt plates were detached and placed in the hands of Prof. Magnus. He saw no moisture, and he expressed himself satisfied that there was none. I may add that there is the strongest reason to believe that the substance as a liquid film, even if such existed, would not exert any greater action than the same film as vapour. However, the film did not exist, and it is therefore useless to speculate about it. Prof. Magnus finds another difficulty in the fact that I make air my unit, and refer the action of all other gases to this unit. There is, I submit, no more "difficulty" here than in the tables of atomic weights, where hydrogen is taken as the unit. My object was, and is, to make radiant heat an explorer of molecular condition; and my results seem to me more instructive and emphatic as I presented them than if I had followed the common method pursued by Prof. Magnus. The difficulty referred to does not touch the method of experiment at all, but merely my way of presenting the results obtained by that method.

In conclusion, I would refer the reader, for additional proofs of the action of atmospheric vapour upon radiant heat, to a reprint of a short memoir from the Philosophical Transactions in the present Number of the Philosophical Magazine. I will not dwell upon this paper, as the competent reader will draw his own conclusions from it. I may add that, in the paper presented to the Royal Society last Thursday, the action of all the vapours which I have examined is compared with that of the liquids from which these vapours are derived. The order of absorption of vapours and liquids is precisely the same. At the bottom of the list stands water, as the most opake liquid examined. It would form a most remarkable exception to what, so far as I can see, is a *general law*, if the vapour of this liquid proved so ineffectual as the experiments of Prof. Magnus make it to be. One word with reference to the importance of this subject. In a certain sense Prof. Magnus is quite right in rating it low. It derives its importance from the accident that aqueous vapour is everywhere present in our atmosphere, and from the fact that, for the future, the proved action of this vapour must form one of the chief foundation-stones of the science of meteorology.

Royal Institution,
June 19, 1863.

VII. *On the Discovery of the Metal Thallium.*

By William Crookes, *F.R.S.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

May I request you to give me an opportunity of replying to certain accusations recently made against me by M. Lamy in reference to this subject?

Chemists are doubtless aware that my title to the discovery of the metallic nature of thallium has been disputed by M. Lamy, and that he has been supported in this position by eminent scientific men in France. It has been stated, both in M. Dumas' report to the Académie des Sciences*, and even more pointedly in M. Lamy's recent memoir†, that I first obtained a knowledge of the metallic nature of thallium from M. Lamy, and that I appropriated that knowledge as my own. I should gladly have abstained from any controversy as to the mere question of priority in the discovery or publication of this fact, and am well content with the recognition of my claims which has been accorded to me by English chemists; but the purport of the above statements is of such a nature that it becomes necessary for me, both from regard for my own credit and from respect to those who have acknowledged my title to the original discovery of thallium and of its metallic nature, to show that the accusations above referred to are unfounded.

Your readers may remember that I published in this Journal, in April 1861, a paper "On the Existence of a new Element, probably of the Sulphur Group," and in May following, "Further Remarks on the supposed New Metalloid"‡. From that time my attention was almost exclusively directed to the further elucidation of the chemical history of the new element; and about the middle of April 1862 it was arranged that I should exhibit some specimens of thallium and its compounds in the International Exhibition §. My intention was to have exhibited a large series of specimens; but as I had not been able to meet with

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* *Comptes Rendus,* vol. lv. p. 866, Dec. 15, 1862.

† *Annales de Chimie et de Physique,* 3 sér. vol. lxvii. April 1863.

‡ Chemical News, May 18, 1861, vol. iii. p. 303.

§ "Exhibition Building, South Kensington, W.,
April 17, 1862.

"My dear Sir,—I shall be greatly pleased to see a specimen of thallium in the Exhibition. Will you kindly call on me on Saturday the 19th inst. between 6½ a.m. and 7 p.m., and I will arrange the matter with you.
"In great haste,
"Yours very truly,
"*W. Crookes, Esq.*" "C. W. Quin."

any abundant source of thallium, my scanty store proved too limited for dividing into more than three portions. Consequently I was only able to prepare specimens of the metal itself, of its oxide and sulphide. These were displayed, at the opening of the Exhibition on May 1, 1862, in a case with the label "*Thallium, a new metallic element, discovered by means of spectrum analysis,*" and with a card on which was written "*Chemical reactions of thallium, by which it is distinguished from every other known element. It appears to have the character of a heavy metal, forming compounds which are volatile below a red heat. It is reduced from its acid solutions by zinc in the form of a dense black powder, difficultly soluble in hydrochloric acid, readily soluble in nitric acid,*" *&c.*

On the 7th of June following, being at the Exhibition, I learnt from Mr. Quin that a M. Lamy had just been to him, in company with M. Balard, and shown him an ingot of thallium. He had taken them to my case (Class II. No. 634) and translated to them the labels, whereupon M. Lamy remarked that the substance exhibited by me as thallium was not the metal but its sulphide*.

Two days afterwards, the 9th of June, I was introduced to M. Lamy at Dr. Hofmann's house, and on that occasion first saw the ingot of thallium described to me by Mr. Quin. Our intercourse was, however, confined to a few complimentary phrases, since M. Lamy could not speak a word of English, and my ability to converse in French was very limited. But it became evident to me that M. Lamy was working diligently on the same subject that had occupied my attention during the past year, and that it was necessary for me to take immediate steps to make known the results I had obtained, without waiting, as I had intended, until the investigation was complete.

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* "9 Cowley Place, Cowley Road, North Brixton, S.,
May 13, 1863.

"My dear Sir,—The circumstances of my interview with M. Lamy are as follow:—

"M. Lamy, in company with M. Balard, called on me at my office at the Exhibition on the 7th of June last and showed me an ingot of thallium. I took them both to your case and showed them your specimens, particularly pointing out the one marked *metallic thallium*, and translating the label to them. There can be therefore no possible doubt of M. Lamy having seen your case containing thallium described as a metallic body long before you could have been aware either of his presence in England, or of his having made the same discovery as yourself.

"I am, dear Sir,
"Yours very faithfully,
"C. W. Quin,
"Ex-Superintendent of Classes 2, 3, and 4,
International Exhibition, 1862."

"*W. Crookes, Esq.,*
&c. &c."

With this view I communicated with Dr. Wm. Allen Miller*, and, acting on his recommendation, sent in to the Royal Society a statement of my results, under the title "Preliminary Researches on Thallium." This paper was read on the 19th of June, 1862.

On the 23rd of June a paper was read at the Académie des Sciences by M. Lamy, "On the Existence of a New Metal, Thallium," in which he stated that three months previously he

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* "20 Mornington Road, N.W.,
June 12, 1862.

"Dear Sir,—I called upon you yesterday, but was unfortunate enough to find you away. My object was to ask your advice about 'thallium.' I have been hard at work on the subject for six or eight months past, and have found out a great deal about it, and, indeed, have a 'Preliminary Note on Thallium' nearly finished, which I was going to send to the Royal Society in a week or so. Not suspecting that anyone else was working on the same subject, I purposely avoided publishing any of my recent results, in the hope of giving the Royal Society paper more importance; and for the same reason I have not spoken much about them. I can, however, prove by witnesses that I obtained thallium in the pure metallic state as early as January in this year, and several other salts of it soon after. I was therefore much astonished to find, on Monday last, that M. Lamy had been working on the same subject; and from the much larger quantity of material at his disposal, I am pretty certain that in his memoir recently sent to the Society at Lille, he has anticipated most, if not all, of my results, and most likely has gone much further than I have. I therefore wish to ask you, both as a friend as well as in your official connexion with the Royal Society, what I had better do under these circumstances. Shall I still send in my imperfect note with the knowledge that a more perfect paper on the same subject has already been communicated to another Society, or shall I withhold my present paper until M. Lamy's is published, and then see if I can supply any fact which he may not have found out? I have worked long and hard on this subject, and cannot help feeling annoyed at the prospect of losing the greater part of the credit of the discovery.

"Pray excuse my thus troubling you, and believe me
"Very truly yours,
"William Crookes."

"*Dr. W. A. Miller, F.R.S.
&c. &c. &c.*"

"King's College,
Friday, June 13, 1862.

"My dear Sir,—Send in your note on thallium to the Royal Society *at once* by all means. Though much pleased to see a lump of thallium, I must own I was not a little disappointed to find you had been so far forestalled; but your exhibits at Kensington have quite established your priority even in securing a tangible quantity. You must, however, send in your note before Thursday next, which is the last day of meeting. It will only be announced, I dare say, but will no doubt appear at full in the 'Proceedings.' I think you had better head it 'Note for the Proceedings.' It will not prevent you from giving a full paper if afterwards it should seem desirable. A foot-note stating why you are thus induced to publish might be desirable.

"Very truly yours,
"Wm. Allen Miller."

"*W. Crookes, Esq.*"

had discovered a green band in the spectrum without knowing that I had observed the same fact fifteen months before. He mentioned that I had shown this green band to be due to the presence of a new element, to which I had given the name of thallium, and added that the small quantity of material on which I operated did not admit of my isolating the element and ascertaining its true nature.

In a communication made to the Société Impériale at Lille on the 16th of May, 1862*, he also described thallium to be a metal, and, referring to my papers of 1861, stated that the black powder which I regarded as thallium was nothing but sulphide of thallium.

In regard to this statement, I must, in the first place, point out that M. Lamy entirely ignores the fact of my having exhibited thallium as a metal on the 1st of May, 1862, and of having then described it as a "heavy metal, reduced from its acid solutions by zinc in the form of a dense black powder." The labels in my case were unmistakeably clear and definite in their wording; the metallic nature of thallium was both stated and implied by all of them; they did not leave the least possibility for the supposition that I considered thallium to be a metalloid; and the publication was in every respect as full as the reading of a paper at a provincial scientific society.

These, then, are the real facts of the case. As to the question of priority, I would submit that there can be no doubt. Even on the showing of M. Lamy and his friends, I was prior to him in every step of the discovery. M. Lamy claims to have first noticed the presence of the green band in the spectrum in April 1862; whilst I had discovered it thirteen months previously, and published a description of the substance by which it was produced.

M. Lamy claims to have isolated thallium in the interval between the 2nd and the 16th of May, 1862, and states that he for the first time exhibited the metal at Lille on the latter date†; whilst I isolated thallium in September 1861, and exhibited it as a metal on the 1st of May, 1862.

Still, M. Dumas, in reporting on the subject to the Academy, has stated that the history of the discovery of thallium presents two authentic dates—the 30th of March 1861, when Mr. Crookes announced the existence of a new elementary substance, and the

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* *Ann. de Chim. et de Phys.* before quoted.

† The following extract from the register of the *procès-verbaux* of the Société Impériale des Sciences, de l'Agriculture et des Arts, at Lille, is given by M. Lamy as the earliest notice of his discovery:—

"At the meeting of the 16th of May, 1862, under the presidency of M. Lamy, M. Lamy announced to the Society that he had succeeded in isolating thallium by decomposing with a voltaic battery the yellow crystalline compound of which he had spoken at the meeting on the 2nd of May."

16th of May, 1862, when M. Lamy made known the new metal.

Here, again, the publication of this fact by myself on the 1st of May, 1862, is ignored, and M. Dumas remarks, "No one could dispute that M. Lamy was the first to isolate thallium, and so to demonstrate that it is a true metal, and not a metalloid, as supposed by Mr. Crookes, who never obtained it uncombined and pure."

But notwithstanding these facts, M. Lamy has since stated that the first time I described thallium as a metal was in my communication to the Royal Society in June; and still ignoring my publication of that fact on the 1st of May, he repeats the statement that I was ignorant of this fact until June, and then he declares it was communicated to me by himself.

It may be that, when M. Lamy read his paper to the Société Impériale at Lille on the 16th of May, he was ignorant that I regarded thallium as a metal; but he must have been aware of this when he read his paper to the Academy in June, after having visited the Exhibition and seen the labels attached to my specimens, for Mr. Quin took the trouble to explain them to him. In proof of this, I may also refer to the account M. Lamy gives of his motive for coming to London in June 1862, viz., "to ascertain for himself what results I had obtained, before announcing to the Academy a discovery he was no longer certain of having made." Why this doubt, I may ask? He says he had heard my thallium was in the Exhibition. M. Lamy has not claimed the discovery of thallium itself; he only claims to have been the first to isolate it, and to ascertain it to be a metal. This is the discovery as to which he was in doubt whether he had not been anticipated; and his visit to the Exhibition gave him ample opportunity for learning that he had been anticipated both in the discovery and in the publication of it. And yet, with the full knowledge of what he saw there written, he now has the hardihood to assert that I was ignorant of the metallic nature of thallium until he told me.

But M. Lamy will not recognize the evidence which he saw in the Exhibition, that I regarded thallium as a metal, and had so described it since the 1st of May, 1862. He still maintains that, before he showed his piece of thallium in June, no one in England had seen thallium; that the substance I exhibited "was not thallium, and could not be thallium," but only a black powder to which I gave that name. He says, no one knew it was a metal; that chemists even doubted its existence as an elementary substance; that I first learnt from him that it was a metal, and that having done so I hastened to communicate to the Royal Society a statement of nearly all the properties of thallium he had described to me.

The question now, therefore, is scarcely one of mere priority in the discovery or the publication of a fact, but a question of common honesty. It is not so much whether I knew thallium to be a metal in May, but whether I stole that knowledge from M. Lamy in June.

The fact that M. Lamy dates his first publication of the metallic nature of thallium as having been made on the 16th of May, would be in itself a sufficient proof that this accusation is unfounded, since I had publicly described thallium as a metal on the 1st of May, before having heard of M. Lamy's existence, or having the most remote suspicion that any one but myself was working on the same subject. M. Lamy, however, not only ignores this fact, but also asserts that the substance then exhibited by me was not thallium. By what authority he does so is not stated. If it be for the same reason that in his paper of the 16th of May he declared the substance which I regarded as thallium to be nothing but sulphide, he makes the statement now, as he made it then, upon very insufficient grounds. I am unable to perceive why M. Lamy should assume that the powder exhibited as thallium on the 1st of May, 1862, was obtained by precipitation with sulphuretted hydrogen, and not the powder precipitated by zinc, and actually so described on the label in the case as well as in my first paper in 1861*. It is only by means of this assumption that M. Lamy's assertion can be of any value; and that this assumption is totally gratuitous and unfounded will, I trust, be apparent to any one who compares the reactions of thallium described by me in this Journal in April 1861, with the brief account written on my labels in the Exhibition.

But really this has little to do with the question at issue. It is of no importance whether the body which I had ticketed "thallium, a new *metallic* element," was in the state of powder or lump, pure or impure, or, indeed, whether or not there was any thallium at all in it. M. Lamy cannot deny that the *label*, at all events, was there publicly exhibited from the 1st of May; and that, I maintain, is ample publication of the fact that I knew thallium to be a "metallic element" from my own independent researches.

Fortunately I am able to bring forward positive evidence that long before May 1862 I knew thallium to be a metal, and had obtained it in a melted metallic state, and by electrolysis so far back as January 1862, even before M. Lamy had seen the green band in the spectrum.

When it was first suggested that I should exhibit illustrations of thallium, I prepared a set of labels for the purpose; and

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* See note, p. 62.

though these labels and their history, as given below*, might not be worth much in a question of mere priority, I do not doubt that they will be useful in regard to the question whether the metallic nature of thallium was known to me before M. Lamy came to London in June, or whether that knowledge was then first obtained by me from M. Lamy. I may appeal to any chemist to say whether these labels do not prove that the writer had a perfect knowledge of the metallic nature of thallium at the time the order for them was given to Messrs. Silverlock.

I can also refer to the testimony of persons who saw metallic thallium in my laboratory in January 1862, long before M. Lamy appeared in the matter †.

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Thallium.	Oxide of Thallium.	Sulphide of Thallium.	Basic Chloride of Thallium.
Iodide of Thallium.	Sulphate of Thallium.	Chloride of Thallium.	Nitrate of Thallium.
Ferrocyanide of Thallium.	Cyanide of Thallium.	Phosphate of Thallium.	Carbonate of Thallium.
Thallium (θαλλος).	Chromate of Thallium.	Thallium, Sublimed.	Oxalate of Thallium.

"Printing Office,
Wardrobe Terrace, Doctor's Commons,
London, 18th May, 1863.

"SIR,—In reply to your request, I beg to inform you that I have examined the books of this firm, and find that 50 sheets of labels (Thallium, Sulphate of Thallium, Nitrate of Thallium, &c.) were printed here and delivered on or about April 25, 1862; the order having been given a few days prior to that date. The original manuscript of the labels is still in my possession.

"I am, Sir,
"Yours obediently,
"Pro H. Silverlock,
"P. PERKINS."

"*Mr. W. Crookes, 20 Mornington Road.*"

† "5 New Cavendish Street, London, W.,
June 16, 1862.

"MY DEAR SIR,—I well remember paying you a visit in January last to see the 'thallium' you had then got into a more definite shape. As I had seen its reaction in the spectroscope many months before, and was much interested in the remarkable discovery, I felt great pleasure in watching the progress you had then made in the investigation.

"You had several compounds of the body, including, if I remember rightly, the sulphide (of which I possess a specimen), and the oxide which you had obtained in crystals. The quantity of material you had, however, was but small; and it was only by exercising the greatest amount of ingenuity that you were able to demonstrate the nature of its compounds.

"The most interesting point, I well remember, was the metal itself, deposited by means of a weak galvanic current,—first on a bar of copper, In addition to this positive evidence, I will also refer to the probabilities of the case. Though M. Lamy's results were said to be obtained within the short space of one month, both he and his friends assume that, during the whole year that elapsed between my first publications on thallium and the exhibition of the specimens in 1862, I had either ceased to work on the subject, or that my knowledge of the element had not advanced one step between May 1861 and May 1862; that the black powder I exhibited as thallium was obtained by precipitating with hydrosulphuric acid, not the black powder described as precipitated by zinc *. They assume that because at the former date I was, though doubtful as to the point, inclined to class thallium with the semimetals, therefore at the latter date I was ignorant that it possessed true metallic properties.

I trust the improbability, nay, the impossibility of this being the case, will be recognized by every chemist who has examined thallium. It is an element as easily reduced to and preserved in a metallic state as lead: can it, then, be imagined that I, who was so much interested in determining its characters—who had been for twelve months leaving no means untried to obtain a more copious source of thallium—who during that time had scarcely for a day relaxed working on this subject exclusively—is it likely, I say, that I should have been such an egregious blunderer as not to find out that it was a metal? Why, as soon as I had obtained a dozen grains of one of its compounds fairly pure, I could scarcely try the simplest experiment without having the fact of its metallic character forced upon me in too positive a manner to admit of doubt. Consistent with this probability that I considered thallium to be a metal, is the fact that it was exhibited as being a metal.

Having thus stated the grounds on which I claim the dis-

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where it presented a coherent metallic appearance, of dark colour, but which, when freshly scraped with a knife, gave a coloured metal similar to lead. You also had a larger quantity precipitating on platinum in the spongy form, which, compressed, gave metallic lustre, and when tried in the spectroscope, gave the green line magnificently.

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"I remain yours very truly,

"*W. Crookes, Esq., F.C.S.*" "John Williams."

"· · · · · having ourselves been shown by Mr. Crookes the wonderful green line in the spectrum in 1861, and a small disc of the metal itself in January 1862,—dates earlier than any referred to by Lamy in the late controversy."—*The London Review*, April 4, 1863.

* In my first paper in this Journal for April 1861, I wrote, "From its hydrochloric acid solution it is readily precipitated by metallic zinc in the form of a heavy black powder, insoluble in the acid liquid."

covery of this metal, I may leave your readers to form their own opinion as to the justice with which my right to that discovery has been disputed, and as to the sufficiency of the argument that I could not be aware of the metallic nature of thallium, because I had not enough of it to melt into an ingot.

But besides these points involving merely scientific credit, M. Lamy and his friends have unmistakeably implied that I have acted dishonourably in regard to him. I am accused of having met him in company, of having gathered from him a knowledge of the properties of thallium, and of having hastened to publish that knowledge as the result of my own researches.

It is chiefly on account of this insinuation that I have requested you to allow me to make known a statement of the whole facts of the case, and to show that the claim put forward by M. Lamy rests on nothing but a partial statement of those facts, and upon assumptions that have no real foundation.

I have the honour to be, Gentlemen,
Your obedient Servant,
WILLIAM CROOKES.

20 Mornington Road, N.W.
May 21st, 1863.

VIII. *Note on the Tides and the Earth's Rotation.*
By THOMAS CARRICK, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

THE speculations of Dr. J. R. Mayer on the effect produced by the tides on the earth's rotation, which appear in his 'Essay on Celestial Dynamics,' reprinted in your Journal for May 1863, are based upon the assumption that the tidal wave always lies to the east of the moon's meridian, and has a slow progressive motion from west to east, opposite in direction to the rotation of the earth; an assumption which cannot, I believe, be justified consistently with the known facts of tidal motion in any sense which warrants the conclusions sought to be indorsed thereon.

As Prof. Tyndall in his "Remarks on the Dynamical Theory of Heat," published in the same Number of your Journal, has indorsed Dr. Mayer's views, and as the subject appears likely to undergo further discussion, it seems desirable to call attention to the untenable nature of the assumption.

Our knowledge of the position and progress of tidal waves is derived from observations of the time of high water on ocean coasts, the direction in which the hours increase along a given coast being held to indicate the direction of the progressive motion of the wave. The hypothesis of a wave always following in the rear of the moon's motion, as assumed by Dr. Mayer, obtains little confirmation from these recorded observations, which, indeed, at first sight would seem rather to indicate a progression nearly everywhere at right angles to the direction of the motion of the moon.

To this anomalous progression we owe the hypothesis of free ocean waves of translation, whose progress is illustrated by maps of cotidal lines. This hypothesis, however, has been pronounced untenable by its ablest expounder.

Guided by speculation in cosmical dynamics, the nature of which need not here be alluded to, I have arrived at the conclusion that tidal phenomena ought to be grouped in relation to land areas as causal centres.

In a paper read before the Physical and Mathematical Section of the Literary and Philosophical Society of Manchester on the 30th of April last *, I discussed from this point of view the hours of high water at full and change for the principal places on the globe as given in the Tide Tables for 1863 published by the Admiralty (these hours being first reduced to Greenwich mean time), and thereby arrived at the following law of the progression of the wave of high water:—

"*In all land areas in the northern hemisphere the wave of high water tends to revolve round the coast in the direction of the hands of a watch, and in like areas in the southern hemisphere against the hands of a watch.*"

The term wave is not here used in the sense of a free wave of translation. Theoretically, this law ought to hold good in proportion as land areas approximate to the circular form, with wide uninterrupted ocean spaces all round.

The complicated nature of the data of tidal hours has always constituted a serious obstacle in the way of any causal theory; but the above recited law, even if considered as a mere empirical grouping of the facts, comprehends in harmonious relation a much greater range of these facts than any hypothesis hitherto propounded.

Such being the case, it is obvious that the waves of different hemispheres and of opposite shores will tend to neutralize each other, without leaving even a residual easting in the tidal wave from which any action on the rotation of the earth can be clearly inferred.

Heathfield, Manchester,
June 18, 1863.

THOMAS CARRICK.

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* The abstract of this paper appears at page 79 of the present Number.

IX. *Remarks on* Professor Tait's *last Letter to* Sir David Brewster. *By* JOHN TYNDALL, *F.R.S., &c.*[*]

THE scientific public has already formed its own estimate of the charge made against me in the October Number of 'Good Words,' and it is therefore not necessary that I should refer to it further. But in the last paragraph of Prof. Tait's last communication to the Philosophical Magazine a new point is raised against me, which I now beg permission to examine.

In the article which generated this discussion Profs. Thomson and Tait write as follows:—"In 1843 he (Mr. Joule) published the results of a well-planned and executed series of experiments, by which he ascertained that a pound of water is raised 1° Fahr. in temperature by 772 foot-pounds of mechanical work done upon it." And in reference to this passage, I state that "it was in 1849, and not in 1843, that Mr. Joule proved the mechanical equivalent of heat to be 772 foot-pounds. His determinations in 1843 varied from 1040 to 587 foot-pounds." To men of science throughout the world I leave it to decide whether this statement is not strictly true. What, then, is the "*remarkable exception*" referred to by Prof. Tait? If the reader have patience to follow me he shall learn. In an appendix which follows my letter to Prof. Thomson, I quote from my book on Heat a *résumé* of the results obtained by Mr. Joule in 1843. Mr. Joule describes each series of his experiments, gives the numbers obtained, and deduces from these numbers the mechanical equivalent of heat, which he places in each case at the end of a paragraph. When I wrote my book I went carefully over Mr. Joule's paper,—first looking at his numerical data, and then taking up his result. This paper was dated from "Pendlebury, near Manchester, July 1843."

A postscript followed the paper, commencing with words which indicated a reference to the results previously recorded. I cast my eye over the postscript; I saw no numerical data, and no mechanical equivalent at the end of any one of its three paragraphs, and hence I concluded that the results ended with the paper itself. Prof. Tait, however, has discovered that in the middle of the first paragraph of the postscript a determination is alluded to. Here is Mr. Joule's whole statement regarding it:—"I have myself proved that *heat is evolved by the passage of water through narrow tubes*. My apparatus consisted of a piston, perforated by a number of holes, working in a cylinder glass jar, and containing about 7 lbs. of water. I thus obtained one degree of heat per pound of water from a mechanical force capable of raising about 770 lbs. one foot high." This, then, is

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[*] Communicated by the Author.

the "remarkable exception" of Prof. Tait. Had it caught my eye, of course it would have been gathered up as the others, but it would no more have affected my statement that the determinations of 1843 varied from 1040 to 587 foot-pounds, than the number 742 actually contained in my Table.

But have I really omitted to give Mr. Joule due credit for this experiment? Assuredly not. I was well acquainted with it, though when collecting the results for my book I overlooked it. In my morning lectures I referred to it thus:—"He (Mr. Joule) also urged water through capillary tubes, and determined the amount of heat generated by the friction of the water against the sides of the tubes; and the results of all his experiments leave no shadow of doubt upon the mind, that, under all circumstances, the quantity of heat generated by the same amount of force is fixed and invariable." I have publicly repeated this statement in print three times, to Prof. Tait's own knowledge, since it was first uttered.

And now I would ask my assailants, as men of honour, whether they had this experiment in their minds when they wrote the passage commented on? If so, by what right do they call this isolated result, stuck into the middle of a paragraph, without data, and without a word to indicate the precautions employed, "a well-planned and executed series of experiments?" Why, moreover, did they displace the number 770 by the exact equivalent 772? When I think of the men who will read this last paragraph of Prof. Tait and pass judgment on it, I am restrained from making any further effort to disentangle it; nor shall I deem it necessary to answer any further remarks of this kind.

One word in conclusion. For reasons far too long to be here stated, I regarded Prof. William Thomson as my real assailant in 'Good Words,' and wishing to show my readiness to settle our differences in an open manly way, I addressed him to the exclusion of Prof. Tait. To cast a slight upon Prof. Tait was not necessary to my defence; and as far as the first paragragh of my letter to Prof. Thomson appears to convey such a slight, I should wish it unwritten. For his sake, and not for mine, I could wish the same with regard to the last paragraph of Prof. Tait's last communication.

Royal Institution, June 23, 1863.

I had the happiness of receiving a few days ago a letter from Dr. Mayer—the first which has ever passed between us—an extract from which will interest many of the readers of the Philosophical Magazine:—

"ESTEEMED SIR,—I hardly know how to find words to express the feelings which move me at the present moment. On the 16th of last June Prof. Clausius conveyed to me the pleasant intelligence of your lecture at the Royal Institution. The hopes which in stillness I ventured to cherish were exceeded by the recognition which you there accorded me, and I am still more deeply affected by the receipt of your last communication to the Philosophical Magazine. Your kindness makes all the deeper impression from the fact that for many years I have been forced to habituate myself to a precisely opposite mode of treatment.

"The question of priority as to the mechanical equivalent of heat I regarded as exhausted by my communication to the Academy of Sciences in Paris (*Comptes Rendus,* vol. xxix. p. 534), as my celebrated rival Mr. Joule did not to my knowledge reply to me. I have referred to this document in a communication to the Academy of Sciences in Vienna (1851, vol. vi. No. 5). Certainly, however, it has never been my desire to diminish in the slightest degree the achievements of the great Manchester naturalist. I have never regarded him as an antagonist, but, as you have truly expressed it, have always considered him to be my esteemed and renowned fellow-labourer in the same domain of thought. I gladly avow that, were it not for his excellent experimental investigations, the doctrine of the Conservation of Force, or, as I would express it, Physical Stoichiometry, would not be able to show the blooming fruits which it now exhibits. The name of Joule, moreover, is quite as renowned in Germany as in England. To Messrs. Thomson and Tait I feel myself also indebted for having drawn from you the communication with which I have been favoured *.

"Heilbronn (Kingdom of Würtemburg), "D. J. R. MAYER."
31st May, 1863."

I would here state once for all, that I should not think of putting Mayer above Joule, or Joule above Mayer. In the firmament of science they are, in my opinion, a double star, the light of each being in a certain sense complementary to that of the other.—J. T.

X. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from vol. xxv. p. 552.]

November 20, 1862.—Major-General Sabine, President, in the Chair.

THE following communications were read :—

"Notice of Remarkable Hailstones which fell at Headingley,

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* This was my first brief remonstrance: my letter to Prof. Thomson I have not yet sent to Dr. Mayer.

near Leeds, on the 7th of May, 1862." By Thomas Sutcliffe, Esq., in a letter to Dr. Sharpey, Sec. R.S.

Headingley, July 16th, 1862.

Dear Sir,—Allow me to offer you some memoranda, which I made at Headingley, near Leeds, on the 7th of May last, respecting a hailstorm which visited several parts of England on that day. It appears that it arrived at Newark about 5 o'clock p.m., and was succeeded by a tornado which did much damage; then, pursuing a N.N.Westerly direction it reached Wakefield at 6.41. The hail continued to fall till about 6.58. The afternoon at Headingley had been remarkably hot and close, and the atmosphere densely charged with vapour; at 6.45 the sky had become so overspread with dark clouds that it was impossible to see anything within doors without artificial light. There were several peals of thunder and repeated flashes of rose-coloured lightning. The storm visited the villages on the west of Leeds with especial violence, the hailstones knocking down several people, and breaking nearly all glass exposed to the W.S.W.

The hailstones did not fall in a continuous shower, but in irregular clusters; sometimes a field would be thickly strewed with them, whilst an adjoining one escaped with scarcely any; one part of a greenhouse would be much broken, and the remainder, similarly exposed, escape uninjured. The district over which hail fell was very narrow.

To illustrate the force of the falling stones, I may mention that circular holes were cut in glass without the sheet being otherwise injured. I have the end of a pendulous branch of beech, 12 inches long and $\frac{3}{8}$ths of an inch in circumference, which was cut from the tree, also several larger branches from apple and lilac trees, which appeared to have been split from the adjoining boughs. Some muslin curtains spread on the grass to dry were torn by the hail with numerous crucial rents.

The hailstones were of different forms and sizes. I sketched about forty varieties; but as many bear a certain resemblance to each other, I select four of them for illustration. These were taken out of deep grass nearly half an hour after they had fallen. Figures 1 to 4 represent them of the size and shape they had when I picked them up. The heaviest I weighed was only 2 ozs., but other persons assert that they weighed some upwards of 5 ozs. each. No. 1 had a creamy white colour, with linear markings from the centre outwards; this variety appeared to constitute the nucleus of most of the larger ones, around which transparent ice had accumulated in rounded continuous masses. From the outside of some of the masses protruded icicles; the remains of two may be seen attached to the side of No. 2. When the stones first fell, some of these icicles were $1\frac{1}{2}$ and 2 inches long, and grotesquely shaped. It has been asserted that all the hailstones had the white nucleus, but this was not the case in our neighbourhood; 35 per cent. of those I gathered were without it, and assumed something of the shape of No. 3, which seemed an aggregate of crystals of clear ice. I found one which was composed

Fig. 2.

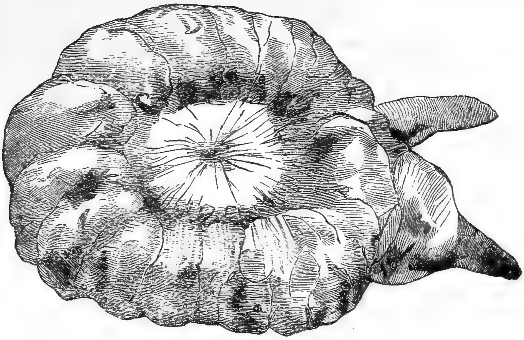


Fig. 3.

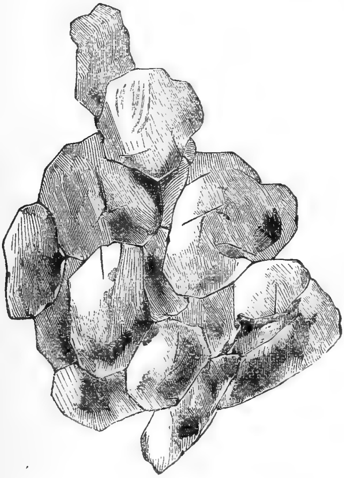


Fig. 1.

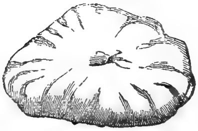


Fig. 4.

of five large masses of ice, quite clear, and in size like five nutmegs. There were some which did not correspond with any of the above descriptions; thus No. 4 has the round white radiated mass on the outside of the clear ice.

I annex meteorological reports for the day of the storm.

No. 1. From the 'Times' of May 8th.
May 7th, 1862, 8 o'clock A.M. Towns selected.

	B.	E.	M.	W.	F.	C.	I.	R.	S.
Aberdeen ...	29·93	50°	48°	N.N.E.	2	8	3 r.	0·15	1
Berwick ...	29·93	55	53	Calm.	0	24 f.	8 r.	0·43	2
Scarborough	29·89	57	55	N.E.	1	4	f.	—	2
Liverpool ...	29 89	55	54	E.	1	9	8 r.	0·43	1
Dover	29·83	62	60	S.E.	1	1	b.	—	1
Portland ...	29·85	55	54	E.S.E.	1	9	8 r.	0·35	2

No. 2. Report kept by Leeds Philosophical Society.
Leeds, May 7, 1862, 5 P.M.

Barometer.	At. Therm.	Dry Bulb.	Wet Bulb.	Wind.	Force.	Cloud.	Shade.		Max. Sun.
							Max.	Min.	
29·380 in.	70°	64°	60°	N.E.	1	10	70	51	100

I am, &c.,
THOMAS SUTCLIFFE.

"On the true Theory of Pressure as applied to Elastic Fluids." By R. Moon, M.A. Received June 26, 1862.

It is the author's object—

I. To show that, in elastic fluids in motion, or tending to move, it is not generally true, or at least not accurately true, that the pressure depends solely on the density, as is assumed in the ordinary theory of the motion of elastic fluids.

II. To show that, within certain limits and under certain circumstances, pressure may be transmitted instantaneously from one point of an elastic fluid to other points situated at finite distances from the first, before any change has been effected in the density of the intermediate fluid—in a manner analogous to that in which, in the theory of dynamics as applied to rigid bodies, force is assumed to be propagated instantaneously from one point to another.

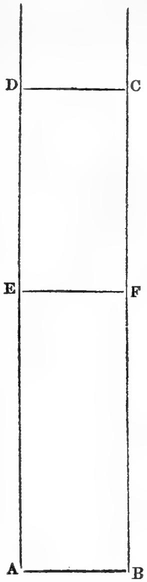
III. To show that in elastic fluids in motion, or tending to move, the pressure at any point in a given direction will consist of two parts:—one depending solely on the density, which will be equal in all directions; the other depending on the state of motion throughout the fluid generally, and which will vary with the direction in which the pressure is estimated. The former of these two constituents the author proposes to designate the statical pressure; the latter, the instantaneous pressure. The true pressure at any point in a given direction will be found by taking the sum or difference of the statical and instantaneous pressures, according to circumstances.

IV. To indicate the manner in which the instantaneous pressure may be represented mathematically.

V. To show the bearing of the proposed correction on the received theory of sound.

A B C D is a vertical cylinder closed at the base A B, and having an air-tight piston C D capable of moving freely in the upper part of it.

Below the piston the tube is filled with air, which at the time t is wholly free from impressed velocity, but in which the density varies in the following manner: viz., from A B up to an imaginary horizontal plane E F, the density is uniform; while from E F the density gradually increases up to C D, in such a manner that the effective force at every point of the air between E F and C D is exactly the same, and equal to f*. Above the piston a vacuum exists. The piston is supposed to have weight, but, for the sake of simplicity, the air under the piston is supposed to be unaffected by gravity. The weight of the piston is supposed to be such that the effective force on each particle of the piston is the same as that on each particle of the mass of fluid E C, viz. f.



If the pressure exerted by the air which originally occupied the space A F on that which originally occupied the space E C were to continue during the time t_1 the same that it was at the time t, every particle of the former mass of air (which we will designate as the air in A F) would during the time t_1 be under the action of the same effective force f, and would therefore in that time describe the same length of path, viz. $\frac{ft_1^2}{2}$; and on this supposition no change would take place in the density of the air in E C during the time t_1. But, according to the received theory, the pressure of the air in A F on that in E C will continue unchanged until the density of the part of the air in A B which abuts on the common boundary of the two masses of fluid has changed. Hence *change in the density of the air in A F must precede change in the density of E C.*

On the other hand, so long as the pressure of the air in E C on the air in A F remains unchanged, the air in A F will remain at rest, and will therefore undergo no change of density. But as, according to the received theory, the pressure of the air in E C on the air in A F depends on the density of the part of the air in E C which abuts on the common boundary of the two masses of air, it follows that *change in the density of the air in E C must precede change in the density of the air in A F.*

But we have before proved the exact contrary, viz. that change in the density of the air in A F must precede change in the density of

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* This will be the case if $\frac{1}{\rho}\frac{dp}{dx}=f$, or putting $p=a^2\rho$, $a^2\log_2\rho=fx+c$; where ρ denotes the density at the distance x measured vertically, and c is a const.

the air in E C. It is evident therefore that, according to the received theory, no change can, under the circumstances above supposed, take place in the density of either mass of air.

If, however, the density in A F remain unchanged, we have already seen that every particle in E C will in the time t_1 describe a space equal to $\frac{ft_1^2}{2}$; and if the density in E C remain unchanged, we have equally seen that every particle of A F will have remained at rest during t_1; which is a contradiction. It appears therefore that in the case we have been considering the received theory leads us to an absurd result.

It can with still more facility be shown that the received theory leads to an absurd result in the following case.

A B C D is such a tube as before described; but in the present case we shall suppose it filled below the piston with air of uniform density in equilibrium, the pressure of the air being such as to exactly sustain the weight W_1 of the piston. As before, a vacuum is supposed to exist above the piston, and the air is assumed to be unaffected by gravity.

If a second weight W_2 be placed upon the piston, we know that the equilibrium will be destroyed. But if it be true, as the received theory asserts, that the pressure of an elastic fluid depends solely on its density, the pressure of the air on the lower surface of the piston will be exactly the same after W_2 has been introduced as it was before W_2 was introduced; and, since action and reaction are equal and opposite, whatever be the pressure of the air in the piston, the same will be the pressure of the piston on the air; so that the pressure downwards of the piston on the air beneath will be the same after W_2 was introduced as it was before; and the system therefore will continue in equilibrium after W_2 has been introduced; which is absurd.

By an argument too elaborate to be indicated within the limits of this abstract, the cause of the failure of the existing theory in the instance first above considered is shown; and it is proved that in the second case the effect of the introduction of the weight W_2 is instantaneously to propagate through the air to a definite distance below the piston a finite increase of pressure; such increase of pressure having its maximum immediately underneath the piston, and thence gradually diminishing till, if the tube be long enough, it finally vanishes. The depth to which the instantaneous increase of pressure will extend will be defined by means of two considerations:—1st, that the effective force on every particle of the piston and weight must be exactly the same as that on the air immediately below it; and 2nd, that the aggregate moving force developed in the piston W, the weight W_2, *and the portion of the air in the tube through which the instantaneous pressure extends,* must be equal to the moving force developed by gravity in W_2 when free to move *in vacuo.*

It is also shown that if instead of the weight on the piston being suddenly increased it were to be suddenly diminished, exactly analogous results, *mutatis mutandis*, would occur,—the effect of the sudden removal of part of the weight being instantaneously to *diminish* the pressure to a finite distance below the piston—such diminution having its maximum immediately beneath the piston, and thence gradually diminishing till, at a certain distance below the piston, the whole pressure will be exactly the same as it was before any part of the weight was removed.

If the piston were wholly removed, the pressure of the air originally in contact with it at the instant of removal would be zero.

It is then shown that the addition to or diminution from the weight on the piston in the case last considered will produce no immediate change in the horizontal pressure in the air below the piston.

It is next shown that in cases where there is no impressed velocity, as in the case first considered in this paper, the instantaneous pressure p_i may be expressed in terms of its partial differential coefficients, and of the density at the point where the pressure is being considered.

It is also shown that, in the general case, where the whole or a portion of the fluid is endued with velocity, the instantaneous pressure may be ascertained by adding to the expression of the last paragraph a term involving the density and the partial differential coefficients of the velocity at the point where the pressure is being considered.

It is finally shown that, in the case of the transmission of a pulse through a cylindrical tube where the motions are small, the equation of motion will be of this form,

$$\frac{d^2y}{dt^2}=\frac{a^2d^2y}{dx^2}-\frac{b^2d^2y}{dxdt};$$

where x denotes the distance from the origin measured parallel to the axis of a given stratum in the state of rest, y the same distance at the time t, and a^2 and b^2 are constants, the value of a^2 being the same as in the ordinary theory.

As this equation leads to the conclusion that there are two velocities, it results that, except perhaps in very rare instances, in which a duplication has been observed in sounds heard at very great distances, the proposed correction of the theory of the motion of elastic fluids will not practically affect the theory of sound.

By the method adopted in the case of elastic fluids, the author conceives himself to have established that, in what are commonly termed inelastic fluids, the pressure during motion will not be equal in all directions.

XI. *Intelligence and Miscellaneous Articles.*

BIOGRAPHICAL NOTICE OF THE LATE JOHN TAYLOR, ESQ., F.R.S., F.G.S., ETC.

MR. JOHN TAYLOR was the eldest of five sons of Mr. John Taylor of Norwich, whose name is still held in traditional respect in his native city, and in affectionate remembrance by the few who survive to cherish his memory.

Mr. Taylor early evinced a strong turn (perhaps it is not too much to say genius) for mechanical pursuits.

In the flat and agricultural county of Norfolk there was little to suggest projects for the engineer. The nearest approach to an employment suited to a young man of such a turn seemed to be that of land-surveying, to which John Taylor was accordingly brought up. But at the early age of nineteen he was invited by some of the shareholders (or, as they are called in Cornwall, adventurers) in Wheal Friendship Mine to undertake the management of that important concern. The friends who thus sought to commit their interests to his care were perfectly aware of his inexperience in all that regarded mining. But they wanted a man of talents and industry upon whose honour and integrity they could implicitly rely, and they believed that the principles in which he had been trained afforded a guarantee for their choice.

It had been the practice for the agents to supply the necessary articles of consumption in the mines, such as timber, candles, &c.,—a practice which necessarily led to great abuse. Mr. Taylor immediately introduced the system of open contract, which has continued in operation ever since.

As early as 1800 we find in the Philosophical Magazine an article by him on the history of mining in Devon and Cornwall. In following years contributions appeared in that and other journals on mining-machinery, on the ventilation of mines*, on the economy of mines, on smelting, and on kindred subjects. These papers embodied the results of his practical experience†.

Among the proofs of his constant and earnest desire to elevate the art of mining and to found it on a scientific basis, we may mention that, in 1829, he undertook the editorship, and, to a considerable extent, the cost, of a work which he hoped would conduce to the end he had so much at heart, entitled "Records of Mining." The volume, or part, contains four papers from his own pen, the first of which is a "Prospectus of a School of Mines in Cornwall"‡. "If," says he, in this excellent and well-digested paper, "what has become the theme of praise in other parts of Europe be not applicable to England, it must be either because our mines do not require intelligence and skill for their management, or that our miners are not

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* In 1810 he received the Silver Medal of the Society of Arts for his "ingenious invention for ventilating mines."

† A list of these papers will be found at the end.

‡ The volume was entitled Part I., and Mr. Taylor hoped that a sufficient interest existed in mining and the allied subjects to support it; but no second Part appeared.

likely to have their intelligence and skill advanced by the most obvious means for doing so.

"As to the first, it is well known that one effect of late efforts in mining in England has been to deepen the mines with a rapidity totally unprecedented, to consolidate smaller concerns into larger ones, to explore more perfectly the ground in all directions, to adapt means that might render labour productive of profit, to stimulate the labourer by combining his interest with that of his employer, to watch every symptom with care, and to employ every device that ingenuity could suggest to overcome difficulties. It must then obviously follow that there is a greater demand for skill in the conduct of these affairs, as the mines are increasing in depth and extent; numerous expedients to counterbalance these difficulties are required; and as the expenses increase, compensation must be looked for in the aids that science may afford."

In the foregoing description of the progress made in the art of mining, Mr. Taylor does in fact trace the history of his own labours. Those who are conversant with what has been done since the beginning of the century, know how much of it may be ascribed to him.

The prospectus affords ample evidence not only of his zeal in the cause he advocates, but of the remarkable talent for organization and administration which from the first had distinguished his practical career. After pointing out the need for a school of mines, Mr. Taylor proceeds to what he calls the detail, which he arranges under the following heads:—

"1. The things most proper to be taught.
"2. The class of persons who may be expected to be scholars.
"3. The professors.
"4. The situation of a school of mines.
"5. The probable expense of the institution.
"6. The means of providing the necessary funds.
"7. The government or direction."

Each of these heads is treated specially, and shows not only perfect acquaintance with the whole matter to be considered, but perfect judgment and good sense. Nothing is proposed to be done or learned in order to give an air of scientific depth to the studies; no curious but inapplicable research, or ambitious flights into purely speculative regions are attempted; the end is kept steadily in view, and the road to it distinctly marked out.

This plan, excellent as it was, met with no encouragement at the time. It was, however, adopted some years after. The School of Mines which now exists, though he had no active share in its establishment, may be traced to Mr. Taylor's earnest representations.

Owing to circumstances upon which it is needless here to dilate, he quitted Tavistock in the year 1812 and came to settle in London. On his removal he, together with his brother Mr. Philip Taylor, engaged in a chemical manufactory at Stratford in Essex. The science of chemistry, which had always been a favourite pursuit with him, now became his profession. He was indefatigable in his private laboratory as well as in the manufactory. And he was in communication with the greatest chemists of the day, both English and foreign. We have the highest authority for affirming that, up to a certain period, he was regarded by them as one of the first metallurgical chemists of this country*.

Mr. Taylor's ardour in chemical discovery was stimulated and encouraged by daily intercourse with his brother. In conjunction with him, he made a series of experiments upon the gas obtained from oil, with a view to test its illuminating quality, and apply it to economical uses. These experiments led to the invention of an apparatus for making and distributing oil-gas. That pure and brilliant gas was introduced into some towns and public buildings, especially Covent Garden Theatre, and the Imperial Palace at St. Petersburg. Its greater costliness, however, prevented its general adoption.

But the increasing extent and importance of his mining business compelled him to withdraw from all other pursuits, and give himself up exclusively to the management of mines. Those which he directed from 1799 to 1812 were, as we have said, on the borders of Devonshire and Cornwall. To these were now added the Consolidated Mines, and others in the latter county.

About the year 1820 he was requested to take the management of the mineral property of the Duke of Devonshire in Staffordshire, Yorkshire, and Ireland; in 1822 he undertook the direction of Lord Grosvenor's mines in Wales, of the Mold mines in the same country; and in 1823, of those in Cumberland belonging to Greenwich Hospital.

Mr. Taylor was a member of many scientific and learned societies, British and foreign. In 1825 he was elected a Fellow of the Royal Society. He was one of the earliest Fellows of the Geological Society, and in 1816 was appointed its Treasurer, an office which he retained till 1844.

Mr. Taylor was one of the earliest and most active members of the British Association for the Advancement of Science. The first meeting of the first Council of that body was held at his house in Bedford Row, on the 26th of June 1832.

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* In 1823 he delivered a course of lectures on metallurgy at the London Institution.

Mr. Taylor was then requested to undertake the office of Treasurer, which he held till Sept. 1861, when a letter was read from his two sons, requesting, on the part of their aged father, that he might be relieved from the office he had held so long. This elicited a warm and unanimous expression of respect for his character, and gratitude for his long and valuable services to the cause of science.

Mr. Taylor was also Treasurer of the London University College for many years; a member of the Club of Social Chemists, which then (about the year 1820) numbered among its members Mr. Brande, Mr. Bingley (Master of the Mint), Dr. Fitton, Dr. Paris, Mr. Richard Phillips, Mr. Haseldine Pepys, John George Children, Barto Valle, and other eminent chemists; and also of the Smeatonian Club, to which the Rennies and other eminent engineers belonged. The profession of Mining Engineer was elevated by him, and the domain of mechanics extended and enriched by the knowledge and application of Geology, Mineralogy, Chemistry, and Metallurgy.

He wrote little, for he had other and more enduring ways of employing his peculiar talents, and he wrote only when he had something to say which he wanted to make known. His work, although always guided and enlightened by science, was mainly practical, and consisted rather in the application of scientific principles to methods, processes, and machinery than in the discovery of the principles themselves. His writings have never been collected. Indeed he seems to have attached little importance to them; and the very existence of some was unknown to his family,—for instance the excellent introduction to Risden's 'Survey of Devon,' ed. 1811. This full and able paper treats of the soil, agriculture, manufactures, means of defence, and (more especially) mineral products of the county. The only part relating to himself and his own works is the description of the canal which he cut from Tavistock to the river Tamar. Of this work, including a long and difficult tunnel, he was the sole engineer; and he had to encounter obstacles which at that early period the experience of others had scarcely taught the way to surmount. In this enterprise, and in all the more difficult operations in which he was engaged, he had to a considerable extent to invent his own methods.

For a long time before his death his memory had been failing, and disease had gradually impaired the higher faculties of his mind. But, even amidst the decay of his intellectual powers, the kindness of his heart, and the instinctive politeness which struck and charmed all who approached him, did not desert him. The gleams of his benevolent consideration for others, which broke forth out of the mental twilight in which he sat, were inexpressibly affecting. He passed away peacefully to the enjoyment of that light which shall know no cloud nor dimness.

Mr. Taylor leaves two sons and one married daughter, and numerous descendants in the second generation.

LIST OF CONTRIBUTIONS TO SCIENTIFIC JOURNALS.

In the Transactions of the Geological Society.

1st Series.

Vol. ii. p. 309. On the Economy of the Mines of Cornwall and Devon.
Vol. iv. p. 146. Description of the Tunnel of the Tavistock Canal, through Morwel Down, in the County of Devon.
Vol. v. p. 358. On the Smelting of Tin Ores in Cornwall and Devonshire.

2nd Series.

Vol. v. p. 137. Observations on the Strata penetrated in sinking a well at Diss, in Norfolk.

Philosophical Magazine.

1st Series.

Vol. v. p. 357. Sketch of the History of Mining in Devon and Cornwall. —January, 1800.
Vol. xxx. p. 97. On blasting Rocks and Tamping.—May 1808.
Vol. xxxvii. p. 129. On Water-pressure Engines.
Vol. xxxviii. p. 120. Method of ventilating Mines or Hospitals, by extracting the foul air from them.
(From the Transactions of the Society of Arts. The Society's Silver Medal was voted to Mr. John Taylor for this communication.)
Vol. lix. p. 417. On the Smelting of Tin Ores in Cornwall and Devonshire.
Vol. lxi. pp. 283, 368, 448. Sketch of Lectures on Metallurgy delivered at the London Institution in 1823.

2nd Series.

Vol. i. p. 126. On the accidents incident to Steam-boilers.
Vol. ii. p. 406. Description of a Rain-gauge: and in later volumes two short papers on "Rotatory Steam-engines."
Vol. iv. p. 108, and vol. vi. p. 133. On the Duty of Steam-engines in Cornwall.—1835.

British Association Report.

Vol. i. p. 578. Collection and arrangement of Vein-stones, and their connexion with the rocks in which they occur.—1832.
Vol. ii. p. 1. Report on the state of knowledge respecting Mineral Veins. —1833.
" p. 427. On the Depth of Mines.
Vol. v. p. 144. On the Value of Mineral Productions of Great Britain.— 1836.
Vol. x. p. 106. On the Water-power at Wheal Friendship Mine.—1841.

ON THE WAVE OF HIGH WATER, WITH HINTS TOWARDS A NEW THEORY OF THE TIDES. BY THOMAS CARRICK, ESQ.

The author, starting from a new hypothesis on the relations of terrestrial matter to cosmical force, has arrived at the conclusion that the tidal motions of ocean surfaces are caused by a differential action of force centring on land areas. The nature of this hypothesis, and its relation to those motions, were briefly illustrated from the point of view of an assumed nebulous origin of the solar system. The author nevertheless declined to endorse the received "nebular hypothesis" as a genetic theory, and adopted its ideas and phraseology in his paper solely from considerations of brevity in this incidental portion of his subject.

Assuming the existence of a diffused nebula, composed of ultimate atoms of matter each having a normal rotation on a fixed axis in a uniform direction, and with simple forces of attraction and repulsion arising thereout, then, from causes arising out of diverse molecular groupings of these atoms and their poles, the nebulous matter in condensing upon a centre might take up three successive states, constituting the normal types of the solid, liquid, and gaseous states of terrestrial science,—the solid matter forming a spherical nucleus, everywhere covered with a concentric layer of fluid, and this overlaid with a gaseous envelope; these varying layers of matter being in stable equilibrium at the respective surfaces of contact. The force exerted upon such a sphere by another of like origin would therefore act by and through the intermediation of these three states,—each successive stage of condensation, alike with a residual uncondensed nebulous matter of space, thus forming an essential link in the chain of gravitative action. But so soon as any portions of the solid nucleus emerge above the surface of the fluid covering, into abnormal contact with the gaseous envelope, a differential action of enormous magnitude, centring upon these upheaved land areas, would be at once originated, the first measure of which would be the cosmical value of the latent forces by which the fluid state of matter was constituted an essential intermediate link between the solid and gaseous states. In the view of the author, terrestrial matter in all its phases is now related to space, and to bodies in space, in a manner analogous to that which might have resulted from such a hypothetical origin. Not only does this matter exist in the three leading states of earth, water, and air, but each of the simpler forms of inorganic matter can, under given conditions, successively assume the solid, liquid, or gaseous state without undergoing any chemical change. This universal threefold relation of terrestrial matter points strongly towards the simple hypothesis, that the causal laws which now regulate these interchanges of state are the reflex of fundamental laws underlying the entire constitution of matter in the solar system.

Passing over the possible relation of the first land-upheaval to the early changes recorded by geology, and to the formation of heterogeneous solids, liquids, and gases, the differential force arising therefrom would be the initiating cause in the formation of the envelope of comminuted water or vapour which now encircles the whole globe.

This vapour-ocean constitutes an intermediate state of matter in unstable equilibrium with other states at all surfaces of contact. By interactions arising thereout, the simple static conditions of force existing prior to land-upheaval are now, in the view of the author, partly replaced by more complex phases of force; and thus light, heat, electricity, and magnetism, which are expressions of these complex phases, have their root in local reactions between unstable states of terrestrial matter at surfaces of abnormal contact when under the tension of cosmical force—just as all these "imponderable elements" are evoked in the voltaic battery by surface reactions of dissimilar solids and liquids in presence of atmospheric tension. In short, the ceaseless molecular changes and local motions of terrestrial matter would, on this hypothesis, be mainly referred to the differential action arising out of land-upheaval.

Recurring to the "wave of high water" which formed the special subject of the paper, another phase of the present residual of that differential action would give rise to the tidal motions of ocean surfaces, the perturbative action centring on land areas, and attaining a maximum value on the shores of those areas. By discussing the hours of high water at full and change for the principal places of the globe, given in the Admiralty Tide Tables for 1863 (the data being first reduced to Greenwich mean time), the author arrived at the following law of the progression of the wave of high water:—

In all land areas in the northern hemisphere the wave of high water tends to revolve round the coast in the direction of the hands of a watch, and in like areas in the southern hemisphere against the hands of a watch.

Theoretically, this law should hold good in proportion as land areas approximate to the circular form, with wide uninterrupted ocean spaces all round. In a perfectly circular area of this kind, the differential action would have points of maximum and minimum effect on opposite shores at every instant,—these together forming a nodal line, both ends of which would move simultaneously round the coast as the moon passed across the heavens, the wave of high water being everywhere the instantaneous expression of the differential force at its nodal point of maximum action.

By enclosing the continents and land areas which approach nearest to the prescribed conditions within one or more circles intersecting the salient parts of the coast, the author showed that whenever any systematic progression of the hour of high water could be distinctly traced, that progression is almost invariably in the required direction. Owing, however, to the irregular shape of all existing land areas, to the impossibility of including some of these in a single circle approximating to the coast line, and to the way in which some large areas are massed upon others with little or no intervening ocean spaces, many instances of anomalous results are found; and yet, when rightly considered in relation to disturbing causes, even these tend indirectly to confirm the method of grouping the data of tidal hours in relation to land areas as causal centres.—*Proceedings of the Manchester Literary and Philosophical Society, Physical and Mathematical Section*, April 30, 1863.

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[FOURTH SERIES.]

AUGUST 1863.

XII. *On the Theory of Light.* *By* L. LORENZ*.

If we consider all the hypotheses belonging to our present theory of light, that is to say, all those that are regarded as necessary for the explanation of the double refraction, chromatic dispersion, and polarization of light, it is difficult to guard ourselves from all doubt of so complex a theoretical apparatus, seeing that the probability of its truth must rapidly diminish as the number of hypotheses involved in it increases, notwithstanding that we may be convinced of the probability of each hypothesis taken by itself.

I have therefore tried to develope the theory of light with the smallest possible number of hypothetical assumptions, whether in regard to the nature of light itself, to that of the luminiferous medium, or to that of material bodies; and it will appear, as the result of the present investigation, that an essential part of the ordinary physical hypotheses are not needed for the explanation of the phenomena of light, inasmuch as the theory is capable of being carried through in a manner different from that which has been hitherto followed in the investigation of this subject, and consisting in the further development of the *formal* side of the theory.

I. *The Differential Equations of the Motion of Light in* HETEROGENEOUS, *non-absorbent Media.*

When the laws of the motion of light in homogeneous isotropic media, and those of its passage from one such medium to another of the same kind, are known, it is evidently possible to generalize

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* Poggendorff's *Annalen der Physik und Chemie*, vol. cxviii. p. 111 (February 1863).

the calculation so far that it shall also give the laws of the motion of light in heterogeneous media, since this motion must arise as the resultant effect of the rays that are transmitted and those that are reflected to infinity. I have in fact worked out the calculation for the comparatively simple case in which the medium is made up of very thin parallel layers; and the result shows that such a medium not only disperses light, but doubly refracts it, like an optically uniaxial crystal whose optic axis is perpendicular to the layers. Nevertheless, although the possibility of solving the problem in a more general form, and without any further physical assumptions, is already apparent, an imperfection is inherent in the method itself which led me on to insurmountable difficulties as soon as I attempted the case of a medium composed of two systems of parallel layers making an angle with each other. It then becomes necessary to have recourse to the more perfect mathematical methods, and to express the *laws* of the motion in heterogeneous media by means of partial differential equations, and to deduce thence, by subsequent integration, the path of the rays of light.

It is then also further needful to define our hypotheses with greater precision, and especially to introduce the conception of the plane of vibration instead of that of the plane of polarization. The motion must be determined in relation to magnitude and direction by the amplitude and direction of the vibrations; but although these expressions are borrowed from our common mode of considering the vibrations of elastic bodies, it is not intended to attach to them here any such absolute meaning; thus, for instance, the vibrations may be conceived of as *rotatory*, in which case the "direction of vibration" would be the direction of "the axes of rotation," and the "amplitude of vibration" would be the angular distance of the molecules from their position of equilibrium.

I must at once insist upon this double point of view, because the following calculation renders no further determination of these primary physical hypotheses necessary; and inasmuch as I shall not have another opportunity of returning in this memoir to this physical side of the theory of light, I may be allowed to point out at once that, so far from this latter mode of conceiving the vibrations being a mere mathematical speculation, the results of the present investigation appear to indicate it as probably the true foundation for the physical theory of light.

Retaining, then, the latitude of interpretation that has been indicated, the following are our assumptions:—

1. The "amplitude of vibration" and the "direction of vibration" give us three components ξ, η, ζ in the direction of the three rectangular axes of coordinates for determining the motion

at the point considered. We will now assume that the law of these vibrations is expressed, for isotropic homogeneous media, by the following partial differential equations:—

$$[\Delta^2\xi=\frac{1}{\omega^2}\frac{d^2\xi}{dt^2},\quad \Delta^2\eta=\frac{1}{\omega^2}\frac{d^2\xi}{dt^2},\quad \Delta^2\zeta=\frac{1}{\omega^2}\frac{d^2\zeta}{dt^2}, \quad . \quad (1)$$

where Δ^2 denotes $\frac{d^2}{dx^2}+\frac{d^2}{dy^2}+\frac{d^2}{dz^2}$, ω denotes a constant, and t the time.

Hence in such a medium, supposing it to be unlimited, th amplitude of vibration may be expressed by terms of the form

$$a\cos\,(kt-lx-my-nz+d),$$

or

$$(a+b\sqrt{-1})e^{(kt-lx-my-nz)\sqrt{-1}}.$$

Light accordingly consists of periodic undulations which propagate themselves in the direction of the perpendicular to the plane $lx+my+nz=0$, with the constant velocity

$$\omega=\frac{k}{\sqrt{l^2+m^2+n^2}}.$$

2. In homogeneous isotropic media the direction of vibration is perpendicular to the ray; or, more generally (including, that is, the case in which the medium is limited), the three components are connected by the differential equation

$$\frac{d\xi}{dx}+\frac{d\eta}{dy}+\frac{d\zeta}{dz}=0. \quad . \quad . \quad . \quad . \quad . \quad (2)$$

3. The plane drawn through the direction of vibration and through the direction of the ray is *perpendicular* to the *plane of polarization* of the ray. This assumption can scarcely affect the results in relation to the propagation of light in heterogeneous media, in so far as they come within the scope of our consideration, since, as already mentioned, it is possible to conceive this as deduced without any assumptions as to the position of the direction of vibration relatively to the plane of polarization. I consider it, nevertheless, as in any case proved by my experiments on the diffraction of light (Poggendorff's *Annalen*, vol. cxi. p. 315); for, as I shall show in the sequel, the assumptions that are there made can be deduced from the equations (1) and (2) alone.

These three hypotheses, then, in conjunction with Fresnel's formulæ for the refraction and reflexion of light at the limiting surface of two isotropic transparent media, form the foundation of the present theory of light. There can, in fact, no longer be any doubt that these formulæ correspond exactly with experiment, and that the slight discrepancies which Jamin has detected receive a natural explanation from the fact that the passage from one body to another takes place gradually. (Cf. Poggendorff's *Annalen*, vol. cxi. p. 460; also vol. cxiv. p. 244.) These slight discrepancies only serve as a further confirmation of the exact applicability of the formulæ when the refractive index of the two bodies undergoes a sudden or *infinitely small* alteration; of which cases the latter only will be here taken as the basis of the calculation. On the other hand, it is to be borne in mind that, although these formulæ are taken as the starting-point of the theory, they are to be regarded as deduced solely from experiment, so that equal weight must be given to any other formulæ which lead to equally concordant results.

If we accordingly endeavour to give to the formulæ their most general and most fully developed form, it becomes evident, in the first place, that we may add as a factor to the expression for the amplitude of vibration of the refracted ray an arbitrarily chosen power of the refractive index, since, in the refracting body, neither the intensity nor the direction of polarization are determined by experiment, and, besides this, the factor in question will disappear again. Even if we could determine the intensity experimentally in the refracting body, this factor would still be undetermined; for we may consider it as experimentally demonstrated that in one and the same substance the intensity is proportional to the square of the amplitude of vibration; but it is unknown what power of the index of refraction enters here as a factor. We shall nevertheless immediately show that this factor is the same for vibrations taking place in the plane of incidence and for those perpendicular thereto, an assumption which is necessary for the attainment of homogeneity in the following results. On this supposition, the factor in question has no influence on the rotation of the plane of polarization in the refracting body, and hence it cannot be determined by means of experiments on this rotation.

Again, the sign of the excursion of the reflected ray is to some extent undetermined. If the angle of incidence be denoted by α, and the angle of refraction by β, the ratio of the excursion of the incident, refracted, and reflected light will be, according to Fresnel's formula thus expanded, for vibrations in the plane of incidence, as

$$1:\frac{2\cos\alpha\sin\beta}{\sin(\alpha+\beta)\cos(\alpha-\beta)}\left(\frac{\sin\alpha}{\sin\beta}\right)^p:\pm\frac{\tan(\alpha-\beta)}{\tan(\alpha+\beta)};$$

and for vibrations perpendicular to the plane of incidence, as

$$1:\frac{2\cos\alpha\sin\beta}{\sin(\alpha+\beta)}\left(\frac{\sin\alpha}{\sin\beta}\right)^p:\mp\frac{\sin(\alpha-\beta)}{\sin(\alpha+\beta)}.$$

p is here the above-mentioned undetermined exponent, which in Fresnel's expressions is equal to nothing. The second source of uncertainty is the double sign, which, however, it must be observed, is *reversed* in the two cases. The *positive* direction of vibrations which take place in the plane of incidence may, for instance, be chosen so that, the plane of incidence being supposed horizontal, it is to the *left* of the observer when he is turned towards the ray, whether incident or reflected. Now it is a fact that when light is reflected at an angle of incidence of nearly 90°, for example, the azimuth of the plane of vibration does not change its sign, calculated in the manner already indicated, whence it follows that $\frac{\tan(\alpha-\beta)}{\tan(\alpha+\beta)}$ and $\frac{\sin(\alpha-\beta)}{\sin(\alpha+\beta)}$ are in this case $(\alpha+\beta>90^\circ)$ positive or negative at the same time, which can occur only if the two expressions have opposite signs.

Nevertheless we use the formulæ that have been given only for the case in which the angle of incidence differs infinitely little from the angle of refraction. If $\beta=\alpha+d\alpha$, the ratios for the vibrations in the plane of incidence become as

$$1:1+\left(\frac{1}{\sin 2\alpha}-\frac{p}{\tan\alpha}\right)d\alpha:\pm\frac{d\alpha}{\tan 2\alpha}; \quad . \quad . \quad (3)$$

and for those which take place perpendicularly to the plane of incidence, as

$$1:1+\left(\frac{1}{\sin 2\alpha}-\frac{p}{\tan\alpha}\right)d\alpha:\mp\frac{d\alpha}{\sin 2\alpha}. \quad . \quad . \quad (4)$$

We will now, in the first place, suppose the heterogeneous body divided into layers in some given direction, in such wise that each layer perpendicular to the axis of x may be regarded as homogeneous for an infinitely small thickness, and as differing to an infinitely small amount from the next following layer. Within the limits of such a layer the ray moves as in a homogeneous body; at the limiting surface it is partially reflected; and both in the incident and in the reflected ray the vibrations are perpendicular to the direction of the ray. We here take into consideration the elementary rays or the *virtual* motion of the light; the *actual* motion thence resulting may perhaps exhibit a different direction of vibration, since, for example, all the rays reflected from the different layers may neutralize each other, although they may still exert an influence on the direction of the vibrations in the transmitted ray.

If we assume that the plane of coordinates xz coincides with the plane of incidence, the excursion at the entrance of the light into the substance, at the surface $x=0$, may be expressed by terms of the form

$$Ae^{(kt-nz)\sqrt{-1}}=p. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

As the ray penetrates further into the substance, it undergoes continually a partial reflexion, while in the transmitted portion both A and the exponent vary. The expression for the excursion in a layer which we may call the layer (α), wherein the angle of incidence is α, takes consequently the form

$$\rho \mathrm{P} e^{-\delta\sqrt{-1}}, \quad \ldots \ldots \quad (6)$$

in which ρ and δ (the "retardation of phase") are functions of x.

In the following layer α becomes $\alpha+d\alpha$, and ρ becomes $\rho+d\rho$, and, by (3) and (4), whatever the direction of vibration may be,

$$d\rho=\rho\left(\frac{1}{\sin 2\alpha}-\frac{p}{\tan \alpha}\right)d\alpha.$$

From this equation we get by integration

$$\rho=\sqrt{\frac{\tan \alpha}{\tan a}}\left(\frac{\sin a}{\sin \alpha}\right)^{p}, \quad \ldots \ldots \quad (7)$$

where a denotes the angle of incidence for $\rho=1$, or for the point where the ray enters the substance.

For the ray reflected at this same layer $(\alpha+d\alpha)$, ρ becomes $\mp\rho\frac{d\alpha}{\tan 2\alpha}$ when the vibrations are in the plane of incidence, and $\pm\rho\frac{d\alpha}{\sin 2\alpha}$ when they are perpendicular thereto. Let *both* these values be denoted by ρdu, where, in the first case,

$$u=\mp\tfrac{1}{2}\log \sin 2\alpha, \quad \ldots \ldots \quad (8)$$

and in the second

$$u=\pm\tfrac{1}{2}\log \tan \alpha. \quad \ldots \ldots \quad (9)$$

For the various layers (α_1), (α_2), &c., we will use the same symbols ρ, δ, and u, distinguished by the indices 1, 2, . . . &c., respectively, and we will denote the corresponding functions for the layers (a) and (b), which we will suppose to be the last layer of the substance, by ρ_a, δ_a, u_a, and ρ_b, δ_b, u_b.

The amplitude of vibration for the ray reflected at the layer (α_2) is accordingly

$$\rho_2 \mathrm{P} e^{-\delta_2\sqrt{-1}} du_1.$$

When this ray, whose retardation of phase is here δ_2, passes into a subsequent layer (α_2), its phase is further retarded by $\delta_2-\delta_1$; the corresponding amplitude is therefore now

$$\rho_1 \mathrm{P} e^{(-2\delta_2+\delta_1)\sqrt{-1}} du_2.$$

At the surface of the next following layer the ray is again partially reflected, and the amplitude for the reflected portion is

$$-\rho_1 \mathrm{P} e^{(-2\delta_2+\delta_1)\sqrt{-1}} du_2 \,.\, du_1.$$

Finally, the ray reaches the layer (α), after a further retardation of phase amounting to $\delta-\delta_1$, with the amplitude

$$-\rho \mathrm{P} e^{(-2\delta_2+2\delta_1-\delta)\sqrt{-1}} du_2 \,.\, du_1.$$

The *sum* of the amplitudes of the rays which have been *twice* reflected at *all* the layers is then given by the double integral of this expression, u_2 varying from u_1 to u_b, and u_1 then varying from u_a to u. This sum is therefore

$$-\rho \mathrm{P} e^{-\delta\sqrt{-1}} \int_{u_a}^{u} du_1 \int_{u_1}^{u_b} du_2 e^{2(\delta_1-\delta_2)\sqrt{-1}}; \quad . \quad . \quad (10)$$

whereas, according to (6), the amplitude for the unreflected ray was

$$\rho \mathrm{P} e^{-\delta\sqrt{-1}}.$$

Now let

$$\rho \mathrm{P} e^{-\delta\sqrt{-1}} \mathrm{U} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (11)$$

be the amplitude for the compound ray, made up of *refracted* rays that have been reflected 0, 2, 4 . . . . times; we shall then be able to express U by the equation

$$\mathrm{U}=1-\int_{u_a}^{u} du_1 \int_{u_1}^{u_b} du_2 e^{2(\delta_1-\delta_2)\sqrt{-1}} \mathrm{U}_2, \quad . \quad . \quad . \quad (12)$$

where U_2 denotes the same function of u_2 that U does of u. Introducing into the expression this value of U_2, we get U expressed by an infinite series. The accuracy of the expression (11) is apparent, not only from this evolution of an infinite series, the several terms of which give the amplitudes of the rays that have been reflected 0, 2, 4, ... times respectively, but from its being possible to conceive this compound ray as again twice reflected at every layer, without the expression for the amplitude undergoing any modification whatever.

Again, the amplitude for the compound *reflected* ray, which has suffered 1, 3, 5, ... reflexions, will be expressed by

$$-\rho \mathrm{P} e^{-\delta\sqrt{-1}} \frac{d\mathrm{U}}{du}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (13)$$

By (12) this expression is equivalent to

$$\rho \mathrm{P} \int_{u}^{u_b} du_2 e^{(\delta-2\delta_2)\sqrt{-1}} \mathrm{U}_2,$$

and this integral is precisely the sum of the amplitudes of the compound refracted ray which has been once reflected to the layer (α).

The two compound rays, reflected and refracted, represent the actual movement of the light in the substance. If the vibrations take place perpendicularly to the plane of incidence, the direction of vibration is the same in both rays, and the actual amplitude is then the sum of the two expressions (11) and (13). If, on the other hand, the vibrations take place in the plane of incidence, they make different angles with the axes in the two rays. They are, as already stated, perpendicular to the ray; and if the *positive* direction is taken as to the *left* of the observer when turned towards the ray, the vibrations of the incident ray make in the layer (α) an angle of $90°-\alpha$ with the axis of x, and an angle of $180°-\alpha$ with the axis of z; while in the reflected ray they make an angle of $90°-\alpha$ with the axis of x, and of α with the axis of z. Hence, if the components of the actual excursion in the direction of these two axes be denoted by ξ and ζ, we have

$$\xi = \sin \alpha \rho \mathrm{P} e^{-\delta\sqrt{-1}}\left(\mathrm{U}-\frac{d\mathrm{U}}{du}\right), \quad . \quad . \quad . \quad (14)$$

$$\zeta = -\cos \alpha \rho \mathrm{P} e^{-\delta\sqrt{-1}}\left(\mathrm{U}+\frac{d\mathrm{U}}{du}\right). \quad . \quad . \quad (15)$$

The function U is determined by (12), or by the differential equation deduced from this value,

$$\frac{d^2\mathrm{U}}{du_2} - 2\sqrt{-1}\frac{d\delta}{du}\cdot\frac{d\mathrm{U}}{du} = \mathrm{U}. \quad . \quad . \quad . \quad . \quad (16)$$

If we now put

$$e^{-\ \sqrt{-1}}\left(\mathrm{U}-\frac{d\mathrm{U}}{du}\right)=s, \quad e^{-\delta\sqrt{-1}}\left(\mathrm{U}+\frac{d\mathrm{U}}{du}\right)=s', \quad . \quad (17)$$

we have, by writing $d\delta$ in place of $\frac{d\delta}{du}du$,

$$\frac{de^u s}{d\delta} = -\sqrt{-1}e^u s', \quad \frac{de^{-u}s'}{d\delta} = \sqrt{-1}e^{-u}s,$$

whence

$$\frac{d}{d\delta}\left(e^{-2u}\frac{de^u s}{d\delta}\right)+e^{-u}s=0, \quad \frac{d}{d\delta}\left(e^{2u}\frac{de^{-u}s'}{d\delta}\right)+e^{u}s'=0. \quad (18)$$

We will further substitute other expressions for $\sin\alpha$ and $\cos\alpha$ in (14) and (15). Let it be remembered that by (5) and (6) the amplitude for the simple transmitted ray was determined by terms of the form

$$\mathrm{A}\rho e^{(kt-\delta-nz)\sqrt{-1}}.$$

In the element of time dt the motion has been propagated through the small space $\cos\alpha\, dx$ or $\sin\alpha\, dz$ with the velocity ω, determined accordingly by

$$\omega dt = \cos \alpha dx \text{ or } \omega dt = \sin \alpha dz.$$

But while the time and the coordinates of space vary simultaneously, the phase $(kt - \delta - nz)$ remains unaltered. We thus have

$$0 = kdt - \frac{d\delta}{dx} dx \text{ and } 0 = kdt - ndz,$$

which equations, in conjunction with the foregoing, give

$$k \cos \alpha = \omega \frac{d\delta}{dx} \text{ and } k \sin \alpha = \omega n. \quad . \quad . \quad . \quad (19)$$

If the values of p and ρ given in (5) and (7) be now introduced into the expressions for the components ξ and ζ, and if we further put

$$\omega^p \xi = \bar{\xi} \text{ and } \omega^p \zeta = \bar{\zeta}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (20)$$

we get

$$\bar{\xi} = A_1 \frac{\omega n s}{\sqrt{\frac{d\delta}{dx}}} e^{(kt-nz)\sqrt{-1}}, \quad . \quad . \quad . \quad . \quad . \quad (21)$$

$$\bar{\zeta} = -A_1 \omega \sqrt{\frac{d\delta}{dx}} s' e^{(kt-nz)\sqrt{-1}}, \quad . \quad . \quad . \quad (22)$$

in which A_1 represents a constant factor.

We have, further, by (8),

$$u = \mp \tfrac{1}{2} \log \sin 2\alpha = \text{const.} \mp \tfrac{1}{2} \log \left(\omega^2 \frac{d\delta}{\delta x}\right),$$

from which last expression the constant may be omitted. We thus have, in one of the two cases resulting from the double sign,

$$u = -\tfrac{1}{2} \log \left(\omega^2 \frac{d\delta}{\delta x}\right). \quad . \quad . \quad . \quad . \quad . \quad . \quad (23)$$

By putting this value of u into the expressions for s and s', and regarding x as an independent variable, we obtain by the equations (21) and (22),

$$\frac{d}{dx}\left[\omega^2 \frac{d \frac{1}{\omega^2} \bar{\xi}}{dx}\right] + \left(\frac{d\delta}{dx}\right)^2 \bar{\xi} = 0, \quad . \quad . \quad . \quad (24)$$

$$\frac{d}{dx}\left[\frac{1}{\omega^2 \left(\frac{d\delta}{dx}\right)^2} \frac{d\bar{\zeta}}{dx}\right] + \frac{1}{\omega^2} \bar{\zeta} = 0. \quad . \quad . \quad . \quad (25)$$

By putting

$$\frac{1}{\omega^2\left(\frac{d\delta}{dx}\right)^2}\frac{d\bar{\zeta}}{dx}=\phi$$

in the last equation, it is transformed into

$$\frac{d\phi}{dx}+\frac{1}{\omega^2}\bar{\zeta}=0, \quad \ldots \ldots \ldots \quad (26)$$

$$\frac{d\omega^2\frac{d\phi}{dx}}{dx}+\omega^2\left(\frac{d\delta}{dx}\right)^2\phi=0. \quad \ldots \ldots \quad (27)$$

According to equations (19), $\left(\frac{d\delta}{dx}\right)^2=\frac{k^2}{\omega^2}-n^2$; and calling to mind that t and z enter into the magnitudes $\bar{\xi}$, $\bar{\zeta}$, ϕ only by the factor $e^{(kt-nz)\sqrt{-1}}$, it is evident that we may put

$$-\frac{d^2}{dt^2},\quad \sqrt{-1}\frac{d}{dz},\quad -\frac{d^2}{dz^2}$$

in place of the respective factors k^2, n, n^2.

We thus get from equation (27)

$$\frac{d\omega^2\frac{d\phi}{dx}}{dx}+\omega^2\frac{d^2\phi}{dz^2}=\frac{d^2\phi}{dt^2}. \quad \ldots \ldots \quad (28)$$

Equation (24) may also be deduced from the same equation (27) if we put

$$\omega^2\phi=a\bar{\xi}.$$

The constant a cannot, however, assume whatever value we please; for if this value of ϕ be introduced into (26), we obtain

$$a\frac{d\frac{1}{\omega^2}\bar{\xi}}{dx}+\frac{1}{\omega^2}\bar{\xi}=0,$$

an equation which must also hold good if ω is independent of x. But in this case, by our second hypothesis (2), $\frac{d\xi}{dx}+\frac{d\zeta}{dz}=0$, or here $\frac{d\bar{\xi}}{dx}-n\sqrt{-1}\bar{\zeta}=0$, whence follows $a=\frac{\sqrt{-1}}{n}$. The equation thus becomes

$$\frac{d\frac{1}{\omega^2}\bar{\xi}}{dx}+\frac{d\frac{1}{\omega^2}\bar{\xi}}{dz}=0. \quad \ldots \ldots \quad (29)$$

By now differentiating equation (28) in relation to z and in relation to x, and by putting $\frac{d\phi}{dz}=\frac{1}{\omega^2}\overline{\xi}$ and $\frac{d\phi}{dx}=-\frac{1}{\omega^2}\overline{\zeta}$, we have

$$\frac{d^2\overline{\xi}}{dz^2}-\frac{d^2\overline{\zeta}}{dx\,dz}=\frac{1}{\omega^2}\frac{d^2\overline{\xi}}{dt^2}, \quad . \quad . \quad . \quad . \quad . \quad (30)$$

$$\frac{d^2\overline{\zeta}}{dx^2}-\frac{d^2\overline{\xi}}{dx\,dz}=\frac{1}{\omega^2}\frac{d^2\overline{\zeta}}{dt^2}. \quad . \quad . \quad . \quad . \quad . \quad (31)$$

The calculation can be conducted in the same manner when, in the expression for u, the lower sign is taken, that is, when we take

$$u=\tfrac{1}{2}\log\left(\omega^2\frac{d\delta}{dx}\right);$$

but in this case the results are no longer equally simple. For instance, $\frac{d\delta}{dx}=\sqrt{\frac{k^2}{\omega^2}-n^2}$ enters as a factor into the magnitudes $\overline{\xi}$ and $\overline{\zeta}$, and hence it becomes impossible to determine them by differential equations of the second degree. In order for this to be possible, we are obliged to read the *upper* sign in Fresnel's formulæ as they are given above, and we will not further pursue the consequences of the opposite assumption.

If the vibrations of the ray are perpendicular to the plane of incidence, we have by (9), in case the upper sign be alone taken,

$$u=\tfrac{1}{2}\log\tan\alpha=\text{const.}-\tfrac{1}{2}\log\frac{d\delta}{dx}.$$

The actual excursion η is the sum of the two expressions (11) and (13), or

$$\eta=\rho\mathrm{P}e^{-\delta\sqrt{-1}}\left(\mathrm{U}-\frac{d\mathrm{U}}{du}\right)=\rho\mathrm{P}s.$$

If we put

$$\omega^p\eta=\overline{\eta}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (32)$$

we get

$$\overline{\eta}=\mathrm{A}_1\frac{ks}{\sqrt{\frac{d\delta}{dx}}}e^{(kt-nz)\sqrt{-1}}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (33)$$

where A_1 is, as before, a constant factor. The first equation (18) now gives

$$\frac{d^2\overline{\eta}}{dx^2}+\left(\frac{d\delta}{dx}\right)^2\overline{\eta}=0$$

or

$$\frac{d^2\overline{\eta}}{dx^2}+\frac{d^2\overline{\eta}}{dz^2}=\frac{1}{\omega^2}\frac{d^2\overline{\eta}}{dt^2}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (34)$$

From the equations thus obtained for $\overline{\xi}, \overline{\eta}, \overline{\zeta}$ the general equations, applicable to the case in which the components are also dependent on y, can be deduced. They must of necessity assume the following form:—

$$\left.\begin{aligned}\Delta^2\overline{\xi}-\frac{d\overline{\theta}}{dx}&=\frac{1}{\omega^2}\frac{d^2\overline{\xi}}{dt^2},\\ \Delta^2\overline{\eta}-\frac{d\overline{\theta}}{dy}&=\frac{1}{\omega^2}\frac{d^2\overline{\eta}}{dt^2},\\ \Delta^2\overline{\zeta}-\frac{d\overline{\theta}}{dz}&=\frac{1}{\omega^2}\frac{d^2\overline{\zeta}}{dt^2},\end{aligned}\right\} \quad \ldots\ldots \quad (A)$$

in which

$$\Delta^2=\frac{d^2}{dx^2}+\frac{d^2}{dy^2}+\frac{d^2}{dz^2},$$

and

$$\overline{\theta}=\frac{d\overline{\xi}}{dx}+\frac{d\overline{\eta}}{dy}+\frac{d\overline{\zeta}}{dz}.$$

These equations bring us back again to the equations (30), (31), and (34), if the components are independent of y.

We can further conclude from the symmetrical form of the equations (A), which is unaltered by changing the axes of coordinates, that *they would still hold good if* ω, which has been hitherto considered a function of x only, *were any function whatever of* x, y, *and* z. Accordingly they express the general laws of the motion of light in any non-absorbent heterogeneous medium.

The *principles* which must serve for calculating the diffraction, reflexion, and refraction of light, or the conditions which must obtain when light passes from one medium into another, can now be easily deduced from these general equations. Let the plane of coordinates yz be the limiting plane for which the conditions are investigated. The equations will then be multiplied by dx and by $dx\,dx$, and simply and doubly integrated from $x=0$ to $x=\epsilon$, where ϵ is a very small quantity. Now, however, all the integrals will disappear except those whose elements become infinite, and this will be the case with those only which contain differential coefficients in relation to x. The second equation (A) thus gives

$$\left[\frac{d\overline{\eta}}{dx}-\frac{d\overline{\xi}}{dy}\right]_{x=0}^{x=\epsilon}=0 \text{ and } [\overline{\eta}]_{x=0}^{x=\epsilon}=0. \quad \ldots \quad (35)$$

Similarly, the third equation (A) gives

$$\left[\frac{d\overline{\zeta}}{dx}-\frac{d\overline{\xi}}{dz}\right]_{x=0}^{x=\epsilon}=0 \text{ and } [\overline{\zeta}]_{x=0}^{x=\epsilon}=0. \quad \ldots \quad (36)$$

In general the first equation (A) would likewise give two limiting equations, so that altogether six such equations would be thus obtained; but in the present case the first equation gives only

$$\left[\frac{d\eta}{dx}+\frac{d\zeta}{dz}\right]_{x=0}^{x=\epsilon}=0,$$

which could have been deduced from the foregoing, and the identical expression $0=0$.

The four limiting equations (35) and (36) are, however, just sufficient for the calculation. The theory of elasticity, on the other hand, gives six limiting conditions, whence we are also obliged to assume the formation of longitudinal vibrations in every case of refraction.

It has been previously mentioned that the hypotheses which I took (in Poggendorff's *Annalen*, vol. cxi.) as the basis for the calculation of the diffraction of light, are deducible from our first assumptions (1) and (2). This is possible by the methods here employed, if I multiply the equations (1) by dx and by $dx\,dx$, and integrate simply and doubly from $x=0$ to $x=\epsilon$. The six equations thus obtained express that the components of the vibrations, and the differential coefficients in relation to x, are the same on both sides of the plane $x=0$.

[To be continued.]

XIII. *An Experimental Examination of the so-called Storm-glass.* By Charles Tomlinson, *Lecturer on Physical Science, King's College School, London*[*].

THE storm-glass did not, I believe, hold rank as a scientific instrument until it was introduced by Admiral FitzRoy in his useful public labours. In the Report for 1862 issued by the Meteorological department of the Board of Trade, it is stated that "if fixed, undisturbed, in free air, not exposed to radiation, fire, or sun, but in the ordinary light of a well-ventilated room or outer air[†], the chemical mixture in a so-called storm-glass varies in character with the *direction* of the wind, not its force, *specially* (though it *may* so vary in *appearance* only) from another cause, *electrical tension.*

"As the atmospheric current veers toward, comes from, or is *approaching* only from the polar direction, this chemical mixture

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[*] Communicated by the Author.

[†] In Admiral FitzRoy's 'Weather Book' (1863) it is said "preferably in the outer air."

(microscopically watched) grows like fir or fern leaves, hoar frost, or crystallizations.

"As it tends to the opposite quarter, the lines or spikes—all regular hard and crisp features—gradually vanish. Before and in a continued southerly wind, the mixture sinks gradually downwards in the vial till it becomes shapeless like melted sugar.

"Before or during the continuance of a northerly wind (polar current) the crystallizations are beautiful (if the mixture is correct, the glass a *fixture*, and duly *placed*). The least motion of the liquid disturbs them. The glass should be wiped clean now and then gently if exposed to rain or dust*.

"While any *hard* or *crisp* features are visible below, above, or at the top of the liquid (where they form for much north wind), there is *plus* electricity in the air; a *mixture* of polar current coexisting *in that locality* with the opposite or southerly. When nothing but soft, melting, sugary substance is seen, the atmospheric current (feeble or strong, as it may be) is southerly†, unmixed with, and *uninfluenced* by, the contrary wind‡.

"By repeated trials with a delicate galvanometer, applied to measure electrical tension in the air, I have proved these facts, and now find them useful for aiding, with the barometer and thermometer, in forecasting weather.

"Temperature affects the mixture much, but not solely, as many comparisons of winter with summer changes of temperature have fully demonstrated."

In the 'Weather Book' it is stated that "clearness of the liquid, with more or less perfect crystallizations, accompanies a combination or a contest of the main currents by the *west*. And very remarkable these differences are,—the results of these air currents acting on each other *from* eastward, or entirely from an opposite direction, the *west*" §.

The composition of the storm-glass is said to be "camphor, nitrate of potassium and sal-ammoniac, partly dissolved by alcohol, with water and some air, in a hermetically sealed glass

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* In the 'Weather Book' it is added, "once or twice in a year the mixture should be disturbed by inverting and gently shaking the glass vial."

† In the 'Weather Book' it is added, "with *minus* electricity."

‡ "A confused appearance of the mixture with flaky spots or stars in motion and less clearness of the liquid indicates south-east wind, perhaps a gale."

§ Several incidental passages in the 'Weather Book' confer a high scientific value on the storm-glass. Thus at page 209 it is said that "thermometers and camphor-glasses also contribute their indications" in predicting local storms. And again, page 232, "camphor-glasses in proper positions and duly attended are most useful to a quick eye and skilled perception."

vial;" and it is further added that "there are many imitations more or less incorrectly made."

The important services which this glass is said to afford to meteorology render it desirable to know which is the imitation and which the incorrect instrument.

In 'The Druggist's General Receipt Book,' by Henry Beasley, fifth edition, 1861, page 428, the following recipe is given:—"Take 2½ drachms camphor, 38 grains nitre, and 38 grains of sal-ammoniac, dissolve in 9 drachms water and 6 drachms of rectified spirit with a gentle heat. The mixture is to be put into a long glass tube and closed with a brass cap with a small hole in it to admit air, or in an eau-de-cologne or other long bottle tied over with bladder." The account goes on to state that the various changes of the storm-glass "are supposed to indicate changes of weather, but the indications are not to be relied on."

It will be seen that the above directions do not agree with Admiral FitzRoy's, that the glass be hermetically sealed.

In the last edition of 'Ure's Dictionary of Arts,' &c., edited by Mr. Robert Hunt, under the head CAMPHOR STORM-GLASSES a similar recipe is given, except that 11 instead of 7 drachms of rectified spirits of wine are prescribed; and it is stated, on the authority of Dr. Parrion, that "the weather-predicting qualities of the instrument are false, and that *light* is the agent which, together with *temperature*, influences the condition."

I have been unable to ascertain where Dr. Parrion's paper is published, nor have I succeeded in tracing the history of the instrument. Admiral FitzRoy states that "considerably more than a century ago what were called 'storm-glasses' were made in this country. Who was the inventor is now very uncertain; but they were sold *on old London Bridge*, at the sign of the 'Goat and Compasses.'"

Some years ago I purchased a storm-glass at an instrument-maker's shop in Hatton Garden, and received with it a paper, of which the following is a copy:—

"CHEMICAL WEATHER-GLASS, usually called a *storm-glass*, which will correctly indicate the coming rain, high wind, storm, or tempest.

"This instrument will be preferable to others on account of its portability and cheapness.

"*Indications.*

"1st. For fine weather, the substance will be low and smooth at the bottom.

"2nd. For rain, the substance will rise gradually in the shape of feathers.

"3rd. For high wind or storm, the substance will rise as above, but much higher, partly at the top; sometimes white spots will appear in motion, particularly in the first part of the change—that denotes storm.

"Experience has proved this instrument to be highly useful in the prognostics of the changes that take place in the atmosphere, *if properly and correctly made.*

"The above instrument has been observed to indicate rain forty-eight hours before it took place."

I have been in the habit of observing this instrument for some years past, and had long ceased to regard it as of any value as a weather-indicator, but supposed the various phenomena to be brought about by the action of heat and light. While engaged, however, in investigating the phenomena which accompany the motion of camphor towards the light*, I was led to attend more minutely to the storm-glass, and arrived at the conclusion that heat is the only agent concerned, although the phenomena may be complicated somewhat by the composition of the mixture and the repeated crystallizations and solutions of the ingredients. For example, nitre, which is freely soluble in water, is insoluble in alcohol, the addition of which throws down a portion of the salt from its aqueous solution; camphor, on the other hand, is freely soluble in alcohol, and very sparingly so in water, so that the addition of water to camphorated spirit precipitates camphor. The mixture is so composed that a portion of the solid is always precipitated; and the effect of a rise of temperature is to take up more of the solid, and of a fall in temperature to precipitate it. As the liquid is in a state of saturation at a certain temperature, the top crust and the lower deposit come into contact as that temperature is approached; but this seldom happens, since the crystallization of the solid renders sensible an amount of latent heat, which again increases the solvent power of the menstruum independently of the external temperature. In fine weather also, especially if the glass have access to the external air by means of a small hole or otherwise, there is a strong tendency for crystallization to take place at the surface where the air, the crystals, and the sides of the glass meet. Adhesion is thus promoted, so much so that the surface of the crust may become dry and throw off vapour of camphor, which forms a crystalline deposit on the dry portion of the glass above the liquid on the side nearest the light, that is, on the coldest side. This upper crust is the principal seat of manufacture of the beautiful feathers with transparent quills which may be often seen heaped up on the lower deposit. As the feathers form above, they push the feathers already formed away from their contact with the

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* See Philosophical Magazine for November 1862.

crust; and they gently subside and become heaped up below. A very large storm-glass (which will be described presently) shows this action well. The feathers formed above are not only pushed down by other feathers, but are also detached by a slight tremor or agitation; as also by a rise in temperature, which loosens their hold; and in descending slowly through the clear liquid they often turn over and settle down on the lower deposit, thus presenting the appearance of having started up from that deposit, which is the general notion. The feathers that turn over often have small lumps of the top crust attached to them, so that the root end, so to speak, is the heavier. The minute stars which circulate between the upper crust and the lower deposit, which are said to indicate high wind or storm, are simple effects of cooling. During the months of April, May, and June 1862, a storm-glass hanging in an east window so as to receive the rays of the rising sun, constantly exhibited these stars moving in currents in the clear space between the two deposits after the sun had left the window. These minute stars are exceedingly beautiful, and consist of four, six, and eight radii, with several anomalous forms.

The ordinary storm-glass, suspended so as to be near a window-pane, will have its deposits often heaped up on the side near the cold glass, an effect which is most strikingly produced by putting the storm-glass mixture into a large white glass bottle in sufficient quantity to fill about two-thirds of it. In an experiment of this kind, the mixture was compounded of 20 drachms of camphor, 304 grains of nitre, 304 grains of sal-ammoniac, 72 fluid-drachms of distilled water, 96 fluid-drachms of spirits of wine, and 8 drops of oil of camphor. The mixture was gently warmed in an evaporating-dish until it reached 98°; it was then put into a quart white-glass bottle; and the stopple was tied over, the object being to see the effect of hermetically sealing.

On comparing this bottle with the storm-glass placed by the side of it, the results were certainly such as to warrant the conclusion that heat alone is the principle at work. The deposit was invariably heaped up towards the window in the form of an inclined plane, the perpendicular of which coincided with the tangent formed by the cold window-pane and the curve of the bottle; and this perpendicular varied in height from near the surface of the liquid to about halfway down, according as the night was cold or frosty, or the afternoon sun shone upon the window. The film at the top would sometimes be confined entirely to the side nearest the window. A little sunshine by day would clear away this top crust entirely; the cold of night would bring it back, and, if colder than usual, would thicken it and extend it over the whole surface, and it would be found in the morning dropping down the beautiful camphor feathers. A journal of this bottle, kept during November and December last, with the temperatures noted, satisfy me that heat is the efficient agent concerned in the results.

The effect of hermetically sealing the bottle seemed to be in this case a diminution of the upper crust, which was thin and scanty compared with what it was in another large bottle to which the mixture was afterwards transferred, and where access was had by a small opening to the external air. This is exactly what would be expected; for in the one case the space above the liquid would always be saturated and prevent adhesion between the surface and the air, while in the other case the enclosed air would partake of the pressure and hygrometric condition of the external air, and not only allow a crust to be formed, but assist it in throwing off vapour of camphor, which would condense on the coldest side of the glass above the mixture, as already noticed.

In order to ascertain the part that each ingredient plays in the storm-glass mixture, it was examined separately under circumstances calculated to exaggerate the phenomena. A solution of nitre, for example, was raised to the boiling-point in a long glass similar to that of the storm-glass, and the phenomena which accompanied its cooling, together with the temperatures at the top and at the bottom, were noted. A similar experiment was performed with sal-ammoniac, camphor-julep, spirits of wine, and water. The vessel selected for the purpose was a large test-tube of thin German glass, 8½ inches in length and 1½ inch in diameter, suspended from the ring of a retort-stand. Into this tube were poured 4 fluid-ounces of distilled water, heat was applied, and 32 drachms of nitre were gradually added, the last portions being suspended in a muslin bag just below the surface. The solution was raised to the boiling-point and left to cool in a room the temperature of which was 49°. Two thermometers were kept in the solution, the bulb of one penetrating to the bottom nearly in contact with the glass, and the bulb of the other just below the surface of the solution. Both the thermometers range well together, and their stems are enclosed in tubes welded to the bulbs and sealed at the top. The experiment was conducted about two feet from a window, so that the effects could be watched by transmitted light. The following Table gives the results of this experiment:—

Nitrate of Potash in Crystals.

32 drachms in 4 fluid-ounces of distilled water. Temperature of room 49° F.

Remarks.	Difference.	Lower thermometer.	Time.	Upper thermometer.	Difference.	Remarks.
	°	°	h m	°	°	
	6	Boiling 228	12 9 Lamp removed.	Boiling 222		
	2	210	10	208		
		196	12	201	5	
Ascending and descending currents very active.		184	14	190	6	
		170	16	179	9	
		157	19	170	13	
		149	21	162	13	
		143	23	154	11	
Crystals first noticed at the bottom.		140	25	146	6	
		140	27	143	3	Crystals forming on surface and flakes falling from them.
	2	139	29	137		
	1	137	30	136		
	3	136	31	133		
	5	137	32	132		Fan-shaped masses of crystals projecting from the surface downwards.
	6	136	33	130		
	7	134	35	127		
	8	133	36	125		
	9	130	38	121		
	10	130	39	120		Fan-shaped crystals 2½ inches long.
	11	128	40	117		
	11	126	42	115		
	10	122	44	112		Do. 3¼ inches long and 1 inch wide.
Crystals increasing from the bottom until the prisms rose to the height of 3 inches and met those projecting from top.	10	120	46	110		
	12	118	48	106		
	12	116	50	104		
	13	113	53	100		
	12	111	55	99		
	11	107	59	96		
	11	105	1	94		
Lower thermometer buried in crystals.	8	96	10	88		
	4	74	50	70		

There are some points connected with the above experiment which, taken in connexion with a similar experiment with sal-ammoniac, seem worthy of attention. The difference between the boiling points of water at the bottom and just below the surface was noticed by Gay-Lussac*; but after the boiling has been stopped

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* *Annales de Chimie,* xlix. 394 (1832). "Suppose a volatile liquid of a certain depth to be submitted to ebullition. Two thermometers plunged into the liquid, one near the bottom, the other near its surface, will indi-

and the solution left to cool, the great differences in temperature between the bottom and top layers has not, so far as I know, been hitherto observed*. It is known in a general way that there is a difference, so that, in comparing thermometers in hot water, means are taken to keep it stirred; but it has not, I believe, been suspected that in a liquid column 7 inches high and 1½ inch in diameter, supported in the air, as before described, there may be a difference of 20° and upwards in the case of water, and of 30° and upwards in the case of certain saline solutions between the bottom and the top of the column. To show the reality of the difference, the lower thermometer has been raised repeatedly to the level of the upper one, and the two readings corresponded. In the experiment with nitre the maximum difference was 13° in favour of the upper thermometer, but in the course of a few minutes this difference was reduced to 6°; soon after which a few short crystalline shoots were observed at the bottom, and a little after crystals were seen at the surface; the lower thermometer now marked a higher temperature than the upper one, and went on increasing until the maximum difference in its favour was also 13°. As soon as crystallization set in, the temperatures declined slowly as compared with the cooling before that event. From the removal of the lamp to the crystallizing-point, the solution cooled down 88° in 16 minutes, viz. from 228° to 140°. From the crystallizing-point to the conclusion of the experiment, the cooling was 66° in 85 minutes, viz. from 140° to 74°, and that in a room without a fire, where the temperature was 49°, and the solution was within about 2 feet of the window.

A similar experiment was tried with sal-ammoniac, the details of which are given in the following Table:—

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cate two different temperatures corresponding to the pressures in these positions: the vapour formed at the bottom of the vessel, experiencing a less degree of pressure in proportion as it ascends, dilates and cools until the moment when, attaining the surface of the liquid, its elastic force becomes equal to the pressure of the atmosphere. The temperature of the emerging vapour, or, what is the same thing, the last liquid layer, is exactly that of boiling under a given atmospheric pressure."

[*] Count Rumford, in one of his essays to prove the non-conductibility of water for heat, pointed out how a column of water with a lump of ice attached to the bottom might show various temperatures between 40° and 180° F. Saussure also noticed in Lake Lucerne that the water at a depth of 640 feet had a temperature of 41° F., while at the surface it was 68½°.

Sal-Ammoniac.

16 drachms in 4 fluid-ounces of distilled water. Temperature of room 47°.

Lower thermometer.	Time.	Upper thermometer.	Difference.	Remarks.
Boil-ing 232°	h m 10 40 Lamp removed.	Boil-ing 228°	4°	
214	41	213	1	
195	43	200	5	
182	45	193	11	
156	50	173	17	Crystallization began and gradually extended from the bottom upwards, and subsided as the two temperatures became equal.
136	55	156	20	
128	58	146	18	
126	11	140	14	
124	2	135	11	
120	4	130	10	
118	5	128	10	
115	7	125	10	
116	8	122	6	
112	10	118	6	
112	12	118	6	
110	14	114	4	
104	17	108	4	
100	20	104	4	
96	24	100	4	
92	31	92	0	

In the sal-ammoniac experiment, the phenomena of crystallization are striking and beautiful. Crystals like miniature ferns or fir-trees start up from the bottom; and just over this minute forest a snow-storm sets in and quickly buries it in a multitude of minute six-rayed stars perfectly sharp and distinct to the naked eye, each radius fringed with spikes of unequal length diminishing to the point. The stars descend gently, conveying the noiseless idea of falling snow, and the storm becomes slowly propagated upwards until the whole tube is filled with these falling stars, and the lower thermometer is half buried. The heat liberated by the crystals sets currents in motion, and some of the stars struggle to force their way upwards by the axis of the tube. Meanwhile a crust forms on the surface like an inverted forest, the temperatures decline, and, when the difference between them is extinguished, the liquid between the top crust and the mass of snow beneath becomes bright and still.

I cannot fancy a prettier sight than this exhibited to an audience by means of the oxyhydrogen or electrical lamp. The experiment may be prepared for such a purpose, or for any one who wishes to see this beautiful effect in miniature, in the following manner:—pour 3 drachms of distilled water into a small test-tube, and put into this about 40 grains of sal-ammoniac; suspend the tube by a piece of twine from the ring of a retort-stand or in any other convenient manner, and carefully boil the solution by means of a spirit-lamp. As soon as the lamp is removed, lower the tube into cold water so as to chill the solution to the crystallizing-point, and, the moment crystals appear at the bottom of the tube, remove it from the water and leave it suspended in the air. The minute snow-storm will soon set in, and will continue for nearly an hour. When the action is over, the tube may be corked and set aside for repetition at some future time.

These experiments show how important a part sal-ammoniac plays in the storm-glass. The fern-leaf figures and the stars, which are usually referred to the camphor, are produced chiefly by the sal-ammoniac. The stars, moreover, show very decidedly the action of cooling, not of windy weather, as it is said; or if wind has any effect, it is in cooling the storm-glass mixture; and the appearance of these stars in greater or less galaxies is but a sign of the greater or less, or more slow or more rapid rate of cooling.

A solution of camphor in cooling gives no definite figures, but flakes very much like snow-flakes, which descend from the surface, and minute particles of camphor, which ascend in such quantities as to make the solution nearly opake. The solution was in the first instance made with 4 ozs. of distilled water, 1 oz. of spirits of wine, and $\frac{1}{2}$ a drachm of camphor. The solution boiled at 194° for the lower, and 192° for the upper thermometer. When the readings were 136° and 140°, a cloudy appearance set in at the bottom, and multitudes of what looked like air-bubbles ascended, and soon formed ascending and descending currents. When the readings were 127° and 137°, the flakes descended as before described. Next morning the flakes were collected at the bottom as in the storm-glass, and particles were sailing up and down in the clear liquid. The tube was placed in hot water, and the solution was filtered; it now contained much less camphor, and at 116° and 118° the minute particles appeared, which increased with the cooling, and flakes smaller than those before obtained also ascended and descended in the bright liquid. On the former occasion it was turbid from the multitude of particles.

Several experiments were made with distilled water, one of which was in a tube 9 inches long, $1\frac{1}{2}$ inch in diameter; the length of the liquid column was about $7\frac{1}{2}$ inches, 5 fluid-ounces of water being employed, and the temperature of the room was 63°.

Lower thermometer.	Time. h m	Upper thermometer.	Difference.
Boiling 214°	9 50 A.M.	Boiling 212°	2°
156	10 „	170	14
103	11 5 „	126	23
66	11 55 „	80	14
62	12 7 „	72	10
55	12 45 „	59	4

It is perfectly well known that in an experiment of this kind the cooling takes place chiefly by means of currents set in motion in the external air, which generate a similar current in the liquid, the warmer particles ascending by the axis of the tube, and the colder ones descending by the sides; so that while this process goes on, the upper thermometer will mark a higher temperature than the lower. In a saline solution several additional phenomena are to be observed, many of which do not belong to this inquiry; but it may be noticed that even in salts which do not form supersaturated solutions, such as sal-ammoniac, the solution remains fluid some time after the lamp has been removed, while crystallization endeavours to establish itself at the surface where the air and the liquid meet. Although it is quite true that the upper thermometer marks a considerably higher temperature than the lower, yet the coldest portion of the solution before crystallization sets in is that very thin film which is in actual contact with the air. I have seen in a saturated solution of sal-ammoniac at the boiling-point, directly after the lamp has been removed, a crystalline film form and disappear many times before it could establish itself on the surface. A similar struggle goes on in the case of supersaturated solutions, although in such case the film may not be visible. It is dissolved, so to speak, in the very act of forming; but its existence is made quite evident by the saccharine-like streams which descend from the surface during the cooling, and are identical in character with those which accompany actual crystallization in other parts of the tube. There are many other points connected with the cooling and crystallization of solutions of hydrated and anhydrous salts, which I may perhaps be allowed to describe on another occasion. In conclusion, I give a few details respecting a storm-glass bottle so arranged as to note the upper and lower temperatures.

A white glass cylindrical bottle, 5½ inches high exclusive of the short neck, and 3 inches in diameter, of the capacity of 20 fluid-ounces, with an opening at the top and another at the side near the bottom, was filled about five-sixths full with a storm-glass mixture, and a thermometer, passed through a cork, was placed in each tubulure; a third thermometer was suspended on one side, and a fourth (a register) was placed on the other side. The bottle with its thermometers was mounted in a west window at noon on the 29th of December, 1862, all four thermometers marking 51°. This arrangement was maintained about three months, and a register was kept of the temperatures and the appearance of the bottle two or three times a day. A few short selections from this register will be sufficient for our purpose. The upper thermometer had an elongated bulb, half of which dipped below the surface of the mixture. This thermometer will be referred to by the word "upper," while that in the lower tubulure by the word "lower," the third thermometer by the word "outside," and the fourth by the word "register."

On the morning after the arrangement was made the readings were as follows:—

Dec. 30, 1862.
8.30 A.M. Upper, 48°. Lower, 45°. Outside, 46°.
Sun on window.
3.15 P.M. „ 58. „ 57. „ 53.

The top crust, which was abundant and much clustered about the bulb of the upper thermometer at 8.30, had disappeared when the afternoon reading was made. At night numerous stars were circulating in the clear portion of the mixture.

Dec. 31. 8.30, A.M. Upper, 42°. Lower, 41°. Outside, 41°.
An abundant lower deposit heaped up towards the light to the height of more than four inches, the upper crust also abundant.
2.0 P.M. Upper, 53°. Lower, 52°. Outside, 55°.
Sun on window; lower deposit much looser; feathers remarkably fine.
At 3.45 P.M. Upper, 53°. Lower, 51°. Outside, 48°.
„ 5.0 „ „ 50. „ 48. „ 45.
Soft feathery deposit heaped up at the side nearest the light; top crust with feathers thick about bulb.

Jan. 1, 1863. During the night the register stood at 39°.
11.30 A.M. Upper, 50½°. Lower, 51°. Outside, 52°.
Windy and wet; feathers abundant and fine.
3.30 P.M. Upper, 52°. Lower, 52°. Outside, 52°.
Windy weather equalizes the temperature, and produces a deposit not heaped up towards the window, but nearly equally distributed in the bottle.

Jan. 2. Stormy, wet, windy; afterwards fine.
8.0 A.M. Upper, 52°. Lower, 52°. Outside, 53½°.
2.0 P.M. „ 53. „ 52½. „ 55.
Deposit much shrunk from afternoon's warmth; height 2¾ in.
5.35 P.M. Upper, 49°. Lower, 47°. Outside, 45°.

Jan. 3. Register, 37°.

9.0 A.M. Upper, 44¼°. Lower, 43°. Outside, 43°.

Upper crust abundant, and numerous floating feathers just below it.

Sun. 3.0 P.M. Upper, 54°. Lower, 52°. Outside, 55°.
Jan. 4. 9.30 A.M. „ 47. „ 46. „ 46.
3.25 P.M. „ 52. „ 51. „ 49.

Top crust still abundant; thermometer bulb acting both as nucleus and support. Rain; deposit softening down.

Jan. 9. Register 32°. Top feathers very abundant, like an inverted forest of pines; completely covering the surface.

11.0 A.M. Lower, 44¼°. Upper, 42°. Outside, 42°.
Jan. 15.
8·30 A.M. „ 41½. „ 38. „ 42½.
3.22 P.M. „ 46. „ 46. „ 47.

The lower deposit heaped up towards window like a steep hill hollowed out and covered with beautiful foliage. Fern-like figures also projecting down from the top crust, but most abundantly on the side nearest the window.

All these various phenomena seem to be perfectly accounted for by difference of temperature. In order to exaggerate these differences, I attached to the side of the bottle nearest the window, about the middle, a black paper ring 1⅜ inch in outer diameter, and ⅝ths of an inch inner diameter, so as to leave a black ring ⅜ths of an inch wide. In putting on the ring the bottle was disturbed a little, so that some of the top feathers fell down, and turned over in doing so. The ring was just below the summit of the deposit, and was quite invisible from the room. In about an hour, however, that is, about 2 P.M., its dark outline was quite evident, the heat absorbed by it having thinned away the crystals.

Jan. 16. 8.0 A.M. Ring quite concealed, the cold of night having heaped up the lower deposit.

2.0 P.M. Upper, 48°. Lower, 47°. Outside, 47°.
Form of ring distinctly visible.

Jan. 20. 3.0 P.M. Upper, 59°. Lower, 56°. Outside, 53°.
Sun on window; ring visible.

6.40 P.M. Upper, 46°. Lower, 44½°. Outside, 45°.
Stars circulating in clear liquid. Hitherto the form of the ring only visible. To-day a portion of the ring itself, about a quadrant, on the side nearest the sunset laid bare.

8.30 P.M. Upper, 42°. Lower, 41½°. Outside, 41°.
Much wind; ring all concealed; liquid turbid with stars from the rapid cooling.

Jan. 21. 8.30 A.M. Upper, 43°. Lower, 42½°. Outside, 43°. Feathers heaped up, and ring quite concealed.

4.10 P.M. Upper, 54°. Lower, 53°. Outside, 50.

Sun just left the window; good part of the ring exposed, and the form of the rest visible; lower deposit very loose and woolly; feathers of the upper crust lost their sharpness.

Jan. 23. 12.0 P.M. Upper, 54°. Lower, 54½°. Outside, 55°. Deposit very loose, and evidently avoiding ring.

Jan. 27. 11.0 P.M. Shutters have been closed two or three hours; opened them, and found stars circulating in the clear liquid.

Jan. 28. 8.0 A.M. Upper, 42°. Lower, 39°. Outside, 39°. Register, 32°. Abundant deposit; top crust very dense, and joining the bottom deposit on the side near the window.

Jan. 29. 2.10 P.M. Upper, 60°. Lower, 59°. Outside, 62°. Deposit greatly diminished.

10.15 P.M. Upper, 50°. Lower, 49°. Outside, 50°.

Currents of stars in clear solution produced by cooling after the shutters had been closed; bright moon.

Feb. 1.

2.30 to 3.0 P.M. Upper, 61°. Lower, 59°. Outside, 56°.

Sun on window after wet morning; ring laid bare to a greater extent than before noticed, a hollow vacant space being cleared away behind it; deposit very loose and woolly; top crust more compact and arched, with fern-like crystals projecting down.

8.30 P.M. Upper, 47°. Lower, 46. Outside, 45°.

Clear liquid full of stars, and lower deposit closed in upon ring, only the top of which is now visible.

Feb. 2. This morning the bottle had to be moved for window-cleaning; shook up mixture, wiped bottle, and returned it about 1 P.M. to the window. The deposit subsided, but no crust formed to-day, nor on the 3rd, until the shutters were shut in the evening. Wind a little northerly; lower deposit about half-way over ring. At 11 P.M. clear liquid full of stars; crust forming all round where the air, the glass, and the liquid meet; some feathers from crust falling upon the lower deposit: the shutters being closed, the bottle about equal in temperature all round.

Feb. 4. 9.30 A.M. Upper, 44°. Lower, 43°. Outside, 44°. Very large feathers projecting from top crust downwards, and also heaped up at bottom, evidently derived from the top crust, and some now are slowly falling.

Feb. 9. Deposits for some days very scanty on account of the mild weather; much hollowed out; last night frosty, when the clear liquid was full of stars; no wind; this morning top crust and lower deposit very fine; feathers distinct and large.

Feb. 12. Action of black ring under the influence of afternoon's sun cleared away nearly all the lower deposit, and when the temperatures were read some time after the sun had left the window they were remarkable.

4.30 P.M. Upper, 64°. Lower, 62½°. Outside, 55°.
9·0 „ „ 50. „ 48. „ 45.

Clear liquid full of stars.

10.30 P.M. Upper, 48°. Lower, 46½°. Outside, 45°.

Not so opake with stars as at 9.0, but the flocculent deposit heaped up below, and the ring nearly concealed.

Feb. 13. 8.0 A.M. Upper, 44°. Lower, 42°. Outside, 40½°. Register, 32°. Abundant deposit and top crust.

3.0 P.M. Upper, 61°. Lower, 59°. Outside, 65°.
11.0 „ „ 46. „ 43. „ 41.

Feb. 17. This morning register 28°; large feathery masses from the top united with the lower deposit; afternoon's sun raised the temperature to 60°; lower deposit caverned out, and masses falling. This evening at 11 opened shutters and found stars very numerous and active in clear liquid.

11.0 P.M. Upper, 44°. Lower, 42°. Outside, 40°.

Feb. 18. Register 27°. Bottle nearly full of deposit at 8 A.M. Mercury in the lower thermometer was in the bottle, so that the reading could not be taken.

Feb. 22. 3.20 P.M. Upper, 64°. Lower, 61°. Outside, 61°. Sun on window, portions from upper crust descending; scanty lower deposit piled up on side nearest light.

Feb. 27. 10.0 A.M. Upper, 52°. Lower, 51°. Outside, 51°. Register, 38°; large feathers falling from top; lower deposit much hollowed out.

4·45 P.M. Upper, 64°. Lower, 63°. Outside, 56°.

Ring all visible; very little lower deposit; woolly mass on side nearest light.

Mar. 3. Sunny afternoon; deposit nearly all gone top and bottom; crust over surface, but of no thickness.

Upper, 74°. Lower, 72°. Outside, 66°.
10.0 P.M. „ 52. „ 52. „ 52.

Shutters shut, when the temperatures became equal all round; wonderful change since afternoon; a dense amorphous deposit and thick crust; about 2 inches of clear liquid full of stars.

Mar. 13. For some days the lower deposit has been nearly equal all round; but a few days of cold weather heaped it up against the side nearest the window. This afternoon it was hollowed out: as soon as sun left window, the top thermometer sank about two degrees, and the stars set in in currents.

Mar. 29.

Sun on.	4.40 P.M.	Upper, 72°.	Lower, 69°.	Outside, 62°.
Sun off.	5.40 P.M.	„ 66.	„ 66.	„ 58.
Mar. 30.	8.40 A.M.	„ 54.	„ 53.	„ 52.

Register, 47°; feathers fine and falling.
6.0 P.M. Upper, 56°. Lower, 55°. Outside, 53°.
Feathers softening down by solution.

April 1. 8.0 A.M. Upper, 42°. Lower, 39°. Outside, 39°.
Register, 31°. Feathers sharp.

I think it may fairly be concluded from these experiments and observations that the storm-glass acts as a rude kind of thermoscope, inferior, for most of the purposes of observation, to the thermometer. It does not seem to be capable of reference to a standard, and hence observations made with it scarcely admit of being registered, although attempts at a scale are made by some instrument-makers. If, however, two or more of such graduated instruments be placed in and about a house, their indications will vary considerably, according as they are more or less exposed to the action of radiation; and it is difficult to see how the glass can be protected from radiation except by enclosing it in another glass; and under such circumstances its action will be very feeble.

Instrument-makers insist on the integrity of the mixture, the composition of which they keep secret. This, of itself, ought to exclude the instrument from the Meteorological Observatory. It is possible that some storm-glass maker may reflect on the character of my paper on the ground that the mixture adopted by me is not the correct one. I have formed a number of these glasses according to the recipe given, and find them to act very well. The mixture must be heated, but gently, otherwise the sal-ammoniac will separate from the other ingredients. When poured into the tube, corked and suspended, the glass may require a day or two before it begins to act properly; but a little shaking or exposure to the sun, or thinning down by the addition of a small portion of spirit or of water, will soon make the glass act properly. Two tubes containing the same mixture, were placed, one in the window, and the other in a test-glass within a foot of the window; the first acted well, the second did not act at all, on account of its cooling being interfered with by the shelter of the test-glass; but on taking it out of the glass, and placing it on the window-frame, it began to act in a few hours, and has behaved well for many weeks.

King's College, London.
18th May, 1863.

XIV. *On the Dynamical Theory of Heat.*

By JOSEPH GILL, *Esq.*

To John Tyndall, Esq., F.R.S. &c.

SIR,

AT the present time, when the subject of thermo-dynamics is beginning to attract more general attention from the scientific public, every little contribution of new light may be useful in aiding the progress of discovery in "a region which," as you have justly remarked, "promises possessions richer than any hitherto granted to the intellect of man." As an obscure labourer in this grand field of science, I ought perhaps to apologize for asking from you, as an Editor of the Philosophical Journal, the favour of making public, through its pages, a few remarks which may not be useless towards clearing away some of the obscurity which appears to me still to overhang the very first steps of our progress; and in explanation I may be allowed to say that the subject has been of deep and fascinating interest to me, from the dim foreshadowing of the theory sketched by Séguin twenty-four years ago, and the powerful outlines drawn soon after by the master hand of Mayer, down through the mass of important experimental evidence of Joule, backed by the labours of Thomson, Clausius, Regnault, and Helmholtz, to Professor Rankine's thermo-dynamics in his recent work on Prime Movers. And while I have been endeavouring to grasp in some degree the philosophy of the subject from the works of others, I have not omitted to aid my own speculations and researches with numerous original experiments made under circumstances of no little difficulty, but, I venture to hope, not utterly in vain.

I ought candidly to declare at once that my long investigation of the practical working of the steam-engine, and my numerous experiments with hot air as a motive agent, forced me many years ago reluctantly to doubt that any heat, contained under the common form of heat by the elastic fluids, is directly converted into work in the action of thermic prime movers; and it has been a source of sincere regret to me that a beautiful theory, elaborated with admirable skill by some of the first intellects of the age, and which, applied to some grand cosmical phenomena, gives results bearing the impress of correctness, should still be obviously erroneous as applied to the working of the commonest steam-engine.

The current doctrine of thermo-dynamics assumes that all the heat which passes from the fire to a steam-boiler takes the form of molecular motion, or *vis viva,* in the steam, and that the work performed is so much of this molecular motion given out by the steam in its passage through the engine; consequently that the steam must contain less heat or molecular motion after it has done work than before, and that the quantity of heat which disappears by being thus transformed into work is in the proportion of the mechanical equivalent of heat, which experiment has fixed at 772 lbs. raised one foot by a quantity of heat which will raise the temperature of 1 lb. of water 1 degree of Fahrenheit. According to this doctrine, molecular motion is transferred from the fire to the steam, and a small portion of this molecular motion is transferred from the steam to the work done, or transformed into this work, by far the largest portion remaining in the steam after the operation is finished.

Like Séguin a quarter of a century back, and like M. Hirn of Colmar very recently, I find in the condenser of the engine all the heat originally contained in the steam, after allowing for known losses; but, unlike M. Hirn, my reason will not allow me to alter my views and to interpret facts to suit a theory, however well supported by authority. I hope this open declaration of partial dissent will not prevent my obtaining a fair hearing while I endeavour to show that the observed facts are not necessarily discordant with sound theory, though in the brief space to which I must here limit myself, my exposition must be very imperfect; but my chief end will be attained if my suggestions should fortunately induce an unprejudiced reconsideration of the subject by superior intellects.

It may be well to remark here that I am quite convinced of the general correctness of that part of the dynamical theory of heat which treats of the conversion of work into the molecular motion supposed to constitute heat in common matter; and I hope that science will soon clear up the obscurity which still envelopes the converse phenomenon, or the change of heat into work.

The material universe in its lowest or most external manifestation, presents to a superficial glance only matter and motion. On deeper investigation it is perceived that matter is under the influence of principles of energy or force, or is endowed with such principles which cannot be satisfactorily explained as general results of motion,—though much ingenuity has been displayed in attempting to account for the phenomena of terrestrial gravitation as the result of the earth's axial rotation and revolution in its orbit; and also in applying the well-known principle of centrifugal force of matter in masses to the phenomena of molecular repulsion or elasticity in vapours and gases, by hypotheses of molecular vortices which may hold good partially, but are not sufficient to explain all the phenomena of this class which come under our observation. The two great principles of energy or force which seem to include all others, are attraction and repulsion; and it might be more reasonably argued that, in the majority of phenomena, motion is the result of these forces. Perhaps the most probable supposition is that they were coexistent with motion from the creation of things. It must, however, be allowed that, as far as our observation extends, there is a universal and immutable connexion between the motion of matter or *vis viva*, and the more occult principles or properties of attraction and repulsion in bodies at rest; so that whenever motion or *vis viva* disappears, it is replaced by equivalent energy under the form of attraction or repulsion in a statical condition of disturbed equilibrium. And, conversely, every case of restoration of disturbed statical equilibrium must give rise to equivalent motion or *vis viva*. In other words, dynamical energy is the force of motion, or *vis viva*; and statical (or potential) energy is a state of disturbed equilibrium of attraction or repulsion; and, as first clearly expounded by Mayer, these different forms of energy are convertible. Heat, as we observe it in common matter, is allowed to be molecular motion, and the calorific energy of the hottest flame to be only the *vis viva* of the excited material particles. As heat is the proximate cause of work in thermic prime movers, it is reasonable to suppose that all the energy corresponding to the work done by a steam-engine must pass from the fire into the steam, and from the steam to the work done. The heat required to form the steam is allowed to be constant, or nearly so, under different pressures; and one cubic foot of steam under a pressure of ten atmospheres, quietly condensed in the vessel which contains it, will communicate to the condensed water only the same quantity of heat as ten cubic feet of steam of atmospheric pressure equally condensed. If the cubic foot of high-pressure steam be allowed to expand gradually, by enlarging the space it occupies, against a moderated resistance until its bulk becomes ten cubic feet, supposing no heat to be lost or applied in the process, it is perceived that it will be identical with the ten cubic feet of steam formed at atmospheric pressure; but expansion under moderated resistance necessarily produces work, and, in fact, the work produced by the high-pressure steam would be three times more than the low-pressure steam can produce. The energy equivalent to this remarkable difference of amount of work must exist in the high-pressure steam,—but not as heat or molecular *vis viva;* for the steam contains only the *vis viva* of its heat of conversion, or change of state.

Imagine a mass of ten cubic feet of atmospheric-pressure steam to be compressed by exterior force into the space of one cubic foot, supposing no heat to be lost or applied in the process, and excluding the idea of friction; it will be allowed that the resulting cubic foot of steam will be identical with a cubic foot of steam formed directly by the agency of fire under a pressure of ten atmospheres. A radical mistake in the doctrine of thermodynamics is the assumption that *compression, apart from the idea of friction, is a source of heat.* In elastic fluids it is evidently only a cause of concentration of heat, or increase of temperature resulting from a change of latent heat into sensible, as it would be commonly expressed. This all-important difficulty must be fairly met, and this corner stone in the foundation of the theory must be satisfactorily placed before the superstructure can be of permanent value. It is obvious that a very large amount of energy exists in high-pressure steam (and in compressed gases) which is not molecular motion, or heat as described by the dynamical theory, but quiet, statical energy, or force wound up, ready to give out equivalent work when the fluid is allowed to expand against moderated resistance, or to be converted into interior heat, or molecular motion of its own mass when allowed to increase its volume by free tumultuous expansion.

This packed up energy must be repulsion in some shape; but it cannot be the centrifugal repulsion of molecular vortices, because *that* must be a result of existing motion; and it is seen that no corresponding motion exists in the compressed fluids. Physical phenomena present numerous instances of the action of statical repulsion quite distinct from the idea of centrifugal force; and it was said above, that when motion or *vis viva* disappears, it is replaced by equivalent energy under the form of attraction or repulsion in a statical condition of disturbed equilibrium. The heat of conversion of water into steam passes from the fire to the fluid as molecular motion, whether of vibration or rotation is a question of minor importance, if we allow that the work-producing power is not dynamical centrifugal force. Something equivalent to the work-producing power must also be supposed to pass from the fire to the steam. If it is energy existing in the fire as molecular *vis viva* or dynamical force, it must be supposed to disappear as such in the act of transfer to the steam, and to assume, for the time, the equivalent form or condition of molecular repulsion in the state of statical force, under the influence of some opposing statical resistance. And, conversely, when the opposing statical resistance is removed, *vis viva* or motion reappears either as exterior work, or interior molecular motion, in re-establishing the previous state of equilibrium. It is not requisite to suppose the direct or immediate reappearance of the *vis viva* in restoring equilibrium in any one operation; for equilibrium may be equally restored by a transfer of force which may disappear from one body to reappear in another under the same form; but sooner or later in the rotation of phenomena the transformation takes place, for when one form of energy disappears altogether, it must be substituted by another.

But if high-pressure steam be quietly condensed in the vessel which contains it, by simply withdrawing the heat of conversion, or change of state, the excess of energy above mentioned disappears entirely. What becomes of it? I submit that this question must be answered before the dynamical theory of heat can be received as satisfactory or rational, as far as human knowledge at present extends.

The recent experiments of M. Hirn showed that when high-pressure steam expands tumultuously into a condenser, *heat is created*; that is to say, more heat appears in the condenser than was contained originally in the steam. The fact is not disputed, but it is explained by saying that the particles of the steam rush into the condenser with immense velocity, while the water which results from their condensation is quiet; and as the *vis viva* co-existent with velocity in the motion of translation of the particles remains in the condenser, it must assume the form of molecular motion known as heat. I submit that this explanation is not satisfactory, though it may be a correct statement of the molecular phenomena. The steam, as high-pressure steam, contains only the heat (molecular motion) of conversion, or change of state, which is allowed to be a nearly constant quantity under different circumstances of temperature and pressure; and this is the quantity which appears when the steam is condensed under pressure. But the *vis viva* or molecular motion of the steam is more after tumultuous expansion than before, and therefore steam so expanded is superheated, as directly proved by experiments of Siemens, and by my own; consequently it must be allowed that in its compressed state it possesses something which is not quantitative thermometric heat for the time, though always accompanied by higher temperature in the steam, but which produces work if the steam expands under moderated resistance, and heat if it expands tumultuously.

Analogous to the foregoing case are Joule's remarkable experiments of 1845, in which compressed air was expanded to an equal degree by two different methods—first, by a gradual enlargement of the containing space under moderated pressure, by which exterior work was performed, accompanied with (apparent) loss of heat; and secondly, by free tumultuous expansion from the containing vessel into an annexed vacuous receiver, by which no exterior work was performed, and no loss of heat was observed. It was deduced that heat disappeared in the first case, being converted into the equivalent work produced; and that in the second case no heat disappeared because no work was done. These striking experiments drew the attention of scientific men to the subject of the new theory; and Regnault in particular interested himself in repeating them with the variety and accuracy suggested by his great experience in thermic researches. He announced to the Academy of Sciences ten years ago that his experiments fully confirmed Joule's results, and from that time he became an open partisan of the new theory

The facts of these experiments are correct (indeed they could hardly have been otherwise in the hands of such able experimenters); but I submit that the explanation given to them is incorrect. The compressed air was of the high tension of twenty-two atmospheres, and to produce this tension no small amount of work must have been expended. The energy equivalent to this work was certainly taken from some exterior source and transferred to the air. If no heat escaped during the compression, the temperature would rise very considerably, from the change of latent heat to sensible; and when the air was allowed to expand back to its former volume under moderated resistance, an amount of work would be produced equal to that expended on the compression, the temperature falling to its original point by a changing back of sensible heat to latent. If the escape of heat is not prevented during compression, the dense air, at the temperature of the surrounding atmosphere, will contain much less heat than it held at the initial density with the same temperature, and its tension will be proportionately less than if the heat were retained. If no heat be allowed to re-enter the air-vessel during the expansion, the temperature will fall very considerably, and less work will be produced by the smaller amount of expansion which, under these circumstances, will restore the equilibrium. In the former case all the energy put into the compressed fluid remains in it as statical energy, or packed-up force, while the state of compression lasts; and in the latter case, part of the communicated energy escapes with the heat, and the rest remains stored up in the compressed fluid. This latter state of things represents the initial condition of the compressed air in Joule's experiments; and it is evident that if the air be allowed to expand by gradually enlarging the space which contains it, exterior work will be produced, and the temperature of the expanding fluid will fall by a change of part of its sensible heat into latent, the whole quantity of heat remaining the same notwithstanding the fall of temperature.

But as the water of the calorimeter was, as well as the compressed air, of the atmospheric temperature at the commencement of the experiment, the fall of temperature in the expanding air will naturally cause it to take heat from the surrounding water until an equilibrium of temperature is established. Hence results in the expanded air what is called a disappearance of heat, which is supposed to be converted into the work produced by the expansion—the real state of the case being that the expanded air has produced the work without any loss of heat, but only a fall of temperature; and therefore it must be conceived that the operation of compression gave it energy which it did not possess before, and that this energy is for the time of a form very different from heat. In fact the compressed air contained statical energy like the tension of a wound-up spring, and the principle of conservation is satisfied by the recovery of the mechanical work expended to compress the air in preparing the experiment; so that loss of heat is not required to strike the balance.

In the other case of Joule's experiment, where the compressed air expanded tumultuously into the annexed vacuous receiver, no exterior work was produced; but, as in Hirn's experiment of the tumultuous expansion of high-pressure steam, an increase of the total heat of the fluid must result. And as the expansion in the emitting vessel must be attended with a corresponding fall of temperature (though without loss of heat), the extra heat produced by the tumultuous motion of the exhausting air in the other vessel would compensate this fall of temperature by satisfying the increased capacity of the expanded fluid for heat, and no reduction of temperature would be perceived in the water of the calorimeter. This state of things is explained by saying that, as no work was performed, no heat disappeared; but the real fact is, that the quantity of heat in the apparatus at the end of the experiment exceeds the initial quantity by the additional heat developed by the tumultuous expansion of the air into the comparatively vacuous receiver. And here, again, the result accords with the principle of conservation, as the mechanical work spent on the compression in preparing the experiment is not recovered, but instead of it we have the equivalent heat.

In considering of this and of analogous experiments, the candid inquirer must perceive that in no case can he detach or isolate his operations from the mass of physical conditions around him, and that consequently results which might otherwise be regarded as absolute are only relative—a consideration of prime importance in such investigations; for where everything is relative, and no absolute initial and final points can be determined in the ever-changing circuit of elemental phenomena, the principle of conservation may be strictly preserved where we do not directly perceive it. It must be allowed that our ideas of the action of heat as a source of work are still very obscure. The patient and almost silent researches of twenty years, perfectly unbiassed by partial influences of any kind, have convinced me that work is directly convertible into heat by friction, but that heat is not *directly* convertible into work. It is certain that in elastic fluids it is the quality or condition of the heat, and not its quantity, which is the measure of the dynamical equivalent. Calorific energy is not simply the molecular motion known as heat; and "the phenomenon which we call heat, as a dynamical agent, may be only the exhibition of certain states (or conditions) of the particles of matter, dependent on, and correlative with, forces in which they are involved."

Believe me, Sir,
Your most obedient Servant,
Palermo, June 6, 1863. JOSEPH GILL.

XV. *On the Expansion of Water at High Temperatures.*
By J. J. WATERSTON, *Esq.*[*]

[With Three Plates.]

IN the account given of a law of liquid expansion in the Philosophical Magazine for June 1861, it was stated that water alone, of all the liquids examined, did not conform to the law in the lower part of the scale of temperature, and that even up to 200°[†] it showed no tendency to do so. My attention has lately been drawn to this subject again by having occasion to graduate a water-thermometer. The inequality in the rate of expansion of water is so great that, to obtain even moderate accuracy in the scale, it was requisite to find the law of the differences. To do this a scale of equal parts was attached to the stem, and λ, the length between t_0 and t_1 (two near temperatures), was divided by t_1-t_0, and the quotient set off as ordinate to $\frac{1}{2}(t_0+t_1)$, the middle temperature; the points were found to range in a straight line that came down and crossed the axis at 4° nearly. It would thus appear that the rate of expansion increased exactly in the simple ratio of the distance from 4°, and it would seem that the curve of expansion was the conic parabola having its vertex at the point of minimum volume.

Let PL represent unity or volume at 4°, and $\mathrm{NQ}=v$ the volume at t. Then

$$t-4=\mathrm{LN}=\mathrm{PR}=y,$$

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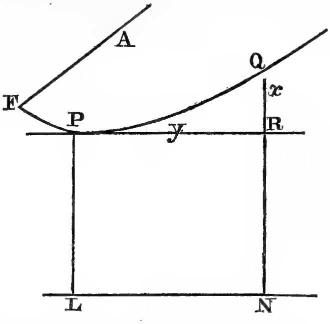
[*] Communicated by the Author.

[†] The temperatures are Centigrade.



and $RQ=x=v-1$. If the curve of expansion PQ is a parabola, $x \propto y^2$, and $\frac{dx}{dy} \propto y$. I had previously examined the observations of M. Despretz both above and below the minimum point, and found that from 50° downwards to as far below 0° as the liquid condition can (by perfect stillness of the water) be maintained, they could be very accurately represented, not by a parabola, but by an ellipse FPQ, the curvature of expansion below P being greater than that above, and FA, the axis of the ellipse, being inclined to LN, the axis of temperature. On this account the curve was to be considered as only empirical, and therefore of no value in advancing the thermo-molecular physics of water. But now the question occurred, after seeing how the ordinates of graduation ranged, might not the curve above 50° conform to the parabola? might not the small deflection from it in the lower part of the scale be the abnormal effect of the crystallizing force gradually increasing towards the lower temperatures?

§ 2. To test this, reference was made to the Table of the expansion of water up to 210°, given in note E of the paper "On Liquid Expansion" above referred to; and since, if the curve was a parabola, dx or $dv \propto y$ or d^2v constant, it was easy to test the point in question by examining the second differences of the volumes in that Table, which had been carefully reduced to intervals of 5° of the air-thermometer (C.A.).

The following is the Table with first and second differences:—

t.	*v.*	*dv.*	d^2v.	*t.*	*v.*	*dv.*	d^2v.	*t.*	*v.*	*dv.*	d^2v.
−10°	1·00184			65°	1·01940	272	18	140°	1·08130	537	17
− 5	1·00068	116	61	70	1·02230	290	18	145	1·08684	554	19
0	1·00013	55		75	1·02538	308	17	150	1·09257	573	17
5	1·00001			80	1·02863	325	17	155	1·09847	590	19
10	1·00025	24	34	85	1·03205	342	16	160	1·10456	609	18
15	1·00083	58	27	90	1·03563	358	18	165	1·11083	627	21
20	1·00168	85	27	95	1·03939	376	18	170	1·11731	648	21
25	1 00280	112	24	100	1·04333	394	16	175	1·12400	669	24
30	1·00416	136	23	105	1·04743	410	19	180	1·13093	693	25
35	1·00575	159	21	110	1·05172	429	19	185	1·13811	718	24
40	1·00755	180	20	115	1·05620	448	18	190	1·14553	742	28
45	1·00955	200	20	120	1·06086	466	18	195	1·15323	770	31
50	1·01175	220	18	125	1·06570	484	19	200	1·16124	801	33
55	1·01413	238	17	130	1 07073	503	17	205	1·16958	834	37
60	1·01668	255	17	135	1·07593	520	17	210	1·17829	871	
65	1·01940	272	18	140	1·08130	537	17				

It will be remarked that from 50° to 150° the second differences are sensibly uniform, their exact mean being 17·5. Now the parabola being defined by the equation $ax=y^2$, to find a we have the differential equation $2(dy)^2=ad^2x$, in which $dy=5°$, $d^2x=·000175$, hence $a=285714$; and since $x=v-b$ and $y=t-c$, in which v is the volume at t temperature, we have $285714(v-b)=(t-c^2)$, in which b and c may be computed from two observations, or they may be computed from one observation and its first difference. But the most exact method is to compute a, b, and c directly from the volumes at 50°, 100°, and 150°; we thus obtain

$$\log a=5·45196, \quad b=·99710, \text{ and } c=-14·4025.$$

This parabola passes through the tabular volumes at 50°, 100°, and 150°; and at 75° the volume is smaller than the tabular number by one-twelfth of a degree, and at 125° it is larger than the tabular by one-sixteenth of a degree. Above 150° the parabola sensibly diverges from the tabular volumes; at 200° the difference corresponds to 1°. But, as stated in Note E, these higher observations were of inferior value. The tubes were unfortunately of soft glass, which are corroded by steam under 200°. It was thus still an open question whether water expanded in a parabola. The simplicity of the curve was inviting, although, if confirmed at the higher temperatures, it would be fatal to the generality of the law of expansion. I therefore endeavoured to obtain observations at such high temperatures as would settle the point.

§ 3. This has proved a much more difficult task than was anticipated, and has occupied several months.

Below 200° it is comparatively easy to obtain good observations in tubes 20 inches long. In previous researches I employed concentric boxes or hoods, in which a current of heated air, after ascending on the outside of the inner one (which contained the tubes and thermometers), was directed downwards upon the tubes before escaping by the funnel, which also passed upwards through the inner hood. Some attempts to follow this method on a smaller scale having failed, I resorted to an open tube held over Bunsen-flames, and after a good deal of labour and many failures was at last successful in obtaining good observations up to 335°, which afford distinct proof that the curve of expansion above 200° conforms to the general law.

To establish this decisively, requires observations above 300°; and unfortunately it is just at this temperature that difficulties accumulate on all sides. To overcome these, required a kind of apprenticeship to be served.

I shall now describe the method finally adopted and employed in the last series, so that they may be repeated without obstacle to success.

§ 4. The difficulties that beset the inquiry come from three sources. 1. The glass tube that contains the water must be vertical; and, to prevent the liquid column from breaking up by the formation of vapour-bubbles below its summit, the temperature should be an increasing one upwards, whereas the natural gradient of a simple heater is a decreasing one upwards. 2. The hard German glass, though more impervious to the solvent power of water than soft glass, begins to whiten and cloud below 300°, and generally becomes mottled with opake patches just at the very part of the tube where clear vision is indispensable. There is also a certain amount of the liquid absorbed, apparently, but which may be partially recovered by heating. 3. To obtain certainty as to the mean temperature of the liquid column, it was thought proper to use as thermometer a tube of the same length as the water-tube, filled with a liquid of great expansibility and high boiling-point. To graduate such thermometers by comparison with ordinary bulb-thermometers, it was necessary to have an open bath of a similar liquid and transparent in a glass vessel sufficiently deep to contain the tube held vertical. The fumes of such liquids heated towards 300° become overpowering, and reliable comparisons can hardly be obtained above 250°.

§ 5. The ordinary chemical thermometers, having a vacuum at the top of the stem, are not to be depended upon at temperatures near the boiling-point of mercury, as vapour is sometimes generated below. Besides they are graduated so as to require the stem to be of the same temperature as the bulb. I therefore had one made with short stem and bulb strong enough to withstand high pressures, containing air over the mercury. It was about the same length as the water-tubes, and 100° measured about $\frac{5}{4}$ths of an inch. After being heated to 400°, it was graduated and marked off in divisions of 10°, and did not alter in its reading afterwards. It is represented by Pl. III. fig. 7. Another two of a similar kind were also provided; and, each being graduated by itself and afterwards compared with the others, the highest temperatures would have been well determined if it depended only upon thermometers.

§ 6. *Graduating tubes.*—Fig. 3 (Pl. II.) is a full-size representation of the last of the six water-tubes employed. It was prepared by first cleaning out, before sealing, with sulphuric acid, then by distilled water, then dried by heating. The bottom was then closed at the flame, and allowed to remain in it until the glass thickened. The top was then sealed before filling, employing a very small Bunsen-flame. In operating with small flame, the tube after a pull will contract of itself without being drawn; the glass is thus kept thick and strong; and the flame being slowly withdrawn while the tube is kept revolving, a strong conical point may be obtained. This is filed down until a small needle-point opening appears. Before filling the tube, a scale of twenty divisions to the inch is finely and carefully marked upon the glass, as shown in fig. 3. The mode of doing so is shown in fig. 6. The feather edge of the ivory scale resting on the glass, the lines were drawn with a fine-pointed lithographic steel pen, using a watchmaker's lens. The ink was of gamboge, rubbed down in water and darkened with black. When dry, the marked portion of the tube was passed through a small flame, gradually heating until, when near red heat, the lines are observed suddenly to turn black; it is then to be quickly withdrawn. The lines are thus ingrained upon the glass, and cannot be easily rubbed off. The tube is then filled with distilled water to about the proportion required. The small hole is then sealed by holding the extreme point only in the flame for a few minutes.

§ 7. *To find total contents of the tube and the proportion occupied by the liquid.*—At a small Bunsen-flame the upper part of the tube is dried, beginning at the top and working downwards; then, immersing in a water-bath of known atmospheric temperature, the scale-reading of the lower part of the capillary cup is noted. The liquid is then shaken to the other end of the tube and a similar reading noted, taken by applying the ivory scale. Halfway between these is the middle point. Add to this the distance between this middle point and the zero of scale, the sum is the value of zero of scale, to which we have to add the scale-reading of an observation, and half the depth of the capillary cup is taken by estimation as the volume to be added to reading of bottom of cup, which is always a very distinct line by transmitted light.

Next shake a small part of the liquid into the conical top; then dry, and take readings. Thus, and by allowing for capillary cup, we obtain the contents of half the tube, and thence that of the liquid with great exactness. By further operations of the same kind we may discover any tapering of the tube. This was slight, and omitted in the reductions.

§ 8. *Graduating the tube thermometers.*—The liquid employed for these thermometers was burnt linseed oil. After the tubes were marked with scale divisions and sealed, they were subjected for an hour to a temperature of 350° before graduation. Three were constructed (the least of them is shown in fig. 3, lashed with copper wire to the water-tube). In the top of the funnel F, fig. 1, a large test-tube was fixed and filled with lard. In this bath the standard mercurial thermometer, fig. 7, and an oil-tube were placed. The heating took place by means of two Bunsen-burners placed below, as shown in fig. 1. At the top of each Bunsen-tube there was fixed a spiral wire (fig. 5), which effectually prevented the flame from jumping downwards when at its lowest. With these and a lever with graduated arc applied to the gas-cock *s*, fig. 1, the temperature could be gradually raised and held in command if the day was calm. In repeating such experiments it would be well to have a gas-cock made specially, so as to be able to regulate the supply by uniform motion.

In the lard-bath the convection currents were very rapid, and the temperature throughout nearly the same. The standard thermometer was kept moving up and down for some time before making the comparison (which requires a perfectly steady temperature to be maintained).

The value of the scale-readings of the oil-tubes were thus determined, and their curves of expansion drawn to a large scale up to about 280°. Beyond this the curve was carried on the hypothesis that it was an arc of a hyperbola, which from previous experience I had found to answer well as an empirical curve in liquid expansion.

§ 9. Another method was employed to measure the higher temperatures of the last water-tube. A tube thermometer containing a 5½-inch column of sulphuric acid was prepared. The value of its scale-readings above 210° depended on a very good observation in acid-bath at 203° (the temperature having remained steady for half an hour). To this and the reading at atmospheric temperature the formula derived from the law of expansion was applied, and the value of the higher scale-readings computed. It was then lashed to the oil thermometer, and simultaneous readings taken on both up to 350°. On comparing results, it was found that 330° by the acid-tube was 343° by the oil. This corresponds to the highest observation of the last water-tube. A comparison was finally made with acid-tube and standard in acid-bath at 270°, but this did not help to a nearer accordance.

§ 10. Granting that the higher temperatures are uncertain and doubtful in their exact position on the scale of temperature, the method of curves generally employed will cause the divergence to be gradual, so that the law of expansion, if it existed, would not by this cause be masked.

§ 11. Sulphuric acid has little more than half the expansibility of oil, but has the advantage of being colourless and easier managed. Glycerine was not employed, although it may be the best liquid upon the whole, being only one-fifth less expansible than oil. In using oil-tube thermometers it is not proper to regard observations under 100°.

§ 12. *Mode of conducting the observations.*—The figures 1, 2, 3, and 4 (Pl. II.) show the arrangements adopted. The heater, C C, is a thin brass tube 2 inches diameter and 27 inches long, held vertical over Bunsen-flames by screws as shown in figs. 1 and 2. To a length of 8½ inches down from the top two slits were cut one inch wide and exactly opposite. To these were applied slips of plate glass (G G, fig. 2). One inch lower down a plate of talc was fixed to intercept the upward current of heated air from the lower part and direct it upon the upper end of the tubes. An inspection of figs. 2, 3, and 4 will show the size and position of this plate, without which the observations would have been impracticable.

The tubes of oil and water O, W were suspended as shown, having a small cork stuck on at the lower end of the water-tube. The additional glasses on the slit near which the tubes hung were required to prevent loss of heat by radiation. A lens was used in reading the position of the bottom line of the capillary cup of the liquid on the scale. It is a very distinct object by transmitted light.

The only way to avoid the difficulty caused by corrosion of the glass, is to make the preliminary arrangement so that a single series of observations shall complete the range of temperature in as short a time as possible without expecting a second from the same tube. The temperature must always be kept ascending, and is regulated by the stopcock (*s*, fig. 1), which ought to be made, if possible, so that the angular motion should increase the flame at a uniform rate. The readings are taken when, although the temperature is still ascending, it is going very slow. The gas-cock is then touched slightly to increase the flame a little, then another observation, and so on. A windy day is to be avoided.

§ 13. The following series was completed in less than an hour; the tubes *in situ* are shown in fig. 3, and the top surface of liquids at the beginning and end are marked at β and ϵ. The glass of the water-tube appeared but slightly acted on, and there was no sensible absorption when afterwards examined. No liquid had collected in the top cone, and the highest observation was quite distinct and unexceptionable.

Oil.	Water.	Temperature C.M. by		Oil.	Water.	Temperature C.M. by	
Inch.	Half inch.	Acid-tube.	Oil-tube.	Inch.	Half inch.	Acid-tube.	Oil-tube.
−0·035	0·89	10°	10°	+0·960	3·48	297°·7	303°·7
+0·650	2·23	225·3	224	0·950	3·40	295·3	301·3
0·750	2·58	250	251	1·030	3·90	311·7	320·7
0·800	2·73	262	263·5	1·050	3·96	315·8	325·5
0·840	2·97	271	273·8	1·060	4·07	317·5	327·9
0·850	3·00	273·5	276·3	1·075	4·17	320·5	331·4
0·900	3·15	284·6	288·9	1·100	4·27	325·5	337·4
0·925	3·30	290	295·1	1·115	4·47	328·3	341·0
0·950	3·43	295·3	301·3	1·120	4·48	329·3	342·0

§ 14. These observations will be found projected in fig. 8, (Pl. III.) the readings of the water-tube being coordinate with the C.M. (Centigrade mercury) temperatures by acid-tube thermometer. The thick dotted line is drawn by means of a thin flexible straight edge bent round and held so as to suit the general trend of the points. The vertical dotted lines intercepted by the thick dotted line represent the ordinates to each 10° of the air-thermometer (C.A.), which were read off by scale as follows:—

320° C.A. . . .	4·486	260° C.A. . . .	2·835
310 " . . .	4·068	250 " . . .	2·660
300 " . . .	3·745	240 " . . .	2·490
290 " . . .	3·455	230 " . . .	2·350
280 " . . .	3·230	220 " . . .	2·222
270 " . . .	3·023	210 " . . .	2·102

The value of zero of scale is 6·36, and ·04 is half the depth of capillary cup, so that 6·4 added to each ordinate gives absolute liquid volume at each 10° of the air thermometer. The reading at 4°, the point of unit or minimum volume, is 0·89, to which adding 6·4 gives 7·29, the absolute liquid volume at 4°. If there was no correction required for expansion of glass and the density of vapour in the upper part of the tube, the quotient $\frac{6\cdot4+4\cdot486}{7\cdot29}=\frac{10\cdot886}{7\cdot29}$ would represent the volume of water at 320° C.A. in terms of volume unity at 4°. If the glass that contained the 7·29 of water were to assume the volume it has at 320°, the apparent volume 7·29 would be diminished ·047, $[\pi]$ being at the rate of $\frac{1}{500}$th per 100°, which has been assumed as a probable value. Again, if we find the volume that the steam in the upper part of the tube would obtain if condensed to water at 4° and call this y, then $\frac{10\cdot886}{7\cdot29-(\pi+y)}$ is the true volume at 320° To find y, the whole contents of the tube is 12·33, from which taking 10·89, leaves 1·44. The density of steam at t is expressed by the formula $\left\{\frac{t+76\cdot8}{h_1}\right\}^6=\text{D}$, which rests on Regnault's observations and the law of vapour-density (see Phil. Mag. March 1858, Appendix I., and June 1861, § 11). The density of steam at 100° in terms of water-unity at 4° being ·00061337, we have $\left\{\frac{100+76\cdot8}{h_1}\right\}^6=\cdot00061337$, and hence $\log h_1=2\cdot78296$.

Thus D for $t=320$ is $\left\{\frac{320+76\cdot8}{[2\cdot78296]}\right\}^6=0\cdot109$, and this, multiplied by 1·44 the volume of top space, gives $y=0\cdot157$, and $\pi+y=\cdot204$, which, subtracted from 7·29, gives 7·086 as corrected denominator, and $\frac{10\cdot886}{7\cdot086}=1\cdot5363$, the volume at 320° C.A.

§ 15. The second column of the following Table was thus computed. In the third column are the differences for 1°. These are projected as ordinates to the temperatures in fig. 9, then equalized as shown by the *small* dotted curved line. The ordinates to this line are read off by scale and set down in the fourth column as the equalized differences. These are values of $\frac{dv}{dt}$. Now, since the law of liquid expansion requires that these differences, divided by their respective volumes, should, when projected as ordinates to the temperatures, range in a conic hyperbola, the reciprocals, if the observations were perfect, ought to range in a straight line.

1.	2.	3.	4.
320° C.A.	1·5363	·00621	·00592
310 ”	1·4742	·00462	·00486
300 ”	1·4280	·00424	·00410
290 ”	1·3856	·00338	·00356
280 ”	1·3518	·00314	·00313
270 ”	1·3204	·00292	·00292
260 ”	1·2912	·00267	·00267
250 ”	1·2645	·00260	·00243
240 ”	1·2385	·00214	·00220
230 ”	1·2171	·00193	·00198
220 ”	1·1978	·00178	·00178
210 ”	1·1800		

The first reciprocal is $\frac{1\cdot5052}{\cdot00592}=254\cdot3$, which in fig. 10 is set off as ordinate to 315°, and so on down to $\frac{1\cdot1890}{\cdot00178}=668$, which is set off to 215°.

These points are distinguished by small circles. It will be remarked that they range in a flat ogee curve. The strong broken *straight* line is drawn through them and transferred to fig. 9, showing that if the equalized differences had been taken from the strong dotted line instead of the faint dotted line, the points in fig. 10 would have ranged exactly in a straight line, as theory requires. The difference this involves in the curve of observations (fig. 8) would hardly be sensible.

§ 16. Such accordance is indeed much greater than at first sight might be expected from the observations, which in fig. 8 are plotted in the rough from the note-book. But the method of drawing the curve through them by means of a flexible straight edge held in one position for the whole length accounts for this. If the law of continuity is maintained, the true line must be one of a similar kind with regular curvature; and if not exactly coincident with this, must intersect it with flat loops of very limited divergence, or be nearly parallel to it, because the straight edge is fixed from end to end of the range of points, and is assisted by the observations quoted in the previous paper; one of which, viz. that at 200° is computed backwards, and marked off on fig. 8. In this figure a faint dotted line has been drawn below the one now discussed. It coincides with certain of the observations, which, from some recurring cause (connected with the increasing of the temperature by steps), appear on a lower level than the others. I have carefully analysed this curve in the same way as the first, and find the projection of the points in fig. 10 is so close to those given that it would only confuse to add them.

§ 17. The question now occurs, what is the value of $\frac{1}{\rho}$ given by the line AA? (see § 2 in original paper on Liquid Expansion, Phil. Mag. June 1861.)

The general equation that expresses the law is $v^{\frac{1}{\rho}}=\frac{k}{\gamma-t}$. To find γ, we produce AA to meet the axis of temperature, which it does at 389°. This therefore is the value of γ indicated by these observations. The value of k, which is equal to $\gamma-t$ when $v=1$, must not be taken from the temperature 4° at which the volume of water is unity, because the curve of expansion departs from the theoretical curve at the lower part of its range. We must compute it from a scale-reading of an ordinate of the line A, as nn at 245°, which is $517=\frac{dt\,.\,v}{dv}$; and since $\frac{dt\,.\,v}{dv(\gamma-t)}=\frac{1}{\rho}$ and $\gamma-t=144$, we have $\frac{1}{\rho}=\frac{517}{144}=3{\cdot}59$.

What is the value of $\frac{1}{\rho}$ required by the theory that connects the expansion of a liquid with the density of its saturated vapour? By referring to § 3 of original paper, the rule is that $h\times\frac{1}{\rho}$ is a constant quantity F (French measures) for all bodies; and referring to the value of h derived from M. Regnault's observations given in Appendix I. to paper in Phil. Mag. March 1858, we find the theoretical value of $\frac{1}{\rho}=3{\cdot}21$.

§ 18. The results hitherto have been with temperatures by the curve of the acid tube thermometer produced. I have drawn similar curves with the temperatures by the curve of the oil tube thermometer produced, and have projected the results on fig. 10, the points being shown by crosses and the straight line B B drawn through them. We have here a similar flat ogee range, and the value of $\frac{1}{\rho}$ that comes out by a process of computation similar to the above is 2·69. The mean is 3·14, which is remarkably close to 3·21.

§ 19. That the deviation of the acid-tube curve should be so nearly the same as that of the oil-tube and in the opposite direction, is a curious coincidence if the true line really lies, as it appears to do, halfway between A A and B B. While suspecting that there may be a latent cause for this (though each curve was as carefully determined as if the results depended on it alone), it cannot of course be said that $\frac{1}{\rho}$ has been fully brought out. There may be a deviation in its value corresponding to the deviation in expansion that undoubtedly exists from about 185° downwards.

§ 20. With the view of obtaining further evidence on this point, I have examined by the differential process (detailed in § 27) the results of three series of observations of three of those tubes first experimented upon in which the temperature was carried up to from 260° to 280°. Assuming $\frac{1}{\rho}=3{\cdot}21$, the values of γ obtained by computing several of the values of $\frac{dv}{dt}$ at the higher temperatures accord with 411·6, varying from 400° to 430°. The thermometers employed with these water-tubes were linseed-oil tubes, but different from the last (represented in fig. 3), and somewhat longer and wider.

§ 21. To arrive at the probably true value of the highest temperature observed, we may divide the difference 12°·3 (between the scale of acid- and oil-tubes) in the ratio of the values of $\frac{1}{\rho}$. Thus $3{\cdot}59-2{\cdot}69=0{\cdot}90$, and $3{\cdot}59-3{\cdot}21=0{\cdot}38$; so ·90 : ·38 : : 12°·3 : 5°·2, which, added to 320°, gives 325°·2 C.A. as the temperature at which the volume is 1·5363. We have to draw the curve $v^{3{\cdot}21}=\frac{k}{\gamma-t}$ through this point, touching or cutting the lower branch or verging into it. If we arrange the terms so as to make the theoretical curve touch the parabola which answers so exactly between 50° and 150°, we find that contact is impossible. This shows that there is a gradual rise from the parabola to the theoretical. Judging from the curve of differences, the deflection from theory commences about 210°; and taking 1·18 as the volume at this temperature, and drawing the curve (by computation) through this and the preceding observation at 325·2, we find $\gamma=411\cdot6$, $k=342\cdot8$, and

$$v^{3\cdot21}=\frac{342\cdot8}{411\cdot6-t} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (Q)$$

is the equation for the volume of water in terms of its temperature from 210° upwards.

This cuts the lower parabola at about 185°. The intersection is shown upon a large scale in fig. 11. Also the line of observations, which coincides with the parabola at about 175° and with the normal theoretical at about 190°.

§ 22. The annexed Table contains the general results of the inquiry.

Column 1 is the C.A. temperatures.
„ 2 is the volumes computed from (Q).
„ 3 is the observed volumes. Above 210° the mean of the oil and acid thermometers is taken.
„ 4 is the observed minus the computed volumes.

1.	2.	3.	4.
411·6°			
400	2·8715		
390	2·3660		
380	2·1014		
370	1·9286		
360	1·8038		
350	1·7069		
340	1·6287		
330	1·5637		
320	1·5084	1·5098	+·0014
310	1·4606	1·4584	−·0022
300	1·4185	1·4181	−·0004
290	1·3810	1·3804	−·0006
280	1·3475	1·3479	+·0004
270	1·3171	1·3182	+·0011
260	1·2893	1·2896	+·0003
250	1·2640	1·2645	+·0005
240	1·2405	1·2397	−·0008
230	1·2190	1·2186	−·0004
220	1·1986	1·1986	0
210	1·1799	1·1800	+·0001
200	1·1622	1·1612	−·0010

1.	2.	3.	4.	5.	6.	7.
200	1·1622	1·1612	−·0010	·0009		−·0060
190	1·1456	1·1455	−·0001	·0012	……	−·0049
180	1·1299	1·1309	+·0010	·0012	……	−·0036
170	1·1151	1·1173	+·0022	·0013	……	−·0020
160	1·1011	1·1046	+·0035	·0013	……	−·0002
150	1·0878	1·0926	+·0048	·0013	……	+·0017
140	1·0752	1·0813	+·0061	·0014	……	·0037
130	1·0632	1·0707	+·0075	·0017	……	·0058
120	1·0517	1·0609	+·0092	·0018	……	·0079
110	1·0407	1·0517	+·0110	·0021	……	·0103
100	1·0302	1·0433	+·0131	·0024	……	·0128
90	1·0201	1·0356	+·0155	·0027	……	·0154
80	1·0104	1·0286	+·0182	·0030	3333	·0183
70	1·0011	1·0223	+·0212	·0034	2941	·0213
60	0·9921	1·0167	+·0246	·0036	2778	·0247
50	·9835	1·0117	+·0282	·0042	2381	·0283
40	·9752	1·0076	+·0324	·0047	2128	·0325
30	·9671	1·0042	+·0371	·0052	1923	·0371
20	·9594	1·0017	+·0423	·0060	1667	·0423
10	·9519	1·0002	+·0483	·0072	1389	·0483
0	·9446	1·0001	+·0555	·0087	1149	·0555
−10	·9376	1·0018	+·0642			·0644

These differences, it will be observed, increase regularly from 190° downwards. We have here presented to us the effect of the abnormal cause which constrains the molecules of water and raises them to a greater volume than they would assume at the respective temperatures if they had retained the character of a perfect liquid. Do the numbers in this column indicate any quantitative law of divergence? This has been tested by tabulating the differences of these numbers in column 5, and the reciprocals of these differences in column 6. These reciprocals have been laid off as ordinates to the temperatures in fig. 13 (Pl. IV.). It will be observed that the points range in the straight line *s s* nearly. This line meets the axis of temperature at $-47^{\circ}{\cdot}8$. Let w represent a number in column 4 at temperature t. The values of $\frac{dw}{dt}$ are given in column 5, and of $\frac{dt}{dw}$ in column 6. Fig. 13 represents $\frac{dt}{dw} \propto (t+47{\cdot}8)$. If this were a governing law, we should have $w \propto \log\,(t+47{\cdot}8)$, which affords the numerical equation

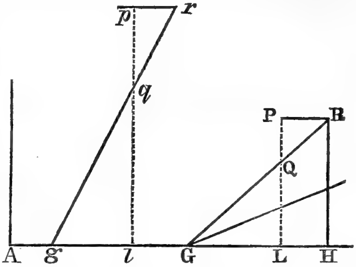
$$c\left\{\log\frac{t+47{\cdot}8}{47{\cdot}8}\right\}={\cdot}0555-w,$$

in which $\log c=8{\cdot}94037$. The numbers in column 7 have been computed from this equation: on comparing them with those in column 4, it will be remarked that it exactly represents the curve of observed expansion below 100°, but that above that the observed volumes gradually depart from it and enter the curve of normal liquid expansion at about 190°. The curve of column 7 crosses the normal at 159°, and at this temperature the observed volume is ·0036 above the point of intersection, an amount which represents 3°.

§ 23. It may be that the equation for column 7 is only to be considered as empirical; yet it may be remarked that if it expresses a physical law, that law is very simple, and has reference to a limiting temperature, as do also the laws of saturated vapour and normal expansion. The equation $\frac{dt}{dw} \propto (t+47{\cdot}8)$ means that the *absolute* increment of divergence from the normal liquid volume increases with a descending temperature in the inverse ratio to the distance of that temperature from the limiting point $-47{\cdot}8$. The normal law of expansion is, that the *proportionate* increment of volume at a given temperature increases in the inverse ratio of the distance of that temperature from the upper limit $\gamma=411^{\circ}{\cdot}6$.

§ 24. Since the law of liquid expansion has all the appearance now of being general, and assumes the character of the quantitative exponent of perfect liquidity, it may be as well, before concluding this paper, to describe the method of projecting on a general chart the line which represents both the vapour and liquid condition of a body throughout its range of temperature.

The law of density of the saturated vapour is $\left\{\frac{t-g}{h}\right\}^6=\Delta$, and of the liquid $\left\{\frac{\gamma-t}{k}\right\}^\rho=\mathrm{D}=\frac{1}{v}$, or $v^{\frac{1}{\rho}}=\frac{k}{\gamma-t}$. The connexion between them is that $h \propto \rho$, or $\frac{1}{h}\propto\frac{1}{\rho}$.



On the axis of temperature A H take A G $=g$ and A H $=\gamma$, reckoning all temperatures t as well as γ from A the zero of gaseous tension. Draw R H ⊥ to A H, and set off H R $=(\gamma-g)\frac{1}{\rho}$. Join GR and draw R P ∥ to axis. At a point L corresponding to a given temperature $t=$ A L draw the ⊥ L Q P, cutting G R in Q. We have P Q a constant function of the density of the liquid in terms of the temperature, and Q L a constant function of the density of the saturated vapour in terms of the temperature. P R is $(\gamma-t)$, and P Q is $(\gamma-t)\frac{1}{\rho}=\frac{v\,dt}{dv}$; so that the inverse of P Q represents the proportionate increment of volume or decrement of density for 1 degree. Again, we have G L $=(t-g)$, and Q L $=(t-g)\frac{1}{\rho}$; and since $\frac{h}{\rho}=\mathrm{F}$ (the constant 504), we have $\frac{1}{h}=\frac{1}{\rho\mathrm{F}}$ and $\frac{t-g}{h}=(t-g)\frac{1}{\rho\mathrm{F}}=\frac{\mathrm{Q\,L}}{504}$; and since the sixth power of $\frac{t-g}{h}$ is equal to density Δ, we have $\frac{\mathrm{Q\,L}}{504}$ to represent the sixth root of the density of the saturated vapour—taking the word density to mean the quotient of the pressure or tension by the temperature reckoned from the zero of gaseous tension (A).

Suppose $g\,r$ to represent the chart-line of another body, say cyanogen, and $p\,q\,l$ to be the vertical of t_1. If $p\,q=$ P Q, then the value of the proportionate increment of volume of cyanogen at t_1 would be the same as the proportionate increment of water at t, and so on; if it is double or half, the proportionate increment is double or one-half. If $q\,l=$ Q L, the gaseous density of cyanogen vapour at t_1 is equal to the gaseous density of water vapour at t; that is to say, their absolute densities bear the same ratio to each other as their vapours do when *in æquilibrio* of pressure and temperature at 15° separated from their liquids.

If $q\,l$ is one-half Q L, the density is $\frac{1}{64}$th, being the sixth power of one-half.

The proportionate increment of vapour-density for 1 degree is represented by the reciprocal of G L, because $\frac{d\Delta}{\Delta}=\frac{6dt}{t-g}$. Thus, suppose $l\,q=$ L Q, but $g\,l=\frac{1}{2}$G L, the proportionate increment of density in the vapour represented by the line $g\,l$ for rise of 1° is double that of the vapour represented by G L, although their densities are equal.

§ 25. The simplicity of the relation between the proportionate increments of density in the vapour and in the liquid is noteworthy. In the vapour the proportionate increment of density is $\frac{6}{t-g}$; in the liquid it is $\frac{1}{(\gamma-t)\frac{1}{\rho}}$. A numerical example will illustrate this. Let $g=0°$ and $\gamma=100°$, and $\frac{1}{\rho}=3$, and suppose $t=50°$. The proportionate increment of vapour-density for 1° is $\frac{6}{\rho}$, or eighteen times that of the liquid; the first being $\frac{6}{50}$ths, and the second being $\frac{1}{150}$th. When they are equal, $\frac{6}{\rho}(\gamma-t)=(t-g)$, or $t=94°{\cdot}8$. Thus, to find the point when these proportionate increments are equal, the rule is to divide $(\gamma-g)$ by $\left(\frac{6}{\rho}+1\right)$and take the quotient from γ.

It is remarkable that in alcohol and ether this corresponds very exactly with the point of transition. Is the temperature of this point always to be thus found? If so, what is the transition-point of water? $\gamma-g=489$, $\frac{6}{\rho}+1=20{\cdot}26$; so $\frac{489}{20{\cdot}26}=24$, and $\gamma-24°=388°$ C.A.$=399°$ C.M.

§ 26. As an example of the mode of laying down a line upon the chart, chloroform may be taken. We have observations of its vapour-tensions by Regnault, and of its expansion by M. Pierre.

At 60° C.M.$=60{\cdot}48$ C.A$=t_0$ its tension is 738 millims.
At 100° C.M.$=100°$ C.A. $=t_1$ its tension is 2354·6.

$$\left\{\frac{738}{274+60{\cdot}48}\right\}^{\frac{1}{6}}=1{\cdot}1410, \qquad \left\{\frac{2354{\cdot}6}{274+100}\right\}^{\frac{1}{6}}=1{\cdot}3588.$$

The difference is 0·2178, which, divided by t_1-t_0, gives

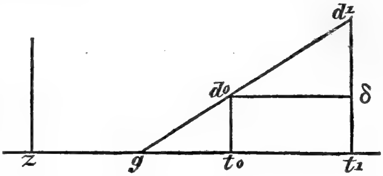
$7{\cdot}74124 = \log\frac{1}{h}$, which, added to $\log F = 2{\cdot}70285$, gives

$$0{\cdot}44406 = \log\frac{1}{\rho},$$

and thus $\frac{1}{\rho} = 2{\cdot}758$. This, multiplied by $t_1 - t_0$ or $d_0\delta$, gives δd_1; and since

$$\delta d_1 : \delta d_0 :: d_0 t_0 : t_0 g,$$

we have $t_0 g = 207$; and since $t_0 z = 274 + 60{\cdot}48$, we have $zg = 127{\cdot}5$ and $gt_1 = 246{\cdot}5$. This gives the position of g on the axis of temperature.



To find $t_1 d_1$, we multiply $gt_1 = 246{\cdot}5$ by $\frac{1}{\rho} = 2{\cdot}758$, and set off the product 685 from the vertical scale of the chart as the length of $t_1 d_1$. Having thus found g and d_1, the chart line is drawn.

Densities deduced from high tensions are generally less than the true, in consequence of deviation from Mariotte's law, the pressure increasing in somewhat less proportion than the density. On this account the vapour-tensions taken below the boiling-point are to be preferred in fixing the *direction* of the chart line.

§ 27. To find the upper terminal γ of the chloroform line, we have M. Pierre's observations (*Ann. de Chim.* October 1851, p. 208) to apply to the equation $(\gamma - t)\frac{1}{\rho} = \frac{v\,dt}{dv}$, in which γ is the unknown quantity. This equation is adapted to a series of dilatation observations with small range of temperature. It is to be noted that those taken below the atmospheric temperature are to be avoided as generally faulty (see figures appended to former paper on Expansion).

$$\begin{array}{llcl}
15^{\circ}{\cdot}90 \text{ C.M.} = & 16^{\circ}{\cdot}19 \text{ C.A.} & \ldots & 1{\cdot}0187 \\
23{\cdot}40 & \underline{23{\cdot}79} & \ldots & \underline{1{\cdot}0276} \\
 & 7{\cdot}60 = dt & & \underline{{\cdot}0089 = dv} \\
 & & & 1{\cdot}0231 = v
\end{array}$$

Hence

$$\gamma = \frac{v\,dt\,\rho}{dv} + t = 334^{\circ}.$$

The two extreme observations at 0° and 62°·72 being computed in the same way, give $\gamma = 329^{\circ}$. So this value is probably not far from the truth.

§ 28. In fig. 12 (Pl. IV.) is presented a small chart with the lines of a few bodies computed and laid down as above, chiefly from the observations of Regnault. The parallelism of certain lines (as those marked W), and the convergence of others to the same point in the axis of temperature, is well marked out.

§ 29. It would be well if the constants g, γ, and $\frac{1}{\rho}$ or $\frac{F}{h}$ were fully determined for the various bodies in a state of purity where it is practicable. They are supplementary to specific gravity and boiling-point; and when tabulated both numerically and graphically, would perhaps carry us a step further on the road that connects the physical with the chemical.

The description of a body with regard to its quantitative physical characteristics embraces:—(1) specific gravity, (2) boiling-point, (3) constants of its chart line, (4) constitution of a gaseous volume in volumes or fractional parts of a volume of its constituents: [*e. g.* $HO^{\frac{1}{2}}=9$, which at once exhibits its chemical constitution and specific gravity in the gaseous or vaporous form; so also $H^{1\frac{1}{2}}N^{\frac{1}{2}}=8{\cdot}5$ (ammonia), $O\,S^{\frac{1}{6}}=32$ (sulphurous acid), $O^{1\frac{1}{2}}S^{\frac{1}{6}}=40$ (sulphuric acid), $O^{\frac{1}{2}}N=22$ (protoxide of nitrogen), $O^{\frac{1}{2}}N^{\frac{1}{2}}=15$ (binoxide of nitrogen), &c.

A fractional index to a symbol as showing that in the act of chemical union what was a single molecule has separated into two or more, is demanded by the dynamical theory of gases; and we cannot have a distinct idea of the potential nature of the dynamic convulsion or evolution of a dynamic integral that accompanies chemical union without keeping both the symbolic letters and their indices united in idea. No other indices can be substituted; for although others may define the *chemical* constitution, they do not define the *molecular* constitution, which involves the antecedent separation as well as consequent union.

MM. Deville and Troost have by heat alone separated S into $S^{\frac{1}{3}}$; we are therefore justified in entertaining the idea of the ultimate molecule as an integral part of a gas or vapour being capable of subdivision. How far this may go, it would be improper to limit, as the forces at the command of the chemist are so insignificant in comparison to the heat and pressure with which the elements of matter have to contend in the body of the sun or even below the surface of our planet. But it is daily becoming more manifest that the elementary molecule, although minute beyond conception, is to be studied as a microcosm essentially dynamical in its internal constitution, its apparent statical condition being simply the antagonism of transcendant *vis viva* potents.] (5) molecular volume; (6) other points with reference to specific heat, latent heat, combining-force, compressibility, capillarity, which may be expected gradually to assume a more simple aspect as materials for comparison accumulate.

June 15, 1863.

Appendix.

I. To compute the weight of a cubic inch of a liquid and of its saturated vapour at any temperature within range from the constants of its chart-line.

The general expression for the weight u (in grains) of one cubic inch of saturated vapour at t(C.A. temp.) is

$$\frac{m\delta}{\tau+274}\left\{\frac{t+274-g}{\tau+274-g}\right\}^6=u,$$

in which $\log m=0{\cdot}77262$, $\delta=$ vapour-density of the body on the hydrogen-scale (*e. g.* if the body is water, $\delta=9$; if mercury, $\delta=101$); $\tau=$ the C.A. boiling-temperature under pressure 760 millims. $\left[\text{obtained from } \frac{760}{\tau+274}=\left\{\frac{t+274-g}{h}\right\}^6\right]\left[\frac{1}{h}=\frac{1}{504\rho}\right]$.

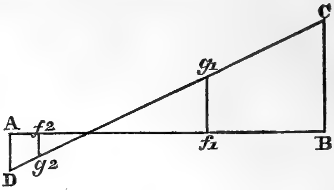
The general expression for the weight W (in grains) of one cubic inch of a liquid is $\mathrm{N}\left\{\frac{\gamma-t}{\gamma}\right\}^\rho=\mathrm{W}$, in which N is the weight of a cubic inch at 0° C.

The details of the above may be found in § 11 of previous paper on liquid expansion (Phil. Mag. June 1861). The two following typographical errors require correction:—

§ 11, line 15, *for* $\tau=247^\circ{\cdot}45$ *read* $g=247^\circ{\cdot}45$.
„ 16, *for* $g=611^\circ{\cdot}28$ *read* $\tau=611^\circ{\cdot}28$.
line 17, *for* $\mu h=\frac{\tau-g}{\Lambda}$ *read* $\mu h=\frac{\tau-g}{\beta}$.

II. The complete method of observing in sealed tubes.

If two or more tubes are filled with the same liquid in different proportions of their volume and then sealed, the increase of density in the vapour and of volume in the liquid throughout the range of temperature may be obtained distinctly as separate phenomena, independent of any theory, from readings of their apparent liquid volume. Also the number of tubes employed above two affords the means of checking their general accuracy. *E. g.*, the liquid volume in No. 1 tube (given in decimals of the volume of the whole tube considered as unity) at the lower temperature being m_1, and at the upper temperature n_1, on the line A B reckoned as unity mark off $Af_1=n_1$, and the ordinate $f_1g_1=n_1-m_1$. In the same way for No. 2 tube, lay off $Af_2=n_2$ and $f_2g_2=n_2-m_2$, which, if a negative quantity, is marked off below A B. The points g_1, g_2, &c. for any number of tubes 1, 2, &c. range in a straight line. Draw perpendiculars at A and B meeting the line g_1, g_2, &c. in D and C. It may be proved that (the expansion of glass being allowed for) $\frac{AD}{AB}=$ difference of vapour-density at upper and lower temperature in terms of the density of the liquid at lower temperature considered as unity. Also $\frac{AB-BC}{AB}=$ density of the liquid at the upper temperature in the same terms.



The curves of the readings of several such tubes about 4 inches long provide the ordinate fg; and if extended to sufficiently high terms, afford data to compute the chart-line independently of boiling-point or any observation of tension. A perfectly unknown liquid treated in this way may be established on the chart.

(See observations on the density of vapours and their liquids at the point of transition, British Association Report, 1853. Also paper in Royal Society's Archives for 1853.)

XVI. *Mineralogical Notes.* *By* Professor N. S. MASKELYNE *and* Dr. VIKTOR VON LANG, *of the British Museum.*

[Continued from vol. xxv. p. 453.]

Notices of Aërolites. *By* Nevil Story Maskelyne.

20. *Khiragurh.*

IN the article numbered 14 of these notices, I have given a description of an aërolite that fell on March 28, 1860, at a place called in the description Kheragur, and which I have assumed to be an erroneous spelling of the word Dhenagur. I have to thank Dr. Forbes Watson and Mr. Walker for the information that there is a place called Khiragurh, some 28 miles south-east of Bhurtpoor, lat. 26° 56′ 30″, long. 77° 53′. Khiragurh therefore is without doubt the locality of the fall, and represents the orthography of its name. I trust it may not be long before I may be able to add some facts about the circumstances associated with so recent an event.

21. *Manegaum.*

The fall of an aërolite at a place variously spelt as Manegaon, Manjegaon, Manicgaon, and *Manegaum*, in the collectorate of Khandeish in India, on the 16th of July, 1843, has long been on record. For the account of the occurrence we are indebted to a communication from Captain J. Abbott, of the Bengal Artillery, made to the Asiatic Society of Bengal in 1844. (Proceedings of that Society for 1844, p. 880.)

The points of interest connected with the descent of this aërolite are as follows:—It fell only fifty paces from two inhabitants of the village of Manegaum, a place "on the banks of the river Pourna," and therefore not to be confounded (as I have in a previous article confounded it) with Mallygaum, the town on the great Bombay and Agra road, nor yet with Menjergaum, the only place with a similar name known to the authorities at the East India Office, and which is 130 miles south-west of Eidulabad. This small village would seem to be a place of insignificant importance, on the confines of Khandeish and to the west of Eidulabad.

The two villagers describe the fall as having been witnessed by them. There had been several claps of thunder with lightning some two hours previously, and the northern heavens were heavily charged with clouds; but no rain had fallen for eight days before, nor did any fall for four days after the event. Their attention was arrested by "several heavy claps of thunder and lightning," and they ran out of a shed to look round, when they saw the aërolite fall in a slanting direction from north to south, "preceded by a flash of lightning." It buried itself 5 inches in the ground, and appeared as a mass of about 15 inches long and 5 inches in diameter. It exhibited a black vitreous exterior, and was of a greyish yellow inside. At first the observers stated it to have been (as is recorded of the Bokkeveldt aërolite) comparatively plastic (?), and at any rate to have become more hard and compact subsequently*! There was only one stone seen, and that was smashed to pieces. Another witness mentions that the stone was at first cool, but in a short time became rather warm.

Descriptions such as this of events so startling, so sudden and momentary, so entirely outside of one's everyday experience, are characterized, as one would expect them to be, with a certain sharpness of feature and simplicity of statement that reflect impressions vividly and indelibly stamped on the mind by phe-

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* The phrase in the translation is the unintelligible one "mouldy." It may mean "crumbly;" but the subsequent expression that it hardened afterwards, implies apparently that a change came over the solidity of structure of the stone.

nomena so extraordinary. In their main features such accounts are no doubt true; but their truth is mixed with the exaggeration due to mental excitement, and with the erroneous observation that must needs result where an almost instantaneous event has been witnessed, or rather where a series of events have succeeded each other so rapidly as only to interrupt during one brief moment of terror and amazement the placid calm of a summer's day.

It may be doubted whether a highly educated and a scientifically trained person would in general relate the facts better (that is to say, more simply) than a poor Indian villager, whose graphic expressions and simple language deserve a careful estimate at the hands of those who would interpret them fairly. What is remarkable in these narratives in general is, their frequent concurrence upon certain points. And where evidence gathered in different latitudes, at various times, and from persons to whom the phenomena are entirely new, concurs in associating certain facts with the fall of an aërolite, we are bound to attach weight to that evidence, however apparently inexplicable the phenomena it records may be. The sound as of thunder that *precedes* instead of following the aërolite whose motion is so much swifter than that of sound in the air in its ordinary state—the appearance of a light, as of lightning falling from heaven, distinct from the actual mass of the aërolite itself (perhaps a dissociation due only to an optical delusion)—the occasional coldness of the stone (so well explained by Hofrath Haidinger)—the extraordinary loudness of the explosions—the supposed alteration in the solidity of the mass after exposure to (damp ?) air—the frequent connexion of the fall of an aërolite with a derangement of the electrical condition of the atmosphere, as asserted by the statements which associate the fall with a thunder *shower*, or otherwise with thunder and lightning,—these are some of the phenomena on which evidence has on several occasions been concurrent; and assuredly the statements regarding them are the records of *facts*, though they may have to be cleared of whatever is the result of imperfect observation before we can see those facts in their true light and eliminate from them the element of the marvellous with which they are alloyed. Nor does the circumstance that the phenomena recorded as witnessed are not the same in the case of every aërolite, at all militate against the accuracy of or degree of reliance to be placed on these several records. We have a remarkable instance of this in the association with some cases of aërolitic falls, and the absence from others, of explosive reports heard at the actual locality of the fall, as also in the diversity of circumstances recorded regarding the temperature of the freshly fallen mass in different cases.

The fall at Manegaum is chiefly interesting for the incidental testimony it bears in confirmation of some of the phenomena just alluded to.

The original softness ("mouldiness") of the stone, and its supposed ultimate hardening, may perhaps have been due to a sort of process of setting, arising from the action of damp on the flocculent whitish mineral which seems to hold the granules of the aërolite together in the very loose and incompact state of solidity in which it exists. The native who described the coolness of the stone, records also its being shivered to fragments in consequence of its loose state of aggregation. He spoke of it as at first black (previous to its being taken up? and so with only its crust visible?), afterwards blue, and finally becoming white.

I think, however, that both this statement and that of the change in solidity may perhaps be explained by the natives having spoken of different parts of the stone seen by them at different times. Thus there are parts of it of a bluish grey from the intermixture of a black mineral in vein-like bands; while again the fragments that were preserved were probably those that were the most solid and compact—the central portions, in fact, of the otherwise shattered pieces—so that the crumbling of the mass in the hands that first raised it may have given rise to an idea that it was originally *all* like "mould." Of these fragments only 2½ ozs. have been preserved, and they were in the collection of the Asiatic Society of Bengal at Calcutta. The specimen in the British Museum was one of the many liberal contributions for which the national collection of meteorites is indebted to that body.

The Manegaum stone is a singularly interesting one. It belongs to the "Howardite" class of Rose, embracing Mässing, Bialystok, Luotolaks, and Nobleborough. I can only speak from my own experience of the first two of these; and of those it is with Mässing rather than Bialystok that Manegaum presents a really close similarity. It exhibits a large number of crystalline, generally very irregularly-shaped fragments, rarely indeed with one or two crystal planes, but generally with only very uneven surfaces of fracture, or of imperfect cleavage. These crystalline grains are of a delicate primrose-yellow, passing in some instances into a darker shade and greener hue. It is probable that they are olivine, but their long resistance to the action of hydrochloric acid makes it very desirable that their analysis should be effected. I have found minute fragments with one very decided cleavage, and with a second perpendicular to it presenting a very uneven surface; and this, coupled with the evidence afforded by the directions of the planes of polarization as seen in the microscope in favour of its being prismatic in crystalline system, would seem to confirm the view of its being olivine, and help to show that Manegaum, like the other Howardite aërolites, is mainly composed of that mineral. Like these other stones, it contains this olivine ingredient in grains of some considerable size, larger grains, indeed, than are usually met with among the constituent minerals of aërolites. They, however, are seen to be present in every gradation of minuteness, and the whole are cemented together, or rather are loosely entangled in a sort of network of another remarkable mineral, the same probably which Rose has hypothetically described as anorthite.

It is, however, very unlike the anorthite as seen in the microscope in Juvenas, Stannern, or Jonsac, and seems by no means a characteristic feature of aërolites which the calculations of the chemist would proclaim to be rich in that felspar. It presents, on the other hand, a remarkable resemblance to the opake parts of trachytic and porphyritic rocks, such as those of the Drachenfels and the Perlenhardt, or of the bluish Andesitic porphyry of the Esterels. Probably the opake portions of the felspathic ingredients of granites, in exhibiting much similarity with these, give a further ground for the attribution of a felspathic nature to this substance.

It is a remarkably opake, white or yellowish-white mineral, and occurs as an ingredient in a considerable number of aërolites. Sometimes as a flocculent or curdy network, sometimes in opake round spherules, and often, too, in irregular pieces that look like fragments of these last, or, again, deposited along laminæ in crystals of other minerals, it appears to be a very common though a sparsely distributed ingredient of every different variety of aërolite. In Manegaum (and even to a greater degree in Mässing) it rises into a somewhat important constituent of the aërolite—which indeed consists almost exclusively of the olivine-like substance I have described, with some of this flocculent mineral. There is present also meteoric pyrites (or, as we must now call it since Hofrath Haidinger's recent interesting notice, *Troïlite*[*]) in a small amount; and a mineral which seems certainly to be chromite forms veins or reticulations and little dark bands here and there, in which its opake and crystalline particles are sprinkled, as it were, through the other ingredients, and which imparts to these portions of the stone the bluish cast to which allusion has been already made. The crust is of a very rich brownish black, and is tolerably thick, but devoid of the lustre peculiar to what may be called the "*enamel*" on the Eukrite aërolites. No iron is perceptible in the small pieces I have had the opportunity of examining; but a negative argument of this kind would be a very fallacious one, as I have found

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[*] *Sitzungsber. der Akad. der Wissensch.* Wien, March 1863.

evidences of the presence of this substance in aërolites that do not on a first scrutiny appear to contain it.

There seems also to be in Manegaum a very small amount of a transparent colourless crystalline mineral; but whether it is a distinct substance from the olivine-like ingredient, I am not able at present to say. The specific gravity of the aërolite is about 3·22.

A remark or two may not be out of place here in respect to the aërolites that are ascribed to the Howardite class of Rose.

The only two besides Manegaum that I have had the opportunity as yet of scrutinizing present very important differences. The Bialystok stone contains, to an amount by no means insignificant, very distinct crystals of what I have little hesitation in pronouncing to be an augite. It is a dark brown mineral with its planes of polarization oblique to the crystal and to a well-marked striation indicative of cleavage, and presenting a decided and interesting similarity to the augite of Juvenas, Jonsac, Petersburg, &c.*

The other observation I have to make is, that the Howardite kind of aërolite is not *sui generis*. In several aërolites belonging to other classes, one meets with nodules and spherules, often of considerable size, of aërolitic matter belonging strictly to this "Howardite" kind. It is seen in Richmond, and forms a considerable ingredient in Petersburg. It occurs also in Benares, Marion County, and Nanjemoy; and I have met with it in a large nodule from the stone of Quenggouk, and otherwise disseminated in that aërolite; and this list will doubtless be considerably extended.

A fact like this may serve to warn us against the endeavour to separate by too sharp lines of demarcation the different groups into which a first inspection of their lithological characteristics might induce us to divide the various aërolites in our collections.

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* Since the text was in the press, I have had an opportunity, through the civility of Prof. Shepard, of inspecting a small fragment of the Nobleborough stone. It is a curious aërolite. More like Bialystok than Mässing or Manegaum, it is rich in a very black and opake substance (probably two such substances, one of which is) like the dark augite in the Eukritic aërolites, and especially in Juvenas. There is much of a colourless mineral as well as of olivine, of every tint from pale yellow to yellowish brown; and the opake white mineral above alluded to is abundant. There is also a small amount of iron, and of what seems to be troïlite. The crust is an *enamel* with fine lustre. The transparent colourless mineral may possibly be a felspar (anorthite?), but it requires further scrutiny.

XVII. *On the Effects of Nocturnal Radiation within the Tropics.* *By* Professor Marcet, *of the Academy of Geneva**.

It is now a recognized fact, that about the period of sunset and during a great part of the night, provided the sky be clear and the atmosphere calm, the temperature of the stratum of air in immediate contact with the earth is lower by 2° or 3° Cent. than the temperature of the atmosphere at an altitude of a few feet. The observations relative to the cold produced by nocturnal radiation, on which the above data are founded, have hitherto been confined to the temperate climates of Europe; they have never, to my knowledge, been repeated in the warmer atmosphere of the torrid zone. I therefore thought it expedient to take advantage of a residence of my son during several months in Australia, district of Peak Downs, Queensland (latitude 22° south), to obtain from him a series of observations on the effects of nocturnal radiation similar to those which I had myself made at different periods in the neighbourhood of Geneva†, and which have more recently been repeated by Professor Martens at Montpellier‡. With this view I sent him, with the necessary instructions, a couple of carefully graduated Centigrade thermometers, in which the tenth part of a degree could be easily distinguished. My impression, I own, was that the phenomenon of a nocturnal increase of temperature at a few feet above the ground would, in all probability, be more apparent in a very hot country situated within the tropics than in the more temperate climates of Europe, both on account of the greater transparency of the atmosphere and because, as the surface of the earth receives during the day a far greater quantity of caloric, it appeared natural to infer that the nocturnal radiation would be proportionably greater. It was therefore with considerable surprise that, on examining the observations made by my son at his station on Peak Downs in March and April 1862, I found my conjecture to be completely erroneous. According to these observations, the increase of temperature of the air between 5 and 6 feet above the ground, compared to that of the stratum at about an inch and a half above the surface of the earth, appears to be so slight as to be frequently barely appreciable, generally not exceeding from 0°·1 to 0°·2 Cent., and only in one instance reaching 0°·4. And this was the case under circumstances reckoned most favourable to nocturnal radiation, the nights being generally splendid, and the sky almost always perfectly clear.

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* Communicated by the Author.

† See *Mémoires* of the Société de Physique et d'Histoire Naturelle of Geneva, vol. viii. 1839; and *Bibliothèque Universelle* for November 1861.

‡ See *Mémoires* of the Académie des Sciences of Montpellier, vol. v.

This apparent anomaly, if correctly stated, as I have every reason to believe, can hardly be attributed to a decrease in the quantity of radiant heat with which the earth parts at the moment of sunset; for the amount of nocturnal radiation being mainly dependent on the quantity of caloric that the earth has acquired during the day, must obviously, *cæteris paribus*, be greater in the torrid zone than in the more temperate regions of Europe. It may, however, I think, be explained by taking into account the following circumstances. First, the heat of the sun's rays within the tropics is probably sufficiently intense to warm not only the surface of the earth, but to penetrate to a certain depth below the surface. The consequence is, that after sunset, when the earth begins to cool rapidly by nocturnal radiation, the heat that has penetrated into the interior is gradually brought back to the surface, and thus contributes to prevent the cooling of the stratum of air in immediate contact with it. The second circumstance, and probably that which tends most to prevent nocturnal radiation producing the same effects as with us, depends, I think, on the greater quantity of water which must exist in the atmosphere, under the form of elastic vapour, in a country where the mean temperature is so much higher than it is in Europe. Professor Tyndall's recent experiments have shown to what extent aqueous vapour is capable of intercepting the passage of radiant heat, since he has calculated that even in England, in ordinary clear weather, the tenth part of the heat radiated by the earth into space is arrested at a distance of less than ten feet from its surface. In the tropical regions, particularly those which are at no very great distance from the sea*, the quantity of aqueous vapour which the atmosphere can take up and maintain in the elastic form must be far more considerable, and consequently the decrease in the earth's radiation rendered still more apparent.

The following fact tends to corroborate, to a certain extent, the results my son obtained in Queensland. My friend M. Lucien De la Rive undertook last year, at my request, to make a few observations on the effects of the cold produced by nocturnal radiation in the plains of Egypt bordering on the Nile. Unluckily circumstances prevented their being made with regularity, the weather not always being favourable, and M. De la Rive's stay in Egypt having been shorter than he had intended. But he made a sufficient number to be able to inform me that the differences of temperature he observed about sunset, between the stratum of air in contact with the earth and that of the atmo-

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* My son's station in the neighbourhood of Peak Downs was not much above a hundred miles in a direct line from the sea, and in the neighbourhood of Isaac's River.

sphere a few feet above it, were decidedly less considerable than what had been ascertained by M. Martens and myself to be the case in France and Switzerland under similar circumstances. I was, at the time I received this communication, totally unaware of the still more striking differences my son has since found to exist in Queensland.

It may perhaps be alleged that the explanation I have ventured to offer can hardly be reconciled with the striking and often dangerous contrast travellers in the great African deserts have observed between the oppressive heat of the day and the sudden cold which occurs immediately after sunset. The objection, I think, may be answered as follows:—In the vast deserts of Africa, the almost complete absence of water of any kind tends to maintain the atmosphere in a state of comparative absolute dryness; for although it is true that the quantity of aqueous vapour the atmosphere can take up depends mainly on its temperature, still water must be present; and if there be none, or hardly any, the air, however warm, can no longer take up a sufficient quantity of vapour to intercept any considerable proportion of the heat radiated by the earth. Under these circumstances, there is no longer any reason why the cold arising from nocturnal radiation should not attain its *maximum*, producing a striking and sometimes dangerous contrast in the temperature of the surrounding atmosphere the moment the sun disappears beneath the horizon.

XVIII. *Experiments on the Adhesion of Liquids to Mercury.*

By G. Gore, *Esq.**

If a drop of Nordhausen sulphuric acid, about one-tenth of an inch in diameter, is carefully placed by means of a glass rod upon the centre of a clean globule of pure mercury about 80 grains in weight, it instantly diffuses itself in a thin film over the surface of the metal, and the mercury becomes flattened and exhibits vortical movements all over its surface: but if the experiment is made with a strong aqueous solution of ammonia or of caustic potash, no such results occur; the alkaline solution contracts itself into a spherical form and persistently floats to the side of the mercury without spreading itself over the surface, especially if the mercurial globule weighs less than 60 grains.

To ascertain the order in which various liquids stood with regard to this particular behaviour with mercury, a number of experiments were made. Pure mercury, contained in a clean

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* Communicated by the Author.

watch-glass and of a uniform weight of 80 grains, was employed; a small drop of liquid, as nearly as possible of uniform size, was very carefully placed upon it, and the degree of rapidity with which it spread and the diameter it attained repeatedly noted. Uniformity of size of the drop was important. Liquids which produced a film upon the mercury, such as bromine, or dissolved the mercury, such as nitric acid, were not employed. The following order of substances with mercury at 60° F. was obtained, the substance first named being the one that spread most readily, and those braced together being about equal. The experiment was repeated several times with allied liquids, to correctly determine their relative positions.

Nordhausen sulphuric acid.
Saturated solution of mononitrate of mercury, slightly bibasic.
Essential oil of lemon.
Essential oil of turpentine.
Saturated solution of persulphate of iron.
1 ounce of teroxide of antimony dissolved in 6 ounces by measure of hydrochloric acid.
Ether.
Pure sulphuric acid.
Chloroform.
Concentrated solution of phosphoric acid.
Olive oil.
Solution of chloride of zinc, spec. grav. 1·81.
Alcohol.
Glacial acetic acid.
Saturated solution of carbazotic acid.
Saturated solution of sulphate of copper.
Benzole.
Anhydrous terchloride of arsenic, containing no free chlorine.
Spirit of wine.
Strongest aqueous solution of ammonia.
Saturated solution of carbonate of potash.
Strong solution of hydrate of potash.

The results indicate that, with regard to this phenomenon, acids are at one extreme and alkalies at the other, water and solutions of neutral salts being intermediate. The difference between the behaviour of the substances at the two extremes of the Table is very considerable.

XIX. *On the Conservation of Energy.*
By P. G. Tait, *M.A. &c.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

I REQUEST the favour of your inserting this additional communication. It is rendered necessary by the course which Prof. Tyndall has followed, and by my not having, like Prof. Thomson, declined to take further part in personal controversy with him. But henceforth I must do so, as the pages of the Philosophical Magazine have been already far too much occupied with personal matters, totally irrelevant to the question originally at issue—such as Prof. Tyndall's unwarrantable and *utterly erroneous* assumption that what he pleases to consider an elaborate attack on himself (in 'Good Words') was due to Prof. Thomson, and his insinuation of "reasons too long to state."

That question I understood to be the respective claims of Joule and Mayer, and to that I have kept closely. I am happy to find that Prof. Tyndall (by silence) intimates a tardy acquiescence in the statements which Prof. Thomson and I have made, and which I shall condense here.

(*a*) That Newton enunciated, in a complete form, the Conservation of Energy in abstract dynamics.

(*b*) That Davy, having experimentally proved that heat is motion and that its laws of communication are the same as those of the communication of motion, extended Newton's principle to one class of molecular motions.

(*c*) That Carnot supplied, among other valuable developments, that most important branch of the theory, Reversible Cycles in Thermo-dynamics, from which has been evolved the grand consideration of the dissipation of energy; but that some of his results require modification, as he assumed the materiality of heat.

(*d*) That Séguin in 1839, and Mayer in 1842, from unwarrantable assumptions combined with Davy's discovery, attempted to find the mechanical equivalent of heat, but obtained very erroneous, although coincident, results.

(*e*) That Joule experimentally proved that their *method* should have given an approximately accurate result, and supplied, from his own researches, the requisite data.

(*f*) But that *before* Joule did so, he gave in 1843 the almost exact value 770 foot-pounds. [To Prof. Tyndall's remarks on this point I shall presently advert.]

(*g*) That, in 1840 and subsequent years, Joule made tremendous strides in the experimental proof of the generality of the Conservation of Energy, and that it is by these experiments that the principle has been finally established. [On this point I may remark that Joule's early papers contain stores of most important truths, and constitute a whole province of Thermo-dynamics, as yet known to very few. Some of the simplest of the results which they contain have gained great credit for many able naturalists who have recently rediscovered them.]

(*h*) That Joule was the first (in print) to apply the principle to organic processes and to celestial dynamics. [I may mention here that Prof. J. Thomson remarked the effect of tidal friction on the earth's rotation as early as 1840, and stated it to the President of the Mathematical and Physical Section of the British Association on the occasion of its meeting in Glasgow in that year; and that his brother (Prof. W. Thomson) has taught it, both privately and publicly, in the Universities of Cambridge and Glasgow for at least twenty years.]

In our article in 'Good Words' there are a few trifling slips, easily explained by our absence from books when it was written; but none of those that we are aware of at all impair the accuracy of the whole. One of these has been commented on by Prof. Tyndall. It consists in saying "series of experiments" instead of "experiment," in describing Joule's important numerical result of 1843. The former expression *happens to be correct in fact*, but the latter would more justly describe Joule's *published* account which was not before us when we wrote.

Having mentioned no latitude, or locality, we gave the number 772 (instead of 770 for the latitude of Manchester, or 772·5 for equatorial gravity), and we said that this agreed, within $\frac{1}{500}$th of difference, with the more precise result of Joule's later researches. Our effort to explain the principle to an unscientific reader would not have been promoted by our entering on such perplexing minutiæ.

That we referred to Joule's experiments with the perforated piston, in giving the date of 1843, must be evident to every candid reader of the article. I am, Gentlemen,

&c. &c.,

6 Greenhill Gardens, Edinburgh,
July 21, 1863.

P. Guthrie Tait.

XX. *On the Dynamical Theory of Heat.*
By James P. Joule, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

Douglas, Isle of Man,
July 16, 1863.

Gentlemen,

I AM unwilling to do anything that might seem to prolong a controversy which must already have well nigh exhausted the patience of your readers, but I must trouble you with a few words in reference to M. Mayer's letter to Dr. Tyndall, inserted in your last Number, in which he says that he regarded the question of priority as to the mechanical equivalent of heat as exhausted by his communication to the Academy of Sciences in Paris, inasmuch as I did not reply to him. That I did not so reply to M. Mayer was not because I was unable to do so, and is still less to be attributed to any disrespect for the celebrated German physicist, but was simply because his communication, being a statement of opinion contrary to mine, did not appear to call for a rejoinder, which would probably have led to an unprofitable controversy. I have found no reason to alter my own view, as stated in my article in the *Comptes Rendus* for 1849, part 1. p. 132, to which M. Mayer's communication was a reply, which view is stated as follows at p. 134:—"Avant mes expériences, il n'y avait aucuns faits sur lesquels on pût avec certitude baser la conclusion que la chaleur spécifique d'un gaz est la même dans ses divers états de densité. Au contraire, l'opinion générale, conformément aux expériences de MM. De la Rive et Marcet, était que la chaleur spécifique d'un gaz varie avec la pression à laquelle il est soumis; d'où il découle que la conclusion non appuyée de M. Mayer, qui n'est pas en concordance avec les faits connus à cette époque, n'avait pas dû appeler l'attention des savants."

In M. Mayer's reply he remarks—"M. Gay-Lussac a démontré qu'un gaz coulant d'un ballon dans un autre vide et d'une capacité égale, se refroidit dans le premier autant qu'il se chauffe dans le second. Il s'ensuit évidemment de cette expérience que la chaleur spécifique d'un gaz n'est pas altérée par la raréfaction."

I have not been able to consult the first volume of the *Mémoires d'Arcueil*, which contains the description of Gay-Lussac's experiment; but it is certain that the result of it, as stated by M. Mayer, was not received or remembered by the scientific world, so that M. Regnault and myself were considered by others, as we considered ourselves, to be original inquirers on this subject. Moreover, it may be gathered from M. Mayer's papers that he knew nothing of this experiment of Gay-Lussac when he wrote his celebrated memoir of 1842. This, however, so far from detracting from the merit of M. Mayer, must, on the contrary, increase our admiration of his wonderful prescience. But at the same time this merit (and Dr. Tyndall agrees with me in this point) cannot in the smallest degree interfere with the merit of those who either before M. Mayer, or after him, pursued the course of strict logical deduction from facts ascertained by others and by themselves—the method by which alone the fabric of science is securely built.

Permit me to seize the present occasion to express my warmest thanks to Professors Thomson and Tait for the ability and friendly zeal with which they have asserted my claims, and allow me to reciprocate M. Mayer's remark, by thanking Dr. Tyndall for his lecture in the Royal Institution which called forth those expressions in my behalf.

I remain, Gentlemen,
Yours very respectfully,
James P. Joule.

XXI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 73.]

November 20, 1862.—Major-General Sabine, President, in the Chair.

THE following communications were read:—

"On the Synthesis of Tribasic Acids."—Preliminary Notice. By Maxwell Simpson, M.B., F.R.S.

"On the Nerves of the Liver, Biliary Ducts, and Gall-bladder." By Robert Lee, M.D., F.R.S.

"On the Volumes of Pedal Surfaces." By T. A. Hirst, F.R.S.

"On the Causes of various Phenomena of Attraction and Adhesion, as exhibited in Solid Bodies, Films, Vesicles, Liquid Globules, and Blood-Corpuscles." By Richard Norris, Esq., Birmingham.

"On Stasis of the Blood, and Exudation." By Richard Norris, Esq.

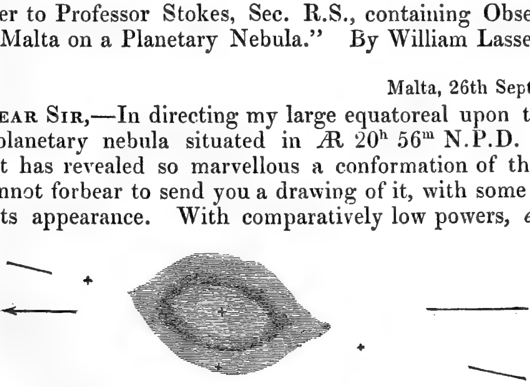
"Additional Observations on the Proximate Principles of the Lichens." By John Stenhouse, LL.D., F.R.S.

"On the Theory of Parallels." By Lieut.-General T. Perronet Thompson, F.R.S.

"Letter to Professor Stokes, Sec. R.S., containing Observations made at Malta on a Planetary Nebula." By William Lassell, Esq., F.R.S.

Malta, 26th Sept. 1862.

My dear Sir,—In directing my large equatoreal upon the well-known planetary nebula situated in Æ 20^h 56^m N.P.D. 101° 56′ (1862), it has revealed so marvellous a conformation of this object that I cannot forbear to send you a drawing of it, with some description of its appearance. With comparatively low powers, *e. g.* 231



and 285, it appears at first sight as a vividly light-blue elliptic nebula, with a slight prolongation of the nebula, or a very faint star, at or near the ends of the transverse axis. In this aspect the nebula resembles in form the planet Saturn when the ring is seen nearly edgewise. Attentively viewing it with higher powers, magnifying respectively 760, 1060, and 1480 times, and under the most favourable circumstances which have presented themselves, I have discovered within the nebula a brilliant elliptic ring, extremely well defined, and apparently having no connexion with the surrounding nebula, which indeed has the appearance of a gaseous or gauze-like envelope, scarcely interfering with the sharpness of the ring, and only diminishing somewhat its brightness. This nebulous envelope extends a little further from the ends of the conjugate than from the ends of the transverse axis; indeed, it is but very faintly prolonged, and only just traceable towards the preceding and following stars. There is a star near its border, northwards, in the projection of the conjugate axis.

The breadth or thickness of the ring is, unlike that of Saturn, nearly uniform, or equal in every part, so that its form most probably is either really elliptic, and seen by us in a line nearly perpendicular to its plane; or, if really circular, and seen foreshortened, a section throughout any part of it, limited by the internal and external diameters, must be a circle. In other words, it will be like a circular cylinder bent round. It could scarcely fail to bring to my mind the annular nebula in Lyra, especially as there is a conspicuous central star (proportionally, however, much brighter than that which is in the centre of that nebula), and yet the resemblance is only rudely in form, for this ring is much more symmetrical and more sharply defined, suggesting the idea of a solid galaxy of brilliant stars.

The ring is not perfectly uniform in brightness, the south-preceding part being slightly the most vivid. The transverse axis is inclined to the parallel of declination about 13°. A series of micrometrical measures of the length and breadth of the ellipse gives a mean of $26''\cdot2$ for the transverse, and $16''\cdot6$ for the conjugate axis.

The accompanying drawing has not been at all corrected by these measures, but is the result of several sketches made during different observations, and is a faithful transcript of the appearance of the nebula to my eye, when most favourably seen.

The object is, as may be supposed, one of extreme difficulty, requiring in the highest degree the combination of light and definition in the telescope, and a favourable state of atmosphere,—which will further appear when I state that it was not until I was favoured with an unusually fine night, and had applied a power of 1480, that the whole of the details were brought out.

I confess I have been greatly impressed by the revelation of this most wonderful object, situated on what perhaps we may consider as the very confines of the accessible or recognizable part of the universe, affording ground for the inference that more gorgeous systems exist beyond our view than any we have become acquainted with.

I am, &c., William Lassell.

"On the Fossil Remains of a long-tailed Bird (*Archeopteryx macrurus*, Ow.) from the Lithographic Slate of Solenhofen." By Prof. Richard Owen, F.R.S.

November 27.—Major-General Sabine, President, in the Chair.

The following communications were read:—

"Dynamical Problems regarding Elastic Spheroidal Shells and Spheroids of Incompressible Liquid." By Professor William Thomson, F.R.S.

"On the Exact Form and Motion of Waves at and near the Surface of Deep Water." By Professor W. J. Macquorn Rankine, C.E., F.R.S., &c. See Phil. Mag. vol. xxiv. p. 420.

"On the Tides of the Arctic Seas.—Part II. The Semidiurnal Tides of Port Leopold, North Somerset." By the Rev. Samuel Haughton, M.A., F.R.S., &c.

The first part of the author's researches on the Tides of the Arctic Seas was forwarded to the Royal Society in November 1861, and contained the discussion of the Diurnal Tides of Port Leopold. In the present communication the Semidiurnal Tides of the same port are discussed, and the following results obtained. The eccentricity of the moon's orbit is calculated from the parallactic inequality, and found to be 0·5303.

The solitidal interval is 56^m.

The lunitidal interval 4^h 54^m.

The ratio of the solar to the lunar coefficient is found to be 0·3956.

The mass of the moon $\frac{1}{71\cdot 11}$.

And the depth of the Atlantic is calculated from received tidal theories. The most probable results are found to be,—

From semidiurnal tidal intervals . . 3·529 miles.
From diurnal coefficients . . . . 3·690 ,,

There are other values of the depth of the sea, much greater than these, which follow from other considerations of the tidal theory; and the author is unable to explain why theory should give results so different. The preceding, however, he believes to be most in accordance with facts.

"On the Action of Chloride of Iodine on Iodide of Ethylene and Propylene Gas."—Second Notice. By Maxwell Simpson, M.B., F.R.S.

"On certain Developable Surfaces." By A. Cayley, Esq.

December 11.—Major-General Sabine, President, in the Chair.

The following communications were read:—

"Observations on several Mineral Substances, including their Analysis, &c." By Dr. T. L. Phipson, F.C.S.

"On the Strains in the Interior of Beams." By George Biddell Airy, F.R.S., Astronomer Royal.

The author states that he had long desired to possess a theory which should enable him to compute numerically the strains on every point in the interior of a beam or girder, but that no memoirs or treatises had given him the least assistance. He had therefore constructed a theory which solves completely the problems for which he wanted it, and which appears to admit of application at least to all ordinary cases.

The theory contemplates forces acting in one plane. A beam, therefore, is considered as a lamina in a vertical plane,—the same considerations applying to every vertical lamina of which a beam may be conceived to be composed.

The author remarks that it is unnecessary to recognize every possible strain in a beam. Metallic masses are usually in a state of strain from circumstances occurring in their formation; but such strains are not the subject of the present investigation, which is intended to ascertain only those strains which are created by the weight of the beam and its loads. The algebraical interpretation of this remark is, that it is not necessary to retain general solutions of the equations which will result from the investigation, but only such solutions as will satisfy the equations.

After defining the unit of force as the weight of a square unit of the lamina, and the measure of compression-thrust or extension-pull as the length of the ribbon of lamina whose breadth is the length of the line which is subject to the transverse action of the compression or tension, and whose weight is equal to that compression or tension, the author considers the effect of tension, &c., estimated in a direction inclined to the real direction of the tension, and shows that it is proportional to the square of the cosine of inclination. He then considers the effect of compounding any number of strains of compression or tension which may act simultaneously on the same part of a lamina, and shows that their compound effect may, in every case, be replaced by the compound effect of two forces at right angles to each other, the two forces being both compressions, or both tensions, or one compression and one tension. Succeeding investigations are therefore limited to two such forces.

Proceeding then to the general theory of beams, it is remarked that if a curve be imagined, dividing a beam into any two parts, the further part of the beam (as estimated from the origin of coordinates) may be considered to be sustained by the forces which act in various directions across that curve, taken in combination with the weight of the further part of the beam, the load upon that part, the reaction of supports, &c. Expressing the forces in conformity with the principles already explained, the three equations of equilibrium are formed, in which are involved several integrals depending on the form of the curve and on the forces. As the same equations must apply to any adjacent curve, the author remarks that this is a proper case for application of the Calculus of Variations; and on making that application, a remarkable relation is found to exist among the three functions depending on the forces acting at one point, from which it is immediately inferred that their algebraical expressions are the partial differential coefficients (of the second order) of a single function of the coordinates of the point of action. On substituting the partial differential coefficients, the integrations can be immediately performed; and the three equations assume a form of great simplicity, from which the sign of integration has entirely disappeared.

A form is then assumed for the principal function, with indeterminate coefficients, and it is shown that some of the constants may be eliminated by means of the three equations. But in the actual applications it is necessary to determine some remaining constants by considerations peculiar to each case. Now there is one modification of the strains whose value can be ascertained by ordinary mechanics, namely, the horizontal part of compressive force in the part of the beam above the neutral line, and the horizontal part of tension force in the part of the beam below the neutral line. (These words apply to a beam supported at both ends; in the case of a beam projecting from a wall, the words "compression" and "tension" must be reversed.) By determining the corresponding expression on the theory of this memoir, and comparing the two, the remaining constants and the form of the function are completely determined. From its partial differential coefficients are found the three functions depending on the forces acting at any one point (as already mentioned), and from these three functions are found the magnitudes of the two principal forces of compression or tension, and the angle which one of them makes with y, in a form admitting of numerical calculation.

The author then applies the theory to six cases, namely, (1) a beam projecting from a wall; (2) a beam supported at its ends; (3) a beam supported at its ends and carrying a load on its center; (4) a beam supported at its ends and carrying an excentric load; (5) a beam strained at both ends by the connexion of other beams, in the manner of the tubes of the Britannia Bridge; (6) a beam strained at one end only. Cases (3) and (4) require the use of discontinuous functions. Tables are given, exhibiting the numerical magnitudes of the two principal forces and the angle made by one of them with y, for 121 points in case (1), and for 231 points in each of the other cases. By means of these numbers, diagrams are formed, exhibiting in each case the directions of the lines of compression-thrust and tension-pull in every part of the beam.

"Photochemical Researches.—Part. V. On the Measurement of the Chemical Action of Direct and Diffuse Sunlight." By R. W. Bunsen, For. M.R.S., and H. E. Roscoe, B.A.

In one of the four communications which the authors have already had the honour of presenting to the Royal Society on the subject of the measurement of the chemical action of light, the attempt was made to determine experimentally the laws regulating the distribution of the chemical action of the sunlight and diffuse daylight on the surface of the earth when the sky is perfectly unclouded and the atmosphere clear. The methods of measurement there employed do not, unfortunately, apply to the much more usually occurring case of cloudy skies and hazy atmosphere. The aim of the present communication is to describe an entirely different mode of measuring the chemical action effected at any point on the earth's surface by the total sunlight and diffuse daylight, under the most widely varying conditions of situation, climate, and state of the atmosphere.

In spite of the various futile attempts which have been made to register and measure the chemical action of light by means of photographic tints, it still appeared possible in this way to attain the desired end. No instruments founded on such a mode of measurement can yield reliable results unless we know the conditions under which photographic surfaces of a constant degree of sensitiveness can be obtained, and unless the relations be determined which exist between the degree of tint produced, and the time and intensity of the light acting to effect such a tint.

The first point which the authors examine, is whether the photographic tints produced vary in shade in the direct ratio of the intensities of the acting light. Several experiments proved that no direct ratio between the degree of blackening and the intensities of the light exists. Hence it is necessary to relinquish the idea of employing any mode of measurement founded on the comparison of photographic tints of different shades. The next point examined is whether equal shades of blackness always correspond to equal products of the intensities of the acting light into the times of insolation. For the purpose of testing the truth of this proposition, an instrument is employed by which photographic sensitized paper can be exposed for times which can be exactly measured to within small fractions of a second. This instrument consists essentially of a pendulum vibrating about $\frac{3}{4}$ seconds, by whose oscillation a sheet of darkened mica is withdrawn from, and brought back over, a horizontal strip of paper prepared with chloride of silver, and fixed in a constant position relative to the pendulum and sheet of mica. The time during which each point in the length of the strip is exposed is different, and the time of insolation for each point can be calculated when the length and position of the strip, and the duration and amplitude of the pendulum's vibration are given. A Table exhibits for each millimetre in length of the strip, as measured by a scale attached, the time of exposure in seconds which the corresponding point of the strip undergoes in one vibration of the pendulum. These numbers require to be multiplied by n if the paper has been insolated for n vibrations.

The paper insolated whilst the pendulum is oscillating, exhibits throughout its length a regularly diminishing shade from dark to white; and the time of insolation of any point is found by reference to the Table. If we wish to determine which of these shades corresponds to another tint produced by a separate insolation, we cannot make the comparison by daylight or ordinary lamp-light, as these lights produce considerable changes of tint in the sensitive paper. The two shades may, however, be perfectly and safely compared by the light of a bright soda-flame; this light possesses the great advantage of being chemically inactive, and likewise of rendering imperceptible those slight differences of colour which make the comparison of two shades by the ordinary light so difficult.

In order to compare any other photographic tint with the point of equal shade on a strip, the latter, together with its millimetre scale, is attached to a board, in a darkened room. The board slides in a groove, so that it can be moved horizontally; and in front of the paper strip a small block holds in a fixed position a small piece of the tinted paper which it is desired to compare. On throwing the light of a bright soda-flame upon both surfaces it is easy, by moving the board from side to side, to find the exact point at which the shade of the strip is identical with that of the other tinted paper. It is then only necessary to consult the Table in order to find the time in seconds during which the paper must have been exposed in order that it should attain the tint in question. A series of lights of known intensities was obtained, by allowing the sun to shine through holes of known size. The images thus formed fell on to a piece of prepared paper; and the tints produced were compared with a strip darkened in the pendulum-apparatus, and thus the time of exposure necessary to effect the shade determined. Experiments made with intensities varying from 1 to 50, show that within these limits equal shades of blackness correspond to equal products of the intensities of the acting light into the times of exposure; so that the light 1 acting for the time 50, produced the same degree of blackening as the light 50 acting for the time 1.

A method for measuring the chemical action of light by simple observations is then founded upon this proposition. Thus, if we assume as the unit of photochemical action that intensity of light which produces in the unit of time a given degree of shade, we have only to determine, on a strip of paper tinted in the pendulum-apparatus, the point where the shade of the strip coincides with the given tint; the reciprocals of the times which correspond to these points of equal shade give the intensities of the light expressed in terms of the above unit.

This method of measurement is available only—

1. If the phenomena of photochemical induction do not interfere with the blackening of the paper.
2. If a photographic surface of a constant degree of sensitiveness can be prepared.
3. If an unchangeable tint can be obtained which can be exactly compared with the photographic paper.

The result of a series of experiments made by varying the number of the vibrations and calculating the intensity from each observation, showed that photochemical induction does not exert any prejudicial effect upon the measurements.

The question into which the authors enter at greatest length as being the most important for determining the exactitude of the measurements, relates to the mode of preparing a standard paper possessing a constant degree of sensitiveness. The relative degree of sensitiveness is determined by exposing the papers to one and the same light for the same length of time, and then comparing their tints with the shades of a strip prepared in the pendulum-apparatus, fixed in a solution of hyposulphite of soda, and furnished with an arbitrary scale. The influence of the strength of the nitrate-of-silver solution upon the sensitiveness is first examined; a series of experiments shows that with the same homogeneously salted paper, the sensitiveness of the film does not alter when the strength of the silver solution varies from 8 to 10 or 12 parts of nitrate of silver to 100 of water. Further examination showed that the time during which the paper lies upon the surface of the silver bath may vary from 15 seconds to 8 minutes, without any difference in the sensitiveness of the paper being noticed; and no difference is found by the employment of silver solutions which had been long in use and those freshly prepared. The papers thus silvered may be preserved for from 12 to 15 hours in the dark without undergoing any change in their sensitiveness.

If the paper be allowed to float on the surface of the solution of chloride of sodium as on that of the silver bath, the sheet after silvering exhibits, on drying, a very unequal degree of sensitiveness in its various parts. If, on the contrary, the sheet be well soaked in the salt-bath no such irregularity appears, and the sheet is of an equal degree of sensitiveness throughout its whole surface. This fact is determined by several extended series of experiments. The effect of change of concentration of the salt-bath upon the sensitiveness of the film is very great; and, as far as the observations extend, no limit exists beyond which an increase or a diminution of the percentage of salt in solution ceases to affect the sensitiveness of the film. Hence, in order to obtain constant results it is necessary to employ a solution of chloride of sodium of constant strength. By using solutions of the same strength, papers of a constant degree of sensitiveness are obtained.

The influence of the thickness of the paper employed is next, examined. Experiment shows that differences in the thickness of white paper, such as is usually employed for photographic purposes, is without influence upon the sensitiveness of the film of chloride of silver.

The changes in atmospheric temperature, from 3° C. to 50° C., and in atmospheric moisture are likewise found not to influence the sensitiveness of the prepared paper.

From the experimental results detailed in the communication, it appears that by adhering to a certain mode of preparation, a standard paper can be obtained, which at all times possesses a degree of sensitiveness sufficiently constant for the purposes of exact measurement. In the following extract from a larger Table, the readings are given which were made from papers prepared in three different salt solutions of the strengths mentioned, and silvered in a solution containing 12 of nitrate of silver to 100 of water. Equality in the numbers in each of the columns III. and IV. denotes equality in the readings and in the tint, and therefore equality in the sensitiveness of the prepared surfaces. Three sheets of paper were dipped into each solution. These numbers likewise show the great degree of accuracy with which tints can thus be compared.

I. Paper.	II. Na Cl to 100 parts of water.	III. Intensity No. 1.	IV. Intensity No. 2.
Upper part of sheet No. 2 ...	3·026	87·0	75·4
Middle part of sheet No. 3 ...	2 950	86·3	74·4
Middle part of sheet No. 2 ...	3·028	86·0	74·9
Lower part of sheet No. 2 ...	3·000	85·9	74·4

The next subject considered is the preparation of an unvarying tint which can be easily obtained and used as the standard of comparison. This is effected by grinding together 1000 parts of pure oxide of zinc with 1 part of pure lamp-black. A series of experiments showed that a colour can thus be prepared which possesses a constant and unalterable shade; and this can be used as a measure of the standard tint.

Having proved that a standard photographic paper of constant sensitiveness, and a standard tint of unvarying shade can be prepared, it is only necessary to apply the proposition that equal products of the intensities of the light into the times of insolation effect equal shades of blackness, in order to found a method of comparative measurement of the chemical action of the total daylight. As the *unit* of measurement, the authors propose to adopt that intensity of the light which in one second produces the standard tint of blackness upon the standard paper.

When the standard paper is insolated in the pendulum-apparatus, a strip is obtained which is tinted with every gradation of shade from dark to white. If the point on this strip is determined which coincides in shade with a paper covered with the standard tint, we have only to look into the Table to obtain the time of insolation (t), in seconds, which is necessary to produce the shade corresponding to the reading on the millimetre scale. If this time of insolation were found to be one second, the intensity of the light then acting would be $I=1$; for any other time the intensity of the chemical rays would be $\frac{1}{t}$.

As an example of such measurement, the authors append three series of observations, giving the total amount of chemically active rays falling on a horizontal surface at Manchester in summer and winter, made at intervals of 10 minutes throughout three separate days. These observations are likewise graphically represented as curves, which show maxima and minima exactly corresponding to the appearance and disappearance of the sun; and from them some idea may be formed of the vast differences which occur in the intensity of the chemical rays falling on the earth's surface during the longest and the shortest days.

In conclusion the authors state that it is possible, by using the pendulum-apparatus, to construct a portable instrument by means of which a large number of observations can be made upon a few square inches of paper. They reserve the description of their instrument for a future occasion.

"Notes of Researches on the Poly-Ammonias.—No. XXI. On Paraniline." By A. W. Hofmann, LL.D., F.R.S.

GEOLOGICAL SOCIETY.

[Continued from vol. xxv. p. 555.]

April 22, 1863.—Sir P. de M. G. Egerton, Bart., Vice-President, in the Chair.

The following communications were read:—

1. "On the Gneiss and other Azoic Rocks, and on the superjacent Palæozoic Formations of Bavaria and Bohemia." By Sir R. I. Murchison, K.C.B., F.R.S., F.G.S. &c.

The recent important discovery by M. Gümbel of a representative, near Hof, of the Primordial Zone of the Silurian Basin of

Prague, induced the author to obtain some further particulars regarding the older rocks of Bavaria and Bohemia; and a large part of this communication referred to the sketch map and section supplied to him by that gentleman.

One of Sir Roderick's principal objects in exploring this region was to convince himself, if possible, of the existence of a fundamental gneiss of as high antiquity as the Laurentian rocks of Canada and Scotland, and of the truth of M. Gümbel's view of the distinction of the gneiss into older and younger; but, after a survey of the whole district, he was unable to distinguish any order of superposition between its two members, the so-called younger gneiss, in one extensive tract near the Danube, dipping, according to M. Gümbel, under the older; and their variable strike rendered it as difficult to judge by that character as by their dip, its direction in some places being at right angles to what it is in others. For the present, therefore, though with the greatest respect for the labours of M. Gümbel, he considers the gneiss-rocks to constitute one great series, it being certain that the two varieties are not separated by any different intervening sediment, as in the N.W. of Scotland.

A clear illustration of the whole ascending succession is afforded by the order of superposition exhibited in M. Gümbel's section from Hof to Selb, a distance of about seven or eight miles. Gneiss is there seen resting against granite, and passing up into mica-slate underlying concretionary, quartzose, chloritic masses, which form the base of the Urthonschiefer. This primary clay-slate is followed by quartzites and black roofing-slate, in the latter of which the fossils of the Silurian Primordial Zone of Barrande occur, and ultimately by other Silurian, Devonian, and Lower Carboniferous strata in conformable succession, the latter passing conformably upwards into Mountain Limestone, which is shown to be quite unconformable to the Upper Carboniferous of Germany. In the remaining north-west portion of the section the strata are repeated in inverted succession, having been dislocated by the intrusion of igneous rocks.

Sir Roderick next adverted to the question of the parallelism of the Silurian Rocks of Bohemia with those of Britain, pointing out that the Austrian Geological Survey, whose new map he exhibited, had adopted, for this occasion only, the colours used by the Geological Survey of Great Britain; and he stated his belief that too close a parallelism between the subformations had been attempted, and that the parallelism of such large groups only as Lower and Upper Silurian, as proposed by Barrande, with a possible interpolation of "Middle Silurian," could be maintained.

The author then contrasted the absence of Devonian and Lower Carboniferous Rocks, coupled with the full development of Lower and Upper Silurian life, in Bohemia with the fuller and unbroken succession in Bavaria. He concluded by observing that the conformable succession of strata in Bavaria and other tracts shows the existence of beds which bridge over the gaps, represented by unconformities, that occur in the British series; and pointedly adverted to the two facts, that the enormous thickness of clay-slate beneath the Primordial Zone, though unaltered over large areas, had afforded no vestiges of life, and that the transitional groups of strata uniting two great systems had not afforded in any country a link connecting one class of animals with another.

2. "Notice of a Section at Moktree." By R. Lightbody, Esq.

The lower portion of this section was stated to exhibit the Aymestry limestone with its characteristic honeycomb structure, and showing at its upper limit a basin-shaped depression containing beds of sandy clay deposited parallel to its sides, and unconformable to the Aymestry limestone, though at a little distance from the trough this unconformity disappears. These argillaceous beds contain Lower Ludlow fossils, though they overlie the Aymestry limestone; the author consequently proposes that the latter term should no longer be used as a separate subdivision. He also remarked that, in consequence of a fault bringing the Upper and Lower Ludlow beds against one another, without having altered their dip, many fossils have been stated to occur in the Upper Ludlow which really belong to the Lower.

May 6.—Leonard Horner, Esq., Vice-President, in the Chair.

The following communications were read:—

1. "On the Brick-pit at Lexden, near Colchester." By the Rev. Osmond Fisher, M.A., F.G.S.; with a Note on the *Coleoptera,* by T. V. Wollaston, Esq., F.L.S.

Lexden is a village about a mile west of Colchester, and is situated on a plateau on the south side of the Valley of the Colne. The brick pit shows this tableland to consist of thick beds of gravel and sand, resting upon the London Clay, and containing at its southern extremity a talus of old gravel. This stratified gravel is overlain by brick-earth and soil, and is believed by the author to be that which elsewhere underlies the Boulder-clay; and he states that between it and the brick-earth there is, in one locality, a layer of peat containing bones of *Elephas primigenius,* and the remains of many insects; the latter are considered by Mr. Wollaston to differ from British recent species, and to indicate a warmer climate than now obtains in the district.

2. "On the original nature and subsequent alteration of Mica-schist." By H. C. Sorby, Esq., F.R.S., F.G.S.

When ripples are formed whilst material is being deposited, there is a structure generated which the author has, in former papers, termed "ripple-drift," and which he now described. This structure he stated might frequently be seen in polished sections of clay-slates, and also, in a form modified through metamorphism, in many mica-schists. From a consideration of the facts revealed by an examination of those rocks, he concluded that mica-schist is of sedimentary origin, metamorphosed after deposition, and sometimes after the production of cleavage and other physical changes, and that the bands of different minerals represent the planes of original deposition.

3. "On the Fossil Corals of the West Indies."—Part I. By P. Martin Duncan, M.B. Lond., F.G.S.

The aucity of information concerning the Geology and Palæontology of the West Indies, and the deficiency of carefully described species of recent corals, were stated to have involved this subject in great obscurity. Dr. Duncan, however, remarked that the paper by Dr. Nugent, published more than forty years ago, showed the existence in Antigua of three consecutive Coral-formations, called by him (in ascending order)—1, the inclined strata; 2, the Chert; 3, the Marl.

After describing in detail the seventy species and varieties of Fossil Corals from the West Indian Islands which he had been able to determine, Dr. Duncan exhibited in the form of tables the relation which this fossil fauna bears to the existing fauna of the Caribbean Sea, and to that of the Pacific, South Sea, and Indian Ocean, showing that it is more nearly related to the latter than to the former. He also showed that it bears a closer relation to the European Miocene coral-fauna than to the recent West Indian; and he therefore considered it to be most probably of Miocene age. The author concluded by describing what he believed to be the chief features of the physical geography of the Miocene Period, substituting a series of Archipelagos for the Atlantis of Professor Heer, and stating that the Pacific Ocean must have been at that period in immediate connexion with the Caribbean Sea.

XXII. *Intelligence and Miscellaneous Articles.*

ON AN APPARATUS FOR THE STATICAL MEASURE OF GRAVITY.

To the Editors of the Philosophical Magazine and Journal.

Observatory, Trevandrum,
May 21, 1863.

Gentlemen,

I SHALL feel obliged if you will publish in your Magazine the accompanying translation of a note which I have addressed to M. Elie de Beaumont, Perpetual Secretary of the French Academy of Sciences.

I am, Gentlemen,
Your obedient Servant,
John Allan Broun.

Ghats of Travancore, India,
April 20, 1863.

Sir,—I have just read a description by M. Babinet of an apparatus for the statical measure of gravity, read before the Academy of Sciences on the 9th of February, 1863. This apparatus was also devised by me several years ago, and it was constructed at my request in 1861 by Mr. Adie, optician, 395 Strand, London.

Ten years ago I employed a method of determining the coefficient of the bifilar-magnetometer scale, in which small weights were added to or taken from the weight of the suspended magnet*. This method induced me to think that the variation of the lunar attraction might have some share in the result found for the effect of the moon upon the horizontal magnetic force. A short calculation proved

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* See 'Report on the Observatories of H. H. the Rajah of Travancore' (1857), a copy of which was presented to the Academy.

that the variation of the attraction should not be visible on the bifilar magnetometer. But it was evident also that an arrangement might be made sufficiently sensible to show this variation; and I tried in 1858–59, by means of native workmen, to make an apparatus in which the magnetic force would be replaced by a constant torsion-force.

This trial did not succeed, on account of defects of workmanship, and I was obliged to wait till my return to Europe.

In June 1860 I communicated the principle of the apparatus to the optician already named; afterwards I sent him the necessary drawings, and I believe the instrument was made in the first months of 1861. We found, however, that the spring (like the balance-spring of a watch) employed under the weight acted badly, and I substituted a simple gold wire in the same year.

I ought to add to this, that in the end of 1860 I sent a description of my instrument to be communicated to the Royal Society of Edinburgh; that the instrument was seen in 1861 by several persons in Mr. Adie's workshop; that I mentioned the apparatus and its principle to different persons in Paris; and I owe to M. Henri Robert, the well-known watchmaker, the gold wire which he recommended to me as the best for the objects of my instrument.

I ought, before ending, to notice a difference in M. Babinet's and my method of observing.

My apparatus consists of a cubic weight suspended by two gold wires; three sides of the cube have mirrors attached, the planes of the two on opposite sides making each an angle of 93 degrees with the plane of the third; a single gold wire is hung from the centre of the lower side of the cube. (I have also noticed the plan of fixing the wire higher, given by M. Babinet.) This wire supports a lever by the middle, which lever can be turned by the hollow cylinder in which the lever hangs without being fixed. On the exterior of the cylinder there is another mirror. The instrument is adjusted in such a way that a revolution of the cylinder (and of the lower wire) will produce a movement of the weight of about 90 degrees; the number of degrees less than 90 depends upon the degree of accuracy desired, and of which the instrument is capable.

It is evident that if W be the weight suspended and ΔW any variation produced by a change of gravity, then

$$\frac{\Delta W}{W}=\text{cotan}\, v\Delta v\,;$$

where v is the angle of rotation of the weight at a given station or time, and Δv the variation of this angle at any other station or time, *the force of torsion being supposed constant.*

Thus, if we make v near enough to 90 degrees, we may perceive an exceedingly small variation of W.

A small telescope furnished with a prism of glass behind the wire in the eyepiece shows accurately the coincidence of the images reflected from the mirrors and of the wire seen directly by the eyepiece (according to the ingenious arrangement in Dr. Lamont's instruments); and two glass scales, one above the other, are employed to determine the variations of the normal angles or of the torsion of the wires.

I expect to be able by means of this instrument to determine the height of mountains more accurately than by any other plan (the trigonometrical excepted). I hope to be able to publish soon the results of my experiments upon this and upon the other questions relating to the variations of gravity.

I beg you, Sir, to do me the honour of communicating this letter to the Academy of Sciences.

Receive, Sir, &c.,

M. Elie de Beaumont. JOHN ALLAN BROUN.

ELECTRIC INDUCTION IN THE PYRENEES. EXTRACT OF A LETTER FROM CHARLES PACKE, JUN.

"Gavarnie, June 28, 1863.

". . . . Three nights ago I passed two days and one night on the summit of the Pimené, 8200 feet. I went up and returned by myself heavily laden with rifle, barometers, telescope, and other instruments, but Laurent came up at nightfall with my sleeping-bag and provisions. At daybreak Laurent returned, as he is busy constructing a grange and écurie; but I remained till after midday, when a threatened thunder-storm sent me down. A most singular circumstance befel me, which I do not expect you to believe; indeed I should not believe any one who told me the same. On leaving the Pimené I had occasion to remount a second and somewhat lower peak of the crête, to repeat some experiments I had made in ascending. The road lay over a snow slope; and as I approached the top, I heard what appeared to be an excessive creaking of the straps at my back; at least, being heavily laden and feeling the weight at my back, this was my first thought. As I reached the top the noise increased, and on taking the rifle off, which was slung at my back, it made such a noise as if it was trying to go off. It was loaded, but I knew this was impossible; still I could not help keeping it pointed away. The noise was as if some large bee or beetle had got down the barrels and was trying to escape. I shook the rifle to shake it out; and directly the barrels were lowered to the ground the noise ceased, but was renewed directly I raised them. The air was much charged with thunder-cloud, and there had been two or three thunder-claps; and it then first occurred to me that the phenomenon might be connected with electricity. On raising the sympiesometer to look at it, it produced the same noise, as also did my pole, though neither of them so loudly as the rifle. The noise much resembled that given off by an electric conductor when a wire is approached, but not near enough to cause a discharge. I am convinced myself that this was the cause, and I wish I had made more experiments; but as soon as this occurred to me I did not care to prolong my proximity to the lightning, not knowing how near it might be, and I hastened down. As soon as I had descended a little, the noise ceased, and I could hold up my rifle with impunity. There was no wind, and scarcely any rain. The storm did not reach Gavarnie, but at Barèges I hear it was very violent."

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

[FOURTH SERIES.]

SEPTEMBER 1863.

XXIII. *On the Conducting-Power of Copper and Iron for Heat at different Temperatures.* By J. A. Ångström*.

§ 1. IN a paper sent to the Royal Academy on January 9, 1861, I described a new method of estimating the conducting-power of metals for heat, by which method that important constant is obtained in absolute values (or in values easily reducible into absolute measure), whereas the old methods in use give only relative values. In order still better to establish that the method is practicable, and also to obtain a first estimation of the alteration of conducting-power brought about by changes of temperature, I have continued the experiments upon copper and iron which I described in my first paper, having moreover varied them by employing bars of much greater dimensions. The question, whether or not the conducting-power of metals be a function of the temperature, seemed to me to have a not unimportant bearing, not alone on the theory of heat, but also on a branch of science intimately connected therewith, viz. electricity. This question has been variously handled by former experimenters. Some of them, as Despretz, suppose that the value of k (the conducting-power) is independent of the temperature, and that the departure from the geometrical law shown in the passage of heat through bars of lead, iron, or other bad conductors of heat, is chiefly owing to the thickness of the bar, and the consequent difference of temperature between the different points in one and the same transverse section (a supposition which is in opposition to the foundation of Fourier's theory); whilst others, as Langberg, Wiedemann, and Franz, assume that

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* Translated from Poggendorff's *Annalen*, vol. cxviii. part 3, by Prof. Wanklyn.

k is constant for good conductors of heat, such as copper, but variable with the temperature in the case of the other metals.

Indeed, the question cannot be answered in the way taken by the above-named experimenters, viz. by a determination of the temperature at different points in a bar which is heated at one end to a constant temperature. For, in the first place, the observed departures from the geometrical law may be due to an altered value of the radiating power of the surface as well as to an alteration in the conducting-power of the bar. And in the second place, in the case of the better conductors of heat, the method is too little sensitive to indicate with accuracy slight differences in conducting-power. We see this from the last experiments of Despretz, wherein he took bars of a bad conductor (marble), and gave to them so great a diameter that the heat could only very slowly pass away along the bar, and found the departure from the geometrical law to be insensible, at least when the temperature of the bar did not exceed that of the room by more than 30° or 40° C.

Lastly, it is also clear that if the proportionality between the conducting-power for heat and that for electricity found by Wiedemann and Franz is to have any theoretical importance, and not be a merely accidental coincidence, it will not suffice for the metals to deport themselves similarly in regard to their conducting-powers for heat and electricity, but they must do this at all temperatures; that is, the coefficient of conducting-power for one and the same metal must be the same for heat and for electricity. Since, up to the present time, no body has been found which does not vary in conducting-power for electricity with alteration of temperature, it follows that this must also be the case with regard to the corresponding relation to heat.

Having made these remarks, I go on to the experiments themselves.

§ 2. The bars which were used were 1178 millims. long and 35 millims. thick. The distances between the holes for the reception of the thermometer amounted to 200 millims.; and the diameter of each hole was 2 millims. in the experiments with the copper bar and in the first experiments with the iron bar. The holes in the iron bar were afterwards enlarged in order to permit of the use of more sensitive thermometers. The duration of the periods in the experiments with the copper bar was, with one exception, twenty-four minutes, and that in the experiments with the iron bar thirty-two minutes.

The apparatus for raising and lowering the temperature was altered, so that cold water and vapour of water were not alternately made to surround the bar in one and the same bath, but for each of these agents a special bath was provided. Inasmuch as these baths admitted of changes of position about the bar and alterations of distance from one another, it was possible thereby to heat the bar to different mean temperatures, and so to arrive at its conducting-power at different temperatures. In the part of the apparatus for generating vapour, this alteration was made: the vapour, after passing through the cooling apparatus, was brought back to the vessel for boiling; and so the generation of steam could continue for any length of time without a fresh supply of water.

In the reduction of the observations, I have followed the same method as in my former paper. In order to eliminate from the periodical variations the error which is introduced by the variation in the mean temperature of the bar, and which may be called the secular variation, I have combined in the calculation

$$\frac{1+1'}{2} \text{ with } 12, \quad \frac{2+2'}{2} \text{ with } 13 \text{ \&c.,}$$

$$1' \text{ with } \frac{12+12'}{2}, \quad 2' \text{ with } \frac{13+13'}{2} \text{ \&c.,}$$

where the observations taken in periods consisting of twenty-four divisions are denoted by

(1. Period): 1, 2, 3, ... 12, 13, 14, ... 24.
(2. Period): 1′, 2′, 3′, ... 12′, 13′, 14′, ... 24′.

The necessity of this was particularly manifest in the case of the iron bar, which, after the lapse of three hours occupied in the experiments, had not acquired a constant value for its mean temperature; moreover, in experiments which last several hours, the mean temperature must always vary somewhat on account of the alteration in the temperature of the room, a circumstance which cannot be avoided.

The thirty-two observations making up the periods for the iron bar have not been taken together, but have been divided into two series of sixteen each. Out of these two series, values have been calculated, and the mean of these calculated values has been ultimately taken.

With three exceptions, the calculated values in the following Table are the results of a double series of observations in which the thermometers had their positions changed, whereby the error in the value of the scale due to the unequal sensitiveness of the thermometer was eliminated. Inasmuch as each double series of this kind consisted of at least four full periods, it follows that the values deduced in the case of the periods divided into twenty-four divisions must be the results of at least 192 readings, and the values deduced in the case of the iron bar must be the results of 256 readings.

In all the experiments one thermometer was read by M. Thalén, and the other by myself.

Copper Bar, Thermometers No. II. and No. III.

No.	Period.	$\frac{k}{c\delta}$.	Temp.	Remarks.
1	24′	62·19	71°·5	Single series. Heating by means of a gas-flame.
2	24	63·61	55·0	„ „ vapour of water.
3	24	63·64	56·6	Double series. „ „ „
4	16	65·77	41·0	„ „ „ „
5	24	65·95	42·4	„ „ „ „
6	24	66·07	32·2	„ „ „ „
7	24	66·45	28·8	„ „ „ „

Iron Bar.

No.	Period.	$\frac{k}{c\delta}$.	Temp.	Remarks.
1	32	10·487	43·4	Single series. Thermometers Nos. II. and III.
2	32	10·429	43·7	Double series. Thermometers A and B.
3	32	10·279	18·9	

If the value for the copper bar of

$$\frac{k}{c\delta}$$

be calculated by the method of the least squares (it being assumed that all the observations are of equal authority), the following equation will be obtained,

$$\frac{k}{c\delta}=69{\cdot}77(1-0{\cdot}001519\ t).$$

If the observed values of $\frac{k}{c\delta}$ be calculated according to this formula, they will be found as follows:—

No.	Temperature, t.	Calculated, $\frac{k}{c\delta}$.	Difference between observation and calculation.
1	71°·5	62·192	−0·002
2	55·0	63·941	−0·331
3	56·6	63·771	−0·131
4	41·0	65·424	+0·346
5	42·4	65·276	+0·674
6	32·2	66·357	−0·287
7	28·8	66·717	−0·267
			$\Sigma\epsilon^2=0{\cdot}8544$

The probable error in the value of the constant 69·77, or the conducting-power at 0°, is thence obtained,

$$=\pm 0{\cdot}1093.$$

In like manner the probable error in the value 15·19, or the percentage alteration in conducting-power for 100° C., equals

$$\pm 0{\cdot}3392.$$

If, for the calculation of $\frac{k}{c\delta}$, the formula

$$\frac{k}{c\delta}=a(1-\alpha t+\beta t^2)$$

be taken, then $\Sigma\epsilon^2$ would certainly be diminished; but since the observations fall within too narrow limits to admit of the obtaining of β with sufficient accuracy, I regard it as unnecessary to give this calculation.

The alterations of temperature which are expressed by δc should be made to enter into α, the value of the coefficient of temperature which was obtained above. But inasmuch as these differences, so far as is known, are very slight, at any rate within the range of temperatures of the foregoing observations, and inasmuch as for a rise of temperature δ diminishes and c increases, it is evident that they must have very little influence upon the value of the coefficient of temperature.

Let

$$c\delta=0{\cdot}84476,$$

then

$$k=58{\cdot}94\ (1-0{\cdot}001519\,t).$$

Calculating the value of $\frac{k}{c\delta}$ from the two last determinations for iron given in the Table, which are fully comparable with one another, there is obtained

$$\frac{k}{c\delta}=13{\cdot}458\ (1-0{\cdot}002874\,t).$$

Putting

$$c\delta=0{\cdot}8862,$$

it follows that

$$k=11{\cdot}927\ (1-0{\cdot}002874\,t).$$

The first of the observations for iron given in the Table, and which was not used in the obtaining of the formula, may serve for a further control of the correctness of observations; and the more so as it was made with other thermometers, and the holes for the reception of the thermometers were 2 millims. in diameter, while in the last experiments they were 3 millims. in diameter.

For comparison I have reduced the observations given in the former paper, and taken upon the bars of copper used in former experiments, and get

$$\frac{k}{c\delta}=72{\cdot}96\,(1-0{\cdot}00214\,t),$$

whence

$$k=61{\cdot}63\,(1-0{\cdot}00214\,t).$$

The inequality in the value both of a and of α in these determinations may be satisfactorily explained by assuming a difference in the chemical nature of the bars used in the two sets of experiments. And here it should be remarked that the great copper bar used in the experiments described in this paper was obtained last year direct from Avesta, and consisted of Fahlun copper; whilst the smaller copper bars taken for the former investigations were procured many years ago here in Upsala, and whence they came I am unable to say. From analyses made many years ago, it is moreover known that Fahlun copper contains iron.

§ 3. The values of the coefficients for the heat-conducting power of copper and iron deduced from the foregoing observations are smaller than the coefficients of those metals for electricity have been usually found. Since, however, the latest researches of Matthiessen* upon copper prove that this coefficient varies considerably (from 8 to 42 per cent. for 100° C. according to the purity and peculiar character of the metal), it is impossible to say at present whether or not the coefficients which have been obtained hold for electricity. This question can only be answered by a direct determination of the electrical conducting-power of both metals. In order to be rigidly accurate, it is further necessary to make these determinations with bars in precisely the same state as they were in when a determination of the heat-coefficient was made, since the act of drawing out the metal into wire might easily bring about an alteration in the conducting-power. In satisfying these conditions, a difficulty is encountered which is by no means small, and which relates to the thickness of the bars. A preliminary experiment in which the large copper and iron bars formed parts of a so-called Wheatstone's balance gave no certain result, inasmuch as the least alteration in the contact between the bars and the other parts of the balance exercised a great influence upon the result. I hope, however, to overcome these difficulties.

§ 4. This investigation has shown

1. The practicability and sufficient accuracy of the method,

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* See Matthiessen's paper in the Philosophical Magazine, Sept. 1861. The quotation above is not quite correct. The 8 and 42 refer to the variation in electrical resistance and not to conducting-power.—J. A. W.

even when it is required to estimate the differences of conducting-power which are brought about by alterations of temperature.

2. That the conducting-power for good conductors, such as copper, undergoes variations which are capable of comparison with those which worse conductors, such as iron, present; and therefore

3. That the proportionality which has been assumed to exist between the conducting-powers of bodies for heat and for electricity is probably a fact.

XXIV. *On the Centre of Gravity of a Truncated Triangular Pyramid, and on the Principles of Barycentric Perspective.* By J. J. SYLVESTER, *F.R.S., Professor of Mathematics at the Royal Military Academy**.

THERE is a well-known geometrical construction for finding the centre of gravity of a plane quadrilateral, which may be described as follows.

Let the intersection of the two diagonals (say Q) be called the *cross-centre*; the intersection of the lines bisecting the middle points of pairs of opposite sides (say O) the *mid-centre* (which, it may be observed, is the centre of gravity of the four angles viewed as equal weights); then the centre of gravity is in the line joining these two centres produced past the latter (the mid-centre), and at a distance from it equal to one-third of the distance between the two centres; in a word, if G be the centre of gravity of the quadrilateral, QOG will be in a right line, and $OG=\frac{1}{3}QO$.

The frustum of a pyramid is the nearest analogue in space to a quadrilateral *in plano*, since the latter may be regarded as the frustum of a triangle. The analogy, however, is not perfect, inasmuch as a quadrilateral may be regarded as a frustum of either of two triangles, but the pyramid to which a given frustum belongs is determinate. Hence *à priori* reasonable doubts might have been entertained as to the possibility of extending to the pyramidal frustum the geometrical method of centering the plane quadrilateral. The investigation subjoined dispels this doubt, and will be found to lead to the perfect satisfaction, under a somewhat unexpected form, of the hoped for analogy.

Let abc, $\alpha\beta\gamma$ be the two triangular faces, $a\alpha$, $b\beta$, $c\gamma$ the edges of the quadrilateral faces of a pyramidal frustum. Then this frustum may be resolved in six different ways into the sum total of three pyramids, as shown in the annexed double triad of schemes.

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* Communicated by the Author.

$$\begin{array}{cccc} a\,b\,c\,\alpha & b\,c\,a\,\beta & c\,a\,b\,\gamma \\ b\,c\,\alpha\,\beta & c\,a\,\beta\,\gamma & a\,b\,\gamma\,\alpha \\ c\,\alpha\,\beta\,\gamma & a\,\beta\,\gamma\,\alpha & b\,\gamma\,\alpha\,\beta \end{array}$$

$$\begin{array}{cccc} b\,a\,c\,\beta & a\,c\,b\,\alpha & c\,b\,a\,\gamma \\ a\,c\,\beta\,\alpha & c\,b\,\alpha\,\gamma & b\,a\,\gamma\,\beta \\ c\,\beta\,\alpha\,\gamma & b\,\alpha\,\gamma\,\beta & a\,\gamma\,\beta\,\alpha \end{array}$$

If, then, taking any one of the above schemes we draw a plane through the centres* of the three pyramids of which it is composed, the six planes thus drawn will meet in a point, which will be the centre of the frustum†.

Let the point in which αa, βb, γc meet when produced be the origin of coordinates, and $bc\beta\gamma$, $ca\gamma\alpha$, $ab\alpha\beta$ be taken as the planes of x, y, z; and let $4a, 0, 0$; $0, 4b, 0$; $0, 0, 4c$ be the coordinates of a, b, c, and $4\alpha, 0, 0$; $0, 4\beta, 0$; $0, 0, 4\gamma$ those of α, β, γ. Consider the first of the schemes above written.

$a+\alpha$, b, c will be the coordinates of the centre of $abc\alpha$,
α, $b+\beta$, c „ „ „ $bc\alpha\beta$,
α, β, $c+\gamma$ „ „ „ $c\alpha\beta\gamma$;

because, as everyone knows, the centre of a pyramid is the same as that of its angles regarded as of equal weight. But again, if we define as the mid-centre the centre of the six angles of the frustum regarded as of equal weight, its coordinates will be

$$\frac{2a+2\alpha}{3}, \quad \frac{2b+2\beta}{3}, \quad \frac{2c+2\gamma}{c};$$

and if we substitute for each of the three centres last named points lying respectively in a right line with them and the mid-centre on the opposite side of the mid-centre and at distances from it double those of these centres themselves, these quasi-images of the centres in question will have for their coordinates

$$\begin{array}{ccc} 0\,, & 2\beta, & 2\gamma, \\ 2a, & 0\,, & 2\gamma, \\ 2a, & 2b, & 0\,. \end{array}$$

These points are accordingly the centres of the lines $\beta\gamma$, γa, ab respectively.

And a similar conclusion will apply to each of the six schemes. Hence using in general (p, q) to mean the middle of the line

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* I shall throughout in future for greater brevity hold myself at liberty to use the word *centre* to mean *centre of gravity*.

† I shall hereafter show that these six planes all touch the same cone, of which, as also of its polar reciprocal, I have succeeded in obtaining the equations.

p, q, and by the collocation of the symbols for three points understanding the plane passing through them, it is clear

1. That the six planes,

$$\beta, \gamma;\ \gamma, a;\ a, b;\quad \gamma, \alpha;\ \alpha, b;\ b, c;\quad \alpha, \beta;\ \beta, c;\ c, a;$$
$$\gamma, \beta;\ \beta, a;\ a, c;\quad \alpha, \gamma;\ \gamma, b;\ b, a;\quad \beta, \alpha;\ \alpha, c;\ c, \beta,$$

will meet *in a single point* which may be called the *cross-centre,* being the true analogue of the intersection of the two diagonals of a quadrilateral figure in the plane.

2. That if we join this cross-centre (say Q) with O the mid-centre, and produce QO to G making $OG=\frac{1}{2}QO$, G will be the centre of the frustum $abc\alpha\beta\gamma$.

It may be satisfactory to some of my readears to have a direct verification of the above.

Let, then,

$$A=\frac{a^2bc-\alpha^2\beta\gamma}{abc-\alpha\beta\gamma},\quad B=\frac{ab^2c-\alpha\beta^2\gamma}{abc-\alpha\beta\gamma},\quad C=\frac{abc^2-\alpha\beta\gamma^2}{abc-\alpha\beta\gamma}.$$

A moment's reflection will serve to show that A, B, C are the coordinates of the centre of the frustum.

Again, the first three of the six planes last referred to will be found to have for their equations respectively,

$$\beta\gamma x+\gamma a y+abz=2a\gamma(b+\beta),$$
$$bcx+\gamma\alpha y+\alpha bz=2b\alpha(c+\gamma),$$
$$\beta cx+cay+\alpha\beta z=2c\beta(a+\alpha).$$

The determinant

$$\begin{vmatrix} \beta\gamma & \gamma a & ab \\ bc & \gamma a & \alpha b \\ \beta c & ca & \alpha\beta \end{vmatrix}=(abc-\alpha\beta\gamma)^2.$$

The determinant

$$\begin{vmatrix} \gamma a & ab & 2a\gamma(b+\beta) \\ \gamma\alpha & \alpha b & 2ba(c+\gamma) \\ ca & \alpha\beta & 2c\beta(a+\alpha) \end{vmatrix}$$

$$=2\alpha a(bc-\beta\gamma)(abc-\alpha\beta\gamma),$$
$$=2\left\{(\alpha^2\beta\gamma-a^2bc)(abc-\alpha\beta\gamma)+(a+\alpha)(abc-\alpha\beta\gamma)^2\right\}.$$

Hence if x, y, z be the coordinates of the intersection of the above-mentioned three planes,

$$x=-2A+2(a+\alpha),$$
$$y=-2B+2(b+\beta),$$
$$z=-2C+2(c+\gamma);$$

and the same will evidently be true of the other ternary system of planes; so that all six planes intersect in a single point Q, of which x, y, z above written are the coordinates. And the coordinates of O being

$$\frac{2a+2\alpha}{3},\quad \frac{2b+2\beta}{3},\quad \frac{2c+2\gamma}{3},$$

and those of G being

$$A,\quad B,\quad C,$$

it is obvious QOG is a right line, and $OG=\frac{1}{2}OQ$, as was to be shown.

The analogy with the quadrilateral does not end here. There is a construction* for the centre of a quadrilateral still easier than that above cited, which may be expressed in general terms by aid of a simple definition. Agree to understand by the *opposite* to a point L on *a limited line* AB a point M, such that L and M are at equal distances from the *centre* of AB but on opposite sides of it; then we may affirm that the centre of a quadrilateral is the centre of the triangle whose apices are the intersection of its two diagonals (*i. e.* the cross-centre), and the *opposites* of that intersection on those two diagonals respectively. So now if we agree to understand by opposite points on a limited triangle two points in a line with the centre of the triangle and at equal distances from it on opposite sides, and bear in mind that the cross-centre of a pyramidal frustum is the intersection of *either* of two distinct ternary systems of triangles which may be called the two systems of cross-triangles†, we may affirm that the centre of a pyramidal frustum is the centre of a pyramid whose apices are its cross-centre, and the opposites of that centre on the three components of either of its systems of cross-planes. This is easily seen; for if we take the first of the two systems, their respective centres will evidently be

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* This is the mode of statement (except that the important notion of opposite points was not explicitly contained in it) which, accidentally meeting my eye in a proof sheet of some Geometrical Notes (by an anonymous author) intended for insertion in the forthcoming (if not forthcome) Number of the Quarterly Journal of Mathematics, led to the long train of reflections embodied in this paper, which but for that casual glance would never have seen the light. The same construction, under another and somewhat less eligible form, is given in the 'Mathematician' (a periodical now extinct, edited by Dr. Rutherford and Mr. Fenwick, both of the Royal Military Academy), 1847, vol. ii. p. 292, and is therein stated by the latter gentleman to have, "as he believes, first appeared in the 'Mechanics' Magazine,' and subsequently in the 'Lady's Diary' for 1830."

† From the description given previously, it will be seen that *a cross-triangle* of the frustum is one which has its apices at the centres of either diagonal of any quadrilateral face and of the two edges coterminous but not in the same face with that diagonal.

$$\frac{4a}{3}, \quad \frac{2b+2\beta}{3}, \quad \frac{4\gamma}{3},$$

$$\frac{4\alpha}{3}, \quad \frac{4b}{3}, \quad \frac{2c+2\gamma}{3},$$

$$\frac{2a+2\alpha}{3}, \quad \frac{4\beta}{3}, \quad \frac{4c}{3}.$$

Thus the three opposites to the cross-centre whose coordinates are $-2A+2(a+\alpha)$, $-2B+2(b+\beta)$, $-2C+2(c+\gamma)$, will have for their x coordinates

$$\frac{2a}{3}-2\alpha+2A,$$

$$-2a+\frac{2\alpha}{3}+2A,$$

$$-\frac{2a}{3}-\frac{2\alpha}{3}+2A;$$

for their y coordinates

$$\frac{2b}{3}-2\beta+2B,$$

$$-2b+\frac{2\beta}{3}+2B,$$

$$-\frac{2b}{3}-\frac{2\beta}{3}+2B;$$

and for their z coordinates

$$\frac{2c}{3}-2\gamma+2C,$$

$$-2c+\frac{2\gamma}{3}+2C,$$

$$-\frac{2c}{3}-\frac{2\gamma}{C}+2C;$$

and consequently the centre of the pyramid whose apices are the cross-centre and its three opposites will be A, B, C, *i. e.* will be the centre of gravity of the frustum, as was to be shown*.

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* I at one time supposed that a, b, c; α, β, γ formed two systems of *diagonal* planes, and that there were thus two cross-centres; and dreamed a dream of the construction for the centre of gravity of the pyramidal frustum based upon this analogy, inserted (it is true as a conjecture only) in the Quarterly Journal of Mathematics; but the nature of things is ever more wonderful than the imagination of men's minds, and her secrets may be won, but cannot be snatched from her. Who could have imagined *à priori* that for the purposes of this theory a diagonal of a quadrilateral was to be viewed as a line drawn through two opposite angles of the figure regarded,

It is clear that these results may be extended to space of the higher dimensions. Thus in the corresponding figure in space of four dimensions bounded by the hyperplanar quadrilaterals $abcd$, $\alpha\beta\gamma\delta$, which will admit of being divided into four hyperpyramids in twenty-four different ways, all corresponding to the type

$$\begin{array}{ccccc} a & b & c & d & \alpha \\ b & c & d & \alpha & \beta \\ c & d & \alpha & \beta & \gamma \\ d & \alpha & \beta & \gamma & \delta \end{array}$$

there will be a cross-centre given by the intersection of any four out of twenty-four hyperplanes resoluble into six sets of four each,—one such set of four being given in the scheme subjoined, where in general pqr means the point which is the centre of (p, q, r), and the collocation of four points means the hyperplane passing through them, viz.

$$\begin{array}{llll} \beta\gamma\delta; & \gamma\delta a; & \delta ab; & abc; \\ \gamma\delta\alpha; & \delta\alpha b; & \alpha bc; & bca; \\ \delta\alpha\beta; & \alpha\beta c; & \beta cd; & cdb; \\ \alpha\beta\gamma; & \beta\gamma d; & \gamma da; & dac. \end{array}$$

The mid-centre will mean the centre of the eight angles $a, b, c, d, \alpha, \beta, \gamma, \delta$, regarded as of equal weight; and to find the centre of the hyperpyramidal frustum, we may either produce the line joining the cross-centre with the mid-centre through the latter and

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not as themselves, but as their *own centres of gravity*! Some of my readers may remember a signal case of a similar autometamorphism which occurred to myself in an algebraical inquiry, in which I was enabled to construct the canonical form of a six-degreed binary quantic from an analogy based on the same for a four-degreed one, by considering the *square* of a certain function which occurs in the known form as consisting of two factors, one the function itself, the other a function morphologically derived from, but happening for that particular case to coincide with the function. This parallelism is rendered more striking from the fact of 4 and 6 being the *numbers* concerned in each system of analogies, those numbers referring to degrees in the one theory and to angular points in the other. It is far from improbable that they have their origin in some common principle, and that so in like manner the parallelism will be found to extend in general to any quantic of the degree $2n$, and the corresponding barycentric theory of the figure with $2n$ apices (n of them in one hyperplane and n in another), which is the problem of a hyper-pyramid in space of n dimensions. The probability of this being so is heightened by the fact of the barycentric theory admitting, as is hereafter shown, of a *descriptive* generalization, descriptive properties being (as is well known) in the closest connexion with the theory of invariants. Much remains to be done in fixing the canonic forms of the higher even-degreed quantics; and this part of their theory may hereafter be found to draw important suggestions from the hyper-geometry above referred to, if the supposed alliance have a foundation in fact.

measure off three-fifths of the distance of the joining line on the part produced (as in the preceding cases we measured off two-fourths and one-third of the analogous distance), or we may take the four opposites of the cross-centre on the four components of any one of the six systems of hyperplanar tetrahedrons of which it is the intersection, and find the centre of the hyperpyramid so formed. The point determined by either construction will be the centre of gravity of the hyperpyramidal frustum in question. And so on for space of any number of dimensions. It will of course be seen that a general theorem of determinants* is contained in the assertion that for space of n dimensions there will be $\pi(n)$ quasi-planes all intersecting in the same point, as also in the general relation connecting this point (the cross-centre) with the mid-centre and centre of gravity, of each of which it is easy to assign the value of the coordinates in the general case.

But returning to the case of the ordinary pyramidal frustum, the preceding results lead at once to an easy geometrical proof of the well-known analytical formula for finding the centre of gravity of a pyramidal frustum in the case where the base and its opposite plane are parallel.

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* We learn indirectly from this how to represent under the form of determinants of the ith order, and that in a certain number of different ways, the general expressions

$$(l_1 l_2 \ldots l_i - \lambda_1 \lambda_2 \ldots \lambda_i)^{i-1}$$

and

$$l_1 \lambda_1 (l_2 l_3 \ldots l_i - \lambda_2 \lambda_3 \ldots \lambda_i)(l_1 l_2 \ldots l_i - \lambda_1 \lambda_2 \ldots \lambda_i)^{i-2},$$

a strange conclusion to be able to draw incidentally from a hyper-theory of centre of gravity! Thus, *ex. gr.*, on taking $i=4$, we shall find

$$\begin{vmatrix} bcd & cd\alpha & d\alpha\beta & \alpha\beta\gamma \\ \beta\gamma\delta & cda & da\beta & a\beta\gamma \\ b\gamma\delta & \gamma\delta\alpha & dab & ab\gamma \\ bc\delta & c\delta\alpha & \delta\alpha\beta & abc \end{vmatrix} = (abcd - \alpha\beta\gamma\delta)^3.$$

And again,

$$\begin{vmatrix} \alpha d(bc + c\beta + \beta\gamma) & cd\alpha & d\alpha\beta & \alpha\beta\gamma \\ \beta a(cd + d\gamma + \gamma\delta) & cda & da\beta & a\beta\gamma \\ \gamma b(da + a\delta + \delta\alpha) & \gamma\delta\alpha & dab & ab\gamma \\ \delta c(ab + b\alpha + \alpha\beta) & cd\alpha & \delta\alpha\beta & abc \end{vmatrix} = a\alpha(bcd - \beta\gamma\delta)(abcd - \alpha\beta\gamma\delta)^2.$$

The number of these representations will not be twenty-four, *i. e.* $\pi(4)$, but only twelve, the half of that number, because it will easily be seen that the cycles $abcd$, $\alpha\beta\gamma\delta$ will lead to the same determinants, only differently arranged, as the cycles $bcda$, $\beta\gamma\delta\alpha$. I believe the law is, that the number of varieties of such representations is $\pi(i)$, or $\frac{1}{2}\pi(i)$, according as i is odd or even. The expression $ab - \alpha\beta$ at once conjures up the idea of a determinant. We now see that there is an equally natural determinantive representation, or system of representations, of $(abc - \alpha\beta\gamma)^2$, $(abcd - \alpha\beta\gamma\delta)^3$, &c.

As we know that the centre of gravity in this case is in the line joining the centres of the opposite faces, what is wanted here is merely the proportion of the segments into which this joining line is divided at the centre in question, or, in other words, the ratio to each other of the distances of the centre from the parallel faces.

Let

$$ab : \alpha\beta = bc : \beta\gamma = ca : \gamma\alpha = l : \lambda.$$

Then obviously

$$\text{vol. } abc\alpha : \text{vol. } bc\alpha\beta = ab\alpha : b\alpha\beta : l : \lambda,$$

$$\text{vol. } bc\alpha\beta : \text{vol. } c\alpha\beta = \gamma bc\alpha : c\alpha\gamma : l : \lambda :$$

hence

$$abc\alpha : bc\alpha\beta : c\alpha\beta\gamma = l^2 : l\lambda : \lambda^2 ;$$

also if h be the distance between abc, $\alpha\beta\gamma$, the distances of the centres of $abc\alpha$, $bc\alpha\beta$, $c\alpha\beta\gamma$ respectively from abc will be $\frac{h}{4}, \frac{h}{2}, \frac{3h}{4}$.

Hence the distance of the centre of the frustum from abc will be $\frac{h}{4}\left(\frac{l^2+2l\lambda+3\lambda^2}{l^2+l\lambda+\lambda^2}\right)$, and so from $\alpha\beta\gamma$ it will be $\frac{h}{4}\left(\frac{\lambda^2+2l\lambda+3l^2}{l^2+l\lambda+\lambda^2}\right)$, agreeing with the well-known formula applicable to this case*.

But I pass on to a subject of much deeper interest.

The geometrical constructions included in the preceding inquiry (such for instance as depend on the properties of *centres* and *opposites*), like those which occur in the more ordinary theory of the triangle and pyramid, at once suggest the existence of descriptive propositions in which harmonic centres and harmonic opposites, and in general harmonic multiplications and divisions, take the place of the corresponding arithmetical operations.

To make my meaning perfectly clear, let us conceive a fixed plane; and by a harmonic succession of points A, B, C, D ... in a line meeting the fixed plane† (which we may term the plane of

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[*] If we agree to denote by a, b, c; α, β, γ, the planes $a\beta\gamma$, $b\gamma\alpha$, $c\alpha\beta$; αbc, βca, γab respectively, it may easily be shown that each quaternary system of planes a, b, α, β; b, c, β, γ; c, a, γ, α passes through a single point, we have thus given three points which determine a plane; the intersection of this plane with the line a, b, c; α, β, γ is a sort of centre to the frustum, and must possess properties deserving closer investigation.

[†] It will of course be understood that in dealing with figures lying in the same plane, a line of relation (viz. the intersection of the plane of relation with the plane of the figures) may be substituted instead of the former plane, since the distances from the one and the other are in an invariable ratio; and so for different segments in a right line, we may substitute a point of relation on the line itself instead of the plane. I deal with a plane of relation as comprising implicitly all the subordinate cases; were it required to go out into space of four or a higher number of dimensions, it would of course become necessary to deal with hyper-planes of relation.

relation) in O, let us understand that ABCO, BCDO, &c. form so many harmonic systems of points; B may be then called a harmonic centre of AC, A and C opposites to B; also we may call AB, BC harmonic steps of the succession, so that by multiplying a line AB *n* times, or making AX equal to *n* times AB, we are constructing the point X to which A will be transferred by *n* harmonic steps, of which AB is the first; and by *n*-secting a line AX, we mean finding a point B in it such that a succession of *n* harmonic steps, commencing with AB, will carry A to X.

In all this there is of course nothing new: those principles are familiar to all geometers, and have received their fullest development at the hands of Professor Cayley. We know *à priori* that the descriptive properties included in the preceding (or similar) constructions, such, *e. g.*, as that the six cross-triangles of a frustum all meet in a point, will remain true when, adopting a fixed plane of relation, we substitute harmonic centres in respect to that plane in lieu of arithmetical centres*. Or, again, we may affirm that the lines joining the harmonic centres of the opposite edges of a tetrahedron will all *intersect* and *harmonically* bisect each other, and so on. But what is further wanted, and what I will proceed to supply, is a firm quantitative basis to this enlarged theory, so formed as that we shall be able in the general case to follow step by step the reasoning used in the common theory where the plane of relation goes off to infinity, and to assign to every point determined in the general constructions as distinctive a character as it possesses in the special ones. This may be done by the aid of very elementary considerations, which I proceed to unfold, and which will be seen at once to bring the general or perspective theory under the dominion of the so-called integral calculus or calculus of continuity.

The arithmetical centre of two *points* A, B is the centre of gravity of two equal atoms at A and B; let us then so assign the weights of the atoms A, B in the general case as to make their centre of gravity fall on the harmonic centre: this may evidently be done by considering their weights as proportional to their inverse distances from the plane of relation, and accordingly we shall understand by the weight of an atom at any point a quantity proportional to its inverse distance from the plane of relation. But, moreover, the centre of gravity of the homogeneous line AB ought to fall at this same point, which we may if we please consider as an inference at the limit from the same

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* Geometers have long been familiar with the idea of the *pole* or *harmonic centre* of a triangle in respect to a line in its plane; the principles now about to be developed will enable us to attach a precise signification to the *pole* or *harmonic centre* of every geometrical figure of any form whatever.

thing being true for equal atoms at distances dividing the line into any even number of equal parts. Hence in the general analogical theory we must take the infinitesimal intervals of our atoms at points in *harmonic* succession.

Let P, Q, R be any three such points, and let x, $x+dx$, $x+2dx+d^2x$ be their respective distances from the plane of relation; and let q be the *frequency* at P, *i. e.* a quantity proportional to the number of atoms which occur in a given infinitesimal space about P; then evidently qdx is constant, and $qd^2x+dxdq=0$; but by virtue of the harmonic relation between P, Q, R, we have

$$(x+2dx+d^2x)(dx)=x(dx+d^2x),$$

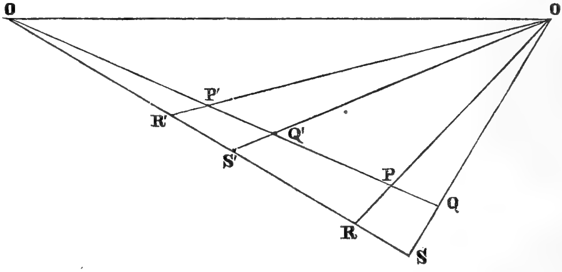
or

$$xd^2x=2(dx)^2, \text{ or } -\frac{dq}{q}=2\frac{dx}{x},$$

$$\text{i. e. } q \text{ varies as } \frac{1}{x^2}.$$

Moreover the weight of each atom varies as $\frac{1}{x}$, hence the *density* of any element in a line must be taken to vary as the inverse cube of its distance from the plane of relation.

Let us now endeavour to obtain the law of density for any element of a *plane*. Let O, O′ be any two points in the line in



which the plane in question meets the plane of relation, and let the plane be divided into infinitesimal elements similar to P Q S R in the figure by pencils whose rays are in harmonic succession proceeding from O and O′; then one atom belongs to every such element, which will be the analogue of a rectangular element in the common theory; but the area of this element, as compared with any similar element, say P′Q′S′R′ in the infinite sector QOS, varies as

$$OP.RS+OR.PQ;$$

where P Q, R S, by what has been last shown, vary as the square of the distance of the element from the plane of relation, and O P, O R vary directly as the distance; hence the *frequency* of the atoms at any element in *either sector* will vary as the inverse cube of its distance from the plane of relation, and hence this will be the law of frequency for elements all over the plane, and is irrespective of the particular positions of O, O′; and consequently, the density being proportional to the product of the frequency of the atoms by their atomic weights, the law of density is that it varies about any point as the inverse *fourth* power of its distance from the plane of relation. In like manner, by taking three points O, O′, O″ in the plane of relation and dividing space into solid elements by plane bundles passing through O O′, O O″, O′ O″ respectively, it may be proved that the law of density for a solid figure will be that it varies as the inverse *fifth* power of the distance from the plane of relation*.

Atoms whose weights vary inversely as their distances from the plane of relation may be termed *like* atoms; lines, areas, and solids whose elements vary in *density* inversely as the cubes, fourth powers and fifth powers respectively, may be termed *qualiform* figures, or figures of *qualiform* density, the terms *like* and *qualiform* being adopted as the closest analogues to *equal* and *uniform*. It now becomes true, and may easily be verified, that the centres of gravity of a *qualiform* finite line, triangle, and tetrahedron are respectively identical with the centres of gravity of *like* atoms placed at their apices †; and so every known or discoverable

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* The law of density for a solid is the inverse fifth power, for an area the inverse fourth power, and for a line the inverse third power. Here we must stop, for a *point* is that which has no parts: we can speak of the law of atomic *weights* at a point, but not of density, for the latter implies the existence of *elements* which are wanting to the point. In a hyper-ontological sense there would be no objection to saying that for an element of a point the law of density in this theory is as the inverse square, always remembering that no such element exists.

† As regards the finite line, these results may be very easily verified by the integral calculus. For the triangle, it may be made to depend in the preceding case by drawing from the point where the direction of any side intersects the plane of relation, rays dividing the triangle into infinitesimal portions; the centre of gravity of every one such portion will easily be seen to be in the right line joining the harmonic centre of the intersecting side with the opposite angle; and an analogous method applies to the tetrahedron.

The same results may also be obtained analytically. Thus, *ex. gr.*, for a qualiform triangle whose apices are distant h, k, l from the opposite sides, and $\frac{1}{\alpha}, \frac{1}{\beta}, \frac{1}{\gamma}$ from the plane of relation, the distances of the centre of gravity from the respective sides will be

$$\frac{h\alpha}{\alpha+\beta+\gamma}, \quad \frac{k\beta}{\alpha+\beta+\gamma}, \quad \frac{l\gamma}{\alpha+\beta+\gamma}.$$

The masses, say M, of a qualiform line, triangle, or tetrahedron, using α, β;

theorem whatever relating to the centre of gravity of uniform figures bounded by right lines or planes becomes immediately transferable to that of *qualiform* figures of the same kind. Thus, to take a most simple example, since the centre of gravity of a parallelogram is at the intersection of its diagonals, it must be and is true that the centre of gravity of a quadrilateral whose density at any point varies as the inverse fourth power at that point from the line joining the intersections of its two pairs of opposite sides, will also be at the intersection of the diagonals of that figure. I am informed by Prof. Cayley that a somewhat analogous consideration of altered density has been employed by our eminent friend Professor William Thomson in his theory of images, in reference to the distribution of electricity, given in Liouville's *Journal*.

It is an easy inference* from what has been established concerning the law of *frequency*, that if in the perspective of any plane figure by tinting or relief we express the degree of crowd-

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α, β, γ; $\alpha, \beta, \gamma, \delta$ for the inverse distances of the apices from the plane of relation, and V for the length, area, or volume, in the three cases respectively become expressible under the very noticeable forms

$$\frac{(\alpha+\beta)}{2}\alpha\beta V, \quad \frac{\alpha+\beta+\gamma}{3}\alpha\beta\gamma V, \quad \frac{\alpha+\beta+\gamma+\delta}{4}\alpha\beta\gamma\delta V,$$

their moments in respect to the plane of relation being respectively

$$\alpha\beta V, \quad \alpha\beta\gamma V, \quad \alpha\beta\gamma\delta V;$$

so that the *mean* density $\frac{M}{V}$ is in each case a simple symmetric function of the atomic weights of the apices (it being of course understood that the *absolute* atomic weight and frequency are each taken as unity). As the same figure may be variously partitioned, and the sum of the component areas and of their moments is unaffected by the mode of partition, the preceding formulæ obviously give rise to, or imply the existence of, a class of purely geometrical theorems relating to systems of points. It may be here observed that the moment of a qualiform figure in respect to its plane of relation represents the size, so to say, of (*i. e.* the number of atoms contained in) the single molecule which, placed at the centre of gravity, will be the statical equivalent of such figure; for if n be this number, and d the distance of the centre from the plane of the relation, and w the weight of the figure, since the atomic weight is $\frac{1}{d}$, we must have $\frac{n}{d}=w$, or

$$n=dw=\text{moment of } w \text{ in respect to the plane of relation.}$$

So in like manner, wherever the plane of relation is situated, two molecules A and B, placed at two *points*, will be equivalent to the molecule A+B placed at their centre of gravity.

* It may here also incidentally be noticed that the area of the primitive of any perspective projection of a figure in a given plane is proportional to the *attraction* exercised upon it by the object plane indefinitely extended, the force of attraction between any two elements being supposed to vary inversely as the fifth power of the distance.

ing of any element, and proportion the tint or elevation to the inverse *cube* of its distance from the vanishing line, then any portion of the picture will accurately represent (and indeed if we use relief, the *volume* or weight of such portion will be strictly proportional to) the area (or its weight) of the corresponding part in the object plane. Supposing different object planes to be represented in perspective on the same picture plane, with liberty for the position of the eye to vary, it may be shown without difficulty* that if the *absolute* intensity of tint or relief for any object plane varies as the square of the distance of its trace upon the picture plane from its vanishing line, and as the first power of the distance of the eye from the same line, the ratio between corresponding portions of object and picture will be alike for every plane.

In the corresponding problem for right lines, the relief or tint of any element in the perspective of a given right line must vary as the inverse square of the distance from the vanishing point, and the absolute intensity for different lines must vary as the product of the distance between the trace and the vanishing point into the distance of the eye from that point. In *barycentric* perspective we have seen the further consideration of atomic weight enters, so that the density follows the law of the inverse fourth and third powers for planes and lines respectively*, instead of third and second powers as in geometrical perspective; in fact in the geometrical theory the quantities visibly represented correspond to the *moments*† in respect to the vanish-

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* For if we take T the trace of an object line, V its vanishing point, and through O (the eye) draw OP*p* meeting TV in P and the object line in *p*, T*p* the quantity of $\mathrm{TP}=\frac{\mu\mathrm{TP}}{\mathrm{TV}\,.\,\mathrm{PV}}$, so that $\mu=\mathrm{TV}\frac{\mathrm{T}p}{\mathrm{TP}}\mathrm{PV}=\mathrm{TV}\,.\,\mathrm{OV}$; and again, if *t*T*t'* be the trace of an object plane, V the foot of the perpendicular from O on the vanishing line VT perpendicular to *t*T*t'*, P a point in VT, and *p* the point where OP meets the object plane, we have *tpt'* (the quantity of *t*P*t'*)$=\mu\frac{t\mathrm{P}t'}{\mathrm{TV}\,.\,\mathrm{TV}\,.\,\mathrm{PV}}$, or

$$\mu=\mathrm{TV}^2\,.\,\frac{tpt'}{t\mathrm{P}t'}\,.\,\mathrm{PV}=\mathrm{TV}^2\,.\,\frac{\mathrm{T}p}{\mathrm{TP}}\,.\,\mathrm{PV}=\mathrm{TV}^2\,.\,\mathrm{OV}.$$

The preceding calculations assume the expressions $\mu\alpha\beta$, $\mu\alpha\beta\gamma$ applicable to a linear and triangular space, given in a preceding foot-note.

† And consequently if, in the pictorial representation of any plane surface, there is taken a triangular patch of given area, the quantity in the object corresponding thereto will vary inversely as the product of the distances of the three angles of the patch from the vanishing line,—a proposition in perspective which I imagine to be new, and at all events is certainly little known. This may be applied to determine instantaneously the area of an ellipse of which the perspective projection is a circle of radius *r*, and whose centre is at the distance *h* from the vanishing line. Writing μ equal to the distance of the vanishing line from the eye, multiplied by the square of its distance from the trace of the ellipse upon the plane of the

ing line of the quantities visibly represented in the barycentric theory*.

I have termed this a theory of barycentric perspective, because it includes a method whereby the centre of gravity of a plane figure is retained in perspective with the centre of gravity of its projection; by what has preceded, it appears that this may be effected by regarding its projection, not as of uniform density, but of a density following the law of the inverse cube of the distance. From this it follows that the distance of the perspective position in the picture of the centre of gravity of the

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circle, the area of the ellipse (regarded as made up of infinitesimal sectors with the centre of the projection for their common vertex) becomes

$$\int_0^{2\pi} d\theta \frac{\frac{1}{2}\mu r^2}{h(h-r\sin\theta)^2} = \frac{\mu\pi r^2}{h^3\left(1-\left(\frac{r}{h}\right)^2\right)^{\frac{3}{2}}} = \frac{\mu\pi r^2}{(h^2-r^2)^{\frac{3}{2}}};$$

so that the area of any ellipse in a *given* plane, the perspective representation of which ellipse is a circle, will vary directly as the area of the circle, and inversely as the cube of the tangent drawn to meet it from the orthogonal projection of its centre on the vanishing line. More generally, if the figure in the plane of projection be an ellipse with semiaxes a, b, eccentricity e, inclination of minor axis to vanishing line α, and distance of one of its foci from that line h, then calling V the area of the primitive and μ the absolute ratio between a primitive element and its projection, we shall have

$$V=\frac{\mu}{2h}\int_0^{2\pi} d\theta \frac{r^2}{(h-r\sin\theta)^2}, \text{ where } r=\frac{a(1-e^2)}{1+e\sin\theta}.$$

This integration may be performed with extreme facility, and gives

$$V=\mu\pi ab\left(h^2+2hea\cos\alpha-a^2(1-e^2)\right)^{-\frac{3}{2}},$$

say $\quad \frac{\mu}{D^3}\pi ab,$

where to find D we may use the following construction:—*Draw a circle in the plane of, and concentric with, the projection, and such that a common tangent to the two shall be parallel to the vanishing line, and from the foot of the perpendicular upon that line from the centre draw a tangent to the circle, the length of the tangent so drawn will be* D; so that the area of any ellipse will be to the area of its perspective projection as the product of the square of the distance of the trace into that of the eye from the vanishing line is to the cube of the tangent just described,—a very remarkable proposition in perspective, if new. By varying the origin of our polar coordinates, as by taking it, for instance, at the centre of the projection or any other point, we may obtain a new class of definite integrals of known values, and which it might be exceedingly difficult to determine by any direct method. It may be added that all ellipses in the same plane will bear a constant ratio to their projections if those latter have a common tangent parallel to the vanishing line, and their centres be in another line also parallel to the same.

* The above statements, combined with the varying law of *frequency*, amount to the following propositions in perspective:—

1. If O be a *linear* element, P its perspective representation, H, h the

primitive from the vanishing line becomes immediately known by a process of differentiation when the area of the primitive is expressed as a function of the distance of any arbitrarily fixed point in the plane of projection from the vanishing line. For if this area, which is the moment of the qualiform projection in respect to the vanishing line, be called M, and the mass of the same be termed Q, and if h, d be the distances of the origin and of the centre of gravity from the vanishing line, we have $d=\frac{\mathrm{M}}{\mathrm{Q}}$, where

$$\mathrm{M}=\mu\int_0^{2\pi}\frac{r^2d\theta}{(h-r\sin\theta)^2h},$$

$$\mathrm{Q}=\frac{1}{3}\mu\int_0^{2\pi}\frac{r^2d\theta}{(h-r\sin\theta)^2h}\left(\frac{1}{h-r\sin\theta}+\frac{1}{h-r\sin\theta}+\frac{1}{h}\right);$$

hence

$$\mathrm{Q}=-\frac{1}{3}\frac{d\mathrm{M}}{dh},$$

and

$$d=\frac{\frac{1}{3}\mathrm{M}}{\frac{d\mathrm{M}}{dh}}.$$

Thus, *e. g.*, if we wish to find the perspective position of the centre of gravity of the primitive of a given elliptic projection we have found in a preceding foot-note,

$$\mathrm{M}=\mu(h^2+2hae\cos\alpha+a^2e^2-a^2)^{-\frac{3}{2}};$$

hence

$$d=\frac{h^2+2hae\cos\theta+a^2e^2-a^2}{h+ae\cos\alpha};$$

or, calling R the radius of the circle concentric with the given projection, and having with it a common tangent parallel to the vanishing line, and H the distance of the centre of this circle

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distances of the eye and P from the line of O, and d of the eye from the line of P, then

$$\mathrm{O}:\mathrm{P}::d\mathrm{H}:(\mathrm{H}-h)^2.$$

2. If O be a *plane* element, P its perspective, H, h the distances of the eye and P from the plane of O, and d the distance of the eye from the plane of P, then

$$\mathrm{O}:\mathrm{P}::d\mathrm{H}^2:(\mathrm{H}-h)^3.$$

These formulæ would become necessary in applying (as might be done perhaps advantageously) in some cases the integral calculus to the quantification of curved lines and surfaces by a perspective method more general than the one in ordinary use, which is essentially a method of orthogonal projection.

from that line, $d=\dfrac{H^2-R^2}{H}$, an equation the geometrical interpretation whereof is readily obtained.

More generally, if we take $x\cos\alpha+y\sin\alpha-h=0$ as the equation to the vanishing line, using, as before, M to denote the moment of the qualiform projection in respect to that line (well worthy in this theory of being termed the principal moment), or, which is the same thing, the area of the primitive, and take M_x for the moment of the same in respect to the axis of y, we shall have

$$M=\iint\frac{dx\,dy}{(x\cos\alpha+y\sin\alpha-h)^3},$$

$$M_x=\iint\frac{dx\,dy\,x}{(x\cos\alpha+y\sin\alpha-h)^4},$$

from which it is easy to deduce

$$M_x=\cos\alpha\left(M+\frac{1}{3}h\frac{d}{dh}M\right)+\frac{1}{3}\sin\alpha\frac{d}{d\alpha}M;$$

and consequently $\dfrac{M_x}{Q}-h\cos\alpha$, which is the distance of the perspective of the centre of gravity of the primitive in the direction of x from the foot of the perpendicular from the assumed origin upon the vanishing line, will be $\dfrac{3\cos\alpha.M+\sin\alpha\dfrac{dM}{d\alpha}}{\dfrac{dM}{dh}}$. And thus we are led to the remarkable proposition, that when we know the area of the primitive in terms of the parameters of its vanishing line, we can completely determine the perspective position of its centre of gravity by means of processes of differentiation only; so that a method closely akin to (if not identical with) that of potentials in the theory of attraction has a necessary place also in the theory of perspective.

If, as is most convenient, we fix the perspective of the centre of gravity of the object figure by its distance from the vanishing line and its distance from the line through the origin perpendicular to the vanishing line, we see, by making α successively zero and $\dfrac{\pi}{2}$ in the above formula, that these distances are

$$\frac{3M}{\dfrac{d}{dh}M}\quad\text{and}\quad\frac{\dfrac{d}{d\alpha}M}{\dfrac{d}{dh}M}\quad\text{respectively*.}$$

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* In the case of the ellipse, we have found in a preceding foot-note,

$$M=\mu(h^2+2aeh\cos\alpha+a^2e^2-a^2)^{\frac{3}{2}},$$

Analogous results may be obtained for solid figures, substituting the more general notion of homography for that of perspective, as will more fully appear in the sequel.

Remembering that M is the area of the primitive plane object, it seems to result as an indirect inference from the preceding theory, that whenever we can determine the area of an oval section (whether the bounding curve be the whole or a part of the curve of section) of an algebraical cone, then we can determine the position of the centre of gravity of that oval in its own plane by processes of differentiation only; and, *mutatis mutandis*, the same conclusion will admit of extension to solids bounded by algebraical surfaces; so that $\iint dx\,dy$ or $\iiint dx\,dy\,dz$ being given, subject to certain conditions of limit, $\iint (ax+by)\,dx\,dy$, $\iiint (ax+by+cz)\,dx\,dy\,dz$, subject to the same conditions, become known by algebraical and differentiation processes only, and so obviously for any number of variables*.

Cowley House, Oxford,
July 1863.

[To be continued.]

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so that

$$\frac{3\mathrm{M}}{\frac{d}{dh}\mathrm{M}}=\frac{h^2+2eah\cos\alpha+a^2e^2-a^2}{h+ea\cos\alpha}=y,$$

$$\frac{\frac{d\mathrm{M}}{d\alpha}}{\frac{d\mathrm{M}}{dh}}=\frac{ea\sin\alpha h}{h+ea\cos\alpha}=x,$$

where y and x are the coordinates of the point referred to in the text, if we take the vanishing line and a line perpendicular thereto from the focus for the axes of x and y. Consequently, if we remove the origin of coordinates to the centre of the ellipse, preserving the directions of the axes, and call x', y' the new coordinates, we shall have

$$x'=ae\sin\alpha-x=\frac{a^2e^2\sin\alpha\cos\alpha}{h+ae\cos\alpha},$$

$$y'=h+ae\cos\alpha-y=\frac{a^2\left(1-e^2(\sin\alpha)^2\right)}{h+ae\cos\alpha},$$

$$\frac{y'}{x'}=\frac{1-e^2(\sin\alpha)^2}{e^2\sin\alpha\cos\alpha},$$

which may easily be shown to be the equation to the diameter drawn to the point of the ellipse where the tangent is parallel to the vanishing line; and consequently the perspective of the centre of gravity of the original lies in this diameter, as evidently it ought to do, since every infinitesimal slice of the *qualiform* area contained between parallels to the vanishing line is of uniform density throughout, and is bisected by the diameter conjugate to the direction of that line.

* The inference made hesitatingly in the text, upon further reflection

XXV. *On the Artificial Production of Hail, and on a new Theory of this Meteoric Product.* *By* P. J. M. SANNA-SOLARO[*].

THE following is an extract from a memoir by Sanna-Solaro, read before the French Academy of Sciences in April last.

Meteorologists and natural philosophers concur without exception in the belief that hail is formed in the atmosphere by an act of successive incrustation. The hailstones have hitherto been considered to consist of a central nucleus around which the several component layers are deposited; and the prevalence of this hypothesis has on the one hand proved an obstacle to the discovery of the true origin of this meteor, and on the other leaves unexplained several phenomena which accompany it. For our part, we believe that hailstones are produced, from the first, such, very nearly, as they are when precipitated to the earth. Admitting that the congelation commences externally, it becomes easy to explain all the peculiarities met with in their centre, and all the other phenomena which these meteoric products exhibit.

The external envelope having been formed (how this takes place will be presently shown), that portion of liquid in contact with the crust begins to congeal, bubbles of air are disengaged from it and converge towards the centre. From this an amount

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appears to me perfectly clear, and will become so, I think, to the reader with the aid of a few words of explanation.

Let Q be a closed curve of the kind supposed lying in a plane which will be treated as a constant plane of projection; and for greater simplicity, and in order to steady the ideas, imagine that the vanishing plane (meaning thereby the plane passing through the eye and the vanishing line), and the plane of the object to be put in perspective, are retained at a constant distance from each other and always perpendicular to the picture plane, and also that the height of the eye above the vanishing line is invariable. Take any fixed line and point in the picture Q, and determine the equation to the curve boundary of its primitive O corresponding to a given distance h between the fixed point and the variable vanishing line and to a given angle of inclination α between the fixed line and this variable line. Then by hypothesis the area of O, say M, is known in terms of its coefficients, which will be known functions of α and h; hence $\frac{dM}{d\alpha}$ and $\frac{dM}{dh}$ are known, and consequently the position of the perspective of the centre of gravity of O on the picture is known; and from this the position of that centre in its own plane can be constructed, and therefore will have been found by aid of algebraical and differentiation processes only, as was to be shown.

The above explanation may be made still more distinct if we suppose that we *begin* with an object Ω (the curve for which is expressed by an equation in its most general form), wherein we have, say, $\alpha=0$ and $h=1$; that from this we deduce the equation of P in the preceding investigation, and from P pass to O as before; then, having found the coordinates of the perspective of the centre of gravity of O as functions of h and α, make $\alpha=0$, $h=1$, and pass back to the coordinates of the centre of gravity in Ω, of which the centre of gravity last named then becomes the perspective.

[*] Translated by J. T. Arlidge, A.B., M.B. Lond.

of pressure results, to which the crust at length gives way. The shock or impulse consequent on this determines a fresh act of congelation and the formation of a new layer. The latter must consist of two distinct portions; one devoid of air-bubbles and consequently transparent, the other opake by reason of the intermingling of such bubbles within it. The same phenomenon recurs in each successive congelation. If the hailstones fall on the ground before their congelation is complete, their centre may be found liquid, or contain at the same time air-bubbles, water, and needles of ice. These last will occur when the internal liquid has cooled down very slowly, for threads of ice are only seen in water under such circumstances. If the congelation be suddenly brought about, the nucleus will have the whiteness of snow. If the cold which acted on the mass of water be very intense, the crust will be more solid and thicker: the internal pressure caused by the dilatation of the liquid may be so augmented that it will cause the bursting of the hailstones into fragments, especially at the moment when congelation is accomplished. Hence it can be understood why hailstones fall in a pyramidal form.

I have frozen water in different quantities in perfectly transparent caoutchouc capsules, and have obtained artificially all these phenomena. Between the natural and the artificial hailstones there is no difference except in the number of layers, which for equal dimensions is greater in the latter. This proves that the cold which produces the natural hailstones is much more intense than that, viz. −17° C., by the aid of which I have formed them artificially. It is true indeed that, other conditions remaining the same, the number of layers decrease if the portion of water sought to be congealed be strongly agitated; but in that case the opake zones present a fibrous or radiated structure. In my experiments I have compared artificial hailstones with natural ones of equal dimensions which did not present this structure, and have always found the number of zones greater in the artificial. Besides, it is evident that the congelation of the crust must take place almost instantaneously, for otherwise we should never meet with other than small hailstones. But in artificial congelation more or less time is needed to obtain an envelope of ice capable of containing the unfrozen water; and therefore it follows that the cold which seizes on the collections of water in the atmosphere must, as we have said, be much greater than −17° C.

In artificial congelation a phenomenon transpires which elucidates a similar occurrence which often accompanies the formation of hail. After the liquid has remained in the freezing-mixture for a few minutes, slight cracking or detonating noises are heard. These are due to the effort produced by the dilatation of the enclosed water, which more or less violently distends and ruptures the envelope formed by each successive congelation. If the icy capsule be not sufficiently thick and compact, these noises are but slightly heard, or not perceived at all by the observer. This is why this circumstance does not always accompany the meteoric product; for to render it appreciable, the icy crusts of the hailstones must be compact, and consequently also produced by a very intense degree of cold. We are of opinion that the cold required for this object must at times be from 30° to 40° C. below zero. No causes of cold hitherto recognized are capable of producing so low a temperature as that just spoken of; and further, supposing that they could produce it, the refrigeration could not be instantaneous. In the theory proposed, we have to account for two circumstances: the first, how the liquid masses to be transformed into hailstones are formed in the atmosphere; the second, how the cold is produced that attacks these collections of fluid and more or less rapidly freezes their surface to a certain depth. We explain the former circumstance by the reaction of electricity on a cloud at the moment of its discharge from it, and the latter by the sudden expansion which ensues on the reaction.

To make this explanation clear, we assume a storm-cloud to be charged with electricity, then at the instant that this fluid has reached its maximum tension it must escape or be discharged. In the act of escape it exercises a violent reaction on the cloud, which compels a portion of its vapour to assume a liquid form. But, on the other hand, as this reaction has produced a condensation in the cloud in which that vapour floated, this air, in order to regain its previous volume, rushes instantaneously into the void caused by the electric discharge, and consequently undergoes expansion to an equal degree with the preceding condensation. Thus the liquid masses are subjected to rapid evaporation, and thereby to a loss of heat, more or less considerable in amount, and hence the congelation of the surface to a greater or less depth. When the cold is not sufficiently intense to freeze the masses of water, these fall in a liquid state; and we have in this fact an explanation why the first rain-drops of a thunder-storm are commonly the largest in size, and why such a prodigious rainfall often ensues immediately after a thunder-clap. Let us see how far these statements are borne out by recorded facts.

M. Beudant, speaking of a hail-storm observed by him in 1838, says, "There was a clap of thunder, and almost immediately the number of hailstones was greatly augmented" (*Comptes Rendus*, vol. vi.). M. Elie de Beaumont, speaking of hail upon which he made observations in 1837, says, "Three thunder-claps of moderate violence occurred during the hail-storm, each of which was attended by a redoubled fall of hailstones" (ibid. vol. iv.). Tessier, in his 'Notes on the Locality in France ravaged by the Hail-storm of 1788,' remarks, "the hail followed immediately after the lightning and thunder" (*Mémoires de l'Académie*, 1789). We might cite many other facts in illustration; some others are quoted in the memoir from which this extract is derived, but those already referred to suffice to show that hail is in all probability formed at the moment of the electric discharge from the storm-cloud.

This theory does not require the supposition of the presence of two clouds, which frequently does not happen. It is not necessary to suppose the existence of two contrary winds, which often is not met with. Again, it is not necessary to suppose the storm-clouds to be at a very great elevation. On the contrary, this hypothesis furnishes an explanation why hail falls in these climates during the summer and at the hottest portions of the day; it is because at such periods, when the air is most dry, the electric tension becomes more considerable than in strata of air of a higher elevation, and at hours of the day when the temperature is less.

In fine, the former of the two portions of this theory is not hypothetical but a demonstrable fact; whilst, as to its latter portion, it may be remarked that if it be indisputable that hailstones are almost instantaneous in their formation, their cause also must be equally instantaneous, and there is no other cause than the one suggested which can operate in such a manner.

XXVI. *On the Action of Oils in arresting the Motions of Camphor on Water.* By CHARLES TOMLINSON, *Lecturer on Physical Science, King's College School, London*[*].

AMONG the numerous physicists who have studied the motions of camphor on water, from the time of Volta in 1787 to that of Dutrochet in 1842, no one doubted or qualified the statement that if the surface of the water be touched with oil the motions are instantly arrested. Thus Volta says, "If the water be defiled with any foreign substance, or its surface only slightly fouled with oily matter, if only the dust of the room be upon it, the looked-for motions of camphor and of benzoin will not take place, or will be so feeble as to be scarcely sensible."

In a paper read before the Royal Society about eighteen months ago[†], one of my conclusions was, that an essential oil may arrest

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[*] Communicated by the Author.

[†] See Philosophical Magazine, vol. xxiv. p. 490.

the camphor-motions only during its solution and evaporation, and that when got rid of by those processes the motions will set in as before. This remark was not meant to apply to all the essential oils; and some results that I have lately obtained define the conditions of the limitation.

A flat glass dish, 6 inches in diameter, made chemically clean, was nearly filled with clean water, and some fragments of camphor from a freshly-cut surface were scraped upon the water with the point of a penknife-blade. The fragments rotated with great vigour. A drop of "rectified spirits of turpentine" was gently delivered to the surface of the water from the end of a glass rod; the turpentine flashed out into a film, and the camphor fragments were struck motionless—*comme foudroyées,* as Prevost has it, or "as if by magic," according to Venturi's remark. Fresh fragments of camphor were thrown on the surface at intervals during thirty hours, but no motion was produced.

Camphor was set spinning in a similar clean vessel, and a drop of oil of rosemary was placed on the surface; the film instantly arrested the motions, but in about forty minutes fresh fragments rotated briskly.

The experiment was repeated several times with different oils, &c., with similar results: the oils were cajeput, patchouli, carraway, cubebs, eucalyptus, and some others, as also creosote and carbolic acid. This additional fact was noticed with respect to the oily films of carraway and cubebs—that fresh camphor fragments rotated briskly in them. It was also found that a drop of the oil of bitter almonds or of aniseed does not arrest the motions of the camphor fragments at all.

I may here remark that oil of aniseed is well adapted to explain what seems to me to be the true cause of the camphor-motions, namely that a film is detached from the camphor itself by the adhesion of the water, which film, reacting on the fragment, produces motion after the manner of the electrical star, Barker's mill, &c. The oil of aniseed becomes solid so readily at a moderate degree of cold, that fragments of it may be used to show rotations on water after the manner of camphor. In the case of camphor, the motions are very rapid, and the film that produces them is, under ordinary circumstances, invisible; whereas in the case of solid aniseed these rotations are slow and the film visible, so that the action can be studied under the great advantage of having all the conditions at command. The film from the fragment of aniseed is pushed forward in one direction, while the fragment itself moves in the opposite direction; and this continues until the adhesion of the surface is satisfied, and then the motions are brought to an end. If the film, as fast as it is formed, is got rid of by solution and evaporation, as in the case of camphor, the motions go on until the whole of the solid is disposed of. If the surface be touched with an oil or any substance that spreads out into a film so as to prevent the camphor film from forming, the rotations cease. A little flour from the point of a penknife will spread out over the surface and stop the motions. Oils do this very effectually; and it was supposed, up to the date of my essay, that any kind of oil would permanently arrest the camphor-motions. My experiments led me to the conclusion that volatile oils that leave no residue, arrested the motions only during their solution and evaporation. I now go much further, and show that certain oils do not arrest the camphor-motions at all, and that in such cases the camphor film has a stronger adhesion to the surface of the water than that of the oils in question.

The reason why the fixed oils stop these motions is, that they form permanent films, displacing the camphor film, and preventing its adhesion to the water. Volatile oils such as turpentine, contain products of oxidation which are neither soluble nor volatile; and these arrange themselves in the form of a delicate permanent network over the surface of the water, and effectually prevent the adhesion of the camphor fragments. Such products of oxidation can, however, be got rid of by distillation. For this purpose about an ounce of turpentine was distilled from off caustic potash; and when a drop of the distillate was placed on the water among the rotating fragments, the resulting film displaced them for a moment, but did not stop the rotation. On the contrary, they immediately invaded the oily-looking film, skating in and through and about it, and leaving long trails free from oil behind.

This, then, is a case in which the camphor film has a stronger adhesion for the water than the camphene film has. A large number of oils, isomeric with oil of turpentine, and reputed commercially pure, were tried, but they all more or less stopped the rotation of the fragments. On rectifying them by distillation, they either did not arrest the motions at all, or those motions set in again as soon as the film had evaporated.

Oil of bitter almonds is not liable to be acted upon by the air in the same manner as the turpentine oils; it partly evaporates and partly forms benzoic acid, which is one of the gyrating substances. It is, moreover, displaced by the camphor film; hence it does not arrest the camphor-motions. The same remark applies to Persian naphtha, belmontine, Young's paraffine oil, some specimens of rock-oil (rectified) from Canada and Newfoundland. One specimen, of a deep yellow colour, arrested rotation; it was distilled, and the distillate had no retarding effect.

In order to stop the camphor-motions, the oily film, whether visible or not, must cover the whole surface of the water. If the point of a pin be dipped into turpentine and then into the water in the 6-inch vessel, the fragments will be repelled, but will not cease to rotate except in and about the small film produced by the pin. The head of a pin dipped in turpentine produced a film large enough to cover the whole surface and arrest the motions.

It was suggested to me by Professor Miller to add a minute quantity of resin to the film of camphene while the camphor fragments were moving about in it. A little powdered resin dusted on the surface had no other effect than that of mechanically clogging the motions without arresting them; but the head of a pin dipped into an alcoholic solution of resin and then into the camphene film on the water, had the effect of stopping the motions of the camphor fragments.

King's College, London,
August 10, 1863.

XXVII. *On the Position of the Oscillations of the Æther-particles in a Rectilinearly Polarized Ray of Light. By* Dr. G. Quincke*.

THE question whether the vibrations of the æther-particles are perpendicular to the plane of polarization, as Fresnel assumed †, or, as Prof. Neumann maintains ‡, take place in the plane of polarization itself, is closely connected with the question whether the density or elasticity of the æther is different in different media. Neumann arrived at the latter view, that the vibrations take place in the plane itself, by developing the theory of double refraction from the same principles as those which Fresnel had already established. Considering further that Fresnel, in developing the laws of reflexion for transparent substances, assumes that the density of the æther is variable, but afterwards, in the theory of double refraction, assumes that the elasticity is variable, it is even doubtful for which assumption he would ultimately have decided §.

Opinions are divided which is the just assumption; and while most of the French philosophers, along with MM. Ångström ||,

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* Communicated to the Berlin Academy of Sciences by Prof. Magnus.

† *Mém. de l'Acad. Roy. d. Scienc.* vol. vii. Pogg. *Ann.* vol. xxiii. 1831, p. 539.

‡ Pogg. *Ann.* vol. xxv. 1832, p. 451, and *Abhandl. d. Berl. Akad.* 1835, p. 5.

§ Compare also Fresnel, lettre à M. Arago, *Ann. de Chim.* vol. ix. 1818, p. 287.

|| Pogg. *Ann.* vol. xc.

Stokes[*], Haidinger[†], and Lorenz[‡], have decided for the first view, MM. Babinet[§], Holtzmann[‖], and Jamin[¶] adhere to the latter. Cauchy[**], who in his theory assumes independent particles of æther, was originally for Neumann's view, which, however, he afterwards, in a letter to M. Libri, exchanged for Fresnel's, since he could not assume that the forces which in a normal condition are exerted *in vacuo* on the æther disappeared. As far as is known to the author, Cauchy has never published the reasons which led him to this assumption.

Of the methods of experimentally deciding between the two assumptions, that of MM. Stokes, Holtzmann, and Lorenz, of deducing the direction of the vibrations from the position of the plane of polarization in diffracted rays, had given discordant results; and according to Fizeau's newest experiments[††] no conclusion appears possible from this method.

In like manner, Haidinger's experiment for deciding the question by considering the absorption of light in crystals in its dependence on the position of the direction of vibration of the æther-particles, has led to no result; and objections might be raised against Babinet's proof, who, from the position of the plane of polarization of light which is reflected from paper surfaces at an extremely oblique incidence, concludes that the vibrations are in the plane of polarizition.

Jamin[‡‡], again, in an elementary proof of Cauchy's formulæ for the reflexion and refraction of light at the surface of transparent bodies, has defended Neumann's assumption. For if the components of the vibrations normal to the limiting surface are constructed for the rays in each of the media vibrating in the plane of incidence, these, according to Jamin, must stand in a ratio independent of the angle of incidence. According to Neumann's assumption, these components are equal; according to Fresnel's assumption, they are as $1 : \left(\frac{\sin i}{\sin r}\right)^2$, where i and r are angles of incidence and refraction.

In this Jamin sees a refutation of Fresnel's assumption; but since $\left(\frac{\sin i}{\sin r}\right)$ = the refractive index, and therefore = a constant magnitude, by this assumption the required condition would be satisfied.

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[*] Cambridge Phil. Trans.

[†] *Wien. Ber.* vols. xii. & xv.

[‡] Pogg. *Ann.* vol. cxi. p. 315, and vol. cxiv. p. 250.

[§] *Compt. Rend.* vol. xxix. p. 514.

[‖] Pogg. *Ann.* vol. xcix. p. 446.

[¶] *Ann. de Chim. et de Phys.* (3) vol. lix. p. 413.

[**] *Compt. Rend.* 1836, vol. ii. p. 342.

[††] *Ann. de Chim. et de Phys.* (3) vol. lxiii. p. 385.

[‡‡] Ibid. vol. lix. p. 413.

In the belief that the question can alone be decided by experiment, the author has adopted the following way:—

From Malus's laws of the property of luminous rays polarized perpendicularly to one another, and from Fresnel and Arago's laws of the interference of polarized rays, it follows that the vibrations are perpendicular to the luminous ray itself, and either parallel or perpendicular to the plane of polarization. In a perpendicular incidence, the rays which are reflected from any surface must have the same deportment whether their vibrations are perpendicular or parallel to the plane of incidence. But if the angle of incidence increases, there is no change for the rays with vibrations perpendicular to the plane of incidence, while for the rays whose vibrations are in that plane the inclination of the path of the æther-particles in reference to the reflecting surface changes. From the elliptic polarization of light which is reflected at the limiting surface of metals and air, or of glass and air, it is known that the rays with vibrations perpendicular, and those with vibrations parallel to the plane of incidence, undergo a change of phase which is different in each case, and that the difference of the change of phase alters with the angle of incidence. According to the author's view, the ray with vibrations perpendicular to the plane of incidence would undergo the same alteration of phase for all different angles of incidence; while the alteration of phase of the ray whose vibrations are in the plane of incidence, as follows from Jamin's experiments*, would increase by a magnitude corresponding to half a wave-length, $\frac{\lambda}{2}$, if the angle of incidence increased from 0° to 90°. However this may be, the phase of the rays which vibrate in the plane of incidence must, in any case, change to a greater extent with the incident angle than that of the rays vibrating perpendicular to the plane of incidence; and with the same angle of incidence this change must be different according to the nature of the media whose limiting surface reflects the rays. In the reflexion from metallic surfaces, the change of phase, at an angle of incidence which is considerably less than the principal incidence (in which the difference of phase of the components polarized parallel and perpendicular to the plane of incidence amounts to $\frac{\pi}{2}$), is already perceptible, but in reflexion from glass it is imperceptible. If, therefore, rays reflected from the same surface, half glass and half metal, and which in other respects are the same, could be made to interpose, the

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* *Ann. de Chim. et de Phys.* (3) 1850, vol. xxix. p. 279, and vol. xxxi. p. 165.

interference-bars for rays with vibrations perpendicular to the plane of incidence in the case of glass and metal reflexion must coincide for all angles of incidence; while for rays with vibrations parallel to the plane of incidence, the interference-bars arising from glass and metallic reflexion must be displaced in reference to one another, and the displacement must increase, to a determinate limit, with an increasing incident angle*.

By means of a heliostat the author projected solar rays in a horizontal direction upon a vertical slit, which was in the focus of an achromatic lens. The rays emerged from the lens almost parallel, and then fell upon a plane-parallel glass plate, and, after reflexion from this, upon a second parallel glass plate of exactly the same thickness, at a distance of about 8 inches, which was very slightly inclined towards the first; so that the rays reflected from the posterior surface of the first and from the anterior surface of the second plate could interfere with the rays reflected from the anterior surface of the first and the posterior surface of the second, as in a similar apparatus which Jamin† has used and described. A screen darkened the rays reflected from both anterior or both posterior surfaces of the parallel glasses, and the rays transmitted by the screen fell upon a bisulphide-of-carbon or flint-glass prism, with a refractive angle of 60°, and a vertical refracting edge, in which a spectrum with thick dark interference-bars was seen with the naked eye. By suitably choosing the breadth of the slit, and altering its distance from the lens, the Fraunhofer's lines could be made to be distinctly seen in the spectrum; and by a suitable adjustment of the parallel glasses, the interfeence-bars were made parallel to Fraunhofer's lines in the spectrum.

The second plane-parallel glass was covered on the lower half of the posterior surface with a metallic mirror, and in the spectrum the rays reflected from glass-glass and glass-metal could be simultaneously seen, inasmuch as the upper part of the spectrum had been reflected at the surface of air and the posterior surface of the second glass, the lower half had been reflected at the limit of metal and the same posterior glass surface. Although both

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* From Jamin's observations on elliptic polarization in reflexion from metals or opake substances (*loc. cit.*) with the aid of Babinet's compensator, the question cannot be decided for which components of the rays polarized parallel or perpendicular to the plane of incidence the phase of the reflected ray continually alters with the angle of incidence, as it remains dubious whether, by altering the compensator (in positive reflexion), the component polarized in the plane of incidence must be continuously retarded, or the component polarized perpendicular to the plane of incidence must be continuously accelerated, in order to nullify the difference of phase of both components.

† *Ann. de Chim. et de Phys.* (3) 1858, vol. lii. p. 163 *et seq.*

interfering rays had different intensity in the lower spectrum, the interference-bars could be perfectly well seen, and appeared displaced towards the interference-bars of the upper spectrum, when looked at through a Nicol's prism the principal section of which was parallel to the plane of reflexion of the glasses, that is, when only rays polarized parallel to the plane of reflexion reached the eye. If the Nicol's prism was turned 90°, the interference-bars in both spectra coincided, in which case luminous rays, polarized perpendicular to the plane of reflexion, could alone reach the eye. In the latter case the intensity of light was, of course, feebler. In a position of Nicol's prism in which its principal section is almost parallel to the plane of reflexion, a different position of the interference-bars in both spectra is seen, so long as the intensity of the light polarized in the plane of incidence predominates; and for the same reason the phenomenon is seen without a Nicol's prism, just as with a Nicol's prism whose principal section is parallel to the plane of reflexion.

The author used various glass plates of from 4 millims. to 8·5 millims. in thickness, whose refractive index varied between 1·45 and 1·5. For metallic coating, mercury (ordinary mirror-surface), silver, gold, and platinum were used.

The angle of incidence ϕ at which the rays met the anterior surface of the plane-parallel glasses was varied from 20° to 60° or 70°; yet not under all angles of incidence could the interference-bars in the lower spectrum (glass-metal) be seen for rays polarized perpendicular to the plane of incidence, on account of the too small intensity of the light of the rays reflected at the limit of air and glass, as compared with that reflected at the surface of glass and metal. The displacement of the interference-bars towards each other in both spectra, for rays polarized parallel to the plane of incidence, increased in all metals with increasing angle ϕ; yet when ϕ is $>$ 60° the phenomenon cannot be well seen, as too much light is lost by reflexion at the anterior glass surfaces. The phenomenon is best seen with an angle of incidence ϕ of 45° or 60°, in which case the displacement of the interference-bars amounted to about 0·3 or 0·4 of the distance of the fringe.

With an incident angle $\phi = 20°$ and less, the intensity of the rays reflected at the limit of glass and air, and of the rays reflected from glass and metal, is so different, that the author could not discern with certainty a displacement of the interference-bars in both spectra towards one another; yet from his experiments it may be concluded that if it exists it is less than 0·1 of the breadth of a fringe.

It may here be observed that this apparatus, by means of a compensator of Soleil and Duboscq's construction, permitted a determination, with plane-parallel glasses slightly inclined to each other, whether the ray reflected at the limit of glass and air was retarded towards the other. It was seen, at least with the angles of incidence used by the author, that the ray reflected from metal must always be retarded, in order to bring the interference-bars, glass-metal, into the same position as the interference-bars, glass-glass.

It would thus follow from these experiments, that the luminous rays polarized in the plane of reflexion undergo the greatest alterations of phase, that vibration takes place in the plane of polarization, in accordance with Prof. Neumann, and that the elasticity of the æther differs in different media.

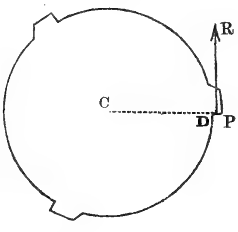
XXVIII. *Note on the Ratio between the Forces tending to produce Translation and Rotation in the Bores of Rifled Guns. By* Captain NOBLE (*late Royal Artillery*)*.

THE magnitude which the rifled ordnance of the present day have attained, and the large charges which are consumed in their bores, render it an object of great interest that we should be able to assign the pressures on the grooves (or other driving-surfaces intended to give rotation) due to different modes of rifling, as well as to determine the increment in the gaseous pressure arising from the nature of rifling adopted.

The formulæ, which I shall hereafter give, have, with slight modifications, been used at Elswick for nearly three years, and are now given, partly, because no investigation of the question has, to my knowledge, been published, and partly because, as several erroneous statements on the subject have appeared, the formulæ themselves may possibly be of use to some artillerists.

The case we shall first examine will be that in which the rotation is given by means of grooves, the driving-surfaces of which are such that if a section of the gun, perpendicular to the axis, be made, the line drawn from the centre of the bore to the groove is coincident with the section of the driving-surface. A section of such a form of rifling is shown in fig. 1. The reader is supposed to be looking from the muzzle towards the breech of the gun, and the direction of rotation is shown by the arrow A B. It will be seen that the radius C D is coincident with the section of the driving surface D P.

Fig. 1.



In entering upon this investiga-

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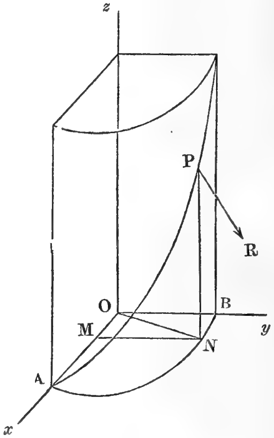
* Communicated by the Author.

tion, it will be more convenient to consider the projectile in its motion along the bore of the gun as moving on a fixed axis, and, further, to suppose that the motion of rotation is communicated to the projectile by a single groove. These suppositions will not interfere with the accuracy of our results, and will enable us very much to simplify the equations of motion.

Take (fig. 2) as the plane of $x\,y$, the plane passing through the commencement of the rifling at right angles to the axis of the gun. Let the axis of x pass through the groove under consideration, and let the axis of z be that of the gun. Let A P be the helix, and let (see figs. 1 and 2) $\mathrm{P}(x, y, z)$ be the point at which the resultant of all the pressures on the groove may be assumed to act, the projectile being in a given position. Let the angle $\mathrm{AON}=\phi$.

Fig. 2.



Let us now consider the forces which act upon the projectile. We have, first, the gaseous pressure acting on the base of the shot. Let us call this force, the resultant of which acts along the axis of z, G. Secondly, if R be the pressure between the projectile and the groove at the point P, this pressure will be exerted normally to the surface of the groove, and if we denote by λ, μ, ν the angles which the normal makes with the coordinate axes, the resolved parts of this force will be

$$\mathrm{R}\,.\cos\lambda,\ \mathrm{R}\,.\cos\mu,\ \mathrm{R}\,.\cos\nu.$$

Thirdly, if μ_1 be the coefficient of friction between the rib of the projectile and the driving-surface, the force $\mu_1\mathrm{R}$ will tend to retard the motion of the projectile. This force will act along the tangent to the helix which the point P describes; and if α, β, γ be the angles which the tangent makes with the coordinate axes, we have as the resolved portions of this force $\mu_1\mathrm{R}\,.\cos\alpha$, $\mu_1\mathrm{R}\,.\cos\beta$, $\mu_1\mathrm{R}\,.\cos\gamma$; and summing up these forces, we have the forces which act

$$\left.\begin{array}{l}\text{parallel to } x=\mathrm{X}\ =\mathrm{R}\{\cos\lambda-\mu_1\cos\alpha\},\\ \text{parallel to } y=\mathrm{Y}\ =\mathrm{R}\{\cos\mu-\mu_1\cos\beta\},\\ \text{parallel to } z=\mathrm{Z}\ =\mathrm{G}+\mathrm{R}\{\cos\nu-\mu_1\cos\gamma\}\,;\end{array}\right\}. \quad (1)$$

and the equations of motion will be

$$M\,.\frac{d^2z}{dt^2}=G+R\,.\{\cos\nu-\mu_1\cos\gamma\}\,, \quad \ldots \quad (2)$$

$$\frac{d^2\phi}{dt^2}=\frac{Yx-Xy}{M\rho^2}; \quad \ldots \quad (3)$$

ρ being the radius of gyration.

We proceed to determine the value of the angles $\alpha, \beta, \gamma, \lambda, \mu, \nu$. Let the equations to the helix described by the point P be put under the form

$$x=r\,.\cos\phi, \quad y=r\sin\phi, \quad z=kr\phi, \quad \ldots \quad (4)$$

k being the tangent of the angle at which the helix is inclined to the plane of xy. Then

$$dx=-r\sin\phi d\phi, \quad dy=r\cos\phi d\phi, \quad dz=krd\phi,$$
$$ds=r\sqrt{1+k^2}\,.\,d\phi,$$

and

$$\left.\begin{aligned}\cos\alpha&=\frac{dx}{ds}=\frac{-\sin\phi}{\sqrt{1+k^2}},\\ \cos\beta&=\frac{dy}{ds}=\frac{\cos\phi}{\sqrt{1+k^2}},\\ \cos\gamma&=\frac{dz}{ds}=\frac{k}{\sqrt{1+k^2}}.\end{aligned}\right\} \quad \ldots \quad (5)$$

To determine the values of λ, μ, ν, we shall first seek the equation to the driving-surface of the groove. In the case under consideration, the surface is a well-known conoidal one, the "skew heliçoid," and is familiar to the eye as the under surface of a spiral staircase. It is generated by a straight line which, passing through the axis of z, always remains perpendicular to it, and meets the helix described by the point P. The equations to the director being given in (4), if x_1, y_1, z_1 be the current coordinates of the generator, its equations are

$$x_1y-y_1x=0, \quad z_1=z. \quad \ldots \quad (6)$$

Hence

$$x=r\,.\cos\frac{z_1}{kr}, \quad y=r\sin\frac{z_1}{kr};$$

and the equation to the surface is

$$y_1\,.\cos\frac{z_1}{kr}-x_1\,.\sin\frac{z_1}{kr}=0,$$

or, dropping the suffixes,

$$y\,.\cos\frac{z}{kr}-x\,.\sin\frac{z}{kr}=0. \quad \ldots \quad (7)$$

Now λ, μ, ν being the angles which the normal to (7) makes with the axes,

$$\left.\begin{aligned}\cos\lambda &= \frac{\left(\frac{dF}{dx}\right)}{\left\{\left(\frac{dF}{dx}\right)^2+\left(\frac{dF}{dy}\right)^2+\left(\frac{dF}{dz}\right)^2\right\}^{\frac{1}{2}}},\\ \cos\mu &= \frac{\left(\frac{dF}{dy}\right)}{\left\{\left(\frac{dF}{dx}\right)^2+\left(\frac{dF}{dy}\right)^2+\left(\frac{dF}{dz}\right)^2\right\}^{\frac{1}{2}}},\\ \cos\nu &= \frac{\left(\frac{dF}{dz}\right)}{\left\{\left(\frac{dF}{dx}\right)^2+\left(\frac{dF}{dy}\right)^2+\left(\frac{dF}{dz}\right)^2\right\}^{\frac{1}{2}}}.\end{aligned}\right\} \quad . \; . \quad (8)$$

Now

$$\left(\frac{dF}{dx}\right) = -\sin\frac{z}{kr}, \quad \left(\frac{dF}{dy}\right) = \cos\frac{z}{kr},$$

$$\left(\frac{dF}{dz}\right) = -\frac{1}{k}\left\{\frac{x}{r}\cos\frac{z}{kr} + \frac{y}{r}.\sin\frac{z}{kr}\right\};$$

but since in the case we are now considering (x, y, z) is a point both in the surface given by equation (7) and in the directing helix, we have from (4),

$$\frac{x}{r} = \cos\phi = \cos\frac{z}{kr}, \quad \frac{y}{r} = \sin\phi = \sin\frac{z}{kr};$$

and

$$\therefore \left(\frac{dF}{dz}\right) = -\frac{1}{k}$$

$$\left\{\left(\frac{dF}{dx}\right)^2+\left(\frac{dF}{dy}\right)^2+\left(\frac{dF}{dz}\right)^2\right\}^{\frac{1}{2}} = \frac{1}{k}\cdot\sqrt{1+k^2}.$$

Hence

$$\left.\begin{aligned}\cos\lambda &= -\frac{k\sin\phi}{\sqrt{1+k^2}},\\ \cos\mu &= \frac{k.\cos\phi}{\sqrt{1+k^2}},\\ \cos\nu &= -\frac{1}{\sqrt{1+k^2}}.\end{aligned}\right\} \quad . \; . \; . \; . \; . \; . \; . \quad (9)$$

Now substituting the values of the direction cosines given in equations (5) and (9), in (1), (2), and (3), we have as the equations of motion,

$$M\cdot\frac{d^2z}{dt^2}=G-\frac{R}{\sqrt{1+k^2}}\{\mu_1k+1\}, \quad . \quad . \quad . \quad . \qquad (10)$$

$$\frac{d^2\phi}{dt^2}=\frac{Rr}{\sqrt{1+k^2}}\cdot\frac{k-\mu}{M\rho^2}; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (11)$$

and hence the normal pressure on the rib of the projectile,

$$R=\frac{M\rho^2}{r}\cdot\frac{\sqrt{1+k^2}}{k-\mu_1}\cdot\frac{d^2\phi}{dt^2}.$$

But if ϖ be the angular velocity of the projectile, and h be the pitch of the rifling, we have the following relation between the velocities of translation and rotation,

$$\varpi=\frac{d\phi}{dt}=\frac{2\pi}{h}v=\frac{2\pi}{h}\cdot\frac{dz}{dt}.$$

Hence

$$\frac{d^2\phi}{dt^2}=\frac{2\pi}{h}\cdot\frac{d^2z}{dt^2},$$

and

$$R=\frac{M\rho^2}{r}\cdot\frac{\sqrt{1+k^2}}{k-\mu_1}\cdot\frac{2\pi}{h}\cdot\frac{d^2z}{dt^2}. \quad . \quad . \quad . \quad . \quad . \qquad (12)$$

Now substituting in this equation the value of $\frac{d^2z}{dt^2}$ derived from (10), we have

$$R=\frac{2\pi\rho^2}{rh}\cdot\frac{\sqrt{1+k^2}}{k-\mu_1}\left\{G-\frac{R}{\sqrt{1+k^2}}(\mu_1k+1)\right\},$$

or

$$\frac{R}{G}=\frac{2\pi\rho^2\sqrt{1+k^2}}{hr(k-\mu_1)+2\pi\rho^2(\mu_1k+1)}. \quad . \quad . \quad . \quad . \quad . \quad . \qquad (13)$$

And this equation gives the ratio between the pressures producing translation and rotation.

We now proceed to determine the increment of the gaseous pressure due to the resistance, &c. offered by the rifling to the forward motion of the shot. We shall imagine a smooth-bored gun to fire a shot of the same weight as that of the rifled gun. We shall further suppose that the two projectiles are delivered with the same velocity; and we wish to know, the same ballistic effect being produced by the two guns, what is the increased pressure which the rifled gun has had to sustain. Now the equation of motion in the case of the smooth-bored gun is

$$M\frac{d^2z}{dt^2}=G, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (14)$$

and in the case of the rifled gun,

$$M.\frac{d^2z}{dt^2}=G'-\frac{R}{\sqrt{1+k^2}}\{\mu_1k+1\}. \quad . \quad . \quad . \quad (15)$$

Now if the velocity-increments in the two cases be taken as equal, we shall have from equations (14) and (15),

$$G'=G+\frac{R}{\sqrt{1+k^2}}(\mu_1k+1). \quad . \quad . \quad . \quad . \quad (16)$$

And the second term of the right-hand member of equation (16) represents the increment of pressure due to the rifling.

Let us now examine the pressures which subsist when a polygonal form of rifling is adopted; and we shall suppose the polygon to have n sides. The equations of motion given in equations (2) and (3) hold here as in the last case, and the values of α, β, γ given in (5) remain the same. The driving-surface is, however, different, being traced out by a straight line which always remains parallel to the plane of xy, meets the helix described by P, and touches the cylinder whose radius is $=r\cos\frac{\pi}{n}$ (see fig, 3, where PA represents the generating line drawn from a point P of the helix to touch the cylinder BC). Now the equations to the helix being

$$x=r\cos\phi,\ y=r\sin\phi,\ z=kr\phi, \quad (17)$$

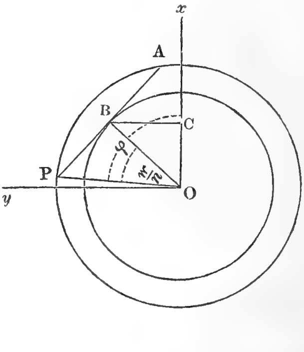
while that to the cylinder is

$$x^2+y^2=\left(r.\cos\frac{\pi}{n}\right)^2=r_1{}^2\text{suppose}, \quad (18)$$

if we draw from the point $P(x, y, z)$ of the helix a tangent in the plane $z=kr\phi$ to (18), the coordinates of the point of contact (see fig. 3) will be

$$\left.\begin{aligned} x_1&=r_1.\cos\left(\phi-\frac{\pi}{n}\right), \\ y_1&=r_1.\sin\left(\phi-\frac{\pi}{n}\right). \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad (19)$$

Fig. 3.



Now the equation to the tangent drawn through the point $x_1\,y_1$ of the circle $x^2+y^2=r_1{}^2$ is

$$xx_1+yy_1=r_1{}^2. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (20)$$

And substituting in this equation the values of x_1 and y_1 derived from (19), we obtain as the equations of the generator,

$$xr_1\,.\cos\left(\phi-\frac{\pi}{n}\right)+yr_1\,.\sin\left(\phi-\frac{\pi}{n}\right)=r_1^2,\ z=kr\phi, \qquad (21)$$

and as the equation to the driving-surface,

$$x\,.\cos\left(\frac{z}{kr}-\frac{\pi}{n}\right)+y\,.\sin\left(\frac{z}{kr}-\frac{\pi}{n}\right)=r\cos\frac{\pi}{n}. \quad . \quad . \qquad (22)$$

Now

$$\left(\frac{d\mathrm{F}}{dx}\right)=\cos\left(\frac{z}{kr}-\frac{\pi}{n}\right),\quad \left(\frac{d\mathrm{F}}{dy}\right)=\sin\left(\frac{z}{kr}-\frac{\pi}{n}\right),$$

$$\left(\frac{d\mathrm{F}}{dz}\right)=\frac{1}{k}\left\{\frac{y}{r}\cdot\cos\left(\frac{z}{kr}-\frac{\pi}{n}\right)-\frac{x}{r}\,.\sin\left(\frac{z}{kr}-\frac{\pi}{n}\right)\right\};$$

or, since $\mathrm{P}(x, y, z)$ is a point at once in the helix and the skew surface,

$$\left(\frac{d\mathrm{F}}{dz}\right)=\frac{1}{k}\,.\sin\frac{\pi}{n}.$$

Also

$$\left\{\left(\frac{d\mathrm{F}}{dx}\right)^2+\left(\frac{d\mathrm{F}}{dy}\right)^2+\left(\frac{d\mathrm{F}}{dx}\right)^2\right\}^{\frac{1}{2}}=\frac{1}{k}\sqrt{k^2+\left(\sin\frac{\pi}{n}\right)^2}.$$

And substituting these values of $\left(\frac{d\mathrm{F}}{dx}\right)$, &c. in (8), we have for the direction cosines at the point P,

$$\left.\begin{aligned}
\cos\lambda &= -\frac{k\,.\cos\left(\frac{z}{kr}-\frac{\pi}{n}\right)}{\sqrt{k^2+\left(\sin\frac{\pi}{n}\right)^2}},\\[2ex]
\cos\mu &= -\frac{k\,.\sin\left(\frac{z}{kr}-\frac{\pi}{n}\right)}{\sqrt{k^2+\left(\sin\frac{\pi}{n}\right)^2}},\\[2ex]
\cos\nu &= -\frac{\sin\frac{\pi}{n}}{\sqrt{k^2+\left(\sin\frac{\pi}{n}\right)^2}}.
\end{aligned}\right\} \quad . \quad . \qquad (23)$$

And putting the values of α, β, γ, λ, μ, ν in the equations of motion (2) and (3), we have

$$\mathrm{M}\,.\frac{d^2z}{dt^2}=\mathrm{G}-\mathrm{R}\left\{\frac{\mu_1 k}{\sqrt{1+k^2}}+\frac{\sin\frac{\pi}{n}}{\sqrt{k^2+\left(\sin\frac{\pi}{n}\right)^2}}\right\}, \quad . \qquad (24)$$

$$\begin{aligned}\frac{d^2\phi}{dt^2} &= \frac{Rr}{M\rho^2}\cdot\left\{\left\{\frac{k\cos\left(\frac{z}{kn}-\frac{\pi}{n}\right)}{\sqrt{k^2+\left(\sin\frac{\pi}{n}\right)^2}}-\frac{\mu_1\sin\frac{z}{kr}}{\sqrt{1+k^2}}\right\}\sin\frac{z}{kr}\right.\\ &\qquad\left.-\left\{\frac{\mu_1\cdot\cos\frac{z}{kr}}{\sqrt{1+k^2}}+\frac{k\cdot\sin\left(\frac{z}{kr}-\frac{\pi}{n}\right)}{\sqrt{k^2+\left(\sin\frac{\pi}{n}\right)^2}}\right\}\cos\frac{z}{kr}\right\}\\ &= \frac{Rr}{M\rho^2}\cdot\left\{\frac{k\cdot\sin\frac{\pi}{n}}{\sqrt{k^2+\left(\sin\frac{\pi}{n}\right)}}-\frac{\mu_1}{\sqrt{1+k^2}}\right\}\end{aligned}\cdot\quad(25)$$

Hence

$$R=\frac{M\cdot\rho^2}{h\cdot\left\{\frac{k\cdot\sin\frac{\pi}{n}}{\sqrt{k^2+\left(\sin\frac{\pi}{n}\right)^2}}-\frac{\mu}{\sqrt{1+k^2}}\right\}}\cdot\frac{d^2\phi}{dt^2}.$$

But

$$\frac{d^2\phi}{dt^2}=\frac{\pi r}{h}\cdot\frac{d^2z}{dt^2};$$

and making the necessary substitutions, we obtain for the ratio between the forces producing rotation and translation,

$$\frac{R}{G}=\frac{2\pi\rho^2}{\frac{\mu_1}{\sqrt{1+k^2}}(2\pi\rho^2k\quad rh)+\frac{\sin\frac{\pi}{n}}{\sqrt{k^2+\left(\sin\frac{\pi}{n}\right)^2}}\left(2\pi\rho^2\sin\frac{\pi}{n}+rhk\right)}\cdot\quad(26$$

In precisely the same manner as in the former case, and on the same hypotheses, we may show that if G'' denote the gaseous pressure in a bore rifled on the system we are now considering, and G denote the gaseous pressure in a similar smooth-bored gun, we shall have

$$G''=G+R\left\{\frac{\mu_1k}{\sqrt{1+k^2}}+\frac{\sin\frac{\pi}{n}}{\sqrt{k^2+\left(\sin\frac{\pi}{n}\right)^2}}\right\}\cdot\quad\cdot\quad(27)$$

Hence if we have three guns of the same diameter of bore, viz. a smooth-bore gun, a rifled gun, the grooves of which are similar to those shown in fig. 1, and a third rifled polygonally, and if we suppose that the shot in each case are of the same weight, and, further, that in each case the velocity-increments at the moment under consideration are equal, then the pressures upon the base of the shot will be as follow:—In the case of the

$$\left.\begin{array}{l}\text{Smooth-bored gun, pressure} = G;\\[4pt] \text{First rifled gun, pressure} = G + \dfrac{R}{\sqrt{1+k^2}}(\mu_1 k + 1);\\[4pt] \text{Polygonally-rifled gun, pressure}\\[4pt] \qquad = G + R\left\{\dfrac{\mu_1 k}{\sqrt{1+k^2}} + \dfrac{\sin\frac{\pi}{n}}{\sqrt{k^2+\left(\sin\frac{\pi}{n}\right)^2}}\right\}.\end{array}\right\}. \quad (28)$$

We shall now give examples of the cases we have been discussing to exhibit numerically the above results.

Let us suppose that two seven-inch guns are rifled—the first according to the method shown in fig. 1, with a pitch of one turn in 294 inches, the other octagonally, with a pitch of one turn in 130 inches. It is required to determine in each case the pressure on the driving-surface in terms of the pressure on the base of the shot. Now, in the first case, from (13),

$$\text{Pressure on driving surface} = \frac{2\pi\rho^2\sqrt{1+k^2}}{hr(k-\mu_1)+2\pi\rho^2(\mu_1 k+1)}\,.\,G,$$

where

$$\pi = 3{\cdot}14159, \quad \rho = r\sqrt{\tfrac{1}{2}} = 2{\cdot}475, \quad k = 13{\cdot}3697, \quad h = 294,$$
$$r = 3{\cdot}5, \quad \mu_1 = {\cdot}1666,$$

whence we obtain

$$R = {\cdot}0375\ G. \quad (29)$$

In the second case, from (26),

Pressure =

$$\frac{2\pi\rho^2}{\dfrac{\mu_1}{\sqrt{1+k^2}}(2\pi\rho^2 k - rh) + \dfrac{\sin\pi}{\sqrt{k^2+\left(\sin\frac{\pi}{n}\right)^2}}\left(2\pi\rho^2\sin\frac{\pi}{n} + rhk\right)}\,.\,G,$$

where

$$\pi = 3{\cdot}14159,$$

$$\rho = \frac{1}{12}c^2\,.\,\frac{2+\cos\frac{2\pi}{n}}{1-\cos\frac{2\pi}{n}} = 2{\cdot}350 \ (c = \text{length of side of polygon}),$$

$k=5{\cdot}9117,\quad h=130,\quad r=3{\cdot}5,\quad n=8,\quad \mu_1={\cdot}1666,$

$\frac{\pi}{n}=22°\ 30'',$

whence $$\text{R}={\cdot}1706\,\text{G}. \quad \ldots\ldots \quad (30)$$

That is, on the supposition of the same pressure on the base of the shot, the pressure on the driving-surface is in the latter case nearly five times as great as in the former, and is, in fact, no inconsiderable fraction of the propelling force.

Let us now compare the gaseous pressures on the base of shot of the same weight supposed to be fired from the guns above described, and from a smooth-bored gun. From equations (28) we have the pressure upon base of shot fired from

Smooth-bored gun . . . . . =G,
First rifled gun . . . . . . =1·009 G,
Polygonal gun . . . . . . =1·041 G.

In these calculations we have taken the coefficient of friction $=\frac{1}{6}$. It is necessary, however, to observe that very little is known concerning the value of this constant at pressures so high as those with which we have here to do. It is evident that in the case of the contact of similar metals, when the point of seizure is approached, the coefficient of friction cannot be considered independent of the pressure; and it is probable that when the rubbing surfaces of both projectile and groove (or other driving-surface) are of the same hard material, the coefficient of friction may be occasionally enormously increased.

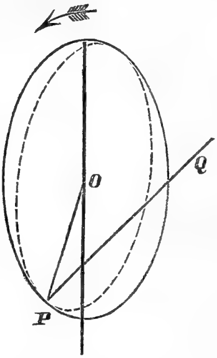
The resistance due to this cause might, under certain circumstances, be sufficient to ensure the destruction of the gun; and this view is to some extent corroborated by the occasional bursting of guns, the failure of which it is difficult to attribute to any other cause; and in the instances referred to, the recovered fragments of the shot were thought to exhibit decided appearances of seizure.

If in equation (26) we substitute δ for $\frac{\pi}{n}$, we shall have

$$\frac{\text{R}}{\text{G}}=\frac{2\pi\rho^2}{\frac{\mu_1}{\sqrt{1+k^2}}(2\pi\rho^2k-rh)+\frac{\sin\delta}{\sqrt{k^2+(\sin\delta)^2}}(2\pi\rho^2\sin\delta+rhk)}. \quad (31)$$

And this equation will represent the ratio of the pressures R and G in any system of rifling, δ being the angle which the radius makes with the normal to the driving-surface. Thus in

Fig. 4.



an elliptically-bored gun (see fig. 4), the angle O P Q represents the angle δ, and we obtain $\frac{R}{G}$ by substituting in (31) the value of this angle; by putting $\delta = 90°$, we may derive equation (13) directly from (31).

We have not in this note entered into the question of the absolute pressures existing in the bores of ordnance of various natures, as the subject is too extensive and of too great importance to be disposed of within the limits of a short paper.

Artillerists acquainted with the subject will be able to form rough approximations to these pressures from the experiments made abroad with smooth-bored guns, with a view to the elucidation of this important question. It is much to be regretted that no experiments of the nature referred to have been attempted in England under Government auspices, as they are of a description which precludes their being satisfactorily made by private individuals, and as the information to be derived from them would be especially important in the case of rifled cannon, where so many new conditions are introduced into the problem as to render previous investigations of but little value.

We shall, however, in a future note endeavour to discuss this subject, making use of the data at present at our disposal.

Elswick Engine Works,
June 1863.

XXIX. *On the Theory of Light.* By L. Lorenz.

[Concluded from p. 93.]

II. *Integration of the Differential Equations: Double Refraction and Chromatic Dispersion.*

IN the differential equations (A) which express the laws of the motion of light in heterogeneous substances, without, however, making any distinct assumption as to the nature of the vibrations, there occurs only one function, namely ω, which is directly dependent on the heterogeneity of the medium, and can therefore be any function whatever of x, y, and z. Such a function, as is well known, can by Fourier's theorem be represented generally by the equation

$$\frac{1}{\omega^2} = \frac{1}{\Omega^2}\left[1 + \Sigma\epsilon_p \cos\rho_p\right], \quad \ldots \ldots \quad (1)$$

in which

$$\rho_p = \frac{a_p x + b_p y + c_p z + d_p}{d_p}, \quad a_p^2 + b_p^2 + c_p^2 = 1.$$

The coefficients Ω, ϵ_p, &c. which occur here are constant, and Σ denotes the sum for all values of the index p.

Thus expressed, the function ω or $\frac{1}{\omega^2}$ is retained with all its generality; but if we preserve this degree of generality, which comprehends every possible conglomeration of transparent substances, the integral can obviously represent only a confused mixture of luminous motions. Without altering the form of equation (1), we will therefore introduce an essential limitation, by making the quantities d_p *very small*. On this supposition the formula will express a periodicity and a regularity such that it will rapidly repeat itself at the various points of the body.

We shall thus obtain a first approximation by taking the quantities α_p *very small*; this case is by no means the same as that of perfect homogeneity, but directly leads, as we shall see, to double refraction.

The components $\bar{\xi}$, $\bar{\eta}$, $\bar{\zeta}$ can be expressed by a series of the following form:

$$\bar{\xi} = \bar{\xi}_0 \mathrm{C} + \Sigma \bar{\xi}(\pm\rho_p)\mathrm{C}(\pm\rho_p) + \Sigma\Sigma \bar{\xi}(\pm\rho_p \pm\rho_q)\mathrm{C}(\pm\rho_p\pm\rho_q) + \ldots (2)$$

In this expression $\bar{\xi}_0$, $\bar{\xi}(\pm\rho_p)$, &c. stand for constant coefficients of the variable quantities C, $\mathrm{C}(\pm\rho_p)$, &c., which latter are abbreviations of the following values,

$$\cos(kt - lx - my - nz) = \mathrm{C},$$
$$\cos(kt - lx - my - nz + \rho_p) = \mathrm{C}(+\rho_p),$$
&c.

The double sign denotes the sum of the two expressions; and lastly, Σ and $\Sigma\Sigma$ are the sum and double sum for all indices p and q, both of which pass through the same series of values as index p in equation (1).

In the double sum, as well as in the subsequent terms, such terms as have already occurred in previous terms must be considered as excluded; thus, for example, the term

$$\bar{\xi}(\rho_p - \rho_q)\mathrm{C}(\rho_p - \rho_q)$$

would be excluded when q was $= p$.

It will be understood that *two* quantities ρ_p and ρ_q, when p and q are unequal, can be taken as different in such wise that their sum or difference cannot become constant. On the contrary, it is conceivable that 3, 4, . . . &c. of the quantities ρ_p may give constant values by addition or subtraction. We will not, however, consider this case until the next section.

Two developments for $\bar{\eta}$ and $\bar{\zeta}$ analogous to equation (2) may be obtained by changing the letter ξ for η or ζ.

We now get from equations (1) and (2) by multiplication

$$\frac{\Omega^2}{\omega^2}\bar{\xi}=\left[\bar{\xi}_0+\Sigma\frac{\epsilon_p}{2}\bar{\xi}(\pm\rho_p)\right]\mathrm{C}$$

$$+\Sigma\left[\bar{\xi}(\pm\rho_p)+\frac{\epsilon_p}{2}\bar{\xi}_0+\Sigma\frac{\epsilon_q}{2}\bar{\xi}(\pm\rho_p\pm\rho_q)\right]\mathrm{C}(\pm\rho_p)+\ldots\quad(3)$$

This value, together with the corresponding values of $\frac{\Omega^2}{\omega^2}\bar{\eta}$, $\frac{\Omega^2}{\omega^2}\bar{\zeta}$, is to be introduced into the right-hand member of the differential equations (A); then, by comparison of the coefficients of C, we get from the first equation (A),

$$\bar{\xi}_0+\Sigma\frac{\epsilon_p}{2}\bar{\xi}(\pm\rho_p)=\frac{\Omega^2}{k^2}[(l^2+m^2+n^2)\bar{\xi}_0-l(l\bar{\xi}_0+m\bar{\eta}_0+n\bar{\zeta}_0)].\quad(4)$$

Corresponding equations may be derived from the two other equations (A), and may also be deduced from (4) by changing ξ for η and l for m, or ξ for ζ and l for n. We will denote the expressions on the right of these three equations by $\tilde{\omega}(\bar{\xi}_0)$, $\tilde{\omega}(\bar{\eta}_0)$, and $\tilde{\omega}(\bar{\zeta}_0)$.

By comparison of the coefficients of $\mathrm{C}(\pm\rho_p)$, $\mathrm{C}(\pm\rho_p\pm\rho_q)$, &c., we may also get the coefficients $\bar{\xi}(\pm\rho_p)$, $\bar{\xi}(\pm\rho_p\pm\rho_q)$, $\bar{\eta}(\pm\rho_p)$, &c. expressed, with any degree of approximation, as linear functions of $\bar{\xi}_0$, $\bar{\eta}_0$, $\bar{\zeta}_0$. Equation (4) and the two analogous equations thus acquire the following form:—

$$\left.\begin{array}{l}a_{1,1}\bar{\xi}_0+a_{1,2}\bar{\eta}_0+a_{1,3}\bar{\zeta}_0=\tilde{\omega}(\bar{\xi}_0),\\ a_{2,1}\bar{\xi}_0+a_{2,2}\bar{\eta}_0+a_{2,3}\bar{\zeta}_0=\tilde{\omega}(\bar{\eta}_0),\\ a_{3,1}\bar{\xi}_0+a_{3,2}\bar{\eta}_0+a_{3,3}\bar{\zeta}_0=\tilde{\omega}(\bar{\zeta}_0).\end{array}\right\}\quad\ldots\quad(\mathrm{B})$$

These equations determine the non-periodic portion of the components of the excursion—that is to say, the sensible portion of the motions of light; for the periodic motions neutralize one another, inasmuch as they disappear by integration over a small extent.

It can now be shown that the following three relations exist among the coefficients a in the equations (B); namely,

$$a_{1,2}=a_{2,1};\quad a_{1,3}=a_{3,1};\quad a_{2,3}=a_{3,2};\quad\ldots\quad(5)$$

and that, when α_p denotes infinitely small quantities, they are independent of l, m, and n; whereas in the contrary case they can be developed according to powers of l, m, and n, so that they only occur as factors in *even* numbers.

I may be allowed to confine the demonstration to the case in which ϵ_p represents small quantities. By comparison of the coefficients of $C(\rho_p)$, we get then only

$$\bar{\xi}(\rho_p)+\frac{\epsilon_p}{2}\bar{\xi}_0=\frac{\Omega^2}{k^2}\{(l_p^2+m_p^2+n_p^2)\bar{\xi}(\rho_p)$$
$$-l_p[l_p\bar{\xi}(\rho_p)+m_p\bar{\eta}(\rho_p)+n_p\bar{\zeta}(\rho_p)]\}, \quad . \quad . \quad (6)$$

where

$$l_p=l+\frac{a_p}{\alpha_p};\quad m_p=m+\frac{b_p}{\alpha_p};\quad n_p=n+\frac{c_p}{\alpha_p}.$$

The equation is multiplied by l_p, and then two analogous equations are obtained by substituting first η and m, then ζ and n, for ξ and l. By adding the three equations thus formed, we get

$$l_p\bar{\xi}(\rho_p)+m_p\bar{\eta}(\rho_p)+n_p\bar{\zeta}(\rho_p)+\frac{\epsilon_p}{2}(l_p\bar{\xi}_0+m_p\bar{\eta}_0+n_p\bar{\zeta}_0)=0.$$

This, with the foregoing equation, gives

$$\left(l_p^2+m_p^2+n_p^2-\frac{k^2}{\Omega^2}\right)\bar{\xi}(\rho_p)$$
$$=\frac{\epsilon}{2}\left[\left(\frac{k^2}{\Omega^2}-l_p^2\right)\bar{\xi}_0-l_p m_p\bar{\eta}_0-l_p n_p\bar{\zeta}_0\right], \quad . \quad . \quad (7)$$

by means of which the sum $\Sigma\frac{\epsilon_p}{2}\bar{\xi}(\pm\rho_p)$ in equation (4) admits of being expressed as a linear function of $\bar{\xi}_0$, $\bar{\eta}_0$, $\bar{\zeta}_0$. The coefficient of $\bar{\eta}_0$, for instance, becomes

$$-\Sigma\left(\frac{\epsilon_p}{2}\right)^2\frac{(a_p\pm l\alpha_p)(b_p\pm m\alpha_p)}{(a_p\pm l\alpha_p)^2+(b_p\pm m\alpha_p)^2+(c_p\pm n\alpha_p)^2-\frac{k^2}{\Omega^2}\alpha_p^2},$$

and is the coefficient denoted in the equations (B) by $a_{1,2}$. From this expression we can also get $a_{2,1}$ by putting b for a and m for l; but since this would not alter the expression, we have

$$a_{1,2}=a_{2,1}.$$

The truth of the two other equations (5) may be demonstrated in the same way.

If the above expression, or the value of any other coefficient a, is developed according to powers of α_p, the odd powers of these magnitudes will cancel each other in the sum; so that l, m, n also only occur as factors in even numbers.

For $\alpha_p=0$ we have

$$a_{1,2}=-\Sigma\left(\frac{\epsilon_p}{2}\right)^2 a_p b_p\,; \quad a_{1,3}=-\Sigma\left(\frac{\epsilon_p}{2}\right)^2 a_p c_q\,;$$

$$a_{2,3}=-\Sigma\left(\frac{\epsilon_p}{2}\right)^2 b_p c_p.$$

Since these quantities are independent of l, m, n, the direction of the axes of coordinates may be chosen so that we have

$$a_{1,2}=0\,; \quad a_{1,3}=0\,; \quad a_{2,3}=0. \quad \ldots\ldots \quad (8)$$

If we further denote the velocity of light in the body by s, the wave-length by λ, and the cosines of the angles which the perpendicular to the plane of the wave makes with the axes by u, v, w, we have

$$C=\cos\,(kt-lx-my-nz)=\cos\frac{2\pi}{\lambda}(st-ux-vy-wz)\,;$$

accordingly

$$s=\frac{k}{\sqrt{l^2+m^2+n^2}}\,; \quad l=\frac{2\pi}{\lambda}u\,; \quad m=\frac{2\pi}{\lambda}v\,; \quad n=\frac{2\pi}{\lambda}w.$$

Lastly, by putting

$$a_{1,1}=\frac{\Omega^2}{a^2}\,; \quad a_{2,2}=\frac{\Omega^2}{b^2}\,; \quad a_{3,3}=\frac{\Omega^2}{c^2},$$

we have, by the equations (B),

$$\left.\begin{aligned}
\frac{s^2}{a^2}\bar{\xi}_0&=\bar{\xi}_0-u(u\bar{\xi}_0+v\bar{\eta}_0+w\bar{\zeta}_0),\\
\frac{s^2}{b^2}\bar{\eta}_0&=\bar{\eta}_0-v(u\bar{\xi}_0+v\bar{\eta}_0+w\bar{\zeta}_0),\\
\frac{s^2}{c^2}\bar{\zeta}_0&=\bar{\zeta}_0-w(u\bar{\xi}_0+v\bar{\eta}_0+w\bar{\zeta}_0).
\end{aligned}\right\} \quad \ldots\ldots \quad (9)$$

Hence it follows that the velocity s of the light is determined by the following equation:

$$\frac{u^2}{a^2-s^2}+\frac{v^2}{b^2-s^2}+\frac{w^2}{c^2-s^2}=0. \quad \ldots\ldots \quad (10)$$

The body under consideration behaves therefore like a biaxal crystal, and *doubly refracts* light according to the known laws of double refraction.

The reader will bear in mind that the components $\bar{\xi}, \bar{\eta}, \bar{\zeta}$ are not identical with the components ξ, η, ζ, inasmuch as we put

$$\xi=\frac{1}{\omega^p}\bar{\xi}\,; \quad \eta=\frac{1}{\omega^p}\bar{\eta}\,; \quad \zeta=\frac{1}{\omega^p}\bar{\zeta}.$$

We will now investigate specially two cases, namely those in which the exponents p have the respective values 0 and 2. It will then be seen that the result in both these cases agrees with experiment, since the non-periodic portions of the excursion are in both cases in the same plane through the perpendicular to the plane of the wave. Let this plane be determined by the equation

$$\mathrm{A}x+\mathrm{B}y+\mathrm{C}z=\mathrm{D}.$$

Since it passes through the perpendicular to the plane of the wave and the non-periodic part of the excursion, we have

$$\mathrm{A}u+\mathrm{B}v+\mathrm{C}w=0;$$

and for $p=0$,

$$\mathrm{A}\bar{\xi}_0+\mathrm{B}\bar{\eta}_0+\mathrm{C}\bar{\zeta}_0=0.$$

For $p=2$, we get

$$\xi=\frac{1}{\omega^2}\bar{\xi}=\frac{1}{\Omega^2}\left[\bar{\xi}_0+\Sigma\frac{\epsilon_p}{2}\bar{\xi}(\pm\rho_p)\right]\mathrm{C}+\ldots$$

If, therefore, the non-periodic part of the component of the excursion is here denoted by $\xi_0\mathrm{C}$, equation (4) gives

$$\xi_0=\frac{1}{s^2}\left[\bar{\xi}_0-u(u\bar{\xi}_0+v\bar{\eta}_0+w\bar{\zeta}_0)\right].$$

By multiplying this equation by A, and forming two analogous equations by putting in one case η, B, v, and in the other ζ, C, w for ξ, A, u, we get by addition of all three equations,

$$\mathrm{A}\xi_0+\mathrm{B}\eta_0+\mathrm{C}\zeta_0=0.$$

Hence this excursion also lies in the plane D, which was to be proved.

It will consequently make no essential difference whether we take $p=0$ or $p=2$, for in both cases the plane of polarization is the same. In the first case the components of the excursion are determined by (9), and accordingly do not lie in the plane of the wave; on the contrary, it is easily proved that they are perpendicular to the *ray*, which in doubly refracting media differs, as is well known, from the perpendicular to the plane of the wave. In the other case, however, $p=2$, we have

$$\xi_0=\frac{1}{a^2}\bar{\xi}_0=\frac{1}{s^2}\left[\bar{\xi}_0-u(u\bar{\xi}_0+v\bar{\eta}_0+w\bar{\zeta}_0)\right];$$

and this equation, in conjunction with the analogous equations for η_0 and ζ_0, gives

$$\frac{a^2-s^2}{u}\xi_0=\frac{b^2-s^2}{v}\eta_0=\frac{c^2-s^2}{w}\zeta_0=a^2u\xi_0+b^2v\eta_0+cw\zeta_0. \quad (11)$$

But since we also find $u\xi_0+v\eta_0+w\zeta_0=0$, the vibrations lie in the plane of the wave, and the result in regard also to the direction of the vibrations is now in perfect accordance with the common theory. Thus whether we assume $p=0$ or $p=2$, the plane which passes through the excursion and the perpendicular to the plane of the wave coincides, as in the ordinary theory, with the plane passing through the perpendicular to the plane of the wave and the corresponding ray.

We only require to go a step further, that is, to take the higher powers of α_p into consideration as well as the lower ones, in order to arrive at the theory of *chromatic dispersion.* As already pointed out, the coefficients a in the equations (B) admit of being developed according to powers of l, m, n, so that only even numbers of them occur as factors; hence they can also be developed according to powers of $\frac{1}{\lambda^2}$ by introducing the values given above for l, m, n. Herein lies the law of chromatic dispersion of light so far as it is known to us. There would be no difficulty in carrying out the calculation in its most general form; but, as it seems to me, there would be no practical interest in doing so. If the body is not crystalline, that is, if no one direction is different from the rest, the velocity s will be given by

$$\frac{\Omega^2}{s^2}=a_{1,1}=a_{2,2}=a_{3,3},$$

where $a_{1,1}$ admits of being developed according to even powers of $\frac{1}{\lambda}$.

According to this theory, chromatic dispersion appears as a property of material bodies, dependent on their periodic heterogeneity, whereas, on Cauchy's theory, the absence of chromatic dispersion in a vacuum can only be explained by new hypotheses. It has already been shown by M. F. Eisenlohr (Poggendorff's *Annalen*, vol. cix.) that, even when we retain the common conception of the nature of luminous vibrations and of the æther, the phenomena of chromatic dispersion lead to the assumption of periodical changes in the density of the æther in material bodies. It also results from the present investigation, that double refraction can likewise be deduced from this assumption, which is in fact the most general that we can make.

Fresnel's supposition, that the explanation of double refraction lies in an unequal elasticity in different directions, may perhaps appear to receive confirmation from the fact that uncrystalline bodies become doubly refracting by pressure. It must, however, be likewise observed that the dimensions of the body are altered by pressure, and that consequently the periodic constants, and the homogeneity in different directions, must alter at the same time. Thus by a vertical pressure the vertical dimension will be diminished, and thus all the small irregularly lying layers of which the body consists will become horizontal; the body must then behave like a doubly refracting crystal with one vertical optic axis.

III. *Integration of the Differential Equations: Circular Polarization.*

In the foregoing calculations we have come upon rectilinear vibrations only, and we should not be able to deduce any others by further approximation. But, as already mentioned, the calculation presupposes that 3, 4, ... &c. of the magnitudes ρ_p can acquire no constant value by addition and subtraction. This possibility, as the more general case, must nevertheless necessarily be taken into consideration; and it then becomes apparent that the formulæ thus generalized comprehend also elliptic vibrations dependent on the uneven powers of the small quantities α_p.

Retaining our previous notation, and likewise putting

$$\begin{aligned} \mathrm{S} &= \sin(kt-lx-my-nz), \\ \mathrm{S}(\rho) &= \sin(kt-lx-my-nz+\rho), \end{aligned}$$

the expression (2) assumes the following more general form:—

$$\bar\xi = \bar\xi_0 \mathrm{C} + \Sigma \bar\xi(\pm\rho_p)\mathrm{C}(\pm\rho_p) + \ldots + \bar\xi'_0 \mathrm{S} + \Sigma \bar\xi'(\pm\rho_p)\mathrm{S}(\pm\rho_p) + \ldots, \qquad (1$$

where the coefficients of S, S$(\pm\rho_p)$ are denoted by accents. The components $\bar\eta$ and $\bar\zeta$ may be determined in an analogous manner.

It can now be proved that, in place of the equations (B), the following will be obtained:—

$$\left.\begin{aligned} a_{1,1}\bar\xi_0 + a_{1,2}\bar\eta + a_{1,3}\bar\zeta_0 + b_{1,1}\bar\xi'_0 + b_{1,2}\bar\eta'_0 + b_{1,3}\bar\zeta'_0 &= \pi(\bar\xi_0), \\ a_{2,1}\bar\xi_0 + a_{2,2}\bar\eta + a_{2,3}\bar\zeta_0 + b_{2,1}\bar\xi'_0 + b_{2,2}\bar\eta'_0 + b_{2,3}\bar\zeta'_0 &= \pi(\bar\eta_0), \\ a_{3,1}\bar\xi_0 + a_{3,2}\bar\eta + a_{3,3}\bar\zeta_0 + b_{3,1}\bar\xi'_0 + b_{3,2}\bar\eta'_0 + b_{3,3}\bar\zeta'_0 &= \pi(\bar\zeta_0). \end{aligned}\right\} \quad (\mathrm{C})$$

These three equations give rise to three others again, which are formed from them by putting $\bar\xi'_0, \bar\eta'_0, \bar\zeta'_0$ in place of $\bar\xi_0, \bar\eta_0, \bar\zeta_0$, and $-\bar\xi_0, -\bar\eta_0, -\bar\zeta_0$ in place of $\bar\xi'_0, \bar\eta'_0, \bar\zeta'_0$. For if equation (12) is differentiated in regard to kt, C is transformed into $-$S, and S into C, which is the same thing as changing all the ξs on the right-hand side of the equation into ξ's and all the ξ's into $-\xi$s. If, however, $\bar\xi, \bar\eta, \bar\zeta$ satisfy the differential equations (A), their differential coefficients with respect to kt would do so likewise, and hence it will always be allowable to make the specified changes in the derived equations.

It can be proved further, that while the previous relations (5) still subsist among the coefficients a, the coefficients b, on the other hand, will satisfy the general equation

$$b_{p_1 q} = -b_{q_1 p}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (13)$$

or

$$b_{1,2} = -b_{2,1}; \quad b_{1,3} = -b_{3,1}; \quad b_{2,3} = -b_{3,2};$$
$$b_{1,1} = 0; \quad b_{2,2} = 0; \quad b_{3,3} = 0.$$

Uneven powers only of α_p will occur in these coefficients; they are accordingly small in comparison with the coefficients a, and will disappear in the first approximation ($\alpha_p = 0$), and hence also, upon our present supposition, the results of the previous section remain unchanged for the first approximation.

I have carried out the calculation for the two cases of three or four of the quantities ρ_p giving a constant sum; the latter case, however, I have treated only upon the hypothesis that the quantities ϵ_p are very small. But since the results already given are always the same, I take the liberty of limiting the demonstration to the following case,

$$\rho_1 + \rho_2 + \rho_3 = \Delta, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (14)$$

where Δ is a constant. It will also be assumed that both ϵ_p and α_p are small quantities.

While equation (4) still holds, an alteration will now, on the contrary, occur in equation (6) every time that the index p has one of the values 1, 2, or 3. Thus, if we look for the coefficients of $C(\rho_1)$ in the differential equations (A), we find for $p=1$ on the left of equation (6),

$$\bar{\xi}(\rho_1) + \frac{\epsilon_1}{2}\bar{\xi}_0 + \left[\frac{\epsilon_2}{2}\bar{\xi}(-\rho_3) + \frac{\epsilon_3}{2}\bar{\xi}(-\rho_2)\right]\cos\Delta$$
$$-\left[\frac{\epsilon_2}{2}\bar{\xi}'(-\rho_3) + \frac{\epsilon_3}{2}\bar{\xi}'(-\rho_2)\right]\sin\Delta. \quad . \quad . \quad ; \quad (15)$$

If we neglect the second and higher powers of α_p, we get from equation (6) for all values of p,

$$\frac{\bar{\xi}(\rho_p)}{l_p} = \frac{\bar{\eta}(\rho_p)}{m_p} = \frac{\bar{\zeta}(\rho_p)}{n_p} = \frac{r(\rho_p)}{\sqrt{l_p^2 + m_p^2 + n_p^2}}. \quad . \quad . \quad (16)$$

We will further introduce into these equations the following symbols,

$$l_{\bar{p}} = l - \frac{a_p}{\alpha_p}, \quad m_{\bar{p}} = m - \frac{b_p}{\alpha_p}, \quad n_{\bar{p}} = n - \frac{c_p}{\alpha_p},$$

which take the place of l_p, m_p, n_p when $-\rho_p$ is substituted for ρ_p. Similarly, we will express $r'(\rho_p)$ by $\bar{\xi}'(\rho_p)$.

If equation (15) be now multiplied by l_1, and the analogous expressions modified by changing ξ and l into η and m or into ζ

and *n*, the sum of these three expressions will, by (6), be equal to nothing; and if for shortness we put

$$\frac{l_p}{\sqrt{l_p^2+m_p^2+n_p^2}}=u_p, \quad \frac{m_p}{\sqrt{l_p^2+m_p^2+n_p^2}}=v_p, \quad \frac{n_p}{\sqrt{l_p^2+m_p^2+n_p^2}}=w_p,$$

$$u_p u_q+v_p v_q+w_p w_q=\theta_{p,q}=\theta_{q,p},$$

$$u_p \bar{\xi}_0+v_p \bar{\eta}_0+w_p \bar{\zeta}_0=\mathrm{E}_p, \quad u_p \bar{\xi}'_0+v_p \bar{\eta}'_0+w_p \bar{\zeta}'_0=\bar{\mathrm{E}}'_p,$$

we get

$$r(\rho_1)+\frac{\epsilon_1}{2}\mathrm{E}_1+\frac{\epsilon_2}{2}\vartheta_{1,\bar{3}}[r(-\rho_3)\cos\Delta-r'(-\rho_3)\sin\Delta]$$

$$+\frac{\epsilon_3}{2}\vartheta_{1,\bar{2}}[r(-\rho_2)\cos\Delta-r'(-\rho_2)\sin\Delta]=0.$$

If we introduce into this equation the approximate values of $r(-\rho_2)$, $r'(-\rho_2)$, $r(-\rho_3)$, $r'(-\rho_3)$, namely $r(-\rho_2)=-\frac{\epsilon_2}{2}\mathrm{E}_2$, &c., which may be found from the same equation by exchanging the suffixes, it is transformed into

$$r(\rho_1)=-\frac{\epsilon_1}{2}\mathrm{E}_1+\frac{\epsilon_2\,\epsilon_3}{4}\vartheta_{1,\bar{3}}(\mathrm{E}_{\bar{3}}\cos\Delta-\mathrm{E}'_3\sin\Delta)$$

$$+\frac{\epsilon_2\,\epsilon_3}{4}\vartheta_{1,\bar{2}}(\mathrm{E}_{\bar{2}}\cos\Delta-\mathrm{E}'_{\bar{2}}\sin\Delta). \quad . \quad . \quad . \quad (17)$$

By substituting $-\rho_1$ for ρ_1 in this equation, ρ_2 and ρ_3, and, by (14), Δ also, acquire the opposite sign; moreover E_1 is transformed into $\mathrm{E}_{\bar{1}}$, $\vartheta_{1,\bar{3}}$ into $\vartheta_{\bar{1},3}$, &c. By exchange of suffixes, analogous expressions are also obtained for $r(\rho_2)$, $r(-\rho_2)$, $r(\rho_3)$, $r(-\rho_3)$.

If we now investigate the value of

$$\Sigma\frac{\epsilon_p}{2}\bar{\xi}(\pm\rho_p)=\Sigma\frac{\epsilon_p}{2}[u_p r(\rho_p)+u_{\bar{p}}r(-\rho_p)],$$

in order to put it into equation (4), the calculation will be the same as before when *p* has any other value than 1, 2, or 3; while, on the other hand, the sum of the six terms corresponding to these indices will have to be separately investigated.

In the sum, for instance, the following expression occurs as coefficient of η_1,

$$\mathrm{S}v_1\left[-\frac{\epsilon_1^{\,2}}{4}u_1+\frac{\epsilon_1\,\epsilon_2\,\epsilon_3}{8}(u_{\bar{2}}\vartheta_{\bar{2},1}+u_{\bar{3}}\vartheta_{\bar{3},1})\cos\Delta\right]$$

$$+\mathrm{S}v_1\left[-\frac{\epsilon_1^{\,2}}{4}u_{\bar{1}}+\frac{\epsilon_1\,\epsilon_2\,\epsilon_3}{8}(u_2\vartheta_{2,\bar{1}}+u_3\vartheta_{3,\bar{1}})\cos\Delta\right],$$

where S denotes the sum of the subsequent expression and two others derived from it by the exchange of 1 for 2 and for 3. Since this sum is not altered by exchanging u for v, or l for m, we conclude, as before, that we shall have $a_{1,2}=a_{2,1}$. In the same way we have $a_{1,3}=a_{3,1}$ and $a_{2,3}=a_{3,2}$. It is easy to show further that the odd powers of α_p disappear in the coefficients a.

The coefficient of $\bar{\xi}'_0$ is

$$S\frac{\epsilon_1\,\epsilon_2\,\epsilon_3}{8}\left[u_1(u_{\bar 2}\vartheta_{\bar 2,1}+u_{\bar 3}\vartheta_{\bar 3,1})-u_{\bar 1}(u_2\vartheta_{2,\bar 1}+u_3\vartheta_{3,\bar 1})\right]\sin\Delta.$$

The sum is, however, equal to nought, whence $b_{1,1}=0$; and similarly, $b_{2,2}=0$, and $b_{3,3}=0$.

Lastly, the coefficient of $\bar{\eta}_0'$ is

$$S\frac{\epsilon_1\,\epsilon_2\,\epsilon_3}{8}\left[v_1(u_{\bar 2}\vartheta_{\bar 2,1}+u_{\bar 3}\vartheta_{\bar 3,1})-v_{\bar 1}(u_2\vartheta_{2,\bar 1}+u_3\vartheta_{3,\bar 1})\right]\sin\Delta.$$

If n is here exchanged for v, or l for m, we get the same value with the *contrary* sign, thus $b_{1,2}=-b_{2,1}$; and similarly, $b_{1,3}=-b_{3,1}$, $b_{2,3}=-b_{3,2}$. It is easy to see also that the coefficients b will contain only odd powers of α_p.

From the equations (C) and the relations that have been found between the coefficients, it will be at once obvious to any one acquainted with the theory of polarized light, that this theory is completely contained therein. I nevertheless take the liberty to indicate this by a few lines a little more distinctly.

We can, as before, choose the direction of the coordinates so as to satisfy the equations

$$a_{1,2}=0,\quad a_{1,3}=0,\quad a_{2,3}=0.$$

We will further put

$$a_{1,1}=\frac{\Omega^2}{a^2},\quad a_{2,2}=\frac{\Omega^2}{b^2},\quad a_{3,3}=\frac{\Omega^2}{c^2},$$

$$b_{2,3}=\Omega^2 d,\quad b_{3,1}=\Omega^2 e,\quad b_{1,2}=\Omega^2 f.$$

The values on the right of the equations (C) are

$$\pi(\bar\xi_0)=\frac{\Omega^2}{s^2}[\bar\xi_0-u(u\bar\xi_0+v\bar\eta_0+w\bar\zeta_0)],$$

&c.

These values having been transferred to the left side, we may write

$$\frac{1}{a^2}-\frac{1-u^2}{s^2}=c_{1,1};\quad \frac{1}{b^2}-\frac{1-v^2}{s^2}=c_{2,2};\quad \frac{1}{c^2}-\frac{1-w^2}{s^2}=c_{3,3};$$

$$\frac{vw}{s^2}=c_{2,3}=c_{3,2};\qquad \frac{uw}{s^2}=c_{1,3}=c_{3,1};\qquad \frac{uv}{s^2}=c_{1,2}=c_{2,1}.$$

To the three equations thus formed correspond three other analogous equations, derived from these by changing $\bar{\xi}_0$, $\bar{\eta}_0$, $\bar{\zeta}_0$ into $\bar{\xi}'_0$, $\bar{\eta}'_0$, $\bar{\zeta}'_0$, and $\bar{\xi}'_0$, $\bar{\eta}'_0$, $\bar{\zeta}'_0$ into $-\bar{\xi}_0$, $-\bar{\eta}_0$, $-\bar{\zeta}_0$. The six components are determined by these six equations, and the velocity s results from the determinant

$$\left.\begin{array}{cccccc} 0 & f & -e & c_{1,1} & c_{1,2} & c_{1,3} \\ -f & 0 & d & c_{2,1} & c_{2,2} & c_{2,3} \\ e & -d & 0 & c_{3,1} & c_{3,2} & c_{3,3} \\ -c_{1,1} & -c_{1,2} & -c_{1,3} & 0 & f & -e \\ -c_{2,1} & -c_{2,2} & -c_{2,3} & -f & 0 & d \\ -c_{3,1} & -c_{3,2} & -c_{3,3} & e & -d & 0 \end{array}\right\} = 0. \quad (18)$$

This is a left symmetric determinant, and may therefore be put equal to H². If H be now determined according to the known method, we get from the equation H=0,

$$c_{1,1}\,c_{2,2}\,c_{3,3}+2c_{2,3}\,c_{3,1}\,c_{1,2}-c_{1,1}\,c_{2,3}{}^2-c_{2,2}\,c_{3,1}{}^2-c_{3,3}\,c_{1,2}{}^2$$
$$=c_{1,1}\,d^2+c_{2,2}\,e^2+c_{3,3}f^2+2c_{2,3}\,ef+2c_{3,1}fd+2c_{1,2}de. \quad (19)$$

When d, e, and f are $=0$, we get from this equation the same expression for s (10) as that previously found. In general, however, these are small quantities of the same order as α_p; the right-hand member of (19), which we will denote by q^2, is accordingly not a small quantity of the second degree; and the previously found value of s will in general admit of development according to powers of q^2, and will therefore undergo only an *imperceptible* increase. This, however, becomes no longer possible if the left-hand member of (19) becomes *quadratic*, or differs only very little from a square; for in this case we should be able to take the roots on both sides and to develope s by powers of $\pm q$. This case occurs when we put $v=0$, and take $a>b>c$, or $a<b<c$. The left side of equation (19) then becomes

$$\left(\frac{1}{b^2}-\frac{1}{s^2}\right)\left[\frac{1}{a^2c^2}-\frac{1}{s^2}\left(\frac{u^2}{a^2}+\frac{w^2}{c^2}\right)\right],$$

and this expression, if

$$u^2=\frac{a^2-b^2}{a^2-c^2}, \quad w^2=\frac{b^2-c^2}{a^2-c^2},$$

becomes equal to the square of $\frac{b}{ac}\left(\frac{1}{b^2}-\frac{1}{s^2}\right)$.

By putting the approximate value of s, namely $s=b$, on the right of equation (19), we get

$$\frac{1}{b^2}-\frac{1}{s^2}=\pm\left(\frac{c^2}{b^2}\sqrt{\frac{a^2-b^2}{a^2-c^2}}\,d+\frac{a^2}{b^2}\sqrt{\frac{b^2-c^2}{a^2-c^2}}f\right). \quad (20)$$

The velocity s thus receives two notably different values in the case of the perpendicular to the plane of the wave coinciding with the optic axis of the crystal; for the assumed values of u, v, w are precisely those of the cosines of the angles which the optic axis makes with the axes of coordinates.

In passing on now to the calculation of the amplitude of the excursion, we must bear in mind that, so long as we only consider the cases in which the value of the exponent p is either 0 or 2, it makes no essential difference which of these values be taken in applying the generalized form of Fresnel's formula, since the plane of polarization is the same in both cases. Still, inasmuch as in one case ($p=2$) the vibrations take place in the plane of the wave, this gives the simplest results, and we will accordingly confine ourselves to the consideration of this case.

Taking the values that have been assumed for u, v, w, and applying the symbols ξ_0, η_0, ζ_0 that have been previously used, we get from the equations (C)

$$\left.\begin{aligned} \xi_0 &= \frac{1}{a^2}\bar{\xi}_0 + f\bar{\eta}'_0 - e\bar{\zeta}'_0 = \frac{w}{s^2}(w\bar{\xi}_0 - u\bar{\zeta}_0), \\ \eta_0 &= \frac{1}{b^2}\bar{\eta}_0 - f\bar{\xi}'_0 + d\bar{\zeta}'_0 = \frac{1}{s^2}\bar{\eta}_0, \\ \zeta_0 &= \frac{1}{c^2}\bar{\zeta}_0 + e\bar{\xi}'_0 - d\bar{\eta}'_0 = -\frac{u}{s^2}(w\bar{\xi}_0 - u\bar{\zeta}_0); \end{aligned}\right\} \quad . \quad . \quad (21)$$

in addition to which we have analogous equations formed from these by changing the accented factors for unaccented factors, and the unaccented for accented factors with opposite signs.

Since d, e, and f are small quantities, we may substitute for $\bar{\xi}'_0$, $\bar{\eta}'_0$, $\bar{\zeta}'_0$ in (21) their approximate values, namely $\bar{\xi}'_0 = a^2\xi'_0$, $\bar{\eta}'_0 = b^2\eta'_0$, $\bar{\zeta}'_0 = c^2\zeta'_0$, whereby the following equations are obtained,

$$\begin{aligned} \frac{s^2}{w}\xi_0 = -\frac{s^2}{u}\zeta_0 &= wa^2(\xi_0 - fb^2\eta'_0 + ec^2\zeta'_0) \\ &\quad - uc^2(\zeta_0 - ea^2\xi'_0 + db^2\eta'_0)\left(\frac{1}{b^2} - \frac{1}{s^2}\right)\eta_0 \\ &= -\frac{c^2}{b^2}d\zeta'_0 + \frac{a^2}{b^2}f\xi'_0, \end{aligned}$$

together with three other analogous equations formed in the way already indicated.

All these equations are solved by the following values:

$$\left.\begin{aligned} &\xi_0 = 0, &&\eta_0 = \mathrm{A}, &&\zeta_0 = 0, \\ &\xi'_0 = \pm w\mathrm{A}, &&\eta'_0 = 0, &&\zeta'_0 = \mp u\mathrm{A}; \end{aligned}\right\} \quad . \quad . \quad (22)$$

since these values either lead to identical expressions or back to equation (20), it being always understood that d, e, f stand for small quantities.

If r denotes the space moved through within the crystal by the plane wave, the components of the excursion for the case $p=2$ will be given by the equations

$$\left.\begin{aligned} \xi &= \pm w\mathrm{A} \sin k\left(t-\frac{r}{s}\right), \\ \eta &= \mathrm{A} \cos k\left(t-\frac{r}{s}\right), \\ \zeta &= \mp u\mathrm{A} \sin k\left(t-\frac{r}{s}\right). \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (23)$$

The ray divides itself accordingly into two circularly polarized rays, which advance with somewhat unequal velocities. These latter may be deduced from (20); thus

$$s_1 = b(1+\tfrac{1}{2}uc^2d+\tfrac{1}{2}wa^2f),$$
$$s_2 = b(1-\tfrac{1}{2}uc^2d-\tfrac{1}{2}wa^2f),$$

where s_1 corresponds to the upper sign in (23), and s_2 to the lower sign. In the latter case the circular polarization is *right-handed*, because $\frac{d\eta}{dt}$ and $\frac{\xi}{u}$ then have contrary signs.

When both rays have advanced by the distance r, the plane of vibration has rotated through the angle

$$\frac{k}{2}\left(\frac{r}{s_1}-\frac{r}{s_2}\right) = -\frac{kr}{2}(uc^2d+wa^2f). \quad . \quad . \quad . \quad (24)$$

The rotation is thus proportional to the distance traversed, and is towards the right when d and f are negative. Moreover, since k is equal to $s\frac{2\pi}{\lambda}$, the rotation is nearly proportional to $\frac{1}{\lambda^2}$, or to the inverse square of the wave-length.

The transition is easy from the general case here treated to the particular cases of optically uniaxal and isotropic bodies. Circular polarization has been really demonstrated only in these two cases; but the hope of detecting it in biaxal crystals also ought not to be given up, especially since this property escaped observation till lately in several uniaxal crystals (as chlorate of soda, sulphate of strychnine, cinnabar, &c.).

In nature, circular polarization appears as the exceptional case, while in the mathematical treatment of the subject it appears as the most general case. This results from the symmetry which pervades all nature, whereby the constants Δ become nothing, and consequently d, e, f disappear also. And even were this not the case, in a perfectly symmetrical arrangement these latter quantities would be made up of terms which would mutually destroy each other. Circular polarization therefore presupposes a want of symmetry, which in the calculation is the more general, in nature the rarer case.

The *conclusion* to which the present investigation leads is, *that simply by the further development of the formal side of the laws of light, that is, the laws of refraction and reflexion by isotropic transparent bodies,* without doubtful hypotheses, and in fact by keeping to just the most general form, *it is possible to arrive at a complete theory of double refraction, of chromatic dispersion, and of circular polarization.*

A step further may still be made in the direction we have here entered upon, and I will briefly point out how. The velocity of light is regarded as a function of x, y, and z; it may, however, be taken still more generally as a function of the time t, for it is plainly a limitation to suppose the particles of the body originally at rest. This can be easily introduced into the calculation by giving to ρ_p the value

$$\frac{k_p t + a_p x + b_p y + c_p z + d_p}{\alpha_p}.$$

Since, however, the differential equations (A) were not formed on this supposition, they cannot in this case be taken as a safe basis for the calculation, and the more generally valid differential equations would have to be deduced in another way.

XXX. *On the Radiation and Absorption of Gases.* *By* Balfour Stewart, *M.A., F.R.S.**

As the relation of radiant heat to gases and vapours is at present engaging the attention of two very eminent experimentalists, and is also a subject of much importance in meteorology, the attempt, however imperfect, to discuss it from a different point of view may not be without interest.

It would appear to be established that the spectra of gases preserve to a great extent the same character or type through a considerable range of temperature. We know that this is the case with the vapour of sodium, which consistently absorbs the double line D; and we know also that the characteristic line of thallium is always prominent in its spectrum, although, when the temperature becomes very high, other but weaker lines are added. Coupling this with the proposition which asserts that the absorptive power of a body for any kind of heat is propor-

* Communicated by the Author.

tional to its radiative power, it would seem very probable that for many gases and vapours a sensible or even a large proportion of the heat absorbed is that which corresponds to a few definite and disconnected wave-lengths.

To take a particular case—suppose that we wished to ascertain the absorptive power of sodium-vapour for the heat corresponding to that portion of the spectrum around the double line D. If the spectrum be a continuous one, we are quite certain that the sodium-vapour will select the two rays forming the well-known double line, which will be absorbed with great avidity, while the remainder of the heat will be allowed to pass. Also a small quantity of sodium-vapour would be sufficient to do this, and a large quantity would do no more. Perhaps a foot of vapour would absorb no more than an inch, at least sensibly no more. If, therefore, we wish to ascertain the absorptive power of sodium-vapour, we must not compare one foot of vapour with one inch, but one foot or one inch of vapour with a total absence of all vapour.

The same line of reasoning must, I imagine, be extended to all gases and vapours; nor do I think that the absorptive power of any gas for radiant heat can be considered as determined until the gas which is interposed as a screen between the source of heat and the instrument which measures the radiation be compared with a total absence of all such gas: at any rate that tenuity ought to be reached for which an increase of gas produces an increase of absorption.

I would next remark that there is probably a connexion, of the precise nature of which we are ignorant, between the kind of heat radiated by gases and vapours, and the boiling-points[*] of the liquids which give rise to these.

To make an extreme supposition, let us suppose that sodium-vapour does not radiate any heat of a lower refrangibility than the double line D. If we suppose at the same time that heat of this refrangibility is entirely absent in the radiation of low temperatures, then we cannot imagine sodium-vapour to exist at these temperatures. If these suppositions be considered extreme, nevertheless they suggest to the mind the probability of a connexion of the kind indicated above, and lead us to imagine that gases which radiate heat of a high refrangibility belong to substances which have high boiling-points.

Now, of all known substances, oxygen, hydrogen, and nitrogen have the lowest boiling-points, indeed we have not yet been able to liquefy these gases; so that, according to the above hypothesis, we should associate the description of heat radiated by

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[*] The word "boiling-point" is here used as a single word which sufficiently represents the meaning to be conveyed.

these gases with that which belongs to an extremely low temperature, or, in other words, with heat of a very low refrangibility or of very great wave-length. Of course I do not imagine that this hypothesis has been proved, but certainly think there is a probability that some such connexion exists.

Again, with respect to the kind of heat which is radiated and absorbed by rock-salt, I have endeavoured* to show that this is heat of very great wave-length. It may therefore be to some extent the same description of heat as that which I have supposed to be radiated and absorbed by the three permanent gases, which, in conformity with the above hypothesis, is probably heat of great wave-length.

I would further remark that, in any arrangement where a source of heat such as boiling water is used, and where the other parts of the apparatus are at a temperature, let us say of 60° F., the heat which is operated upon is not the radiation of 212° F., but the radiation of 212° F. *minus* the radiation of 60° F. Now, since the average wave-length of the radiation diminishes as the temperature increases, it is easily seen that the radiation of 212° F. *minus* the radiation of 60° F. has a less average wave-length than the whole radiation of 212° F. Indeed by such an arrangement we separate from the whole body of heat which, by Prevost's theory, is radiated by boiling water, that portion of it which possesses the smallest wave-length or greatest refrangibility, and upon this heat we make our experiments.

Let us now consider how atmospheric air is situated with respect to an arrangement in which the source of heat is boiling water, and the temperature of the thermo-pile 60° F., and in which the vacuum-tube is closed with plates of rock-salt, while it is presumed that there is *absolutely* no atmospheric air in the arrangement except that which is the subject of experiment.

1. If it be supposed that the heat which atmospheric air radiates and absorbs is that of a very low refrangibility, it is evident that the total radiation and absorption of such air will be less increased by raising the temperature from 60° F. to 212° F. than those of another gas which radiates heat of a high refrangibility—the reason of this being that the rays between 60° F. and 212° F. are of a comparatively high refrangibility, and that atmospheric air radiates and absorbs these only to a small extent.

2. Moreover, part of the radiant heat is stopped by the plate of rock-salt; but it is possible that many of the rays which rock-salt stops are those which are also stopped by atmospheric air. The rock-salt would therefore in this case sift the radiant heat,

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* Transactions of the Royal Society of Edinburgh, vol. xxii. part 1, p. 63.

and reduce it to that quality which is passed without absorption by atmospheric air.

On the whole, therefore, if we consider the above hypothesis as at all probable, such an arrangement cannot, I think, be viewed as a favourable one for exhibiting the absorptive power of atmospheric air, the effect being apparently to diminish this property of air.

Let us now, on the other hand, reflect how the radiation of aqueous vapour is influenced by the same arrangement. According to hypothesis, the heat radiated and absorbed by aqueous vapour possesses a comparatively high refrangibility. A considerable portion of it will therefore be included in the radiation between 60° F. and 212° F., and this will not be stopped by rock-salt. The effect of the arrangement will therefore be apparently to increase the radiation of aqueous vapour, and also of many of those vapours which possess high boiling-points.

Finally, suppose that we introduce into the vacuum-tube a mixture of air and of a vapour possessing a high boiling-point. Here it is evident that while little or no heat will be absorbed by the first of these substances, owing to the peculiarity of the arrangement, a great deal will be absorbed by the second, and the result will be that air *plus* vapour will appear much more athermanous than air alone.

Owing to these considerations, I should hesitate to conclude that the results hitherto obtained regarding the radiative and absorptive powers of gases and vapours afford ground for that hypothesis which asserts that the absorptive and radiative powers of simple bodies are less than the corresponding qualities of compound bodies. But, inasmuch as aqueous vapour would appear to absorb a different description of heat from atmospheric air, the very important meteorological conclusion deduced by Prof. Tyndall would still appear to hold good on the present hypothesis; that is to say, air with vapour will probably stop more of the heat radiated by the earth than air without vapour.

It is even possible that there may be an additional safeguard against the cooling of our globe by radiation into space, in the fact that when the temperature falls very low and there is scarcely any vapour in the air, the heat radiated is then of a description which is stopped by dry air, which would thus serve as a cloak or covering, performing the same office as that which is performed at a higher temperature by air loaded with aqueous vapour.

Kew Observatory,
August 19, 1863.

XXXI. *Concluding Remarks on a recent Mathematical Controversy. By* His Honour James Cockle, *M.A., of Trinity College, Cambridge, F.R.A.S, F.C.P.S. &c., The Chief Justice of Queensland**.

AMONG mathematicians there are those who will lend but an academic faith to Mr. Jerrard's assertion that he has succeeded in rescuing from the class of impossible problems the noted problem of equations. His theory is erroneous, unsupported by calculations of his own, and at variance with the results of the calculations of others. Mr. Jerrard may regard article 4 of his paper of December 1862 (I refer to the date of the Number of this Journal in which the paper appeared) as a sufficient answer to me, but I do not so consider it; and mathematicians will form their own opinion as to whether objections which I have urged against Mr. Jerrard's analysis are not fatal to his theory. Mr. Cayley's objections Mr. Jerrard only attempts to answer by general observations in articles 1 and 2 of his paper, and by a verbal criticism in article 3,—article 2 consisting in great part of a repetition of a fallacious argument, the use of which leads me, I confess, to the conclusion that Mr. Jerrard has misapprehended Lagrange's theory of similar functions. Following an analogous method to that pursued by Mr. Jerrard, we might dispute the validity of any mathematical proposition whatever on such grounds as these:

$$x=x, \quad \therefore x-x=(1-1)x=0,$$

$$\therefore x=\frac{0}{1-1}=\frac{0}{0},$$

and all formulæ into which x enters are illusory. If Mr. Jerrard were to form a sextic of which the roots should be the fifth powers of those of the sextic in θ discussed by me, and verified by the independent processes of Mr. Harley, the sextic so formed would (according to Mr. Jerrard, I mean) depend directly on an Abelian equation, and therefore involve in its solution quadratic and cubic radicals only. If Mr. Jerrard should refuse to test his theory by forming and trying to solve the latter sextic, there may be no impropriety in suggesting the following test. Assuming the coefficients at his pleasure, let Mr. Jerrard construct an irreducible quintic into a root of which an irreducible cubic surd shall enter. That the former test will not have for its

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* Communicated by the Author.

issue the solution of the sextic is manifest from the circumstance that the expression

$$\theta_1^5\,\theta_4^5+\theta_2^5\,\theta_6^5+\theta_3^5\,\theta_5^5$$

is a rational function of one root only of the quintic, and consequently that the root of the quintic will contain no quintic surds. That the expression in question is such a rational function may be inferred from the foot-note to art. 88 of my paper of March 1860, from the third division of my "Notes on the Higher Algebra" in the 'Quarterly Journal of Mathematics,' June 1860, or more directly from my "Supplementary Researches in the Higher Algebra" in the first volume of the third series of the 'Manchester Memoirs.' Mr. Jerrard has not avoided a difficulty which, as he has pointed out in his paper of January 1846, must arise; nor has he given any definite enunciation of the theorem to which, at least according to his paper of February 1846, that of Cauchy must yield its place. On the cover of this Journal for November 1860 Mr. Jerrard announced that he was preparing for the press an Appendix (to his 'Essay') relating to the mode in which cubic radicals enter into the expressions for the roots of equations of the fifth degree. Until that Appendix shall have appeared, I retire from a controversy the continuance of which seems only to elicit from Mr. Jerrard a repetition of refuted arguments. However much I may differ from Mr. Jerrard on certain points, I repeat my acknowledgements of the instruction and advantage which I have derived from the study of his writings.

Brisbane, Queensland, Australia,
June 16, 1863.

P.S.—The writer desires to add that he hopes shortly to forward a paper extending the correlations, between algebra and the differential calculus, implied in the term differential critical function, to other functions for which he will suggest the name "differential covariants." He also adds that, to the best of his recollection, the first step in the theory of six-valued functions of six letters, commented on in Prof. Sylvester's paper of May 1861, was made, in the Paris *Mémoires* for 1771, by Vandermonde, who also, in the paper just alluded to, gave a cyclical process, connected with that the working powers of which have been so admirably developed by Mr. Harley.—Brisbane, June 18, 1863.

XXXII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 155.]

December 18, 1862.—Major-General Sabine, President, in the Chair.

THE following communications were read :—

"Description of a new Specimen of *Glyptodon*, recently acquired by the Royal College of Surgeons of England." By Thomas Henry Huxley, F.R.S. &c.

"Distribution of the Surface of the Third Order into Species, in reference to the absence or presence of Singular Points, and the reality of its Lines." By Dr. Schläffle.

"Experimental Investigations on the Stratified Appearance in Electrical Discharges."—"Effect obtained by varying the resistance." By John P. Gassiot, F.R.S.

January 8, 1863.—Major-General Sabine, President, in the Chair.

The following communications were read :—

"Applications of the Theory of the Polyedra to the Enumeration and Registration of Results." By the Rev. Thomas P. Kirkman, M.A., F.R.S. &c.

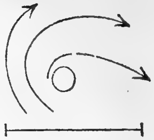
"Contributions towards the History of the Monamines.—No. VI. Note on the Action of Iodide of Methyle on Ammonia."—No. VII. "Transformation of Aniline into Benzoic Acid." By A. W. Hofmann, LL.D., F.R.S.

"A Development of the Theory of Cyclones." By Francis Galton, F.R.S.

Most meteorologists are agreed that a circumscribed area of barometric depression is usually a locus of light ascending currents, and therefore of an indraught of surface winds which create a retrograde whirl (in our hemisphere), because they bring to their destination a lateral impulse, partly due to the greater easterly speed of the earth's surface whence the southern portion of the indraught took its departure, and partly due to the less easterly, or we may say greater westerly, speed of its northern portion.

Conversely, we ought to admit that a similar area of barometric elevation is usually a locus of dense descending currents, and therefore of a dispersion of a cold dry atmosphere, plunging from the higher regions upon the surface of the earth, which, flowing away radially on all sides, becomes at length imbued with a lateral motion due to the above-mentioned cause, though acting in a different manner and in opposite directions. The currents necessarily travel with diminished radial speed as they widen out from their central area of dispersion, and the eastward tendency of the northern portion of the system and the westward tendency of the southern become more overpowering. It may be presumed, on consideration of the extreme mobility of the air, that a continuous dispersion of currents would result in the yielding of the east and west winds, which had no tangential movement of their own, to the curvature of the others, and that we should witness a disposition of currents like those in the

Fig. 1.

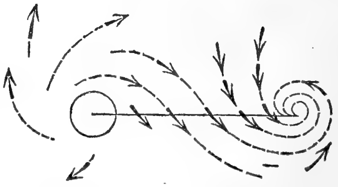


annexed diagram, which is copied from an actual occurrence on December 2, 1861. The appearance is that of a centre of calms whence currents flow in radial lines, rapidly curving to the right and forming a sort of "anticyclone."

Dove's law of gyration is so fertile in result, that it accounts for the same direct rotation of a cold wind by a wholly different process. As an antithesis to his theory of cyclones being due to an equatorial current pressing against quiescent air, he adds (Law of Storms), with a view of illustrating his position, and not of meeting cases that practically occur, polar cyclones, "if they exist," would have a direct rotation. It is not necessary to allude further to his well-known theory—it is sufficient to show that two separate causes cooperate in producing a rotation or curvature of currents such as I have described. I have not the slightest doubt that a strong curvature of atmospheric currents to the right does frequently exist, owing to the descent of cold air from above; for in lately charting the weather of Europe thrice daily during a month, I found it more or less present on from fifty to sixty occasions. Its existence is consonant to what we should expect. It is hardly possible to conceive masses of air rotating in a retrograde sense in close proximity, as cyclonogists suppose, without an intermediate area of direct rotation, which would, to use a mechanical simile, be in gear with both of them, and make the movements of the entire system correlative and harmonious.

The result I have thus far arrived at, and which I should look for hereafter, is that whenever the barometer shows circumscribed areas of marked elevation and depression at distances not exceeding 1500 miles apart, a line drawn from the locus of highest to that of lowest barometer would be cut by parallel wind-currents at an angle of about 45°, in the way shown in the diagram.

Fig. 2.



I doubt if it be of advantage to investigate the changes of wind produced by a system of indraught and dispersion passing over any locality, because the barometrical sections vary so rapidly as to make the incoming portion unsymmetrical with that which has already passed over.

"On the Immunity enjoyed by the Stomach from being digested by its own Secretion during Life." By Frederick W. Pavy, M.D.

January 15.—Major-General Sabine, President, in the Chair.

The following communications were read :—

"Notes of Researches on the Poly-Ammonias.—No. XXII. Secondary Products formed in the Manufacture of Aniline." By A. W. Hofmann, LL.D., F.R.S.

"On the Form of Crystals of Peroxide of Benzoyle." By William Hallows Miller, M.A., For. Sec. R.S. &c.

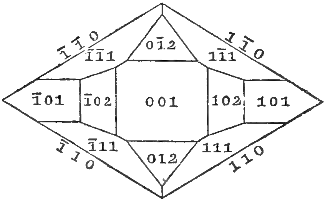
The peroxide of benzoyle, $C_{14} H_{10} O_4$, or carbon 69·42, hydrogen 4·13, oxygen 26·45, was discovered by Professor (now Sir B. C.) Brodie, and described by him in Phil. Mag. S. 4. vol. xvii. p. 301. The crystals were obtained from a solution in ether of the product of the mutual decomposition of equivalent proportions of chloride of benzoyle and peroxide of barium mixed in water. The faces of the crystals, though brilliant, were not very even, so that, in order to obtain a moderately accurate result, it was necessary to measure a large number of crystals. The column headed 'observation' contains the means of the observed angles; the column headed 'calculation,' the most probable values of the angles, taking into account the quality of the faces containing them, and the number of the observations in each case.

System prismatic:—

1 0 1, 0 0 1=33° 24′; 1 1 0, 1 0 0=57° 50′·5.

Observed forms:—

0 1 0, 0 0 1, 0 1 2, 1 0 2, 1 0 1, 1 1 0, 1 1 1.



Angles.	Calculated.		Observed.	
	°	′	°	′
0 1 2, 0 $\bar{1}$ 2	55	21	55	20
1 0 1, $\bar{1}$ 0 1	66	48	66	49
1 0 2, $\bar{1}$ 0 2	36	30		
0 1 0, 0 0 1	90	0		
0 0 1, 1 1 0	90	0		
1 1 0, $\bar{1}$ 1 0	64	19	64	18
1 1 1, $\bar{1}$ 1 1	48	56		
1 1 1, 1 $\bar{1}$ 1	82	24		
1 1 1, 1 1 $\bar{1}$	77	49		
0 0 1, 1 1 1	51	5	51	3
1 1 1, 0 1 2	30	26		
1 0 1, 0 1 2	42	19		
1 0 1, 1 1 1	41	12		
1 1 0, 0 1 2	66	51		
1 1 0, 1 0 1	72	58		

No cleavage observable.

The minimum deviations of the brightest part of the solar spectrum were observed through the faces 0 1 2, 0 1 $\bar{2}$, through the faces

1 0 1, 1 0 $\bar{1}$, and through the faces 1 1 0, 1 $\bar{1}$ 0, the crystal being immersed in water contained in a vessel bounded by plates of glass parallel to the plane bisecting the dihedral angle formed by the refracting faces in each case. From these observations it appears that for a ray in the plane 1 0 0, and polarized in that plane, the index of refraction is about 1·837; for a ray in the plane 0 1 0, and polarized in that plane, the index of refraction is between 1·545 and 1·546; and for a ray in the plane 0 0 1, and polarized in that plane, the index of refraction is about 1·545. Hence the optic axes are in the plane 0 1 0, and they make with each other a small angle which is bisected by the line [1 0 0].

A crystal having two opposite faces of the form 1 1 0 much larger than the two remaining faces, being immersed in oil for which $\mu=1{\cdot}4793$, and placed in a polarizing apparatus, the rings surrounding the optic axes were seen through the large faces of the form 1 1 0. The angle included between the directions of the optic axes within the oil was about 4°.

"On the Synthesis of Leucic Acid." By Dr. Edward Frankland, F.R.S.

"On the Artificial Production of Fibrine from Albumen." By Alfred Hutchinson Smee, Jun.

"Note on the Spectrum of Thallium." By Professor William Allen Miller, M.D., LL.D., Treasurer and V.P.R.S.

My friend Mr. Crookes, the discoverer of the new metal thallium*, having kindly put into my hands a small quantity of the metal, which he believes to be chemically pure, I have been enabled to make some experiments upon its spectrum, the results of which may not be without interest to the members of the Royal Society.

Thallium, as is well known, when examined in the usual way by the spectroscope, yields a spectrum of remarkable simplicity, furnishing a single intense green line, the occurrence of which, as is familiar to chemists, led Mr. Crookes to the discovery of the metal, and suggested to him the name by which it is known. In order to try the effect of a progressively increasing temperature upon the spectrum furnished by the metal and its compounds, the following experiments were made.

Portions of metallic thallium, as well as of an alloy formed by fusing a bead of thallium upon the end of a platinum wire, and portions of the sulphate of the metal were introduced successively, first, into the flame of burning hydrogen, and then into the oxyhydrogen jet, and were in each case viewed by the spectroscope. As the temperature increased in intensity, the brilliancy of the thallium green line increased also, but no new lines made their appearance.

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* It has been made the subject of question abroad, whether Mr. Crookes or M. Lamy was the first to recognize the metallic nature of thallium, and thus to dispute the claim of Mr. Crookes to the full credit due to him for his investigation (with only about twenty grains of the element) of its leading characters where no previous clue existed to guide him. It may be sufficient to state in answer to this suggestion, that Mr. Crookes had exhibited it at the International Exhibition, and marked as *metallic* his scanty store, though in the form of a precipitate, in the beginning of May, unquestionably before M. Lamy had published anything relating to thallium.

Two pieces of stout thallium wire were then arranged as electrodes to the secondary wire of an induction coil. A continuous torrent of sparks was maintained without melting the wires or producing very rapid oxidation, or volatilization of the metal; the light, however, was much whiter than its ordinary monochromatic character would have led us to expect. Mr. Crookes, who was with me during the experiments, projected the image of the points by means of a lens upon a distant white screen, when it was at once obvious that the extremities of the spark were of a fine green colour, whilst the flickering luminous arc, which filled up the interval, due chiefly to ignited air, was much whiter.

On viewing the sparks from the induction-coil by the spectroscope, several new lines, independently of well-marked air-lines, made their appearance. These lines were distinguished from air-lines by the peculiar character which distinguishes most metallic lines, viz. the much greater intensity of their extremities than of their central portions. Besides the usual intense line in the green, five others were particularly observable: first, a very faint one in the orange; next, two of nearly equal intensity in the green, more refrangible than Tlα, with a third much fainter, these three lines in the green being nearly equidistant; whilst, 5th, in the blue was a bright well-defined line: all these were strong at each extremity and evanescent in the central portions.

The induction-spark of thallium was then observed when produced in a current of hydrogen gas. The air-lines disappeared, the peculiar lines of hydrogen were very manifest, particularly the line in the red and one of the lines in the blue; whilst the new thallium lines were preserved, with the exception of the feeblest, though all were reduced in intensity.

Finally, a photographic impression of the thallium spectrum upon collodion was obtained by the method which I have described in a paper communicated to the Royal Society in June last. An impression extending to about division 154 of the scale then adopted was obtained. This spectrum contains several very characteristic groups of lines; it recalls the features of the spectra of cadmium and zinc, and less strongly that of lead.

Measuring by the scale already adopted in my former paper, it is found that there are two strong groups of lines at about 103 and 106. At 116, 121, and 126 are three groups—the first two less intense than the third, which is of about the same strength as the earliest two. Several feebler pairs of dots follow, and the spectrum terminates rather abruptly with four nearly equidistant groups, commencing respectively at 136, 141, 145, and 151. The first of these groups is very strongly marked, the others are fainter, but of nearly equal intensity.

The remarkable way in which a spectrum at low temperatures so simple becomes increased in complexity, both in the visible and in the extra-visible portions, is of high interest considered in relation to the physical cause of these phenomena; and it is not without interest in a chemical sense, from its bearing upon the view supported by Dumas, that thallium belongs to the alkaline group.

Potassium and sodium exhibit no new lines in the induction-spark, merely a diffuse light filling up the air-lines, and lithium but a single strong group at about 124. This physical character, added to the more purely chemical ones of the insolubility of the sulphide, the chromate, the iodide, the sparing solubility of the chloride, the phosphate, the oxalate, the ferrocyanide, the occurrence of a powerfully basic oxide, and of a higher feebly acid oxide, may therefore assist in showing the resemblance of thallium to silver or to lead, which latter metal in density, colour, softness, and external appearance it so closely simulates.

It would be easy to point out other particulars in which the properties of thallium are in strong contrast with those of the alkali metals. The chemical energy of these metals, lithium, sodium, potassium, rubidium, and cæsium, increases in the order mentioned, which is that of their equivalents. Thallium, with a higher equivalent than any of these, shows a greatly diminished chemical activity. The metal is readily reduced by zinc from its solutions. Its oxide, instead of being like that of all the alkalies, excessively deliquescent, is permanent in air, and forms a closely adhering coat like that which is produced upon the surface of zinc or lead, protecting the metal beneath from further change.

In many points the chemical reactions of thallium resemble those of silver, to which metal it is also further approximated by the circumstance that the atomic heat of the metal, like that of silver, is double that of the series to which lead belongs. Although therefore in other physical properties thallium differs greatly from silver, it seems to be more closely allied to that metal than to any other.

January 22.—Major-General Sabine, President, in the Chair.

The following communications were read:—

"Researches on some of the Artificial Colouring Matters.—No. I. On the Composition of the Blue Derivatives of the Tertiary Monamines derived from Cinchonine." By A. W. Hofmann, LL.D., F.R.S.

The chemical visitors of the International Exhibition will not easily forget the magnificent collection of products displayed in the French court by M. Menier of Paris. Among these compounds, equally remarkable for their variety and beauty, the large crystals of cyanine, rivalling in splendour and purity Mr. Nicholson's acetate of rosaniline, have attracted general attention. M. Menier, who has produced this new dye on a very large scale, has most liberally placed at my disposal some of the finest of these crystals for examination, hoping that their more minute investigation might perhaps lead to a method of giving solidity to this new colour, which in brilliancy and purity of tint is second to none of the several blues lately derived from coal-tar. The composition of cyanine and its mode of formation having hitherto remained unknown, I have gladly availed myself of this opportunity of performing some experiments with this interesting compound. I am sorry to say that, in a practical point of view, these experiments have failed entirely; but my studies have led me to some observations on this substance which, as a contribution to the history of cyanine, deserve to be recorded, and which I beg leave to communicate to the Royal Society.

The discovery of the blue compounds from chinoline and its homologues dates as far back as 1856. In that year Mr. G. Williams engaged in a renewed examination of the base extracted by Runge from coal-tar and obtained by Gerhardt from the alkaloids of the cinchona bark, the identity in composition of which I had established in one of my earlier researches. Among the numerous compounds of these bases most carefully examined by Mr. G. Williams on this occasion, were also their methylated and ethylated derivatives, one of which, the iodide of methyl-leucolylammonium, I had discovered when studying the action of iodide of methyl upon ammonia and its analogues. It was in preparing this compound from the chinoline obtained by the distillation of cinchonine, and in separating the ammonium-base corresponding to the iodide by means of oxide of silver, that Mr. Williams first observed the splendid coloration which has led him to the discovery of the new dye now commercially known under the name of cyanine. Precisely similar phenomena were subsequently (in 1857) observed by M. von Babo, who produced them by treating chinoline with the sulphates of methyl and ethyl, and described the coloured substances thus obtained as methylirisine and ethylirisine. Mr. Williams was inclined to attribute the formation of the blue compounds, in which he recognized distinctly basic properties, to a process of oxidation; M. von Babo represents his methyl- and ethylirisine, although with very great reserve, by the formulæ

$$C_{11}H_{14}N_2O_2 \quad \text{and} \quad C_{13}H_{18}N_2O_2.$$

No attempt has since been made to establish the composition of these singular compounds by a more minute examination. In fact several years elapsed without any further notice being taken of them, until the development of the aniline industry revived the memory of these remarkable colour phenomena, which have since attracted the general attention of dyers and printers. Mr. G. Williams showed that, among the several coloured compounds produced by the action of iodides of alcohol radicals upon chinoline bases, the one obtained by means of iodide of amyl is particularly rich in tinctorial power; he has given a very interesting account of this new dye, and accurately described the mode of manufacture of this body, which, under the name of cyanine, soon became an article of commerce.

Unfortunately the tint produced by cyanine is less fast than beautiful, and the hopes entertained of the industrial future of the new compound has not been realized; nevertheless the importance attached by dyers to Mr. Williams's discovery is well marked by the fact of a gold medal, together with a prize of 10,000 francs, having been proposed for the discovery of a means of rendering stable the beautiful colours dyed by cyanine.

The crystals submitted to me for examination by M. Menier were distinct prisms, sufficiently well formed for crystallographical determinations. They are at present in the hands of Quintino Sella. Their substance possesses a beautiful green metallic lustre with a golden tint, by which, as well as by crystalline form, they are readily distinguished from acetate of rosaniline, which they in other respects much resemble. The crystals are insoluble in anhydrous ether, difficultly soluble in water, but dissolve readily in alcohol. The solution has a magnificent blue colour, with a coppery iridescence on its surface. Addition of acids destroys this colour. Ammonia and the fixed caustic alkalies leave the colour apparently untouched; but it is now produced by a finely divided deep-blue precipitate suspended in the liquid, which may be filtered off, the filtrate separated from it being colourless.

The green crystals were found to be the iodide of a peculiar basic compound. The iodine is rather firmly held in this compound; but it may be precipitated from the alcoholic solution by oxide of silver, and exchanged for bromine or chlorine by treatment of this solution with bromide or chloride of silver, when the bromide or chloride corresponding to the iodide are produced. The analysis of the crystals gave results indicating unequivocally the formula

$$C_{30}\,H_{39}\,N_2\,I,$$

which received a close confirmation by the examination of a fine platinum-salt crystallizing in rhombic tablets, which is obtained by precipitating the chloride corresponding to the iodide, strongly acidulated with hydrochloric acid, by dichloride of platinum. Nevertheless slight discrepancies between the theoretical values of the formula and the results obtained led me to assume the existence in the crystals of a compound containing less carbon and hydrogen, indeed of a homologous iodide,

$$C_{28}\,H_{35}\,N_2\,I.$$

This hypothesis, not countenanced at first by the remarkable constancy which the composition of the iodide presented even after three or four crystallizations, was fully confirmed when the chloride was submitted to a systematic partial precipitation by dichloride of platinum. After several repetitions of the process, the partially precipitated platinum-salt being decomposed by sulphuretted hydrogen and the chlorides again partially precipitated, two platinum-salts were obtained, one of which, the less soluble one, proved to be the pure platinum-salt corresponding to the iodide with 30 equivalents of carbon, whilst the other one was sufficiently pure to show that it belonged in reality to the homologous iodide with 2 equivalents of carbon less.

The amount of the iodide

$$C_{28}\,H_{35}\,N_2\,I,$$

which contaminated (if the term may be applied to so beautiful a substance) the iodide

$$C_{30}\,H_{39}\,N_2\,I,$$

is, however so small that its presence did not materially influence the analytical results obtained in the further examination of the compound.

The explanation of the formation of the iodide presents no difficulty; this substance obviously derives from lepidine,

$$C_{10}\,H_9\,N,$$

whilst only the slight admixture is due to the presence, in the original bases submitted to the action of iodide of amyle, of a small quantity of chinoline,

$$C_9H_7N.$$

In fact Mr. Williams, in describing the preparation of his dye, distinctly states that the chinoline by no means requires to be pure for the purpose. M. Menier has moreover kindly furnished me with a considerable quantity of the crude material from which the green crystals are obtained. This proved to be a mixture of several bases, in which the presence of lepidine and chinoline was traced without the slightest difficulty, by the analyses of platinum-salts.

In the genesis of the new iodide two different phases have to be distinguished, viz., 1, the transformation of lepidine into iodide of amyllepidyl-ammonium,

$$\underbrace{C_{10}H_9N}_{\text{Lepidine.}} + \underbrace{C_5H_{11}I}_{\text{Iodide of amyle.}} = \underbrace{C_{15}H_{20}NI}_{\text{Iodide of amyl-lepidyl-ammonium.}};$$

2, the condensation under the influence of potash of two molecules of the compound into one molecule of a higher order,

$$\underbrace{2(C_{15}H_{20}NI)}_{\text{Iodide of amyl-lepidyl-ammonium.}} + KHO = \underbrace{C_{30}H_{39}N_2I}_{\text{New iodide.}} + KI + H_2O.$$

It became indispensable to verify these reactions by the analysis of additional compounds.

The green crystals dissolve with facility in boiling dilute hydriodic acid; the colourless solution deposits on cooling yellow needles of remarkable beauty, the analysis of which has furnished the values of the formula

$$C_{30}H_{40}N_2I_2 = C_{30}H_{39}N_2I, HI.$$

These crystals are isomeric with iodide of amyl-lepidyl-ammonium, from which, however, they are distinguished by all their properties. They dissolve in cold water without decomposition, but on addition of alcohol they immediately assume a blue coloration, the original monacid compound being reproduced. The same change takes place at 100°; so that in preparing the compound for analysis it was necessary to dry it *in vacuo*. In the facility with which the diacid compounds are converted into the monacid salts, this substance resembles rosaniline, which, as I have pointed out in a recent paper, forms likewise colourless acid salts of little stability.

The green iodide dissolves with equal facility in hydrochloric and hydrobromic acid, yielding perfectly colourless solutions, and giving rise to the formation of well-crystallized compounds, which contain, in addition to iodine, respectively bromine and chlorine.

On submitting the green iodide in alcoholic solution to the action of chloride of silver, the whole of the iodine is separated in the form of iodide of silver, a blue solution being obtained from which the monacid chloride crystallizes, on slow evaporation, in green metal-lustrous sharply-defined prisms of surpassing beauty. This salt was found to contain

$$C_{30}H_{39}N\ Cl.$$

Dissolved in hydrochloric acid, this salt furnished a diacid compound which, on evaporation *in vacuo*, separates in long straw-coloured needles. The highly deliquescent character of this substance has hitherto prevented me from analysing it; but if there was the slightest doubt of this compound having the composition

$$C_{30}H_{40}N_2Cl_2=C_{30}H_{39}N_2Cl, H\,Cl,$$

it would be dispelled by the analysis of a fine-yellow difficultly soluble platinum-salt crystallizing in small well-defined rhombic plates, which falls directly on addition of dichloride of platinum to the alcoholic solution of the diacid chloride, containing a considerable amount of hydrochloric acid, and which, by analysis, was found to be represented by the formula

$$C_{30}H_{40}N_2Cl_2, 2\,Pt\,Cl_2.$$

The gold-salt is obtained by precipitating the solution of the acid chloride with trichloride of gold, when a yellow, scarcely crystalline precipitate is formed, which, dried *in vacuo*, contains

$$C_{30}H_{40}N_2Cl_2, 2Au\,Cl_3.$$

I have, moreover, prepared the monacid bromide, which forms beautiful metal-lustrous prisms easily crystallizable; the diacid nitrate as a crystalline network, on evaporating a solution of the base in nitric acid *in vacuo*; and, lastly, the acid sulphate, which crystallizes in white, well-formed rhombic tables, very soluble in water, but insoluble in alcohol, by which it is not decomposed like the other diacid compounds.

I have refrained from multiplying the analytical evidences by the minute examination of these salts, because I was happy enough to observe a reaction which supported the interpretation of the results of analysis in an unequivocal manner. Remembering the simple scission which I had formerly accomplished by exposing the iodide of tetrethylammonium to the action of heat, when the compound splits into iodide of ethyle and triethylamine, I was induced to submit the green iodide to distillation. The green crystals rapidly fuse into a blue liquid, the surface of which presents a peculiar coppery lustre. On raising the temperature, decomposition takes place, and in the receiver is condensed a mixture of lepidine and iodide of amyle, the reunion of which to iodide of amyl-lepidyl-ammonium may be prevented by collecting them in hydrochloric acid; at the same time a gas is evolved, burning with a brilliant flame and readily absorbed by bromine, and which could easily be condensed by passing it through a serpentine surrounded with ice. I was thus enabled to collect enough of the volatile hydrocarbon to determine its boiling-point, which proved it to be pure amylene. If the heat be carefully regulated, the amount of charcoal remaining in the retort is comparatively small. The interpretation of the phenomena observed is given in the following equation:—

$$\underbrace{C_{30}H_{39}N_2I}_{\text{Green iodide.}}=\underbrace{2C_{10}H_9N}_{\text{Lepidine.}}+\underbrace{C_5H_{11}I}_{\text{Iodide of amyle.}}+\underbrace{C_5H_{10}}_{\text{Amylene.}}$$

Here, again, I have had an opportunity of proving the presence in the crystals of a small quantity of the homologous chinoline compound; for on submitting, after separating the iodide of amyle, the hydrochlorate of the volatilized base to distillation with potassa, and collecting apart the first quantity of the basic liquid which came over with the vapour of water, this substance proved by the platinum determination to be chiefly chinoline, while the portion of the base distilling last proved by the same mode of analysis to be pure lepidine.

The results obtained in these experiments furnish new illustrations of the tendency to molecular accumulation by which the ammonias and their derivatives are distinguished. Only a few weeks ago I had the honour of submitting to the Royal Society a short account of this class, which is obtained as a secondary product in the manufacture of aniline. The coloured derivatives of the bases of the chinoline series present in their composition considerable analogy with paraniline.

Aniline series.

C_6H_7N Aniline.	C_6H_7N, HCl Chloride.	
$\begin{matrix}C_6H_7N\\C_6H_7N\end{matrix}$ Paraniline.	$\left.\begin{matrix}C_6H_7N\\C_6H_7N\end{matrix}\right\} HCl$ Monacid chloride.	$\left.\begin{matrix}C_6H_7N\\C_6H_7N\end{matrix}\right\} \begin{matrix}HCl\\HCl\end{matrix}$ Diacid chloride.

Lepidine series.

$C_{15}H_{19}NH_2O$ Hydrated oxide of amyl-lepidyl-ammonium.	$C_{15}H_{19}N, HCl$ Chloride.	
$\left.\begin{matrix}C_{15}H_{19}NH_2O\\C_{15}H_{19}NH_2O\end{matrix}\right\} ?$ Free blue base.	$\left.\begin{matrix}C_{15}H_{19}N\\C_{15}H_{19}N\end{matrix}\right\} HCl$ Green iodide, monacid.	$\left.\begin{matrix}C_{15}H_{19}N\\C_{15}H_{19}N\end{matrix}\right\} \begin{matrix}HCl\\HCl\end{matrix}$ Yellow iodide, diacid.

I have written the formulæ of the coloured compounds so as to bring out their analogy with the paraniline salts—in fact, so as to characterize them as para-compounds of the amyl-lepidyl-ammonium salts, but I am far from attributing to these formulæ any other value. In fact the molecular construction of this new class of compounds remains to be established by further experiments.

The theory which (in 1852) satisfactorily represented the constitution of the nitrogen bases then examined, requires an expansion to include the tinctorial ammonias added to our knowledge during the last decade. The time for the enunciation of this amplified theory has not yet arrived.

Here only a few experiments may still be mentioned, which were made with the oxide corresponding with the salts described.

The action of oxide of silver upon the iodide dissolved in alcohol liberates the base, which, on evaporation of the alcohol, separates as an indistinctly crystalline deep-blue mass, moderately soluble in water, less soluble in anhydrous ether, easily soluble in alcohol. Ether precipitates the base from its alcoholic solution; I have not examined it.

Submitted to distillation, the free oxide gives rise to an oily base, which I naturally expected to be lepidine; but the experiments which I have hitherto made with this substance appear to negative this assumption. I have undertaken a more minute examination of the compound, because, if it be different, its study will probably throw some light upon the still uncertain constitution of the tertiary bases of the chinoline series, which I have frequently attempted to decipher. It remained now only for me to examine the mode of formation of the remarkable compound the nature of which I have endeavoured to clear up. With this view I have studied the action of iodide of methyle and amyle upon chinoline and lepidine, large quantities of which were kindly placed at my disposal by my friend Mr. David Howard. The products obtained in this reaction I have not submitted to a minute examination, having satisfied myself that their principal phases are well illustrated by the equations which I have given for the formation of the substances produced by the action of iodide of amyle upon lepidine. Nor have I followed out in detail the complicated secondary changes, and more especially the generation of the red colouring matter which is abundantly formed in these reactions. I have nothing to add to the perfect description of these phenomena by the distinguished discoverer of this pigment.

In conclusion I may be allowed to express my best thanks to M. Menier: without the magnificent crystals furnished by his ateliers, I could not have even attempted to clear up this question.

Though proud of her office as guide of industry, science acknowledges without blushing that there are territories on which she cannot advance without leaning on the strong arm of her powerful companion. Joint labours of this kind cannot fail to seal the pledge of alliance between industry and science.

"On some new Compounds obtained by Nitrogen-substitution, and new Alcohols derived therefrom." By Peter Griess, Esq.

"On the Differential Equations of Dynamics. A sequel to a Paper on Simultaneous Differential Equations." By George Boole, F.R.S. &c.

XXXIII. *Intelligence and Miscellaneous Articles.*

ON THE ELECTRICAL CONDUCTIVITY OF THALLIUM. BY M. LUCIEN DE LA RIVE.

THE conductivity of thallium was measured by Wheatstone's method, by comparing the resistance of a wire of this metal with that of a standard of distilled mercury. The thallium used was a specimen given by M. Lamy to the laboratory of the Ecole Normale. It was formed into wire by being first melted, then hammered so as to lengthen the bar, and finally passed through the draw plate. The latter operation is rendered difficult by the feeble tenacity of the thallium; the metal must be frequently moistened, and the wire carefully drawn, so as to obtain a certain degree of fineness.

The conductivity of a substance determined by measuring the resistance of a cylindrical wire is expressed thus, $C=\frac{L^2D}{PR}$, where L is the length, P the weight, R the resistance, and D the density of the wire.

Determination of the Density.—The density of thallium, as referred to water, cannot be directly obtained, for it dissolves to an extent which cannot be neglected; 5 grammes lose during the operation 10 milligrammes. Naphtha was used which had stood over sodium, and the following densities refer to naphtha at a temperature of 11°:—

			Mean.
Thallium in plate prepared by M. Lamy	(1)	14·351	14·346.
	(2)	14·342	
Thallium melted....................	(1)	14·332	14·330.
	(2)	14·329	
Thallium in wire 1 millim. in diameter..	(1)	14·260	14·276.
	(2)	14·292	

From these results it appears that the mechanical action of the hammer and of the draw plate produce a slight diminution of density of about $\frac{4}{1000}$. The density of the naphtha having been found to be 0·8275, the density of melted thallium at 11° is 11·853,—a value which well agrees with M. Lamy's numbers, 11·862 at 0°, the density of the same metal in the form of wire being 11·808.

Determination of the Conductivity.—Four different wires were experimented upon; the ends of the wires were held in copper clamps, and the wire was immersed in a vessel filled with naphtha.

Length of the wire, L.	Weight, P.	Resistance, R.	Value of $C=\frac{L^2D}{PR}$.	Temperature.
mm.	mg.			°
311	1773	120·0	5·36	15
190	1255	64·6	5·38	10
260	1182	129·6	5·21	11
190	504	161·4	5·24	12
		Mean	5·30	12

The resistance was always determined a short time after the wire had been drawn; but its surface was nevertheless covered with a layer of oxide; yet, as it was not possible to observe an increase of resistance from this time, it is presumable that the influence of this circumstance on the conductivity may be neglected. A wire exposed to the air for twenty-four hours experienced an increase of resistance of about $\frac{2}{100}$. The mean of the values of C is 5·3 at the temperature of 12° compared with mercury at 14°. The conductivity of mercury is 1·63, that of silver being 100, which gives for thallium 8·64, a value between that of lead 7·77, and that of tin 11·45, and which is far less than the conductivity of the alkaline metals.

Variation of the Resistance with the Temperature.—The resistance of the same wire was determined at three different temperatures:—

Temperature.	Resistance.
° 15	120·0
35	128·6
53	137·4

The coefficient K, being calculated by the formula $R_t = R_0(c + Kt)$, gives 0·0038, which is within the limits found for this coefficient in the case of most metals.—*Comptes Rendus*, March 31, 1863.

ON THE MAGNETICAL DEPORTMENT OF SOME CYANOGEN COMPOUNDS OF IRON, NICKEL, AND COBALT. BY DR. J. WIESENER.

The following conclusions have been arrived at after a lengthened investigation of this subject:—

1. Those cyanogen compounds of iron, nickel, and cobalt in which these metals are present as in an haloid, or ordinary salt, are *paramagnetic*.

2. Those compounds in which the cyanogen appears to form with either of the three metals a compound radical (in which therefore iron, nickel, or cobalt cannot be directly detected) may either be *paramagnetic* or *diamagnetic*.

3. The compounds of the general formula $Ni\,Cy_2\,Me$ are, as far as the present investigations have extended, *diamagnetic*.

4. The compounds constituted like hydroferrocyanic acid, $Fe\,Cy_3\,H\,H$, are diamagnetic if the hydrogen is replaced by *diamagnetic* metals, but *paramagnetic* if by magnetic metals.

5. The cobalticyanide compounds show the same deportment.

6. The compounds analogous to hydroferricyanic acid are without exception *paramagnetic*.—*Sitzungsbericht der Wiener Academie*, vol. xlvi. p. 175.

RESEARCHES ON THE OPTICAL PROPERTIES DEVELOPED IN TRANSPARENT BODIES BY THE ACTION OF MAGNETISM. BY M. VERDET.

I have the honour to submit to the Academy the result of a series of experiments on the relation existing between the magnetic rotation of the plane of polarization of a ray of homogeneous light, and its wave-length. An experiment by M. Edmond Becquerel, which consisted in compensating the action of a piece of *heavy glass* placed between the branches of an electromagnet by the action of a column of solution of sugar, seemed to show that, for this substance at least, the law of rotations differed little from the law of the inverse ratio of the square of the lengths of undulation. More recent researches by M. Wiedemann led to the conclusion, 1st, that the law did not apply to sulphide of carbon, and is therefore wanting in generality; 2nd, that when an *active* substance, such as essence of lemon or oil of turpentine, was submitted to the magnetic influence, there was, for each colour, proportionality between the magnetic rotation of the plane of polarization, and the rotation due to the special action of the substance.

In my experiments I have used the general method of M. Fizeau and M. Foucault, which consists in intercepting on a prism the light originally polarized and transmitted by the transparent body, and studying the state of polarization of the various parts of the spectrum. To the rays whose plane of polarization is parallel to the principal section of the analysing Nicol, a black band corresponds, the middle of which is successively brought to coincide with rays whose wave-lengths are known by Fraunhofer's experiments; the displacement which the analyser must undergo in order to re-establish coincidence with a given ray, when the direction of the current is changed, is exactly double the rotation due to the action of magnetic forces.

The following Table contains the relative values of the rotations corresponding to the five rays C, D, E, F, G*, for substances which I have studied, the rotation corresponding to the ray E being taken as unity:—

	C.	D.	E.	F.	G.
Distilled water	0·63	0·79	1·00	1·20	1·55
Solution of chloride of calcium ..	0·61	0·80	1·00	1·19	1·54
,, chloride of zinc	0·61	0·78	1·00	1·19	1·61
,, protochloride of tin ..	..	0·78	1·00	1·20	1·59
Oil of bitter almonds	0·61	0·78	1·00	1·21	
Oil of anise	0·58	0·75	1·00	1·25	
Sulphide of carbon	0·60	0·77	1·00	1·22	1·65
Commercial creosote	0·60	0·76	1·00	1·23	1·69
Essence of *Laurus cassia*	0·59	0·74	1·00	1·23	

The exact law of the inverse ratio of the square of the wave-lengths would have required the series of rotations

C.	D.	E.	F.	G.
0·64	0·80	1·00	1·18	1·50,

which does not much differ from any of the series in the preceding Table. Considering the nature of the liquids which deviate most from the law (sulphide of carbon, essences, creosote), the results of my experiments may be summed up in the following three propositions:—

1. The magnetic rotations of the plane of polarization of rays of different colours follow approximately the law of the inverse ratio of the square of the wave-lengths.

2. The exact law of the phenomena, is that the product of the rotation by the square of the wave-length increases from the least to the most refrangible end of the spectrum.

3. The substances for which this increase is most perceptible are also those which have the greatest dispersive power.

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* Any observation of the rays B and H is impossible; and I have only obtained even slightly satisfactory results for the rays C and G by placing in front of the eye coloured glasses which extinguished the middle and brilliant region of the spectrum without sensibly weakening the lustre of the adjacent portion of these rays.

A mathematical analysis, which cannot be given in this abstract, shows that, from these laws, to the differential equations of the motion of a system of plane waves normal to the axis of z in a medium submitted to magnetic influence there cannot be assigned the form

$$\frac{d^2\xi}{dt^2}=\mathrm{A}_0\frac{d^2\xi}{dz^2}+\mathrm{A}_1\frac{d^4\xi}{dz^4}+\ldots+m\frac{d\eta}{dt},$$

$$\frac{d^2\eta}{dt^2}=\mathrm{A}_0\frac{d^2\eta}{dz^2}+\mathrm{A}_1\frac{d^4\eta}{dz^2}+\ldots-m\frac{d\xi}{dt},$$

which M. Charles Neumann deduced from a particular hypothesis on the cause of these phenomena, and which Mr. Airy had proposed seventeen years ago, a few months after the publication of Mr. Faraday's discoveries. On the contrary, these laws agree equally either with the equations

$$\frac{d^2\xi}{dt^2}=\mathrm{A}_0\frac{d^2\xi}{dz^2}+\mathrm{A}_1\frac{d^4\xi}{dz^4}+\ldots+m\frac{d^3\eta}{dz^2\,dt},$$

$$\frac{d^2\eta}{dt^2}=\mathrm{A}_0\frac{d^2\eta}{dz^2}+\mathrm{A}_1\frac{d^4\eta}{dz^4}+\ldots-m\frac{d^3\xi}{dz^2\,\delta t},$$

which Mr. Maxwell has deduced from an hypothesis quite different from that of M. Charles Neumann, or with the equations

$$\frac{d^2\xi}{dt^2}=\mathrm{A}_0\frac{d^2\xi}{dz^2}+\mathrm{A}_1\frac{d^4\xi}{dz^4}+\ldots+m\frac{d^3\eta}{dt^3},$$

$$\frac{d^2\eta}{dt^2}=\mathrm{A}_0\frac{d^2\eta}{dz^2}+\mathrm{A}_1\frac{d^4\eta}{dz^4}+\ldots-m\frac{d^3\xi}{dt^3}.$$

The precision of the experiments, moreover, leaves no room for choice between the two systems*.

In conclusion, experiments on the magnetic rotations of dissolved tartaric acid have shown that the proportionality imagined by M. Wiedemann between the magnetic rotations and the special rotations of an active substance do not in reality exist. In fact I obtained for the two kinds of phenomena the following series of results:—

	C.	D.	F.	G.
Magnetic rotations ..	0·79	1·00	1·52	2·01
Natural rotations....	0·85	1·00	1·01	0·89

The exact law of the square of the wave-length would have required

C.	D.	F.	G.
0·80	1·00	1·48	1·88

—*Comptes Rendus*, April 6, 1863.

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* It is immaterial to these conclusions whether we admit, with Cauchy, that the coefficients A_0, A_1, A_2 . . . form a rapidly decreasing series, or, with M. Christoffel, that the coefficients A_0 and A_1 are of the same order of magnitude, all the others being negligible.

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XXXIV. *On Cæsium.* *By* Professor BUNSEN*.

[With a Plate.]

IN the first detailed communication which Kirchhoff and I made respecting cæsium and some of its compounds, published in vol. xxii. pp. 329 & 498 of this Magazine, we distinctly stated that the determination of the atomic weight of the new metal there described could only be considered as preliminary. The quantity of material, extracted from nearly one-hundred thousand pounds of the mineral water of Dürkheim, with which the whole investigation had to be made, amounted only to from 2 to 3 grammes, and therefore did not permit an exact and definitive estimation of the atomic weight. Since the publication of this memoir, I hoped to obtain a more abundant supply of material from the water of the "Murquelle" in Baden-Baden, which exhibits the reactions of cæsium and rubidium more plainly than the Dürkheim mineral water. I found, however, after evapoting 30,000 lbs. of this water, using for this purpose nearly 5 tons of coal, that the mother-liquor yielded only 1·5 grm. of pure chloride of cæsium. According to an investigation made by Professor Bixio, 1000 parts of this mother-liquor possesses the following composition:—

Sulphate of calcium	1·861
Sulphate of strontium	0·492
Biborate of calcium	0·035
Chloride of sodium	218·964
Chloride of potassium	53·416
Bromide of potassium	0·303
Chloride of rubidium	0·336
Chloride of cæsium	0·229
Chloride of lithium	17·825
Chloride of calcium	10·495
Chloride of magnesium	0·254
	304·210

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* Communicated by Professor Roscoe, F.R.S.

The residues which I obtained from the preparation of half a pound of pure chloride of rubidium from lepidolite likewise gave no larger a yield of cæsium-salt; so that the quantity of material with which I have been obliged to conduct the following experiments has also been but very small.

Whilst I was thus engaged with the preparation of cæsium-salt for the purpose of making a more exact determination of the atomic weight of this metal, Messrs. Johnson and Allen* were fortunate enough to obtain a specimen of American lepidolite so rich in cæsium that they were able to prepare about 30 grms. of bitartrate of cæsium from a few kilogrammes of the mineral. With this material, Messrs. Allen and Johnson prepared the acid tartrates of cæsium and rubidium, employing the great difference in the solubility of these salts for the purpose of separating the two metals by repeated crystallization. They thus obtained a chloride of cæsium which, according to four analyses made by them, possessed the following composition:—

Chlorine.	Cæsium.
21·044	78·956
21·031	78·969
21·043	78·957
21·063	78·937

The purest chloride compound which I was able to obtain by help of the method which I first used contained†—

	Chlorine.	Cæsium.
First purification	22·334	77·666
Second purification	22·334	77·666
Third purification	22·316	77·684

From the close agreement of the above numbers, it is clear that in the method of separation which I employed at the time of the discovery of cæsium a limit is reached beyond which the purification cannot be carried. The method employed by Messrs. Johnson and Allen is likewise inapplicable in the case of the small quantity of material to which my experiments were limited, and I have therefore endeavoured to purify the salt by another method. This is based upon the fact that the *acid* tartrate of cæsium, like the corresponding rubidium-salt, is unalterable in the air, whilst the *neutral* tartrate of cæsium deliquesces very readily. This method of separation answers very well even with very small quantities. Having determined, by precipitation with silver, the quantity of rubidium in a mixture of the chlorides free from potassium, lithium, and sodium, the

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* American Journal of Science and Arts, vol. xxxv. p. 94, Jan. 1863; and Phil. Mag. S. 4. vol. xxv. p. 196.

† Phil. Mag. vol. xxii. p. 503.

chlorides are converted into carbonates: rather more tartaric acid is then added to the solution of the carbonates than is sufficient to convert the cæsium-compound into the neutral, and the rubidium-compound into the acid tartrate. The finely divided solid mass obtained by evaporation and trituration is brought on to a funnel containing a small filter, and then left for some time exposed to a moist atmosphere. The deliquescent cæsium-salt drops slowly down, whilst the acid tartrate of rubidium remains unaltered in the funnel. In this way a substance was obtained which, when converted into chloride, gave upon analysis the following numbers:—

1·3033 grm. of this chloride of cæsium yielded 1·1422 grm. chloride of silver. This corresponds to

Chlorine.	Cæsium.
21·709	78·291

This salt exhibited in the spectroscope but a faint rubidium reaction, but it had taken up a small quantity of lithium- and potassium-compounds from the tartaric acid. In order to separate these impurities, a boiling and somewhat dilute solution of the salt was precipitated by chloride of platinum, and the precipitate, after having been washed by decantation with hot water and dried, was reduced in a stream of hydrogen. This mode of purification was repeated until the composition of the chlorine-compound remained unaltered. In this way the following results were obtained:—

After the second purification with $PtCl^2$,

1·8987 grm. of substance yielded 1·6342 grm. chloride of silver.

After the fourth purification with $PtCl^2$,

1·3835 grm. of substance yielded 1·1781 grm. chloride of silver.

After the fifth purification with $PtCl^2$,

1·3682 grm. of substance yielded 1·1644 grm. chloride of silver.

After the sixth purification with $PtCl^2$,

1·2478 grm. of substance yielded 1·0623 grm. chloride of silver.

These numbers correspond respectively to the following composition of the chloride:—

Chlorine.	Cæsium.
21·293	78·707
21·057	78·943
21·045	78·955
21·052	78·948

The mean of the three last experiments, in which the composition may be assumed to have become constant, shows that

21·0513 per cent. of chlorine was contained in the salt. This number very closely agrees with the mean from Johnson and Allen's experiments, viz. 21·0452. The more exact atomic weight of cæsium is therefore—

According to Johnson and Allen's determinations .	133·03
According to my own determinations	132·99

Hence, until a more exact determination be made, we may take the atomic weight of cæsium, as the mean of these two determinations, to be 133.

In the paper already referred to*, I remarked that chloride of cæsium readily attracts moisture from the air and deliquesces. Johnson and Allen, on the contrary, state that the salt prepared by them, and used for their determination of the atomic weight, did not possess this property. They say in their memoir, "We thus obtained an amorphous mass, of a pure white colour, which, unlike Bunsen's chloride, was *not perceptibly deliquescent* even in a very moist atmosphere." And further, "As to the properties of the chloride of cæsium, we observed that not only is it not deliquescent, but it is hardly hygroscopic. The unfused and porous salt may be weighed in moist air with as much accuracy as chloride of sodium." In spite of this statement, I must adhere to my previous observation. Even the chloride of cæsium which may be considered to be spectrum-analytically pure, having the atomic weight of 133, exhibits a high degree of deliquescence *in moist air*, as is seen from the following experiments:—0·02715 grm. of powdered chloride of cæsium, which had been evaporated with a few drops of hydrochloric acid and dried for two hours at 130°, when spread out upon a platinum capsule deliquesced in a few hours to a liquid in which no solid substance could be seen. In the first two hours the weight of the salt had increased by 0·0068 grm., in the second two hours 0·0065 grm., and in the third two hours 0·0030 grm., thus taking up within six hours a quantity of water from the air equal to half its own weight. 22 milligrammes of the fused salt deliquesced quickly in the air, and after fourteen hours had taken up 19 milligrammes, or nearly its own weight of water. In dry air only, this liquid dries up to a solid mass. The chlorides of potassium and sodium, portions of which were exposed simultaneously to the same air as the chloride of cæsium, did not exhibit any deliquescence even after an exposure of twenty-four hours. Hence Messrs. Allen and Johnson seem to have made their experiments in comparatively dry air, in which, as is well known, many deliquescent substances

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* Phil. Mag. vol. xxii. p. 502.

do not exhibit this property; and for this reason they have failed to remark the hygroscopic nature of the chloride of cæsium.

In our memoir on the new metals* we have distinctly stated that we have represented in our drawing only those lines which, on account of their position, sharpness, or intensity, serve as the best means of detecting the presence of the substances in question. Messrs. Johnson and Allen remark with regard to this, "Kirchhoff and Bunsen, in the figure given by them (Phil. Mag. vol. xxii. 1861), represent eleven lines. We find without difficulty seven more lines, and observe further that some of those figured by Kirchhoff and Bunsen are not mapped in their correct positions." In reply to this remark, we must regret that Messrs. Johnson and Allen appear to have neglected to make an exact comparison of their spectrum with ours, otherwise they would have easily convinced themselves that the lines which they have drawn correspond as closely as can be expected with those which we represented, whilst they would at the same time have noticed that three lines occur on our drawing which are not seen at all in the cæsium spectrum. These three lines were printed in a large number of the lithographs by a mistake of the printer, and lie close to the right of the lines marked VI., X., and XV. by Messrs. Johnson and Allen. Setting aside these three lines, introduced by a mistake of the lithographer, we have the following agreement of the several readings:—

1.	2.	3.	4.
I.	75	25	
II.	80	30	
III.	82·5	32·5	
IV.	85	35	
V.	87·5	37·5	
VI.	91	41	41·7
VII.	97·5	47·5	47·6
VIII.	101	51	
IX.	106	56	57·1
X.	107·5	57·5	58·1
XI.	109	59	
XII.	111	61	
XIII.	112·5	62·5	64·1
XIV.	114·5	64·5	65·8
XV.	118	68	68·7
XVI.	121	71	
XVII.	157·5	107·5	106·3 β
XVIII.	160	110	109·0 α

The first column contains the numbers by which Messrs. Johnson and Allen have designated the lines observed by them in

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* Phil. Mag. vol. xxii. p. 508.

the cæsium spectrum; the second gives the readings on the scale of their apparatus corresponding to these lines; the third column contains numbers representing the same readings reduced to the scale which is given in the Plate published with our memoir in Fresenius's 'Journal of Analytical Chemistry'*. This reduction is founded upon the statement made by Messrs. Johnson and Allen, that on their scale the sodium line lay at 100, and the blue strontium line at 156. On the scale contained in Fresenius's Journal, the left edge of the sodium line lies at 50, that of the blue strontium line at 56; hence the numbers of the third column are easily obtained from those of the second simply by subtracting 50. The fourth column contains the positions of the cæsium lines from our drawing in Poggendorff's *Annalen* reduced to the same scale.

On comparing the corresponding numbers in the third and fourth columns we do not find any differences of real moment. A more exact agreement can hardly be expected when we remember that in the absolute position of the lines of the spectrum seen upon a scale such as we used, and as Messrs. Johnson and Allen, as far as can be gathered from their description, appear to have used, unavoidable errors of observation are introduced, which are rendered less evident (owing to the achromatism of the glass lenses and of the eye) by the fact that, in whatever position the apparatus may be placed, only *one* point on the spectrum can be distinctly read off on the scale. In constructing our apparatus with a photographic scale, we did not deviate without good reason from the much more accurate means of measurement used by physicists in the determination of the refractive indices of transparent bodies, inasmuch as the chemist, for whom our apparatus is specially designed, does not require so much an exact knowledge of the absolute position of the single lines in the spectrum as he needs to be able to observe quickly and easily, especially when lines have to be recognized which only flash out for a moment. The point of greatest importance in spectrum-analysis is the degree of certainty with which it is possible to decide whether two lines are identical in position or not; and this decision is most easily attained by means of the small prism on the slit.

The slight differences which appear between the numbers in the third and those in the fourth column may further be caused from errors in the interpolation which has to be made in order to reduce the readings of one apparatus to the scale of another. For the purpose of comparing their observations with ours, Messrs. Johnson and Allen have reduced their readings to the scale from which the numbers in column 3 are derived, and they have then repre-

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* Fresenius' *Zeitschrift für Analytische Chemie*, 1862, Heft 1, fig. 2.

sented their reduced observations in a drawing. This drawing does not agree at all well with the numbers in column 3, and it is therefore evident that a different mode of interpolation must have been used by Messrs. Johnson and Allen from that which I have employed. In their paper they do not describe the method which they adopted for reducing their observations; the following example shows, however, that their method cannot have been a suitable one. According to the above statement, the value of one division on the two scales from which the numbers in columns 1 and 2 are derived is *nearly* equal, and the divisions 100 and 50 correspond exactly to each other as the sodium line stood at these points on both scales. Now Messrs. Johnson and Allen read off on their instrument division 101 for the line VIII.; hence, when this is reduced to the other scale, we must obtain a value *nearly* equal to 51. In Messrs. Johnson and Allen's drawing, however, the line VIII. has the limits 52·5 and 53·5.

A glance at the foregoing Table shows that Messrs. Johnson and Allen have observed a large number of lines which are not represented in our drawing. The greater number of these we have also measured; but they were not included in the drawing, because, as we have already frequently remarked, we did not endeavour to catalogue the lines completely, but to represent as truly as possible the characteristic appearances of the spectra of the several substances. Owing to the difficulty of accurately distributing many shades of the same colour in chromolithographic printing, the weaker lines have necessarily been omitted in our drawings.

I have still to observe, in reference to Messrs. Johnson and Allen's remarks, that it is line XVI., and not line XV. as they state, which is coincident with Fraunhofer's line E.

In order to avoid errors similar to those which I have exposed in the foregoing, and for the purpose of facilitating the numerical comparison of the data of various spectrum-observations, we give in Plate V. graphical representations of the observations which are taken from the guiding lines given in the chromolithograph drawings of the spectra published in our former memoirs and in which the prism was placed at the angle of minimum deviation. The ordinates of the edges of the small blackened surfaces, referred to the divisions of the scale as abscissæ, represent the intensity of the several lines with their characteristic gradations of shade. These drawings were made when the slit was so broad and the flame of such a temperature, that the fine bright line upon the broad band Ca α began to be distinctly visible. This breadth of the slit was equal to the fortieth part of the distance between the sodium line and the lithium line α. For the sake of perspicuity, the continuous spectra which some bodies exhibit are specially represented on the upper edge of the scale, to the divisions of which they are referred as abscissæ. In order to render these drawings, which have reference to our instrument, applicable to observations upon the scale of any other apparatus, which we may call B, it is only necessary to prepare a reduced scale which is laid upon the several drawings and used in place of the divided scale given on the Plate. The lines marked at the bottom of Plate V. serve for the preparation of this new scale: these lines denote the distances between the lines Kα, Li α, Na, Tl, Sr δ, Rb α, and Kβ, measured according to the scale of our instrument. The position of each of these lines is determined by the edge of the line, which does not change its place on altering the breadth of the slit. The position of these same lines is read off on the scale of the instrument B, and the corresponding number written under each. A series of fixed points on the scale is thus obtained, and the complete divisions for the scale of the instrument B are got by interpolating the values of the portions of the scale situated between the fixed points. The sodium line is then inserted in this scale, which is pasted upon a straight-edge, and the divisions numbered in tens and fives. If this measure be now laid upon any one of the drawings so that the sodium line on the measure coincides with the division 50 on the drawing, the scale on the measure will give the position of all the lines in the particular spectrum exactly as they are seen in the photographic scale of the instrument B. When the position of the line under observation has in this way been ascertained, it is easy to assure oneself of its exact identity by means of the small prism on the slit.

Heidelberg, April 26, 1863.

XXXV. *On Chemical Notation in conformity with the Dynamical Theory of Heat and Gases.* *By* J. J. Waterston, *Esq.*[*]

IN a lecture "On the Molecule of Water," lately delivered at the Royal Institution, Albemarle Street, by Mr. Odling, Secretary to the Chemical Society[†], the question as to the molecular weight of water was discussed with much detail, and arguments adduced to show that the correct number is 18, represented by the formula H^2O, and that it cannot be 9, as might be inferred from the specific gravity of the vapour, following the idea that equal volumes of bodies in the gaseous state contain an equal number of atoms.

The dynamical theory of heat and gases imperatively requires us to believe that in equal volumes of gases and vapours in æqui-

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[*] Communicated by the Author.

[†] A complete report of the lecture is given in 'The Engineer' for June 26.

librio of pressure and temperature (and conforming to the primary laws of Mariotte, and of Dalton and Gay-Lussac), there exists the same number of separate parts. On the other hand, these parts cannot be in all cases the atoms that enter into chemical union, or what is understood by the term "combining element." The researches of Dumas and Mitscherlich have brought out this fact very obviously in the case of sulphur, phosphorus, arsenic, and selenium. In sulphur the ultimate element of the vapour or gaseous molecule must consist of six chemical atoms up to the temperature of 860° C., at which, according to MM. Deville and Troost's researches, the vapour-density is reduced to one-third, showing that the gaseous molecule had then split into three, each of which was made up of two chemical atoms. In selenium, the vapour molecule must consist of four chemical atoms; and at 1040° C. there was found such alteration of vapour-density as led to the anticipation that from about 1200° to 1400° the ultimate vapour molecule would contain only two chemical atoms (MM. Deville and Troost, 1860).

If we were to view the ultimate moving part of all the primary gases as consisting of *at least* two chemical atoms, the same symbols would express not only the combining proportions of all the bodies entering into union, but the actual composition of a volume of the compound, and we should get rid at once of all the ambiguity and difficulties in the arithmetic of combining proportions. The absolute necessity of doing so may, I think, be easily demonstrated from the examples chosen by Mr. Odling in his lecture.

The argument of the lecture is to show from the phenomena of substitution that the hydrogen of the water molecule is experimentally divisible into two equal parts; for it is from such phenomena that the chief, if not only chemical proof is to be found. To quote Mr. Odling's words, "It is evident, for instance, that the molecule of marsh-gas must contain four proportions of hydrogen, because we are able to replace one-fourth, two-fourths, three-fourths, and four-fourths of its hydrogen by four successive substitutions to produce a series of bodies differing from one another by a regular gradation of properties."

In marsh-gas the ratio of Hy, the quantity of hydrogen by weight, to the carbon Cx, is $\frac{1}{3}$. If the vapour-density of carbon were 3 to hydrogen unity, the numerator added to the denominator of the fraction $\frac{1}{3}$, making 4, would represent the vapour-density of marsh-gas, and be represented by HC ($x=y=1$), in which the letters represent the vapour-densities of each of the constituents respectively. But the vapour-density of marsh-gas is 8; and assuming the vapour-density of carbon to be 12, we have $Hy+Cx=8$ and $\frac{Hy}{Cx}=\frac{1}{3}$; substituting $1=H$ and $12=C$, we have $y+12x=8$ and $\frac{y}{12x}=\frac{1}{3}$. From these two simple equations we find $x=\frac{1}{2}$ and $y=2$. Hence marsh-gas is expressed by $C\frac{1}{2}+H2=C^{\frac{1}{2}}H^{2}$, which means that a gaseous molecule of marsh-gas is composed of half a molecule of carbon united to two gaseous molecules of hydrogen.

In monochloromethene, or muriate of methyle, the ratio of the hydrogen to the carbon is $\frac{1}{4}=\frac{Hy}{Cx}=\frac{y}{12x}$. The ratio of the hydrogen to the chlorine is $\frac{1}{12}=\frac{Hy}{Clz}=\frac{y}{36z}$, and the vapour-density of the compound is

$$25\tfrac{1}{2}=Hy+Cx+Clz=y+12x+36z.$$

From these three equations we find $x=z=\frac{1}{2}$ and $y=1\frac{1}{2}$. Hence this compound is expressed by $H1\frac{1}{2}+C\frac{1}{2}+Cl\frac{1}{2}=H^{1\frac{1}{2}}C^{\frac{1}{2}}Cl^{\frac{1}{2}}$; which means that its vapour-molecule is composed of one and a half gaseous molecules of hydrogen, half a molecule of carbon, and half a gaseous molecule of chlorine united together.

In the same way dichloromethene, or chloride of methyle, is expressed by $HC^{\frac{1}{2}}Cl$, the vapour-density being

$$43=\left\{\begin{array}{l}H+C^{\frac{1}{2}}+Cl\\ 1+6+36\end{array}\right.;$$

and chloroform by $H^{\frac{1}{2}}C^{\frac{1}{2}}Cl^{1\frac{1}{2}}$, its vapour-density being

$$60\tfrac{1}{2}=\left\{\begin{array}{l}H^{\frac{1}{2}}+C^{\frac{1}{2}}+Cl^{1\frac{1}{2}}\\ \frac{1}{2}+6+54\end{array}\right.;$$

and the tetrachloromethene, or perchloride of muriate of methyle, by $C^{\frac{1}{2}}Cl^{2}$, its vapour-density being 78.

Following the same system, water is expressed by

$$HO^{\frac{1}{2}}=9=\left\{\begin{array}{l}H+O^{\frac{1}{2}}\\ 1+8\end{array}\right.,$$

its vapour-molecule being composed of one gas-molecule of hydrogen united to half a gas-molecule of oxygen.

Phosphoretted hydrogen consists of one part by weight of hydrogen united to 10·5 phosphorus, or $\frac{Hy}{Px}=\frac{1}{10{\cdot}5}$, and its vapour-density is $17{\cdot}5=Hy+Px$. The vapour-density of phosphorus being $64=P$, we have

$$\frac{y}{64x}=\frac{1}{10{\cdot}5}\text{ and }y+64x=17{\cdot}5.$$

From these two equations we have $y=1\frac{1}{2}$ and $x=\frac{1}{4}$; and phosphoretted hydrogen is represented by $\mathrm{H}^{1\frac{1}{2}}\,\mathrm{P}^{\frac{1}{4}}$, which signifies that a gaseous molecule of phosphoretted hydrogen is composed of $1\frac{1}{2}$ gas-molecule of hydrogen united to one-quarter of a gaseous molecule of phosphorus. In the sesquichloride of phosphorus the weight of phosphorus is to the weight of chlorine as 1 to 3·375, hence $\frac{\mathrm{P}x}{\mathrm{Cl}z}=\frac{1}{3{\cdot}375}=\frac{64x}{36z}$. The vapour-density of the compound is $70=\mathrm{P}x+\mathrm{Cl}z=64x+36z$. From these two equations x comes out $=\frac{1}{4}$, and $z=1\frac{1}{2}$. Hence $\mathrm{P}^{\frac{1}{4}}\,\mathrm{Cl}^{1\frac{1}{2}}$ expresses the constitution and vapour-density or weight of a gaseous molecule of the sesquichloride. Thus the combining element or chemical atom of phosphorus is still one-quarter the gaseous molecule.

In the preceding examples the smallest combining element of chlorine is half the gaseous molecule. It combines also in the proportion 1, $1\frac{1}{2}$, and 2 gaseous molecules.

The smallest combining element of hydrogen is half the gaseous molecule. It combines also in the proportion 1, $1\frac{1}{2}$, 2 gaseous molecules; and further, in the organic compounds its combining proportions extend up to 16 gaseous molecules, as in petrolin or colophon, $\mathrm{H}^{16}\,\mathrm{C}^{10}$.

The smallest combining element of oxygen is one-half the gaseous molecule. It combines also in the proportion 1, $1\frac{1}{2}$, 2, $2\frac{1}{2}$, 3 gaseous molecules: *e. g.* an arsenious acid vapour-molecule is composed of one arsenic vapour-molecule united with three oxygen gas-molecules.

Muriatic acid gas, $\mathrm{Cl}^{\frac{1}{2}}\,\mathrm{H}^{\frac{1}{2}}$, is composed of single half molecules of its constituents united together.

Cyanogen, NC, is composed of whole single molecules of its constituents united together; and this compound molecule assumes the deportment of a chemical element by splitting into two in some of its combinations with hydrogen, &c., as $\mathrm{N}^{\frac{1}{2}}\mathrm{C}^{\frac{1}{2}}\mathrm{H}^{\frac{1}{2}}$, prussic acid; $\mathrm{N}^{\frac{1}{2}}\,\mathrm{C}^{\frac{1}{2}}\,\mathrm{Cl}^{\frac{1}{2}}$, chloride of cyanogen; and $\mathrm{N}^{\frac{1}{2}}\,\mathrm{C}^{\frac{1}{2}}\,\mathrm{Br}^{\frac{1}{2}}$, bromide of cyanogen.

In silicated hydrogen the ratio of hydrogen to silicon is $\frac{1}{7}=\frac{\mathrm{H}y}{\mathrm{Si}.x}$, $\mathrm{H}=1$, $\mathrm{Si}=$?, $\mathrm{Si}\,.\,\mathrm{H}=$?. Hence there are no data to compute x and y. By analogy we might infer $\mathrm{Si}=28$; and if $\mathrm{Si}\,x\;\mathrm{H}y=16$, we should have $x=\frac{1}{2}$ and $y=2$; and silicated hydrogen, $\mathrm{Si}^{\frac{1}{2}}\,\mathrm{H}^{2}$, homologous with marsh-gas, $\mathrm{C}^{\frac{1}{2}}\,\mathrm{H}^{2}$.

In ammonia the ratio of hydrogen to nitrogen is 1 to 4·67, or $\frac{\mathrm{H}y}{\mathrm{N}x}=\frac{1}{4{\cdot}67}$. The specific gravity of the gas being $8{\cdot}5=\mathrm{N}x+\mathrm{H}y$, and N=14, we have $\frac{y}{14x}=\frac{1}{4\cdot67}$, and $14x+y=8\cdot5$; hence $x=\frac{1}{2}$, and $y=1\frac{1}{2}$. Ammonia is thus expressed by $N^{\frac{1}{2}}H^{1\frac{1}{2}}$, which means that a gas-molecule of ammonia is composed of half a gas-molecule of nitrogen united to one and a half gas-molecule of hydrogen.

In some cases of composite bodies, the components separate on boiling or sublimation. When this happens, the specific gravity of the vapour reveals the fact, as in the following instance.

In sal-ammoniac the weight of ammonia is to the weight of muriatic acid as 1 to 2·235. Its vapour-density is 13·5 $(N^{\frac{1}{2}}H^{1\frac{1}{2}})=8\cdot5$, $H^{\frac{1}{2}}Cl^{\frac{1}{2}}=18\cdot5$. Hence

$$\frac{8\cdot5x}{18\cdot5y}=\frac{1}{2\cdot235},\quad 8\cdot5x+18\cdot5y=13\cdot5,$$

from which we find $x=y=\frac{1}{2}$; and following the usual formula, we might say half a gas-molecule of ammonia *unites* with half a gas-molecule of muriatic acid to form one gas-molecule of muriate of ammonia; or if this splitting of $(N^{\frac{1}{2}}H^{1\frac{1}{2}}$ into two $(N^{\frac{1}{4}}H^{\frac{3}{4}})$, and $H^{\frac{1}{2}}Cl^{\frac{1}{2}}$ into two $(H^{\frac{1}{4}}Cl^{\frac{1}{4}})$, is inadmissible, we have the alternative of the two whole molecules remaining separate, and the ammonia and acid vapour forming a simple mixture with specific gravity 13·5, equal to the mean of the two $\left[\frac{8\cdot5+18\cdot5}{2}=13\cdot5\right]$.

The same is the case with the nitrous, formic, and acetic ethers, but not so with the oxalic and succinic ethers.

The three first are

$$\frac{O^{1\frac{1}{2}}N+H^4C^2.HO^{\frac{1}{2}}}{2}=\frac{38+37}{2}=37\tfrac{1}{2},$$

$$\frac{O^{1\frac{1}{2}}CH+H^4C^2.HO^{\frac{1}{2}}}{2}=\frac{37+37}{2}=37,$$

$$\frac{O^{1\frac{1}{2}}C^2H^3+H^4C^2.HO^{\frac{1}{2}}}{2}=\frac{51+37}{2}=44.$$

The two last

$$O^{1\frac{1}{2}}C+H^4C^2.HO^{\frac{1}{2}}=73,$$

$$O^{1\frac{1}{2}}H^2C^2+H^4C^2.HO^{\frac{1}{2}}=87.$$

It will be remarked that the symbols and notation, compared with the vapour-density, reveal such facts without any effort of the attention.

Mr. Odling advocates Gerhardt's system of notation; but although it is nominally based, like that of Berzelius, on the so-called hypothesis that all simple gases contain equal numbers of atoms in equal volumes, it does not recognize the difference between gaseous molecule and chemical atom so distinctly as to avoid obscurity and misrepresentation: *e. g.* Mr. Odling states that "when water is acted upon by metallic sodium, one equivalent of sodium expels one equivalent of hydrogen to form the well-defined body hydrate of sodium, and the action proceeds no further; but when melted hydrate of sodium at a temperature approaching that of redness is acted upon by metallic sodium, a second equivalent of sodium turns out a second equivalent of hydrogen to produce oxide of sodium." The diagram appended represents the half of the hydrogen in *one* molecule of water as expelled by a molecule of sodium to form the solid hydrate, and afterwards the second half to produce the oxide of sodium. Now the fact is, that two molecules of water are engaged in forming the one molecule of the hydrate, and the hydrogen of only one of them is expelled in forming the hydrate; and in the terminal action, the hydrogen of the other molecule of water is expelled by a repetition of the same action between the newly supplied sodium and the oxygen of that second molecule.

Thus also Dr. Graham, in his second volume, p. 514, states with reference to the analogous action between potassium and water, "The representation of water by $HH\Theta$, as in Gerhardt's system, possesses the additional advantage of corresponding with the important fact that it is possible to replace either the half or the whole of the hydrogen in[*] water by a metal. Thus potassium thrown into water displaces half the hydrogen and forms hydrate of potash, $HK\Theta$; and when this compound, in the solid state, is heated with an additional quantity of potassium, the remaining half of the hydrogen is displaced, and anhydrous potash, $KK\Theta$, is formed. On the contrary, when potassium acts on hydrochloric acid, HCl, it displaces the whole of the hydrogen and forms chloride of potassium, KCl. This is an important difference, which is easily understood on the supposition that water contains two atoms, and hydrochloric acid only one atom of hydrogen; whereas if these two compounds are represented by the analogous formulæ HO and HCl, the cause of the difference of action is by no means apparent." The ambiguity here is caused by Gerhardt's symbols actually representing *two* gaseous molecules; but they are not distinctly recognized as having this meaning either by Mr. Odling or Dr. Graham, as the following shows. In the above extract $HK\Theta$ represents one molecule of the hydrate of potash, but $HH\Theta$ represents two molecules of water, and $KK\Theta$ two molecules of anhydrous potash. The actions expressed by the fractional system of notation (submitted

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[*] Here is an ambiguity. Is one molecule meant, or two?

as the *only* system mathematically conformable to the law of volumes) is as follows:—Potassium, K, in contact with water, $HO^{\frac{1}{2}}$, $HO^{\frac{1}{2}}$, $HO^{\frac{1}{2}}$, &c., expels the H of one $HO^{\frac{1}{2}}$ and unites with another $HO^{\frac{1}{2}}$, forming the solid $KO^{\frac{1}{2}}\,HO^{\frac{1}{2}}$. Another K heated with this solid expels the H and takes the $O^{\frac{1}{2}}$ of the $HO^{\frac{1}{2}}$, the result being $KO^{\frac{1}{2}}$, $KO^{\frac{1}{2}}$. The first is a double act of combination, forming one molecule out of three. The second is a case of resolution by preponderating affinity.

With reference to chloride of potassium: when potassa, $KO^{\frac{1}{2}}\,HO^{\frac{1}{2}}$, or $KO^{\frac{1}{2}}$ is presented to $H^{\frac{1}{2}}\,Cl^{\frac{1}{2}}$, $H^{\frac{1}{2}}\,Cl^{\frac{1}{2}}$ in water, the $O^{\frac{1}{2}}$ in one molecule of the oxide unites with the two $H^{\frac{1}{2}}$ of two molecules of the acid to form water (no hydrogen is evolved), and the two $Cl^{\frac{1}{2}}$ of the same two molecules of acid unite with the K, forming KCl, the chloride of potassium.

Potassium in hydrochloric acid gas, K, $\begin{matrix} Cl^{\frac{1}{2}}\,H^{\frac{1}{2}} \\ Cl^{\frac{1}{2}}\,H^{\frac{1}{2}} \end{matrix}$, releases two $H^{\frac{1}{2}}$ of two molecules of the acid to form H (which is evolved), and the two $Cl^{\frac{1}{2}}$ unite with K to form KCl. Lastly, potassium, K, in chlorine, Cl, takes fire and burns into KCl.

After discussing several cases of substitution and of chemical parallels as arguments adduced "to show that the molecular weight of water is 18, corresponding to the formula $H^2\,O''$, and not 9, corresponding to the formula HO', Mr. Odling makes the following remark:—"In addition to the above-mentioned chemical parallel, there is one very remarkable physical relation subsisting between marsh-gas, ammonia, water, and hydrochloric acid, namely that all four molecules occupy the same bulk. Two cubic feet, for instance, of hydrochloric acid gas, HCl, yield one cubic foot of hydrogen and one cubic foot of chlorine. Two cubic feet of steam, $H^2\,O$, yield two cubic feet of hydrogen and one cubic foot of oxygen, &c." The meaning is apparently that the symbols attached to the letters express the number of volumes of the constituent gases in *two* volumes of the compounds. That this should be noticed as "a very remarkable physical relation" at once indicates that the mode of notation, although it is founded on the law of volumes, is nevertheless defective. *All* molecules, simple or compound, in the gaseous state, occupy the same bulk. This the dynamic theory proclaims as a fact. Let the notation be founded thereupon, and we obtain, expressed with perfect conciseness, at once the chemical composition, the physical density of a *unit* volume, and the constitution of a molecule. The analytical chemist may object to the introduction of fractional exponents, from the preconceived notion that a gaseous molecule is with more propriety to be viewed as a combination of two or more chemical indestructible atoms than that such primary atom is a fraction of a gaseous molecule; but if this feeling can be overcome, the advantage obtained would, I believe, fully repay the effort. To the mathematical student of thermo-molecular physics, there is as distinct evidence of the gaseous molecule of oxygen splitting into two equal parts when combining with hydrogen to form water, as there is to the astronomer of any of the results of the law of gravitation. When, besides expressing such facts, the vapour-molecule system of exponents is also so much more comprehensive in the information it affords, surely an impartial and deliberate consideration of the question cannot fail to have its influence.

I have before me a Table of nearly two hundred gases and vapours whose densities have been experimentally ascertained, all expressed in this way. Each symbol or formula expresses the actual constitution of the gas-molecule in terms of the gas-molecules of its constituents, or, in other words, each expresses the constitution of a unit volume of the body in a state of vapour in terms of the unit volume of its constituents; also the equivalent weight by adding up the value of the symbols. Thus sulphurous acid, $OS^{\frac{1}{6}}=16+\dfrac{96}{6}=32$, which is the vapour-density and also the equivalent. Muriatic acid, $Cl^{\frac{1}{2}}\,H^{\frac{1}{2}}=18+\frac{1}{2}=18\frac{1}{2}$, which is the vapour-density, but only one-half of the equivalent usually given.

The mixing up of single and double numbers in tables of equivalents, which still continues in works of authority, seems prejudicial to advancement in theoretical chemistry. For example, if the individuality of the molecule were properly kept in view, alcohol could never have been viewed as the hydrate of oxide of ethyle in a system of notation like that of Gerhardt's, which professes to recognize equal number of atoms in equal volumes,—the fact being that two vapour-molecules of alcohol contain the same elements as one vapour-molecule of ether *plus* one vapour-molecule of water. If the system recognizes only the simple gases as having the same number of atoms in equal volumes, and the compounds as having varying multiples of these according to what is termed their "*rational formula,*" then in a cubic foot which contains n number of atoms of oxygen there is $\frac{1}{2}n$ number of ether-atoms as the greatest number possible; because if the number were n, each atom of ether would contain half an atom of oxygen (4), which is inadmissible, as an atom applies to the least combining element. In a cubic foot of alcohol vapour, considered as the hydrate of ether, there cannot be more than half the number of atoms that ether has, or $\frac{1}{4}n$, for the same reason in respect to the atom of oxygen. On the other hand, if a cubic foot of the vapours of ether, of alcohol, and of *all* other compounds of oxygen and other bodies, simple or composite, contains $\frac{1}{2}n$ number of atoms or smallest parts of the respective compounds, then must a cubic foot of oxygen contain n atoms: or giving to the ultimate moving part of a gas or vapour the name *molecule*, we find that in all compounds containing oxygen the atom of each corresponds to the molecule, and has the same constant volume in all, if we regard only the molecule of oxygen to be dual in its nature, or composed of two atoms. Reverting to the usual definition of gaseous volume being the quotient of the equivalent weight by the specific gravity, we have to make an exception to this in the case of the primaries, and define their gaseous volume as being the quotient of twice the equivalent weight by the specific gravity; but it would be better to substitute the word *molecule* for *volume*, as the theory of heat and gases which attributes to the ultimate separate part, whatever its weight or constitution, a constant volume, because of the *vis viva* with which it is charged, seems to be favourably received by professional chemists (Graham, vol. ii.).

With respect to ethyle, &c., and the "rational formulæ" in which they enter, some further remarks seem necessary, as the existence of such radicals is incompatible with the dynamical theory. In the 'English Cyclopædia,' art. "Chemical Formulæ," the following occurs:—"All formulæ are partial because they cannot express the whole of the molecular arrangements of the bodies to which they refer, and arbitrary because they cannot but give undue prominence to particular theoretical views and tendencies." In Graham's second volume, p. 521, it is stated that "the rational formula of a compound is inferred from its modes of formation and decomposition." Such mode applied to alcohol would define it as a hydrate of olefiant gas. At p. 522 the following occurs:—"It appears, then, that the same compound may have several rational formulæ. . . . . . The greater the number of elementary atoms entering into the constitution of a compound, the more numerous will be the possible arrangements of those atoms, and the greater therefore the number of rational formulæ which may be assigned to the compound." It thus appears that rational formulæ are as yet little better than possible arrangements, and the insufficiency of the chemical evidence to determine one in preference to another is admitted. To those who consider the truth in nature as of paramount importance, here is a motive for seeking evidence in departments not strictly chemical, more especially since the discovery of the alcohol-mixed metals, and of hydrocarbons having the constitution of ethyle and methyle, seem to be regarded as settling the point in favour of the modern theory.

A hydrocarbon, $C^2H^5=29$, has been discovered in addition to etherine, $C^2H^4=28$, and olefiant gas, $CH^2=14$. Ether, $C^2H^5O^{\frac{1}{2}}=37$, is considered better represented by $C^2H^5.O^{\frac{1}{2}}$ than by $C^2H^4.HO^{\frac{1}{2}}$; alcohol better represented by $C^2H^5.O^{\frac{1}{2}}.HO^{\frac{1}{2}}$, with some mark or expression appended to signify that such a molecule occupies two volumes, than by $CH^2.HO^{\frac{1}{2}}=23$ without any mark.

As the chemical evidence with respect to the formation of ether and decomposition of alcohol is not deemed of importance, I may revert to a fact which appears from the researches of MM. Fabre and Silberman, namely that the quantity of heat evolved by the combustion of alcohol and ether is exactly equal to what it would be if they were simple hydrates of CH^2 and C^2H^4. These (and all other?) hydrocarburets, when burnt, evolve the same amount of heat as if the carbon and hydrogen were burnt separately; so that the force of union between the C and H of these bodies appears to have no sensible dynamical equivalent as between the C and O of carbonic acid, and H and O of water, which unite with an immense evolution of force. The heat developed in burning a unit weight of ether, $C^2H^4.HO^{\frac{1}{2}}=37$, is 16249; in burning the C^2, or $\frac{24}{37}$ths of a unit weight of carbon separate, the heat is 9430, and the H^4, or $\frac{4}{37}$ths of a unit weight of hydrogen separate, is 6700: these two added make 16130, being almost exactly the same as the unit of ether. If the other element of hydrogen belonged to a hydrocarbon and not to a water-molecule, there would be 1675 to add. The same mode applied to alcohol and pyroxylic spirit gives results even more striking, and precisely such as might be expected if they were hydrates of C^2H^4, CH^2, $C^{\frac{1}{2}}H$ respectively.

What is the nature of the evidence afforded by the organo-metallic bodies? "Ethyle is a colourless gas burning with a brilliant white flame, and decomposes at less than a red heat,"—so far being similar to olefiant gas. Its specific gravity is 29, the carbon being 24, and hydrogen 5. The sulphide and bisulphide of ethyle have vapour-densities such that they may be regarded either as primary compounds of this gas and sulphur, or secondary compounds of etherine and the sulphuretted hydrogens. The chloride of ethyle has vapour-density $32\frac{1}{2}$, being 18 chlorine, 12 carbon, and $2\frac{1}{2}$ hydrogen. It may thus be viewed as *half* a gas-molecule of chlorine combined with *half* a gas-molecule of ethyle, or *one* gas-molecule of olefiant gas in union with *one* gas-molecule of muriatic acid. The same remark applies to the iodide and bromide, also to the oxide (ether).

The hydride of ethyle has specific gravity 15, the carbon being 12 and hydrogen 3, being *half* a molecule of ethyle combined with *half* a gas-molecule of hydrogen; or it has nothing to do with ethyle, but is a primary hydrocarbon isomeric if not identical with methyle. Marsh-gas, called hydride of methyle, has its molecule composed of *half* a gas-molecule of methyle united with *half* a gas-molecule of hydrogen; or it has nothing to do with methyle, but is a primary hydrocarbon.

Zincethyle has vapour-density 61·5, showing that a vapour-molecule is composed of 32·5 zinc, 24 carbon, and 5 hydrogen. If it is a primary compound of one molecule zinc with one of ethyle, the combining affinity of the elements for oxygen must in consequence be much exalted, because it takes fire instantly on coming in contact with the air. Or it may be viewed as a secondary compound of a metallic hydride with etherine. There is at least one metallic hydride, the hydride of copper, which presents the same exaltation of affinity in respect to chlorine. Its composition is 1 hydrogen to 64 copper, or $\frac{1}{2}$ to 32, which gives the formula $H^{\frac{1}{2}} Cu$. When treated with muriatic acid, $Cl^{\frac{1}{2}} H^{\frac{1}{2}}$, the $H^{\frac{1}{2}}$ of the cupride unites with the $H^{\frac{1}{2}}$ of the acid to form H, which is the hydrogen evolved, and the Cu unites with the $Cl^{\frac{1}{2}}$ to form the dichloride of copper, $Cl^{\frac{1}{2}} Cu$. This action is stated to be very remarkable (Graham, vol. ii. p. 96), inasmuch as metallic copper is scarcely acted on by muriatic acid; *i. e.* Cu and $H^{\frac{1}{2}} Cl^{\frac{1}{2}}$ are passive, while $Cu H^{\frac{1}{2}}$ and $Cl^{\frac{1}{2}} H^{\frac{1}{2}}$ are intensely active. At p. 517 there is the following remark:—"To reduce these bodies (viz. the elementary) to the same standard, it is necessary to assume that each molecule of an elementary body in the free state consists of two elementary atoms." And at p. 517, "It appears indeed as if [such] atoms could not exist in a state of isolation. *An atom* [*i. e.* $O^{\frac{1}{2}}$, $H^{\frac{1}{2}}$, $Cl^{\frac{1}{2}}$, &c.] *of an elementary body must unite either with an atom of another element or with one of its own kind.*" We thus may see the propriety in every way of viewing them and expressing them as fractions of a molecule: with such symbols the peculiar action of the hydride of copper on muriatic acid no longer appears remarkable.

In the case of zincethyle, considered as composed of a hydrocarbon $C^2 H^4$ (etherine) and a metallic hydride, the latter must be Zn combined with H, a whole gas-molecule of hydrogen. The action on being presented to the atmosphere is ZnH presented to O of the atmosphere, *i. e.* to whole gas-molecules of oxygen—causing, with their exalted affinity, the division $O^{\frac{1}{2}} O^{\frac{1}{2}}$, one of which burns with Zn, forming the protoxide of zinc, $ZnO^{\frac{1}{2}}$; the other burning with H to form water, $HO^{\frac{1}{2}}$.

The apparent exaltation of affinity in these and other similar cases may be caused simply by the atoms of metal existing unfettered by cohesion, the integral of which bears comparison with the more intense of the chemical forces. The H in its connexion with the metal may be compared to the H in the hydrocarburet $C^{\frac{1}{2}}H$, which, being one-half CH^2 the olefiant, and one quarter $C^2 H^4$ etherine, we may by analogy hold as possible, yet, like a metallic hydride, having the affinity of its constituents for oxygen and the other electro-negative elements too exalted to exist in a state to be recognized. In pyroxylic spirit it appears combined with water, $C^{\frac{1}{2}} H . HO^{\frac{1}{2}}$; in chloride of methyle, $C^{\frac{1}{2}} HCl$, combined with chlorine, &c.

However this may be, it cannot be disputed that chemical elements unite in modes that, estimated dynamically, are specifically distinct. Besides those above noted, there seems to be another, as in the case of chloride of nitrogen, where not only force, but heat and light accompany the disunion of elements—is evolved instead of being absorbed. In the usual case, force or heat causes the separation; in the fulminate, the separation causes (apparently) force and heat. Such considerations must enter into any system of classification of chemical compounds that is not quite artificial and arbitrary. To include under the same type a liquid of which the elements are in a burnt condition, with another the major part of which is in an unburnt condition, is simply to ignore the dynamical theory of heat, to reject as a guide a principle that has never failed to yield rich returns to those who have resigned themselves entirely to its guidance in the study of nature.

Edinburgh, August 1, 1863.

XXXVI. *Determination of the Length of the Waves of certain bright Lines in the Spectrum.* By Dr. JOHN MÜLLER *of Freiberg*[*]

As well-divided glass screens offer the simplest and most accurate means of ascertaining the length of the waves for the differently coloured rays, I have had recourse to an unexceptionably perfect screen that I recently obtained from Nobert, of Barth in Pomerania, to determine the length of the wave of the red lithium line (Li α), the yellow sodium line (Na α), and the blue strontium line (Sr δ). This screen had 2001 lines in the width of 4 Paris lines; the distance from the middle of one line to the middle of the following one, a magnitude which we shall designate by b, amounted to $0'''\cdot 002$. It was so placed on the centre stage of a small goniometer of Babinet's construction, that its plane was at right angles to the axis of the telescope provided with the slit. A small gas-lamp was then placed in-

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[*] Translated from Poggendorff's *Annalen*, vol. cxviii. p. 641, and communicated by W. G. Lettsom, Esq.

front of the slit, and the flame thereof was *coloured red by means of lithium.*

The telescope was now so directed that the cross-lines were brought to bear on the red lithium line of the first spectrum to the left, and then on the red lithium line of the first spectrum to the right, the reading of each of these positions being taken.

Designating the red lithium line of the first side-spectrum by Li α 1, the position of the nonius was for

Li α 1 right,	72° 41′,
Li α 1 left,	55° 28′;

consequently the angle of deviation of each of the red bands Li α 1 from the centre of the image of inflection gives $x=8^\circ.36'{\cdot}5$.

For the length of wave, λ, of the red lithium light we obtain

$$\lambda=b\sin x=0{\cdot}002\,.\,0{\cdot}149679=0'''{\cdot}00029936.$$

The deviation of the red lithium band Li α 2 from the centre of the inflection-image was measured in the same manner, and its value, y, was found to be $=17^\circ\ 27'{\cdot}5$. In conformity therewith, the length of wave of the red lithium light is ascertained to be

$$\lambda=b\frac{\sin y}{2}=0{\cdot}002\frac{\sin(17^\circ\ 27'{\cdot}5)}{2}=0{\cdot}00030000.$$

As a mean of these two measures, we therefore obtain as the length of wave of Li α the value

$$\lambda=0'''{\cdot}0002997,$$

or

$$\lambda=0{\cdot}0006733 \text{ millimetre.}$$

A similar measure was next carried out for *the yellow sodium line.* The result arrived at was for

Na α 1	$x=\overset{\circ}{7}\ \overset{\prime}{32}{\cdot}5$,	and therefore	$\lambda=\overset{\prime\prime\prime}{0}{\cdot}0002626$
Na α 2	$y=15\ \ 12{\cdot}5$	„ „	$\lambda=0{\cdot}0002623$
Na α 3	$z=23\ \ 8$	„ „	$\lambda=0{\cdot}0002619$
Consequently the mean is			$\lambda=0{\cdot}00026227$
or			$\lambda=0{\cdot}0005918$ millim.

From Fraunhofer's measures we obtain for the dark line D of the solar spectrum, which, as we know, coincides with Na α, a length of wave of 0·0005888 millim. The result of my measures agrees therefore satisfactorily with those of Fraunhofer, when one takes into consideration that the division on the goniometer does not admit of an accurate reading of less than 1 minute. The result arrived at by me differs from that of Fraunhofer by about ½ per cent., and the extreme values obtained by Fraunhofer himself differ by ¼ per cent.

For the blue strontium line Sr δ I obtained

Sr δ 1 $x = 5^{\circ}\ 54'$, and hence $\lambda = 0'''\cdot0002056$
Sr δ 2 $y = 11\ 49$, „ $\lambda = 0\cdot0002048$
The mean therefore is $\lambda = 0\cdot0002052$
or $\lambda = 0\cdot0004631$ millim.

The setting the instrument on the blue strontium line involved some difficulty, owing to its little intensity.

The attempt to determine in the same manner the length of wave of the two potassium lines was not successful, because they were too weak even to be seen at all.

To this communication I subjoin the remark that the bright spectrum-lines of certain coloured flames appear very appropriate for determining the index of refraction of various substances for the differently coloured rays. Hitherto, as we know, Fraunhofer's lines A, B, C, D, E, F, G, and H have been employed for this purpose.

The observation of the bright spectral lines of coloured flames is at all events easier of application, and may be had recourse to at any time, whereas the employment of Fraunhofer's lines is dependent on the weather, and requires a suitable locality. As long ago as 1859, Plücker suggested for the determination of the index of refraction the employment of the three bright lines obtained by the passage of the spark from a Ruhmkorff's coil through rarefied hydrogen, instead of Fraunhofer's lines. And Landolt has already carried this into effect with respect to a series of different fluids (Poggendorff's *Annalen*, vol. cxvii.). But it is even yet simpler and less troublesome to make use of a Bunsen's burner than to put a Ruhmkorff's coil in action. The lines best suited for the end in view would probably be

The red potassium line . .	Ka α.
The red lithium line . . .	Li α.
The yellow sodium line . .	Na α.
The blue strontium line . .	Sr δ.
The violet potassium line. .	Ka β.

To which might probably be added the *green thallium line* when this metal shall have become somewhat more accessible. All these lines, owing to their great distinctness, are very suitable for accurate determinations.

Freiberg, March 1863.

I am happy to have it in my power to annex already to the above notices the length of wave of *the green thallium line*.

Professor von Babo, during the course of the last vacation, received from Professor Erdmann of Leipzig a small quantity of sulphate of oxide of thallium, and he gave me as much thereof as I required for effecting the measurements, the results of which were the following:—

Th α 1 on the right . . .	$126^\circ\ 15'\cdot5$,
Th α 1 on the left . . .	$139\ \ 52\cdot5$,

whence

$$x=6^\circ\ 48'\cdot5, \text{ and } \lambda=0'''\cdot000237088.$$

I obtained further for

Th α 2 on the right . . .	$146^\circ\ 46'$,
Th α 2 on the left	$119\ \ 22$;

and hence

$$y=13^\circ\ 42', \text{ and } \lambda=0'''\cdot000236838.$$

As a mean, therefore,

$$\lambda=0'''\cdot00023696,$$

or

$$\lambda=0\cdot0005348 \text{ millim.}$$

Freiberg, April 25, 1863.

XXXVII. *On the Stability of Arches.*

By Archdeacon J. H. Pratt.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

HAVING been asked to give an opinion, on theoretical grounds, regarding the stability of a bridge about to be erected in this country, I calculated some formulæ, which, as I have not seen the subject treated in this way anywhere else, I think may be of use to some of your readers.

Fig. 1.

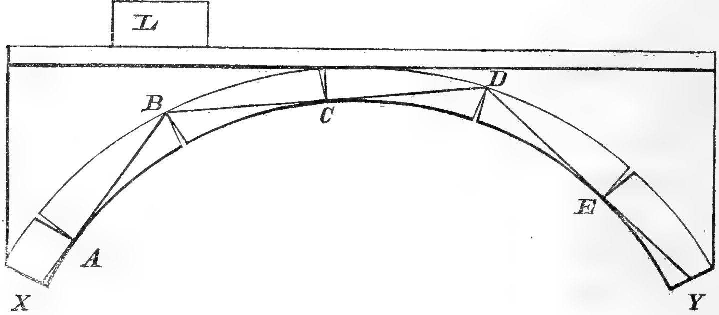
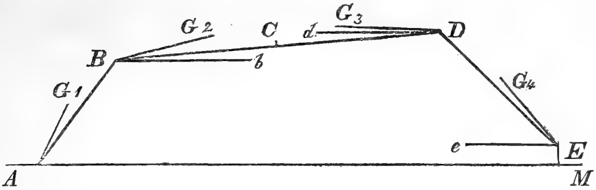


Fig. 2.



$\angle \mathrm{BAM}=\alpha_1$, $\angle \mathrm{CB}b=\alpha_2$, $\angle \mathrm{CD}d=\alpha_3$, $\angle \mathrm{DE}e=\alpha_4$,
$\angle \mathrm{G_1AB}=\beta_1$, $\angle \mathrm{G_2BC}=\beta_2$, $\angle \mathrm{G_3DC}=\beta_3$. $\angle \mathrm{G_4E}e=\beta_4$,
$\mathrm{AB}=a_1$, $\mathrm{BC}=a_2$, $\mathrm{CD}=a_3$, $\mathrm{DE}=a_4$, $\mathrm{AM}=m$, $\mathrm{ME}=n$,
$\mathrm{AG_1}=b_1$, $\mathrm{BG_2}=b_2$, $\mathrm{DG_3}=b_3$, $\mathrm{EG_4}=b_4$.

$h_1, \mathrm{H}_1, h_2, \mathrm{H}_2, h_3, \mathrm{H}_3, h_4, \mathrm{H}_4$ are the lengths of the projections of $\mathrm{AG_1}$, AB, $\mathrm{BG_2}$, BC, $\mathrm{DG_3}$, DC, $\mathrm{EG_4}$, ED upon the horizontal line AM.

I. Suppose a load, L (fig. 1), to be placed on the bridge over the arch, and not in the centre. B is the point of the extrados under the middle of the load. Draw BAX, BCD, DEY tangents to the intrados. Imagine the arch at the points A, B, C, D, E to be divided into four parts, and to be forcibly held in the position shown in the diagram, in which the arch is slightly open at joints through those points. If the equilibrium of the arch is stable, the parts will fall back into their places when left to themselves; if unstable, the arch will fall.

Let W_1, W_2, W_3, W_4 be the weights of these four portions of the arch, including the total load each carries (not omitting L). G_1, G_2, G_3, G_4 (in fig. 2) the centres where these weights may be supposed to be collected. Let V be the height of the centre of gravity of these four weights above the horizontal AM. Then

$$(\mathrm{W}_1+\mathrm{W}_2+\mathrm{W}_3+\mathrm{W}_4)\mathrm{V}=\mathrm{W}_1 b_1 \sin(\alpha_1+\beta_1)$$
$$+\mathrm{W}_2\big(a_1\sin\alpha_1+b_2\sin(\alpha_2+\beta_2)\big)+\mathrm{W}_3\big(n+a_4\sin\alpha_4+b_3\sin(\beta_3-\alpha_3)\big)$$
$$+\mathrm{W}_4(n+b_4\sin\alpha_4).$$

Also the angles are connected by the following relations (N.B. $\alpha_3=\alpha_2$, but I shall at first reason on the more general supposition that they are not necessarily so),

$$a_1\cos\alpha_1+a_2\cos\alpha_2+a_3\cos\alpha_3+a_4\cos\alpha_4=m,$$
$$a_1\sin\alpha_1+a_2\sin\alpha_2+a_3\sin\alpha_3-a_4\sin\alpha_4=n.$$

Differentiating these with respect to the angles, which alter when the arch is slightly moved out of its position,

$$a_1\sin\alpha_1\,\delta\alpha_1+a_2\sin\alpha_2\,\delta\alpha_2+a_3\sin\alpha_3\,\delta\alpha_3+a_4\sin\alpha_4\,\delta\alpha_4=0,$$
$$a_1\cos\alpha_1\,\delta\alpha_1+a_2\cos\alpha_2\,\delta\alpha_2+a_3\cos\alpha_3\,\delta\alpha_3-a_4\cos\alpha_4\,\delta\alpha_4=0.$$

I have kept α_2 and α_3 general that I might obtain these equations by differentiation. Now consider them equal, and obtain $\delta\alpha_3$ and $\delta\alpha_4$ in terms of $\delta\alpha_1$ and $\delta\alpha_2$ by these equations. Multiply the first by $\cos\alpha_2$ or $\cos\alpha_3$, and the second by $\sin\alpha_2$ or $\sin\alpha_3$, and subtract,

$$\therefore\ a_1 \sin(\alpha_1-\alpha_2)\delta\alpha_1 + a_4 \sin(\alpha_3+\alpha_4)\delta\alpha_4 = 0,$$

$$\therefore\ \delta\alpha_4 = -\frac{a_1 \sin(\alpha_1-\alpha_2(}{a_4 \sin(\alpha_3+\alpha_4)}\delta\alpha_1.$$

Again, multiply the two equations by $\cos\alpha_4$, $\sin\alpha_4$, and add; then

$$a_1 \sin(\alpha_1+\alpha_4)\delta\alpha_1 + (a_2\,\delta\alpha_2 + a_3\,\delta\alpha_3)\sin(\alpha_3+\alpha_4) = 0,$$

$$\therefore\ \delta\alpha_3 = -\frac{a_2}{a_3}\delta\alpha_2 - \frac{a_1 \sin(\alpha_1+\alpha_4)}{a_3 \sin(\alpha_3+\alpha_4)}\delta\alpha_1.$$

Now differentiate the expression for V, observing that $\beta_1 \ldots$ are constant, and substitute the values of $\delta\alpha_4$ and $\delta\alpha_3$; also substitute $h_1 \mathrm{H}_1 \ldots$ which have been explained above. Hence

$$\begin{aligned}
&(\mathrm{W}_1+\mathrm{W}_2+\mathrm{W}_3+\mathrm{W}_4)\delta\mathrm{V} \\
&= \mathrm{W}_1 h_1\,\delta\alpha_1 + \mathrm{W}_2(\mathrm{H}_1\delta\alpha_1 + h_2\delta\alpha_2) + \mathrm{W}_3(\mathrm{H}_4\,\delta\alpha_4 - h_3\delta\alpha_3) + \mathrm{W}_4 h_4 \delta\alpha_4 \\
&= \delta\alpha_1\left\{\mathrm{W}_1 h_1 + \mathrm{W}_2\mathrm{H}_1 - (\mathrm{W}_4 h_4 + \mathrm{W}_3\mathrm{H}_4)\frac{a_1 \sin(\alpha_1-\alpha_2)}{a_4 \sin(\alpha_3+\alpha_4)}\right. \\
&\qquad \left. + \mathrm{W}_3 h_3 \frac{a_1 \sin(\alpha_1+\alpha_4)}{a_3 \sin(\alpha_3+\alpha_4)}\right\} + \delta\alpha_2\left\{\mathrm{W}_2 h_2 + \mathrm{W}_3 h_3 \frac{a_2}{a_3}\right\}.
\end{aligned}$$

Suppose that AB and ED are produced to meet in the point F (not drawn in the diagram), and that the angles of the triangle FBD are called F, B, D. Then the above formula becomes

$$\begin{aligned}
&(\mathrm{W}_1+\mathrm{W}_2+\mathrm{W}_3+\mathrm{W}_4)\delta\mathrm{V} \\
&= \delta\alpha_1\left\{\mathrm{W}_1 h_1 + \mathrm{W}_2\mathrm{H}_1 - (\mathrm{W}_4 h_4 + \mathrm{W}_3\mathrm{H}_4)\frac{a_1 \sin \mathrm{B}}{a_4 \sin \mathrm{D}} + \mathrm{W}_3 h_3 \frac{a_1 \sin \mathrm{F}}{a_3 \sin \mathrm{D}}\right\} \\
&\qquad + \delta\alpha_2\left\{\mathrm{W}_2 h_2 + \mathrm{W}_3 h_3 \frac{a_2}{a_3}\right\}.
\end{aligned}$$

$\delta\alpha_1$ and $\delta\alpha_2$ are independent and arbitrary variations of α_1 and α_2: $\delta\alpha_1$ cannot be positive and $\delta\alpha_2$ cannot be negative, owing to the structure of the arch and the way in which the openings take place. Take the case where $\delta\alpha_2=0$, that is, suppose the point C slides slightly to the right, along CD. Then $\delta\mathrm{V}$, or the variation of height of the centre of gravity of the four weights (measured upwards), will be positive or negative according as

$$\mathrm{W}_1\frac{h_1}{a_1} + \mathrm{W}_2\frac{\mathrm{H}_1}{a_1} + \mathrm{W}_3\frac{h_3}{a_3}\frac{\sin \mathrm{F}}{\sin \mathrm{D}} \text{ is } < \text{ or } > \left(\mathrm{W}_4\frac{h_4}{a_4} + \mathrm{W}_3\frac{\mathrm{H}_4}{a_4}\right)\frac{\sin \mathrm{B}}{\sin \mathrm{D}}.$$

In the first case the arch will be stable, because the slight displacement raises the centre of gravity; and as this will fall, the openings will close up again when the arch is left to itself. In the latter case the centre of gravity descends, owing to the displacement of the parts of the arch, and will go on descending when the arch is left to itself, and therefore the arch will fall.

When any design of an arch is determined upon, it is an easy matter to take different portions of the load L and find all the quantities involved in the above formula by construction and measurement by a scale; and the character of the proposed arch for stability can be determined in this way.

Example.—Suppose W_1, W_2, W_3, W_4 are as 10, 6, 5, 9 *independently of* L, and $\sin B = \sin 55° = 0·819$, $\sin D = \sin 45° = 0·707$, $\sin F = \sin 80° = 0·985$: also let

$$\frac{h_1}{a_1}=\frac{1}{6},\quad \frac{H_1}{a_1}=\frac{1}{2},\quad \frac{h_3}{a_3}=\frac{3}{7},\quad \frac{h_4}{a_4}=\frac{2}{5},\quad \frac{H_4}{a_4}=\frac{9}{14}.$$

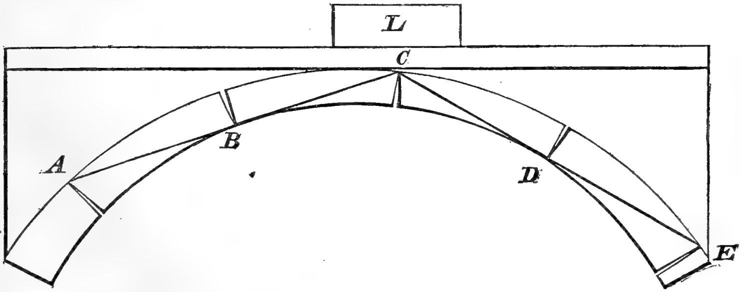
Putting these in the above formula of comparison, the two sides are 7·65 and 7·89. The first is less than the second, and therefore the arch would be in itself stable. Now introduce L, and suppose the centre of gravity to be over B, and half added to W_1 and half to W_2. Then the two sides of the comparison are

$$7·65+0·75\,L \text{ and } 7·89,$$

and therefore the arch will be at its limit between stability and instability when $L=0·32$, or about $\frac{1}{94}$th part of the weight of the whole bridge. Practical men must decide whether this would be safe.

II. Suppose the load L is near the crown of the arch, and that the arch is forcibly held in the position marked in fig. 3.

Fig. 3.



By reasoning precisely as before, and observing that in this case $\alpha_2=\alpha_1$ and $\alpha_3=\alpha_4$, we obtain

$$a_1\cos\alpha_1+a_2\cos\alpha_2+a_3\cos\alpha_3+a_4\cos\alpha_4=\text{constant},$$

$$a_1\sin\alpha_1+a_2\sin\alpha_2-a_3\sin\alpha_3-a_4\sin\alpha_4=\text{constant};$$

$$\therefore\ a_1\sin\alpha_1\,\delta\alpha_1+a_2\sin\alpha_2\,\delta\alpha_2+a_3\sin\alpha_3\,\delta\alpha_3+a_4\sin\alpha_4\,\delta\alpha_4=0,$$

$$a_1\cos\alpha_1\,\delta\alpha_1+a_2\cos\alpha_2\,\delta\alpha_2-a_3\cos\alpha_3\,\delta\alpha_3-a_4\cos\alpha_4\,\delta\alpha_4=0;$$

which, since $\alpha_2=\alpha_1$, $\alpha_3=\alpha_4$, give

$$a_1\,\delta\alpha_1+a_2\,\delta\alpha_2=0,\quad a_3\,\delta\alpha_3+a_4\,\delta\alpha_4=0.$$

Also

$$(W_1+W_2+W_3+W_4)V$$

$$=W_1b_1\sin(\alpha_1+\beta_1)+W_2\big(a_1\sin\alpha_1+b_2\sin(\alpha_2+\beta_2)\big)$$

$$+W_3\big(a_4\sin\alpha_4+b_3\sin(\alpha_3+\beta_3)\big)+W_4b_4\sin(\alpha_4+\beta_4)+\text{constant};$$

$$\therefore\ (W_1+W_2+W_3+W_4)\delta V$$

$$=\delta\alpha_1\left(W_1h_1+W_2H_1-W_2h_2\frac{a_1}{a_2}\right)+\delta\alpha_4\left(W_3H_4-W_3h_3\frac{a_4}{a_3}+W_4h_4\right),$$

$\delta\alpha_1$ and $\delta\alpha_4$ must both be positive. Hence the equilibrium will be stable if

$$W_1\frac{h_1}{a_1}+W_2\frac{H_1}{a_1}\text{ be not }<W_2\frac{h_2}{a_2}\text{ and }W_3\frac{H_4}{a_4}+W_4\frac{h_4}{a_4}\text{ not }<W_3\frac{h_3}{a_3}.$$

In the example aready given, the arch would be stable in this position of the load, however great the load is.

J. H. **Pratt.**

Murree, August 1, 1863.

XXXVIII. *On the Products of the Combustion of Gun-cotton and Gunpowder under circumstances analogous to those which occur in practice.* *By* L. v. Karolyi*.

THE gun-cotton manufactured according to Major-General Freiherr von Lenk's method at Hirtenberg near Wiener Neustadt, has, in consequence of special previous experiments, been used by the Genie Corps for mining-purposes; and notwithstanding the fact that there are still numerous difficulties in the way of its use for gun-charges, it is also used by the Royal Imperial Artillery for hollow projectiles.

The first-mentioned use led the Genie Committee, to which I belong, to cause experiments to be made which are calculated to give greater insight into the chemical deportment of this substance. Among these is the attempt to ascertain the products of combustion of the gun-cotton produced in Hirtenberg; and in

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* Translated from Poggendorff's *Annalen*, April 1863, by Dr. Atkinson, Royal Military College, Sandhurst.

the course of the investigation it seemed advisable to extend the method I used to gunpowder.

I. *Analysis of the Products of Combustion of Gun-cotton.*

The rapid deflagration of gun-cotton, and its necessary accompaniment, the bursting action, prevented me from using in the analysis of the products of combustion the excellent method which Professor Bunsen* devised, for obtaining the combustion products of gunpowder for the purpose of analysis. It was necessary to effect the combustion *in vacuo*, and for this purpose I used a eudiometer about a metre in length, in which, instead of two wires, as in the ordinary eudiometer, a single very thin platinum wire was drawn across. To this from 15 to 20 milligrammes of gun-cotton were affixed, the tube filled with mercury, and the Torricellian vacuum produced in the usual manner. By means of a galvanic battery the wire could be ignited, and hence the gun-cotton exploded; thereupon all eudiometrical operations were carried out in the tube in the usual manner, after a preliminary experiment had shown that the gas produced in this manner consisted of nitrogen, binoxide of nitrogen, carbonic acid, carbonic oxide, marsh-gas, and aqueous vapour.

	Volume.	Pressure.	Temp.	Vol. at 0° and 1 m.
Original volume . . .	374·53	0·1156	12°	42·37
In the steam-bath. . .	415·83	0·1768	95	54·56
After absorption of NO^2.	361·80	0·1078	11·2	37·47
After absorption of CO^2 .	328·06	0·0850	10·5	26·85
After addition of air . .	481·25	0·2372	12·3	109·26
After addition of oxygen.	497·56	0·2510	12·5	119·41
After explosion	466·21	0·2212	11·2	99·07
After absorption of CO^2.	430·57	0·1855	10·4	76·97
After addition of H . .	477·25	0·2301	11·7	105·29
After explosion . . .	443·38	0·1983	12·6	84·08

The quantity of NO^2 and CO^2 is obtained from the absorptions, the quantity of water from the increase in volume in the steam-bath; the quantity of nitrogen is obtained from the volume 76·97, which remains after removing the carbonic acid resulting from the combustible gases, by subtracting the uncombined oxygen and the nitrogen contained in the atmospheric air added; while the combustible gases are calculated from the formulæ in Bunsen's gasometric method,

Carbonic oxide $=P_1-\frac{2P_2-P}{3}$, Marsh-gas $=\frac{2P_2-P}{3}$,

Hydrogen . $=P-P_1$,

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* Phil. Mag. vol. xv. p. 489.

in which P is the quantity of combustible gases, P_1 the carbonic acid produced in combustion, P_2 the oxygen used in combustion.

Hence the gases from gun-cotton contain in 100 parts,

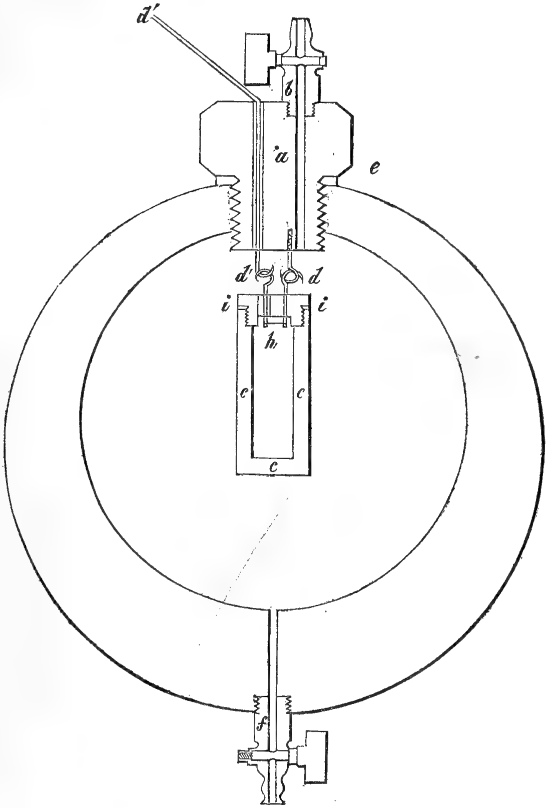
	By volume.	By weight.
Carbonic oxide . . .	28·55	28·92
Carbonic acid . . .	19·11	30·43
Marsh-gas	11·17	6·47
Binoxide of nitrogen .	8·83	9·59
Nitrogen	8·56	8·71
Carbon	1·85	1·60
Aqueous vapour . .	21·93	14·28
	100·00	100·00

The gun-cotton used had the average composition $C^{24}H^{17}N^{5}O^{33}$, from which, after subtracting the results of the analysis, the separated carbon is obtained which is included in the above analysis.

This simple and apparently faultless method has repeatedly shown that, by using a somewhat large quantity of gun-cotton under the same circumstances, when therefore the combustion takes place under comparatively greater pressure, the quantities of the products of combustion change, and the quantity of binoxide of nitrogen diminishes as the pressure increases. Hence the deoxidation of nitrogen-compounds during the combustion takes place the more completely the greater the work which the gun-cotton has to perform during its combustion.

This circumstance suggested to me the idea of exposing the gun-cotton during its combustion to a determinate resistance, and regulated so that it just gives way at the moment gun-cotton is completely burnt away. This condition led me to the experiment of placing a vessel filled with gun-cotton which offered the necessary resistance, in a 60-pound mortar, which was then exhausted and the gun-cotton exploded by galvanism.

The detailed plan of this apparatus is seen from the diagram. In the touchhole of the mortar a strong iron nut, *a*, is screwed, which by the aid of a good stuffer, *e*, enables the mortar to be made air-tight. The screw is provided at *b* with a short tube closed by a cock, by which the vacuum is obtained in the subsequent operations. At *d′* a copper wire, well insulated by gutta percha, passes through the nut, while at *d* is a small hook; to this and to the insulated copper wire the vessels filled with the gun-cotton intended for explosion are fixed, and, as the figure shows, allow the ignition of the charge to be effected. In the experiment the mortar is exhausted, the tap closed, and the platinum wire in the charge ignited by the current from six Smee's elements, and thus the vessel burst. It is readily seen that in this way it is possible not only to burn the gun-cotton under different resistances, but also to obtain the resultant gases free from atmospheric air.



The resistance of the explosion-vessels must be so chosen that the gas in the mortar, after explosion, has an excess of pressure of half an atmosphere, in order that it may subsequently be transferred to the measuring-vessels.

The explosion-vessels which I used were made according to the directions of the late Lieut.-Colonel Ebner, and consisted of hollow cast-iron cylinders closed at one end, while at the other was a nut through which the arrangement for a galvanic explosion passes. For this purpose the nut is provided with an excavation in which is a thin platinum wire fastened on the one hand to the insulated copper wire, and on the other to the copper wire which passes directly through the nut. Outside the cover the wires are bent into knots, which, as previously mentioned, serve to support the cylinders and to complete the voltaic circuit.

The weight of the gun-cotton whose gases shall fill the exhausted mortar of 5216 cubic centims. contents so that there shall be the tension above mentioned, I have empirically determined, and find that it is 10 grms. The fact that 10 grms. of cotton somewhat compressed occupy a space of 10·5 centimetres in length and 2 centimetres in diameter, determined the internal dimensions of the cylinder. The thickness of the sides of the cylinder was also obtained from an empirical experiment, which showed that with a thickness of 8 millims. the cylinder just exploded with production of flame, and that thus, in accordance with the condition stated, the gun-cotton burnt away the moment the cylinder burst. I must here mention a peculiar circumstance which attracted my attention in determining the thickness of the side of the cylinder, and which serves to characterize gun-cotton. For the above investigation I successively filled with gun-cotton cylinders 4, 6, and 8 millims. thick in the side and exploded them in a hole. Although the cylinders of 4 and 6 millims. in thickness contained comparatively a larger charge, the pieces produced were considerably larger than those of the cylinders 8 millims. in thickness. The former were often only split lengthwise; their cover and bottom remained unchanged, while the pieces of the cylinder of 8 millims. in thickness were scarcely larger than hazel-nuts.

The above bursting vessels might also probably be constructed of glass. Very strong thick glass tubes are taken, and at each end corks cemented in, one of which has been provided with a galvanic conduction and the small platinum wire. The length of the vessels and the thickness of their sides could then be regulated by the quantity of gas and the desired resistance.

The qualitative analysis of the products of the combustion of gun-cotton under the circumstances described gave carbonic oxide, carbonic acid, nitrogen, marsh-gas, and a trace of a sulphurous gas (probably a bisulphide of carbon compound), which, from its small quantity, escaped analysis and could only be detected by the smell. This probably arises from a small trace of sulphuric acid adhering to the gun-cotton, which either was not removed in washing, or by subsequent treatment with potash remained as sulphate.

The quantitative gas-analysis was made according to the following plan:—

Absorption Analysis.

	Volume.	Pressure.	Temp. °	Vol. at 0° and 1 metre.
Original volume . . .	114·78	0·6242	19·1	66·94
After absorbing CO^2 .	84·88	0·6048	20·2	47·81

Combustion Analysis.

	Volume.	Pressure.	Temp.	Vol. at 0° and 1 metre.
Original volume. . .	159·38	0·3144	19°·8	46·72
After addition of air .	238·48	0·4108	19·4	87·75
After addition of oxygen	293·77	0·4436	18·7	121·98
After explosion . . .	248·16	0·3954	19·1	91·71
After absorption of CO^2	181·12	0·3504	19·5	59·19
After addition of H .	251·65	0·4344	21·6	101·32
After explosion. . . .	174·85	0·3389	20·4	55·15

The quantity of carbonic acid obtained from the absorption analysis =19·13. The nitrogen found by known methods is 11·37 volume. The combustible gases are determined, by the formula already mentioned in the case of the analysis in the Torricellian vacuum, as

$$\text{Carbonic oxide} = P_1 - \frac{2P_2 - P}{3} = 26{\cdot}01,$$

$$\text{Marsh-gas} = \frac{2P_2 - P}{3} = 6{\cdot}51,$$

$$\text{Hydrogen} = P - P_1 = 2{\cdot}83\,;$$

in which $P=35{\cdot}35$, $P_1=32{\cdot}52$, $P_2=27{\cdot}44$. The gun-cotton used in the analysis corresponded to the formula $C^{24}H^{17}N^5O^{38}$, from which the water which cannot be directly determined, as well as the eliminated charcoal, may be found. Hence the mixture of gun-cotton gases consists of—

	By volume.	By weight.
Carbonic oxide . .	28·95	29·97
Carbonic acid . .	20·82	33·86
Marsh-gas . . .	7·24	4·28
Hydrogen . . .	3·16	0·24
Nitrogen . . .	12·67	13·16
Carbon	1·82	1·62
Aqueous vapour .	25·34	16·87
	100·00	100·00

As previously mentioned, the bursting vessels were filled with 10 grammes of cotton, which, by an accurate measurement, was found to yield a quantity of gas of 5740 cubic centims. at 0° and 1 metre pressure. The contents of the mortar at 16° and 0·7382 metre pressure amounts to 5292 cubic centims.; the quantitity of gas issuing at this pressure amounted to 2939 cubic centims.; hence 10 grms. gun-cotton yielded 8231 cubic centims. at 16° and 0·7382 metre pressure. If the quantity of gas is calculated from the results of the analysis, it is found that 10 grms. of gun-cotton yield 5764·2 of gases, which sufficiently agrees with the quantity actually found.

Comparing the results of the above-described analysis with those of the analysis in the Torricellian vacuum, it is found

(1) That the gases in both cases are combustible from the large quantity of carbonic oxide they contain.

(2) That the gases produced *in vacuo* contain a considerable quantity of binoxide of nitrogen, while by burning gun-cotton under appropriate resistance, the nitrogen compounds are deoxidized in favour of the carbon in marsh-gas and of the hydrogen, by which an increase in the carbonic oxide, carbonic acid, water, and a separation of free hydrogen are caused. Hence it follows that the red gun-cotton vapours can never occur if the entire gun-cotton is burnt away at the moment in which it begins to overcome the resistance offered to it.

These facts have a practical application in the use of gun-cotton to mining purposes.

II. *Analysis of the Products of Combustion of Gunpowder.*

After finding that the combustion of gun-cotton under circumstances resembling those which occur in mines is of decisive influence on the products evolved, it apeared desirable to investigate the combustion of gunpowder under similar circumstances. Unfortunately, since my investigations had a specifically military object, I could only analyse the Austrian small-arms and ordnance powder; hence only a superficial comparison can be instituted with the results which Prof. Bunsen obtained with freely-burning sporting-powder.

The combustion of the gunpowder was effected in the exhausted mortar in the same way as the combustion of the gun-cotton, with the exception that, on account of the smaller action of the powder, and in order to obtain as large a quantity of gas, the exploding vessels had to be larger, but with thinner sides than those in which the gun-cotton was exploded. The excavation in the cores was filled with meal powder.

The composition of the two kinds of powder used for investigation are obtained from the following analyses.

Ordnance Powder.—4·5487 grms. gave 3·3562 grms. saltpetre and 1·1923 grm. of a residue insoluble in water. Bisulphide of carbon dissolved 0·5823 sulphur. The remainder was charcoal.

Small-arms Powder.—8·8653 grms. contained 6·8408 saltpetre: the residue of 2·0245 grms. contained 0·765 grm. sulphur, and there remained 1·2595 grm. of charcoal. The organic analysis of the charcoal, carefully freed from sulphur, gave—

For Ordnance Powder.

Carbon	81·200
Hydrogen	2·865
Oxygen	13·599
Ash	2·336
	100·000

For Small-arms Powder.

Carbon	82·90
Hydrogen	2·99
Oxygen	12·14
Ash	1·97
	100·00

Hence the percentage composition of both these kinds of powder is as follows:—

Ordnance Powder.

	Nitrate of potash	73·78
	Sulphur	12·80
Charcoal.	Carbon	10·88
	Hydrogen	0·38
	Oxygen	1·82
	Ash	0·31
		100·00

Small-arms Powder.

	Nitrate of potash	77·15
	Sulphur	8·63
Charcoal.	Carbon	11·78
	Hydrogen	0·42
	Oxygen	1·79
	Ash	0·28
		100·00

The composition of the powder analysed by Bunsen and Schischkoff was—

	Nitrate of potash	78·99
	Sulphur	9·84
Charcoal.	Carbon	7·69
	Hydrogen	0·41
	Oxygen	3·07
	Ash	0·00
		100·00

For the qualitative analysis of the products of combustion, two cylinders were filled with the two kinds of powder, made air-tight, and successively exploded in the mortar in the manner described.

For both kinds there were found in the solid residue:—(1) sulphate of potash, (2) carbonate of potash, (3) hyposulphite of potash, (4) sesquicarbonate of ammonia, (5) sulphur, (6) charcoal, (7) sulphide of potassium. The latter, in the case of the small-arms powder, was only formed in very small quantities.

The gaseous products of combustion were (1) nitrogen, (2) carbonic acid, (3) carbonic oxide, (4) hydrogen, (5) sulphuretted hydrogen, (6) marsh-gas, and a very small quantity of a bisulphide of carbon compound, which was distinctly recognized by its odour as being that produced in the gases from gun-cotton. The whole mixture is colourless, and contains no fume or vapour.

(a) *Ordnance Powder.*

For the quantitative determination of the products of combustion, 36·8366 grms. were used.

The gas passed into three absorption-tubes amounted to 75·3 cubic centims.; the gas issuing from the mortar until the rest was under the atmospheric pressure amounted to 5480·7 cubic centims. at 16° C. and 0·749 metre pressure; under these circumstances the mortar holds 5216 cubic centims.; hence the above quantity yielded 7621·96 cubic centims. gas at 0° and 1 metre pressure.

The absorption-analysis produced—

	Volume.	Pressure.	Temp.	Vol. at 0° and 1 m. pressure.
Original volume	90·72	0·6028	16°·2	51·63
After absorption of CO^2 and HS	53·71	0·5705	14·3	29·12

From the estimation of the potash-bulb with iodine solution, it followed that the sulphuretted hydrogen corresponded to 0·44 division. Hence the above gas consisted of 0·44 vol. sulphuretted hydrogen, 22·07 carbonic acid, and of 29·12 nitrogen and combustible gases.

The explosion-analysis of the gas freed from sulphuretted hydrogen and carbonic acid and transferred to the eudiometer, was as follows:—

	Volume.	Pressure.	Temp.	Vol. at 0° and 1 metre pressure.
Original volume	113·26	0·2729	15°·6	28·8
After addition of air . .	183·36	0·3494	16·8	60·36
After addition of oxygen .	204·32	0·4295	16·4	71·79
After explosion	185·62	0·3522	15·4	61·89
After absorption of CO^2 .	167·90	0·3476	15·1	55·31
After addition of H . .	224·67	0·4068	16·2	86·30
After explosion	166·38	0·3355	15·7	52·79

By applying the formulæ of the gasometric methods already mentioned in the analysis of gun-cotton, since the gases are qualitatively the same, the values are obtained, for carbonic oxide =5·21, hydrogen =3·03, marsh-gas =1·38, and nitrogen =19·18.

Hence the total gas calculated for 100 parts consisted of—

42·74	vols.	Carbonic acid
0·86	,,	Sulphuretted hydrogen
10·19	,,	Carbonic oxide
2·70	,,	Marsh-gas
5·93	,,	Hydrogen
37·58	,,	Nitrogen
100·00		

The determination of the solid residue in the mortar was effected, after removing the gases, by digestion with water, which was drawn off through a tap in the bottom and rapidly filtered.

The results of the analyses were as follows:—

1. *Sulphide of Potassium.*—The entire filtered liquid was digested in three large boiling flasks with well-ignited oxide of copper, thereupon filtered, and the residue dissolved in fuming nitric acid. Treated with nitrate of baryta, it gave 0·1015 grm. of sulphate of baryta, which corresponds to 0·0478 grm. sulphide of potassium in the residue of 36·8366 grms. powder.

The liquid filtered from the oxide of copper was made up to 6 litres for the sake of further investigation.

2. *Carbonic Acid.*—A litre of this liquid gave with nitrate of silver a precipitate of carbonate and sulphide of silver. Treated with ammonia, the carbonate dissolved, and was separated from sulphide by a weighed filter, and precipitated in the filtrate as chloride of silver by means of hydrochloric acid. Its weight amounted to 3·0475 grms., which corresponds to 0·4687 grm. combined carbonic acid; hence in the entire residue there were 2·8126 grms. combined carbonic acid.

As a control, the carbonic acid was determined by chloride of manganese by mixing a litre of the liquid with a solution of this salt which had been previously fused; a precipitate of carbonate of manganese was obtained, the carbonic acid of which, determined in the usual manner, corresponded to 2·8337 grms. in the entire residue.

3. *Hyposulphite of Potash.*—The sulphide of silver (2) weighed on the tared filter, when dried at 120°, 0·2261 grm., which corresponds to 0·1733 grm. hyposulphite of potash; hence the entire residue contained 1·0400 grm. hyposulphite of potash.

A determination of the hyposulphite was also made by metrical analysis. A litre of the solution was acidulated with acetic acid, mixed with starch, and determined by means of a standard solution of iodine. A litre required 22·57 cubic centims. iodine solution; hence, according to the formula $s = \alpha \frac{2\,\mathrm{KO,\ S^2O^2}}{\mathrm{I}} = t$, in which $t = 22{\cdot}57$, $\alpha = 0{\cdot}000517$; this litre contained 0·1746 grm. hyposulphite of potash, and the entire residue 1·0476 grm. of this salt.

4. *Sesquicarbonate of Ammonia.*—According to Bunsen's method, a litre of the liquid was boiled with caustic potash, the distillate passed into a solution of hydrochloric acid of known strength, and the hydrochloric acid which had not been changed to chloride of ammonium determined with a standard ammonia solution. I found $\alpha = 0{\cdot}04853$, the quantity of hydrochloric acid taken; $t = 19{\cdot}87$, the number of divisions of an ammoniacal liquid which would have saturated a volume of hydrochloric acid equal to that taken with $t' = 41{\cdot}30$ divisions of the burette. Using the formula $x = \frac{2\,\mathrm{NH^4O,\ 3CO^2}}{2\,\mathrm{HCl}} \frac{\alpha(t'-t)}{t'}$, I found in the $\frac{1}{4}$ litre the sesquicarbonate of ammonia to be 0·041275 grm.; hence 0·9908 grm. corresponds to the entire quantity of sesquicarbonate of ammonia.

5. *Carbonate of Potash.*—Subtracting the carbonic acid in the sesquicarbonate of ammonia =0·5541 grm. from the total quantity found (2), =2·8337 grms., there remains a quantity corresponding to the carbonate of potash =2·2796 grms. Hence the entire residue contains 7·1498 grms. carbonate of potash.

6. *Sulphate of Potash.*—A litre of the liquid mixed with chloride of barium gave 3·0244 grms. sulphate of baryta, which corresponds to 2·2683 grms. sulphate of potash for one litre of liquid, and 13·6100 grms. for the whole residue.

7. *Potash.*—To determine the entire quantity of potash contained in the various salts, a litre of the liquid was carefully evaporated to dryness with sulphuric acid and ignited in a platinum vessel. 3·8466 grms. of sulphate of potash were thus

obtained, corresponding to 2·0786 grms. of potash. Hence 100 grms. of ordnance powder contain 33·85 grms. of potash, which agrees closely with the result of the direct analysis of the powder. After finding, by direct observation, that 36·8366 grms. of the powder furnished 7621·9 cubic centims. of gas, the composition of the products of combustion of this powder may be stated as follows:—

Sulphate of potash	13·61
Carbonate of potash	7·14
Hyposulphite of potash	1·04
Sulphide of potassium	0·04
Charcoal	0·94
Sulphur	1·73
Sesquicarbonate of ammonia	0·99
Nitrogen	3·60
Carbonic acid	6·40
Carbonic oxide	0·97
Hydrogen	0·04
Sulphuretted hydrogen	0·10
Marsh-gas	0·15
Loss	0·07
	36·82

in which sulphur and charcoal are calculated from the deficiency.

(b) *Small-arms Powder.*

34·153 grms. were used for the combustion. The quantity of the gaseous products was obtained from the following gasometric experiments:—

Absorption Analysis.

	Volume.	Pressure.	Temp.	Vol. at 0° and 1 metre pressure.
Original volume	136·94	0·6331	22°·1	80·21
After absorbing CO^2 and SH	75·04	0·5024	21·9	40·46

If the solution of the potash-bulb is determined with solution of iodine, it is found that 0·535 division corresponds to sulphuretted hydrogen, from which it follows that the above quantity of gas consists of—

Carbonic acid	39·22
Sulphuretted hydrogen	0·53
Combustible gases and nitrogen	40·46
	80·21

The explosion analysis with the transferred gas gave—

	Volume.	Pressure.	Temp.	Vol. at 0° and 1 metre pressure.
Original volume . . .	120·12	0·3432	20·4°	38·36
After addition of air . .	198·51	0·4263	20·3	78·77
After addition of oxygen.	230·33	0·4478	20·5	95·89
After explosion . . .	201·14	0·4323	19·6	81·47
After absorption of CO^2.	189·46	0·4276	21·0	75·23
After addition of H . .	261·02	0·4817	21·2	116·66
After explosion . . .	174·20	0·4130	18·3	67·43

If the formulæ previously used be applied, we get

Carbonic acid = 3·95, Marsh-gas = 2·29,
Hydrogen = 5·24, and Nitrogen = 26·88.

Hence the small-arms powder contains in 100 volumes—

Carbonic acid	48·90
Sulphuretted hydrogen .	0·67
Carbonic oxide	5·18
Marsh-gas	3·02
Hydrogen	6·90
Nitrogen	35·33
	100·00

The solid residue in the mortar was treated with hot water and the liquid filtered. No sulphide of potassium was found to be present. The analysis was executed in the same manner as with the ordnance powder.

From the results of this analysis and from those of the gas-analysis, direct measurement having shown that 34·153 grms. of ordnance powder give 7738 cubic centims. of gas, the following scheme for the products of combustion of the ordnance powder may be given:—

Sulphate of potash	12·354
Carbonate of potash	7·096
Hyposulphite of potash . . .	0·605
Charcoal	0·887
Sulphur	0·397
Sesquicarbonate of ammonia .	0·908
Nitrogen	3·432
Carbonic acid	7·442
Carbonic oxide	0·504
Hydrogen	0·047
Sulphuretted hydrogen . . .	0·079
Marsh-gas	0·167
Loss	0·235
	34·153

The results hitherto obtained for the products of combustion of both kinds of powder may now be compared with each other, and also with those obtained by Bunsen and Schischkoff in the analysis of sporting-powder:—

I. *Composition.*

		Sporting-powder.	Small-arms powder.	Ordnance powder.
	Nitrate of potash.	78·99	77·15	73·78
	Sulphur . . .	9·84	8·63	12·80
Charcoal.	Carbon . . .	7·69	11·78	10·88
	Hydrogen . . .	0·41	0·42	0·38
	Oxygen . . .	3·07	1·79	1·82
	Ash	0·00	0·28	0·31
		100·00	100·00	100·00

II. *Gaseous Products of Combustion by Volume.*

Nitrogen	41·12	35·33	37·58
Carbonic acid	52·67	48·90	42·74
Carbonic oxide	3·88	5·18	10·19
Hydrogen	1·21	6·90	5·93
Sulphuretted hydrogen . . .	0·60	0·67	0·86
Oxygen	0·52		
Marsh-gas		3·02	2·70
	100·00	100·00	100·00

III. *Total Products of Combustion by Weight.*

Sulphate of potash	42·27	36·17	36·95
Carbonate of potash	12·64	20·78	19·40
Hyposulphite of potash . .	3·27	1·77	2·85
Sulphide of potassium . . .	2·13	. .	0·11
Sulphocyanide of potassium .	0·30		
Nitrate of potash	3·72		
Charcoal	0·73	2·60	2·57
Sulphur	0·14	1·16	4·69
Sesquicarbonate of ammonia .	2·86	2·66	2·68
Nitrogen	9·98	10·06	9·77
Carbonic acid	20·12	21·79	17·39
Carbonic oxide	0·94	1·47	2·64
Hydrogen	0·02	0·14	0·11
Sulphuretted hydrogen . . .	0·18	0·23	0·27
Oxygen	0·14		
Marsh-gas		0·49	0·40
Loss		0·68	0·19
	100·00	100·00	100·00
Quantity of gas for a gramme of powder	190·00	226·59	200·91

A comparison of these results shows at first sight that, on the whole, the products of combustion of powder are little dependent on the manner in which the combustion takes place. But that the composition of the powder has a great influence, is seen from the fact that in Bunsen's powder, which contains much nitre, nearly 4 per cent. of this substance are found in the residue; while, on the other hand, in the residue of the ordnance powder, which contains less nitre, almost 7 per cent. of sulphur and charcoal are separated unburnt. The influence of the composition on the nature of the products of combustion is still more surprising. Where the reducing body preponderates, the combustion of the carbon is more imperfect. Whereas the gases of sporting-powder only contain 3 per cent. of carbonic oxide, the gas from ordnance powder contains nearly 10 per cent. The quantity of hydrogen and of marsh-gas increase in the same direction, so that the ordnance-powder gas contains nearly 20 per cent. of combustible gases. Hence it is not surprising that the gases of ordnance powder, as well as those of gun-cotton, may be ignited, as direct experiment showed, by a glimmering piece of wood.

There might apparently be no difficulty, from the results of these analyses, in arriving at a right composition of powder; yet in this respect practice prefers its own empirical path. But in any case the results obtained serve as an additional proof of the inaccuracy of the view which prevails in many chemical text-books and in almost all artillery institutions,—that powder must decompose, in burning, into sulphide of potassium, carbonic acid, and nitrogen. If practice has no other reason for the composition of powder than the possibility that these products may occur, it is certainly allowable to attempt to prove experimentally that the products of combustion, even under the circumstances which prevail in practice, can never be formed alone, and that, indeed, one of them—sulphide of potassium—in many cases is not formed at all.

XXXIX. *On* Newton's "*Foundation of all Philosophy.*"

By Professor Challis, *M.A., F.R.S., F.R.A.S.*[*]

IN the Third Book of the *Principia*, under the head of "Regula Tertia Philosophandi," Newton concludes the statement of certain views respecting the ultimate qualities of bodies by saying, "Et hoc est fundamentum philosophiæ totius." An assertion of this kind, coming from such an authority, and made so emphatically, may well excite an inquiry as to what is its precise

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[*] Communicated by the Author.

meaning, and how far it can be accepted as true. I will state at once that I have been induced to enter upon this inquiry because it will give occasion for discussing the characteristics and mutual relations of the theoretical and experimental departments of natural philosophy,—a discussion which the existing state of physical science urgently demands, much confusion of thought prevailing as to the principles, objects, and limits of the two departments, and in particular as to what is fundamental in theoretical research. If in consequence of the inquiry I should be led to criticise somewhat freely certain views of my contemporaries, I trust that I shall be considered to be influenced by no other personal feeling than the very natural desire to gain acceptance for the mathematical principles of natural philosophy which I have now for a long time urged on the attention of mathematicians, and which I consider to be indispensable to the progress of theoretical physics. I have reason to say that the physical theories I have proposed have not received the attention which is requisite for forming a judgment on their merits, chiefly because the minds of physicists are prepossessed with a totally different order of ideas, of comparatively recent growth, resting on arbitrary hypotheses, and having no reference to what, according to Newton, is the foundation of philosophy. This being the case, no course is left me but that of exposing the insecurity of the opposite views.

The dictum of Newton above quoted is preceded by an enumeration of those qualities of bodies which are to be regarded as "universal," and which admit of neither increase nor diminution (quæ intendi et remitti nequeunt). After premising the caution that, in regard to such quantities, fancies are not to be formed (somnia non confingenda), nor the analogy of nature as gathered from experience to be departed from, he proceeds as follows:—"The extension of bodies is only known by the senses, nor is it sensibly perceived in all; but because it belongs to all sensible bodies, it is affirmed of bodies universally. That various bodies are hard, we know by experience. But the hardness of the whole arises from the hardness of the parts; and hence not only of those bodies which are perceived by the senses, but also of all others, we justly conclude that the undivided particles are hard. That all bodies are impenetrable we gather, not from reasoning, but from sensation. Those which we handle are found to be impenetrable, and thence we conclude that impenetrability is a property of all bodies. That all bodies are moveable, and by a certain force, which we call *vis inertiæ,* continue in a state of motion or rest, we gather from these properties of visible bodies. The extension, hardness, impenetrability, mobility, and *vis inertiæ* of the whole arises from the extension, hardness, impenetrability, mobility, and *vis inertiæ* of the parts; and thence we conclude that all the least parts of all bodies are extended, hard, impenetrable, moveable, and endued with *vis inertiæ*. And this is the foundation of all philosophy." Now, although there is much here to which a modern metaphysician would object, and with good reason, the main idea running through the whole is undoubtedly true, and of the utmost scientific importance, viz. that the experience of the *senses* relative to masses is necessary and sufficient for revealing to us the universal properties of the ultimate constituents of the masses. This doctrine seems to have been very firmly apprehended in the Newtonian epoch of philosophy. Locke has expressed it in more precise terms than those of Newton. In the 'Essay on the Human Understanding' (Book 2, Chap. 4, § 1) he says, "The idea of solidity we receive by our touch . . . . There is no idea which we receive more constantly from sensation than solidity." Then, after saying that he does not object to the term impenetrability, but prefers solidity as being in common use to express the same idea, he proceeds to say, "This of all other seems the idea most intimately connected with and essential to body; so as nowhere else to be found or imagined but only in matter. And though our senses take no notice of it but in masses of matter of a bulk to cause a sensation in us, yet the mind having once got this idea from such grosser sensible bodies, traces it further, and considers it, as well as figure, in the minutest particle of matter that can exist; and finds it inseparably inherent in body wherever and however modified." Again in § 4 he draws a distinction between solidity and hardness, considering the latter to consist in a firm cohesion of parts of matter constituting a mass susceptible of change of figure. According to this definition, hardness, as admitting of degrees, cannot be ranked among Newton's universal properties susceptible of neither increase nor diminution. In fact, absolute hardness is not an idea distinguishable from impenetrability or solidity.

In the same Regula III. Newton admits that contiguous parts of bodies are separable, and that undivided parts may in the applications of mathematical reasoning be conceived to be divisible *in infinitum*. But he considers it to be uncertain whether distinct and hitherto undivided parts can be separated by the forces of nature. This doubt has been removed by the progress of chemical science, it being found that after any amount of synthesis and analysis the properties of the same portion of a simple body remain the same; which is, at least, presumptive evidence that it consists of parts which do not admit of being divided by natural forces. Thus the least parts of bodies are properly called *atoms*. There is also another point which experiment seems to have settled. As the atoms of bodies have extension, they must necessarily have form; and the question arises, what is their form? Since it is found that the movement of the parts of a fluid body *inter se* alters in no manner or degree the mutual relations of the parts or the sensible qualities of the body, it seems inevitably to follow that each atom has the same relation to space and to surrounding atoms in all directions from its position, and consequently that its form is *spherical.*

Inertia is a universal quality of bodies known from experience to belong to masses; and on the principle that a universal quality of masses reveals a universal quality of the constituent atoms, we may affirm that each atom is *inert.* But this property, which Newton calls "vis insita," and affirms to be "immutabilis," does not, in fact, appear from any *sensible* evidence to be susceptible of measure or degree. There may be more or less of inert matter, but not more or less of inertia. Accordingly all atoms are of the same intrinsic inertia, and differ only in magnitude.

The fundamental ideas respecting *matter,* to which the foregoing discussion leads, may be concisely expressed in the following terms:—All matter consists of inert spherical atoms of different, but constant, magnitudes.

The fundamental ideas respecting *force* admit of similar investigation. On this head Newton has said but little, but perhaps as much as the state of physical science in his day allowed him to say. With respect to the force of gravity, he has left on record (in Reg. III.) that he by no means considered it to be "essential to bodies," assigning as the reason for not admitting it to be such, that it is mutable as to space, changing with the distance of the attracting body. Also in the very last paragraph of the *Principia* he allows of the existence of "a certain very subtle spirit pervading gross bodies and concealed in them; by the force and agency of which the particles of bodies mutually attract at very small distances, and when contiguous, cohere; and electric bodies act at greater distances, both repelling and attracting neighbouring bodies; and light is emitted, reflected, refracted, inflected; and bodies are warmed; and all sensation is excited, and the limbs of animals are moved at will, &c." But true to the principle of deriving fundamental ideas from the indications of experience, he refrains from pursuing these views further, because "there was not a sufficient abundance of experiments proper for determining and exhibiting the laws of the action of this fluid." Not only were experiments wanted in Newton's time for such determinations, but the principles of the mathematical reasoning by which those laws might be deduced from appropriate experimental data were unknown.

The case, however, is different in the present state of our physical and mathematical knowledge, which allows of taking up on fresh grounds the inquiry as to what is fundamental and universal in the conception of force. After Newton's researches in physical astronomy had established the fact that masses attract each other with forces varying inversely as the square of the distance separating them, the attraction of gravity was assumed to be an inherent quality of bodies, resident in their minutest parts, and acting through space without the intervention of a medium. And certainly, on a first view, such an inference appears to be drawn from observation and experience relative to masses, just as from the sensible qualities of extension and form in masses it is inferred that these same qualities belong to atoms. But that action at a distance is not a universal condition of force is proved by the modern discovery that light and heat, which are modes of force, are transmitted through space by the intervention of a medium. If one kind of force requires a medium of transmission, why not another? Again, it is found by experience that the same portion of matter may attract or repel, according to circumstances. But inherent force cannot possibly be so changed by circumstances; in the same matter it must continue to be always the same. And in addition to these reasons for not admitting the idea of inherent force acting at a distance, the principle of deriving fundamental conceptions from the indications of the senses furnishes positive argument for a different idea. According to that principle, we must, in seeking for the fundamental idea of force, have regard to the indications of the sense of *touch*. Now by this sense we obtain a perception of force as pressure, distinct and unique, and not involving the variable element of distance which enters into the conception of force as derived by the sense of sight alone. Hence, according to such principles as those laid down by Newton in Regula III., we cannot do otherwise than conclude that the fundamental idea of force is *pressure*.

But where there is pressure there must be a substance which presses. Newton, as we have seen, admitted the existence of a subtle and all-pervading medium, to the action of which he attributed various dynamical effects. And in recent times even experimentalists speak familiarly of the *æther*, designating by that name a universal elastic fluid. The conception of an æther is of very ancient date, and the evidence for its reality has been gradually accumulating up to the present time; but perhaps its existence is most convincingly indicated by the explanations it supplies of phenomena of light, and of their analogy in several respects to phenomena of sound. Apart, however, from these reasons, drawn from experience and the antecedents of science, the supposition of a universal æther is demanded by the foregoing *à priori* conclusion relative to the fundamental idea of force. For if all force be in reality pressure, a substance which presses must be as universally diffused as force itself. With respect to the quality of the pressing body, observation and experience leave us no room to doubt that it must be fluid, because, assuming its existence, it is matter of experience that all sensible bodies move readily through it. Again, because all known fluids press more the more they are compressed, the same law must be predicated of the æther. We may even assume, taking experience for guide, that variations of its pressure are exactly proportional to variations of the density, there being an actual instance of this law in air of given temperature. Moreover this is the simplest relation conceivable; and in an investigation of the present kind a law less simple than the simplest is inadmissible. The rule of simplicity is recognized by Newton, where he says (Reg. I.), "Natura simplex est et rerum causis superfluis non luxuriat;" and again (Reg. III.), "Natura simplex esse solet et sibi semper consona." On the same ground of simplicity the æther must also be conceived to be always and everywhere of the same density, excepting so far as variations of density are superinduced by a state of motion.

The fundamental idea relative to *force*, to which these considerations conduct, may be thus expressed :—All force is action by pressure of a uniform fluid, the pressure of which is proportional to its density.

I have now to meet some objections which I anticipate will be raised against these views. It will probably be urged that since the progress of science has shown that bodies consist of discrete parts, there can be no continuity of surface, and the forms of solids are not actually what they appear to be; that absolute constancy of form is not actual, all known bodies being capable of yielding to force; and that as two solids cannot be made to come into actual contact, mutual pressure of their parts is not actual. After all this has been fully admitted, there remains the fact that we *do* acquire, by our senses and the power of abstraction, distinct perceptions of extension, of continuity of surface, of form, of constancy of form, of contact, and of pressure by contact. If this be denied, the common sense of mankind would reclaim. It is the very essence of philosophy to rest upon the foundation of common perceptions, and by reasoning from these to account for phenomena. If the reasoning be good, and an explanation of the phenomena be shown to result from the antecedent fundamental conceptions of matter and force, we may be said to know their *causes* in the sense in which we know anything else. Also, that the fundamental ideas, in their exact form, are not mere abstractions, but conceptions of existences as real as anything else is real, will be demonstrated by such explanations, and with more and more evidence as the number of the explanations is greater. This mode of philosophizing was fully recognized in Newton's time,—the necessity of appealing to observation and experience, both for the foundations of philosophy and for the data requisite to make and verify particular theoretical researches, being clearly understood. It is particularly to be remarked that these two uses of experience are broadly distinguished in Newton's writings. The latter is met with continually; but the former is placed under the head of Rules of Philosophizing, and is perhaps nowhere so distinctly stated as in the third Rule. Newton there says, "hoc est fundamentum," using the singular number to indicate that the *principle* of the method is the foundation of philosophy.

Another objection may be made to these views on the ground that if the pressure of the æther, and the relation of the pressure to the density, be among the necessary foundations of theoretical research, these facts must themselves remain unaccounted for. To this I might reply that if the fundamental ideas of matter and force suffice for the explanation of all phenomena and laws, they will account for the pressure, and law of the pressure, of air of given temperature, which are facts the same in kind as those attributed to the æther. But it is unnecessary to adopt this line of reasoning, which brings us to the verge of metaphysics. It is sufficient to maintain that, if all phenomena and laws can be shown by reasoning to be consequences of fundamental facts which are perfectly intelligible from sensation and experience, the phenomena and laws are intelligibly accounted for.

It is important further to remark that the fundamental ideas above laid down respecting matter and force are by no means arbitrary. I hold myself in no manner responsible for any of them. The course of the argumentation by which they were reached sufficiently shows, I think, that after the adoption of the Newtonian principle of seeking for universal and fundamental ideas from sensation and experience, no other than these could be arrived at. All that I have done is to employ the same principle in giving to the foundation of philosophy the larger extent which is required by the existing state of experimental physics. Not only, as I said above, are all phenomena explained if the explanations are shown to rest on this foundation, but I say further that there is absolutely no evidence that intelligible explanations can rest on any other. Those who choose may call the fundamental ideas *hypotheses*. They are, in fact, hypotheses in the proper sense of the Greek word, *i. e.* foundations. But being in that respect necessary, they are quite distinct from those which Newton referred to when he said "hypotheses non fingo," of which the history of physical science has afforded too many examples.

The foregoing discussion has prepared the way for making the proper distinction between the theoretical and the experimental departments of natural philosophy. The province of experiment is to discover facts and the relations of facts, i. e. *laws*, without respect to *causation*, or the operation of force. Kepler's discovery of the laws of the motions of the planets is an instance. Experiment also furnishes the data necessary for making theoretical calculation apply to actual instances, and for verifying the results of such calculation. The province of theory is to deduce, by reasoning, *from the fundamental ideas of matter and force*, the explanations of facts and laws established by experiment, and so to refer them to their *causes*. In the experimental department, a law is a grouping of observed facts; in the theoretical, the law is shown to be the consequence of certain primary facts. Every fact and every law which experiment makes known, is a problem for the theorist to solve by mathematical reasoning.

It should here be remarked that physical astronomy does not *immediately* come under the above definition of theory, because, while it refers the observed motions of the heavenly bodies to the action of forces, it rests on two particular hypotheses, made *pro hâc vice*, viz., that all bodies attract in proportion to their masses, and that the attraction varies according to the law of the inverse square. The truth of these hypotheses is established by the coincidence of the results of calculation founded on them with facts of observation. That being the case, the hypotheses may be regarded as *facts*, to be placed in the category of facts in general, which require to be referred, for their explanation, to the fundamental ideas of matter and force. Thus the law of gravity, being a quantitative law, should be deducible by reasoning.

The theoretical reasoning above spoken of is, of course, mathematical reasoning conducted by symbols. It would not be possible, without the aid of such means, to embrace with the deductive faculty the consequences of the fundamental hypotheses. In physical astronomy the symbolic reasoning consists in the formation and solution of differential equations of the first order in regard to the number of variables. A general physical theory in which force is regarded as the action of a fluid medium by pressure, evidently demands the determination by mathematics of the laws of the motion and pressure of the fluid, to effect which the application of a higher and more comprehensive order of differential equations is required. Perceiving the absolute necessity of this research for making progress in theoretical physics, I have given special attention to the application of partial differential equations in hydrodynamics, and, after a long-sustained intellectual effort, succeeded at length in demonstrating certain propositions which hold the same place relatively to general theoretical physics that the dynamical problems which Newton first solved held with respect to physical astronomy. Much more, I am fully aware, is required to be effected in the same direction. But I claim to have done for general physics what Newton did for physical astronomy; that is, to have entered upon the appropriate course, and to have overcome the initial difficulties. Those who would dispute this claim, must either call in question the truth of the hydrodynamical theorems, or contest their general applicability to theoretical physics. I am prepared to maintain both these positions.

A few more general remarks respecting the relation of theory to experiment remain to be made. It is admitted that scientific knowledge does not consist merely in acquaintance with the facts and laws which experiment reveals; to be complete, it must embrace the results of experiment by a *theory*—that is, as the term implies, by a perception of their causes resting on an intelligible basis. For proof that theory is recognized to be the end of experiment, I may appeal to the attempts which are constantly being made by experimentalists to theorize—that is, to assign causes. "Felix qui potuit rerum cognoscere causas." But, because experiment is thus subordinate to the establishment of theory, those experiments are the best which are best adapted to this purpose,—on which account correct theoretical notions are of much importance to the experimentalist, as serving to control and guide the course of his experiments. In this respect I venture to affirm, from acquaintance with the theoretical applications of experiments, that experimental research might be greatly aided by the recognition of the fundamental ideas respecting matter and force which I have so much insisted upon in this communication, and by the acceptance of the mathematical results which I have obtained relative to the laws of the motion and pressure of the ætherial medium.

The theoretical principles which it is the object of the foregoing discussion to explain and to establish, are precisely those on which are founded the theories of light, heat, gravity, electricity, galvanism, and magnetism which I have proposed in various communications to this Journal. I might appeal to the large number of successful explanations of phenomena which those theories exhibit, for evidence of the truth of the principles and the correctness of the deductive reasoning applied to them. But I feel it to be vain to expect that the appeal will be attended to, or the explanations receive any consideration, so long as the minds of physicists are preoccupied with modes of philosophizing totally at variance with that which I have adopted. For this reason I propose to conclude this communication with a brief exposition of the principles of the antagonistic methods.

The prevailing physical theories may be classed under two heads, sufficiently distinguished in their general characteristics. In the one class, the atomic constitution of all substances is admitted; the existence of the æther is recognized; certain movements of the atoms, both of the æther and of the grosser bodies, relatively to each other, and about axes they are supposed to have, are assumed; dynamical effects are attributed to these motions; an undefined use is made of the word "polarity;" and the application of differential equations is in a great measure dispensed with. An example of this method of philosophizing is exhibited by the work on 'Matter and Æther,' criticized in the Number of this Magazine for last April, which shows most remarkably how easy it is to invent causes of phenomena, if all reference to an independent foundation of philosophy, such as that insisted upon by Newton in Regula III., be omitted, and the necessity of deduction by mathematical reasoning be consequently avoided. Theorizers of this class have recourse to personal conceptions, which they form *ad libitum*, and which, undoubtedly, are to be ranked among the "somnia" that Newton refers to. Also these hypotheses are generally such as require explanation quite as much as the phenomena they are supposed to explain, and therefore do not advance scientific knowledge a single step.

The hypothesis of the isotropic constitution of the æther, invented to account for phenomena of light, is very closely allied to the class of hypotheses just spoken of. If it should be more successful than it has as yet been in explaining phenomena, it would still require to be itself explained. Having already so often expressed my views on the undulatory theory of light, I will only remark here that the necessity for the isotropic hypothesis has arisen from the adoption of a theory which does not recognize the fundamental ideas of matter and force, and consequently does not regard the æther as a *continuous fluid.*

The other class of physical theories, which is now much in vogue, is characterized by the deduction of generalizations from special facts and laws. The most complete exhibition of this method that I am acquainted with is contained in the article "On Energy" in 'Good Words' (Oct. 1862), to which I have before referred in a communication "On the Source and Maintenance of the Sun's Heat" (Phil. Mag. for June 1863). Professors Thomson and Tait consider that "the simple experiment of melting two pieces of ice by rubbing them together" justifies the general conclusion that "sensible heat is *motion.*" And again, the *law* of the dynamical equivalence of heat, established by experiment, is made the foundation of a dynamical *theory* of heat, and of one more comprehensive, termed the "Conservation of Energy." This mode of philosophizing avowedly belongs to the age of Bacon. It certainly is not the philosophy of the Newtonian epoch. A philosopher of Newton's school would regard the above-mentioned experiment, or the still more significant one of making a bar of iron red-hot by repeated blows, as simply a fact which he is required to explain by a course of reasoning founded on ultimate principles. So the law of the dynamical equivalence of heat would only be looked upon as a law which must admit of explanation by the proper course of reasoning starting from the same fundamental principles. If these explanations be given, all the demands of science are satisfied. It would be contrary to the principle of this method to theorize on any other foundation than that of ultimate ideas respecting matter and force, and their necessary concomitant the æther.

But let us look a little more closely into the above generalizations. Heat is affirmed to be motion. But heat, whatever else it be, is *force*, for it does what only force can do. The very law of the dynamical equivalence of heat, of which Professor Thomson has made so much use, proves that heat is force; for what but force can have a *dynamical* equivalent? Now force is not motion, but something extraneous which causes a *change* of motion. How then can heat be defined or described as motion? I observe that Professor Tyndall calls heat "a mode of motion;" but as this designation only implies that there are other modes of motion besides heat, and does not admit the idea of force, it is open to all the foregoing objections. Another objection may be raised on the ground that to assert heat to be motion is to contradict common understanding. Let us suppose an unscientific person, after witnessing the experiment of striking a bar of iron till it was red-hot, to ask a philosopher what was the cause of the heat, and to get the answer, "heat is motion;" he might reasonably reply, "I know what heat is, and what motion is, and I *know* that heat is not motion." The logic of this answer, which amounts to saying that the language of the philosopher is inconsistent with common experience, is irresistible, because scientific language is not exempt from the necessity of fulfilling the condition of informing common understandings.

For the sake of contrasting this mode of philosophy with that which I am advocating, I will now give the answer to the above question which is supplied by the principles of the latter. The hammer on striking the bar of iron is suddenly arrested in its course, and the force which stops it is the upward resistance of the superficial parts of the bar. But as this resistance does a great deal of work in a very short time, the constituent atoms must run into closer proximity to supply the requisite force, the resistance of pressure being universally greater the greater the density of the atoms. On the cessation of the shock the atoms tend to return, through a succession of vibrations, to their normal positions; but the repetition of the blows maintains and intensifies the vibrations. The vibratory motions of the atoms in the æther causes vibrations of the æther, the dynamical action of which is heat. (This assertion rests on my mathematical theory of heat.) Heat-vibrations are always accompanied by light-vibrations. (This also is a mathematical deduction.) The light-vibrations become sensible to the eye when of sufficient intensity. The same explanation applies, so far as regards the production of heat, to the experiment of rubbing two pieces of ice together. In both experiments mechanical force is employed to produce the heat; both are simple instances of the law of the dynamical equivalence of heat. Necessarily, therefore, regard being had to that law, heat is a *mode of force*. I cannot imagine a reason why in drawing an inference respecting the quality of heat from these facts, the word "force" should be avoided. The foregoing explanation of these instances may be considered to be some approximation towards an *à priori* theory of the law of dynamical equivalence.

Again, the generalization which Professor Thomson has based on the law of the dynamical equivalence of heat, is open to weighty objections both in respect to the philosophic principles it involves, and the conclusions drawn from it. I have already urged that a theory cannot be based upon a particular experimental law, the province of theory being to account for a law by mathematical deduction from universal and fundamental hypotheses. On this ground I object to calling the above-mentioned generalization "The Dynamical Theory of Heat," nothing having hitherto been published in science to which those terms properly apply except the theory of heat which I have proposed in the Philosophical Magazine for March 1859. This diversity of view as to the foundation of a dynamical theory of heat leads to widely different results. For instance, Professors Thomson and Tait deduce from "the contemplation of dynamical energy and its laws of transformation in dead matter," that "all energy tends ultimately to become heat, which cannot be transformed into any other modification;" and consequently that "we must conclude that when all the chemical and gravitation energies of the universe have taken their final kinetic form, the result will be an arrangement of matter possessing no realizable potential energy, but uniformly hot—an undistinguishable mixture of all that is now definite and separate—chaos and darkness as in the beginning."

On the contrary, my researches, which proceed on the hypothesis that all the forces of the universe are different modes of the dynamical action of the vibrations and currents of the æther, do indeed lead to the result that various forms of force are convertible into that of heat, and the associated form of light; but at the same time they indicate how the repulsion of heat may, by the modification which the dynamic action of undulations undergoes when they are compounded together, be changed to attraction. This transformation I have discussed particularly in arts. 2 and 3 of my "Theory of Molecular Forces" contained in the Philosophical Magazine for February 1860, and in the Essay on the Sun's Heat already mentioned. I have there adverted to the important consequence of that theory, that the numberless vibrations which radiate from the individual atoms of a body, producing by their dynamical action the body's internal heat, by their subsequent composition form undulations to the action of which the attraction of gravitation may be attributed. Moreover, instead of concluding that "the sun must at present be in the condition of a heated body cooling," I am led by my researches to the inference that both the sun and the other bodies of the solar system are, in regard to internal heat, in a state of permanence, the fluctuations, if any, taking place about a mean value which is constant so long as the dynamic action of the universe is constant. Thus in two most important particulars, one relating to the convertibility of heat into other forms of force, and the other to the stability of the intrinsic heat of masses, my results are directly opposed to those of Professor Thomson. The analogy of nature, as inferred from the movements of the planets, is certainly in favour of the stability of the heat of the bodies of the solar system. I think, therefore, I may justly conclude, having also indicated a possible source of error in Professor Thomson's mode of philosophy, that there is not the "sober scientific certainty" which he supposes in the inferences he has drawn respecting the duration of the present order of the universe. The power that made can unmake; but as to when and how, physical science can reveal nothing. I have felt the less scruple in discussing thus freely Professor Thomson's views, because, although they admit of question, he has published them in a popular periodical, and because he has since expressed (Phil. Mag. for June) his readiness to support the correctness of the opinions contained in the article "On Energy" when he sees proper occasion for so doing.

Cambridge, September 19, 1863.

XL. *Note on a Theorem of the Integral Calculus.*

By Professor SYLVESTER, *F.R.S.*[*]

I PROPOSE briefly to lay before the mathematical readers of the Magazine a wide generalization, and at the same time a more precise statement, of the theorem contained at the close of my paper in the last Number. The theorem, as therein enunciated, was drawn from geometrical considerations, it having first manifested itself dimly to the author by a sort of indirect reflection from a metrical theory of perspective. I have since obtained a very easy proof of it in its extended form, which in spirit amounts to a free algebraical paraphrase of the method indicated in the final foot-note of the paper in question. The ultimate form of the perfected theorem is particularly interesting from its simplicity of application, and from its connexion with the grand and growing theory of invariants. The proof of it will appear in its proper place in the continuation of the paper in which, in its incipient state, it first came to light[†].

Theorem.—Let a figure, whether plane, solid, or hyperspatial, be supposed to be limited by a locus or loci defined by one or more algebraical equations, not necessarily the most general of their respective degrees, but each at least the most general of its degree and kind[‡], and let the density at any point of the figure be any *homogeneous* function of the coordinates, and let the mass of such figure be supposed to be known in terms of the constants which enter into the defining equations; next let the density at each point of the mass be multiplied by a new factor, which may be any rational integral homogeneous function of the coordinates. Then the theorem affirms that the expression for the new mass may be obtained by operating upon the expression for the original one with differential operators precisely identical with combinations of certain of those which serve to define an invariant of the given system of equations, and which will be found set forth in my paper "On the Calculus of Forms," in the 'Cambridge and Dublin Mathematical Journal'[§]. Thus, for

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[*] Communicated by the Author.

[†] Strange cradle this for the inception of a quasi-invariantive theory of integration, "A geometrical construction of the centre of gravity of a truncated pyramid"! Où la vérité va-t-elle se nicher?

[‡] By *kind* I mean descriptive character, *i. e.* such character as is not affected by perspective or homographical deformation. Thus, *ex. gr.*, the case of a cone may be treated apart from the more general case of a surface of the second degree. So, again, a curve of the third degree with a multiple point, or having one or both of its fundamental invariants zero, may be treated apart from the case of a general cubic curve.

[§] The partial differential equations for invariants, covariants, and contravariants will be found therein stated with absolute generality for any example, by means of the known expressions for the area or content of a triangle, ellipse, pyramid, ellipsoid, or cone, this theorem enables us by differentiation and algebraical processes alone to obtain the parameters which define the centres of gravity, moments of inertia, principal axes, &c. of such figures.

I must add an important observation, viz. that the theorem remains true when one of the defining equations (supposing there to be more than one), instead of being the most general of a certain degree and kind, is affected with arbitrary numerical coefficients (zeros or others), provided only that it be *homogeneous* in the variables. Again, the theorem continues to hold when the original density, instead of being a homogeneous function of the variables, is such function multiplied by any Covariant of the defining equations taken separately or in groups—using the word covariant in its most extended sense, so as to comprehend fractional and irrational as well as integral forms,—the only effect of the introduction of such new factor into the density being to modify the form of the differential operators. There are certain very special cases, to which it is not necessary to allude here in detail, in which the theorem becomes illusory: such will be the case, *ex. gr.*, for a plane area when the given density is a homogeneous function in the variables of the negative degree 3, and for a solid content when that density is of the negative degree 4*.

K, Woolwich Common,
September 1863.

XLI. *On a Question arising from a recent Mathematical Controversy.* By G. B. Jerrard, *Esq.*†

HAVING for some time turned my thoughts away from mathematical subjects, I did not discover till lately that the present Number (that for September) of the Philosophical Magazine contains a passage involving an objection the relevancy of which I ought not for a moment to let pass unquestioned.

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number of functions and any number of variables. Dr. Armhold, in the last Number of Crelle's *Journal*, states erroneously that these equations were given by me for binary functions only, and subsequently generalized by Cayley and Clebsch.

* A similar method applied to *extents* (as curves, surfaces, &c.) gives rise to curious theorems. Thus I find that the mass of a plane curve affected with a density varying at each point as the square of the cosine of the inclination of the curve to a fixed line, is a differential derivative of the length of the curve. So, again, the moment of inertia of a curve in respect to any axis perpendicular to its plane, is a differential derivative of its moment in respect to an arbitrary line in its plane.

† Communicated by the Author.

"Following," it is said, "an analogous method to that pursued by Mr. Jerrard, we might dispute the validity of any mathematical proposition whatever on such grounds as these:

$$x=x, \quad \therefore\ x-x=(1-1)x=0,$$

$$\therefore\ x=\frac{0}{1-1}=\frac{0}{0},$$

and all formulæ into which x enters are illusory." The alleged analogy, however, does not exist. Instead of a similitude, I discern a contrast. The presumed objection (which, if applicable, would be instantly fatal to my researches) is unsupported by any proof. It seems to have sprung up in the mind of the writer from the delusive idea that Lagrange's theory is capable of furnishing us with some equation or other which, unlike (e_1) *, shall not conduct us to an illusory result when $v=u^5$. But whatever doubt there may be respecting the grounds on which he proceeds, one thing is certain—that his objection is nugatory †. What difficulty can there be, I would ask, in accepting the conclusion that when $v=u^5$ we cannot by Lagrange's method, or indeed by any conceivable method, superadd a second rational relation between v and u which shall be independent of the one with which we set out? I leave the reader to reflect on this.

September 1863.

XLII. *On the Focal Adjustment of the Eye.*

By Barnard S. Proctor, *Esq.* ‡

[With a Plate].

THERE is considerable difficulty in accumulating statistical evidence regarding the powers of the eye; and, as far as I am aware, there has not been anything published upon precisely the same subject as that which forms the material of this communication. These considerations, I hope, will justify me in bringing forward the subject in its present crude condition.

In March 1860 Mr. Wharton Jones read a paper before the Royal Society § giving an analysis of his own sight; the present paper was designed to show how far his observations coincide with general experience, and to afford answers to the following questions:—

Is it occasionally, generally, or universally that the human eye has distinct foci for vertical and horizontal lines?

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* For an explanation of the symbols (e_1), v, u see my paper in the Phil. Mag. for December 1862.

† See art. 4 (note) of the paper just referred to.

‡ Communicated by the Author, having been read before the British Association at Newcastle-upon-Tyne, August 1863.

§ See Phil. Mag. S. 4. vol. xx. p. 480.

Is the power of altering the focus of the eye exceptional or general? and what is the extent of the change?

Can eyes, having distinct vertical and horizontal foci, be made to adjust these foci to any particular distance at the same time?

Have the two eyes generally different focal lengths?

Does a good resolving power always accompany a good adjusting power?

Are markings on a flat surface resolved better by one eye or both?

What is the appearance of a vertical line, and what of a horizontal line and a point, when within or beyond focus?

Do the powers of the eye vary much with time of day, bodily or mental fatigue, &c.?

After a few preliminary experiments, the following apparatus and observations were adopted as the most suitable means of eliciting the information sought.

Upon a pair of parallel wooden rods, A, Pl. VI., a stage is made to slide; the stage consists of two flat pieces of wood, B, C, at right angles to each other—the vertical one, C, carrying a test-object, and the horizontal one a taper, D. The light being on the stage with the test object, gives it a constant illumination, as they both move together. The eye being placed at one end of the rods, the object is between it and the light. The object consists of a slip of glass coated with a thin film of silver or other material, upon which fine lines may be scratched so as to transmit light; upon this surface is drawn a cross consisting of fine double lines such as shown at E, the width between the double lines being varied according to the observer and the nature of the experiment in hand. The focus is considered to be accurate when the fine double lines are perceived to be double; and the adjustment is considered imperfect when the cross appears to consist of misty lines or a multiplicity of lines.

The test object for ascertaining the resolving power of the eye consists of two lines diverging at a very acute angle, such as shown at F, and supplied with an index showing how many thousandths of an inch space there is between the lines at any part of their length. This object being made to slide behind the perforation in the upright portion of the stage, commencing at the end where the space is greatest, it is pushed towards the narrow end till the lines just cease to be distinguishable as two; the number of thousandths is then read off as an indication of the resolving power of the eye.

This test-object, and the same mode of using it, was adopted in experimenting upon the power of various eyes to change focus: the resolving power having been ascertained for the shortest comfortable focus, the same was tried for double and three times the distance. In all these experiments, the person whose sight was being examined did not know the measurements till the conclusion of the experiments; thus was avoided any tendency which there might have been to fancy that at double the distance double the width of the lines was requisite to admit of resolution.

The adjustment of the eye is only partly under command of the will, but the power over it increases with practice, various observers being able to obtain a focus with facility at the end of a few experiments which was not readily obtained at the commencement; they also gain a facility in deciding when the test-object is in perfect focus, or the best attainable focus.

While some have hesitated long before deciding, others have come to a definite and fixed conclusion at once, and on repeating the experiments, have given the same results, although they had not the means of knowing that the distance or size which they required the second time was the same as they selected the first.

The following Tables give the results of an analysis of the sight of nine individuals:—

Table of Shortest Foci.

		Vertical line.	Horizontal line.	Note.
		Inches.	Inches.	
B. S. P., age 33.	Right eye.	7½	6	The observers, with two exceptions (F.S. and M. J.P.), were accustomed to the use of the microscope, the telescope, or both; and only one (M.S.) was in the habitual use of spectacles. The distances indicated are the shortest at which the lines were seen in clear focus. Some observers could not retain these short foci for more than a few seconds.
	Left eye.	6¼	5¾	
	Both eyes.	6¼	5½	
T. P. B., age 44.	Right eye.	12	8½	
	Left eye.	11	8½	
	Both eyes.	11	8½	
F. S., age 19.	Right eye.	4	3½	
	Left eye.	4½	4	
	Both eyes.	8½	4	
J. D., age 43.	Right eye.	7½	8	
	Left eye.	7	8	
	Both eyes.	8½	8	
C. B., age 36.	Right eye.	10½	11½	
	Left eye.	10½	11½	
	Both eyes.	10½	11½	
M. J. P., age 35.	Right eye.	8	8¾	
	Left eye.	8	8	
	Both eyes.	9	8½	
F. R., age 19.	Right eye.	5½	4½	
	Left eye.	6¼	4¼	
	Both eyes.	4½	4¼	
W. W. P., age 26.	Right eye.	5	5	
	Left eye.	5	5	
	Both eyes.	5½	5	
M. S., age 47.	Right eye.	9¾	9¾	
	Left eye.	9¾	9¾	
	Both eyes.	9½	9¾	

Table of Resolving Powers.

	Vertical.	Horizontal.	Note.
B. S. P. D	9 inches.	6½ inches.	The numbers given as the resolving power indicate the thousandths of an inch width of dark space between two luminous lines at the point where they approach so closely that the separating space just ceases to be distinguishable. The distance (D) in each case is the shortest convenient focus.
Right eye.	15	4½	
Left eye.	10	4½	
Both eyes.	15	4½	
T. P. B. D	24 inches.	9½ inches.	
Right eye.	25	5	
Left eye.	25	5	
Both eyes.	25	5	
F. S. D	4½ inches.	4½ inches.	
Right eye.	5	5	
Left eye.	3	4	
Both eyes.		4	
J. D. D	8 inches.	8 inches.	
Right eye.	5	4½	
Left eye.	5	5	
Both eyes.	5	4½	
C. B. D	11½ inches.	11½ inches.	
Right eye.	5	4	
Left eye.	5	4	
Both eyes.	5	3½	
M. J. P. D	8 inches.	8 inches.	
Right eye.	7½	3	
Left eye.	5	3	
Both eyes.	5	3	
F. R. D	6 inches.	6 inches.	
Right eye.	6½	7½	
Left eye.	6½	8	
Both eyes.	6½	5½	
W. W. P. D	6 inches.	6 inches.	
Right eye.	5	4½	
Left eye.	4½	5	
Both eyes.	4½	4½	
M. S. D	9¾ inches.	9¾ inches.	
Right eye.	11½	10	
Left eye.	10	10	
Both eyes.	10	10	

Table of Adjusting Powers.

		Vertical.						Horizontal.					
		Right eye.			Two eyes.			Right eye.			Two eyes.		
B. S. P. ...	D	10	20	30	10	20	30	10	20	30	10	20	30
	W	14	14	23	14	17	23	10	15	27	10	15	20
T. P. B. ...	D	...	...	...	...	...	...	...	...	...	11	22	33
	W	...	...	...	...	...	...	...	...	...	5	12	21
F. S.......	D	9	18	...	...	...	...	9	18				
	W	12	22	...	...	...	...	9	20				
M. J. P....	D	8	16	24	8	16	24	8	16	24	8	16	24
	W	5½	7½	12½	5	7½	12½	3	7	12	2¾	7½	11
F. R.	D	6	12	18	6	12	18	6	12	18	6	12	18
	W	6½	10	13	5	8	11	5	10	16	6	10	14½
W. W. P...	D	7	14	...	7	14	...	7	14	...	7	14	
	W	6	10	...	6	10	...	5	9	...	5	10	
M. S.	D	11	22	33	11	22	33	11	22	33	11	22	33
	W	10	20	20	10	20	20	10	20	20	10	20	20

Note—The lines marked D give the distance in inches at which observations were made, any convenient short focus being taken for comparison with the results obtained at twice and three times the distance.

The lines marked W indicate the resolving power in thousandths of an inch according to the principle made use of in the last Table.

In examining the results indicated in these Tables, there must necessarily be some allowance made for the impracticability of deciding *exactly* when the object is in its best focus. Few observers could indicate their focal distance with decision to within a quarter of an inch; consequently when there is only a fraction of an inch between the foci of the eyes, or between the foci for the vertical and horizontal lines, that may be disregarded, unless the same fraction recurs in parallel experiments.

Of nine observers, seven have the same focus for both eyes. In one case the focus for the right eye is longest, and in one that for the left is longest.

Four have a longer focus for vertical lines. Two have a shorter focus for vertical lines. Three have equal foci for both.

Of seven observers, six have the power of varying the focus for both horizontal and vertical lines, and one for the horizontal line only. All have the power of bringing both lines into focus at once at distances beyond the shortest focus, particulars of which are as follows:—

B. S. P. sees both horizontal and vertical lines clearly at the same time at 9 inches distance with the left eye or both eyes, and at greater distances with the right eye.

T. P. B. with the right eye sees both lines clearly at 30 inches;

with the left, temporarily clear at 19; with both eyes at once, 15 inches is his best focus, and that is evidently imperfect.

F. S. with *either* eye sees both lines clearly at distances between 4 inches and a foot; with *both* eyes, at distances from 8½ inches to a foot. The vertical line seen with both eyes appears split at distances less than 8½ inches.

M. J. P. with the right eye, by an effort sees both clearly at 8 inches; with the left at 10, and with both at 8 to 11½ inches.

F. R. with either or both eyes sees both lines clearly at 6 inches and greater distances.

W. W. P. with either or both eyes sees both lines clearly at 5 inches and greater distances.

M. S. with either or both eyes sees both lines clearly at 10 to 12 inches and greater distances.

Of nine observers, four have the best resolving power for horizontal lines, one for vertical, and four equally good for either. The better resolving power generally coincides with the shorter focus; but in one instance (C. B.) the horizontal line had the longer focus and the better resolving power.

Theoretically, at double the distance from the eye the lines should have double the space between them to be resolvable; in most cases less than double the space is required at double the distance; in some cases the proportion is preserved; and in two (T. P. B. and M. J. P.) I found it reversed for the horizontal lines, though in all cases in which the measurements were taken the observer considered the focus to be accurately obtained at both distances.

It is probable that there may frequently be a considerable imperfection in the best focus which can be obtained by a given eye at any distance, and that most observers will believe the object to be in perfect focus when it is in the best focus which they can obtain. It may thus happen that both vertical and horizontal lines may be reported in focus if they are both at once in as good focus as either can be placed in when taken separately. It probably also will occur with some eyes that they have a large range of focal adjustment, but that the degree of aberrations will vary; thus there may be one adjustment more free from aberrations than others. An eye capable of adjustment with very little aberration for rays diverging at one foot distance, may be capable of adjustment for parallel rays, but then have a greater aberration; thus when a star is looked at, it may not be in *perfect* focus on account of the aberrations, though any nearer object would, while that adjustment is retained, be in worse focus. It is likewise probable that a line would be reported in perfect focus if it were not very evidently out of focus, and it were found that increasing or decreasing the focal length of the eye only rendered it less clear; this we may take to be the explanation of the discrepancies in the Table of Adjusting Powers. Though the eye is really focused to the various distances at which observations are made, the focus is more free from aberrations when the resolving power is greater in proportion to the distance than it should theoretically be.

When an observation is made with both eyes, we generally find the results coincide with those of the best eye, the clear impression apparently eclipsing the misty one.

Different observers describe the appearance of a line out of focus in various manners as follows: a faint band but with clear edges—a line with misty edges—a band consisting of two or more misty lines, sometimes nearly clear and not constant in number; the motions of the eyelid will frequently alter the number.

M. J. P. says a line within focus looks like multiple lines, and beyond focus, like a line with softened edges.

F. R. believes he can focus to any distance, but at long distances a line looks *rough* at the edges.

W. W. P. finds that when beyond focus, the horizontal line splits, and the vertical becomes misty.

I find a dark line at about a foot distance, while the eye is adjusted for parallel rays, looks like two indistinct spectra, the order of the colours being blue, yellow, and blue, with a dark band between each of the blues and the yellow. When *slightly* within focus (say 9 inches distant with a focus for 1 foot), narrower spectra of brighter colours are seen. The same is observed less distinctly of a horizontal line. With a great discrepancy of focus the horizontal line splits into several lines, which change their shade and position with the involuntary winking of the eyelid. A spot at the distance of two or three yards, seen by both eyes while a distant object is looked at, appears as four spots thus ·· ·· ; but if seen at the same distance while a near object is looked at (say 12 or 18 inches), it appears as four spots thus : :—the greater distance between the spots being the result of the degree of convergence of the optic axes, the shorter distance being the result of a focal aberration. Of course the spots seen in this way were not clear; and when luminous points were used instead of dark spots, the forms they took, being more easily examined, were perceived to be characteristic. A star seen with a short focus appears like a luminous ring with radiations, the principal part of which spread in the vertical direction.

Looking at a light through a pin-hole in a card held at a foot or two distance while the focus was for parallel rays, the appearance was that of a group of misty luminous spots spreading principally in the horizontal direction.

Fatigue diminishes the power of the eyes to obtain the extremes of long or short focus, and decreases their resolving power at all distances. Some observers find that a focus which cannot be immediately obtained will come after a little perseverance; others find that a sudden effort is most successful; in both cases, when the focus has been obtained only by an effort, it cannot be long retained. Thus on the Table, the shortest focus for vertical lines which could be obtained by T. P. B. is given as 11 or 12 inches; but that was obtained only by an effort, and the shortest focus, for the same lines, which could be retained suitably for examining the resolving power, was found to be about 2 feet.

When the length of the ocular focus varies, the relation between the horizontal and vertical foci do not change, that which is longest at one time being always longest. Some observers say they cannot do fine work before breakfast; others say they can read before breakfast, with the naked eye, type which requires spectacles at any other time of the day; some others, myself included, have failed to find any difference, with carefully tried experiments.

I have used the words "resolving power" to express a power which varies with several physical circumstances, but it is not very easy to decide which are the usual or principal causes of its variation. It is not unlikely that, as regards the formation of an image, chromatic and spherical aberrations, and aberrations depending upon the curves of the eye being distorted by muscular and other causes, are at least the principal causes of its variation. And as regards the perception of the image, the sensitiveness of the optic nerve, variation in the degree of its ramifications, and education are the most important.

The resolving power is necessarily greatest when the light is moderate; with the apparatus before described, the naked flame behind the test-object when near the eye was generally found too bright; greater resolving power was obtained by interposing a piece of oiled paper, and in some cases the interposition of writing-paper without oil was still better; at a distance of two or three feet the stronger illumination was generally preferred. In like manner, when the test-object was placed at the end of a telescope-tube (an arrangement I used for making observations by daylight), I found that for my own eye, sunlight reflected from white paper was too powerful an illumination, and even sunlight reflected from a carpet of dull colours was less advantageous than diffused daylight reflected from the same. But it is probable that the degree of illumination most advantageous will vary greatly with even the same eye according to the nature of the object, grey lines requiring a stronger light when upon either black or white ground than white lines on a black ground, or black on white.

The faculty of perceiving what we see is one which depends so much upon practice, that any observer, however critical he may be in other observations, is not unlikely to fail in his first attempts to ascertain what are the peculiarities of his own vision in respect to some of the particulars indicated in the above experiments. The habit of careful scrutiny of objects *in focus* does not facilitate our perception of the impressions produced by focal and chromatic aberrations in objects under abnormal circumstances. In all experiments upon vision, it is necessary for us to remember that the impression produced upon the retinæ differs from the impression produced upon the mind in many important particulars, though in *how many* it is difficult to say.

11 Grey Street, Newcastle-upon-Tyne,
August 1863.

XLIII. *Abstract of an Investigation on Plane Water-lines.* By W. J. Macquorn Rankine, *C.E., LL.D., F.R.SS.L. & E.&c.*[*]

[With a Plate.]

1. THIS paper contains an abstract of a mathematical investigation which has been communicated in detail to the Royal Society. By the term "plane water-line" is meant one of those curves which a particle of a liquid describes in flowing past a solid body, when such flow takes place in plane layers. Such curves are suitable for the water-lines of a ship; for during the motion of a well-formed ship, the vertical displacements of the particles of water are small compared with the dimensions of the ship; so that the assumption that the flow takes place in plane layers, though not absolutely true, is sufficiently near the truth for practical purposes[†].

2. The author refers to the researches of Professor Stokes (Camb. Trans. 1842) "On the Steady Motion of an Incompressible Fluid," and of Professor William Thomson (made in 1858, but not yet published), as containing the demonstration of the general principles of the flow of a liquid past a solid body[‡].

3. Every figure of a solid, past which a liquid is capable of flowing smoothly, generates an endless series of water-lines,

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[*] Communicated by the Author, having been read to the British Association at Newcastle-upon-Tyne in August 1863.

[†] As water-line curves have at present no single word to designate them in mathematical language, it is proposed to call them *Neoïds*, from *νηὸς*, the Ionic genitive of *ναῦς*.

[‡] See also a paper by Dr. Hoppe, in the 'Quarterly Journal of Mathematics' for March 1856.

which become sharper in their forms as they are more distant from the primitive water-line of the solid. The only exact water-lines whose forms have hitherto been completely investigated, are those generated by the cylinder in two dimensions, and by the sphere in three dimensions. In addition to what is already known of those lines, the author points out that when a cylinder moves through still water, the orbit of each particle of water is one loop of an elastic curve.

4. The profiles of waves have been used with success in practice as water-lines for ships, first by Mr. Scott Russell (for the explanation of whose system the author refers to the 'Transactions of the Institution of Naval Architects' for 1860–62), and afterwards by others. As to the frictional resistance of vessels having such lines, the author refers to his own papers—one read to the British Association in 1861 and printed in various engineering journals, and another read to the Royal Society in 1862 and printed in the Philosophical Transactions.

5. The author proceeds to investigate and explain the properties of a class of water-lines comprising an endless variety of forms and proportions. In each series of such lines the primitive water-line is a particular sort of oval characterized by this property—that the ordinate at any point of the oval is proportional to the angle between two lines drawn from that point to two foci. (In Plate VI., L B represents a quadrant of such an oval, O being its centre, and A one of the foci; the other focus is at an equal distance to the other side of the centre.) Ovals of this class differ from ellipses in being considerably fuller at the ends and flatter at the sides.

6. The length of the oval may bear any proportion to its breadth, from equality (when the oval becomes a circle) to infinity. (In the Plate the length O L is to the breadth O B nearly as 17 : 6.)

7. Each oval generates an endless series of water-lines, which become sharper in figure as they are further from the oval*. In each of those derived lines, the excess of the ordinate at a given point above a certain minimum value is proportional to the angle between a pair of lines drawn from that point to the two foci.

8. There is thus an endless series of ovals, each generating an endless series of water-lines; and amongst those figures a continuous or "fair" curve can always be found, combining any proportion of length to breadth from equality to infinity, with any degree of fullness or fineness of entrance, from absolute bluffness to a knife-edge.

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* As a convenient and significant name for these water-lines, the term "Oögenous Neoïds" is proposed (from 'Ωογενὴς, *generated from an egg,* or *oval*).

9. The lines thus obtained present striking likenesses to those at which naval architects have arrived through practical experience; and every successful model in existing vessels can be closely imitated by means of them, from a Dutch galliot to a racing-boat.

10. Any series of water-lines, including the primitive oval, are easily and quickly constructed with the ruler and compasses as follows. Parallel to the longitudinal axis O X, draw a series of straight lines at equal distances apart. Through the foci draw a series of circular arcs $A\,C_1$, $A\,C_2$, &c. so as to contain a series of angles found by dividing those distances by

$$\frac{OL^2-OA^2}{2OA}.$$

Each of those circular arcs indicates the direction of motion in still water of each of the particles that it traverses. Then through the angles of the network formed by the straight lines and circular arcs draw a series of curves; these will be the required water-lines*.

The centre of curvature of the oval at L is the focus A.

11. The following curves, traversing certain important points in the water-lines, are exactly similar for all water-lines of this class, and are easily and quickly constructed with the compasses.

L M is a hyperbola having a pair of asymptotes crossing the axes at O at angles of 45°. It traverses all the points at which the motion of the particles in still water is at right angles to the water-lines.

L Q N and L P are the two branches of a curve of the fourth order, having a pair of asymptotes which traverse O, making angles of 30° with O X. A straight line joining L and P makes an angle of 30° with L O. The two branches cross the axis O X at L, making angles of 45°. The branch L Q N traverses a series of points, at each of which the velocity of gliding of the particles of water along the water-line is less than at any other point on the same water-line. The branch L P traverses a series of points, at each of which the velocity of gliding is greater than at any other point on the same water-line.

12. The axis O Y from B to P traverses a series of points of minimum velocity of gliding: from P onwards it traverses a series of points of maximum velocity of gliding.

13. Every water-line, complete from bow to stern, which passes within the point P, has three points of minimum and two of maximum velocity of gliding; while every water-line which

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* The first employment of a graphic process of this kind is due, it is believed, to Professor Clerk Maxwell, who applied it to certain curves connected with electricity and magnetism.

passes through or beyond P has only two points of minimum and one of maximum velocity of gliding. Hence the latter class of lines cause less commotion in the water than the former.

14. On the water-line P Q which traverses the point P itself, the velocity of gliding changes more gradually than on any other water-line having the same proportion of length to breadth. Water-lines possessing this character can be constructed with any proportion of length to breadth, from $\sqrt{3}$ (which gives an oval through L and P) to infinity. The finer of those lines are found to be nearly approximated to by wave-lines; but are less hollow at the bow than wave-lines are.

15. The author shows how horizontal water-lines at the bow, drawn according to this system, may be combined with vertical plane lines of motion for the water at the stern, if desired by the naval architect.

16. In this, as in every system of water-lines, a certain relation (according to a principle first pointed out by Mr. Scott Russell) must be preserved between the form and dimensions of the bow and the maximum speed of the ship, in order that the appreciable resistance may be wholly frictional and proportional to the square of the velocity (as the experimental researches of Mr. J. R. Napier and the author have shown it to be in well-formed ships), and may not be augmented by terms increasing as the fourth and higher powers of the velocity through the action of vertical disturbances of the water.

XLIV. *Notices respecting New Books.*

A Dictionary of Chemistry and the Allied Branches of other Sciences. By Henry Watts, *B.A., F.C.S.* Vol. I. 8vo. pp. 1150. London: Longman and Co., 1863.

THE completion of the first volume of this work enables us to form a more definite estimate of its general scope and plan, as well as of the degree of success with which that plan is being carried out, than could be formed from the first two Parts, a notice of which was given in this Magazine four months ago (Phil. Mag. for June 1863, p. 473). We then expressed the fear that the limits assigned to the work would prove too narrow to allow of the latter portions being treated with the same fulness of detail as the beginning; we are therefore glad to find that it has been decided to increase its size by nearly one-third, the intention now being to complete the work in twenty-one, instead of sixteen, monthly parts, making four volumes, which will together contain about 4000 pages. Careful economy of the space thus gained will still be needed in order to avoid the necessity of unduly curtailing the concluding portions of the work.

Great credit is due to editor and publishers for the regularity and punctuality which have hitherto attended the issue of the monthly Parts; but it is to be feared that it will not be found possible to complete the publication of the work at the same rapid rate. There has appeared each month, since the publication began, a Part containing 12 sheets of 16 pages, or altogether 192 pages, each containing, on the average, rather more matter than two pages of the Edinburgh or Quarterly Review. The mere revision of this quantity of scientific matter is more than it seems to us possible for any one to accomplish properly in the course of a month; for the labour of efficiently correcting the proof-sheets of a book like the present—requiring, as it does, that the attention be directed simultaneously to the matter, to the language, to the punctuation, and to the detection of errors of a technical kind, which in works on science are more than commonly plentiful—is such as cannot be supported for more than a few hours consecutively. Hence, unless the printing of the Dictionary is at present very considerably in advance of the publication, a time will come, before the issue of the work is completed, when, at the present rate, the whole of the printed matter will have been given to the public. Should this occur, it would be most fatal to the scientific character of the work to attempt to go on with the issue of it as rapidly as at present; for it appears to us clearly impossible that it could then receive the amount of revision necessary to ensure that degree of accuracy which, in a work of this kind, is a matter of the very first importance. Unless, therefore, the arrangements already made are such as to allow ample time for the revision of the remaining portions, we would urge in the strongest manner, either that the amount of matter issued each month should be reduced, say to six sheets, or that the publication should take place at longer intervals.

We feel that the volume before us sufficiently justifies these remarks. The known character of Mr. Watts, both as a writer and as a man of science, is a guarantee that the fullest experience and most thorough knowledge have been brought to bear on its production: yet the last Part of the volume contains a list of errata extending to nearly three pages. This alone is sufficient proof of the difficulty of the undertaking; for the careful revision after publication which is evidenced by it, may be fairly taken to indicate that equal care had been bestowed upon the work during its preparation.

There is another point connected with the mode of publication of works like the one at present under consideration, to which it is worth while to direct attention. In Germany, where the publication of elaborate scientific works is of considerably more frequent occurrence than it is in this country, the usual practice, in the case of works issued in a considerable number of separate Parts, is to publish each Part as soon as it is ready, without attempting to make the separate issues follow each other at perfectly regular intervals of time. Consequently the purchasers of such works receive each portion with the smallest possible delay after the time of its actual composition,—a circumstance which is often by no means unimportant when the subject treated of is a science in a state of such rapid advance as Modern Chemistry. On the other hand, however, the likelihood of the publication being spread over an unnecessarily long period is greater under this system than when the publishers feel that they are to some extent breaking faith with their subscribers if a certain number of pages does not appear on a certain day. It thus sometimes happens that the beginning and the end of the same book represent very different stages in the development of a science, as in the case of the *Handwörterbuch der reinen und angewandten Chemie*, the publication of which commenced in 1837, and is now nearly completed. In contrast with this system is that which has been adopted in the case of Mr. Watts's Dictionary, and is the one usually adopted, as far as practicable, in the publication of English works of a similar character, namely, the issue of Parts of a uniform size at regular intervals of time. This mode of publication ensures the completion of the work within a limited period after the appearance of the first Part; but since it is necessary, in order to prevent subsequent delay, that the printing of the whole work should be far advanced before any portion is published, the earlier Parts must be already somewhat behind the actual state of the science by the time they reach the purchaser's hands. The method of publication which would combine to the greatest possible extent the advantages of the two foregoing systems, while excluding their disadvantages, would be, in the first place, to complete the manuscript of the whole work; then to proceed with the printing at such a rate as would allow the author or editor ample time not only to correct the press, but to make such alterations in or additions to each portion of copy, before delivering to the printer, as may have become necessary since it was first written; lastly, to issue the work as fast as it is printed, keeping the press only so much in advance of the publication as may be deemed necessary to allow for accidental and extraordinary delays.

So far as we can judge by internal evidence, the composition of the volume before us appears to have been completed about the end of the year 1861, the latter portions showing but very few important omissions in respect to information published before that date; the matter contained in the first two or three Parts, on the other hand, has not always been brought down to a later date than about the end of 1859. Throughout the volume, however, the information contained in the various articles is very complete up to a certain point; so that it is only fair to suppose that the reason why the whole volume has not been completed up to a still later period, was the necessity of going early to press in order to render possible the rapid issue of the work. The value of a work of reference, containing so great a mass of facts as this does, is of course not very materially lessened by its not containing a few more, if only it is complete in all important points so far as it goes, and this Mr. Watts's Dictionary seems to be in an eminent degree. It would, however, often be a great convenience if every volume of such works bore, besides the date of publication, the date up to which the information contained in it is intended to be complete.

The contributions to this volume of the Dictionary by other writers than Mr. Watts, amount, according to a rough estimate, to about 310 pages, or to not much more than one quarter of the whole. The names of the contributors of articles in this volume, arranged according to the extent of their contributions, are:—Mr. F. T. Conington, who has written the articles on *Amides, Amines, &c., Ammonia* and its compounds, *Inorganic Analysis*, compounds of the *Anisic group*, and compounds of the *Benzoic group*; Mr. G. C. Foster, author of *Acids, Alkali, Alkaloids, Ammonium-bases, Anhydrides, Classification*, and *Cæsium*; Dr. Odling, author of *Atomic Weights*, and the articles on *Chlorine* and its oxides and acids; Dr. Atkinson, author of *Bismuth, Butyric, Capric, Caproic* and *Citric acids*, and of the articles on the compounds and derivatives of these substances; Mr. W. S. Jevons, author of *Balance, Barometer*, and *Clouds*; Dr. W. J. Russell, author of *Analysis of Gases*; Dr. Ditmar, author of *Volumetric Analysis*; the late Mr. C. E. Long, author of *Blood* and *Casein*; Mr. A. W. Wills, author of *Coals* and *Coal-Gas*; Dr. Roscoe, author of *Atmosphere*; the late Dr. Alexander Ure, author of various short articles, chiefly descriptive of minerals (these articles are taken from Dr. Ure's Dictionary of 'Chemistry and Mineralogy,' edit. 1831, of which Mr. Watts's work was at first intended to be a new edition; but so completely has it been rewritten, that the whole of the articles retained in the first volume would scarcely occupy nine pages if collected together); and Dr. F. Guthrie, author of articles on the compounds and derivatives of *Amyle*.

The above list illustrates to some extent the range of subjects included in this work; but we ought further to point out that it is really, as stated on the title-page, a "Dictionary of the Allied Branches of other Sciences" as well as of Chemistry. Mineralogy especially occupies a large share of space, Mr. Watts having apparently endeavoured to render his work a complete Dictionary of Chemical Mineralogy, by giving, as far as possible, the locality, physical characters, crystalline form, and analysis of every described mineral. The results of the chemical investigation of plants are also very completely recorded, including in all cases the published analyses of their ashes,—a circumstance which will no doubt render the work very valuable to agriculturists, at least to those who accept Liebig's mineral theory of agriculture. Meteorology, again, is represented in Mr. Jevons's articles already mentioned on the Barometer and on Clouds, which, as well as the same author's article on the Balance, are among the very best in the volume. The principles involved in the applications of chemistry to the purposes of every-day life (as in the preparation of *Beer* and *Bread*), to Pharmacy, to Metallurgy and to other manufacturing arts are also very fully elucidated, and technical processes are described with as much detail as the nature of the work permits. Still another collateral subject which occupies a prominent place is Physiological Chemistry: among the more important articles in this branch of the science, those on *Bile*, *Blood*, and *Chyle* may be mentioned.

There are many other articles of a more purely chemical nature which possess great value and interest. Among the number we may mention particularly those on *Combustion* and *Chemical Affinity*. In the latter article, the distinctive characters of chemical action are first considered, then its different kinds, next an account is given of the various investigations that have been made in order to determine the laws which regulate its amount, and lastly the various theories which have been propounded, as to the nature of chemical affinity, are explained. Altogether this article forms the best treatise on the subject with which we are acquainted. Nevertheless it is impossible to read it without coming to the conclusion that the ascertained facts which throw any distinct light on the nature and mode of action of the force which produces chemical action are very few indeed: by far the most important is, no doubt, the constant quantitative relation subsisting between chemical action and other forms of energy. For this part of the subject we are referred to the articles *Heat* and *Electricity*, where it will no doubt be fully discussed; but we think that it might also have been treated with considerable advantage from a chemical point of view.

The most complete exposition, with which we are acquainted, of the principles to be adopted in fixing the atomic weights of the elementary bodies, is Dr. Odling's article *Atomic Weights* in this volume. The author there sets forth with singular clearness the various arguments, physical as well as chemical, which bear upon the subject; and does not so much strive to illustrate the advantages of a particular system, as to explain the grounds upon which any logical system must be based. We do not, however, agree with his conclusion that Gerhardt's scale of atomic weights affords a satisfactory representation of the present state of knowledge; on the contrary, we think that the consistent application of the spirit of Gerhardt's reforms would lead us now to go much further than he did.

Considering the very wide range of subjects included in this volume, and the uniform excellence of the execution, so far as we feel competent to form an opinion, it reflects the very highest credit upon the editor, and cannot fail to raise his already high reputation. Watts's 'Dictionary of Chemistry' will doubtless long remain a standard work, and one of the most valuable in our language. It is from this conviction that we thought it worth while to indicate at the beginning of this notice one or two points connected with the issue of this volume, attention to which will, it may be hoped, ensure even greater excellence in its successors.

XLV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 236.]

January 29, 1863.—Dr. William Allen Miller, Treasurer and Vice-President, in the Chair.

THE following communication was read:—

"On the Absorption of Gases by Charcoal.—No. I." By Dr. R. Angus Smith, F.R.S.

The following is a summary of the author's observations:—

1. Charcoal absorbs oxygen so as to separate it from common air, or from its mixtures with hydrogen and nitrogen, at common temperatures.
2. Charcoal continues the absorption of oxygen for at least a month, although the chief amount is absorbed in a few hours, sometimes in a few seconds, according to the quality of the charcoal.
3. It does not absorb hydrogen, nitrogen, or carbonic acid for the same period.
4. Although the amount absorbed is somewhat in the relation of the condensibility of the gases by pressure, this is not the only quality regulating the absorption, of oxygen at least.
5. When it is sought to remove the oxygen from charcoal by warmth, carbonic acid is formed, even at the temperature of boiling water, and slowly even at lower temperatures.
6. Charcoals differ extremely in absorbing power, and in the capacity of uniting with oxygen, animal charcoal possessing the latter property in a greater degree than wood-charcoal.
7. Nitrogen and hydrogen, when absorbed by charcoal, diffuse into the atmosphere of another gas with such force as to depress the mercury three-quarters of an inch.
8. Water expels mercury from the pores of charcoal by an instantaneous action.
9. The action of porous bodies is not indiscriminate but elective.

Theoretical Considerations.

1. The elective nature of porous bodies may be closely allied to three properties:—
   a. The condensibility of the gases.
   b. The attraction and perhaps inclination to combine.
   c. The capacity of combination.
2. In either case the attraction which results in condensation of the gas is exercised at distances greater than the distances of atoms or molecules in combination.
3. The gases in porous bodies lie in strata, the outside and more distant being less attracted than the atoms nearer the solid body.
4. We cannot separate chemical from physical attraction; but attraction may exist without its ultimate result (combination), which is distinctly chemical.
5. It is exceedingly probable that as physical attraction moves

onwards to chemical combination, it produces the phenomena which have been attributed to so-called masses.

Chemical affinity is supposed to involve an attraction which is purely chemical; we have no proof of any such attraction as a separate power, we have only a proof of the combination. Attraction may exist without the capacity of combining chemically, or, in other words, without chemical affinity. Chemical affinity (a very inappropriate term) is only known by combination; the previous attraction has never yet been shown to be of two kinds; and it seems more in accordance with Nature to diminish than to increase the number of original powers.

February 5.—"On the Embryogeny of *Comatula rosacea* (Linck)." By Professor Wyville Thomson, LL.D., F.R.S.E.

February 12.—Major-General Sabine, President, in the Chair.

The following communications were read:—

"On some Compounds and Derivatives of Glyoxylic Acid." By Henry Debus, M.A., Ph.D., F.R.S.

"On the Telescopic Appearance of the Planet Mars." By John Phillips, M.A., LL.D., F.R.S., F.G.S., &c.

Notwithstanding the descriptions and drawings of Mars, for which we are indebted to eminent observers*, there remains much uncertainty as to the permanent boundaries of the bright and shady parts of the planet, to which respectively, on a first view, we attach, perhaps too readily, the idea of land and seas. The extremely variable aspects under which this planet appears in its excentric orbit, the axis being inclined more than 30° to the ecliptic, the different regions very unequally presented to incident light, and very unequally influenced by vicissitudes of heat and cold, may account for much of the uncertainty. Other difficulties arise when the work of different instruments is compared; for it is established that reflectors will on the whole give the best results for colour, while achromatics of fine quality discover more of detail than instruments of less perfect definition.

The author having devoted some evenings between the 27th of September and 13th of December 1862 to the examination of Mars with a 6-inch achromatic by Cooke, equatorially mounted, and moved by clockwork, at Oxford, presented to the Society some results of these observations combined with others, also made with achromatics, by Mr. Grove, Mr. Main, and Mr. Lockyer.

These various observations, made entirely without concert, were rendered comparable by a calculated reduction of each to the longitude on Mars corresponding to the epoch of each, according to one standard. [Tables of these reductions were given in the paper.] The sketches were then arranged on sheets in the order of the computed longitudes; and, in addition, two globes were exhibited, on one of which the main results of the author's observations were drawn, the data for the other being supplied by Mr. Lockyer's sketches.

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* Herschel, Mädler, Jacob, De la Rue and Secchi have all published careful drawings of Mars.

He was also aided in the explanations by large drawings made with reflectors by Mr. De la Rue and Mr. Nasmyth.

From the author's sketches, three, representing opposite hemispheres, and one intermediate quadrature, have been selected for engraving,—one central to the assumed meridian of 0° or 360°, the others to the meridians of 90° and 180° nearly. See figs. 1, 2, 3 (pp. 314, 315).

On considering the surface of the planet, either as seen in the telescope, or delineated on paper, we feel in some doubt as to the meaning of what we see. Are the bright parts (often seen of a red tint) land, the darker parts (often appearing of a greenish grey) water? or, as in the moon, are the reflecting powers of different parts of a dry surface very unequal? Is there any considerable change in the aspect of the masses or boundaries between one epoch and another, so as to indicate atmospheric vicissitudes like those on Jupiter and our own planet?

Taking the latter question first, the author found, on the experience of his observations during 74 days, that no material change took place in the main and prominent features about the longitude which he marks 0°. Not that after this considerable interval the appearances remained exactly as at first: that was not, and could not be expected to be the case, after the planet had increased his distance from the earth to nearly double that when the observations began. Adding to his own the experience of Mr. Lockyer, whose observations began 35 days earlier, this inference, of permanence in the main boundaries of lights and shades, is extended to above 100 revolutions of Mars; and on comparison of these with the earlier sketches of Mädler, Herschel, Jacobs, and De la Rue, the conclusion appears to embrace the whole series of more than thirty years.

The author regards as one of the main features very firmly defined in the late opposition, the broad white or rather reddish band which from about 65° of north latitude (the north pole being invisible in these observations) spreads up into large bright cloud-like prominences toward and beyond the equator, and retires into one principal and several smaller bays toward the pole. From this bright space, which in many parts is sharply defined, a broad dusky tint spreads toward the south, partially relieved by half-lighted expansions with shades of various depths between. The south pole itself is surrounded (excentrically as it appears) by a bright white mass, obviously glittering in the telescope. This is believed to be snow; and the effect of its whiteness is increased in most parts of its circumference by the contrast of a dark ring round it, which expands here and there into broader spaces. Thus a great part of the northern area appeared in the late opposition bright, and often reddish, as if it were land, while a great part of the southern area was of the grey hue which is considered to indicate water, but relieved by various tracts of a tint more or less approaching to that of the brighter spaces of the northern hemisphere. The principal boundary of light and shade, for the most part very well defined, ran obliquely across the equator of Mars, so as to reach latitudes

Fig. 1.—Mars as seen on the 27th of September and on several other occasions till the 13th of December. (Longitude 0°.)

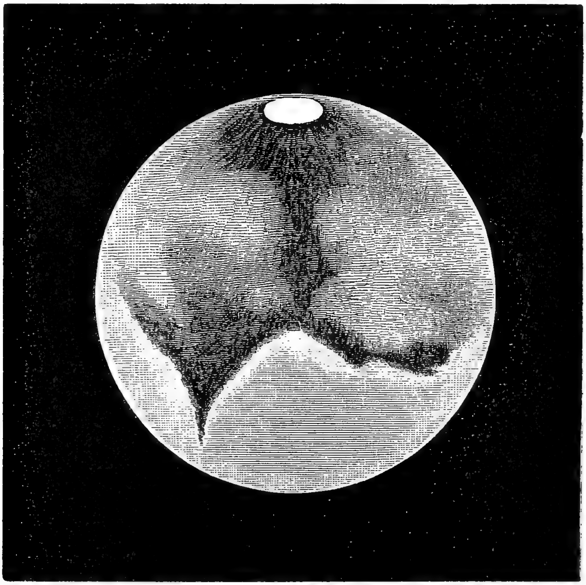
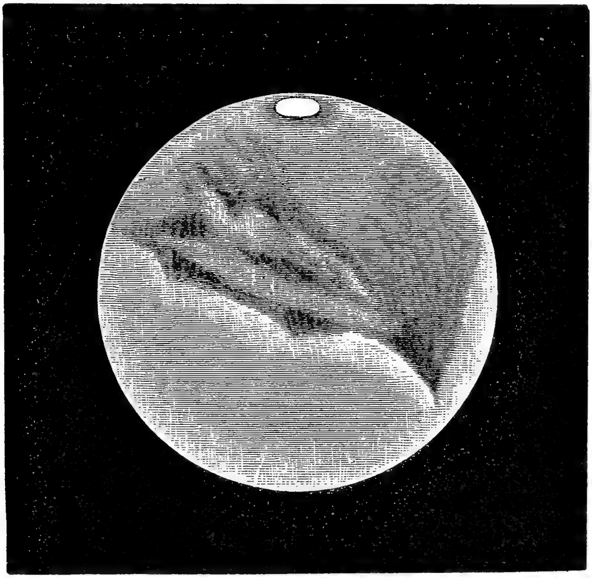
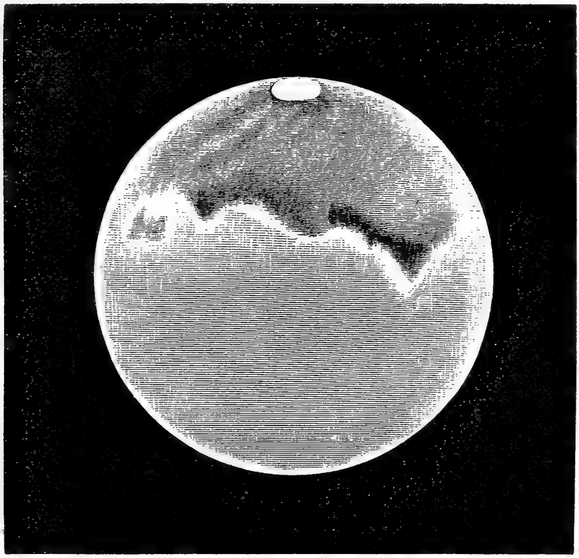


Fig. 2.—The appearance of Mars at longitude 90°, with long oblique ridges south of the great boundary, and nearly or quite running into the northern land, here less broad than in fig. 3: seen November 11th.



from 20° to 30° north and south of that line. This may perhaps be understood by the drawings selected for illustration, especially if compared with an orthographic projection of the latitudes*. (Still better by means of the globes which accompanied the communication, constructed by the author, one from his own sketches, the other from those of Mr. Lockyer.)

Fig. 3.—The hemisphere of Mars, opposite to fig. 1: seen October 15th and 16th. With a specially dark band.



Allowing the white spaces to be land, which reflects light as the moon in opposition, it seems a natural supposition that the shady spaces should be called sea; and this may be supported by the obvious requirement of water somewhere on Mars, to agree with the alternate gathering and melting of the snow round the poles. Still, every observer remarks no small resemblance of some of these shady tracts with particular parts of the unequally tinted grey surfaces of the moon. A positive proof of ocean on the disk of Mars would be afforded by the star-like image of the sun reflected from the quiet surface†, or the more diffused light thrown back from the waves;

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[*] The inclination of the axis of Mars to the observer was, on the 1st of October, 1862, 25½°, as Mr. Main has informed me.

[†] The quiet image here alluded to would not exceed $\frac{1}{20}$th of a second of angle at the opposition, if no allowance be made for irradiation. But much allowance must be made for this. A thermometer-bulb, with diameter half an inch, reflects the sun as a star visible by the eye at 25 yards' distance, the reflecting surface in this case being about $\frac{1}{240}$th of an inch in diameter, and therefore (if no irradiation were allowed for) the angle subtended at the eye would be only about 1″. By employing on Mars a power of 300, the $\frac{1}{20}''$ becomes relatively magnified to 15″.

The reflective power of water at a nearly vertical incidence is, however, so but nothing of this sort has been placed on record, nor is there such a variation in the appearance of these spaces from the centre toward the edges as to give any special reason for thinking them occupied by water.

Atmospheric vicissitudes, however, appear to be recognized in the somewhat variable aspect of many portions of the grey spaces; for these, though not much changed in the situation of the masses of light or shade, are sufficiently inconstant in their shapes and details to suggest the idea of a vaporous envelope, brooding over and about some parts more than others, and variable from one epoch to another. The drawings of Mr. Lockyer supply the best evidence of these variations; for Professor Phillips, except on a few occasions, confined his attention chiefly to the stronger and apparently more settled boundaries of light and shade.

The tints on the body of Mars were observed by each of the gentlemen named, but with different results. To Mr. Nasmyth, with a large reflector, the 'land' appeared of a decidedly red tint, the 'water' green. The 'land' appeared red in some parts, but bright and almost silvery in other parts, to Professor Phillips, looking through his achromatic, which also showed the 'water' of a grey or greenish tint. No redness appeared in Mr. Lockyer's instrument, which, like many others of excellent quality for astronomical research, is intentionally 'over-corrected.'

Mr. Nasmyth saw the snow-patch on the south pole so distinctly bordered, as to give him the impression of its having a cliff-boundary. The south snow-patch did not appear to him to agree with the south pole of the planet, but, on the contrary, to be considerably excentric to it; and he supposed this to be due to the relative distribution of land and water, influencing the position of the centre of greatest cold. Only a faint glimmering of the snowy surfaces round the north pole was seen by any observer.

On the whole, the author of this paper concluded that, over a permanent basis of bright and dusky tracts on the surface of Mars, a variable envelope gathers and fluctuates, partially modifying the aspect of the fundamental features, and even in some cases disguising them under new lights and shades, which present no constancy,—a thin vaporous atmosphere probably resting on a surface of land, snow, and water.

Addendum.

Since the reading of the paper the author has been enabled, by the kindness of the Earl of Rosse, to examine a series of sketches of Mars during the late opposition, from the great telescopes at Birr. These drawings, six in number, were made on July 22, Sept. 14, Sept. 16, Oct. 6, Oct. 29, and Nov. 6. They confirm in a remarkable manner the conclusions already presented by the author, and

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much reduced, that Professor Phillips found it possible, under that condition, to observe the sun's image in water without a protecting dark glass. It seems probable, therefore, that even in very large reflectors the direct solar reflexion from water on Mars would be too faint for observation.—Feb. 17, 1863.

suggest some interesting questions for further observation and study. On the 22nd of July the southern snow was a large patch, meeting the limb by its diametral line. It must then have had a radius of 500 miles at least: in the later observations it was reduced to less than half this measure.

One of the drawings nearly corresponds to longitude 180° on the author's scale, and represents the specially dark short band which distinguishes that aspect of the planet (fig. 3). Two correspond nearly to fig. 1, and contain the remarkable deep angular bay which extends so far towards the north pole. In these and the remaining three drawings, general resemblances and special differences appear on comparison with the sketches of Prof. Phillips and Mr. Lockyer. The differences affect principally the grey southern parts, and are remarkable enough to justify serious doubts whether any of our drawings of those parts are much to be trusted as representing permanent physical boundaries. Nor should this be thought surprising; owing to the high inclination of the axis of Mars to the plane of his orbit, the regions round each pole are presented alternately to the sun through periods somewhat less than our whole year. The effect is seen in the vast outspread of snows round the cold pole, and the contraction of those white sheets to a small glittering ellipse round the warm pole. The enormous transfer of moisture from one hemisphere to the other while the snows are melting round one pole and growing round the other must generate over a great part of the planet heavy storms and great breadths of fluctuating clouds, which would not, as on the quickly rotating mass of Jupiter, gather into equatorial bands, but be more under the influence of prominent land and irregular tracts of ocean.

February 19.—Dr. William Allen Miller, Treasurer and Vice-President, in the Chair.

The following communication was read:—

"On Thallium." By William Crookes, Esq.

After discussing the occurrence and distribution of the new metal in different parts of the globe, the author proceeds to describe the method adopted by him for extracting it from its ore. Thalliferous pyrites is distilled at a bright red heat, in quantities of about 1 cwt. at a time, in cast-iron retorts. The resulting sulphur, varying from 13 to 17 per cent. of the pyrites taken, is then dissolved in aqueous caustic soda, which leaves the sulphide of thallium as an insoluble black precipitate; this is filtered off, dissolved in acids, and the thallium precipitated in the form of iodide. This is then converted into sulphate, and the metal reduced from the solution by electrolysis. It is obtained in the coherent form by fusion under cyanide of potassium.

The physical characteristics of thallium are then described. In appearance it most resembles tin and cadmium, but has a distinct colour of its own; it has a brilliant metallic lustre, and is susceptible of taking a very high polish; it oxidizes in the air with almost the rapidity of an alkaline metal, but when coated with oxide, the metal may be freely handled and exposed to the air with scarcely any further change. An oxidized surface applied to the tongue is very biting and caustic, and has a sweetish metallic taste. It is the softest known metal admitting of free exposure to the atmosphere, being scratched by soft lead with the greatest ease. It makes a dark blue mark upon paper, rapidly turning yellow, which in the course of a few hours nearly fades out, but can be restored with sulphide of ammonium. It has little tenacity, is very malleable, and may be readily pressed into wire.

The specific gravity of thallium varies from 11·81 to 11·91, and it is probably capable of still greater condensation.

When freshly prepared, thallium wire is perfectly amorphous, but when kept in water it gradually assumes a superficial crystalline appearance: this effect is immediately produced when thallium in wire, ingot, or plate, tarnished or clean, is boiled in water.

Its melting-point is 550° F., being between bismuth and lead, and the metal does not become pasty before undergoing complete fusion. Two pieces of clean metal weld together by pressure in the cold. It begins to volatilize at a red heat, and boils below a white heat; it may be distilled in a current of hydrogen.

It is a pretty good conductor of heat and electricity, and stands electro-chemically very near cadmium. It is strongly diamagnetic, ranking in this respect near bismuth. The alloys which thallium forms with different metals are next described.

Further details are given respecting the spectrum of thallium: the characteristic green line is perfectly single under a very high magnifying power and after refraction through nine heavy glass prisms; and no new lines make their appearance at the temperature of the oxyhydrogen blowpipe,—although, with the electric spark, Dr. Miller has shown that several new lines come into existence.

The delicacy of the optical test for thallium is roughly estimated, the $\frac{1}{5,000,000}$th of a grain being easily perceptible.

The atomic weight of thallium is given as 203, being the mean of five experiments. The author states, however, that this is not to be regarded as a final result.

The chemical properties of thallium are next described. It does not decompose water even at the boiling-point, but remains bright under this liquid. The superficial tarnish is a powerful base soluble in water, and reacting like an alkaline solution. Melted in the air, thallium forms a readily fusible oxide, its behaviour resembling that of lead.

The formation of thallic acid and the properties of some of the thallates are described. Sulphate, nitrate, the chlorides, sulphide, iodide, and other salts of thallium are described in detail. The metal may be quantitatively determined by precipitation, either as protochloride, iodide, or platinochloride.

The position of thallium amongst elementary bodies is then discussed. Although one or two of its properties show a resemblance to the alkaline metals, the author does not agree with continental chemists in classing it with this group,—numerous facts proving that its true position is by the side of mercury, lead, or silver. The ready dehydration of its basic oxide; the insolubility of its sulphide, iodide, chloride, bromide, chromate, phosphate, sulphocyanide, and ferrocyanide; its great atomic weight; its ready reduction by zinc to the metallic state; its power of forming a strongly acid oxide; and, according to Dr. Miller, the complexity of its photographic spectrum,—all prove that thallium cannot consistently be classed anywhere but amongst the heavy metals, mercury, silver, lead, &c. No weight is attached to M. Dumas's argument in favour of thallium being related to potassium and sodium because its equivalent is rather near a figure obtained by adding twice the atomic weight of one metal to four times the atomic weight of the other. The author shows that, by similar processes of addition, multiplication, or subtraction, it is not difficult to prove that thallium is related to any desired group of elements.

The author gives full analytical notes on thallium, showing where it would occur in the ordinary course of analysis, and detailing accurate methods of separating it from every metal with which it can be accompanied.

February 26.—Major-General Sabine, President, in the Chair.

The following communications were read:—

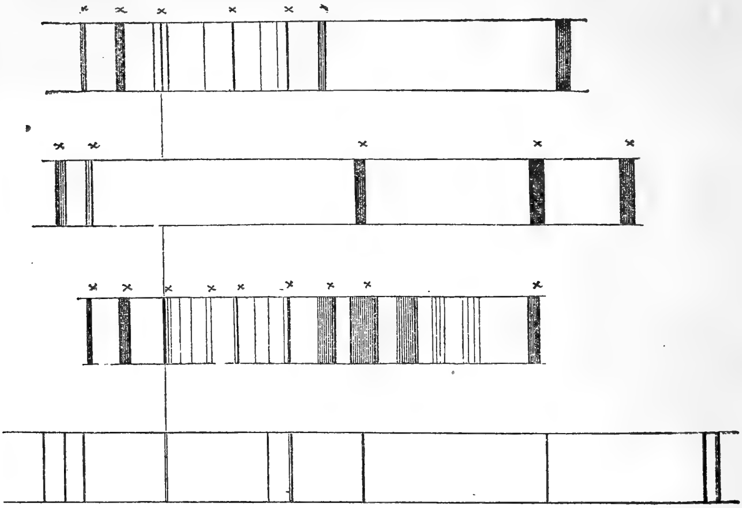
"On the effect of Temperature on the Secretion of Urea, as observed on a Voyage to China, and at Hong Kong." By Emil Becher, M.D.

"On Clinant Geometry, as a means of expressing the General Relations of Points in a Plane, realizing Imaginaries, reconciling Ordinary Algebra with Plane Geometry, and extending the Theories of Anharmonic Ratios." By Alexander J. Ellis, B.A., F.C.P.S.

"Note on the Lines in the Spectra of some of the Fixed Stars." By William Huggins, Esq., F.R.A.S., and William Allen Miller, M.D., LL.D., Treasurer and V.P.R.S.

The recent detailed examination of the solar spectrum, and the remarkable observations of Kirchhoff upon the connexion of the dark lines of Fraunhofer with the bright lines of artificial flames, having imparted new interest to the investigation of spectra, it has appeared to the authors of the present note that the Royal Society may not consider a brief account of their recent inquiry upon the spectra of some of the self-luminous bodies of the heavens unworthy of attention, although the investigation is as yet far from complete.

After devoting considerable time to the construction of apparatus suitable to this delicate branch of inquiry, they have at length succeeded in contriving an arrangement which has enabled them to view the lines in the stellar spectra in much greater detail than has been figured or described by any previous observer. The apparatus also permits of the immediate comparison of the stellar spectra with those of terrestrial flames. The accompanying drawing shows with considerable accuracy the principal lines which the authors have seen in Sirius, Betelgeux, and Aldebaran, and their position relatively to the chief solar lines.



Without at present describing in detail, as they propose to do when the experiments are completed, the arrangements of the special apparatus employed, it may be sufficient to state that it is attached to an achromatic telescope of 10 feet focal length, mounted in the observatory of Mr. Huggins at Upper Tulse Hill. The object-glass, which has an aperture of 8 inches, is a very fine one by Alvan Clark of Cambridge, U.S.; the equatorial mounting is by Cooke of York, and the telescope is carried very smoothly by a clock motion.

It may further be stated that the position in the stellar spectra corresponding to that of Fraunhofer's line D, from which the others are measured, has been obtained by coincidence with a sodium line, the position of which in the apparatus was compared directly with the line D in the solar spectrum.

The lines in the drawings against which a mark is placed have been measured.

Addendum.—Since the foregoing Note was presented to the Royal Society, the authors have learned that a paper on the same subject, accompanied by diagrams of the spectra of the Moon, Jupiter, Mars, and several of the fixed stars, by Mr. L. M. Rutherfurd, has appeared in the January Number of the 'American Journal of Science' for the current year. The method of observing finally employed by Mr. Rutherfurd much resembles that adopted by the authors of this Note.

They therefore desire to add that, during the past twelvemonth, they have examined the spectra of the Moon, Jupiter, and Mars, as well as of between thirty and forty stars, including those of Arcturus, Castor, α Lyræ, Capella, and Procyon, some of the principal lines of which they have measured approximatively. They have also observed β and γ Andromedæ, α, β, ε and η Pegasi, Rigel, η Orionis, β Aurigæ, Pollux, γ Geminorum, α, γ and ε Cygni, α Trianguli, ε, ζ and η Ursæ Majoris, α, β, γ, ε and η Cassiopeiæ, and some others.—[Feb. 21, 1863.]

GEOLOGICAL SOCIETY.

[Continued from p. 158.]

May 20, 1863.—Professor A. C. Ramsay, President, in the Chair.

The following communications were read:—

1. "Further Observations on the Devonian Plants of Maine, Gaspé, and New York." By J. W. Dawson, LL.D., F.R.S., F.G.S.

Since the preparation of his paper on the Devonian Flora of North-Eastern America, published in the Society's Journal, vol. xviii. pp. 296 *et seq.*, the author has been enabled to explore more thoroughly than before the plant-bearing beds of Perry, in Maine, Gaspé, and New York. The results of these further explorations, together with descriptions of the new species of plants discovered, were given in this paper,—as also were some new and important facts respecting the distribution of the Devonian rocks of the State of New York, which give to several of the plants from that region a somewhat older geological position than that heretofore assigned to them.

2. "Notice of a new Species of *Dendrerpeton*, and of the Dermal Coverings of certain Carboniferous Reptiles." By J. W. Dawson, LL.D., F.R.S., F.G.S.

This paper referred to new facts ascertained in the course of a re-examination of the remains of Reptiles from the Coal-formation of Nova Scotia, and first to the characters of a new and smaller species of *Dendrerpeton*, for which Dr. Dawson proposed the name of *D. Oweni*. The author then described the remains of skin and horny scales which he had lately discovered, and which he supposed to belong to *Dendrerpeton Oweni*, *Hylonomus Wymani*, and *H. Lyelli*. He also gave restorations of these animals, according to what he regarded as the more probable arrangement of the parts; and, after expressing his belief that *Hylonomus* may have Lacertian affinities, he stated that should they prove to be really Batrachian, a new Order must be created for their reception, many of the characters of which would coincide with those of the humbler tribes of Lizards.

3. "On the Upper Old Red Sandstone and the Upper Devonian Rocks." By J. W. Salter, Esq., F.G.S., A.L.S.

The conclusions arrived at by Mr. Salter were based upon certain sections in Pembrokeshire, Devonshire, and the North and South of

Ireland, which he described in full, and compared with one another as well as with the Upper Devonian of the Continent and America, giving lists of fossils from the English localities. The sections in Devonshire and South Ireland were contrasted with those in Pembrokeshire and North Ireland; and it was shown that, although the physical features in the two cases very nearly correspond, the Marwood series is constant, and the Pilton group of the former districts is a series unknown in Pembrokeshire, or only represented by beds, a few feet thick, at the base of the Carboniferous slate. The author endeavoured further to prove the intercalation of marine beds in the Upper Old Red Sandstone, and, by the fossils, the correlation of the Marwood Group of Devonshire with the Uppermost part of that series. He stated his belief in Sir R. I. Murchison's suggestion, that the Caithness Flags belong to the Middle, and the Cephalaspis-beds of Scotland to the Lower Old Red, which divisions he considered equivalent to the Middle and Lower Devonian respectively; and concluded by assigning the Tilestones (or Ledbury shales) to the Lowest Devonian.

June 3, 1863.—Professor A. C. Ramsay, President, in the Chair.

The following communication was read:—

"On the Section at Moulin-Quignon, and on the peculiar character of some of the Flint Implements found there." By J. Prestwich, Esq., F.R.S., F.G.S.

The recent discovery of a human jaw and of flint implements of a peculiar type and fresh appearance at Moulin-Quignon has led to questions having been lately raised as to the age of the gravel-beds, and the antiquity of these remains. Mr. Prestwich showed how conflicting had been the evidence in support of the views formerly entertained, and dwelt upon the appearances which had raised the doubts as to the genuineness of certain implements and the fossil nature of the jaw. He also gave a *résumé* of the proceedings of the Conference lately held at Paris and Abbeville, and remarked that the intrinsic evidence appeared to prove the spurious nature of the objects, while the evidence derived from the study of the beds had led to the opposite conclusion.

The author then showed that, from the physical configuration of the district, corroborated by the presence of extinct mammalia distinctly of contemporaneous age, the modern age assigned to these beds by some geologists could not be maintained, and that, from the occurrence of freshwater shells in both the high- and low-level gravels, their formation must have been due to river-action, and not to a wave of translation or other modification of marine action.

Mr. Prestwich concluded by stating that, whatever may be the conclusions drawn from the jaw and the flint implements, the age of the deposits is to him perfectly well determined as being of the early quaternary or post-pliocene period, older than the Menchecourt gravels, and anterior to the excavation of the valley of the Somme;

and as some flint implements, the genuineness of which is not doubted, have been found in these deposits, the question of the antiquity of man will not be affected, whatever the conclusions arrived at with regard to certain others.

XLVI. *Intelligence and Miscellaneous Articles.*

ON THE EARTH'S CLIMATE IN PALÆOZOIC TIMES.

BY T. STERRY HUNT, M.A., F.R.S.

THE late researches of Tyndall on the relation of gases and vapours to radiant heat are important in their bearing upon the temperature of the earth's surface in former geological periods. He has shown that heat, from whatever source, passes through hydrogen, oxygen, and nitrogen gases, or through dry air, with nearly the same facility as through a vacuum. These gases are thus to radiant heat what rock-salt is among solids. Glass and some other solid substances, which are readily permeable to light and to solar heat, offer, as is well known, great obstacles to the passage of radiant heat from non-luminous bodies; and Tyndall has recently shown that many colourless vapours and gases have a similar effect, intercepting the heat from such sources, by which they become warmed, and in their turn radiate heat. Thus while for a vacuum the absorption of heat from a body at 212° F. is represented by 0, and that for dry air is 1, the absorption by an atmosphere of carbonic acid gas equals 90, by marsh-gas 403, by olefiant gas 970, and by ammonia 1195. The diffusion of olefiant gas of one inch tension in a vacuum produces an absorption of 90, and the same amount of carbonic acid gas an absorption of 5·6. The small quantities of ozone present in electrolytic oxygen were found to raise its absorptive power from 1 to 85, and even to 136; and the watery vapour present in the air at ordinary temperatures in like manner produces an absorption of heat represented by 70 or 80. Air saturated with moisture at the ordinary temperature absorbs more than five hundredths of the heat radiated from a metallic vessel filled with boiling water; and Tyndall calculates that of the heat radiated from the earth's surface warmed by the sun's rays, one-tenth is intercepted by the aqueous vapour within ten feet of the surface. Hence the powerful influence of moist air upon the climate of the globe. Like a covering of glass, it allows the sun's rays to reach the earth, but prevents to a great extent the loss by radiation of the heat thus communicated.

When, however, the supply of heat from the sun is interrupted during long nights, the radiation which goes on into space causes the precipitation of a great part of the watery vapour from the air, and the earth, thus deprived of this protecting shield, becomes more and more rapidly cooled. If now we could suppose the atmosphere to be mingled with some permanent gas which should possess an absorptive power like that of the vapour of water, this cooling process would be in a great measure arrested, and an effect would be produced similar to that of a screen of glass, which keeps up the temperature beneath it, directly by preventing the escape of radiant heat, and indirectly by hindering the condensation of the aqueous vapour in the air confined beneath.

Now we have only to bear in mind that there are the best of reasons for believing that, during the earlier geological periods, all of the carbon since deposited in the forms of limestone and of mineral coal existed in the atmosphere in the state of carbonic acid, and we see at once an agency which must have aided greatly to produce the elevated temperature that prevailed at the earth's surface in former geological periods. Without doubt the great extent of sea, and the absence or rarity of high mountains, contributed much towards the mild climate of the carboniferous age (for example) when a vegetation as luxuriant as that now found in the tropics flourished within the frigid zones; but to these causes must be added the influence of the whole of the carbon which was afterwards condensed in the form of coal and carbonate of lime, and which then existed in the condition of a transparent and permanent gas, mingled with the atmosphere, surrounding the earth, and protecting it like a dome of glass. To this effect of carbonic acid it is possible that other gases may have contributed. The ozone, which is mingled with the oxygen set free from growing plants, and the marsh-gas, which is now evolved from decomposing vegetation under conditions similar to those then presented by the coal-fields, may by their great absorptive power have very well aided to maintain at the earth's surface that high temperature the cause of which has been one of the enigmas of geology.

Montreal, August 1, 1863.

ON SOME PHENOMENA PRODUCED BY THE REFRACTIVE POWER OF THE EYE. BY A. CLAUDET, F.R.S.*

One of the most beautiful and extraordinary results of the structure of the human eye is the considerable field of vision it embraces. The external objects which are represented on the retina are included in an angle much larger than one-half of the sphere at the centre of which the observer is placed; and from this point of view a single glance encompasses a vast and splendid panorama, extending in a horizontal and vertical direction to an angle of 200°.

If we lie on the ground, we see the whole vault of the sky, and 10° all round lower than the horizon. In that position, having our face directed to the zenith, if, at the time of opposition, the moon was just rising in the east, and the sun setting in the west, we could see simultaneously the moon with one eye and the sun with the other; and, although both forming actually an angle of 180°, they would appear to form one only of 160°.

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* Communicated by the Author, having been read at the British Association Meeting, Newcastle-upon-Tyne, August 27, 1863.

The field of vision of each eye subtends an angle of 200°, but it is reduced on one side in the horizontal direction according to the projection of the nose. It is also reduced in the vertical line according to the projection of the frontal bone.

With the two eyes, the shadow of the nose, being in an opposite position for each of them when the two eyes are open, does not impede the full range of vision, which embraces the whole extent of 200°.

To explain how the optical combination of the eye can form on the retina an image of 200°, we have to assume that the rays of light in passing through the cornea and the crystalline lens are more and more refracted in proportion to the angle at which they strike the spherical surface of the cornea. By this refraction the rays which enter the eyes at an angle of 90° are bent 10°, and appear to come from an angle of 80°.

This phenomenon, which I have not seen noticed in any treatise on optics, produces a very curious illusion. When we are lighted by the sun, the moon, or any other light, if we endeavour to place ourselves in such a position that the front of the body should be on a line with its own shadow and with the light which produces it, if we look straight before us we see unexpectedly with one eye the light and with the other the shadow of our body; and although they must form an angle of 180°, they appear by the refraction to form one of only 160°. In fact the light and the shadow do not seem to be connected at all; instead of being in a line they appear bent to an angle. The shadow seems to come not from the light we actually see, but from one which ought to be a little behind us; and as to the light, it seems that it should only be capable of forming the shadow of our body enough behind us to be invisible. In other words, the ray of light and the shadow which ought to coincide and form only one line, are bent by the refractive power of the eye, like the stick, one half of which being immersed in water, appears crooked or bent into an angle at the point of immersion.

This curious phenomenon evidently proves that the process of vision is affected by the common law of refraction, and that the only objects which appear in their true position are those the image of which, entering the eye in the direction of the optic axis, undergo no refraction.

So that mathematically we see in their right place only the objects which reflect their light on the centre of the retina, and all the other objects are more and more refracted as they enter the eye in a more and more oblique direction. Therefore the panoramic picture which is represented on the retina is correct only in the centre of this organ, and all the other parts of the picture are seen not exactly where they are. But this is no imperfection in the general effect, as it does not affect in the least the perception of the picture, because, as we have only a distinct vision for the parts which are represented on the centre of the retina, when we want to see the whole panorama we have to direct gradually the optic axis upon the various parts of which it is composed. By this means, and the rapidity of the motion of the eye, we have always a correct perception of the whole, and gradually we see every part of the picture in its true position.

The representation on the retina of a whole field of 200° is one of the innumerable and wonderful resources of Nature—in this case being a contrivance by which the beauty of the effect is increased, and by which our attention is gradually called to the various parts which, although not exactly in the direction of the optic axis, and even very far from it, are sufficiently conspicuous and appear to form an interesting or, in any way, a desirable point of observation. By means of this extensive field of vision we are warned of any danger from objects coming to us in the most oblique direction. If a furious animal were running from behind we should see it as soon as it had reached an angle of 100° with the optic axis, and this would enable us to take immediate measures for our safety. But the advantages of an enlarged field of vision are particularly felt in our crowded towns, where we are obliged to be constantly on the look-out for all that is passing around us.

In every respect the vision of the eyes is as complete and as perfect as can be imagined, and we cannot improve it artificially by single or even double achromatic glasses, which, although magnifying considerably the objects, cannot embrace a field larger than the angle formed by the lens at the distance of the eye-glass. With a telescope, if we have the advantage of enlarging a small part of the landscape, we lose the fulness and extent of its panoramic beauty.

Notwithstanding all the resources of the science of optics, no artificial instrument can approach the perfection of the eye; and it has not yet been given to man even to understand satisfactorily all the laws in accordance with which this marvellous organ has been created, or how it can perform so many wonderful things.

ON THE CONSERVATION OF ENERGY.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

My colleague's letter, published in your Number for August, contains a mistake which, if the controversy is continued, I think I ought to rectify. I recollect that Prof. J. Thomson stated to me at Glasgow in 1840, the effects of tidal friction on the earth's rotation; but it was at the subsequent Meeting of the British Association in Glasgow that I had the honour of being President of Section A. This fact divests the communication of the semipublic character attributed to it by Prof. Tait.

I am, Gentlemen,
Your obedient Servant,
Philip Kelland.

University of Edinburgh,
September 12, 1863.

THE STAR CHROMATOSCOPE: AN INSTRUMENT TO EXAMINE AND COMPARE THE RAYS OF THE STARS. BY A. CLAUDET, F.R.S.*

The scintillation of the stars is probably due to the evolution, in different degrees of swiftness, of the various rays their light emits. These rays seem to divide during their long and rapid course through space, as if they were dispersed by a refractive medium, and we see them following each other in quick succession. The change is so instantaneous that, although we see distinctly the various colours, we cannot judge of the separate lengths of their duration.

It has occurred to me that if we could increase on the retina the length of the sensation produced by each ray, we should have the better means of examining them separately, of comparing their intensities, and the length of every vibration.

The retina having the power of retaining the sensation of light during a fraction of time, which has been found to be one-third of a second, one ray succeeds another before the entire sensation is exhausted; so that, supposing the seven rays reach the eye in the third part of a second, two or more sensations are felt at the same moment, and consequently the perception is mixed and confused, although the predominating ray is more conspicuous. But if, while a ray is acting upon one part of the retina, we could bring the next ray on another part, so dividing the sensations, we should be enabled to avoid their confusion, and to compare the colour and duration of each.

If this were done, and moreover, if we could not only separate the various sensations, but make them travel on the retina during the whole time that each sensation lasts, we should have a still better means of comparison.

This problem may be solved by transforming the infinitely small spot of the star into a large circle, as can be done with an incandescent charcoal at the end of a wire rapidly revolving round a centre. It is obvious that if the incandescent charcoal during its revolution was evolving successively various rays, we could measure the length and duration of every ray by the angle each would subtend during its course.

This is precisely what can be done with the light of the star. It can be made to revolve like the incandescent charcoal, and form a complete circle on the retina.

When we look at a star with a telescope, we see it fixed on one definite part of the field of the glass; but if with one hand we slightly move the telescope, the image of the star changes its position; and during that motion, on account of the persistence of sensation on the retina, instead of appearing like a spot, it assumes the shape of a continued line. Now if, instead of moving the telescope in straight line, we endeavour to move it in a circular direction, the star appears

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* Communicated by the Author, having been read at the British Association Meeting, Newcastle-upon-Tyne, August 28, 1863.

like a circle, but very irregular, on account of the unsteadiness of the movement communicated by the hand.

Such is the principle that has led me to the construction of the instrument I am going to describe, and which communicates the perfect circular motion that it is impossible to impart by the hand.

I establish on the top of a stand a conical tube, lying horizontally at each end upon two vertical wheels, by which it is steadily supported. These wheels revolve in a groove, cut on the tube at each of its extremities. By means of these grooves, and the wheels on which the tube is supported, we can make it revolve freely and regularly upon its axis. In the middle of the tube there is another groove, in which an india-rubber ring is enrolling itself and from thence passing on a large grooved wheel which, being turned by means of a crank, impresses a rotatory motion on the tube. Inside the tube I place a small telescope or an opera-glass, the eye-glass of which is exactly centrical with the small end of the conical tube; while the object-glass is placed in an eccentric position by means of two screws fixed on the two extremities of the diameter of the large end of the tube. The axis of the telescope can be so inclined as to take various degrees of eccentricity, so that, while the whole machine makes the conical tube revolve upon its axis, the axis of the telescope revolves round the former in an eccentric direction.

The eccentric motion of the telescope is such that any star corresponding with the axis of the external tube is refracted through the object-glass, and its rays dispersed as if that object-glass were a revolving prism; and during that revolution the image of the star describes upon the retina a circle proportionate to the eccentricity of the telescope.

If, while the telescope is revolving, we look into it with one eye, we may with the other see the star; and from this binocular vision we have the phenomenon of a star revolving round another star, forming a perfect luminous circle with a luminous point at its centre. This is a very curious observation, independently of the more curious and interesting object of the instrument, which is to develope an infinitely small spot of light into a large circle, exhibiting on its periphery the various rays emitted by the star, all following each other in spaces corresponding with their duration—showing also blank spaces between two contiguous rays, which must correspond with the black lines of the spectrum. We have, in fact, a kind of spectroscope, by which we can analyse the particular light of any star; and, further, by this instrument we may arrive at the discovery of the real cause of the scintillation, and compare its intensity in various climates and at different altitudes of a given star. At all events the instrument will be found to be a curious application of optics, and in proper hands may perhaps afford the means of making many interesting observations and experiments upon the constitution and properties of the light of the stars, so difficult to examine by the spectroscope. I hope the idea will be fruitful, and not end trivially in the contrivance of a mere scientific toy.

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

[FOURTH SERIES.]

NOVEMBER 1863.

XLVII. *On the Numerical Expression of the Destructive Energy in the Explosions of Steam-boilers, and on its comparison with the Destructive Energy of Gunpowder. By* GEORGE BIDDELL AIRY, *Astronomer Royal*[*].

1. A LITTLE consideration of the changes in the state of the water and steam, which occur during the bursting of a steam-boiler, will show that very little of the destructive effect of an explosion is due to the steam which is contained in the steam-chamber at the moment of the explosion. The rupture of the boiler is effected by the expansive power common at the moment to the steam and the water, both at a temperature higher than the boiling-point; but as soon as steam escapes, and thereby diminishes the compressive force upon the water, a new issue of steam takes place from the water, reducing its temperature; when this escapes, and further diminishes the compressive force, another issue of steam of lower elastic force from the water takes place, again reducing its temperature; and so on; till at length the temperature of the water is reduced to the atmospheric boiling-point, and the pressure of the steam (or rather the excess of steam-pressure over atmospheric pressure) is reduced to 0. It is the enormous quantity of steam, of gradually diminishing power, which is thus produced from water during the course of the explosion, that causes the disastrous effects of the explosion: compared with this quantity, the small volume of gas which may happen to be in the steam-chamber at the time is, in boilers of ordinary construction, wholly insignificant, and

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[*] Communicated by the Author. An abstract was communicated to the Mechanical Section of the British Association at their late Meeting at Newcastle.

may be entirely put out of sight in the succeeding investigations.

2. If we compare the course of changes, in bursting, in two boilers, a large one and a small one, we see that the order of changes is the same in both; but that to reduce the temperature of a large body of water by a certain number of degrees a large volume of steam must escape, whereas, to reduce the temperature of a small body of water by the same number of degrees, it will suffice that a smaller volume of steam (smaller in the same proportion as the bulk of water) escape. Thus it will appear that the whole volume of escaping steam at a given pressure, and the whole destructive energy of the steam, are proportional to the bulk of water.

3. For measure of the destructive energy of the steam, we must suppose the simplest and most easily measurable case—namely, that the steam, in expanding, drives a piston along a uniform cylinder. It is necessary to ascertain the value of the pressure F when the steam has expanded so far as to have pushed the piston to the distance x: then the measure of the total energy is $\int dx . \mathrm{F}$, the integral being taken from the point where the piston was in contact with the water to the point where the excess of pressure of the steam above atmospheric pressure $=0$.

4. In the case of gunpowder fired in a cannon, where the weight of the ball and its velocity on emergence are found by experiment, the energy of the gunpowder as acting on the ball will be thus found. The pressure at distance y being F', acting on a ball whose weight is W, and g being the numerical measure of the acceleration produced in one second of time by gravity ($g=32{\cdot}1908$ if the unit of measure is the English foot, or $=9{\cdot}8116$ if the unit is the French mètre), v being the velocity at distance y, and V the whole velocity acquired, then the acceleration is $\frac{\mathrm{F}'g}{\mathrm{W}}$, and therefore $v\frac{dv}{dy}=\frac{\mathrm{F}'g}{\mathrm{W}}$, $v^2=\frac{2g}{\mathrm{W}}\int dy . \mathrm{F}'$, and $\mathrm{V}^2=$ total integral $\frac{2g}{\mathrm{W}}\int dy . \mathrm{F}'=\frac{2g}{\mathrm{W}}\times$ total energy, whence the total energy $=\frac{\mathrm{W} . \mathrm{V}^2}{2g}$. And if w be the weight of the gunpowder, the energy of one unit-weight of gunpowder $=\frac{\mathrm{W} . \mathrm{V}^2}{2g . w}$.

5. Several years ago (before 1849) I had desired in this way to compare the destructive energy of steam from a bursting boiler with that of gunpowder; and I had requested the assistance of my friend Professor W. H. Miller of Cambridge (to whose knowledge of the progress of accurate science in every

department of physics I have often been indebted), to enable me to give numerical values to the expressions involved. At that epoch, however, the theories and experiments on steam were not sufficiently advanced, and I was compelled to lay aside the inquiry for a time.

6. In the spring of the present year I requested Messrs. Ransomes and Sims, of Ipswich, to furnish me with an experimental result on the quantity of water escaping from a high-pressure boiler in the form of steam when the valve is gradually opened. This experiment was undertaken by George A. Biddell, Esq., Engineering Superintendent of the Orwell Works. The result was that, when the bulk 22 cubic feet of water in a locomotive boiler was raised to the temperature which produced a pressure of 60 lbs. per square inch, and when after raking out the fire the valve was gradually opened with every precaution against priming, the quantity of water which escaped in the form of steam was $2\frac{3}{4}$ cubic feet, or one-eighth of the whole.

7. Possessed of this experimental fact, I again referred to Professor Miller for such theories and citations of experiments as might be required. And by his kind assistance I was enabled to complete the investigation. And here I may state that the whole which follows is Professor Miller's, with the exception of the integration of the steam-pressures, the inference from the cannon-experiments, and the comparison of steam and gunpowder.

8. In giving the heads of Professor Miller's theory, I must premise that the temperatures are Centigrade, the unit of linear measure is the mètre, and the unit of weight is the kilogramme. The formula adopted as connecting the volume of steam with the volume of water (at maximum density) from which it was generated, is Fairbairn's and Tate's (Phil. Trans. 1860, p. 219). The formula for the number of *calories* required to convert water into saturated steam of temperature T, and the pressure of saturated steam at temperature T, are from Regnault (*Mémoires de l'Institut*, vol. xxi. pp. 748 and 728).

9. The first part of Professor Miller's investigation applies to Mr. Biddell's experiment. The steam-pressure of 60 lbs. per square inch is represented by a column of mercury (at 0°) 3·1028 mètres in height. Adding the atmospheric pressure 0·76 mètre, the entire elastic force of the steam is represented by a column of mercury 3·8628 mètres in height. The corresponding temperature of saturated steam, by Regnault's Table, is 152°·84. Now the quantity of water is 22 cubic feet, which at 100° weigh 597·1 kilogs., and the heat requisite to raise the temperature of this water from 0° to 152°·84 is $597{\cdot}1 \times 154{\cdot}38$ *calories* (the last number being derived from Regnault's formula

$T+0{\cdot}00002\,T^2+0{\cdot}0000003\,T^2$, where T for this instance $=152{\cdot}84$), or 92182 *calories*. When all the steam has been blown off, the 597·1 kilogs. of water are separated into x kilogs. of steam at 100°, and $(597{\cdot}1-x)$ kilogs. of water at 100°. [This applies strictly when the steam has blown into a cylinder and has driven a piston, because then there may be such intercommunication of temperature between the portions of steam as will ensure that the final state of the steam is that of saturated steam at 100°; it is probably true or very approximate when the steam has blown out at different temperatures and has been lost in the atmosphere.] To heat $(597{\cdot}1-x)$ kilogs. of water from 0° to 100° requires $(597{\cdot}1-x)\times100{\cdot}5$ *calories*; and to convert x kilogs. of water at 0° into steam at 100° requires $(606{\cdot}5+0{\cdot}305\times100)\times x$ *calories* (by a formula of Regnault's). Supposing, then, that the amount of heat as measured by the number of *calories* is not altered by the blowing out from the boiler,

$$92182=(597{\cdot}1-x)\times100{\cdot}5+637\times x,$$

whence x, the weight blown out as steam, $=59{\cdot}8$. This, however, is equivalent to only 2·2 cubic feet of water, instead of 2·75, the quantity which Mr. Biddell found to have passed away in steam.

10. Professor Miller supposes the difference to be caused principally by the heat of the mass of iron which surrounds the water; any burning fuel which may have been left in the fire-box would add slightly to its effect. It appears best therefore to assume the experimental fact, and to infer from it what quantity of heated water we ought to add (in investigations) to the quantity of water really present in the boiler, in order to produce correctly the amount of water which in the experiment was blown out as steam. Now 2·75 cubic feet of water at 100° weigh 74·638 kilogs. Let y represent the number of kilogs. in the bulk of water which may be considered equivalent to the compound consisting of 22 cubic feet of water, the unknown weight of iron, and the unknown quantity of fuel. To heat y kilogs. of water from 0° to 152°·84 requires $154{\cdot}38\times y$ *calories*; and this is the amount of heat in the complex equivalent before blowing off. To heat $(y-74{\cdot}638)$ kilogs. of water from 0° to 100° requires $(y-74{\cdot}638)\times100{\cdot}5$ *calories*; and to convert 74·638 kilogs. of water at 0° into steam at 100° requires $74{\cdot}638\times637$ *calories*; and the aggregate of this with the last, or $(y-74{\cdot}638)\times100{\cdot}5+74{\cdot}638\times637$, represents the number of *calories* in the complex equivalent after the blowing off. Making this equal to the number before blowing off,

$$154{\cdot}38\times y=(y-74{\cdot}638)\times100{\cdot}5+74{\cdot}638\times637,$$

whence $y=743{\cdot}2$.

Comparing this with the weight of the 22 cubic feet of water alone, or 597·1 kilogs., it appears that the heated materials extraneous to the water produce the effect of 146·1 kilogs. of water.

11. Assuming then that there are really 743·2 kilogs. of heated water, the investigation of the destructive energy proceeds thus. To heat 743·2 kilogs. of water from 0° to 152°·84 requires $743 \cdot 2 \times 154 \cdot 38$ *calories* = 114740 *calories*; and this is the quantity of heat for which we must account in every stage of the expansion, when the steam is allowed to blow into a cylinder and drive a piston before it. Now at any instant let w be the number of kilogs. of water converted into saturated steam; T the common temperature of the water and steam; Q_T the number of *calories* required to heat 1 kilog. of water from 0° to T; λ_T the number of *calories* required to convert 1 kilog. of water at 0° into steam at T; P_T the pressure of saturated steam at T in millimètres of mercury at 0°; K_T the same pressure in kilogrammes per square décimètre (all which are given for numerical values of T by Regnault); V_T the ratio of the volume of saturated steam under pressure P_T to the volume of the water at 0° from which that steam is derived (which is given by Fairbairn's formula). Then, forming the expressions for the number of kilogs. of water and steam respectively, and multiplying each by its corresponding number of *calories*, and equating the aggregate to the original number of *calories*,

$$114740 = (743 \cdot 2 - w) \times Q_T + w \times \lambda_T.$$

From this formula, with any assigned numerical value of T, w (the number of kilogrammes of water converted into steam) is found in numbers. And V_T, the ratio of the volume of the steam generated to that of the water from which it is generated, is taken in numbers from Fairbairn's formula. And a kilogramme of water occupies one cubic décimètre of volume. Therefore the volume of steam, in cubic décimètres, is $w \times V_T$, of which w are left in the boiler to occupy the place of the expanded water; and the volume of steam expelled from the boiler is $w \times (V_T - 1)$ in cubic décimètres, or $\frac{w}{1000} \times (V_T - 1)$ in cubic mètres.

12. Suppose now that the steam in escaping enters a cylinder whose section is 1 square mètre, driving a piston before it. Let z be the distance to which the piston has travelled (the unit being the mètre). Then $z = \frac{w}{1000} \times (V_T - 1)$. And the pressure of the steam on the piston (the unit being the kilogramme) is $100 \times K_T$. Therefore the two elements, the distance of the piston and the pressure upon it, can be calculated numerically

for any number of numerical values of T. To find the effective pressure, the pressure first found must be diminished by the atmospheric pressure, or by the pressure of steam at 100°, and it thus becomes $100 \times (K_T - K_{100})$. The limit of the length of the cylinder will be determined by finding where the steam-pressure = atmospheric pressure. By Fairbairn's formula, 74·638 kilogs. of saturated steam at 100° (the quantity which escaped in Mr. Biddell's experiment) occupy 122·28 cubic mètres: of this, 0·0746 cubic mètre remains in the boiler, taking the place of the water from which it was produced; the whole volume expelled is therefore 122·21 cubic mètres, and the limiting length of the cylinder is 122·21 linear mètres.

13. By these methods Professor Miller calculated the following corresponding values of *z*, the distance to which the piston has travelled (the unit being the mètre), and F the effective pressure on the piston (the unit being the kilogramme). The degrees of temperature are also given, as they are the elements from which *z* and F are computed; but they are not in any way used in the subsequent calculations.

T. Degrees Centigrade.	*z*. Mètres.	F. Kilogrammes.
152·84	0	42185
150	1·621	38356
145	4·743	32162
140	9·345	26615
135	14·740	21668
130	21·541	17271
125	30·136	13377
120	40·976	9943
115	54·708	6926
110	72·186	4288
105	94·325	1991
100	122·21	0

14. The effective energy of the expanding steam, as shown (for instance) by the momentum communicated to a material piston, will be represented by the integral $\int dz \,.\, F$. As the symbolical form of the function F is not known, it is necessary to perform the integration by quadrature. For this purpose I laid down the twelve data of this Table graphically (taking *z* as the abscissa, and F as the ordinate), and drew a curve by hand through the points so defined. Then I measured the ordinate for each of the values of *z*; 0, 1, 2, 3, &c. And I integrated them by the formula $\frac{1}{2}$ (first ordinate + last ordinate) + sum of intermediate ordinates $-\frac{1}{12}$ sum of second differences; where it

is seen that sum of second differences is sensibly equal to — first of first differences. Thus I found the integral

1131400,

which is the true measure of the energy of the 22 cubic feet of water at the temperature which produces the pressure 60 lbs. to the square inch, in a hot iron boiler, the units of the energy being the mètre and the kilogramme.

15. If this be diminished in the ratio of 743·2 kilogs. (the fictitious weight of water on which Professor Miller's calculations are made) to 597·1 kilogs. (the real weight of water), then we shall have for the measure of the energy of the 22 cubic feet of water at the same temperature, unassisted by the hot iron of the boiler,

909000;

and if we divide the two numbers by 22, we have for the energy of one cubic foot of water at temperature producing a pressure of 60 lbs. to the square inch,

As surrounded by hot iron, 51400. (This is the practical value.)

Without influence from surrounding iron, 41300. (This is the philosophical value.)

16. I now proceed with the evaluation of the energy of gunpowder. The formula applicable to cannon-experiments is given in art. 4. Professor Miller referred me to a series of experiments by General Didion, in his *Traité de Balistique*, p. 485. These experiments were made with cannon of four different bores, and with eighteen different charges of powder in each cannon. The first thing to be done was, to find by trial for each cannon, by means of the formula $\frac{W.V^2}{2g.w}$ (which for a single cannon may be reduced to $\frac{V^2}{w}$), what was the charge of powder in which the momentum produced bore the greatest proportion to the weight of the powder. It was found to be the following:—

With ball of 12 kilogrammes, 1·500 kilog. of powder.
" 8·07 " 1·250 "
" 6·08 " 0·875 "
" 4·05 " 0·625 "

Then adopting these four for comparison among themselves, by the formula $\frac{W.V^2}{w}$, it was found that the cannon in which the powder was most efficient was that with a ball of 6·08 kilogs. Here it may be desirable to state that the bore was 0·1213 mètre, the diameter of the ball 0·1182 mètre, the length of bore 2·815 mètres; and, with the charge of powder 0·875 kilog., the velocity of the issuing ball was 400 mètres per second. Applying the formula $\frac{W \times V^2}{19{\cdot}623 \times w}$, the energy of 1 kilogramme of gunpowder (as fired in a cannon) is found to be

$$56656,$$

and that of an English pound of gunpowder

$$25700,$$

the units being in all cases the mètre and the kilogramme.

17. Comparing this with the numbers found in art. 14, we have,

> The destructive energy of 1 cubic foot of water at the temperature which produces the pressure 60 lbs. to the square inch, surrounded by hot iron, is precisely equal to the destructive energy of two pounds of gunpowder as fired in a cannon.

18. The destructive energy of the hot water, however, abstracting the effect of the surrounding hot iron, is considerably less than the number used in this comparison; and the destructive energy of the gunpowder, abstracting the effects of windage, cold iron, and short barrel, is considerably greater than the number used for it. Without pretending to form an accurate estimate of these effects, I think that their combination with that affecting the energy of the water may have diminished the apparent proportion of the energy of gunpowder by one-half. In that case

> The destructive energy of 1 cubic foot of water at the temperature which produces the pressure of 60 lbs. to the square inch is equal to that of 1 pound of gunpowder.

Royal Observatory, Greenwich,
September 3, 1863.

XLVIII. *On the Measurement of High Temperatures.*

By MM. H. Sainte-Claire Deville *and* Troost*.

IN the experiments which chemists have constantly to make at very high temperatures, they are frequently stopped by the difficulty of finding suitable vessels. Platinum vessels appear at first sight perfectly suitable; but we have always discarded them, from the mistrust with which we are inclined to regard a metal that is generally considered as having the property of condensing

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* *Comptes Rendus*, vol. lvi. p. 977 (May 25, 1863).

on its surface the gases with which it comes in contact. The perusal of M. Edmond Becquerel's last paper has suggested the experiments which led us to the discovery of the real reason why platinum is not to be trusted when we wish to try experiments on gases or vapours at a high temperature.

We take a platinum tube made from a well-worked ingot*, which is joined and drawn like a wire, so as to give a very sound, perfectly homogeneous tube, without any soldering. (The two tubes upon which we experimented were in this state, and had been made with the utmost care by MM. des Moutis, Chapuis and Quennessen.) This tube is introduced into another of Bayeux porcelain much wider and shorter, and the annular space between the two is closed by means of corks. This annular space is filled with broken porcelain, and a current of dry pure hydrogen is sent through it, which, entering and issuing by two glass tubes, cannot have any direct communication with the inside of the platinum tube. This tube, which is closed by two india-rubber stoppers previously heated, receives air which has been dried by passing through an Alvergniat's washing-bottle (*laveur de M. Alvergniat*), a glass filled with pumice moistened with sulphuric acid, and, lastly, a vessel with pieces of fused potash. The apparatus thus prepared† is put into a furnace fed with air and carbon from gas-retorts. Thus in our apparatus the air and hydrogen circulate separated by a solid and continuous partition of platinum. On collecting the air which leaves the platinum tube at the common temperature, it is found to have the normal composition—

Oxygen . . .	20·9	21
Nitrogen . . .	79·1	79
	100·0	100

At the ordinary temperature the hydrogen which issues from the annular space is entirely absorbed by oxide of copper without residue.

But if the temperature is slightly raised, the effect alters with surprising regularity: the air gradually loses its oxygen, and there condenses in the delivery-tube water which we have collected and weighed; its proportion keeps increasing with the temperature. At last, when this has reached about 1100° (by

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* We are only speaking here of common spongy platinum made coherent by hammering in the way that it has always been prepared until lately. We are now having a tube made of cast platinum, with which we shall repeat our experiments.

† This is the same apparatus which one of us has already used, except that a tube of porous earthenware was then employed instead of the platinum tube (see *Comptes Rendus*, vol. lii. p. 524).

estimate), the platinum tube into which dry air is sent yields nothing but nitrogen and water*; at the same time the number of bubbles of hydrogen which issue from the tube leading from the annular space is seen to diminish†. Moreover, when the temperature of 1100° (by estimate) is exceeded, the gases leaving the platinum tube contain an appreciable quantity of hydrogen.

If the apparatus is allowed to cool, the same effects are reproduced, but in the contrary order, the gas which traverses the platinum tube returning to pure air.

During the whole course of the experiments, the hydrogen which leaves the annular space is entirely absorbed by oxide of copper.

If at the moment when the temperature is very high we sud-

* *Composition of the Air at regularly increasing Temperatures, starting from Red Heat.*

Oxygen ..	19·0	16·7	15·5	12·3	10·5	8·8	5·9	3·0	
Nitrogen ..	81·0	83·3	84·5	87·7	89·5	91·2	94·1	97·0	100·0
	100·0	100·0	100·0	100·0	100·0	100·0	100·0	100·0	100·0

Starting from about 1100° *and passing a little beyond this temperature,*

Hydrogen............	1·3	21·9
Nitrogen	98·7	78·1
	100·0	100·0

Quantities of Water obtained per hour, air passing at the rate of about one litre per hour.

89 milligrammes.
183 ,,
282 ,,
554 ,,

This water contains a small quantity of nitric acid.

If carbonic oxide is substituted for hydrogen, this gas does not penetrate in any perceptible quantity into the platinum tube. The air which leaves the tube possesses very nearly the normal composition—

Oxygen	20·7	20·6
Nitrogen	79·3	79·4
	100·0	100·0

Liebig's tubes, through which the air and carbonic oxide which issue from the apparatus are passed, do not change their weight, and the gases are perfectly dry.

These trials have been made on two tubes of about 1 millim. thick, heated for a length of 15 centims., or the quarter of the whole length. These tubes, intended to be used for the concentration of sulphuric acid, hold a vacuum, and have been tested under great pressure at ordinary temperatures.

† The air and hydrogen are supplied by apparatus yielding strictly regular currents.

denly close the cock which conducts the hydrogen into the annular space, and immediately plunge the tube which lets out the gas into a mercury trough, the mercury is seen to rise gradually in this tube to a height of 602 millims. (the barometer being at 753). The hydrogen is therefore still forcing its way into the platinum tube, and an almost complete vacuum is produced in the annular space. And yet we also ascertained that our apparatus, difficult as it was to construct, did not close perfectly. This is perhaps the only reason which can explain this slight difference of 15 centims. between the height of the barometer and the height of the mercury in the delivery-tube, a difference which measures the pressure of the gas remaining, or introduced by leaks in the porcelain tube.

The result of the foregoing is, that platinum behaves at high temperatures like the porous vessels with which the beautiful experiments by M. Jamin on the diffusion of gases and those already published by one of us are obtained.

A sufficiently convincing proof of this porosity may also be had by substituting, in the preceding experiment, pure and dry carbonic acid for air, and maintaining the current of hydrogen in the annular space. At the end of the platinum tube a great quantity of carbonic oxide and hydrogen may be collected at the same time as carbonic acid*.

These results show that it is impossible to construct gas-pyrometers of platinum when they have to come into contact with the reducing gases, or with the hydrogen of a furnace†. It is

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* Working with the same tubes at high and increasing temperatures, gases are obtained containing—

Hydrogen	12·7	7·3
Carbonic oxide	..	17·7
Carbonic acid	87·3	75·0
	100·0	100·0

The last analysis relates to the gas which issues from the platinum tube and is collected at the highest temperature: the result establishes the presence of carbonic oxide and of moisture in the gases collected.

† M. Pouillet, in his experiments on the pyrometer which he first employed, avoided to a great extent this source of error, as he heated his apparatus in an iron muffle very nearly closed. His temperatures are also much higher than those of M. Ed. Becquerel, especially if M. Pouillet's results are modified by adopting, as he himself recommends, the new coefficient of expansion $\frac{1}{273}$ instead of Gay-Lussac's coefficients, with which they were originally calculated (*Traité de Physique*, par M. Pouillet, 6^{me} édit. 1853, t. 1er, pp. 237, 238, 239, 269, plate 9. fig. 8). M. Ed. Becquerel places the platinum reservoir of his pyrometer inside an *earthenware* tube —that is to say, in a porous material through which the hydrogen of the furnace can penetrate with the greatest ease.

also easy to understand why the temperatures ascertained by M. Ed. Becquerel, and which differ from those which we have ourselves published, are lower by about 100° than all those which have been obtained either by ourselves or by others.

This porosity of the platinum perhaps prevents the gases from being maintained at a high temperature and at a high pressure; but the most important effect is that endosmose causes the gases of the furnace to enter to the air of the pyrometer in spite of a contrary pressure. Water is thus formed, and there is a diminution of volume due to the disappearance of oxygen. It will be hence understood why, in M. Ed. Becquerel's experiments, it was necessary to keep pieces of chloride of calcium constantly in the pyrometer, and even to change them between the different experiments (see p. 88 of his *Mémoire*).

It is thus evident how the mass of gas, which in the platinum pyrometer ought to remain the same (constant C), has, on the contrary, varied from 23·8622 to 19·9164 in the series of experiments in which M. Ed. Becquerel appears to place most confidence. This variation of the constant attains the proportion of 16·5 per cent. of its maximum value, which approaches the proportion of oxygen which the air contained at the beginning of the experiment. M. Ed. Becquerel explains (page 90) this considerable variation, on the supposition that the mercury in his manometer could combine in the cold with the oxygen of the air contained in the apparatus. This hypothesis is inadmissible: it would take away all precision from experiments made with his or similar apparatus; it would throw doubt upon all coefficients of expansion, &c. . . . . and in fact upon numbers which have received every possible experimental confirmation.

The true explanation is plainly to be derived from our experiments; it is that, at a temperature not necessarily very high, platinum becomes capable of producing the phenomena of endosmose, and perhaps even of the losses by pressure which we find in porous substances.

The differences between the temperatures observed by M. Ed. Becquerel and ourselves are the following:—

	E. Becquerel.	Deville and Troost.	Difference.
Boiling-point of cadmium .	746°·3	860°	113°·7
Boiling-point of zinc	932·0	1040	108·0

We have made use, for our determinations, of a direct process independent of any hypothesis, and free from any capital source of error; it is the measurement, under the same pressure, of a volume of air taken successively at 0°, then at the temperature which we wish to measure, and enclosed in impermeable vessels of Bayeux porcelain absolutely rigid at this temperature, at which they hold a vacuum perfectly, and are still very far from their point of fusion or softening. These vessels, through which a current of dry air is sent until incipient red heat, are closed at the end of the experiment by an oxyhydrogen blowpipe, just as we close a glass air thermometer with a mouth-blowpipe. The only sources of error consist in the observation of barometric pressure, of the temperature of the surrounding atmosphere, and lastly in the reading of the volume of the gases in a graduated tube. This latter being all that we have to consider, we should say that in working with flasks of 300 to 330 cubic centims. capacity, a very considerable and inadmissible error of half a cubic centim. would not sensibly affect our results. The temperatures found by M. Ed. Becquerel would ascribe to us an error of about 10 cubic centims. upon 80, which is out of the question.

The only causes of uncertainty are such as may arise from the apparatus in which the metals are heated to ascertain their boiling-point. To arrive at a degree of exactitude which does not affect our vapour-densities, we are now repeating our measurements of temperature by aid of the most delicate methods. But as M. Ed. Becquerel seems to have placed himself very nearly in the same conditions as ourselves, it was needful to look elsewhere for the cause of our difference. It is this consideration that led us to seek in platinum for a property unknown hitherto, though in perfect harmony with its catalytic action, and the feebleness of its conducting-power for heat and electricity*.

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* While this paper was printing, we received from Mr. Matthey, the great London manufacturer, a cast platinum tube weighing 1070 grammes, 60 centims. long and about 2 millims. thick. All the experiments which we have already described have been repeated with this tube, and gave exactly the same result, causing the formation of water by means of hydrogen and at the expense of the air, and producing a vacuum in the annular space, &c. The immense thickness of this tube and its quality have in no respect altered the phenomena, either in their direction, in their intensity, or in their activity. Mr. Matthey, knowing what we were in need of, has had this tube manufactured with part of the ingot of 100 kilogs. of cast platinum which he exhibited in 1862, and, anticipating our wishes, has obligingly sent it to us for the benefit of science. We publicly express our obligations to him.

XLIX. *The Mass of the Earth is arranged in nearly spherical strata around its centre; and if the outer surface be a spheroid of equilibrium, then all the strata are so also, whether they acquired that form from once being fluid or not.* *By* Archdeacon J. H. Pratt.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

It has been asserted that all we know with any certainty regarding the constitution of the earth's mass is contained in the following facts derived from observation and experiment: (1) That the level-surface obtained by geodesy, to which the plumbline is everywhere perpendicular, is almost exactly a spheroid, of small ellipticity, about $\frac{1}{295}$th, and coincides very nearly, if not exactly, with the mean surface of the earth. (2) That the force of gravity is nearly the same on all points of the surface, and changes, in passing from place to place, almost exactly as the change in the square of the sine of the latitude. (3) That the mean density is nearly equal to 5·66 times the density of water. It has been said that any notions we have of the interior arrangement of the mass are altogether theoretical. In connexion with this subject it has also been stated that nothing can be gathered from pendulum experiments (and therefore from the value of gravity and the ellipticity thence deduced) regarding the fluid arrangement of the earth's mass. There appears to me to have been some confusion of ideas on this subject. My present object is to show that the first of the three facts I have enumerated above leads logically to the two following results:—

I. That the earth's mass is, as a matter of fact, arranged in nearly spherical layers of equal density about the earth's centre.

II. That if we assume, as is generally done, that at least the mean exterior surface of the earth is a spheroid of equilibrium, then it follows as a necessary consequence that the forms of all the interior strata are also spheroids of equilibrium, and follow the fluid law, whether they are fluid or not now, or ever were so, in part or in whole.

I. *The earth's mass, as a matter of fact, consists of nearly spherical strata of equal density about the earth's centre.*

2. Let r, θ, ω be polar coordinates to any point on the earth's surface, the centre being origin ($\cos\theta=\mu$); r', θ', ω' coordinates to any point in the interior ($\cos\theta'=\mu'$), ρ' the density at that point; a the mean radius of the surface; $r=a+\alpha\,.\,u$, u being a function of μ and ω, and α a small fraction, because the surface is nearly spherical. Then the potential of the whole mass for

the point on the surface is

$$V=\int_{-1}^{1}\int_{0}^{2\pi}\int_{0}^{r'}\frac{\rho' r'^2\,d\mu'\,d\omega'\,dr'}{\{r^2+r'^2-2rr'p\}^{\frac{1}{2}}},\quad r'=a+\alpha\,.\,u',$$

where

$$p=\mu\mu'+\sqrt{1-\mu^2}\,\sqrt{1-\mu'^2}\cos(\omega-\omega').$$

By expansion this becomes

$$V=\int_{-1}^{1}\int_{0}^{2\pi}\int_{0}^{r'}\rho'\left\{\frac{r'^2}{r}P_0+\ldots+\frac{r'^{i+2}}{r^{i+1}}P_i+\ldots\right\}d\mu'\,d\omega'\,dr',$$

where $P_0\ldots P_i\ldots$ are Laplace's coefficients.

Put $\rho'=R'+\beta\,.\,U'$, where R' is a function of r' only, independent of μ' and ω', and U' is a function of all three, r', μ', ω'; β is a constant. What I am going to prove is, that β is a small fraction of the order of α. Suppose that

$$\int_{0}^{r'} r'^{i+2}\rho' dr'=\phi(r')+\beta\psi(r',\mu',\omega')=\phi(a+\alpha u')+\beta\psi(a+\alpha u',\mu',\omega')$$
$$=A+\alpha B(u'_0+\ldots+u'_i+\ldots)+\beta(\psi'_0+\ldots+\psi'_i+\ldots),$$

these being series of Laplace's functions. Then by a well-known property of these functions, M being a constant,

$$V=\int_{-1}^{1}\int_{0}^{2\pi}\left\{\frac{P_0}{r}M+\ldots+\frac{P_i}{r^{i+1}}(\alpha Bu'_i+\beta\psi'_i)+\ldots\right\}d\mu',\ d\omega',$$

$$=\frac{4\pi}{r}M+\ldots+\frac{4\pi}{(2i+1)r^{i+1}}(\alpha Bu_i+\beta\psi_i)+\ldots$$

by another property; u_i and ψ_i being the same as the same functions with an accent, μ and ω being put for μ' and ω'.

Now gravity is the attraction of the earth's mass diminished by the centrifugal force, which is always extremely small. Since, then, gravity very nearly equals the attraction of the mass, and its direction is very nearly towards the earth's centre (being a normal to the spheroid of equilibrium of small ellipticity), it follows that

$$\frac{\sqrt{1-\mu^2}}{r}\frac{dV}{d\mu}\ \text{and}\ -\frac{1}{r\sqrt{1-\mu^2}}\frac{dV}{d\omega},$$

which are the attractions in the meridian and prime vertical at right angles to r, must both be very small quantities. Hence

$$\alpha B\frac{du_i}{d\mu}+\beta\frac{d\psi_i}{d\mu},\ \text{and}\ \alpha B\frac{du_i}{d\omega}+\beta\frac{d\psi_i}{d\omega}$$

are both very small, and this whatever values μ and ω have—that is, for all points on the earth's surface. This cannot be the case unless β be a very small fraction, of the same order as α.

Hence the term in ρ' which depends upon μ' and ω' is very small; and therefore ρ' is a function of $r'+\alpha\,.\,\nu'$, where $\alpha\,.\,\nu'$ is some small function of μ' and ω'; and $r'+\alpha\,.\,\nu'=$ constant will be the general equation to the layers of equal density; and these, then, are all nearly spherical, because $\alpha\,.\,\nu'$ is small.

II. *If the* exterior surface *be a spheroid of equilibrium, the layers are also spheroids of equilibrium, and follow the fluid law, whether they are or have been fluid or not.*

3. If the surface is one of fluid equilibrium, then

$$\text{constant} = \mathrm{V}+\tfrac{1}{2}w^2(1-\mu^2)r^2 = \mathrm{V}+\frac{m}{2}\frac{\mathrm{E}}{a^3}(1-\mu^2)r^2,$$

where $m=\frac{1}{289}$, E$=$ the earth's mass.

By differentiating the value of V first given in paragraph 1, it is not difficult to show that

$$\frac{d}{d\mu}\left\{(1-\mu^2)\frac{d\mathrm{V}}{d\mu}\right\}+\frac{1}{1-\mu^2}\frac{d^2\mathrm{V}}{d\omega^2}=-r\frac{d^2\,.\,r\mathrm{V}}{dr^2}.$$

Let V be expanded in a series of Laplace's functions, and terms of the ith order on the two sides equated. Then remembering that by Laplace's equation the first side will be equal also to $-i(i+1)\mathrm{V}_i$, we have

$$r\frac{d^2\,.\,r\mathrm{V}}{dr^2}-i(i+1)\mathrm{V}_i=0.$$

The solution of this is $\mathrm{V}_i=\mathrm{W}_i r^{-i}+\mathrm{Z}_i r^i$, where W_i and Z_i are independent of r. The complete value of V becomes

$$\mathrm{V}=\frac{\mathrm{W}_0}{r}+\frac{\mathrm{W}_1}{r^2}+\frac{\mathrm{W}_2}{r^3}+\ldots+\mathrm{Z}_0+\mathrm{Z}_1 r+\mathrm{Z}_2 r^2+\ldots$$

Now V must evidently equal zero when r is infinitely great: hence $\mathrm{Z}_0=0$, $\mathrm{Z}_1=0$, $\mathrm{Z}_2=0$, ... Also the greater r is, the more nearly equal are the distances of the attracted point from the various points of the earth's mass; and ultimately as r is increased, the potential must become $\mathrm{E}\div r$. Hence

$$\mathrm{V}=\frac{\mathrm{E}}{r}+\frac{\mathrm{W}_1}{r^2}+\frac{\mathrm{W}_2}{r^3}+\ldots\ldots$$

The first term of this is the value V would have if the earth's mass were arranged in exactly spherical shells. This is not exactly the state of the earth, but is very nearly so, as I have already shown. Hence W_1, W_2.... are all small quantities.

Let $r=a\big(1+\epsilon(\frac{1}{3}-\mu^2)\big)$ be the equation to the ellipse generating the spheroid of equilibrium of the surface. Put this and V in the equation of equilibrium at the beginning of this paragraph, and neglect small quantities of the second order;

$$\therefore \text{const.} = \frac{\mathrm{E}}{a}\left(1-\epsilon\left(\tfrac{1}{3}-\mu^2\right)\right)+\frac{\mathrm{W}_1}{a^2}+\frac{\mathrm{W}_2}{a^3}+\ldots+\frac{m}{2}\frac{\mathrm{E}}{a}(1-\mu^2).$$

Equate Laplace's functions of the same order on both sides, observing that $\frac{1}{3}-\mu^2$ is one of the second order, and

$$a\,.\,\text{const.} = \mathrm{E}+\tfrac{1}{3}m\mathrm{E},\quad \mathrm{W}_1=0,\quad \mathrm{W}_2=\mathrm{E}a^2(\epsilon-\tfrac{1}{2}m)(\tfrac{1}{3}-\mu^2),$$
$$\mathrm{W}_3=0,\quad \mathrm{W}_4=0,\ \ldots.$$
$$\therefore \mathrm{V}=\frac{\mathrm{E}}{r}+\left(\epsilon-\frac{m}{2}\right)\frac{\mathrm{E}a^2}{r^3}\left(\tfrac{1}{3}-\mu^2\right).$$

From this formula is derived an expression for the value of gravity, which

$$=-\frac{d\mathrm{V}}{dr}-\text{cent. force},=-\frac{d\mathrm{V}}{dr}-m\frac{\mathrm{E}}{a^2}(1-\mu^2);$$

and therefore by pendulum experiments the value of ϵ may be found.

This formula for V is not so general as it appears to be. It is true only for a certain arrangement of the mass E; for it has been obtained upon the supposition that a part of the mass at the surface (it may be to a very small depth) was once fluid, or is arranged as if so now. Through this portion of the mass, then, the arrangement or law of density must be according to the fluid law. From the manner in which E enters into the above expression for V, viz. as the total mass, without any apparent reference to the manner in which that mass is arranged, it may be thought by some that although the exterior part follows the fluid law, the other parts may follow any law, so long as the total mass is the same*. That this is an erroneous view I proceed to show. The equilibrium of a fluid in a vessel is unaffected by the form or density of the material of the vessel, *unless* we take into account the attraction of the parts of the vessel. This is the case with the earth, the solid part of which may be regarded as a vessel holding the portion which is fluid by hypothesis, and altogether regulating its form by the attraction of its several parts. If the whole were fluid, the equation of equilibrium with which the calculation begins would appertain directly to the

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* It is the fact of my using the value of V above given in two papers in the Philosophical Magazine for December 1862 and Supplement, giving *Tests of the Fluid Theory*, which has led to my writing the present paper. As E enters into V without any apparent condition as to the distribution of the mass, it might be thought that the changes of that distribution which I suggested as tests ought not to have produced any effect on the pendulum; which would be directly contrary to the results of my papers.

whole mass, and the arrangement of the mass E would obviously be according to the fluid law throughout. Suppose any part of this to become solid, retaining this law of arrangement, the equilibrium of the free fluid-surface would remain undisturbed. Now consider the effect upon the free-fluid surface of any rearrangement of this solid part within its own limits. The resultant effect of this change in the arrangement of the attracting matter upon the fluid-surface would be a force which might at particular points of the surface equal zero, but which would in general vary from point to point of the surface. The equilibrium of the fluid would therefore be disturbed by this change of arrangement. It will be obvious from this, that no departure of the arrangement of the attracting mass from that of the fluid law, even in the solid parts of the mass, will allow the free surface to retain its form. Hence if the outer surface is one of equilibrium all the strata are so too; and, as the fluid theory shows, they are all spheroids of small ellipticity.

The more nearly, therefore, geodesy and pendulum experiments bring out the mean form of the earth, represented by the level surface, to be that of a spheroid, the more nearly spheroidal are the strata. This is a logical consequence of the free surface being held in equilibrium and having its form regulated by the attraction of the whole mass. It does not prove how much or how little of the mass is fluid, if, indeed, any part of it is; but it shows that (if the surface is a spheroid of equilibrium) the arrangement of the whole mass must be that peculiar one, and no other, which coincides precisely with the fluid arrangement in every part of the mass. If, then, the value of ϵ obtained from geodesy, when substituted in the formula for V, or rather in that derived from it for gravity, makes it accord with the result of pendulum experiments, we have a very strong evidence that the superficial parts of the earth's mass have the equilibrium form, and *consequently* that the whole mass throughout, solid or not, is arranged according to the fluid law. No more convincing argument, short of an absolute knowledge of the fact, could be produced to show that the earth's mass has derived its arrangement and form from having once been in a fluid condition—especially as no other conceivable cause than rotation acting on a fluid mass could have made the interior of the mass in every stratum bulge out towards the equator, and in every part by the precise quantity required by the fluid theory.

J. H. PRATT.

Murree, August 1, 1863.

L. *On the Laws of the Expansion of the Transparent Liquids by Increase of Temperature. By* Professor POTTER, *A.M.*[*]

IN my paper "On the Definition of the Temperature of Bodies, and on its Measurement by Thermometers," published in this Magazine, vol. xxiv. p. 447, I showed that the expansion of mercury, as determined by the experiments of MM. Dulong and Petit, accorded very nearly to the law of uniform expansion, or if V_0 were the volume at any temperature, V the volume at t° above it, α a constant depending on the magnitude of the degrees on the thermometric scale employed, and ϵ the base of the hyperbolic logarithms, then

$$V = V_0 \, . \, \epsilon^{\alpha t^\circ} ;$$

and for the natural unit of temperature, taken to be the interval of sensible heat between the freezing- and boiling-points of water under ordinary circumstances, the value of α was

$$\alpha = \cdot 0178576.$$

The determinations of M. Regnault for the expansion of mercury are found to be also according to the law of uniform expansion very accurately, but with the value of α as follows:—

$$\alpha = \cdot 0179901.$$

The differences between the calculated quantities and those given by M. Regnault for a volume unity at the freezing-point of water commence only at the fourth place of decimals. With our thermometric scales still unsettled, it is useless to expect greater accordance.

On examining the valuable results of M. Ch. Drion and M. J. Is. Pierre, given in the *Annales de Chimie et de Physique*, on the expansion of many transparent liquids by increase of temperature, it will be found that few, if any of them, are subject to the law of uniform expansion.

When many years ago I was engaged in experiments on photometry, I found that the quantity of light reflected out of a given incident pencil of rays, at the surfaces of crown, plate, and flint glass, was a hyperbolic function of the angle of incidence. The results were first published in vol. iv. (for 1831) of Brewster's 'Edinburgh Journal of Science'; and the formulæ, with the values of the constants for the three kinds of glass used, are also published in my 'Treatise on Physical Optics,' vol. ii., "On the Corpuscular Theory of Light," p. 36.

Finding that the results of M. Drion for the apparent volumes of hydrochloric ether at temperatures from 0° C. to 133° C., approaching its point of entire vaporization, were very far from

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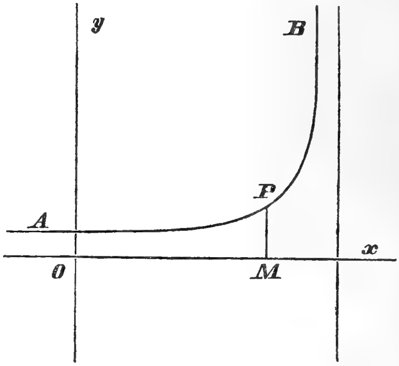
[*] Communicated by the Author.

following a law of uniform expansion, not being nearly represented by the formula $V = V_0 \cdot \epsilon^{\alpha t^\circ}$, I remembered my investigations in photometry, and resolved to apply a hyperbolic formula to M. Drion's results. I have found that the volumes at different temperatures are as nearly represented by a hyperbolic function of the temperature as can be expected, since the volumes are the apparent ones shown by an instrument in the form of a thermometer, and uncorrected for the amount of vapour in the stem and the compressibility of the liquid under its pressure, from want of the requisite data.



Let O x, O y in the annexed figure be the rectangular axes of coordinates, and A P B an arc of a rectangular hyperbola. Then for any point P, putting P M $= y$, and O M $= x$, we have the equation of the curve in the form

$$(y-a)(b-x)=c^2,$$

and of the five quantities a, b, c^2, x, y we must have the constants a, b, c^2 given in order that the curve may be known.

Now in the expansion of hydrochloric ether, if O M $= x$ represents the temperature, and P M $= y$ the corresponding volume of the liquid, then the three quantities a, b, c^2 can be calculated from three sets of corresponding volumes and temperatures. Having found the values of the constants in this manner, the value of y for any other given value of x can be calculated by the equation of the curve:

$$\text{the volume } = y = a + \frac{c^2}{b-x}.$$

By taking the first relation in the Table below, or that the volume is taken unity at the freezing temperature of 0° C., always for one of our three equations, and one of the others near the middle, with the remaining one near the extreme of the series of observations, the values of a, b, c^2 were found; and then the formula being applied to the remaining observed temperatures, and the volumes calculated, they were found to show only small differences from the observed results in every part of the series. By varying the two latter equations near the middle and end of the series, somewhat different values of a, b, and c^2 were found;

and taking the averages of four such sets of values, I obtained as follows:—

$$a = \cdot 592869,$$
$$b = 288\cdot 5649,$$
$$c^2 = 117\cdot 7508.$$

Further research might have yielded values giving results in the calculation of the volumes nearer, on the whole, to the observed volumes than these do; but it appeared unnecessary, since it is admitted that the apparent volumes need small but unascertained corrections, and some small allowance is needed for unavoidable error of experiment.

The results of M. Drion's observations* are placed in the first two columns of the Table annexed, and the volumes calculated by the hyperbolic formula in the third column:—

Temperature by Centigrade thermometer.	Apparent volumes of hydrochloric ether observed.	The volumes calculated by the formula.	Differences.
0°·	1·00000	1·00000	·00000
12·6	1·01952	1·01956	+·00004
26·4	1·04285	1·04202	−·00083
35·0	1·05856	1·05725	−·00131
45·5	1·07867	1·07731	−·00136
59·0	1·10670	1·10580	−·00090
71·5	1·13536	1·13534	−·00002
83·0	1·16470	1·16568	+·00098
93·5	1·19460	1·19652	+·00192
103·0	1·22479	1·22742	+·00263
112·0	1·25669	1·25977	+·00308
120·5	1·29079	1·29350	+·00271
128·0	1·32489	1·32622	+·00133
133·0	1·35073	1·34979	−·00094

The differences at the utmost are only small numbers, commencing in the third place of decimals; and I think we are entitled to conclude that hydrochloric ether, one of the very expansible liquids, has its volume a hyperbolic function of the temperature, and the general physical properties are connected by *three constant* magnitudes.

M. Drion shows that the expansion is equal to that of air at about the temperature 110° C.; at 130° C. it is nearly 3½ times as expansible as at the freezing-point, and it becomes entirely vapour at about 170° C.

In M. Drion's experiments with the liquid nitrous acid (*acide hypazotique*), they could be carried only from 0° C. to about 90 C., on account of the termination of the liquid in the stem

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* *Annales de Chimie et de Physique* for May 1859.

of the instrument becoming barely visible beyond the latter temperature, from the strong colour of the vapour above the liquid.

The values of the constants below were determined for the temperatures 0° C., 32°·1 C., 89° C.; so that the differences between the observed and calculated volumes disappear at these temperatures, and at the other temperatures commence only at the third place of decimals. Values of a, b, and c^2 might have been found by further investigation which would have left smaller differences upon the whole, but it did not seem necessary to examine further.

$$a = \cdot057963,$$
$$b = 553\cdot2889,$$
$$c^2 = 521\cdot2185.$$

The annexed Table shows the results:—

Temperature by Centigrade thermometer.	Apparent volumes of liquid nitrous acid observed.	The volumes calculated by the formula.	Differences.
·0°	1·00000	1·00000	·00000
12·55	1·01856	1·02186	+·00330
32·10	1·05802	1·05802	·00000
49·35	1·08286	1·09225	+·00939
68·50	1·12462	1·13310	+·00848
89·00	1·18058	1·18058	·00000

M. Drion found the coefficient of expansion of *liquid sulphurous acid* to equal that of air at the temperature of about 80° C., and at 130° C. it is nearly triple that of air, and the liquid is five times as dilatable as at the freezing temperature. It becomes entirely vapour at about 140° C.

The values of a, b, c^2 below for liquid sulphurous acid were obtained by taking the average of different determinations, and they give results which still leave the differences as seen by the Table annexed, commencing in the third place of decimals with large numbers in some parts of the series; but the accordance, considering the corrections needed, is still sufficient, through so long a series and to a point so near that of entire vaporization, to enable us to conclude that liquid sulphurous acid follows the law of hyperbolic expansion. With

$$a = \cdot589227,$$
$$b = 243\cdot453,$$
$$c^2 = 100\cdot0039,$$

we obtain the results in the third column of the Table:—

Temperature by Centigrade thermometer.	Apparent volumes of liquid sulphurous acid observed.	The volumes calculated by the formula.	Differences.
0°·	1·00000	1·00000	·00000
12·6	1·02300	1·02242	−·00068
26·4	1·05086	1·04996	−·00090
35·0	1·06983	1·06897	−·00086
49·5	1·10479	1·10484	+·00005
62·5	1·14029	1·14188	+·00159
72·5	1·17100	1·17421	+·00321
82·5	1·20576	1·21055	+·00479
91·0	1·23932	1·24519	+·00587
100·0	1·27958	1·28635	+·00677
108·5	1·32379	1·33025	+·00646
115·5	1·36641	1·37079	+·00438
122·0	1·41325	1·41262	−·00063
126·8	1·45380	1·44650	−·00730

M. J. Is. Pierre investigated, in most valuable experiments, the expansions of forty-four transparent liquids, at temperatures below their boiling-points under atmospheric pressure. He concluded that they follow very different and complicated laws of expansion. I believe it will be found generally (water being the only known exception) that the transparent liquids follow the same *law of hyperbolic expansion*, but that the three constants a, b, and c^2 have very different values for different liquids.

Aldehyde being the most expansible of the liquids which he tried, and, as he concluded, the most expansible of all liquids then known after the liquid carbonic acid, I examined first the degree of its accordance with the hyperbolic law.

The circumstances of M. Pierre's experiments are a little different from those of M. Drion, the liquid being under atmospheric pressure at its free upper surface in his instrument in the form of a thermometer; whilst the experiments of M. Drion were made with a closed instrument of like form, and the liquid was thus subject to the variable pressure due to the elastic force of its vapour only at its surface. The compressibility of liquids, according to Canton's law, is so very small that the correction of the volumes on this account for even high pressures will be very small.

The values of the constants a, b, c^2 for aldehyde were determined from M. Pierre's results at the temperatures −20°·69 C., +12°·07 C., and +21°·32 C., the liquid boiling at 22° C., and its specific gravity being ·80551.

The values, as follows,

$$a = \cdot 5688698,$$
$$b = 261\cdot 5555,$$
$$c^2 = 112\cdot 8826,$$

differ only in a moderate degree from the like values for hydrochloric ether, but very considerably from those for nitrous acid.

The Table annexed gives the observed and calculated results only to five places of decimals, though M. Pierre's results are given to nine places in his papers:—

Temperature by Centigrade thermometer.	Volumes of aldehyde observed.	Volumes calculated by the formula.	Differences.
−20°·69	0·96881	0·96881	·00000
−12·50	0·98053	0·98077	+·00024
+12·07	1·02133	1·02133	·00000
16·54	1·02995	1·02959	−·00036
21·32	1·03875	1·03875	·00000

The differences commence only in the fourth place of decimals, and we need not search for more correct values of a, b, and c^2.

After examining the conformity of aldehyde to the law of hyperbolic expansion, I have undertaken a few others of the best-known liquids. The computations, otherwise somewhat laborious, have in these cases been comparatively easy, since, the constants a, b, c^2 having been determined from the first three conditions taken up, the remainder of the experimental results were found to give differences from the calculated values commencing at the most in the fourth place of decimals, and generally with small numbers.

For alcohol, taking the experimental volumes at the temperatures −32°·22 C., 0° C., 76°·73 C., as given in the Table annexed, I find as follows:—

$$a = \cdot 3538069,$$
$$b = 617 \cdot 5750,$$
$$c^2 = 399 \cdot 0727.$$

Temperature by Centigrade thermometer.	Volumes of alcohol observed being 1 at 0° C.	Volumes calculated by the formula.	Differences.
−32°·22	·9679586	·9679586	·0000000
−27·02	·9728477	·9729130	+·0000653
−15·44	·9842263	·9842386	+·0000123
− 9·66	·9900518	·9900480	−·0000038
− 4·54	·9952424	·9952843	+·0000419
+ 6·98	1·0075626	1·0073869	−·0001757
7·04	1·0076592	1·0074512	−·0002080
16·93	1·0183223	1·0182139	−·0001084
22·86	1·0244951	1·0248389	+·0003438
28·00	1·0307472	1·0306889	−·0000583
33·46	1·0371362	1·0370160	−·0001202
39·93	1·0447938	1·0446687	−·0001251
47·52	1·0535632	1·0538667	+·0003035
50·33	1·0567636	1·0573349	+·0005713
56·26	1·0641601	1·0647672	+·0006071
60·41	1·0698872	1·0700629	+·0001757
73·70	1·0878048	1·0875652	−·0002396
76·73	1·0916758	1·0916758	·0000000

The figures in the Table, it will be seen, are carried to seven places of decimals; but the differences between the observed and calculated volumes of alcohol commence only in the fourth place of decimals when largest, and generally with only small numbers. We may conclude that alcohol is subject to the law of hyperbolic expansion; and in the theory of the *spirit thermometer* this point must be remembered.

For ether (*oxyde d'éthyle*), specific gravity =0·73581, taking the experimental volumes at the temperatures −15°·36 C., 0° C., and +38°·14 C., that at 0° C. being unity, the values of the constants were found as follows:—

$$a = \cdot1969908,$$
$$b = 530\cdot8687,$$
$$c^2 = 426\cdot2927,$$

and they give the results in the third column of the Table.

Temperature by Centigrade thermometer.	Volumes of ether observed, being 1 at 0° C.	Volumes calculated by the formula.	Differences.
−15°·36	0·9774193	0·9774195	+·0000002
−14·60	·9785231	·9785068	−·0000163
−13·25	·9803646	·9804458	+·0000812
−12·19	·9818079	·9819751	+·0001672
−11·51	·9828267	·9829590	+·0001323
−10·11	·9847827	·9849932	+·0002105
− 7·36	·9889160	·9890195	+·0001035
− 5·54	·9916470	·9917068	+·0000598
+ 7·71	1·0116393	1·0118347	+·0001954
10·26	1·0155083	1·0158258	+·0003175
13·04	1·0197512	1·0202218	+·0004706
15·09	1·0233022	1·0234937	+·0001915
18·98	1·0294103	1·0297746	+·0003643
20·64	1·0318251	1·0324837	+·0006586
23·36	1·0365665	1·0369618	+·0003953
26·16	1·0412668	1·0416218	+·0003550
28·83	1·0458173	1·0461138	+·0002965
31·86	1·0508910	1·0512698	+·0003788
31·99	1·0512383	1·0514922	+·0002539
34·07	1·0548987	1·0550700	+·0001713
35·85	1·0581201	1·0581556	+·0000355
38·14	1·0621575	1·0621578	+·0000003

The differences in the Table commence when largest only in the fourth place of decimals, and generally with small numbers. With one exception they are positive; and by reducing the value of *a* by unity in the fourth place of decimals, several would have become negative but small, and the remaining positive differences would have been reduced in value. Ether evidently follows the law of hyperbolic expansion.

For esseutial oil of turpentine (*térébène*), taking the temperatures 0° C., 68°·86 C., and 149°·59 C., I find the values of a, b, and c^2 as follows:—

$$a = \cdot 1833844,$$
$$b = 884\cdot 4142,$$
$$c^2 = 722\cdot 2265,$$

which give the results in the Table annexed.

Temperature by Centigrade thermometer.	Volumes of oil of turpentine observed, being 1 at 0° C.	Volumes calculated by the formula.	Differences.
0°	1·0000000	1·0000000	·0000000
21·81	1·0204450	1·0206474	+·0002024
43·76	1·0425062	1·0425088	+·0000026
68·86	1·0689505	1·0689494	−·0000011
90·08	1·0926244	1·0926066	−·0000178
90·14	1·0926861	1·0926754	−·0000107
111·27	1·1178598	1·1175264	−·0003334
130·96	1·1423470	1·1419384	−·0004086
149·59	1·1662405	1·1662404	−·0000001
157·28	1·1767301	1·1766349	−·0000952

The differences between the observed and calculated values are small, commencing when largest with small numbers in the fourth place of decimals; and we must conclude that oil of turpertine is subject to the law of hyperbolic expansion.

In the geometrical representation of our problem we have employed the equation $(y-a)(b-x)=c^2$, and it must be homogeneous; that is, y representing a volume of the liquid, a represents a volume also; and x representing degrees of temperature, b represents degrees of temperature also. The quantity c^2 consequently represents a product of a volume and a temperature. For convenience, putting c' for the volume, and c'' for the temperature, and $c^2=c'.c''$, we have

$$\text{volume } y = \text{vol. } a + \text{vol. } c' \times \frac{c''}{b-x}.$$

In this expression it is evident that we may change the unit of volume without altering the units or degrees of temperature, and the converse, and the equation will remain homogeneous. As, for instance, we might, with M. Pierre, take the volume of the liquid at its boiling-point for the unit of volume, and leave the degrees of temperature those of the Centigrade scale.

On the other hand, we may leave the unit of volume that of the liquid at the freezing-point of water, and change the unit of temperature to that of the natural scale with the freezing-point zero, and the interval from freezing to boiling of water unity or 1°. We may in the same way adapt the formula to Fahrenheit's scale.

When x is expressed in terms of the natural scale of temperatures, we have with the previous values of a, b, and c^2, merely to divide b and c^2 each by 100.

For example, in this case we should have for alcohol

$$a = \cdot 3538069,$$
$$b = 6\cdot 175750,$$
$$c^2 = 3\cdot 990727,$$

and similarly for the other liquids.

When the temperatures are expressed on Fahrenheit's scale, we should have

$$\frac{c''}{b-x} = \frac{\frac{9}{5}c''}{(\frac{9}{5}b+32)-(\frac{9}{5}x+32)}.$$

Putting the degrees on Fahrenheit's scale $= x' = \frac{9}{5}x + 32$, we must multiply c^2 and b by $\frac{9}{5}$, and add 32 to the product $\frac{9}{5}b$ for the required formula.

For example, the formula for the volumes of alcohol, when the temperatures are given in Fahrenheit's degrees x', becomes

$$\text{the volume} = \cdot 3538069 + \frac{718\cdot 33086}{1143\cdot 635 - x'};$$

and in like manner formulæ are easily obtained for the other liquids when a, b, and c^2 are known for given scales.

LI. *Some results of Experiments instituted with Lucifer Matches and others ignited by friction.* By F. A. Abel, *F.R.S.*[*]

TWO distinct experimental inquiries, upon which I have recently been engaged, involved the chemical examination of a considerable number of matches of different kinds, manufactured in London, the Provinces, Germany, and Sweden; and included determinations of the comparative susceptibility to ignition by friction or percussion, and by heat, and of other points bearing reference to the safety and keeping qualities of the matches of different manufacture. Believing that the information furnished by the results of these experiments may be of some general use and interest, I venture to lay a brief account of them before the Members of the British Association.

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[*] Communicated by the Author, having been read at the British Association at Newcastle-upon-Tyne in August 1863.

Composition of Matches of different classes.

Thirty-five different kinds of matches were examined and experimented with; these included, besides ordinary lucifer or Congreve matches, varieties of wax or Vesta matches, of cigar lights (Fusees, Vesuvians, &c.), and of so-called "safety matches."

The following points were noted with reference to the nature of the Congreve and Vesta matches.

I. The inflammable coating applied to the wooden matches (beneath the igniting composition) was sulphur, with very few exceptions. In two instances the tips had been soaked in or coated with fatty matter (apparently stearic acid); in two others, paraffin was employed as the inflammable coating; and the ends of the round Austrian matches (*Pollak* and *Hermann* and *Gabriel*) were found to have a coating of resin applied over the igniting composition and on the match some little distance beyond—thus serving the double purpose of protecting the composition from the effects of moisture, and rendering the wood in proximity to the former more inflammable. In a variety of Congreve match of French manufacture, the sulphur coating is similarly applied *over* the composition, and a perfectly waterproof, though somewhat difficultly ignitable match is thus produced.

II. The igniting compositions of the Congreve and Vesta matches present considerable variations in their characters. The following are the principal points noted in their analytical examination.

Those of all the matches of English manufacture examined were found to contain chlorate of potassa. It appears, indeed, that the employment of this salt in place of nitrate of potassa has become universal in England. On the other hand, not one description of the foreign matches examined was found to be prepared with chlorate of potassa. Some German matches of inferior make contained nitrate of potassa; but the majority of German and all the Swedish matches examined contained nitrate of lead as the oxidizing agent, mixed with ordinary phosphorus, alone or in addition to red lead, litharge, or vermilion. One description of German match contained binoxide of lead with nitrate of lead, the two having probably been prepared as a mixture by the action of nitric acid upon red lead.

By far the larger proportion of matches are still prepared with ordinary phosphorus; sulphide of antimony and powdered glass are very general additional constituents of the igniting composition. The following statements show the principal variations in the nature of the igniting materials prepared with ordinary phosphorus:—

English Matches.

General constituents.	Additional constituents, varying in matches of different manufacture.
Ordinary phosphorus.	Ultramarine.
Chlorate of potassa, and binding material (gum, glue, &c.).	Prussian blue.
	Grey sulphide of antimony, red lead, glass (large quantity).
	Prussian blue, glass (large quantity).
	Sesquioxide of iron.

Foreign Matches.

General constituents.	Other constituents, varying in different matches.
Ordinary phosphorus.	Nitrate of potassa, glass.
Binding material (gum, glue, or resin).	Nitrate of lead only.
	Nitrate of lead, binoxide of lead, and vermilion.
	Nitrate of lead, red lead.
	Nitrate of lead, red lead, and litharge.

The matches of several extensive English manufacturers were found to be prepared with an igniting composition containing both ordinary and amorphous phosphorus, the latter being employed in some instances in considerable quantity, and bearing in others only a very small proportion to the ordinary phosphorus contained in the mixture. Golden sulphide of antimony and powdered glass were found in all the igniting compositions of this class, and a small proportion of free sulphur was contained in several of them.

III. The Congreve and Vesta matches of English manufacture which are tipped with compositions containing no phosphorus whatever, are unquestionably great improvements upon the earlier continental matches of this class. They are inflamed with ease and certainty when pressed over the amorphous phosphorus *rubber* on the box; and the latter does not rapidly deteriorate from the effects of atmospheric moisture upon it, as was formerly the case. I have found these matches as readily ignitable by the phosphorus rubber after the latter had been kept in a moist atmosphere for several months, as they were when freshly purchased*.

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* The very perfect manner in which mixtures containing amorphous phosphorus are protected from the effects of moisture under the most severe conditions, by means of shell-lac, has been exemplified most strikingly in the case of the so-called "detonators" in the fuses for ordnance, constructed by Sir William Armstrong. A proportion of powdered shell-lac is added to the

The compositions with which these matches are tipped consist mainly of chlorate of potassa, sulphide of antimony and glass, red lead and binoxide of manganese being additional constituents. The amorphous phosphorus, mixed with a small quantity of glass-powder, is attached to the surface of the box by means of gum.

IV. In the preparation of the older variety of cigar lights, known as fusees, nitrate of potassa appears to be extensively employed. The brown paper composing the match is impregnated with saltpetre (some were found to contain, besides, a considerable proportion of chloride of sodium, which may have existed in the crude saltpetre employed, or have been added on account of its ready fusibility, whereby the ash of the match is rendered firmer, and less liable, therefore, to fall away while glowing). The igniting composition with which these matches are tipped consists of nitrate of potassa, ordinary phosphorus, and a small quantity of vermilion.

V. The other class of cigar lights (known as Vesuvians, Etnas, Stars, &c.) consist, as is well known, of three parts—the support or handle (of wood or glass), the ball or thick coating which constitutes the body of the match, and the igniting composition with which this is tipped. The latter resembles the compositions which are applied to the ordinary Congreve or Vesta matches; the bodies of these cigar lights vary somewhat in composition; they generally consist of chlorate of potassa, coal- or coke-dust, some binding material, and in some instances an admixture of clay or brickdust, and of an aromatic substance.

Memoranda relating to the influence of Heat and of Friction or Percussion upon Matches.

It need scarcely be observed that great differences exist between the temperatures at which the igniting material of different matches will inflame *spontaneously,* and in the degree of facility with which they are ignited by friction and percussion.

The degree of sensitiveness of a match to ignition by heat only was determined by its exposure to gradually ascending temperatures, both in an air-bath and in a very confined space surrounded by heated water; and also by rapidly plunging the tip of the match into, and again withdrawing it from, heated mercury. For reasons which will be evident from some of the following state-

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materials composing the igniting or detonating composition employed in these fuses, and their mixture is effected by the aid of sufficient alcohol to convert the mass into a paste. The material thus obtained, and protected additionally (after it has been placed in its receptacle) by a coating of shell-lac varnish, resists effectually the most searching and protracted exposure to moisture, even at elevated temperatures.

ments, the latter method generally furnished the most trustworthy results.

By far the greater number of matches experimented with did not inflame at temperatures ranging up to, and somewhat exceeding, that of boiling water. Some few, which contained a large proportion of ordinary phosphorus in their igniting composition, inflamed at temperatures ranging between 77° and 100° C. Three or four different kinds inflamed after some time when surrounded by boiling water, though a considerably higher temperature was required for their ignition by contact with heated mercury or in an air-bath. This difference was traced to the slight but continuous concussions to which the head of the match was subject in consequence of the ebullition of the water. Some other matches (the cylindrical kinds from Austria), the tips of which were coated with varnish, only inflamed after a time when surrounded by boiling water, and required a temperature of 102° C. for their ignition by immersion into mercury. Upon partially removing the thin coating of varnish from the tips of these matches, they were inflamed at once at 96° C. Many of the matches tried did not inflame spontaneously below 115° C.; two or three varieties of common lucifers required a temperature of 138° C. for their ignition; and one description, containing a comparatively small proportion of ordinary phosphorus, did not inflame below 160° C.

In exposing the matches separately or in numbers (*i. e.* a box full) to gradually increasing temperatures in an air-bath, it was observed that the sensitiveness to ignition by heat alone (and by friction also) diminished in proportion to the period of exposure of the match to elevated temperatures. Thus, it was repeatedly found that a match was almost directly ignited on its introduction into a heated atmosphere in which matches of the same kind had been for some time exposed without any such result, they having been gradually raised, together with the air, to the particular temperature. The extent to which the sensitiveness of a match could be reduced by long exposure to heated air was remarkable, though it is a result which must be expected to arise from the promotion, by heat, of the gradual volatilization and oxidation of the phosphorus in the igniting material.

The order of sensitiveness to ignition by friction or percussion of a match was found to correspond with its susceptibility to ignition by heat alone, in the case of those which contained ordinary phosphorus and chlorate of potassa, provided that glass was included in the composition of the igniting material. Where this was not the case, the matches, though they might be more sensitive to the effects of heat alone than others, on account of the predominance of phosphorus in them, were considerably less so to the effects of friction or percussion. The only Congreve match examined which was prepared with nitrate of potassa and phosphorus (and contained an admixture of glass), though moderately sensitive to the effects of heat, ranked lowest as regarded its sensitiveness to ignition by friction or percussion; and the same observation applies to all the matches which contained nitrate of lead in the place of chlorate of potassa.

Observations relating to the possible sources of accident in the transport of Matches.

The results of experimental observations, of which the principal points have been noted in the foregoing, prove conclusively that no degree of heat to which, under all ordinary circumstances, matches are likely to be exposed in their transport or otherwise, would suffice to lead to their spontaneous ignition. Even a temperature such as, for example, can be attained only under exceptional conditions by the atmosphere in the hold of a vessel, would not, unaided, bring about the ignition of the more sensitive matches.

The effect, however, of even a very moderate degree of heat in facilitating the ignition, by friction or percussion, of mixtures such as those with which the tips of the matches are prepared, is well known, and admits of easy demonstration. It may be readily shown by means of matches themselves. If a box full of lucifer matches (*e. g.* one of the more carefully made and pretty tightly closing slide boxes), in which the matches, as is very generally the case, are not sufficiently long to fill the box entirely, be subject to repeated concussion, in such a way that the heads of the matches strike repeatedly with some violence against one end of the box, a proportion of the matches will eventually inflame (the number ignited and the rapidity of the ignition being regulated by the sensitiveness of the match); but the composition upon the head of the match alone burns, and the flame generally does not even spread from the head of the match ignited to those immediately surrounding it; so that only isolated matches will be inflamed in a box full with all the heads placed in one direction. If, however, a box of the same matches be exposed to a heated atmosphere (from 27° to 37° C.) sufficiently long for the matches to become warm throughout, the extent to which it will be necessary to submit them to concussion before ignition occurs will be comparatively very slight, and in all probability the whole of the matches in the box will become inflamed almost simultaneously. In common matchboxes, with which this experiment has been tried, and which did not close at all tightly, the wood of the matches was partially burned, and the box itself caught fire at the edges.

It appears, therefore, at least within the range of possibility that in the transport of matches on board ship, and in the absence of due precautionary measures, continuous concussion, combined with a degree of heat not unfrequent in the hold of a vessel, may bring about the accidental ignition of lucifer matches—an occurrence which is known to have taken place, and which, it is believed, is much more frequently the cause of fires on board ships than manufacturers and exporters of matches may be disposed to admit. While it may be granted that the accidental ignition of one or two boxes in a securely closed case of matches may frequently occur, especially under ordinary atmospheric conditions, almost without the possibility of the fire spreading even to other boxes in the same case, yet a knowledge of the very remote causes to which the accidental ignition of gunpowder and other explosions have been traced, and of the impossibility of securing infallibility in extensive manufactories—with regard to such a point as, for example, the air-tight closing of cases containing lucifer matches intended for shipment—renders it, to say the least, advisable that such precautionary measures as are obvious and easily observed should be attended to in the shipment of matches, with the view to reduce to the minimum the possibility of their accidental ignition.

Among such measures would be included—

(1) The special appropriation, if possible, of some part of the vessel in which matches are transported, to the reception of the packages containing them—or, at any rate, the stowage of such packages together (as far as practicable), distinct from other merchandise of a combustible nature, and in such positions that they may be readily removed in the event of any accidental ignition occurring;

(2) The efficient ventilation of that part of a vessel in which matches are stowed;

(3) The enforcement of rules on board ship to prevent the possibility of fire being brought by sailors or others into the vicinity of packages of matches;

(4) The careful packing of match-boxes into cases, so as to prevent their being subjected to any independent motion by the movement of the vessel; and

(5) The bestowal of a more uniform attention upon the production of safe and sufficiently stable match-boxes. It need hardly be observed that a very great variation exists in the form and quality of the match-boxes used in this country and abroad. The metal boxes with hinge lids, in which Vesta matches are most generally packed, rank highest as regards the security they afford to the match; and the circumstance that no accident, however slight, is on record as having attended the shipment of

Vesta matches, may stand in close connexion with the employment of such boxes with these particular matches. There are several other kinds of boxes much used by manufacturers (*e. g.* the cylindrical boxes of wood or cardboard with tightly fitting lids, and the superior kinds of slide boxes made of cardboard or well-covered chip) which are calculated to afford security to the matches, or rather which would do so if their length (or height) were properly proportioned to the length of the matches. In the majority of instances, however, they are longer or higher than the matches, and sometimes very considerably so; hence if the latter are not most firmly wedged into the boxes, the construction of these is positively calculated to afford opportunity for submission of the match-heads to concussion. The slide-boxes containing the Swedish matches, which are now largely imported, are all made to correspond in length with the matches they contain. A very extensive importer of these matches states that he has never known of a single instance of accidental ignition occurring in their transport.

Many of the boxes roughly constructed of thin chip, partly held together by paper coverings, which contain the commoner descriptions of English congreves, not only afford but little protection to their contents, but may actually be in many instances sources of danger, in consequence of their imperfect construction, their very fragile nature, and their ready inflammability. Unless boxes of this kind are packed with the greatest possible care (whether slide boxes, or boxes with lids secured by a paper hinge), some proportion is almost certain to sustain injury resulting in the immediate escape of matches from the box. Indeed many of these boxes in their original state readily allow of the escape of matches, if simply tilted; or at any rate the tips of the matches partially protrude from the box and become fixed in that position. In either case, some matches will thus be exposed to very greatly increased risk of ignition by friction or percussion; and if a match is fired in a package consisting of boxes such as those now referred to, the risk of the fire extending must be comparatively considerable.

The degree of security with which the frictional material (sand, glass-, or emery-powder) is attached to the match-boxes, is also subject to great variation. In some instances a piece of well-made glass- or emery-paper is pasted on to the bottom of the box, or some fine emery- or glass-powder is securely fixed to one of its surfaces by means of glue and coatings of varnish; but in many instances either glass- or emery-paper of very inferior quality is used, or the frictional powder is very loosely and carelessly attached to the box by being merely dusted over one of its surfaces, upon which a small quantity of glue has first been applied. In such instances the fragments of glass, emery, or sand are so loosely attached that a great proportion may be removed by passing the finger lightly over them. It is evident that, in packing match-boxes of this kind, much of the frictional powder will become detached, and further quantities will speedily be rubbed off and be loose in the cases if the slightest play for movement of the boxes during transport exists in the packages. The hard angular particles thus liberated will speedily insinuate themselves into the boxes and between the matches; and it is self-evident that they may then contribute their share towards augmenting the possibility of accident.

It is almost needless to mention that the foregoing remarks do not in any way refer to the matches spoken of in the beginning of this communication, which are ignited only by being passed over a surface of their box which is coated with a preparation of amorphous phosphorus. The compositions with which the matches of this class are tipped require very powerful friction or percussion for their ignition, and are of course inflamed at temperatures far above that required to ignite the least sensitive of the matches prepared with phosphorus. The possibility of their accidental ignition must therefore be, to say the least, extremely remote.

In conclusion, it may be asserted with little hesitation, that no difference of opinion is likely to exist regarding the comparatively very great risk which must be incurred in the transport of the new varieties of cigar lights, and, indeed, the impossibility of adopting precautionary measures which can warrant their shipment with any confidence in the safety of the vessel.

LII. *On a new Series of Metallic Oxides.* *By* H. Rose*.

BERZELIUS determined, by numerous and variously modified experiments, the atomic weights of the simple substances, paying especial regard to the analogous properties of their oxides, in which he evinced wonderful circumspection and a fine tact. With the greatest care he took into consideration all circumstances; so that his assumptions, with few exceptions, could be recognized as being correct, even when, after the discovery of two very important laws, it was possible to determine the atomic weights of the simple bodies with greater certainty than before. One of these laws is that discovered by Dulong and Petit, that the specific heats of the simple bodies are inversely as their atomic weights. This law has been especially

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* Translated from Poggendorff's *Annalen* for September 1863, by Dr. Atkinson, Royal Military College, Sandhurst.

confirmed by a long series of experiments by Regnault, by which he showed that the specific heat of the same substance may be different according as it is taken in the solid, the liquid, or in the gaseous condition—but that with simple bodies in the solid form, which are otherwise in the same physical condition, the product of the specific heat into the atomic weight varies between the numbers 38 and 41 (if the atomic weight is taken at 100), and that these variations depend less on errors in the experiments than on the fact that the specific heat of bodies, as determined by experiment, contains foreign elements which hitherto could not be eliminated—and that, further, simple bodies in their various allotropic forms have a somewhat different specific heat.

Dulong and Petit found that the atomic weights of the then known metals of easy preparation, and of sulphur, were the same as those which Berzelius had found, with the exception of silver. In the case of this metal, they found that the product of the specific heat into the atomic weight, as assumed by Berzelius, only agrees with the numbers which the other simple bodies give when the atomic weight is divided by 2,—a fact which Regnault's experiments also confirmed.

The second law which serves to determine the atomic weights of simple bodies is that of isomorphism, discovered by Mitscherlich. According to this, compounds of the same atomic composition may, when they can be crystallized, assume the same forms. That, exceptionally, there may be bodies of the same crystalline form whose atomic composition could not hitherto be connected, does not disprove the accuracy of this law. But both isomorphism, and the law of the connexion between the specific heat and atomic weight of bodies, led to the conclusion that the atomic weight assumed by Berzelius for silver must be diminished by one-half.

In an investigation of the crystallized compounds of antimony and arsenic occurring in nature, I had found that the composition of the crystallized Fahlerz and Polybasites, which latter have a simpler composition than the former, can only be explained by assigning to sulphide of silver an analogous composition to sulphide of copper, $Cu^2 S$. Numerous analyses of these two minerals gave the result that in them sulphide of silver and sulphide of copper can replace one another in the most varied proportions*. But the isomorphism of sulphide of silver and of sulphide of copper, $Cu^2 S$, is still further confirmed by the composition of crystallized argentiferous sulphuret of copper (*Silberkupferglanz*), which consists of sulphide of silver and sulphide of copper, $Cu^2 S$,

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* Poggendorff's *Annalen*, vol. xv. p. 576, and vol. xxviii. p. 156.

and has the same form as the native vitreous copper (*Kupferglanz*), Cu^2S*.

Although it indubitably follows from these facts and numerous others that the atomic weight assumed by Berzelius for silver must be changed, this change has scarcely been accepted by any chemist. Almost all assume, as hitherto, that oxide of silver consists of an atom of metal and an atom of oxygen, and consider that in composition it is analogous to oxide of copper and oxide of lead, and not to suboxide of copper and suboxide of mercury.

Yet, on the other hand, it cannot be denied that there are also weighty reasons for the retention of the universally adopted atomic weight of silver. The chief reason against halving the atomic weight of this metal is doubtless the fact that some compounds of silver would acquire an altogether unusual composition. For if we assume that in oxide of silver 2 atoms of the metal are combined with 1 atom of oxygen, suboxide of silver will contain no less than 4 atoms of metal for 1 atom of oxygen. This composition appears so entirely unusual, that the numerous reasons which speak for changing the atomic weight of silver have been disregarded, and the old assumption has been retained.

When such an unusual composition is assigned to a compound as the suboxide of silver, assuming that it consists of 4 atoms of metal for 1 of oxygen, this assumption will always be doubted until analogous compounds have been discovered whose composition can be explained in no other way.

Hence I have taken much trouble to prepare oxides of the same composition as suboxide of silver, and after many fruitless experiments I have been successful. I have succeeded in obtaining an oxide of copper which contains twice as much oxygen as suboxide of copper, and hence is analogous to suboxide of silver. I have hitherto not succeeded in preparing a similar oxide in the case of mercury, but I hope to do so. I shall further show that the number of such oxides is by no means small, but that their preparation will be attended with great difficulties.

Nomenclature.

Before I proceed to describe the preparation and properties of the new copper compound, it is necessary to assign a name to this class of oxides, whose chief representative is suboxide of silver.

Chemical nomenclature, more especially the German, is very imperfect. The most imperfect, however, is the designation of the different metallic oxides; it is not only inconvenient, but may give rise to much confusion. Originally we only had names for two oxides; and when several were discovered, help was

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* Poggendorff's *Annalen*, vol. xxviii. p. 427, and vol. xl. p. 313.

sought in various ways, but seldom according to right principles. The French and the English nomenclature have this advantage over the German, that the several degrees of oxidation of a metal can be designated much more readily. But all have the defect, that the oxides of the same name do not always belong to one and the same group, the members of which with the same atomic composition have similar properties. Thus, as regards their atomic composition, protoxide of iron, protoxide of tin, with oxide of copper and oxide of zinc, belong to the same group, while they differ in their nomenclature; and while oxide of iron and oxide of lead have similar designations, they have different composition and properties. This has frequently unpleasantly surprised the chemist, and a remedy for it has on many occasions been sought. Thus oxide of nickel and oxide of cobalt are now often called protoxide of nickel and protoxide of cobalt, to bring them into the same category with protoxide of iron and protoxide of manganese, to which, indeed, oxide of zinc and oxide of copper belong. But as many chemists, for good reasons, have not adopted this innovation, an unavoidable confusion arises from these various designations.

This great defect may be obviated by taking the atomic composition as basis for the designation of the various metallic oxides. Similar compositions correspond to similar properties, and in this way natural groups are formed. In this respect I take upon myself to make proposals. The names which I propose seem to avoid all the disadvantages of the previous ones, and at the same time suit all languages.

In atomic relations we can distinguish five distinct classes of salifiable oxides, of the following composition:—

(1) 4 atoms of metal with 1 atom of oxygen.
(2) 2 ,, ,, 1 ,, ,,
(3) 1 atom ,, 1 ,, ,,
(4) 2 atoms ,, 3 atoms ,,
(5) 1 atom ,, 2 ,, ,,

The last division of oxides can scarcely be considered as belonging to the salifiable ones.

If now the oxides of the different divisions are named according to the number of oxygen atoms which are combined with an atom of metal, they may have the following names:—

(1) $M+\frac{1}{4}O$, Quadrantoxide.
(2) $M+\frac{1}{2}O$, Semioxide.
(3) $M+O$, Isoxide.
(4) $M+1\frac{1}{2}O$, Sesquioxide.
(5) $M+2O$, Diploxide*.

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* The oxides of the composition $R^3 O^4$ are usually and justly regarded

The corresponding compounds of the metals with chlorine, bromine, iodine, cyanogen, and sulphur may be named in a similar manner.

I have used this nomenclature in what follows.

Quadrantoxide of Copper.

If a solution of blue vitriol is treated with isochloride of tin (protochloride of tin) to which some hydrochloric acid has been added, semichloride (protochloride) of copper is obtained. If the semichloride thus obtained is brought while still moist into a dilute solution of isochloride of tin in hydrate of potash, the white chloride is changed into a greenish body, which, however, is reduced to metallic copper if a less dilute alkaline solution be employed, and in excess

If a solution of blue vitriol be added to an excess of the very dilute alkaline tin solution, at first blue hydrated semioxide of copper is precipitated; but this rapidly assumes a yellow colour and changes into semioxide of copper. The yellow colour of the precipitate changes also on agitation, and passes into an olive-green colour. The precipitate, which at first was very voluminous, now begins to become dense and to assume a brownish-red colour. It now rapidly settles on being shaken, and consists of metallic copper, which, moistened with hydrochloric acid or with ammonia, remains almost unchanged. But on agitation, after the separation of metallic copper, the liquid does not become clear rapidly, but the part of the green body which is not yet reduced to oxide remains long suspended. It finally sinks also, and undergoes the reduction to metallic copper.

The green body thus obtained has an extraordinary tendency to become more highly oxidized. Even with a very slight access of air, it oxidizes on the surface under water into yellow semioxide of copper (suboxide of copper), and finally changes into dirty bluish isoxide of copper.

By its extremely ready oxidizability on the one hand, and, on the other, by its easy reductibility to metallic copper by even a small excess of the dilute solution of tin, the preparation of the

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as saline compounds of an isoxide and a sesquioxide. In their solutions, the sesquioxide is precipitated by weak bases, such as carbonate of baryta, while the isoxide is not precipitated. Further, the weak base (the sesquioxide) is, by the gradual addition of a strong base, an alkali for instance. precipitated first, and then afterwards the strong base, the isoxide. Moreover by heating with a solution of chloride of ammonium, the isoxide can be dissolved out of the oxides $R^3 O^4$, provided they have not been strongly heated, with disengagement of ammonia, while the sesquioxide remains undissolved. It is only the oxide of cerium, $Ce^3 O^4$, which cannot in this way be decomposed into protoxide of cerium, CeO, and a sesquioxide of cerium, $Ce^2 O^3$, which hitherto has not been isolated.

green body is made much more difficult, and fails in many cases in spite of the greatest care.

It was found as the result of a great many experiments, that, in preparing the new body, only exactly that quantity of isochloride of tin must be used which is necessary to convert the isoxide of the copper salt into quadrantoxide.

A solution of isochloride of tin in potash is prepared which contains 50 grammes of the latter in a litre, and so much isoxide of tin that 30 grammes of iodine are necessary to convert it into diploxide of tin. The heat produced by the solution of the tin-salt in the alkaline solution must be lowered by careful cooling. To a litre of the cooled solution 300 cubic centims. of a solution of blue vitriol, which contains 10 grammes of metallic copper, are added, and the mixture shaken in a flask, which is almost filled with it, about every five minutes, being placed in the meanwhile in cold water. In this case blue hydrated isoxide of copper is first precipitated, which, on continued agitation, changes into reddish yellow semioxide, which thereupon gradually changes its colour, and after some hours becomes olive-green. After twenty-four hours, the liquid above the precipitate, when supersaturated with hydrochloric acid, is no longer coloured by the addition of solution of iodine.

For the washing out, at first water is used which contains hydrate of potash, and when binoxide of tin is no longer dissolved, pure water is taken. After the removal of the hydrate of potash, the green quadrantoxide is very slowly deposited. If then ammonia is added to the wash-water, the precipitate agglomerates without undergoing any other change, and may then be rapidly washed out with pure water. The ammonia, which does not dissolve quadrantoxide of copper, dissolves traces both of semioxide and of isoxide of copper.

On account of the very ready oxidizability of the quadrantoxide, the washing must be performed with extreme care. Of course water must be used which has been freed from air by continued boiling, and has been cooled in well-closed bottles. The stopper of the bottle was replaced by a doubly perforated caoutchouc stopper, through one hole in which passed a tube bent below, and which reached nearly to the surface of the precipitate, while a short tube which ended just below the cork passed through the other. Outside the cork both were bent at right angles. Hydrogen gas was passed through the short tube, by which the liquid was removed from the flask. Thereupon both tubes, which were provided with caoutchouc tubes, were closed by pinchcocks. The hydrogen apparatus was now connected with a vessel containing boiled out water, of the same size and arranged in the same manner as that in which the precipitation of the copper was effected. After the longer tube of the water-bottle had been connected with the longer tube of the bottle which contained the precipitate, the hydrogen apparatus was connected with the shorter tube of the water-bottle. The cock of the hydrogen apparatus, and then the pinchcocks, were successively opened, beginning with the one nearest the hydrogen apparatus. The flask with the precipitate was filled in this way with boiled-out water, which was forced out of the vessel in which it was contained by hydrogen. After the wash-water had been mixed with the precipitate by repeated shaking, the precipitate was allowed to settle in order to repeat the operation. To the wash-water, boiled hydrate of potash solution was added until no more tin could be discovered in the wash-water drawn off. But this could not be completely attained even by often repeated washing. Hence when only traces of tin could be detected in the wash-water, the hydrate of potash was washed out until the precipitate no longer settled down well after washing. Ammonia was then added to the wash-water, and the washing completed.

Although the hydrogen was liberated as pure as possible, before passing into the apparatus it was led through a tube which contained pumice saturated with a concentrated solution of pyrogallate of potash. This precaution is by no means unnecessary, because air diffuses into the apparatus from the dilute sulphuric acid.

In order to obtain small quantities of the precipitate for the various experiments, the larger tube in the flask was depressed to the bottom through the precipitate, and by means of the hydrogen current some of the precipitate mixed with water was forced either into small flasks or into a flask arranged like that with the precipitate, and which was closed by the wash-water.

But notwithstanding all care, the preparation of a pure substance was not always successful, but instead of it a mixture of copper and of semioxide and isoxide of copper was frequently obtained. All the causes of failure have not been accurately ascertained; but they undoubtedly lie in the temperature and concentration of the solutions taken. But if the preparation of the compound has been successful, it may be kept under water and protected against the access of air without being in the slightest degree changed.

If the compound has been kept for some time under water with entire exclusion of air, it becomes gradually denser, and more indifferent to oxidation.

For the purpose of analysis, a mixture of the compound with water was forced, in the manner described above, into a flask which contained dilute sulphuric acid. The compound was immediately decomposed: while metallic copper was deposited.

the liquid became blue from dissolved copper. After the complete deposition of the copper, the solution was removed from the flask in the manner described above, and replaced by boiled-out water, which operation was continued until no more copper could be detected in the wash-water by means of sulphuretted hydrogen. Both the copper deposited and that in the sulphuric acid solution contained tin. The latter was mixed with ammonia until it had only a feeble acid reaction, and it was thereupon boiled, by which diploxide of tin was deposited, whose weight was determined. The copper was precipitated from the solution by sulphuretted hydrogen, the sulphide of copper ignited in a current of hydrogen and weighed. The precipitated metallic copper was dissolved in nitric acid, sulphuric acid added to the solution, and the same process repeated as in the case of the other solution.

There were obtained—

(1) From the sulphuric acid solution,

0·3636 grm. semisulphuret of copper and 0·0348 grm. diploxide of tin.

(2) From the residue of metallic copper,

1·0760 grm. semisulphuret of copper and 0·0304 grm. diploxide of tin.

If the new oxide of copper is decomposed by dilute sulphuric acid into isoxide of copper and metallic copper, the copper in the isoxide produced stands to the separated copper in the ratio of 1 : 2·959; hence, in the new oxide, 1 atom of oxygen is combined with 4 atoms (in the experiment 3·96) of copper. But the quadrantoxide investigated consisted in the dry condition of

Quadrantoxide of copper	94·93
Diploxide of tin	5·07
	100·00

The latter was, however, only mixed with the former, and might, by repeated and renewed treatment with hydrate of potash, have been removed from the quadrantoxide.

The results of a second experiment of a preparation made at another time, which had been repeatedly purified with hydrate of potash, were as follows:—0·469 grm. of semisulphuret of copper was obtained from the sulphuric solution of the quadrantoxide decomposed by sulphuric acid. It had been precipitated along with some tin, and, after weighing, it was dissolved in nitric acid, and then freed in the manner above described from tin; after this purification the semisulphuret weighed only 0·432 grm. The metallic copper separated by dilute sulphuric acid gave, after solution in nitric acid, 1·2731 grm. semisulphuret of copper, which, after purification from tin, weighed only 1·2501 grm. Both quantities of sulphuret of copper, which were obtained as semisulpurets from the quadrantoxide decomposed by dilute sulphuric acid, are in the ratio 1 : 2·9 ; and the dry quadrantoxide of copper consisted therefore of a mixture of—

Quadrantoxide of copper	96·56
Diploxide of tin	3·44
	100·00

It follows indubitably from these experiments, that there is a degree of oxidation of copper which consists of 1 atom of oxygen united with 4 of copper, which is decomposed by dilute sulphuric acid into an atom of sulphate of copper and 3 atoms of metallic copper.

If quadrantoxide of copper in its mixture with water is treated with dilute hydrochloric acid, there is formed at first a dark body (perhaps quadrantichloride of copper), but on agitation metallic copper is separated, above which is a layer of white semichloride of copper, which, on further agitation, dissolves up entirely in the excess of hydrochloric acid. Undoubtedly the quadrantoxide is decomposed by hydrochloric acid into an atom of semichloride, and into 2 atoms of metallic copper.

If sulphuretted hydrogen is added to the mixture of quadrantoxide with water, it is changed after agitation into a black body, which long remains suspended in the liquid. The black body has quite the appearance of a homogeneous body, and not that of a mixture of isosulphuret and semisulphuret with metallic copper, and is therefore a quadrantisulphuret. But if it stands for some time with an excess of sulphuretted hydrogen, it begins to decompose; a disengagement of hydrogen takes place, and the quadrantisulphuret is changed into a higher sulphuret; but into what sulphuret it passes has not been made out.

If the mixture of quadrantoxide of copper is treated with aqueous hydrocyanic acid, it is also changed into a homogeneous black body, which is quadranticyanide of copper. It readily dissolves in dilute sulphuric acid at ordinary temperatures with disengagement of red fumes, and forms a blue solution; the solution smells of hydrocyanic acid, and gives with nitrate of silver a copious precipitate of semicyanide of silver. If a solution of nitrate of silver is poured upon the quadranticyanide, a blue solution is formed, and a blackish-brown mixture of semicyanide of silver and metallic silver is formed. If this is treated with nitric acid, a blue solution of nitrate of isoxide of copper is formed, and semicyanide of silver is left.

If the moist quadrantoxide of copper is treated with ammonia, it is not dissolved, and is thus characteristically distinguished from semioxide and isoxide of copper. But if only a limited access of air is permitted, the green quadrantoxide is first covered with a yellow coating of semioxide of copper, and gradually the supernatant liquid becomes somewhat blue. Even when a mixture of ammonia and carbonate of ammonia is used, quadrantoxide of copper undergoes not the least change if air is completely excluded.

By these experiments it is shown that the compound prepared is a definite oxide of copper, of a constitution not hitherto suspected, except in the case of the quadrantoxide of silver. That the oxide of silver, which hitherto has been called suboxide of silver consists of 4 atoms of silver and 1 of oxygen, can now be as little doubted as the necessity for halving the atomic weight of silver which has previously been adopted. Quadrantoxide of copper and quadrantoxide of silver belong to the same group. The oxides also, which are generally called suboxide of copper and oxide of silver, and which are semioxides, belong to one and the same group; as do the oxides which are called binoxide of silver and oxide of copper, which are isoxides. The properties of both these oxides (of the same atomic composition) are, it is true, very different; for hitherto it has not been possible to combine isoxide of silver with acids to form salts, as in the case of isoxide of copper. On the other hand, no chemist will doubt the atomic weight of copper, or the atomic composition of the hitherto known oxides of copper, and call what is known as oxide of copper a diploxide, and the so-called suboxide of copper an isoxide.

I have stated above that the number of metallic quadrantoxides is probably by no means small; I must even assume that the group is numerous.

Bunsen has succeeded in removing by electrolysis chlorine from the chlorides of alkaline metals, especially from chloride of potassium, chloride of rubidium, and chloride of cæsium, and changing them into lower chlorides of intense smalt-blue colour, which dissolve in the fused metallic chlorides*. These chlorides can be prepared without electricity by fusing potassium with chloride of potassium in a current of hydrogen. An intense dark blue saline mass is thus obtained which dissolves in water, liberating hydrogen and forming chloride and hydrated alkali. Chloride of sodium fused with sodium in a current of hydrogen gives a bluish-grey fused mass†. It may be assumed with great

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* Poggendorff's *Annalen*, vol. cxiii. p. 344.

† The blue rock-salt which occurs in nature does not owe its blue colour to quadrantichloride of sodium, or rather potassium. The clear blue rock-salt of Stassfurth is not uniformly coloured; along with clear blue cubes there are some well-defined perfectly colourless cubes, as well as some

probability that these subchlorides contain half as much chlorine as the chlorides from which they are prepared. But the oxides and chlorides of the alkaline metals are not isoxides and isochlorides, but semioxides and semichlorides, and the atomic weights of the five known alkaline metals (as well as that of thallium) must be divided by 2. These subchlorides are then quadrantichlorides, to which quadrantoxides probably correspond; the latter will probably, however, be difficult of preparation. The group of quadrantoxides might then be called a large one[*].

LIII. *On Differential Equations and Umbilici.* By A. Cayley, *Sadlerian Professor of Pure Mathematics at Cambridge*[†].

I.

CONSIDER the integral equation

$$Az^2+2Bz+C=0,$$

where z is the constant of integration: the derived equation is

$$\Omega=(AC'+A'C-2BB')^2-4(AC-B^2)(A'C'-B'^2)$$
$$=(CA'-C'A)^2-4(AB'-A'B)(BC'-B'C)=0;$$

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which are reddish brown in colour. The blue coloured cubes consist only of chloride of sodium (with a very small quantity of sulphate of soda); the colourless and the reddish brown ones contain a great deal of chloride of potassium; the colourless ones 2 atoms chloride of potassium and 1 of chloride of sodium, hence 73 per cent. chloride of potassium.

A similar relation prevails in the case of the blue rock-salt of Kalucz in Galicia. In these also, blue coloured cubes trench on perfectly colourless ones. The latter consist of chloride of sodium only, the former of pure chloride of potassium without any mixture of chloride of sodium. At the same time there are cubes which are of a faint brownish colour, and yet contain no chloride of potassium, but consist of pure chloride of sodium.

Not in all cases do the colourless cubes, which lie close to the blue ones, contain much chloride of potassium, or consist of it, while the blue salt contains none. Bluish-coloured rock-salt of Hallstadt was found to be quite free from chloride of potassium (it sometimes contained very small quantities); but the adjacent colourless cubes consisted also of chloride of potassium. Something similar was found in the case of a feebly blue rock-salt from Wieliczka.

The bluish-coloured rock-salt, like the colourless, forms a perfectly colourless solution which has no alkaline reaction. All kinds of the blue rock-salt, even those which are tolerably intensely coloured, like that of Kalucz, give rise to no disengagement of hydrogen when dissolved.

[*] The experiments described in this paper have been made by Dr. Finkener, who has devoted himself to this research with unusual zeal. It was only by his skilful arrangement of the apparatus that it was possible to prepare the very oxidizable quadrantoxide of copper; and I confess that without his help I could not have obtained the results I have described.

[†] Communicated by the Author.

and if for greater simplicity we write $A=1$, then the derived equation is

$$\Omega=C'^2-4BC'B'+4CB'^2=0,$$

corresponding to the integral equation

$$z^2+2Bz+C=0.$$

Writing the integral equation under the form

$$(z+X)(z+Y)=0,$$

we have

$$2B=X+Y,\quad C=XY,$$

whence also

$$2B'=X'+Y',\quad C'=XY'+X'Y,$$

and the derived equation becomes

$$\Omega=-(X-Y)^2X'Y'.$$

And if we represent the roots X, Y in the form $P\pm Q\sqrt{\square}$, so that $P=-B$, $Q\sqrt{\square}=\sqrt{B^2-AC}$, Q^2 being the greatest square factor of B^2-AC, then

$$(X-Y)^2=4Q^2\square,\ X',\ Y'=P'\pm\left(Q'\sqrt{\square}+\frac{Q\square'}{2\sqrt{\square}}\right),$$

$$X'Y'=P'^2-\frac{1}{4\square}(2Q'\square+Q\square')^2;$$

and the derived equation is

$$\Omega=-Q^2\{4\square P'^2-(2Q'\square+Q\square')^2\}=0.$$

If B, C, &c. are functions of the coordinates (x, y), the equation $z^2+2Bz+C=0$ (z an arbitrary constant) represents a series of curves in the plane of xy; but if we consider z as a coordinate, then the equation represents a surface, and the curves in question are the orthogonal projections on the plane of xy of the sections of the surface by the planes parallel to the plane of xy. To fix the ideas, the plane of xy may be taken to be horizontal, and the ordinates z vertical.

Writing the equation in the form

$$(z+B)^2-(B^2-C)=0,$$

we see that the surface contains upon it the curve $z+B=0$, $B^2-C=0$, which is the line of contact with the circumscribed vertical cylinder: such curve may be termed the envelope, or, when this is necessary, the complete envelope. The equation of the surface has however been taken to be $(z-P)^2-Q^2\square=0$ (viz. it has been assumed that $B=-P$, $B^2-C=Q^2\square$); the envelope thus breaks up into the curve ($z-P=0$, $Q=0$) taken twice, and the curve $z-P=0$, $\square=0$; the former of these is in general a nodal curve on the surface, and it may be spoken of as the nodal curve; the latter of them is the reduced or proper envelope, or simply the envelope. And the terms nodal curve and envelope may also be applied to the curves $Q=0$ and $\square=0$, which are the projections on the plane of xy of the first-mentioned two curves respectively. There is however a case of higher singularity which it is proper to consider. Suppose that Q and $\square$ have a common factor K, say $Q=KR$, $\square=K\nabla$. The complete envelope $Q^2\square=R^2K^3\nabla=0$ here breaks up into the nodal curve $R=0$ twice, the cuspidal curve $K=0$ three times, and the reduced or proper envelope $\nabla=0$ once.

Reverting for a moment to the form $(z+X)(z+Y)=0$, the derived equation $\Omega=-(X-Y)^2X'Y'=0$ is satisfied by $(X-Y)^2=0$, this equation, or say the equation of the envelope, being in fact the singular solution of the differential equation. This assumes however that the differential equation is given in the form in which it is immediately obtained by derivation from the integral equation, without the rejection of factors which are functions of the coordinates (x, y) only. It is proper to consider the reduced equation obtained by rejecting such factors. Thus if X and Y are rational functions, the reduced form is $X'Y'=0$, which is no longer satisfied by the equation $(X-Y)^2=0$. In the before-mentioned case where the roots are $P\pm Q\sqrt{\square}$ (or $(X-Y)^2=Q^2\square$), P, Q, and $\square$ being rational functions of (x, y), the derived equation

$$\Omega=-Q^2\{4\square P'^2-(2Q'\square+Q\square')^2\}=0$$

divides out by the factor Q^2, but it does not divide out by $\square$; the reduced form is therefore

$$4\square P'^2-(2Q'\square+Q\square')^2=0,$$

which is not satisfied by $Q=0$, while it is still satisfied by $\square=0$ (since this gives also $\square'=0$); that is, the nodal curve $Q=0$ is not a solution of the differential equation, but we still have the singular solution $\square=0$, which corresponds to the reduced or proper envelope. In the case $Q=KR$, $\square=K\nabla$ of a cuspidal curve, the above form of the differential equation becomes

$$4K\nabla P'^2-\{3KK'R\nabla+K^2(2\nabla R'+\nabla' R)\}^2=0,$$

which divides out by K; and when reduced by the rejection of this factor, it is no longer satisfied by the equation $K=0$, which belongs to the cuspidal curve; that is, neither the nodal curve $R=0$ nor the cuspidal curve $K=0$ is a solution of the differential equation, but we still have the singular solution $\nabla=0$, which corresponds to the reduced or proper envelope. It would appear that the conclusion may be extended to singularities of a higher nature, viz. the factor corresponding to any singular curve which presents itself as part of the complete envelope divides out from the derived equation; and such singular curve does not constitute a solution of the reduced equation, but we have a singular solution corresponding to the reduced or proper envelope.

II.

Consider the differential equation

$$y(p^2-1)+2mxp=0,$$

where, to fix the ideas, $m >$ or $=1$; the integral equation may be taken to be

$$z=(mx+\sqrt{m^2x^2+y^2})(mx^2+y^2+x\sqrt{m^2x^2+y^2})^{m-1};$$

or rather, writing for shortness $\Box=m^2x^2+y^2$, and putting

$$z=(mx+\sqrt{\Box})(mx^2+y^2+x\sqrt{\Box})^{m-1}=\mathrm{P}+\mathrm{Q}\sqrt{\Box},$$

the integral equation is

$$(z-\mathrm{P})^2-\mathrm{Q}^2\Box=0, \text{ or } z^2-2\mathrm{P}z+\mathrm{P}^2-\mathrm{Q}^2\Box=0,$$

where

$$\mathrm{P}^2-\mathrm{Q}^2\Box=(m^2x^2-\Box)\{(mx^2+y^2)^2-x^2\Box\}^{m-1}$$
$$=-y^{2m}\{y^2+\overline{2m-1}x^2\}^{m-1}.$$

In the particular case $m=1$ the equation is

$$z=x+\sqrt{x^2+y^2}, \text{ or } z^2-2zx-y^2=0.$$

Before going further, I remark that, m being a positive integer greater than unity, we have

$$z=\mathrm{P}+\mathrm{Q}\sqrt{\Box}=mx(mx^2+y^2)^{m-1}$$
$$+(mx^2+y^2+\overline{m-1}mx^2)(mx^2+y^2)^{m-2}\sqrt{\Box}+\&\text{c.},$$

the subsequent terms being divisible, the rational ones by $\Box$, and the irrational ones by $\Box\sqrt{\Box}$. Hence, observing that

$$mx^2+y^2+(m-1)mx^2=m^2x^2+y^2=\Box,$$

we see that Q contains the factor $\Box$, and the equation $\Box=0$ belongs to a cuspidal curve on the surface. If however $m=1$, then the equation is $z=x+\sqrt{\Box}$, so that $\mathrm{Q}=1$ does not contain the factor $\Box$; and $\Box=x^2+y^2=0$ is not a singular curve on the surface, but is in fact the reduced or proper envelope.

The curve represented by the integral equation will pass through the origin ($x=0$, $y=0$) for the value $z=0$ of the constant of integration. In fact, for this value, the integral equation becomes

$$-y^{2m}\{y^2+(2m-1)x^2\}^{m-1}=0,$$

which belongs to a set of $2m+(m-1)+(m-1)$ lines coinciding with the lines $y=0$, $y=ix\sqrt{2m-1}$, and $y=-ix\sqrt{2m-1}$ respectively. The directions at the origin are therefore $p=0$, $p=\pm i\sqrt{2m-1}$, which are the same values of p as are obtained from the differential equation; viz. since this is satisfied identically at the point in question, proceeding to the derived equation, we have

$$p(p^2-1)+2mp=0;$$

that is,

$$p(p^2+2m-1)=0.$$

But it is to be observed that these values of p are different from the values given by the equation $\square=m^2x^2+y^2=0$, which are $p=\pm im$. The reason is that the curve $\square=0$ being, as was shown, a cuspidal curve on the surface, the equation $\square=0$ is not a solution of the differential equation.

If however $m=1$, then the integral equation gives at the origin no longer three values of p, but only the value $p=0$. The differential equation however gives, as in the general case, three values; viz. we have $p(p^2+1)=0$. And the values $p=\pm i$ obtained from the factor $p^2+1=0$ are precisely the values of p obtained from the equation $\square=x^2+y^2=0$, which in the case now under consideration belongs to the reduced or proper envelope of the surface, and *is* therefore the singular solution of the differential equation.

III.

The two curves of curvature which pass through any given point of a surface are distinct curves, not branches of one indecomposable curve. In fact if P, Q are the two curves of curvature for a point A, then for a point A′ on P the two curves of curvature will be P, Q′; and if P, Q were branches of an indecomposable curve, then P, Q′ would also be branches of an indecomposable curve, and we should have P a branch of two different indecomposable curves, which is of course impossible. In the case of an umbilicus, the two curves P and Q coincide together; or, as we may express it, the curves of curvature through an umbilicus are the duplication of a single, in general indecomposable curve; and in general this curve has at the umbilicus a *trifid* node. I use this expression to denote a point at which there are three distinct tangents, or, more accurately, three distinct directions of the curve: an ordinary triple point is of necessity a trifid node, but not conversely. The umbilicus of an ellipsoid or other quadric surface is a peculiar exceptional case.

In support of the foregoing conclusions, consider a surface having an umbilicus at the origin, and take $z=0$ as the equation of the tangent plane at that point; the equation of the surface in the neighbourhood of the umbilicus will be

$$z=\tfrac{1}{2}k(x^2+y^2)+\tfrac{1}{6}(ax^3+3bx^2y+3cxy^2+dy^3);$$

so that, writing as usual p and q for the first, and r, s, t for the second differential coefficients of z, we have

$$\begin{aligned} p&=kx+\tfrac{1}{2}(ax^2+2bxy+cy^2),\\ q&=ky+\tfrac{1}{2}(bx^2+2cxy+dy^2),\end{aligned}$$

$$\begin{aligned} r&=k+ax+by,\\ s&=\phantom{k+{}}bx+cy,\\ t&=k+cx+dy.\end{aligned}$$

The differential equation of the curves of curvature projected on the plane of xy is

$$\left(\frac{dy}{dx}\right)^2[(1+q^2)s-pqt]+\frac{dy}{dx}[(1+q^2)r-(1+p^2)t]-[(1+p^2)s-pqr]=$$

and substituting therein the foregoing values of p, q, r, s, t, but attending only to the terms of the lowest order in (x, y), and using moreover in the sequel p in the place of $\frac{dy}{dx}$, the equation becomes

$$(bx+cy)(p^2-1)+[(a-c)x+(b-d)y]p=0;$$

which may be taken as the differential equation of the curves of curvature at and in the neighbourhood of the umbilicus. The equation is satisfied identically by the values $x=0$, $y=0$, which correspond to the umbilicus; and to find p, we have to differentiate the equation, and then substitute these values of x and y; we thus obtain

$$(b+cp)(p^2-1)+[(a-c)+(b-d)p]p=0,$$

or, what is the same thing,

$$p(a+2bp+cp^2)-(b+2cp+dp^2)=0,$$

a cubic equation for the determination of p.

I remark that we may without loss of generality write $d=0$; but to simplify the investigation, I suppose in the first instance that we have also $b=0$. This comes to assuming that one of the three planes $ax^3+3bx^2y+3cxy^2+dy^3=0$ bisects the angle formed by the other two planes. The differential equation consequently is

$$cy(p^2-1)+(a-c)xp=0;$$

or, putting for shortness

$$\frac{a-c}{c}=-2m,$$

it is

$$y(p^2-1)+2mxp=0,$$

which is the differential equation previously considered. Hence, writing now h in the place of z, the equation of the curve of curvature in the neighbourhood of the umbilicus is

$$h=(mx+\sqrt{\square})(mx^2+y^2+\sqrt{\square})^{m-1}=\text{P}+\text{Q}\sqrt{\square},$$

where $\square=m^2x^2+y^2$; or, what is the same thing, the equation is

$$h^2-2\text{P}h+\text{P}^2-\text{Q}^2\square=0;$$

and the equation (in the neighbourhood of the umbilicus) of the curve through the umbilicus is

$$\text{P}^2-\text{Q}^2\square=-y^{2m}\{y^2+(2m-1)x^2\}^{m-1}=0;$$

so that the umbilicus is a trifid node. In the case however of an ellipsoid or other quadric surface, we have $m=1$, so that the equation of the curve of curvature in the neighbourhood of the umbilicus is

$$h=+x\sqrt{x^2+y^2},$$

or, what is the same thing,

$$h^2-2hx-y^2=0:$$

and for the curve through the umbilicus, in the neighbourhood of the umbilicus, the equation is $y^2=0$, so that there is only a single direction of the curve of curvature. The differential equation gives, however, at the umbilicus $p(p^2+1)=0$; the value $p=0$ is that which corresponds to the curve of curvature; the other two values $p=\pm i$ correspond to the curve (pair of lines) $x^2+y^2=0$, which is the envelope of the curves of curvature, or, more accurately, the envelope of the projections of the curves of curvature on the tangent plane at the umbilicus.

Blackheath, October 17, 1863.

LIV. *Remarks on Chemical Notation.*
By William Odling, *M.B., F.R.S.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

In your last Number, Mr. J. J. Waterston brings forward some interesting views on chemical notation, which he considers antagonistic to the notions propounded by me in a semipopular lecture upon an entirely different subject, which I delivered a little while back at the Royal Institution, and upon which he accordingly goes out of his way to make a not very courteous onslaught, affirming that chemists in general, and Mr. Graham and myself in particular, are not so well acquainted as we ought to be with the relations subsisting between atoms, molecules, volumes, and formulæ.

Wherefore, in order to set us right, he tells us with exquisite gravity what it is that the dynamical theory proclaims as a fact—namely, that "*all* molecules, simple or compound, in the gaseous state, occupy the same volume,"—intelligence scarcely less startling than he might have imparted by saying that, according to a fundamental rule in arithmetic, the sum of two and two is *always* four.

Again, in the first paragraph of his communication, he charges me with having contended that the molecular weight of water must be represented by the number 18, and "cannot be 9, as might be inferred from the specific gravity of the vapour, &c." Now I certainly never before heard that the vapour-density of any given body pointed to some particular number as the expression of its molecular weight, but looked upon it as merely showing the comparative molecular weight of that body in relation to the molecular weight of some other body chosen as a standard; and taking the molecular weight of hydrogen as a standard, the molecular weight of water will, I conceive, be 9, or 18, or 112·5, according as we take 1 (Dalton), or 2 (Laurent and Gerhardt), or 12·5 (Berzelius) for the molecular weight of hydrogen. My argument, by which I still abide, was simply this: if the molecular weight of muriatic acid is 36·5, then the molecular weight of water is 18, and not 9.

In illustration of what I believe to be the generally received opinion among modern chemists as to the correlation of volume and formula—an opinion very different from that attributed to them by Mr. Waterston—I have written down in column I. the ordinarily recognized molecular formulæ of a few well-known bodies. Each of these formulæ represents the same bulk of gas, whether that bulk be considered as 1, 2, 3, 4, or 100 volumes.

Col. I.		Col. II.
H^2	Hydrogen	H
Cl^2	Chlorine	Cl
O^2	Oxygen	O
$H\,Cl$	Muriatic acid	$H^{\frac{1}{2}}\,Cl^{\frac{1}{2}}$
$H^2\,O$	Water	$HO^{\frac{1}{2}}$
$H^3\,N$	Ammonia	$H^{1\frac{1}{2}}\,N^{\frac{1}{2}}$
$H^4\,C$	Marsh-gas	$H^2\,C^{\frac{1}{2}}$
$H^6\,C^2\,O$	Alcohol	$H^3\,CO^{\frac{1}{2}}$
$H^{10}\,C^4\,O$	Ether	$H^5\,C^2\,O^{\frac{1}{2}}$
$H\,NO^3$	Nitric acid	$H^{\frac{1}{2}}\,N^{\frac{1}{2}}\,O^{1\frac{1}{2}}$

Now, if all the above formulæ be halved, as Mr. Waterston suggests, but not newly suggests, or if they be doubled, or trebled, or quadrupled, they will still all of them represent one and the same bulk of gas, which may still be considered as 1, 2, 3, 4, or 100 volumes.

At present a large section of chemists, more especially of foreign chemists, who use the comparable formulæ given in column I., select a quadruple volume for their molecular unit, while a majority, I believe, prefer for various reasons to look upon the bulk expressed by each of the above formulæ as corresponding to two volumes, or they take a double volume for their molecular unit; but both parties are perfectly aware that these are mere matters of convention, and that each formula might equally well be considered to represent one volume only.

For myself, however, I fully admit that the relative advantages of a 1-volume and a 2-volume unit, as discussed by Mr. Waterston, and the best mode of constructing a 1-volume notation, whether by integral or fractional formulæ, are topics which, though not by any means novel, are well worthy of serious consideration on their own bases—that is to say, quite irrespective of my lecture, which had nothing whatever to do with them.

But Mr. Waterston's impression, that what are known as Gerhardt's formulæ must necessarily be halved, and consequently fractional, as shown in column II., in order to correspond respectively with one gaseous volume, is altogether opposed to the long-established general understanding, and, I would add, though I have no wish to discuss the point, is not, in my opinion, well founded.

Thus on page 83 of Laurent's posthumous *Méthode de Chimie*, published so long ago as 1854, and, I think, in some of his earlier memoirs, it is distinctly stated that Gerhardt's formulæ may be taken to represent one volume only. He says, "La notation par

1 volume serait donc exactement la même que la notation par 2 volumes."

At any rate, when looked upon in this manner, not as "actually representing *two* gaseous molecules," but as corresponding each to a single volume, Gerhardt's formulæ given in column I. will, I should think, even in Mr. Waterston's opinion, be quite in accordance with the view which regards "the ultimate moving part of all the primary gases as consisting of *at least* two chemical atoms, &c.," and will also fully recognize that "splitting of the gaseous molecule of oxygen into two equal parts when combining with hydrogen to form water" which, thanks to the chemical researches of Brodie and Laurent, is now an old story*.

The object of my lecture at the Royal Institution, which has had the misfortune to fall under Mr. Waterston's censures, was not to advocate a 2-volume notation as opposed to a 1-volume notation—indeed my personal preference is inclined the other way—but to show on chemical grounds that the chemical molecule of water, if I may so say, is identical with its physical molecule, and consequently that this molecule† contains, not, as was formerly held, the same amount of hydrogen, but double the amount of hydrogen contained in the molecule of muriatic acid. Or, in other words, that the molecular weights of water, muriatic acid, and hydrogen are to one another as 18 : 36·5 : 2, or as

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[*] Brodie, in publishing some further experiments on the subject, has recently given the following *résumé* of his conclusion originally announced in 1850. "On these ideas, as we regard the weight of two volumes of oxygen, that is to say the weight of a molecule of oxygen, O^2, as differing from the weight of two volumes, that is, the weight of a molecule of water, H^2O, in the fact that this weight contains sixteen parts of oxygen in the place of two of hydrogen, so do we regard the event of the synthesis or diæresis of oxygen as differing from the event of the synthesis or diæresis of water, in the fact that in the one change the two atoms of oxygen fulfil the same functions, and are respectively in the same polar conditions as the two atoms of hydrogen and the one atom of oxygen in the other. This theory is of a purely relative character; it is connected with no special hypothesis as to the nature of oxygen or water, but it states that, if we make a certain assertion as to the molecular nature of water, we must, in consistency, make certain parallel assertions as to the molecular nature of oxygen. Our molecular hypotheses may change, but this relation will still remain." (Phil. Trans. 1862, p. 837.) Laurent's research had reference chiefly to the dual character of the chlorine molecule, Cl^2.

[†] In accordance with the views of the school of chemists to which I belong, the chemical and physical molecules of all bodies should be identical; but, owing doubtless to defective knowledge, some exceptions must for the present be admitted. Thus there is no known chemical molecule of sulphur corresponding to the physical molecule of sulphur-vapour at 500°, which is ninety-six times as heavy as that of hydrogen; and, on the other hand, there is no known physical molecule of phosphorus-vapour corresponding to the ordinary chemical molecule of phosphorus, which is thirty-one times as heavy as that of hydrogen.

9 : 18·25 : 1—a conclusion for which I certainly did not expect to be reproached by a thermo-molecular physicist.

Now the determination of this point depends almost entirely upon a consideration of the molecular relations subsisting between water, hydrate of sodium, and oxide of sodium, and between water, alcohol, and ether (for I have yet to learn when "the chemical evidence with respect to the formation of ether and decomposition of alcohol" ceased to be "deemed of importance"); that is to say, upon the question whether the molecules of hydrate of sodium and hydrate of ethyle contain double the amount of oxygen, or the same amount of oxygen as the molecule of water; in which last case the three molecules will be represented by integral formulæ thus, H^2O, $HNaO$, and $H(C^2H^5)O$; or by fractional formulæ thus, $HO^{\frac{1}{2}}$, $H^{\frac{1}{2}}Na^{\frac{1}{2}}O^{\frac{1}{2}}$, and $H^{\frac{1}{2}}(CH^{2\frac{1}{2}})O^{\frac{1}{2}}$*.

But this is a question which it is obviously impossible to argue with a gentleman who, in opposition to the interpretation which has gradually forced itself upon the reluctant minds of a majority of the most distinguished chemists of all nations, disposes of the whole matter offhand by asserting *ex cathedrâ*, "Now the fact is, that two molecules of water are engaged in forming one molecule of the hydrate"†.

It would at the same time be gratifying to learn how and when this very decisive fact was ascertained, and by whom. Probably by some favoured "mathematical student of thermo-molecular physics," from whose transcendental faculties nothing chemical is concealed, rather than by a mere plodding chemist whose most inspired notions are but of the earth, earthy.

In conclusion, I would suggest to Mr. Waterston that when he again condescends to direct, with no uncertain voice, poor benighted chemists into the right path, he should first make

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* The volume of a chemical equivalent of sodium $\left(\frac{Na}{23}=\frac{H}{I}\right)$ has not been ascertained, but from analogy is believed to be the same as that of hydrogen and ethyle; whence we have Gerhardt's molecular formulæ for the sodic derivatives of hydrogen, muriatic acid, water, ammonia, marsh-gas, alcohol, and nitric acid, namely Na^2, $NaCl$, $NaHO$, NaH^2N, NaH^3C, NaH^5C^2O, and $NaNO^3$ respectively; or in fractional formulæ, Na, $Na^{\frac{1}{2}}Cl^{\frac{1}{2}}$, $Na^{\frac{1}{2}}H^{\frac{1}{2}}O^{\frac{1}{2}}$, $Na^{\frac{1}{2}}HN^{\frac{1}{2}}$, $Na^{\frac{1}{2}}H^{1\frac{1}{2}}C^{\frac{1}{2}}$, $Na^{\frac{1}{2}}H^{2\frac{1}{2}}CO^{\frac{1}{2}}$, and $Na^{\frac{1}{2}}N^{\frac{1}{2}}O^{1\frac{1}{2}}$ respectively. *Vide* columns I. and II.

† In criticising a sentence from the Supplement to Mr. Graham's 'Elements of Chemistry,' Mr. Waterston writes, "In the above extract HKΘ represents one molecule of the hydrate of potash, but HHΘ represents two molecules of water, and KKΘ two molecules of anhydrous potash." Be this as it may, I am permitted by Mr. Watts, the author of the Supplement, to state that the three formulæ were meant to express a single molecule each, the three quantities represented being exactly comparable.

himself better acquainted with the path they are of themselves pursuing, and with the progress they have made therein, when he may possibly discover that, though less far advanced than is perhaps desirable, they are nevertheless not quite so ignorant of thermo-molecular physics as he now seems to imagine.

I am, Gentlemen,
Your obedient Servant,
WILLIAM ODLING.

P.S.—In your review of Mr. Watts's 'Dictionary of Chemistry,' after praising my article on *atomic weights*, for which accept my best thanks, you say that you do not agree with my conclusion that Gerhardt's scale of atomic weights affords a satisfactory representation of the present state of knowledge, but that, on the contrary, a consistent application of the spirit of Gerhardt's reforms would lead us now to go much further than he did. My conclusion certainly was, when writing the article in question, that Gerhardt's system, with a slight extension, afforded a tolerably satisfactory representation of the *then* state of knowledge. But I quite agree with you that, in consequence of researches made more particularly within the last two or three years, a much further extension in the direction advocated by Messrs. Cannizzaro and Wurtz is *now* required, which extension I am to have an opportunity of discussing in some future Number of the Dictionary.—W. O.

LV. *Note on* Laplace's *Correction for the Velocity of Sound.*

By JOHN TYNDALL, *F.R.S.*[*]

NEWTON calculated the theoretic velocity of sound in air, and found that it differed from the observed velocity by about one-sixth of the whole amount. Laplace accounted for this discrepancy by showing that over and above the changes of elasticity due to changes of density, of which alone Newton took account, the effective elasticity of the medium is augmented by the changes of temperature which occur in the condensations and rarefactions of the sonorous wave. He succeeded in proving that, to obtain the true velocity of sound in air, Newton's result would have to be multiplied by the square root of the quotient obtained by dividing the specific heat of air at constant pressure by its specific heat at constant volume. This ratio was not at the command of Laplace; but in the absence of experiment, and knowing that the velocity of sound in air had been determined with extreme accuracy, he reversed the process, and deduced from

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[*] Communicated by the Author.

the velocity of sound the ratio of the two specific heats. It was found to be 1·421.

We now know that the excess of this number above unity, namely 0·421, expresses the amount of heat consumed in external work when the air is allowed to expand under constant pressure; and from this number we can at once deduce the mechanical equivalent of heat. The almost absolute identity of the equivalent thus found, with that established by Mr. Joule by direct experiment, leaves no shadow of doubt upon the mind as to the correctness of Laplace's result.

Still, notwithstanding this striking verification, and the greatness of the names connected with this question, I have for some years thought it probable that the legitimacy of Laplace's proceeding in calculating the ratio of the two specific heats depends upon an accident. At the time he wrote, and for a long time subsequently, it was universally believed that elastic fluids possessed no sensible radiative power; and the practical absence of this power is virtually assumed in his calculation. He supposed that the heat and the cold produced their full effect in changing the elasticity of the air; that there was no loss of heat either by radiation or conduction; and he would at the time have extended the same assumption to elastic fluids generally, and deduced from any one of them the ratio of the two specific heats.

The point to which I now wish to direct attention is whether, in the present state of our knowledge, this extension of the assumption is warranted; and whether the correctness of Laplace's result may not depend upon a peculiarity of air unproved in his day, but which distinguishes it from most other elastic fluids.

My experiments taught me some years ago, and every additional day's experience only confirms the result, that air possesses no sensible power of absorption or radiation; hence a condensation in air, in presence of its associated rarefaction, has no sensible power to neutralize, by radiation, the differences of temperature in the condensed and dilated portions of the sonorous wave. But while air is thus neutral, other gases, ammonia and olefiant gas for example, show themselves competent to absorb 80 or 90 per cent. of the entire radiation from an obscure source; and they possess a radiative power proportionate to their enormous absorbing power. Let us imagine then a series of sonorous waves propagated through an atmosphere of olefiant gas; every heated condensation is a powerful radiator, and every chilled dilatation is a powerful absorber. These two portions of a wave cannot, I think, subsist for an instant in the presence of each other without loss of heat on the one hand, and gain on the other. The tendency certainly is to diminish that portion of the elastic force on which the correction of Laplace depends; and hence the velocity of sound in such a medium ought to approximate more to that deduced from the formula of Newton than its velocity in air. It is hard to conceive that this effect could be prevented by the rapidity of the vibrations. From the moment any molecule receives, by the act of condensation, an accession to its calorific motion, that motion is in part wasted upon the æther in which the molecule swings. No conceivable rapidity of sonorous vibration can, I think, prevent this loss from taking place; and if it take place in any sensible degree, the correction of Laplace would be no longer applicable, nor could a correct ratio be deduced from the velocity of sound in the medium.

The necessary data exist for the determination of the theoretic velocity of sound in olefiant and other gases, and from its pitch in organ-pipes its actual velocity in any gas may be deduced. We are thus in a position to compare the actual and theoretical velocities, and to deduce from them the ratio of the two specific heats. If then the radiative power of the gas comes sensibly into play, it will diminish the actual velocity, and therefore make the ratio of the two specific heats appear less than in the case of air. Conversely, if the ratio for the gas be less than that found for air, it would show that a partial equalization of temperature between the condensed and rarefied portions of the sonorous wave had been effected by the radiative power of the gas.

In the state indicated by this reasoning, the question rested in my mind for a considerable time; and it is not entirely without diffidence that I now bring it forward. I had thought of an apparatus with a view of testing the question, forgetting that the ratio of the two specific heats had been determined in this very way by a celebrated experimenter. Dulong, it is well known, pursued this very method in the case of seven gases, three of which were elementary, and four compound*. For the elementary gases he found a ratio which was sensibly the same as that found for air; this, according to my reasoning, would prove the absence of radiative power on the part of the elementary gases,—a result in exact accordance with my experiments. For the compound gases he found ratios sensibly less than that found for air, and least of all for olefiant gas, which, according to my experiments, is the greatest radiator. In the case of this gas the excess of the ratio above unity is little more than half what it is in the case of air. Dulong of course drew from his experiments conclusions very different from those to which my reasoning points, and which, I submit, need reconsideration. In this state for the present I leave the question to the appreciation of philosophers. Apart

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* *Ann. de Chim. et de Phys.* vol. xli.; and Pogg. *Ann.* vol. xvi.

from other important consequences, it would certainly be an interesting and beautiful result if, from a thing apparently so remote as the velocity of sound, we could deduce *à priori* the fact which has been now illustrated by nearly five years' experiments—that the elementary gases have no sensible radiative power, while the compound gases are endowed with this power in very different degrees.

Royal Institution,
October 24, 1863.

LVI. *On the Application of Barycentric Perspective to the Transformation of Structures. By* W. J. Macquorn Rankine, *C.E., LL.D., F.R.SS.L. & E.**

1. THIS paper contains the substance of some remarks which I made at the recent Meeting of the British Association, on the elegant investigation by Professor Sylvester of the principles of Barycentric Perspective and Homalographic Projection.

2. In the 'Proceedings of the Royal Society' for the 6th of March 1856, I published a theorem called that of "the Transformation of Structures," which may be briefly expressed as follows:—

If a structure of a given figure be balanced and stable under forces represented by given lines, then will any structure whose figure is a parallel projection of the original figure be balanced and stable under forces represented by the corresponding projections of the lines representing the original forces.

3. By a parallel projection of a figure is meant a figure derived from the original figure by altering the coordinates in uniform proportions, or by substituting oblique for rectangular coordinates; and it is called *parallel* because to every pair of equal and parallel lines in the original figure there correspond a pair of equal and parallel lines in the transformed figure. For example, every orthographic projection of a plane figure is a parallel projection; all ellipsoids are parallel projections of each other and of a sphere, &c.

4. That theorem was applied in 'A Manual of Applied Mechanics' to the deduction of the figures of a skew arch and of a ramping arch from that of a common arch, of an equilibrated rib from a common catenary, of arches for supporting earth from arches for supporting the pressure of a liquid, &c.

5. Its applications, however, were limited by the condition of *parallel projection*; and there were consequently many conceivable transformations of structures to which it could not be applied.

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* Communicated by the Author.

6. The theorems discovered by Mr. Sylvester now afford the means of greatly extending the art of designing structures by transformation from structures of more simple figures; for they obviously give at once the solution of the question—*given the figure of a structure which is balanced and stable under a load distributed in a given way; given also any perspective or homalographic projection of that figure; to find how the load must be distributed on the transformed structure in order that it also may be balanced and stable.*

7. This is not the first instance in which theorems of pure science have proved to be capable of practical applications unexpected, perhaps, by their discoverers.

LVII. *On the Expansive Energy of Heated Water.*

By W. J. Macquorn Rankine, *C.E., LL.D., F.R.SS.L. & E.*[*]

As the question of the quantity of mechanical energy which a given weight of water, heated to a given temperature, is capable of exerting in the act of partially evaporating without receiving heat until it falls to a given lower temperature, has been raised in connexion with recent researches as to the bursting of steam-boilers, I may point out that the complete solution of that question for any given liquid, together with a numerical example in the case of water, is given under the head of Proposition XVII. of a paper on Thermo-dynamics, which was communicated by me to the Royal Society in December 1853, read in January 1854, and published in the Philosophical Transactions for 1854.

That solution is expressed by the following formula (page 161, equation 65):

Energy exerted by each pound of fluid

$$=\mathrm{K}\left\{t_1-t_2\left(1+\text{hyp.}\log\frac{t_1}{t_2}\right)\right\}; \quad . \quad . \quad . \quad (1)$$

in which K denotes the dynamical value of the specific heat of the liquid, being the product of its specific heat expressed in the ordinary way by "Joule's Equivalent;"

t_1 and t_2 the initial and final absolute temperatures,—the absolute zero being 274° C., or 493°·2 F. below the melting-point of ice[†].

Another equation (equation 63 of the paper) gives the follow-

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[*] Read to the Institution of Engineers in Scotland. Communicated by the Author.

[†] In the original paper the absolute zero of heat was assumed to be $272\frac{1}{2}$° C. below the melting-point of ice. The value now adopted, 274° C., is deduced from later experimental data.

ing value for the excess of the final volume to which the mixed liquid and vapour expand above the original volume of the liquid:

$$\frac{\mathrm{K\ hyp.\ log}\frac{t_1}{t_2}}{\frac{dp_2}{dt_2}}; \quad \ldots\ldots \quad (2)$$

in which $\frac{dp_2}{dt_2}$ denotes the rate at which the pressure, in lbs. on the square foot, varies with temperature at the final temperature.

When applied to the water in a steam-boiler, these equations take the following form:—

The value of K for liquid water is—

772 foot-pounds per degree of Fahrenheit in a pound of water, or

1389·6 foot-pounds per Centigrade degree in a pound of water, or

423·55 kilogrammetres per Centigrade degree in a kilogramme of water.

The final absolute temperature is $212°\ \mathrm{F.} + 461°{\cdot}2 = 673°{\cdot}2\ \mathrm{F.}$

The corresponding value of $\frac{dp}{dt}$, for Fahrenheit's scale and British measures, is 42; and $772 \div 42 = 18{\cdot}38$.

Let T denote the initial temperature on Fahrenheit's ordinary scale, so that $t_1 = \mathrm{T} + 461°{\cdot}2$. Then

Energy, in foot-pounds, exerted by each pound of water

$$= 772\left\{\mathrm{T} - 212° - 673°{\cdot}2\ \mathrm{hyp.\ log}\left(\frac{\mathrm{T} + 461°{\cdot}2}{673°{\cdot}2}\right)\right\}^{*}; \quad (3)$$

Final volume of expansion of mixed water and steam, in cubic feet per pound

$$= 18{\cdot}38\ \mathrm{hyp.\ log}\left(\frac{\mathrm{T} + 461°{\cdot}2}{673°{\cdot}2}\right). \quad \ldots \quad (4)$$

It is worthy of remark, *that the energy developed depends solely on the specific heat of the substance in the liquid state, and the initial and final temperatures,* and not on any other physical property of the substance.

The following Table gives some results of the formulæ.

The first column contains the temperature on the ordinary scale of Fahrenheit, with intervals of 36° F. = 20° C.

The second column contains the expansive energy of one pound of water, in foot-pounds.

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* This agrees with the formula given by J. B. in 'The Engineer' of the 2nd of October 1863, page 200.

The third column contains the velocity, in feet per second, which that energy would impress on a projectile of the weight of the water itself, that is, one pound.

The fourth column, the final volume of expansion of the water and steam, in cubic feet per pound.

For convenience a fifth column is added, containing the initial absolute or total pressures in pounds on the square inch.

The last line of the Table has reference to the case in which the water would be totally evaporated.

TABLE I.

Initial temperature. Fahrenheit.	Energy. Foot-pounds.	Velocity. Feet per second.	Final expansion. Cubic feet.	Initial absolute pressure. Pounds per square inch.
212°	0	0	0	14·70
248	726	214	0·95	28·83
284	2779	423	1·87	52·52
320	6052	624	2·73	89·86
356	10422	819	3·56	145·8
392	15826	1010	4·36	225·9
428	22156	1194	5·11	336·3
...	......	......	......	......
about 2360	about 912500	about 7666	26·36	unknown

In the absence of logarithmic Tables, the following approximate formulæ may be used for temperatures not exceeding 428°:

$$\text{Energy, nearly} = \frac{772\,(\mathrm{T}-212^\circ)^2}{\mathrm{T}+1134^\circ{\cdot}4}; \quad . \quad . \quad . \quad (5)$$

$$\text{Expansion, nearly} = \frac{36{\cdot}76\,(\mathrm{T}-212^\circ)}{\mathrm{T}+1134^\circ{\cdot}4}. \quad . \quad . \quad . \quad (6)$$

Glasgow, October 5, 1863.

Note added October 19, 1863.

In explanation of the formulæ and Tables, it may be added that the mechanical energy in column 2 is the equivalent of the heat which disappears during the process, being the difference between the whole heat expended and the latent heat of that portion of the water which at the end of the process is in the condition of steam at atmospheric pressure.

For the information of those who consider that the liquid portion of the water, owing to its small compressibility, acts like a volley of hard projectiles, a Table is added, showing, for each of the initial temperatures in the previous Table, what fraction of a pound of water continues in the liquid state, and how much of the energy developed is possessed by that liquid water.

Table II.

Initial temperature. Fahrenheit.	Proportion of the water which remains liquid. Pound.	Energy possessed by that liquid water. Foot-pounds.
212°	1·000	0
248	0·964	700
284	0·931	2587
320	0·897	5429
356	0·865	9015
392	0·835	13215
428	0·806	17858
...	......	......
about 2360	0	0

In the formulæ and Tables, it has not been considered necessary to take into account the small increase which the specific heat of water undergoes as the temperature rises.—W. J. M. R.

LVIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 321.]

March 5, 1863.—Major-General Sabine, President, in the Chair.

THE following communication was read:—

"On the Change of Form assumed by Wrought Iron and other Metals when Heated and then Cooled by partial Immersion in Water." By Lieut.-Col. H. Clerk, R.A., F.R.S.

Origin of the Experiments.—A short time ago, when about to shoe a wheel with a hoop-tire, to which it was necessary to give a bevel of about $\frac{3}{8}$ths of an inch, one of the workmen employed suggested that the bevel could be given by heating the tire red-hot and then immersing it one-half its depth in cold water. This was tried, and found to answer perfectly, that portion of the tire which was out of the water being reduced in diameter. The tire was 3 inches wide, $\frac{1}{2}$ inch thick, and 4′ 2″ in diameter.

As this result was curious and not generally known, I considered it desirable to institute some further experiments in order to try how far, by successive heatings and coolings, this change of form could be augmented, and also whether the same effect could be produced on other metals than wrought iron.

Mode of carrying out the Experiments.—The experiments were made on cylinders of wrought iron of different dimensions, both hollow and solid; immersed, some to one-half of their depth, others to two-thirds; also on similar cylinders of cast iron, steel, zinc, tin, and gun-metal.

The specimens experimented on were all accurately turned in a lathe to the required dimensions, which were carefully noted; they were then heated to a red heat in a wood-furnace used for heating the tires of wheels. As soon as they had acquired the proper heat, they were taken out and immersed in water to one-half or two-thirds of their depth (as stated in the experiment). The temperature of the water ranged from 60° to 70° Fahr.

The specimens were allowed to remain in the water about two minutes, in which time the portion in the air had lost all redness, and that in the water had become sufficiently cool to handle. These alternate heatings and coolings were repeated till the metal showed signs of cracking or giving way.

The dimensions were noted after every five heatings. The circumferences were measured in preference to the diameters, as the true circular form was liable to alter.

General Results.—It will be seen by an inspection of the figures that the general effect is a maximum contraction of the metal about one inch above the water-line; and that this is the same whether the metal be immersed one-half or two-thirds of its depth, or whether it be nine, six, or three inches deep. With wrought iron the heatings and coolings could be repeated from fifteen to twenty times before the metal showed any signs of separation; but with cast iron after the fifth heating the metal was cracked, and the hollow cylinder separated all round just below the water-line after the second heating. Cast steel stood twenty heatings, but was very much cracked all over its surface. As respects the change of form of cast iron and steel, the result was similar to that in wrought iron, but not nearly so large in amount. The cast iron did not return to its original dimensions, but the smallest diameter was about one inch above the water-line.

Tin showed no change of form, there being apparently no intermediate state between the melting-point and absolute solidity. Brass, gun-metal, and zinc showed the effect slightly; but instead of a contraction just above the water-line, there was an expansion or bulging.

The effect on wrought iron is best seen in the solid cylinder (figs. 9 and 10), where the displacement of particles just above the water-line appears to be compensated by the bulgings at the two extremities.

The specimens of wrought iron were submitted by Mr. Abel (Chemist to the War Department) to chemical analysis, and he informs me that he found nothing noteworthy in the composition of the metal; nor was there any appreciable difference in the specific gravity of the metal taken from different parts of the specimen. It appears therefore to be simply a movement of the particles whilst the metal is in a soft or semifluid state.

The following is an account of the experiments, which were carried out under the superintendence of Mr. Butter, Draughtsman of the Royal Carriage Department, to whom also I am indebted for the accompanying diagrams. The exact dimensions of each specimen before and after heating are given in a tabulated form at the end of the paper, to facilitate comparison.

In figs. 22 and 23 the changes in form of the 9″ cylinders (one immersed one-half, the other two-thirds its depth) are shown in section after every five heatings (half the full size).

Experiment. 1—A 4 ft. 2 in. hoop-tire of 3 inches breadth and $\frac{3}{8}$ths inch in thickness (fig. 1) was heated and cooled by being immersed to half its depth in cold water five times, by which the effect shown in fig. 2 was produced.

Fig. 1.

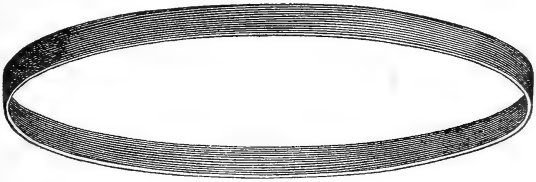
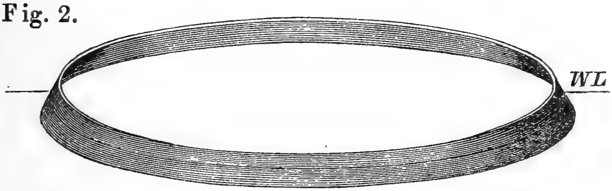


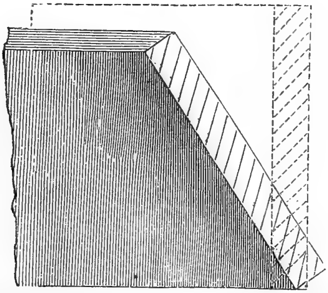
Fig. 2.



One-eighteenth of full size.

The upper edge, or that cooled in air, had contracted 8 inches, or $\frac{1}{20}$th its entire length, and slightly increased in thickness; while the lower edge, cooled in water, had expanded ·875 inch, making a difference between the two circumferences of 8·875 inches. The breadth remained unaltered (3 inches), and kept perfectly straight.

Fig. 3.



Section showing the amount of contraction. One-half the full size. The dotted lines show the original form.

The quality of the iron was afterwards tested by pieces taken from the upper and lower edges, and also from the centre; the fibrous condition had remained unchanged, the specific gravity had not altered appreciably, and there appeared to be no deterioration in any part of it.

Experiment 2.—Two hollow cylinders of wrought iron, 12 inches diameter and $\frac{1}{2}$ inch thick each, and respectively 9 inches and 6 inches deep, were heated to redness, and cooled by half-immersion in cold water twenty times; for effects see figs. 4 and 5.

Fig. 4.

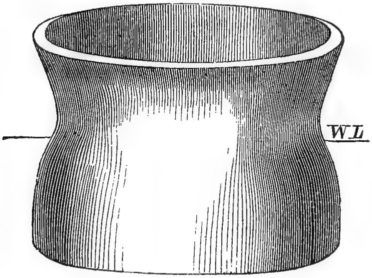
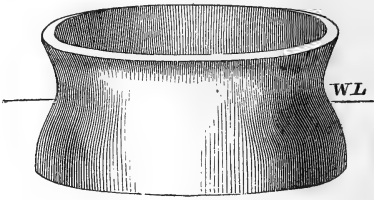


Fig. 5.



One-eighth of full size.

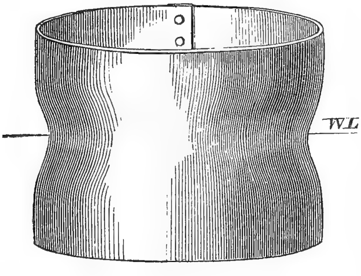
The 9-inch cylinder did not alter on the upper edge, cooled in air; but the lower edge, cooled in water, contracted ·6 inch, and the circumference, at about one inch above the water-line, was reduced 5·5 inches; the internal surface had increased in depth ·35 inch.

The small cylinder diminished ·7 inch on the upper edge, increased ·3 inch on the lower edge, and contracted 5·25 inches at about 1 inch above the water-line; the internal surface had increased in depth ·3 inch.

Experiment 3.—A cylinder of very thin wrought iron, so thin that it could not be welded, and was therefore riveted, of the same external dimensions as the 9-inch one of the foregoing experiment, was heated to redness and cooled by half-immersion ten times, in order to test the effect when the thickness of the metal was reduced as much as possible.

The upper and lower edges were not altered materially, while the greatest contraction took place on the water-line, instead of 1 inch above it as in the last experiment, and amounted to 3·5 inches. The depth measured on the curve had increased ·15 inch (see fig. 6).

Fig. 6.



One-eighth of full size.

Experiment 4.—Two wrought-iron cylinders, exactly similar to those used in experiment 2, were heated and cooled by being immersed to two-thirds their depth in water twenty times.

The upper edge of the large cylinder was reduced 2·1 inches, and the lower edge ·9 inch; it contracted 5·9 inches at about an inch above the water-line, and the inside surface had increased in depth ·35 inch (see fig. 7).

Fig. 7.

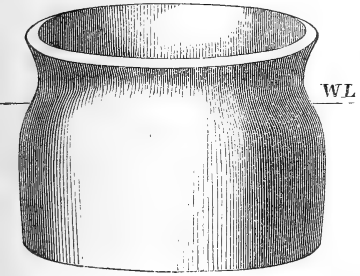
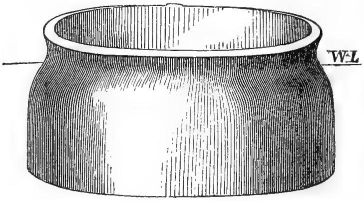


Fig. 8.



One-eighth of full size.

The upper edge of the small cylinder was reduced in circumference 3·6 inches and the lower edge ·65 inch, while the greatest contraction at about one inch above the water-line was 4·6 inches; and the internal surface had increased ·15 inch in height (see fig. 8).

Experiment 5.—A solid cylinder of wrought iron, 3 inches in diameter and 6 inches deep, was heated and cooled by being immersed half its depth in water fifteen times.

The greatest contraction took place a little above the water-line and on the lower edge, being in each case ·45 inch; the upper edge was reduced only ·1 inch.

A swell of metal took place on the two ends, but was greatest on the bottom, or that cooled in water, being ·15 inch in height.

The fibre of the iron opened at the fifteenth cooling (see fig. 9).

Experiment 6.—A wrought-iron cylinder exactly similar to the last was cooled by being immersed to two-thirds its depth fifteen times.

The greatest contraction, amounting to ·4 inch, took place a little above the water-line; the upper edge was ·05 inch smaller, and the lower edge ·35 inch, while the swellings on the ends were nearly the same as in the last experiment (see fig. 10).

The separation of the fibre took place at the fifteenth cooling.

Experiment 7.—Two flat pieces of wrought iron, each 12 inches long, 6 inches deep, and ·5 inch thick, were heated and cooled twenty times, one being immersed to half, and the other to two-thirds its depth in water.

That immersed one-half had contracted or become indented on the ends fully ·3 inch; the other had similar indentations, but to only one-half the amount. They were both turned up into the form of an arc, had thickened on their upper edges, and increased ·1 inch in thickness where the contractions on the ends took place (see figs. 11 & 12).

Fig. 9.

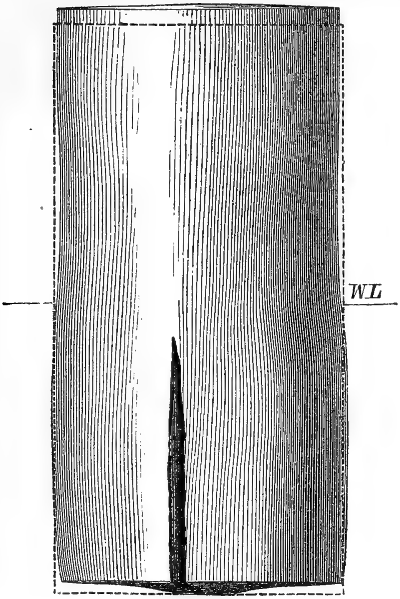
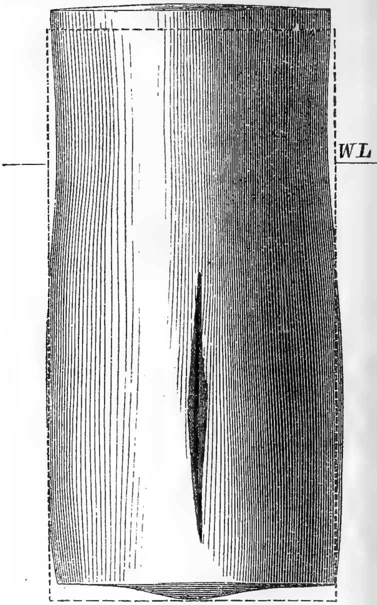
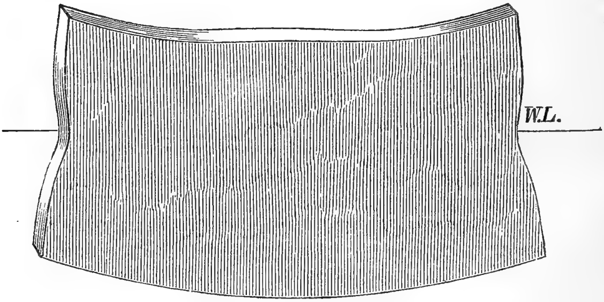


Fig. 10.



One-half of full size. The dotted lines indicate the original form.

Fig. 11.



One-fourth of full size.

Experiment 8.—Two hollow wrought-iron cylinders, 9 inches deep and 12 inches in diameter, were heated and cooled, one by simple exposure to air (fifteen times), and the other by total immersion in water (ten times). No alteration occurred in the form of either*.

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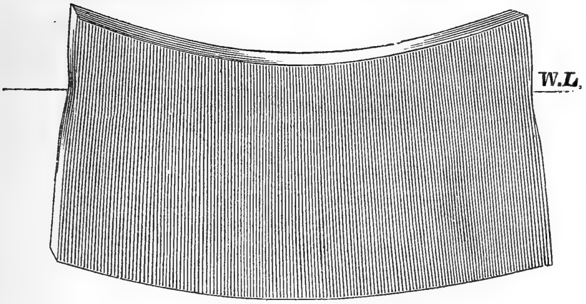
* The cylinder which was cooled in air weighed, before the experiment, 49 lbs. 14·5 ozs., and after the experiment 49 lbs. 11 ozs., showing a loss by scaling of 3·5 ozs.

During the progress of the experiment, however, it was frequently weighed, and

Fig. 12.



One-fourth of full size.

Experiment 9.—A solid cast-steel cylinder, of the same dimensions as that used in Experiment 5, was heated and cooled by half-immersion twenty times.

The effect obtained was similar to that produced upon the solid wrought-iron cylinders, but the breaking up of the structure was different (see fig. 13). The greatest contraction was slightly above the water-line, and amounted to ·38 inch; the bulgings on the ends were ·075 inch, being much less than on the wrought-iron cylinders.

Experiment 10.—A hollow brass cylinder, 6 inches long, 2 inches in diameter, and $\frac{1}{16}$th of an inch thick, was heated to redness and cooled by half-immersion thirty-four times.

The effect produced was the opposite to that which took place with the iron cylinders, being an expansion instead of a contraction at the water-line, the amount of which was ·175 inch, and it was also expanded on the lower edge ·1 inch (see fig. 16).

Experiment 11.—A hollow gun-metal cylinder was heated to redness and cooled twenty times by half-immersion.

The thickness of metal being greater than in the last experiment, the effect at the water-line was much less, but the lower edge had expanded ·1 inch. It began to crack all over at the last cooling.

Experiment 12.—A hollow tin cylinder was heated in linseed-oil which was brought to a temperature of 400° Fahr.; it was cooled by half-immersion in water five times.

The form was not altered in the least, though the heat was raised in the last instance to the melting-point, as shown by the lower part of the cylinder beginning to melt.

Experiment 13.—A hollow zinc cylinder was heated and cooled by half-immersion fifty times.

It was heated in a wood furnace, the degree of heat to which it

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was found each time to have increased in weight up to the tenth heating, at which point it weighed 50 lbs. 1·125 oz., or 2·625 ozs. heavier than it was at the commencement; from the tenth to the fifteenth heating the accumulated scales peeled off, and the weight was gradually reduced to that stated above.

That which was cooled in water weighed 50 lbs. 12·5 ozs. before the experiment, and 48 lbs. 14·5 ozs. at its conclusion, giving a loss of 1 lb. 14 ozs., which was due to the action of the water peeling off the scale each time the cylinder was cooled.

was brought being regulated by the melting of a piece of tin which was conveyed at the same time with it into the furnace. Several experiments with pieces of tin and zinc had been previously made, by means of which it was ascertained that in the same temperature tin melted in two-sevenths of the time requisite to melt zinc; hence when the zinc cylinder and piece of tin were placed in the furnace together, the time occupied by the tin in reaching its melting-point was carefully noted, and the cylinder was left in the furnace as long again as the time thus observed; by this means it was brought very nearly to its melting-point without incurring any danger of its actually melting. The last five times, however, it was allowed to remain a little longer in the flame; and the melting upon the top was retarded the last four times by placing a piece of iron upon it, which conducted heat from that part, allowing it to remain half a minute longer in the furnace.

Fig. 13.

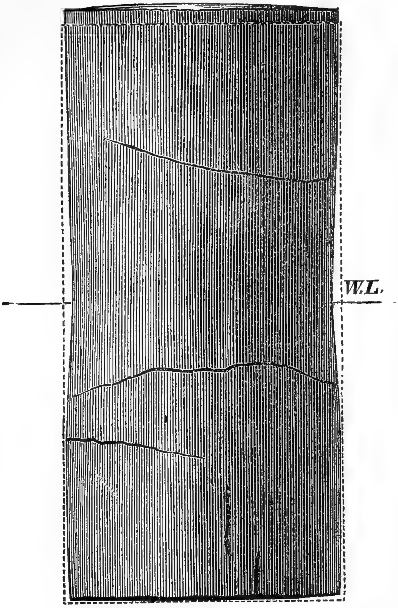


Fig. 14. (Top of fig. 13.)

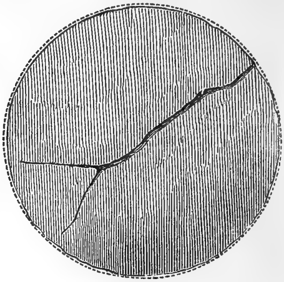
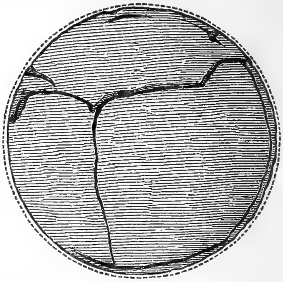


Fig. 15. (Bottom of fig. 13.)



One-half of full size. The dotted lines indicate the original figure.

The effect obtained was the same as that produced upon the brass cylinder (Exp. 10), or the opposite of what took place with iron; an expansion of ·175 inch occurred upon the water-line, and of ·115 inch upon the lower edge.

Experiment 14.—The hollow wrought-iron cylinder was heated to redness and cooled by half-immersion on its *side*, instead of on its end as in other experiments, twenty times.

Fig. 16.

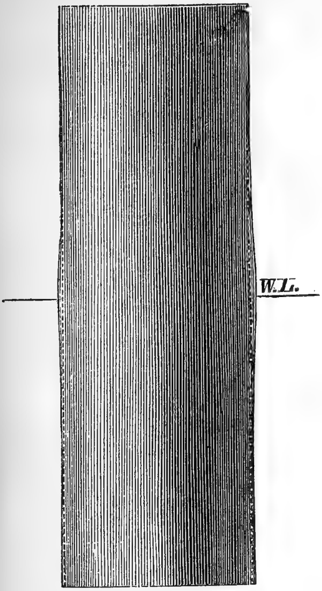


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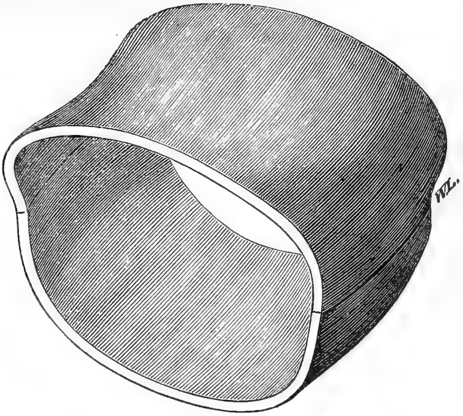
Fig. 17.



The effect was a very complicated one (see figs. 17, 18, and 19); the dotted lines show the original form.

g. 18. (Side view of fig. 17.)

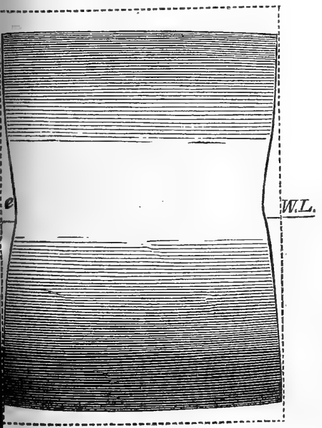
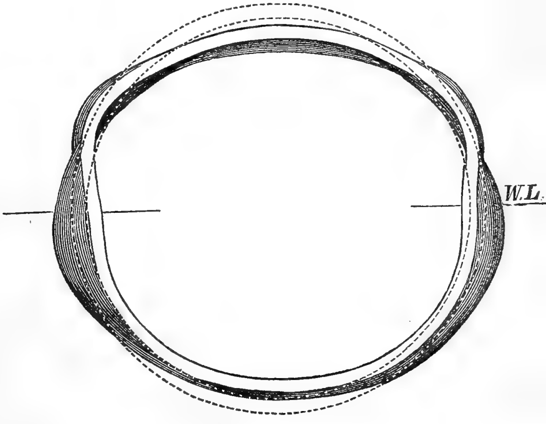


Fig. 19. (Front view of fig. 17.)



The three figures are one-sixth of full size.

Experiment 15.—A solid wrought-iron cylinder was heated to redness and cooled by half-immersion on its side twenty times.

The effect was of a similar nature to that of the last experiment (see figs. 20 and 21).

Fig. 20.

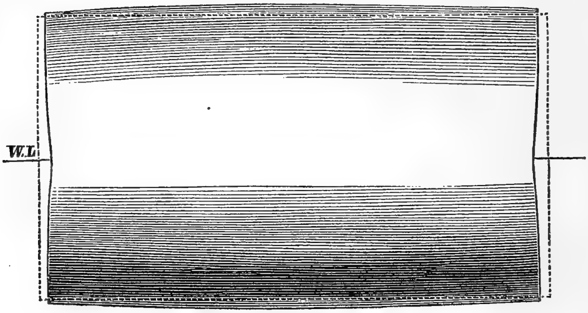
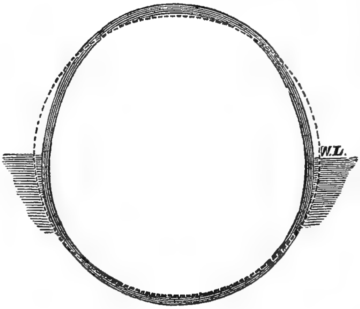


Fig. 21.



One-half of full size. The dotted line indicates original figure.

Experiment 16.—A hollow cast-iron cylinder, the dimensions of which were the same as those of the deep cylinder of Experiment 14, was heated to redness and cooled twice by half-immersion.

At the second cooling it fractured nearly all round, about an inch below the water-line. It expanded all over, but the expansion was least about an inch above the water-line, *i. e.* it did not contract to its original dimensions.

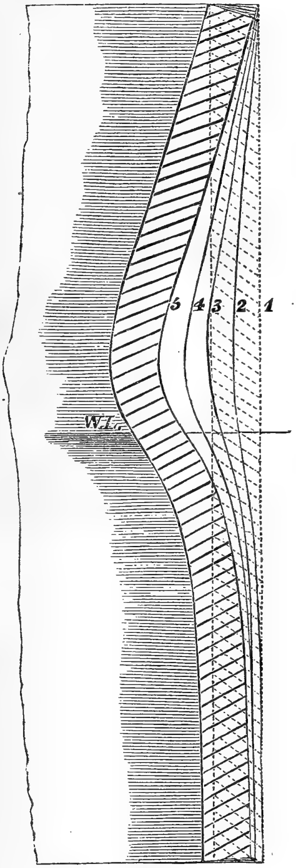
Experiment 17.—A solid cast-iron cylinder, 3 inches in diameter and 6 inches deep, was heated and cooled five times by half-immersion.

At the fifth cooling it cracked across the bottom; it also expanded throughout, and the expansion was least a little above the water-line, *i. e.* it did not contract to its original dimensions.

The subjoined figures (half the full size) show the changes produced on the 9-inch cylinders after every five heatings. (Experiments 2 and 4.)

Fig. 22.

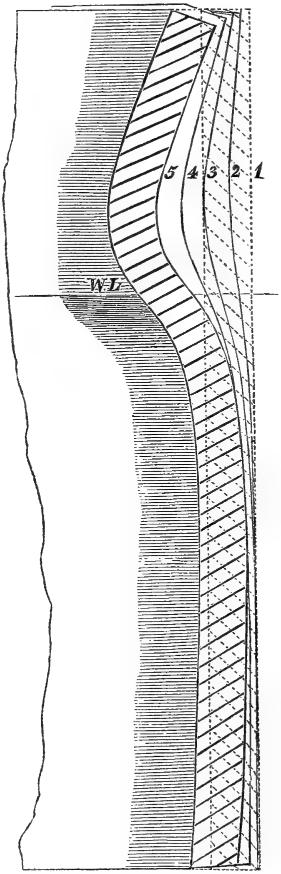
12″ Cylinder, 9″ high, ½″ thick.
Vide fig. 4. Cooled by ½-immersion.



No. 1. External surface, original form.
2. „ „ after 5 coolings.
3. „ „ „ 10 „
4. „ „ „ 15 „
5. „ „ „ 20 „

Fig. 23.

12″ Cylinder, 9″ high, ½″ thick.
Vide fig. 7. Cooled by ⅔-immersion.



No. 1. External surface, original form.
2. „ „ after 5 coolings.
3. „ „ „ 10 „
4. „ „ „ 15 „
5. „ „ „ 20 „

Tabulated Statement of the Results of the Experiments.

Number of experiment.	Kind of metal.	Number of coolings.	Amount of immersion.	Form of article, &c.	Dimensions, in inches.		
					Before experiment.	After experiment.	Difference.
1[a].	Wrought iron.	5	½	Hoop-tire for a 4′ 2″ wheel:—			
				External circumf. of upper edge...	155·5	147·5	−8·0
				do. do. lower edge...	155·5	156·375	+0·875
				Bevel of face	90°	69°	−21°
2[b].	Wrought iron.	20	½	12″ cylinder, 9″ deep and ½″ thick:			
				Internal circumf. of upper edge...	37·6	37·6	0·0
				do. do. contraction ..	37·6	32·1	−5·5
				do. do. lower edge...	37·6	37·0	−0·6
				Depth, perpendicular	9·0	8·8	−0·2
				do. on curve, external	9·0	9·15	+0·15
				do. do. internal	9·0	9·35	+0·35
2[c].	Wrought iron.	20	½	12″ cylinder, 6″ deep and ½″ thick:			
				Internal circumf. of upper edge...	37·6	36·9	−0·70
				do. do. contraction...	37·6	32·35	−5·25
				do. do. lower edge...	37·6	37·9	+0·30
				Depth, perpendicular..............	6·0	5·7	−0·30
				do. on curve, external	6·0	6·05	+0·05
				do do internal	6·0	6·30	+0·30
3[d].	Wrought iron.	10	½	12″ cylinder, 9″ deep, thin sheet:—			
				External circumf. of upper edge...	38·40	38·40	0·00
				do. do. contraction...	38·40	34·90	−3·50
				do. do. lower edge...	38·40	38·45	+0·05
				Depth, on curve	9·00	9·15	+0·15
4[e].	Wrought iron.	20	⅔	12″ cylinder, 9″ deep and ½″ thick:—			
				External circumf. of upper edge...	40·90	38·80	−2·10
				do. do. contraction...	40·90	35·00	−5·90
				do. do. lower edge...	40·90	40·00	−0·90
				Depth, perpendicular	9·00	8·80	−0·20
				do. on curve, external	9·00	9·00	0·00
				do. do. internal	9·00	9·35	+0·35
4[f].	Wrought iron.	20	⅔	12″ cylinder, 6″ deep and ½″ thick:—			
				External circumf. of upper edge...	40·8	37·2	−3·6
				do. do. contraction...	40·8	36·2	−4·6
				do. do. lower edge...	40·8	40·15	−0·65
				Depth, perpendicular..............	6·0	6·0	0·0
				do. on curve, external	6·0	6·05	+0·05
				do. do. internal	6·0	6·15	+0·15
5[g].	Wrought iron.	15	½	3″ cylinder, 6″ deep, solid:—			
				Circumference, upper edge	9·4	9·3	−0·1
				do. contraction	9·4	8·95	−0·45
				do. lower edge	9·4	8·95	−0·45
				Bulge on upper end	0·00	0·04	+0·04
				do. lower end	0·00	0·15	+0·15

[a] For remarks see end of Table, p. 404.

TABLE (*continued*).

Number of experiment.	Kind of metal.	Number of coolings.	Amount of immersion.	Form of article, &c.	Dimensions, in inches.		
					Before experiment.	After experiment.	Difference.
6[h].	Wrought iron.	15	$\frac{2}{3}$	3″ cylinder, 6″ deep, solid:—			
				Circumference, upper end	9·40	9·35	−0·05
				do. contraction.........	9·40	9·00	−0·40
				do. lower edge	9·40	9·05	−0·35
				Bulge on upper end	0·00	0·05	+0·05
				do. lower end	0·00	0·20	+0·20
7[i].	Wrought iron.	20	$\frac{1}{2}$	Flat piece, 12″ × 6″ × $\frac{1}{2}$″:—			
				Length on curve, upper edge ...	12·00	10·75	−1·25
				do do lower edge ...	12·00	12·10	+0·10
				Breadth, ends...	6·00	5·75	−0·25
				do. centre	6·00	6·00	0·00
				Upper edge, out of straight	0·00	0·60	+0·60
				Indentation on ends	0·00	0·30	+0·30
7[k].	Wrought iron.	20	$\frac{2}{3}$	Flat piece, 12″ × 6″ × $\frac{1}{2}$″:—			
				Length on curve, upper edge......	12·00	11·10	−0·90
				do. do. lower edge......	12·00	12·20	+0·20
				Breadth, ends	6·00	5·87	−0·13
				do. centre	6·00	5·95	−0·05
				Upper edge, out of straight	0·00	0·50	+0·50
				Indentation on ends	0·00	0·15	+0·15
8[l].	Wrought iron.	15 10	0 total	12″ cylinder, 9″ deep, $\frac{1}{2}$″ thick do. do. do.	No	effect.	
9[m].	Cast steel.	20	$\frac{1}{2}$	3″ cylinder, 6″ deep, solid:—			
				Circumference, upper edge	9·03	8·93	−0·10
				do. contraction	9·03	8·65	−0·38
				do. lower edge	9·03	8·93	−0·10
				Depth, perpendicular	6·00	6·10	+0·10
10[n].	Brass.	34	$\frac{1}{2}$	2″ cylinder, 6″ deep, $\frac{1}{16}$″ thick:—			
				External circumf. of upper edge...	6·175	6·175	0·000
				do. do expansion ...	6·175	6·350	+0·175
				do. do. lower edge...	6·175	6·270	+0·095
11[o].	Gun-metal.	20	$\frac{1}{2}$	3″ cylinder, 6″ deep, $\frac{1}{2}$″ thick:—			
				External circumf. of upper edge...	9·25	9·24	−0·01
				do. do. on water-line...	9·25	9·26	+0·01
				do. do. of lower edge..	9·25	9·38	+0·13
12.	Tin.	5	$\frac{1}{2}$	2″ cylinder, 5″ deep, $\frac{1}{4}$″ thick	No	effect.	
13.	Zinc.	50	$\frac{1}{2}$	3″ cylinder, 6″ deep, $\frac{1}{2}$″ thick:—			
				External circumf. of upper edge...	9·525	9·575	+0·050
				do. do. expansion ...	9·525	9·700	+0·175
				do. do. lower edge...	9·525	9·630	+0·105

TABLE (*continued*).

Number of experiment.	Kind of metal.	Number of coolings.	Amount of immersion.	Form of article, &c.	Dimensions, in inches.		
					Before experiment.	After experiment.	Difference.
14[p].	Wrought iron.	20	½ on its side.	12″ cylinder, 9″ deep, ½″ thick :—			
				External circumference of edges..	40·65	39·86	−0·79
				do. do. centre..	40·65	41·05	+0·40
				Depth on curve, part cooled in air.	9·00	9·00	0·00
				do. do. water-line.	9·00	8·25	−0·75
				do. do. in water...	9·00	8·80	−0·20
				Swell of side, 1″ below W.L. (at *a*, *b*)	0·00	1·00	+1·00
				Hollow of side, 4″ above do. (at *c*, *d*)	0·00	0·40	+0·40
				Longest ex. diam. 1″ below W. L.	12·94	14·275	+1·335
				Shortest do. at rt. angles to W.L.	12·94	12·00	−0·94
				Indentation of edges a little above water-line at *e*	0·00	0·45	+0·45
15[q].	Wrought iron.	20	½ on its side.	3″ cylinder, 5⅜″ deep, solid :—			
				External circumference of edges..	9·4	9·2	−0·2
				do do. centre.	9·4	9·475	+0·075
				Depth along part cooled in air ...	5·375	5·150	−0·225
				do. do. on W.L.	5·375	5·100	−0·275
				do. do. in water.	5·375	5·225	−0·150
				Longest diam. at rt. angles to W.L.	3·000	3·100	+0·100
				Shortest do. parallel with W.L. and a little below it	3·000	2·760	−0·240
16.	Cast iron.	2	½	12″ cylinder, 9″ deep, ½″ thick :—			
				External circumf. of upper edge...	40·90	41·05	+0·15
				do. do. least expansion	40·90	40·95	+0·05
				do. do. of lower edge—	40·90	41·15	+0·25
17.	Cast iron.	5	½	3″ solid cylinder, 6″ deep :—			
				External circumf. of upper edge...	9·4	9·55	+0·15
				do. do. least expansion	9·4	9·50	+0·10
				do. do. of lower edge...	9·4	9·55	+0·15

Remarks.

[a] The width was unaltered, and the thickness of the upper edge slightly increased. Figs. 1 and 2. [b] Fig. 4. [c] Fig. 5. [d] Fig. 6. [e] Fig. 7. [f] Fig. 8.

[g] The fibre opened at the fifteenth cooling. Fig. 9.

[h] The fibre opened at the fifteenth cooling after having exhibited a slight crack for two or three previous coolings. Fig. 10.

[i] The thickness of the metal at the indentation on ends increased ·1″. Fig. 11.

[k] The thickness of the metal at the indentation on ends increased similarly to the last. Fig. 12.

[l] Cooled in air 15 times. Cooled in water 10 times.

[m] The ends became slightly rounded. Fig. 13.

[n] At the last cooling the lower end of the cylinder began to crumble away in the water. Fig. 16.

[o] The expansion of the lower end may probably be due to the cracking of the metal, which was greatest at that part.

[p] Figs. 17, 18, 19. There was an increased thickness of metal at *e*.

[q] Figs. 20, 21.

[The cause of the curious phenomenon described by Colonel Clerk in the preceding paper seems to be indicated by some of the figures, especially those relating to hollow cylinders of wrought iron, which are very instructive.

Imagine such a cylinder divided into two parts by a horizontal plane at the water-line, and in this state immersed after heating. The under part, being in contact with water, would rapidly cool and contract, while the upper part would cool but slowly. Consequently by the time the under part had pretty well cooled, the upper part would be left jutting out; but when both parts had cooled, their diameters would again agree. Now in the actual experiment this independent motion of the two parts is impossible, on account of the continuity of the metal; the under part tends to pull in the upper, and the upper to pull out the under. In this contest the cooler metal, being the stronger, prevails, and so the upper part gets pulled in, a little above the water-line, while still hot. But it has still to contract on cooling; and this it will do to the full extent due to its temperature, except in so far as it may be prevented by its connexion with the rest. Hence, on the whole, the effect of this cause is to leave a permanent contraction a little above the water-line; and it is easy to see that the contraction must be so much nearer to the water-line as the thickness of the metal is less, the other dimensions of the hollow cylinder and the nature of the metal being given. When the hollow cylinder is very short, so as to be reduced to a mere hoop, the same cause operates; but there is not room for more than a general inclination of the surface, leaving the hoop bevelled.

But there is another cause of deformation at work, the operation of which is well seen in figs. 2 and 3. Imagine a mass of metal heated so as to be slightly plastic, and then rapidly cooled over a large part of its surface. In cooling, the skin at the same time contracts and becomes stronger, and thereby tends to squeeze out its contents. This accounts for the bulging of the ends of the solid cylinders of wrought iron and the rents seen in their cylindrical surface. The skin at the bottom is of course as strong as at the sides in the part below the water-line; but a surface which resists extension far more than bending has far less power to resist pressure of the nature of a fluid pressure when plane than when convex. The effect of the cause first explained is also manifest in these cylinders, although it is less marked than in the case of the hollow cylinders, as might have been expected.

The tendency of the cooled skin of a heated metallic mass to squeeze out its contents appears to be what gives rise to the bulging seen near the water-line in the hollow cylinder of brass. Wrought iron, being highly tenacious even at a comparatively high temperature, resists with great force the sliding motion of the particles which must take place in order that the tendency of the cooled skin to squeeze out its contents may take effect; but brass, approaching in its hotter parts more nearly to the state of a molten mass, exhibits the effect more strongly. It seems probable that even in the case of brass a *very* thin hollow cylinder would exhibit a contraction just above the water-line. Should there be a metal or alloy which about the temperatures with which we have to deal was stronger hot than cold, the effect of the cause first referred to would be to produce an expansion a little below the water-line.—G. G. S.]

LIX. *Intelligence and Miscellaneous Articles.*

REMARKS ON SOME NEW ALGEBRAIC SYMBOLS IN PHIL. TRANS. FOR 1862, PART I.* BY S. M. DRACH, F.R.A.S.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IN Mr. B. Gompertz's paper "On the Science connected with Human Mortality," the learned and venerable author proposes, in p. 519, the new foot index of crosier shape as indicating a common or hyperbolic logarithm, or their antilogarithms, instead of the usual exponential bases e and ϵ. The four positions of this mark ↼ having each a separate meaning, which might be mistaken by an error of the writer or printer, and as its attachment to a binomial or multinomial would be attended with some indistinctness, I beg to propose the following logarithmic mark, viz. $\rfloor$ before the quantity of which the logarithm is wanted, and $\rceil$ before the quantity (a logarithmic one) of which the natural number is required. Whether the Briggean or Napierian log is wanted may be indicated by a prefixed b or n; and the following synopsis will give an idea of the powers of my notation.

$\mathrm{b}^i \rfloor a =$ (com. log. of a) raised to the ith power.

$\mathrm{b} \rfloor a^i =$ com. log. of $(a^i) = i\,(\mathrm{b} \rfloor a)$.

$\mathrm{b}^i \rceil a =$ ith power of number whose common log is a.

$\mathrm{b} \rceil a^i =$ number whose common log is a^i.

$\mathrm{n}^i \rfloor a =$ (nat. log of a) raised to the ith power.

$\mathrm{n} \rfloor a^i$; $\mathrm{n}^i \rceil a$; $\mathrm{n} \rceil a^i$; similar to b, changing "common log" to "natural log."

Hence we may combine $\mathrm{n}^k \rfloor \mathrm{b}^i \rfloor a = k$th power of nat. log of a number N, which $\mathrm{N} = i$th power of com. log of a natural number a. We might further apply $\rfloor$ to sines, &c., $\sin^i \rfloor \theta = i$th power of $\sin\theta$, $\cos^k \rfloor \sin^i\theta = k$th power of cosine of an arc = a number N, which N is $(\sin\theta)^i$; similarly $\tan^m \rfloor \sin^i\theta = m$th power of tangent of an arc $=\mathrm{N}$; $\sin \rfloor \theta^i = \mathrm{n}^k \rfloor a =$ sin of $(\text{arc } \theta)^i = k$th power of nat. log of a nat. number a, &c.

Thus to the recognized $> < \perp \parallel$, the symbols $\rceil$ $\rfloor$ might fitly be added. Mr. Gompertz's embodiment of zeros is quite a boon. He puts ·00000000763 = ⑧763, and 89600000 = 896⑤; the former shows that 7 is preceded by eight decimal zeros, the latter that 6 is followed by five zeros before the decimal point. This suggestion is particularly valuable in large and approximate results, as the number of vibrations of a light-ray in an inch, the English mile radius vector of a planet from the sun, the English mile distance of a fixed star, parallax $=1''$, which is 206000 times 95 millions of miles = 1957⑩ miles in Mr. Gompertz's notation. There are two errata (p. 517, line 26, *for* L_{m+n} *read* L_{m+2n}; and p. 543–4, *for* x^0 *read* $\mathrm{S}x_0$) that may be pointed out.

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* Erratum in page 367, line 14, *for* 4° *read* 44° (Mr. De la Rue's paper).

In Mr. A. Cohen's paper, p. 470, I fear that the author's "complete sum" will rather confuse terms. His A B is the "complete sum of O A and O B," while it is really less than either; and the angular opening of O A and O B is totally disregarded, the parenthesized (O A) and (O B) being a questionable caution-mark.

It is much to be deplored that Mr. John Long's mode of raising logarithms (Phil. Trans. 1714, 'Hutton's Tracts') has not been generally adopted for teaching in schools. It rests simply on arithmetical notions of progression. Thus log 10=1, log 100=2; let $N=A.B.C.D.E.F.G$, &c., and $\log A=\frac{a}{10}$, $\log B=\frac{b}{100}$, $\log C=\frac{c}{1000}$, &c., so that with his Table of the 10th, 100th, 1000th parts of 10, we find $10^{0\cdot a}=A$, $N\div A=B\,C\,D$, &c. Tabular $10^{0\cdot 0b}=B$; $B.C.D$, &c. $\div B=C.D.E$, &c., and thus we finally get

$$\cdot a+\cdot 0b+\cdot 000c+\cdot 000d,\ \&c.=\log N$$

without any *previous algebraic knowledge being required.* In countries where the decimal division of coins, weights, and measures is in use, this popular mode of teaching logarithms ought surely to be a branch of primary education, as much as exchanges, equation of payments, &c.

London, Jan. 12, 1863. S. M. Drach.

ON A PARTICULAR CASE OF THE FORMATION OF SOAP-BUBBLES. BY M. FÉLIX PLATEAU.

Being merely a student of the Faculty of Sciences, I should have thought it premature to submit to the Academy the results of an observation to which chance alone had led me; but I have yielded to the pressing wish of my father, who finds the observation curious in itself, and important in its bearings on a meteorological question.

I was emptying a dish containing some waste liquor which had been used for cleaning metal plates, and I tried by darting it obliquely in the air to spread it out as a sheet. I obtained a sheet, in fact, but I was surprised to see it change into a bubble of 8 to 9 centims. in diameter.

I repeated the experiment several times, simply using soap-water, and I soon succeeded in producing the effect with certainty; but almost always several bubbles were formed, sometimes as many as fifteen. Their diameter, which might reach 8 to 9 centims. in the largest, was less as they were more numerous.

The conditions which appeared the best are as follows. A dish is taken about 15 centims. in diameter; success may be obtained, though less certainly, with vessels of other shapes and different dimensions. There ought to be a large quantity of liquid; it is projected at an angle of about 45 degrees, being turned rapidly on itself, so as to produce the most extended surface possible. The liquid which gave me the best results is a solution of 1 part of Marseilles soap in 40 parts of water. In order the better to make out the mode in which the phenomenon takes place, I observed it from an upper window, while another person below made the experiment as I have indicated.

I thus observed that the liquid sheet, of a very irregular form and indented on the edges, separates at its edges into numerous solid drops, while the remainder is torn generally into several portions, each of which rapidly closes so as to form a complete hollow bubble.

My father sees in this phenomenon an argument in support of the vesicular state of the vapour of clouds. In fact one of the principal objections against this hypothesis consists in the impossibility of conceiving how the molecules of gaseous vapour could, when this repasses into the liquid state, agglomerate so as to constitute envelopes enclosing air; but we see now that this immediate agglomeration into closed envelopes is not necessary; it is sufficient that the molecules unite in open plates of any shape and curvature; each of these plates would then quickly close of itself and give rise to a vesicule. Doubtless the production of these plates is not easy to understand, but it appeared at least much more admissible than the formation, complete in all respects, of the vesicules.—*Bulletin de l'Académie Royale de Belgique*, sér. 2. vol. xiii. No. 4.

NOTE ON ALBERT DURER'S HEPTAGON-CHORD.

BY S. M. DRACH, F.R.A.S.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

In Prof. De Morgan's "Notes on Perspective" in the Athenæum Journal for September 12, p. 335, the author says that the $\perp$(C H) on the base A B=1 of an equilateral $\triangle$, as in Euclid I. 1, was proposed by A. Durer for a near value of

$$\mathrm{ch}\,\frac{360^\circ}{7}=51^\circ\ 24'\ 42''\tfrac{6}{7}=0{\cdot}8677674789,$$

which draw (see fig. Euclid I. 1), and call C G. Now $CH=\frac{1}{2}\sqrt{3}$, $CG-CH={\cdot}001742075=CH\times{\cdot}00201158$. I think this is improveable by drawing a straight line C K from C cutting A B in K, so that C K=C G, and K H on line A B

$$=\sqrt{CK^2-CH^2}=\sqrt{{\cdot}00302039809943}={\cdot}05495814859=CH$$

divided by 15·7579072; which last number can be gradually approximated by 16, $\frac{63}{4}$, $\frac{520}{33}$ (true to 1-45000th). If therefore H K =this aliquot part of C H, and C K be joined, C K=true C G very nearly. If H K, instead of coinciding with H A, be drawn to make angles of 30°, 45°, and 60° with it, C K still remaining =C G, H K will respectively =·00347372, ·002461198, ·002000109. The simplest approximation is, however, the above of $CK=\frac{3}{40}\cdot\frac{11}{13}\,CH$ and $\perp$ to it. The true angle $KCH=3^\circ\ 37'\ 44''{\cdot}2$, indicative of the closeness of A. Durer's assumption. S. M. DRACH.

London, October 14, 1863.

Errata.

In my Polyhedric Fan paper (April 1863),

$EF=\frac{5}{4}PD$; $EPF+3DPE-4CPD=49^\circ\ 59'\ 59''$, &c.

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

[FOURTH SERIES.]

DECEMBER 1863.

LX. *On the Molecular Mobility of Gases.*

By THOMAS GRAHAM, *F.R.S., Master of the Mint*[*].

THE molecular mobility of gases will be considered at present chiefly in reference to the passage of gases, under pressure, through a thin porous plate or septum, and to the partial separation of mixed gases which can be effected, as will be shown, by such means. The investigation arose out of a renewed and somewhat protracted inquiry regarding the diffusion of gases (which depends upon the same molecular mobility), and has afforded certain new results which may prove to be of interest in a theoretical as well as in a practical point of view.

In the Diffusiometer, as first constructed, a plain cylindrical glass tube, about 10 inches in length and rather less than an inch in diameter, was simply closed at one end by a porous plate of plaster of Paris, about one-third of an inch in thickness, and was thus converted into a gas-receiver[†]. A superior material for the porous plate has since been found in the artificially compressed graphite of Mr. Brockedon, of the quality used for making writing-pencils. This material is sold in London in small cubic masses about 2 inches square. A cube may easily be cut into slices of a millimetre or two in thickness by means of a saw of steel spring. By rubbing the surface of the slice without wetting it upon a flat sand-stone, the thickness may be further reduced to about one-half of a millimetre. A

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[*] From the Philosophical Transactions, Part II. for 1863, having been read at the Royal Society June 18, 1863.

[†] "On the Law of the Diffusion of Gases," Transactions of the Royal Society of Edinburgh, vol. xii. p. 222; or Philosophical Magazine, 1834, vol. ii. pp. 175, 269, 351.

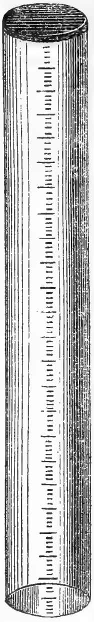

Fig. 1.

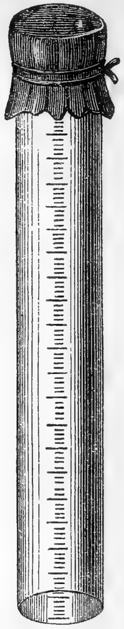

Fig. 2.

circular disc of this graphite, which is like a wafer in thickness but possesses considerable tenacity, is attached by resinous cement to one end of the glass tube above described, so as to close it and form a diffusiometer (fig. 1). The tube is filled with hydrogen gas over a mercurial trough, the porosity of the graphite plate being counteracted for the time by covering it tightly with a thin sheet of gutta percha (fig. 2). On afterwards removing the latter, gaseous diffusion immediately takes place through the pores of the graphite. The whole hydrogen will leave the tube in forty minutes or an hour, and is replaced by a much smaller proportion of atmospheric air (about one-fourth), as is to be expected from the law of the diffusion of gases. During the process, the mercury will rise in the tube, if allowed, forming a column of several inches in height —a fact which illustrates strikingly the intensity of the force with which the interpenetration of different gases is effected. Native graphite is of a lamellar structure, and appears to have little or no porosity. It cannot be substituted for the artificial graphite as a diffusion-septum. Unglazed earthenware comes next in value to graphite for that purpose.

The pores of artificial graphite appear to be really so minute, that a gas *in mass* cannot penetrate the plate at all. It seems that molecules only can pass; and they may be supposed to pass wholly unimpeded by friction, for the smallest pores that can be imagined to exist in the graphite must be tunnels in magnitude to the ultimate atoms of a gaseous body. The sole motive agency appears to be that intestine movement of molecules which is now generally recognized as an essential property of the gaseous condition of matter.

According to the physical hypothesis now generally received[*], a gas is represented as consisting of solid and perfectly elastic spherical particles or atoms, which move in all directions, and are

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[*] D. Bernoulli, J. Herapath, Joule, Krönig, Clausius, Clerk Maxwell, and Cazin. The merit of reviving this hypothesis in recent times and first applying it to the facts of gaseous diffusion, is fairly due to Mr. Herapath. See 'Mathematical Physics,' in two volumes, by John Herapath, Esq. (1847).

animated with different degrees of velocity in different gases. Confined in a vessel the moving particles are constantly impinging against its sides and occasionally against each other, and this contact takes place without any loss of motion, owing to the perfect elasticity of the particles. If the containing vessel be porous, like a diffusiometer, then gas is projected through the open channels, by the atomic motion described, and escapes. Simultaneously the external air is carried inwards in the same manner, and takes the place of the gas which leaves the vessel. To this atomic or molecular movement is due the elastic force, with the power to resist compression, possessed by gases. The molecular movement is accelerated by heat and retarded by cold, the tension of the gas being increased in the first instance and diminished in the second. Even when the same gas is present both within and without the vessel, or is in contact with both sides of our porous plate, the movement is sustained without abatement—molecules continuing to enter and to leave the vessel in equal number, although nothing of the kind is indicated by a change of volume or otherwise. If the gases in communication be different but possess sensibly the same specific gravity and molecular velocity, as nitrogen and carbonic oxide do, an interchange of molecules also takes place without any change in volume. With gases opposed of unequal density and molecular velocity, the permeation ceases of course to be equal in both directions.

These observations are preliminary to the consideration of the passage through a graphite plate, in one direction only, of gas under pressure, or under the influence of its own elastic force. We are to suppose a vacuum to be maintained on one side of the porous septum, and air or any other gas, under a constant pressure, to be in contact with the other side. Now a gas may pass into a vacuum in three different modes, or in two other modes besides that immediately before us.

1. The gas may enter the vacuum by passing through a minute aperture in a thin plate, such as a puncture in platinum foil made by a fine steel point. The rate of passage of different gases is then regulated by their specific gravities, according to a pneumatic law which was deduced by Professor John Robison from Torricelli's well-known theorem of the velocity of efflux of fluids. A gas rushes into a vacuum with the velocity which a heavy body would acquire by falling from the height of an atmosphere composed of the gas in question, and supposed to be of uniform density throughout. The height of the uniform atmosphere would be inversely as the density of the gas, the atmosphere of hydrogen, for instance, sixteen times higher than that of oxygen. But as the velocity acquired by a heavy body in falling is not directly as the height, but as the square root of the

height, the rate of flow of different gases into a vacuum will be inversely as the square root of their respective densities. The velocity of oxygen being 1, that of hydrogen will be 4, the square root of 16. This law has been experimentally verified*. The relative times of the effusion of gases, as I have spoken of it, are similar to those of molecular diffusion; but it is important to observe that the phenomena of effusion and diffusion are distinct and essentially different in their nature. The effusion movement affects masses of gas, the diffusion movement affects molecules; and a gas is usually carried by the former kind of impulse with a velocity many thousand times as great as is demonstrable by the latter.

2. If the aperture of efflux be in a plate of increased thickness, and so becomes a tube, the effusion-rates are disturbed. The rates of flow of different gases, however, assume again a constant ratio to each other when the capillary tube is considerably elongated, when the length exceeds the diameter by at least 4000 times. These new proportions of efflux are the rates of the "Capillary Transpiration" of gases†. The rates are found to be the same in a capillary tube composed of copper as they are in glass, and appear to be independent of the material of the capillary. A film of gas no doubt adheres to the surface of the tube, and the friction is really that of gas upon gas, and is consequently unaffected by the tube-substance. The rates of transpiration are not governed by specific gravity, and are indeed singularly unlike the rates of effusion.

The transpiration-velocity of oxygen being 1, that of chlorine is 1·5, that of hydrogen 2·26, of ether vapour the same or nearly the same as that of hydrogen, of nitrogen and carbonic oxide half that of hydrogen, of olefiant gas, ammonia, and cyanogen 2 (double or nearly double that of oxygen), of carbonic acid 1·376, and of the gas of marshes 1·815. In the same gas the velocity of transpiration increases with increased density, whether occasioned by cold or pressure.

The transpiration-ratios of gases appear to be in direct relation with no other known property of the same gases, and they form a class of phenomena remarkably isolated from all else at present known of gases.

There is one property of transpiration immediately bearing upon permeation of the graphite plate by gases. The capillary offers to the passage of gas a resistance analogous to that of friction, proportional to the surface, and consequently increasing as the tube or tubes are multiplied in number and diminished in diameter, with the area of discharge preserved constant. The

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* "On the Motion of Gases," Philosophical Transactions, 1846, p. 573.

† Ibidem, p. 591; and Philosophical Transactions, 1849, p. 349.

resistance to the passage of liquid through a capillary was observed by Poiseuille to be nearly as the fourth power of the diameter of the tube. In gases the resistance also rapidly increases; but in what ratio, has not been observed. The consequence, however, is certain, that as the diameter of the capillaries may be diminished beyond any assignable limit, so the flow may be retarded indefinitely, and caused at last to become too small to be sensible. We may then have a mass of capillaries of which the passages form a large aggregate, but are individually too small to allow a sensible flow of gas under pressure. A porous solid mass may possess the same reduced permeability as the congeries of capillary tubes. Indeed the state of porosity described appears to be more or less closely approached by all loosely aggregated mineral masses, such as lime-plaster, stucco, chalk, baked clay, non-crystalline earthy powders like hydrate of lime or magnesia compacted by pressure, and in the highest degree perhaps by artificial graphite.

3. A plate of artificial graphite, although it appears to be practically impermeable to gas by either of the two modes of passage previously described, is readily penetrated by the agency of the molecular or diffusive movement of gases. This appears on comparing the time required for the passage through the plate of equal volumes of different gases under a constant pressure. Of the three gases, oxygen, hydrogen, and carbonic acid, the time required for the passage of an equal volume of each through a capillary glass tube, in similar circumstances as to pressure and temperature, was formerly observed to be as follows:—

	Time of capillary transpiration of equal volumes.
Oxygen	1
Hydrogen	0·44
Carbonic acid	0·72

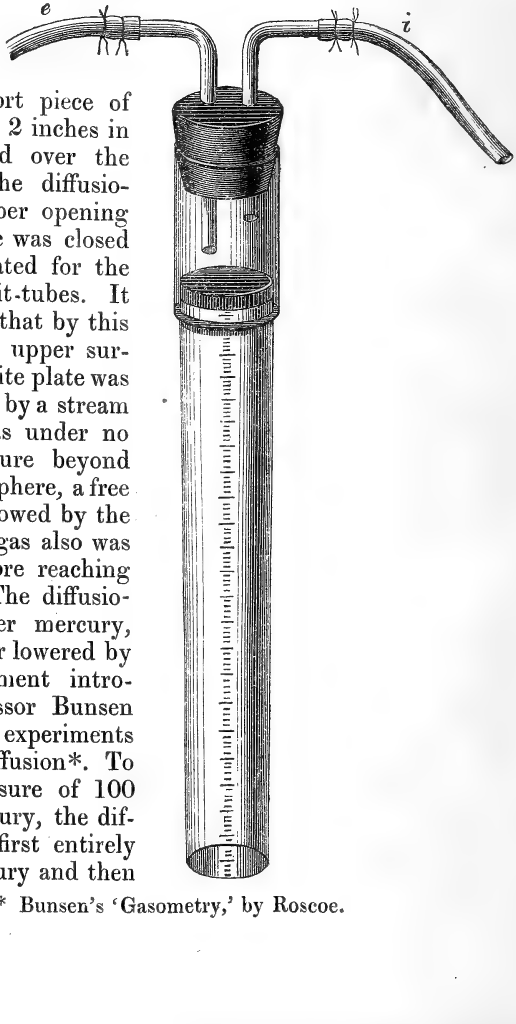
Now through a plate of graphite, half a millimetre in thickness, the same gases were observed to pass, under a constant pressure of a column of mercury of 100 millims. in height, in times which are as follows:—

	Time of molecular passage.	Square root of density (oxygen 1).
Oxygen . . .	1	1
Hydrogen . .	0·2472	0·2502
Carbonic acid .	1·1886	1·1760

It appears that the times of passage through the graphite plate have no relation to the capillary transpiration-times of the same gases as first quoted. The new times in question, however, show a close relation to the square roots of the densities of the respective gases, as is seen in the last Table; and they so far agree with theoretical *times of diffusion* usually ascribed to the same gases.

These results were obtained by means of the graphite diffusiometer already referred to, which was a plain glass tube about 22 millims. in diameter, closed at one end by the graphite plate. In order to conduct gas to the upper surface of the graphite plate, a little chamber was formed above the plate, to which the gas was conveyed in a moderate stream by the entrance-tube *e* (fig. 3); while the gas brought in excess was constantly escaping into the air by the open issue-tube *i*. The chamber was formed of a short piece of glass tube, about 2 inches in length, cemented over the upper end of the diffusiometer. The upper opening of this short tube was closed by a cork perforated for the entrance- and exit-tubes. It will be observed that by this arrangement the upper surface of the graphite plate was constantly swept by a stream of gas, which was under no additional pressure beyond that of the atmosphere, a free escape being allowed by the exit-tube. The gas also was always dried before reaching the chamber. The diffusiometer stood over mercury, and was raised or lowered by the lever movement introduced by Professor Bunsen in his very exact experiments upon gaseous diffusion*. To obtain the pressure of 100 millims. of mercury, the diffusiometer was first entirely filled with mercury and then

Fig. 3.



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[*] Bunsen's 'Gasometry,' by Roscoe.

raised in the trough. Gas gradually entered till the column of mercury in the tube fell to 100 millims. The mercury was then maintained at this height, by gradually raising the tube in proportion as gas continued to enter and the mercury to fall, so as to maintain a constant difference of level of 100 millims., as observed by the graduation inscribed upon the tube itself, between the level of the mercury in the tube and trough. The experiment consisted in observing the time in seconds which the mercury took to fall 10 millimetre divisions with each gas. The constant volume of gas which entered was 2·2 cubic centims. (0·1342 cubic inch). Two experiments were made with each gas.

Oxygen entered in 898 and 894 seconds; mean 896 seconds.
Hydrogen in 222 and 221 seconds; mean 221·5 seconds.
Carbonic acid in 1070 and 1060 seconds; mean 1065 seconds.

In such experiments the same gas exists on both sides, and also occupies the pores of the diaphragm. But the molecular movement within the pores in a downward direction is not fully balanced by the molecular movement in an upward direction, owing to the less tension, by 100 millims., of the gas below the diaphragm and within the tube than the gas above and without. The influx of gas indicates the difference of molecular movement in opposite directions. Taking the full tension of the gas above the diaphragm at 760 millims., that below would be 660 millims., and the movement downwards and that upwards are represented by these numbers respectively.

To increase the inequality of tension and favour the passage of gas through the graphite plate, a diffusion-tube was now used, 48 inches in length, or of the dimensions of a barometer-tube, by which a Torricellian vacuum could be commanded. The pneumatic trough in which this gas-tube was suspended consisted of a pipe of gutta percha of equal length, closed at the bottom by a cork, and widening into a funnel-form at the top. In one modification of the instrument it was found convenient to cement a capillary glass tube to the side of the glass diffusiometer, within about 15 millims. of the upper end of the tube. An opening into the upper part of the glass tube was thus obtained, by means of which the gas contained in the diffusiometer could escape when the latter was depressed in the mercurial trough. A flexible tube with clip was attached to the capillary tube referred to, so that the latter could be closed. From the same opening a specimen of the gas contained in the diffusiometer could be drawn when required for examination.

In another and more serviceable modification of this barometrical diffusiometer a large space was obtained above the mercurial column, by surmounting the long glass tube, unprovided with a graphite plate, by a glass jar about half a litre in capacity. This jar was more correctly a small bell-jar (fig. 4) open at top. It was fitted in an inverted position, as in fig. 5, to the open end of the long glass tube *d*, by means of a cork and cement. The large upper opening was closed by a circular plate of gutta percha (fig. 5), about 10 millims., or nearly half an inch, in thickness. This disc of gutta percha had two perforations, at *f* and *g* (fig. 6), the former of which was fitted above with a wide glass tube. The tube *f* was closed below by the plate of graphite, and above with a perforated cork carrying a quill tube, *e*. This quill tube was the entrance-tube for gas, and was accompanied by the usual issue-tube, *i*. The other aperture in the gutta-percha cover was fitted with a plain quill tube, *h*, which did not descend below the level of the gutta percha, and formed a tube of exit. No difficulty was found in making all these junctions air-tight, by applying the heated blade of a knife to fuse the gutta percha in contact with the glass. Gutta percha is indeed of no ordinary value in the construction of pneumatic apparatus. The graphite plate itself required to be not less than 1 millim. in thickness, in order to support the pressure of a whole atmosphere, to which it is exposed in the present apparatus. This barometrical diffusiometer is supported from above by a cord passing over a pulley, and is duly counterpoised by a hanging weight.

Fig. 4.

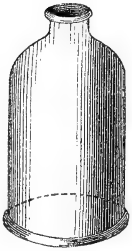


Fig. 5.

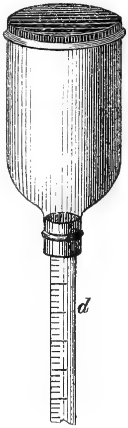
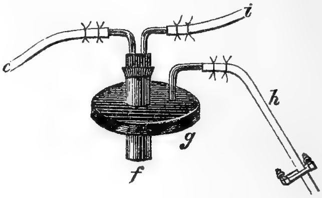
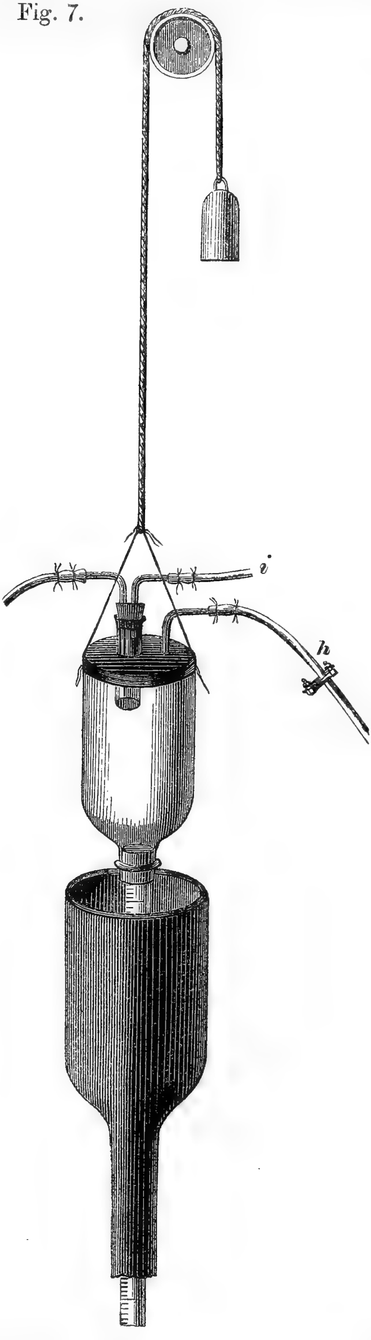


Fig. 6.



In operating, the first point is to expel the air from the barometer-tube and upper chamber. The instrument (fig. 7) is sunk completely in the mercurial trough previously described, till the whole is filled, and mercury enters the quill tube of exit, *h*. The caoutchouc extension of this tube is then closed by a pinch. The diffusiometer is now elevated 30 or 40 inches, when the mercury sinks in the glass tube till it comes to stand at the barometric height for the time, leaving the upper chamber entirely vacuous. The gas to be tried has in the mean time been made to stream over the upper surface of the graphite plate, exactly as in the experiment with the former diffusiometer. The graphite is permeated by the gas, and the mercury in the diffusiometer-tube begins to fall, but it now falls slowly, owing to the considerable vacuous space to be filled. It is allowed to fall about half an inch, and the exact time is then noted, by a watch, when the mercury passes a certain point in the graduation of the tube, and again when the mercury descends to another fixed point an inch or two below the former. The time of permeation of a certain volume of gas is thus ascertained in seconds. The experiment is immediately repeated with two or more gases in succession, in similar circumstances as to pressure and with great care taken to ensure uniformity of temperature during the whole period.

Fig. 7.



In a series of four experiments made with hydrogen, the mercury fell from 758 to 685 millims. (29·9 inches to 27 inches) in 252, 256, 254, and 256 seconds; mean 254·5 seconds.

In three experiments with oxygen the mercury fell through the same space in 1019, 1025, and 1024 seconds; mean 1022·7 seconds:

$$\frac{1022 \cdot 7}{254 \cdot 5} = 4 \cdot 018.$$

The times of these gases appear therefore to be as 1 to 4·018, while the times calculated as being inversely as the square root of the densities of the same gases are as 1 to 4.

On another day, with a different height of the barometer, four gases were passed through the graphite plate in succession through a somewhat shorter range, namely, from 754 to 685 millims. (29·7 to 27 inches).

The time of permeation of air was 884 and 885 seconds; mean 884·5 seconds.

The time of carbonic acid was 1100 and 1106 seconds; mean 1103 seconds.

The time of oxygen was 936, 924, and 930 seconds; mean 930 seconds.

The time of hydrogen was 229, 235, and 235 seconds; mean 233 seconds.

These times of permeation are in the following proportion:—

	Times of the permeation of equal volumes of gas through graphite.
Oxygen	1
Air	0·9501
Carbonic acid . . .	1·1860
Hydrogen	0·2505

These numbers approach so closely to the square roots of the density, or the theoretical diffusion-times of the same gases, namely, oxygen 1, air 0·9507, carbonic acid 1·176, and hydrogen 0·2502, that they may be held to indicate the prevalence of a common law. They exclude the idea of capillary transpiration, which gives to the same gases entirely different numbers.

The movement of gases through the graphite plate appears to be solely due to their own proper molecular motion, quite unaided by transpiration. It seems to be the simplest possible exhibition of the molecular or diffusive movement of gases. This pure result is to be ascribed to the wonderfully fine (minute) porosity of the graphite. The interstitial spaces appear to be sufficiently small to extinguish capillary transpiration entirely. The graphite plate is a pneumatic sieve which stops all gaseous matter in mass, and permits molecules only to pass.

It is worth observing what result a plate of more open structure, such as stucco, will give in comparison with graphite. For the graphite plate, a cylinder of stucco, 12 millims. in thickness, was accordingly substituted, and gas allowed to percolate at both low and high pressures, as in the former experiments with graphite.

1. Under a constant pressure of 100 millims. of mercury, gas was allowed to enter through 100 millim. divisions of the diffusiometer.

With air, the time in two experiments was 515, and again 515 seconds.

With hydrogen 178 seconds, and again 178 seconds:

$$\frac{515}{178}=2·894.$$

2. Under a pressure beginning with 710 millims. (28 inches) and ending with 660 millims. (26 inches), the time with air was 374 and 375 seconds; mean 374·5 seconds. The time with hydrogen was 129 and 130 seconds; mean 129·5 seconds:

$$\frac{374·5}{129·5}=2·891.$$

The stucco cylinder of the preceding experiments had been dried over sulphuric acid, without the application of heat. It was further desiccated at 60° C. for twenty-four hours, in order to find whether the porosity would be altered. The ratio of the time of hydrogen to that of air now became 1 to 2·788 at the lower degree of pressure, and 1 to 2·744 at the higher degree of pressure.

It will be observed that the theoretical diffusion-ratio of hydrogen to air, which is 1 to 3·80, is greatly departed from in these experiments with stucco. The ratio appears to be tending to the proportion of the transpiration-times of the same gases, namely, 1 to 2·04. In an experiment recorded by Bunsen, the ratio observed between the times of hydrogen and oxygen in passing, under a small degree of pressure, through stucco dried by heat was so low as 1 to 2·73, the stucco being probably less dense than in the experiments before us.

With stucco the permeation of gases *under pressure* appears to be a mixed phenomenon—to some extent molecular diffusion into a vacuum, such as holds with the plate of graphite, but principally capillary transpiration of gas in mass.

The diffusiometer was now closed by a plate of white biscuit-ware, 2·2 millims. in thickness. The time of fall at the constant pressure of 100 millims., through a range of forty divisions of the diffusiometer, was, for air 1210 seconds, for hydrogen 321 seconds.

$$\begin{array}{l}\text{Air . . . .}\\ \text{Hydrogen . . . .}\end{array}\ \frac{1210}{321}=3{\cdot}769.$$

The time, again, from 736 to 685 millims. (29 to 27 inches) was, for air 685 and 684 seconds; mean 684·5 seconds; and for hydrogen 183, 183, and 184 seconds; mean 183·3 seconds.

$$\begin{array}{l}\text{Air . . . .}\\ \text{Hydrogen . . . .}\end{array}\ \frac{684{\cdot}5}{183{\cdot}5}=3{\cdot}754.$$

The stoneware was evidently of a much closer texture than stucco, and the ratio appears again less influenced by capillary transpiration. In fact the molecular ratio of 1 to 3·80 is approached within 1 per cent. Biscuitware therefore appears to be but little inferior to graphite for such experiments, a circumstance which is important, as the latter is not easily procured and cannot be converted into tubes and other convenient forms like plastic clay.

Further, the rate of passage of gas through the plate of graphite appears to be closely proportional to the pressure. The resistance was increased by augmenting the thickness of the plate to 2 millims.; and with air and hydrogen at a pressure maintained constant at 50 and 100 millims., the time was observed that the gas took to enter 10 linear millimetre divisions of the tube.

	Seconds.	Ratio.
Air under pressure of 100 millims. . .	1925	1
Air under pressure of 50 millims. . .	3880	2·015
Hydrogen under pressure of 100 millims.	497	1
Hydrogen under pressure of 50 millims.	1022	2·056

By halving the pressure, the time of passage is doubled, or increased somewhat more. Greater pressures might probably give a rate of passage corresponding more exactly with the pressure.

The ratio between the comparative times of the two gases in the last experiments may also be noticed, the observations having been made in similar circumstances as to pressure and temperature.

Barom. 760 millims.; Therm. 12°·9 C.	At pressure of 50 millims.	Barom. 760 millims.; Therm. 12°·9 C.	At pressure of 100 millims.
Air . . . . . Hydrogen . .	$\frac{3880}{1022}=3{\cdot}796.$	Air . . . . Hydrogen .	$\frac{1925}{497}=3{\cdot}873.$

The observation was repeated at the pressure of 100 millims. with barometer at 754 millims. and thermometer at 10° C.

$$\begin{array}{ll} \text{Air} \;.\;.\;.\;.\;.\;. & 1920 \\ \text{Hydrogen} \;.\;.\;.\;. & 498 \end{array} \quad \frac{1920}{498}=3{\cdot}855.$$

The velocity of hydrogen appears, as usual, to be nearly 3·8 times that of air; $\frac{1}{\sqrt{{\cdot}06926}}=3{\cdot}7994.$

An experiment was made at the same time as the former series upon a mixture of 95 hydrogen and 5 air, which gave an unlooked-for result that led to a great deal of inquiry. It is known that such a mixture is effused through an aperture in a fine plate in a time which is as the square root of the density of the mixture, and therefore nearly the arithmetical mean of the two gases effused separately. But in transpiration by a capillary, a mixture of 95 hydrogen and 5 air requires a considerably longer time than the gases transpired separately. In fact 5 per cent. of air retards the transpiration of hydrogen nearly as much as 20 per. cent. of air would retard the effusion of hydrogen*. Now the mixture in question permeates the graphite plate in 527·5 seconds, while the calculated mean of the times of the two gases is 562·1 seconds.

The mixture has therefore passed neither in the effusion time, nor in a longer time as it would do by capillary transpiration, but, singular to say, in a time considerably shorter than either. The gas that came through was found by analysis to be *altered in composition.* It contained more hydrogen and less air than the original mixture. Hence it passed through with increased rapidity. On consideration it appeared that such a separation of the mixed gases must follow as a consequence of the movement being molecular. Each gas is impelled by its own peculiar molecular force, which, as has been seen, is capable of causing hydrogen to permeate the graphite plate about 3·8 times as rapidly as air.

Each gas may permeate a graphite plate into a vacuum with the same relative velocity as it diffuses into another gaseous atmosphere, but it remains a question whether the velocities of permeation and diffusion are absolutely as well as relatively the same. To illustrate this point, hydrogen and air were first allowed to permeate into a vacuum, and then to diffuse into each other, through the same graphite plate, which was 1 millim. in thickness. The plate was a circular disc of 22 millims. in diameter.

The mercurial column in the barometrical diffusiometer fell

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* Philosophical Transactions, 1846, p. 628.

from 762 to 685 millims. (30 inches to 27) with air in 878 seconds, and with hydrogen in 233 seconds.

$$\begin{array}{ll} \text{Air} \ldots\ldots & 878 \\ \text{Hydrogen} \ldots & 233 \end{array} \quad \frac{878}{233}=3{\cdot}768.$$

The volume of gas which produced this effect was found by the calibration of the tube to be 8·85 cub. centims. Hence 1·22 cub. centim. of the hydrogen entered the diffusiometer in 60 seconds, or one minute. But the pressure under which the hydrogen gas entered was the mean of 762 to 685 millims., or 723·5 millims.; while a whole atmosphere (the height of the barometer at the time) was 765 millims. The volume of the gas has therefore to be increased as 723·5 to 765 to give the full action of a vacuum. The volume becomes 1·289 cub. centim. in one minute.

When the diffusiometer was filled with hydrogen and the gas allowed to diffuse into air, the rise of the mercury was pretty uniform for the first five minutes, being 15·5 millim. divisions in the first two minutes, 7 in the third minute, 7·5 in the fourth minute, and 7 in the fifth minute, making 37 divisions in five minutes. But as in diffusion 1 air may be supposed to enter the tube for 3·8 hydrogen which escape, the hydrogen which diffused was more than 37 divisions, by $\frac{1}{3{\cdot}8}$, that is, by about 10 divisions. Hence 47 divisions of hydrogen have diffused into air in five minutes. These divisions measured, by the calibration of the tube, 6·215 cub. centims. One-fifth of this amount, that is, 1·243 cub. centim., diffused in one minute. The result of the whole is that in one minute there passed of hydrogen through the graphite plate,

1·289 cub. centim. by permeation into a vacuum,
1·243 cub. centim. by diffusion into air.

The numbers indicate a close approach to equality in the velocities of permeation into a vacuum and of diffusion into another gas, through the same porous diaphragm. The diffusion appears the slower of the two by a small amount; but this is as it should be, our estimate of the diffusion-velocity being certainly underrated; for the initial diffusion, or even the diffusion in the first minute, must obviously be somewhat greater than the average of the first five minutes, which we have taken to represent it—the hydrogen necessarily diffusing out in a diminishing progression, or more slowly in proportion as air has entered the diffusiometer. It is strictly the initial velocity of diffusion (that of the first second if it could be obtained) that ought to be compared with the percolation into a vacuum.

In fine, there can be little doubt left on the mind that the permeation through the graphite plate into a vacuum and the diffusion into a gaseous atmosphere, through the same plate, are due to the same inherent mobility of the gaseous molecule. They are the exhibition of this movement in different circumstances. In interdiffusion we have two gases moved simultaneously through the passages in opposite directions, each gas under the influence of its own inherent force; while with gas on one side of the plate and a vacuum on the other side, we have a single gas moving in one direction only. The latter case may be assimilated to the former if the vacuum be supposed to represent an infinitely light gas. It will not involve any error, therefore, to speak of both movements as gaseous diffusion,—the diffusion of gas into gas (double diffusion) in one case, and the diffusion of gas into a vacuum (single diffusion) in the other. The inherent molecular mobility may also be justly spoken of as the diffusibility or diffusive force of gases.

The diffusive mobility of the gaseous molecule is a property of matter fundamental in its nature, and the source of many others. The rate of diffusibility of any gas has been said to be regulated by its specific gravity, the velocity of diffusion having been observed to vary inversely as the square root of the density of the gas. This is true, but not in the sense of the diffusibility being determined or *caused* by specific gravity. The physical basis is the molecular mobility. The degree of motion which the molecule possesses regulates the volume which the gas assumes, and is obviously one, if not the only, determining cause of the peculiar specific gravity which the gas enjoys. If it were possible to increase in a permanent manner the molecular motion of a gas, its specific gravity would be altered, and it would become a lighter gas. With the density is also associated the equivalent weight of a gaseous element, according to the doctrine of equal combining volumes.

Diffusion of mixed gases into a vacuum, with partial separation—Atmolysis.

Oxygen and Hydrogen.—A diffusiometer of the same construction as that described (fig. 3, p. 414), with a graphite plate of 1 millim. in thickness, was now employed. The upper surface of the plate was swept by a current of the mixed gas proceeding from a gas-holder, the excess of gas being allowed to escape into the atmosphere, as usual, by an open exit-tube. The gas was drawn through the graphite by elevating the diffusiometer containing a column of mercury, from its well, so as to command a partial vacuum in the upper part of the tube. Care is taken that any gas, left in the upper part of the diffusiometer-tube before the experiment begins, should be of the same composition as the gas to be allowed afterwards to enter, so that, on starting, the gas may be uniform in composition on both sides of the graphite plate. The height of the mercurial column, which measures the aspirating force of the diffusiometer, is preserved uniform by gradually raising the tube in the mercurial trough in proportion as gas enters and the mercury falls. The diffusiometer is suspended from the roof of the apartment by a cord passing over a pulley and properly weighted, as in former experiments.

The mixture to be diffused consisted of nearly equal volumes of oxygen and hydrogen. The effect of different degrees of pressure on the amount of separation produced was first observed. It will be seen that as the pressure or aspirating force is increased the amount of separation becomes greater. Barom. 0·759 millim.; therm. 18°·3 C.

Diffusion into a partial vacuum.

	Oxygen.	Hydrogen.
Composition of original mixture in 100 parts.	49·3	50·7
Diffused by pressure of 100 millims.	47·0	53
Diffused by pressure of 400 millims.	37·5	62·5
Diffused by pressure of 673 millims. (mean of 635–710)	26·4	73·6
Diffused by pressure of 747 millims. (mean of 736–759)	22·8	77·2

In the last observation, or that with the greatest pressure (747 millims.), the oxygen is reduced to 22·8 per cent. and the hydrogen increased to 77·2 per cent. of the diffused mixture, showing a considerable separation. The mixed gases appear to make their way through the graphite plate independently, each following its own peculiar rate of diffusion.

But it is only under the aspiration of a complete vacuum that the separation can attain its maximum, and reach the full difference that may exist between the special diffusibilities of the two gases. The reason is that while we have the original mixture on both sides of the plate, and of equal tension, the gases are not at rest, but diffusion is proceeding as actively through the plate, in opposite directions, as if the gases were different or the tension unequal on the two sides. This is a condition of the molecular mobility of gases (p. 411). The tension therefore being supposed to differ by 100 millims. only, as when the gas above the plate was of 759 millims. tension, and below of 659 millims. (in the first experiment of the last series), then 100 volumes only out of 759 of the mixture are subject to separation. But while these 100 volumes press through they are accompanied by 659 volumes of unchanged mixture. The latter 659 volumes are replaced by an equal bulk of unchanged mixture diffused from below, so that the volumes are not disturbed by this portion of the molecular interchange.

The amount of separation, then, attainable by transmitting a mixed gas through a porous diaphragm by pressure will be in proportion to the pressure—that is, to the inequality of tension on different sides of the diaphragm.

Oxygen and Nitrogen.—The separation of the gases of the atmosphere by transmission through the graphite plate has a peculiar interest.

In an experiment resembling those last described, atmospheric air was swept over the upper surface of a graphite plate having a thickness of 2 millims. The gas that penetrated into the vacuum contained, as was to be expected, the lighter and more diffusible constituent in excess. It gave by the pyrogallic acid and potash process of Liebig,

Oxygen	20
Nitrogen	80

This was an increase in the nitrogen of quite 1 per cent.; for air, analysed for comparison at the same time and in the same manner, gave oxygen 21·03, and nitrogen 78·97.

It may be legitimately inferred from the last experiment, that if pure hydrogen in a diffusiometer were allowed to diffuse into the atmosphere through a porous plate, the portion of air which then enters the diffusiometer should also have its composition disturbed. A diffusion of hydrogen through a graphite plate was interrupted before completion. The air which had entered was found to consist of

Oxygen	19·77
Nitrogen	80·23
	100·00

The increase of nitrogen is 1·23 per cent.

While the nitrogen is increased and the oxygen diminished in the air which makes its way under pressure through the graphite, the converse effect must be produced on the air left behind. But the latter result of atmolysis cannot be made apparent without a change in the mode of experimenting.

With the view of effecting an increase in the proportion of oxygen, a volume of air, confined in a jar suspended over mercury, was allowed to communicate through a graphite plate of 2 millims. in thickness, with a vacuum sustained by means of an air-pump, the gauge being about 1 inch only below the height of the barometer during the whole time of experimenting.

The jar containing the air to be atmolysed was formed of a plain glass cylinder, open at both ends, and about 400 millims. in height (15·75 inches). The upper end was closed by a thick plate of gutta percha cemented on. This plate was itself penetrated by a wide glass tube, descending about an inch into the jar. The last tube carried the graphite disc, which was 27 millims. (1·04 inch) in diameter, sufficient to close the lower end of the tube upon which it was cemented. The other or upper end of the same tube was fitted with a cork and quill tube, and was put into communication with a large bell jar upon the plate of the air-pump.

The permeation was slow, owing to the unusual thickness of the graphite plate, occupying three hours to drain away one-half of the original volume of air in the jar. The air remaining behind in the jar was examined in a series of experiments, in which the original volume was reduced to one-half, one-fourth, one-eighth, and one-sixteenth.

The residual air, reduced to one-half, gave in two experiments 21·4 and 21·57 per cent. of oxygen, the air of the atmosphere being by the same analytical process 21 per cent.

Reduced to one-fourth of its volume, the residual air gave, in two experiments, 21·95 and 22·01 per cent. of oxygen.

Reduced to one-eighth of its volume, the air gave 22·54 per cent. of oxygen.

Reduced to one-sixteenth of its volume, the air gave 23·02 per cent. of oxygen. The proportion of oxygen had therefore increased about one-tenth in the last experiment, where the effect is greatest.

When the numbers are compared, it appears that by a reduction to half its volume the air gains about one-half per cent. of oxygen; when this last air is reduced to one-half again, another half per cent. of oxygen is gained, and so on—the gain in the proportion of oxygen increasing in an arithmetical ratio, while the volume of air is diminished in a geometrical ratio, or as the powers of the number 2.

Reduction of 1 volume of air.	Proportion of oxygen per cent.	Increase of oxygen.
To 1 volume	21	0
To 0·5 volume	21·48	0·48
To 0·25 volume	21·98	0·98
To 0·125 volume	22·54	1·54
To 0·0625 volume	23·02	2·02

The densities of oxygen and nitrogen approach too nearly to admit of any considerable separation being effected by this method. The density of oxygen being taken as 1, that of nitrogen is 0·8785. The square roots of these numbers are 1 and

0·9373, which are inversely as the diffusive velocity of the two gases.

	Diffusive velocity.
Oxygen	1
Nitrogen	1·0669

The velocity of nitrogen therefore exceeds that of oxygen by about 6·7 per cent. Hence by a simple diffusion of a whole volume of air the oxygen could only be increased 6·7 per cent., according to theory. In experiments such as the preceding, only one-half of the volume of the air is diffused, and consequently only one-half of the stated amount of concentration of oxygen could possibly be produced at each step. About three-fourths of the theoretical separation is actually obtained, although the apparatus works at an obvious disadvantage from the air within the jar being at rest.

This diffusive method of separation recalls the original observation of Döbereiner on the escape of hydrogen gas from a fissured jar standing over water, which will always hold its place in scientific history as the starting-point of the experimental study of gaseous diffusion. That observation proved to be an instance of double diffusion, air entering the jar by the fissure at the same time that hydrogen escaped by it—although, as Döbereiner looked upon the phenomenon, it was more akin to single diffusion or the passage of gas in one direction only*.

The atmolytic power of other diffusing-plates was tested, besides the artificial graphite.

The barometrical diffusiometer already described was closed by a plate of *red unglazed earthenware,* 4 millims. in thickness, which was attached to the glass by resinous cement.

Dry air was swept over the upper surface, as in operating with the graphite plate. With a mercurial column of 340 millims. falling to 200 millims., the air which entered was found to contain 79·45 per cent. of nitrogen, instead of 79. With a column of mercury, maintained at 508 millims. in the tube, the air entering contained 79·72 nitrogen, and with a column beginning at 761 millims., the full barometrical height, and falling to 679 millims. in seven minutes, the air entering contained 80·21 nitrogen. This is a full degree of separation, exceeding 1 per cent., while the time was greatly shorter than with graphite. Thermometer 19°·5 C.

With a diffusing-plate of gypsum (stucco) 10 millims. in thickness, the proportion of nitrogen was also increased, although less considerably than with biscuitware. The standard pro-

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* *Annales de Chimie,* 1825.

portion of nitrogen observed in atmospheric air being 78·99 per cent., the air drawn into the diffusiometer was as follows:—

	Proportion of nitrogen per cent.
In air entering over column of 330–200 millims. mercury	79·26
In air entering over column 508 millims.	79·32
In air entering over column 761–685 millims	79·53
In air entering over column 761–685 millims	79·69

The separation is sufficiently decided, and is certainly remarkable considering the comparatively loose texture of the stucco plate. The gas entered in the two last experiments in about one minute, which appears too rapid a passage, and not to be attended with increased separation, compared with the immediately preceding experiment, in which the pressure was less and the passage of the gas proportionally slower. In all such highly porous plates, we have always to apprehend the passage of a large proportion of the gas in the manner of capillary transpiration, where no separation takes place.

It may be concluded that all porous masses, however loose their texture, will have some effect in separating mixed gases moving through them under pressure. The air entering a room by percolating through a wall of brick or a coat of plaster will thus become richer in nitrogen, in a certain small measure, than the external atmosphere.

The Tube Atmolyser.

In the application of diffusion through a porous septum to separate mixed gases, as a practical analytical method, it is desirable that the process should be more rapid than it can be made with the use of graphite and other diffusing-plates of small size, and also that the process should if possible be a continuous one. Both objects are attained in a considerable degree by adapting a tube of porous earthenware to the purpose. Nothing has been found to answer better than the long stalk of a Dutch tobacco-pipe used as the porous tube. A tube of this description, about 2 feet long and having an internal diameter of 2·5 millims., is fixed by means of perforated corks within a glass or metallic tube, a few inches less in length and about $1\frac{1}{2}$ inch in diameter (*e*, *i*, fig. 8), as in the construction of a Liebig condenser. A second quill tube (*v*) is inserted in one of the end corks, and affords the means of communication between the annular space and the vacuum of an air-pump. The external surface of the corks, and of those portions of the pipe-stalk which project beyond the enclosing tube, should be coated with a resinous varnish, to render them impermeable to air. Now, a good vacuum being obtained within the outer tube, and sustained by the action of an air-pump, the mixed gas is made to enter and traverse the clay tube. More or less of gas is drained off through the porous walls and pumped away, while a portion courses on and escapes by the other extremity of the clay tube, where it may be collected. The stream of gas diminishes as it proceeds, like a river flowing over a pervious bed. The lighter and more diffusive constituent of the mixed gases is drawn most largely into the vacuum, leaving the denser constituent, in a more concentrated condition, to escape by the exit end of the clay tube. The more slowly the mixed gas is moved through that tube, the larger the proportion of light gas that is drained off into the vacuum, and the more concentrated does the heavy gas become. The rate of flow of the mixed gas can be commanded by either discharging it from a gas-holder, or drawing it into a gas-receiver, in either case by a regulated pressure.

To observe the effect of a more or less rapid passage through the tube atmolyser, the impelling pressure was varied so as to allow a constant volume of half a litre of atmospheric air to pass through and be collected in different periods of time. The clay tube used in these particular experiments was not a tobacco-pipe,

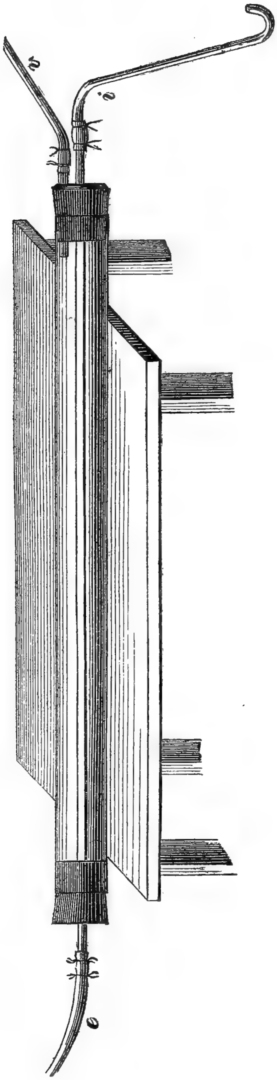

Fig. 8.

but a wide unglazed tube, about 431 millims. (17 inches) long and 19 millims. (0·75 inch) in internal diameter. It was required to place so wide a tube in a vertical position, and to admit the air by the upper and draw it off by the lower extremity of the tube. The proportion of oxygen in the half-litre of air collected was as follows:—

	Oxygen per cent.		
	Experiment 1.	Experiment 2.	Mean.
When collected in 1 minute......	21·00		
When collected in 13 minutes ...	22·33	22·25	22·29
When collected in 75 minutes ...	22·77	23·02	22·89
When collected in 120 minutes ...	23·25	23·22	23·23
When collected in 304 minutes ...	23·54	23·51	23·53

The proportion of oxygen in the air circulated appears thus to increase with the slowness of its passage through the tube atmolyser. The proportion of air drawn into the air-pump vacuum must be very large when the time is protracted; but the additional concentration of oxygen appears small.

The preceding observations being made by means of a porous tube which may be considered wide and of considerable capacity with reference to its internal surface, the experiment was varied by substituting a porous tube about eight times as long, very narrow, and therefore of small internal capacity. This second atmolyser was composed of twelve ordinary tobacco-pipe stems, each about 10 inches in length and of 1·9 millim. internal diameter, connected together by vulcanized caoutchouc adapters so as to form a single tube. Having flexible joints, the tube was folded up and placed within a glass cylinder that could be exhausted. Air was then circulated through this atmolyser by the pressure of several inches of water. The instrument appeared to work with most advantage when the air delivered at the exit-tube amounted to about one-fourth of a litre per hour. A volume of 268 cubic centimetres, which had circulated in one hour, was found to contain 24·37 per cent. of oxygen. The current was then made slower, so that only 108 cub. centims. of gas passed and were collected in one hour, but with little further concentration of the oxygen. The result, however, is interesting as being the highest concentration of oxygen yet obtained by an instrument of this kind. The air collected was composed of

Oxygen	24·52
Nitrogen	75·48
	100·00

The increase of oxygen is 3·5 per cent.; that is, an increase of 16·7 upon 100 oxygen originally present in the air.

With the single pipe-stalk, 24 inches long, first described, the oxygen of atmospheric air was concentrated about 2 per cent. when one litre was transmitted in one hour. Of 450 cub. centims. of air collected in that time, the composition proved to be

Oxygen	23·12
Nitrogen	76·88
	100·00

About 9 litres were drawn into the vacuum at the same time.

The separation of the gases of atmospheric air is a severe trial of the powers of the atmolyser, owing to the small difference in the specific gravities of these gases. But where a great disparity in density exists, the extent of the separation may become very considerable.

Several experiments were made upon a mixture of equal volumes of oxygen and hydrogen carried through the single tube atmolyser, 24 inches in length.

1. Of the mixture described, 7·5 litres entered the tube and 0·45 litre was collected in one experiment. The mixture was composed as follows:—

	Oxygen.		Hydrogen.
Before traversing the atmolyser .	50	+	50
After traversing the atmolyser . .	92·78	+	7·22

2. In another similar experiment, 14 litres of the mixed gas entered the tube and 0·45 litre was delivered in a period of two hours. The result was—

	Oxygen.		Hydrogen.
Before traversing the atmolyser .	50	+	50
After traversing the atmolyser .	95	+	5

Here the proportion of hydrogen is reduced from 50 to 5 per cent.

3. Of the explosive mixture, consisting of 1 volume oxygen and 2 volumes hydrogen, 9 litres were transmitted and 0·45 litre collected in one hour. The change effected was found to be as follows:—

	Oxygen.		Hydrogen.
Before traversing the atmolyser .	33·33	+	66·66
After traversing the atmolyser . .	90·7	+	9·3

The result in such experiments is striking, as the gas ceases to be explosive after traversing the porous tube, and a lighted taper burns in it as in pure oxygen. A mixture of oxygen and hydrogen is not explosive till the hydrogen rises to 11 per cent.

To illustrate the analogy of diffusion into a vacuum with diffusion into air, the outer glass tube of the diffuser was now withdrawn, and the porous tube of the instrument was exposed directly to the air of the atmosphere. A mixture of equal volumes of oxygen and hydrogen was again transmitted at the same rate of velocity as in experiment 1.

The gas atmolysed and collected was found to consist of

Oxygen	51·75
Hydrogen	5·47
Nitrogen	42·78
	100·00

And may be represented as containing

Oxygen	40·38
Hydrogen	5·47
Air	54·15
	100·00

A nearly similar concentration of the oxygen of the mixed gas is here observed as appeared in experiment 1; but the gas collected is now diluted with air which has entered by diffusion. The external air manifestly discharges the same function in the latter experiment which the air-pump vacuum discharged in the former experiment.

Interdiffusion of Gases—double diffusion.

The diffusiometer was much improved in construction by Professor Bunsen, from the application of a lever arrangement to raise and depress the tube in the mercurial trough; but the mass of stucco forming the porous plate in his instrument appears too voluminous, and, from being dried by heat, is liable to detach itself from the walls of the glass tube. The result obtained of 3·4 for hydrogen, which diverges so far from the theoretical number, is, however, no longer insisted upon by that illustrious physicist. It is indeed curious that my old experiments generally rather exceeded than fell short of the theoretical number for hydrogen, $\sqrt{0·06926} = 3·7994$. With stucco as the material, the cavities existing in the porous plate form about one-fourth of its whole bulk, and affect sensibly the ratio in question according as they are or are not included in the capacity of the instrument. Beginning the diffusion always with these cavities, as well as the tube, filled with hydrogen, the numbers now obtained with a stucco plate of 12 millims. in thickness and dried without heat, were 3·783, 3·8, and 3·739 when the volume of the cavities of the stucco is added to both the air and hydrogen volumes diffused; and 3·931, 3·949, and 3·883 when such addition is not made to these volumes. The graphite plate, on the other hand, being very thin, and the volume of its pores too minute to require to be taken into account, its action is not attended with the same uncertainty. With a graphite plate of 2 millims. in thickness, the number for hydrogen into air was 3·876 instead of 3·8; and for hydrogen into oxygen 4·124 instead of 4. With a graphite plate of 1 millim. in thickness, hydrogen gave 3·993 to air 1. With a plate of the same material 0·5 millim. in thickness, the proportional number for hydrogen to air rose to 3·984, 4·068, and 4·067. An equally considerable departure from the theoretical number was observed when hydrogen was diffused into oxygen or into carbonic acid, instead of air. All these experiments were made with dry gases and over mercury. It appears that the numbers are most in accordance with theory when the graphite plate is thick, and the diffusion slow in consequence. If the diffusion be very rapid, as it is with the thin plates, something like a current is possibly formed within the channels of the graphite, taking the direction of the hydrogen and carrying back in masses a little air, or the slower gas, whatever it may be. I cannot account otherwise for the slight predominance which the lighter and faster gas appears always to acquire in diffusing through the porous septum.

Interdiffusion of Gases without an intervening septum.

The relative velocity with which different gases diffuse is shown by the diffusiometer, but the absolute velocity of the molecular movement cannot be ascertained by the same instrument. For that purpose it appears requisite that a gas should be allowed to diffuse into air through a wide opening.

In certain recent experiments, a heavy gas, such as carbonic acid, was allowed to rise by diffusion into a cylindrical column of air, pretty much as a saline solution is allowed to rise into a column of water in my late experiments upon the diffusion of liquids. This method of gaseous diffusion appears to admit of considerable precision, and deserves to be pursued further. A glass cylinder of 0·57 metre (22·44 inches) in height had the lower tenth part of its volume occupied with carbonic acid, and the upper nine-tenths with air, in a succession of experiments: thermometer 16° Cent. After the lapse of a certain number of minutes, the upper tenth part of the volume was drawn off from the top of the jar and examined for carbonic acid. Before the carbonic acid appeared above, it had ascended (that is, it had diffused) a distance of 0·513 metre, or rather more than half a metre. After the lapse of 5 minutes, the carbonic acid so found in two experiments amounted to 0·4 and 0·32 per cent. respectively. In 7 minutes the carbonic acid observed was 1·02 and 0·90 per cent.; mean 0·96 per cent. The effect of diffusion is now quite sensible, and it may be said that about 1 per cent. of carbonic acid has diffused to a distance of half a metre in seven minutes.

A portion of carbonic acid has therefore travelled by diffusion at an average rate of 73 millims. per minute. It may be added that hydrogen was found to diffuse downwards, in air contained in the same cylindrical jar, at the rate of 350 millims. per minute, or about five times as rapidly as the carbonic acid ascended. In these experiments the glass cylinder was loosely packed with cotton wool, to impede the action of currents in the column of air; but this precaution was found to be unnecessary, as similar results were afterwards obtained in the absence of the cotton. To illustrate the regularity of the results, I may complete this statement by exhibiting the proportion of carbonic acid found in the upper stratum already referred to, after the lapse of different periods of time.

	Carbonic acid per cent.		
	Experiment 1.	Experiment 2.	Mean.
After 5 minutes	0·4	0·32	0·36
After 7 minutes	1·02	0·90	0·96
After 10 minutes	1·47	1·56	1·51
After 15 minutes	1·70	1·68	1·69
After 20 minutes	2·41	2·69	2·55
After 40 minutes	5·60	5·15	5·37
After 80 minutes	8·68	8·82	8·75

In eighty minutes the proportion of carbonic acid had risen to 8·75 per cent., 10 per cent. being the proportion which would indicate the completion of the process of diffusion.

The same intestine movement must always prevail in the air of the atmosphere, and with even greater velocity, in the proportion of 1 to 1·176, the relative diffusion-ratios of carbonic acid and air. It is certainly remarkable that in perfectly still air its molecules should spontaneously alter their position, and move to a distance of half a metre, in any direction, in the course of five or six minutes. The molecules of hydrogen gas disperse themselves to the distance of a third of a metre in a single minute. Such a molecular movement may become an agency of considerable power in distributing heat through a volume of gas. It appears to account for the high convective power observed in hydrogen, the most diffusive of gases.

LXI. *On Thermo-electrical Currents from the Condensation of Vapour, and the Evaporation of Water.* By JOHN MICKLE*.

IF we take 12 inches each of iron and copper wire (No. 20 is what I have used) and twist them together at one end, we have a thermo-pair giving very good deflections when the loose ends are attached to a sensitive galvanometer, and the twisted end applied to any source of heat. But by uniting a series of such pairs, in lengths of 6 inches each, a very cheap and useful pile can be constructed capable of appreciating radiant heat from the hand at a distance of 6 inches, and from a luminous source, such as a jet of gas, at a distance of 12 inches.

I was working with a wire pile of this description, consisting of 40 pairs, endeavouring to obtain minus deflections by blowing upon it through a glass tube 24 inches in length and $\frac{1}{4}$ inch in diameter, but found the deviations produced indicating plus instead of minus—due to heat instead of cold. Knowing that air from the lungs contains much moisture, it appeared to me probable that the deflections proceeded from the heat consequent upon the condensation of the vapour upon the wires of the pile. To test the truth of this conjecture, a current of dry air was directed upon the face of the pile from a pair of bellows, but without result. The friction of the air upon the wires of the pile could therefore have nothing to do with the production of the current. Since dry air did not seem to be concerned in the result, my next step was to try vapour. With this object in view, I heated some water in a glass retort and allowed the steam to escape freely from the mouth of the vessel. On bringing one face of the pile into contact with the ascending column of vapour, at a distance of 12 inches from the orifice of the retort, a powerful deflection of the needle of the galvanometer instantly manifested itself. There remained yet another consideration. Would not a voltaic effect be produced by the oxidation of the iron in contact with copper in an atmosphere of steam? To satisfy this objection, the experiment was repeated with copper and platinum wires; but the results differed only in the direction and intensity of the current—the former being contrary, and the latter weaker than with iron and copper wires.

Assuming that the previous experiments justify the conclusion that the condensation of vapour originates a positive current, then obviously a negative current ought to result from evaporation. To determine whether this deduction was accordant with fact, the following arrangement was adopted. Taking only a single pair of the iron and copper wires twisted together

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* Communicated by the Author.

at one end, as previously described, the twisted part having a length of 4 inches, I united the loose ends to the electrodes of the galvanometer; and over the twisted portion of the wires forming the pile I doubled a piece of tinfoil 4 inches square. On touching a part of the foil with the finger, the needle exhibited a deviation in the same direction as in the previous experiments in which the effect was attributed to the heat liberated by condensation; and on wetting the foil with a sponge having the water squeezed out of it by the hand, a deflection in the same direction occurred; but as soon as the heat communicated to the water by the hand began to be carried off by evaporation of the fluid from the foil, a contrary deviation was established, lasting as long as any moisture remained upon the foil. If the wet foil was exposed to the sun's rays when these were not too intense, a negative deflection was maintained until evaporation ceased, when the needle experienced a contrary deviation due to direct solar action.

Since obtaining these results, I have tried to adapt the arrangement described to hygrometric purposes, by merely suspending a strip of cotton over the tinfoil with its ends immersed in a cup of water. The evaporation which then takes place from the surface of the cotton of course varies with the dryness or humidity of the atmosphere; but as these latter causes accelerate or retard evaporation from the cotton cloth, so also is a greater or lesser quantity of heat abstracted from the wires, and a corresponding variation in the deflection produced. The readings of a wet- and dry-bulb thermometer have been compared with those of the above arrangement (which may be designated a thermo-electrical hygrometer), and their general correspondence observed; but the details of these experiments, and of some other peculiar thermo-electrical phenomena, must be reserved for a subsequent communication.

Victoria Street, Newcastle-on-Tyne.

LXII. *On the Expansive Energy of Heated Water.*

By W. J. Macquorn Rankine, *C.E., LL.D., F.R.SS.L. & E.*[*]

[Supplement to a Paper published in the Philosophical Magazine for November 1863.]

FOR the sake of more easy comparison with the investigations of the Astronomer Royal and Professor Miller, the formulæ and Tables of this paper are here given in metrical measures.

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[*] Communicated by the Author.

Energy, in kilogrammetres, exerted by each kilogramme of water heated to T° Centigrade and suddenly set free

$$=423{\cdot}55\left\{\mathrm{T}-100^\circ-374^\circ\ \mathrm{hyp.\,log}\frac{\mathrm{T}+274^\circ}{374^\circ}\right\}.$$

Final volume of expansion of mixed water and steam, in cubic metres per kilogramme

$$=1{\cdot}145\ \mathrm{hyp.\,log}\frac{\mathrm{T}+274^\circ}{374^\circ}.$$

TABLE.

Initial temperature. Centigrade.	Initial absolute pressure. Atmospheres.	Energy. Kilogrammetres.	Velocity of projection. Metres per second.	Final expansion. Cubic metres.	Fraction remaining liquid. Kilogrammes.	Energy of liquid portion. Kilogrammetres.
100°	1·00	0	0	0	1·000	0
120	1·96	221	65	0·059	0·964	213
140	3·57	847	129	0·117	0·931	789
160	6·11	1845	190	0·171	0·897	1655
180	9·92	3177	250	0·222	0·865	2748
200	15·37	4824	309	0·272	0·835	4028
220	22·88	6753	364	0·319	0·806	5443
......	......	......	......	......	......	......
about 1293	unknown	about 278130	about 2337	1·645	0	0

Approximate formulæ for temperatures not exceeding 220° C.

$$\text{Energy, nearly} = \frac{423{\cdot}55(\mathrm{T}-100^\circ)^2}{\mathrm{T}+648^\circ}.$$

$$\text{Expansion, nearly} = \frac{2{\cdot}29(\mathrm{T}-100^\circ)}{\mathrm{T}+648^\circ}.$$

Glasgow, November 4, 1863.

LXIII. *On the Spheroidal Condition of Liquids at Common Temperatures.* By CHARLES TOMLINSON, *Lecturer on Physical Science, King's College School, London*[*].

IN my "Experiments on Films" (Phil. Mag. August 1861), and "On the Cohesion-Figures of Liquids" (Phil. Mag. October 1861 and March 1862), I had frequent occasion to notice the spheroidal condition of liquids at ordinary tempera-

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[*] Communicated by the Author.

tures. For example, if a single drop of a liquid, such as an essential oil, be gently deposited from the end of a glass rod on the surface of perfectly clean water in a chemically clean glass, it forms a characteristic film or cohesion-figure; but if a second drop be gently delivered to the surface of the film, it will rest or roll about upon it, without contact, in the spheroidal form.

In a note communicated by M. Meunier to the Academy of Sciences at Paris (*Comptes Rendus,* August 17, 1863), the results of some experiments are given on the globular form that liquids may assume on surfaces of their own kind. M. Meunier's mode of manipulating is to take up alcohol, for example, in a pipette and drop it upon the surface of alcohol. The globules formed last but a very short time; they roll about on the surface without being in contact, as was ingeniously proved by dropping alcohol on tincture of iodine, when the globules during their brief existence did not become coloured. Another method was to dip a glass rod into the liquid so as to take up a sufficient quantity to deposit a drop on its surface. In this way all liquids formed globules with more or less difficulty; but there was no difficulty if the liquid were first covered with a layer of another liquid not miscible with it. M. Meunier's conclusion is that all liquids possess the property of passing into the spheroidal state.

M. Meunier does not seem to have pursued the subject further; had he done so, he would probably have noticed other circumstances which seem to me to deserve attention, if only as further illustrations of that wonderful property of cohesion in liquids, of which the spheroidal state is an example. If, for example, the experiment be made with essential oils, and the duration of the liquid in the spheroidal state be noted, we may, I think, obtain a rough approximative method of determining the age of an essential oil; for I have found that the fresher the oil the greater is its cohesive force as expressed by its duration in the spheroidal form at common temperatures.

For example, oil of turpentine, as purchased at an oil and colour shop, was examined in the following manner. Three glass capsules, No. 1, No. 2, and No. 3, each about 2½ inches in diameter, were nearly filled with water and placed in the mouths of test-glasses before the light. A drop of the turpentine was deposited on the surface of each from the end of a glass rod; the drop in each case flashed out into a film which nearly covered the surface of the water. A second drop of turpentine was gently delivered to the surface of the film No. 1, when it formed a spheroid, the duration of which was measured in seconds. When the spheroid vanished, a similar one was formed on the film in No. 2, and its duration measured; and so for No. 3. Eight observations were taken on each film.

Capsule.	Obs. 1.	2.	3.	4.	5.	6.	7.	8.	Sum.
No. 1 . . .	17″	16″	8″	15″	17″	16″	16″	21″	= 126
No. 2 . . .	17	14	16	10	11	17	18	18	= 121
No. 3 . . .	11	11	19	19	12	10	17	18	= 117
									24)364
					Mean				$15\frac{1}{6}$

The mean duration of a spheroid of common turpentine in this series of observations was $15\frac{1}{6}$ seconds. This turpentine was distilled off caustic potash, and a similar series of observations immediately made with the fresh distillate. The superior cohesive force of the distillate over that of the common turpentine was shown at once by the greatly diminished proportions of the film. The film of the common turpentine was nearly $2\frac{1}{2}$ inches in diameter; that of the distillate was at first only a flattened drop, which gradually increased in size, by the accretion of other drops, to about the diameter of a florin or half-a-crown. On going through a similar series of observations with these films, the duration of the spheroids was as follows:—

Capsule.	Obs. 1.	2.	3.	4.	5.	6.	7.	8.	Sum.
No. 1 . . .	35″	33″	44″	45″	33″	10″	40″	40″	= 280
No. 2 . . .	34	41	35	38	35	37	37	33	= 290
No. 3 . . .	42	43	40	44	34	31	33	33	= 300
									24)870
					Mean				$36\frac{1}{4}$

So that the mean duration of a globule of the distillate compared with that of the common turpentine is as about 7 to 3. Both the above series of experiments were made on the same morning; the weather was fine; the reading of the dry-bulb thermometer gave $62\frac{1}{2}$°, and that of the wet-bulb $55\frac{1}{2}$°. If the spheroidal condition of liquids depends on the existence of a layer of vapour which prevents contact between the globule and the surface beneath, it is quite intelligible why the experiment in this form should succeed better when the evaporative force is strong than when it is weak. Indeed in damp or wet weather the duration of the globules is much diminished; or it may be difficult to form them at all, even in a warm room.

That there is no contact between the globule and the film may be proved in several ways:—(1) There is a slight depression in the film just below the globule; and this depression accompanies the globule when it rolls about, which it sometimes does. (2) The drop may fall with some force from the end of the glass rod, as from the height of an inch, and roll about without being absorbed.

(3) The glass rod, with the drop hanging from it, may be lowered upon the film several times so as to flatten the drop, without its being absorbed. In some cases, however, there is contact between the spheroid and the surface beneath at a single point; for in rolling about, the spheroid will sometimes leave a trail of liquid, and so diminish in size until it suddenly vanishes by being absorbed. In all cases the drop vanishes suddenly, apparently by flattening at its under surface, and thus coming into contact with the liquid film below; but at the moment of disappearing a small globule is sometimes shot up from below, and this continues on the surface for a short time.

The spheroid, whether rolling about or stationary, displays by the rapid motions of its particles the struggle that is going on between gravity and cohesion. If a minute portion of lycopodium powder be shaken over the globule, and this rude treatment does not cause it to break up, the powder will be seen streaming rapidly down the outer surface of the spheroid, and ascending by the central vertical axis to separate again at the top, and stream over the surface again to the bottom—presenting, in fact, a figure something like that given in the books to represent the boiling of water.

The longer duration of a spheroid of the fresh or superior over that of the common oil has been verified in a number of cases. For example, a common oil of bergamot had a duration of about 5″, while that sold to me as a very choice oil (marked *super*) lasted from 21″ to 27″. Rosemary lasted 7″; the distillate 13″; origanum 4″ to 5″; the distillate 10″ to 11″. Several paraffine oils from different makers showed great variations in their cohesive force.

There is one curious circumstance that must be noted, and that is that a drop of one liquid will not form a spheroid on a film of another liquid at ordinary temperatures; or it does so with difficulty, and the duration is very short. This remark applies to the paraffine oils of different makers, and even to oils from plants of the same genus. Thus the oil of *Eucalyptus amygdalina* will not assume the spheroidal condition on a film of *E. oleosa*, or on one of *E. goniocalyx*. A drop of origanum is immediately absorbed on a film of rosemary; as is lavender or turpentine on origanum; but after these failures, a drop of the same kind of liquid as that of the film will form a spheroid without much difficulty, other circumstances being favourable.

In no case was I able by my method of experimenting to produce a spheroid of a fatty oil on a film of fatty oil at common temperatures. No sooner, however, was heat introduced, than the difficulty ceased. A very clear limpid sample of a fish-oil was raised to 320°, and spheres, not spheroids, of fish and other oils, and of water, alcohol, ether, &c., were readily formed, and rolled about upon its surface with great rapidity.

In conclusion, I may perhaps be allowed to refer to a method of producing the spheroidal condition of liquids in the cold which was published by me, together with an illustrative woodcut, in a forgotten journal so long back as 1836. If a glass goblet be about three parts filled with spirits of wine, and a fiddle-bow be drawn across the edge, the usual crispating fans from the vibrating segments will be produced; but, in addition to this, the spirit at the surface will be thrown into globules which will be rolled upon the less agitated portions, and will mark the nodes in the form of a four-, or six-, or eight-rayed star very perfectly. These figures of Chladni may also be produced in globules with admirable sharpness by pouring turpentine on water at about 150° in a goblet, so as to form a thin layer.

King's College, London,
November 12, 1863.

LXIV. *On Differential Equations and Umbilici (Sequel to the Paper in the November Number, pp.* 373–379). *By* A. Cayley, *Sadlerian Professor of Pure Mathematics at Cambridge*[*].

IV.

The differential equation for the curves of curvature in the neighbourhood of an umbilicus was obtained in a form such as

$$(bx+cy)(p^2-1)+2(fx+gy)p=0;$$

and it was only because this equation did not appear to be readily integrable, that I considered, instead of it, the particular form

$$y(p^2-1)+2mxp=0.$$

But the general equation can be integrated; and the result presents itself in a simple form. For, returning to the differential equation

$$(bx+cy)(p^2-1)+2(fx+gy)p=0,$$

and assuming

$$\frac{bx+cy}{fx+gy}=\frac{-2v}{v^2-1},$$

or

$$(bx+cy)(v^2-1)+2(fx+gy)v=0,$$

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[*] Communicated by the Author.

we have

$$\frac{p^2-1}{v^2-1}=\frac{p}{v}, \text{ or } (p-v)(vp+1)=0,$$

and we may write

$$p-v=0.$$

Assuming also

$$y=ux, \text{ or } u=\frac{y}{x},$$

the relation between u and v is

$$\frac{b+cu}{f+gu}=\frac{-2v}{v^2-1};$$

or, as this may be written,

$$v^2-1+2\frac{f+gu}{b+cu}v=0,$$

giving

$$v=\frac{-(f+gu)-\sqrt{(b+cu)^2+(f+gu)^2}}{b+cu},$$

where for convenience the radical has been taken with a negative sign. We have moreover

$$u=-\frac{b(v^2-1)+2fv}{c(v^2-1)+2gv}.$$

The equation $p-v=0$, substituting for y its value ux, then becomes

$$x\frac{du}{dx}+u-v=0;$$

or, as this may be written,

$$\frac{dx}{x}+\frac{du}{u-v}=0;$$

or, what is the same thing,

$$\frac{dx}{x}+\frac{dv-du}{v-u}-\frac{dv}{v-u}=0.$$

But

$$v-u=v+\frac{b(v^2-1)+2fv}{c(v^2-1)+2gv}=\frac{\mathrm{V}}{c(v^2-1)+2gv},$$

where

$$\begin{aligned}\mathrm{V}&=v[c(v^2-1)+2gv]+b^2(v-1)+2fv\\&=(b+cv)(v^2-1)+2(f+gv)v,\end{aligned}$$

and the differential equation takes thus the form

$$\frac{dx}{x}+\frac{dv-du}{v-u}-\frac{[c(v^2-1)+2gv]dv}{\mathrm{V}}=0;$$

and hence, writing

$$V=(b+cv)(v^2-1)+2(f+gv)v=c(v-\alpha)(v-\beta)(v-\gamma),$$

and

$$\frac{c(v^2-1)+2gv}{V}=\frac{c(v^2-1)+2gv}{c(v-\alpha)(v-\beta)(v-\gamma)}=\frac{A}{v-\alpha}+\frac{B}{v-\beta}+\frac{C}{v-\gamma},$$

so that

$$A=\frac{c(\alpha^2-1)+2g\alpha}{c(\alpha^2-1)+2g\alpha+2\{f+(b+g)\alpha+c\alpha^2\}},$$

with the like values for B and C—values which are such that

$$A+B+C=1,$$

the integral equation is

$$\text{const.}=x(v-u)(v-\alpha)^{-A}(v-\beta)^{-B}(v-\gamma)^{-C},$$

or, substituting for $v-u$ its value $=\dfrac{c(v-\alpha)(v-\beta)(v-\gamma)}{c(v^2-1)+2gv}$,

$$\text{const.}=x\{c(v^2-1)+2gv\}^{-1}(v-\alpha)^{1-A}(v-\beta)^{1-B}(v-\gamma)^{1-C}.$$

But

$$v=\frac{-(f+gu)-\sqrt{U}}{b+cu};$$

if for shortness $U=(b+cu)^2+(f+gu)^2$, and thence

$$v^2=\frac{2(f+gu)^2+(b+cu)^2+2(f+gu)\sqrt{U}}{(b+cu)^2},$$

and

$$c(v^2-1)+2gv=\frac{2(cf-bg)(f+gu+\sqrt{U})}{(b+cu)^2},$$

$$v-\alpha=\frac{-(f+gu)-\sqrt{U}-\alpha(b+cu)}{b+cu},\ \&c.$$

Substituting these values, and observing that the exponent of $b+cu$ is $(-2+1-A+1-B+1-C=1-A-B-C)=0$, the integral equation is

$$\text{const.}=x(f+gu+\sqrt{U})^{-1}$$

$$\times\left(f+gu+\alpha(b+cu)+\sqrt{U}\right)^{1-A}\left(f+gu+\beta(b+cu)+\sqrt{U}\right)^{1-B}$$

$$\left(f+gu+\gamma(b+cu)+\sqrt{U}\right)^{1-C}.$$

Or, observing that the exponent 1 of x is

$$=-1+(1-A)+(1-B)+(1-C),$$

and putting for shortness $\square=(fx+gy)^2+(bx+cy)^2$, the integral equation finally is

$$\text{const.} = (fx+gy+\sqrt{\square})^{-1}$$
$$\times\big(fx+gy+\alpha(bx+cy)+\sqrt{\square}\big)^{1-\mathrm{A}}\big(fx+gy+\beta(bx+cy)+\sqrt{\square}\big)^{1-\mathrm{B}}$$
$$\big(fx+gy+\gamma(bx+cy)+\sqrt{\square}\big)^{1-\mathrm{C}},$$

where the quantities α, β, γ, A, B, C are given by

$$(b+cv)(v^2-1)+2(f+gv)=c(v-\alpha)(v-\beta)(v-\gamma),$$
$$\frac{c(v^2-1)+2gv}{c(v-\alpha)(v-\beta)(v-\gamma)}=\frac{\mathrm{A}}{v-\alpha}+\frac{\mathrm{B}}{v-\beta}+\frac{\mathrm{C}}{v-\gamma}.$$

Consider the curve

$$0=\big(fx+gy+\alpha(bx+cy)+\sqrt{\square}\big)^{1-\mathrm{A}}\big(fx+gy+\beta(bx+cy)+\sqrt{\square}\big)^{1-\mathrm{B}}$$
$$\big(fx+gy+\gamma(bx+cy)+\sqrt{\square}\big)^{1-\mathrm{C}},$$

which corresponds to the value $=0$ of the constant. If, for instance,

$$fx+gy+\alpha(bx+cy)+\sqrt{\square}=0,$$

this equation gives

$$(bx+cy)\{(bx+cy)(\alpha^2-1)+2(fx+gy)\alpha\}=0;$$

or say

$$(bx+cy)(\alpha^2-1)+2(fx+gy)\alpha=0.$$

But we have

$$(b+c\alpha)(\alpha^2-1)+2(f+g\alpha)\alpha=0,$$

and the equation therefore is

$$(bx+cy)(f+g\alpha)-(fx+gy)(b+c\alpha)=0;$$

that is,

$$(cf-bg)(y-\alpha x)=0;$$

or simply

$$y-\alpha x=0;$$

that is, the directions of the curve at the origin, or point $x=0$, $y=0$, are given by the equations $y-\alpha x=0$, $y-\beta x=0$, $y-\gamma x=0$. This is right, since from the differential equation we obtain at the origin

$$(b+cp)(p^2-1)+2(f+gp)p=c(p-\alpha)(p-\beta)(p-\gamma)=0.$$

V.

The particular case of the equation

$$y(p^2-1)+2mxp=0$$

is obtained from the general equation by writing therein

$$b=0,\quad c=1,\quad g=0,\quad f=m;$$

we have therefore

$$v(v^2+2m-1)=(v-\alpha)(v-\beta)(v-\gamma),$$

or say

$$\alpha=0,\quad \beta=i\sqrt{2m-1},\quad \gamma=-i\sqrt{2m-1};$$

and thence

$$\frac{v^2-1}{v(v^2+2m-1)}=-\frac{1}{2m-1}\frac{1}{v}+\frac{2m}{2m-1}\frac{v}{v^2+2m-1}$$
$$=\frac{\mathrm{A}}{v}+\frac{\mathrm{B}}{v+i\sqrt{2m-1}}+\frac{\mathrm{C}}{v-i\sqrt{2m-1}},$$

giving

$$\mathrm{A}=-\frac{1}{2m-1},\quad \mathrm{B}=\mathrm{C}=\frac{m}{2m-1}.$$

The integral equation thus is

$$\text{const.}=(mx+\sqrt{\square})^{-1}(mx+\sqrt{\square})^{\frac{2m}{2m-1}}$$
$$\{(mx+i\sqrt{2m-1}\,y+\sqrt{\square})(mx-i\sqrt{2m-1}\,y+\sqrt{\square})\}^{\frac{m-1}{2m-1}},$$

where $\square=m^2x^2+y^2$; or, observing that

$$(mx+i\sqrt{2m-1}\,y+\sqrt{\square})(mx-i\sqrt{2m-1}\,y+\sqrt{\square})$$
$$=(mx+\sqrt{\square})^2+y^2$$
$$=2m(mx^2+y^2+x\sqrt{\square}),$$

the integral equation is

$$\text{const.}=(mx+\sqrt{\square})^{\frac{1}{2m-1}}(mx^2+y^2+x\sqrt{\square})^{\frac{m-1}{2m-1}},$$

or, what is the same thing,

$$\text{const.}=(mx+\sqrt{\square})(mx^2+y^2+x\sqrt{\square})^{m-1},$$

the result given in the former part of the present paper.

VI.

I annex the following *à posteriori* verification of the solution

$$\text{const.}=(mx+\sqrt{\square})(mx^2+y^2+x\sqrt{\square})^{m-1}$$

of the particular equation

$$y(p^2-1)+2mxp=0.$$

Putting for shortness

$$\mathrm{A}=mx+\sqrt{\square},$$
$$\mathrm{B}=mx^2+y^2+x\sqrt{\square},$$

where it will be remembered that

$$\square = m^2x^2+y^2,$$

then we have

$$2mB = A^2+(2m-1)y^2.$$

The integral equation may be written

$$h = P+Q\sqrt{\square} = U = AB^{m-1}.$$

And we have

$$\frac{U'}{U} = \frac{A'}{A}+(m-1)\frac{B'}{B} = \frac{\Theta}{\sqrt{\square}},$$

if

$$\Theta = \sqrt{\square}\left\{\frac{A'}{A}+(m-1)\frac{B'}{B}\right\}.$$

Or, since

$$\begin{aligned} A'\sqrt{\square} &= m\sqrt{\square}+m^2x+yp = mA+yp, \\ B'\sqrt{\square} &= (2mx+2yp)\sqrt{\square}+\square+x(m^2x+yp) \\ &= 2m^2x^2+y^2+xyp+(2mx+2yp)\sqrt{\square} \\ &= A^2+pyx+2\sqrt{\square}, \end{aligned}$$

and

$$\frac{1}{B} = \frac{2m}{A^2+(2m-1)y^2},$$

this is

$$\begin{aligned} \Theta &= \frac{mA+yp}{A}+(2m^2-2m)\frac{A^2+py(x+2\sqrt{\square})}{A^2+(2m-1)y^2} \\ &= \frac{1}{A[A^2+(2m-1)y^2]}\{(mA+yp)[A^2+(2m-1)y^2] \\ &\qquad +(2m^2-2m)[A^3+Apy(x+2\sqrt{\square})]\}, \end{aligned}$$

where the expression in { } is

$$\begin{aligned} &= (2m^2-m)A(A^2+y^2) \\ &+ yp\{A^2+(2m-1)y^2+(2m^2-2m)A(x+2\sqrt{\square})\}. \end{aligned}$$

Here the coefficient of yp is $=(2m^2-m)(A^2+y^2)$; in fact we have identically

$$A^2+y^2-2A\sqrt{\square}=0,$$

and thence

$$(2m^2-3m+1)(A^2+y^2)-2(2m-1)(m-1)A\sqrt{\square}=0,$$

that is

$$(2m^2-m-1)A^2+(2m^2-3m+1)y^2-(2m-2)A\{A+(2m-1)\sqrt{\square}\}=0;$$

or

$$(2m^2-m-1)A^2+(2m^2-3m+1)y^2-(2m^2-2m)A(x+2\sqrt{\square})=0,$$

and therefore

$$A^2+(2m-1)y^2+(2m^2-2m)A(x+2\sqrt{\square})=(2m^2-m)(A^2+y^2).$$

Hence the term in $\{\ \}$ is

$$=(2m^2-m)(A^2+y^2)(A+yp);$$

or, what is the same thing, it is $=(4m^2-2m)A\sqrt{\square}(A+yp)$. Hence, restoring for $A^2+(2m-1)y^2$ its value $2mB$, we find

$$\Theta=\frac{(2m-1)\sqrt{\square}}{B}(A+yp),$$

or

$$\frac{U'}{U}=\frac{2m-1}{B}(A+yp).$$

But writing U_1, U_2 to denote the values corresponding to $+\sqrt{\square}$, $-\sqrt{\square}$ respectively, we have

$$U'_1=\frac{(2m-1)U_1}{B_1}(mx+yp+\sqrt{\square}),$$

$$U'_2=\frac{(2m-1)U_2}{B_2}(mx+yp-\sqrt{\square}),$$

and thence

$$U'_1U'_2=\frac{(2m-1)^2U_1U_2}{B_1B_2}\{(mx+yp)^2-\square\}$$

$$=\frac{(2m-1)^2U_1U_2}{B_1B_2}y\{y(p^2-1)+2mxp\}.$$

Or, since

$$U'=P'+Q'\sqrt{\square}+\frac{Q\square'}{2\sqrt{\square}}=\frac{1}{2\sqrt{\square}}(2Q'\square+Q\square'+2P'\sqrt{\square},$$

and thence

$$U'_1U'_2=-\frac{1}{4\square}\{(2Q'\square+Q\square')^2-4P'^2\square\},$$

and moreover

$$U_1U_2=P^2-Q^2\square=A_1A_2(B_1B_2)^{m-1},$$

where

$$A\ A_2=m^2x^2-\square=-y^2,$$

$$B_1B_2=(mx^2+y^2)^2-\square=y^2\{y^2+(2m-1)x^2\};$$

we have

$$-\frac{1}{4\square}\{(2Q'\square+Q\square')^2-4P'^2\square\}$$
$$=(2m-1)^2A_1A_2(B_1B_2)^{m-2}y^2\{y(p^2-1)+2mxp\}$$
$$=-(2m-1)^2y^{2m-1}[y^2+(2m-1)x^2]^{m-2}\{y(p^2-1)+2mxp\}.$$

So that, the derived equation being

$$Q^2\{(2Q'\square+Q\square')^2-4P'^2\square\}=0,$$

this is

$$Q^2\square y^{2m-1}\{y^2+(2m-1)x^2\}^{m-2}\{y(p^2-1)+2mxp\}=0.$$

Hence, besides the factor Q^2 corresponding to the nodal curve, and the factor $\square$ corresponding to the cuspidal curve, we have the factors y^{2m-1} and $\{y^2+(2m-1)x^2\}^{m-2}$; and, rejecting all these, the differential equation in its reduced form is

$$y(p^2-1)+2mxp=0;$$

and the required verification is effected. The occurrence of

$$Q^2\square y^{2m-1}\{y^2+(2m-1)x^2\}^{m-2}$$

as a factor in the complete derived equation would give rise to some further investigations, but I will not now enter on them.

I remark however that if $m=1$, viz. if the integral equation be const. $=x+\sqrt{x^2+y^2}$, or say $z=x+\sqrt{x^2+y^2}$, or, what is the same thing,

$$z^2-2zx-y^2=0,$$

then observing that $y^2+(2m-1)x^2$ is here $=x^2+$ which is $=\square$, so that $\square\{y^2+(2m-1)x^2\}^{m-2}=\square.\square^{-1}=1$, the differential equation in its complete form is

$$y(p^2y+2px-y)=0;$$

so that we have here the factor y which divides out. The last-mentioned result is most readily obtained directly from the equation

$$\Omega=Q^2(2Q'\square+Q\square')^2-4P'^2\square=0,$$

which is the derived equation corresponding to the integral equation $z=P+Q\sqrt{\square}$. We in fact have $P=x$, $Q=1$, $\square=x^2+y^2$, and the derived equation thus is

$$(x+yp)^2-(x^2+y^2)=0,$$

that is, $y(p^2y+2px-y)=0$.

I mention also, in connexion with the foregoing investigation, the integral equation $z=x+\sqrt{2x^2-y^2}$, or $z^2-2zx-x^2+y^2=0$, for which the derived equation in its complete form is

$$(2x-yp)^2-(2x^2-y^2)=0,$$

or, what is the same thing, $y^2p^2-4xyp+2x^2+y^2=0$, and for which therefore there is no factor to divide out.

VII.

The conics confocal with a given conic form a system similar in its properties to that of the curves of curvature of a quadric surface; and the theory of the last-mentioned system may be studied by means of the system of confocal conics. Consider then the equation

$$\frac{x^2}{a^2+z}+\frac{y^2}{b^2+z}=1,$$

which, if z be an arbitrary parameter, belongs to the conics confocal with the ellipse $\frac{x^2}{a^2}+\frac{y^2}{b^2}=1$. Treating z as a coordinate, the equation represents a surface of the third order, which is such that its section by any plane parallel to the plane of xy is a conic; and the confocal conics are the projections on the plane of xy, by lines parallel to the axis of z, of the sections of the surface.

The sections by the planes of zx, zy are the parabolas $x^2=z+a^2$ and $y^2=z+b^2$ respectively. When $z>-b^2$, the ordinates in each parabola are real, and these ordinates give the semiaxes of the elliptic section. When $z>-a^2<-b^2$, then only the parabola section in the plane of zx has a real ordinate, and the sections are hyperbolic; and when $z<-a^2$, the section is altogether imaginary. The section in the planes $z=-b^2$ is the pair of coincident lines $y^2=0$, $z=-b^2$, and the section in the plane $z=-a^2$ is the pair of coincident lines $z=-a^2$, $x^2=0$; or, in other words, the plane $z+b^2=0$ touches the surface along the line $y=0$, and the plane $z+a^2=0$ touches the surface along the line $x=0$: this at once appears from the integral form

$$(z+a^2)(z+b^2)-x^2(z+b^2)-y^2(z+a^2)=0.$$

The points ($z=-b^2$, $y=0$, $x=\pm\sqrt{a^2-b^2}$) and ($z=-a^2$, $x=0$, $y=\pm\sqrt{b^2-a^2}$) are conical points; the last two are however imaginary points on the surface. To find the nature of the surface about one of the first-mentioned two points, say the point ($z=-b^2$, $y=0$, $x=\sqrt{a^2-b^2}$), taking this point for the origin and writing therefore $\sqrt{a^2-b^2}+x$, y, and $-b^2+z$ in the place of x, y, z respectively, the equation becomes

$$(a^2-b^2+z)z-\left((a^2-b^2)+2x\sqrt{a^2-b^2}+x^2\right)z-(a^2-b^2+z)y^2=0,$$

that is

$$z^2-2zx\sqrt{a^2-b^2}-(a^2-b^2)y^2-z(x^2+y^2)=0\ ;$$

so that there is a tangent cone the equation whereof is

$$z^2-2zx\sqrt{a^2-b^2}-(a^2-b^2)y^2=0,$$

or, as it might be written,

$$(z-x\sqrt{a^2-b^2})^2-(a^2-b^2)(x^2+y^2)=0.$$

The equation is that of a cone of the second order, meeting the plane of zx in the lines $z=0$, $z=2x\sqrt{a^2-b^2}$ (and therefore such that its sections parallel to the plane of xy are parabolas), and meeting the plane of yz in the lines $z=\pm y\sqrt{a^2-b^2}$ (the origin being at the vertex of the cone or conical point of the surface).

Returning to the original origin, and to the equation of the surface written in the form

$$z^2+z(a^2+b^2-x^2-y^2)+a^2b^2-b^2x^2-a^2y^2=0,$$

calling this for a moment $z^2+2\mathrm{B}z+\mathrm{C}=0$, the differential equation is $\mathrm{C}'^2-4\mathrm{BB}'\mathrm{C}'+4\mathrm{CB}'^2=0$; or, substituting, this is

$$(b^2x+a^2yp)^2-(a^2+b^2-x^2-y^2)(x+yp)(b^2x+a^2yp)$$
$$+(a^2b^2-b^2x^2-a^2y^2)(x+yp)^2=0\ ;$$

or, reducing, this is

$$(a^2-b^2)xy\{xy(p^2-1)-(a^2-b^2-x\ \ +y^2)p\}=0,$$

or say

$$xy\{xy(p^2-1)-(a^2-b^2-x^2+y^2)p\}=0,$$

where the factor xy arises from the level lines $(z+b^2=0,\ y=0)$ and $(z+a^2=0,\ x=0)$. Throwing out this factor, the equation becomes

$$xy(p^2-1)-(a^2-b^2-x^2+y^2)p=0,$$

which is satisfied identically by $z+b^2=0$, $y=0$, $x^2=a^2-b^2$. The first derived equation is

$$(xp+y)(p^2-1)+2(x-yp)p=0,$$

which for the values in question gives

$$p(p^2+1)=0,$$

where the factor $p=0$ corresponds to the section $y=0$ by the plane $z+b^2=0$: and taking the conical point for origin, and observing that the polar of the line $x=0$, $y=0$ in regard to the tangent cone is $z-x\sqrt{a^2-b^2}=0$, then writing the equation of the tangent cone in the form

$$(z-x\sqrt{a^2-b^2})^2-(a^2-b^2)(x^2+y^2)=0,$$

the two tangent planes through ($x=0$, $y=0$) are given by the equation $x^2+y^2=0$; and for these planes we have $p^2+1=0$. The factor $p^2+1=0$ determines therefore the directions of the envelope at the conical point.

VIII.

In verification of the equation

$$z=\tfrac{1}{2}k(x^2+y^2)+\tfrac{1}{6}ax(x_2+y^2)$$

for a quadric surface in the neighbourhood of the umbilicus, I remark that, starting from the equation

$$\frac{x^2}{a^2}+\frac{y^2}{b^2}+\frac{z^2}{c^2}=1$$

of an ellipsoid, and taking α, 0, γ as the coordinates of the umbilicus, and θ as the inclination to the axis of x of the tangent to the principal section through the umbilicus, then transforming to the umbilicus as origin and the new axes through that point, viz. the axes of x, z being the tangent and normal in the plane of ac, and the axis of y being at right angles to this or in the direction of b, the equation becomes

$$\frac{(\alpha+x\cos\theta-z\sin\theta)^2}{a^2}+\frac{y^2}{b^2}+\frac{(\gamma-x\sin\theta-z\cos\theta)^2}{c^2}=1,$$

or, expanding,

$$\left(\frac{\alpha^2}{a^2}+\frac{\gamma^2}{c^2}-1\right)+2x\left(\frac{\alpha\cos\theta}{a^2}-\frac{\gamma\sin\theta}{b^2}\right)-2z\left(\frac{\alpha\sin\theta}{a^2}+\frac{\gamma\cos\theta}{c^2}\right)$$
$$+x^2\left(\frac{\cos^2\theta}{a^2}+\frac{\sin^2\theta}{c^2}\right)+\frac{y^2}{b^2}+z^2\left(\frac{\sin^2\theta}{a^2}+\frac{\cos^2\theta}{c^2}\right)$$
$$-2zx\sin\theta\cos\theta\left(\frac{1}{a^2}-\frac{1}{c^2}\right)=0.$$

But we have

$$\alpha=a\sqrt{\frac{a^2-b^2}{a^2-c^2}},\quad \gamma=\frac{c\sqrt{b^2-c^2}}{\sqrt{a^2-c^2}},$$
$$\tan\theta=\frac{c}{a}\frac{\sqrt{a^2-b^2}}{\sqrt{a^2-c^2}},$$

and thence

$$\sin\theta=\frac{c\sqrt{a^2-b^2}}{b\sqrt{a^2-c^2}}=\frac{c}{ba}\alpha,\quad \cos\theta=\frac{a\sqrt{b^2-c^2}}{b\sqrt{a^2-c^2}}=\frac{a}{bc}\gamma;$$

and substituting these values, the equation becomes

$$-2z\frac{b}{ca}+\frac{x^2}{b^2}+\frac{y^2}{b^2}+z^2\frac{(a^2+c^2)b^2-a^2c^2}{a^2b^2c^2}+2\frac{\sqrt{a^2-b^2}\sqrt{b^2-c^2}}{b^2ca}zx=0,$$

or, what is the same thing,

$$z=\frac{ca}{b^3}\frac{1}{2}(x^2+y^2)+\frac{1}{b^3}\sqrt{a^2-b^2}\sqrt{b^2-c^2}zx+\frac{a^2b^2+c^2b^2-a^2b^2}{2b^3ac}z^2;$$

whence approximately

$$z=\frac{ca}{b^3}\frac{1}{2}(x^2+y^2),$$

and thence to the third order in x, y,

$$z=\frac{ca}{b^3}\frac{1}{2}(x^2+y^2)+\frac{ca}{2b^6}\sqrt{a^2-b^2}\sqrt{a^2-c^2}\,x(x^2+y^2),$$

which is of the form in question.

5 Downing Terrace, Cambridge,
November 2, 1863.

LXV. *On the Dynamics of the Galvanic Battery.*
By Professor A. W. WILLIAMSON*.

IN the title given to this evening's discourse, I have used a hard word—Dynamics, which suggests the study of active forces, and of calculations about those forces. I thought it was no more than fair to put such a hard word into the title of the discourse, because the title was intended as some sort of description of the thing itself; and there is no doubt that the matter which I have to bring under your consideration tonight requires for its appreciation some of that sustained and careful attention which is given to mathematical subjects. The conclusion, of which it will be my endeavour to convince you, depends on a chain of facts: each link of the chain is a very simple fact, but yet, if we lose one of them, the chain is broken, and the conclusion falls to the ground. Having given you this warning of a very dry discourse, I am bound to state, in mitigation, that of actual calculations we shall only need addition and subtraction.

I have on this table a couple of stout copper wires, one of which is in contact with one pole of a Grove's battery of 100 cells, whilst the other wire is in contact with the other pole. They are very quiet common-place looking copper wires, though capable of giving signs of their pent-up energies; and if I were to disconnect them from the poles of the battery, they would be found just like any other copper wires; but whilst in contact with the respective poles, these wires are endowed with latent energies which, according to the direction given to them, are capable of producing effects the most varied and the most mar-

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* Communicated by the Author, having been delivered at the Newcastle Meeting of the British Association, August 28, 1863.

vellous. We get from them heat sufficient to melt the most refractory substances, or light of a dazzling brilliancy, which approaches nearer to that of the sun than does any other artificial light. With a small fraction of the actual power of these wires we can get volleys of electrical discharges, each one so powerful that the strongest man could not endure it; or we can endow iron with the force of magnetism in its greatest intensity; and the compass forgets its steadfast direction when brought near one of these wires, and commits vagaries which would certainly appear most alarming to navigators who are accustomed to trust their lives to its directions; and last, but not least, we can decompose the strongest compounds, or combine the most sluggish and inert of elements by the action of the forces at work in these wires.

If a fairy gave you her wand, and told you that it was endowed with power to evoke five of the chief forces in nature, as they are often called, viz. heat, light, electricity, magnetism, and chemical affinity, with all their wondrous effects, you would surely not despise the gift, or neglect to learn its use. Now this wire is such a wand, and Science is the fairy who gives it you; but, like a wise and beneficent fairy, she tells you that you must patiently examine and study its nature, and that its use will be proportioned to the progress of that study.

The successive discoveries and theories which have rapidly succeeded one another in this field of inquiry, and given us a knowledge of this wonderful instrument, constitute one of the most brilliant and instructive chapters of scientific history. It is particularly interesting to notice how the metaphysical theory of contact force, invented by the illustrious Volta, has gradually given way before the steady advance of chemical discoveries, and how we now explain it by describing the actual nature of the transformations which occur in the working of the battery. Many a clear head and steady hand has helped to work out our knowledge of the subject; but towering above them all in undisturbed preeminence, we see the figure of our own Faraday, a man to whose fertile genius and indefatigable industry we owe the chief discoveries relating to the chemistry of the galvanic battery.

But chemistry alone is unable to explain the forces at work in this wonderful instrument, she is obliged to call other sciences to her aid, and to combine physical and mechanical discoveries with chemical ones in her final chain of facts.

It has been proved beyond all dispute that heat is a motion of atoms. Whenever the motion of masses is gradually or partially arrested by the friction of the particles of one against those of the other, we find that the particles get heated. Yet there is no production of force in such a process, only a transfer of motion from masses to atoms. Picture to yourselves a heavy train at the top of a railway incline; let the train run freely down the incline impelled by its own weight alone, and while it rushes down with headlong speed let the breaks be applied with the utmost force. The train is stopped; yet the motion which is taken from it is not destroyed, it is merely transformed into heat, for the friction at the breaks evolves such intense heat that it is no easy matter to prevent it setting fire to the wooden parts above them. Again, if we drive the piston of a steam-engine up and down by the pressure of steam, first below, then above it, and if this piston is connected by suitable machinery with moveable masses, we get those masses set in motion as effectually as if they were drawn by hundreds or even thousands of horses. And yet in this case the force is not created any more than in the former instance it was destroyed; for the steam which presses upon the piston loses heat by the effort, and the heat is transformed into work, from which it can be again recovered.

One of the most important steps of modern science has been the exact measurement, by Mr. Joule, of the quantity of heat which corresponds to a given amount of mechanical force. The force of one pound weight falling 772 feet makes what we call the unit of heat, viz. that quantity needed to raise the temperature of a pound of water 1° F. Whenever mechanical force is expended in heat, or heat transformed into mechanical force, we now know exactly the proportion of the one which can be got from the other. You perceive from this that we not only know heat to be a motion of atoms, but we have even a definite measure of it, as we have of the weights of the atoms themselves.

Philosophers had long since suspected that matter must be built up of atoms; but it is within the last half century that chemistry has given actual proof that such is really the case. No doubt there are some eminent men, for whom everyone entertains very great respect, who speak of the atomic theory as a mere hypothesis; and there are also eminent men who speak of the existence of matter in the same sceptical terms. There is no reasoning upon physical questions without assuming the existence of matter, and our friends only deceive themselves when they fancy they can do without it. And, in like manner, there is no reasoning upon chemical transformations without the atomic theory. No other theory has ever been found to explain the facts; and without it chemical science, such as it now exists, and I may say flourishes, would be transformed into a confused and unmeaning chaos of facts. Every chemist uses the atomic theory when he attempts to explain chemical facts; and chemists are bound to inform those who study nature and her forces from other points of view, that the existence of matter may just as well be denied as the existence of atoms.

I felt it my duty to be the more explicit upon this point, because I believe that we have to look to the further development of the atomic theory for the chief progress of scientific chemistry. I have to speak to you not only of atoms as really existing, but also of the movements of those atoms as productive of various important phenomena. It is now a good many years since I showed that chemical decompositions are the results of preexisting atomic movements, in fact that the force which produces chemical change is no other than a particular form of atomic motion. Some of you may probably feel a little hesitation in admitting that, while a given mass of matter remains in the same place, the atoms of which it is composed are yet in a state of intensely rapid vibration. Let us take the example of a gas, say common air. If air really consists of little atoms in a state of rapid vibration striking against the sides of any vessel in which the air is confined, and rebounding in virtue of their elasticity, what must we expect to happen if air was introduced into a vessel having a porous side, or a side with a number of very small holes in it? Of course the atoms would pass through the small holes and get out.

In illustration of this, I will show you one of those beautiful phenomena of so-called Diffusion which have attracted the attention of so many able investigators of late years, especially of my distinguished friend the Master of the Mint. I have here a vessel of which the upper part consists of a porous material, viz. unglazed earthenware. The vessel contains air, and I assert that the atoms of which this enclosed air consists are striking against the porous sides of the vessel and escaping through them. But the outer air is getting into the vessel as fast as the inner air gets out, so that the vessel does not get emptier or fuller, and the blue liquid in this tube, which is really the neck of the bottle, remains unmoved. I will now put the apparatus (still full of air) into a chamber full of a gas consisting of much lighter and more moveable atoms than those of air, in fact into a chamber full of hydrogen, and you see already that the blue liquid is being driven out by the neck of my vessel. This proves to us that the light atoms of hydrogen are getting in through the porous sides quicker than the heavy atoms of air get out. Each atom of hydrogen weighs only $\frac{1}{14}$th as much as an atom of nitrogen, which is the chief constituent of air: but it gets no benefit from its lightness beyond the power of moving faster, and I will prove to you that gases need not differ in density in order to diffuse into one another, by filling this bell jar with a gas called nitric oxide, which is very nearly equal in density to atmospheric air. At the other side of the porous disc which closes the mouth of the bell jar, will be a similar jar marked by a black band, containing common air; and if we get red fumes in the jar with a black rim, that will be a proof that nitric oxide has come out and mixed with the air in that black jar. If, on the other hand, we get red fumes in the jar with the white rim, that will prove to us that air has got into that jar and mixed with the nitric oxide contained in it. You see by the result that each gas is moving through the porous septum. I must now go a step further, and ask you to follow me into some chemical considerations—in fact the consideration of that wonderful process which it has been the chief business, and, I may add, the glory of chemistry to explain, I mean the process of combustion. We chemists think of combustion mainly as a process of combination of different materials with each other, forming compounds differing from those materials in their properties. But much better for our present purpose is the popular view of combustion, which consists in looking upon it as a plan for getting heat. You really do not burn coals in order to get carbonic acid and water by combining the carbon and hydrogen with oxygen from the air; but you burn them in order to heat your houses, or your factories, or steam-boilers, &c. Combustion is the process by which some of the inherent motion of atoms is taken from them on combining, and transferred in the form of heat to surrounding objects. If you burn a hundredweight of coal, and collect all the heat which is given off by its complete combustion, leaving the products as cold as the original materials before use, you have enough heat to boil about 80 hundredweight of water, that is, to heat 80 hundredweight from 32° F. to 212° F. Now in such an experiment we may say that all this heat is taken out of the coals and oxygen by merely combining them, and accordingly the products contain just so much less heat, or the motion which constitutes heat, than the original materials before combination.

If we burn iron, as it very often is burnt in forges, we have heat given off, and the oxide of iron contains just that much heat less than the iron and oxygen before combination. Precisely the same holds good when I break this little bulb, and allow the iron which is contained in it to burn in the chlorine contained in this tall cylinder. You see evidences of the heat which is given off by the direct formation of the chloride of iron from its elements; and for every pound of chlorine used to form this compound, there are 1492 degrees of heat evolved, assuming that the experiment is performed in the presence of water.

If chlorine is used to burn copper in the same manner, forming protochloride of copper, there are 962 degrees of heat evolved for every pound of chlorine taken up; and the less intensity of the combustion which you witness is an indication of that smaller evolution of heat.

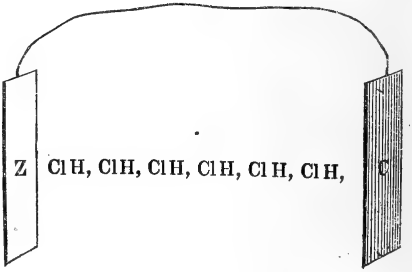
Now iron can take chlorine away from any feebler metal, forming chloride of iron, and turning the feebler metal out of its compounds. For instance, if I take a solution of chloride of copper (this deep-green liquid) and break a bulb containing finely-divided iron inside it, two things will happen at the same time: chloride of iron will be made, and chloride of copper will be unmade; we shall have a yellow solution of iron, and a fine red powder consisting of precipitated metallic copper. Before I begin the experiment, I want you to notice that the chloride of copper does not act on this air-thermometer, as both are at the temperature of this room. You see how rapidly the powder of metallic iron which I throw into the copper solution acts upon it, destroying the green colour, and precipitating the copper, but at the same time the liquid is getting hot. The air-thermometer now shows a considerable rise of temperature. If you measured carefully all the heat which is given off when chloride of iron is formed in this indirect manner, you would find it less than when direct combination takes place. Every pound of chlorine which goes to form protochloride of iron in this experiment gives off only 530 degrees of heat instead of 1492. Whence this difference of 962 degrees of heat? The reason is obvious: we have not only made chloride of iron, evolving thereby 1492 degrees of heat, but we have at the same time, and in the same liquid, *unmade* chloride of copper; and the chloride of copper absorbs when *unmaking* just as much heat as it *evolved* when making, viz. 962 degrees; so that 962 degrees of heat are consumed in the decomposition of the chloride of copper: $1492-962=530$ only are obtained. In this experiment the iron was used in the form of a fine powder for the sake of rapidity of action, and the copper remains in a similar form, each little fragment of copper being precipitated beside the little fragment of iron which was taking its chlorine. In fact the two chemical processes (of combination and decomposition) are mixed up with one another throughout the liquid. But I can effect the same decomposition and the same combination in such a way as to keep them separate, the combination in one part of the liquid, the decomposition at some distance from it. For this purpose I put a piece of hard carbon into this porous cylinder, and pour some chloride of copper in with it. The porous cylinder thus filled is placed in a jar containing hydrochloric acid, and a plate of amalgamated zinc is immersed in this acid. I must then attach the zinc outside the porous cell to the carbon inside it by means of a metallic wire. The zinc will then dissolve, forming chloride of zinc, and metallic copper will be deposited from the chloride on the surface of the carbon. The zinc acts just like the iron in my last experiment, only with a little more power; and the carbon does not undergo any change whatever: all the combination takes place on the surface of the zinc plate, all the decomposition on the surface of the carbon plate, where the copper is deposited.

How is it, then, that we have not the full heat of combustion evolved when the zinc combines, and the corresponding heat absorbed or cold produced where the copper is deposited from the chloride? That nothing of the kind occurs is easily seen; but the answer to the question involves the theory of the battery, in fact the understanding of our fairy's wand; for this little wire has an electric current running through it as long as it connects the combining and reducing plates. It has the same properties (as you can see by the deflection of the magnetic needle suspended beside it) which I mentioned as existing in our wires on this table, and can produce the same kind of effect. Our little instrument is, in fact, one cell of a galvanic battery. If we want more power than this cell affords, we can select a chemical process in which the heat evolved by the combination is more in excess of the heat absorbed by the decomposition, so that more heat will be actually available. Another way of getting more power is by increasing the surface of the plates; and a third process is to increase the number of cells, taking care to render the action in each cell dependent on that in the next by connecting the zinc plate in the first with the carbon plate in the second, the zinc in the second with the carbon in the third cell, and so on in so-called *polar order*.

Every cell of a galvanic battery consists necessarily of two solid plates capable of conducting electricity, moistened either with one liquid, or with two liquids kept from mixing by a porous cell, in the pores of which they touch one another. On the surface of one of these plates, which is called *positive*, a process of combination of some kind goes on, whether oxidation or chlorination, of which the effect is to evolve heat; and at the surface of the other plate, which is called *negative*, a process of unburning goes on, by which heat is absorbed. It is not necessary that the process of combination at the surface of the positive plate should take place, as in the instance before us, between metallic particles from the very substance of the plate, and a constituent of the liquid; nor is it necessary that the decomposition on the surface of the negative plate should be always effected by the deposition of a metallic body upon the surface of the plates. I have here a little battery consisting of two cells, so constructed that the chemical laws of its action are carried out in an entirely different manner. Each cell contains two platinum plates, one of which is quite clean, while the other is coated with the brown binoxide of lead. We may consider this plate as consisting of binoxide of lead, for the platinum beneath it only serves to connect the particles of that substance with the conducting wire. The bright platinum plate is immersed in nitrous acid contained inside a porous cell, and the peroxide of lead is immersed in nitric acid outside that porous cell. When the battery is at work, the nitrous acid becomes oxidized and converted into *nitric* acid on the surface of the *positive* plate, and the peroxide of lead dissolves off from the surface of the *negative* plate by losing oxygen. You can see the facility with which this transfer of oxygen takes place from the one body to the other, by the decolorization of the brown peroxide which takes place when I mix it in this foot glass with a solution of nitrous acid. You perceive by the testimony of this little battery that the oxidation of nitrous acid replaces the *chlorination* of the zinc in our first cell, and that disoxidation of peroxide of lead here occurs where dechlorination of copper took place in that cell. In all galvanic cells the plates communicate not only by the liquids in which they are immersed, but also by the metallic connecting wires; and while the cell is at work a galvanic current passes, not only through the connecting wire, but also through the connecting liquid.

Now there is this important difference between metals and liquid conductors. Wires are very good conductors, whilst liquids are very bad ones in comparison; and if we put the two plates into the liquid of a cell without connecting them by a wire, there is no current, nor is there that chemical process which we know to be its necessary accompaniment. The use of the connecting wire does supply what is still wanted for the occurrence of the combining process at the one plate, and the decomposing process at the negative plate, and we are unavoidably driven to consider that the electrical current is the process by which motion or vibration of atoms is transmitted through that wire from the combustion plate to the separation plate. The evidence in favour of this conclusion is strengthened, and at the same time rendered more precise, by a study of the process which takes place in liquid conductors. It so happens that, inasmuch as the liquids are composed of several elements, and as they undergo a permanent change during the process of conduction, we are able to gain a most valuable insight into the very behaviour of the component atoms while they are conducting the current. This is so very important a part of my argument, that I must beg leave to state it to you, although I must confess that it deals with questions respecting the position and arrangement of atoms which I would gladly have spared you had it been possible to do so.

It will be most convenient for our purpose to select for consideration the very simple case of a cell containing a zinc plate and a copper plate immersed in hydrochloric acid. The zinc dissolves by taking chlorine away from the hydrogen, and hydrogen is evolved from the surface of the copper plate. It is generally admitted that the first step of the process is the establishment of a polar arrangement of the molecules of hydrochloric acid of such kind that the chlorine atoms are turned towards the zinc, the hydrogen atoms towards the copper. The atom of chlorine next to the zinc then combines with it, forming chloride of zinc, and at the same time the atom of hydrogen next to the copper is set free and escapes. But the chlorine cannot combine with the zinc without getting rid of its hydrogen at the same time to the next atom of chlorine in the chain; and this second atom of chlorine must combine with another of hydrogen instead of the one which is thus taken from it. This goes on till, at the end of this chain of particles, we have in this manner an atom of hydrogen which loses its chlorine, and cannot get another in exchange, but escapes uncombined on receiving the necessary impulse from the copper plate. In the annexed diagram, Z represents the zinc plate, C represents the copper plate, and between them we have the chain of molecules of hydrochloric acid arranged in polar order from zinc to copper. If we remove from our diagram the chlorine from one end and the hydrogen from the other, we have a chain of molecules in the order in which they are kept after the first action, an order which we may call antipolar. If matters remained in this order, there could be no further combination of zinc with chlorine, no further liberation of hydrogen on the copper. We see that there can be no continuous current without a motion of the atoms: the atoms of chlorine must come in contact with the zinc to combine with it, and the atoms of hydrogen in contact with the copper to escape from it, and the original polar arrangement must re-establish itself throughout the whole liquid from the zinc to the copper before the next decomposition ensues. It would be unnecessary to discuss at present the particular manner in which this atomic motion takes



place in the conducting liquid; but there is no doubt of the fact that each atom of chlorine has to take the hydrogen from another atom of chlorine after each decomposition, and then to get over so as to touch the positive plate, while the hydrogen gets next to the negative plate. These atomic motions have been long since shown to take place in the conducting liquid. Now when a current of electricity is passing through a liquid, it produces upon any contiguous magnet the same effects as the same current running through a metallic conductor. You will see, when I send the current of my battery through this tube full of liquid, that the needle will be deflected exactly in the same manner as if I sent a current of equal strength through a rod of metal. Metallic conductors transmit with facility from the positive plate those vibrations which carry over the force needed for the liberation of the metals at the negative plate; while liquids sluggishly and reluctantly allow a similar vibration to take place along their constituent atoms, so as to enable the atoms to get into the places where they are wanted for the chemical action; but the liquid is always behindhand with its work, and opposes the chief resistance to the completion of the galvanic circuit.

We have now got to the point to which I wanted to bring you, and I will not attempt any further particulars. Our fairy wand differs from an ordinary copper wire by having pulsations going through its length from atom to atom, which transmit mechanical force from a process of combination which disengages it, to a process of decomposition which absorbs force. Any heating effects, or lighting effects, or magnetic or electrical, or chemical work which the wire is made to do are so much resistance to the chemical action of the battery, and afford outlets for some of that redundant force evolved by its combustions which, if the battery were closed by a short thick wire, would go to heating the liquids in the cells. You see how the resistance opposed by this platinum wire is overcome by the energy of my battery, which forces the heavy atoms of this metal to an unaccustomed energy of action, more, in fact, when I shorten somewhat the wire, than it can bear without fusion; or if I discharge it through the steel substance of this short file, you see how little able is even this material to withstand its action in contact with the air. Again, if I discharge it between these carbon points when in contact with each other, and then screw them a little asunder, I get a luminous discharge from the intensely heated *state of the particles of carbon.*

I must now leave for your consideration this outline of the chemical theory of the battery. What we know of the working of this wonderful instrument is no doubt very little compared to what yet remains to be discovered; and of that little I have described to you only a very small part, in fact only the foundation of the edifice. If you have any doubts on the subject, I trust that you will ascribe their existence to the imperfections and shortcomings of my exposition, and not lay them to the account of the theory itself.

LXVI. *On the Numerical Relations of the Distances between the Planets and Sun.* By EDWARD VANSITTART NEALE*.

THE great difference between the actual distance of Neptune from the Sun, and the distance at which it ought to be found, according to what is commonly called Bode's law, has naturally produced attempts to find some theory of the planetary distances more nearly corresponding to the observed facts than that suggested by Bode. Accordingly, at the last Meeting of the British Association, Mr. R. S. Browne proposed a scheme expressed by the following Table of proportional distances:—

Names.	Theoretical distances.	Totals.	Actual distances.
Mercury	4	4	3·87
Venus	4+ 3	7	7·23
Earth	7+ 4	11	10
Mars	11+ 7	18	15·24
Juno †	18+ 11	29	26·70
Jupiter	29+ 18+11	58	52·02
Saturn	58+ 29+18	105	95·39
Uranus	105+ 58+29	192	191·82
Neptune	192+105+58	355	300·37

If we compare these distances with the series derived from Bode's law, of 4, 7, 10, 16, 28, 52, 100, 196, 388, or with the corrected list of numbers constructed upon the principle suggested by V. Wurm (see 'Kosmos,' iii. p. 443), *i. e.* 3·87, 6·80, 9·73, 15·59, 27·31, 50·75, 97·63, 191·39, 378·91, it will be apparent that, with exception of the numbers falling to Neptune and Uranus, these series approximate to the observed distances more closely than that of Mr. Browne. Therefore his series cannot be considered as any material improvement on its predecessors, though escaping the objection raised to them by Gauss, that the distance betwen Mercury and the Sun does not harmonize with the succeeding parts of the series (see 'Kosmos,' p. 444). Nevertheless Mr. Browne's system suggests a mode of investigating the numerical relations subsisting between the distances of the planets whence more satisfactory results are obtainable.

In the series suggested by him, the group of planets beyond the asteroids is distinguished from that within them by the cir-

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* Communicated by the Author.

† Juno is taken as the representative of the asteroids,—their mean centre being uncertain, since the total number is not known.

cumstance that, to obtain an approximation to the observed distance of any planet comprised in the outer group, we must employ the theoretical distances of the *three* planets last preceding; while to obtain a similar approximation in the inner group, we require the theoretical distances of the *two* last preceding planets only.

It would seem, then, that the law regulating the distances between the planets in the two groups changes in passing from the one group to the other; so that, to discover its nature, it would appear desirable to take the asteroids as the starting-point, and examine the relations of distance between the inner and outer planets separately. Thus we obtain the following results. Beginning at Mercury, we have for the comparative distances of the inner group of planets from that planet these numbers: Venus, 3·36; Earth, 5·13; Mars, 11·37; Juno, 22·83. Again, beginning at Juno, we have for the comparative distances of the outer group of planets from that planet—Jupiter, 25·33; Saturn, 68·69; Uranus, 165·12; Neptune, 273·67.

At first sight these numbers are not promising. Except that the distance between Juno and Jupiter is not far different from the total distance between Juno and Mercury, there appears nothing like a law of progression in either series. However, we must consider that if the distances between the planets are governed by any law of symmetry, it is highly improbable that this law should be directly observable. If, as many concurrent arguments indicate, the solar system has arisen out of the gradual consolidation of a mass of material formerly diffused over the whole area now occupied by the orbits of the planets, whatever symmetry exists between their respective distances must belong to the distribution of the *original* centres of aggregation whereby the planets have been produced, and concern the primitive, not the present position of these bodies. So that a theory which exhibits this symmetry must be one which can account for the present places of the planets by means of the probable changes in their original arrangement due to the consolidation of the constitutive mass. Now these changes may be expected to occur in the following order:—

1. Since the consolidation depends upon the attraction exercised by the central mass upon its outer portions, the first act must be to draw these parts nearer to the centre.

2. The differences of pressure produced by the effort of the outer particles to approach the centre would give rise to a rotation of the whole mass, and thus throw its equatorial portion further from the centre.

3. When the consolidation became considerable, a great amount of heat would be developed from the collisions of the

particles with each other, and thus an additional expansive tendency would be produced.

If we suppose a number of centres of planetary aggregation to be symmetrically distributed through a mass subjected to these influences, the following results appear probable:—

1. The outermost centre of aggregation might probably be brought somewhat nearer to the common centre than the position due to its symmetrical place.

2. The succeeding centres would be thrown to a somewhat greater distance than their normal places, through the rotatory movement of the mass.

3. On the other hand, these centres would be continually urged nearer to the common centre by the pressures exerted upon them through the matter arrested by them in its progress inwards; so that the more central bodies might probably be ultimately found nearly in their original relative position in consequence of the counteraction of these two forces.

4. Any centres of aggregation so near to the general centre as to be materially affected by the expansive force of the heat there generated, may be expected to be repelled beyond their normal position towards the exterior.

The problem therefore becomes this: To find such an hypothetical symmetrical arrangement of the primitive planetary centres as will probably account for the present relations of distance observed among the planets after they have been subjected to the influences above traced. Such an arrangement appears to be supplied, if, taking the numbers 2 and 3 for the basis of the system, we suppose that the relative positions of the inner and outer groups of planets were originally determined by the respective series following:—

I. Inner group . . 2; 2×3; 2×6; 2×12.
II. Outer group . . 24; 24×3; 24×6 24×12.

The normal distances of the several planets on this hypothesis, with the changes produced in them by the process of consolidation, are shown in the following Table:—

	Names.	Theoretical distances.	Actual distances.	Differences.	
				Amount.	Proportion.
Inner group.	Mercury	—	—	—	—
	Venus	2	3·36	+1·36	+·680
	Earth	2× 3	5·13	−0·87	−·145
	Mars	2× 6	11·37	−0·63	−·052
	Juno	2×12	22·83	−1·17	−·048
Outer group.	Juno	—	—	—	—
	Jupiter............	24	25·33	+ 1·33	+·055
	Saturn	24× 3	68·69	− 3·31	−·045
	Uranus............	24× 6	165·12	+21·12	+·146
	Neptune	24×12	273·67	−14·33	−·049

It will be seen that this Table corresponds to our anticipations, except in the case of Saturn, where the difference is — instead of +, and of the Earth, which is nearer to the Sun than we should expect from the relative distances of Mars and Venus. But in both these cases a probable explanation of the anomaly is supplied by the circumstance of these planets respectively possessing a greater amount of satellitary attendance than their companions. If the satellites have been formed during the consolidation of their primaries, it must be supposed that there was something in the masses composing the Earth and Saturn giving them a greater tendency to throw off satellites than that belonging to their neighbours. It is probable that this something was connected with a greater spheroidicity in the form of the consolidating masses. Hence, since every planetary mass must for a long time have moved in a medium offering a considerable resistance, the form of the masses which have produced Saturn and the Earth would retard their movements more than those of their more spherical companions, and thus diminish the relative magnitude of their orbits.

Thus the anomalies in the distances of Saturn and the Earth strengthen instead of embarrassing the hypothesis suggested, being such as, from the peculiarities of these planets, must be expected to exist if the hypothesis be true. It remains to explain the position of Mercury. Now, since the distance from Mercury to Juno is, according to the theory, one-twelfth of that from Juno to Neptune, the symmetry of the system requires that the distance between Mercury and the Sun should be one-twelfth of that between Mercury and Juno. But if the great extension of the orbit of Venus beyond that theoretically assigned to this planet is due to the expansive force of the heat produced during the latter stages of the process of solar condensation, the orbit of Mercury ought to show a still greater expansion, as is the case. For while, according to the theory, the normal place of Mercury is represented by 2, its actual place is represented by 3·87, equal to an expansion of 93 per cent. Thus do theory and observation again accord.

I subjoin a Table exhibiting the distances of each planet from the Sun according to the theory and to observation, and the differences between the two sets of places in terms of the Earth's radius, and in those of the theoretical radius for each planet,—the proportions stated above being considerably modified when the orbit of Mercury is taken into account, in consequence of its great expansion.

Group.	Names.	Theoretical distances.			Totals.	Actual distances.	Difference.	Proportion of difference to theoretical radius.
I.	Mercury ...	2			2	3·87	+ 1·87	+·935
II.	Venus	2	+2		4	7·23	+ 3·23	+·807
	Earth	2	+2× 3		8	10	+ 2·00	+·200
	Mars	2	+2× 6		14	15·24	+ 1·24	+·089
	Juno	2	+2×12		26	26·70	+ 70	+·027
III.	Jupiter......	2	+24	+24	50	52·02	+ 2·02	+·040
	Saturn	2	+24	+24× 3	98	95·39	− 2·61	−·027
	Uranus......	2	+24	+24× 6	170	191·82	+21·82	+·128
	Neptune ...	2	+24	+24×12	314	300·37	−13·63	−·043 *

LXVII. *The Theory of Double Refraction on the Undulatory Hypothesis of Light. By* Professor CHALLIS, *M.A., F.R.S., F.R.A.S.*†

THE phenomena of light, viewed with reference to the hypothesis of undulations, range themselves naturally in two classes—those which depend only on properties and motions of the medium in which the light is generated and transmitted, and on its immediate action on the organ of sight, and those which in addition depend on particular relations of the motions and dynamical action of the medium to visible and tangible substances. In an article in the Philosophical Magazine for December 1862, I have given explanations of various phenomena of the first class, by referring them to laws and properties, mathematically investigated, of the small undulations of an elastic fluid the pressure of which is proportional to the density. I propose now to enter upon the consideration of a phenomenon of the second class, that of Double Refraction in crystals. The theory of such a phenomenon necessarily rests on hypotheses respecting the qualities and constituency of the refracting medium, as well as on those relating to the æther. In the adoption of both kinds of hypotheses, the ultimate conceptions respecting matter and force, for which *à priori* reasons were given in my article in the October Number, will be strictly adhered to. In fact the pheno-

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* Astronomers appear still uncertain whether the distance of Neptune from the Sun is not somewhat greater than that here attributed to it. If it be, the relation of position indicated here may require to be diminished. But even if it were converted from − into +, the theory would not be affected. The result would show only that in the case of Neptune, as in that of Uranus, the movement *from* the centre had predominated over that *to* the centre.

† Communicated by the Author.

menon of double refraction has been selected for present consideration because the theory I propose to give of it will be a good exemplification of the principles maintained in that communication.

In the article just mentioned the ultimate properties of matter and force are deduced, conformably with a rule of philosophizing laid down by Newton, from the indications of sensation and experience. Being so deduced they are perfectly comprehensible, and, if Newton's rule be good, may be regarded as necessary foundations of physical science. The hypotheses of the following theory, being framed in accordance with these fundamental conceptions, will be treated as *true* hypotheses, from which by right and sufficient reasoning the explanations of the phenomena necessarily result. Those physicists, however, who are not prepared to admit the validity of the *à priori* reasoning, cannot refuse to accept as mere hypotheses the ultimate properties which were reached by it, and as capable of being proved to be true by a sufficient amount of accordance with experiment of results deduced from them mathematically. I shall be content if their truth be considered to rest solely on this kind of evidence.

Now, in the first place, since on these principles the æther is to be regarded as a continuous fluid which presses proportionally to its density, and as experiments indicate that light has the same properties and is subject to the same laws under very different circumstances of original generation, those movements of the æther are admissible as exponents of phenomena of light, which can be mathematically shown to exist in such a medium independently of the particular nature of the disturbance. The investigation of such movements is therefore a necessary preliminary. This requisite I consider to be fully satisfied by the mathematical theory of the vibrations of an elastic fluid contained in the Numbers of the Philosophical Magazine for June, August, and October 1862.

Again, because, according to the same principles, the æther is a uniform fluid, and all the natural forces are modes of its action, its density must be the same within as without the refracting medium, there being no active force resident in the atoms of the medium to produce any alteration of the density. Also, because our principles led to the conclusion that the constituents of visible and tangible substances are inert spherical atoms of constant magnitude, the modifications which vibratory movements of the æther undergo when they pass from vacuum into a refracting medium, can only be due to the obstacles presented by the atoms to the free motion of the æther; and the effect of these obstacles will depend on the form, magnitudes, and arrangement of the atoms, on the number in a given space, and on their motion relative to the motion of the æther. Hence the second principal requisite in a theory of double refraction is to determine in what manner the ætherial undulations are modified by these circumstances. But here it must be remarked that such data cannot be had in the present state of physical science, so far as regards the magnitudes and arrangement of the atoms, and the number in a given space. We must therefore commence with a general and initiatory solution, not requiring these particular data,—and the rather so because there is reason to expect that such a solution may be one means of gaining an insight into the interior constitution of crystalline bodies, and thus obtaining eventually the more precise data.

Before proceeding to the theory of the changes which light undergoes after intromittence into a refracting medium, it will be proper to state briefly those characteristics of ætherial *rays* and *waves* which have been ascertained by mathematical investigation. For more complete information on this head, recourse may be had to the before-mentioned article in the Number of the Philosophical Magazine for December 1862. The principal results bearing on the present question are the following. Previous to the supposition of any instance of disturbance of the fluid, it is found that the differential equations of hydrodynamics are satisfied to the first order of approximation by vibratory motion having these characteristics :—The motion is symmetrically disposed about a rectilinear axis, so as to be at each instant a function of the distance from the axis in any plane cutting it at right angles. This axis of the motion being taken for the axis of z, let V be the velocity transverse to the axis, and w the velocity parallel to it, at a point distant from it by r, and let the pressure at the same point be $a^2(1+\sigma)$. Then the values of V, w, and σ, to the first approximation, are determined by the equations

$$\phi=\frac{m\lambda}{2\pi}\cos\frac{2\pi}{\lambda}(\kappa at-z+c), \qquad \kappa=\left(1+\frac{4}{\pi^2}\right)^{\frac{1}{2}},$$

$$f=1-\left(\frac{2r}{\lambda}\right)^2+\frac{1}{1^2.2^2}\cdot\left(\frac{2r}{\lambda}\right)^4-\frac{1}{1^2.2^2.3^2}\cdot\left(\frac{2r}{\lambda}\right)^6+\&c.,$$

$$\mathrm{V}=\phi\frac{df}{dr}, \qquad w=f\frac{d\phi}{dz}, \qquad \sigma=-\frac{f}{a^2}\cdot\frac{d\phi}{dt}.$$

As these results were obtained independently of any specific disturbance, we may conclude that they apply in all cases of arbitrary disturbance, and that every instance of vibratory motion is *initially* composed of motions defined by the foregoing equations. Since, also, they were deduced from linear differential equations with constant coefficients, to satisfy this condition we have at disposal an unlimited number of such motions, with arbitrary directions of the axes, and arbitrary values of m, λ, and c. A discussion of the function f, contained in art. 30 of the communication in the Philosophical Magazine for August 1862, shows that there are positions of maximum condensation and no transverse velocity, alternating with positions of no condensation and maximum transverse velocity, at fixed distances from the axis, and that the maximum condensations and velocities are continually less as the distances from the axis are greater. From these preliminaries we may proceed to the consideration of a case of motion bearing on the immediate object of this inquiry.

Let us conceive the fluid to be put in motion by a plane surface of indefinite extent, vibrating perpendicularly to its plane according to the law of the sine of a circular arc. It is evident that the resulting motion of the fluid will be wholly in lines perpendicular to the disturbing surface. But, from what has just been argued, the motion is still composed of motions defined by the foregoing equations. Hence, to satisfy the conditions of the disturbance, these component motions must be unlimited in number and in the same phase of vibration, and their axes must all be perpendicular to the disturbing plane and be regularly distributed relatively to that plane. For under these circumstances there will be no reason why the transverse motion at a given point should be in one direction rather than another, and consequently the transverse motions will be destroyed. If we now conceive the fluid to be bounded by a plane of unlimited extent, and a certain limited portion of it to vibrate in the manner above specified, there will be transverse motions to certain distances within and without the geometrical boundary of the disturbance. But from the above-stated properties of the function f, it may be seen that this motion will be restricted within very narrow limits. For since the number of the axes of the component motions within the geometrical boundary is unlimitedly great, at points very little distant from the boundary on the outside the sum of the condensations cannot sensibly differ from the sum of the rarefactions, and beyond that distance there can be no sensible motion; whilst at like distances within the boundary there will on the same account be no reason for a transverse motion in one direction rather than in another, and the motion will thus be wholly longitudinal. These considerations enable us to perceive why the vibrations of an elastic fluid do not spread indefinitely at their lateral boundaries, and why a slender cylindrical portion of light may be propagated through space to an unlimited distance.

If the plane vibrated according to any other law, provided there be no motion of translation, the motion may be regarded as composed of motions obeying the law of the circular sine, and the above reasoning, being applicable to the motions taken separately, applies also, on the principle of the coexistence of small vibrations, to the total motion. On this account, in the subsequent reasoning the initial waves will be supposed to constitute a single series defined by the law of the sine of a circular arc, and having a constant value of λ.

Let now a series of waves, generated as above supposed, be intromitted within a refracting substance, the surface on which they are incident being a plane parallel to the front of the waves. It may here be stated at once that, with the view of simplifying the present inquiry, I shall not enter upon the theories of reflexion and refraction at the surfaces of diaphanous bodies, these being questions which may be treated apart from that of the laws of transmission within the bodies. For the same reason I shall disregard the small difference between the intensities of the light before and after intromittence, caused by reflexion at the surface. This being understood, it will be assumed to be a necessary condition of diaphaneity, that the effect of the presence of the atoms of the body is simply to diminish in a constant ratio the accelerative force due to the actual gradations of the density of the æther. That is, $d\rho$ being the change of the density ρ corresponding to the change ds of a line s drawn in the positive direction of the motion, if the accelerative force without the body be $-\frac{a^2 d\rho}{\rho ds}$, the force within is $-\frac{b^2 d\rho}{\rho ds}$ for the same values of ρ, $d\rho$, and ds, the ratio of b to a being constant and less than unity.

In the present state of physical science we do not know by what difference of atomic constitution, or arrangement, a diaphanous body is distinguished from an opake one. It is only known that the condition of diaphaneity is of a very critical nature, and that the complete opacity of a substance, or its opacity to certain rays, appears in many cases dependent on ingredients in its composition which bear a very small proportion to the rest of the components. With respect, however, to the movement and condensation of the æther, it is certain that the above-stated condition must be fulfilled; and I shall now endeavour to show, by taking account of the spherical form of the atoms, in what manner it may be fulfilled. Conceive, at first, a series of waves after intromittence within the diaphanous body, to impinge upon one of its atoms supposed fixed. What under these circumstances will be the distribution of condensation about the surface of the atom, and the amount and law of variation of the reflected velocity? Respecting the distribution of condensation (which is the subject of express treatment in the "Mathematical Theory of Attractive Forces" contained in the Philosophical Magazine for November 1859), it will suffice for the present purpose to say that it must be symmetrically distributed about a straight line drawn in the direction of propagation through the centre of the atom, that, in the case of waves of the magnitude of those with which we are concerned in phenomena of light, the excess of condensation, or rarefaction, is always on the hemispherical surface on which the waves directly impinge, and that this excess is proportional to the incident condensation or rarefaction. Hence the atom is continually urged in the direction of propagation by a force which, to the degree of approximation with which we propose to conduct this research, is proportional to the condensation, and consequently to the velocity of the incident waves. We will suppose this force to be

$$km \sin \frac{2\pi}{\lambda} (bt - x + c),$$

b being the velocity of propagation in the diaphanous medium.

In the foregoing reasoning it has been assumed that the atoms of the medium are absolutely fixed. But according to our hypothesis of the atomic constitution of bodies, the atoms are retained in positions of stable equilibrium by the action of attractive and repulsive forces. They will consequently be susceptible of movement by the action of any extraneous force. This mobility of the atoms will plainly have an effect on the amount of their retardation of the movements of the ætherial waves. For the complete investigation of this retardation we ought, consequently, to be acquainted with the laws and modes of action of the forces which keep the atoms in equilibrium. The Theory of Molecular Forces, of which I have explained the principles in the Philosophical Magazine for February 1860, might, if fully carried out, furnish these data. The present investigation will, however, be conducted so as not to require a precise knowledge of the laws of molecular forces, but only the admissible assumption that, as each atom is acted upon by equilibrated forces, it will be put in motion by the addition of the luminous undulations. We must now endeavour to obtain on this assumption a mathematical expression for the velocity of each atom, in order to ascertain the effect of the mobility of the atoms on the rate of transmission of the waves.

We have already found, for the case in which the atoms are fixed, the expression

$$km \sin \frac{2\pi}{\lambda} (bt - x + c)$$

for the accelerative force, due to the action of the waves, by which an atom at the distance x from a fixed plane is urged in the direction of the propagation. If the atom be moving with the velocity $\frac{dx}{dt}$, it is evident that a portion of the velocity of the incident waves equal to this velocity, and the corresponding portion of the condensation, will be transmitted through the atom (supposed extremely small) just as if its place were occupied by the fluid itself. Consequently the expression for the accelerative force of the atom due to the waves will in this case be

$$k\left(m\sin\frac{2\pi}{\lambda}(bt-x+c)-\frac{dx}{dt}\right).$$

But, in addition to this force, there will necessarily be brought into play, by the displacement of the atoms from their positions of equilibrium, a force due to the *elasticity of the refracting medium*. Now from the foregoing considerations it may be seen that we are not here concerned with the absolute displacement of a given atom, but with the relative displacement of adjacent atoms, because the accelerative force of elasticity depends only on *relative* displacement. In fact, on account of the large value of λ compared with the mean interval between the atoms, all the atoms included between two planes at the distances $x-\Delta x$ and $x+\Delta x$ from the origin, will have movements very little different from that of the atom at the distance x, $2\Delta x$ being supposed very small compared to λ. That being the case, we shall sufficiently take into account the effect of the elasticity of the medium on the motion of a given atom, by treating the medium as if it were a continuous elastic substance; and whatever may be the relation between its pressure (p) and its density (δ), for the very small movements that will have to be considered, the variations of pressure may be assumed to be proportional to the variations of density. Hence, if the elasticity of the medium be such that $dp=e^2d\delta$, we shall have for the accelerative force of the atom due to the elasticity $-e^2\,.\,\frac{d\delta}{dx}$. But since the movements of the atoms are governed by the action of the ætherial waves, it follows that these movements and the accompanying condensations will be propagated uniformly through the medium with the same velocity as the waves of light—that is, with the velocity b. Motion of that kind, as I have shown elsewhere, and as, in fact, it is easy to verify, must satisfy, without reference to the action of forces, the equations

$$v=b\delta=f(x-bt),$$

v being the velocity of a particle of the medium. Hence it follows that

$$-e^2.\frac{d\delta}{dx}=-e^2.\frac{dv}{b\,dx}=-\frac{e^2}{b}.f'(x-bt)=\frac{e^2}{b^2}\frac{dv}{dt}.$$

But $\frac{dv}{dt}$ may be taken with sufficient approximation to be the actual accelerative force of the atom. Consequently

$$-e^2 \cdot \frac{d\delta}{dx} = \frac{e^2}{b^2} \cdot \frac{d^2x}{dt^2}.$$

Thus the total accelerative force of the atom is

$$k\left(m \sin \frac{2\pi}{\lambda}(bt - x + c) - \frac{dx}{dt}\right) + \frac{e^2}{b^2} \cdot \frac{d^2x}{dt^2}.$$

Equating this to $\frac{d^2x}{dt^2}$, we have for determining the motion,

$$\frac{d^2x}{dt^2} + \frac{k}{1 - \frac{e^2}{b^2}} \cdot \frac{dx}{dt} = \frac{km}{1 - \frac{e^2}{b^2}} \cdot \sin \frac{2\pi}{\lambda}(bt - x + c).$$

In this equation we may neglect in the circular function the small variation of x about its mean value, and suppose the mean value to be the arbitrary quantity c. Then putting, for the sake of shortness, $k = q\left(1 - \frac{e^2}{b^2}\right)$, we have to integrate the equation

$$\frac{d^2x}{dt^2} + q\frac{dx}{dt} = mq \sin \frac{2\pi bt}{\lambda}.$$

I may here take occasion to state that so long ago as the year 1830 I attempted to ascertain the motions of an atom of a medium, resulting from the action of a series of undulations, upon principles identical with those adopted above, with the exception that the action of the elasticity of the medium was supposed to depend on the absolute displacement of the atom instead of the relative displacement of adjacent atoms. (See the Philosophical Magazine for September 1830, p. 169.) In consequence of that supposition the investigation did not altogether succeed in accounting for the different refrangibility of the rays of light, which was its ultimate object. It will presently appear that that problem may be satisfactorily solved by the integration of the foregoing differential equation.

By the integration of the equation, and putting $\cot \phi$ for $\frac{2\pi b}{\lambda q}$, we obtain

$$\frac{dx}{dt} = -m \sin \phi \cos\left(\frac{2\pi bt}{\lambda} + \phi\right) + \mathrm{C}e^{-qt},$$

from which equation the term containing the arbitrary constant will soon disappear. Hence

$$m \sin \frac{2\pi bt}{\lambda} - \frac{dx}{dt} = m \cos \phi \sin\left(\frac{2\pi bt}{\lambda} + \phi\right).$$

Having arrived at this expression for the relative velocity of the æther and an atom, we have next to investigate the retarding effect of the atoms taken collectively on the undulations of the æther. As already explained, the retardation is due to the reflexions of velocity from the surfaces of the atoms, the reflexion from each atom depending only on the relative velocity of the atom and the æther. The *total* reflected velocity consists of two parts, of which one depends on the new condensation to which the reaction of the surface of the atom gives rise, while the other is unaccompanied by condensation, being a velocity of the kind by which the fluid moves so as to fill space continuously without change of density. The latter part, inasmuch as it fulfils that condition and is due to the reaction of a spherical surface, takes place in the directions of the radii produced, and varies with the distance according to the law of the inverse square. Also the mathematical theory of motion in an elastic fluid tending to or from a centre, shows that for all distances from the centre very small compared with the breadth of the waves, or the value of λ, the part of the velocity unaccompanied by condensation is very much larger than the other part. If, therefore, the velocity V be incident on the atom at a point the radius to which makes an angle θ with the direction of incidence, and c be the radius of the atom, the velocity at the distance r from the centre reckoned on the produced radius of the point is $\frac{\mathrm{V}c^2}{r^2}\cos\theta$, r being very small compared with λ. Now it is this reflected velocity which gives rise to a retarding force acting always counter to the accelerative force due to the elasticity of the æther. The mode of action will be such as follows. Since it is an experimental fact that parallel lines, engraven on glass, and separated by intervals less than λ, can be seen with a microscope without the least appearance of discontinuity of the surface of the glass, it follows that the mean interval between the atoms must be almost an infinitesimal quantity compared with λ. Consequently, if we conceive a plane to be drawn in the medium parallel to the front of the waves, the retarding force at any point of the plane will be due not alone to the reflexion of velocity from atoms situated in the plane, but also from simultaneous reflexions from atoms situated within a certain small distance beyond it in the direction towards which the waves are propagated. The limiting distance the reflexions beyond which have no sensible effect at the plane, while it is small compared with λ, must at the same time be supposed to contain many multiples of the mean interval between the atoms. For, considering that in fluids and solids the atoms occupy spaces very small in comparison with the intervening spaces, unless the reflexions take place from a vast number of atoms contained in a slice of the medium of sensible thickness, their mean effect would be inadequate to produce the observed amount of retardation.

Accordingly the calculation of the total retardation will have to be made in the following manner. Let the velocity of the æther at the given distance X from the origin at the time t be $m \sin \frac{2\pi bt}{\lambda}$; and at a greater distance x, reckoned in the direction of propagation, let the velocity be $m \sin\left(\frac{2\pi bt}{\lambda} - \theta\right)$. Then, if we take a slice of the medium such that its limiting planes are at the distances $x - \frac{\alpha}{2}$ and $x + \frac{\alpha}{2}$ from the origin, α being very small, from what is proved above the mean relative velocity of the æther and the atoms will be

$$m \cos \phi \sin\left(\frac{2\pi bt}{\lambda} - \theta + \phi\right),$$

and to this quantity the mean of the reflected velocities will be proportional. We may therefore suppose that the retarding effect of the reflexions, from all the atoms of the slice is, at the given distance X,

$$h\alpha m \cos \phi \sin\left(\frac{2\pi bt}{\lambda} - \theta + \phi\right),$$

the coefficient h being a function of $x - \mathrm{X}$. If the whole of that portion of the medium from the atoms of which the reflexions are effective at the same distance X be divided into like slices, the total retardation may be represented by the summation

$$\Sigma \,.\, h\alpha m \cos \phi \sin\left(\frac{2\pi bt}{\lambda} - \theta + \phi\right),$$

in which h and θ are different for the different slices. But the terms of this series are such that their sum is known to admit of the form

$$\mathrm{K} m \cos \phi \sin\left(\frac{2\pi bt}{\lambda} - c' + \phi\right).$$

This may consequently be taken to be the amount of the retardation which the æther suffers at the position where the velocity is $m \sin \frac{2\pi bt}{\lambda}$. But the condition of diaphaneity, before stated, requires that this retardation should have a constant ratio to the actual velocity of the æther. That this may be the case we must have $-c' + \phi = 0$, which equation may be considered to be the theoretical expression of the condition of diaphaneity. Both ϕ and c' are quantities depending on the number, magnitudes, and arrangement of the atoms of the medium; but for carrying on the present argument it is not necessary to be acquainted with explicit expressions for these functions.

It results from the preceding reasoning that $\mathrm{K}\cos\phi$ is the ratio of the retarding force of the atoms to the actual accelerative force of the æther. But the actual accelerative force is the force due to the actual variations of density, diminished by the retarding force. Consequently,

$$-\frac{b^2 d\rho}{\rho dx} = -\frac{a^2 d\rho}{\rho dx} + \mathrm{K}\cos\phi \cdot \frac{b^2 d\rho}{\rho dx},$$

whence

$$b^2(1+\mathrm{K}\cos\phi) = a^2.$$

The coefficient K will evidently vary as the density of the medium. Hence if δ be the density, and $\mathrm{K}=\mathrm{H}\delta$, and if μ be put for the ratio of a to b, we shall have

$$\mu^2-1=\mathrm{H}\delta\cos\phi.$$

This equation involves the explanation of the different refrangibilities of the rays of light. For since $\cot\phi=\frac{2\pi b}{q\lambda}$, and $a=\mu b$, we have

$$\cos\phi=\frac{2\pi a}{\sqrt{4\pi^2a^2+q^2\lambda^2\mu^2}}$$

and

$$\mu^2-1=\mathrm{H}\delta\,.\,\frac{2\pi a}{\sqrt{4\pi^2a^2+q^2\lambda^2\mu^2}}, \quad . \quad . \quad . \quad . \quad (\alpha)$$

which equation determines the relation between μ and λ. By differentiation it will be found that

$$\frac{d\lambda}{d\mu} = -\frac{4\pi^2a^2}{q^2\lambda\mu^3}\cdot\left(\frac{\mathrm{H}^2\delta^2}{(\mu^2-1)^2}-1+\frac{2\mathrm{H}^2\delta^2\mu^2}{(\mu^2-1)^3}\right).$$

Since $\mathrm{H}^2\delta^2=(\mu^2-1)^2\sec^2\phi$, this equation proves that the variations of λ and μ have different signs. Consequently as λ increases μ diminishes; that is, the rays of greater breadth are the less refrangible. The course of our reasoning, which has thus led incidentally to an explanation of the phenomenon of *dispersion*, now brings us to the more immediate consideration of double refraction.

Resuming the equation (α), we have, for the relation between the partial variations of q and λ with respect to μ,

$$\frac{dq}{d\mu}=\frac{d\lambda}{d\mu}\cdot\frac{q}{\lambda}.$$

But $k=q\left(1-\frac{e^2}{b^2}\right)=q\left(1-\frac{\mu^2e^2}{a^2}\right)$, k being a positive constant. Hence

$$\frac{d\,.\,e^2}{d\mu}=-\frac{2e^2}{\mu}+\frac{a^2}{q\mu^2}\left(1-\frac{\mu^2e^2}{a^2}\right)\frac{dq}{d\mu}$$

$$=-\frac{2e^2}{\mu}+\frac{a^2-\mu^2e^2}{\lambda\mu^2}\cdot\frac{d\lambda}{d\mu}.$$

Since e^2, which measures the elasticity of the refracting medium, is extremely small compared with a^2, which measures the elasticity of the æther, and since we have shown that $\frac{d\lambda}{d\mu}$ is a negative quantity, it follows that the right-hand side of this equation is negative. Hence, the variations of e^2 and μ having opposite signs, the greater the elasticity of the medium the less the refractive index, and therefore the greater the velocity of propagation in the medium.

From the foregoing argument we may conclude generally, that *the velocity of transmission of light in any diaphanous medium depends in part on the elasticity of the medium.*

But experiments have been considered to indicate that the elasticity of certain crystals is different in different directions; and it may reasonably be supposed that this is generally the case in regularly crystallized bodies. We have therefore now to inquire what effect this circumstance may have on the transmission of light in crystals. In the first place, from the facts of crystallography it may be presumed that the elasticity is in some manner connected with *atomic arrangement.* It does not seem possible to account for planes of cleavage on any other principle. If, then, the atomic arrangement should be such as to be symmetrical about any straight line drawn parallel to a fixed direction in the crystal, it seems to be a necessary consequence that the elasticity of the crystal will be the same in all directions perpendicular to that line. For instance, in a uniaxal crystal, as Iceland spar, the elasticities in directions perpendicular to the crystallographical axis would all be equal. But it is allowable to make a more comprehensive supposition respecting the atomic arrangement. We may suppose it to be symmetrical with respect to three planes drawn always parallel to three fixed planes at right angles to each other in the crystal. Taking the intersection of the planes for the axes of coordinates, if a surface be described such that a radius vector drawn in any direction from the origin represents the elasticity of the crystal in that direction, the above-mentioned condition of symmetry of atomic arrangement would require the equation of this surface to be of the form $z^2=f(x^2, y^2)$. Hence, as the elasticity (e^2) is a function of the radius vector (r), we have

$$e^2=\phi\big(x^2+y^2+f(x^2, y^2)\big).$$

This equality shows that e may have a maximum or minimum value independently of the forms of the functions ϕ and f. For by differentiating,

$$\frac{d.e^2}{dx}=\phi'(r^2)\left(2x+2x\,.\frac{df}{d.x^2}\right),\quad \frac{d.e^2}{dy}=\phi'(r^2)\left(2y+2y\frac{df}{d\,.\,y^2}\right).$$

The equations given by equating to zero the right-hand sides of these equalities are respectively satisfied by $x=0$ and $y=0$, showing that the elasticity in the direction of the axis of z is a maximum or minimum. The same is evidently the case with respect to the other two axes. As these results have been obtained independently of the forms and values of the functions ϕ and f, it appears that every crystal which satisfies the assumed law of symmetrical atomic arrangement has three axes of maximum or minimum elasticity at right angles to each other.

Next let the coefficients of elasticity in the directions of the three axes of x, y, x be respectively e_1^2, e_2^2, e_3^2, and such that the force which is produced by a given relative displacement of the atoms in the direction of an axis is equal to the coefficient × displacement. We have now to find the coefficient of elasticity for any direction making the angles α, β, γ with the axes. For this purpose we shall regard, as heretofore, the crystalline medium as consisting of discrete atoms held in positions of stable equilibrium by attractive and repulsive forces, and shall assume that, according to the law of the coexistence of small vibrations, each atom can perform independently simultaneous oscillations in different directions in obedience to simultaneous disturbances. On this principle we may consider an actual displacement (δv) in the given direction to be the resultant of the three displacements $\delta v \cos \alpha$, $\delta v \cos \beta$, $\delta v \cos \gamma$ in the directions of the axes. Now these displacements, by hypothesis, give rise to forces in the directions of the axes equal to $e_1^2 \times \delta v \cos \alpha$, $e_2^2 \times \delta v \cos \beta$, $e_3^2 \times \delta v \cos \gamma$. And it has already been explained that the forces which these expressions represent are due to condensations of the crystal considered as a *continuous* substance, and are not in kind different from the forces we are concerned with in ordinary mechanics. On this account these forces, and also the force due to the original displacement, take effect wholly in the directions of the displacements. For the same reason, the three forces in the directions of the axes may be resolved in the direction of the original displacement, and the sum of the resolved parts, viz.

$$(e_1^2 \cos^2 \alpha + e_2^2 \cos^2 \beta + e_3^2 \cos^2 \gamma) \times \delta v,$$

being the total force in that direction, must be equal to the force due to the original displacement. If, therefore, e^2 be the coefficient of elasticity for the given direction, we shall have

$$e^2 = e_1^2 \cos^2 \alpha + e_2^2 \cos^2 \beta + e_3^2 \cos^2 \gamma. \quad . \quad . \quad . \quad (\beta)$$

Reverting again to the equation (α), it is important to remark that for a ray having a given value of λ in vacuum, which for brevity we shall call a ray of given *colour*, the product $\lambda\mu$ is constant; that is, if λ_0 be the value of λ in vacuum, we shall have for the same colour in a medium $\lambda_0 = \lambda\mu$. The reason for this law will appear by considering that when a series of waves enters perpendicularly into a refracting medium, the ratio of the breadth occupied by a given number (n) of waves without the medium to that occupied by the same number within, is equal to μ; so that $n\lambda_0 = n\lambda\mu$. Hence the rays of given colour propagated in different directions in a crystalline medium will all have the same value of $\lambda\mu$. Taking this into account, and substituting for q from the equality $k = q\left(1 - \frac{\mu^2 e^2}{a^2}\right)$, it will be found that the equation (α) may be put under the form

$$\mu^2 - 1 = \mathrm{A} + \mathrm{B}e^2,$$

terms involving powers of e^2 above the first being omitted on account of the small ratio of this quantity to a^2. The factors A and B are altogether independent of μ and e^2.

The *apparent* elasticity of the æther within the crystal will depend both on the obstacles presented to its free motion by the atoms of the crystal, and, as we have shown above, on the elasticity of the crystal itself. Let a'^2, b'^2, c'^2 be the coefficients of the apparent elasticity of the æther in the directions of the axes of coordinates, and r^2 that for the given direction. Then for rays of given colour we shall have the three equations

$$\frac{a^2}{a'^2} - 1 = \mathrm{A} + \mathrm{B}e_1^2, \quad \frac{a^2}{b'^2} - 1 = \mathrm{A} + \mathrm{B}e_2^2, \quad \frac{a^2}{c'^2} - 1 = \mathrm{A} + \mathrm{B}e_3^2,$$

together with the equation

$$\frac{a^2}{r^2} - 1 = \mathrm{A} + \mathrm{B}e^2.$$

If the three equations be respectively multiplied by $\cos^2\alpha$, $\cos^2\beta$, $\cos^2\gamma$, and the sum of the results be compared with the last equation, account being taken of the equation (β), it will readily be seen that

$$\frac{1}{r^2} = \frac{\cos^2\alpha}{a'^2} + \frac{\cos^2\beta}{b'^2} + \frac{\cos^2\gamma}{c'^2}.$$

This equation we shall afterwards cite by its usual appellation, the equation of elasticity.

We come now to a critical part of our theory, by which it is distinguished from all other attempts to solve the same problem. Although the above equation gives the effective elasticity of the æther in any direction in the crystal, we cannot immediately infer from it the velocity of propagation of a plane wave, because, as I have shown from hydrodynamical principles, the composite character of the wave must be taken into account. The effect this circumstance has on the velocity of propagation may be determined by the following considerations. First, it is clear that the wave cannot be composed of ray-undulations the transverse motions of which are the same in all directions from the axis, inasmuch as this condition cannot be generally fulfilled, because the effective elasticity is different in different directions. But such ray-undulations are the exponents of *common light.* Hence it follows that common light cannot be transmitted through any substance the elasticity of which varies with direction, and therefore it is incapable of transmission through a doubly refracting medium, of which the change of elasticity with direction is an essential property. This inference is conformable with experience. But a *polarized* ray is transmitted through such substances. In order to give a theoretical reason for this fact of observation, it will be necessary to recur to the analytical expressions, applicable to ray-undulations, contained in the early part of this communication. The value of the function f there given being obtained without reference to an arbitrary disturbance, defines the law of transverse velocity and transverse variation of density in rays which have undergone no modification subsequent to their original production, which are always rays of common light. But I have shown under Prop. X. in the Philosophical Magazine for December 1852, that the value of f is determined generally by integrating the partial differential equation

$$\frac{d^2f}{dx^2}+\frac{d^2f}{dy^2}+4ef=0,$$

in which e is a certain constant satisfying the equation $e\lambda_2=4$. The integrals, however, of this equation are subject to the limitation of applying only to modifications which the original rays may receive by successive disturbances. We may presume, because this equation is linear with constant coefficients, that all the parts into which an original ray-undulation may be resolved will by recomposition form the original ray, and that, too, without restriction of the values of x and y. But we are not now concerned with movements and condensations at considerable distances from the axes, which only perform the part of preventing the lateral diffusion of the *waves,* but with those that are extremely near the axes, which alone are productive of the sensation of light. Respecting these, it is proved in the article just referred to, that an original ray is resolvable into two equal rays polarized in planes at right angles to each other, and that the transverse vibrations in each polarized ray, as also the condensations, are symmetrical with reference to the fixed plane coincident with the axis, to which the motion is perpendicular.

This being understood, suppose a polarized ray to be propagated in any direction from the centre of the surface of elasticity, and conceive this surface to be cut by a plane perpendicular to the direction of propagation. The section will be an ellipse, of which the radius-vectors drawn from the centre are proportional to the effective elasticities of the æther in their respective directions. In two directions at right angles to each other, corresponding to the axes of the ellipse, the effective elasticities will be at a maximum or minimum, and through a small variation of direction may be considered to be uniform. Consequently in these two directions the transverse motions of a polarized ray can take place without disturbance; and accordingly rays whose transverse motions are in, and parallel to, either of the two planes which pass through the centre of the surface of elasticity and the axes of the elliptic section, can be transmitted in the crystal.

It remains to show that the velocity of propagation of each ray depends exclusively on the effective elasticity in the direction of the elliptic axis which coincides in direction with its transverse motions. We have seen that $\mu\lambda$ is constant for a ray of given colour. Any cause, therefore, that augments or diminishes λ, will diminish or augment μ. Also since the quantity of matter in the condensed part of a wave, exceeding that which would occupy the same space in the quiescent state of the fluid, must under all circumstances be constant for a given wave, any cause which augments the condensations along the axis of the ray will proportionally diminish λ and increase μ. If the effective elasticity in the direction of the transverse vibrations be the same as that along the axis, the rate of propagation of the ray will plainly be determined by that elasticity. But if the transverse elasticity be altered in any proportion, the effect on the transverse condensations must be to alter them in the inverse proportion. For instance, if the transverse elasticity be increased, the transverse condensations will be proportionally diminished, the accession of elasticity being equivalent to the accelerative force lost by the diminution of the gradations of density. But the transverse condensations cannot be diminished without a like diminution of the condensations estimated along and parallel to the axis. Hence, from what is said above, λ will be proportionally increased, and μ proportionally diminished. Thus μ depends wholly on the transverse velocity.

It is unnecessary, after arriving at this point, to go through the analytical process for finding the equation of the wave-surface, as I have given it in a paper "On Double Refraction" contained in the Cambridge Philosophical Transactions (vol. viii. part 4), and the process is otherwise well known. Respecting the theory of double refraction attempted in that paper, I may here state that it rests fundamentally on the same principles as the present one, but in the details of the argument is not so complete. I will now only add two deductions from the theory.

(1) An optical axis is defined to be such that the section of the surface of elasticity by a plane at right angles to it is a circle. Hence, by the foregoing theory, the effective elasticities in all directions perpendicular to an optical axis are equal. Con-

sequently if a ray be propagated in any principal plane of a uniaxal or biaxal crystal, and its transverse vibrations be perpendicular to the plane, the velocity of propagation will be the same in all directions in the plane, and *the ray will obey the ordinary law of refraction.*

(2) If the principal plane of a uniaxal crystal be called the plane of polarization of the *ordinary* ray, *the transverse motions are perpendicular to the plane of polarization.* This result is in accordance with that obtained, relatively to a ray completely polarized by reflexion, at the end of the article "On the Direction of the Vibrations of a Polarized Ray," in the Philosophical Magazine for February 1859.

I beg to conclude this communication with a few words on the history of the problem of double refraction. Although Fresnel succeeded in obtaining the equation of the wave-surface, he was himself aware that his theory rested on a defective dynamical basis. After stating in his memoir "On Double Refraction" (*Mémoires de l'Institut,* vol. vii. p. 53), that the mechanical theory rests on the two hypotheses, that the luminous vibrations are *transverse,* and that the elasticity of a doubly refracting medium is different in different directions, he adds, "As to the hypothesis on the nature of the luminous vibrations, it appears at first sight much more difficult to admit [than the other], because we do not easily see how transverse vibrations can be indefinitely propagated in a fluid. Nevertheless if the facts which already furnish so many probabilities in favour of the system of undulations and so many objections against that of emission, oblige us to recognize this character in the luminous vibrations, there is more certainty in our referring here to experience than to the notions unhappily too incomplete which the calculations of geometers have hitherto given us respecting the vibrations of elastic fluids." The attempts subsequently made to supply what was defective in the mechanical theory of double refraction, were not directed towards improving the mathematical theory of the motion of fluids, but were based on a hypothetical isotropic medium of discrete atoms or centres of force, respecting which we are left in doubt as to the exact relations of its constituents to the æther and to the doubly refracting substance. These attempts are reviewed at considerable length by Professor Stokes in his "Report on Double Refraction," contained in the Report of the British Association for 1862 (p. 253). After stating the results of the different theories, he adds, "I may perhaps be permitted to express my own belief that the true dynamical theory of double refraction has yet to be found." Then after adverting to the fact, established by experiment, that a ray polarized in a principal plane obeys the ordinary law of refraction, he remarks, "this simple law, which ought, one would expect, to lie on the very surface, as it were, of the true theory of double refraction, is not indicated *à priori* by most of the rigorous theories which have been advanced to account for the phenomenon." I am not aware that it is strictly indicated *à priori* by any.

The reproach which Fresnel cast on the mathematical theory of the motion of fluids I claim to have removed by my hydrodynamical researches. I have shown that transverse vibrations necessarily take place in a fluid the pressure of which varies as its density. The problems by the solution of which this result is obtained are those which, among others, I referred to in my article in the October Number as having the same kind of relation to general theoretical physics, as the dynamical problems that Newton solved to physical astronomy. Not only the theory of double refraction, but the whole of the undulatory theory of light, is placed on an entirely new basis by the discovery of transverse vibrations in an elastic fluid. The explanations of the phenomena of dispersion and double refraction given in this communication, will, I think, convince any mathematician who can be induced to give them sufficient attention, that the basis of the theory must be true. It has this character of truth, that it not only explains phenomena, but accounts for the failure, or partial success, of previous theories. It shows that Fresnel's theory was true so far as it made the double refraction depend on the different apparent elasticities of the æther in different directions, but failed in not sufficiently distinguishing between the crystalline medium and the æther, and especially in not regarding the latter as a continuous fluid. It is wholly on the last account that the supporters of Fresnel's views have not been able to show that the usual law of refraction holds good for an ordinary ray in a principal plane, nor to determine in an unambiguous manner that the direction of the transverse vibrations of an ordinary ray is perpendicular to the plane of polarization —two results which, as is shown above, are very simply deduced from the present theory.

As this theory has been framed in strict accordance with the fundamental hypotheses of Natural Philosophy enunciated (and supported by *à priori* reasoning) in my last communication, and as it satisfactorily exemplifies in several important particulars the principles there maintained, I take occasion to say, not so much with the expectation of receiving credit as for the purpose of expressing my conviction of the truth and necessity of the mode of philosophizing I have so long advocated, that the true theory of double refraction no longer remains to be discovered. The same assertion may be made with respect to the theory of dispersion, the two theories being so intimately connected that they stand or fall together.

Cambridge, November 18, 1863.

LXVIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 405.]

March 5, 1863.—Major-General Sabine, President, in the Chair.

THE following communications were read:—

"On Skew Surfaces, otherwise Scrolls." By A. Cayley, F.R.S.

"Researches on the Refraction, Dispersion, and Sensitiveness of Liquids." By J. H. Gladstone, Ph.D., F.R.S., and the Rev. T. P. Dale, M.A., F.R.A.S.

This communication contains the results of some inquiries which were started by the authors in a previous paper "On the Influence of Temperaure on the Refraction of Light"*. The same apparatus had been employed, but some modifications were introduced in the method of observation, which are described; and the amount of probable error from different sources was determined. The liquids experimented on were either prepared or purified in Dr. Gladstone's laboratory, or were specimens reputed to be pure, and lent for the purpose of this inquiry by Prof. Hofmann, Prof. Williamson, Prof. Frankland, Drs. Warren De la Rue and Hugo Müller, Mr. Buckton, Dr. Odling, Mr. A. H. Church, Mr. C. Greville Williams, and Mr. Piesse. The data are collected in two long tables forming two appendices: the first containing the refractive indices of the lines A, D, and H, of 78 specimens at two or three different temperatures; the second, the refractive indices of all the more important lines for 61 of these liquids, and 10 others at the temperature of the room when the observations were made.

Five points were investigated, and the following are the results arrived at with respect to each point.

I. *The relation between the change of refraction* (*sensitiveness*) *and the change of volume by heat.*—The uniform testimony of about 90 different liquids examined was that both refraction and dispersion diminish as the temperature increases.

The following Table will suffice as an example, showing as it does that the different rays are more sensitive in the order of their refrangibility:—

Liquid.	Temp.	Refractive Indices.						
		A.	B.	D.	E.	F.	G.	H.
Bisulphide of Carbon......	11° C. 36°·5	1·6142 1·5945	1·6207 1·6004	1·6333 1·6120	1·6465 1·6248	1·6584 1·6362	1·6836 1·6600	1·7090 1·6827
Difference	....	0·0197	0·0203	0·0213	0·0217	0·0222	0·0236	0·0263

This change of refraction by heat was compared with the known or ascertained change of volume in bisulphide of carbon, water, methylic, ethylic, and amylic alcohols, ether, acetone, acetic acid, formic, acetic, and butyric ethers, methylic and ethylic iodides,

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* Phil. Trans. 1858, p. 887.

salicylate of methyle, bromoform, benzole, xylole, cumole, nitrobenzole, hydrate of phenyle, the rectified oils of turpentine and Portugal and eugenic acid, and in every case it was found that the refractive index minus unity, multiplied by the volume, gave very nearly a constant at different temperatures. Now every refractive index contains at least two coefficients: the one of refraction, which is represented by the theoretical limit of the spectrum; the other of dispersion, for which the difference between the refractive indices of H and A may be taken as the exponent. The refractive index, minus unity $(\mu-1)$, is termed by the authors the "refractive energy" of the substance, and this multiplied by the volume $(\mu-1)\times$ vol., or divided by the density, is termed the "specific refractive energy." It was not found as a rule that the theoretical limit of the spectrum gave more truly a constant than the line A; but the difference is within experimental errors. The empirical law was therefore expressed as follows:—The refractive energy of a liquid varies directly with its density under the influence of change of temperature, or, in other words, the specific refractive energy of a liquid is a constant not affected by temperature. Yet the influence of dispersion renders this not absolutely accurate in the observed numbers, for the change of dispersion does not follow the same law, the spectrum contracting in some cases much more, and in other cases much less rapidly than the volume increases; indeed no relation is as yet discoverable between the change of dispersion and that of density.

II. *The refraction and dispersion of mixtures of liquids.*—This question has engaged the attention of several experimenters, only one of whom, however, M. Hoek, has offered a solution. His formula depends on μ^2-1. Yet most of the results recorded were equally well explained on the supposition that the specific refractive energy of a mixture is the mean of the specific refractive energies of its components. It was clearly desirable to test this in some cases where the refractive indices of the liquids mixed were very wide apart. Fortunately, bisulphide of carbon and ether, substances almost at the opposite limits of the scale, were found to mix without condensation; and another good experiment was obtained with aniline and alcohol, on mixing which, however, some diminution of volume occurs. In both these cases the experimental numbers were slightly below those deduced from the mean of the specific refractive energies, the discrepancy being beyond the limits of probable error; yet no other formula could be devised which would give a nearer approximation to the indices actually observed.

III. *The refraction, dispersion, and sensitiveness of different members of homologous series.*—Many such series were examined, and the results are tabulated, the refractive index of A and the length of the spectrum or dispersion being reduced, if necessary, to 20° C., and the sensitiveness being taken for the 10 degrees rising above 20° C.; the specific refractive energy, dispersion, and sensitiveness also form part of the Tables. Methylic, ethylic, amylic, and caprylic alcohols are the first series examined, and it is found that on ascending the series the refraction increases; the dispersion does so still more rapidly, while the sensitiveness remains nearly the same.

Other homologous series of the same group, such as the iodides, compound ethers, or mercury compounds, were also examined, and they all agree in exhibiting a progressive change in refraction and dispersion with the advancing members of the series; but in which direction and to what extent depend on the other substances with which the compound radical is combined. Yet, if we regard not the actual indices, but these, minus unity, divided by the density, a pretty regular increase is found to take place as the series advance. The following Tables exhibit this:—

Specific Refractive Energy.

Radical.	Formula.	Alcohol.	Iodide.	Ether of Acid.	Formiate.	Acetate.	Butyrate.	Oxalate.	Mercury Compound.	Stannic Compound.	Hydride.
Methyle	$C_2\ H_3$	·4105	·2359	·3905	...	·389	...	...	·1707	·3727	
Ethyle	$C_4\ H_5$	·4482	·2614	·4127	·3905	·4127	·4402	·3502	·2112	·3376	
Propyle	$C_6\ H_7$	...	...	·4333							
Butyle	$C_8\ H_9$	...	...	·4402							
Amyle.........	$C_{10}\ H_{11}$	·4895	·3213	·4492	·4432	·4527	·4724	·4306			
Œnanthyle ...	$C_{14}\ H_{15}$	...	...	·4750	...	...	...	...	...	...	·5499
Capryle	$C_{16}\ H_{17}$	·5096	...	...	...	...	...	...	...	...	·5522
Laurostearyle	$C_{14}H_{25}$	...	...	·4890							

Specific Dispersion.

Radical.	Alcohol.	Iodide.	Ether of Acid.	Acetate.	Mercury Compd.	Stannic Compd.	Hydride.
Methyle	163	209	168	...	140	256	
Ethyle	190	218	174	174	170	268	
Propyle	...	...	191				
Butyle	...	...	191				
Amyle	212	224	198	198			
Œnanthyle	...	...	...	...	...	...	241
Capryle...........	237	...	...	...	...	...	237

Other groups of homologous bodies were also examined. Benzole, toluole, xylole, cumole, and cymole gave nearly the same numbers, and no regular progression. Pyridine, picoline, lutidine, and collidine showed an augmentation of the specific refractive energy, but a diminution of the specific dispersion with the advancing series. Chinoline and lepidine (which proved to be the most refractive organic liquid known) showed an increase of each of the optical properties by the addition of $C_2\ H_2$. Thus the influence of the added increment on the rays of light differs in different groups, just as it does in respect to the boiling-point.

IV. *The refraction, dispersion, and sensitiveness of isomeric liquids.*—Several of the liquids, isomeric with the different members of the benzole series, were examined; some proved to be identical in all optical properties; others sensibly the same in actual refraction and dispersion, though slightly different in density; some again identical in density, but differing in optical properties; while other isomeric bodies differed slightly in each of these respects. Several hydrocarbons of the type $C_{20} H_{16}$, from essential oils, seemed to be identical in actual refraction, notwithstanding slight differences of their density. In dispersion, too, there were some variations; but not in sensitiveness. Other hydrocarbons, however, of the same ultimate composition, but differing considerably in physical properties, differed also optically. Compound ethers, as valerianic ether and acetate of amyle, which contain the same numbero t carbon, hydrogen, and oxygen elements, though differently arranged, are optically identical, as was partially shown by Delffs some years ago. Aniline and picoline, each empirically $C_{12} H_7 N$, are totally different. The conclusion arrived at is that isomeric bodies are sometimes widely different in these optical properties; but that in many cases, especially where there is close chemical relationship, there is identity also in this respect.

V. *The effect of chemical substitution.*—By observing the amount of change in the optical properties which results from a replacement of one element by another, the chemical type remaining the same, it seemed possible to arrive at a knowledge of the influence of the individual elements on the rays of light transmitted by them. Of the immense number of data required for the perfecting of such an inquiry, the following are afforded by the experiments already made. The replacement of hydrogen by a compound radical, aniline—amylaniline: and water, alcohol, ether (according to Williamson's theory). Of hydrogen by oxygen—alcohol, acetic acid; ether, acetic ether; and carvene, carvole, eugenic acid. Of hydrogen by peroxide of nitrogen—benzole, nitrobenzole, dinitrobenzole (in solution); glycerine, nitroglycerine; and amylic alcohol, nitrate of amyle. Of hydrogen by chlorine—benzole, chlorobenzole, terchlorobenzole; and the substitution of chlorine by bromine—terchloride of phosphorus, terbromide of phosphorus; chloroform, bromoform; and bichloride of chlor-ethylene, bibromide of chlor-ethylene, bibromide of brom-ethylene. When hydrogen is replaced by some other body, there is generally an increase of the actual refraction and dispersion; but this is due to the increased weight, hydrogen having a very low actual, but a very high specific influence on the rays of light. In each of the five instances of two substitution-products, as, for instance, chlorobenzole and trichlorobenzole, the lower one always retains in its optical properties an intermediate position between the original substance and the higher product.

These experiments on substitution sufficed to show, as the examination of isomeric bodies had done, that the special influence exerted on the rays of light by the elements of a compound is greatly dependent on the manner of their combination.

The following is given as a generalization approximately, if not absolutely true:—Every liquid has a specific refractive energy composed of the specific refractive energies of its component elements, modified by the manner of combination, and which is unaffected by change of temperature, and this refractive energy accompanies it when mixed with other liquids.

LXIX. *Intelligence and Miscellaneous Articles.*

ON THE OXIDE OF A NEW METAL:—WASIUM.

In examining a Swedish mineral containing silica, alumina, yttria, oxide of iron, cerium, didymium, lime, manganese, and several other metals or metallic oxides, M. J. F. Bahr observed what appeared to be the oxide of a new metal resembling aluminium. The proportion of the new metal in the mineral seemed to be not higher than one per cent. For its extraction a very tedious process was employed, of which the first stage was solution in hydrochloric acid, and one of the latter stages precipitation with oxalate of an alkali, which throws down oxalate of the oxide of the new metal. The oxide itself, prepared by igniting the oxalate, was a white powder very slightly tinged with red. On treatment with nitric acid, the red tinge becomes somewhat yellowish, but the oxide seems to be otherwise altogether unattacked by the nitric acid.

An attempt to prepare an anhydrous chloride by exposing the oxide to the conjoint action of charcoal and chlorine did not succeed: at any rate no chloride of the new metal could be made to sublime.

The author proposes to call the new metal Wasium, after the god Wasa, and the mineral in which the metal was found Wasite. The symbol proposed is Ws.

The properties of the oxide are the following. Prepared by ignition of the nitrate, it appears as a brown sandy powder of the specific gravity 3·726. Before the blowpipe, it gives with borax a clear and colourless glass, in both oxidizing and reducing flames; with microcosmic salt also a clear and colourless bead. Examinations of its spectrum were without result.—Poggendorff's *Annalen,* 1863, No. 8.

In the *Comptes Rendus* for 2nd November of this year, M. T. Nicklès maintains that the oxide of wasium of M. Bahr is nothing more nor less than impure yttria—yttria containing some didymium or terbium.

INDIUM, ANOTHER NEW METAL.

F. Reich and Th. Richter announce a new metal which they have found in examining a specimen of arsenical pyrites in the mining laboratory of Freiberg. The chloride of indium is volatile, and was obtained along with chloride of zinc. The spectrum of the new metal contains a line of indigo-blue light more refrangible than the strontium blue line. There is a second faint blue line which almost, but not quite, coincides with the blue line of potash.

The reactions of the new metal, which the discoverers call Indium, are the following:—Sulphuretted hydrogen does not give any precipitate in the acid solution of the chloride. Ammonia throws down the hydrated oxide from the solution of the chloride.

The dry chloride of indium is deliquescent. The oxide of indium gives a bead of indium when it is ignited with charcoal and soda. The metal itself gives a yellow sublimate when it is heated before the blowpipe: this sublimate gives no characteristic reaction with cobalt solution. The metal is lead grey, ductile, and very soft.—*Journal für Prakt. Chemie,* September 1863.

Eminent chemists on the Continent consider that indium is *not* a delusion.—J. A. Wanklyn.

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SUPPLEMENT TO VOL. XXVI. FOURTH SERIES.

LXX. *On the Formation of Alpine Lakes.*

By JOHN BALL, *M.R.I.A., F.L.S.**

TWO distinguished Italian geologists, MM. B. Gastaldi and G. de Mortillet, have recently published letters† in which they seek to controvert the objections which have been raised to the theory of the formation of alpine lakes first published in 1859 by the last-named writer.

M. de Mortillet devotes some pages of his letter to the discussion of objections to his theory incidentally urged by me in a paper published in this Journal in the month of February last. The immediate object of that paper was to controvert views respecting the former extent of glacial action which had been advanced in this country by Professors Tyndall and Ramsay; and to that portion of my argument I understand that both writers give their assent. Neither they, nor, so far as I know, any other of the Swiss or Italian geologists who have studied this question, believe with Professor Tyndall that glaciers have been the main agents in the formation of alpine valleys, nor with Professor Ramsay that they are competent to excavate the deep rock basins, or troughs, that now contain the lakes of the Alps.

As the few observations which I made with reference to M. de Mortillet's theory have been evidently misapprehended by that gentleman, I am anxious to state rather more fully the objections which I have to advance, together with some evidence bearing on the subject that I have recently obtained.

The broad facts as to which there is a general agreement amongst those who have discussed this subject are as follows:—

1. The low country at the south side of the Alps, extending about 240 miles from the Isonzo to the west of Piedmont, is covered with a deposit of rolled stones, gravel, and sand, which has been generally called *diluvium*, but to which M. de Mortillet gives the name *ancient alluvium.*

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* Communicated by the Author.

† In vol. v. of *Atti della Società Italiana delle Scienze Naturali.*

2. Towards the opening of the great alpine valleys the diluvium, which extends for a greater or less distance into each of them, is found to be composed of rocks derived from the mountain-district drained by the valley. At a distance from the base of the Alps the materials of the diluvium spread over the plain exhibit a mixture of the rocks derived from different valleys.

3. There is no appearance of any local disturbance of the surface throughout the region in question since the deposit of the diluvium. The latest changes arising from upheaval of the surface must have been completed before that period.

4. All the main valleys descending from the Alps exhibit unequivocal traces of the action of ice. The glaciated surfaces of hard rock, the presence of erratic blocks perched upon steep slopes, and the remains of great moraines at the lower extremity of the valleys, demonstrate the former presence of glaciers, some of which must have been of enormous dimensions, perhaps exceeding 100 miles in length, and 2000 feet in thickness.

5. The deposit of the terminal moraines of these ancient glaciers must have been subsequent to that of the diluvium, as in many places the former may still be seen resting on the latter.

6. Throughout the central portion of the chain, extending from Domo d'Ossola to Riva in the Italian Tyrol, the principal valleys of the Alps contain narrow and very deep lakes, all of which lie within the area marked by the former action of ice.

Starting from these data, and rejecting Professor Ramsay's theory of the excavation of the lake-basins by glaciers, MM. Mortillet and Gastaldi argue that, as the materials of the diluvium are derived from the upper valleys of the Alps, the lake-basins must have been filled up when these materials were borne down to the plain of northern Italy. Refusing to admit that ice may have been the material that filled up the lake-beds, they hold that the diluvium must itself have performed this office, and so bridged over the space that must have been traversed before the great masses of diluvial matter can have been discharged into the plain. To account for the subsequent clearing out of the lake-beds, the glaciers are held to have descended into the lower valleys, and scooped out the incoherent masses of diluvium from the hollows which, on the subsequent retirement of the glaciers, became filled by the existing lakes.

This theory leads to two separate branches of inquiry—the one mainly physical, the other mainly geological. It may be asked, first, whether glaciers as mechanical agents are competent to do the work which has been attributed to them; secondly, whether the evidence fairly interpreted leads us to seek their agency in order to explain the phenomena.

In discussing the first question—that as to the competency of glaciers to excavate deep lake-basins—it will be convenient to fix attention upon a single case. The same arguments will apply, *mutatis mutandis*, to others. Taking, then, the former glacier of the Tessin, which descended into the basin now occupied by the Lago Maggiore, and leaving out of account the branch of the Toce glacier which entered the lake-basin between Pallanza and Baveno, we have the following conditions under which the proposed theory must be tested. Admitting that the lake-basin has been partly filled up in modern times, the ancient glacier of the Tessin, formed by the union of many ice-streams, of which the most important was that flowing from the Val Leventina by Faido, reached the level of the lake-basin at or near Bellinzona. The distance from Faido to that town is about 23 miles, and in that space the bed of the valley falls 1573 feet, so that the average slope is little more than 1 in 80. From Bellinzona to the lower end of the lake an approximately level bed of diluvium would, according to the theory, have extended to the lower end of the lake, a distance of 47 miles. The basin or trough containing this supposed mass of diluvium is in the form of a long valley, for the most part enclosed between steep walls of rock, very sinuous in form, having an average breadth of about two miles, but contracted at one part to about one mile, and enlarged elsewhere to a breadth of three miles. The depth of the trough throughout a great part of its length considerably exceeds 1500 feet, but at one point, about 34 miles from Bellinzona, it exceeds 2600 feet. A rough estimate derived from the ascertained depth of the lake gives the probable contents, supposing it filled up to its present level, at from 15 to 20 cubic miles. The reader is requested to consider how a glacier, under the circumstances here described, could have cleared out this prodigious amount of solid matter. It does not appear to me that those who have written on the subject have considered the mechanical problem at all closely.

The removal must have been effected, if at all, either by the front or tongue of the glacier while it advanced through the channel, or else by some forces brought into play when the mass of diluvium was covered by that of the glacier. I am willing to admit, as I have done in a former paper, that the tongue of the advancing glacier would probably have some slight effect in removing a superficial layer of such matter as the diluvium; but I see no reason to hold with M. Gastaldi that the effect in the case of a gigantic glacier like that in question would be much greater than that seen in existing glaciers. There is no reason why the tongue of a glacier 100 miles long should be thicker than that of one five miles long. The thickness depends on the relation between the rate of advance of the glacier and the rate of melting, and is greatest when a glacier descends rapidly from an upper level to a lower one where it encounters a rapid increase of temperature. A glacier that had flowed for 23 miles through a nearly level valley would taper gradually to a comparatively thin tongue. But the ploughing action of the front of a glacier depends, as I believe, much less on the weight of the advancing mass than on its tenacity. Glacier-ice, as we know, will bend to suit the shape of the rocky channel in which it moves; but some considerable force is necessary for the purpose, and that force is partly expended against the sides or bottom of the channel. Thus it happens that when a glacier descends a rapid slope, and then encounters a less inclined surface, the front does exert a considerable ploughing action, and shoves before it whatever incoherent masses may stand in its way, until it gradually adapts its bed to the new slope on which it has to advance*. I am relieved from the necessity of insisting on the limited power of the front of a glacier to excavate the diluvium, because this is implicitly admitted by both my opponents. M. de Mortillet has, indeed, been good enough to give several sections showing portions of terminal moraine resting on the diluvium, in positions where neighbouring portions of the same diluvium must have remained in their present position under the advancing front of the glacier.

If we abandon the idea that the glacier of the Tessin during the period of its advance pushed before it fifteen or twenty cubic miles of solid matter that had previously filled the channel of the lake, the next alternative will be to suppose that the glacier advanced until it covered over the underlying diluvium, and that by some agency, not yet explained, it gradually effected the clearing out from the trough of this enormous underlying mass.

If we had to consider a glacier lying in a valley with a slope of, say 5°, over a bed of diluvium a quarter of a mile in thickness, we should infer, from the few observations we possess on the retarding effect of the bed upon the motion of the ice, that the motion of the bottom of the glacier would be less than half of that of the surface, yet sufficient to exert such a grinding action on the subjacent stratum that in the course of ages the whole might possibly be ground down and removed. It would be an essential part of the process that, as the materials were pulverized, the streams flowing beneath the glacier would carry them away to some lower level in the form of

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* I am willing to admit that this action, exerted at the base of considerable ice-falls, may excavate in the underlying rock basins of slight depth compared to the thickness of the glacier, and may thus have produced some of the tarns seen in high mountain countries. But I believe these cases to be exceptional and unimportant.

glacial mud or fine débris. If in this imaginary case the slope of the supposed valley were reduced successively to 4°, 3°, and 2°, we should find that the period necessary for the operation of removing the vast mass of underlying matter must be prodigiously increased, as the increasing resistance offered by friction would diminish to a small fraction the onward motion of the ice upon its bed. If the slope were but 1°, we should hesitate to believe in the possibility of such a removal within any calculable lapse of time. Underlying rocks may be scored and glaciated by the under surface of a glacier, though it should advance but a few inches in the year; but the destruction and removal of vast masses of mineral matter require that the motion by which they are effected should be of appreciable amount.

It is needless to say that the above argument applies with still greater force to a glacier supposed to lie on a dead level of vast extent. My conviction is that under such circumstances the resistance offered by the bed would be far greater than that arising from the internal cohesion of the ice. In such a case the upper portions of the glacier would flow over the inferior portions, and the bottom would remain fixed on its bed. Let it not be forgotten that in the case of a monstrous glacier lying in a level channel 47 miles long, gravity would no longer have the least action in urging forwards the lower strata of the ice. The only force that we can imagine to act in impelling these forwards, would be that arising from the onward motion of the glacier in the upper alpine valley before it reached the level channel. But before we can admit that pressure can be transmitted through a vast mass of glacier-ice sufficient to overcome an enormous resistance, as though the whole were a rigid mass, we must burn the observations of Forbes, Agassiz, Tyndall, and other glacier-inquirers, and forget all the results they have recorded. Professor Tyndall found that in one portion of the Mer de Glace a slight increase of resistance to the advance of the glacier caused the longitudinal compression of a section of the glacier 1000 yards in length at the rate of 8 inches daily; and we are asked to believe that an enormously greater resistance, spread over a space 47 miles in length, would not prevent the transmission of force through the lower strata of the ice sufficient to overcome the obstacles to its onward movement. Of course the argument here offered applies *a multo fortiori* if we suppose the bed of the glacier to be concave, instead of being merely a level surface; but there is one further consideration which seems to be, if possible, still more conclusive.

Let us suppose that in the case here chosen for consideration the glacier had by some process, to me inconceivable, cleared out one cubic mile of the diluvium from the lake-basin, and that it were still enabled to exert some grinding action upon the remaining mass, which, at the lowest estimate, must have measured 14 cubic miles. By what imaginable means could this enormous mass of matter have been removed from the hollow trough in which, by hypothesis, it was contained? In the supposed case of a glacier lying in a valley over a stratum of incoherent materials, the streams formed under the ice would carry off the triturated materials as fast they were formed; but no such agency can be admitted here. The stream that would issue from such a basin would be merely the overflow of the water rising above the margin. My imagination fails to discover any process by which this could carry off the mineral contents of the basin, and continue to do so till this was drained to a depth exceeding 2600 feet.

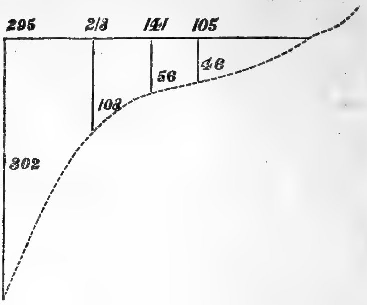
M. de Mortillet seems to suppose that I am not familiar with instances where the glaciated condition of the rock shows that the under surface of a glacier has ascended over a projecting surface. Such cases must be familiar to all alpine travellers, and may sometimes be traced in connexion with existing glaciers; but to infer from these that the entire mass of a glacier could have been urged for miles uphill, with its under surface sliding over the bed, is much as if I should conclude that because a waggon continues to roll down an incline, although a wheel may now and then pass over a stone, the same waggon might of its own accord travel up the incline, where all four wheels must ascend at the same time. When the force of gravity acting on an entire section of the glacier tends to urge it in the same direction as a force acting *à tergo,* it appears that the tenacity of the ice is sufficient to carry particular portions over convex projections in the bed. But we know from direct observation that the tenacity and rigidity of glacier-ice are confined within narrow limits; and if the force of gravity acting on a considerable section of the glacier were ever to act in opposition to the force *à tergo,* the ice acted on by these opposing forces would necessarily move upwards and outwards in the direction of least resistance.

I am well aware that to many geologists arguments derived from mere physical reasoning are not fully satisfactory, and that to such minds the observation of facts seeming to have a direct bearing on the point in dispute will be more conclusive. On this account I desire to mention some observations made during the last summer on a specific point, as to which M. de Mortillet conceived that he had given a satisfactory answer to an objection urged by me.

In the article already referred to, I made the sufficiently obvious remark that if the lake-basins had once been filled by diluvium, and were subsequently cleared out by glaciers, we ought to find the diluvium still *in situ* in those portions of the lake-bed which from local conditions must have been protected from the advance of the glacier. As an illustration, I referred somewhat at random to the rocky promontory of Balbianello, which projects from the west shore of the Lake of Como nearly at right angles to the general direction of the lake. If the glacier had descended from the north as a gigantic graving-tool, and had scooped out the diluvium from the channel of the lake, the promontory in question would have protected the diluvium lying on its south side, and it should still be found there at the present time. M. de Mortillet at once accepted my argument as a fair test of his theory, and has given what appears to be a sufficient answer, by stating that the diluvium, or ancient alluvium, is actually to be found in the locality which I had quoted. During the last summer I repaired to the place in question with the Marchese Giammartino Arconati, who inhabits the well-known villa on the promontory, provided with about 113 metres, or 370 feet, of sounding line.

It is not necessary that I should give the detail of all the soundings made by me on that occasion; but I desire to direct attention to four which have the greatest significance. It will be sufficient to say that, on sounding from the point of the promontory along its south shore, I found the rocks everywhere descend very steeply, apparently at an angle of about 60° with the horizon. Not only the steepness of the declivity, but the suddenness of the descent from one point to another, showed that we were over ledges of rock rising in a precipice as nearly vertical as is commonly found on the steepest face of a limestone mountain.

Having passed rather more than halfway along the southern shore of the promontory, we found a slight concavity, or very diminutive cove, where lay on a slope, stretching for some yards from the water, a small mass of shingle, consisting of waterworn stones with a little finer gravel. This is evidently the diluvium referred to by M. de Mortillet, and believed by him to be the remains of the vast mass which once filled the entire basin now occupied by the lakes of Como and Lecco. I immediately proceeded to make soundings at successive points in a line receding from the shore, opposite the shingle bank, measuring the exact distance from the shore at which each sounding was made. The result gives of course a rough outline of the vertical section of the bed of the lake opposite to this point, and is best seen in the annexed diagram, where the distance from the shore and the depth at each point are given in English feet, the dotted line showing the outline of the bed.



The fact that the bed of shingle seen through the clear water shelved very gradually from the shore, and the results of the two first soundings, appeared to myself and my companion confirmatory of M. de Mortillet's assertion, whatever the explanation of the fact might be. But the two succeeding soundings, coupled with the fact that at a short distance (less than 10 yards) further from the shore no bottom was found with 370 feet of line, proved to demonstration that the subaqueous precipice is continued along the S. face of the promontory, and that the objection originally taken by me subsists in all its force. I may remark that by following along the shore to the S.W., towards the hamlet of Campo, any one who may repeat the observations will approach the mouth of a stream which, like every other torrent falling into the lake, pours into it a mass of detritus that doubtless forms a conical mound beneath the level of the lake, not to be confounded with the ancient diluvium.

With the help of the annexed diagram the explanation of the fact noted by M. de Mortillet is sufficiently obvious. On all the more level spots surrounding the lake, extending from the shore to a height of about 2000 feet, deposits of unrolled or partially waterworn materials are to be found. These deposits are formed from rocks existing *in situ* in the alpine valleys surrounding the head of the lake, and were doubtless conveyed by ice to the spots where we now find them. Whether originally waterworn or not, a deposit of such materials exposed for ages to the action of the water on the lake-shore would necessarily be reduced to the condition in which the small shingle-bank in question was detected by the keen eye of M. de Mortillet, and pressed into service as a witness for his theory.

I shall naturally be asked by some who may have followed me so far, what explanation I have to offer of the relations between the diluvium and the more recent glacial phenomena on the south side of the Alps to which M. de Mortillet has called attention. To this I reply that a complete answer cannot, as I believe, be given until the whole of the facts shall have been minutely studied with a degree of care that has not yet been applied to them, and which indeed would not have been practicable until the nature of the problems to be discussed was somewhat defined.

M. de Mortillet has, if I may venture to say so, done excellent service to science by calling attention to the difficulties that have to be explained, and by observations, in many of which he has preceded other inquirers; but he has proposed an explanation which seems to me essentially defective, while he has turned his eyes away from the direction whence light might have been shed on the subject.

MM. Gastaldi and Mortillet have put aside with little discussion the belief expressed by me that the distribution of the diluvium in the north of Italy, and its relations to the undoubted remains of glacial action, cannot be accounted for without admitting the presence of the sea in the valley of the Po during some portion of the glacial period. I have no pretension to speak dogmatically on the subject, but I may say that my conviction on this subject has arisen altogether from observations made during repeated excursions in the southern valleys of the Alps, and has been more and more confirmed as I have found that, although there may not be evidence amounting to direct demonstration, each new visit to the Alps has added to the list of observations which are consistent with this belief.

In the first place, this hypothesis has in its favour no trifling amount of *à priori* probability. There is no evidence whatever pointing towards any local disturbance of level in the Alps since the miocene period; it is therefore the more probable that the entire chain should have participated in any general change of the relative level of land and sea that affected the adjoining regions during the later geological period. We have conclusive evidence to show that in those islands the sea stood during a portion of the glacial period at least 1500 feet higher than it now does. We know also that a considerable part of North Germany was submerged, as well as a still more extensive region in Northern Africa. Leaving out of account other more disputable instances of recent change of level, these facts suffice, as I think, to create a presumption in favour of the view which I advocate, and which, if I mistake not, is supported by the high authority of M. Omboni.

One of the main reasons which makes probable the presence of the sea in the valley of the Po, is the wide dispersion of the diluvium throughout the great plain at the foot of the Alps. The diluvium is not merely found at the opening of the main valleys that penetrate deeply into the interior of the Alps; it is spread along the base of the entire range, in situations quite out of the path of those imaginary currents that are said to have swept the rolled débris from the valleys into the plain. In some places, as on the plain of Friuli, it is spread out on a dead level in a uniform stratum, without the slightest break, and with scarcely a ruffle on its surface. These are effects which no currents of running water are competent to produce. No known cause, excepting the waves of the sea, or of a great lake beating on a shallow bed, will spread out shingle in the fashion which we see in Northern Italy.

It is supposed that because the diluvium consists of materials derived from the upper valleys of the Alps, and because it is composed of waterworn fragments, it must have originally been conveyed to the plain in the condition in which we see it. The inference appears to me altogether unnecessary, and, for reasons presently to be mentioned, very improbable. I believe ice to have been the main agent for the transport of the diluvium as well as the moraines, and that its waterworn character is due to trituration by wave-action on a shallow shore. Besides the difficulty of admitting that currents descending the valleys can have coated the plain with a uniform stratum of diluvium, it is equally hard to understand how they can have carried the diluvium across the level lake-basins, supposed to have been filled up, so as to reach the plain. A slope of at least 10 feet per mile, or 1 in 500, is necessary to enable a river to transport rolled gravel of the size required. This objection has been perhaps overstated by Lombardini, but is clearly a serious one. We ought to find traces of diluvium at a great height about the head of each of the greater lakes. No such traces have been found. Even if great currents were able to transport and spread out the diluvium as we now find it, which I cannot concede, it is not easy to see how such a violent and continuous current as is required can have originated in each of the alpine valleys. The estimate of rainfall cannot be reasonably increased beyond moderate limits; and the greater cold of the climate, by increasing the proportion of snow in each fall, would have tended to diminish the violence of the current, and to make it more uniform in volume. The only cause likely to have acted at all has not been pointed out, so far as I know. During the period of increase of the glaciers, cases may probably have arisen in which a main valley was barred across by a glacier descending from some lateral gorge or glen; and a lake having accumulated behind the barrier, this grew until it burst its bounds, and caused a flood of the same character as the well-known inundation of the Dranse in 1818. But such events must have always been of a local and accidental character; it is impossible to look to them for the explanation of a phenomenon so vast and so general as the transport of the diluvium. In connexion with the preceding paragraphs, it is worth noting that the Dranse inundation, the most considerable event of this kind accurately recorded, does not seem to have carried any coarse débris below St.

Maurice in the Valais, none certainly was borne nearly so far as Villeneuve.

Several writers on the glacial period seem to have taken it for granted that very extensive glaciers must have produced streams vastly more considerable than those that now flow through the alpine valleys. A moment's consideration shows that this is a mistake, if we admit that whatever climatal changes may have occurred during the period in question were gradual and not abrupt. Any other supposition would imply the sudden destruction of most of the existing species of animals and plants. During the period of the increase of the glaciers a part of the rainfall of each year would have been converted into névé, and conversely, during the period of decrease a corresponding portion of glacier-ice would have been annually melted. The streams could have been no greater than sufficient to carry off the annual rainfall, diminished in the one case, and increased in the other, by the quantity gained or lost by the glacier.

Believing as I do in the presence of the sea in the valley of the Po during a portion of the glacial period, I am prepared to believe that amongst the effects hitherto attributed to glaciers alone, some portion may be due to the action of floating ice. It seems to me highly probable that during the earlier portion of that period the sea entered into the main valleys on the southern side of the Alps, and that moraine-laden glaciers entering these fiords were floated down to the open sea, where they would rapidly melt, depositing their burdens at the sea-bottom. At a later period it seems probable that the sea-level gradually fell, while the glaciers advanced and gradually filled up the lake-basins, finally depositing their moraines about the openings of the greater valleys. Considering the enormous mass of mineral matter brought down during this period, the operation must have covered a vast extent of time; and analogy would lead me to expect that the changes of climate were not uniform. There are indications of halts, during which the glaciers were stationary, and of oscillations that probably caused their retreat and subsequent advance. The time necessary for the melting of such a mass of ice as would have filled one of the great lakes is so great, that it is probable that they may have been partially or entirely occupied by ice after the glacier had retreated to a considerable distance in the valley above the lake.

In regard to some of the vast piles of débris that have been described as moraines of extinct glaciers, and especially those south of the Lake of Garda, it appears to me that a considerable portion was accumulated by the stranding of ice-rafts, or masses of floating glacier on the shallow sea-bottom opposite the mouth of the valley. Some considerable portions of the mass are flattened out and accurately levelled at the top, in a way altogether inconsistent with the idea of their being ordinary moraines left *in situ* by the retiring glacier. It is not allowable to suppose that the lake itself, when formed by the melting of the glacier, can have done this work, unless we admit that at the period when the moraine amphitheatre was formed the sea beat upon its outer face at a level considerably above the present plain. In that case the streams cutting through the terminal moraine to give exit to the drainage of the glacier would have gone as low as the then sea-level, but no lower; so that at a later period, when the sea had retired, and the lake was formed by the melting of the glacier, this might have been kept at a higher level, being held in by the circuit of hills constituting the terminal moraine, which would have been gradually cut through by the Mincio. During the interval, the lake, acting on the materials lying within the amphitheatre, may have modified their form and arrangement, though I think the appearances agree better with the supposition that the level-topped mounds were formed from stranded ice-rafts, as already mentioned. If the glacier when at its furthest had advanced into the plain, this being formed of dry land, the action of the streams issuing from the glacier would have kept a channel open, and no bar would have been formed by the terminal moraine capable of holding up the level of the lake.

It is only by careful levelling along the banks of the lakes, and the alpine valleys connected with them, that it will be possible to distinguish accurately between the traces of extinct glaciers and those of floating ice. Many peculiarities in the distribution of erratic blocks and glacial drift in the valleys of the eastern Alps would lead me to believe that the sea, during some part of the glacial period, must have there reached a considerable height, approaching, if not exceeding, 1800 feet above the present level; but without more complete and more accurate observations I place no confidence in this conclusion.

I think I have shown in the foregoing pages that the theory of M. de Mortillet involves a chain of hypotheses each of which is inadmissible.

It is not admissible to assume that violent currents existed in each of the valleys of the Alps, capable of bearing down such an enormous amount of diluvium as would have filled up the great lakes; it is inadmissible to assume that, if such currents did exist, they could have carried the diluvium along the level floor of the lake-basins so as to reach the plain; it is not admissible to assume that, if it had reached the plain, it could have been evenly spread out over the surface by the supposed currents; lastly, it is an assumption entirely inadmissible that the lake-basins, if filled with diluvium, could have been cleared out by glaciers. Whatever may be the true solution of the interesting problem proposed by M. de Mortillet, that furnished by him will not, I feel sure, be accepted by geologists.

I believe that, in indicating the presence of the sea in the valley of the Po, I have pointed out the quarter whence a true solution of the difficulty is to be derived. The solitary argument that seems to be relied upon by M. de Mortillet to negative this belief, is the fact that marine remains have not been discovered in the diluvium. If negative evidence of this nature were held to be conclusive as to the validity of geological theories, we should have to alter many of the received doctrines of the science. Deposits of enormously greater extent than the diluvium in the valley of the Po are universally believed to have been formed by marine action, although no marine fossils have yet been identified in them; and it is especially in regard to strata formed of rolled fragments of hard rock that we should expect the destruction of whatever scanty organic remains they may once have contained. I may be allowed to cite the following facts as illustrative of the small value of negative evidence in regard to the very geological period now under discussion. The presence of the sea at a considerable height on the mountains in the British Islands during a portion of the glacial period is now universally admitted; but no marine remains have been found on the lower western slopes of the mountains of North Wales, although these must have been in succession portions of the shore during the advance and subsequent retirement of the sea. Only in two or three spots, at a height of from 1200 to 1350 feet, have sea shells been found. The chief deposit, first discovered by Mr. Trimmer, is near the summit of Moel Tryfaen, a sharp conical peak, where no one would suspect its presence. From this locality, as I have been informed by Sir Charles Lyell, fifty-six existing species of shells, for the most part Arctic or northern British, have been extracted, while in another spot, where similar remains were detected by Professor Ramsay, but few and imperfect fragments have been found. When it is remembered that the district in question has been over and over diligently examined by careful geologists, the absence of organic remains over so wide an area affords a striking commentary on the value of negative evidence in geology.

Even though the presence of salt water in the valley of the Po during the glacial period were absolutely disproved, this would not remove the possibility of the existence of an inland freshwater sea in the same area, whose physical effects in the production and diffusion of the diluvium would have been undistinguishable.

While correcting the erroneous statements of Sonnini and others, the labours of Admiral Smyth have verified the existence of a bar extending from Sicily to the opposite coast of Africa; and in the Adriatic it is well known that the depth of the bottom between Otranto and the opposite coast, though considerable, is much less than either north or south of the "narrows." This is not the occasion for discussing the probability of recent changes in a region which has long been the seat of intense volcanic action; it is enough to say that, if the rein be given to speculation, it may take other directions than that in which it has been set free by M. de Mortillet.

The broad facts are simply these, that an enormous quantity of mineral materials has somehow been transported from the central portions of the Alps to the valley of the Po; that the older part consists of a waterworn deposit spread out over the plain, while the newer portion assumes the shape of mounds of unrolled or slightly worn materials disposed about the opening of the main valleys. All are agreed that these mounds are moraines, partially or exclusively deposited in their present site by glaciers which descended the valleys and filled up the basins of the lakes, where such were present in the path of the extinct glaciers. The same vehicle was equally competent to convey the materials of the diluvium; but we have no direct evidence to show how this was reduced to its present rolled and waterworn condition, nor how it was spread out over the low country. It is only by full discussion of the problems remaining to be solved that we are likely to arrive at reliable solutions, and it is to this end that I have offered the foregoing remarks.

LXXI. *On the Elasticity of the Vapour of Sulphuric Acid.* By Thomas Tate, *Esq.*[*]

[With a Plate.]

THE method hitherto employed for finding the elasticity of the vapours of different liquids fails when the liquid (such as sulphuric acid) acts chemically on mercury. In the apparatus which I have used (Plate VII. fig. 8), the liquid does not come into contact with the mercury employed in the experiments. A B C D is a bent glass tube containing the acid, connected with a brass tube D, from which proceeds the barometer-tube D E, dipping into a cup of mercury E; V a union joint; K a cock connecting the tube with an exhausting air-pump P; A an oil-bath, secured to the tube A B by means of a perforated cork;

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[*] Communicated by the Author, having been read to the British Association at Newcastle-upon-Tyne in August 1863.

this bath is heated by gas-jets G; T a thermometer suspended in the bath to determine its temperature when the vapour is being formed in the upper portion of the tube A B, coincident with the column of mercury in the barometer-tube D E. The tube being filled just beyond the bending C with the liquid to be experimented upon, the air is expelled from the liquid by continuous ebullition and exhaustion. It will be observed that whilst the upper portion of the acid in the tube is being heated, the lower portion, owing to the slow conducting-power of the liquid, remains nearly at a constant temperature. The pressure of the vapour when formed is found by adding the column of acid in the tube A B C, converted into inches of mercury, to the column of mercury in the barometer-tube, and then subtracting this sum from the barometric column, measuring the pressure of the atmosphere at the time of observation. After experimenting with strong sulphuric acid having a specific gravity of 1·846, this acid was diluted with different equivalents of water, and then these diluted acids were successively subject to experiment. The following Table gives the results of one of these experiments:—

Experiment.—Elasticity of the Vapour of strong Sulphuric Acid diluted with one equivalent of Water, the formula of composition being $SO^3 HO + HO$.

Temperature in degrees F. T.	Corresponding pressure of vapour in inches of mercury. p.	Value of p by formula $\log p = 6{\cdot}307 - \frac{3062}{T+230}$.	Value of p from the formula $p = p'$.
290°	2·15	2·62	1·89
320	5·00	5·49	4·48
350	10·00	10·65	9·61
370	16·00	15·98	15·28
390	23·04	23·34	23·45
400	28·00	28·00	28·75

Here the results in the third column show that the proposed formula very nearly represents the results of experiment: similar formulæ were found to represent the results of all the other experiments. In the fourth column, p' is put for the pressure of the vapour of water at a temperature of 190° below the corresponding temperature given in the first column: thus, for example, 15·28 is the pressure of the vapour of water at 180°, that is, 190° below 370°, the corresponding temperature in the first column: the near coincidence of the results in the fourth and second columns shows that this law (known as Dalton's law of the elasticity of vapours*) is approximately true as applied to this liquid. This law was found to apply, with nearly equal exactness, to the results of all the experiments with diluted acids.

In the experiment with stong sulphuric acid, it was found that at a pressure of 2 inches the corresponding temperature was about 500°, and at the mean pressure of the atmosphere about 640°. But, owing to the exceedingly high temperatures employed in this case, the temperature corresponding to any given pressure could not be determined with much precision.

Hastings, November 14, 1863.

LXXII. *On the Correct Interpretation of the Electrical Terms Intensity and Tension.* *By* Sir W. Snow Harris, *F.R.S.*†

[With a Plate.]

1. THERE have been few scientific terms so indefinitely and loosely employed in the sister sciences of electricity and magnetism as the terms "intensity" and "tension." The electrical term "quantity" is intelligible enough, and is understood to designate the actual amount of the unknown agency, whatever it be, constituting electrical force. The term "intensity," however, has been made to represent all sorts of hypothetical views of the occult nature of electricity itself, often vague and unsatisfactory: amongst others it has been made to signify an hypothetical elastic quality, the electrical agency being supposed capable of changing its state or condition—just as we may imagine a spring to have greater or less elastic power. M. De la Rive observes, "Besides the quantity of electricity, it is necessary to know its density," thereby inferring a greater or less degree of what may be termed solidification or compactness of particles, and assuming the electrical agency liable to compression or expansion. "La densité," he observes, "peut être déterminée directement au moyen d'un électromètre à poids," that is to say, by an intensity-electrometer.

Again, Riess, as also M. De la Rive, infers that in distributing a given quantity of electricity over three or four jars instead of one jar, its density or elastic force is thereby diminished, and a change effected in the quality of the electric agency; they are hence led to represent this hypothetical change by the expression

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* *At equal distances from the boiling temperatures the elasticities of the vapours of different liquids are for the most part nearly equal.* This law has been found to be approximately true for the vapours of water, alcohol, ether, and sulphide of carbon.

† Communicated by the Author.

$\frac{}{s}$ in which q signifies the quantity of electricity, and s the extent of the coating or surface over which it is expanded.

In like manner it has been commonly observed that in the construction of voltaic batteries a few large plates principally relate to quantity; whilst in breaking up those plates into a long alternating series of smaller ones, we are supposed thereby to impart to the voltaic currents *intensity*, or elastic power.

The term "intensity" has also not been unfrequently employed to signify tension, or reactive force—as when a dielectric medium interposed between metallic coatings becomes ruptured or broken down by polarized molecular action. It has been employed in other senses, all of them conveying hypothetical views of the agency in operation.

These various interpretations of the term intensity have long since appeared to me unsound and unsatisfactory, and unsubstantiated by a rigid inductive philosophy, resting entirely on mere assumptions of the occult nature of electricity.

2. I have in vain sought for experiments which could at all lead us to infer the possibility of effecting a change in the condition of the electric agency (whatever it may happen to be), without at the same time varying the quantity of the agency in operation. We have no means, for example, except by means of induction, of changing the angular divergence of an electrometer attached to an insulated charged surface whilst the quantity, remains the same—that is to say, whilst the quantity affecting the electrometer is invariable; in other words, we have no means of altering the state, condition, or quality of the electrical force absolutely in operation. On the other hand, we have the means of varying the quantity and yet of preserving the angular divergence of the electrometer constant.

Let, for example, A, C, S, figs. 1, 2, 3, Pl. VII. be three insulated conducting bodies of different forms and magnitude; let A be a circular plate of small thickness, C a cylinder, and S a sphere, and suppose each furnished with a delicate electrometer of divergence, *a*, *c*, *s* (the three electrometers being precisely alike); further, suppose these three conductors to be charged with electricity to such an extent as will bring each of the electrometers to the same angle: in this case, as is well known, the quantity of electricity upon each of these bodies will be very different, notwithstanding the sameness of the electrometer indication. Here the electrical intensity of each of the bodies is said to be the same, and the electrical charge to have the same density in each. Again, take two insulated conducting spheres M, N, figs. 4 and 5, of unequal diameters, and suppose the surface of the one to be three times that of the other, having delicate electrometers *m, n* attached to them as in the last case: then, if we charge each of the globes with the same quantity, the electrometer on the small sphere M will have a much greater angular divergence than the electrometer on the large sphere N; from which it has been inferred that the electricity on the small sphere is more intense than the electricity on the large sphere, or, in other words, that we have varied the intensity without changing the quantity.

3. A further investigation of these phenomena, however, leads to a more definite and simple explanation of them. When we place different quantities of electricity on the three bodies A, C, S, figs. 1, 2, 3, without affecting the angular divergence of the electrometer, it is the total quantity which varies, and not the quantity in any given point of the surface (that is to say, the absolute quantity affecting the electrometer): the quantity affecting the electrometer is really the same in each case, although the total quantity upon each of the three bodies may greatly differ. It is in fact quite evident that, since the three bodies vary in extent of surface, the greater the extent of surface the greater the number of points it will contain, and consequently the greater will be the amount of electricity requisite to supply them: hence the quantity in any one point of either of the bodies, granting it not to be in the simple ratio of the surfaces, may still be the same, notwithstanding the quantity in each of the bodies is different.

4. Now the total quantity or charge must be taken to act upon an electrometer projecting from its surface as it would do supposing the electricity were equally distributed; this is evident from the fact that, in whatever point of the surface the electrometer be placed, its angular divergence remains the same. Intensity in this case, therefore, when rigidly interpreted, is nothing more nor less than the quantity of electricity at a given point of a charged surface acting on the electrometer, and cannot possibly be taken to express any difference in the actual state or condition of the electrical agency.

5. Let us now take the case of a different angular divergence of the electroscopes, the quantity of electricity being the same. Take, for example, the case of the two unequal spheres M, N, figs. 4 and 5, just adverted to, each furnished with a delicate electroscope *m, n,* and let the same quantity be transferred to each sphere; the electroscope *m* of the sphere M will in this case, as already observed, have a much greater angular divergence than the electroscope *n* of the sphere N. Here we perceive that the quantity in any one point of the two spheres is not the same, because the quantity being the same in each sphere, and the surface over which it is expanded in the one sphere being of much less extent than the surface over which it is expanded

in the other, there would be a greater number of points in the large sphere for the reception of the charge.

Let the surfaces, for example, be as 3 to 1; then where there is one particle on a point of the large sphere, there will be three on a point of the smaller sphere. Here, again, intensity, when correctly interpreted, is nothing more nor less than the quantity of electrical force at a given point. It by no means follows, however, that in this case the agency in operation has a higher amount of elastic power in the one case than in the other, or is necessarily more or less dense: if it exhibits a greater degree of energy in any given point of the one body than it exhibits in any given point of the other, it may be because there is a greater amount of mere force, in operation in that point. We really cannot say, until we have a clear conception of the nature of electrical force, whether it is susceptible of change in quality or constitution or not; and any inference, therefore, of a change in electrical density or tension must be mere assumption. The idea of a difference of density in the two cases of the small sphere and the large one (figs. 4 & 5), supposing the electrical agency to be material and subject to the laws of ordinary matter, is certainly in accordance with the deduction that, "where there is one particle of force acting in a given point of the large sphere, there are three particles in a given point of the small sphere." So far this corresponds with the theory of density, or, as better expressed perhaps by the French philosophers, with "thickness of stratum;" but, as already observed, since we have no knowledge whatever of the occult nature of the electrical agency, we can scarcely venture to rely upon any hypothesis of this kind, but must be content to consider electricity as mere force, without assigning to it any specific elementary condition—much in the same way as we accept gravity as a mere force, without troubling ourselves as to its occult nature.

6. It follows from this that there is no such element as intensity independent of quantity, and that what we are to understand by intensity is only the greater or less quantity of force in a given point operating on the electrometer. We accordingly find that what is called intensity, as indicated by the electrometer, is subject to all the laws of quantity. It is in this sense I propose the term intensity to be accepted—an acceptation which frees it from all mysterious and hypothetical uncertainty whatever.

7. In considering the term intensity as applied to a charged electrical jar, similar considerations present themselves. The electrical jar is simply an electrical condenser upon Volta's principle. We have here an insulated conductor (that is, the inner coating) in close approximation to a similar conductor (the outer

coating) in connexion with the earth. Now we know, by very familiar experiments, that, if a neutral conducting surface H, fig. 6, in connexion with the earth be caused to approach a charged surface—as, for example, the plane R,—an electroscope *r* divergent to a given angle begins to subside, in consequence of the total force not operating in the direction of the electrometer *r* exclusively, but also in the direction of the approximated surface H; there will not in this case be the same quantity free to act in the direction of the electroscope as there was before. We should require therefore under this new condition an increased quantity to be deposited upon the surface R before the electroscope could again diverge to the same angle. We have evidently in this experiment effected no specific change in the electrical agency; we have merely caused the same or a less quantity to operate on the electrometer in a given point of the surface R.

8. Now this is precisely the condition of the electrical jar, fig. 7. If we suppose the outer coating N not present, and the jar insulated, a small quantity accumulated on the inner coating causes a considerable divergence of the electrometer E attached to the extremity of its charging-rod. Directly, however, we assume the presence of the uninsulated external coating N *n*, the influence of this upon the inner coating through the glass so diverts the force acting in the direction of the electrometer E as to cause its angular divergence to subside. We have here only to consider, as is evident, mere force, and the direction or directions in which it acts, and not an occult material agency possessing variable hypothetical qualities of which we have no clear or intelligible evidence, and the assumption of which only serves to confuse and embarrass a simple idea.

9. Let us now suppose attached to the charging-rod of the jar a Lane's discharger L, the negative ball of which, directly opposed to the positive ball of discharge *m*, is in connexion with the outer coating N*n*, through an external circuit *o m l* N; then, as we continue to add electricity to the inner coating, the force in the direction of the discharging electrometer L begins to increase, and continues to do so until discharge ensues between the inner and outer coatings through the external circuit *o m l* N. We may here consider the total force in operation to be exerted between the coatings in two directions; that is to say, immediately through the intervening glass of the jar on the one hand, and through the external circuit *o m l* N on the other.

As the amount of force through the Lane's discharger increases, the force between the inner and outer coatings through the glass decreases, inasmuch as the operation of the accumulation upon the outer coating through an external circuit in the direction of the discharging electrometer is such as to subvert the negative state of the external coating, and lessen its influence upon the inner coating through the glass; eventually, therefore, as the accumulation increases, the force between the coatings through the glass is entirely superseded by the force between the coatings through the external circuit. We see nothing in these simple operations which would lead us to conclude that the increasing divergence of the electroscope E is indicative of increasing elasticity, tension, or other change of quality in the unknown agency.

10. If, in accumulating a given quantity over three or more equal electrical jars instead of one (as quoted by Riess), we find the divergence of the electroscope greatly reduced, that is evidently because there is less accumulation in any one point of the charged surfaces, as we have already explained (5), and the electroscope is consequently not affected to the same extent as it would be if the accumulation were confined to one of the jars only.

We really cannot infer from these phenomena any change in the quality of the electrical agency. It is true that the force of the discharge will be something less when the accumulation is on three jars instead of one jar; but that may be shown to arise from the increased resistance to discharge, in consequence of the greater number of charging rods and wires the accumulation has to traverse, and other circumstances attendant on the detached surfaces. If the accumulation be confined to a large jar and a small jar taken separately (in which the charging rod is the same), the force of the accumulation would be pretty much alike in each: for example, fifty measures accumulated on a jar exposing 6 square feet of coating, produces nearly the same heating effect upon a fine wire as fifty measures accumulated and discharged from a jar of 3 feet or only half its surface, although the relative electrometer intensities are nearly as 4 : 1 (supposing the thickness of the glass to be the same); the electrometer indication is four times as great on the small jar.

The force of the discharge, therefore, in this case bears no sort of proportion whatever to the relative intensities, considered as indicating any difference in the constitution of the accumulated electricity. The force of the discharge is certainly something less from the large jar, the amount of contact between the coatings and the glass being much greater in the case of the large jar. The accumulated electricity does not in fact escape so freely from the charged side of the large jar as it does from the charged side of the smaller jar; and hence a somewhat greater resistance to discharge. A moment's reflection on the nature and construction of the electrical jar, and the relation subsisting between its coatings, will be sufficient to further elucidate this important fact. It is clear that the positive and negative coatings are more or less attractive of each other through the intermediate glass. Hence the greater the extent of coating upon which a given quantity of electricty is accumulated, the greater will be the number of points of positive and negative force in absolute contact with the intervening glass, and consequently the greater the hold which the coatings may be supposed to have upon each other; hence the greater will be the resistance to discharge through an external circuit. On the other hand, supposing a given quantity to be accumulated on a comparatively small extent of coated surface, then the *fewer* will be the number of points of positive and negative force in immediate contact with the intervening glass, and hence the coatings have a less hold upon each other than in the former case; discharge in the direction of an external circuit will therefore be more free and more readily effected. Now we know, from well-established experiments, that the heat elicited in metallic wires by a given quantity of electricity will be less as the resistance to discharge is greater; a given quantity therefore discharged from a small extent of surface may possibly have in some instances a somewhat greater effect on a metallic wire than the same quantity discharged from a large extent of surface, since the accumulated electricity disengages itself more freely from a small surface than from a large one; the difference, however, is in no case considerable, and has no relation whatever to what has been termed electrical intensity.

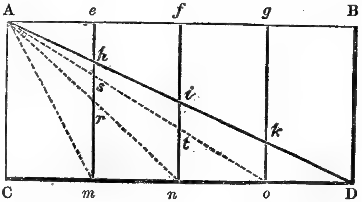
11. Again, as already observed (1), in the construction of voltaic batteries, a few large plates principally relate to quantity; whilst in breaking up those plates into a long alternating series of smaller plates, we are supposed thereby to impart to the electrical agency intensity or tension. Here again we may perceive that the apparently increased energy of the voltaic force in the latter case is solely due to the increased quantity in a given point of the terminating plates. Imagine, for example, four series of voltaic plates of 1 foot square to be divided and arranged in a voltaic series of thirty-six consecutive plates of only 4 inches square. In this case the terminating plates of the small series have only the one-ninth part of the surface of the terminating plates of the large series; there would consequently (supposing the quantity developed by the voltaic action to be the same in each case) be nine times the quantity in a given point of the terminating plates of the small series that there would be in a given point of the terminating plates of the large series.

Intensity therefore still agrees with quantity acting at a given point, and by no means implies elastic power, tension, or any other of those hypothetical occult qualities above alluded to. The heating power of the current-discharge, however, from the large plates, unlike that of the large and small jars above quoted, is greater than from the series of small ones, inasmuch as there is less obstruction in the way of discharge from a series of four large plates than from a prolonged series of thirty-six small plates, whilst, on the other hand, there will be a greater quantity on the terminating small plates of the long series, and consequently a more intense action in those plates. Hence it has been said that heating effect depends on quantity, and chemical action on intensity.

12. The term "*tension*" is more especially applicable to the polarized condition of accumulated electric particles, or to the state of the particles of any dielectric medium intermediate between a positive and negative surface, as, for example, to the particles of the glass of an electrical jar interposed between its inner and outer coatings, or to the particles of the atmosphere or other dielectric medium intermediate between a positive and negative surface. These particles, as Faraday has shown, assume a peculiar polarized state, which state, when the electrical accumulation upon the opposed conducting surfaces exceeds a given amount, they can with difficulty maintain, in which case the electrical strain upon them becomes so great that they return with violence to their normal condition, so that discharge ensues through the intermediate dielectric medium. This electrical strain or condition to which the particles are thus subject may hence be fairly characterized by the term tension; and therefore this term may be defined to be the constrained state of the particles of any electrical accumulation, or of any dielectric medium interposed between two oppositely charged surfaces; whilst the term intensity applies to the quantity of electricity at a given point of a charged surface acting upon the electrometer.

In accordance with these views of tension and intensity, we find that whilst intensity (that is to say, the electrometer indication (1)) is as the square of the quantity, electrical tension, or disruptive force, as measured by a Lane's striking electrometer, is simply as the quantity.

13. The distribution of force alluded to (9) in the charge and discharge of the electrical jar, admits of geometrical representation in a way calculated to elucidate in a satisfactory manner the precise condition of this question.



Let the line A C in the annexed diagram move forward parallel to itself upon the line C D, and generate the equal rectangular spaces A *m*, *en*, *fo*, *g*D; then these spaces may stand for and represent equal and successive quantities of electricity communicated to the electrical jar; and the total rectangular space A B C D may stand for, and represent the total charge. Now, since the force between the coatings continually decreases (9) as the force through the external circuit increases, therefore these forces may be represented by any magnitude supposed to flow, and at the same time continually increase or decrease. Let therefore the line A C move forward parallel to itself as before, and continually decrease by the linear magnitudes *e h*, *f i*, *g k*, BD) which are proportional to the lines A *e*, A *f*, A *g*, A B, that is to the respective quantities of electricity C *e*, C *f*, C *g*, C B), and so generate the triangular space A D C, which may stand for and represent the total force in the direction of the coatings up to the point D, where it vanishes in explosive discharge. Similarly let the extremity A of the line C A flow and gradually increase by the same proportional lines *e h*, *f i*, *g k*, B D, generating the triangular space A B D, which may stand for and represent the total force in the direction of the circuit up to the same point of explosion D. This being understood, we may observe that the first quantity accumulated being represented by the space C *e*, the force in the direction of the coatings will be represented by the rectilinear space C *h*, and the force in the direction of the external circuit (that is, the electrometer indication) by the triangular space A *e h*. Similarly a double quantity of charge will be represented by the double space C *f* = twice C *e*; the force in the direction of the coatings by the rectilinear space C *i*; and the force in the direction of the external circuit by the triangular space A *f i*, and so on. But the triangles A *e h*, A *f i* are to each other as the square of A *e* to the square of A *f*; and since A *f* is double of A *e*, these triangular spaces are as 4 : 1. In the same way it may be shown that the triangles A *g k*, A *e h* are as 9 : 1, and so on. Now, as these triangles stand for and represent the force through the external circuit, that is to say, the respective electrometer indications, whilst the sides A *e*, A *f*, A *g*, &c. are proportional to the respective accumulations C *e*, C *f*, &c., we see that the force in the direction of the circuit is (all other things remaining the same) directly proportional to the square of the quantity accumulating. It is easy to see by the diagram that the decrements of force upon each added quantity will be as the rectilinear spaces C *h*, *m i*, *n k*, *o k* D, whilst the increasing force in the direction of the circuit will be represented by the triangular spaces A *e h*, A *f i*, A *g k*, A B D.

14. In like manner let the lines A *e*, A *f*, A *g*, &c. stand for the intervals, or explosive distances between the balls of the Lane's discharger, L, fig. 7, which the force in the direction of the external circuit can break through, the increasing force being

represented by the triangles A *e h*, A *f i*, &c.: then, since the attractive forces between the exploding points of the balls of the discharger, with a given accumulation, are in the inverse duplicate ratio of the distances A *e*, A *f*, A *g*, &c., these attractive forces will be inversely proportional to the same triangular spaces A *e h*, A *f i*, A *g k*, &c. If, therefore, when force in the direction of the circuit is A *e h*, discharge takes place, quantity being C *e*, distance A *e*, and attractive force between discharging balls = 1, then supposing A *e* to become A *f* = twice A *e*, the force between the balls at distance A *f* would be only one-fourth as great; that is to say, it would be inversely as triangle A *e h* to triangle A *f i*. Hence with the same quantity accumulated = C *e* discharge could not occur at distance A *f*. Let now the first quantity accumulated = C *e* become twice as great; that is to say, let it be represented by rectangular space C *f* = 2 C *e*: in this case the force in the direction of the external circuit would be represented by triangle A *f i* = 4 times A *e h*; and since attractive force between the exploding points of the balls of the discharger is as squares of the quantity accumulated, therefore attractive force through the external circuit with the double accumulation C *f* becomes also four times as great; and is the same at distance A *f* with a double accumulation as at distance A *e* with a single accumulation; in this case explosive discharge again ensues.

In a similar way it may be shown that when distance A *e* is extended to A *g*, attractive force between the balls with the single accumulation C *e* is reduced to one-ninth, in which case no explosive discharge could occur at distance A *g*. Let the quantity accumulated, however, become three times as great, that is to say, let rectanglar space C *e* become C *g*; in this case the force through the external circuit is represented by the triangular space A *g k* = 9 times triangular space A *e h*; but since these spaces are inversely proportional to the attractive forces at distances A *e*, A *g*, attractive force at distance A *g* is the same with a treble accumulation, as at distance A *e* with a single accumulation; explosive discharge will therefore again occur, and so on. Hence the interval at which discharge occurs, as measured by a Lane's discharger L, fig. 7, will be directly as the quantity accumulated; whilst the electrometer indication or force through the external circuit will be as the square of the quantity, being as the triangular spaces A *e h*, A *f i*, A *g k*, &c. M. De la Rive, in his comprehensive treatise on Electricity, considers this result as somewhat remarkable*. It is evident, however, from the

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* Ce qu'il y a d'assez remarquable, c'est que la distance à laquelle une décharge entre deux balles chargées d'électricités contraires peut avoir lieu, est simplement proportionnelle aux quantités d'électricité, tandis que les forces attractives sont proportionnelles aux carrés de ces forces."—*Traite d'Electricité*, tome i. p. 66.

geometrical diagram above referred to, that it could not be otherwise, and is a necessary result of the forces in operation. We may infer from the equality of the triangles A D C, A D B, that at the instant of explosive discharge the force A B D through the external circuit has superseded the force A D C in the direction of the coatings. If, therefore, we suppose discharge to occur with the successive accumulated quantities C *e*, C *f*, C *g*, &c., then discharge with first quantity = C *e* at distance A *e*, force through external circuit will be represented by triangle A *m e*, and force in the direction of coatings by triangle A *m* C. When discharge occurs at distance A *f* = 2A *e* with a double accumulation C *f* = 2C *e*, force through the circuit will be represented by triangle A *n f*, and force in the direction of the coatings by triangle A *n* C, and so on. We have here to observe, however, that, in estimating the forces through the circuit, the force for quantity C *e*, and distance of discharging balls A *f* = 2A *e*, will be no longer represented by triangle A *m e* (when quantity becomes C *f* = 2C *e*), but by triangle A *r e*, or one-fourth of triangle A *n f*, = force through the circuit at the instant of discharge with a double quantity. When, therefore, distance A *e* is increased to A *f* with the unit of charge represented by C *e*, then force acting through the external circuit is represented by triangle A *r e*, and not by triangle A *m e*.

In order therefore to measure a double accumulation by means of a moveable electrometer, we must oppose to the force through the circuit four times the resistance: thus, in the operation of Cuthbertson's ingenious steel-yard discharging electrometer, we must, in order to obtain a double accumulation, set the slider of the balance-arm to four times the number of grains; in order to obtain a treble accumulation, we must set the slider of the balance-arm to nine times the number of grains, and so on. When, in the application of Lane's discharger, therefore, discharge occurs at a double distance, the quantity of electricity accumulated is twice as great, and the respective forces through the circuit as 1 : 4, as represented by the triangles A *e r*, A *f n*; when discharge occurs at a treble distance, the quantity of electricity accumulated is three times as great, and the force through the circuit nine times as great, as represented by the triangles A *e s*, A *g o*, and so on,—the force through the circuit being, as already observed, as the square of the quantity.

15. It may perhaps be as well to further remark that, although, according to the diagram, the forces A B D, A D C are at the instant of discharge considered as equal, we cannot however infer their *precise* equality, or suppose the decreasing force A D C in the direction of the circuit to absolutely vanish at the point D: hence some little residuum, commonly called residual discharge,

may remain in the jar in consequence of the vanishing attractive force which the coatings exert upon each other, as is found by experiment.

Windsor Villas, Plymouth,
November 1, 1863.

LXXIII. *On Chemical Notation.* *By* J. J. Waterston, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

In your last Number, Mr. Odling has remarked on my communication advocating the vapour-density system of chemical notation. I have been in the habit of using it in arranging the thermo-molecular facts of chemistry in a convenient form for easy reference; and finding that it enabled the mind to grasp easily rather complicated molecular relations which could not be done with any other without tedious verbal accompaniment, I endeavoured some ten or fifteen years ago to bring it to the notice either of the Royal Society or British Association; I forget which. The attempt was unavailing, as indeed it is likely to be in the present case, if some professional chemist of eminence does not take it up in a magnanimous spirit. I did not trouble myself further on the subject, because I felt that, as chemical science progressed, it would be certain to be adopted in regular course as the natural symbolic language—indeed that it would be impossible to get on without it—and that it would be better that the suggestion should come from within the schools. But years have passed on, and the chance of this seemed becoming less by degrees, until I was incited to make another effort by Mr. Odling's lecture, in which it appeared as if the school of chemists to which he belongs had permanently adopted an improper system, and were actually making mistakes in consequence.

The vapour-density system (or, for shortness, let us call it the V.D. system) is founded on the bulk of all molecules in the gaseous state being equal. This, according to Mr. Odling, is an *extremely* recognized fact among chemists*. "Let chemi-

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* At p. 513 of Dr. Graham's second volume the following occurs:—"3. The system (of notation) of Gerhardt, based, like that of Berzelius, on the *hypothesis* that all simple gases contain equal number of atoms in equal volumes." See also p. 512, vol. ii. This, I presume, is concurred in by Mr. Odling and the school; so that we are to understand that they are agreed that (1) it is a *fact* that all simple or compound gases and vapours contain equal number of *molecules* in equal volumes; and (2) that the system of notation they adopt is based on the *hypothesis* that the simple gases contain equal number of *atoms* in equal volumes.

cal notation be founded thereupon [upon the fact (1) only, not upon the hypothesis (2) (see note), or upon any hypothesis], and we obtain with perfect conciseness at once the chemical composition, the physical density of a unit volume, and the constitution of a molecule." On this Mr. Odling remarks, "*he certainly never before heard that the vapour-density of any given body pointed to some particular number as the expression of its molecular weight.*" This is an extraordinary admission. Is it possible that the first and most obvious consequence of the equality in bulk of gaseous molecules is a sealed book to Mr. Odling's school? How powerful then must be the influence of preconceived notions when they shut out from intellectual vision the almost self-evident fact that vapour-densities must be proportional to molecular weights if molecules are of equal bulk, and therefore that they (the vapour-densities) point directly to, or in fact actually express, the molecular weights. If it is not self-evident, here is the proof:—

Let $w =$ weight of a molecule of the gas A, and $W =$ weight of a molecule of the gas B. If there are n molecules of the gas A in the space of a cubic foot, there are n molecules of the gas B in the space of a cubic foot. The weight of a cubic foot of gas A is therefore nw, and the weight of a cubit foot of gas B is nW. The vapour-density of gas A is to vapour-density of gas B as $nw : nW :: w : W$. Q.E.D.

Molecular weights are ratios, vapour-densities are ratios; and these ratios are identical if all molecules in the gaseous state have the same bulk.

Mr. Odling may say that by vapour-density he means the specific gravities of the vapours expressed in terms of water or air as unity, and that these *numbers* do not point to "some particular numbers as the molecular weights:" but they certainly do, inasmuch as molecular weights are ratios which are indicated or "pointed to" by the ratio of the numbers that express the vapour-densities. That the density of the gas of least specific gravity should be selected in preference to any other as the standard unit of comparison is clear from this, that it is easier for the mind to compare magnitudes together when they are expressed in terms of the least; because if they were expressed in any other, we should have to compare multiples with reciprocals, reciprocals with reciprocals, and multiples with multiples; whereas expressed in terms of the least, we have to compare multiples with multiples only—one mental process in place of three.

The specific gravity of gases and vapours in terms of hydrogen unity are identical as *numbers* with the molecular weights expressed in terms of hydrogen unity. This Mr. Odling's school must admit is as indisputable as that of the equality in the volume of gaseous molecules, which Mr. Odling informs us is as clearly recognized as the most simple truths of the multiplication table. May we not therefore ask, Why, in the name of Cocker and common sense, are these numbers not given to the chemical symbolic letters? which would thus be made to express vapour-density as well as molecular weight, and in compound or composite molecules would carry the same meaning; while if bodies entered into combination by fractions of molecules they would appear in the formulæ as such, and require no verbal discussion.

If the specific gravity of oxygen is 16 times that of hydrogen, and if the molecular weight of oxygen is 16 times that of hydrogen, is not 16 associated with O and 1 with H irrevocably? and does it not disturb this association, and therefore fatally distract the attention, to give any other numerical values to these symbols? I use the word *fatally*, because I believe that serious errors as well as general confusion have arisen from this apparently insignificant cause. A few of these were noticed in my previous communication. It is specially when chemistry is studied in its thermo-molecular relations that they assume a prominent aspect.

Mr. Odling states that "the object of his lecture was to show on chemical grounds that the chemical molecule of water is identical with its physical molecule, and consequently that this molecule contains double the amount of hydrogen contained in the molecule of muriatic acid." If the lecture was delivered to teach that the hydrogen of the water-molecule was double the hydrogen of the muriatic acid molecule, there would have been no occasion for it if the vapour-density system of chemical notation had been in vogue, because such facts of molecular composition are expressed by it so completely that any verbal addition whatever becomes superfluous, *e. g.* water is $HO^{\frac{1}{2}}=9$, which means that a gaseous molecule of water is composed of a gaseous molecule of hydrogen (1), united with half a gaseous molecule of oxygen (8). Muriatic acid is $H^{\frac{1}{2}}\,Cl^{\frac{1}{2}}=18\frac{1}{2}$, which means that a gaseous molecule of muriatic acid gas is composed of half a molecule of hydrogen united with half a molecule of chlorine.

The verbal explanation here appended to these formulæ would not of course be required if the V.D. system were fully recognized. The difficulty of making known anything that requires a little quiet application and study to master is well illustrated by the formula for nitric acid given by Mr. Odling as expressed on the V.D. system (Phil. Mag. p. 381), $H^{\frac{1}{2}}N^{\frac{1}{2}}O^{1\frac{1}{2}}$. Now dry nitric acid is $NO^{2\frac{1}{2}}=54$, which means that a molecule of its vapour is composed of a molecule of nitrogen gas united with $2\frac{1}{2}$ molecules of oxygen gas; the monohydrate by $HO^{\frac{1}{2}}.NO^{2\frac{1}{2}}=63$ if the water-molecule continues united with the acid molecule when in the vapour form, or $\frac{HO^{\frac{1}{2}}.NO^{2\frac{1}{2}}}{2}=31\frac{1}{2}$ if they separate; the quadrihydrate by $\frac{4HO^{\frac{1}{2}}.NO^{2\frac{1}{2}}}{5}=20{\cdot}4$ if they separate, and so on.

The method of determining these formulæ methodically by simple equation in the most involved cases is given in the previous communication. We ought to be able to read the name of a compound in its V.D. formula; and reversely we may correct the name by means of the same when it has been improperly bestowed.

But without the V.D. system, the ratio of hydrogen in the water- and muriatic acid molecules might be easily proved as follows:—

In muriatic acid, one part by weight of hydrogen unites with 35·5 parts by weight of chlorine, and its vapour-density is 18·25 times that of hydrogen. One cubic foot of muriatic acid gas contains as many molecules as one cubic foot of hydrogen gas (according to Mr. Odling's ridiculously clear two and two fact); therefore the weight of each molecule of muriatic acid gas is 18·25 times that of each molecule of hydrogen gas; but the proportion of the weight of a molecule of the acid gas that is hydrogen is only $\frac{1}{36{\cdot}5}$ part; and since the molecule of the acid gas weighs 18·25 times the weight of the molecule of hydrogen, $\frac{1}{36{\cdot}5}$ part of 18·25 is 0·5, which is the weight of hydrogen in a molecule of the acid gas in terms of the hydrogen molecule unity. In the composition of water, 1 part by weight of hydrogen unites with 8 parts by weight of oxygen, and the vapour-density of steam is nine times hydrogen; the weight of its molecule is nine times the hydrogen molecule, and one-ninth part of the weight of the steam-molecule being hydrogen, the weight of hydrogen in a steam-molecule is 1 expressed in terms of the hydrogen molecule unity. Thus the weight of hydrogen in a molecule of muriatic acid gas is to the weight of hydrogen in a molecule of water as 0·5 : 1, or as 1 : 2. Q.E.D.

Since this obvious deduction from the recognized fact of the equality of gaseous molecules has escaped notice (for if it had not escaped notice, how could a lecture to prove so simple a matter have been thought necessary), it is not incorrect to say that the arithmetical part of chemistry is not in a satisfactory condition, and that any attempt to put it in a more satisfactory condition deserves the support of chemists.

When previously remarking on Mr. Odling's statement with reference to sodium, I find that I have understood what he calls hydrate of sodium to be hydrate of soda, and applied the same formulæ to it as to the hydrate of potash. Thus Na in contact with $HO^{\frac{1}{2}}$, $HO^{\frac{1}{2}}$ expels the H of one $HO^{\frac{1}{2}}$, and unites with the other $HO^{\frac{1}{2}}$ to form the solid hydrate of soda, $HO^{\frac{1}{2}}\cdot NaO^{\frac{1}{2}}$. When this is heated with sodium, the H of the $HO^{\frac{1}{2}}$ is ejected and gives place to a second Na; so that $HO^{\frac{1}{2}}.NaO^{\frac{1}{2}}$ becomes

$$NaO^{\frac{1}{2}}.NaO^{\frac{1}{2}}=2NaO^{\frac{1}{2}}.$$

Of hydrate of sodium (if it is not a mistake) I know nothing, nor can I find mention made of it in any work on chemistry that I have access to. If Mr. Odling would have the kindness to express what he means in the V.D. notation, it would then perhaps be possible to understand him.

With reference to the statement in my previous communication, "that $HK\Theta$ represents one molecule of the hydrate of potash, but $HH\Theta$ not one but two molecules of water, and $KK\Theta$ not one but two molecules of anhydrous potash," Mr. Odling informs us in a note that Mr. Watts (author of the Supplement of Dr. Gregory's 'Chemistry') states to him "that the three formulæ were meant to express a single molecule each, the three quantities represented being exactly comparable." I repeat that this is impossible, and strikingly shows the inefficiency of Gerhardt's system when it can so notoriously mislead eminent chemists. The following is the proof:—Let us assume that they do express a single molecule each; then, since $HH\Theta$ represents a single molecule of water, the value of the symbol H in terms of the weight of a hydrogen gas-molecule considered as unity must be $\frac{1}{2}$; and the value of Θ must be 8, because the single molecule of water weighs nine times the single molecule of hydrogen, and is $\left.\begin{matrix}H+H+\Theta\\ \frac{1}{2}+\frac{1}{2}+8\end{matrix}\right\}=9$. Potassium, K, thrown into water displaces half the hydrogen and forms hydrate of potash, $HK\Theta$, or $\left.\begin{matrix}H+K+\Theta\\ \frac{1}{2}+39+8\end{matrix}\right\}=47\cdot5$; so that, if the symbols H and Θ retain the same value in the single molecule $HH\Theta$ as in the single molecule $HK\Theta$, the hydrate of potassa, $HK\Theta$, must consist of half a gaseous molecule of hydrogen ($\frac{1}{2}$), half a gaseous molecule of oxygen (8) and potassium (39). But 39 of potassium in union with 8 of oxygen is potassa, so that all that is left to represent the water of the hydrate is $H=\frac{1}{2}$, whereas the water-molecule is $HH\Theta$.

Reverting to the single molecule $HK\Theta$, or $\left.\begin{matrix}H+K+\Theta\\ \frac{1}{2}+39+8\end{matrix}\right\}=47\cdot5$. When it is heated with potassium, "the remaining half of the hydrogen is displaced and anhydrous potash, $KK\Theta$, is formed," which $KK\Theta$ Mr. Watts holds is a single molecule. The remaining half of the hydrogen is $H=\frac{1}{2}$, and $K=39$ substituted for it gives: $\left.\begin{matrix}H+K+\Theta\\ \frac{1}{2}+39+8\end{matrix}\right\}=47\cdot5$ converted into $KK\Theta$, or $\left.\begin{matrix}K+K+\Theta\\ 39+39+8\end{matrix}\right\}=86$, which is stated by Mr. Watts to be a single molecule of anhydrous potash, which is thus represented as being composed of 78 potassa in union with 8 of oxygen, instead of 39.

On the other hand, if $HH\Theta$ represents 2 molecules of water, $H=1$, and $\Theta=16$, the line through the O representing $\Theta=2\times8$, showing that Gerhardt understood his symbol Θ to represent a dual nature, but unfortunately avoided fractional exponents. Let $O^{\frac{1}{2}}$ represent one of those components of Θ, and $O^{\frac{1}{2}}$ the other; then $HH\Theta=HO^{\frac{1}{2}}.HO^{\frac{1}{2}}$ = two molecules of water; and K brought in contact displaces one H, forming one molecule of the hydrate $KO^{\frac{1}{2}}.HO^{\frac{1}{2}}$; and this, heated with potassium, exchanges the remaining H for another K, forming

$$KO^{\frac{1}{2}},\ KO^{\frac{1}{2}}=2\,KO^{\frac{1}{2}},$$

or twice the molecule of the potassa contained in the hydrate.

Can anything be clearer than this? Will no chemist enter the list and advocate the V.D. system, which after all is but Gerhardt's system slightly modified, so that there should be nothing but arithmetical symbols required. But the advantages attending the alteration are great, as will be acknowledged in due time if it ever be adopted.

A few formulæ are appended as illustrations of the system.

The value of	C	is taken at	12,	and $C^{\frac{1}{2}}=6$,	unknown as vapours.
”	Bo	”	11,	” $Bo^{\frac{1}{2}}=5\frac{1}{2}$,	
”	Fl	”	19,	” $Fl^{\frac{1}{2}}=9\frac{1}{2}$,	
”	Cl	=	36	= the spec. grav. of its vapour.	
”	As	=	150	” ” ”	
”	S	=	96	” ” ”	
”	P	=	64	” ” ”	
”	Hg	=	100	” ” ”	

The product of 100 by ·033, the specific heat of mercury, is 3·3. This number, divided by specific heat of iron, gives $Fe=56$; by the specific heat of aluminium, gives $Al=27$. And these weights correspond to the vapour-densities of their compounds with chlorine, &c. as follows:—

$HO^{\frac{1}{2}}=9$	. . Water.
$NO^{\frac{1}{2}}=22$	. . Protoxide of nitrogen.
$N^{\frac{1}{2}}O^{\frac{1}{2}}=15$	. . Nitric oxide.
$NO^{1\frac{1}{2}}=38$	. . Hyponitrous acid.
$N^{\frac{1}{2}}O=23$	. . Nitrous acid.
$NO^{2\frac{1}{2}}=54$	. . Nitric acid (dry).
$C^{\frac{1}{2}}O^{\frac{1}{2}}=14$	. . Carbonic oxide.
$C^{\frac{1}{2}}O=22$	. . Carbonic acid.
$S^{\frac{1}{6}}O=32$	. . Sulphurous acid.
$S^{\frac{1}{6}}O^{1\frac{1}{2}}=40$	. Sulphuric acid (dry).
$AsO^{3}=198$	. . Arsenious acid.
$As^{\frac{1}{4}}H^{1\frac{1}{2}}=39$	. Arseniuretted hydrogen.
$S^{\frac{1}{6}}H=17$	. . Sulphuretted hydrogen.
$P^{\frac{1}{4}}H^{1\frac{1}{2}}=17\frac{1}{2}$	. Phosphoretted hydrogen.
$N^{\frac{1}{2}}H^{1\frac{1}{2}}=8\frac{1}{2}$	. Ammonia.

$NC=26$	. . Cyanogen.
$N^{\frac{1}{2}}C^{\frac{1}{2}}H^{\frac{1}{2}}=13\frac{1}{2}$	Hydrocyanic acid.
$N^{\frac{1}{2}}C^{\frac{1}{2}}Cl^{\frac{1}{2}}=31$.	Chloride of cyanogen.
$H^{\frac{1}{2}}Cl^{\frac{1}{2}}=18\frac{1}{2}$	. Hydrochloric acid.
$H^{\frac{1}{2}}Br^{\frac{1}{2}}=40\frac{1}{2}$	. Hydrobromic acid.
$H^{\frac{1}{2}}I^{\frac{1}{2}}=63\frac{1}{2}$	. Hydriodic acid.
$As^{\frac{1}{4}}Cl^{1\frac{1}{2}}=92$	. Sesquichloride of arsenic.
$As^{\frac{1}{4}}Br^{1\frac{1}{2}}=158$.	Sesquibromide of arsenic.
$As^{\frac{1}{4}}I^{1\frac{1}{2}}=225$	. Periodide of arsenic.
$S^{\frac{1}{6}}Cl=52$	. . Chloride of sulphur.
$S^{\frac{2}{6}}Cl=68$	. . Dichloride of sulphur.
$Bo^{\frac{1}{2}}Cl^{1\frac{1}{2}}=59\frac{1}{2}$.	Terchloride of boron.
$Bo^{\frac{1}{2}}Fl^{1\frac{1}{2}}=33$	. Fluoride of boron.

Note.—In this last, though the vapour-density of Fl and of Bo are both unknown directly, they become so indirectly by the measured vapour-density of the compound.

$N^{\frac{1}{2}}C^{\frac{1}{2}}Br^{\frac{1}{2}}=53$.	Bromide of cyanogen.
$FeCl^{3}=164$	. Sesquichloride of iron.

$AlCl^{3}=135$	. Chloride of aluminium.
$AlBr^{3}=267$	. Bromide of aluminium.
$AlI^{3}=408$	. Iodide of aluminium.

Note.—The values of Fe and Al here become known, though the vapours of iron and aluminium cannot be weighed. The specific heat seems a sure guide to molecular weights of metals.

Organic Compounds.

$C^{\frac{1}{2}}H^{2}=8$	. . Marsh-gas.
$CH^{2}=14$	. . Olefiant gas.
$C^{2}H^{4}=28$	. . Etherine.
$C^{3}H^{3}=39$	. . Benzin.
$C^{3}H^{5}=41$	. . Naphtha.
$C^{5}H^{8}=68$	. . Turpentine.

$CH^{2}.HO^{\frac{1}{2}}=23$	. . Alcohol.
$2(CH^{2}).HO^{\frac{1}{2}}=37$	. S. ether.
$C^{\frac{1}{2}}H.HO^{\frac{1}{2}}=16$	. Pyroxylic spirit.
$2(C^{\frac{1}{2}}H).HO^{\frac{1}{2}}=23$	. Pyroxylic ether.

Note.—Alcohol cannot be expressed by $CH^{3}.O^{\frac{1}{2}}$, because one H is in a burnt condition, and the only constituent it has to have burnt with is $O^{\frac{1}{2}}$.

Pyroxylic spirit cannot be expressed by $C^{\frac{1}{2}}H^{2}.O^{\frac{1}{2}}$ for the same reason. The ethers also for the same reason.

$CH^{2}O^{\frac{1}{2}}=22$ . . . Aldehyde.

Note.—The formula might require to be modified to $CH.HO^{\frac{1}{2}}$, or otherwise if the heat of combustion were known. The same remark applies to chloric ether, $CH^2Cl^{\frac{1}{2}}=32$, and indeed more or less to all organic compounds.

I remain, Gentlemen,
Your most obedient Servant,
Edinburgh, November 8, 1863. J. J. WATERSTON.

LXXIV. *Researches on Chemical Heat and Voltaic Heat.*

By M. RAOULT*.

THE determination of the heat produced by an electric current in the entire circuit, including the pile itself, has hitherto presented great difficulties. I have the honour to submit to the Academy a method by which it can be easily obtained.

Let it be required to determine the total voltaic heat W of an element. I arrange in a calorimeter a spiral formed of a platinum wire rolled round a glass tube and joined by two copper rods to the poles M and N of a strong Daniell's battery P. At the same time I connect the poles M and N with a sine-compass with a long wire and of a suitable degree of sensibility. (This is a sine-compass of variable sensibility which I have described in an "Etude des Forces Electromotrices," presented as a thesis to the Faculty of Sciences at Paris, May 13, 1863; the wire is $\frac{1}{10}$th of a millim. in diameter and 3600 metres long; the heat which the current produces is quite negligible.)

I observe

(1) The intensity f of the derived current in the sine-compass.

(2) The quantity of heat c imparted by the spiral to the calorimeter.

(3) The increase in weight p of the plate of copper in one of the elements of the pile.

(4) The intensity F of the current produced in the compass by the element E, of which it is desired to know the voltaic heat W.

I accordingly obtain W, that is the heat disengaged by the current of the element E from the solution of an equivalent of metal, by means of the formula

$$W=\frac{c\times 31\cdot 6}{p}\times\frac{F}{f}$$

(31·6 being the equivalent of copper).

The demonstration of this formula is very simple. The intensity f of the derived current in the sine-compass is proportional to the difference of the electrical tensions at the points of derivation M and N, and measures this difference; f is then the electromotive force of an element of no resistance, which would

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* Translated from the *Comptes Rendus* for September 14, 1863.

produce in the spiral the same current as the pile P, and which for an equivalent of metal dissolved would disengage a quantity of heat equal to $\frac{c \times 31 \cdot 6}{p}$. As the quantities of heat produced by the current in the same circuit for one equivalent of metal dissolved are proportional to the electromotive forces, an element of no resistance and electromotive force F, that is, producing in the compass a current of the intensity F, would disengage in the spiral, from the solution of an equivalent of metal, a quantity of heat represented by

$$W = \frac{c \times 31 \cdot 6}{p} \times \frac{F}{f},$$

as I have advanced.

The following are the data of an experiment made to determine the voltaic heat of a Daniell's element—copper in sulphate of copper, zinc in sulphate of zinc. The pile P is composed of six large Daniell's elements :

$f = 0 \cdot 5281$.
$c = 502 \cdot 3$ thermal units.
$p = 223$ milligrammes.
$F = 0 \cdot 1752$.

Duration of the experiment 5 minutes. Temperature of the air 25°. From which data

$$W = 23614 \text{ thermal units.}$$

I have modified the experiments by replacing several elements of the pile by Bunsen's elements, varying their number, working for a longer or shorter time, and have always obtained concordant results, the extremes of which were 22859 and 24012. The mean of twenty experiments, where all the data varied from the simple to the double, is

$$W = 23602 \text{ thermal units.}$$

The calorimeter which I used is the mercurial calorimeter of Favre and Silbermann, a little simplified. I measured directly the heat disengaged by the substitution of zinc for one equivalent of copper in a concentrated solution of sulphate of copper, and I found

23564 thermal units.

Favre and Silbermann's number is 23205. It follows from this, that in a Daniell's element the heat produced by the current is very nearly equal to the heat produced by chemical action.

Daniell's element is the only one in which it is so. In other elements, the heat produced by the current in the entire circuit and the heat produced by chemical action are often remarkably different. The following are some of the results obtained :—

			Heat.	
			Chemical.	Voltaic.
A.	Zinc, sulphate of zinc	...Copper, sulphate of copper...	23564	23602
B.	Zinc, acetate of zinc	...Lead, acetate of lead	15691	12438
C.	Copper, nitrate of copper...	Silver, nitrate of silver	16402	7789

Thus, while in the element A the current reproduces the chemical heat in its entirety, the current in the element C does not reproduce more than half. What is the reason of this different action in two cases so similar? This is a serious difficulty, worthy of the attention of physicists.

LXXV. *On the Velocity of Light and the Sun's Distance.*
By Prof. JOSEPH LOVERING, *of Harvard College**.

FOUCAULT'S recent experiment on the velocity of light, though of a less popular character than his celebrated pendulum experiment to prove the earth's rotation, will nevertheless attract even more attention among men of science. If its results are placed beyond doubt, they will affect astronomy to a degree not possible for the pendulum experiment, unless it had come as early as the time of Galileo. I shall examine Foucault's investigation on the velocity of light, (1) as it influences the science of optics, and (2) as it tells upon one at least of the vexed questions in astronomy.

In the circle of the sciences, the centre may be placed anywhere, and the circumference will be everywhere; such is the mutual dependence of each upon all the rest. After the science of optics has furnished astronomy with the telescope, the astronomer discovers with it the satellites of Jupiter and the aberration of light, and with the help of these phenomena assigns the value of the velocity of light, and thus repays to optics the debt incurred by his own special science. Now, for the first time, the science of optics has relinquished the guardianship of astronomy, ascertained by direct experiment one of its own fundamental data, and thereby possibly put astronomy under a new obligation, to be cancelled, doubtless, with interest hereafter.

Let us glance first at the two astronomical methods of measuring the velocity of light. While the senses of touch and taste act only by contact, those of hearing and seeing bring the mind into communication with distant objects. The air and the omnipresent æther supply the delicate and ever ramifying threads by which telegraphic intercourse is maintained with the ear and the eye. When the origin of the sound or the light is at a large distance compared with the velocity of the acoustic or luminous

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* From Silliman's American Journal for September 1863.

wave, allowance must be made for the time taken by the news of an audible or visible event to come to us. Only the vast spaces of astronomy are commensurable with the great velocity of light, and furnish a sufficiently large theatre for a direct experiment upon it. But in stellar astronomy the magnificence of the extent of view so far transceds in magnitude even the velocity of light, that the luminous ray, vast as is its velocity, seems to loiter upon its long way.

Hence in astronomy a distinction exists between the *actual* interval of successive events and the *apparent* interval. For example, the first satellite of Jupiter revolves around its primary in about 42½ hours, and therefore enters the shadow of Jupiter, and is eclipsed, once every 42½ hours. As it takes light more than 40 minutes to pass over the average distance of Jupiter, the eclipse is not seen until so many minutes on the average after it has happened. If this delay were constant, the interval of successive eclipses would not be changed. But in the course of six months the distance of the earth from Jupiter increases by the diameter of the earth's orbit, and in the next six months changes back again; and when the earth is nearest to Jupiter, the news of an eclipse reaches us in about 32 minutes, whereas, if the earth is at the greatest distance, 50 minutes are required.

Consequently the intervals between successive eclipses, as they exist for our eyes, are variable, being sometimes larger and sometimes smaller than the real intervals. This irregularity in the apparent intervals of the eclipses of the same satellite, at first attributed to errors of observation, finally conducted Römer, in 1675, to the discovery of the velocity of light. Delambre, after discussing 1000 of these eclipses observed between 1662 and 1802, calculated the velocity of light to be such as to require 493·2 seconds to pass over the mean distance of the sun. If this time divides 95,360,000 statute miles, which is the sun's distance as given by the transits of Venus in 1761 and 1769 according to Encke's computations, the quotient, or 193,350 statute miles, is the velocity of light in a second.

The second process which astronomy has supplied for obtaining the velocity of light may be called the indirect method. It demands not a *space* but a *velocity* which is commensurable with the velocity of light. If two such velocities are compounded together, according to the principle of the *parallelogram of motions,* there is a resultant motion, the direction of which deviates sensibly from that even of the largest motion which enters into the composition. In nature the velocity of the earth is compounded in this way with the velocity of light, and imparts to the light an apparent path differing by a small angle from the true path. The angular displacement which this causes between the apparent and real places of a star is called aberration, and was first discovered by Bradley in 1726,—this astronomer explaining, on this simple principle, anomalies in observation which had hitherto been considered accidental. As the displacement of the star works opposite ways at opposite seasons of the year, half the difference between the extreme places is the distance from the apparent to the true place, or the constant of aberration. This, when known as an observed fact, establishes the ratio between the velocity of light and the velocity of the earth, and enables the astronomer to assign the value of the one with all the accuracy which pertains to his knowledge of the other. Accepting Struve's determination of the aberration, viz. $20''\cdot45$, the velocity of light is calculated to be 10,088 times as great as the velocity of the earth. The mean velocity of the earth is known with all the certainty which belongs to our knowledge of the magnitude of the earth's orbit, that is, of the sun's distance. Assuming, as before, that the distance derived from Encke's parallax is the most reliable, the velocity of the earth in one second of solar time is 18·977 miles. This, multiplied by the aforesaid ratio, gives 191,513 miles for the velocity of light by Bradley's principle. It appears therefore that the velocities by the two methods of astronomy (the direct and the indirect) differ by 1837 miles—a small quantity comparatively, being only *one per cent.* of the whole velocity. Whatever other value is adopted for the sun's distance will alter these two results proportionally, without disturbing the ratio between them. I may add that the velocity which aberration ascribes to light belongs to it at the earth's surface—that is, in the dense atmosphere; whereas the velocity discovered from the eclipses is that which extends through the planetary spaces. This distinction, however, will do little towards bringing the two results into greater accordance. The velocities of light in different media are proportional to the indices of refraction inversely, which in the case presented are as 1 to 1·000294. This theoretical difference of velocities is less than $\frac{1}{3000}$ of the whole, or less than 70 miles.

Compare with these conclusions of astronomy two experimental results on the same subject. Although Wheatstone's experiment on the velocity of electricity, published in 1834, suggested the possibility of measuring in a similar way other great velocities, I shall consider first a contrivance of Fizeau, equally applicable to light and to electricity. If a wheel finely cut into teeth on its circumference is put in rapid rotation, a ray of light which escapes between two consecutive teeth will, after being reflected perpendicularly by a mirror, return to strike the wheel at a different point, and either be intercepted by a tooth or admitted at another interstice. Suppose the velocity of the wheel just sufficient to bring the adjacent tooth to the position whence the ray first started, in the time which the light occupies in going to the mirror and returning. In this time the wheel has moved over an angle found by dividing 360° by twice the number of teeth which the wheel contains. Therefore the time taken by light in going over a line equal to twice the distance of the mirror, is that portion of a second found by dividing unity by the product of the number of turns the wheel makes in a second multiplied by double the number of teeth on the wheel, the velocity of the wheel being first made the smallest which will cause it to intercept the light. Such an experiment was made in 1849 by Fizeau, the wheel being placed in a tower at Suresne, near Paris, and the mirror upon a hill (Montmartre) at the distance of 8633 metres. As the wheel contained 720 teeth, and the slowest velocity which produced obscuration was 12·6 turns a second, it appeared that light required $\frac{1}{18144}$ of a second to go 8633 metres and return. Hence its velocity was 313,274,304 metres, or 194,667 miles a second. The French Academy thought so favourably of this attempt that they referred the subject to a scientific commission, consisting of Biot, Arago, Pouillet, and Regnault, with authority to procure a grand machine for repeating the experiment.

When Arago advocated the claims of Wheatstone to the vacant place of Corresponding Member of the French Academy in the section of Physics, it was objected that Wheatstone had only made a single experiment without having discovered a principle. Arago engaged to prove that the candidate had introduced a fertile method of experimentation which would be felt in other sciences as well as electricity. For example, the corpuscular theory of light requires that the velocities of light in different media should vary directly as the indices of refraction, whereas the undulatory theory inverts this ratio. Arago prepared for the trial by experiments on rapid rotation, the mechanical difficulties to be overcome, and the comparative advantage of slower rotations assisted by several reflexions in place of a single mirror turning with its maximum speed. Aided by the refined skill of Breguet, he realized velocities in the mirror of 1000 turns a second, and of the axis detached from the mirror, of even 8000 turns. In the meanwhile his eyesight began to fail him, and younger physicists entered into the fruit of his labours. After Foucault and Fizeau, by separate efforts, had decided the question in relation to the velocities of light in air and in water, in favour of the undulatory theory, and thus confirmed a conclusion which Arago reached by *diffraction* in 1838, and after Fizeau had studied the variation of the velocity of light in running water according as the motions agree or differ in direction, Foucault was emboldened to attempt a measure of the *absolute* velocity of light by an experiment which could be brought within the compass of a single room. I translate his own account of the arrangements made for this purpose.

"A pencil of solar light, reflected into a horizontal direction by a heliostat, falls upon the micrometric mark, which consists of a series of vertical lines distant from one another one-tenth of a millimetre. This mark, which in the experiment is the real standard of measure, has been divided very carefully by Froment. The rays which have traversed this initial surface fall upon a plane rotating mirror at the distance of a metre, where they suffer the first reflexion, which sends them to a concave mirror at the distance of four metres. Between these two mirrors, and as near as possible to the plane mirror, is placed an object-glass, having in one of its conjugate foci the virtual image of the mark, and in the other the surface of the concave mirror. These conditions being fulfilled, the pencil of light, after traversing the lens, forms an image of the mark on the surface of this concave mirror.

"Thence the pencil is reflected a second time, in a direction just oblique enough to avoid the rotating mirror, an image of which it forms in the air at a certain distance. At this place a second concave mirror is placed, facing so that the pencil, once more reflected, returns to the neighbourhood of the first concave mirror, forming a second image of the mark. This is taken up by a third concave mirror, and so on to the formation of a last image of the mark on the surface of the last concave mirror of an odd number. I have been able to use five mirrors, which furnish a line twenty metres long for the ray to travel.

"The last of these mirrors, separated from the preceding one, which faces it, by a distance of four metres (equal to its radius of curvature), returns the pencil back on itself—a condition surely fulfilled when the returning image and the original image on the last mirror but one coalesce. Then we are sure that the pencil retraces its steps, returns in full to the plane mirror, and all the rays go back through the mark, point for point, as they went forth.

"This return of the pencil may be proved on an accessible image by reflecting the pencil to one side by a surface of glass at an angle of 45°, and examining it through a microscope of small power. The latter, resembling in all respects the micrometric microscopes in use for astronomical observations, forms, with the mark and the inclined glass, one solid piece of apparatus.

"The real image sent into the microscope, and formed by the returning rays partially reflected, occupies a definite position in relation to the glass and the mark itself. This position is precisely that of the virtual image of the mark seen by reflexion in the glass. At least this is true when the plane rotating mirror is at rest. But when this mirror turns, the image changes its place; for while the light is going and returning between the mirrors, the plane mirror has shifted its position, and the returning rays do not strike at the same angle of incidence as when they left it. Hence the image is displaced in the direction of the rotation, and this displacement increases with the velocity of rotation; it also increases with the length of the route passed over by the rays, and with the distance of the mark from the plane mirror.

"If we call V the velocity of light, n the number of times the mirror turns in a second, l the distance between the plane mirror and the last concave mirror, r the distance of the mark from the turning mirror, and d the observed displacement, we have $V=\frac{8\pi nlr}{d}$; an expression which gives the velocity of light when the other quantities are separately measured. The distances l and r are measured directly by a rule. The deviation is observed micrometrically; it remains to show how the number of turns (n) of the mirror is found.

"Let us describe first how a constant velocity is imparted to the mirror. This mirror, of silvered glass, and 14 millims. in diameter, is mounted directly upon the axis of a small air-turbine, of a well-known model, admirably constructed by Froment. The air is supplied by a high-pressure bellows of Cavaillé-Colle, justly distinguished for the manufacture of large organs. As it is important that the pressure should be very constant, the air after leaving the bellows traverses a regulator, recently contrived by Cavaillé, in which the pressure does not vary by one-fifth of a millimetre in a column of water of thirty centimetres. The fluid flowing through the orifices of the turbine represents a motive power of remarkable constancy. On the other hand, the mirror, when accelerated, soon encounters in the surrounding air a resistance which for a given velocity is also perfectly constant. The moving body placed between these two forces, which tend to equilibrium, cannot fail to receive and to preserve a uniform velocity. Any check whatever, acting upon the flow of the water, allows this velocity to be regulated within very extensive limits.

"It remains to estimate the number of turns, or rather to impress on the moving body a determined velocity. This problem has been completely solved in the following manner. Between the microscope and the reflecting glass a circular disc is placed, the edge of which, cut in fine teeth, encroaches upon the mark and partly intercepts it. The disc turns uniformly on itself, so that, if the image shines steadily, the teeth at its circumference escape detection from the rapidity of the motion. But the image is not permanent: it results from a series of discontinuous appearances equal in number to the revolutions of the mirror; and whenever the teeth of the screen succeed one another with the same frequency, there is produced on the eye an illusion easily explained, which makes the teeth appear immoveable. Suppose then that the disc, with n teeth in its circumference, turns once in a second, and that the turbine starts up. If, by regulating the flow of air, the teeth are made to appear fixed, we are certain that the mirror makes n turns in a second.

"Froment, who made the turbine, wished to invent and construct a chronometric wheelwork to move the disc. It is a remarkable piece of clockwork, which solves in an elegant manner the problem of uniform motion in the particular case in which there is no work to be done. The success is so complete that it is my daily experience to launch the mirror with 400 turns a second, and see the two pieces of apparatus march within $\frac{1}{10000}$ nearly of accordance during whole minutes.

"Notwithstanding the assurance I had gained in the measurement of time, I was surprised at proving, in my results, discordances which were out of proportion to the precision of my means of measuring. After long research, I discovered the source of error in the micrometer, which did not allow of the degree of accuracy willingly attributed to it. To meet this difficulty, I have introduced into the system of observation a modification which amounts simply to a change of the variable. Instead of measuring micrometrically the deviation, I adopt for it a definite value in advance—suppose seven-tenths of a millimetre, or seven entire parts of the image; and I seek by experiment to find the distance between the mark and the turning mirror necessary to produce this deviation: the measures extending over a length of about a metre, the last fractions have a magnitude directly visible, and leave no room for error.

"By this means the apparatus has been purged of the principal cause of uncertainty; henceforth the results accorded, within the limits of errors of observation, and the means are settled in such a way that I am able to assign confidently the new number which appears to me to express nearly the velocity of light in space, viz. 298,000 kilometres in a second of mean time."

This value, reduced to statute miles, shows that the velocity of light is 185,177 miles in a second; which is less by 6336 miles than the velocity for light usually admitted into science, viz. the velocity obtained from the aberration of light. This discrepancy between the results of experiment and that of the astronomical determination which comes nearest to it, is three times greater than the variation between the velocity deduced from aberration and that derived from eclipses.

Foucault states that the extreme difference of the results of various trials amounted to only $\frac{1}{100}$ of the whole quantity, and that the mean result can be trusted to the fraction of $\frac{1}{500}$. Moreover the aberration of $20''\cdot45$ adopted by astronomers cannot be supposed at fault by more than $\frac{1}{1800}$ of the whole. Neither the velocity by Foucault's experiments nor the value of aberration can be charged with a possible error of 3 per cent., or of any error approaching to this large discrepancy. How is the new velocity of light to be reconciled with the old value of aberration? I have said that aberration establishes only the *ratio* between the velocity of light and the velocity of the earth. If this ratio cannot be tampered with, and if one term of it (the velocity of light) must be diminished by 3 per cent. to suit Foucault's experiment, then we must at the same time diminish the other term (the velocity of the earth) proportionally; and the old ratio will be preserved, and the value of aberration will be left unchanged. Is it possible, therefore, that there can be an uncertainty to the extent of 3 per cent. in the velocity of the earth? If so, the tables are turned; and instead of employing the ratio which aberration supplies to calculate the velocity of light from the velocity of the earth, as the best known of the two, we henceforth must calculate the velocity of the earth from the velocity of light; for Foucault has found the latter by experiment more accurately than astronomy gives the former. If there is an error of 3 per cent. in the velocity of the earth, it is an error in space and not in time. To diminish the velocity of the earth sufficiently by a change of time would demand an increase in the length of the year amounting to eleven days nearly.

The only other way of reducing the velocity of the earth is by diminishing the circumference of the earth's orbit; and this, if diminished, changes proportionally the mean radius of the orbit, that is, the sun's mean distance. The question therefore resolves itself into this, Can the distance of the sun from the earth be considered uncertain to the extent of 3 per cent. of the whole distance?

The answer to this question will lead me into a brief discussion of the processes by which the sun's distance from the earth has been determined, and the limits of accuracy which belong to the received value. To see the distance of any body is an act of *binocular* vision. When the body is near, the two eyes of the same individual converge upon it. The interval between the eyes is the little base-line, and the angle which the optic axes of the two eyes, when directed to the body, make with each other is the parallax; and by this simple triangulation, in which an instinctive geometrical sense supersedes the use of sines and logarithms, the distance of an object is roughly calculated. As the distance of the object increases, the base-line must be enlarged; but the geometrical method is the same, even when the object is a star and the base of the triangle the diameter of the earth's orbit. Substitute then for the two eyes of the same observer the two telescopes of different astronomers, planted at the opposite extremities of the earth's diameter, and any one will understand the principle upon which the binocular eye of science takes its stereoscopic view of the universe, and plunges into the depths of space. In this way it is that the distance of the sun from the earth is associated with the *solar parallax,* which is the angle between the directions in which two astronomers point their telescopes when they are looking at the sun at the same moment. To know the sun's distance, the astronomer studies the solar parallax. As Kepler's third law establishes a relation between the distances of the different planets from the sun and their periods of revolution, if the astronomer finds either distance by observation, the others can be computed from this law. As the solar parallax is only about eight seconds, and an error of one-tenth of a second includes an error of more than a million of miles in the sun's distance, he takes advantage of the law of Kepler, and selects a planet which comes occasionally nearer to the earth than the sun. The choice lies between Venus at inferior conjunction and Mars at opposition. The parallax of Mars may vary from 20″·7 to 19″·1, according to the positions of Mars and the earth with respect to the perihelion of the orbit at the time of opposition. The parallax of Venus at conjunction may vary, for the same reasons, from 33″·9 to 29″·9. Venus, therefore, may be nearer to the earth than Mars, and the parallax more favourable. But Venus cannot be seen at conjunction except when its latitude is so small that a transit across the sun's disc occurs. Then the two observers refer its place not to a star but to the sun, and the quantity they determine is the difference of parallax between Venus and the sun; which will vary from about 21″ to 25″. Moreover the difference of parallax is measured, not directly, but through the influence it produces on the duration of the transit at the two stations, and therefore upon a greatly enlarged scale.

What are the results which have been obtained, first, by observations of the transits of Venus? and second, by observations on Mars at opposition?

1. Only two transits of Venus have occurred since the time when the sagacious Dr. Halley invoked the attention of posterity to these rare astronomical events as pregnant with the grandest results to science, viz. those of 1761 and 1769. The astronomers of the last century did not neglect the charge which Halley consigned to them. The transit of 1769 was eminently favourable, offering a chance which comes only once in a millennium, as Professor Winthrop happily explained in his lectures on the last transits.

Whatever verdict posterity shall pronounce on the deductions from the observations then made, they will never, says Encke, reproach astronomers or governments with negligence or want of appreciation towards this golden opportunity. The solar parallax which Encke deduced from an elaborate discussion of all the observations, fifty years after they were made, is 8″·57116. This corresponds to a solar distance of 95,360,000 statute miles.

Although transits of Venus will take place in 1874 and 1882, and astronomers already begin to talk of preparing for them, I have the authority of Encke for declaring that, in comparison with that of 1769, the next two transits will be so unfavourable that nothing short of perfection in the construction of instruments, and in the art of observing, can compensate for the natural disadvantage; so that the reduction of the possible error in the sun's parallax within the limit of $\frac{1}{100}$th of a second is hopeless for at least two centuries more.

2. The solar parallax may also be derived from the parallax of Mars, when this planet is in opposition. In 1740 the French astronomer Lacaille was sent to the Cape of Good Hope, and from the parallactic angle observed between the direction of Mars as seen from that station and from the observatory of Paris (deduced from observations of declination), the horizontal parallax of Mars was computed, and consequently that of the sun. The solar parallax thus found was $10''\cdot20$, with a possible error not exceeding $0''\cdot25$. Henderson, by comparing his own observations of the declination of Mars at its opposition in 1832 with corresponding observations at Greenwich, Cambridge, and Altona, computed the solar parallax at $9''\cdot028$.

The United States Naval Astronomical expedition to Chili, under the charge of Lieut. J. M. Gilliss, during the years 1849–1852, had for its object the advancement of our knowledge of the solar parallax, partly by observations of Mars at opposition, and partly by observations of Venus during the retrograde portion of her orbit, and especially at the stationary points, in conformity with a method suggested by Dr. Gerling—the whole to be compared with simultaneous observations at northern observatories. Although the observations at Chili were made on 217 nights, covering a period of nearly three years, the cooperation of northern astronomers was so insufficient that only 28 corresponding observations were made. On this account the second conjunction of Venus was useless; the other conjunction of Venus and the second opposition of Mars were of little value; and even the first opposition of Mars led to no significant result. Dr. B. A. Gould has computed the solar parallax from the first opposition of Mars, observed at Chili, at $8''\cdot50$.

3. The solar parallax can also be computed from the law of universal gravitation. The principle may be thus stated. The motion of the moon round the earth is disturbed by the unequal attraction of the sun on the two bodies. The magnitude of the disturbance will be in some proportion to the distance of the disturber when compared with the relative distance of the two disturbed bodies; and this ratio of distances is the inverse ratio of the parallaxes of the sun and moon. By selecting one of the perturbations in the moon's longitude particularly adapted to this purpose, Mayer, as early as 1760, computed the solar parallax at $7''\cdot8$. In 1824 Burg calculated this parallax from better observations at $8''\cdot62$. Laplace gives it at $8''\cdot61$. Fon-

tenelle had said that Newton, without getting out of his armchair, found the figure of the earth more accurately than others had done by going to the ends of the earth. Laplace makes a similar reflection on this new triumph of theory. "It is wonderful that an astronomer, without going out of his observatory, should be able to determine exactly the size and figure of the earth, and its distance from the sun and moon, simply by comparing his observations with analysis, the knowledge of which formerly demanded long and laborious voyages into both hemispheres." The accordance of the results obtained by the two methods is one of the most striking proofs of universal gravitation. Pontecoulant makes the solar parallax by this method 8″·63. Lubbock, by combining Airy's empirical determination of the coefficient with the mass of the moon as he finds it from the tides (viz. $\frac{1}{67}$), makes the solar parallax 8″·84. If the mass of $\frac{1}{75}$ is substituted, the parallax is changed to 8″·81. Finally, Hansen, in his new 'Tables of the Moon,' adopts 8″·8762 as the value of the solar parallax. Moreover, Leverrier, in his 'Theory of the Apparent Motion of the Sun,' deduces a solar parallax of 8″·95 from the phenomena of precession and nutation.

The conclusions of this whole review are summed up in the following Table, in which the values of the solar parallax and of the sun's distance, by the three methods of astronomy, and by the experiment of Foucault, are placed in juxtaposition; also the different velocities of light found by astronomical observations and by experiment.

Observer or computer.	Method.	Parallax.	Distances.
		″	miles.
Encke	By Venus (1761)...	8·53	95141830
Encke	„ (1769)...	8·59	95820610
Lacaille	By Mars	10·20	76927900
Henderson	„	9·03	90164110
Gilliss and Gould..	„	8·50	96160000
Mayer	By Moon...........	7·80	104079100
Burg	„	8·62	94802440
Laplace	„	8·61	94915970
Pontecoulant	„	8·63	94689710
Lubbock	„	8·84	92313580
„	„	8·81	92652970
Hansen	„	8·88	91861060
Leverrier	„	8·95	91066350
Foucault	By light	8·86	92087342
Fizeau	„	8·51	95117000
Velocity of light...	By eclipses		193350
„ „	By aberration..............................		191513
„ „	By Fizeau's experiment..................		194667
„ „	By Foucault's experiment...............		185177

Foucault's experiment on the velocity of light has been popularly announced as making a "revolution in astronomical science." But it appears from the preceding sketch that it has raised no new question in astronomy, though it may have attracted popular attention to an old difficulty, and possibly given a solution to it. The three astronomical methods present solar distances which, even if we select the most trustworthy decision of each, differ by three or four millions of miles—that is, by 3 or 4 per cent. of the whole quantity. Though the best products of the first and third methods were at one time within a million of miles of each other, an increase of lunar observations, and especially improvements in the lunar tables, have now carried that difference up to four millions of miles. If Foucault's experiment were allowed to give the casting vote, it would decide in favour of the third method, thus making the reflection of Laplace, which I have already quoted, still more memorable.

In regard to the commonly received distance of the sun, which is based upon Encke's profound discussion of all the observations made at the last two transits of Venus, the case stands thus. Encke decides, from the weights of the observations, discussed in the light of the mathematical principle of *least squares*, that the probable error of the sun's distance, as given by the transits, does not exceed $\frac{1}{230}$ of the whole quantity. Astronomers have also reason to believe that the adopted value of aberration is correct within $\frac{1}{1800}$ of the whole quantity. Moreover, Foucault is confident of his determination of the velocity of light within $\frac{1}{600}$ of the whole quantity; nay, he expects to improve his instruments so as to banish all errors larger than $\frac{1}{6000}$ of the whole quantity. Neither the velocity of light, aberration, nor the sun's distance can be suspected of an error to the extent of 3 or 4 per cent.; and yet one at least must be wrong to this degree, as the best values of the three elements are irreconcilable with each other. Which shall be changed?

It may excite surprise in those who have heard of the *accuracy* of astronomy, without weighing the exact significance of the word as applied to so large a subject, that there should still be a lingering uncertainty, to the extent of three or four millions of miles, in the sun's distance from the earth. But the error, whatever it is, is propagated from the solar system into the deepest spaces which the telescope has ever traversed. The sun's distance is the measuring-rod with which the astronomer metes out the distances of the fixed stars and the dimensions of stellar orbits. An error of 3 per cent. in the sun's distance entails an error of 3 per cent. in all these other distances and dimensions. Trifling as 3 per cent. may seem, the correction runs up to 600,000 millions of miles in the distance of the nearest fixed star!

LXXVI. *Chemical Notices from Foreign Journals.* *By* E. Atkinson, *Ph.D., F.C.S.*

[Continued from vol. xxv. p. 545.]

BÖTTGER*, who has investigated the deposit in the lead chambers of numerous sulphuric acid works, states that he has as yet only observed the occurrence of thallium in two cases. It does not appear always to accompany selenium: a very rich seleniferous deposit from Zwickau contained no thallium; and in a thalliferous deposit from a manufactory at Aix-la-Chapelle no trace of selenium could be detected. Böttger extracted the thallium from a deposit in sulphuric acid chambers at Oker in the Hartz, in the following manner. The reddish crumbly deposit was boiled with six times its quantity of distilled water, and powdered carbonate of soda added until the disengagement of gas had ceased, and the mixture had assumed an alkaline reaction. The boiling was continued with continual stirring until the original red colour of the deposit was changed into black. The whole was then filtered and the residue washed on the filter; to the united filtrate and washings a small quantity of cyanide of potassium was added, the liquid again filtered, and sulphuretted hydrogen passed through the filtrate so long as black sulphide of thallium continued to be precipitated. This was washed by decantation and collected on a filter. The black deposit resulting from the treatment with carbonate of soda still contained thallium; it was accordingly boiled with moderately strong solution of oxalic acid so long as the spectroscopic reaction of thallium could be obtained. The filtered liquid was then made alkaline by carbonate of soda, a quantity of cyanide of potassium added, the whole boiled, filtered, and the filtrate precipitated by sulphuretted hydrogen.

This precipitate consisted of sulphide of thallium containing sulphide of mercury; it was treated with pure nitric acid (of spec. grav. 1·20), by which sulphide of thallium alone was dissolved out. This nitric acid solution was mixed with concentrated sulphuric acid, the whole evaporated to dryness, dissolved in boiling water, the liquid concentrated, and the thallium precipitated by zinc.

In a subsequent paper† Böttger describes the extraction of thallium from a deposit which had formed behind a pyrites furnace in a sulphuric acid works in which Belgian pyrites was used. This had a reddish colour, and consisted of sulphates of zinc, sesquioxide of iron and thallium mixed with charcoal and sand.

This residue was very finely powdered, and boiled with four

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[*] Liebig's *Annalen*, May 1863.

[†] Ibid. June 1863.

times its weight of distilled water, and several large rods of pure zinc placed in the cold filtrate. Within eight to twelve hours all the thallium in the solution was precipitated in the form of a black porous heavy powder, mixed with some hydrated oxide of zinc, from which it could be readily separated by elutriation. This thallium was dissolved in dilute sulphuric acid with the addition of a few drops of nitric acid, evaporated to dryness, redissolved in water, and the thallium again precipitated by the addition of some pieces of distilled zinc. The thallium is separated out to the last trace, often in the form of beautiful metallic lustrous needles.

Böttger* investigated a saline mixture obtained in cold weather from the mother-liquors from which common salt has been extracted at the Nauheim Salt Works. It is a very deliquescent substance, and is in considerable use by the confectioners for freezing-mixtures. It was found to consist of the chlorides of magnesium, potassium, and sodium, besides comparatively large quantities of the *chlorides of cæsium and rubidium*, with traces of *chloride of thallium*. Böttger states that the Nauheim mother-liquor is at present the readiest source of *cæsium* and *rubidium*. According to his observations, thallium almost invariably accompanies potassium, cæsium, and rubidium—from which fact, and from its other properties, spite of its being precipitated by sulphuretted hydrogen, an indication is given that this metal must be classed with the alkalies.

Hautefeuille describes the artificial preparation of Rutile, Brookite, and their varieties†.

Rutile is obtained by heating in a current of hydrochloric acid a mixture of titanate of potash and chloride of potassium. The mixture is placed in a platinum crucible enclosed in an earthen one, in the lid of which are luted two porcelain tubes through which the current of gas passes. The crystals have the same form as those obtained by M. Deville‡, by passing hydrochloric acid over amorphous titanic acid heated to redness. The density of the crystals obtained by M. Hautefeuille is 4·3, the same as that of natural rutile.

Acicular Rutile.—By heating a mixture of titanate and fluotitanate of potassium to bright redness in a current of hydrochloric acid, prismatic crystals are obtained terminated by octahedra. In their shape and colour, which is a golden yellow, they resemble the acicular rutile enclosed in the quartz of Madagascar.

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* Liebig's *Annalen*, September 1863.

† *Comptes Rendus*, July 20, 1863.

‡ Phil. Mag. vol. xxii. p. 516.

Some of these crystals are of a blue colour, so dark that they would be considered black, but that the intensity of the colour is somewhat less at the edges.

Lamellar Rutile.—When titanic acid dissolved in fluosilicate of potash is heated to bright redness in a current of hydrochloric acid, lamellar crystals of rutile are obtained resembling the natural rutile of New Jersey.

Sagenite.—A mixture of titanic acid, silica, and fluosilicate of potash, heated in a current of hydrochloric acid, gives rise to a number of small needles planted edgewise in a skeleton of silica. In their colour and shape they present an undoubted analogy with the Sagenite of Saussure.

Brookite is obtained in transparent very fragile plates by heating to dull redness a mixture of titanic acid, silica, and fluosilicate of potash in a current of hydrochloric acid. There is no silica in the crystals, and their density and shape identifies them with the Brookite of St. Gothard.

Black crystals of *Arkansite* are obtained when the preceding experiment is made in a crucible of gas-charcoal.

When fluotitanate of potash is heated in a current of dry hydrogen charged with a small quantity of hydrochloric acid, the bifluoride of titanium liberated is reduced to protofluoride of titanium, which crystallizes in beautiful prisms of a deep violet colour.

When acicular rutile is prepared in a charcoal crucible, the mixture being heated for some time before passing the current of hydrochloric acid, blue crystals are obtained containing 5 per cent. of fluorine. The colour produced is due to the presence of protofluoride of titanium.

Some of these syntheses are not new. Rutile has been obtained crystallized by several methods, but none of them have given the complete series of these varieties of the mineral.

Wiederhold[*] has described a method for preparing the solid hydride of arsenic, which was first obtained by Davy, by passing an electrical current through water and using arsenic as the negative pole. Davy also observed that when arsenide of potassium was treated with water, hydride of arsenic was left as a brownish-red powder. It has been stated that when arsenide of zinc is treated with hydrochloric acid, hydride of arsenic is left: Soubeiran showed that the residue is an insoluble alloy containing excess of arsenic. Wiederhold has now shown that, by taking an alloy of special composition, the hydride may be obtained. In preparing this alloy, which consists of five parts of zinc and one of arsenic, certain precautions are necessary, and especially that

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[*] Poggendorff's *Annalen*, April 1863.

the materials be chemically pure. The arsenic is placed in a Hessian crucible, and above it the zinc; heat is gradually applied up to the melting-point of the zinc, and the crucible is then kept for a quarter of an hour at the temperature of the wind-furnace. The crucible is then withdrawn, and its contents poured into cold water.

When this substance is treated with hydrochloric acid, a violent disengagement of hydrogen and arseniuretted hydrogen ensues, the liquid becomes yellow-coloured, which, passing through brick-red, changes into dark brown; and finally a reddish-brown powder is left, which is the body in question. From about 300 grms. of alloy, 1 grm. of pure hydride of arsenic was obtained.

The substance was analysed by heating a weighed quantity in a suitable apparatus, and collecting and measuring with all suitable precautions the hydrogen disengaged. From 1 grm., 0·00484 grm. of hydrogen were obtained. The formula $As^2 H$ requires 0·00688 grm., and the formula $As H^2$ requires 0·025 grm. Solid hydride of arsenic, $As^2 H$, is a light voluminous reddish-brown powder, in appearance resembling binoxide of lead. It is insoluble in the ordinary reagents; it decomposes at 200° C. into pure hydrogen and arsenic. It burns in the air with a yellow flame, forming arsenious acid, metallic arsenic, and a small quantity of a black substance which is probably a new oxide of arsenic. Dilute acids are without action upon it, with the exception of nitric acid, which oxidizes it to arsenious and arsenic acids. It is gradually decomposed by the alkalies, forming metallic arseniate. It burns in chlorine, forming chloride of arsenic; in bromine, forming bromide of arsenic and bromide of hydrogen; and in iodine in a similar manner. Wiederhold has not succeeded in combining it with other bodies.

Carius* has found that ethylene and its homologous and analogous hydrocarbons can directly combine with hydrate of hypochlorous acid and with peroxide of hydrogen, while he describes the reactions as being sharp and well defined.

The combination is best effected by means of the solution of hypochlorous acid, which must not be too strong, for otherwise it gives off free chlorine, which produces secondary actions. This solution is prepared by adding to stoppered vessels of a litre capacity filled with chlorine 15 grammes of precipitated oxide of mercury which has been heated to 300°, diffused in a little water. The whole is well shaken, and the operation performed in a dark place. By using larger quantities than these, or oxide which has not been heated to 300°, the experiment fails,

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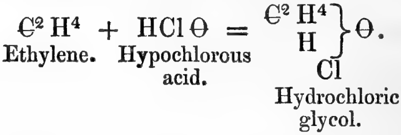
* Liebig's *Annalen*, vol. cxxiv. p. 265; and May 1863.

in consequence of the formation of large quantities of chlorate of mercury.

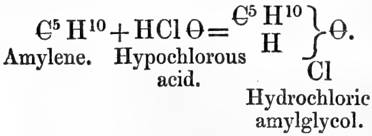
This solution contains 2 to 3 per cent. of hypochlorous acid; it only acts slowly on ethylene gas, and is yet well adapted for preparing hydrochloric glycol, the formation of which simply results from the addition of the two bodies,



$$\underset{\text{Ethylene.}}{\text{Ȼ}^2\,\mathrm{H}^4} + \underset{\text{Hypochlorous acid.}}{\mathrm{HCl}\,\Theta} = \underset{\text{Hydrochloric glycol.}}{\left.\begin{matrix}\text{Ȼ}^2\,\mathrm{H}^4\\ \mathrm{H}\end{matrix}\right\}\Theta.\\ \mathrm{Cl}}$$

Stoppered vessels holding about a litre are filled with ethylene gas, and the hypochlorous acid solution, prepared as above, added, the whole well shaken in a dark place and allowed to stand in cold water. In a few hours the smell of hypochlorous acid has disappeared; the liquid is then filtered, and the filtered liquid distilled. The distillate is exhausted with ether, the etherial solution dehydrated by carbonate of potash, the ether evaporated in the water-bath, and the residue rectified. The body thus prepared is identical in properties with that obtained by Wurtz from glycol.

An aqueous solution of hypochlorous acid readily absorbs amylene, and an excess of acid is not injurious so long as it does not give off chlorine. The hydrochlorate of amylglycol, which is the result of the action, could not be isolated by Bauer by the action of hydrochloric acid on amylglycol*, but can easily be obtained by means of this reaction. After the action of the hypochlorous acid on amylene is complete, which requires to be moderated by cooling, sulphuretted hydrogen is passed through the filtered liquid so as to precipitate any dissolved mercurial salt, care being taken to avoid an excess of sulphuretted hydrogen; the filtrate is then neutralized with carbonate of soda, and the hydrochlorate extracted with ether. The rest of the purification is quite analogous with that for the corresponding ethylene derivative. The reaction is as follows:—



$$\underset{\text{Amylene.}}{\text{Ȼ}^5\,\mathrm{H}^{10}} + \underset{\text{Hypochlorous acid.}}{\mathrm{HCl}\,\Theta} = \underset{\text{Hydrochloric amylglycol.}}{\left.\begin{matrix}\text{Ȼ}^5\,\mathrm{H}^{10}\\ \mathrm{H}\end{matrix}\right\}\Theta.\\ \mathrm{Cl}}$$

The new body is a colourless, not very mobile liquid, with an odour like that of valerianic acid. It is heavier than water, in which it is soluble, and which it also dissolves, and can only with difficulty be freed from by distillation: when pure it boils at

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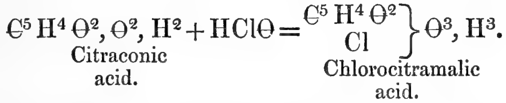
* Phil. Mag. vol. xx. p. 44.

155°. That this body is really the homologue of hydrochloric glycol, was proved by treating it with caustic potash. It was thus converted into oxide of amylene, which was identical in all its properties with that obtained by Bauer.

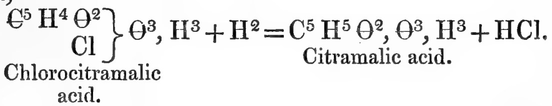
Carius likewise obtained a corresponding compound, $\text{Ȼ}^{16}\text{H}^{33}\text{Cl}\Theta$, from cetene, $\text{Ȼ}^{16}\text{H}^{32}$. A compound obtained by the action of hypochlorous acid on benzole, which appears to have the formula $\text{Ȼ}^{6}\text{H}^{9}\text{Cl}^{3}\Theta^{3}$, is still under investigation.

He has also found that other classes of bodies assimilate hypochlorous acid; and he describes some experiments on citraconic acid, $\text{Ȼ}^{5}\text{H}^{6}\Theta^{4}$, only the results of which can be here given. By the above action he obtains a new acid which he calls *chlorocitramalic acid*; thus



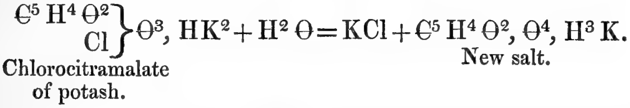
$$\underset{\text{Citraconic acid.}}{\text{Ȼ}^{5}\text{H}^{4}\Theta^{2},\Theta^{2},\text{H}^{2}}+\text{HCl}\Theta=\underset{\text{Chlorocitramalic acid.}}{\left.\begin{matrix}\text{Ȼ}^{5}\text{H}^{4}\Theta^{2}\\ \text{Cl}\end{matrix}\right\}\Theta^{3},\text{H}^{3}}.$$

This is a strong acid, which, dried *in vacuo*, forms a solid, colourless, uncrystallizable substance; it readily exchanges two atoms of hydrogen for metal. When it is treated with zinc, gas is disengaged; the chlorine of the acid becomes replaced by hydrogen, forming a new acid, *citramalic acid*, homologous with malic acid,



$$\underset{\text{Chlorocitramalic acid.}}{\left.\begin{matrix}\text{Ȼ}^{5}\text{H}^{4}\Theta^{2}\\ \text{Cl}\end{matrix}\right\}\Theta^{3},\text{H}^{3}}+\text{H}^{2}=\underset{\text{Citramalic acid.}}{\text{C}^{5}\text{H}^{5}\Theta^{2},\Theta^{3},\text{H}^{3}}+\text{HCl}.$$

When the neutral potash salt of chlorocitramalic acid is boiled, it is rapidly decomposed, forming potassic chloride, and the potassic salt of a new acid,



$$\underset{\text{Chlorocitramalate of potash.}}{\left.\begin{matrix}\text{Ȼ}^{5}\text{H}^{4}\Theta^{2}\\ \text{Cl}\end{matrix}\right\}\Theta^{3},\text{HK}^{2}}+\text{H}^{2}\Theta=\text{KCl}+\underset{\text{New salt.}}{\text{Ȼ}^{5}\text{H}^{4}\Theta^{2},\Theta^{4},\text{H}^{3}\text{K}}.$$

This new acid is homologous with *tartaric* acid, and Carius names it citratartaric acid. With the investigation of both these new acids he is still engaged.

Rieth and Beilstein[*] have investigated the decomposition of the aldehydes and acetates by zincethyle. Ordinary aldehyde acted but slowly on zincethyle, and the action had to be promoted by gentle heating, by which it was effected in a few hours. When there was no more free zincethyle, the decomposition was complete, and the excess of aldehyde was distilled off in the waterbath. The residue consisted of a tough mass, which was a simple

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[*] Liebig's *Annalen*, May 1863.

compound of aldehyde with zincethyle. When treated with water, the mass became heated and hydride of ethyle was disengaged, while a liquid distilled over which had the properties and the composition of acetal. The reaction is thus expressed:—

$$2(\text{Ȼ}^2 H^4 \Theta + Zn\,\text{Ȼ}^2 H^5) + H^2 \Theta = \underbrace{\text{Ȼ}^6 H^{14} \Theta^2}_{\text{Acetal.}} + \text{Ȼ}^2 H^6 + Zn^2\Theta$$

As no higher homologues of acetal are known, the authors tried the decomposition of valeraldehyde, which is much more readily attacked than ordinary aldehyde. The reaction is, however, quite different, and their investigation is not yet complete. The main product of it is identical with a substance which Fittig obtained by the action of caustic lime on valeraldehyde, and which he represented by the formula $\text{Ȼ}^8 H^{14} \Theta$. Rieth and Beilstein consider that its formula is $\text{Ȼ}^{10} H^{18} \Theta$, and they represent its formation in their experiment thus:

$$2\,\text{Ȼ}^5 H^{10} \Theta + Zn\,\text{Ȼ}^2 H^5 = \text{Ȼ}^{10} H^{18} \Theta + \text{Ȼ}^2 H^6 + Zn\,H\,\Theta.$$

Œnanthole, $\text{Ȼ}^7 H^{14} \Theta$, yields an entirely similar body, $\text{Ȼ}^{14} H^{26} \Theta$.

The authors found that zincethyle acted very energetically on acetone, and they found that the action is essentially a dehydrating one, quite analogous to that which caustic lime exerts on acetone, and which Fittig has investigated*. The main product was phorone, which agreed in all its properties with that obtained by Fittig; and, like him, they could not obtain it of a constant boiling-point. Its formula from acetone may be represented by the equation

$$3\,\text{Ȼ}^3 H^6 \Theta - 2\,H^2 \Theta = \text{Ȼ}^9 H^{14} \Theta,$$

or

$$3\,\text{Ȼ}^3 H^6 \Theta + 2\,Zn\,\text{Ȼ}^2 H^5 = \text{Ȼ}^9 H^{14} \Theta + 2\,Zn\,H\Theta + 2\,\text{Ȼ}^2 H^6.$$

The same authors give detailed accounts of their improved methods of preparing zincethyle and iodide of ethyle.

LXXVII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 487.]

March 12, 1863.—Dr. William Allen Miller, Treasurer and Vice-President, in the Chair.

The following communications were read:—

"On the Influence of Temperature on the Electric Conducting Power of Thallium and Iron." By A. Matthiessen, F.R.S., and C. Vogt, Ph.D.

Thallium.—The experiments detailed in this paper were made with specimens of thallium lent to us by Mr. Crookes and Professor Lamy of Lille. The values obtained for the conducting power,

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* Phil. Mag. vol. xix. p. 116.

together with the formulæ for the correction of the conducting power for temperature of the different specimens, were:—

For Mr. Crookes's metal,

1st wire.	2nd wire at 0°.
$\lambda=9{\cdot}364-0{\cdot}037936t+0{\cdot}00008467t^2$;	9·169.

For M. Lamy's first specimen,

	2nd wire at 0°.	3rd wire.
$\lambda=9{\cdot}419-0{\cdot}039520t+0{\cdot}00009656t^2$;	9·082;	9·223.

Second specimen,

	2nd wire at 0°.
$\lambda=9{\cdot}054-0{\cdot}034697t+0{\cdot}00006554t^2$;	9·226;

or as mean of all the determinations, some of which are not given here,

$$\lambda=9{\cdot}163-0{\cdot}036894t+0{\cdot}00008104t^2.$$

The conducting power of thallium therefore decreases between 0° and 100° 31·420 per cent., which is a larger percentage decrement than that obtained for many other pure metals, namely 29·307 per cent.*

Iron.—The specimens of iron experimented were, with two exceptions, lent us by Dr. Percy. In the following Table we give the results obtained with them:—

(1.) Electrotype iron, deposited from solution of pure sulphate of iron. The strips were very thin and porous; we could not therefore obtain concordant values for the conducting power, but we were able to determine the percentage decrement in the conducting power between 0° and 100°. We have, for the above reason, taken the first observed conducting power equal 100.

$$\lambda=100-0{\cdot}51182t+0{\cdot}0012915t^2,$$

corresponding to a percentage decrement of 38·262 per cent.

(2.) No. 1, annealed and cooled in hydrogen.

$$\lambda=100-0{\cdot}51894t+0{\cdot}0013415t^2,$$

corresponding to a percentage decrement of 38·479 per cent.

(3.) Electrotype iron, a strip cut from the same foil as No. 1.

$$\lambda=100-0{\cdot}51355t+0{\cdot}0013221t^2,$$

corresponding to a percentage decrement of 38·134 per cent.

(4.) No. 3, annealed in air.

$$\lambda=100-0{\cdot}50895t+0{\cdot}0002735t^2,$$

corresponding to a percentage decrement of 38·160 per cent.

(5.) This, as well as Nos. 6, 7, 8, were specimens of iron which have been analysed. They were all hard drawn.

$$\lambda=15{\cdot}719-0{\cdot}074370t+0{\cdot}0001763t^2,$$

corresponding to a percentage decrement of 36·070 per cent.

(6.) $\lambda=15{\cdot}672-0{\cdot}074045t+0{\cdot}0001761t^2$,

corresponding to a percentage decrement of 36·010 per cent.

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* Phil. Trans. 1862, Part I.

(7.) $\lambda=14{\cdot}269-0{\cdot}064133t+0{\cdot}0001456t^2$,
corresponding to a percentage decrement of 34·742 per cent.

(8.) $\lambda=12{\cdot}342-0{\cdot}055894t+0{\cdot}0001379t^2$,
corresponding to a percentage decrement of 34·117 per cent.

(9.) Strip of iron, heated in a current of hydrogen at a red heat for two hours. This, as well as Nos. 10, 11, 12, were hardened.
$\lambda=14{\cdot}673-0{\cdot}067999t+0{\cdot}0001597t^2$,
corresponding to a percentage decrement of 35·459 per cent.

(10.) As No. 9, heated for three hours under sugar charcoal in a current of hydrogen; the carbon taken up was 0·99 per cent.
$\lambda=10{\cdot}654-0{\cdot}044560t+0{\cdot}00009789t^2$,
corresponding to a percentage decrement of 32·637 per cent.

(11.) As No. 9, heated for four hours under sugar charcoal in a current of hydrogen; the carbon taken up was 0·933 per cent.
$\lambda=9{\cdot}925-0{\cdot}040097t+0{\cdot}00009168t^2$,
corresponding to a percentage decrement of 31·163 per cent.

(12.) As No. 9, heated for three hours under sugar charcoal in a current of hydrogen; the carbon taken up was 1·06 per cent.
$\lambda=9{\cdot}457-0{\cdot}037573t+0{\cdot}00008642t^2$,
corresponding to a percentage decrement of 30·592 per cent.

(13.) Thin music wire, melted with one quarter of its weight of peroxide of iron under a flux of plate glass.
$\lambda=13{\cdot}381-0{\cdot}056829t+0{\cdot}0001230t^2$,
corresponding to a percentage decrement of 33·278 per cent.

(14.) A piece of narrow watch-spring.
$\lambda=8{\cdot}565-0{\cdot}029099t+0{\cdot}00005383t^2$,
corresponding to a percentage decrement of 27·689 per cent.

(15.) Commercial iron wire.
$\lambda=13{\cdot}772-0{\cdot}058970t+0{\cdot}0001242t^2$,
corresponding to a percentage decrement of 33·801 per cent.

From the results obtained, it is obvious that the higher the cor ducting power the higher the percentage decrement in the conducting power between 0° and 100°. This has been proved to be the case with about 100 alloys with which we have experimented. We have also found that we may deduce the conducting power of a pure metal from an impure one when the impurity does not reduce the conducting power more than, say, 10 to 20 per cent. According to our experiments, the percentage decrement in the conducting power of an impure metal between 0° and 100° varies in the same ratio as the conducting power of the impure metal at 100°, compared with that of the pure metal at 100°.

Thus, from specimens Nos. 5, 6, 7, 9, 13, and 15, the conducting power of pure iron was found to be at 0°=16·725.

In conclusion, we give the values found for specimens of cobalt and nickel wire lent to us by Professor Wöhler. They were a follows :—

Cobalt wire.

$$\lambda = 12{\cdot}930 - 0{\cdot}035521t + 0{\cdot}00004887t^2,$$

corresponding to a percentage decrement of 23·692 per cent.

Nickel wire.

$$\lambda = 12{\cdot}222 - 0{\cdot}040787t + 0{\cdot}00007088t^2,$$

corresponding to a percentage decrement of 27·573 per cent.

Although these metals were said to be chemically pure, the results obtained seem to indicate that they are not so, having probably taken up some impurities in the process of fusion.

The following Table of the conducting powers of pure metals shows the place which the metals treated of in this paper take in the series.

	Conducting power at 0°.
Silver (hard drawn)	100·00
Copper (hard drawn)	99·95
Gold (hard drawn)	77·96
Zinc	29·02
Cadmium	23·72
Cobalt *	17·22
Iron * (hard drawn)	16·81
Nickel *	13·11
Tin	12·36
Thallium	9·16
Lead	8·32
Arsenic	4·76
Antimony	4·62
Bismuth	1·245

"On the Amyloid Substance of the Liver, and its ultimate destination in the Animal Economy." By Robert McDonnell, M.D.

March 19.—"On Peculiar Appearances exhibited by Blood-corpuscles under the influence of Solutions of Magenta and Tannin." By William Roberts, M.D.

"On Quinidine, and some Double Tartrates of the Organic Bases." By John Stenhouse, LL.D., F.R.S.

March 26.—Major-General Sabine, President, in the Chair.

The following communications were read:—

"On Diffusion of Vapours: a means of distinguishing between apparent and real vapour-densities of Chemical Compounds." By J. A. Wanklyn and J. Robinson, Esq.

The density of the vapour given off when a chemical compound is heated is not necessarily the *vapour-density* of that chemical compound; sometimes it is only the mean density of the products of decomposition. Some of the best-known substances, such as hydrated sulphurous acid, ammoniacal salts, and pentachloride of phosphorus, suffer decomposition when they are vaporized, and thus have an

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* Probable value for the pure metal deduced from the observations with the impure one.

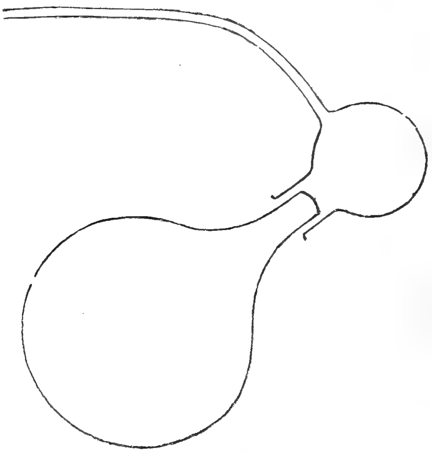
apparent vapour-density, which is in reality nothing more than the mean density of the products of their decomposition.

We recognize such cases—in which the apparent is not the real vapour-density—by making a diffusion-analysis of the vapours. This method of solving questions of the kind was proposed by one of us two years ago*. In carrying it out practically, it was resolved from the first not to diffuse through a porous diaphragm, but to recur to Graham's original method, namely, to let our vapours diffuse through a simple aperture or through a short tube.

Independently of the experimental difficulties attending the use of a porous diaphragm at high temperatures, there is a fatal objection to it, founded upon the inconclusiveness of the results obtained in such a way.

Our resolve to avoid porous substances was not by any means shaken by Pebal's memoir† on the diffusion of chloride-of-ammonium vapour through asbestos; for what is more likely than that a finely-divided silicate (a salt of an acid of indefinite capacity of saturation) should decompose ammoniacal salts at elevated temperatures?

The apparatus used in our experiments is of the simplest kind. It is represented in the drawing, and consists of two glass flasks, the necks of which do not fit air-tight: the narrow tube proceeding from the upper one is fused to the flask. The lower flask is for the re-



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* Playfair and Wanklyn on Vapour-densities, Transactions of Roy. Soc. of Edinburgh, 1861, vol. xxii. part 3. p. 458. In this paper it was proposed to extend to vapours what had previously been applied to gases. One of the earliest, perhaps the earliest example of a precise diffusion-analysis of a gas was given by Frankland in his research upon the isolation of ethyle (see Quart. Journ. Chem. Soc. vol. ii. p. 285, 1850). After describing his diffusion-apparatus and its use in the case of ethyle, Frankland proceeds, "This method might in almost every case be employed with advantage to determine whether or not any specimen of gas be simple or mixed."

† Ann. de Chim. et de Phys. January 1863.

ception of the vapour to be operated upon; the upper flask is for the atmosphere into which the vapour is to be diffused. The atmosphere of dry air, or other suitable gas, is kept constantly renewed by the transmission of a slow stream of gas, which enters the upper flask by the narrow tube above, and passes out by the space between the two necks, which, as we have said, do not fit air-tight. When in use, the whole apparatus is kept at a temperature above the condensing-point of the vapour by means of an air-bath.

After a diffusion has gone on for a sufficient length of time the apparatus is allowed to cool, and the contents of the lower flask are analysed, by which means it is seen whether diffusion has effected any alteration in the composition of the vapour.

We have used a lower flask of about 500 cubic centimetres capacity, with a mouth 10 millimetres in diameter; the capacity of the upper flask was 100 cubic centimetres.

The first substance taken for experiment was sulphuric acid, which is converted at high temperatures into vapour of sulphuric anhydride and vapour of water. Inasmuch as vapour of water is lighter than vapour of sulphuric anhydride, the former should diffuse more rapidly than the latter. Accordingly, the residue after diffusion should be richer in sulphuric anhydride than the acid before diffusion.

In one experiment we took an acid composed of

95 Mono-hydrated sulphuric acid.
5 Water.
——
100

After diffusion for an hour at about 520° C., the residue was composed of

60 Mono-hydrated sulphuric acid.
40 Sulphuric anhydride.
——
100

In another experiment we took an acid containing

99 Mono-hydrate.
1 Water.
——
100

and after diffusion for a shorter time at 445° C. found the residue to consist of

75 Mono-hydrate.
25 Anhydride.
——
100

In both cases the residues after diffusion fumed strongly on exposure to the air, and consisted partly of crystals and partly of liquid.

The substance next submitted to diffusion was pentachloride of phosphorus, which is decomposed by heat into terchloride and free chlorine.

The pentachloride which we used gave no reaction with iodide of potassium and starch, and therefore contained no free chlorine; it gave no precipitate with corrosive sublimate, and therefore contained no terchloride of phosphorus. An analysis of it gave

Percentage of chlorine=84·67
The formula requires... 85·13

In one experiment we diffused into carbonic acid gas* for three-quarters of an hour at about 300° C., and afterwards dissolved the contents of the lower flask in water, and precipitated with corrosive sublimate, with the addition of a little hydrochloric acid. ·0175 gramme of calomel was obtained. In another experiment (also into carbonic acid) the time of diffusion was two hours, temperature 300° C., quantity of calomel obtained ·0285 gramme.

These two results leave no doubt as to the existence of terchloride of phosphorus in the residue after diffusion; for the reduction of corrosive sublimate to calomel cannot be otherwise explained. Moreover, the presence of free chlorine in the diffused gases was shown by the reaction with iodide of potassium and starch.

GEOLOGICAL SOCIETY.

[Continued from p. 323.]

June 17, 1863.—Professor A. C. Ramsay, President, in the Chair.

The following communications were read:—

1. "On the Relations of the Cromarty Sandstones containing Reptilian Footprints." By the Rev. George Gordon, LL.D., and the Rev. J. M. Joass. With an Introduction by Sir R. I. Murchison, K.C.B., F.R.S., F.G.S.

In the introduction Sir R. I. Murchison gave a sketch of the geology of the Tarbatness promontory, which is composed of variously coloured sandstones, having a conformable dip to the N.W. In these strata the authors had found footprints (of animals believed to be Reptiles) similar to those found in the sandstones on the coast of Elgin; and it was therefore desirable to prove whether these rocks really belonged to the Palæozoic series, or, as some geologists suppose with regard to the Elgin sandstones, to the Trias. In order to solve this problem, if possible, the Rev. Mr. Joass made a careful survey of the coast from Geanies to Tarbatness Lighthouse, and round along the north shore of the promontory to the inlet at Inver, and found a conformable succession between the undoubted Old Red Sandstone of Geanies and the track-bearing sandstone of Tarbatness.

The Rev. Dr. Gordon gave a description of the various tracks: the smaller kind are referred by him to an unknown Crustacean; the larger and more definite impressions, however, he considered to

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* If pentachloride of phosphorus be diffused into air, the residual terchloride combines with oxygen to form oxychloride of phosphorus, which does not reduce corrosive sublimate.

be the footsteps of some kind of reptile. He also stated, as confirmatory of the 'Old Red' age of the beds, that the Oolitic beds of Shandwick are unconformable to the Old Red Sandstone.

2. "On some Tertiary Shells from Jamaica." By J. Carrick Moore, Esq., F.R.S., F.G.S. With a Note on the Corals, by P. Martin Duncan, M.B. Lond., F.G.S.; and a Notice of some *Foraminifera*, by Prof. T. Rupert Jones, F.G.S.

An examination of 71 species of Tertiary Mollusca from Jamaica, mostly collected by the late Mr. Barrett, showed that 12 are still living, and that 28 are common to the Tertiary beds of Jamaica and St. Domingo. The same relation between those deposits had been found to exist by Dr. Duncan through a comparison of the Corals. The 'Pacific' affinity of many of these Shells and Corals was noticed as confirmatory of a conclusion arrived at by the author in a former paper; and it was shown, from the occurrence of Tertiary beds on the Panama Isthmus at a height of 250 feet above the sea, that the complete separation of the Atlantic and Pacific Oceans did not take place until after the commencement of the Tertiary period. The distribution and affinities of the *Nummulinæ* and *Orbitoides* were described by Prof. Jones, and those of the Corals by Dr. Duncan.

3. "On the Geology and Mineralogy of a part of Borneo." By M. Cornelius de Groot.

A collection of specimens from Borneo and Java, presented by the author to the Museum of Practical Geology, was accompanied by some explanatory notes, in which it was stated that the Steam-coal of Borneo underlies the Nummulitic Formation, and probably belongs to the 'Étage Suessonien' of D'Orbigny. The occurrence of tin in stream-works, and as veins in granite, at several places in the Western portion of Borneo and the Islands of Blitong and Banka, was particularly described, as well as the presence of ores of copper and manganese. Some Tertiary fossils from Borneo and Java were also noticed.

4. "Description of a new Fossil *Thecidium* from the Miocene Beds of Malta." By J. Denis Macdonald, Esq., F.R.S.

Amongst the many fossils occurring in the lower part of the Calcareous Sandstone of Malta is a small *Thecidium*, nearly allied to the recent *T. Mediterranea*, but much smaller than that species, and, on close inspection, presenting other and very decided specific distinctions. Mr. Macdonald gave a detailed description and figures of this new species, and proposed for it the name of *Thecidium Adamsii*.

5. "On the Sandstones and Shales of the Oolites of Scarborough, with descriptions of new species of fossil Plants." By J. Leckenby, Esq., F.G.S.

The true position of the well-known Plant-bed at Gristhorpe Bay, below the grey limestone, was first pointed out by Prof. Williamson, and afterwards by Dr. Wright; and Mr. Leckenby showed that all the plants, hitherto referred to the Upper Sandstone, Shale, and Coal, belong to the Lower Sandstones and Shales, but few plants having been found in the true Upper Sandstones and Shales overlying the grey limestone. The author gave lists of fossil Plants occurring in the two sets of strata, as well as of those occurring at the separate localities; and he concluded by describing some new and some imperfectly known species of Ferns.

6. "A Monograph of the Ammonites of the Cambridge Greensand." By H. Seeley, Esq., F.G.S.

7. "On a new Crustacean from the Glasgow Coal-field." By J. W. Salter, Esq., F.G.S.

In a previous paper the author collected together what was known of the Macrurous Crustaceans of the British Coal-fields. Since then Mr. James Russell has discovered a new species, remarkable on account of the antennules being larger than the antennæ, and which Mr. Salter now described in detail.

8. "On the Occurrence of a Bituminous Substance near Mountgerald, Scotland." By Dr. G. Anderson. In a letter to Sir R. I. Murchison, K.C.B., F.R.S., F.G.S., &c.

The section exposed by some deep cuttings for the Ross-shire Railway, two miles north of Dingwall, exhibits a grey micaceous and almost horizontal conglomerate, traversed by fissures containing a black bituminous substance, which the author stated might have infiltrated down from a bed of bituminous schist which apparently overlies the conglomerate.

9. "On the Occurrence of Albertite at Mountgerald, Scotland." By A. C. Mackenzie, Esq.

In making a drain on a farm near Mountgerald, a fissure was discovered which contained a bituminous substance very similar to the Albertite of New Brunswick; a deposit of a similar nature was also discovered in making a cutting through "The Craig," near Mountgerald. The author described the stratigraphical and lithological characters of the rocks met with in making this cutting, and stated that thirty-six veins containing this mineral were passed through.

10. "On the Occurrence of Rocks of Upper Cretaceous Age in Eastern Bengal." By Dr. T. Oldham, F.R.S., F.G.S.

During the years 1851–52 the author examined the rocks of the Khasi Hills, in Eastern Bengal, and made a collection of the fossils occurring there. These fossils were mostly from a Nummulitic Limestone, and were evidently of Eocene date; but some were obtained from a sandstone underlying this limestone, and appeared to be of Cretaceous rather than Tertiary age.

November 4, 1863.—Professor A. C. Ramsay, President, in the Chair.

The following communications were read:—

1. "On some Ichthyolites from New South Wales sent over by the Rev. W. B. Clarke, F.G.S." By Sir P. de M. Grey Egerton, Bart., M.P., F.R.S., F.G.S.

Two specimens and three photographs, sent to England for the author's determination, enabled him to distinguish four genera, two of which are new, and allied to *Acrolepis* and *Platysomus* respectively; the known genera being *Urosthenus,* Dana (allied to *Pygopterus*), and *Palæoniscus,* Agass. Sir Philip was of opinion that these genera were sufficient to stamp the deposit in which they occur—namely, the Coal-formation of New South Wales—as belonging to the Palæozoic period, if they may be regarded as representative genera living at the same period as, but geographically distant from, their nearest allies; but, as regards the actual age of the formation, the allied genera are more abundantly represented in the Magnesian Limestone and the Kupferschiefer than in the Coal-measures; the materials were, he considered, too meagre to justify a conclusion.

2. "Notes on the Geology of a portion of the Nile Valley north of the Second Cataract, in Nubia, chiefly with the view of inducing further search for Fluviatile Shells at High Levels." By A. Leith Adams, A.M., M.B., Surgeon 22nd Regiment. With a Note on the Shells, by S. P. Woodward, Esq., F.G.S.; and a Note on some Teeth of *Hippopotamus,* by Hugh Falconer, M.D., F.R.S., F.G.S.

In company with the late Mr. A. H. Rhind, F.S.A., the author made the usual boat-voyage from Cairo to the Second Cataract during last November and the two following months, when he was enabled to make some observations on the geology of that portion of the Nile Valley. In this paper he first described the physical features of the district, beginning at Selsileh and proceeding southwards, and then the lithological and stratigraphical characters of the Nile sandstone, as well as its mode of junction with the granite, noticing also the evidences of the Nile having shifted its bed, and of other physical changes occurring in Nubia. Near the Second Cataract were abundant proofs of the river having formerly flowed at higher levels, the author having found river shells, such as *Cyrena fluminalis, Paludina bulimoides, Iridina Nilotica,* and *Œtheria semilunata* (the Nile Oyster), as also *Bulimus pullus* and a *Unio* like *U. pictorum,* in beds of alluvium on elevated plateaus at various heights, ranging up to 130 feet, above the highest inundations of the present day.

Dr. Adams concluded from these facts that the Nile was formerly a more rapid river than it is now, and that the force and wearing power of the stream has been steadily declining since the upheaval of the valley ceased.

The determinations of the shells were made by Mr. Woodward, who gave a complete list of them in a Note. Dr. Falconer also described two molars imbedded, *in situ,* in a fragment of the left maxillary of a very large Hippopotamus; the specimen was dug up near the old Temple of Kalábshe, in Nubia; and Dr. Falconer was of opinion that it belonged to the same species as the existing Hippopotamus of that country.

November 18, 1863.—Professor A. C. Ramsay, President, in the Chair.

The following communications were read:—

1. "On the Fossil Corals of the West Indies."—Part II. By P. Martin Duncan, M.B., F.G.S.

This communication embodied the second part of the author's researches on the Fossil Corals of the West Indies (the first part having been read before the Society last session), and consisted chiefly of a description of Corals returned to the Society's Museum by Mr. Lonsdale soon after the reading of the first part. Some portions of these descriptions were stated to have been taken from an unpublished MS. by Mr. Lonsdale, now in the Society's Library, with the permission of that gentleman, such quotations having been duly indicated.

The predominance of simple fossil Corals in San Domingo, and their complete absence in Antigua, were pointed out; and it was remarked that the same kind of distribution occurs at the present day, pedunculated compound forms being very common around the northern Antilles, but rare around the north-eastern, although the corals are mostly of different genera from those found in the fossil state. The author concluded with some remarks on the physical conditions of the Miocene period in the West Indies, observing that the Nivaje shales and associated deposits are the remains of an ancient barrier-reef, and giving an analytical table of the affinities of the species, in which it was shown that the Pacific and East Indian element greatly preponderated.

2. "Notes to accompany some Fossils from Japan." By Captain Bullock. Communicated by Sir R. I. Murchison, K.C.B., F.R.S., F.G.S.

There having been no geologist attached to the late surveying-expedition of H.M.S. "Dove," the Commander of that vessel endeavoured to repair the consequent loss to science, so far as his professional duties would allow, by collecting fossils, and by recording their localities and the circumstances under which they were found. The specimens were presented to the Geological Society through Sir R. I. Murchison; and this paper contained all the information, respecting their occurrence, which Captain Bullock had been enabled to obtain.

3. "On some Miocene Mollusca from Mount Séla, in the Island of Java." By H. M. Jenkins, Esq., F.G.S. With a Description of a new Coral from the same locality, and a Note on the Scindian fossil Corals; by P. Martin Duncan, M.B., F.G.S.

A short notice of the scanty literature of Javan Geology having been given, the author described briefly the geological and physical features of the Mount-Séla district, and made some general observations on, and gave descriptions of, the fossils which were the subject of the paper, and which had been sent to England by M. Corn. de Groot.

Of sixteen determinable species, only three (or 19 per cent.) are now known to exist, the remainder being new species; but Mr. Jenkins showed that the fossils were probably more recent than this small percentage of living species would appear to indicate, in consequence of there having occurred an emigration eastwards of at least a part of the Southern- and Middle-European Miocene and Eocene fauna. This emigration was proved by the identity of many species which occur fossil in the European Miocene, and now exist in the Eastern seas, and also by certain genera being represented in that formation and the Eocene, and confined in the living state to the Indo-Pacific region.

One of the Javan species being closely related to *Vicarya Verneuilii* from Scinde, the author was induced to investigate the claims of the Nummulitic Formation of India to be considered altogether of Eocene date; and he inferred that there was a probability of some of the beds belonging to a less remote period. This inference was supported by Dr. Duncan in a Note upon the Scindian fossil Corals, many of which (unnamed by M. Haime) were shown to have Miocene and recent, but not Eocene, affinities.

Mr. Jenkins next referred to the diminutive character of many of these Javan fossils, and then reviewed the opinions of former writers upon the Tertiary Formation of that island, coming to the conclusion that the Mount-Séla shells were probably of late Miocene date, and that the plants described by Dr. Goeppert were probably newer than the Eocene.

The fossil Coral from Mount Séla was shown by Dr. Duncan, in a Note to this paper, to be allied to *Astræa quadrangularis*, Edwards and Haime, the habitat of which is unknown.

LXXVIII. *Intelligence and Miscellaneous Articles.*

ON THE COMPOSITION OF THE WATER OF THE DEAD SEA.

BY M. ROUX.

THE water was taken from the northern part of the Dead Sea, not far from the mouth of the Jordan, on the 24th of April 1862. It was slightly alkaline to turmeric paper, became of a red colour by the addition of a few drops of solution of logwood, and when heated for some minutes was not appreciably turbid. The residue after evaporation, heated to a tolerably high temperature in a retort, gave a white sublimate which had all the characters of sal-ammoniac.

Treated by Boussingault's method, the water of the Dead Sea gave appreciable quantities of ammonia. Carefully evaporated at 100° C., 100 grms. left a greyish-white residue weighing 23·576 grms. This, when deprived of its water of crystallization by heating it to dull redness for some time, and allowing for the hydrochloric acid liberated by the decomposition of the chloride of magnesium, weighed 20·60 grms. The analysis showed that this residue was constituted as follows:—

	grms.
Chloride of magnesium	9·466
Chloride of sodium	6·126
Chloride of calcium	3·152
Chloride of potassium	1·388
Bromide of magnesium	0·364
Sulphate of lime	0·058
Chloride of ammonium	0·004
Carbonate of lime Sesquioxide of iron Alumina	0·032
Loss	0·010
	20·600
Water	79·400
	100·000

Hence the water, taken as above, contained 206 grms. of salts in a litre. No mineral water that we possess is so charged with mineral substances; none contains so much bromine.

M. Roux points out that the large quantity of bromine which this water contains may give it special therapeutic properties. A cubic metre of the water contains upwards of 3 kilogs. of bromide of magnesium—and, indeed, possibly more, for Gmelin found 4·393 grms. in 1000 of the Dead Sea water*.—*Comptes Rendus*, October 5, 1863.

DIRECT DETERMINATION OF OXYGEN IN ORGANIC BODIES.

Messrs. Wanklyn and Frank have found that organic bodies, when they are heated to redness in excess of hydrogen, evolve *some*, if not *the whole*, of their oxygen in the form of water, and found upon this fact at least a qualitative, if not a quantitative method of determining oxygen.

ON THE VOLUMETRIC RELATIONS OF OZONE. BY M. T. L. SORET†.

Referring to his method ‡ of obtaining ozonized oxygen very rich in ozone by means of electrolysis, the author described experiments made on ozonized oxygen prepared by that method. These experiments related to the action of iodide of potassium, of several other chemical reagents, and of heat upon the gas; and their result was the same as that of Messrs. Andrews and Tait.

Thus, having treated his ozonized oxygen with iodide of potassium, M. Soret confirms the remarkable result of Messrs. Andrews and Tait, that ozonized oxygen undergoes no change of volume when it acts upon iodide of potassium, but liberates an amount of iodine which is equivalent to a very appreciable volume of oxygen.

With arsenite of soda a similar result was obtained,—viz., contraction insensible; volume of oxygen which entered into combination with the arsenite quite appreciable.

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* Compare Boussingault's Analysis, Phil. Mag. vol. xiii. p. 504.

† Abstract by Professor Wanklyn of a Communication made to the Society of Natural History at Heidelberg, 17th July 1863.

‡ Phil. Mag. March 1863.

The action of heat upon ozonized oxygen was found to bring about a sensible permanent expansion (*i. e.* an expansion which was permanent on cooling down to the original temperature). M. Soret heated his gas by means of a spiral of platinum wire maintained at a red heat by an electric current. Furthermore this expansion of the ozonized oxygen was just equal to the volume of oxygen which the same gas could give up to iodide of potassium.

Caustic potash acts like heat, and not like iodide of potassium or arsenite of soda: thus, it produces an expansion nearly equal to the volume of oxygen which the gas could give to iodide of potassium if it were treated with that reagent.

The author suggests this explanation of the action of potash upon ozonized oxygen: it first becomes peroxide of potassium by seizing the oxygen given off by the decomposition of ozone, and then the peroxide of potassium itself decomposes, and so the expansion is produced. The oxygen freshly given off by peroxide of potassium is liberated in a solution; it would therefore dissolve more readily than the rest of the oxygen, or rather would form, as it were, a supersaturated solution; and so the very slight inequality between the observed expansion of ozonized oxygen on treatment with potash, and the volume of oxygen which the same sample of ozonized oxygen is capable of giving to iodide of potassium, is accounted for.

The author remarks that his results, which agree with those of Messrs. Andrews and Tait, are capable of explanation by the theory which has been maintained by Weltzien and Von Babo, and which consists in representing the molecule of ozone as consisting of several molecules of oxygen.

In producing oxidation, *i. e.* in acting upon iodide of potassium or arsenite of soda, a volume of ozone gives up oxygen to the body which undergoes oxidation, and liberates a volume of inactive oxygen equal to the volume of the original ozone. Thus there is no alteration in the volume of the gas, and yet oxygen is absorbed by the reducing agent. In being acted upon by heat, the ozone is simply decomposed into ordinary oxygen, and consequently the result is an expansion which is equal to the volume of oxygen capable of being given up to reducing agents.

M. Soret calls attention to the fact that, until a determination of the density of ozone shall have been made (for which either pure ozone would be required, or some reagent which could absorb both the active and the inactive oxygen set at liberty on the decomposition of ozone), we cannot say how many molecules of oxygen go to form one molecule of ozone; and, after a notice of Messrs. Andrews and Tait's notion that oxygen itself is a compound, concludes with the observation that the theory of M. Clausius, that ozone exists in combination in ordinary oxygen, is tenable if the addition be made that ozone at the moment of its liberation combines with ordinary oxygen. This latter view would involve just the same experimental facts as the former one*, and, according to the author, would still be in harmony with the reasoning employed by M. Clausius.

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* The theory would indeed involve the experimental facts above given, but it would involve further experimental facts which the theory ascribed to Weltzien and Von Babo would not involve.—J. A. W.

INDEX TO VOL. XXVI.

END OF THE TWENTY-SIXTH VOLUME.

ALERE FLAMMAM.

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RED LION COURT, FLEET STREET.

Fig. 1.

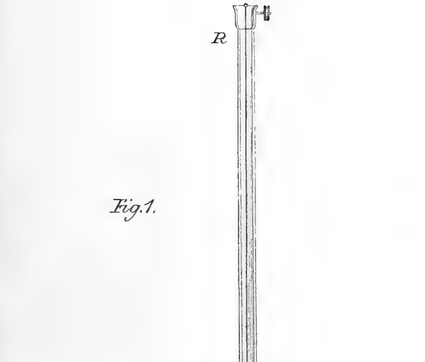
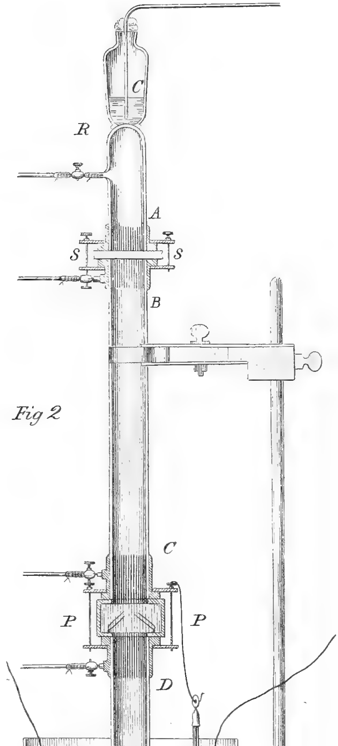


Fig 2



Scale to Fig. 1.

Scale to Fig. 2.

J. Basire sc.

Fig 1.

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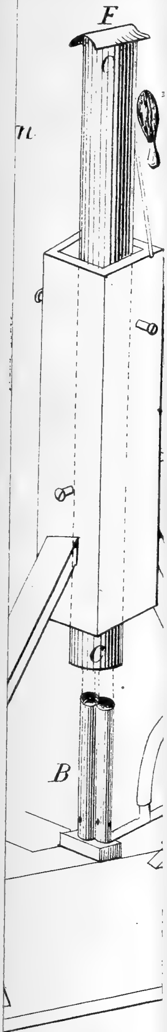

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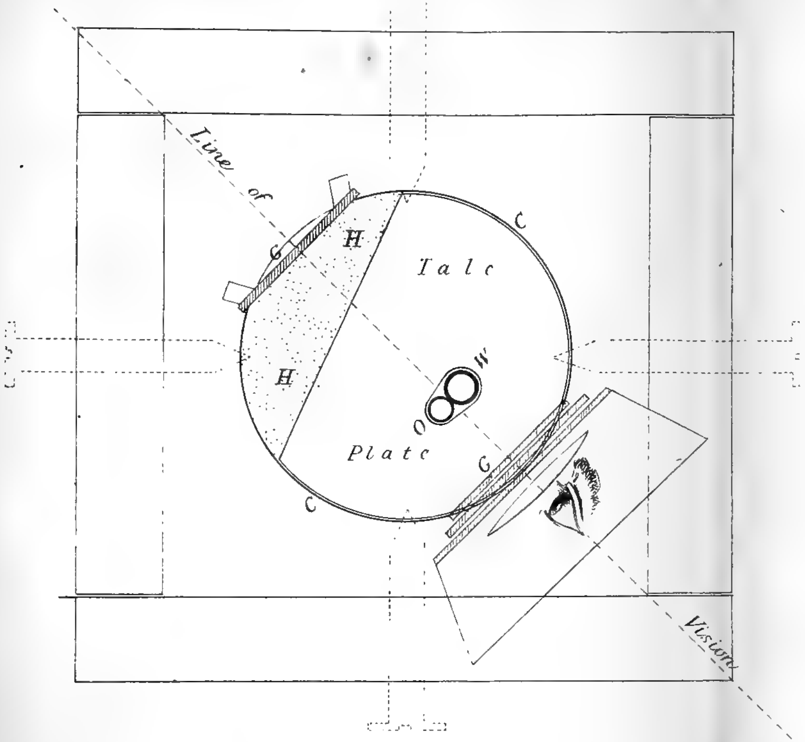
Fig. 2.
Plan of upper part of Heater
(Full size)



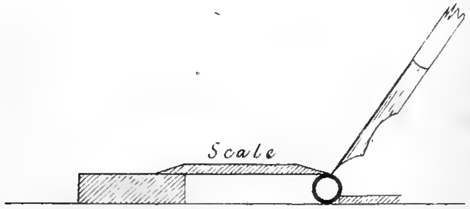


Fig. 6. *Side view of Scale marking*

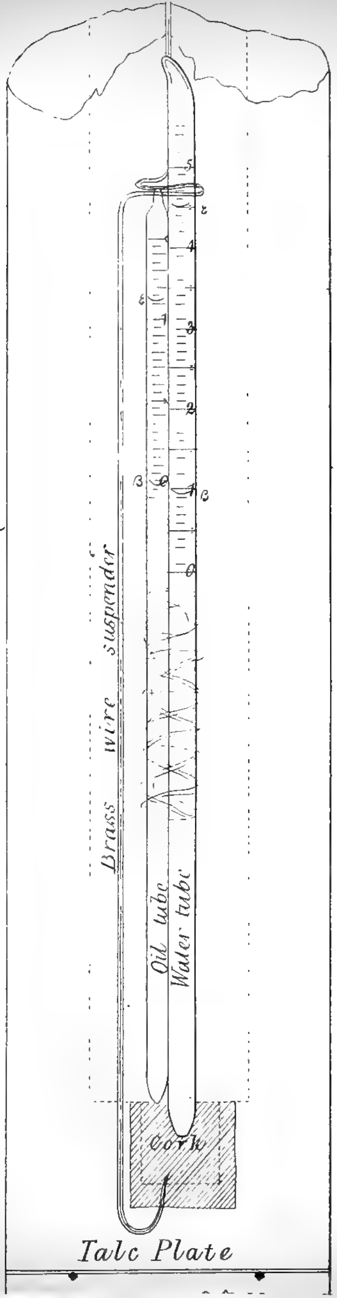
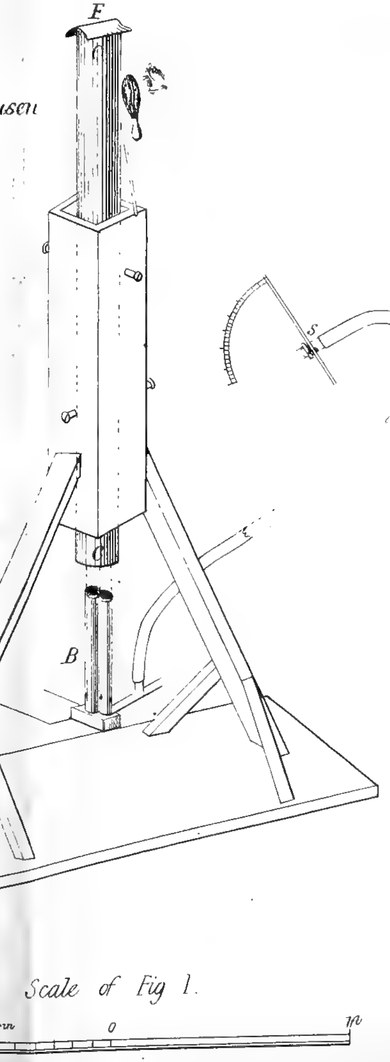


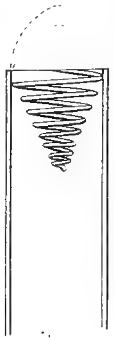
Fig. 3.
Front View of Tubes
(Full size)

Fig 1.
General View of Heating Apparatus



Scale of Fig 1.

Fig. 5
Top of Bunsen



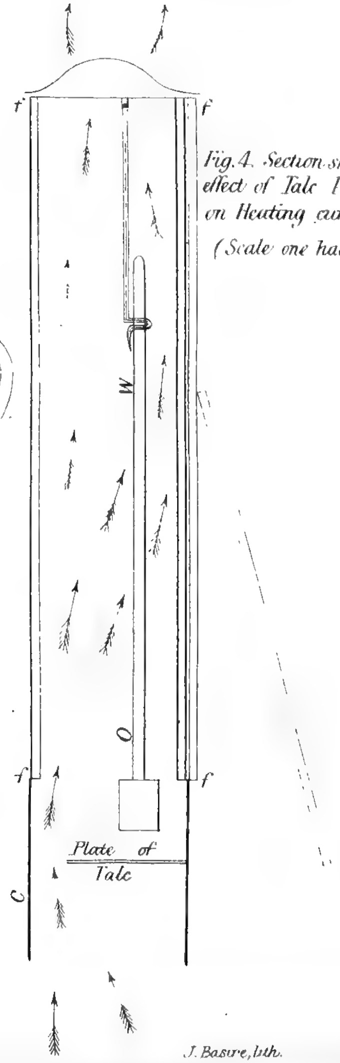


Fig. 4. Section shewing effect of Talc Plate on Heating current
(Scale one half)

J. Basire, lith.

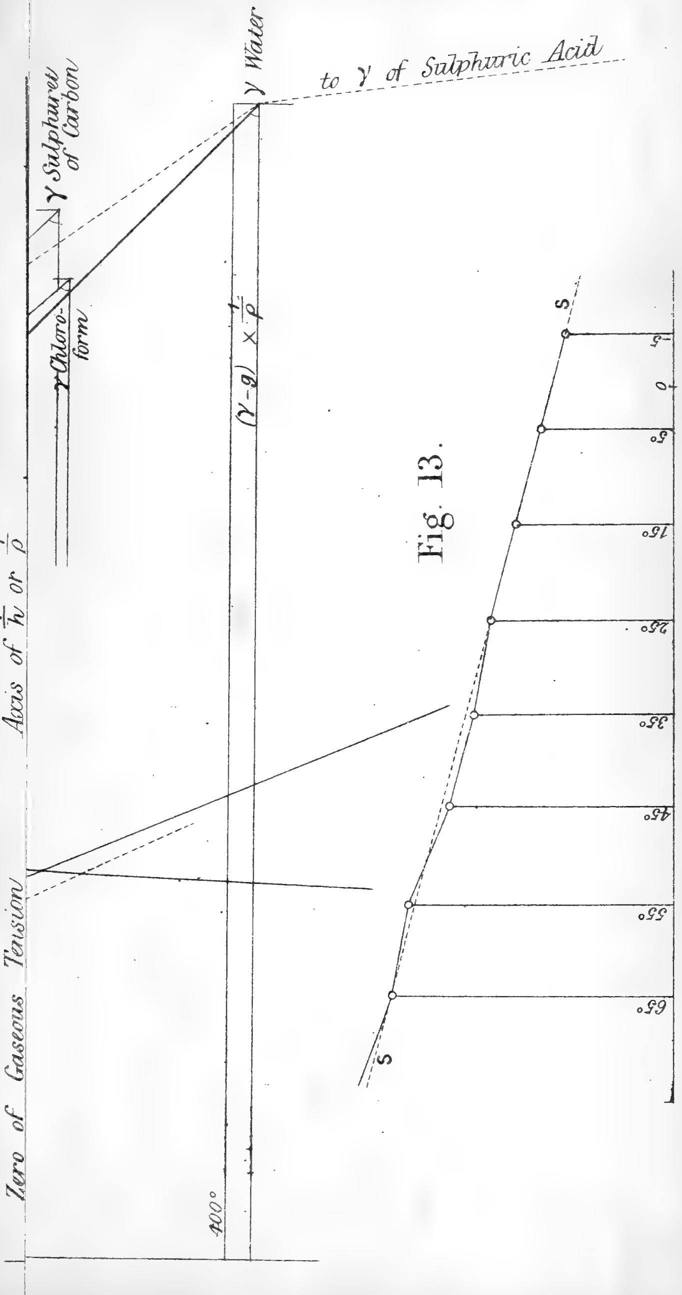
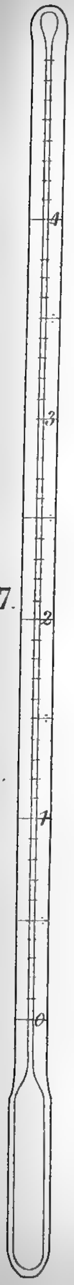


Fig. 13.

J. Basire, lith.



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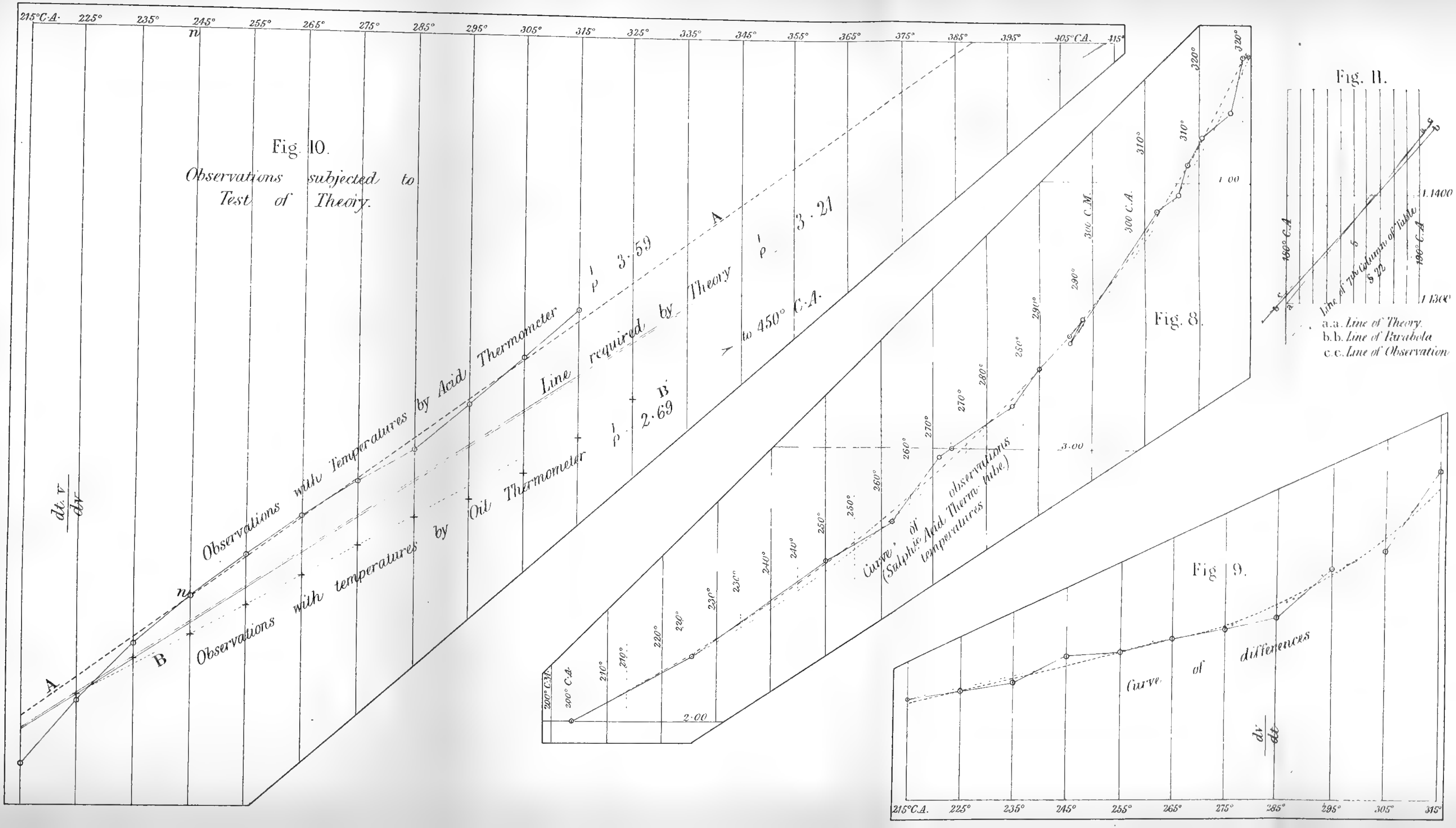

Fig. 10.

Observations subjected to Test of Theory.

Fig. 8.

Fig. 9.

Fig. 11.

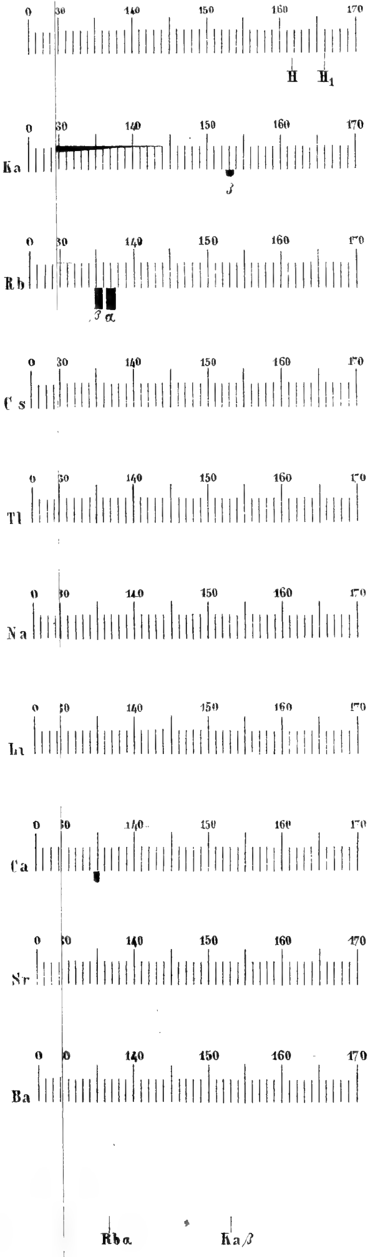
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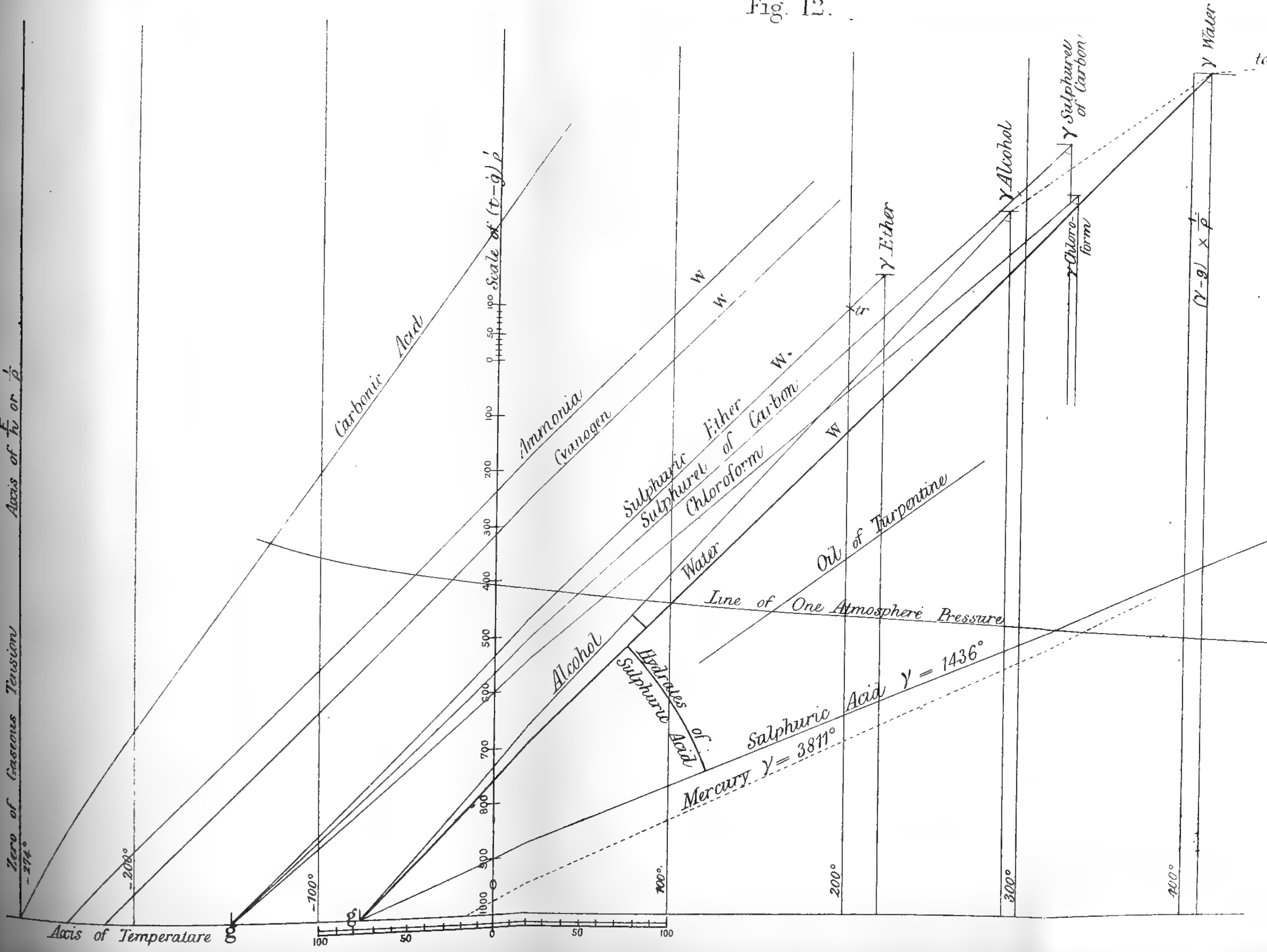
Fig. 12.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Sulphuric Ether
Sulphuret of Carbon
Chloroform
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Alcohol
Oil of Turpentine
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γ Sulphuret of Carbon
γ Chloroform
γ Water
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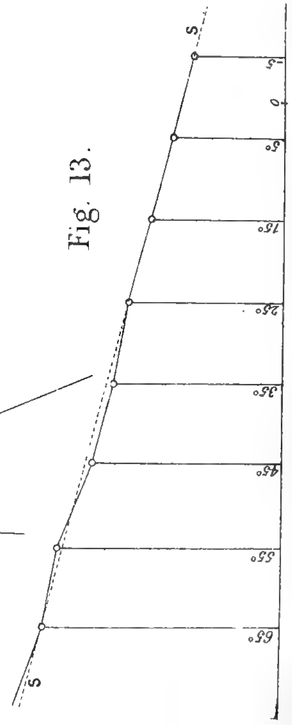
to γ of Sulphuric Acid

Fig. 13.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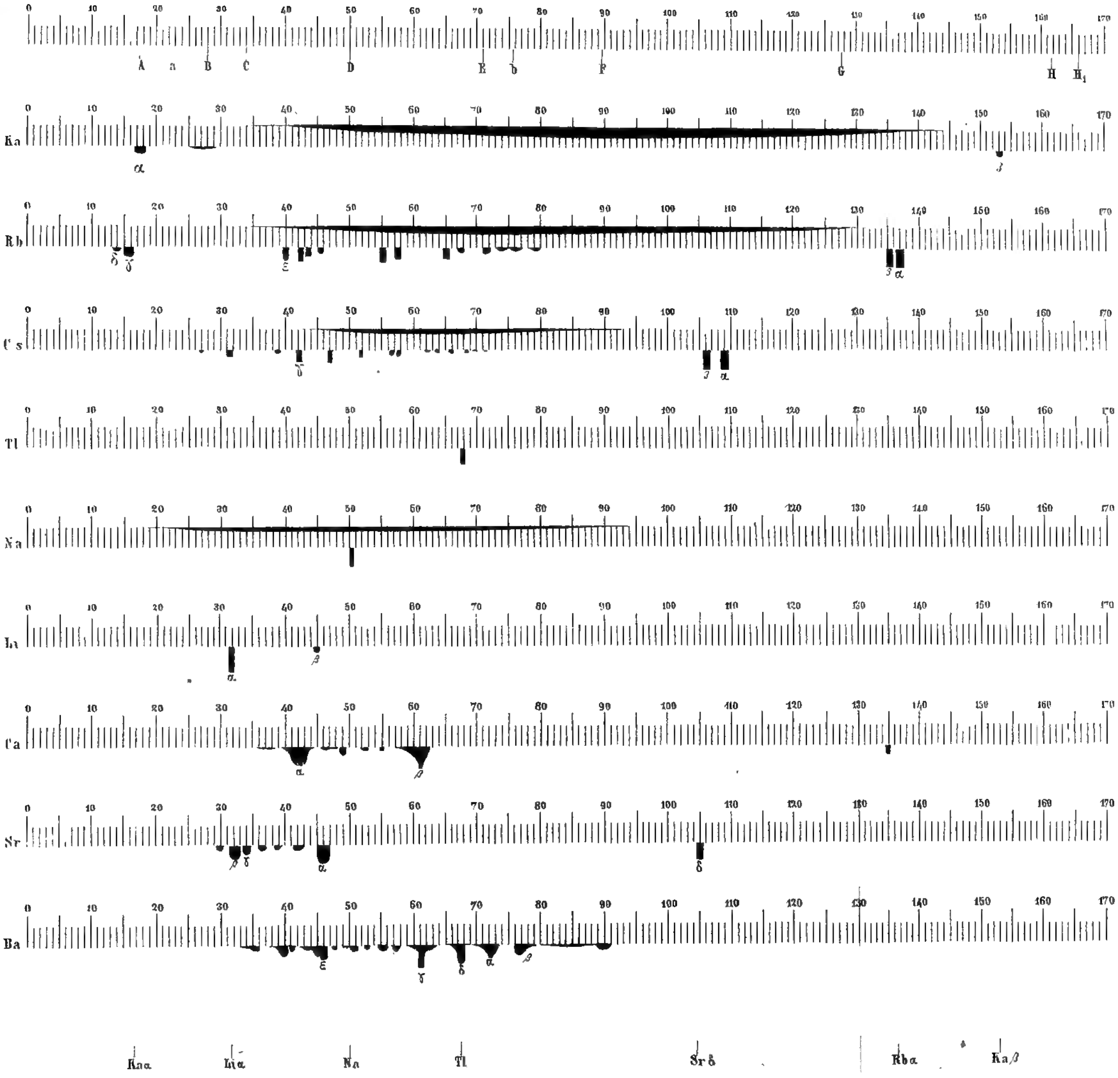
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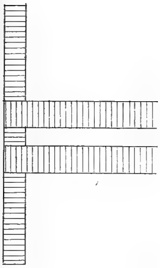
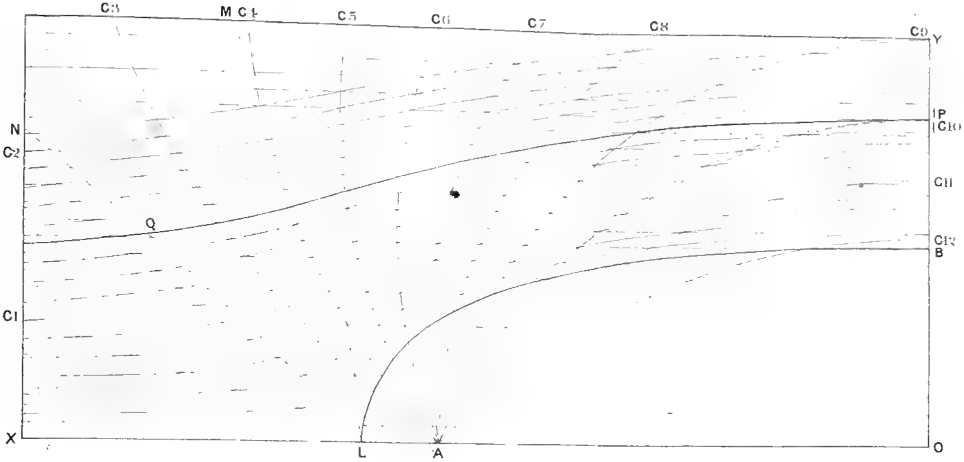
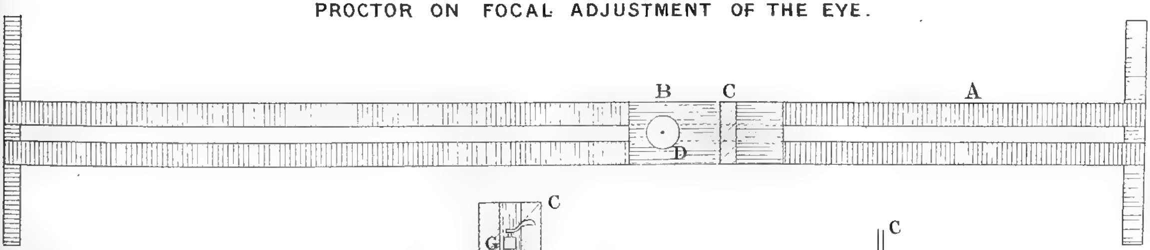
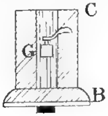


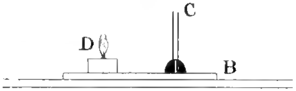
RANKINE ON WATER-LINES, 1863.



PROCTOR ON FOCAL ADJUSTMENT OF THE EYE.







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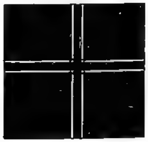

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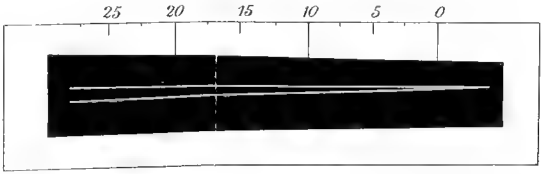
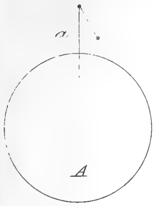


J. Basire lith.



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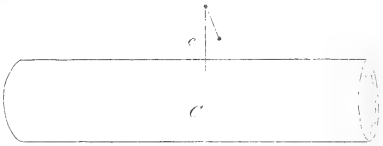

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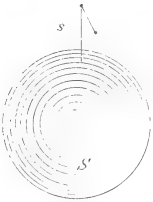

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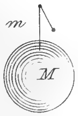

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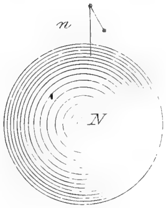

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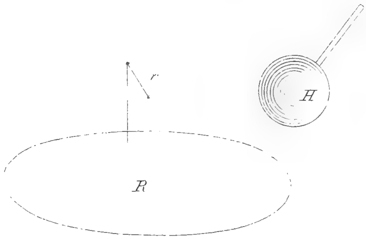

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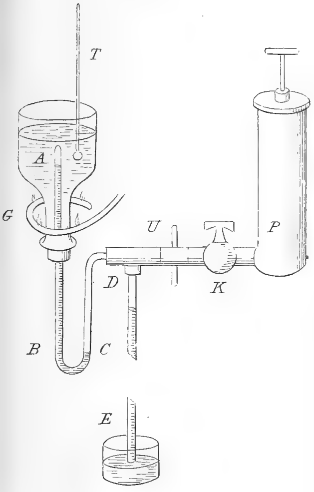

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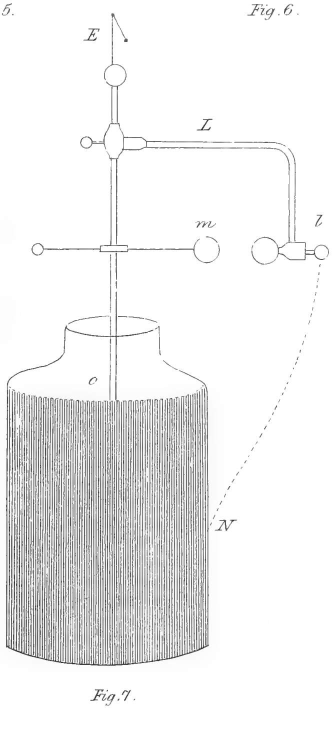

Fig. 7.

J. Basire sc.

VIEWS

IN

ETHNOGRAPHY,

THE CLASSIFICATION OF LANGUAGES,

THE PROGRESS OF CIVILIZATION,

AND

THE NATURAL HISTORY OF MAN.

BY

CHARLES T. BEKE, PH.D., F.S.A., F.R.G.S.,

AUTHOR OF 'ORIGINES BIBLICÆ,' 'THE SOURCES OF THE NILE,' ETC.

LONDON:
TAYLOR AND FRANCIS, RED LION COURT, FLEET STREET.
1863.

THE following paper was published in the 'Edinburgh New Philosophical Journal' for April 1835 (vol. xviii. pp. 285–296). It was to have been read at the Edinburgh Meeting of the British Association for the Advancement of Science in the preceding month of September, 1834, but did not reach the Secretary in time.

Notwithstanding the lapse of years since the enunciation of the opinions thus expressed, I believe they will not be found inapplicable at the present day, when the subjects of Man's origin, antiquity, primeval condition, and early progress excite so much interest.

Were I giving utterance to my present opinions, I should naturally see reason to modify in some respects the expressions contained in this paper. But, as in all material points my early opinions remain unchanged, and as I am not able from want of time to rewrite the article with the requisite alterations, I have thought it better to reprint it as originally produced, merely omitting certain passages and adding a note.

BEKESBOURNE,
August 31st, 1863.

VIEWS IN ETHNOGRAPHY, &c.

In the remarks which will be offered in the present paper, it is not intended to enter upon the question, which has so often and so ably been discussed by physiologists and natural historians, as to whether the numerous and strongly marked diversities in the human race have proceeded from distinct stocks, or whether they are derived from a common origin and are consequently to be regarded as forming merely varieties of one and the same species: it is sufficient to state, that, for the purposes of this disquisition, the latter hypothesis is adopted, as being that which appears to be the more in accordance with reason, and to have on its side the greater weight of authority.

It is also scarcely necessary to do more than allude to the opinion generally entertained by physiologists, historians, jurists, political economists, and others, who have investigated and treated of the subject of the primitive condition of man, and the rise and progress of society, that in the first ages the human race existed in the lowest state of civilization*; namely, that of the mere consumer of the spontaneous productions of nature; and that mankind thence progressively advanced through the several conditions of the hunter, the herdsman, and the agriculturist, or through states nearly corresponding with them:—until, in this last state, an absolute property having been acquired in the land, which was then first subjected to cultivation, the residences of mankind became fixed and permanent; whilst, by the same progressive advancement, societies were formed, which at first were simply patriarchal, but which, from their

---

* ["It seems no longer possible to doubt that the human race has existed on the earth in a barbarian state for a period far exceeding the limit of historical record."—Address of Sir William G. Armstrong, President of the British Association for the Advancement of Science, at the Meeting at Newcastle, August 26th, 1863.]

subsequent increase and union, required the institution of laws for their government and mutual protection; whence ultimately resulted the establishment of the various forms and conditions of civil rule.

This opinion of the gradual progress of civilization, whatever ground it may have gained, is at the best purely hypothetical. So far, indeed, is it from being borne out by facts, that it is actually at variance with the evidence of all history and experience: for, in the early historical remains, whether real or fabulous, of all nations,—with the remarkable exception of those of the progenitors of the Israelites,—we find that instruction and improvement are considered to have been introduced, from an extrinsic source, by individuals possessed of a higher degree of culture; whilst among those savage people with whom civilization may be said not to exist, there is not manifested even the remotest tendency towards progressive improvement from the exercise of that unaided reason, which, as the characteristic of the human race, has been deemed to be entirely sufficient for that purpose.

It is to be considered, then, whether the direct converse of the hypothesis here adverted to ought not rather to be maintained; and whether in fact it will not be more in accordance with the truth to assert, that the savage and uncultivated condition of mankind, which has usually been designated *the state of nature*, is in reality nothing else than a degeneration from a previous social state, in which a high degree of culture and of artificial attainments were possessed; and that consequently this latter condition, and not the former, ought to be regarded as the primitive condition of the present human race.

However paradoxical such an hypothesis may at first sight appear, there is in reality nothing unreasonable in it. If we consider the history of the European settlements in the New World and especially in North America, we find the fact to be, that some of the members of a previous social state, which had existed in a highly civilized condition during several ages, arrived in that continent; where their descendants, and especially those who spread themselves most widely over the newly settled countries, speedily degenerated from the cultivation of the parent stock. Could it so have happened that all further communication with the Old World had ceased, the deterioration which had commenced would unquestionably have proceeded still further: but this process has been checked by the continual arrivals of fresh settlers from the mother country and the constant communications between the two continents, which have in a great measure maintained an equality between their respective inhabitants. But let it be supposed that these European settlers in America had been the only remains of a former race of mankind: it is evident that, whatever in the course of ages might be the character and condition of their descendants,—even if some of them, in the extreme western provinces of America or in other countries into which they might have spread, had become so debased and brutalized as not to be recognized as belonging to the same race;—still, in the consideration of their history, and in the endeavour to trace to their pristine state their laws, their customs, and their religion, however altered, however perverted or corrupted they might become, it would be utterly inconsistent that reference should (in the first instance at least) be made to any other stock than the European colonists from whom they had sprung, or to any other condition of society than that previous *artificial* one of which those Europeans themselves had been members.

May not, then, the history of the whole human race be considered in a similar point of view to that in which the history of the American colonists has thus hypothetically been regarded? If we look to the histories, traditions, and fables of all nations, we find that they all coincide in expressly recording or in alluding to a cataclysm,—the particulars of which are the most fully and circumstantially detailed in the Sacred Writings of the Israelitish nation,—which overwhelmed the whole of mankind, with the exception of a few favoured individuals, who became the founders of the subsequent human race: and if, therefore, we only admit the fact of the occurrence of such an event, we can at once understand how the condition of the first ancestors of the present race of mankind was not a natural but an artificial one, derived from the previous social state of the antediluvian world. Hence we can have no difficulty in conceiving how the social condition of man may have fallen from the culture of that artificial primitive state to the condition of the uncultivated savage, through all those intermediate stages of civilization which, according to the contrary hypothesis, have been regarded as the steps by which man has progressed upwards.

The process of this declension in civilization may be thus briefly stated. When mankind first began to separate and to be scattered abroad upon the face of all the earth, it is manifest that the amount of knowledge in every department of pursuit must have diminished at every step that was taken from the centre, unless each tribe could have ensured to itself (which would have scarcely been possible) the possession of individuals imbued with the *aggregate* of the acquirements of the parent society. Knowledge can in no case remain perfectly stationary: it must either advance or recede: and the latter must universally have been the case in the first instance, and must have continued to be so, until the numbers of mankind had sufficiently increased to allow them again to begin to accumulate—each nation in its own particular sphere of acquirements—the knowledge which had been retained by direct transmission from the common centre, or which had subsequently been derived from the circumstances in which they had respectively been placed.

Subsequently to the [original] dispersion of mankind, the pressure of population would doubtless have been the primary cause of the general distribution of the human race over the earth, and of their consequent descent in the scale of civilization. To this, however, are to be added disputes among neighbouring people, too often ending in warfare; the dislike of some races to the countries in which they had voluntarily settled, or into which they had been compelled to migrate; and the desire, or probably the necessity, of obtaining possessions more suited to their inclinations or their requirements. As the social tie gradually became weaker, the growth of erratic habits, and the consequent rapid declension in civilization which universally attends the settling of new lands, would operate; leading at length to a confirmed nomadic state. In any of these intermediate stages of degradation, however, further deterioration may have been prevented, and an impulse may indeed have been given to a progressive state of improvement, by any causes, whether natural or artificial, which would prevent the further disintegration of society, and bring its members into more intimate connexion, so as to preserve the means for the mutual impartation of knowledge. Thus, in maritime countries, where the further progress and dispersion of mankind has been stopped by the ocean;—in islands;—in cities, where men have been congregated together for the purposes of commerce;—and even in rich alluvial countries, of which, by means of agricultural knowledge, the products have afforded subsistence to a dense population;—civilization, so far from remaining stationary, has generally continued to advance: whilst in champaign, barren, and desert countries, on the contrary, where nomadic habits have been induced, the people have descended in the scale of civilization in an equal ratio to the quality of the country and its means of affording subsistence, operating conjointly with its extent and the consequent absence of the necessity for its inhabitants to adopt any means of support, beyond those which have spontaneously presented themselves and which have thence become congenial to them; such as the pasturing of their flocks in countries sufficiently fertile for that purpose, and the hunting of wild animals, where the physical condition of the country has not been adapted to the support of tame ones.

From this last state,—in which, owing to the loss of the knowledge of accumulating capital, whether in the form of money or of merchandize and ultimately even in that of cattle, a large tract of country would become necessary for the support of a much smaller number of persons; and in which also, from the disintegration of society, the traditive knowledge of each successive generation would become less and less,—the progress to the condition of the mere savage, or man in the lowest state of cultivation, is easy to be traced. In cold and inhospitable countries, however, where the uncivilized races inhabiting them would be compelled to use every exertion in order to procure a scanty and precarious subsistence, the lowest mechanical arts would still be retained, until the inclemencies and privations to which those races were subjected had caused their extinction (a result which there is good reason to believe has in many instances taken place); whilst in more genial climates, where the spontaneous productions of nature were sufficient for the support of mankind, the absence of motives for exertion would lead to the total declension of their debased inhabitants, so that at length they would become almost assimilated with the brute creation.

The hypothesis which is thus advocated removes very many of the difficulties which, under the opposite one, have attended the consideration of primeval history; and it more especially accounts for the existence, in the earliest ages, of nations whose civilization and power, even allowing to them the utmost precocity, were always incompatible with what was conceived to have been at the same periods the state of society generally.

A still more important result is, that we have afforded to us a satisfactory means of explaining the existence of the various diversities in the human species; which diversities, so far from being referrible to any permanent distinctive characters, or even to the action of climate and other physical causes alone, must principally be derived from the operation of changes in the moral and intellectual state of the various races. Indeed, it must never be lost sight of, that man is a reasonable being and not a mere animal; and that consequently it is absolutely necessary, in all investigations of his natural history, to consider him not *physically* only, but also *psychologically*.

Upon the hypothesis, then, that the origin of the numerous and widely differing races of man is to be referred to a single parent stock possessed of a high degree of cultivation, the following principle presents itself: namely, That (allowing for circumstances by which the progress of deterioration may have been accelerated or retarded or otherwise modified) the culture or the degradation of an aboriginal race* will be in proportion to the geographical distance of its residence from the common centre of dispersion. For example, if we take the primitive residence of the post-diluvian race to have been in the north-western portion of Mesopotamia,—the reasons for which location are given at length in the work† of which this paper is designed

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* By the term "aboriginal race" is simply meant the people who were the earliest inhabitants of any country.

† Origines Biblicæ, or Researches in Primeval History, vol. i. London, 1834.

to make known the principal conclusions with respect to philology, ethnography, and the natural history of man,—it will be seen that the countries more immediately surrounding that central point,—namely, Assyria, Chaldea, Egypt, Phœnicia, and Asia Minor,—are those whose inhabitants were in the earliest ages possessed of the highest degree of culture; whilst, on the other hand, at the points most distant from the same centre, the Papuans, the Hottentots, the Esquimaux, and other savage races have degenerated almost to the lowest state compatible with the retention of rational endowments.

A second principle resulting from the same hypothesis is, that (except where invasions have introduced foreign tribes, as in the case of the Hindoos in India) the more degenerate races, whose positions are considerably removed from the centre, must have derived their origin from that centre through the medium of the more civilized people geographically situate between it and them, and must consequently have received from them their languages, their religion, and their customs; although, in consequence of the recession from the centre of these more degenerate races, and their gradual corruption and debasement, the changes in all those particulars, as well as in their physical structure and appearance, may have become such as to render it a task of the utmost difficulty to trace the resemblance and the connexion between them and their more civilized ancestors. Thus the primitive inhabitants of the whole of Southern and Eastern Asia must have sprung from ancestors who originally occupied the countries situate to the northward of the Persian Gulf: so the aborigines of Africa must be descended from the earliest settlers of Arabia, Ethiopia, and Egypt; whilst the tribes who peopled the islands and continent of Europe, and who from thence also spread themselves eastward into the northern portions of Asia, must have had their origin in Asia Minor.

It is also to be inferred, that where different races have, in their corresponding removal from the centre, undergone a corresponding degradation, at the same time that they have been subjected to the operation of similar physical conditions, the results will be analogous in those races, both with respect to their physical conformation and as regards their moral and intellectual character. This is remarkably exemplified in the separate existence of the Hottentots of the south of Africa, and of the Papuans or Asiatic Negroes; which two races—in spite even of the authority of Cuvier himself—cannot, without violating the most obvious principles of science and of history, be referred to the same class, but must be regarded as "deviations from the type of the species by different routes, in *parallel* extreme states of degradation."

I will now attempt briefly to trace the outline of a classification of the various races of mankind, in accordance with the principles which have thus been enunciated. In doing so, I shall avail myself of the aid afforded by the numerous additions which during the last few years have been made to our philological knowledge, arising principally and more especially from the improvements which have taken place in the science of philology itself. It may indeed be asserted that, in the present condition of physiology and of the natural history of man, the affinities of languages, if they be not the sole guides which we possess for enabling us to arrange the varieties of the human species in an order at all approximating to the truth, must at least be regarded as the only one upon which any real dependence is to be placed.

With the assistance then of this guide, we may divide the races of mankind into the following principal classes. The first is that which is composed of the nations to whom belong the various languages of cognate origin, distinguished by the common designation of Indo-Germanic or Indo-European. These consist of the Sanscrit, the Zend or ancient Persic, the Phrygian, the Lydian, the Greek, the Latin and its derivations, the languages of the great Germanic family, the Celtic, and the Slavonian.

Of these, the various languages of Europe and Asia Minor may be regarded as aboriginal; that is to say, as having been spoken by the people who were the first inhabitants of those portions of the globe. On the other hand, the Sanscrit is admitted to be the language, not of the aborigines, but of a race of conquerors, who entered the Indian peninsula from the northwest and extirpated or drove southward before them the native races. In like manner must it be considered that the Zend (the intimate connexion of which language with the Sanscrit is well established) was not the primitive language of Persia, but was introduced into that country also by the same exotic race, whose original seat must be looked for in the mountainous country to the *west* of the Caspian.

To this class of languages, and to the people among whom the various dialects of them are spoken,—which people are in the present day spread not only over Europe and a considerable portion of Asia, but, by means of European settlements and conquests, over the vast continent of America also, and who have likewise taken root in what may be regarded as a fifth quarter of the globe, namely, Australia,—the designation of Japetic or Japhthitish may with the strictest propriety be applied.

The next grand division of mankind is composed (in part) of the nations to whom belong the so-called Semitic or Aramean languages; namely, the Hebrew, the Arabic, the Chaldee, and the Syriac.

The reason of this nomenclature is, that the Hebrew and the Arabic are the languages spoken by the people who are regarded as the descendants of Abraham; whilst the Chaldee and Syriac are considered to have been vernacular in Mesopotamia and Syria among the descendants of Aram; both those patriarchs being of the posterity of Shem, the eldest son of Noah. Philologists have already discovered, however, that affinities exist between these so-called Semitic tongues and other languages, such as the Phœnician, the Coptic, the Geez, and the Amharic of Abyssinia, and the Berber of Northern Africa, to which the same designation cannot with any correctness of nomenclature be applied, and which are in reality entitled to the appellation of *Hamitish* alone.

Since then the languages spoken by the descendants of Isaac and of Ishmael, the sons of Abraham,—namely, the Hebrew and the Arabic,—are thus found to be cognate with those which are so widely spread among the descendants of Ham, it would seem most reasonable to imagine that the former languages, instead of being the representatives of the Shemitish tongue which was spoken by Abraham either in Chaldea or Aram, are the Hamitish languages which were vernacular in the countries in which that patriarch and his descendants took up their residence, and were, in fact, acquired by them during their residence therein, to the exclusion of their paternal tongue. This hypothesis has been advocated at length in 'Origines Biblicæ.' In the same work it has also been attempted to be shown that the so-called Chaldee is merely a corruption of the Hebrew spoken by the Jews during their captivity in Babylon, and not the native language which at that epoch was vernacular in Babylon itself. In like manner is the Syriac to be regarded as only a further degradation and corruption of the Hebrew. Under no circumstances, indeed, can it claim to be the primitive native tongue of the countries in which it was spoken about the period of the commencement of the Christian era: for, subjected as they had been to repeated and continued foreign invasion and occupation, it is impossible that any native language should during more than twenty centuries have continued to exist without very considerable alterations, if indeed it must not have been altogether extirpated.

The appellation of Semitic or Shemitish, as applied to these languages, must therefore be superseded by that of Hamitish; under which designation will have to be comprised not only the Canaanitish, Arabian, and African languages which have been enumerated, but also the whole of the native dialects spoken throughout the continent of Africa; all the inhabitants of which continent must, agreeably to the hypothesis advocated in this paper, have derived their origin from the centre, through the medium of the more civilized countries of Arabia and Egypt.

The remaining class of mankind which will here be mentioned is that of which the Chinese, and the various Indo-Chinese nations, may in the present day be regarded as the principal representatives. In tracing back these people to their common origin with the rest of mankind, in accordance with the foregoing hypothesis, it is manifest that their progenitors must in the earliest ages have occupied the more western portions of Asia, and that they were, in fact, of like origin with the aborigines of the Peninsula of India, of whom traces are yet left in the Bheels and other savage races scattered over various portions of that peninsula, and in the people found in greater numbers towards its southern extremity; whose languages, of a totally distinct character from the Sanscrit and its derivative dialects, plainly point to the separate origin of those people from that of the Japhthitish Hindoos.

It is to this division of mankind that I conceive the designation of Semitic or Shemitish ought properly to be applied; and within this division must also be comprised the whole of the aboriginal inhabitants of the Indian Archipelago, of Polynesia, and likewise of America; but upon the subject of these people I will refrain from enlarging, as the grounds upon which the classification of the varieties of the human species has been attempted in this paper are intended to be essentially philological, and that department of knowledge does not (as far as I am acquainted with it) afford sufficient data upon which the proposed classification should be thus far extended.

When, however, our philological knowledge shall become yet further enlarged, it will be seen whether or not all the languages of the earth, and the people speaking them, are referrible to the three distinct divisions which have thus been enumerated; and it will also be definitively ascertained whether these divisions of languages are, like the distinct races of mankind to whom they belong, reducible to one common source. With respect to the former of these questions, there is good reason to believe that, sooner or later, it will be determined in the affirmative; but with regard to the latter of them, the opinion of philologists is already very decidedly in the negative. If this be established, we shall be compelled to have recourse to an original formation of more than one primitive tongue; an hypothesis which in reality is attended with no greater difficulty than that of the original formation of a single language.

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