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

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

J. B. Pinelli ad Mazonium.

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PLATES.

ERRATA.

Page 17, fifteen lines from bottom, *after* acid *insert* a.
— 19, four lines from top, *for* $N^2 O^5$ *read* NO^3.
— 239, line 24, *for* M. Fresca *read* M. Tresca.

THE
LONDON, EDINBURGH, AND DUBLIN
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[FOURTH SERIES.]

JULY 1865.

I. *On the Law of Density of Saturated Steam expressed by a New Formula.* By Thomas Rowe Edmonds, *B.A. Cantab.*[*]

THE expansive force of a unit weight of saturated steam, according to temperature, is usually measured by the product of the pressure P in pounds to the square foot, multiplied by the volume V in cubic feet occupied by one pound weight of such steam. The product PV represents the work done or resistance overcome in foot-pounds, and is equal to the effect produced by a constant force P pounds driving a piston, whose area is one square foot, through the linear space represented by the number which expresses V in cubic feet. When the laws of pressure and volume are both known, the law of expansive force represented by the product PV will be known also. But such a compound will not be entitled to the appellation of a third law, if the two formulæ for P and V, when united, do not yield for result a third formula equal in simplicity to either of the two constituent formulæ.

The pressure P of saturated steam appropriate to every temperature, from $-30°$ to $+230°$ Centigrade, has been satisfactorily and accurately determined through the observations of M. Regnault, published in the year 1847. The law of pressure deduced from these observations has been investigated in the Philosophical Magazine for March 1865, and the formula indicating the law has been found to be

$$\text{hyp.}\log P=\frac{\alpha x}{n}\left\{1-\left(1+\frac{t}{a}\right)^{-n}\right\}.$$

This formula was directly obtained from the Table of pressures adopted by M. Regnault, through observing that the differential

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[*] Communicated by the Author.

coefficient of log P, at any temperature t, was always represented by

$$\frac{d.\log \mathrm{P}}{dt}=\alpha\left(1+\frac{t}{a}\right)^{-\frac{1}{k}},$$

the exponent $\frac{1}{k}(=n+1)$ being equal to the number 2·302585, which is the hyperbolic logarithm of 10.

The first and only satisfactory observations made for determining by experiment the density of saturated steam at various temperatures, are those of Messrs. William Fairbairn and Thomas Tate. They are published in the Philosophical Transactions of the year 1860. The difficulties are great in the way of making correct observations on the density of saturated steam in free communication with water. Such steam has not yet been obtained in a pure state, there being always an admixture of water with such steam. Part of the water is suspended in the form of cloud or mist, and part is pressed as a film of fluid against the sides of the containing vessel. Messrs. Fairbairn and Tate appear to have overcome the chief impediments to correct observation by the use of their "saturation-gauge." On examining the results obtained, it will be found that the law of progression according to temperature for the density as given by these observations, is not much less regular than the law of progression for pressure as given by the observations of M. Regnault. It will be found that the function of the variable t involved in the law of density is identical with the function of the variable t involved in the law of pressure. The differential coefficient of log P has already been found to be $+\alpha\left(1+\frac{t}{a}\right)^{-2·302585}$. It will now be found that the differential coefficient of log V is $-\alpha_1\left(1+\frac{t}{a}\right)^{-2·302585}$. The constant α for pressure at temperature 100° C. was found to be +·03580. The new constant α_1 for volume at the same temperature will be found to be −·03365.

In the investigation of the law of pressure above referred to, it has been shown (Philosophical Magazine, vol. xxix. p. 179) that for any given interval of temperature, the logarithm of the pressure P of saturated steam is a simple function of the logarithm of the expansive force p of a unit weight of a perfectly elastic vapour maintained at a constant volume. The quantity p being $=1+\frac{t}{a}=e^{t_1}$, it was there shown that

$$\frac{d.\log \mathrm{P}}{d.\log p}=\alpha a p^{-n}=\alpha a e^{-nt_1}=\alpha a e^{-n\log p}.$$

By integration was obtained for value of hyp. log P,

$$\log P = \frac{\alpha a}{n} \left\{ 1 - e^{-n \log p} \right\}.$$

If we put

$$\phi \log p = \frac{a}{n} \left\{ 1 - e^{-n \log p} \right\},$$

we get

$$\log P = \alpha \phi \log p, \text{ and } P = e^{\alpha \phi \log p}.$$

The formula for the pressure of saturated steam being $P = e^{\alpha \phi \log p}$, the formula for volume is $V = e^{-\alpha_1 \phi \log p}$; and the formula for the expansive force of a unit weight of such steam is

$$PV = e^{(\alpha - \alpha_{,}) \phi \log p} = e^{\alpha_{,,} \phi \log p}.$$

The value of α in the formula for pressure has been shown to be ·03580, which represents the rate of increase per degree (Centigrade) of the pressure at the absolute temperature 376°, or at 100° on the Centigrade scale. In order to represent the law of volume, we have to put (for the same temperature) $\alpha_{,} = -·03365$; and to represent the law of expansive force we have to put $\alpha_{,,} = +·00215$. We thus obtain the following expressions for pressure, volume, and expansive force, reckoned from 100° Centigrade :—

$$P = e^{\alpha \phi \log p}, \text{ and } \log P = +·03580\, \phi \log p,$$

$$V = e^{-\alpha_{,} \phi \log p}, \text{ and } \log V = -·03365\, \phi \log p,$$

$$PV = e^{\alpha_{,,} \phi \log p}, \text{ and } \log PV = +·00215\, \phi \log p.$$

By the aid of the three foregoing equations any one of the three quantities P, V, or PV may be expressed in terms of either of the other two quantities. In order to express V in terms of P, we obtain, by dividing log V by log P,

$$\frac{\log V}{\log P} = -\frac{\alpha_1 \phi \log p}{\alpha \phi \log p} = -\frac{\alpha_1}{\alpha} = -\frac{·03365}{·03580}$$

$$= -\frac{1}{1·06389} = -·939944;$$

consequently

$$V = P^{-·939944},$$

and

$$PV = P^{+·060056}$$

Similarly may be obtained

$$P = V^{-1·06389},$$

and

$$PV = V^{-·06389}$$

According to the notation herein used, the quantities P, V, and PV represent ratios of pressure, volume, and expansive force at absolute temperature $(a+t)$ to similar quantities at absolute temperature (a) degrees. That is to say, P, V, and PV are used to represent quantities usually represented by $\frac{P}{P_0}$, $\frac{V}{V_0}$, and $\frac{PV}{P_0V_0}$, the quantities P_0, V_0, and P_0V_0 being constants determined, or assumed to be determined, by observation for the absolute temperature (a) degrees. The theoretical numbers contained in the four annexed Tables have been obtained by adopting the absolute temperature 376° (corresponding to 100° C.) as the fixed point from which the variable (t) is measured. At this point $P_0=2116{\cdot}4$ pounds to the square foot, as determined by observation. At the same temperature, $V_0=26{\cdot}36$ cubic feet occupied by one pound weight of water when converted into steam. And at the same temperature, $P_0V_0=2116{\cdot}4\times26{\cdot}36=55788$ foot-pounds expansive force exerted by one pound weight of saturated steam. These three constants, P_0, V_0, and $(PV)_0$, may, however, be more conveniently denoted by the letters H, $H_{\prime}$, and $H_{\prime\prime}$.

In Table I. are exhibited the results of twenty out of the twenty-three experiments on the density of saturated steam made by Messrs. Fairbairn and Tate. Three of the experiments made at high pressure and numbered 9′, 10′, and 14′ respectively, have been excluded, because the particulars given of these experiments show them to have been defective. Nine of the selected observations were made for temperatures below 100° C.; the remaining eleven observations were made for temperatures above 100° C. In the last column of this Table are given, for purposes of comparison, the densities of saturated steam at corresponding temperatures according to Professor Rankine's theoretical Tables as stated by himself in a paper communicated to the Royal Society of Edinburgh in the year 1860. In that paper Professor Rankine expresses his opinion that the results of the experiments of Messrs. Fairbairn and Tate are in near accordance with his own theoretical Tables at all temperatures. He adds, however, that at temperatures above 100° C., the aggregate of experiments indicate an excess of density above the theoretical density. This excess he states to amount on an average to ·27 parts of a cubic foot, being the difference between the experimental and theoretical volumes occupied by one pound weight of saturated steam. It will be found on reference to the column of differences in the Table annexed, that the above discrepancy has been reduced to one-fifth part of a cubic foot. This has been effected by excluding the three defective experiments mentioned, and by correcting an erroneous number given by Professor Rankine.

In the year 1850, M. Clausius published a formula by means of which the density of saturated steam may now be calculated. This formula is given in the Philosophical Magazine for August 1851, at page 107. It contains two quantities which are superfluous, as they are not found in the new formula above given. Nevertheless the results yielded by M. Clausius's formula for volume, are nearly identical with the results of the new formula for volume. M. Clausius deduced his formula from a theory of the moving force of heat originating with M. Carnot in 1824, and improved by M. Clapeyron in 1834.

The formula of M. Clausius is an expression for the latent heat of saturated steam relative to water (L), in terms of Joule's mechanical equivalent for a unit of heat (J), absolute temperature $(a+t)$, excess of volume of a unit weight of saturated steam above volume of same weight of water $(V-v)$, and the differential coefficient $\frac{dP}{dt}$. If v be omitted as insignificant and within the errors of observation, the formula of M. Clausius may be written thus,

$$L=\frac{1}{J}(a+t)V\frac{dP}{dt}.$$

The value of $\frac{dP}{dt}$ obtained from the new formula for pressure has been shown to be $P\alpha\left(1+\frac{t}{a}\right)^{-(n+1)}$. If this value be substituted in the formula of Clausius, we shall have

$$L=\frac{1}{J}(a+t)V\times P\alpha\left(1+\frac{t}{a}\right)^{-(n+1)}=\frac{1}{J}\alpha a PV\left(1+\frac{t}{a}\right)^{-n};$$

i. e.,

$$L=\frac{1}{J}\alpha a PVp^{-n}.$$

In the preceding equation the variables L and PV represent ratios to constants L_0 and P_0V_0. The actual latent heat is represented by $L\times L_0$, and the actual expansive force by $PV\times P_0V_0$. On substitution of these values in this equation, we get

$$L\times L_0=\frac{1}{J}\cdot\alpha a\,PV\times P_0V_0p^{-n};$$

or separating variables from constants, we have

$$L=\frac{1}{J}\cdot\frac{\alpha a\,P_0V_0}{L_0}PVp^{-n}.$$

If $t=0$, we have

$$\mathrm{PV}=1, \quad p^{-n}=1, \text{ and } \mathrm{L}=1;$$

consequently

$$1=\frac{1}{\mathrm{J}}\cdot\frac{\alpha a \mathrm{P}_0 \mathrm{V}_0}{\mathrm{L}_0} \text{ or } \mathrm{J}=\frac{\alpha a \mathrm{P}_0 \mathrm{V}_0}{\mathrm{L}_0};$$

that is,

$$\mathrm{J}=\frac{\cdot 0358 \times 376 \times 2116{\cdot}4 \times 26{\cdot}36}{536{\cdot}5}=1399{\cdot}7.$$

The quantity J, whose value has thus been determined to be 1399·7, is the mechanical equivalent in foot-pounds of a Centigrade unit of heat, the corresponding equivalent for a Fahrenheit unit of heat being 777 foot-pounds. The value of such mechanical equivalent as determined by Dr. Joule is 1389·6 for a Centigrade unit, or 772 for a Fahrenheit unit of heat. If the constant V_0 be assumed to represent 26·17 cubic feet instead of 26·36 cubic feet, the resulting value of J will be identical with the value adopted by Dr. Joule himself.

It having been found that

$$\mathrm{L}=\frac{1}{\mathrm{J}}\,\frac{\alpha a \mathrm{P}_0 \mathrm{V}_0}{\mathrm{L}_0}\,\mathrm{PV}p^{-n} \text{ and } \mathrm{J}=\frac{\alpha a \mathrm{P}_0 \mathrm{V}_0}{\mathrm{L}_0},$$

it follows that

$$\mathrm{L}=\mathrm{PV}p^{-n},$$

an equation which represents the corresponding values of L and PV when the latent heat, as well as the expansive force at the absolute temperature a, whence t is measured, are taken equal to unity.

By means of the last equation and the formula for PV in terms of $\log p$, the latent heat may be expressed in terms of the differential coefficient of the expansive force PV. The formula mentioned gives on differentiation,

$$\frac{d\,.\log \mathrm{PV}}{d\,.\log p}=\alpha_{\prime\prime} a p^{-n}, \text{ or } \mathrm{PV}\frac{d\,.\log \mathrm{PV}}{d\,.\log p}=\alpha_{\prime\prime} a \mathrm{PV} p^{-n};$$

consequently, since $\frac{d\,.\mathrm{PV}}{\mathrm{PV}}=d\,.\log \mathrm{PV}$ and $\mathrm{L}=\mathrm{PV}p^{-n}$,

$$\frac{d\,.\mathrm{PV}}{d\,.\log p}=\alpha_{\prime\prime} a \mathrm{PV} p^{-n}=\alpha_{\prime\prime} a \mathrm{L}=\cdot 80840\,\mathrm{L}.$$

That is, the latent heat of saturated steam (relative to water at the same temperature) is equal to the differential coefficient of the expansive force PV, divided by $\alpha_{\prime\prime} a(=\cdot 80840)$.

By means of the differential coefficient of log PV above given, we may find the temperature at which $d\,.\log PV$ becomes equal to $d\,.\log p$; that is, the temperature at which the rate of increment of the expansive force of a unit weight of saturated steam is equal to the rate of increment of expansive force of a unit weight of a perfectly elastic gas maintained at a constant volume. Since

$$\frac{d\,.\log PV}{d\,.\log p}=\alpha_{\prime\prime}ap^{-n}=\cdot 80840\left(1+\frac{t}{376}\right)^{-1\cdot 302585},$$

we shall have $d\,.\log PV=d\,.\log p$, when

$$\left(1+\frac{t}{376}\right)^{1\cdot 302585}=\cdot 80840.$$

The value of t which satisfies the above equation is negative, and equals $-56°$. Consequently the absolute temperature at which $d\,.\log PV=d\,.\log p$ is $(376-56)$, or 320°, which corresponds to $(320-276)$, or 44° of the Centigrade thermometer. The ratio $\frac{d\,.\log PV}{d\,.\log p}$ is less than unity for all temperatures above 44° C., and greater than unity for all temperatures below that point.

In Tables II. and III., the values at different temperatures of V and PV given by the new formulæ are compared with similar values obtained from the published Tables of Professor Rankine, and with similar values obtained from the formula for volume by M. Clausius, in combination with the new formula for pressure of saturated steam. On comparing together the three series of results, it will be perceived that for all temperatures above 60° C. the three series of results may be said to be identical one with another. At temperatures below 60° C., the two series of M. Clausius and Professor Rankine continue to agree with one another, but they disagree slightly with the results of the new formula. This disagreement arises from the circumstance that the first two series of results are regulated by the law of latent heat adopted by M. Regnault, which law is in complete accordance with the formula $L=PVp^{-n}$ for all temperatures above 60° C., whilst it deviates slightly from such formulæ for all lower temperatures.

In Table IV. will be seen, for various intervals of temperature from 0° C. to 220° C., the relation between the numbers for latent heat given by M. Regnault, and the true numbers for latent heat which flow from the adoption of M. Clausius's formula for volume in combination with the new formula for pressure of saturated steam. It will be seen that for all temperatures above 60° C. there is no appreciable difference between experimental and theoretical numbers. At temperatures below 60° C. there is a slight discrepancy between the alleged experimental numbers and the theoretical numbers. On reference, however, to the account given by M. Regnault of the various unsatisfactory experiments which he has made for temperatures below 60° C., there will be found no ground for believing that the new law for latent heat (expressed by the equation $L=PVp^{-n}$), which is indisputably true for all temperatures above 60° C., is not also true for all lower temperatures. (See *Mémoires de l'Académie,* vol. xxi. pp. 712 and 722.)

The latent heat of saturated steam adopted by M. Regnault is deduced from a well-known empirical law propounded by him, as affording a near representation of the total heat absorbed by a unit weight of saturated steam during its conversion from water, and its elevation in temperature from 0° C. to t° Centigrade. This "total heat" is called λ, and the formula is

$$\lambda=606{\cdot}5+{\cdot}305t,$$

this quantity representing units of heat for temperature measured by the Centigrade thermometer, and a unit of heat representing the quantity of heat absorbed by a unit weight of water on elevation of its temperature from 0° C. to 1° Centigrade. The latent heat is obtained by subtracting from the "total heat" (λ) the number of units of heat absorbed by a unit weight of water whilst its temperature is being raised from 0° C. to t° Centigrade. For example, if $t=200$° C., then we have

$$\lambda=606{\cdot}5+{\cdot}305\times200=667{\cdot}5.$$

From this number is to be deducted the heat absorbed by a unit weight of water whilst its temperature is being elevated from 0° to 200° C. This is 200 units of heat, together with 3·2 units addition made for increased specific heat or capacity of water at 200° C. We have consequently the resulting latent heat of saturated steam at 200° C. $=667{\cdot}5-203{\cdot}2=464{\cdot}3$ units of heat, which is the number adopted by M. Regnault, as stated in the last page of vol. xxi. of the *Mémoires de l'Académie,* &c.

The lowest average temperature at which observations on latent heat were made by M. Regnault was +10° C. At this point the latent heat observed is stated to have been 599·5, whilst the new formula would give 587·4 Centigrade units of heat. The difference is 12·1, which is the maximum difference between theory and a selected series of experiments. M. Regnault remarks that the difficulties are great in obtaining results to be relied upon from observations on latent heat of saturated steam made at temperatures below 60° C., or at pressure less than one-fifth part of an atmosphere. He says that he has tried various methods of observation at these low temperatures; he assigns no reason for the preference given to the method of observation finally adopted; and he expresses no confidence in the results which he adopts. In speaking of the "total heat" adopted for the lowest temperature observed, +10° C., M. Regnault says that he "believes it will be found not very far from the truth."

M. Regnault's Table of latent heat is mainly dependent on his empirical law for "total heat" expressed by the formula $A+Bt$. This law is of extreme simplicity, and for that reason the chances are very much against its being true for the entire range of temperature from 220° C. to 0° C., to which M. Regnault tries to make it apply. That it should be found to be true from 220° C. to 60° C. is something extraordinary, and ought to satisfy all reasonable expectation. There is, however, a good reason for anticipating failure in this empirical law when extended to low temperatures. The point of temperature where the suspected failure occurs is not very distant from a remarkable point of temperature (44° C.) indicated (as already stated) by the new formula for PV, where $d \,.\, \log PV = d \,.\, \log p$. It is highly improbable that an empirical law so simple as $A+Bt$ should indicate correctly the "total heat" for ranges of temperature *below* this remarkable point (44° C.) as well as for ranges of temperature *above* this point.

Table I.—Volumes of a unit weight of saturated Steam at different Temperatures, as deduced from the experiments of Messrs. Fairbairn and Tate, compared with the volumes given by the new formula and by Dr. Rankine's theory respectively.

Temperature.		Pressure of saturated steam according to Regnault.	Specific volume relative to water according to Fairbairn and Tate.	Volume of one pound weight according to			
Fahrenheit.	Centigrade.			Fairbairn and Tate.	Differences.	New formula.	Rankine.
		atmospheres		cub. feet.	cub. feet.	cub. feet.	cub. feet.
136°·8	58°·2	·180	8275·3	132·56	− ·49	132·07	132·20
155·3	68·5	·288	5333·5	85·44	− ·39	85·05	85·10
159·4	70·8	·317	4920·2	78·82	−1·19	77·63	77·64
170·9	77·2	·415	3722·6	59·63	+ ·56	60·19	60·16
171·5	77·5	·421	3715·1	59·51	− ·08	59·43	59·43
174·9	79·4	·455	3438·1	55·08	+ ·12	55·20	55·18
182·3	83·5	·537	3051·0	48·87	−1·57	47·30	47·28
188·3	86·8	·611	2623·4	42·02	− ·17	41·85	41·81
198·8	92·7	·764	2149·5	34·43	− ·50	33·93	33·94
242·9	117·2	1·793	943·1	15·11	+ ·12	15·23	15·25 *
244·8	118·2	1·855	908·0	14·55	+ ·20	14·75	14·77
245·2	118·5	1·869	892·5	14·30	+ ·34	14·64	14·67
255·5	124·2	2·238	759·4	12·17	+ ·19	12·36	12·39
263·1	128·4	2·548	649·2	10·40	+ ·54	10·94	10·96
267·2	130·7	2·728	635·3	10·18	+ ·08	10·26	10·29
269·2	131·8	2·819	605·7	9·70	+ ·25	9·95	9·98
274·8	134·9	3·087	584·4	9·36	− ·22	9·14	9·16
282·6	139·2	3·500	497·2	7·96	+ ·16	8·12	8·14
287·2	141·8	3·767	458·3	7·34	+ ·24	7·58	7·60
292·5	144·7	4·086	433·1	6·94	+ ·08	7·02	7·04

* Instead of 15·61 stated in error by Dr. Rankine.

Table II.—Volumes occupied by a unit weight of saturated Steam at different Temperatures, according to the new formula, the formula of M. Clausius, and the formula of Dr. Rankine respectively.

Temperature.			V.	$V \times H_1$.		
			Relative volume according to new formula.	Volume of one pound weight of saturated steam according to		
Fahrenheit.	Centigrade.	Absolute.		New formula.	Clausius.	Rankine.
				cub. feet.	cub. feet.	cub. feet.
32°	0°	276°	12360	3258	3350	3390
50	10	286	6400	1687	1722	1732
68	20	296	3486	919·0	932·7	934·6
86	30	306	1988	524·0	529·5	529·2
104	40	316	1180	311·2	313·4	312·8
122	50	326	727·2	191·7	192·5	192·0
140	60	336	463·0	122·1	122·4	122·0
158	70	346	303·8	80·08	80·18	80·02
176	80	356	204·8	53·98	54·01	53·92
194	90	366	141·5	37·30	37·30	37·26
212	100	376	100·0	26·36	26·36	26·36
230	110	386	72·15	19·02	19·02	19·03
248	120	396	53·04	13·98	13·98	14·00
266	130	406	39·68	10·46	10·46	10·48
284	140	416	30·17	7·95	7·96	7·97
302	150	426	23·27	6·14	6·14	6·15
320	160	436	18·20	4·80	4·80	4·82
338	170	446	14·42	3·80	3·80	3·81
356	180	456	11·55	3·05	3·05	3·06
374	190	466	9·360	2·47	2·47	2·48
392	200	476	7·661	2·02	2·02	2·02
410	210	486	6·330	1·67	1·66	1·67
428	220	496	5·276	1·39	1·38	1·39
446	230	506	4·434	1·17	1·16	(not stated)

Table IV.—The Latent Heat of saturated Steam according to M. Regnault, compared with the Latent Heat deduced from the formula of M. Clausius, which is $L = \frac{1}{J}(a+t)V\frac{dP}{dt} = PVp^{-n} \times$ by a constant.

Temperature.		$p^{-n} = \left(1 \pm \frac{t}{a}\right)^{-n}$.	PV.	L.	$L \times L_0$.	Latent heat (Regnault).	Differences.
Centigrade.	o-lute.			PVp^{-n}.	$PVp^{-n} \times 536 \cdot 5$.		
					units of heat.	units of heat.	units of heat.
0°	276°	1·4959	·7351	1·0996	589·9	606·5	− 16·6
20	296	1·3656	·7970	1·0883	583·9	592·6	8·7
40	316	1·2541	·8541	1·0711	574·7	578·7	4·0
60	336	1·1578	·9067	1·0498	563·2	564·7	1·5
80	356	1·0738	·9552	1·0257	550·3	550·6	·3
100	376	1·0000	1·0000	1·0000	536·5	536·5	·0
120	396	·9347	1·0414	·9734	522·2	522·3	·1
140	416	·8766	1·0796	·9464	507·7	508·0	·3
160	436	·8246	1·1150	·9195	493·3	493·6	·3
180	456	·7778	1·1479	·8928	479·0	479·0	·0
200	476	·7355	1·1784	·8667	465·0	464·3	+ ·7
220	496	·6971	1·2068	·8413	451·4	419·4	2·0

Table III.—The Expansive Force of a unit weight of saturated Steam at different Temperatures, according to three different formulæ, compared with the Expansive Force of a unit weight of a perfectly elastic gas maintained at a constant volume.

Temperature.		$p=1\pm\frac{t}{a}$.	PV.		$PV\times H_{\prime\prime}$.	
Centigrade.	Absolute.	Relative expansive force of a unit weight of a perfectly elastic vapour occupying a constant volume.	Relative expansive force of a unit weight of saturated steam.		Absolute expansive force of one pound weight of saturated steam.	
			New formula.	Clausius.	New formula.	Rankine.
					ft.-pounds.	-pounds.
0°	276°	·7340	·7351	·7557	41008	41580
10	286	·7606	·7666	·7824	42769	43170
20	296	·7872	·7970	·8088	44461	44730
30	306	·8138	·8261	·8348	46085	46250
40	316	·8404	·8541	·8601	47646	47720
50	326	·8670	·8809	·8847	49145	49160
60	336	·8936	·9067	·9091	50583	50560
70	346	·9202	·9315	·9326	51964	51940
80	356	·9468	·9552	·9558	53291	53250
90	366	·9734	·9781	·9781	54564	54520
100	376	1·0000	1·0000	1·0000	55788	55788
110	386	1·0266	1·0211	1·0211	56964	56960
120	396	1·0532	1·0414	1·0415	58095	58130
130	406	1·0798	1·0608	1 0611	59182	59240
140	416	1·1064	1·0796	1·0801	60228	60300
150	426	1·1330	1·0976	1·0981	61235	61320
160	436	1·1596	1·1150	1·1157	62204	62330
170	446	1·1862	1·1317	1·1319	63138	63260
180	456	1·2127	1·1479	1·1479	64037	64180
190	466	1·2393	1·1634	1·1625	64904	65040
200	476	1·2660	1·1784	1·1766	65740	65860
210	486	1·2926	1·1928	1·1894	66546	66670
220	496	1·3191	1·2068	1·2016	67326	67470
230	506	1·3457	1·2203	1·2126	68077	(not stated)

II. *On Chemical Nomenclature.*

By Prof. W. A. Miller, *M.D., LL.D., &c.*[*]

It has been long felt by most chemists that some revision of the nomenclature of their science has become necessary, owing to the alteration which has taken place in the fundamental ideas now prevalent concerning the constitution of chemical compounds, and which are no longer in harmony with much of the system of nomenclature introduced by Lavoisier and his colleagues eighty or ninety years ago. The present seems by

[*] Communicated by the Author.

general consent to be a convenient time for carrying out such an attempt.

Professor Williamson, in the paper which he published in the June Number of this Magazine, has particularly urged the importance of improving the existing nomenclature by the systematic adoption and extension of a method largely used by Berzelius—a proposal which appears to have found very general acceptance.

As, however, the carrying out of this plan will involve a somewhat extensive alteration of the names at present employed by chemists to designate inorganic compounds, it appears to be desirable to consider whether, before such changes be generally adopted, other alterations may not be beneficially introduced at the same time. It is with a view of aiding in a discussion, the results of which must be fraught with important practical consequences, that the suggestions contained in the present paper are offered*.

The fundamental alteration proposed by Professor Williamson admits of a simple illustration. When a metal, for example, has the power of forming two basic oxides, and consequently of furnishing two distinct classes of salts, these oxides are distinguished from each other by the terminations *ous* and *ic*, the oxide with the smallest proportion of oxygen being indicated by the termination *ous*.

Iron, for example, forms two basic oxides, FeO and Fe^2O^3; the first is termed *ferrous* oxide, the second *ferric* oxide; whilst the salts formed by the action of acids upon the first constitute the ferrous salts, such as $FeSO^4$, ferrous sulphate; those from the second oxide the ferric salts, such as $Fe^2 3SO^4$, ferric sulphate.

To this plan, which gives simple and brief names, probably no exception could reasonably be taken, if all the oxides designated by these terminations had an analogous atomic composition; but it is universally admitted that this is not the case, as will be immediately seen by comparing a few selected examples.

Argentous oxide .	Ag^4O	Aurous oxide . .	Au^2O
Argentic oxide .	Ag^2O	Auric oxide . .	Au^2O^3
Cupreous oxide .	Cu^2O	Stannous oxide .	SnO
Cupric oxide . .	CuO	Stannic oxide .	SnO^2

Now it appears that in the introduction of any change in our nomenclature it would be proper to aim at a greater degree of precision than the plan in its present form permits. By a

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* In the following remarks the now usual atomic weights are adopted; viz. $O=16$, $C=12$, $S=32$; and the old atomic weights of all the metals referred to are doubled, with the exception of potassium, sodium, thallium, silver, gold, arsenic, antimony, and bismuth.

modification quite in harmony with the principles recognized in the present mode of constructing names for chemical compounds, the requisite precision may readily be given. For instance, the oxides of the four metals above mentioned represent four different types of atomic composition, which it is desirable to indicate in the name, and which may be done by giving to them the following designations:—

Ag^4O	. Tetrargentous oxide.	Au^2O	. Diaurous oxide.
Ag^2O	. Diargentic oxide.	Au^2O^3	. Auric sesquioxide.
Cu^2O	. Dicupreous oxide.	SnO	. Stannous oxide.
CuO	. Cupric oxide.	SnO^2	. Stannic dioxide.

No doubt some of these names are longer and more clumsy than those for which it is proposed to substitute them; but the ear soon becomes reconciled to words which are found to be useful.

The Greek numerals are those which in most cases make the best words, and for some time past the tendency has been towards a systematic and exclusive use of these numerals as prefixes, which are thus gradually displacing their Latin equivalents. It is true that the use of Greek prefixes to Latin derivatives is in itself objectionable; but unfortunately the nomenclature of science so abounds already in barbarisms of this nature, that it would be mere affectation in framing our names to discard an old, convenient, and recognized term such as stannic or auric, because it is of Latin origin, whilst the characteristic numeral prefix with which it is to be associated is of Greek derivation.

In order to enable the reader to judge fairly of the general effect of the modifications proposed by Professor Williamson with the extensions now suggested by myself, it will be useful to enumerate the more important oxides and chlorides, giving the names at present current, together with those which it is proposed to substitute for them. The application of a few of these altered terms to some of the salts of the common acids, such as the sulphates and nitrates, will then sufficiently illustrate the class of names which would be thus introduced, and in this way it will be seen with how much precision compounds of this kind may be indicated.

Let it not be for a moment imagined that any abolition of trivial names in common use is contemplated in the changes suggested. Names such as calomel, corrosive sublimate, caustic potash, baryta, and the like, have become an integral part of the language of common life; they are recognized by the mass of educated men, and are, moreover, in a scientific sense, valuable, as giving precision in many cases to the shifting names which, from the rapidly-varying views of chemical constitution, have often been given to the same substance. Tetrapotassic ferrocyanide and tripotassic ferricyanide, though scientifically expressing the views now entertained regarding the salts they represent, will never supersede the commercial names of yellow and of red prussiate of potash; and muriatic acid will probably long maintain its ground against hydrochloric or chlorhydric acid, or its newer equivalent hydric chloride.

The point that I wish here to insist upon is this:—If a scientific name be imposed on any compound, it should be strictly systematic and *correctly* expressive of composition, provided it professes to convey any knowledge at all of the composition of the body to which it is assigned. The formation and occasional or frequent use of such names is not intended to exclude the employment of other arbitrary but already-admitted terms for the same substance, whenever the use of such arbitrary names may appear to be expedient.

The following Table affords examples of some of the more important applications of the changes proposed.

	Oxides.		*Chlorides.*
$K^2 O$	Potash. Dipotassic oxide.	$Na Cl$	Chloride of sodium. Sodic chloride.
$K^2 O^4$	Peroxide of potassium. Dipotassic tetroxide.	$Tl Cl$	Chloride of thallium. Thallic chloride.
$Tl^2 O$	Oxide of thallium. Dithallic oxide.	$Tl Cl^3$	Terchloride of thallium. Thallic trichloride.
$Ba O$	Baryta. Baric oxide.	$Ba Cl^2$	Chloride of barium. Baric dichloride.
$Ba O^2$	Peroxide of barium. Baric dioxide.	$NH^4 Cl$	Hydrochlorate of ammonia. Ammonia hydrochlorate, or Ammonic chloride.
$Al^2 O^3$	Alumina. Aluminic sesquioxide.	$Al^2 Cl^6$	Chloride of aluminum. Aluminic trichloride, or Dialuminic hexachloride.
$Zn O$	Oxide of zinc. Zincic, or zinc oxide.	$Zn Cl^2$	Chloride of zinc. Zincic dichloride.
$Co O$	Protoxide of cobalt. Cobaltous oxide.	$Co Cl^2$	Chloride of cobalt. Cobaltous dichloride.
$Fe O$	Protoxide of iron. Ferrous oxide.	$Fe Cl^2$	Protochloride of iron. Ferrous dichloride.
$Fe^2 O^3$	Sesquioxide of iron. Ferric sesquioxide.	$Fe^2 Cl^6$	Sesquichloride of iron. Ferric trichloride, or Diferric hexachloride.
$Cr O$	Protoxide of chromium. Chromous oxide.	$Cr Cl^2$	Protochloride of chromium Chromous dichloride.
$Cr^2 O^3$	Sesquioxide of chromium. Chromic sesquioxide.	$Cr^2 Cl^6$	Sesquichloride of chromium. Chromic trichloride, or Dichromic hexachloride.
$Cr O^3$	Chromic acid (anhydride). Chromic trioxide, or Chromic oxyl.		

TABLE (*continued*).

	Oxides.		*Chlorides.*
$Sn\,O$	Protoxide of tin Stannous oxide.	$Sn\,Cl^{2}$	Protochloride of tin. Stannous dichloride.
$Sn\,O^{2}$	Binoxide of tin. Stannic dioxide.	$Sn\,Cl^{4}$	Bichloride of tin. Stannic tetrachloride.
$As^{2}\,O^{3}$	Arsenious acid (anhydride). Arsenious sesquioxide,	$As\,Cl^{3}$	Terchloride of arsenic. Arsenious trichloride.
$As^{2}\,O^{5}$	Arsenic acid (anhydride). Arsenic oxyl, or Diarsenic pentoxide.	$Sb\,Cl^{3}$	Terchloride of antimony. Antimonious trichloride.
$Bi\,O$	Protoxide of bismuth. Bismuthous oxide.	$Sb\,Cl^{5}$	Pentachloride of antimony. Antimonic pentachloride.
$Bi^{2}\,O^{3}$	Sesquioxide of bismuth. Bismuthic sesquioxide.		
$Bi^{2}\,O^{5}$	Bismuthic acid (anhydride) Bismuthic oxyl.	$Bi\,Cl^{3}$	Terchloride of bismuth. Bismuthic trichloride.
$Pb\,O$	Protoxide of lead. Lead oxide, or Plumbic oxide.	$Pb\,Cl^{2}$	Chloride of lead. Plumbic dichloride.
$Pb\,O^{2}$	Peroxide of lead. Plumbic dioxide.	$Ag^{2}\,Cl$	Subchloride of silver. Diargentous chloride.
$Ag^{2}\,O$	Oxide of silver. Diargentic oxide.	$Ag\,Cl$	Chloride of silver. Silver chloride, or Argentic chloride.
$Hg^{2}\,O$	Suboxide of mercury. Dimercurous oxide.	$Hg\,Cl$	Calomel. Mercurous chloride.
$Hg\,O$	Red oxide of mercury. Mercuric oxide.	$Hg\,Cl^{2}$	Corrosive sublimate. Mercuric dichloride.
$Pt\,O$	Protoxide of platinum. Platinous oxide.	$Pt\,Cl^{2}$	Protochloride of platinum. Platinous dichloride.
$Pt\,O^{2}$	Binoxide of platinum. Platinic dioxide.	$Pt\,Cl^{4}$	Bichloride of platinum. Platinic tetrachloride.

	Sulphates.		*Nitrates.*
$K^{2}\,SO^{4}$	Sulphate of potash. Dipotassic sulphate.	$K\,N\,O^{3}$	Nitrate of potash. Potassic nitrate.
$KH\,SO^{4}$	Bisulphate of potash. Hydropotassic sulphate.	$NH^{4}\,NO^{3}$	Nitrate of ammonia. Ammonic nitrate.
$Ca\,SO^{4}$	Sulphate of lime. Calcic sulphate.	$Ca\,2NO^{3}$	Nitrate of lime. Calcic dinitrate.
$Al^{2}\,3SO^{4}$	Sesquisulphate of alumina. Aluminic sesquisulphate.	$Zn\,2NO^{3}$	Nitrate of zinc. Zincic dinitrate.
$Pb\,SO^{4}$	Sulphate of lead. Lead sulphate, or Plumbic sulphate.	$Pb\,2NO^{3}$	Nitrate of lead. Plumbic dinitrate.
$Ag^{2}\,SO^{4}$	Sulphate of silver. Diargentic sulphate.	$Ag\,NO^{3}$	Nitrate of silver. Silver nitrate, or Argentic nitrate.

The application of names formed on this principle to the salts of the tribasic acids is comparatively simple, and the atomicity of the metal is readily expressed. For example,—

$3Ca2PO^4$	Bone phosphate of lime. Tricalcic diphosphate.	$Ag^3 AsO^3$	Arsenite of silver. Triargentic arsenite.
H^4Ca2PO^4	Superphosphate of lime. Tetrahydrocalcic diphosphate.	$HCu As O^3$	Scheele's green. Hydrocupric arsenite.

The same principle may in most cases easily be applied in the construction of names for the compounds of organic chemistry. Sulphate of methyle, $(CH^3)^2 SO^4$, for example, becomes dimethylic sulphate; acetate of ethyl, $(C^2 H^5, C^2 H^3 O^2)$, ethylic acetate; and so on. Care must, however, be taken in the construction of names intended to designate the salts of the organic bases, such as sulphate of dimethylia, $(C^2 H^7 N)^2 H^2 SO^4$. Dimethylic sulphate would be liable to be confounded with the ether above mentioned; dimethylia sulphate, or didimethylia sulphate would distinguish it; and instead of triethylic phosphate, $(C^6 H^{15} N)^3 H^3 PO^4$, it should be triethylia phosphate, or tritriethylia phosphate.

In the simpler department of inorganic chemistry there are certain classes of compounds which offer difficulties in any consistent system of nomenclature. Among these are the substances which have been termed *anhydro-salts,* such as certain chromates and iodates.

Potash forms several such compounds with chromic acid. We have, for instance,

The normal chromate .	$K^2 Cr O^4$.
The so-called bichromate.	$K^2 Cr^2 O^7$, not $K H Cr O^4$.
The trichromate . . .	$K^2 Cr^3 O^{10}$, not $K^2 H^4 3 Cr O^4$.
The tetrachromate. . .	$K^2 Cr^4 O^{13}$, not $K H^3 2 Cr O^4$.

The formulæ given in the second column of the foregoing symbols would in these cases represent salts formed on the normal plan, in which the potassium had partially or completely displaced the hydrogen in one or more atoms of chromic acid. Such compounds, however, have not been obtained in the solid form. In the salts really known, it appears that the normal chromate has been combined with quantities of chromic *oxyl,* $(Cr O^3)$, progressively increasing; and it seems to be necessary in naming them to adopt an arbitrary method. If the normal chromate, $(K^2 O, CrO^3)$, be called dipotassic chromate, the others successively may be named—

Dipotassic dichromate . .	($K^2O, 2CrO^3$),
Dipotassic trichromate . .	($K^2O, 3CrO^3$),
Dipotassic tetrachromate .	($K^2O, 4CrO^3$).

And in order to point out the unusual nature of these salts, a corresponding alteration in the mode of writing the formulæ should be adopted.

A similar principle, both of nomenclature and of notation, would be usefully applied in representing the composition of the silicates and borates.

Cases like these are not of unfrequent occurrence, though they are on the whole *comparatively* rare and exceptional; but upon their existence Professor Williamson originally founded his chief argument in support of his proposal to limit the use of the term acid to the anhydrous electro-negative oxides.

While admitting the ingenuity of his arguments, I do not share the views adopted by Professor Williamson in this restriction of the term acid. It is true that the use of so awkward a word as *anhydride* is objectionable, though it has been to some extent adopted from Gerhardt as a substitute for what appears to me still more objectionable, the phrase "anhydrous acid."

The difficulty which really is felt in the use of the word acid at all arises from its ambiguity, inasmuch as it has been loosely and indiscriminately applied to two different sets of bodies,—one set consisting of the electro-negative oxides of Berzelius, such as CO^2, SO^3, N^2O^5; the other set, the compounds of a halogen with hydrogen, such as HCl, HI, and HBr. The compound HCl was regarded as possessing in an eminent degree the properties of an acid, long before its chemical composition had been ascertained; and it certainly has as good a right to be described as an acid as any known substance, so long as chemists regard as an acid compound which possesses a sour taste, reddens vegetable blues, and neutralizes the alkalies. Hydric chloride has properties exactly answering to this description, which, if not strictly expressive of the scientific sense of the term acid, unquestionably expresses the sense in which it is understood in ordinary cases. CO^2, SO^3, and N^2O^5 possess none of these properties until they have become dissolved in, or till they have combined with water. The use of the term acid for bodies which possess properties like those of hydric chloride cannot, as it seems to me, be displaced from our popular language; and chemists of late years have been gradually accepting the necessity, and have adopted Gerhardt's definition of an acid, as a salt whose base (or rather *basyl*) is hydrogen—a definition which includes all the substances popularly known as acids, and others which are chemically analogous to them.

If therefore an acceptable term for the class now known as *anhydrides* can be suggested, one principal difficulty will disappear: for these bodies, might not the term *oxyl* be introduced? If this be adopted, SO^3 would be sulphuric oxyl; N^2O^5 nitric oxyl; N^2O^3, nitrous oxyl; CO^2, carbonic oxyl; and so on.

There is no need to disturb the meaning which the word *base* has long possessed. Berzelius's definition of a base as an electro-positive oxide sufficiently expresses its nature; and K^2O, CaO, and $(NH^4)^2O$ would still be bases as at present. The formula (CaO, CO^2) would represent calcic carbonate as a compound of a base and an oxyl.

But if the application of the term base continue to be limited to the class originally represented by it, another term will be needed to express the member of the salt which occupies the position of calcium indicated by the formula for calcic carbonate, Ca, CO^3. For this purpose the term *basyl,* suggested by Mr. Graham many years ago, and since extensively used in this sense, is well adapted. Calcium is then the basyl in calcic carbonate.

We now want a word to represent the other portion of the salt which is electrically opposed to the basyl; and here we might probably adopt with advantage a term suggested by one used by the late Professor Daniell.

If the metal or electro-positive portion of a salt, such as hydrogen or ammonium, be termed its basyl, we might term the electro-negative portion the *oxion* of the salt, each oxion being, when necessary, distinguished by a name derived from the acid of the salt. Ca, CO^3 would then be a salt consisting of the basyl calcium and of the *oxion* CO^3, which might be termed *carbion*; Fe'', SO^4 would be a salt with the basyl ferrosum, and the oxion, SO^4, or *sulphion*; $Na^2 SO^3$ would be a salt with the basyl sodium, and the oxion, SO^3, which in this case would be *sulphosion,* indicative of its relation to sulphurous acid. The nitrates, upon a similar view of their constitution, would consist of a metallic basyl with NO^3, *nitrion*; the nitrites would contain *nitrosion,* NO^2; the acetates *acetion,* $C^2H^3O^2$; the oxalates *oxalion,* C^2O^4; and so on.

Basyls.	*Oxions.*		*Basyls.*	*Oxions.*	
(Ag,	NO^3)	. Silver nitrate.	(Na^2,	SO^3)	. Disodic sulphite.
(K,	NO^2)	. Potassic nitrite.	(Pb,	$2C^2H^3O^2$).	Plumbic diacetate.
(Mg,	SO^4)	. Magnesic sulphate.	(Ca,	C^2O^4)	. Calcic oxalate.

In describing the decompositions of many salts, we are at present obliged to use much circuitous statement for want of terms corresponding to those now suggested. In so simple a case as the displacement of lead from a solution of plumbic diacetate by zinc in preparing the "lead tree," we are obliged to say zinc displaces the lead from its combination with the other elements of the salt, instead of saying that the zinc displaces the lead from its combination with acetion. Again, when silver nitrate and sodic chloride, each in solution, are mixed, the chlorine and the nitrion change places, 62 of nitrion, N^2O^5, being equivalent in saturating power to 35·5 of chlorine. Let any one try to explain this decomposition in our ordinary chemical language, and he will see the advantage gained by adopting the use of some such terms as those now proposed.

King's College, London,
June 10, 1865.

III. THE ASTRONOMER ROYAL *on Hemiopsy*.

To the Editors of the Philosophical Magazine and Journal.

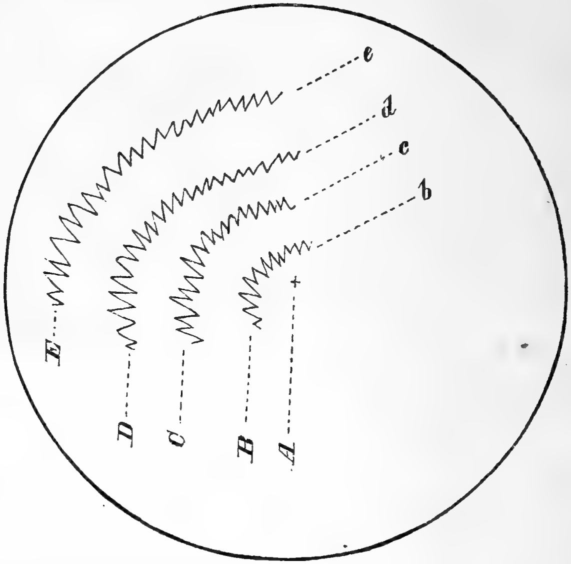
Royal Observatory, Greenwich, S.E.,
June 5, 1865.

GENTLEMEN,

I REMARK, in a late Number of the Philosophical Magazine, a paper "On Hemiopsy" by Sir David Brewster, in which Sir David has given the results of the experience of that singular malady by Dr. Wollaston and by himself. I have myself been frequently attacked by it, certainly not fewer than twenty times, probably much oftener; and I am acquainted with two persons who have suffered from it, one of them at least a hundred times. From the information of my friends, and from my own experience, I am able to supply an account of some features of the malady which appear to have escaped the notice of Dr. Wollaston and Sir David Brewster.

One of my friends attributes each *accès* of the disease to mental anxiety or overcrowding of business. In my own case, I have never been able to refer it distinctly to any antecedent cause. In a few instances I have known it to follow that state of the brain or eye in which a sudden pressure of the breath causes the appearance of numerous spherical globules floating across the field of view.

I discover the beginning of the attack by a little indistinctness in some object at which I am looking directly; and I believe the locality of this indistinctness upon the retina to be, not the place of entrance of the optic nerve, but the centre of the usual field of vision. Very soon I perceive that the indistinctness is caused by the image being crossed by short lines which change their direction and place. In a little time the disease takes its normal type, and presents successively the appearances shown in the following diagram. In drawing this, I have supposed that the principal obscuration of objects is apparently on the left side; by reversing the figure, left to right, the appearances will be given which present themselves when the principal obscuration appears to be on the right side. (In my own experience, I believe it is an even chance whether the obscuration is to the right or to the left.) The bounding circle shows roughly the extent to which the eye is sensible of vision more or less vivid. Only one arch is seen at one time: the arch is small at first, and gradually increases in dimensions.



A, the beginning of the disease.
B*b*, C*c*, D*d*, E*e*, successive appearances, as the arch gradually enlarges.

The zigzags nearly resemble those in the ornaments of a Norman arch, but are somewhat sharper. Those near the letters B, C, D, E are much deeper than those near *b, c, d, e.* The zigzags do not change their relative arrangement during the dilatation of the arch, but they tremble strongly: the trembling near B, C, D, E is much greater than that near *b, c, d, e.* There is a slight appearance of scarlet colour on one edge, the external edge, I believe, of the zigzags. As the arch enlarges, vision becomes distinct in the centre of the field. The strongly-trembling extremity of the arch rises at the same time that it passes to the left, and finally passes from the visible field, and the whole phenomenon disappears.

I have never been able to decide with certainty whether the disease really affects both eyes. The first impression on the mind is that only one eye is affected (in the instance depicted above, the left eye). There is general obscurity on one side; but the tremor and boiling are so oppressive, that, if produced only in one eye, they may nearly extinguish the corresponding vision in the other.

The duration of this ocular derangement with me is usually from twenty to thirty minutes, but with one of my friends it sometimes lasts much longer. In general, I feel no further inconvenience from it; but with my friends, it is followed by oppressive head-ache.

In one instance it was remarked that the mouth of the person afflicted was sensibly distorted on one side. And in one attack on myself, which occurred while I was conversing with an acquaintance in a railway-carriage, I soon became painfully sensible that I had not the usual command of speech, that my memory failed so much that I did not know what I had said or had attempted to say, and that I might be talking incoherently.

I entertain no doubt that the seat of the disease is the brain; that the disease is a species of paralysis; and that the ocular affection is only a secondary symptom.

G. B. Airy.

IV. *On Elliptic Ribs.* *By* James H. Cotterill, *B.A.*, *Scholar of St. John's College, Cambridge*[*].

1. SINCE my last paper was written, I have seen a memoir by Professor W. Thomson (Phil. Trans. 1863), in the appendix to which it is explicitly stated that the work done in equilibrium of elasticity is a minimum. Had I been fortunate enough to have seen this memoir earlier, I should probably have been able to avoid the difficult task of discussing the general principle, and have confined myself to its application to that class of problems for which, as I have elsewhere said, I conceive it to be peculiarly adapted. Enough has been said to enable an opinion to be formed on this point, and I therefore should not have proceeded further had I not, on application of the general method explained in a recent paper to the case of arched ribs of elliptical form, obtained some results which appear to me interesting when compared with Mr. Fairbairn's experiments on elliptical boiler-flues. The present paper will therefore be devoted to calculating the stress on any section of an elliptical rib; and the results obtained may perhaps be of service in the theory of arches.

In the paper referred to, it was shown that the stress on any section of an arched rib may be expressed by the equations

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[*] Communicated by the Author.

$$F=K+F_0\cos\phi+H_0\sin\phi,$$

$$H=p\rho+\frac{dK}{d\phi}+H_0\cos\phi-F_0\sin\phi,$$

$$M=\int_0^\phi K\rho d\phi+F_0\int_0^\phi\rho\cos\phi\,d\phi+H_0\int_0^\phi\rho\sin\phi\,d\phi+M_0,$$

and that the three constants F_0, H_0, M_0, which depend on the physical constitution of the rib, may be approximately determined by making

$$U=\int\left(\frac{M^2}{2EI}+\frac{H^2}{2EA}\right)ds \text{ a minimum.}$$

It was further remarked that when, as in the present case, the angle subtended by the rib was considerable, and the distribution of the load different from that necessary for the equilibrium of a linear arch of like form, then it was sufficient to use $\int\frac{M^2}{2EI}.ds$ for U. In fact in the present case, whatever be the load, the error in the parts of the coefficients containing the constants is of the order $\frac{k^2}{a^2}$, where k is the radius of gyration of the transverse section, and a the semiaxis major; and $\frac{k^2}{a^2}$, even in such an extreme case as the link of a chain, is always very small*. The same thing is true for the parts of the coefficients which do not contain the constants, provided that the load is not distributed as for a linear arch. But the problems in which this is the case, though common in practice, it is probably not going too far to say, cannot at present be satisfactorily treated, the question being a complex case of crushing by bending, and involving other difficulties besides. Thus for practical purposes it is generally useless to calculate any terms in the coefficients arising from H when the rib subtends a large angle, though there may be cases in which it is advisable to consider such of them as do not involve the constants.

I proceed therefore to calculate the differential coefficients $\frac{dU}{dF_0}$, $\frac{dU}{dH_0}$, $\frac{dU}{dM_0}$ from the simpler value of U with reference to an elliptic quadrant.

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* The effect of curvature on the *strength* of beams appears to me important. I hope to attempt to estimate it at a future time; pending which investigation the question of the stress on links of a chain is omitted, though naturally belonging to this paper.

I begin by calculating the parts of the coefficients which contain the arbitrary constants.

We have, so far as these constants are concerned,

$$\mathrm{M}=\mathrm{F}_0\int_0^{\phi}\rho\cos\phi\,d\phi+\mathrm{H}_0\int_0^{\phi}\rho\sin\phi\,d\phi+\mathrm{M}_0.$$

Let the semiaxes of the ellipse be a and $a\cos\beta$, so that $\sin\beta$ is the eccentricity; then if the initial section for which $\phi=0$ be supposed at the extremity of the axis major, we shall have

$\rho=\dfrac{a\cos^2\beta}{\Delta^3}$, where $\Delta=\sqrt{1-\sin^2\beta\sin^2\phi}$;

$$\therefore\int_0^{\phi}\rho\cos\phi\,d\phi=a\cos^2\beta\int_0^{\phi}\frac{\cos\phi}{\Delta^3}\,d\phi=a\cos^2\beta\frac{\sin\phi}{\Delta},$$

and

$$\int_0^{\phi}\rho\sin\phi\,d\phi=a\cos^2\beta\int_0^{\phi}\frac{\sin\phi}{\Delta^3}\,d\phi=a\left(1-\frac{\cos\phi}{\Delta}\right).$$

Assume $\cos\phi'=\dfrac{\cos\phi}{\Delta}$, then

$$\sin\phi'=\frac{\sqrt{\Delta^2-\cos^2\phi}}{\Delta}=\frac{\sin\phi\cos\beta}{\Delta},$$

and therefore

$$\mathrm{M}=\mathrm{F}_0\,a\cos\beta\sin\phi'+\mathrm{H}_0\,a(1-\cos\phi')+\mathrm{M}_0,$$

a value of M which can be easily obtained from a figure, ϕ' being in fact the eccentric angle of the section considered.

Now we have, beginning with $\dfrac{d\mathrm{U}}{d\mathrm{F}_0}$, the factor EI being, as before, merged in U,

$$\frac{d\mathrm{U}}{d\mathrm{F}_0}=\int_0^{\frac{\pi}{2}}\mathrm{M}\frac{d\mathrm{M}}{d\mathrm{F}_0}\rho\,d\phi$$

$$=a\cos\beta\int_0^{\frac{\pi}{2}}\mathrm{M}\rho\sin\phi'\,d\phi;$$

and it is easily proved that $\rho d\phi=a\Delta' d\phi'$, where

$$\Delta'=\sqrt{1-\sin^2\beta\cos^2\phi'},$$

$$\therefore\frac{d\mathrm{U}}{d\mathrm{F}_0}=a^2\int_0^{\frac{\pi}{2}}\mathrm{M}\sin\phi'\Delta' d\phi'.$$

Substituting the value of M just now given, we find

$$\frac{dU}{dF_0}=F_0a^3\cos^2\beta\int_0^{\frac{\pi}{2}}\sin^2\phi'\Delta'd\phi'+H_0a^3\cos\beta\int_0^{\frac{\pi}{2}}\sin\phi'(1-\cos\phi')\Delta'd\phi'$$

$$+M_0a^2\cos\beta\int_0^{\frac{\pi}{2}}\sin\phi'\Delta'd\phi'.$$

Let

$$\int_0^{\frac{\pi}{2}}\sin^2\phi'\Delta'd\phi'=P\,;\quad\int_0^{\frac{\pi}{2}}\sin\phi'\cos\phi'\Delta'd\phi'=Q\,;\quad\int_0^{\frac{\pi}{2}}\sin\phi'\Delta'd\phi'=R.$$

Then

$$P=\int_0^{\frac{\pi}{2}}\sin^2\phi'\Delta'd\phi'=\int_0^{\frac{\pi}{2}}\sin^2\phi'\sqrt{1-\sin^2\beta\cos^2\phi'}\,d\phi'$$

$$=\int_0^{\frac{\pi}{2}}\cos^2\phi'\sqrt{1-\sin^2\beta\sin^2\phi'}\,d\phi'$$

$$=\int_0^{\frac{\pi}{2}}\frac{\cos^2\phi'-\sin^2\beta\sin^2\phi'\cos^2\phi'}{\sqrt{1-\sin^2\beta\sin^2\phi'}}\,d\phi'$$

$$=\int_0^{\frac{\pi}{2}}\frac{1-(1+\sin^2\beta)\sin^2\phi'+\sin^2\beta\sin^4\phi'}{\sqrt{1-\sin^2\beta\sin^2\phi'}}\,d\phi'.$$

Now by a known formula of reduction (Hymer's 'Integral Calculus,' p. 191),

$$\int_0^{\frac{\pi}{2}}\frac{\sin^4\phi'd\phi'}{\sqrt{1-\sin^2\beta\sin^2\phi'}}=\frac{2}{3}\cdot\frac{1+\sin^2\beta}{\sin^2\beta}\int_0^{\frac{\pi}{2}}\frac{\sin^2\phi'd\phi'}{\sqrt{1-\sin^2\beta\sin^2\phi'}}$$

$$-\frac{1}{3\sin^2\beta}\int_0^{\frac{\pi}{2}}\frac{d\phi'}{\sqrt{1-\sin^2\beta\sin^2\phi'}}\,;$$

$$\therefore\ P=\int_0^{\frac{\pi}{2}}\frac{\frac{2}{3}-\frac{1}{3}(1+\sin^2\beta)\sin^2\phi'}{\sqrt{1-\sin^2\beta\sin^2\phi'}}\,d\phi'$$

$$=\frac{1+\sin^2\beta}{3\sin^2\beta}\int_0^{\frac{\pi}{2}}\frac{\frac{2\sin^2\beta}{1+\sin^2\beta}-\sin^2\beta\sin^2\phi'}{\sqrt{1-\sin^2\beta\sin^2\phi'}}\,d\phi'.$$

If then the complete elliptic functions of the first and second orders be denoted by F_1 and E_1,

$$P=\frac{1+\sin^2\beta}{3\sin^2\beta}\left\{-\frac{\cos^2\beta}{1+\sin^2\beta}F_1+E_1\right\}$$

$$=\frac{(1+\sin^2\beta)E_1-\cos^2\beta F_1}{3\sin^2\beta}.$$

The functions E_1 and F_1 are tabulated for each degree of the angle β in Moseley's 'Engineering and Architecture.'

The integrals Q and R are easily found to be

$$Q=\frac{1-\cos^3\beta}{3\sin^2\beta};\quad R=\frac{\sin\beta\cos\beta+\beta}{2\sin\beta}.$$

Again,

$$\frac{dU}{dH_0}=\int_0^{\frac{\pi}{2}}M\frac{dM}{dH_0}\rho d\phi$$

$$=a\int_0^{\frac{\pi}{2}}M(1-\cos\phi')\rho d\phi$$

$$=a^2\int_0^{\frac{\pi}{2}}M(1-\cos\phi')\Delta' d\phi',$$

and consequently, substituting the value of M,

$$\frac{dU}{dH_0}=F_0a^3\cos\beta\int_0^{\frac{\pi}{2}}\sin\phi'(1-\cos\phi')\Delta' d\phi'+H_0a^3\int_0^{\frac{\pi}{2}}(1-\cos\phi')^2\Delta' d\phi'$$

$$+M_0a^2\int_0^{\frac{\pi}{2}}(1-\cos\phi')\Delta' d\phi'.$$

Since $\int_0^{\frac{\pi}{2}}\Delta' d\phi'$, or $\int_0^{\frac{\pi}{2}}\sqrt{1-\sin^2\beta\cos^2\phi'}d\phi'$ is equal to

$$\int_0^{\frac{\pi}{2}}\sqrt{1-\sin^2\beta\sin^2\phi'}d\phi',$$

that is, to E_1, and since moreover

$$\int_0^{\frac{\pi}{2}}(1-\cos\phi')^2\Delta' d\phi'=2\int_0^{\frac{\pi}{2}}(1-\cos\phi')\Delta' d\phi'-\int_0^{\frac{\pi}{2}}\sin^2\phi'\Delta' d\phi'$$

$$=2E_1-P-2\int_0^{\frac{\pi}{2}}\cos\phi'\Delta' d\phi',$$

the only new integral required is

$$\int_0^{\frac{\pi}{2}} \cos\phi\,\Delta\,d\phi' = \mathrm{S} \text{ suppose};$$

and after some reductions it is readily found that

$$\mathrm{S} = \frac{\sin\beta + \cos^2\beta \log\{\tan\beta + \sec\beta\}}{2\sin\beta}.$$

Again,

$$\frac{d\mathrm{U}}{d\mathrm{M}_0} = \int_0^{\frac{\pi}{2}} \mathrm{M}\rho\,d\phi = a\int_0^{\frac{\pi}{2}} \mathrm{M}\Delta' d\phi';$$

and substituting, as before, the value of M, we find

$$\frac{d\mathrm{U}}{d\mathrm{M}_0} = \mathrm{F}_0 a^2 \cos\beta \int_0^{\frac{\pi}{2}} \sin\phi'\Delta' d\phi' + \mathrm{H}_0 a^2 \int_0^{\frac{\pi}{2}} (1-\cos\phi')\Delta' d\phi'$$

$$+\mathrm{M}_0 a \int_0^{\frac{\pi}{2}} \Delta' d\phi'.$$

These integrals are the same which have already been considered, and we consequently have, as the parts of the coefficients due to the constants,

$$\left.\begin{aligned}
\frac{d\mathrm{U}}{d\mathrm{F}_0} &= \mathrm{F}_0 a^3 \cos^2\beta\,\mathrm{P} + \mathrm{H}_0 a^3 \cos\beta(\mathrm{R}-\mathrm{Q}) + \mathrm{M}_0 a^2 \cos\beta\,\mathrm{R},\\
\frac{d\mathrm{U}}{d\mathrm{H}_0} &= \mathrm{F}_0 a^3 \cos\beta(\mathrm{R}-\mathrm{Q}) + \mathrm{H}_0 a^3(2\mathrm{E}_1 - \mathrm{P} - 2\mathrm{S}) + \mathrm{M}_0 a^2(\mathrm{E}_1 - \mathrm{S}),\\
\frac{d\mathrm{U}}{d\mathrm{M}_0} &= \mathrm{F}_0 a^2 \cos\beta\,\mathrm{R} + \mathrm{H}_0 a^2(\mathrm{E}_1 - \mathrm{S}) + \mathrm{M}_0 a \mathrm{E}_1.
\end{aligned}\right\}(\mathrm{A})$$

The numerical values of P, Q, R, S to three places of decimals when $\beta = 60°$, are written below for use in examples which follow:—

$$\mathrm{P} = \cdot 702,\quad \mathrm{R} = \cdot 854,\quad \mathrm{E}_1 = 1\cdot 211.$$

$$\mathrm{Q} = \cdot 389,\quad \mathrm{S} = \cdot 690,$$

2. The equations (A) are sufficient for the solution of all questions concerning elliptical or semielliptical ribs acted on by forces at the extremities of its axes. For example, take the following case.

A semielliptic rib is fixed at the springing, and a weight (W) is placed at the extremity of its major axis, which is supposed vertical, to find the stress on any section. Here the equations for the constants are

$$F_0=-\frac{W}{2};\quad \frac{dU}{dH_0}=0;\quad \frac{dU}{dM_0}=0,$$

the abutments being supposed sensibly immoveable.

Therefore, from equations (A), putting $F_0=-\frac{W}{2}$,

$$H_0a\{2E_1-P-2S\}+M_0(E_1-S)=\frac{W}{2}a\cos\beta(R-Q),$$

$$H_0a\{E_1-S\}+M_0E_1=\frac{W}{2}a\cos\beta R.$$

Let now $\beta=60°$, in which case the rise of the rib is equal to its span, then these equations become

$$136H_0a+208M_0=46Wa,$$

$$208H_0a+484M_0=85Wa;$$

whence

$$M_0=\frac{1}{9}Wa \text{ and } H_0=\frac{4}{27}W \text{ nearly},$$

which are the values of the bending moment at the crown and the horizontal thrust of the rib. The bending moment in any other section is given by

$$M=Wa\{\tfrac{7}{27}-\tfrac{1}{4}\sin\phi'-\tfrac{4}{27}\cos\phi'\};$$

thus M diminishes to zero as ϕ' increases, becomes negative, attains a maximum when $\tan\phi'=\frac{27}{16}$, diminishes again to zero, becomes positive, its final value being $(\phi'=90°)\frac{1}{108}Wa$. On substitution of $\tan^{-1}\frac{27}{16}$ for ϕ', the value of M at the negative maximum will be found to be about $\frac{1}{30}Wa$.

Thus the maximum stress on the rib is at the crown where the bending moment is $\frac{1}{9}Wa$.

Let it now be supposed that the same formula which gives the work accumulated in a rib of homogeneous material, will also give the work accumulated in a brick or stone arch-ring, in other words, that this ring would yield under the action of forces acting on it in the same manner as a solid arch-ring would, provided only that the deviation of the centre of pressure is not beyond the limit necessary for the stability of the ring. This supposition has been employed by Professor Rankine in his work on Civil Engineering.

If now such a semielliptical arch-ring be supposed in equilibrium under any forces consistent with its stability, and H' be the thrust on any section, then, if a weight W be suspended at the crown, the additional bending moment M will be given by the equation just obtained, and the position of the centre of pressure of the section will change by the quantity $\frac{M}{H'}$ nearly (W being supposed small), and thus it can be found whether the arch-ring is of sufficient depth for the stability of the arch under the load W. If, for instance, an arch sustain the pressure of an indefinite mass of earth of such frictional tenacity that the linear arch, which is in equilibrium under it, has its axes in the ratio 1 : 2, the thrust at the crown produced by a pressure of earth p is $\frac{pa}{4}$, and the bending moment produced at the crown by a weight W hung from it was shown to be $\frac{1}{9}Wa$; the centre of pressure at the crown therefore moves upwards through a space $\frac{4W}{9p}$ approximately. It is here supposed that no work is done in the mass of earth, that is, that the spreading of the arch caused by W does not cause any *additional* pressure between the arch and the earth resting on it. And on the other hand, it is also supposed that the change of form produced in the arch by W does not sensibly change the stress produced by the forces originally acting. The first condition is not realized in practice, nor if the arch be originally linear can the second either; but the errors so produced counteract each other more or less.

Again, suppose a semielliptical rib placed with its major axis horizontal, and let it be fixed at the springing and loaded with a weight W at the crown; the initial section being now at the springing, the equations for the constants will be

$$H_0 = \frac{W}{2}; \quad \frac{dU}{dF_0} = 0; \quad \frac{dU}{dM_0} = 0;$$

and having determined the values of F_0 and M_0, the stress on any section will be known; but, on account of the rigidity of the backing, the actual deviation of the centre of pressure at the crown of a semielliptical arch would probably be very different from that obtained from this solution in the manner indicated above. It is therefore unnecessary to pursue the subject.

3. I shall next calculate the parts of the coefficients due to the action of uniform fluid pressure on an elliptic quadrant, which do not contain the constants.

The general value of the function K is

$$K = \left\{1 + \frac{d^2}{d\phi^2}\right\}^{-1} \left\{q\rho - \frac{d}{d\phi}(p\rho)\right\};$$

and in this case p is constant and q is zero;

$$\therefore\ \mathrm{K}=-p\left\{1+\frac{d^2}{d\phi^2}\right\}^{-1}\frac{d\rho}{d\phi}$$

$$=-pa\frac{d}{d\phi}\left\{1+\frac{d^2}{d\phi^2}\right\}^{-1}\frac{\cos^2\beta}{\Delta^3},$$

where, as before,

$$\Delta=\sqrt{1-\sin^2\beta\sin^2\phi}\,;$$

but

$$\left(1+\frac{d^2}{d\phi^2}\right)\Delta=\frac{\cos^2\beta}{\Delta^3},$$

$$\therefore\ \mathrm{K}=-pa\frac{d\Delta}{d\phi}.$$

Thus, omitting terms which contain the constants,

$$\mathrm{F}=\mathrm{K}=-pa\frac{d\Delta}{d\phi},$$

$$\mathrm{H}=p\rho+\frac{d\mathrm{K}}{d\phi}=pa\left\{\frac{\cos^2\beta}{\Delta^3}-\frac{d^2\Delta}{d\phi^2}\right\}=pa\Delta,$$

$$\mathrm{M}=\int_0^\phi \mathrm{K}\rho d\phi=-pa^2\cos^2\beta\int_0^\phi\frac{1}{\Delta^3}\frac{d\Delta}{d\phi}d\phi$$

$$=\tfrac{1}{2}pa^2\cos^2\beta\left(\frac{1}{\Delta^2}-1\right)$$

$$=\tfrac{1}{2}pa^2\cos^2\beta\frac{\sin^2\beta\sin^2\phi}{\Delta^2}.$$

But

$$\sin\phi'=\frac{\cos\beta\sin\phi}{\Delta},$$

$$\therefore\ \mathrm{M}=\tfrac{1}{2}pa^2\sin^2\beta\sin^2\phi'.$$

Here, as before, ϕ' is the inclination to the major axis of the corresponding section of a circular rib from which the elliptic rib can be derived by projection.

Now

$$\frac{d\mathrm{U}}{d\mathrm{F}_0}=\int_0^{\frac{\pi}{2}}\mathrm{M}\frac{d\mathrm{M}}{d\mathrm{F}_0}\rho d\phi=a\cos\beta\int_0^{\frac{\pi}{2}}\mathrm{M}\sin\phi'\rho d\phi.$$

Substituting for M its value just given, and for $\rho d\phi$, as before, $a\Delta'd\phi'$, where $\Delta'=\sqrt{1-\sin^2\beta\cos^2\phi'}$,

$$\frac{d\mathrm{U}}{d\mathrm{F}_0}=\tfrac{1}{2}pa^4\cos\beta\sin^2\beta\int_0^{\frac{\pi}{2}}\sin^3\phi'\Delta'd\phi'.$$

But

$$\int_0^{\frac{\pi}{2}} \sin^3 \phi' \Delta' d\phi' = \mathrm{R} - \int_0^{\frac{\pi}{2}} \cos^2 \phi' \sin \phi' \Delta' d\phi',$$

where $\mathrm{R} = \int_0^{\frac{\pi}{2}} \sin \phi' \Delta' d\phi'$ is an integral used before, and by parts

$$\int_0^{\frac{\pi}{2}} \cos^2 \phi' \sin \phi' \Delta' d\phi' = \left[\frac{\Delta'^3}{3\sin^2 \beta} \cos \phi'\right]_0^{\frac{\pi}{2}} + \int_0^{\frac{\pi}{2}} \frac{\sin \phi' \Delta'^3}{3\sin^2 \beta} d\phi'$$

$$= -\frac{\cos^3 \beta}{3\sin^2 \beta} + \frac{\mathrm{R}}{3\sin^2 \beta} - \tfrac{1}{3}\int_0^{\frac{\pi}{2}} \sin \phi' \cos^2 \phi' \Delta' d\phi';$$

$$\therefore \int_0^{\frac{\pi}{2}} \cos^2 \phi' \sin \phi' \Delta' d\phi' = \frac{\mathrm{R} - \cos^3 \beta}{4\sin^2 \beta},$$

and

$$\int_0^{\frac{\pi}{2}} \sin^3 \phi' \Delta' d\phi' = \frac{(4\sin^2 \beta - 1)\mathrm{R} + \cos^3 \beta}{4\sin^2 \beta}.$$

Again, proceeding in like manner for $\frac{d\mathrm{U}}{d\mathrm{H}_0}$, we have

$$\frac{d\mathrm{U}}{d\mathrm{H}_0} = a^2 \int_0^{\frac{\pi}{2}} \mathrm{M}(1 - \cos \phi') \Delta' d\phi'$$

$$= \tfrac{1}{2} pa^4 \sin^2 \beta \int_0^{\frac{\pi}{2}} \sin^2 \phi' (1 - \cos \phi') \Delta' d\phi'$$

$$= \tfrac{1}{2} pa^4 \sin^2 \beta \left\{ \mathrm{P} - \int_0^{\frac{\pi}{2}} \sin^2 \phi' \cos \phi' \Delta' d\phi' \right\};$$

but by parts

$$\int_0^{\frac{\pi}{2}} \sin^2 \phi' \cos \phi' \Delta' d\phi' = \frac{1}{3\sin^2 \beta} - \frac{1}{3\sin^2 \beta} \int_0^{\frac{\pi}{2}} \cos \phi' \Delta'^3 d\phi';$$

$$= \frac{1}{3\sin^2 \beta} - \frac{\mathrm{S}}{3 \sin^2 \beta} + \tfrac{1}{3}\int_0^{\frac{\pi}{2}} \cos^3 \phi' \Delta' d\phi';$$

also

$$\int_0^{\frac{\pi}{2}} \sin^2 \phi' \cos \phi' \Delta' d\phi' = \mathrm{S} - \int_0^{\frac{\pi}{2}} \cos^3 \phi' \Delta' d\phi';$$

$$\therefore \int_0^{\frac{\pi}{2}} \sin^2 \phi' \cos \phi' \Delta' d\phi' = \frac{1 - \mathrm{S} \cos^2 \beta}{4\sin^2 \beta};$$

so that

$$\frac{dU}{dH_0}=\tfrac{1}{2}pa^4\left\{P\sin^2\beta+\frac{S\cos^2\beta-1}{4}\right\}.$$

Again,

$$\frac{dU}{dM_0}=\int_0^{\frac{\pi}{2}} M\rho d\phi=a\int_0^{\frac{\pi}{2}} M\Delta' d\phi'$$

$$=\tfrac{1}{2}pa^3\sin^2\beta\int_0^{\frac{\pi}{2}}\sin^2\phi'\Delta' d\phi'.$$

Thus the parts of the coefficients due to constant fluid pressure, so far as they arise from M, are

$$\left.\begin{aligned}\frac{dU}{dF_0}&=\tfrac{1}{8}pa^4\cos\beta\{(4\sin^2\beta-1)R+\cos^3\beta\},\\ \frac{dU}{dH_0}&=\tfrac{1}{2}pa^4\left\{P\sin^2\beta+\frac{S\cos^2\beta-1}{4}\right\},\\ \frac{dU}{dM_0}&=\tfrac{1}{2}pa^3\sin^2\beta P,\end{aligned}\right\}\quad . \quad (B)$$

P, R, S being the same quantities as in the first section, and whose values were there given for $\beta=60°$. Equations (A) and (B) give the complete coefficients for constant fluid pressure when β is not small, quantities of the order $\frac{k^2}{a^2}$ being neglected.

4. I proceed to apply these values to find the stress on any section of an elliptic tube exposed to uniform fluid pressure; the tube being supposed of great length so as to eliminate the effect of the ends. Here

$$H=pa\Delta+H_0\cos\phi-F_0\sin\phi.$$

But it is plain that $H=pa$ when $\phi=0$, and pb when $\phi=90°$, the initial section being at the extremity of the axis major; therefore the equations for the constants are

$$H_0=0,\quad F_0=0,\quad \frac{dU}{dM_0}=0;$$

∴ from (A) and (B),

$$M_0E_1+\tfrac{1}{2}pa^2\sin^2\beta P=0,$$

$$M_0=-\tfrac{1}{2}pa^2\sin^2\beta\frac{P}{E_1}.$$

For example, let $\beta=45°$, then $\frac{P}{E_1}=\cdot 542$, and

$$M=\tfrac{1}{4}pa^2\{\sin^2\phi'-\cdot 542\},$$

an equation which gives the bending moment on any section whose eccentric angle is ϕ'.

On account of the thinness of the tube the strength is inversely as the maximum bending moment, which is always at the extremity of the major axis; so that the strength varies inversely as M_0. Suppose ρ_1 the maximum radius of curvature, that is, $\frac{a^2}{a\cos\beta}$, then $a=\rho_1\cos\beta$;

$$\therefore\ M_0=-\tfrac{1}{2}pa\rho_1\cos\beta\sin^2\beta\frac{P}{E_1}.$$

The annexed Table gives the values of $\cos\beta\sin^2\beta\frac{P}{E_1}$ for values of β between 45° and 65°, differing by $2\frac{1}{2}$°; whence it appears that the mean of the values of $\cos\beta\sin^2\beta\frac{P}{E_1}$ for values of β between $47\frac{1}{2}$° and 65° is ·212, and that the extreme deviation from this mean value is not more than 5 per cent. of the whole.

β.	$\cos\beta\sin^2\beta\frac{P}{E_1}$.
45°	·192
$47\frac{1}{2}$	·201
50	·209
$52\frac{1}{2}$	·214
55	·218
$57\frac{1}{2}$	·219
60	·217
$62\frac{1}{2}$	·213
65	·206

Whence it appears that between these limits of β (which correspond to the values $\frac{2}{3}$rds and $\frac{7}{16}$ths nearly of the ratio of the axes) $M_0\propto a\rho_1$ nearly, and the strength inversely as $a\rho_1$. Now Mr. Fairbairn states that the strength of elliptical tubes varies inversely as the length, and inversely as ρ_1; so that, for tubes of the same *proportion of breadth* (*a*) *to length*, the strength varies inversely as $a\rho_1$. Thus, within the limits specified, the relative strength of tubes of indefinite length is the same as that of tubes of a given ratio of breadth to length*.

5. Next let it be required to find the stress at any point of a semielliptic rib, with major axis horizontal, fixed at the springing, and sustaining the pressure of a depth of water so great that the pressure at different points is sensibly constant.

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* For reasons previously referred to, the solution is inapplicable when the tube is nearly circular.

Here the equations for the constants are

$$H_0=0, \quad \frac{dU}{dF_0}=0, \quad \frac{dU}{dM_0}=0,$$

the initial section being at the springing; that is, using the values given in (A) and (B),

$$F_0 a\cos\beta P+M_0 R+\tfrac{1}{8}pa^2\{(4\sin^2\beta-1)R+\cos^3\beta\}=0,$$
$$F_0 a\cos\beta R+M_0 E_1+\tfrac{1}{2}pa^2\sin^2\beta P=0.$$

For example, suppose the rise of the rib one-fourth the span, so that $\beta=60°$, then these equations become

$$351F_0 a+\ 854M_0+228pa^2=0,$$
$$427F_0 a+1211M_0+263pa^2=0.$$

The roughly approximate values of M_0, H_0 from these equations are $M_0=\cdot 08pa^2$, $F_0=-\cdot 85pa$,

$$\therefore\ M=\tfrac{1}{2}pa^2\sin^2\beta\sin^2\phi'+F_0 a\cos\beta\sin\phi'+M_0$$
$$=\tfrac{1}{2}pa^2\{\tfrac{3}{4}\sin^2\phi'-\cdot 85\sin\phi'+\cdot 16\}$$
$$=\cdot 03pa^2 \text{ when } \phi'=90°, \text{ that is, at the crown.}$$

M is a maximum when $\frac{dM}{d\phi'}=0$, that is, when

$$\tfrac{3}{2}\sin\phi'=\cdot 85,$$

or

$$\sin\phi'=\cdot 56\ (\phi'=34°);$$

substituting which value of $\sin\phi'$, we find

$$M=-\cdot 16pa^2.$$

The points of contrary flexure will be found, on solving the equation $M=0$, to be given by $\phi'=15°$ and $\phi'=61°$ nearly.

Also

$$H=pa\Delta+\cdot 85pa\sin\phi$$
$$=\tfrac{1}{2}pa+\cdot 85pa \text{ when } \phi=90°$$
$$=1\cdot 35pa \text{ at the crown.}$$

This solution serves to determine approximately the greatest admissible fluctuation of the surface of water whose weight, when the surface is at its normal height, is sustained by a linear arch; for a semielliptic arch approaches closely to an hydrostatic arch which has its radii of curvature at crown and springing in the same ratio as the corresponding radii of the semiellipse.

V. *On the Velocity of Sound.*

By Dr. H. W. Schröder van der Kolk*.

ALTHOUGH the determination of the velocity of sound is in itself a problem of great interest, the exact knowledge of this constant has become doubly interesting since the observation that it was fitted to furnish one of the most accurate methods of determining the mechanical equivalent of heat. The attentive examination of this problem is therefore a matter of the greatest interest.

The methods that have been employed to measure the velocity of sound may be divided into direct and indirect.

To the latter class belongs Wertheim and Masson's method by means of organ-pipes, which depends on the observation of the pitch and of the wave-length. The latter, however, is very difficult to determine with precision; and the difficulty of obtaining accurate numerical results is increased by the fact, that the quantity which is directly measured requires to be multiplied by a large number, in order to obtain the distance through which sound travels in a second, and hence the errors of observation, increased in the same proportion, are contained in the result. Accordingly this method has not yielded any accurate results.

The second indirect method is that of Clément and Desormes, subsequently put in practice also by Gay-Lussac and by Masson. In this method, as is well known, the magnitude directly furnished by experiment is the value of $\frac{c}{c_1}$ (where c and c_1 are the specific heats under constant pressure and constant volume), and the velocity of sound is calculated from this by means of the well-known formula. This method is susceptible of greater accuracy, and in the hands of Masson it appears actually to have led to good results. But even these determinations are incapable of yielding a perfectly exact value, inasmuch as, so far as I am aware, the full details of the experiments have not been published.

Hence there remain for us to consider only the direct methods.

The method that has been most generally adopted consists in determining the time in which sound traverses a given distance. At one station a signal is given, usually by means of a cannon, and at the other station the time is noted which elapses between the flash of the powder and the arrival of the sound.

Another method proposed by Dr. Bosscha† is probably susceptible of great accuracy, but hitherto it has not been tried upon a large scale.

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* Translated from Poggendorff's *Annalen*, vol. cxxiv. p. 453 (1865, No. 3), by G. C. Foster, B.A.

† Poggendorff's *Annalen*, vol. xcii. p. 485.

We need, therefore, examine closely only the second method.

Of all the experiments made by this method, those instituted by Moll and Van Beek in Holland, in 1823, are unquestionably the most accurate. The results obtained by the French Academicians in the year 1822, between Monthléry and Villejuif, are not at all to be compared with them. On the first night (that of the 21st of June, 1822) twelve shots were heard at Monthléry and only seven at Villejuif, and on the following night only one shot was heard at Villejuif, wherewith the experiments were ended.

The signals moreover were not made simultaneously at the two stations, but every five minutes a shot was fired at one or other of the stations. It was thus impossible to apply the proper correction for the influence of the wind; this can only be done when the cannons are fired simultaneously at both stations.

The velocity found was 331·2 metres per second, which does not differ much from the result of the later experiments. Nevertheless this determination has much less weight than that of Moll and Van Beek. Since this time no similar experiments have been carried out on so extended a scale.

Parry and Ross in the north-polar regions, and Bravais and Martins on the Faulhorn in Switzerland, obtained tolerably concordant results. Those of the former, however, are not very accurate, and there are at least some corrections which cannot be applied with perfect precision to the latter.

Hence we may confine ourselves to the consideration of Moll and Van Beek's determinations. Although their experiments were performed with the utmost care, the result which they arrived at cannot be accepted, in consequence of the coefficient of reduction not having been accurately known at that time, and of their having, in addition, neglected some corrections in their calculations. Considering, however, the accuracy of the experiments, it appeared to me worth while to repeat the calculations with as much accuracy as possible.

I was nevertheless convinced beforehand that the value of $\frac{c}{c_1}$, and consequently that of the mechanical equivalent of heat, could not be accurately determined by means of the velocity of sound thus ascertained. In support of this opinion, the following consideration may be urged. It is not improbable *à priori* that these intense air-waves travel quicker than weaker waves; and the statement of Ross, that in high northern latitudes, in the experiments on the velocity of sound, the report of the gun was heard at the further station before the command to fire, appears to confirm this supposition. If this be the case, determinations made by firing a cannon would give a greater velocity than such as were made by means of less intense waves of air, and hence for different intensities we should find different values for $\frac{c}{c_1}$, and the accurate determination of the mechanical equivalent of heat would be impossible. Experimentally this has not been made out; experiments upon this point have never been instituted; for the observation that a melody can be recognized equally well near or at a considerable distance only proves that the effect in question is very slight, since in the first place the distance, and secondly the difference of intensity of the various tones, can never be very great.

Accordingly, if I venture in the following pages to calculate the amount of this effect with greater exactitude, I must at once state that I by no means undertake to give a complete theory of sound. It may be true that the theory of elasticity leads to different fundamental formulæ; but just as, in experimental investigations which yield only approximate values of the magnitude under examination, the influence of one factor may often be accurately ascertained by means of comparative experiments, so I believe that the influence of intensity as calculated below would be found to be the same, even if the fundamental formulæ for the propagation of a wave-motion in air were differently developed.

Theoretical Development.

We have in general, for the velocity of propagation of longitudinal waves, the formula $s=\sqrt{\frac{e}{d}}$, where s is the velocity, e the force with which the molecules attract each other when the distance between has been doubled, and d the density of the medium. Hence if a particle is displaced in the direction of two molecules by the amount δ, the force tending to bring it back is $=e\delta$.

In the case of air, $e=\mathrm{H}\beta$, and $d=\frac{b}{g}$, if H denotes the height of the barometer, g the constant force of gravity, and β and b the specific gravities of mercury and air respectively, and the above formula consequently becomes $s=\sqrt{\frac{g\mathrm{H}\beta}{b}}$.

This formula then would express the velocity of waves of air if it were not for the development of heat by the condensation in the crest of the wave, and the absorption of heat in the trough of the wave; in the first case the elasticity, or the height of the column by whose pressure it is measured, is increased, and in the second case it is diminished; both ways, therefore, the difference of pressure, which is the cause of the advance of the wave, is increased.

Accordingly, for a displacement represented by δ the force is not $e\delta$, but it is this quantity increased by $e\delta k$, where k is a quantity as yet unknown. The force is therefore $=e\delta(1+k)$, and the above formula becomes*

$$s=\sqrt{\frac{e}{d}(1+k)}=\sqrt{\frac{g\beta \mathrm{H}}{b}(1+k)}. \quad . \quad . \quad . \quad (1)$$

The theory of heat gives the formula

$$\frac{p_1}{p_0}=\left(\frac{\mathrm{V}_0}{\mathrm{V}_1}\right)^\gamma=\left(\frac{\mathrm{T}_1}{\mathrm{T}_0}\right)^{\frac{\gamma}{\gamma-1}}$$

for the case of a mass of air under the pressure p_0, of the volume V_0, and at the temperature T_0, the latter reckoned from the absolute zero, being compressed until these quantities become respectively $=p_1$, V_1, and T_1, no heat being either communicated or withdrawn. γ is here $=\frac{c}{c_1}$. In the case of sound, the condensations take place so quickly that this formula is certainly applicable.

If H is the pressure read off upon the barometer, the pressure at the crest of the wave will be greater.

Let the compression be denoted by $\Delta\mathrm{V}=\mathrm{V}_0-\mathrm{V}_1$, we then have

$$h_1=\mathrm{H}\left(\frac{\mathrm{V}_0}{\mathrm{V}_1}\right)^\gamma$$

as the expression for the height of the barometer at the crest of the wave.

This, however, is not the pressure which must be taken for the calculation. The formula $\sqrt{\frac{g\mathrm{H}\beta}{b}}$ would be correct if the condensation took place without development of heat; but even then the pressure at the wave-crest would not be $=\mathrm{H}$, but $=h_2=\mathrm{H}\frac{\mathrm{V}_0}{\mathrm{V}_1}$, in accordance with Mariotte's law.

Hence the additional pressure due to the evolution of heat is

$$=h_1-h_2=\mathrm{H}\left\{\left(\frac{\mathrm{V}_0}{\mathrm{V}_1}\right)^\gamma-\frac{\mathrm{V}_0}{\mathrm{V}_1}\right\};$$

and this pressure is equal to $e\delta k$ or $\mathrm{H}\delta k$, which also denotes the increase of pressure caused by the heat.

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* Up to this point I have followed the statement of the theory of wave-motion given by Dr. Wüllner, in his *Experimental-Physik*, vol. i. pt. 1. pp. 386 *et seq.* and 470.

We have therefore

$$k=\frac{\left(\frac{V_0}{V_1}\right)^{\gamma}-\frac{V_0}{V_1}}{\delta},$$

where δ is the quantity by which the molecules approach, expressed in terms of their original distance, and is therefore $=\frac{V_0-V_1}{V_0}$. Substituting this value in the above equation, we get

$$k=\frac{\left\{1-\left(1-\frac{V_1}{V_0}\right)\right\}^{-\gamma}-\frac{V_0}{V_1}}{\frac{V_0-V_1}{V_0}}=\frac{\left(1-\frac{\Delta V}{V_0}\right)^{-\gamma}-\left(1-\frac{\Delta V}{V_0}\right)^{-1}}{\frac{\Delta V}{V_0}}$$

$$=\frac{1+\gamma\frac{\Delta V}{V_0}+\frac{\gamma(\gamma+1)}{1.2}\left(\frac{\Delta V}{V_0}\right)^2\ldots-\left(1+\frac{\Delta V}{V_0}+\left(\frac{\Delta V}{V_0}\right)^2+\ldots\right)}{\frac{\Delta V}{V_0}}$$

$$=\gamma+\frac{\gamma(\gamma+1)}{1.2}\frac{\Delta V}{V_0}\ldots-1-\frac{\Delta V}{V_0}.$$

Neglecting the higher powers of $\frac{\Delta V}{V_0}$, this gives

$$k=\gamma-1+\left\{\frac{\gamma(\gamma+1)}{2}-1\right\}\frac{\Delta V}{V_0},$$

and hence

$$1+k=\gamma+\left\{\frac{\gamma(\gamma+1)}{2}-1\right\}\frac{\Delta V}{V_0};$$

and by substituting this value for $1+k$ in equation (1),

$$s=\sqrt{\frac{gH\beta}{b}\left\{\gamma+\left[\frac{\gamma(\gamma+1)}{2}-1\right]\frac{\Delta V}{V_0}\right\}};$$

or, by binomial development of the second factor,

$$s=\sqrt{\frac{gH\beta}{b}}\left\{1+\frac{\gamma(\gamma+1)-2}{4\sqrt{\gamma}}\cdot\frac{\Delta V}{V_0}\right\}. \quad . \quad (2)$$

If we neglect ΔV, we obtain the common formula, which is therefore to be regarded as the first term of the series.

In deducing this formula, we have supposed that the crest of the advancing wave was foremost; but there is no difficulty in seeing that the formula still remains applicable if the trough of the wave goes first. The propagation of the wave is the propagation of the motion of the particles of the air; and this is occasioned by a difference of pressure, in the one case, between the wave-crest and the surrounding air, and in the other between the air and the trough of the wave. And since it follows from the formula that these differences are equal to each other, the two cases are identical.

Accordingly the velocity varies with the intensity.

When sound is propagated in a tube, the intensity remains nearly unchanged, and is then given by formula (2).

If, on the contrary, the wave spreads out in space, the intensity diminishes, and the velocity is consequently different every instant. The time in which the wave will traverse a given distance is easily found in the following manner.

Let the radius of the sound-wave, which in this case is a sphere, be R, when the amount of condensation is represented by $\frac{\Delta V}{V_0}$; we have then to determine the time which must elapse before the wave has attained the radius r, that is to say, the time in which it traverses the distance $r-R$. The work done in the condensation, or the energy of the wave, is $=4R^2\pi p\Delta V$, if p is the pressure measured by the barometer.

The energy of the wave when the radius is ρ is $=4\rho^2\pi p dV$; and since the energy of every wave is constant, we have

$$4R^2\pi p\Delta V=4\rho^2\pi p dV,$$

$$dV=\frac{R^2}{\rho^2}\Delta V;$$

and hence equation (2) becomes

$$s=\sqrt{\frac{gH\beta}{b}\gamma}\left\{1+\frac{\gamma(\gamma+1)-2}{4\sqrt{\gamma}}\cdot\frac{\Delta V R^2}{V_0\rho^2}\right\},$$

where ΔV is the condensation in the sound-wave R, and ρ alone is variable.

If, for the sake of simplicity, we put

$$\sqrt{\frac{gH\beta}{b}\gamma}=\mu, \text{ and } \frac{\gamma(\gamma+1)-2}{4\sqrt{\gamma}}\cdot\frac{\Delta V}{V_0}=\lambda^2,$$

we get

$$s=\mu\left(1+\lambda^2\frac{R^2}{\rho^2}\right).$$

From the equation

$$\frac{d\rho}{dt}=\mu\left(1+\lambda^2\frac{R^2}{\rho^2}\right),$$

we can then get t by integration,

$$t=\int_{\mathrm{R}}^{r}\frac{d\rho}{\mu\left(1+\lambda^2\frac{\mathrm{R}^2}{\rho^2}\right)}=\frac{r-\mathrm{R}}{\mu}-\frac{\lambda\mathrm{R}}{\mu}bg\tan\frac{\lambda(r-\mathrm{R})}{\lambda^2\mathrm{R}+r}.$$

Hence the time required by sound to traverse the distance $r-\mathrm{R}$, when it is propagated in a tube, is

$$t_1=\frac{r-\mathrm{R}}{\mu(1+\lambda^2)},$$

and when it is propagated in space,

$$t_2=\frac{r-\mathrm{R}}{\mu}-\frac{\lambda\mathrm{R}}{\mu}bg\tan\frac{\lambda(r-\mathrm{R})}{\lambda^2\mathrm{R}+r}.$$

Let us now test this formula by applying it to a particular case.

For $\mu=\sqrt{\frac{g\mathrm{H}\beta}{b}\gamma}$, I will adopt the mean value of 331 metres deduced from the experiments of Wertheim and of Masson; this value is arrived at by means of organ-pipes, in which the intensity of the sound is not great. Hence we get, by means of the ordinary formula, $\gamma=1{\cdot}398$, and

$$\lambda^2=0{\cdot}2857\frac{\Delta\mathrm{V}}{\mathrm{V}_0}.$$

The value $\frac{\Delta\mathrm{V}}{\mathrm{V}_0}$ can only be found approximately; I have made the attempt which follows to apply the formula to Moll and Van Beek's experiments.

Following the statements of Regnault (*Cours de Chimie*, vol. ii. p. 291), I assume for the volume of the gases generated by the combustion of gunpowder, 329 times the bulk of the powder itself, at 0° C. and under a pressure of 0·760 metre of mercury; but as the temperature is in reality much higher than this, we may certainly assume that we have three times this volume. If, after Regnault, we take the specific gravity of gunpowder as 1, the volume of a kilogramme of the gases will be $=329\times0{\cdot}003=0{\cdot}987$ cubic metre. In the experiments above mentioned, the quantity of powder expended was always 3 kilogrammes, and consequently the volume of gases formed was 2·961 cubic metres.

The wave-length now remains to be determined also. As I had no means of ascertaining this directly, I adopted for the pitch of the report of a cannon the tone AAA, making $27\frac{1}{2}$ vibrations in a second, the lowest orchestral tone. The wave-length is thus $\frac{331}{27\cdot5}=12\cdot04$ metres. Now at the muzzle of the gun half a wave-crest is formed, as indicated in the annexed figure, the length of which is about 3 metres, and the volume of a sphere with this radius is 113·1 cubic metres. If 2·961 cubic metres of gas are evolved in this space, the condensation will be $=\frac{2\cdot961}{113\cdot1}=\frac{1}{38}$ nearly.

We have, further, $R=3$, r in Moll and Van Beek's experiments 17669 metres, $\lambda^2=0\cdot2857\times\frac{1}{38}=0\cdot00752$ and $\lambda=0\cdot0872$.

The formulæ above mentioned then give

$$t_1=52\cdot974 \text{ and } t_2=53\cdot371.$$

The second term is 0·0000687, and therefore negligible.

The result is therefore in this case that the difference in the time required by sound to traverse the given distance in a tube and in open space is 0·4 second, or that the distance through which it travels in this time is 132·4 metres.

It is interesting to observe that the second term in the expression for t_2 has an inappreciable value. Disregarding it, we have $t_2=\frac{r-R}{\mu}$, that is to say, the ordinary formula. Hence it follows that, in free space, the intensity of the report of a cannon-shot has no perceptible effect on its velocity of transmission.

I had at first supposed that the formula would have afforded an explanation of the observation of Ross, spoken of above. This, however, is not the case, for the difference of velocity is much too slight. On the contrary, this observation seems to be as yet entirely unexplained; and even Le Conte, in his Memoir on Sound (Phil. Mag. S. 4. vol. xxvii. p. 1) treats it as only a completely isolated observation which is not sufficiently established. In the *Fortschritte der Physik* (Berlin, 1860, p. 167) it is explained as a psychological illusion*.

The result we have arrived at of the unequal velocities does not in any way contradict Poisson's theory. It is true that he found the velocity to be independent of the intensity; but in his investigation he neglected the velocity of vibration, which is precisely the point in question, in order to be able to integrate the partial differential equations.

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* [See, however, Earnshaw, Phil. Mag. S. 4. vol. xx. p. 37, and vol. xxvii. p. 98.—Transl.]

Since, therefore, the effect of the intensity upon the velocity is quite imperceptible in the case of a cannon-shot, a peculiar interest attaches to a recalculation of the most exact experiments that have been made by this means, which, as we have already said, are those of Moll and Van Beek, in order to deduce hence the value of γ, and from that the mechanical equivalent of heat.

Calculation of Moll *and* Van Beek's *Experiments.*

These experiments were made on the 27th and 28th of June, 1823. The two stations were two hills, Kooltjesberg and Zevenboomen, near Amersfoort, 5 [German] miles from Amsterdam. Moll was at the former station, and Van Beek at the latter.

The signals were made by discharging cannon simultaneously at both stations. All the observations which were not simultaneous were excluded, so as to eliminate the influence of the wind.

The data of the observations are—

Day.	Number of determinations.	Velocity per second.	Barometer.	Thermometer.	Tension of aqueous vapour.
		m.	m.	° C.	m.
27	44	340·06	0·74475	11·16	0·00925307
28	28	339·34	0·74815	11·00	0·00840465

The barometer and thermometer were read continuously at both stations, and the above values are the means of all the readings. The observations of the barometer are corrected for capillarity and temperature.

The statement of the temperature for the 28th of June differs by 0·21 from that employed in the previous reduction of the results, in consequence of an error of calculation having been made on that occasion.

The velocity of sound in dry air, calculated from these results for 0° and a pressure of 0·76 metre, was—

27th of June	332·38
28th „	331·72
	332·05

As some corrections were neglected in calculating this result, and the constants employed in the reductions have since been determined with much greater accuracy by Regnault, we may here repeat the whole calculation. Moll and Van Beek found for the length of the basis 17669·28 metres, a value which was arrived at by triangulation founded upon Krayenhoff's measurements. I am, however, informed by my highly esteemed teacher Professor Van Rees of Utrecht, that by a recalculation, in which he applied the various equations of condition which had not previously been taken into account, he obtained the following results from the four combinations of triangles previously calculated by Moll:—

17669·510
17669·498
17669·510
17669·518

Mean . 17669·509 metres.

The direct determinations of time were made by means of clocks with conical pendulums indicating thirds. During the observations these were constantly compared with two chronometers, one of which was at each station.

Eighty-nine comparisons at the Zevenboomen station are recorded, and hence the value of one minute of mean time is calculated as $=69{\cdot}63$ revolutions of the pointer of the thirds-clock. The probable error of this mean was found to be 0·0036 or $\frac{0{\cdot}0036}{69{\cdot}63}=\frac{1}{23000}$* of its value.

Twenty-one comparisons at the Kooltjesberg station are recorded, according to which one minute was equivalent to 69·433 revolutions, with a probable error of 0·023 or $\frac{1}{3400}$ †.

The rate of the chronometers was determined on the preceding days, but not during the observations. The Zevenboomen chronometer indicated almost exactly mean time, but the other lost about 14 seconds daily. This retardation was not taken into account in the former reduction; its influence, however, is found to be quite imperceptible, since it amounts to only $\frac{14}{24\times 60}=\frac{1}{100}$th of a second per minute, which is about the time required by the velocity of transmission.

Hence we see also that a slight alteration in the daily rate would have no effect upon the result.

The belief that the influence of wind is eliminated by means of simultaneous discharges is erroneous. This is the case even when the direction of the wind coincides with that of the base-line, and it is so in a still greater degree when the directions are not the same.

This has been already pointed out in a paper by Professor van Rees (Garnier and Quetelet, *Correspondance math. et phys.*

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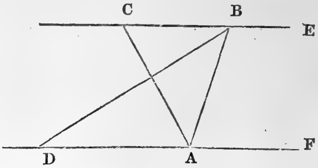
* [These two fractions do not agree. If the first is correctly given, the second should be $\frac{1}{19000}$.—Transl.]

† [$\frac{0{\cdot}023}{69{\cdot}433}=\frac{1}{3000}$.—Transl.]

1826, vol. ii. p. 22), where the effect of wind is calculated as follows:—A and B are the two stations, C E and D F indicate the direction of the wind.



Now let sound travel through still air from A to C in the time t, and in the same time let the particle of air C be carried by the wind to B; an observer at B will then hear the sound from A at the end of the time t; and in like manner an observer at A will hear the sound from B at the end of the time t', if during this time the sound reaches D, and the wind in the same time carries the particle D to A.

Let s be the velocity of sound in still air,
v the velocity of the wind,
$AB=\alpha$,
and $\angle BAF=\phi$.

Then $AC=st$, $CB=vt$, $BD=st'$, $AD=vt'$.

Now the triangles give us

$$s^2t^2=\alpha^2+v^2t^2-2\alpha vt\cos\phi, \quad \ldots\ldots \quad (1)$$

$$s^2t'^2=\alpha^2+v^2t'^2+2\alpha vt'\cos\phi; \quad \ldots\ldots \quad (2)$$

whence, eliminating s^2, we have

$$v=\frac{\alpha(t'-t)}{2tt'\cos\phi}; \quad \ldots\ldots \quad (3)$$

α, t, t', and ϕ are known quantities, and accordingly the velocity of the wind can be calculated from this equation. By substitution in (1) or (2), we then get

$$s=\alpha\sqrt{\frac{1}{tt'}+\frac{(t'-t)^2}{4t^2t'^2\cos^2\phi}}. \quad \ldots\ldots \quad (4)$$

All the observations have been recalculated by this formula.

The direction of the wind is recorded at both stations for every observation in divisions of the wind-rose. The azimuth of the base-line, in the direction from Zevenboomen to Kooltjesberg, was 32° 30′ 55″·8 West of North. The angle ϕ is accordingly known.

When there was any appreciable difference between the directions of the wind at the two stations, the mean direction was taken.

In this manner we find, for the 27th of June, the following determinations:—

Zeven-boomen.	Kooltjes-berg.	φ.	*s.*	Zeven-boomen.	Kooltjes-berg.	φ.	*s.*
52·90	51·17	32°½	52·02	52·99	51·07	32°½	52·01
·69	50·89	43¾	51·77	·90	·08	55	51·96
·71	·68	26½	51·67	·64	·28	59½	51·93
·92	·80	37½	51·83	·90	·21	59½	52·02
·84	·86	43¾	51·83	·87	·18	59½	51·99
53·04	·89	32½	51·93	·92	·33	59½	52·08
52·89	51·01	43¾	51·93	·91	·38	43¾	52·13
·79	·00	49½	51·87	·64	·35	55	51·96
·83	50·99	48½	51·89	·57	·32	55	51·93
·77	·96	32½	51·83	·90	·14	37½	52·01
·79	51·10	48½	51·92	·96	·01	43¾	51·96

The mean is 51·94 seconds, whence it follows that the velocity per second is 340·37 metres.

For the 28th of June we find—

Zeven-boomen.	Kooltjes-berg.	Mean.	Zevenboomen.	Kooltjes-berg.	Mean.
51·81	52·12	51·96	52·40	52·19	52·30
·94	·10	52·02	·27	·62	·44
·77	51·28	51·52	·27	51·66	51·96
·98	52·51	52·24	·23	·52	·88
52·17	·46	52·32	·49	·99	52·25
·15	·28	52·22	·56	·60	·08
·25	53·10	52·67			
52·18	50·17	51·17	Mean 52·18	51·97	52·07

Although the directions of the wind are given for this day also, it was not possible to apply the correction directly to each observation as on the 27th of June, since the force of the wind was not observed but calculated from the time-observations; hence the correction becomes altogether uncertain if the errors of observation are at all considerable. On the 28th of June this was the case, for on that day the observations were not so well made as on the previous one. The Zevenboomen determinations ought always to be greater than those made on the Kooltjesberg, in consequence of the direction of the wind, yet we often find the contrary to be the case, a result which can be due only to errors of observation. I have for this reason applied the correction to the mean, taking the mean direction of the wind. This gives 52·08 seconds, whence for the velocity per second we have 339·27 metres.

These two determinations have now to be reduced to what they would be for dry air at 0° C., and under 0·760 metre pressure.

The mean tension p of aqueous vapour was

0·00925307 on the 27th of June,

and

0·00840467 ,, 28th ,,

If we take for the density of aqueous vapour Gay-Lussac's value, namely five-eighths of that of the air, which nearly agrees with Regnault's determination, the formula $s_0=s\sqrt{\frac{h-\frac{3}{8}p}{h}}$ gives for the two determinations

339·57 and 338·55,

which we have still to correct for temperature and pressure.

From the formula $s=\sqrt{\frac{\mathrm{H}\beta g}{b}\cdot\frac{c}{c_1}}$, we get for the velocity of air corresponding to two different conditions, if we neglect the variations of $\frac{c}{c_1}$ as inconsiderable,

$$s:s_0=\sqrt{\frac{\mathrm{H}}{b}}:\sqrt{\frac{\mathrm{H}_0}{b_0}}=\sqrt{pv}:\sqrt{p_0v_0}=\sqrt{k_h^{\mathrm{T}}}:\sqrt{k_{760}^{0}\,\mathrm{T}_0},$$

H being directly proportional to the pressure p per square metre, b inversely proportional to the volume v, and $pv=k\mathrm{T}$, if T is the absolute temperature, and k a constant depending on the pressure and temperature, as has been shown in a previous communication*.

If we take as the mean value of H corrected for aqueous vapour the number 0·744, we get by the formula above given,

$$k_h^{4}=1{\cdot}00032.\quad k_h^{100}=1{\cdot}00065,\ \text{and hence}\ k_h^{11}=1{\cdot}0034;$$

$$k_{0\cdot76}^{h}=1{\cdot}00029,\ k_{0\cdot76}^{100}=1{\cdot}00065,\qquad ,,\qquad k_{0\cdot76}^{0}=1{\cdot}00028;$$

whereby the departures from the laws of Mariotte and Gay-Lussac are allowed for.

Placing the absolute zero at −273°·15 C., which is its most accurate value, calculated from the properties of hydrogen†, we have

For the 27th of June . . . T=284·31,
,, 28th ,, . . . T=284·15.

By substituting these values, we find for the velocity of sound,

27th of June . . . 332·82 metres.
28th ,, . . . 331·91 ,,
Difference . = 0·91

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* Pogg. *Ann.* vol. cxvi. p. 429. "On the departures from Mariotte's law in the real gases."

† Poggendorff's *Annalen*, vol. cxvi. p. 434.

In order to be able to compare approximately the values of these determinations, the probable errors of each are calculated. For the 27th of June this calculation is made from the column *s*, and for the 28th of June from the last column. The sum of the squares is thus found to be

27th June.	28th June.
2210,	18715;

whence it follows that the probable error of one observation in seconds is

0·069, 0·256;

and for the mean values,

0·0147, 0·0684.

This is the probable amount of the errors in the determinations of the velocity during 52 seconds; therefore upon one second they would be

0·00028, 0·0684*;

and in metres,

0·096, 0·442.

In consequence of this difference in the probable errors, we must not take the arithmetical mean of the two determinations, but must calculate the relative weight to be given to each. These are to each other as

$$\frac{1}{96^2}:\frac{1}{442^2}=20:1 \text{ nearly.}$$

Hence we have

		Probable error.	Value.
27th of June . . .	332·82	0·096	20,
28th „ . . .	331·91	0·442	1,

and this gives for the velocity of sound in dry air at 0° C., and under 0·760 metre pressure,

332·77 ± 0·093 metre.

The result is accordingly 0·72 metre higher than was given by the previous calculation.

In one respect the probable error that has been found is still a little too great. During each day's observations the temperature and pressure were not constant, but slightly variable. If we take the mean of all the readings, the final result remains the

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* [Should apparently be 0·00132.—Transl.]

same, but the probable error would be somewhat smaller if each observation had first been reduced to the same temperature and pressure. But, on the other hand, this statement of the probable error does not include the errors of the constants employed in the reduction, nor a possible error in the rate of the clocks. The former, however, are taken from Regnault's determinations, and are known within much less than $\frac{1}{2000}$th of the truth, and the latter cause could not well occasion any larger error than this.

Still, though it is not quite certain that the true value lies within the limits indicated by the probable error, I am of opinion that the value we have found is certainly accurate within $\frac{1}{1000}$, and hence it can be asserted with confidence that the true value lies between 333·1 and 332·44.

There can be no question of controlling this value by theory, for the value of $\frac{c}{c_1}$ is not known with anything like the same accuracy.

Conversely, however, we can calculate the value of this function from the formula $s=\sqrt{\frac{H\beta g}{b}\cdot\frac{c}{c_1}}$.

$$\begin{aligned} s &= 332{\cdot}77 \\ H &= 0{\cdot}760, \\ \beta &= 13{\cdot}59593, \\ g &= 9{\cdot}8096, \\ b &= 0{\cdot}001293187. \end{aligned}$$

Hence we get

$$\frac{c}{c_1}=1{\cdot}4128 \pm 0{\cdot}0008,$$

taking account of the probable error on the velocity of sound.

If we again assume $\frac{1}{1000}$ to be the extreme limit of the error on the velocity of sound, we find that the value of $\frac{c}{c_1}$ must in any case lie between 1·4104 and 1·4152.

Strictly speaking, this value must still undergo a slight correction, for we have here assumed that the value of $\frac{c}{c_1}$ is the same for air and for aqueous vapour. The quantity of the latter at the time of these observations was about $\frac{1}{131}$ of that of the air. The value of $\frac{c}{c_1}$ for aqueous vapour is not known; but if we take the value 1·2 which Dulong found for easily condensible gases, such as sulphurous acid, we get a correction of 0·0026, and hence for dry air $\frac{c}{c_1}=1{\cdot}4144$. Considerable uncertainty, however, attaches to this correction.

Hence, then, we can calculate the mechanical equivalent of heat, a point to which I intend to return in a subsequent communication.

Zütphen, November 19, 1864.

VI. *The Thermic Equivalent of Magnesium.*
By Thomas Woods, *M.D.**

THE great amount of light produced by the combustion of magnesium would make it desirable, if there was no other reason, to know if a corresponding quantity of heat is evolved during the same process. I however felt interested in this matter, chiefly on account of the researches I made a few years ago "On the amount of Heat produced by the combination of several Metals with Oxygen," and the occurrence of these amounts in multiple proportions (see Phil. Mag. November 1852, and July 1856).

As a matter of curiosity in the history of magnesium, I must mention that at the time I speak of (in 1856) I wrote to a firm in London to know the cost of some of the rare metals I intended to experiment with, and the price fixed for magnesium was sixteen shillings for three grains!

The method I adopted to find the quantities of heat of oxidation of the metals, mentioned in the paper in this Magazine for November 1852, I used in the present instance also to ascertain that evolved by magnesium; that is, I dissolved it in dilute sulphuric acid, marked the rise of temperature of the fluid, and corrected the result by making the proper allowances for the heat absorbed by the decomposition of the water, and that evolved by the combination of the magnesia with the acid.

No description of the apparatus, or the several steps of the process, need be detailed, as examples are given in the papers referred to; and since then the same experiments (without acknowledgment) have been worked out by Favre and Silberman, and published in the *Annales de Chimie* for 1853.

It will suffice to state that the result of many experiments is that the quantity of heat developed by the combustion or oxidation of an equivalent of magnesium is exactly twice as much as that produced by the oxidation of an equivalent of zinc.

Zinc will raise the temperature of 1000 grains of water 9°·6 F. by the combustion of 4 grains, or 1 equivalent, oxygen =1.

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* Communicated by the Author.

Magnesium, by the combustion of 1½ grain, or 1 equivalent, will raise the temperature of 1000 grains of water 19°·2 F.

This is the greatest amount of heat produced by an equivalent of any substance with which we are acquainted.

Potassium and sodium, hitherto the largest heat-evolvers known, produce only a rise of 17°·5 F. in 1000 grains of water by the combustion of one equivalent, oxygen =1. Magnesium produces 19°·2 F.

The quantity of heat evolved by the combination of magnesium with *chlorine* is also greater than that of any other substance. By dissolving a metal in hydrochloric acid, the chlorine unites with the metal, and decomposition of the acid of course takes place, the hydrogen escaping. Taking, therefore, the rise of temperature caused by the dissolving, and adding the amount of heat absorbed by the decomposition, we get the heat actually generated by the combination of the chlorine with the metal.

In this way I find that when one equivalent of magnesium, oxygen =1, combines with chlorine there is heat produced sufficient to raise the temperature of 1000 grains of water 25°·2 F. One equivalent of zinc, by combining with chlorine, will evolve heat sufficient to raise the same quantity of water 11°·25 F., and potassium 22°·9 F.

Parsonstown, June 1865.

VII. *Note on the Projection of the Ellipsoid.*
By Professor Cayley, *F.R.S.*[*]

CONSIDER an ellipsoid, situate any way whatever in regard to the eye and the plane of the picture; the apparent contour of the ellipsoid is an ellipse, the intersection of the plane of the picture by the tangent cone having the eye for vertex; this cone touches the ellipsoid along a plane curve (the intersection of the ellipsoid by the polar plane of the eye), which may be called the contour section; and the apparent contour is thus the projection of the contour section. Consider any other plane section; the projection thereof has double contact (real or imaginary) with the projection of the contour section: the common tangents are the intersections with the plane of the picture of the tangent planes of the tangent cone which pass through the pole of the section; or, what is the same thing, they are the tangents to the projection of the contour section, or to the projection of the section, from the point which is the projection of the pole of the section. The projection of the pole lies in the line which is the projection of the diameter conjugate to the plane of

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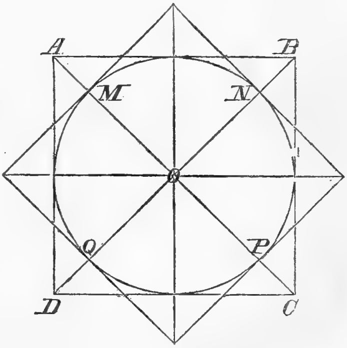
[*] Communicated by the Author.

the section; and in particular, if the section is central, that is, if the plane thereof passes through the centre of the ellipsoid, then the pole is the point at infinity on the conjugate diameter; whence also if the eye be at an infinite distance, so that the projection is a projection by parallel rays, then the projection of the pole is the point at infinity on the projection of the conjugate diameter; and therefore the common tangents of the projections of the section and the contour section are in this case parallel to the projection of the diameter conjugate to the plane of the section.

Suppose that the plane of the picture is parallel to a principal plane of the ellipsoid, and that the projection is by parallel rays; then if OA, OB, OC are the projections of the semiaxes (OA, OC will be at right angles to each other if the plane parallel to the plane of the picture is that of xz), the projections of the principal sections are the ellipses having for conjugate semidiameters OB, OC; OC, OA; OA, OB respectively. Hence to the ellipse OB, OC drawing the two tangents which are parallel to OA, to the ellipse OC, OA the two tangents which are parallel to OB, and to the ellipse OA, OB the two tangents which are parallel to OC, we have on each of these ellipses the two points which are the points of contact therewith of the ellipse which is the projection of the contour section, or apparent contour of the ellipsoid; that is, we know six points, and at each of these points the tangent, of the last-mentioned ellipse; and the ellipse in question, or apparent contour of the ellipsoid, can thus be traced by hand accurately enough for ordinary purposes.

In connexion with with what precedes, I may notice a convenient construction for the projection of a circle. Suppose that we have given the projection of the circumscribed square A B C D; then if we know the projection of one of the points M, N, P, Q, say of the point M, the projections of all the points and lines of the figure can be obtained graphically by the ruler only with the utmost facility; that is, in the ellipse which is the projection of the circle we have *eight* points, and the tangent at each of them, and the ellipse may then be drawn by hand. And to find the projection of the point M, it is only necessary to remark that in the figure the anharmonic ratio



$\frac{AM \,.\, OC}{AC \,.\, MO}$ of the points A, M, O, C is $=\frac{1}{2}(\sqrt{2}-1)$; hence the corresponding anharmonic ratio of the projections of the four points is also $=\frac{1}{2}(\sqrt{2}-1)$; and the projections of A, B, C, D, and consequently those of A, C, O, being known, the projection of M is thus also known.

Cambridge, June 15, 1865.

VIII. *On Numerical Relations of Gravity and Magnetism.* *By* Pliny Earle Chase, *M.A., S.P.A.S.**

In the fifth century before the Christian era, Leucippus and his disciple Democritus taught that heat is the soul of the world, the principle of life and intelligence, and that space is an infinite plenum, pervaded by material atoms too minute to be perceptible to the senses, which, by their constant motions, unions, and separations, form the beginnings and ends of things. In this theory, which is said to have been borrowed from the priests of Isis and Osiris, we may trace the origin of the modern belief in a universal kinetic æther, and of the attempts to resolve all forces into "modes of motion," which were practically inaugurated by our own countryman, Benjamin Thompson, Count Rumford, and which have been so successfully prosecuted by Carnot, Seguin, Mayer, Colding, Joule, Grove, and their collaborators.

The mutual convertibility of Light, Heat, Electricity, Magnetism, Chemical Affinity, and Vital Energy, may be now regarded as one of the most probable physical hypotheses. Faraday has endeavoured also to connect gravitation and magnetism, or electric action by experimental results, but in vain. Still the conviction of such a connexion is almost irresistible, and various physicists have given us incidental pointings in that direction. Ampère discovered the magnetic effect of electric currents circulating around iron bars; Arago, whose experiments were repeated and extended by Babbage, Herschel, Barlow, Christie, and others, showed that simple rotation produces magnetic disturbances which are governed by fixed laws; the distribution of induced magnetism in masses of iron, as determined by Barlow and Lecount, is the same as would follow from the relative centrifugal motions of different portions of the earth, provided the magnetic axis corresponded with the axis of rotation†; Hansteen suspected, and Sabine practically demon-

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* From Silliman's American Journal for May 1865. The Magellanic Gold Medal was awarded to the author for this memoir.

† This fact was first announced by me at the Society's meeting, April 15, 1864. See Proceedings of the American Philosophical Society, vol. ix. p. 367.

strated, the influence of the sun upon terrestrial magnetism; Secchi ascertained that "the diurnal excursion of the needle is the sum of two distinct excursions, of which the first depends solely on a horary angle, and the second depends, besides, on the sun's declination"*, and that "all the phenomena hitherto known of the diurnal magnetic variations may be explained by supposing that the sun acts upon the earth as a very powerful magnet at a great distance"†.

This hypothesis has been objected to, on the ground that it is difficult to understand how any conceivable intensity of solar magnetism, by its simple induction, could produce so great a disturbance as is daily observed. Therefore it will probably follow the fate of the earlier ones, which attributed terrestrial magnetism to one or more powerful magnets lying nearly in the line of the earth's axis, while Barlow's idea that the magnetism is superficial and in some manner induced‡, will still remain in the ascendant. Secchi's conclusions are, however, none the less interesting, and from the fact that magnetism is, like gravity, a central force, varying inversely as the square of the distance, they lend encouragement to those who are endeavouring to find new evidences of the unity of force.

My own experiments and researches have led me to the belief that all magnetism is a simple reaction against a force which disturbs molecular equilibrium, that the numerical equivalent of the magnetic force is therefore equal and opposite to that of the disturbing force, ($\pm M = \mp D$), and that all the phenomena of terrestrial magnetism result from tidal and thermal changes in terrestrial gravitation.

Sullivan§ and Reinsch‖ have pointed out the effect of musical vibrations upon the magnetic needle; and I have shown the controlling influence of a purely mechanical polarity¶. A careful examination of the polarizing thermal and rotation currents** will show that the spirals, which they have a tendency to produce, are quasi-horizontal cyclones—one set flowing in a nearly constant direction along the magnetic meridian, and the other toward the momentarily shifting solar meridian. From an investigation of these currents and a comparison of various observations, I have deduced the following theses:—

I. The daily magnetic variations, though subject to great disturbances at different hours, show an average approximation to the differences of the gravitation-tidal currents.

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* Phil. Mag. S. 4. vol. viii. p. 396.

† Ibid. vol. ix. p. 452.

‡ Phil. Trans. 1831.

§ See De la Rive's 'Electricity,' vol. ii. p. 635.

‖ Phil. Mag. S. 4. vol. xiii. p. 222.

¶ Proc. Amer. Phil. Soc. vol. ix. p. 359.

** Ibid. p. 367 *et seq.*

Hours from mean	1 h.	2 h.	3 h.
Means of theoretical ratios	·500	·866	1
Means of observed ratios	·563	·865	1

II. Marked indications of an accelerating force are discoverable in the magnetic fluctuations, especially during the hours when the sun is above the horizon.

Hours from mean	1 h.	2 h.	3 h.
Mean ratios of hourly tidal differences	100	73	27
Mean ratios of squares of hourly magnetic differences	100	74	26

See also Thesis V.

III. There are lunar-monthly barometric and magnetic tides, which may be explained by differences of weight or momentum*, occasioned by the combined influences of solar and lunar attraction, and terrestrial rotation.

IV. The solar-diurnal variations of magnetism between noon and midnight are nearly identical in amount with the variations of weight produced by solar attraction at the same hours.

The ratio of the solar to the terrestrial attraction for any particle at the earth's surface being directly as the mass, and inversely as the square of the distance ($M \div R^2 = 354{,}936 \div 23{,}000^2$), is ·00067. The weight of any particle is therefore increased by this proportionate amount at midnight, and diminished in the same proportion at noon, making a total half-daily variation of ·00134 in the atmospheric weight, and consequently, according to my theory, in the terrestrial magnetism.

Theoretical variation ·00134. Observed variation ·00138.

V. The magnetic variations at intermediate hours, between noon and midnight, indicate the influences of an accelerating force, like that of gravity, modified by fluctuations of temperature, and by atmospheric or æthereal currents.

Every particle of air may be regarded as a planet revolving about the sun, in an orbit that is disturbed by terrestrial attraction and other causes. In consequence of these disturbances, there is an alternate half-daily fall toward the sun, and rise from the sun. By the laws of uniformly accelerated and retarded motions, the mean fall and the consequent mean magnetic disturbances should occur at $12^h \div \sqrt{2} = 8^h\ 29'$ from midnight.

Theoretical mean $8^h\ 29'$. Observed mean $8^h\ 31'$.

VI. Some of the magnetic influences appear to be transmitted instantaneously, through the rapid pulsations of the kinetic

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* I believe there can be no weight without some degree of momentum. See Proc. Amer. Phil. Soc. vol. ix. p. 357.

æther; others gradually, through the comparatively sluggish vibrations of the air.

VII. The comparative barometric disturbances of the sun and moon exhibit an approximate mean proportionality between their comparative differential-tidal and magnetic disturbances.

Let the solar differential-tidal force be represented by A′, and the lunar by A″, the respective barometric disturbances by B′ and B″; and the magnetic disturbances by M′ and M″. If M′ and B″ are required, we have

	A′÷A″.	B′.	B″.	M′.	M″.
Theoretical values,	. .	. .	·00012	·00144	
Observed values,	2·55	·00057	·00013	·00140	·0000255

VIII. The theoretical gravitation-variation of magnetism (Prop. IV.) is slightly less, while the theoretical barometric variation (Prop. VII.) is slightly greater, than the corresponding observed variation. The excess in one case exactly counterbalances the deficiency in the other, the sum of the theoretical being precisely equal to the sum of the observed variations.

IX. The total daily magnetic variations, like the barometric, can be resolved into a variety of special tides, which may be severally explained by well-known constant or variable current-producing and weight-disturbing forces.

Hours from midnight.	A. Theoretical gavitation-tide.	B. Theoretical differential solar tide.	A+B. Theoretical mean tide.	Observed mean tide.
0	−·00067	+·00024	−·00043	−·00043
6	·00000	−·00024	−·00024	−·00023½
12	+·00067	+·00024	+·00091	+·00095

The hours are counted from midnight, in each half-day.

Column A contains the hourly differences from mean weight, attributable to solar gravitation, with changed signs—diminution of weight being accompanied by increase of magnetism, and *vice versâ*.

The form of the tide in column B is evidently such as should be determined by solar action. The magnitude of the tide is estimated by comparing the relative amounts of motion down the diagonal and down the arc of a quadrant

$$\left(\cdot 00067 \times \left[1 - \left(\frac{\pi}{4} - \frac{1}{2}\right)\right] = \cdot 00048\right).$$

The mean-tidal difference [(·00067 − ·00048) ÷ 2] is very nearly equivalent to the average theoretical inertia-disturbance of weight.

The atmospheric inertia at St. Helena (regarding the fluctuations as uniform between successive hourly observations) produces retardations of 59′, 85′, 26′, and 31′, at 0^h, 6^h, 12^h, and 18^h respectively. The mean retardation is 50′, or $\frac{5}{72}$ of a half-day. The theoretical daily gravity-variation being ·00134, the average variation in $\frac{5}{72}$ of a half-day is ·00009$\frac{1}{36}$, the mean-tidal difference being ·00009$\frac{1}{2}$.

The consideration of the moon's disturbance of the atmospheric gravitation is complicated by the magnitude of its differential attraction, the position of the centre of gravity of the terrestrial system, the varying centrifugal force, and other circumstances involved in the lunar theory. Still there are indications, in the following synopsis, of the influence of gravity sufficiently striking to encourage a hope that our knowledge of the moon's perturbations may be improved by a thorough comparative study of the lunar-astronomical, atmospheric, and magnetic Tables.

Lunar-daily disturbances of Magnetic Force at St. Helena, in millionths of the total force.

Hours.	0.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.
Before lunar m.	+5	−1	+4	−2	−5	−5	−6	−3	−2	− 1	+14	+15	+16
After lunar m....	+5	−1	−5	−6	−7	−6	+1	+1	−2	+18	+25	+22	+16
Mean	+5	−1	−0·5	−4	−6	−5·5	−2·5	−1	−2	+ 8·5	+19·5	+18·5	+16
Rotation-tide....	0	0	±4·5	±2	±1	±5	∓3·5	∓2	0	∓ 9·5	∓ 5·5	∓ 3·5	0

The above Table shows that—

1. The moon's attractive force ($M \div R^2 = {·}016 \div 60^2 = {·}000004$) multiplied by the coefficient of its differential attraction (2·55) gives ·0000113, which is nearly the same as the mean meridional magnetic disturbance [(·000005 + ·000016) ÷ 2 = ·0000105].

2. The increase of magnetism at 12^h is nearly equivalent to the attractive force, multiplied by the square of the distance from the centre of gravity of the system, and divided by the square of the earth's radius

$$({·}000004 \times 7707^2 \div 3963^2 = {·}0000168).$$

3. There is a tendency to equality of disturbances on each side of the meridian at 1^h and 8^h, as in the solar magnetic tide.

4. The greatest disturbance occurs at the hours of 10^h and 11^h P.M., both in the solar and in the lunar tide.

5. There are some indications of an increase of gravity, and decrease of magnetic force when the tidal flow is toward the centre of gravity of the terrestrial system, and *vice versâ*.

6. The rotation-tide has the customary quarter-daily phases of alternate increase and diminution.

X. The phenomena of magnetic storms indicate the existence of controlling laws analogous to those which regulate the normal fluctuations. See Proceedings of the American Philosophical Society, October 21, 1864.

The foregoing comparisons have been based on General Sabine's discussions of the St. Helena records. It would be desirable, if it were possible, to confirm them by observations at other stations near the equator; but the need of such confirmation is in great measure obviated by the variety of ways in which I have shown the probable connexion of gravity and magnetism. At extra-tropical stations the rotation tide becomes so preponderating that it is difficult to trace the diminished gravitation- and differential-tides; still I shall look confidently to a fuller development of the theory of tidal action for future additional support to my views.

IX. *On Chemical Nomenclature. By* G. C. Foster, *B.A., Lecturer on Natural Philosophy in Anderson's University, Glasgow.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN, London, June 26, 1865.

I BEG to offer a few remarks on some observations contained in a paper "On Chemical Nomenclature," by my respected friend and teacher Professor Williamson, published in the June Number of your Magazine (vol. xxix. p. 464). With most of the suggestions contained in this paper, and in the Professor's previous communication to the Chemical Society on the same subject, I have already expressed complete agreement; I will therefore confine myself here to the only important point on which I have ventured to differ from him.

This is as to the correct use of the word "acid." Professor Williamson contends for the application of this word to such substances as those represented by the formulæ

$$SO^3,\ N^2O^5,\ (C^2H^3O)^2O,\ \&c.,$$

and alleges, in words which I have previously quoted (Phil. Mag. S. 4. vol. xxix. p. 262), that this was clearly the original sense of the term. My own impression being that the word acid was long in general use before any meaning was attached to it which it was possible to define with accuracy, I pointed out (*loc. cit.*) a few facts relating to the former use of this word which seemed to me inconsistent with the statement above alluded to; and also drew attention to its having been employed even by modern chemists, down to the time of Gerhardt and Laurent, not only for oxides such as SO^3, N^2O^5, &c., but for hydrogen-salts such as

HCl, H^2SO^4, &c. Hence I concluded that the original meaning of the term was anything but particularly clear, and that previously to the time of Laurent and Gerhardt it could not be said to have had a strictly scientific and logical sense at all.

Professor Williamson pays me the unexpected compliment of calling my discussion of what I conceive to be the original meaning of the word acid "an elaborate exposition"; and he so far agrees with the conclusion arrived at as to admit that, instead of that meaning having been particularly clear, I "might safely have called it 'particularly cloudy'."

This admission and the passage which immediately follows it, seem to indicate that, in speaking of the "original meaning," Professor Williamson did not intend to go further back than Berzelius. He says:—

"Every chemist knows that the great Berzelius epitomized the prevailing definition by saying that an acid is an electro-negative oxide, and a base is an electro-positive oxide. No definition is complete and perfect; but this definition is certainly clear," &c. I have no desire to put upon the word "original" a different interpretation from that intended by my distinguished friend when he employed it, and am therefore willing to accept it as referring to Berzelius's definition. I contend, however, that this definition does not completely express the sense in which the word was currently used; and maintain that the term acid has *always* been considered applicable to such compounds as those represented by the formulæ HCl, H^2SO^4, HNO^3, &c., whereas many chemists of high authority have objected to, and avoided, the application of it to such compounds as SO^3, N^2O^5, &c. It is admitted on all hands that this word cannot consistently be applied to both these classes; and if what is urged in the last sentence is true, it must, I think, be also admitted that, if we are to retain it for either class, usage is in favour of its application to the former.

Professor Williamson further urges that Gerhardt's definition of an acid as a salt of hydrogen involves the fallacy of arguing in a circle. In this, doubtless, there is some truth; but the objection does not lie to Gerhardt's definition alone. All the definitions of acids, bases, and salts that I have ever examined have appeared to me to amount to more or less direct statements that "*acids* are bodies which form salts by acting upon bases"; that "*bases* are bodies which form salts by acting upon acids"; and that "*salts* are bodies formed by the mutual action of acids and bases." The fact is, that acids, bases, and salts are so intimately related that no one class can be defined without reference to the other two; and I do not see that the above definitions would be much improved by reading "electro-negative" instead of "acids," or "electro-positive" instead of "bases"; for how are we to define *electro-negative*, except as the opposite of electro-positive, or *electro-positive*, except as the opposite of electro-negative? Gerhardt's definition of acids, taken alone, certainly does not tell us much more than most other definitions of chemical terms; but it seems to me to state accurately the *relation* in which acids stand to other salts.

I will allude to only one other portion of Professor Williamson's paper. One of the objections to the use of the term acid in Gerhardt's sense, which he brought forward in his communication to the Chemical Society, was that the action of the compound HNO^3 upon the compound HKO cannot be described as a combination of nitric acid with potash, without concealing the fact that what takes place is truly a double decomposition. In answer to this, I pointed out that, even with the definitions of acids and bases which Professor Williamson advocates, the action which takes place between these bodies must often be described as a double decomposition, and I instanced the action of water upon the compounds $(C^2H^3O)^2O$, $\left.\begin{matrix}C^2H^3O\\C^7H^5O\end{matrix}\right\}O$, and $(C^2H^3O)^2S$. I foresaw at the time the reply, that double decompositions, "in which the resulting molecules are less various than the original molecules," are to be regarded as processes of combination; and it was precisely for this reason that I adduced the second and third of the above instances, instead of being content with the first only.

I am, Gentlemen,
Your obedient Servant,
G. C. FOSTER.

X. *Proceedings of Learned Societies.*[*]

ROYAL SOCIETY.

[Continued from vol. xxix. p. 550.]

May 4, 1865.—Major General Sabine, President, in the Chair.

THE following communication was read:—

"On the Rate of Passage of Crystalloids into and out of the Vascular and Non-Vascular Textures of the Body." By Henry Bence Jones, A.M., M.D., F.R.S.

The paper is divided into five sections—

1st. On the method of analysis, and its delicacy.
2nd. Experiments on animals to which salts of lithium were given, upon the rate of their passage into the textures.
3rd. On the rate of the passage of lithium-salts out of the textures.
4th. Experiments on healthy persons, and on cases of cataract.
5th. On the presence of lithium in solid and liquid food.

1. Three methods of analysis were followed, according as much or little lithium was present: first, simply touching the substance with a red-hot platinum-wire; secondly, extracting the substance with water; thirdly, incinerating the substance and treating it with sulphuric acid, and exhausting with absolute alcohol. $\frac{1}{12,000,000}$ of a grain of chloride of lithium in distilled water could be detected, and $\frac{1}{6,000,000}$ to $\frac{1}{2,000,000}$ of chloride of lithium in urine.

2. *On Rate of Passage into the Textures through the Stomach.*

Even in a quarter of an hour three grains of chloride of lithium, given on an empty stomach, may diffuse into all the vascular textures, and into the cartilage of the hip-joint and the aqueous humour of the eye. In very young and very small guinea-pigs which have received the same quantity of lithium, in thirty or thirty-two minutes it may be found even in the lens; but in an old pig in this time it will have got no further than the aqueous humour. If the stomach be empty, in an hour the lithium may be very evident in the outer part of the lens, and very faintly traceable in the inner part; but if the stomach be full of food, the lithium does not in an hour reach the lens. Even in two hours and a half lithium may be more marked in the outer than in the inner part of the lens. In four hours the lithium may be in every part of the lens; but less evidence of its presence will be obtained there than from the aqueous humour. In eight hours, even, the centre of the lens may show less than the outer part. In twenty-six hours the diffusion had taken place equally throughout every part of the lens. If the lithium is injected under the skin, in ten minutes it may be found in the crystalline lens, and even in four minutes, after the injection of three grains of chloride, the lithium may be in the bile, urine, and aqueous humour of the eye.

3. *On the Rate of Passage out of the Textures.*

After two grains of chloride of lithium, in six hours the lithium was more distinct in the outer than in the inner part of the lens. In twenty-four hours no difference in the different parts of the lens was detectable. In forty-eight hours no difference was observed. In ninety-six hours no lithium was detectable in the lens or cartilage of the hip-joint. The urine showed lithium very distinctly even in one drop.

After one grain of chloride of lithium, in five hours and a half the lithium was more distinct in the outer than in the inner part of the lens. In twenty-four hours and a half there was no difference throughout the lens. In forty-eight hours the watery extract of the lens showed faint traces of lithium. In seventy-two hours and a half (three days) the alcoholic extract of the lens showed no lithium. The urine still showed lithium distinctly in one drop, and it continued to be found in the watery or alcoholic extract for twenty-one days.

After half a grain of chloride of lithium, in three hours and fifty minutes traces of lithium could be found in the lens, and for thirty-seven or thirty-eight days traces of lithium could be found in the urine.

After a quarter of a grain of chloride of lithium, in five hours and a quarter the aqueous humour showed lithium, and all the organs showed lithium, but none was in the lens. In another pig, in twenty-four hours all the organs showed less lithium, and none was found in the aqueous humour.

After a quarter of a grain, in five hours and thirty-five minutes lithium was distinct in the aqueous humour, and very faintly traceable in the lens; and after sixteen days the minutest traces of lithium could be detected in the lens, the liver, the kidney; but no trace could be found in the blood.

After three grains of chloride of lithium, in four hours lithium was in the hair of the belly, and for thirty-two days the urine showed lithium very distinctly. The thirty-third day after the lithium the lens was found to contain minute traces of lithium, and even after thirty-nine days the lithium was in the alcoholic extract of the urine.

With three grains of chloride of lithium, a young pig in half an hour had lithium in the watery extract of the lens. In the same time an old pig had no lithium in the lens.

With two grains, a young pig in six hours had lithium distinctly throughout the whole lens. An old pig in the same time had lithium in the outer part of the lens, but scarcely the minutest trace in the inner part of the lens.

4. *Experiments on Healthy Persons and on Cases of Cataract.*

Ten grains of carbonate of lithia, taken three or four hours after food by a man, require from five to ten minutes to pass from the stomach to the urine, and this quantity of lithia will continue to produce traces of lithium in the urine for from six to seven days.

Two grains of chloride or carbonate of lithia, taken shortly after food by a boy, gives no appearance in the urine until from ten to twenty minutes; and this quantity continues to pass out for five, seven, or eight days.

Experiments made by the ordinary mode of analysis showed that four grains of sulphate of protoxide of iron, taken by a man almost fasting, gave a trace in the urine in seven minutes. Seven grains gave distinct appearance in ten minutes; and in ten minutes and a half one grain of iodide of potassium, taken by the same man fasting, appeared in the urine in twelve minutes.

When no lithia had been taken, seven cataracts were examined most carefully, and only one showed an exceedingly feeble trace of lithium.

When twenty grains of carbonate of lithia were taken twenty-five minutes before the operation, the lens showed no lithium.

When twenty grains of carbonate of lithia were taken two hours and a half before the operation, the lens showed lithium in the watery cataract.

When twenty grains of carbonate of lithia were taken between four and five hours before the operation, the lens showed lithium in each particle.

When twenty grains of carbonate of lithia were taken seven hours before the operation, the lens showed lithium in each particle.

When twenty grains of carbonate of lithia were taken seven days before the operation, the lens showed not the slightest trace of lithium.

Twenty grains of carbonate of lithia, taken between six and thirty-six hours before death, showed the faintest indications of lithium in the lens. The cartilage showed lithium very distinctly.

Ten grains of carbonate of lithia, taken five hours and a half before death, gave only faint traces of lithium in the lens, but the cartilage showed lithium very distinctly.

5. *On the Presence of Lithium in Solid and Liquid Food.*

Potatoes showed traces of lithium once in five trials.
Apples showed traces of lithium thrice in four trials.
Carrots showed no lithium in two trials.
Bread showed traces of lithium thrice in three trials.
Cabbage ,, ,, twice in two trials.
Tea ,, ,, eight times in ten trials.
Coffee ,, ,, four times in five trials.
Port wine ,, ,, six times in six trials.
Sherry ,, ,, six times in six trials.
French wine ,, ,, four times in four trials.
Rhine wine ,, ,, eight times in eight trials.
Ale ,, ,, twice in three trials.
Porter ,, ,, twice in three trials.

Mutton, beef, and sheep's kidney showed no lithium: one kidney had a slight trace.

CONCLUSIONS.

1. *On the Rate of Passage of Solutions of Lithium into the Textures of Animals.*

Chloride of lithium taken into the stomach in quantities varying from one quarter of a grain to three grains, will pass into all the vascular parts of the body, and even into the non-vascular textures, in from one quarter of an hour to five hours and a half.

2. *On the Rate of Passage out of the Textures of Animals.*

Chloride of lithium passes out by the skin as well as by the urine; and thus the animals can redose themselves with chloride of lithium from the hair and feet, and prevent accurate observations. Hence probably chloride of lithium, in quantities varying from half a grain to three grains, will continue to pass out of the body for thirty-seven, thirty-eight, or thirty-nine days; and even after thirty-three days, traces may be found in the lens; but in three or four days no lithium may be detectable in the non-vascular textures.

3. In man, carbonate of lithia, when taken in five- or ten-grain doses, may appear in the urine in five to ten minutes if the stomach is empty, or twenty minutes if the stomach is full, and may continue to pass out for six, seven, or eight days.

In two hours and a half, traces may be in the crystalline lens, and in five or seven hours it may be present in every particle of the lens and in the cartilages. In thirty-six hours it may be very evident in the cartilages. And in seven days not the slightest trace may be detectable in the crystalline lens.

4. Though in the solid and liquid food infinitesimal quantities of lithium may enter the body, usually no proof of their presence in the organs or secretions can be obtained.

May 18.—Major-General Sabine, President, in the Chair.

The following communications were read:—

"On the Elasticity and Viscosity of Metals." By Prof. W. Thomson, LL.D., F.R.S., F.R.S.E.

Among the experimental exercises performed by students in the physical laboratory of the University of Glasgow, observations on the elasticity of metals have been continued during many years. Numerous questions of great interest, requiring more thorough and accurate investigation, have been suggested by these observations; and recently they have brought to light some very unexpected properties of metallic wires. The results stated in the present communication are, however, with one or two exceptions, due to the careful experimenting of Mr. Donald Macfarlane, official assistant to the Professor of Natural Philosophy, whose interested and skilful cooperation have been most valuable in almost everything I have been able to attempt in the way of experimental investigation.

The subject has naturally fallen into two divisions, Viscosity, and Moduli of Elasticity.

Viscosity.—By induction from a great variety of observed phenomena, we are compelled to conclude that no change of volume or of shape can be produced in any kind of matter without dissipation of energy. Even in dealing with the *absolutely perfect* elasticity of volume presented by every fluid, and possibly by some solids, as for instance homogeneous crystals, dissipation of energy is an inevitable result of every change of volume, because of the accompanying change of temperature, and consequent dissipation of heat by conduction or radiation. The same cause gives rise necessarily to some degree of dissipation in connexion with every change of shape of an elastic solid. But estimates founded on the thermodynamic theory of elastic solids, which I have given elsewhere*, have sufficed to prove that the loss of energy due to this cause is small in comparison with the whole loss of energy which I have observed in many cases of vibration. I have also found, by vibrating a spring alternately in air of ordinary pressure, and in the exhausted receiver of an air-pump, that there is an internal resistance to its motions immensely greater than the resistance of the air. The same conclusion is to be drawn from the observation made by Kupffer in his great work on the elasticity of metals, that his vibrating springs subsided much more rapidly in their vibrations than rigid pendulums supported on knife-edges. The subsidence of vibrations is probably more rapid in glass than in some of the most elastic metals, as copper, iron, silver, aluminium †; but it is much

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* "On the Thermo-elastic Properties of Solids," Quarterly Journal of Mathematics, April 1857.

† We have no evidence that the precious metals are more elastic than copper, iron, or brass. One of the new bronze pennies gives quite as clear a ring as a two-shilling silver piece tested in the usual manner.

more rapid than in glass, marvellously rapid indeed, in some metals (as for instance zinc) *, and in india rubber, and even in homogeneous jellies.

The *frictional resistance* against change of shape must in every solid be infinitely small when the change of shape is made at an infinitely slow rate, since, if it were finite for an infinitely slow change of shape, there would be infinite rigidity, which we may be sure does not exist in nature†. Hence there is in elastic solids a *molecular friction* which may be properly called *viscosity of solids*, because, as being an internal resistance to change of shape depending on the rapidity of the change, it must be classed with fluid molecular friction, which by general consent is called *viscosity of fluids*. But, at the same time, it ought to be remarked that the word viscosity, as used hitherto by the best writers, when solids or heterogeneous semisolid-semifluid masses are referred to, has not been distinctly applied to molecular friction, especially not to the molecular friction of a highly elastic solid within its limits of high elasticity, but has rather been employed to designate a property of slow continual yielding through very great, or altogether unlimited, extent of change of shape, under the action of continued stress. It is in this sense that Forbes, for instance, has used the word in stating that "Viscous Theory of Glacial Motion" which he demonstrated by his grand observations on glaciers. As, however, he, and many other writers after him, have used the words plasticity and plastic, both with reference to homogeneous solids (such as wax or pitch even though also brittle, soft metals, &c.), and to heterogeneous semisolid-semifluid masses (as mud, moist earth, mortar, glacial ice, &c.), to designate the property ‡ common to all those cases of experiencing, under continued stress, either quite continued and unlimited change of shape, or gradually very great change at a diminishing (asymptotic) rate through infinite time, and as the use of the term *plasticity* implies no more than does *viscosity* any physical theory or explanation of the property, the word viscosity is without inconvenience left available for the definition I propose.

To investigate the viscosity of metals, I have in the first place taken them in the form of round wires, and have chosen torsional vibrations, after the manner of Coulomb, for observation, as being much the easiest way to arrive at definite results. In every case

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* Torsional vibrations of a weight hung on a zinc wire subside so rapidly, that it has been found scarcely possible to count more than twenty of them in one case experimented on.

† Those who believe in the existence of indivisible, infinitely strong and infinitely rigid very small bodies (finite atoms!) may deny this.

‡ Some confusion of ideas on the part of writers who have professedly objected to Forbes's theory while really objecting only (and I believe groundlessly) to his usage of the word viscosity, might have been avoided if they had paused to consider that no one physical explanation can hold for those several cases, and that Forbes's theory is merely the proof by observation that glaciers have the property that mud (heterogeneous), mortar (heterogeneous), pitch (homogeneous), water (homogeneous), all have of changing shape indefinitely and continuously under the action of continued stress.

one end of the wire was attached to a rigid vibrator with sufficient firmness (thorough and smooth soldering I find to be always the best plan when the wire is thick enough); and the other to a fixed rigid body, from which the wire hangs, bearing the vibrator at its lower end. I arranged sets of observations to be made for the separate comparisons of the following classes:—

(*a*) The same wire with different vibrators of equal weights (to give equal stretching-tractions), but different moments of inertia (to test the relation between viscous resistances against motions with different velocities through the same range and under the same stress).

(*b*) The same wire with different vibrators of equal moments of inertia but unequal weights (to test the effect of different longitudinal tractions on the viscous resistance to torsion under circumstances similar in all other respects).

(*c*) The same wire and the same vibrator, but different initial ranges in successive experiments (to test an effect unexpectedly discovered, by which the subsidence of vibrations from any amplitude takes place at very different rates according to the immediately previous molecular condition, whether of quiescence or of recurring change of shape through a wider range).

(*d*) Two equal and similar wires, with equal and similar vibrators, one of them kept as continually as possible in a state of vibration, from day to day; the other kept at rest, except when vibrated in an experiment once a day (to test the effect of continued vibration on the viscosity of a metal).

Results.

(*a*) It was found that the loss of energy in a vibration through one range was greater the greater the velocity (within the limits of the experiments); but the difference between the losses at low and high speeds was *much less* than it would have been had the resistance been, as Stokes has proved it to be in fluid friction, approximately as the rapidity of the change of shape. The irregularities in the results of the experiments which up to this time I have made, seem to prove that much smaller vibrations (producing less absolute amounts of distortion in the parts of the wires most stressed) must be observed before any simple law of relation between molecular friction and velocity can be discovered.

(*b*) When the weight was increased, the viscosity was always at first much increased; but then day after day it gradually diminished and became as small in amount as it had been with the lighter weight. It has not yet been practicable to continue the experiments long enough in any case to find the limit to this variation.

(*c*) The vibration subsided in aluminium wires much more rapidly from amplitude 20 to amplitude 10, when the initial amplitude was 40, than when it was 20. Thus, with a certain aluminium wire, and vibrator No. 1 (time of vibration one way 1·757 second), in three trials the numbers of vibrations counted were—

	Vibrations.	Vibrations.	Vibrations.
Subsidence from 40 initial amplitude to 20	56	64	64
And from 20 (in course of the same experiments) to 10 ..	96	98	96

The same wire and same vibrator showed—

Subsidence from 20 initial amplitude to 10 (average of four trials)............ 112 vibrations.

Again the same wire with vibrator No. 2* (time of vibration one way 1·236), showed in two trials—

	Vibrations.	Vibrations.
Subsidence from 40 initial amplitude to 20	54	52
And continued from 20 to 10......	90	90

Again same wire and vibrator,—

From initial amplitude 20 to 10.. 103 vibrations (mean of eight trials).

This remarkable result suggested the question (*d*).

(*d*) Only one comparison was made. It showed in a wire which was kept vibrating nearly all day, from day to day, after several days, very much more molecular friction than in another kept quiescent except during each experiment. Thus two equal and similar pieces of wire were put up about the 26th of April, hanging with equal and similar lead weights, the tops and bottoms of the two wires being similarly fixed by soldering. No. 2 was more frequently vibrated than No. 1 for a few days at first, but no comparison of viscosities was made till May 15. Then

No. 1 subsided from 20 initial range to 10 in 97 vibrations.
No. 2, the same subsidence in 77 vibrations.

During the greater part of May 16 and 17, No. 2 was kept vibrating, and No. 1 quiescent, and late on May 17 experiments with the following results were made :—

							Time per vibration.
No. 1. Subsided from 20 to 10 after	99	vibrations in	237	seconds	......		2·4
" " "	98	"	235	"	......		2·4
" " "	98	"	235	"	......		2·4
No. 2. Subsided from 20 to 10 after	58	"	142	"	......		2·45
" " "	60	"	147	"	......		2·45
" " "	57	"	139	"	......		2·45
" " "	60	"	147	"	......		2·45

[Addition, May 27, since the reading of the paper.]—No. 1 has been kept at rest from May 17, while No. 2 has been kept oscillating more or less every day, till yesterday, May 26, when both were oscillated, with the following results :—

		Time per vibration.
No. 1. Subsided from 20 to 10 after	100 vibrations in 242 seconds...	2·42
" 2. " "	44 or 45 vibrations	2·495

Moduli of Elasticity.—A *modulus* of elasticity is the number by which the amount of any specified stress, or component of a stress, must be divided to find the strain, or any stated component of the strain, which it produces. Thus the cubic compressibility of water

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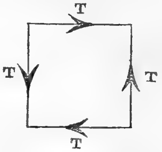
* Of same weight as No. 1, but different moment of inertia.

being $\frac{1}{21000}$ per atmosphere, its "modulus of compressibility" or its "volume modulus of elasticity," is 21000 atmospheres, or

$$76\times 13\cdot 596\times 21000=21\cdot 7\times 10^6$$

grammes weight per square centimetre (as 13·596 is the density or specific gravity* of mercury, and 76 centimetres the height of the barometric column corresponding to the pressure defined as "one atmosphere"). Or, again, Young's "modulus," which has generally been called simply *the* modulus of elasticity of a solid, is the longitudinal traction of a stretched rod or wire of the substance, divided by the extension produced by it. Or, lastly, the "modulus of rigidity," or, as it is conveniently called, simply "the rigidity" of an isotropic solid, is the amount of tangential stress divided by the deformation it produces,—the former being measured in units of force per unit of area applied, as shown in the diagram, to each of four faces of a cube, and the latter by the variation of each of the four right angles, reckoned in circular measure.



Measurements of Young's modulus have been made for many bodies by many experimenters; but hitherto there have been very few determinations of rigidity, notwithstanding the great ease with which this can be done for wires by Coulomb's method. Accordingly, although several accurate determinations of Young's modulus have been made upon wires of different substances hung in the College Tower of the University of Glasgow (which, by giving 80 feet of clear protected vertical space, affords great facilities for the investigation), I shall in this paper only refer to some of the results as bearing on the question, *how are moduli of elasticity affected in one substance by permanent changes in its molecular condition?* which was my starting-point for all I have attempted to do experimentally regarding the elasticity of solids.

To determine rigidities by torsional vibrations, taking advantage of an obvious but most valuable suggestion made to me by Dr. Joule,

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* The one great advantage of the French metrical system is, that the mass of the unit volume (1 centimetre) of water at its temperature of maximum density (3°·945 Cent.) is unity (1 gramme) to a sufficient degree of approximation for almost all practical purposes. Thus, according to this system, the density of a body and its specific gravity mean one and the same thing; whereas on the British no-system the density is expressed by a number found by multiplying the specific gravity by one number or another, according to the choice (of a cubic inch, cubic foot, cubic yard, or cubic mile) that is made for the unit of volume, and the weight of a grain, scruple, gun-maker's drachm, apothecary's drachm, ounce Troy, ounce avoirdupois, pound Troy, pound avoirdupois, stone (Imperial, Ayrshire, Lanarkshire, Dumbartonshire), stone for hay, stone for corn, quarter (of a hundredweight), quarter (of corn), hundredweight, or ton, that is chosen for unit of force. It is a remarkable phenomenon, belonging rather to moral and social than to physical science, that a people tending naturally to be regulated by common sense should voluntarily condemn themselves, as the British have so long done, to unnecessary hard labour in every action of common business or scientific work related to measurement, from which all the other nations of Europe have emancipated themselves. I have been informed, through the kindness of Professor W. H. Miller, of Cambridge, that he concludes, from a very trustworthy comparison of standards by Kupffer, of St. Petersburgh, that the weight of a cubic decimetre of water at temperature of maximum density is 1000·013 grammes.

I used as vibrator in each case a thin cylinder of sheet brass, turned true outside and inside (of which the radius of gyration must be, to a very close degree of approximation, the arithmetic mean of the radii of the outer and inner cylindrical surfaces), supported by a thin flat rectangular bar, of which the square of the radius of gyration is one-third of the square of the distance from the centre to the corners. The wire to be tested passed perpendicularly through a hole in the middle of the bar, and was there firmly soldered. The cylinder was tied to the horizontal bar by light silk threads, so as to hang with its axis vertical.

The following particulars show the dimensions of the vibrators of this kind which I have used.

Cylinders.	Outer diameter.	Inner diameter.	Mean radius.	Weight in grammes.	Moment of inertia round axis in gramme-centimetres.
No. 1	15·3 centims.	14·8 centims.	7·525	527·92	29894
„ 2	15·3 „	14·8 „	7·525	523·45	29641
„ 3	10·295 „	9·79 „	5·021	360·54	9089
„ 4	10·3 „	9·81 „	5·027	726·40	18357
„ 5	10·25 „	9·745 „	4·999	718·36	17952
„ 6	10·295 „	9·805 „	5·025	342·45	8647

	Length.	Breadth.	Weight.	Moment of inertia round axis through middle, perpendicular to length and breadth.
Bar 1	24·03 centims.	·965 centim.	38·955 grms.	1877·5
„ 2	24·11 „	·95 „	46·68 „	2255·5

Towards carrying out the chief object of the investigation, each wire, after having been suspended and stretched with just force enough to make it as nearly straight as was necessary for accuracy, was vibrated. Then it was stretched by hand (applied to the cross bar soldered to its lower end) and vibrated again, stretched again and vibrated again, and so till it broke. The results, as shown in the following Table, were most surprising.

Length of wire, in centimetres, l.	Volume, in cubic centimetres, V.	Density.	Moment of inertia of vibrator, Wk^2.	Time of vibration one way (or half-period), in seconds, T.	Rigidity, in grammes weight per square centimetre, $\frac{2\pi^3 l^3 W k2}{g T^2 V^2}$.	Substances.
60·3	1·1845	2·764	31771	1·14	241×10^6	Aluminium [a].
304·9	2·351	7·105	31896	4·31	$359{\cdot}6 \times 10^6$	Zinc [b].
237·7	......	......	......	4·76	$410{\cdot}3 \times 10^6$	Brass.
248·3	......	......	......	5·456	$354{\cdot}8 \times 10^6$	„

Remarks.

[a] Only forty vibrations from initial arc of convenient amplitude could be counted. Had been stretched considerably before this experiment.

[b] So viscous that only twenty vibrations could be counted. Broke in stretching.

TABLE (*continued*).

Length of wire, in centimetres, l.	Volume, in cubic centimetres, V.	Density.	Moment inertia of vibrator, Wk^2.	Time of vibration one way (or half-period), in seconds, T.	Rigidity, in grammes weight per square centimetre, $\frac{2\pi^3 l^3 Wk^2}{gT^2V^2}$.	Substances.
261·9	1·703	8·398	......	5·96	$350·1\times10^6$	Brass.
2435·0	15·30	8·91	38186	16·375	$448·7\times10^6$	Copper.
,,	,,	,,	61412	20·77	$448·4\times10^6$	,,
214·4	1·348	8·864	31771	5·015	$433·0\times10^6$	Copper [c].
,,	,,	,,	61412	6·982	$431·8\times10^6$	,,
143·7	·9096	8·674	......	3·381	$393·4\times10^6$	Copper [d].
286·8	......	......	20612	4·245	$442·9\times10^6$	Copper [e].
291	......	......	,,	4·375	$435·6\times10^6$	,,
293	......	......	,,	4·417	$436·2\times10^6$	,,
296·1	......	......	,,	4·500	$433·8\times10^6$	,,
300·0	......	......	,,	4·588	$434·0\times10^6$	,,
303·4	......	......	,,	4·646	$437·8\times10^6$	,,
309·3	......	......	,,	4·833	$428·6\times10^6$	,,
313·2	......	......	,,	4·931	$427·5\times10^6$	,,
317·4	1·962	8·835	,,	5·040	$425·9\times10^6$	,,
215·6	......	......	31771	8·155	$442·3\times10^6$	Copper [f].
235·5	......	......	,,	9·425	$432·2\times10^6$	,,
251·9	·827	8·872	,,	10·463	$428·6\times10^6$	,,
253·2	1·580	8·91	......	5·285	$472·9\times10^6$	Copper [g].
262·8	......	......	......	5·640	$464·3\times10^6$	,,
270·4	......	......	......	5·910	$460·4\times10^6$	,,
278·7	......	......	......	6·20	$458·5\times10^6$	,,
287·9	......	......	......	6·5325	$455·0\times10^6$	,,
297·5	......	......	......	6·8195	$451·0\times10^6$	,,
308·8	......	......	......	7·3075	$448·9\times10^6$	,,
256·5	1·6145	8·90	......	4·226	$463·5\times10^6$	Copper [h].
267·9	......	......	......	4·5625	$453·3\times10^6$	,,
280·1	......	......	......	4·915	$446·2\times10^6$	,,
292·2	......	......	......	5·240	$445·5\times10^6$	,,
301·9	......	......	......	5·532	$438·2\times10^6$	,,
316·8	......	......	......	6·655	$791·4\times10^6$	Soft iron [i].
322·1	......	......	......	6·88	$778·3\times10^6$	,,
335·1	......	......	......	7·301	$779·0\times10^6$	,,
347·4	......	......	......	7·768	$766·6\times10^6$	,,
366·0	1·357	7·657	......	8·455	$756·0\times10^6$	,,
39·4	·1745	20·805	20612	2·05	$622·25\times10^6$	Platinum [k].
65·9	·1825	19·8	10902	......	281×10^6	Gold [l].
75·7	·1185	10·21	10967	......	270×10^6	Silver [l].

Remarks.

[c] A piece of the preceding stretched.

[d] The preceding made red-hot in a crucible filled with powdered charcoal and allowed to cool slowly, became very brittle: a part of it with difficulty saved for the experiment.

[e] Another piece of the long (2435 centims.) wire; stretched by successive simple tractions.

[f] A finer-gauge copper wire; stretched by successive tractions.

[g] Old copper wire, softened by being heated to redness and plunged in water. A length of 260 centims. cut from this, suspended, and elongated by successive tractions.

[h] Another length of 260 centims. cut from the same and similarly treated.

[i] One piece, successively elongated by simple tractions till it broke.

[k] Not stretched yet for a second experiment.

[l] Added, May 27, after the reading of the paper.

Thus it appears that that specific rigidity which is concerned in torsion is very markedly diminished in copper, brass, and iron wire when the wire is elongated permanently by a simple longitudinal traction. When I first observed indications of this result, I suspected that the diminution in the torsional rigidity on the whole length of the wire might be due to inequalities in its normal section produced by the stretching. To test this, I cut the wire into several pieces after each series of experiments, and weighed the pieces separately. The result proved that in no case were there any such inequalities in the gauge of the wire in different parts as could possibly account for the diminution in the torsional rigidity of the whole, which was thus proved to be due to a real diminution in the specific rigidity of the substance. The following sets of weighings, for the cases of the wires of the two last series of experiments on copper, may suffice for example :—

Wire of 308·8 centims. long, cut into four pieces.

	Length, in centimetres.	Weight, in grammes.	Weight per centimetre, in grammes.
No. 1	109·2	5·023	·04600
„ 2	66·7	3·050	·04573
„ 3	63·2	2·865	·04533
„ 4	69·4	3·143	·04517
	308·5	14·081	

Wire of 301·9 when last vibrated; further elongated by about 8 centimetres, when it broke; then cut into five pieces in all.

	Length, in centimetres.	Weight, in grammes.	Weight per centimetre, in grammes.
No. 1	66·3	3·183	·04801
„ 2	66·4	3·083	·04643
„ 3	66·5	3·039	·04570
„ 4	66·8	3·072	·04599
„ 5	43·4	1·986	·04576

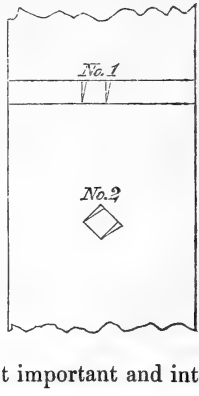
By several determinations of observations on the elongations within the limits of elasticity produced by hanging weights on long wires (about 80 feet) suspended in the College tower, it seemed that Young's modulus was not nearly so much (if at all sensibly) altered by the change of molecular condition so largely affecting the rigidity; but this question requires further investigation. The amount of the

Young's modulus thus found was, in grammes weight per square centimetre, 1159×10^6 for one copper wire, and 1153×10^6 for another which had been very differently treated.

The highest and lowest rigidities which I have found for copper (extracted from the preceding Table) are as follows:—

Highest rigidity, 473×10^6, being that of a wire which had been softened by heating it to redness and plunging it into water, and which was found to be of density 8·91. Lowest rigidity $393{\cdot}4 \times 10^6$, being that of a wire which had been rendered so brittle by heating it to redness surrounded by powdered charcoal in a crucible and letting it cool very slowly, that it could scarcely be touched without breaking it, and which had been found to be reduced in density by this process to as low as 8·674. The wires used were all commercial specimens—those of copper being all, or nearly all, cut from hanks supplied by the Gutta Percha Company, having been selected as of high electric conductivity, and of good mechanical quality, for submarine cables.

It ought to be remarked that the change of molecular condition produced by permanently stretching a wire or solid cylinder of metal is certainly a change from a condition which, if originally isotropic, becomes æolotropic* as to some qualities†, and that the changed conditions may therefore be presumed to be æolotropic as to elasticity. If so, the rigidities corresponding to the direct and diagonal distortions (indicated by No. 1 and No. 2 in the sketch) must in all probability become different from one another when a wire is permanently stretched, instead of being equal as they must be when its substance is isotropic. It becomes, therefore, a question of extreme interest to find whether rigidity No. 2 is not *increased* by this process, which, as is proved by the experiments above described, diminishes, to a very remarkable degree, the rigidity No. 1. The most obvious experiment, and indeed the only practicable experiment, adapted to answer this question, will require an accurate determination of the difference produced in the *volume* of a wire by applying and removing longitudinal traction within its limits of elasticity. With the requisite apparatus a most important and interesting investigation might thus be made.



"On Two New Forms of Heliotrope." By W. H. Miller, M.A., For. Sec. R.S., &c.

A heliotrope is a mirror O provided with some contrivance for adjusting it so that any given distant point T may receive the light

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[*] A term introduced to designate a substance which has varieties of property in various directions (Thomson and Tait's 'Natural Philosophy,' § 676).

[†] See, for example, a paper by the author, "On Electrodynamic Qualities of Metals," Philosophical Transactions, 1856.

of the sun S reflected from the surface of the mirror. This instrument has been constructed on three different principles. In Drummond's (Philosophical Transactions for 1826, p. 324), by a simple mechanism, a normal to the mirror is made to bisect the angle between the axes of two telescopes, one of which is pointed to T, and the other to S; consequently T will receive the light of S reflected from O. In Struve's (Breitengradmessung, p. 49) the mirror is directed by means of two sights attached to its support, which are brought into the line OT. The heliotrope employed in the Ordnance Survey (Ordnance Trigonometrical Survey of Great Britain and Ireland, Account of Observations and Calculations of the Principal Triangles, p. 47) is similar to Struve's, except that a single mark placed at a convenient distance in the line OT is substituted for the two sights. In the two heliotropes invented by Gauss (Astronomische Nachrichten, vol. v. p. 329, and v. Zach's Correspondance Astronomique, vol. v. p. 374, and vol. vi. p. 65), in Steinheil's (Schumacher's Jahrbuch für 1844, p. 12), and in Galton's an optical contrivance is connected with the mirror, so as to throw a cone of sunlight in a direction opposite to the cone of sunlight reflected from the surface of the mirror, the axes of the two cones being parallel, and either very nearly or absolutely coincident. Hence any point T, from which a portion of the former cone of light appears to proceed, will receive the light of the sun reflected from the mirror.

The heliotropes I am about to describe produce two cones of sunlight thrown in opposite directions, like those of Gauss, Steinheil, and Galton, but differ from them in having no moveable parts, and from all but Galton's, and the sextant-heliotrope of Gauss, with a second moveable mirror, in requiring no support except the hand of the operator.

One of these consists of a plane mirror, to an edge of which are attached two very small plane reflectors, *a*, *c*, forming with one another a reentrant angle of 90°, and making angles of 90° with the faces of the mirror. If a ray be reflected once by each of the two planes *a*, *c*, it is obvious that the first and last directions of the ray will be parallel to a plane containing the intersection of *a*, *c*, and will make equal angles with the intersection of *a*, *c*, which is also a normal to the face of the mirror. Therefore, if two parallel rays fall, one on the mirror, and one on either of the planes *a*, *c*, the direction of the ray reflected from the mirror will be parallel and opposite to that of a ray reflected once at each of the planes *a*, *c*. When the small reflectors are made of bits of unsilvered glass, the brightness of the image of the sun is so far reduced after the second reflexion, as not to interfere with the direct vision of T, and the mirror can be pointed without difficulty.

The other consists of a plate of glass having parallel faces *b*, *d*, with two polished plane faces *a*, *c* on its edges, making right angles with one another, and with the faces *b*, *d*, the face *d* being silvered, with the exception of a portion at the angle *adc* not larger than the pupil of the eye. It is easily seen that if a ray of light incident upon *b*, and refracted through *b* so as to be reflected internally once at each of the planes *a*, *c*, emerge through *d*, the planes of incidence and emergence will be parallel, and the incident and emergent rays will make equal angles with the edge *ac*, and therefore with a normal to the faces *b*, *d*. Hence the portion of the incident ray which is reflected from the mirror will proceed in a direction parallel and opposite to that portion of the ray which, after internal reflexion at *a* and *c*, emerges through *d*.

In order to ascertain that the construction of such an instrument presented no unforeseen difficulties, I requested Mr. T. E. Butters, of 4, Crescent, Belvedere Road, the well-known maker of sextant-mirrors and artificial horizons, to form the faces *a*, *c* on the edges of a piece of plate glass, and then had the face *d* coated with chemically reduced silver. Upon trial, the emergent light was found to be too bright; but, after smoking the angle *adc* in the flame of a candle, in order to reduce the intensity of the light, it became perfectly easy to make the centre of the image of the sun coincide with the object T seen by direct vision.

An image of the sun of suitable intensity for pointing might be obtained by attaching to the edge of the mirror a piece of tinted glass, of the form of the corner *abcd*, with the faces *b*, *d* parallel to the plane of the mirror.

XI. *Intelligence and Miscellaneous Articles.*

ON THE SPECTRAL RAYS OF THE PLANET SATURN.

LETTER FROM FATHER SECCHI TO M. ELIE DE BEAUMONT.

IN a communication addressed last year to the Academy, I announced that, in the spectrum of the planet Saturn, rays were observed different from those which are observed in the terrestrial atmosphere, and which are analogous to those of Jupiter. This year I have prosecuted these researches, and I have been able to confirm the accuracy of what I then stated, and to take more exact measurements.

Saturn seen through the spectroscope attached to Merz's telescope gives in the red a strong ray almost black, and which, when the air is calm, is perfectly so. This ray is the most easy to recognize. On the side of the extreme red the spectrum is weak and badly limited, but it permits a trace of another ray to be seen. Between the red and the yellow there is a pretty well defined band, which by its constitution suggests the band D of the telluro-atmospheric spectrum, but whose edges are better defined on the side of the yellow than on the side of the red. Beyond the yellow a trace is seen of the region δ of Brewster, which is nebulous in our atmosphere. Lastly, bands of the rays E, *b*, and F of Fraunhofer are distinguished; but they are more difficult to measure, and their position has not yet been sufficiently studied.

The most interesting part was the red zone. To assure myself of its position relatively to the terrestrial atmospheric bands, independently of the measurements of which I shall afterwards speak, I fixed in the evening the slit of the micrometer which I used in measuring Saturn's rays, and I left the instrument until morning; I then observed our atmosphere at the horizon. I constantly saw that the slit did not coincide with any of the telluric bands. The nearest was the atmospheric band marked C^6 by Brewster; but generally the band of Saturn remained near the middle, between C^6 and C.

To compare Saturn with Jupiter, I used the same method. Having fixed the micrometric slit on Saturn's ray, I waited until Jupiter rose as much as Saturn, and I then saw that the slit coincided sensibly with the black band of Jupiter which corresponds to the same part of the red. Yet Jupiter shows a brighter and more extended red, and he has towards the extreme red a pretty feeble band, at a distance of $1^r \cdot 85$ of my micrometer. In Saturn the band is really not seen sharply in this place; but very near it the light of the spectrum diminishes pretty perceptibly, and it appeared separated at $2^r \cdot 17$; beyond that there is only a very feeble light. Yet spite of the difficulty of taking these measures we can say that the bands are identical.

We have, then, here a new analogy common to the two largest stars of our planetary system, that is, an identity in their atmospheres.

To give an idea of the precision which we can hope to obtain in measurements taken with my instruments, and then to give starting-points for calculating the position of the planetary as compared with the solar rays, I shall adduce here the measures of the principal solar rays seen in our atmosphere at two different points in the horizon, between which the zero of the screw of the micrometer had been completely changed.

Solar Rays.

	B.	C.	C^6.	D.	*b*.	F.
	r.	r.	r.	r.	r.	r.
22nd May, morning.	3·95	5·24	6·17	7·94	14·97	19·83
24th " "	2·51	3·83	4·98	6·51	13·64	18·22
Difference	1·44	1·41	1·19	1·43	1·33	1·61

Excluding the ray C^6, which from its feebleness was uncertain on the 24th; the others give the mean 1·44, which shows the accordance of the partial measurements.

For Saturn we have obtained the following values:—

Rays of Saturn.

	γ in the red.	δ between the red and the yellow.	In the green.	Near *b*.	In the blue near F.
	r.	r.	r.	r.	r.
23rd May, evening.	4·34	6·29	10·55	13·19	18·01
21st May, „	5·94	7·86			
Difference	1·60	1·57			

From these Tables, compared together (the morning of the 24th must be compared with the evening of the 23rd for Saturn, and the morning of the 22nd with the evening of the 21st), we find for the sun

$$D-C=2^{r}\cdot65,$$

and for Saturn

$$\delta-\gamma=1^{r}\cdot93.$$

Supposing D to be identical with δ, which cannot be far from the truth, we get a difference of $0^{r}\cdot72$, which exceeds all possible errors of observation. The mean of several measures between γ and C^6 is $0^{r}\cdot51$. We are thus authorized to establish that this ray γ of Saturn does not coincide with any of the telluro-atmospheric bands which we know.

If we compare the differences between the results obtained for the sun and for Saturn relatively to other rays, we find a systematic difference of about $0^{r}\cdot22$, which, singularly enough, also obtains in the case of Jupiter. I do not know to what this is to be attributed, as the difference much exceeds probable errors. I had conjectured that it might be due to the manner of observing during the night; but this interpretation I greatly doubt. Hence new observations are needed to discover the reason, which doubtless is not in the instrument, as I at first suspected. But even allowing this uncertainty to subsist, we find that the numbers for the differences and the data of direct observation are too evident to be attributed to some error of diversity established by the ray γ.

Among the numerous coloured stars which I have examined, there is one which is remarkable: it is the red star placed after Struve's star No. 928 in right ascension $=6^{h}\cdot27$, and in declination $+38\cdot32$. Its colour is of a reddish violet. In the spectrometer it exhibits three luminous bands—one red, one yellow, and a sharply-defined green, and which are separated by dark divisions. The rest of the spectrum is scarcely visible, and is suspected rather than seen. This spectrum, so to speak, is an ordinary one cut in the middle. The analogy of its colour with that of the electric light in some of Geissler's tubes led me to examine in the spectroscope one of these tubes which gave a similar colour. I found that its spectrum also showed red, yellow, and green bands; but the blue and violet part was divided into a great number of equal and narrow bands. Leaving out these bands (which might be invisible in the star), the spectrum of the star strongly resembles that of the gas in the tube. I do not know what the tube contained; but I observed that the negative pole gave the sharp spectrum of carbon, and that there was a black powder near the electrodes.

These phenomena favour the opinion of those who think that part of the luminous and obscure bands of the celestial atmosphere is not an effect of absorption, but a real radiation, as in the planetary nebulæ.

The two coloured stars ν of Bootes have given me almost continuous spectra, but in which the proportion of colours was different, the red predominating in the red, and the blue in the blue. This proves that these colours are not the effect of an optical contrast, as many astronomers have supposed.—*Comptes Rendus*, June 5, 1865.

ON THE WAVE-LENGTH OF THE BLUE INDIUM-LINE.

BY. J. MÜLLER.

MM. F. Reich and Th. Richter of Freiberg having introduced into the colourless flame of a Bunsen's lamp impure chloride of zinc prepared from zinc-blende, and having examined the coloured flame thus produced by means of a prism, observed a *blue line* which had not previously been noticed. A closer examination showed that this blue line belonged to a hitherto unknown metal, to which its discoverers gave the name *Indium*, and concerning which they published further details in Erdmann and Werther's *Journal für praktische Chemie.*

Professor Reich has been kind enough to give me a small piece of *metallic indium*, as well as a small quantity of *sulphide of indium* (which exhibits the spectral line of this metal in the most lasting manner), in order that I might determine the wave-length of this line.

I have made this determination by the method and by means of the grating which were first described in the first Part of the third volume of the 'Reports of the Society for the Advancement of the Natural Sciences' of Freiburg in the Breisgau, page 29*, and I thus arrived at the following results:—

	°	′
Ind. α, 1, right	63	33
Ind. α, 1, left	52	6

whence $x=5^\circ\ 43'\cdot5$, and $\lambda=0\cdot0001995'''$;

Ind. α, 2, right	69	25
Ind. α, 2, left	56	25

whence we have $y=11^\circ\ 31'\cdot5$, and $\lambda=0\cdot0001998'''$.

The mean is therefore

$$\lambda=0\cdot00019965''',$$

or

$$\lambda=0\cdot000455 \text{ millim.}$$

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* [Also Phil. Mag. S. 4. vol. xxvi. p. 259.—Ed. Phil. Mag.]

When the light of an indium-flame is decomposed by the prism, it shows *two* blue lines, one of which, very intense, is situated close to the blue strontium-line, but a little further towards the violet end of the spectrum; the other line is still more refrangible, but so much fainter that it cannot be perceived at all in the spectrum produced by means of a grating.—Poggendorff's *Annalen*, vol. cxxiv. p. 637 (1865, No. 4). Communicated by the Author from the *Berichte über die Verhandlungen der Gesellschaft zur Beförderung der Naturwissenschaften zu Freiburg in Breisgau.*

ON SOME THERMO-ELEMENTS OF GREAT ELECTROMOTIVE FORCE.

BY. J. STEFAN.

In investigating the thermo-piles constructed by Marcus, some minerals which were just at hand were investigated as to their thermoelectric deportment at high temperatures. The method was as follows:—The mineral to be investigated was laid upon one end of a copper strip, the end of a wire laid on the mineral, and the whole pressed by means of a clamp. This wire, and one from the free end of the copper strip, lead to a galvanometer with great resistance. The copper strip is heated by a spirit-lamp flame. In order to join any two minerals to a thermo-element, a copper strip was laid between them, wires laid on their most distant sides, and the whole pressed by a wooden clamp. The free end of the copper strip was introduced into the flame, and served therefore only as conductor of heat to the place of contact.

In the following recapitulation of the elements, the electropositive body is always placed first. The number given signifies how many of the elements joined together furnish an electromotive force equal to that of a Daniell's element.

1.	Foliated copper pyrites	Copper	26
2.	Compact copper pyrites	Copper	9
3.	Pyrolusite	Copper	13
4.	Compact copper pyrites	Foliated copper pyrites	14
5.	Copper	Crystallized cobalt pyrites	26
6.	Granular cobalt pyrites	Copper	78
7.	Copper	Iron pyrites	15·7
8.	Compact copper pyrites	,,	6
9.	Foliated copper pyrites	,,	9·8
10.	Copper	Variegated copper ore	14
11.	Fine Bleischweif	Copper	9·8
12.	Coarse Bleischweif	Copper	9
13.	Galena in large crystals	Copper	9·8
14.	Bleischweif	Variegated copper ore	5·5

Copper pyrites and pyrolusite have been already investigated by Bunsen[*]. His experiments, made with picked specimens, give num-

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[*] Phil. Mag. S. 4. vol. xxix. p. 159. Poggendorff's *Annalen*, vol. cxxiii. p. 505.

bers which agree with those in 2 and 3. From 1, 2, and 4 the great influence of the structure on the thermo-electric deportment is evident; and this influence is seen still more strongly in 5 and 6. While crystallized cobalt pyrites is strongly negative to copper, amorphous is positive. The following phenomenon is still more striking. The galena mentioned in 13 consisted of a group of crystals; hexahedra combined with octahedra. A group of pure octahedra was found to be positive towards copper in some parts, and negative in others*.

The element 14 has, of all investigated, the greatest electromotive force. Of those investigated by Marcus, at the highest applicable temperatures, 18 go to a Daniell's. Yet the minerals investigated are bad conductors, which is a hindrance to an extended application of the elements adduced. But so much the more are the results available for the physics of the earth, on which account these investigations will be continued as soon as more copious material has been obtained.—Poggendorff's *Annalen*, April 1865.

ON THE TERRESTRIAL RAYS OF THE SOLAR SPECTRUM.

NOTE BY M. JANSSEN.

The memoir which I now submit to the judgment of the Academy contains an exposition and discussion of observations made during a recent journey to the Alps, of which the following is a summary.

On the Faulhorn I observed a general diminution of all the groups of telluric rays of the solar spectrum, a result arising from the altitude of the place, and which shows the telluric origin of these lines. On the contrary, I noticed that the lines of solar origin retained their intensity and even gained in sharpness. The observation of Mr. Glaisher, who after a recent ascent stated that he saw the rays of the solar spectrum diminish with the ascent of the balloon, appears to me to be in opposition to facts.

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* On this occasion it may be allowed to call to recollection the remarkable but also never yet completely published observations, of which Marbach, in the year 1857, has given a short notice in the *Comptes Rendus*, vol. xcv. p. 707. According to these, crystals both of iron pyrites (FeS^2) and of arsenical cobalt (CoS^2+CoAs^2) fall into two classes, not as regards their chemical composition or crystalline form, but as regards their thermo-electric deportment. Denoting them by α and β, Marbach gives the following series, counting from negative to positive:—Glance-cobalt and bismuth α; argentine, platinum, lead, copper, brass, silver, cadmium, iron, antimony, glance-cobalt β.

Becquerel has recently found that sulphuret of copper ($Cu^2 S$), formed by melting together sulphur and copper, forms with copper, which is negative to it, a thermo-pile, of which ten elements, with a difference in temperature of 300° to 400° C., have equal electromotive force to a Daniell's element. Yet it must have a fibrous structure; if in its preparation it is heated too strongly, or several times remelted, so that it forms a homogeneous mass, it has almost entirely lost its power.—*Comptes Rendus*, vol. lx. p. 313.

On high mountains, the rays of the solar spectrum which originate in our atmosphere experience, during the course of the day, variations of intensity far more marked than in the plain; on the Faulhorn I could recognize the telluric origin of important groups, the discrimination of which hitherto was dubious. These groups, which belong to the red end of the spectrum, are as follows:—

1. A portion at least of Fraunhofer's ray B, which is the least refrangible; the intensity of the ray forming the other portion of the spectrum does not enable one to state positively.

2. The groups between B and *a* consist almost exclusively of telluric rays.

3. The group *a* is telluric; that is, the red end of the spectrum from B to A is ruled by rays which almost all originate in the terrestrial atmosphere. The importance of the telluric phenomenon there is almost tenfold that of the solar phenomenon.

I may remark here that M. Kirchhoff's maps for the entire region from A to B exhibit no coincidence between the rays of the solar spectrum and those of metals studied by this eminent physicist. The discovery of the telluric origin of the groups of this region explains this circumstance, and confirms its exactitude.

If we cast a glance at the general distribution of the telluric groups in the solar spectrum, we see that these groups are so much more important and numerous as we consider the least refrangible portion of the solar spectrum; the exact opposite is the case for the rays of solar origin.

The memoir contains an account of an experiment made on the lake of Geneva, between Lyons and Geneva, and in which I could confirm the production of telluric groups in the spectrum of a flame which at a small distance presented none. This experiment shows directly the action of the elective absorption of our atmosphere.

In reference to my former communication on this subject, Father Secchi announced that he had observed an increased intensity in the telluric bands on cloudy days, or when the atmosphere is whitish and vapoury, or also, when the moon is viewed, veiled by the effect of vapours. Secchi concluded from this observation, referred to that of the spectra of the planets, the very probable existence of aqueous vapour in the atmosphere of these stars.

I observed on this occasion, that the results given by the analysis of the light of clouds were in general too complicated to elucidate a question of this kind. "When the atmosphere is slightly veiled by white clouds, a given point of the heavens sends to the eye a larger quantity of light than when the heavens are clear; and this light arises from multiple reflexions from the aqueous particles. Under these conditions the spectrum obtained is more luminous; moreover it consists of rays which, in consequence of their numerous reflexions, have traversed great thicknesses of the atmosphere. These two conditions perfectly explain the easier and more marked vision of the telluric bands which then take place. In this case the vapour of the cloud has only served as reflector to send to the instrument the rays which have traversed great thicknesses of the atmosphere;

but it would not be justifiable to attribute to this vapour itself the presence of telluric bands."

Hence it follows from all this, that aqueous vapour in that particular physical condition in which it constitutes clouds and atmospheric vapours, cannot be invoked as the cause of the telluric rays of the solar spectrum; and hence the conclusion which Secchi draws regarding the constitution of the planetary atmosphere cannot be considered to be well founded.

Later, in a communication on the spectrum of Jupiter, Secchi maintains his primary conclusion and says, "On this occasion I have confirmed anew the influence of fogs on the terrestrial atmospheric rays."

I have continued a long series of observations under the most diverse atmospherical circumstances, but taking care only to use the direct light of the sun, so as not to complicate the question by these circumstances of foreign reflexions, difficult to estimate, and which have thrown such a regrettable confusion into the observations of Father Secchi.

The whole of these observations have shown me that the vapour of water in the state of cloud or of atmospheric vapour did not appear to act*, but that it is aqueous vapour in the state of elastic fluid which has an important part in the production of the telluric rays of the solar spectrum.

For instance, on the 5th of July 1864, the weather being clear, pure, and hot, a telluric group measured on our scale had the intensity 15, the sun being $4^{\circ}\cdot30$ above the horizon; while on the 27th of December, 1864, for the same height of the sun, the weather equally clear but so dry that the dew-point was 8° below zero, the same group had no more than the intensity 4 on the same scale.

An experiment to verify this important point has recently been made at the central government establishment for the construction of lighthouses; it has given a confirmatory result; I expect to make it on a still more considerable scale, where the phenomenon can be studied as it deserves.

Two years ago, when publishing my first spectral studies on the earth's atmosphere, I expressed the opinion that this study would lead later to the knowledge of the atmospheres of planets. I have at present the pleasure of seeing that this expectation is being more and more realized; for apart from the above facts, the recent results obtained by Messrs. Huggins and Miller, who have seen new rays in the spectra of the planets, are a confirmation of these ideas.—*Comptes Rendus*, January 1865.

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* Solar light which had traversed a fog or a mist gave me telluric rays no more intense than when the sky was clear, with a dew-point just as high (other circumstances being the same). The dew-point was determined by means of Regnault's condensation hygrometer.

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XII. *On the different Properties of the Heat radiated by Rough and by Bright Surfaces. By* Professor G. Magnus*.

[With a Plate.]

SINCE Sir John Leslie's researches upon heat, it has been known that metals radiate more heat when their surface is rough than when it is bright; but this remarkable phenomenon has not, upon the whole, been much studied, and many questions connected with it remain still unanswered. Melloni and his successors have examined the heat given out by various bodies at different temperatures; but the differences in the radiation of one and the same body at a constant temperature, but with different surfaces, have hitherto been very little investigated. The first question to be answered in relation to these differences, was whether the points which a rough surface presents are the cause of the increased radiation. Melloni† and Knoblauch‡ have shown that this is not the case, but that increase of radiation depends rather upon the diminution of density which the surface undergoes when it is roughened in a certain manner; and I have myself§, as well as others before me, pointed out that the increased radiation is to be attributed to the state of greater subdivision of the roughened surface, as well as to its diminished density. Beyond this, scarcely anything is known about the differences of radiation.

Does the more abundant radiation which takes place from

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* From Poggendorff's *Annalen*, vol. cxxiv. p. 476 (1865, No. 3).

† *La Thermochrôse*, p. 90, note.

‡ Poggendorff's *Annalen*, vol. lxx. p. 340. [Taylor's Scientific Memoirs, vol. v. p. 214.]

§ *Monatsber. d. Berl. Akad.* 1864, p. 593. [Phil. Mag. Ser. 4. vol. xxix. p. 58.]

rough surfaces depend upon the fact that in them the points from which heat is radiated are more numerous? or is the intensity of the individual rays greater? In other words, are the amplitudes of the oscillating particles of rough surfaces greater? Are the rays which are given out by the two kinds of surfaces of the same, or of different heat-colours? These are questions that have not hitherto been proposed, doubtless because their solution presented difficulties which, for the time being, it was impossible to overcome.

The observation which I communicated to the Academy of Sciences at Berlin in August last, according to which a platinum wire, heated in the flame of a Bunsen's burner, radiates nearly twice as much heat when it is covered with spongy platinum as it does when the surface is bright, indicated the possibility of answering some of these questions. The following considerations served as the starting-point for this investigation.

Transmission through various Media of the Heat radiated by bright and by roughened Platinum.

If the rays given out by the two surfaces, the roughened and the bright platinum, were of the same wave-length and of equal intensity, the greater heating-effect produced by the rough surface could be caused only by there being in it more particles by which heat was given out. In this case, if the rays were transmitted through various media the quantity absorbed would remain always in the same proportion. On the other hand, if the rough surface sent out rays of a different wave-length from those emitted by the bright one, we might assume that among the different media through which the rays were caused to pass, some would be found which would absorb these new rays in greater proportion than the others. The question thus came to be, to examine and compare together the phenomena attending the transmission through various media of the rays emitted by the two kinds of surfaces.

It was necessary for these experiments that the two surfaces employed should be of exactly the same extent. In order to ensure this, two circular plates of 10 millims. diameter were cut, by means of a hollow punch, from the same piece of platinum-foil, the thickness of which was 0·3 millim. Each of these plates was fastened by three very fine platinum wires to a support, constructed expressly for the purpose, by means of which they could be brought, one after the other, exactly into the same part of the flame of a Bunsen's gas-burner. The flame of this burner, which was supplied with gas through a regulator, was very constant. The platinum disk heated by it was placed parallel to the front surface of the thermo-pile, and so that its centre was exactly in a straight line with the axis of the pile. The distance of the platinum plate from the pile was 280 millims. Between them was placed a large screen with a diaphragm, through which the rays arrived at the thermo-pile. The opening in the screen had the same size as the radiating disk. In order that none but perpendicular rays might fall upon the pile, a second screen was placed at a short distance from it, with an opening of the same size as the surface of the pile. With this arrangement, the conical reflector with which thermo-piles are usually provided, would not have caused any increase in the effect. Indeed this was not necessary, in consequence of the extraordinary sensitiveness of the galvanometer employed. The pile itself was placed in a large cardboard box, which had no opening except that of the diaphragm just mentioned. Its front surface formed a square, the side of which, 13 millims. long, was but little greater than the diameter of the disk. The back surface of the pile remained covered with its brass cap; and in order to protect it still further from changes of temperature, it was surrounded by a thick layer of loosely-packed cotton-wool. In order that the temperature of the room might remain as constant as possible, it was heated in the winter late in the evening. The currents of air, which the warm stove would otherwise have produced, did not then exist on the following morning.

Instead of the galvanometer with two nearly astatic steel mirrors which I have previously described*, I have employed one of a different construction with two magnetic needles, and a silvered-glass mirror supported below the needles by a prolongation of the small bar which connects them. This system of needles and mirror has a very much smaller moment of inertia than two steel mirrors, however thin they may be made: consequently this galvanometer gave deflections very much greater than those of one with two steel mirrors which I had previously used †.

In the first place, the two platinum disks were tried with bright unplatinized surfaces, in order to ascertain whether under these conditions they radiated equal quantities of heat to the pile. When this was the case, it was certain that one occupied exactly the same position as the other, and consequently that they were properly attached, as well as that the flame remained constant. A uniform coating of spongy platinum was then applied to one disk, and the radiating powers of both were afterwards compared ‡. The sponge, as has been already stated, raises the

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* Poggendorff's *Annalen*, vol. cxviii. [Phil. Mag. S. 4. vol. xxvi. p. 23.]

† [A foot-note of considerable length occurs at this point of the original, giving a description of the galvanometer used for these experiments.—Transl.]

‡ The platinum-sponge is conveniently applied in the following man-

radiation to double its previous amount. In the next place, plates of various substances were interposed by fastening them in front of the diaphragm mentioned above, and the deflections of the galvanometer were observed, which were produced by the rays transmitted by these substances, both when the source of heat was the bright disk and when it was the one covered with platinum-sponge, which, for the sake of shortness, I shall call in the sequel the platinized disk.

The substances of which plates were interposed were rock-salt, calc-spar, rock-crystal, smoky topaz, agate, plate-glass, flint-glass, and dark green glass, each from 6 to 7 millims. thick. Red, orange, yellow, green, blue, violet, as well as polished and rough colourless glass, were likewise used, the thickness of each of these being about 2 millims.

A number of these plates absorbed proportional quantities of the heat from both sources; so that if I denotes the deflection of the galvanometer caused by the direct radiation of the bright disk, and i the deflection produced when one of the above-mentioned plates was interposed, and I_1 and i_1 denote the corresponding deflections when the platinized plate is used, we have

$$\frac{i}{I}=\frac{i_1}{I_1}.$$

A short time ago Professor Tyndall published the interesting observation that sulphide of carbon, when rendered quite opake by dissolving iodine in it, transmits heat quite as well as it does when pure and transparent. It appeared to me that it would be interesting to interpose these two liquids, namely transparent sulphide of carbon, and the same substance made opake by iodine. For this purpose, layers of them 10 millims. thick were enclosed between two very thin plates of rock-salt. But these two liquids also absorbed the heat from both sources in the same proportion, so that here again we had $\frac{i}{I}=\frac{i_1}{I_1}$.

With a number of the other plates above mentioned, $\frac{i}{I}$ was always found greater than $\frac{i_1}{I_1}$. The difference varied with different plates, but with none of them was it very considerable.

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ner:—A little chloroplatinate of ammonium is stirred up with water to a thin mud, and spread upon the plate in a very thin uniform layer with a hair pencil; the plate is carefully dried and heated in a flame; a second coating is then applied in the same way, and this is repeated till the required thickness is attained. If the chloroplatinate of ammonium is applied in a thick layer to begin with, it collects together in separate masses on heating, and so does not cover the plate uniformly.

When, however, a plate of alum was interposed, the proportion of rays transmitted from the two sources became quite surprisingly different—so much so, indeed, that the absolute quantity of heat transmitted from the platinized plate scarcely exceeded the quantity transmitted from the bright plate.

It is scarcely needful to mention that the experiments were extended to more than one plate of alum. This appeared so much the more necessary, since the total quantity of heat which passes through alum amounts only to a few per cent. The same result was always obtained with six perfectly different plates, varying in thickness from 1·5 millim. to 9·5 millims. With all of them $\frac{i}{I}$ was greater than $\frac{i_1}{I_1}$; and although the quantities of heat which were allowed to pass varied greatly, according to the thickness and condition of the plates, the heat transmitted by each plate from the two sources was nearly the same.

The quantities of heat transmitted, for every 100 parts which fell upon the plates, were as follows:—

Number of the plate.	Thickness.	From the bright platinum, $\frac{i}{I}$.	From the platinized platinum, $\frac{i_1}{I_1}$.
	millims.		
1	1·5	7·77	4·34
2	...	4·16	2·33
3	6·5	4·07	2·32
4	8·0	4·07	2·28
5	9·5	3·00	1·73
6*	9·25	3·14	1·9

Plates of copal of various kinds and of various thicknesses behaved in the same way as plates of alum. With them also $\frac{i}{I}$ differed very considerably from $\frac{i_1}{I_1}$, but the absolute quantities of heat which passed through were not so nearly equal as in the case of alum.

Since the proportional quantities of the heat from the two disks which is absorbed by the several plates of alum and of copal, is different, it follows either that the platinized disk must emit heat of a different colour from that radiated by the other disk, or that the increase in the intensity of the radiation caused by the platinizing does not affect all colours in the same proportion, or, again, that both these effects are produced at once,

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* Rather less clear than No. 5.

and not only is heat of new colours emitted, but also the intensity of some of the rays given out by the bright plate is increased by platinizing, to a greater extent than that of others.

Which of these effects is the one really produced cannot be decided by experiments upon absorption. Supposing that no rays of different wave-length from those previously existing are produced, we might conclude from the fact that alum transmits nearly equal quantities of heat from both sources, that the intensity of those rays which are capable of traversing alum is either not increased at all, or only to a small extent, and therefore that this increase takes place principally in the other colours.

Prismatic Observations.

It seemed worth while to try whether any further information could be obtained by means of prismatic analysis. Such an analysis is best made by means of a rock-salt prism and rock-salt lenses. But inasmuch as rock-salt always absorbs some heat, and as it was possible that it might act upon just those rays with which we were here particularly concerned, the experiment was performed without as well as (in order to obtain a better separation of the several rays) with the aid of rock-salt lenses.

Prismatic Analysis without Lenses.

In order to make the rays fall upon the prism parallel to each other, several diaphragms were employed one behind another.

Instead of the circular disks mentioned above, two strips of platinum, 3 millims. wide and 33 millims. long, were used as radiating surfaces. Like the platinum disks, they were fastened vertically by fine platinum wires in the apparatus which served to place them successively exactly in the same part of the flame of a Bunsen's lamp. The first diaphragm was placed, at a distance of 60 millims. from the glowing strip, in the side of a large box, about 1 metre long and 0·5 metre in height, and the same in width. The length of this diaphragm was the same as, and its width a little less than, that of the strip. A second diaphragm of exactly the same size was placed inside the box, at a distance of 100 millims. from the first, and immediately behind it was the rock-salt prism. A linear thermo-pile, provided with a slit only 0·5 millim. wide, served to examine the various parts of the spectrum thus produced.

In order to prevent the first diaphragm, the one nearest to the glowing strip, or more correctly its edges, getting heated, a double metallic screen was placed between it and the strip. This screen was pushed aside during the observations, and since, in consequence of its nearness to the flame, there was danger of the hinder side as well as the front becoming heated, it was every now and then changed for another one.

To begin with, the prism was removed and the rays of light from the glowing strip which passed through the diaphragms were received on a white screen. It could thus be proved that the strip in the flame and two diaphragms covered each other; and by using both strips one after another, it was ascertained that one of them took exactly the same position as the other. The prism was then put in its place and the heating effect at the various parts of the spectrum determined by means of the thermo-pile, which, by means of a contrivance passing through the side of the box, could be moved into different positions. In order to ascertain that the effect of each strip was the same when their surfaces were alike, they were both tried at first with a bright surface. After having found that the thermo-pile gave always the same deflection at the same part of the spectrum, whichever strip was used as the radiant source, one of the strips was covered with platinum-sponge, and the heating effects which it produced at the different parts of the spectrum were determined, the thermo-pile being kept in the same position while one strip acted upon it after the other.

For the sake of more convenient comparison, the results obtained are combined graphically, as in fig. 1, Plate I. The graphic representation of the results is got by raising perpendiculars from the horizontal line A B, which represent the first swings read off in millimetres on the scale of the galvanometer. The distance which separates these perpendiculars is equal to twice the amount by which the thermo-pile was displaced between each two observations. The curve formed by these ordinates and abscissæ consequently represents the distribution of heat in the spectrum. The longer ordinates belong to the platinized plate. In the drawings, the observed values are connected together by straight lines only.

Before beginning the experiment, the pile was placed at the limit of the visible red. The application of the name of this colour to the ordinate whereby the deflection of the galvanometer is represented for this part, is consequently justified by observation. The same is true of the yellow; the other colours of the spectrum could not be distinctly observed.

Afterwards, in order to get a better separation of the several colours, lenses of rock-salt were employed.

Prismatic Analysis with Lenses.

The rays given out by the glowing strip fell first upon a slit 1 millim. in width, which was applied to the side of the box above mentioned. Behind this there was placed, inside the box, a rock-salt lens at the distance of its focal length, so that the rays fell parallel upon the prism placed a short distance behind it. Behind the prism was another lens, in the focus of which the spectrum was formed. The spectrum was narrow, its visible coloured portion having a width of only 4 or 5 millims. In order to examine its various parts, the linear pile with a slit of 0·5 millim. was used. In other respects the same precautions were adopted as in the experiments without lenses—the strips being first compared before being platinized, one of them being platinized after they had been found to be equal, and the effect of both being then observed.

The results that were obtained are represented graphically in the same manner as the preceding ones. Figs. 2 to 5 show some of the curves obtained. In constructing them, the deflections of the galvanometer read off in millimetres, reduced to two-thirds of their length, are laid down as ordinates. The abscissæ are four times as great as the distances between the several positions of the pile in the spectrum*.

In consequence of the flame radiating heat, as well as the strips, the effect which it alone produced was determined a few times, the bright and the platinized platinum and the flame alone being successively allowed to act. In fig. 2 the smallest ordinates represent the action of the flame alone. If these are deducted from the ordinates which denote the effect of the flame and the bright platinum, and of the flame and platinized platinum, the difference in the action of the two platinum surfaces is seen to be still greater. Fig. 6 represents the curves of these differences as they result from the values given in fig. 2.

Conclusions.

All these curves show that the maximum heating effect falls in the dark part of the spectrum, that is, beyond the red, and in all of them it is nearly at the same point. This maximum is about twice as great for the platinized plate as it is for the bright one. In all cases the increase of radiation shown by the platinized plate takes place principally in the non-luminous part of the spectrum; so far as the coloured portion of the spectrum extends, the heating caused by the platinized strip is scarcely greater than that produced by the bright strip. Consequently the wave-lengths the intensity of which is increased by platinizing are chiefly those lying beyond the red. These wave-lengths are also the most intense among those given out by bright platinum, and indeed by glowing bodies in general.

The superficial areas which, in fig. 6, are contained between the

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* In fig. 4 the word "Blue" has been added by mistake, and "Yellow" and "Red" ought to be further to the left.

curves and their axis of abscissæ, express the total quantities of heat contained in the spectra of the bright and of the platinized platinum. If these areas are calculated, we find that these quantities of heat are in the proportion of 10 : 18.

If we calculate the quantity of heat in the coloured portion of the two spectra, as far as the limit of the red, as given in fig. 6, we find that for the bright plate it amounts to 0·1 of the total quantity; but for the platinized plate it amounts to only 0·068.

The heat-spectra of both the strips are probably continuous. It is nevertheless possible that they are broken at particular points, and that the missing portions, on account of their small width, elude detection by the thermo-pile. If, however, we proceed on the assumption that they are continuous (that is, that throughout the whole space within which a rise of temperature can be detected, rays of all wave-lengths are present without any breaks), it follows that the heat emitted by the platinized platinum contains no rays of different wave-lengths from those contained in the heat radiated by the bright platinum; for if it were otherwise, the distance to which the spectra of the two sources extend would be different; and this is not the case.

Since the absolute quantities of heat from both sources, which a plate of alum allows to pass, are very nearly the same, it seems probable that the wave-lengths transmitted by this substance are those which lie towards the blue side of the spectrum, since these are given out in nearly equal quantities by the bright and by the platinized platinum.

Hitherto I have not succeeded in directly proving that this is the case—namely, that it is the rays belonging to the coloured portion of the spectrum, or those lying nearest to it, which are transmitted in the greatest proportion by alum; for when a plate of alum was interposed in the experiments with the rock-salt prism, the deflections were so slight, in consequence of the small quantity of heat which passed through, that no certain conclusion could be drawn.

It is, however, rendered probable, by Jamin and Masson's* investigations into the distribution of heat in the solar spectrum, that the rays from incandescent platinum, which would by preference be transmitted by a plate of alum, belong to the coloured portion of the spectrum, just as in the case of solar heat. These gentlemen in fact caused the rays of the solar spectrum to traverse plates of *rock-salt*, *glass*, and *alum*, each of the same thickness, and found that the rays belonging to the dark part of the spectrum passed through glass in much smaller proportion than they do through rock-salt, and that the quantity which passes

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* Jamin's *Cours de Physique*, vol. ii. p. 236.

through alum is still smaller than that which traverses glass; but, on the other hand, they found that all three substances transmitted the heat-rays of the coloured portion of the spectrum almost equally well.

Thus much at any rate results from the experiments that have been mentioned:—

That the increased radiation which a plate of platinum shows at the same temperature when its surface is roughened does not result from a uniform increase in the intensity of all the rays which it emits, but that the rays whose intensity is increased to the greatest extent lie in the red part of the spectrum and near to it on the dark side.

Only the amplitudes of the oscillations are altered, not their rate; for, were this to be changed, new wave-lengths would be produced. But so far as the above-mentioned prismatic investigations extend, this does not take place; for the heat-spectra of both the bright and the platinized platinum have the same extent.

In consequence of the roughening of the surface, *the amplitudes of the oscillations are altered, but not the times in which they take place.*

Diminished Intensity of Light with increased radiation of Heat.

It surprises one that the platinized plate, which gives out nearly twice as much heat as the bright plate, is not more luminous than the latter. On the contrary, the intensity of the light which it radiated seemed always less than that of the light from the bright plate; and in like manner the spectrum of the platinized platinum was much less luminous than that of the bright platinum. It is true that the mass of the platinum strip is increased by platinizing, and that its bulk likewise is enlarged, so that it might be supposed it would be less heated; but this cannot be the case; for when the thickness of the bright strip is increased throughout, or when thicker pieces of platinum are stuck on particular parts, the light which these give is no less than that from the thinner portions. The radiation of heat by a thick piece of the platinum is also the same as that of a thinner piece. If a plate of platinum is platinized on one side only, as was the case in almost all the experiments mentioned above, not only the platinized side, but the bright side also appears darker; and if platinum-sponge is deposited upon certain parts only of a bright strip, these parts can be detected on the other side as darker patches when the platinum is made incandescent, just as though it were thereby rendered transparent. But bright platinum attached to various parts of a bright strip cannot be detected upon the other side.

If a thin strip of platinum, bright on both sides, is coated on one side with platinum-sponge, the side which remains bright now radiates less heat than before. And if a strip, already platinized on one side is platinized on the other side also, the radiation of the first side is diminished. This can only be explained by supposing that the strip, notwithstanding that it is entirely surrounded by the flame, has, in consequence of its increased radiation, a somewhat lower temperature at its outer surface. There thus occurs a transformation of light into heat, in so far as, in consequence of the greater radiation, the temperature of the radiating surface falls, and thereby the intensity of the light is diminished.

If we fasten to a thin strip of platinum, which is bright on both sides and heated to redness in a flame, a second thin strip platinized on one side, in such a manner that the platinized side is outwards, the double strip appears darker than before: the change is chiefly apparent on the platinized side of the second strip, but it is perceptible also on the outer bright surface of the first strip. But if the second strip is now turned round with its platinized side inwards, both sides of the double strip appear luminous again.

Comparison of the Spectra of Luminous and Non-luminous Flames.

The great illuminating power which a non-luminous flame acquires when a little soda or some other salt is put into it, suggested the supposition that, in proportion as the intensity of the light increases, so might also the heat be augmented. It is true that I have already mentioned on a previous occasion*, that a flame rendered luminous by soda does not radiate more heat than when it is not luminous. This, however, might be caused by the intensity of the heat-rays being diminished in the same proportion as that of the luminous rays is increased. Whether it is the case that the heat augments uniformly with the light, must be ascertained by examining the heat-spectra of the same flame in its luminous and non-luminous conditions.

The flame of a Bunsen's burner radiates, however, in any case not a large quantity of heat, and of this only a small proportion is contained in the coloured part of the spectrum; so that the heating effect in the luminous portion further than the red was so small that it could not be perceived. This, however, does not prevent the soda-flame being compared with the non-luminous flame; for if the radiation of heat increases at the same rate as the radiation of light, or at anything near the same rate, the heating effect of the soda-flame must increase to such an extent in the yellow as to become perceptible. But, as I have found, this is not the case. In order further to ascertain the relation

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* *Monatsbericht d. Berlin. Akad.* 1864, p. 594. [Phil. Mag. S. 4. vol. xxix. p. 59.]

in which the other rays, especially the non-luminous rays, of the two flames stand to each other, I have compared together their spectra, and found them alike throughout their whole extent. If there do exist any differences, they lie within the limits of the errors of observation. It is of course understood that the proper precautions must be taken in these experiments to prevent rays falling upon the thermo-pile from the solid or liquid soda, or from the platinum wire on which it is supported. Care must likewise be taken that the extent of radiating surface is the same in the luminous and non-luminous flames; for the introduction of the soda alters the size and form of the flame; and hence, if we were to compare the entire flames by letting them radiate into the conical reflector of the thermo-pile, the radiating surface of the luminous flame being greater than that of the non-luminous one, more rays will fall into the cone, although no more heat is produced.

The same thing applies to the comparison of a flame rendered luminous by the presence of carbon, with the non-luminous flame of a Bunsen's burner. In consequence of the gas being already mixed with atmospheric air when it issues from the opening of the burner, the flame is smaller than when the lower holes of the burner are closed so that the air has access only to the outside of the flame, and consequently the carbon which is separated at the inside cannot burn till it gets to the edges of the flame. If, however, the heat-spectra of equal-sized portions of the luminous and non-luminous flames are compared, the two are found to be exactly alike.

The greater part of the heat which the flames give out lies beyond the red; and it is not surprising that in this dark portion of the spectrum the intensity should be the same for both flames; but it is astonishing that, with such a great difference in the illuminating power, the heating effect should remain the same.

At the first glance this result may seem surprising; for it is well known that the solid particles of soda or carbon, which impart luminosity to the flame, radiate more heat than the gaseous particles. But when we consider the small amount of these particles, the smallest trace of which suffices to give the greatest brilliancy to the flame—when we also consider that these particles are ever being renewed, and that their heat must be imparted to them by the flame itself, it no longer appears strange that the difference between the heat radiated by the two flames should be so small as to escape observation. The extraordinary increase in the radiation of light, which this small amount of solid particles occasions, remains, however, on this account only so much the more remarkable.

Concluding Remarks.

It is well known that Melloni, at the beginning of his investigations, concluded from the fact which he was the first to observe, namely that light and heat are transmitted in quite unequal proportions by one and the same substance, that these two agents were not identical. Later on, this view was abandoned by him; nevertheless I am not of opinion that Melloni regarded heat and light as identical, for he speaks only of the "*lien le plus puissant qui réunisse ensemble ces deux grands agents de la nature*"*.

If it is desired to raise the question of the identity of heat and light, it appears to me that the points of difference between heat and light must first of all be distinctly stated.

It is well known that, in order to account for the propagation of heat across empty space, it is necessary to assume, as in the case of light, the existence of an æther which pervades all space, and by the motions of which this propagation takes place; and the simplest assumption is that both light and heat are propagated by means of the same æther. When the heat-motions have arrived at a distant body, they set the æther which this contains in corresponding movement, provided it is capable of assuming it. The questions as to what is the nature of these movements, and how they differ from those which constitute light, must for the present remain unanswered. The heat-movements, however, have the effect of altering the position of the parts of the body to which they are communicated. For every body upon which rays of heat fall is thereby caused to expand, and thus the relative position of its parts is changed. The only case in which the rays would occasion no elevation of temperature, and consequently no change in the position of the parts, would be if the rays fell upon a perfectly diathermanous substance. But a perfectly diathermanous body does not exist; for even atmospheric air does not allow all the rays of heat to pass through it perfectly. If we compare herewith the motions which constitute light, we find that their effects are quite different. It is true that there is no such thing as a perfectly transparent substance, and it would hence appear possible for light to act on the particles of bodies; but no such action has hitherto been observed, except in so far as light can occasion chemical combination, and can produce fluorescence and phosphorescence. In these phenomena, the motions of the material particles, although there can be no doubt of their existence, have as yet entirely eluded direct observation. The characteristic distinction between the movements which constitute light and those which constitute heat may accordingly be stated

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* *La Thermochrôse*, p. 333.

thus:—the latter cause perceptible alterations in the relative positions of the parts of bodies upon which they act, while the former do not produce this effect.

The longer the rays of heat act upon a body, the more it expands. The change in the position of its particles is accordingly a function of the time. But for a given value of the motion imparted to a body, that is, for a given intensity of the radiating body, the position of the parts of the body on which the radiation falls can only be altered up to a certain limit. For the body which receives the radiation can never become hotter than that from which it receives its heat. In most cases, indeed, it only acquires a lower temperature than the latter, since it is constantly radiating off into the surrounding space some of the heat it receives. It is only in the case where this heat is given out exclusively towards the radiating body—when, for instance, the latter as it were completely surrounds the body towards which it radiates—that this acquires the same temperature as the radiating body, since in this case it gives out no more heat than it receives.

A portion of the motion which is imparted to a heated body is then constantly being given off to the surrounding space, or, more correctly, to the surrounding æther; and its temperature can only remain constant when the circumstances are such that a body loses exactly the same quantity of motion as it receives.

We are not acquainted with any similar communication of motion in the case of light. The illuminated body may reflect light, or it may absorb it, but it does not thereby become self-luminous, with the exception of such bodies as become phosphorescent or fluorescent by insolation.

The motion of the parts of a body, which the rays of heat falling upon it produce, consequently always reproduces motion of material parts; for the heated body, by giving out heat again to other bodies, warms them and occasions motion of their parts. It is precisely in consequence of this communication of motion to the material parts of bodies that heat can transform itself into work, and can conversely be reproduced from work.

In this communication of motion, it is remarkable that, however various may be the heat-colour of the rays which fall upon a body, it always gives out, when thus heated, the heat-colours which are peculiar to itself. In the case of light, we find something analogous in relation to fluorescence; for a body fluoresces with the colour peculiar to its own substance, and therefore different from the colour which falls upon it.

Hence, in discussing the identity of heat and light, we must first of all keep in mind that heat depends upon motions of material particles, or of the æther which intervenes between them, thus causing a separation of the material particles of bodies o take place; while the motions which constitute light are not capable of producing such changes. If it does act upon the material particles of bodies, its action is at any rate essentially different from that of heat; for light does not cause such a separation of the material particles as to produce expansion.

As the temperature of a body rises, the number of heat-colours which it gives out increases, and at a certain temperature it begins to radiate light, at first red light, which has the greatest wave-length; and as the temperature becomes still higher, new waves are added until the body appears white-hot. The appearance of new wave-lengths appears to depend upon the fact that, with the increased expansion of the body, or, what is the same thing, with the increased distances between its material particles, the particles of æther which intervene between these, and perhaps also the material particles themselves, are able to make continually new and more rapid movements.

Notwithstanding the difference between heat and light, the question may be raised whether both effects are not produced by one and the same source, something in the same way as electricity and magnetism result from a common force—or, to express it in another way, whether the same movements are capable of rendering themselves perceptible both as light and as heat.

In the case of light, we know that it consists of transverse vibrations; and the polarization of heat necessitates the conclusion that in it also transverse oscillations are present. It is likewise quite possible that our eye, in consequence of its peculiar nature, may perceive, as extremely great, alterations of these transverse oscillations which are not capable of producing perceptible thermal changes, so that the intensity of light of the same wave-length may become very great, without our being able to detect a change of heating-effect. But whether the same movement is in reality perceived by us both as light and as heat, is a point which requires further investigation.

XIII. *On Molecular Physics.* *By* Prof. W. A. NORTON*.

[Continued from vol. xxviii. p. 433.]

IN a former part of this paper a succinct exposition has been given of a consistent general theory of Molecular Forces, and the Molecular Constitution of bodies, and special theories of the different states of aggregation of matter, and the processes of transformation from one state to another, as well as of the essential nature and modes of excitation and propagation of the two agencies of light and heat†.

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* From Silliman's Journal for May, 1865.

† In a paper "On Heat-vibrations," by Mr. James Croll, published in

The conclusions arrived at were all deduced from two fundamental principles, viz.:—

1. That matter exists in the three different forms of *ordinary* or *gross matter*, an *electric æther*, and a more subtile *universal æther*, and that each of these is made up of spherical atoms.

2. That there are two primary forces, *attraction* and *repulsion*. The primary force of attraction is exerted between the atoms of ordinary matter and the two æthers, and between the atoms of the electric æther and those of the universal æther; while a mutual repulsion subsists between the constituent atoms of the two æthers.

From these two postulates the conclusion was derived that each atom of ordinary matter must be surrounded by two atmospheres, one consisting of electric æther, and another of the universal æther, pervading the former, and that the atoms of the electric æther must also be surrounded by atmospheres of the more subtile universal æther which pervades the space between them. Such being the condition of things, it was assumed that the attraction of the central atom of matter for the atoms of electric æther exterior to it was propagated by the intervening universal æther, and that the same was true of the mutual repulsion exerted between the individual atoms of the electric æther.

The primary force of heat, as one of the molecular forces, was deduced from these principles, and found to have its origin in the force of molecular attraction.

We propose now to show that the characteristic phenomena of electricity, comprised under the several heads of *frictional electricity, voltaic electricity, thermo-electricity, magnetism, electro-*

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the Philosophical Magazine, May 1864, it is maintained that the heat-vibration does not consist in a motion of an aggregate mass of molecules, but in a motion of the individual molecules; also that it does not consist in excursions of the molecule or atom across centres of equilibrium, but in alternate expansions and contractions of the atom itself. It will be seen that these ideas are in accordance with the conception of the constitution of a molecule adopted at the beginning of the present memoir (p. 193), and with the theory of heat-vibrations or heat-pulses deduced therefrom (p. 196). The author remarks that his conclusion that "the ultimate atom itself is essentially elastic, is opposed to the ordinary idea that the atom is essentially solid and impenetrable, and favours the modern idea that matter consists of a force of resistance acting from a centre." But in the present communication, the ground is taken that it is the elastic æthereal atmosphere condensed upon the atom that invests it with its panoply of power. Thus armed it becomes an efficient molecule when associated with a kindred molecule—an epitome, in fact, of the universe. In the contractions and expansions that result from the action of the central atom upon its atmosphere is to be discerned the origin, not only of heat-vibrations, but of all the molecular forces; and in the varied possible movements and changes of molecular atmospheres dependent upon their elasticity and mutual action, are to be found the essential causes of physical phenomena.

magnetism, magneto-electricity, induced currents, and *diamagnetism* may be derived as mechanical deductions from the same fundamental conceptions.

We have thus far had no occasion to introduce arbitrary hypotheses, but have in fact discarded those hitherto in vogue, as that of the permanent polarity of atoms, and various hypotheses as to the mode by which such polarity is constituted and maintained. It will be seen that in the wide field we are now entering the same fundamental ideas will suffice, and that the same two forces, attraction and repulsion, operating on the same three forms of matter, are alone concerned in evolving the phenomena. In every different province that we enter we but recognize new results achieved by the same agencies, working by the same essential processes. As transcendentally wonderful as is the infinite variety of Nature, no less so is the all-comprehensive unity of its origin, and the grand simplicity of its evolution. From the point of view now taken, this truth, which has long been discerned with more or less distinctness, stands forth in its full proportions. In the discussion of specific properties of different substances, we have admitted, and shall continue to admit, only differences in degree, not in kind, and differences, too, that fall within the scope of the general theory. No higher requirement than this can reasonably be exacted of any fundamental conception*.

Electricity.

Preparatory to the discussion of this topic, it is important to inquire more minutely than has hitherto been done into the electric condition of molecules, both simple and compound. Each simple atom, as we have seen, is surrounded by an electric atmosphere; but has this atmosphere a definite limit? and if so, what is the condition of the electric æther exterior to it, and in the interstices between molecules? The equilibrium of such an atmosphere is determined by the operation of two antagonistic forces, the attraction of the central atom for each atomette of the atmosphere, and the repulsion experienced by such atomettes from all the others of which the atmosphere is composed. If we regard these forces as in no degree intercepted in their propagation, the atmosphere will extend with decreasing density to that height at which the attraction exerted by the central atom upon an atomette is just neutralized by the repulsion exerted

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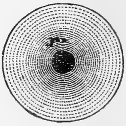
* The author may seem to have adopted, in the scheme of molecular forces presented on pages 195 and 196, an arbitrary hypothesis in assuming the existence of a force of molecular repulsion between the surfaces of contiguous electric atmospheres, as the grounds upon which the theoretical inference was drawn were not stated in the course of the discussion. There will soon be occasion to offer these in another connexion.

upon the same by the entire atmosphere. The centres of repulsion and attraction will also be coincident, and the law of variation of the two forces will be the same; hence beyond the limits of the atmosphere there will be no effective action exerted by the molecule upon any electric æther that may be posited there. But the notion that there is no interception of force does not accord with the fundamental idea of the propagation of force by the universal æther (p. 96). It is impossible that a propagated force of repulsion should take effect upon an atomette of electric æther, unless the universal æther, which is the medium of propagation, be in some degree condensed upon the surface upon which the wave-force falls. Such condensation must give rise to a reflex wave, and the dispersion of a certain amount of force into the surrounding æther. Upon the principle of the conservation of force the amount of force thus dispersed must be abstracted from the original wave-force. Now, if the atomettes of the electric atmosphere intercept a certain portion of the repulsion propagated from other atomettes, it follows that the centre of repulsion of the whole atmosphere cannot be coincident with the centre of attraction, or centre of the atom of matter. It would seem also that the repulsive pulses propagated from the atomettes should be more or less intercepted by the central atom of the molecule, which should tend to displace the centre of repulsion still more. For each side of the molecule this centre must lie somewhere between the centre of the atom and the surface of the atmosphere, as at *r*, fig. 4. In this state of things the atmosphere will have a definite limit, as before; but beyond its limits, since its repulsion must decrease more rapidly than the attraction, an effective force of attraction will be exerted by the entire molecule. As a consequence, the external electric æther will be retained in contact with the atmosphere, and press upon its surface with a certain force. Its density will decrease outward, and doubtless become insensible at sensible distances. If then we confine our attention to a single molecule, we perceive that it must consist of an atom of ordinary matter surrounded with two envelopes of electric æther; of which the outer presses upon the inner, and extends indefinitely, but becomes evanescent at a distance a certain number of times greater than the diameter of the inner envelope*. Hitherto, in speaking of the electric atmosphere of a molecule, we have alluded only to the inner and principal envelope, and have regarded the diameter of a molecule as the same

Fig. 4.



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* It is possible that in some cases the outer atmospheric envelope may be made up of several spherical layers, separated by surfaces of no effective molecular action.

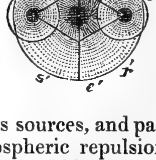
as the diameter of this envelope; and we shall use these terms in the same sense hereafter, unless the exception is distinctly specified.

The outer electric envelopes of molecules serve to establish an electric communication between them, and play an important part in all electric phenomena. By their pressure upon the inner envelopes, or the atmospheres so called, of the molecules, they develope a force of electric repulsion exerted outward at the surface of each atmosphere. This is the force already recognized as one of the molecular forces (p. 196). It has its immediate origin in the compression of the atmosphere at or near its surface, by the outer envelope, which increases the repulsive action of the upper portion of the atmosphere, and so brings into operation an effective repulsion at this surface, where otherwise the effective force would be zero. It is to be borne in mind that the attraction of the central atom is dynamical in its effects, and that attendant upon its exertion the outer envelopes will have an inward and outward movement; but such alternate movements will, when propagated outward, neutralize each other's effects, unless a secondary force is developed in the process. Now it is precisely such a force that is developed in the manner just explained; and this must be propagated by the electric æther of the outer envelope, or the interstitial electric æther of bodies, as a wave-force*.

We have seen that the attraction exerted by the atom upon its atmosphere proper, by forcing outward a portion of the universal æther near the surface of the atom, developes at the same time an effective molecular attractive force, and what we have called the force of heat-repulsion. From the difference that obtains in the circumstances under which these forces and the above-mentioned force of electric repulsion originate, it is highly probable that their constants (*n* and *m*, p. 200) would be different.

To pass now to the case of a *compound molecule*: let *a* and *b* (fig. 5) be two similar molecules, held in equilibrium by their mutual actions, and let us inquire into the condition of the electric æther on the line crossing perpendicularly the line of their centres, at its middle point *m*. At *m* the attractions of the two molecules for an atom of electric æther there will neutralize each other; but at all other points of this line the resultant of the

Fig. 5.



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* The pulses of heat that are received from extraneous sources, and pass from molecule to molecule, augment this force of atmospheric repulsion. Accordingly the value of the "constant" *m* of this force (p. 199) depends not only upon the attractive force, size, and perhaps other peculiarities of the central atom, but also upon the amount of heat received from all extraneous sources. External forces of compression or extension, applied to a body, also tend to augment or diminish the value of *m*.

attractions will have a finite value, and be directed inward toward the line of the centres. This resultant will increase in value as the distance from the line of the centres increases, to a certain point *n*, beyond which it will decrease. The sum of all these resultants will act as a compressing force upon the electric æther about *m*, and determine its density at that point. At other points, as *e*, *r*, *s*, variously situated in the vicinity of the molecules, the joint attraction of the two will generally exceed the attraction of either molecule alone. It follows, therefore, that the two united molecules will be surrounded by an electric atmosphere of their own, spherical or spheroidal in form, and at the same time that the two individual atmospheres will be materially modified. This atmosphere will have an outer envelope, as in the case of a simple molecule.

If we suppose several molecules thus united, the entire group will have its own proper atmosphere. The extent of this atmosphere will depend upon the normal attraction at the surface of the group; and this will also determine the density of the electric æther in the interstices between the molecules*.

Let us next inquire into the true nature of the *electric polarization* of molecules.

A molecule becomes polarized whenever, from any cause, a portion of its electric atmosphere is urged around from one side to the opposite side,—where there is an excess the polarity being *positive*, and where there is a deficiency, *negative*. But it is especially important to observe that the act of polarization is distinct from the result. The one is dynamical, the other statical. The act of polarization, too, does not consist simply in a flow of a certain amount of electric æther from the one side to the other of the atom; in addition to this, a certain quantity of

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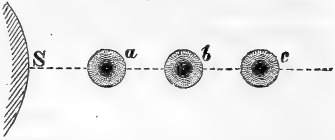
* In treating briefly of solidification (p. 383 *seqq.*), our attention was confined chiefly to the union of simple molecules in one homogeneous mass; but there is good reason to believe that in most cases of solidification, with the exception perhaps of that of perfect crystallization, compound molecules are first formed, and that these combine in various modes to form the solid. In every such instance the formation of such groups of molecules should be attended with the evolution of heat. This will be the inevitable result of the compression of the electric æther between the combining molecules (fig. 5), whether the individual atmospheres be compressed or not.

The heat of combustion and of chemical union generally, has in all probability a similar origin—that is, in a compression of the electric æther between the molecules, or in such a compression combined with a contraction of the individual atmospheres. This topic, and the probable cause of the different effects observed in different instances of combination, will be briefly considered under the head of chemical union.

The varied forms of crystallization assumed by different substances, under similar circumstances, are probably due in a great degree to the diversity that obtains in the number and grouping of the individual molecules of which the ultimate compound molecules consist.

the æther is detached or expelled from the atmosphere on the positive side, and a corresponding amount absorbed into it on the negative side. Accordingly, *when a molecule is being polarized or when its polarization is increasing, there is a flow of electric æther toward its negative side, and away from its positive side. On the other hand, when a polarization once acquired is falling off, there is a flow, or an electric current, in the opposite direction.* To make this more evident, let S (fig. 6) be a surface receiving a charge of positive electricity, and *a*, *b*, *c*, &c. a series of particles of a dielectric medium. The repulsion of the positive charge on S will polarize the nearer side of *a* negatively, and the further side positively; but the same force will urge the interstitial æther toward *a*, and the augmented attraction of the negative side of *a* will add a certain portion of it to the atmosphere of *a*. At the same time the excess of electric æther accumulated on the further side of *a* will act repulsively upon the upper portion of its atmosphere and expel a certain portion of it. It will also urge toward *b* the interstitial electric æther between *a* and *b*, and polarize *b* just as S has polarized *a*. The chain of particles will therefore become polarized in succession; and at the same time there will be an electric movement, a discharge from one to another. To take a more complete view of the matter, the molecular polarization resulting from the electricity received by any single point S will not be confined to the normal line *a*, *b*, *c*, but will extend, though with diminished intensity, along lines radiating outward from S. A similar remark may be made of the flow of electricity that accompanies the polarization.

Fig. 6.



If we take account of the entire spherical surface S, and suppose it to be surrounded by a dielectric medium, as the air, extending to an indefinite distance, each new addition to its positive charge will develope a wave of increasing molecular polarization, which will be propagated in all directions outward through the surrounding medium. This wave, if it may be so termed, will be accompanied by a spherical electric "wave of translation" that will spread indefinitely through the same medium. This latter wave will consist of a series of discharges from one spherical layer of particles to the next, while the former will consist of propagated movements confined to the atmospheres of the particles—these movements being produced by the repulsive action of the electric æther accumulating on the further sides of the particles, transmitted primarily through the universal æther. The quantity of electricity that moves forward from one layer of particles to the next should be equal to the quantity received by the surface; for the determining cause of the electric wave of translation lies in the fact that the force of tension of the propagated electricity urges this quantity between the surface and the first layer of the medium.

The tension of the electricity received results from the compression produced by the expansive action of the charge, which is ultimately in equilibrium with the force by which the disturbed atmospheres of the medium tend to return to their original condition. The tendency of the free electricity to escape between and around the polarized molecules of the medium is counteracted, as will soon be seen, by a resistance developed by the polarization.

This we conceive to be the true process of *induction.* It will be readily perceived that it is in entire conformity with the experimental results obtained by Faraday, and in accordance with his theoretical ideas of the general nature of the process.

We have supposed the electrized surface to be surrounded by a single dielectric medium extending indefinitely; but the result would be essentially the same if the waves were propagated through several such media, as air, glass, &c. If we suppose the first medium to be replaced by another, while the entire quantity of electricity propagated by it, as the polarization of the medium goes on, should remain the same, as Faraday has shown that it actually does, it does not follow that the final degree of polarization received will be the same as before. In this respect substances may differ, or they may have different "specific inductive capacities," as maintained by Faraday. It is in fact just this difference of property that constitutes the difference between conduction and non-conduction, or between the different degrees of conduction or of non-conduction.

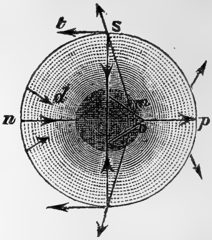
So long as the charge of electricity on the surface S, fig. 6, remains the same, the degree of polarization of the surrounding medium or media will continue the same; but if the charge be drawn off, waves of decreasing polarization will be propagated outward from S, which will be accompanied by electric waves of translation flowing inward toward S. The entire process that went on within the medium while the body was receiving its charge will now be exactly reversed.

If we suppose a body to receive a charge of negative electricity, the process will also be reversed, as when a charge of positive electricity is withdrawn. The final result will be that the particles of the surrounding dielectric medium will be positively polarized on the side toward the electrized surface, and there will be a deficiency of electric æther between the surface and the first layer of the particles of the medium.

Conduction.—We have seen that the act of polarization comprises two different processes—a movement of a certain portion of the æther of the atmosphere of a molecule from one side to another, and contemporaneously with this a discharge of æther from the one side, and an absorption of a corresponding quantity upon the other side. Now the electric constitution of two different bodies may be such that with the one the same amount of electric movement from particle to particle may be accompanied by a feeble degree of polarization, and with the other by a much greeter degree. The polarization is a change determined originally by the forces propagated by the universal æther, while the outward flow of the electricity is partly the result of the mutual repulsion between contiguous atoms of the electric æther.

Let us suppose two extreme cases compatible with our general theory. (1) Let there be an indefinite line of molecules, such that the density of the æther between them is the same as at the surface of the molecular atmospheres, and suppose a quantity of free electricity to arrive at one end of this line. It is plain that the repulsive energy of this electricity will urge before it a certain quantity of electricity throughout the whole length of the line, and that if the flow be unchecked it can exert little or no disturbing action upon the molecular atmospheres except very near their surface, and cannot polarize them. To do this, the moving electricity must have a certain decided tension—that is, be present in a certain sufficient quantity at each point of the line. This would be the case of perfect, or nearly perfect conduction, and there would be little or no polarization of the molecules. (2) Let us suppose that the density of the electric æther is very feeble between the molecules; there being now no continuous mass of æther to give way before the electricity received at the end, the latter will, as a first effect, exert its repulsive action through the universal æther upon the atmospheres of the nearer molecules and polarize them, and this will extend through the line, and the polarization will determine the discharge in the manner already explained. This would be the case of a non-conducting dielectric medium. In all actual instances of good conduction the case lies between these two extremes: the greater part of the electric movement is confined to the outer envelope of the atmosphere, and the outer portion of the inner envelope; and the polarization resulting from the disturbance of the lower envelope, or the atmosphere proper, is slight. With non-conductors, or poor conductors, the flow near the surface of the lower envelope, around the molecule, resulting from the direct repulsive action (or elastic tension) of the electric æther, is comparatively slight; and the polarization of the lower envelope, with the attendant discharge on the further side and absorption on the nearer side, is much greater*.

Fig. 7.



To obtain a more distinct idea of the entire process of imperfect conduction, we must take account of a certain resistance which a polarized molecule offers to the flow of æther around it from the negative to the positive side†. Let *a*, fig. 7, be a polarized molecule. The centre of repulsion of its electric atmosphere will fall at some point *o* on the plus side of the centre. If we inquire into the effective action of the molecule upon a mass of electric æther posited at *s*, midway between *p* and *n*, if the atmosphere were not disturbed the centre of effective repulsion would fall at *r*; in its supposed polarized condition, therefore, it will lie at some point *m* intermediate between *r* and *o*. The two forces exerted upon *s* would therefore act in the direction shown in the figure, and their resultant would act from *s* toward *t*. It will be readily seen that at all points between *p* and *n*, the resultant will have a tangential component directed toward *n*, and a normal component acting outward on the positive side, and inward on the negative side of the molecule. The density of the electric æther at *n* should therefore be greater than at *p*; and while by its elastic tension it is urged around toward *p*, it is resisted by the tangential force just spoken of. And so if the electric æther above *n* is urged by the repulsion of free electricity on that side of the molecule around toward *p*, it has to overcome this resistance. It is to be observed that the attractive or com-

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[*] In the comparison made on p. 426, between the propagation of heat and the propagation of electricity, it is intimated that each is promoted by a polarization of the molecules. This is true only in a limited sense. The circumstances most favourable in general to a conduction of either electricity or heat are a uniformly dense condition of the interstitial electric æther, which may serve as a medium for the direct propagation of the waves or pulses, and such a condition of the electric atmospheres of the molecules that the resistance developed by any polarization that may ensue shall be comparatively feeble (see next page). When, however, the direct conductive action, by the intervention of the interstitial electric æther, is feeble, a greater facility of polarization on the part of the individual molecules would promote the discharge from molecule to molecule.

[†] The terms negative and positive, as here used, have reference to the polarization simply, which is a change in the distribution of the electric æther in the lower portion of the atmosphere. It will be seen in the sequel, that in the upper portions through which the free electricity moves, when a conducting discharge occurs, there will be an excess of electric æther, or a positive state, on the side of the molecule which is negatively polarized, and a deficiency or a negative state on the opposite side.

pressing action about *n* is but the result of the repulsive energy of the free electricity which has determined the polarization.

The outer circle in the figure represents the surface of the undisturbed atmosphere (inner envelope). When the molecule becomes polarized, the surface of its atmosphere takes, or tends to take, the spheroidal form, with the centre of figure lying on the side negatively polarized. At points near this surface, about midway between *p* and *n*, and indefinitely beyond the surface, there will be a resultant force, as shown in the figure, tending to check the flow of free electricity around or past the molecule, in the direction of *n* toward *p*.

It does not follow, because the lines of direction of the attraction and repulsion meet at *s* under a small angle, that the resultant *s t* may not be equal to, or greater than, the force of tension of the free electricity that may reach the molecule, since the absolute intensities of these forces must be very great. Besides, whatever may be the intensity of this resultant, it must be of a corresponding order of magnitude with the tension of free electricity, since it is precisely the resistance exerted by the polarized molecules of air contiguous to an electrized surface that determines the tension of the statical electricity collected upon it.

When a discharge takes place through an imperfect conductor, as rarefied air, the contiguous particles of air are first polarized by the repulsion of the electrized surface propagated by the universal æther. This determines the escaping electricity to fall upon their nearer negative sides; the molecular resistance considered above then comes into play; and if the elastic tension of the electricity is sufficient to overcome this resistance, a conducting discharge takes place; otherwise the electric movement can only be after the manner explained on p. 101—that is, as in the process of induction. According to these views, the greater resistance offered by dense than rarefied air is to be explained by the smaller number of resisting particles encountered in the latter, together with the diminished resistance of each particle. For it will be evident, on referring to fig. 7, that if the molecular atmosphere expands, as it will if the air be rarefied, the resistance at *s* will be diminished. Thus air rarefied to a certain degree should become a conductor of electricity. For a similar reason the surfaces of solid or liquid bodies should conduct electricity better than the internal portions, since the molecular atmospheres are more expanded there*. It will also be observed, in view of the results obtained, that imperfect conduction and non-conduction depend upon certain effects resulting from the polarization of

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* Surfaces of non-conductors resist the flow of free electricity after the same manner as the internal mass; that is, by their molecules becoming polarized.

molecules, and that non-conductors may become imperfect conductors if the tension of the charge presented to them be sufficient. But it should not be overlooked that the amount of polarization induced, and therefore of the resistance resulting from it, must depend upon the condition of the interstitial electric æther (p. 101) and the distance between the molecules, since the interval of time in which the repulsive impulses propagated by the universal æther may come into action before the electric æther impelled from the one molecule to the next shall begin to take decided effect must depend upon these two particulars. The quality of conduction or non-conduction of a substance should therefore depend to a certain degree upon the mode of aggregation of the particles, more especially as different groups of particles, or compound molecules (p. 99), may offer different degrees of resistance.

If an insulated metallic ball, *a*, be placed in good conducting communication with the ground and afterward with the charged prime conductor of an electrical machine, the electricity will flow freely along the route thus opened, and, the tension of the æther passing over *a* being very feeble, there will be no perceptible outward movement from *a* through the surrounding air, and no sensible polarization of the particles of air; but this will no longer be the case if the charge be too large to be conducted off with facility. The effect of connecting the prime conductor with the ground when the machine is worked is to allow a free escape to the æther, which would otherwise pass toward the first layer of air-particles, and displace an equal quantity from each layer in succession and transfer it to the next.

Electric Spark.—If we suppose the insulated metallic ball, *a*, still connected with the prime conductor, to be placed near another ball, *b*, which is in communication with the ground, on working the machine the ball *a*, in receiving its charge, will give rise to electric waves proceeding outward from it (p. 101), and at the same time polarize the particles of the surrounding air. As a consequence, a certain portion of the electric fluid on the nearer side of *b* will be urged away and pass into the ground. This side will thus be electrized negatively by induction, and react upon the air-particles between the two balls, increasing their polarization, and upon the nearer parts of *a*, drawing more electricity to them. All these changes will be attended with increasing wave movements and increasing discharges from one particle of air to another along the line between the nearest points of the balls. The electric æther *in transitu* between the air-particles on this line may thus come to have sufficient density to establish a certain degree of conducting communication between the two bodies, and so to convey a sudden conductive discharge along this line. This result will be partially due to the lateral expansion which the free electricity received by the air-particles on the line will occasion in their electric atmospheres. There will be two causes in operation to produce this effect—the pressure of the stream of æther passing from one particle to the next, against the atmosphere upon which it falls, and the mutual repulsion of the particles that will thus become momentarily overcharged. We have seen (p. 105) that such lateral expansion would give rise to a diminution in the resisting force of the polarized molecules.

As the positive electricity which thus passes over to *b* spreads over its surface, it partially neutralizes its negative state, and thus tends to check the flow and interrupt the passage of the spark. There is also a sudden diminution in the tension of the electricity received by *a* from the prime conductor, which is another cause of this interruption. Apparently another cause conspiring with these two is a reaction to the sudden lateral expansion above mentioned. If *a* were previously charged and insulated and *b* brought continually nearer to it, the mutual inductive action of the two balls upon each other would initiate the electric movement above alluded to through the intervening air, which would finally result in the passage of a spark.

The light of the spark results in part from the vibratory movements in the atmospheres of the air-molecules attendant upon the discharge. (See p. 430.) Experiments by Riess, Masson, and other physicists have conclusively established that the electric light is partially due to the passage of highly luminous metallic particles from the positive to the negative surface. The detachment of such particles, as one result of the discharge, may be explained by the discharges, or flow, of electricity that must take place directly through the ball *a* when the conductive discharge from *a* to *b* occurs. This follows from the fact that the electricity within the ball is no longer in equilibrium under the action of the electrical shell that surrounds it, and the impulses conveyed by the electric current should tend to detach the particles at the end of the metallic line through the ball. The luminosity of the detached particles is to be ascribed to the vibratory movements imparted by the discharge to the electric atmospheres of the particles.

Excitation of Electricity.—There are various special modes of exciting electricity; but they are all only so many different modes of polarizing contiguous molecules, or, more comprehensively, of effecting certain disturbances in the equilibrium of electric atmospheres. The different special causes of disturbance in these cases may all be traced to some action of the molecular forces or of heat; and, as we have seen (p. 196), heat is also, in its primary origin, one of the molecular forces.

Frictional Electricity.—The surface of one body should not in its natural condition exercise any sensible direct electric action upon that of another, unless they are brought into close proximity—certainly much nearer than in any ordinary case of mere contact (see p. 98 and Table I.). When, however, two dissimilar surfaces are *pressed* together, especially if they are brought within the limits at which a force of adhesion would come into operation, the unequal molecular forces of the dissimilar particles might, as will be seen in another connexion (p. 109), give rise to a polarization of the particles. The development of such polarization should establish a wave movement entirely through the two bodies (p. 101); and, as the result, the surface toward which the flow occurred might have an excess of free electricity, while it would be negatively polarized (p. 105); that is, the outer envelopes would on the outer side of the molecules have an excess of electricity, and on the inner a deficiency. The reverse would be true of the other surface. This is the probable explanation of the electrical excitement which may be obtained in a feeble degree by simple *pressure*, as in the experiment by Haüy, of pressing the smooth surfaces of fragments of calcareous spar, quartz, &c. between the fingers. But the process of excitation by friction seems to be different. The most notable distinction to be perceived between the act of rubbing and that of pressure is, that in the former alone the protuberant parts of the two surfaces are pressed against each other laterally. The probability, then, is that this lateral pressure of the surface-particles is the immediate cause of the development of electricity by the rubbing of one surface over another. Now it is easy to see that this pressure should tend to compress the electric atmospheres of the particles laterally, and so to produce an accumulation of electric æther on their outer sides, and a discharge from thence toward the other surface. If the surfaces be dissimilar in nature or condition, the effects of this sort of action should be unequal; and the result should be that upon the one surface there would be an excess of electricity, and upon the other a deficiency. The particles of each body, if non-conductors, should also become polarized after the same manner essentially as already explained (p. 100), and in this condition would serve to retain the surfaces in their disturbed state (p. 104). The explanation here given is sustained by the effect of heat when applied to one of the surfaces, which is to dispose that surface to take the negative state; and in fact the heat-pulses should expand the molecular atmospheres (p. 277) and tend to produce a discharge of electric æther from their outer sides. The like tendency of roughness of surface may be explained in a similar manner.

The tension of the electricity obtained should depend upon the degree of polarization which the surface-molecules are enabled to retain; for upon this will depend the limit of the resistance which they are capable of offering to the free flow of the electricity from molecule to molecule (p. 105). It should therefore be independent of the velocity, the pressure, and the extent of the rubbing surfaces, as established by Peclet. The electrical state of the surface, whether positive or negative, must depend upon the mutual relations of the surfaces, and the comparative energy of the disturbing force in operation upon each (p. 108).

Voltaic Electricity.—In entering upon this topic, we must first endeavour to obtain, from our present point of view, an insight into the nature of *chemical union.* When two dissimilar molecules, *a* and *b*, are brought into close proximity, the effective action of *a* upon the atmosphere of *b* may be quite different in amount from that exerted by *b* upon the atmosphere of *a**. As a consequence, the relative condition of these atmospheres may be disturbed; in other words, they may become polarized. As the act of polarization proceeds, an electric current, or series of waves (p. 101), will pass through the two molecules, and at the same time they will approach each other. As this approximation continues the density of the electric æther between them will increase (p. 99, fig. 5), and a true electric or conducting union may thus be established between them. Under these circumstances, it would seem that finally the non-conducting resistance offered by the polarized molecules (p. 104) should be in a great degree overcome, a comparative equilibrium of tension be established between the outer envelopes of the two atmospheres, and, as a consequence, the polarization materially decrease if not ultimately disappear. In this event the final union would be due chiefly, or entirely, to the molecular attraction proper, as in the case of similar molecules. It is in this final condition essentially that we suppose a particle of water to exist. The two constituent molecules, oxygen and hydrogen, are not polarized, or but feebly so; and they are to a certain degree *in conducting communication with each other*†.

* A disturbing action from the molecular attraction may even come into operation when the molecules are beyond the range of effective attraction —that is, beyond O*c*, fig. 1. For it is to be observed that, in the action of each molecule upon the atmosphere of the next, the attractive impulses prevail over the repulsive beyond O*c* as well as between O*a* and O*c*.

† It is not absolutely essential to the explanation of the voltaic current that the two molecules, when combined, should be regarded as devoid of polarization.

According to the received theory of the constitution of a molecule of

In developing the theory of the voltaic current, let us confine our attention to the case of a single cell, consisting of water, or acidulated water, between a plate of zinc and a plate of copper. We must suppose that the first effect is a mutual polarizing action between a molecule of zinc and one of oxygen, the former being brought into the positive state, and the latter into the negative state, upon the contiguous surfaces. The attraction thus developed will arrange the oxygen, with its associate hydrogen-molecule, on a line normal to the surface of the zinc plate. The further surface of the molecule of hydrogen will be brought to the same positively polarized state as the zinc plate, and will act in a similar manner upon the next particle of water; and so on from one particle to another until a complete chain of polarized molecules extends to the copper plate.

In this chain, as first established, we regard each particle of oxygen as in the negative state on the side nearest to the zinc, and each associate particle of hydrogen as in the positive state on its further side, or at least that they are brought essentially into this condition, and that the true polarization of the contiguous sides is comparatively feeble, by reason of the conducting communication between them, resulting from the condensed state of the electric æther by which they are electrically connected (p. 99). Not only does the positive repulsion that originates at the zinc plate establish, by induction, a chain of polarized water-particles, in which the further or hydrogen side is in the same positive state as the zinc, but it also tends to increase the density of the electric æther posited between the oxygen and hydrogen of the individual water-particles of this chain, and so to urge them asunder*. Before the closing of the circuit, while the mutual polarizing action between the zinc and oxygen is in continual operation, waves of positive electricity spread in an indefinite

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water, we must regard the molecule of hydrogen that combines with a molecule of oxygen as compound, and composed of two simple molecules (p. 99, fig. 5); but this in no degree affects the explanation to be given; for the compound molecule, as it is not decomposed, comports itself throughout essentially as a simple molecule would under like circumstances. It is to be observed that the process of polarization above considered does not occasion an excess of electric æther upon the entire molecule of the one substance, and a deficiency on the entire molecule of the other, since, when a molecule becomes polarized, it absorbs upon the one side the same amount of electric æther that it gives off from the other side (p. 100).

In all these remarks the term molecule is used in the same sense as heretofore.

* If the constituent molecules of each water-particle were not in conducting communication, then the action transmitted along the line would serve to polarize these molecules, and thus to bind them more closely rather than to separate them. This objection seems to hold against Schönbein's theory.

series from the zinc plate through the cell and all the media on that side of the plate (p. 101). A corresponding series of negative waves spread in every direction through the zinc plate and the media lying without it, the electric movement in these being toward the zinc plate. When the two plates are provided with wires leading away from them, we have evidence of this wave movement, and of the polarization that has attended it, in the positive and negative states of the ends of the wires. Now if the ends of these two wires be brought together, the entire series of waves which pass through the copper and zinc plates are condensed, so to speak, upon the wires, and pass through the circuit. The entire quantity of electricity that would be dissipated from the copper plates is thus brought round to the zinc plate again. The arrival of this electricity intensifies the polarizing action going on at the zinc plate, and hastens the union of the zinc and oxygen molecules. It also determines, if not before established, the completion of the line of polarized water-particles traversing the cell. As soon as this takes place, the waves that before spread through the cell are converted into linear currents. At the same time, the electricity discharged from the zinc to the oxygen passes over by conduction to its associate hydrogen-molecule, and by its impulsive and repulsive action urges the latter over to the next particle of oxygen in the chain. This particle of hydrogen, with its charge of positive electricity thus received, acts upon the second particle of oxygen in the same manner that the zinc acted upon the first, and so on throughout the chain. As the detached hydrogen-particle is made, by the same force which detached it, to attract the next oxygen-particle more energetically, there may be no material movement of the common centre of gravity of any of the pairs of particles that are separating or uniting.

The explanation of the voltaic current that has now been given seems to accord with the established laws and phenomena of the current. The primary *electromotive force* must consist in the energy of the natural polarizing or chemical action* exerted between the zinc and the oxygen-molecule, diminished by any opposing action of the same nature that may be in operation at the copper plate. It follows from the principles of induction laid down on p. 101, that the quantity of electricity in circulation, or the *intensity* of the current, must be the same at all points of the circuit. The period of time which the zinc and oxygen-particles occupy in combining should be proportional to the length of the entire circuit, supposing it to be of the same material and cross section throughout—actually should be pro-

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* This chemical action is intensified by the cooperative polarizing action of the sulphuric acid.

portional to the "*reduced*" length of the circuit. During this period all the electricity set in motion by the union of the two particles should pass through the circuit, or, more strictly, be urged forward past each point of the circuit, in electric "waves of translation." The quantity of electricity that moves forward in a given time should then be *inversely proportional to the length of the circuit*, other things being the same. The reason that the quantity of electricity, or the intensity of the current, is *proportional to the area of the cross section of the wire* is, probably, that the number of points of the zinc plate which are contemporaneously in action, with the same degree of energy, would be proportional to this cross section. The *tension* of the electricity circulating in the current should be the greatest where the velocity of the individual particles of the æther is the least. Possible retardations result from the electric relations of contiguous molecules in the line being such that they become more or less polarized, and so offer a resistance to the free flow of the electricity (p. 104), besides that the process of polarization is attended with a retardation. The degree of polarization that exists at any point of the current serves as a measure of the "*resistance*" experienced by the current there.

If an *electrolyte* be disposed between the ends of the wires, the theory of its electrolysis is similar to that of the decomposition of the water in the cell. The only difference is that the ends of the wires are brought by the electromotive force into the same positive and negative states which the natural chemical action in the cell determines upon the zinc and copper plates.

When two or more cells are employed, the natural polarizing action at each zinc plate should be enhanced, and the tension of the free electricity at the ends of the wires of the broken circuit should be augmented. Hence there should be a more energetic force to polarize and decompose an electrolyte interposed between the ends of the wires. But it does not follow that when only good conducting wires are employed to complete the circuit the intensity of the current will be augmented by increasing the number of cells; since the principal retardation of the flow occurs in the cells, and this increases in the same proportion with the number of cells. (See Pouillet, *Eléments de Physique*, vol. ii. p. 732.)

The *heat* developed in the voltaic current is to be ascribed to the impulsive action of the electric æther moving in it upon the universal æther. Currents, or waves of translation, are thus developed in this æther which fall upon the central atoms of the material molecules in the circuit, or the dense æther surrounding these atoms. The impulses thus received are given off, or pass into the molecular atmospheres one after another, and are finally radiated off as heat-pulses. The explanation is the same as that of the molecular absorption and subsequent radiation of the æthereal pulses of radiant heat, already given (p. 426). When the condition of things is such that the particles in the circuit become polarized, a greater amount of heat should be developed, because a part of the electric movement within the molecular atmospheres, which was before confined to their upper portions, now occurs at greater depths, where the universal æther is more dense. Thus, when the resistance to the passage of the current becomes greater, more heat is developed. Heat may also be evolved, under special circumstances, as a consequence of a compression of the molecular atmospheres produced by the current.

We shall see, in the remaining portion of this memoir, that it is to these same *impulses of the moving electric upon the universal æther* that are to be ascribed all the external actions of the current—as attracting or repelling wires conveying currents in the same direction with the given current or in the opposite direction, giving motion to the magnetic needle, developing magnetic or diamagnetic currents in the compound molecules of adjacent masses, and inducing currents in wires or metallic bodies in the vicinity.

[To be continued.]

XIV. *Demonstration of* Newton's *Rule for determining the number of Imaginary Roots in an Equation. By* J. R. Young, *formerly Professor of Mathematics in Belfast College**.

I REQUEST permission to submit the following investigation to the examination of the readers of this Journal for the following reasons.

In 'The Times' of June 28 there appeared an article, in large type, headed "A Mathematical Discovery," which announced that Professor Sylvester had succeeded in demonstrating what everybody else had failed in their attempts to prove, namely, Newton's Rule for Imaginary Roots, and that he would expound his views in a Lecture to be delivered that evening at King's College.

I was present at the Lecture, and felt it to be due to myself to draw Professor Sylvester's attention to the fact that a demonstration of the Rule in question was published, so long ago as the year 1843, in a tract of mine entitled "Researches respecting the Imaginary Roots of Numerical Equations." He stated, in reply, that my investigation was, like all that had preceded it, a failure: it was no proof at all.

Seeing that so much interest has been excited on the subject

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* Communicated by the Author.

—an interest for which I am at a loss to account, since Newton's Rule, whether proved or left unproved, is but of little consequence *now*—I feel naturally anxious that mathematicians in general should judge and decide whether this public condemnation of my efforts was hastily pronounced or not.

I have shown, and very likely others have shown also, that

$$A_nx^n+A_{n-1}x^{n-1}+A_{n-2}x^{n-2}+\dots+A_3x^3+A_2x^2+A_1x+A_0=0$$

being any numerical equation, it will of necessity have one pair of imaginary roots at least, provided any one of the conditions here printed in the margin have place:—

$$\begin{aligned}2nA_0A_2&>(n-1)A_1^2,\\ 3(n-1)A_1A_3&>2(n-2)A_2^2,\\ 4(n-2)A_2A_4&>3(n-3)A_3^2,\\ 5(n-3)A_3A_5&>4(n-4)A_4^2,\\ &\vdots\\ 2nA_{n-2}A_n&>(n-1)A_{n-1}^2.\end{aligned}$$

These criteria of imaginary roots pave the way for the proof of Newton's Rule, which proof I now give.

From the general equation above, the following series of equations are derived in the usual way, namely,

$$nA_nx^{n-1}+\dots+4A_4x^3+3A_3x^2+2A_2x+A_1=0,$$

$$n(n-1)A_nx^{n-2}+\dots+4.5A_5x^3+3.4A_4x^2+2.3A_3x+2A_2=0,$$

$$n(n-1)(n-2)A_nx^{n-3}+\dots+4.5.6A_6x^3+3.4.5A_5x^2+2.3.4A_4x+2.3A_3=0,$$

&c. &c. &c.

And it is well known that if any of these have imaginary roots, as many, at least, must enter the primitive equation. The same remark applies if we reverse the coefficients of each of these equations, as also if we take the limiting equations derived from them when the coefficients are thus reversed.

Reversing, then, the coefficients of each of the above equations, commencing with the primitive, it is readily seen that the derived *cubic* equations will be

$$4.5\dots nA_0x^3+3.4\dots(n-1)A_1x^2+2.3\dots(n-2)A_2x+2.3\dots(n-3)A_3=0,$$

$$4.5\dots(n-1)A_1x^3+3.4\dots(n-2)2A_2x^2+2.3\dots(n-3)3A_3x+2.3\dots(n-4)4A_4=0,$$

$$4.5\dots(n-2)2A_2x^3+3.4\dots(n-3)2.3A_3x^2+2.3\dots(n-4)3.4A_4x+2.3\dots(n-5)4.5A_5=0,$$

&c. &c. &c.

Or, expunging from each equation such numerical factors as are common to all its terms, the equations will be

$$\begin{aligned} n(n-1)(n-2)\ A_0x^3+3(n-1)(n-2)A_1x^2& \\ +2\,.\,3(n-2)A_2x+2\,.\,3A_3&=0, \\ (n-1)(n-2)(n-3)\ A_1x^3+3(n-2)(n-3)2A_2x^2& \\ +2\,.\,3(n-3)3A_3x+2\,.\,3\,.\,4A_4&=0, \\ (n-2)(n-3)(n-4)2A_2x^3+3(n-3)(n-4)2\,.\,3A_3x^2& \\ +2\,.\,3(n-4)3\,.\,4A_4x+2\,.\,3\,.\,4\,.\,5A_5&=0, \\ \&c. \qquad\qquad \&c. \qquad\qquad \&c.& \end{aligned}$$

Now if any of these limiting cubics indicate imaginary roots when tested by the criteria in the margin above, we may be sure that imaginary roots exist also in the primitive equation. But since only one pair of imaginary roots can enter a cubic equation, it follows that whether the criterion of imaginary roots be satisfied by the first three terms of any of the above cubics, or by the last three, or by both sets of three, one pair of imaginary roots, and one pair only, is necessarily implied. Upon examining the coefficients of these cubics, we see that the first three terms of each have a common factor; so that in applying the criterion to these three this common factor may be suppressed. Let the common factor, namely $(n-3)$ in the second cubic, be suppressed; then the product of the first and third coefficients will be $2\,.\,3^2(n-1)(n-2)A_1A_3$, and the square of the middle coefficient will be $2^2.\,3^2(n-2)^2A_2{}^2$. But these are the same results as we should get by employing, in like manner, but without suppressing the common factor, the *last* three coefficients of the preceding cubic; and we see the same to be true of the cubics following; that is, if the criterion of imaginary roots be satisfied by the last three terms of one cubic, it must be satisfied by the first three of the next, and *vice versâ*; so that the fulfilment of the condition by any two consecutive sets of three terms implies, of necessity, but one pair of imaginary roots in the primitive.

We thus arrive at the following conclusions, namely:—

1. If the first three terms in the first cubic (or indeed in any cubic) satisfy the criterion, we may infer the existence of one pair of imaginary roots in the primitive equation.

2. If the next set, the last three terms of the same cubic, also satisfy the criterion, the circumstance supplies no additional information; it is merely a repetition of what had already been indicated, inasmuch as more than a single pair of imaginaries can never enter a cubic equation. In this case, however, the following set of three (the leading terms of the next cubic) *must of necessity* satisfy the criterion; and so on, till we arrive at

a set of three terms for which the condition *fails,* thus putting a stop to the series of concurring indications, and preparing the way for new, distinct, and *independent* indications.

[Now it is to be specially noticed that the last set of three terms in any of the cubics is not in the slightest degree influenced, as to their satisfying the criterion or not, by the first set in that cubic: the two sets, as to this criterion, are wholly independent; if both sets satisfy it, they do so quite independently of each other. But the case is very different as respects the last set in one cubic and the first in the cubic next following. The condition is satisfied or not, in *this* set, accordingly as it is satisfied or not in the preceding set. This distinction between the two cases it is of importance to observe and attend to: the last set in any cubic, as to satisfying or not the condition of imaginary roots, has nothing whatever to do with any *preceding* set; it is totally independent of and uninfluenced by all antecedent results; and if the last set in any cubic should indicate imaginary roots as well as the first set in that cubic, everybody knows that, in the process of developing the real root, the indication would disappear from the final terms, but would still be retained in the leading terms. Yet it is the final set, and that final set *alone,* that determines the character of the leading set in the cubic next following; so that whatever this character may be, no feature of it can possibly have been transmitted to it, as from an origin, by any antecedent *leading* set whatever. Having interpolated these remarks, I now proceed to the third conclusion.]

3. So soon as the criterion is again satisfied, the condition being entirely independent of and non-concurring with the former, must imply another and distinct pair of imaginary roots in the primitive equation, and so on, to the end of the series of cubics.

The criterion which is here supposed to be applied to the terms, taken three at a time, of the successive cubics, supplies, one after another, the entire series of conditions given in the margin above, as will be seen presently. But as the last three terms of any cubic always transfer their character, as respects the fulfilment or non-fulfilment of the condition, to the three leading terms of the cubic next following, the repetitions may be omitted. Attending to this, and applying the proper criterion to each of the foregoing cubics in succession, we have the following conditions for imaginary roots:—namely,

1st. $2^2 . 3^2 n(n-1)(n-2)^2 A_0 A_2 > 2 . 3^2 (n-1)^2 (n-2)^2 A_1^2;$

2nd. $2^2 . 3^3 (n-1)(n-2) A_1 A_3 > 2^3 . 3^2 (n-1)^2 A_2^2;$

3rd. $2^3 . 3^3 . 4 (n-2)(n-3) A_2 A_4 > 2^3 . 3^4 (n-3)^2 A_3^2;$

4th. $2^3 . 3^4 . 4 . 5 (n-3)(n-4) A_3 A_5 > 2^3 . 3^4 . 4^2 (n-4)^2 A_4^2;$

and so on. And if from these inequalities the factors common to each side be expunged, the conditions will be the same as those already given previously in the margin.

It follows from what has now been shown—(1) That commencing with the second coefficient of an equation, as the middle one of the leading three, and applying to those three the proper test given in the margin above, then, with the third coefficient taken as middle term, applying in like manner the suitable test, and so on till we arrive at a case of *failure*, we may conclude that, up to this point, the existence of one pair of imaginary roots, but not of more than one pair, has been detected. (2) That the case of failure thus arrived at is entirely independent of the preceding conclusion; that it is wholly uninfluenced by, and distinct from, every antecedent condition; the indications of the imaginary pair, previously detected, have ceased to be transmitted; and that therefore if, after passing this stage, another indication present itself, it must imply another pair of imaginary roots. [I would here refer to the remarks interpolated above, and direct attention to the fact that the failure here adverted to would, in the limiting cubics, necessarily originate in the final terms, and not in the leading terms of a cubic. By altering the *last* term of a cubic we could make the condition of imaginary roots to either hold or fail, in reference to the final set of three, as we please, without at all disturbing antecedent conditions—a clear proof that the condition implied in that final three is quite independent of the antecedent conditions; but we could not alter *as we please* any term of a *leading* set without disturbing (that is, without absolutely reversing) the immediately preceding condition.]

Such, then, is my demonstration of Newton's Rule for detecting Imaginary Roots in an equation with numerical coefficients. Save the two Notes included within brackets, it is the same in every essential particular as that I first published in 1843. It was also printed in my 'Course of Mathematics' (Allen and Co.) in 1861. I have said that I do not regard Newton's Rule as of such importance as Professor Sylvester appears to do, because I think that there are some things in the work just referred to that enable algebraists to be more independent of it than they were formerly.

July 5, 1865.

XV. *Observations on the Polarization of the Atmosphere, made at St. Andrews in* 1841, 1842, 1843, 1844, *and* 1845. *By* Sir David Brewster, *K.H., D.C.L., F.R.S., &c.**

[With a Plate.]

DURING the last half century observations on the polarization of the atmosphere were made by several eminent observers—by Arago, Delezenne, Babinet, and Zantedeschi; but no result of special importance was obtained till Arago made the great discovery that there existed in the atmosphere a point or spot in which there is *no polarization.* At sunrise or sunset he found that this neutral point was 20° or 30° above the point opposite to the sun, or what we may call the *Antisolar point.* The name of *Arago's neutral point* has been given to this spot without polarization. It is best seen after sunset. At St. Andrews it is above the horizon all the day, between the middle of November and the end of January.

In the year 1840 M. Babinet made the next important discovery respecting the polarization of the atmosphere. When on a visit to the sea-coast, he discovered that there was a neutral point as far above the sun as Arago's neutral point was above the antisolar point†. To this spot the name of *Babinet's neutral point* has been given. It is most distinctly seen immediately after sunset, but is much fainter than the other, on account of the discoloration of the sky by the yellow light of the setting sun.

Upon hearing of this discovery, I saw that we had now the elements for determining the laws of the polarization of the atmosphere; and being ambitious of succeeding in such an inquiry, I devoted four years to the study of the subject.

My observations commenced on the 28th of April, 1841; and I made many hundred on the position of the two neutral points—on their change of place under different states of the weather, different degrees of transparency in the atmosphere, different degrees of light in the sky, and different altitudes of the sun. I measured also the maximum polarization of the atmosphere in different azimuths between that which passed through the sun and the zenith and that which at sunset passed through the sun and the horizon. These observations were not difficult to make when the sky was clear and blue; but in studying the part of the atmosphere between the sun and the horizon, I was perplexed beyond measure with the feeble and uncertain indica-

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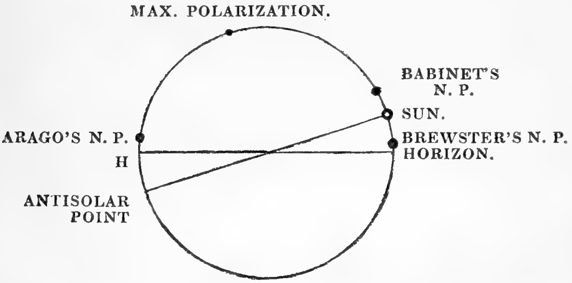
* From the Transactions of the Royal Society of Edinburgh, vol. xxiii. part 2. Communicated by the Author.

† *Comptes Rendus*, &c., 1840, vol. xi. p. 618.

tions of the polariscope. The sky between the sun and the horizon is always the most impure portion of it; and the flood of light streaming from the sun unfits the eye for detecting faint traces of colour. Theoretical considerations had led me to believe that a neutral point should be found between the sun and the horizon, and certain indications of the polariscope, at places around its probable locality, entitled me to infer that it did exist; but as an inference was not satisfactory, I watched every favourable state of the sky in the hope of obtaining a more direct result.

An opportunity of doing this at last presented itself to me on the 28th of February, 1842, when the sun was in the meridian with an altitude of 22°. The spot beneath the sun was fortunately visible from the end of a long dark passage running north and south, and having concealed the sun himself, and every part around him except the probable position of the spot, I obtained a most distinct view of the new neutral point, situated about 15° or 16° below the sun. After communicating this discovery to M. Babinet early in 1845, he made several ineffectual attempts to confirm it, and it was not till the 23rd of July, 1846, that he succeeded in obtaining a distinct view of this neutral point. Following the usual practice, the French have given to this spot the name of *Brewster's neutral point* (p. 128).

Fig. 1.

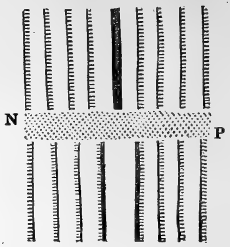


With these three elements of atmospherical polarization, shown in fig. 1, I shall now proceed to determine their position in relation to the sun and the antisolar point, and ascertain the changes which they undergo from variations in the optical and meteorological state of the atmosphere.

Observations on Arago's *Neutral Point.*

In observing the different neutral points, I employed chiefly Savart's band polariscope, which was kindly presented to me by

Fig. 2.



M. Babinet. It consists of two plates of rock crystal which give a system of rectilineal polarized bands[*], attached to an analyzing plate of tourmaline. In very feeble lights I used another instrument, in which the analyzer was a Nicol's prism. When the polariscope is directed to a neutral point, the system of fringes is interrupted, as shown in fig. 2 at N P. The bands on one side of the interruption are oppositely polarized to those on the other, the central band in the one being *black*, and in the other *white*.

In order to measure the altitude of the neutral point thus observed, I used a common quadrant with a plumbline, which gave within half a degree the altitude of the centre of the neutral point. In order to obtain the height of the neutral point above the sun, or the antisolar point, it was necessary to have the sun's altitude, or his depression beneath the horizon. Having asked Professor Henderson for the easiest method of obtaining these with a moderate degree of accuracy, he recommended to me to use Margett's Longitude Tables, from which they were accordingly taken.

The following observations are a selection of some of the most important out of a very great number recorded in my journal[†]. In order not to crowd the page with figures, I have given only the hour of observation, and the altitude of the neutral point above the antisolar point, and occasionally the maximum polarization[‡].

1841, May 12.—Barom. 30·1 in.; therm. 9^{h} P.M. 48°. The sky unusually clear.

Mean time.	R=maximum polarization. In zenith.	In horizon.	Height of neutral point above the antisolar point.
h m	°	°	° ′
7 10 P.M.	30½	28	20 33
7 25 ,,	..	..	20 29
7 35 ,,	30½	29½	21 13
7 45 ,,	28½	28	21 33
7 55 ,,	The sun set.	..	22 8
8 5 ,,	..	..	21 35
8 15 ,,	..	..	20 41
8 30 ,,	The polarized bands faint.		22 26

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[*] See Edinburgh Transactions, 1819, vol. ix. p. 148, where the method of producing rectilineal bands by crossing two plates of rock crystal was first published.

[†] The rest of these observations will, probably, be published in an early volume of the Edinburgh Transactions.

[‡] See the section on the place of maximum polarization, and its intensity or R.

1841, June 3.

Mean time.	R=maximum polarization. In zenith.	In horizon.	Height of neutral point above the antisolar point.
h m	°	°	° ′
6 27 P.M.	Neutral point not risen.		
6 36 ,,	,,	very near the horizon	
6 43 ,,	,,	above and close to the horizon.	
7 8 ,,			18 20
7 35 ,,			20 30
8 4 ,,			21 43
8 33 ,,	The sun set.		

1841, June 10, 2^h^.—Barom. 29·7 in. Singularly fine sky all day; wind east. When the neutral line or point of the polarimeter happened to be above an illuminated wall, the effect was to vary the place of the neutral point, as if the polarization of the sky were diminished. At 3h 47m, when the sky was everywhere pure and free of clouds, the *neutral line* of the polarimeter was curved above the land horizon. At 6h 32m its curvature was greater above the sea horizon, the deviation increasing towards the horizon.

6 35 P.M.	Neutral point in horizon.		
7 13 ,,	..	..	18 20
7 50 ,,	28½	28½	18 56
7 42 ,,	..	..	21 0
8 5 ,,	29½	28½	19 15
8 18 ,,	..	29½	18 56
8 36 ,,	The sun set.	..	19 25
8 52 ,,	29½	30	19 29

1841, July 17.—Barom. 29·60 in.; therm. 55°. Wind south-west. A China-ink sky, with clouds here and there.

7 35 P.M.			19 0
8 0 ,,			17 48
8 18 ,,	Sun set.		17 25
8 30 ,,			17 45
8 47 ,,			17 19
9 2 ,,			16 15
9 12 ,,			17 26
9 22 ,,			19 7

1841, August 31.—Rain till 1h P.M., when it cleared up. Barom. 29·7 in.

5 44 P.M.	27½	18½	17 22
6 0 ,,	28½	22½	19 55
6 20 ,,	..	..	19 59
6 38 ,,	..	..	20 3

1841, September 6.—Barom. 29·5 in.

Mean time.	R=maximum polarization. In zenith.	In horizon.	Height of neutral point above the antisolar point.
h m	°	°	° ′
6 22 P.M.	29½	..	19 7
6 30 "	..	..	19 45
6 35 "	28½	27½	20 21
6 49 "	28½	26½	21 20
7 0 "	..	..	21 0
7 6 "	..	..	21 2
7 15 "	..	..	23 0

1841, September 12.—Barom. 29·75 in. Sky clear.

6 8 P.M.	27½	25½	20 35
6 20 "	..	..	20 45
6 24 "	..	..	21 7
6 46 "	..	..	22 2
6 49 "	..	..	22 10
6 58 "	..	..	23 54

1841, September 29.—Barom. 28·73 in., after rain. Sky clear.

4 23 P.M.	37½	24½	28 10
5 24 "	29½	29½	21 35
5 31 "	30	..	21 0
6 2 "	..	..	23 51
6 12 "	..	..	24 15

At 5h 24m the sky was clear, but whitish, and not blue. A little cloud below the neutral point.

1841, October 23.—Rainy day. Cleared up at 3h P.M.

4 18 P.M.	..	..	21 5
4 24 "	..	..	21 50
4 29 "	27½	25½	21 27
4 37 "	..	..	21 13
4 44 "	..	..	21 23
4 52 "	26	25½	21 45

1841, November 2.—Barom. 30·7 in. Fine day; dry haze.

2 30 P.M.	The neutral point rises.		
2 30 "	26½	22½	13 20
2 43 "	..	..	18 20
3 22 "	25	22½	18 45
3 51 "	..	..	21 32
4 15 "	..	..	19 54
4 36 "	..	..	19 20
4 58 "	..	..	22 22

1841, November 4.—Barom. 30·2 in. A foggy day; sky tolerably clear of clouds.

1 50 The neutral point just risen above the thick haze near the north horizon. The bands of the polariscope are all rugged at the edges, indicating an abnormal state of the air. R=21½° in zenith, and 15° in horizon.

3 13 P.M. 24 52

This singular height of the neutral point arose doubtless from the haze in the horizon.

Mean time.	R = maximum polarization. In zenith.	In horizon.	Height of neutral point above the antisolar point.
h m	°	°	° ′
3 45 P.M.	22½	19½	
4 12 „	..	..	19 56
4 17 „	..	..	20 3

1841, November 25.—Barom. 29·63 in. Singularly fine day.

Apparent time.			
10 31 A.M.	27½	25½	14 30
12 0 noon.	27½	25½	14 6
2 21 P.M.	28½	26½	17 35
3 3 „	..	..	18 45
4 20 „	..	..	19 50
4 24 „	..	..	18 40
4 38 „	27½	27½	19 20

1842, January 29.—Barom. 29·93 in. Fine day; clear sky; snow covers the ground partially.

3 34 A.M.	27	27½	21 46
3 52 „	..	..	20 38
4 10 „	..	..	20 40

1842, February 15.—Rain in morning; fine day afterwards; barom. 30·05 in.; wind west.

3 13 A.M.	26½	18½ in S.H. 22½ in W.H	Neutral point not risen.
3 38 „	..	..	In App. H. 7 30
3 48 „	26½	18½	8 17
3 58 „	..	..	9 7
4 21 „	..	..	14 35
4 47 „	..	..	16 55
4 51 „	27½	22½ in S.E.	17 52

The observations at $3^h\ 38^m$, 48^m, and 58^m are recorded as good, and made in the finest sky.

The state of the atmosphere was peculiar near the horizon, as will be seen in the section on a secondary neutral point (p. 128).

1842, February 16.—Barom. 30·16 in.

12 0	A China-ink muddy sky.		
2 48 A.M.	19½	..	Neut. point not risen.
3 18 „	22½	22½ at 25° alt.	do. do.
3 22 „	..	..	10 30
3 44 „	A secondary neutral point.	..	12 0
4 12 „	..	..	17 30
4 37 „	..	..	17 10
4 58 „	..	..	18 20

1842, February 18.—Cold, wind west and rather strong. $12^h\ 0^m$, I observed a curious effect on the polarized bands in the west, the sun's altitude being about 30°. In carrying the bands vertically round, the neutral line, in place of crossing them at a right angle, was the arch of a circle, to which one of the bands was a tangent. The sky was clear, but in a short time a cloud was formed in that place. I had observed the same phenomenon previously. It indicates a state of the air similar to that which produces mirage.

1842, February 21.—Barom. 29·44 in. Wind west. The sky-line over both the sea and the land unusually distinct and free from haze.

Apparent time.	R=maximum polarization. In zenith.	R=maximum polarization. In horizon.	Height of neutral point above the antisolar point.
h m	°	°	° '
3 45 P.M.	..	..	10 29
3 57 ,,	..	..	12 39
4 31 ,,	..	..	17 40
4 37 ,,	..	..	18 35
5 6 ,,	..	..	19 49

1842, April 5.—Barom. 30·07 in. Splendid sky.

5 25 P.M.	..	..	13 26
5 32 ,,	..	..	15 25
6 21 ,,	30	26½	19 1
6 55 ,,	..	..	18 50
7 12 ,,	30½	..	21 5

N.B. This and April 8 are the only days on which I was able to observe all the three neutral points and determine their place.

1842, April 6.—Barom. 30·05 in. Considerable haze.

5 23			15 26

1842, April 8.

5 30 P.M.	..	..	15 26
5 38 ,,	..	..	19 0
5 46 ,,	..	..	19 0
5 52 ,,	..	..	19 5
6 5 ,,	24½	19½	21 25
6 22 ,,	..	..	20 55
6 47 ,,	27½	..	20 30
7 5 ,,	28¾	..	19 55
7 18 ,,	..	..	20 15

1842, April 13.—Barom. 30·12 in. Fine day.

5 48 P.M.	..	..	16 20
6 20 ,,	29	22½	17 55
6 54 ,,	29½	..	19 40
7 10 ,,	..	..	19 45
7 19 ,,	30½	..	19 4
7 54 ,,	..	..	22 10

At 7^{h} 32^{m} the maximum polarization was 32½, the greatest ever observed.

1842, April 20.—Barom. 30·02 in. Wind west; very fine day.

Apparent time.	R=maximum polarization. In zenith.	R=maximum polarization. In horizon.	Height of neutral point above the antisolar point.
h m	°	°	° ′
5 50 P.M.	..	..	22 50
6 29 ,,	..	..	19 21
6 59 ,,	..	..	20 55
7 45 ,,	..	..	20 10
7 59 ,,	..	..	24 8

1842, April 26.—Barom. 30·00 in. Not a cloud in the sky from morning till night.

5 21 P.M.	..	..	13 43
6 6 ,,	..	..	13 51
6 38 ,,	..	..	19 28
7 9 ,,	..	..	20 10
7 32 ,,	..	..	19 15

1842, April 29.

5 54 P.M.	..	..	18 4
6 1 ,,	..	..	18 35
6 45 ,,	..	..	22 25

1842, May 15.—A haze.

6 29 P.M.	15	15	28 35
6 46 ,,	..	..	28 12
7 26 ,,	20½	..	24 6
8 26 ,,	23½	..	19 59

1842, November 14.—Barom. 29·6 in. Fine frosty and clear morning.

8 55 A.M.	25½	19½	19 50
9 3 ,,	..	..	18 40
9 16 ,,	..	..	15 22
9 31 ,,	..	..	15 0
9 43 ,,	..	..	14 30
9 48 ,,	..	..	13 45

1842, November 20.—Barom. 29·74 in. Cold and clear.

10 8 A.M.	21½	..	18 5
10 15 ,,	..	..	17 30
10 27 ,,	..	..	16 25
10 39 ,,	..	..	45 15
10 55 ,,	..	..	15 5
11 0 ,,	Neutral point lingering in the horizon.		

1842, November 21.—Barom. 29·77 in.; therm. 31°. Frosty morning.

9 1 A.M.	..	..	21 10
9 13 ,,	26½	19½	20 25
11 4 ,,	..	..	15 45
12 22 P.M.	Neutral point below land horizon; bands scarcely visible in horizon.		
12 43 ,,	Neutral point in horizon.		
1 40 ,,	..	..	16 30

The decrease in these numbers as the sun's altitude increased is very interesting. The light of the sky was increasing till noon, whereas, when the numbers increase, the light of the sky diminishes.

The observations on the 14th, 20th, and 21st of November were the only *morning* ones I made.

1842, December 28.—Barom. 29·56 in. Sky very fine at 11h 38m.

Apparent time.	R=maximum polarization. In zenith.	R=maximum polarization. In horizon.	Height of neutral point above the antisolar point.
h m	°	°	° ′
11 38 P.M.	29	..	17 20
11 58 ,,	27	18½	17 21
1 0 A.M.	..	..	18 25
1 28 ,,	..	..	22 16
2 33 ,,	14½	..	22 15

See the state of Babinet's neutral point at this date, when a halo of 45° encircled the sun.

1843, February 2.—Barom. 29·05 in. Snow-storm with wind.

9 55 A.M.	24½	17½	
	Polarization of the sun's light by the snow hardly perceptible, whether we look towards or from the sun.		
2 5 P.M.	Neutral point in sea horizon.		12 5
2 47 ,,	26½	19½	14 35

1843, February 14.—Barom. 29·45 in. Bitter cold day; frost in the morning.

2 48 A.M.	Neutral point below horizon.		12 5
2 53 {Neut. pt in horiz.}	28½	23½	11 45

1843, March 25.—Barom. 29·97 in.

4 35 A.M.	25	Neutral point in horizon.	10 0
5 15 ,,	..	..	11 30
5 44 ,,	28	..	18 31
6 9 ,,	29½	..	17 30

1843, June 21.—Barom. 29·75 in. Fine day; wind west.

7 12 A.M.	27½	22½	{N. pt. in horizon.} 9 40½
7 44 ,,	..	..	16 31
8 30 ,,	29	..	19 22
9 16 ,,	30	..	18 29

1844, February 3.—Barom. 29·90 in. Snow covering the ground.

4 7 A.M.	26	23	18 20

1844, February 21.—Barom. 29·28 in. Snow partially covering the ground.

Apparent time.	R=maximum polarization.		Height of neutral point above the antisolar point.
	In zenith.	In horizon.	
h m	°	°	° ′
3 30 A.M.	27	..	13 3
4 40 ,,	28½	..	12 35
4 13 ,,	28¾	..	17 34
4 40 ,,	..	..	19 24

1844, June 10.—Barom. 29·70 in.

7 36 A.M.	..	..	21 20
7 44 ,,	..	..	21 20
8 11 ,,	24¾	22½	21 35
8 52 ,,	27½	23½	21 50

1844, June 13.—Barom. 29·4 in. Windy; wind south-west.

7 0 A.M.	Neutral point in horizon.		11 10
7 12 ,,	24½	20½	13 2

In the following observations the altitude of the sun was not estimated. The numbers in the fourth column are the altitudes of the neutral point above the horizon.

1845, April 15.

5 48 A.M.	26½	22½	7 25
6 48 ,,	27½	24½	18 55

In the normal state of the atmosphere, as represented in the Map, Plate II.*, namely when the sun is in the horizon, Arago's neutral point is about 18½° above the horizon or above the antisolar point; but when the sun is 11° or 12° above the horizon, and the antisolar point as much below it, the neutral point is in the horizon, and consequently only 11° or 12° above the antisolar point†.

As the sun descends to the horizon, and the antisolar point rises, the distance of the neutral point from the latter gradually increases; and when the sun reaches the horizon the neutral point is 18½° above it, and therefore 18½° distant from the antisolar point.

After the sun has set, the distance of the neutral point from the antisolar point increases; that is, it rises faster than the sun descends, and its maximum distance, when the twilight is very faint, is about 25°.

When the sun is advancing to the meridian, and the light of the sky is increasing, the distance of the neutral point from the antisolar point diminishes, as shown in the *morning* observations on the 14th, 20th, and 21st of November, 1842.

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* This Plate will be given with our next Number.

† In abnormal circumstances sometimes only 7°, 8°, 9°, or 10°, as in 1842, February 15 and 16.

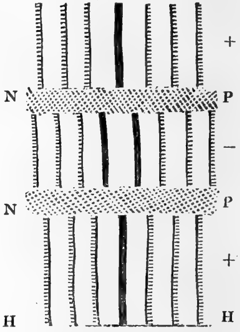
On a Secondary Neutral Point accompanying Arago's *Neutral Point.*

When the sea horizon was terminated by a dark purple belt about 1½° above it, I observed that the vertical bands of the polariscope became brighter over that belt.

The same phenomenon was seen, but less distinctly, over the land horizon. It was difficult to measure the amount of this new polarizing influence, but it was obvious that we should observe it separately when the neutral point came above the belt. In this case it would eclipse, as it were, the neutral point, which would recover itself when it emerged from the belt. It was obvious also that when the negative or oppositely polarized bands came over the belt, the new polarizing influence would extinguish them where they had the same polarizing force, and form a *secondary neutral* point, the primary one being then out of the belt.

On the 8th of June, 1841, at $5^{h}\,50^{m}$ P.M., when the polarized bands were strongest, both on the land and sea horizon, I watched the rise of the neutral point, which, as I had foreseen, *did not appear first in the horizon, but about* 1½° *above it,* the compensation taking place where the vertical or positive polarization was weaker than in the horizon. We had now the singular phenomenon of a neutral point with positive polarization on each side of it. When this phenomenon was more fully developed under a favourable state of the horizon, the positive polarization was overcome by the advancing negative polarization. The negative polarization was then immediately below the ascending neutral point; but at a certain distance (a few degrees below the neutral point) the negative polarization was compensated by the excess of positive polarization close to the horizon, and the beautiful phenomenon was seen of two neutral points, a primary and a secondary, separated by bands of negative polarization, as shown in the annexed figure.

Fig. 3.



1841, June 10, $6^{h}\,40^{m}$.—The neutral point a little above the horizon, with vertical or + polarization *on both sides of it.* The new vertical polarization had more than compensated the horizontal or negative polarization, and left a balance of positive polarization, which soon disappeared when the rising horizontal polarization overpowered it.

1842, February 22.—Both on this day and on the 13th, the neutral point was above the horizon, though not visible, being eclipsed or masked by the cause which produces the secondary neutral point. Over a space of $3\frac{1}{2}^\circ$ above the sea, the positive bands almost wholly disappear before the negative bands are perceptible, and the neutral point is 5° high when the secondary neutral point is distinct in the sea horizon.

Although I have observed the secondary neutral point more than twenty-two times, it has generally appeared under slightly different forms, varying with the intensity of the new polarizing cause which produces it, and with the point of the horizon where the neutral point rises.

It is unnecessary to describe these different forms; I shall mention only an observation made on the 21st of April, 1849, under very favourable circumstances. At $6^h\ 22^m$, when the primary neutral point was about 15° high, the secondary neutral point was $2^\circ\ 50'$ high, the negative bands covering a space of 8° or 9° between them, the positive bands being above the sea-line. A fog prevailed to some extent, and above the sea-line there was the dark purplish belt previously mentioned, over which the positive bands were stronger than on the part of the sky above it.

[To be continued.]

XVI. *On a new form of the Mercurial Air-pump.*
By Professor Poggendorff*.

THE mercurial air-pump, which has recently been brought into use again, is essentially a very old instrument; for only a few years after Otto von Guericke had invented the piston air-pump, the members of the *Accademia del Cimento*, apparently unacquainted with his invention, employed for all their experiments on the behaviour of bodies in the vacuum a straight barometer-tube, of greater length than was needed for measuring the pressure of the atmosphere, and expanded at the top into a sort of receiver, which they filled with mercury and then inverted in a vessel of that metal.

The numerous defects of this rough contrivance, which we may nevertheless regard as a mercurial pump, and especially the wonderful progress which has been made in the construction of piston air-pumps since the times of Boyle and Papin, during the whole of the eighteenth and nineteenth centuries, have caused the idea of the Florentine Academicians to fall completely into oblivion.

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* Translated by Prof. Foster from the *Monatsber. d. Berl. Akad.* 1865, p. 158.

It is not until more than fifty years later that we find the proposal made to revive the use of mercury for the purpose of exhausting, by the construction of a special instrument. This proposal proceeded from the celebrated theosophist Emmanuel Swedenborg, who has described it in his *Miscellanea observata circa res naturales et præsertim circa mineralia, ignem et montium strata*, published at Leipsic in 1722.

Swedenborg's pump consisted essentially of a little table with three long legs, which carried the glass bell-jar that was to be exhausted, and was connected below with an iron vessel, from which descended perpendicularly an iron tube which was joined by a flexible tube of leather with a second iron tube. By placing this moveable tube upright or laying it down, the mercury which filled both tubes was made to rise and fall, and thus the iron vessel, which was provided with the needful valves, was alternately filled with the liquid metal and emptied.

This is the same principle as that employed by Joseph Baader in his second mercurial pump; he, however, improved the construction by substituting a metal elbow-joint for Swedenborg's leather tube, but does not appear to have had any knowledge of the idea of the latter*. In his first pump, which he had described in 1784 in Hübner's *Physikalisches Taschenbuch*, the descending and ascending tubes were connected together immoveably, and the lowering of the mercury in the exhausting-vessel was caused by letting the proper quantity of the metal run out through a cock placed at the lower part of the bend of the tubes. By pouring into the upright tube, after shutting the cock, the mercury that had run out, and opening the exhausting vessel so that the air which had been drawn in might escape, the original state of things was reproduced.

These pumps, however, which, so far as appears, were never constructed by their originators, have not met with the acceptance of physicists; and the same remark applies to all the pumps which have been successively either proposed or actually constructed by Hintenburg, Michel, Cazalet, Kemp, Edelcrantz, Patten, Oechsle, Romershausen, Uthe, Mile, Kravogl, and others, in most of which the mercury was moved by means of a piston.

It is only recently that the mercurial air-pump has attained a better reputation, since the skilful glass-worker, Geissler of Bonn, has employed it in the preparation of the exhausted tubes with which his name is connected, and has by means of it obtained a more perfect vacuum than could be produced by the best piston-pump.

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* The merit of having been the first to call Swedenborg to memory belongs to Gren, who gave the passage in question, together with the figure, in his *Journal der Physik* for 1791 (vol. iv. p. 407).

Geissler's pump, which first became known to the general public through Dr. Theodore Meyer's pamphlet on the stratified electric light, published here [in Berlin] in 1858, does not differ in principle from Swedenborg's, but its form is better adapted to its special purpose. With the exception of the caoutchouc tube, which is substituted for Swedenborg's leather joint, it is made entirely of glass; and in all that relates to the connexion with the pump of the tubes to be exhausted, and the introduction and drying of different kinds of gas, its construction displays great neatness and skill. The contrivance for raising and lowering the moveable tube, however, is but rough and liable to be easily put out of order. This operation was at first performed entirely by hand; but the use of a glass tube and a charge of from 30 to 40 pounds of quicksilver being found too troublesome and dangerous to be managed in this way, it was afterwards done by help of a windlass.

Quite recently Geissler of Berlin has constructed pumps of a smaller pattern, containing only 15 pounds of mercury, in which the two glass tubes are replaced by a long and strong tube of caoutchouc, which connects the oval exhausting-vessel with a glass globe of the same capacity. The operation of exhaustion is performed by hand, by alternately placing the glass globe upon the foot-board and upon a stand placed near the pump. This arrangement is certainly simple, but must excite some apprehension as to how long the caoutchouc tube will last; besides, working the pump requires the exertion of a not inconsiderable amount of force, and may easily expose the apparatus to danger.

A mercurial pump, however constructed, is, on account of its slow working, only adapted to exhaust vessels of small capacity; and although for these it may afford a better vacuum, it will never displace the common air-pump from physical cabinets.

This conviction, together with the circumstance that now-a-days every physicist is already provided with an air-pump, induced me in the course of last summer to consider whether it was not possible so to modify the mercurial pump as to make it an appendage to the common pump, which might, according to circumstances, be either used in conjunction with it or separated from it.

The execution of this idea presents no practical difficulty, does not increase the price of the instrument, allows it to be constructed of any required size, and makes the operation of exhaustion perfectly easy and free from danger.

In order to be able to speak upon the subject from experience, I have had such an auxiliary instrument made as a trial, and, as was to be foreseen, it has completely fulfilled my expectations. I consequently think that its publication may be of some service to physicists.

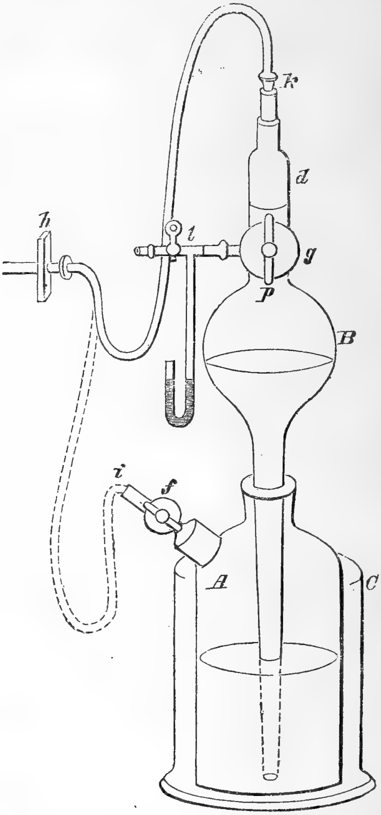
The body of the instrument consists essentially of two parts, namely: (1) A glass bottle A, with a rather wide neck and a tubulus at the side. In my instrument this bottle is about 12 inches high and 6 inches in diameter. (2) An egg-shaped glass vessel B, with a short neck above and a long one below, which is ground so as to fit into the neck of the bottle A, and reaches nearly to the bottom of it.

An iron cap, provided with a passage which opens on the outside by the nozzle *i*, and can be closed air-tight by the cock *f*, is cemented on to the tubulus of the bottle A.

In like manner the upper neck of the egg-shaped vessel B is provided with an iron cap, the opening of which can be closed by the cock *g*. This opening leads in the first instance into the little bottle *d*, which has the iron nozzle *k* cemented on to its neck.

The last-mentioned cock *g* has a threefold purpose, and is correspondingly bored. When its handle stands vertically with the end *p* below, it establishes a communication from the vessel B to the small bottle *d*, and, provided the nozzle *k* is open, to the air outside.

If it is inclined at an angle of 45°, it completely cuts off the vessel B; and when placed horizontal with the end *p* towards the left, it makes a communication between this vessel and a lateral passage *l*, with which whatever is to be exhausted is connected air-tight*.



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* In the figure this lateral passage is represented, for the sake of clear-

This apparatus is connected with the air-pump by means of a caoutchouc tube, provided at both ends with a hollow conical metallic plug.

One of these plugs is put into the hole which the main-cock, *h*, of every air-pump is already provided with, for the purpose of either shutting off exhausted vessels from the pump, or of filling them again with air; and the other plug is put alternately into one of the nozzles *i* and *k*.

When the apparatus is to be worked, it is necessary first of all to fill the bottle A with quicksilver nearly up to the tubulus: in my instrument forty pounds are required for this purpose. The main-cock *h* of the air-pump must then be turned so that the caoutchouc tube connected with it communicates with the barrels.

The other plug of the caoutchouc tube is next put into the nozzle *k*, the handle of the cock *g* is placed vertical with the end *p* downwards, and the pump is set in motion.

Two strokes of the pistons of my pump are sufficient to bring the mercury to the required height; that is, not only to fill the vessel B completely, but also to drive it through the cock *g* into the small vessel *d* placed above it, as is needful to ensure the complete displacement of all air.

When this has been done, the cock *g* is turned through 45° with *p* towards the left, so as to close the vessel B. The plug of the caoutchouc tube is then taken out of the upper nozzle *k* and put into the lower one, and the pumping is continued.

Four strokes of the pistons of my pump suffice to empty again the vessel B of mercury, notwithstanding its considerable size, and thus to produce therein a vacuum such as cannot be surpassed without boiling the mercury.

In order to employ this vacuum for the required purpose, the handle of the upper cock *g* is placed horizontal with the end *p* towards the left, whereby the lateral passage *l*, and the tubes or other pieces connected with it, are put into communication with the exhausted vessel B.

The bottle A may for the sake of safety be previously closed by the cock *f*, although this is not necessary so long as the connexion with the pump is maintained. When the exhaustion is completed, the vessel B must likewise be closed again by turning the cock *g*.

This is in general the course of the exhaustion, which must of course be repeated as often as needful.

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ness, as placed at right angles to the cock *g*; it is better, however, to place it in a line with the axis of the cone of the cock, and to give this a double boring like that of the cock *h* of the air-pump. In this way the quicksilver is prevented from getting into the passage *l*.

It requres, as is easily seen, no great expenditure of strength, and is moreover not attended with any danger; besides which, the apparatus is placed in a strong wooden vessel, large enough to hold the whole quantity of quicksilver in the scarcely conceivable case of an accident.

Every repetition of the operation is begun by separating the bottle A from the pump, and, by carefully turning the cock *f*, allowing the quicksilver to mount slowly up into the vessel B. In consequence of the usually small capacity of the pieces to be exhausted, this vessel fills again almost completely of itself, so that, after opening the cock *g*, only a small quantity of air has to be removed from below the upper nozzle.

It is obvious that this apparatus, when carefully constructed, is capable of performing the same services as Geissler's pump, and that in fact it differs from this only in the way in which the emptying of the mercury is effected.

I will here mention only one of the preliminary experiments which I have made with it,—an experiment which is calculated to refute the common opinion that an absolute vacuum is to be attained by means of the mercurial pump. This consisted in trying whether the vessel B, after being emptied of quicksilver, would allow a current of electricity to pass. This I found to be really the case. If the iron caps of the apparatus are connected with the poles of an induction-coil, an iron wire being put through the cock *f* into the quicksilver in the bottle A, the well-known luminous phenomena are obtained most beautifully developed.

As already remarked, the apparatus that has been described is specially applicable for exhausting small vessels which can be directly connected with the lateral opening *l*. It can, however, be employed also for pumping out larger vessels, such as bell-jars which require to be placed upon a plate.

This is effected by putting the side opening *l*, by means of a flexible metallic tube, into connexion with the air-pump through the cock which, in Pistor's air-pumps, leads to the Hawksbee's barometer-gauge, and is provided for this purpose with a second perforation, similar to that in the main-cock *h*.

By first of all placing the cock *h* so as to establish a communication from the pump-barrels to the bell-jar standing upon the plate, this can be exhausted as far as the pump allows; and by then turning the main-cock through a right angle, and connecting it by the caoutchouc tube with the mercurial apparatus, the exhaustion can be continued by means of the operations previously described, so as to produce a more perfect vacuum.

This, however, of course requires that all the joints should be perfectly air-tight; and even in that case it becomes a tedious matter if the capacity of the bell-jar to be pumped out is at all considerable in comparison with that of the exhausting-vessel B of the mercurial pump.

The idea of the arrangement that has just been described occurred to me last autumn at the Meeting of Men of Science [the annual *Naturforscherversammlung*] at Giessen, where I had an opportunity of being present at some of the experiments made by Geissler of Bonn with his pump, but witnessed also how easily the instrument can be destroyed by unskilled hands.

On my return to Berlin I determined to have my idea carried out, at least in the way of an experiment. I had already entrusted it to a mechanician to execute, when, to my not altogether agreeable surprise, I saw, from the September Number of the Philosophical Magazine, which had in the meantime arrived, that the Rev. T. R. Robinson, the same to whom we are indebted for a valuable investigation concerning the lines of the spectrum, had already had a mercurial pump constructed upon similar principles.

Only one half of his plan, however, is coincident with mine. Like me, he uses a common air-pump to remove the mercury from the exhausting-vessel; but in order to fill it he requires, besides, a separate apparatus, consisting of an upright cylinder of cast iron, 13 inches high and 3·2 inches wide, whence the quicksilver is driven by means of a wooden plunger into the exhausting-vessel.

In consequence of this and other parts of the arrangement, the instrument, which holds only ten pounds of mercury, becomes so complicated and clumsy in form, that it is difficult to believe it will meet with a very favourable reception at the hands of physicists. Consequently I did not consider that I need hesitate to proceed with the practical execution and publication of my idea.

I will in conclusion also remark that, when all that is wanted is to exhaust simple tubes with platinum or aluminium wires melted into them, a regular mercurial pump is not at all needful, even in order to attain as high or even a higher degree of exhaustion than can be reached by means of it.

For this purpose nothing more is needed than a small tubulated gas-generating bottle about half filled with quicksilver, and the neck and tubulus of which are closed by good corks. Through the cork of the tubulus is inserted an iron tube which can be closed by a stopcock, and ends externally in a nozzle; and through the cork in the neck of the bottle is inserted a glass tube about 8 or 10 inches long, which must be melted on to about the middle of the tube that is to be exhausted, and at right angles to it.

At first this adapting tube is only pushed far enough down to dip a little way into the quicksilver. The bottle is then inverted so as to fill with mercury the tube that is to be exhausted, and this, if it be thought proper, is boiled out, the bottle being afterwards slowly set upright again, while the adapting tube is pushed further and further in, at last nearly to the bottom. The nozzle of the cock inserted into the tubulus is now connected with the air-pump by means of a caoutchouc tube, in the manner already indicated. Scarcely half a stroke of the piston will be needed to empty the tube, which can now, after shutting the cock and removing the caoutchouc tube, be either melted off or left connected with the bottle.

Although only a very low degree of rarefaction of the air in the bottle is needed in order to empty the tube completely, it is nevertheless advisable, especially when the tube is not to be melted off immediately, to give both corks a good coat of sealing-wax before pumping out.

XVII. *On the Emission of Luminous Radiation at a Red Heat.*
By M. P. DESAINS*.

M. PROVOSTAYE and I, a dozen years ago, proved that at a red heat different substances may have very different powers of emission for light. And we have also given some indications as to the relative values of light emitted at this red heat by the same extent of surface of oxide of copper, platinum, and gold. In our experiments oxide of copper had about ten times the emissive power of gold. Platinum was between the two, but nearer the second.

In the researches which I now submit to the Academy, I proposed to myself to extend to a larger number of bodies the study of the luminous emission at a red heat; the results I have obtained may be summarized in the following manner:—

At a red heat, and in a perpendicular direction, oxide of copper, oxide of cobalt, green oxide of chromium, brown oxide of manganese, and red oxide of iron have for light emissive powers which are virtually equal. Under these circumstances a coating formed of sulphate of lead and a little borax also emits luminous rays very easily. It is a little less brilliant than the preceding substances, but more so than platinum. Platinum is more so than gold, and this metal, lastly, more so than a layer of oxide of zinc rendered adherent by borax.

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* Translated by Dr. E. Atkinson, from the *Comptes Rendus*, July 3, 1865.

To be more definite, we might, I think, assume that representing by 100 the normal emissive power of oxide of iron and of the bodies we have classed with it, that of platinum would be about 32, that of gold 10, while that of oxide of zinc would not exceed 5, if indeed it amounted to this number. I may finally add that a coating obtained by mixing together chalk, a little borax, and water, appeared in one experiment to approach oxide of zinc in the feebleness of its emissive power.

In the researches the results of which we published in 1854, we heated to redness the coatings whose emissive power we desired to study, by applying them on a small plate of gold or of platinum, which could be traversed by an electric current. In my new experiments, without abandoning this mode of heating, I usually preferred to apply the oxides submitted to my observations on plates of gold or platinum which were heated by eolipyles. These plates, which were tolerably thick, were not appreciably altered in shape during the experiments.

When, without attempting to make any measurements, it is simply desired to show the difference of emissive power for light, a plate of this kind is taken of about 25 or 30 square centims., and on one of its faces two lines are traced at right angles passing through its middle, and thus dividing it into four contiguous portions; on one of these the metallic lustre is left, the second is covered with oxide of zinc, the third with oxide of iron, the fourth with oxide of copper. When the coatings are dry, the plate is placed in a dark chamber and heated with a flame of alcohol or of gas, which strikes against that face which has no coating. As soon as the temperature had reached a red heat, all the effects which we have indicated were observed.

If photometric observations are to be made, it is important to take precautions for preventing rays emitted from the source of heat from being mixed with those which come from the surfaces investigated. In this case the plate may be fixed at the end of an earthen tube whose internal surface has been blackened or covered with oxide of iron. The external face of the plate is heated, and the other is observed by placing the photometer near the open end of the tube.

The extreme smallness of the luminous emissive power of oxide of zinc has surprised me; but although I have greatly varied my experiments on this point, they have all led to the same result. I shall not detail them; I will merely mention one which appears to prove that the small emissive power of this oxide is not at all due to the fact that, from want of conductibility, it only attains at its external surface a temperature materially lower than that of other coatings applied on the same body.

I took a platinum plate, and having completely covered it with zinc-white, I covered half the plate thus whitened with a layer of oxide of iron above that of oxide of zinc; lastly I heated it, and even in this case I found the oxide of iron twenty times as luminous as the other.

Oxide of zinc is one of those bodies which at about 100° have virtually the same calorific emissive power as oxide of iron or lampblack. Seeing how little light it emits at a red heat, I compared at this temperature its calorific emissive power with that of oxide of iron, and I found that it was about six-tenths of that of the latter.

Hence at a red heat the ratio of the emissive powers of these two substances is far greater for heat than for light. This difference, however, is but natural, seeing that of the total sum of the rays which at redness each sends to the thermoscope, there is a strong proportion of obscure rays, which have no influence on the results of the photometric measurements. The thermoscopic measures refer to the total emission; the photometric measure only applies to a particular class of radiations; it omits all those whose wave-length is above a certain limit.

What has been said refers merely to the effect of luminous emission properly so called. The appearances change, the relative lustre of incandescent bodies is modified, when to the emitted rays are joined the reflected or diffused rays, as is the case when the surfaces observed are placed in the middle of an incandescent space. These changes are easily explained, and can be readily produced in the following manner. A porcelain tube is taken about 0·06 metre in diameter; it is bevelled at one of its ends; then after having closed this end by a platinum plate half covered with oxide of iron, it is introduced into a furnace. The other end of the tube is open, and projects from the furnace. It is heated; the plate becomes red first, and the oxide appears far more brilliant than the platinum; but when the sides of the tube are incandescent, the difference in lustre of the metal and of the oxide diminishes or disappears; and then in order to change the function, that is, to make the platinum appear more brilliant than the oxide, it is sufficient, by changing the arrangement of the fire, to make the metallic plate distinctly less hot than the side of the tube whose light can reach the eye by reflexion from the metal.

When in making this experiment the plate which closes the tube is observed with a polariscope, it is easily seen that at first, when the plate alone is incandescent, the light which comes from the platinum to the eye is polarized in a plane perpendicular to that which passes through the axis of the tube and the perpendicular to the plate. In the second phase of the experiment, when the oxide of iron and the metal have the same lustre, the indications of polarization disappear. Lastly, when the platinum appears more brilliant than the oxide, the rays which come from the metal to the eye are polarized in the plane passing through the axis and the perpendicular, which shows that then it is the reflected light which predominates in the pencil coming from the platinum.

XVIII. *Researches on the Mineralogy of South America.* *By* David Forbes, *F.R.S., &c.*[*]

III.

Cassiterite.—As previously mentioned, this mineral occurs in considerable quantity in the sands of the River of Tipuani in Bolivia, where, from its greater specific gravity, it accumulates in the washing-apparatus (Lavaderos) during the process for extracting the gold: this was particularly found to be the case at Playa Gritada, at the establishment of Don Ildefonso Villamil, and at this place a considerable quantity was collected for examination.

The Cassiterite was generally in small semi-rounded nodules, frequently, however, showing one or more crystalline faces, and varying in size from a mustard-seed up to that of a hazel-nut. It was generally pure, that is, without attached rock-substance; but occasionally pieces were found with adherent quartz. The associated minerals found along with the Cassiterite were, besides gold and metallic tin, black tourmaline, garnet, hæmatite, chiastolite, quartz, and small fragments of minerals much resembling ruby and topaz, but as yet not examined.

The colour of the Cassiterite itself was most generally a dirty brownish white or dark black, but fragments were occasionally met with of a fine wine-yellow colour, or even colourless and transparent. A chemical examination of these different varieties yielded the following results:—

Cassiterite, colourless.—Upon examination this was found to be nearly pure binoxide of tin. The specific gravity, taken at 60° Fahr. upon 97·38 grs., in fragments, was found to be 6·8432. In order to see whether any change in this specific gravity could be effected by heating, the above quantity was ignited to a full red heat for half an hour; after cooling, it was found to have lost 0·06 gr. in weight, and a second determination of its specific gravity gave the number 6·8439; so that it may safely be considered as having remained constant. In external appearance the only observable change was in colour, which had now a slight reddish-yellow tint, and in lustre, which was more waxy.

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[*] Communicated by the Author.

Before the blowpipe it afforded only the reactions due to pure oxide of tin.

Cassiterite, brownish.—In addition to the blowpipe reactions for oxide of tin, the yellow colour of the borax globule, whilst warm, indicated the presence of iron, whilst the insoluble white matter in the phosphate-of-soda bead showed silica to be present. A trace of manganese was also indicated on treating the mineral with nitrate of potash on platinum-foil.

The chemical examination was conducted as follows:—

20·52 grs. in impalpable powder were intimately mixed with 130 grs. of a mixture of anhydrous carbonate of soda and sulphur in equal parts, and fused in a Berlin porcelain crucible for some time; on cooling, the mass was dissolved out in water, to which a little sulphide of ammonium was added. An insoluble residue remained amounting to 3·70 grs., which was fused as before, with 24 grs. of the same mixture of sulphur with carbonate of soda, and dissolved in water; a very small quantity still remained insoluble, from which sulphuric acid with a few drops of nitric acid extracted some alumina and oxide of iron, which were separated by potash and determined as usual. The oxide of iron contained a little manganese, which, however, was not separately determined.

From the solution of the sulphide of tin in sulphide of sodium, the former was precipitated by hydrochloric acid, and after being collected on a filter, washed, and dried with the usual precautions, was converted by prolonged ignition into binoxide of tin. The results thus obtained will, when tabulated, stand as follows:—

		In 100.
Binoxide of tin	18·84	91·81
Sesquioxide of iron (with manganese)	0·21	1·02
Alumina	0·15	0·73
Silica and insoluble	1·33	6·48
	20·53	100·04

Cassiterite, black.—The specific gravity of the deep-black variety of Cassiterite, taken on 187·40 grs. at temperature of 60° Fahr., proved to be 7·021. Before the blowpipe, the only difference in behaviour from the last-mentioned specimen was in the much stronger reaction of iron and manganese.

In the examination of this specimen 20·11 grs. in impalpable powder were added by degrees to 300 grs. caustic potash, already in fusion in a gold crucible, and kept in the liquid state for some time. On digestion with water, a residue, apparently of undecomposed mineral, was left, amounting on determination to 1·89 gr., which was again treated as before with potash in fusion. An insoluble residue (1·11 gr.), apparently only undecomposed mineral, still remained. The solution was now acidified with hydrochloric acid and treated with sulphide of ammonium in excess, &c., as in the previous analysis.

The results were as follows:—

Binoxide of tin	18·47	91·80
Oxide of iron and manganese	0·53	2·69
Insoluble matter	1·11	5·51
	20·11	100·00

Cassiterite from Oruro.—The deposits of tin-ores at Oruro are probably the largest in South America. The ore there occurs both as stream-tin (and is the object of extensive explorations), and also in veins in the Silurian rocks of that district.

A specimen sent me from that locality was an aggregate of small crystals (pyramidal), of a dirty honey-yellow colour. Blowpipe reactions and chemical examination showed the crystals to be nearly pure binoxide of tin. A determination of their specific gravity at 60° Fahr. gave 6·704.

Not having visited this district, it is impossible to state whether the Cassiterites are commonly so pure in this locality as the above specimen.

Cassiterite, Carabuco, Bolivia.—Tin-ores occur extensively in the province of Laricaja in Northern Bolivia, on the west slope of the High Andes range close to the eastern shore of the Lake of Titicaca at Carabuco. They occur here associated with several minerals containing silver, and in the time of the Spaniards were worked exclusively for the nobler metal: of late years, however, the deads of these mines have been and still are worked for tin. The gossan of these veins, or, as called in the Aymara Indian language, "*paco*" *, chiefly composed of sesquioxide of iron, is frequently very rich in silver (it is stated, occasionally up to 8 per cent.), probably in the state of chlorides.

The tin ore is in greatest part Cassiterite, which occurs crystallized in prisms, having a specific gravity of 6·4. Mr. Philip Kroeber has forwarded me the subjoined results of his analysis of these crystals.

Water	1·737
Tin	76·805 }
Oxygen	19·534 }
Iron	2·177
Silver	0·015
Tungstic acid	0·020
Lead	0·250
	100·538

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* Which means brown.

Native Tin.—The above analyses of Cassiterite were made in hopes that some light might be thrown upon the occurrence of the metallic tin previously described as occurring in the gold-sands of the river Tipuani. It must, however, be admitted that they do not much tend to dispel the mystery connected with its occurrence, further than that, as the metallic tin contained so much lead alloyed with it, the idea that it could have originated from the reduction of the Cassiterite in the sands of the river Tipuani must seem very improbable.

I must, however, add a postscript to this notice, inasmuch as a late post from Bolivia has brought me from Tipuani, enclosed in a letter from a friend, "two crystals of native tin" from the sands of that river; these so-called crystals are small and distinct six-sided prisms of undoubted metallic tin, the one a little over and the other slightly under one quarter of an inch in diameter, and respectively about three-sixteenths and five-sixteenths of an inch in length. The planes bounding the sides are well defined, but oxidized, so as to have no lustre, or permit any very satisfactory measurement of their angles being made. There were no terminal planes on the ends of either of the two specimens here referred to, and the prisms appeared abruptly terminated.

Summing up the evidence deduced from the facts here produced relative to the occurrence of native tin, I suppose that its existence must be admitted, at least until further evidence is brought forward to prove the contrary. Yet I cannot but retain the impression on my mind that the tin in question does not owe its origin to purely natural causes, however much the balance of evidence goes against me.

Gold, Carabaya, Peru.—This gold occurs in quartz-veins traversing the Silurian slates of the High Andes of the south of Peru on to the confines of Bolivia. The specimen examined was procured from a vein situated in the Montanas de Monte Nello in the Carabaya range. The quartz was colourless or white, and along with the gold contained iron pyrites and a greenish-coloured silicate of undetermined character. The gold was of a very fine colour, and found to possess a specific gravity equal to 18·43 at 60° F. The analysis was conducted in precisely the same manner as that of the specimens of gold previously reported on; and 11·43 grs. afforded 0·38 chloride of silver, or

Gold	11·14	97·46
Silver	0·29	2·54
	11·43	100·00

Gold from Rio de Cajones in Yungas.—This gold is derived from the wearing down of the granitic rocks forming in Yungas the eastern slope of the High Andes, where, although the occurrence of lodes are known, still the greater part of the gold is obtained and larger workings are carried on in the bed of the river itself during the dry season of the year. It is well known in commerce, however, that the gold from the whole of this district is extremely inferior to that of the districts further north, before referred to. It has a much less noble appearance, a smaller specific gravity, and contains a greater quantity of silver. A specimen from the washings carried on by SS. Gove et C^{ie} was examined, and afforded the following results.

The specific gravity was taken on two separate quantities of small pepites, size of from that of a small linseed to that of a melon-seed.

a. Specific gravity taken on 107·19 grs. gave 16·54 at 60° F.

b. Specific gravity taken on 119·12 grs. gave 16·63.

On analysis the results were respectively—

Gold	78·695	79·89
Silver	21·305	20·11
	100·000	100·00

Another specimen of still lighter colour gave a specific gravity of 15·83, but was not analyzed.

Gold from the River Chuquiaguillo.—This river, which flows into the river of La Pay immediately below that city, had been worked before the Spanish conquest from time immemorial by the Aymara Indians, and the gold obtained has been entirely due to the operations carried on for washing the bed of the river, as described in the Report to the Geological Society of London*. The gold obtained from these washings varies greatly in quality, but in general is of very inferior quality. The proprietor of the most extensive workings, Don Pedro Saientz, kindly presented me with the largest nugget which he had obtained, and which weighed 432 grs.

Its specific gravity at 60° Fahr. was found to be 16·693, and a chemical examination gave its composition as consisting of—

Gold	90·86
Silver.	9·14
	100·00

Native Silver from Chuquiaguillo.—Along with the above specimen of gold I received another nugget from the same locality, which was supposed to be native platinum†, but on examination

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* Communicated November 21, 1860: see Quart. Journ. Geol. Soc. 1861.

† I may here mention that although platinum is frequently said to occur in Bolivia, and on the late map of that country by Ondorza and Mujica very many localities of platinum are marked, I never succeeded whilst there in procuring any reliable evidence of its existing, or even ever having been found in that country.

turned out to be native silver, having irregularly disseminated through it patches or spots of black sulphide of silver. The entire nugget weighed 100·83 grs., and had a specific gravity of 10·77 at the temperature of 60° Fahr.

Two analyses were made upon respectively 7·91 and 8·85 grs. of the specimen, and afforded—

Silver	7·75	8·67
Gold	0·02	0·02
Sulphur	0·06	0·07
Insoluble	0·08	0·09
	7·91	8·85

Or, calculated to one hundred parts,—

Silver	97·84	97·98
Gold	0·28	0·22
Sulphur . . .	0·75	0·79
Insoluble . . .	1·13	1·01
	100·00	100·00

XIX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 73.]

May 18, 1865.—Major-General Sabine, President, in the Chair.

THE following communication was read:—

"On the Corrections for Latitude and Temperature in Barometric Hypsometry, with an improved form of Laplace's formula." By Alexander J. Ellis, F.R.S.

Adopting the notation in Table I. (p. 154), and the data of M. Mathieu (*Annuaire du Bureau des Longitudes*, 1865, p. 321), Laplace's hypsometrical formula, after some easy transformations, becomes

$$\begin{aligned} h_1-\mathrm{H}_1 &= [\log \mathrm{B}-\log b-{\cdot}00007\,.\,(\mathrm{M}'-m')\,.\,]\times[500+\mathrm{A}'+a'] \\ &\quad\times\left[\frac{18336}{500\,.\,(1-z\cos 2\,\mathrm{L})}\cdot\left(1+\frac{15926}{6366198}\right)\right]\times\left[1+\frac{h_1+\mathrm{H}_1}{6366198}\right] \\ &= [\log \mathrm{B}-\log b-{\cdot}00007\,.\,(\mathrm{M}'-m')]\times[500+\mathrm{A}'+a'] \\ &\quad\times\frac{36{\cdot}764}{1-z\cos 2\,\mathrm{L}}+\frac{h_1^{\,2}-\mathrm{H}_1^{\,2}}{\mathrm{R}_1}\quad\ldots\ldots\ldots\ldots\quad(a) \\ &= \mathrm{W}\,.\,\mathrm{T}'\,.\,\mathrm{G}_2+v_1-\mathrm{V}_1\quad\ldots\ldots\ldots\ldots\quad(b) \end{aligned}$$

In the last term in (*a*), $h_1-\mathrm{H}_1$ represents the product of the three preceding factors, $\mathrm{W}\,.\,\mathrm{T}'\,.\,\mathrm{G}_2$; and z is left for the present undetermined.

If γ be the total increase of gravity in proceeding from the equator to the pole, the coefficient $z=\gamma\div(2+\gamma)$*, for which most writers

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* The term $1-z\cos 2\,\mathrm{L}$ represents the ratio of the gravity at latitude L°, to the gravity at latitude 45°, which on the spheroidal theory of the earth's shape is

$$[1+\gamma\,.\,(\sin \mathrm{L})^2]\div(1+\tfrac{1}{2}\gamma),$$

and this gives the above value of z.

employ $\frac{1}{2}\gamma$, as they also commonly use $1+z\cos 2L$ for $1\div(1-z\cos 2L)$. The values assigned to z by different writers vary considerably. Laplace makes $z=\cdot002837$, and M. Mathieu (*Annuaire*, l. c.) gives $z=\cdot00265$. I have thought it, therefore, advisable first to consult the authorities who have calculated γ directly from pendulum experiments, next to calculate γ from the compression deduced from measurements of arcs*, and then, having determined z for each of these values of γ, to take the mean result to five places of decimals. The pendulum reductions are taken from Baily (*Mem. of Astron. Soc.* 1834, vol. vii. p. 94); the four first reductions are cited on the authority of the *Engl. Cyclop. A. & S.* vol. iv. col. 362, and the fifth from the *Proceedings of the Royal Society*, vol. xiii. p. 270. The following are the results.

Pendulum Experiments.

Baily, final result	$\gamma=\cdot0051449$	$z=\cdot0025659$
Sabine,	·0051807	·0025837
Airy,	·0051330	·0025599

Measurements of Arcs.

Airy,	$\gamma=\cdot0053273$	$z=\cdot0026566$
Bessel,	·0053252	·0026555
Everest,	·0054530	·0027191
Clarke,	·0052750	·0026306
Pratt,	·0052816	·0026339
Mean values	$\gamma=\cdot0052651$	$z=\cdot0026256$

Hence I adopt the value $z=\cdot00263$. This differs from Laplace's value by ·000207, and from that of M. Mathieu by ·00002. Viewed in relation to the possible errors which may arise from other sources this correction is slight, but it should be made on the principle advocated by Laplace, that it is assignable (*Méc. Cél.* vol. iv. p. 292). Adopting this value of z and reducing the formula (a) to English feet and Fahrenheit degrees, I have constructed Tables I. and II., which give formulæ and figures for calculating heights with every correction of Laplace, more readily than any other that I have seen. As there is no necessity to interpolate, the Tables are even simpler to use than M. Mathieu's (*Annuaire*, l. c.) or Loomis's (*Astronomy*, p. 390), and they are not only simpler but more complete than Baily's (*Astronomical Tables*, 1827, p. 111), which do not give the correction for the variation of gravity on the vertical. They have the further advantage of being applicable to both English and continental measures. The unavoidable uncertainties of the theory make it useless to consider more minute quantities than a foot, or the hundredth of a metre or of a toise. Hence only five-figure logarithms are required. The following examples will show the use of these Tables.

Ex. 1. (*Feet and Fahrenheit.*) Part of Glaisher's Balloon Ascent, 5th Sept. 1862. (*Report of British Association*, 1862.)

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* I have used Airy's formula $\gamma=\cdot008668-1\div c$, and not Biot's where the constant is ·00865, $1\div c$ being the compression.

B'	20·717	A	32·1	H	9885
b'	17·931	a	25·5	L	53
			836·0		
		T	893·6		

log B'	1·31633	W. T. G	3754
log b'	1·25360	H	9885
		v for 140009	
W	·06273	V for 10000	−5
log W	8·79748	h	13643
log T	2·95114		
lat. 53°, log G	1·82583		
log (W. T. G)	3·57445		

Ex. 2. (*Metres and Centigrade.*) Mont Blanc, taking St. Bernard as the lower station. (*Ann. Météorol. de France*, 1852.)

B'	·56803	A'	7·6	H_1	2463
b'	·42429	a'	−9·1	L	46
			500·0		
		T'	498·5		

log B'	9·75437	W. T'. G_2	2322
log b'	9·62766	H_1	2463
		v_1 for 4800	3·6
W	·12671	V_2 for 2400	−0·9
log W	9·10281	h_1	4787·7
log T'	2·69767		
lat. 46°	1·82610		
log G_2	9·73928		
log (W. T'. G_2)	3·36586		

Ex. 3. (*Toises and Centigrade.*) Monte Gregorio (cited by Bessel from *D'Aubuisson's Géognosie*, i. 481).

B	329·013	M'	19·85	A'	19·95	H_2	128·3
b	268·215	m'	10·5	a'	9·9	L	46
					500·0		
			9·35				
		×	·00007	T'	529·85		
		t	·00065				

log B	2·51721	log W	8·94488	W. T'. G_5	880·2
log b	2·42848	log T'	2·72415	H_2	128·3
		lat. 46°	1·82610	v_2 for 1000	0·3
	·08873	log G_5	9·44946	V_2 for 100	−0·0
t	·00065				
		log (W. T'. G_5)	2·94459	h_2	1008·8
W	·08808				

The coefficient 36·764 in (a) results from Ramond's comparison of trigonometrical with barometrical measurements (*Méc. Cel.* iv. 290). Bessel's theory, with the numbers corrected by Plantamour (*Ann. Météor. de F.* 1852), makes it 36·809. If this coefficient were adopted the values of log G in Table II. would have to be increased by ·00053. This would increase the results in the foregoing examples by 4 feet, 2·8 metres, and 1·3 toise respectively. Verification of these numbers by actual levelling is much needed, but it is rendered difficult by the uncertainty attending the correction for temperature *. Thus if $\mathrm{E}=1+\cdot 003665\,.\,\tau$, where τ degrees Centigrade is the temperature of the air at the height of x metres, and $\mathrm{X}=\mathrm{R}_1 x\div(\mathrm{R}_1+x)$, it becomes necessary in the determination of the formula to integrarte $d\,\mathrm{X}\div\mathrm{E}$ (see especially Bessel in *Schumacher's Astron. Nachr.* vol. xv. no. 356. art. 2. eq. 5), and consequently to know the relation between E and X. Laplace then says (*l. c.*), "comme les intégrales ne s'étendent jamais qu'à un intervalle peu considérable, relativement à la hauteur entière de l'atmosphère ; *toute fonction* qui représente à-la-fois les températures des deux stations inférieure et supérieure, et suivant laquelle la température diminue à-peu-près en progression arithmétique de l'une à l'autre, *est admissible,* et l'on peut chosir celle qui simplifie le plus le calcul." Bessel (*l. c.*) says "we are entirely ignorant of this relation, and have therefore no reason to assume the alteration of temperature as otherwise than proportional to the alteration of height." Laplace and Bessel then make an assumption which approximatively fulfils this condition and is equivalent to taking $\mathrm{E}^2+k\,.\,\mathrm{X}=$ a constant, k being determined by the observed temperatures at the two stations. This makes the integration easy, but it is evident that the result should not be applied in cases where the difference of level is not small in relation to the extent of the appreciable atmosphere, or where the temperature does not diminish approximately as the height increases. Now Mr. Glaisher, as the result of his observations on the diminution of temperature with increase of height, gives a series of average decrements such that on assuming the temperature to decrease m degrees Fahrenheit for an elevation of n thousand feet, and representing a degree Fahrenheit and a thousand feet, by a horizontal and a vertical unit of length respectively, we shall find that the resulting curve approaches to a rectangular hyperbola $mn+am+bn=0$, referred to axes parallel to its asymptotes. We may then by the principle of least squares determine the values of a and b from his Tables †. But on comparing such a curve with the

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* The errors in determining the actual temperatures of the air in mountain ascents, arising from the radiation of the ground, are not considered, because they are rather errors of observation than of theory.

† In an article in the *Reader* newspaper (31 Oct. 1863, p. 513), purporting to be an extract from Mr. Glaisher's Report to the British Association in 1863 (the passage does not occur in the published Report of the B. A.), it appears, on correcting two obvious misprints, that he has thus calculated $m=5\cdot 6295\,.\,n\div(1+0\cdot 048\,.\,n)$, giving $mn+20\cdot 8333\,.\,m-117\cdot 281\,.\,n=0$, for which $mn+21m-117n=0$ is a sufficiently close approximation, and represents the mean variation very fairly, after the first 5000 feet of ascent.

curves of alteration of temperature really observed*, the deviation from the average appears so great in particular cases, that no advantage would accrue from complicating the integration by the introduction of such a law.

The only course that appears open to pursue is to confine the limits of the integration to those small amounts which Laplace contemplated in the passage cited, and calculate the height by sections. For it also appears from Mr. Glaisher's curve, that for small alterations of height the alteration of temperature varies approximately as the alteration of height, that is, that the curve does not deviate materially from its tangent for comparatively considerable distances. When the difference of level is many thousand feet the difference of temperature is generally large, and the curve consequently differs materially from a straight line. No dependence can then be placed on the result. It would appear that we should be more likely to obtain correct results by dividing the whole height into a number of partial heights, not exceeding 1000 metres or 3000 feet, and taking fresh observations whenever the temperature altered abnormally. To have a rough notion of when this occurs, an aneroid barometer and common thermometer should be watched on the ascent. Mr. Glaisher's observations tend to show that we may expect on an average a fall of very nearly 4° Fahr. for each inch of depression of the barometer under a *cloudy* sky, the first inch, and the 11th to the 16th inch of depression being accompanied by a slightly more rapid fall of temperature. Under a *clear* or nearly clear sky, there is a fall of about 5° Fahr. for each of the first 4 inches of depression of the barometer; then about 4°·2 per inch from the 5th to the 13th inch, and about 4°·5 per inch from the 14th to the 16th inch†. This may therefore be considered as the normal alteration of temperature. In order to secure simultaneous observations at both stations for each section, it would be necessary to have two ascending parties, one for each variable station, each of which should be able to signal to the other. A stationary observer at the lowest station would serve as a check on the other two. This method introduces many practical difficulties, but the reduction of the observations is rendered very easy by Tables I. and II. The great importance of thus calculating heights by sections will be rendered evident by the following examples.

Taking the data in the *Ann. Météor. de F.* for 1852, p. 70, we have for Geneva as the lower and St. Bernard as the upper station, L 46,

B'	0·72643	A'	8·97	H_1	408,
b'	0·56364	a'	−1·89	h_1	2463.

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* Mr. Glaisher has laid down these in the *Proceedings of the British Meteorological Society*, vol. i. (19 Nov. 1862) plate 13, with which I have compared the theoretical hyperbola.

† These comparisons have been obtained by calculating the height attained for each inch of depression of the barometer, from the 1st to the 16th, taking for the bottom station B′ 30, A 60, H 0, L 45, and supposing the temperature to decrease according to Mr. Glaisher's Tables. The increase of height for each inch of depression was then divided by the number of feet of ascent in which, according to Mr. Glaisher, the temperature falls one degree at the height reached.

Again, for St. Bernard as the lower and Mont Blanc as the upper station,

B'	0·56803	A'	7·6	H_1	2463,
b'	0·42429	a'	−9·1	h_1	4787·7;

which has been calculated as Ex. 2 above.

But taking the data from the *Annuaire du B. des L.*, 1865, p. 324, we have for Geneva as the lower and Mont Blanc as the upper station,

B	729·65	M'	18·6	A'	19·3	H_1	408,
b	424·05	m'	−4·2	a'	−7·6	h_1	4815·9.

That is, the height of Mont Blanc above the sea, when calculated from observations at Geneva, St. Bernard, and the summit, is determined as 4787·7 metres, but when calculated from observations at Geneva and the summit only, is determined as 4815·9 metres, or 28·2 metres more. This is striking enough, but it is by no means clear that even the smaller amount may not be too large*.

Mr. Glaisher's balloon ascents offer a very convenient series of examples on account of the comparative closeness of his observations. I have therefore calculated two, Tables III. and IV., p. 156, which are important from their height or remarkable changes of temperature, first, by determining the height of each station from the lowest (which I call the *total* method); and secondly, by calculating the height of each station from the height of the next lower station (which I call the *gradual* method). I have added the differences of level between the stations as determined from both methods and the differences between them, which are important for discovering how the discrepancies between the two results are produced by temperature. Each station is lettered. Two letters against a number, as *a h* 5720, show that the height of the station *h* above the sea is found as 5720 feet, when station *a* is taken as the lower station with the height assigned to it in the same column. The distance *a h* is termed an interval. A careful examination of these results will show that the gradual method is probably the most trustworthy.

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* In the *Ann. Mét. de F.* (l. c.) M. Plantamour calculates the height of St. Bernard by Bessel's formula (taking account of the humidity of the atmosphere according to his hypothesis, which is, however, not in accordance with Mr. Glaisher's observations) as 2473 metres. In the *Annuaire de la Société Météorologique de France*, 1853, p. 249, M. Plantamour gives the height of the basin of the barometer at the hospice of St. Bernard as 2493 metres, but does not there state how this result was obtained. These heights being respectively 10 and 30 metres greater than that calculated by Laplace's formula, would, if adopted as the height of the lower station in the second calculation, give results more nearly in accordance with those in the third calculation. The object here, however, is to examine the action of Laplace's formula only, and hence the height assumed for St. Bernard must be that due to that formula. But different data give different results for this height. Geneva and St. Bernard are too widely separated horizontally, and have generally too great a difference of temperature, to enable us to calculate the whole height in one section with any degree of confidence, as there are probably many abnormal intermediate changes of temperature which, as will be seen, tend to vitiate the result. Nor can any reliance be placed on adopting the mean barometric pressures and temperatures. If any mean be taken, it must be the mean of many heights separately calculated from their individual data.

In Table III. up to station *i*, both results substantially agree, but in the interval *i j* there is a sudden *increase* of temperature, which is quite abnormal*. The total method, from omitting all considerations of the preceding lower temperatures, makes the height of the interval *i j* exceed its value as determined by the gradual method by 59 feet, an enormous amount in a total height of 7518 or 7579 feet. The temperature again decreasing from *j* to *k*, the difference is not so great, but the total method is 8 feet in *defect* for this interval. Again, for *m n* there is only a slight fall of temperature, and consequently the total method, ignoring the low absolute temperature of the interval, makes the difference of level greater than the gradual method by 27 feet. In *p q* there is absolutely a *rise* of temperature, and for the reason last stated, the total method makes the interval 73 feet greater than the gradual. The interval *q r* is a great contrast to this. The temperature falls very rapidly, 7°·1 for a barometric depression of ·79 inch, which is nearly double the normal amount as previously determined for the 14th inch of depression. Hence the total method, by distributing the cold over the warm parts, makes the interval *q r* 73 feet *less* than the gradual method. Again, *r s* shows an excess of 103 feet in the total method for a steady temperature, and *s t* a defect of 100 feet for a sudden fall of temperature. Mr. Glaisher's observations show that there was a rise and fall of temperature between *r* and *s*, but as there were no simultaneous observations of barometer and thermometer, I have not been able to introduce them into the calculation. The results after *r* are therefore very doubtful. The interval *v w* is liable to grave suspicion, not only from the great length of the interval, but the imperfect manner in which the observations were unavoidably made. Supposing the observations to be correct, the total method makes the interval *v w* greater than the gradual by no less than 610 feet, owing to its distributing the warm temperatures over so large an interval of extreme cold. If we then omit the interval *v w*, we find 359 feet for the sum of all the cases in which the total method was in excess of the gradual, and 201 feet for the cases of defect, leaving a total excess of 158 feet in 26450 or 26292 feet, which is thus shown to be a very inadequate measure of the degree of uncertainty due to the total method.

In Table IV. the results to *c*, or even *d*, substantially agree; but at *d* the temperature decreases very slowly, and soon becomes absolutely stationary. Great differences immediately appear. From *l* to *r* the temperature increases, and the total method gains greatly on the gradual till at *r* it is 541 feet in advance. At stations *s*, *t* the total method indicates a descent with a falling barometer, whereas the gradual method gives a very slow ascent. Mr. Glaisher's observations show that for the same barometric pressure of 14·637 inches, as at *r*, the temperature varied successively through 36°·1, 38°·2,

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* It is readily seen that on the assumed law of temperature, $E^2+k.X=$ constant; the sign of $dx \div dt$ depends on that of k, and is therefore supposed to be constant. When therefore $dx \div dt$ alters its sign during part of the height, the law is vitiated, and the formula inapplicable. The only chance of a decent approximation consists in separately calculating the intervals with decreasing and increasing temperatures.

38°·1, 42°·2 Fahr., which on the total method indicate different heights, whereas the gradual methods cannot admit any variation of height without a variation of pressure. The rapid fall of the thermometer from u to w causes the total method to give very much smaller intervals than the gradual, but the nearly stationary temperatures of x, y, z turn the balance the other way. On the whole, the total method gives 686 feet in excess, and 335 feet in defect of the gradual method, remaining 351 feet in excess. The temperature varied so abnormally in this ascent that little confidence can be reposed in either result after station h, when the total method is only 32 feet out of 9411 or 9379 in advance of the gradual, which is still a large amount.

It may be objected to the gradual method that, by multiplying stations, it multiplies errors of observation. But even when the stations are so unnecessarily multiplied as in Tables III. and IV. (in which nearly every recorded case of a simultaneous observation of barometer and thermometer has been admitted), the error is not likely to approach that arising from the total method. We may, however, calculate the ascent of Table III. as far as r, beyond which, as already remarked, the variation of temperatures renders the results uncertain, in six instead of sixteen stations, as follows.

Abridged Gradual Method.

Intervals abridged.	Gradual Method.		Difference of Level.		Abridged less Table III.
	Abridged.	Table III.	Abridged.	Table III.	
a	490	490	490	490	0
$a\,d$	3655	3655	3165	3165	0
$d\,f$	5017	5019	1362	1364	− 2
$f\,k$	9875	9885	4858	4866	− 8
$k\,n$	13633	13640	3758	3755	+ 3
$n\,p$	17552	17559	3919	3919	0
$p\,r$	20339	20357	2787	2798	−11

The final result is 18 feet less than that obtained in Table III. This difference may be easily accounted for. Up to f both results substantially agree. Between f and k there was first a rise and then a fall of temperature, which are overlooked in the abridged calculation, and it consequently loses 8 feet. In the interval $p\ r$ there was a steady temperature during 1400 feet, which disappears in the abridgement, and consequently it again loses 11 feet. It is evident, therefore, that the sections in this abridgement have been badly selected, and the importance of determining them rather by change of temperature than by height ascended becomes apparent. A better result is obtained by means of the seven sections $a\,i$ 6327, $i\,j$ 7520, $j\,k$ 9887, $k\,n$ 13649, $n\,p$ 17568, $p\,q$ 18963, $q\,r$ 20366, determined with reference to the change of temperature. The result, r 20366, is only 9 feet more than that of the gradual method in Table III., but is 104 feet less than that of the total method.

If β, β', β'' be the barometric readings reduced to 32° F., and α,

a', a'' the corresponding temperatures of the air for any three stations, then the formula (a) shows that, rejecting the small corrections v_1, V_1, the height, as determined by the total method, will be the same as that determined by the gradual method when

$$(a+a'') \cdot (\log\beta-\log\beta'')=$$
$$(a+a') \cdot (\log\beta-\log\beta')+(a'+a'') \cdot (\log\beta'-\log\beta''),$$

that is, when
$$\frac{a-a'}{a'-a''}=\frac{\log\beta-\log\beta'}{\log\beta'-\log\beta''}.$$

When the difference in barometric pressure is not great, and hence $\beta+\beta'$ is nearly $=\beta'+\beta''$, by applying the reductions in 'Proceedings,' vol. xii. p. 516, the above condition becomes very nearly, that the decrement of temperature should vary as the decrement of pressure, and this is the case for the normal decrements. Thus in Table III. the intervals *a i, j k, l m, n p* give for the quotients of the decrements of temperature divided by the decrements of pressure 4·635, 4·07, 3·26, 3·92 respectively, and the differences of the lengths of these intervals, as determined by the total and gradual methods, are only 2, —8, 13, 13 respectively. But for the intervals *i j, m n* these quotients are —3·55, 1·27, and the differences 59, 37. Similarly in Table IV., for the intervals *a d, a e, a h* the quotients are 4·78, 3·91, 3·97, and the differences —9, 31, 32. These results confirm the above conclusion, and also tend to show that the normal quotient is 4, and to explain why the gradual method is the most generally trustworthy.

Since, then, it is advisable to calculate by such short sections, the practical rules which I gave in a former paper ('Proceedings,' March 26, 1863, vol. xii. pp. 513, 514) may be condensed into one, which will enable any traveller to calculate heights without the assistance of any tables whatever. I conclude this paper, therefore, by annexing it in its improved form, together with a rule calculated on the same principles for foreign data, and an example of each to show the method of working.

PRACTICAL RULES WITHOUT ANY TABLES.

1. *English feet, Fahrenheit temperatures.*

Multiply the difference of the barometric readings in any unit by 52400, and divide by the sum of the barometric readings. [If the result be 1000, 2000, 3000, 4000, or 5000, add 0, 0, 2, 6, 14 respectively.]

Subtract $2\frac{1}{3}$ times the difference of the temperatures of the mercury.

Multiply the remainder by the result of first adding 836 to the sum of the temperatures of the air, next dividing by 900, [and finally

adding for latitude	0,	20,	30,	40,	45,
and subtracting for lat.	90,	70,	60,	50,	45,
the decimals........	·0026,	·0020,	·0013,	·0005,	0.]

To this product add the height of the lower station, [and if the sum is

sum is	5000,	10000,	15000,	20000,	25000,
add	1,	5,	11,	19,	30,

subtracting the same numbers when the upper numbers are the heights of the lower station.]

The final result is the height of the upper station above the sea-level according to Laplace's complete formula. [For British heights, the corrections in brackets may be omitted.] Fresh observations should be made whenever the temperature does not decrease about 4 degrees for a fall of one inch in the barometer. Calculate great heights in sections.

Ex. 4. The same data as Ex. 1, with the exception of H being the interval *kn* in the Table of the 'Abridged Gradual Method.'

B′ 20·717	A 32·1	H 9875
b′ 17·931	*a* 25·5	L 53
	836·0	
B′+*b*′ 38·648		
	900)893·6	
B′−*b*′ 2·786		
× 52400	·9929	
	−·0007 for lat. 53°	
38·648)145986·400(3777		
+6	*p* ·9922	
		3754
3783		9875 H
×·9922 *p*		+8 for 13000
		−5 for 10000
Approximative difference of level 3754		*h* 13632 feet.

Since decimals of a foot are rejected, there is always a liability to a difference of 1 or 2 feet between this and the logarithmic method. A difference of 10 feet between this result and that in Ex. 1, is due to the difference in the assumed value of H.

2. *French metres, Centigrade temperatures.*

Multiply the difference of the barometric readings in any unit by 16000, and divide by the sum of the barometric readings. If the result be 300, 600, 900, 1200, subtract 0·6, 0·9, 0·9, 0·2; if 1300, 1600, add 0·2, 2·0 respectively.

Subtract 1·3 times the difference of the temperatures of the mercury.

Multiply the remainder by the result of first adding 500 to the sum of the temperatures of the air, then dividing by 500, and finally

adding for latitude	0,	20,	30,	40,	45,
and subtracting for lat.	90,	70,	60,	50,	45,
the decimals	·0026,	·0020,	·0013,	·0005,	0.

To this product add the height of the lower station; and if the

sum is	1000,	2000,	3000,	4000,	5000,	6000,	7000,	8000
add	·2,	·6,	1·4,	2·5,	3·9,	5·7,	7·7,	10·1,

subtracting the same numbers when the upper numbers are the height of the lower station.

Fresh observations should be taken whenever the temperature does not decrease about 2° for a fall of 25 millimetres in the barometer. Calculate great heights in sections.

Ex. 5. Height of St. Cergues, in the Canton de Vaud, on the road from Paris to Geneva, lat 46°. (*Ann. Météor. de Fr.* 1849, p. 59.)

B 729·71
b 676·73
$B+b$ 1406·44
$B-b$ 52·98
×16000
1406·44)847680·00(602·7
−·9
601·8
$-3·5\ q$
app. diff. of level 598·3

M' 21·5
m' 18·8
2·7
×1·3
q 3·5

A' 21·8
a' 18·8
500·0
500)540·6
1·0812
−·0001 lat. 46°
p 1·0811

H_1 408

598·3
$\times 1·0811\ p$
646·8
$408·0\ H_1$
+·2 for 1000
h_1 1055 metres.

Table I.

NOTATION (CAPITALS, lower station; *small letters*, upper station).

B. b units of length of any kind, height of barometer.
B', b' the same reduced to 32° Fahr.
A, a deg. Fahr., A', a' deg. Cent., A'', a'' deg. Réaum., temperature of air.
M, m ,, ,, M', m' ,, ,, M'', m'' ,, ,, temperature of mercury.
H, h feet, H_1, h_1 metres, H_2, h_2 toises, height above sea.
V, v ,, V_1, v_1 ,, V_2, v_2 ,, correction for height.
R ,, R_1 ,, R_2 ,, mean radius of earth.
$\log R=7·3199534$, $\log R_1=6·8039605$, $\log R_2=6·5141407$.
L degrees, mean latitude of the two stations.
$t=·000039\,.\,(M-m)=·00007\,.\,(M'-m')=·000088\,.\,(M''-m'')$.
$T=A+a+836$; $T'=A'+a'+500$; $T''=A''+a''+400$.
$W=\log B-\log b-t=\log B'-\log b'$.
$\log G=\log 60309·19-\log 900-\log(1-·00263\cos 2L)$.

$\log G'=\log G+0·25527$
$\log G''=\log G+0·35218$
$\log G_1=\log G+9·48401-10$
$\log G_2=\log G+9·73928-10$

$\log G_3=\log G+9·83619-10$
$\log G_4=\log G+9·19419-10$
$\log G_5=\log G+9·44946-10$
$\log G_6=\log G+9·54637-10$

1 metre=3·28090 feet, 1 toise =6·39459 feet, 1 toise =1·94904 metres.
log 3·28090= ·51599, log 6·39459= ·80581, log 1·94904= ·28982.

FORMULÆ.

Result.	*Temp.*	
Feet	Fahr.	$h=W.T.\ G+H+v-V$
,,	Cent.	$=W.T'.\ G'+H+v-V$
,,	Réaum.	$=W.T''.G''+H+v-V$
Metres	Fahr.	$h_1=W.T.\ G_1+H_1+v_1-V_1$
,,	Cent.	$=W.T'.\ G_2+H_1+v_1-V_1$
,,	Réaum.	$=W.T''.G_3+H_1+v_1-V_1$
Toises	Fahr.	$h_2=W.T.\ G_4+H_2+v_2-V_2$
,,	Cent.	$=W.T'.\ G_5+H_2+v_2-V_2$
,,	Réaum.	$=W.T''.G_6+H_2+v_2-V_2$

Log G is found from the latitude in Table II., without interpolation.

V, v; V_1, v_1; V_2, v_2 are found from the nearest number of thousand feet, two hundred metres, or hundred toises in H, h, H_1, h_1, H_2, h_2 respectively, by Table II. without interpolation.

Make fresh observations when the temperature does not decrease about 4° F., or 2° Cent. for a fall in the barometer of 1 inch, or 25 millimetres respectively.

TABLE II.

Lat.	Log G.	Lat.	Log G.	Feet.		Metres.		Toises.	
				h, H ÷1000.	v, V.	h_1, H_1 ÷100.	v_1, H_1.	h_2, H_2 ÷100.	v_2, V_2.
0°	1·827 28	45°	1·826 14	0	0	0	0·0	0	0·0
1	28	46	10	1	0	2	0·0	1	0·0
2	28	47	06	2	0	4	0·0	2	0·0
3	28	48	1·826 02	3	0	6	0·1	3	0·0
4	27	49	1·825 98	4	1	8	0·1	4	0·1
5	1·827 27	50	94	5	1	10	0·2	5	0·1
6	26	51	90	6	2	12	0·2	6	0·1
7	25	52	86	7	2	14	0·3	7	0·2
8	24	53	83	8	3	16	0·4	8	0·2
9	23	54	79	9	4	18	0·5	9	0·3
10	1·827 22	55	1·825 75	10	5	20	0·6	10	0·3
11	20	56	71	11	6	22	0·8	11	0·4
12	19	57	68	12	7	24	0·9	12	0·4
13	17	58	64	13	8	26	1·1	13	0·5
14	15	59	61	14	9	28	1·2	14	0·6
15	1·827 13	60	1·825 57	15	11	30	1·4	15	0·7
16	11	61	54	16	12	32	1·6	16	0·8
17	09	62	50	17	14	34	1·8	17	0·9
18	07	63	47	18	16	36	2·0	18	1·0
19	04	64	44	19	17	38	2·3	19	1·1
20	1·827 02	65	1·825 41	20	19	40	2·5	20	1·2
21	1·826 99	66	38	21	21	42	2·8	21	1·4
22	96	67	35	22	23	44	3·0	22	1·5
23	94	68	32	23	25	46	3·3	23	1·6
24	91	69	29	24	28	48	3·6	24	1·8
25	1·826 88	70	1·825 27	25	30	50	3·9	25	1·9
26	84	71	24	26	32	52	4·3	26	2·0
27	81	72	22	27	35	54	4·6	27	2·2
28	78	73	20	28	38	56	4·9	28	2·4
29	75	74	17	29	40	58	5·3	29	2·6
30	1·826 71	75	1·825 15	30	43	60	5·7	30	2·8
31	68	76	13	31	46	62	6·0	31	2·9
32	64	77	12	32	49	64	6·4	32	3·1
33	61	78	10	33	52	66	6·8	33	3·3
34	57	79	08	34	55	68	7·3	34	3·5
35	1·826 53	80	1·825 07	35	59	70	7·7	35	3·8
36	49	81	06	36	62	72	8·1	36	4·0
37	46	82	04	37	65	74	8·6	37	4·2
38	42	83	03	38	69	76	9·1	38	4·4
39	38	84	03	39	73	78	9·6	39	4·7
40	1·826 34	85	1·825 02	40	77	80	10·1	40	4·9
41	30	86	01	41	80	82	10·6	41	5·2
42	26	87	01	42	84	84	11·1	42	5·4
43	22	88	00	43	89	86	11·6	43	5·7
44	18	89	00	44	93	88	12·2	44	5·9
45	1·826 14	90	1·825 00	45	97	90	12·7	45	6·2

Table III.—Mr. Glaisher's Ascent from Wolverhampton, lat. 53°, 5 September, 1862.

Station.	B′.	A.	Interval.	Total method.	Interval.	Gradual method.	Difference of Level.		Total less Gradual.
							Total method.	Gradual method.	
a	29·40	59·5	*a*	490	*a*	490	490	490	0
b	29·17	59·0	*a b*	708	*a b*	708	218	218	0
c	28·38	55·5	*a c*	1467	*b c*	1467	759	759	0
d	26·19	45·5	*a d*	3655	*c d*	3655	2188	2188	0
e	24·994	42·0	*a e*	4917	*d e*	4911	1262	1256	6
f	24·894	40·7	*a f*	5021	*e f*	5019	104	108	− 4
g	24·30	39·5	*a g*	5671	*f g*	5663	650	644	6
h	24·25	38·0	*a h*	5720	*g h*	5718	49	55	− 6
i	23·70	36·5	*a i*	6327	*h i*	6325	607	607	0
j	22·658	40·0	*a j*	7579	*i j*	7518	1252	1193	59
k	20·717	32·1	*a k*	9938	*j k*	9885	2359	2367	− 8
l	20·17	31·2	*a l*	10646	*k l*	10586	708	701	7
m	18·727	26·5	*a m*	12590	*l m*	12517	1944	1931	13
n	17·931	25·5	*a n*	13740	*m n*	13640	1150	1123	27
p	15·38	15·5	*a p*	17672	*n p*	17559	3932	3919	13
q	14·553	15·6	*a q*	19140	*p q*	18954	1468	1395	73
r	13·76	8·5	*a r*	20470	*q r*	20357	1330	1403	− 73
s	12·754	8·1	*a s*	22459	*r s*	22243	1989	1886	103
t	11·954	0·0	*a t*	23955	*s t*	23839	1496	1596	−100
u	11·254	− 2·0	*a u*	25472	*t u*	25304	1517	1465	52
v	10·803	− 5·0	*a v*	26450	*u v*	26292	978	988	− 10
w	7·0	−12·0	*a w*	37427	*v w*	36659	10977	10367	610

TABLE IV.—Mr. Glaisher's Ascent from Wolverhampton, lat. 53°, 17 July, 1862.

Station.	B′.	A.	Interval.	Total Method.	Interval.	Gradual Method.	Difference of Level.		Total less Gradual.
							Total Method.	Gradual Method.	
a	29·193	59·0	*a*	490	*a*	490	490	490	0
b	26·014	45·0	*a b*	3643	*a b*	3643	3153	3153	0
c	25·215	43·0	*a c*	4487	*b c*	4481	844	838	6
d	24·138	34·8	*a d*	5633	*c d*	5642	1146	1161	− 15
e	22·421	32·5	*a e*	7612	*d e*	7581	1979	1939	40
f	22·023	32·5	*a f*	8095	*e f*	8050	483	469	14
g	21·575	29·8	*a g*	8620	*f g*	8588	525	538	− 13
h	20·927	26·2	*a h*	9411	*g h*	9379	791	791	0
i	19·629	26·0	*a i*	11127	*h i*	11035	1716	1656	60
j	19·281	26·0	*a j*	11633	*i j*	11497	506	462	44
k	18·633	26·0	*a k*	12524	*j k*	12381	891	884	7
l	18·136	24·9	*a l*	13233	*k l*	13079	709	698	11
m	17·235	31·0	*a m*	14693	*l m*	14402	1460	1323	137
n	16·735	31·6	*a n*	15495	*m n*	15173	802	771	31
p	16·036	32·0	*a p*	16655	*n p*	16290	1160	1117	43
q	14·937	37·2	*a q*	18673	*p q*	18163	2018	1873	145
r	14·637	38·2	*a r*	19243	*q r*	18702	570	539	31
s	14·634	36·5	*a s*	19214	*r s*	18707	− 29	5	− 34
t	14·633	34·0	*a t*	19164	*s t*	18709	− 50	2	− 52
u	14·134	31·5	*a u*	20052	*t u*	19620	888	911	− 23
v	13·637	24·5	*a v*	20864	*u v*	20551	812	931	− 119
w	13·137	19·2	*a w*	21742	*v w*	21508	878	957	− 79
x	12·139	17·5	*a x*	23804	*w x*	23519	2062	2011	51
y	11·741	16·0	*a y*	24654	*x y*	24365	850	846	4
z	11·143	16·0	*a z*	26039	*y z*	25688	1385	1323	62

GEOLOGICAL SOCIETY.

[Continued from vol. xxix. p. 551.]

May 10, 1865.—W. J. Hamilton, Esq., President, in the Chair.

The following communications were read:—

1. "On the Azoic and Palæozoic Rocks of Southern New Brunswick." By G. F. Matthew, Esq.

After briefly narrating the History of the Geology of the Region, the author described each of the formations successively in detail; namely, the Laurentian (Portland series), Huronian (Coldbrook group), Lower Silurian (St. John group), Upper Silurian, Middle and Upper Devonian (including the Bloomsbury group, Little River group, and Mispeck group), Lower Carboniferous, and Carboniferous. The only important hiatus, therefore, is that wherein the Trenton limestones and Hudson River shales should fall; and those formations probably form part of the Lower Silurian rocks already known. Mr. Matthew then stated that it is now a well-established fact that throughout Palæozoic time the centre of the North American continent was comparatively stable, the whole series of formations being found in continuous and conformable succession, from the base of the Silurian to the summit of the Permian.

The stratigraphical peculiarities of the several formations, in regard to their mutual relations, were next described; and the author inferred the existence of at least three breaks, and possibly a fourth (between the two sections of the Carboniferous system), in the Palæozoic series of Acadia—namely, between the Huronian and the Silurian, between the Lower and Upper Silurian, and between the "Middle and Upper Devonian" and the "Lower Devonian and Upper Silurian."

2. "Results of Geological Observations in Baden and Franconia." By Dr. F. Sandberger, For. Corr. G.S.

In this paper Dr. Sandberger communicated the results he has arrived at by the study of the Palæozoic, Triassic, and Jurassic Beds of Baden and Franconia. The so-called "transition formation" of the Black Forest he had previously ascertained to be Lower Carboniferous; it is immediately succeeded by the strata of Berghaupten near Offenberg, which also occur in Alsace. Near Oppenau occurs a species of *Pterophyllum* 3 feet long, which affords a new proof of the close connexion between the Triassic and Palæozoic floras; and to this fact may be added the discovery of a true *Schizopteris* in the Letten-coal near Würzburg. This connexion Dr. Sandberger also considers more perceptible in the fauna than has hitherto been supposed.

The Wellenkalk, Muschelkalk, and Letten-coal appear better developed in Franconia than elsewhere in Germany, and the clearness of the stratification leaves no doubt about the order of succession. Amongst the results of a comparison of the Thüringian and Swabian types with those near Würzburg is the discovery of the fauna of Recoaro and Mickelschütz in the Middle Wellenkalk; and the author remarks that as the rocks of the Alpine so-called Muschelkalk entirely agree with the Wellenkalk of his district, that rock ought henceforth to be called Wellenkalk; for no representative of the true (Upper) Muschelkalk has hitherto been observed in the Alps.

The Jurassic rocks occurring in Baden he refers to the Cornbrash and the Inferior Oolite.

3. "On the Changes rendered necessary in the Geological Map of South Africa, by recent discoveries of fossils." By Dr. R. N. Rubidge, F.G.S.

Dr. Rubidge first called attention to a former paper, in which he pointed out the occurrence of horizontal beds of sandstone resting on the upturned edges of gneiss, and continuous with inclined sandstone of like kind interstratified with gneiss. He therefore conjectured that the Clay-slate and Bokkeveldt schist, which Bain considered distinct, belonged to one formation, that they are of the same age as the gneiss, and that the "Carboniferous rocks" of the Eastern province were not separable from the Clay-slate which Mr. Bain had called Primitive clay-slate. It follows from this, that if the clay-slate proved Devonian, as Dr. Rubidge believed it would, the horizontal quartzite must be much newer, and probably an outlying mass of the Dicynodon-rocks. He explained these phenomena by supposing that rocks of widely different ages had been metamorphosed into masses having the same mineralogical characters.

The discovery of certain fossils has lately verified the conjecture respecting the Devonian age of the clay-slates and Bokkeveldt rocks; and Dr. Rubidge therefore infers that the rest of the old rocks are of the same age. Finally, the discovery of a Calamite in the sandstone, not unlike some specimens belonging to the same genus found in the Dicynodon-rocks, renders the probability of the truth of the second conjecture very great.

XX. *Intelligence and Miscellaneous Articles.*

ON AN ELECTRICAL INDUCTION MACHINE.

BY M. HOLTZ.

THE object of this apparatus is to excite by means of induction a greater quantity of electricity than that which exercises the inducing action. In its simplest form it consists of a fixed glass disk, on one side of which an even number of tinfoil sectors are pasted, which receive from a small electrical machine alternately positive and negative electricity. In front of this disk, on the glass side, is a second glass disk provided with the same number of tinfoil sectors, which is put in rapid rotation. As these sectors, one of which is connected with another by means of two tinfoil rings, pass in front of the fixed one, positive and negative electricity are alternately driven out and taken up by two suckers, by means of which it can be further used. The concentrating effect of this apparatus, which has not as yet been brought to its greatest perfection, is unmistakeably seen in the spark which can be obtained with it, in its power of charging batteries and jars, and in the development of luminous phenomena *in vacuo,* in which action it resembles the induction-coil machine. It exerts chemical actions, too, but obviously only feeble ones, like the apparatus which twenty years ago (and unknown to M. Holtz) was constructed solely for this purpose by Mr. Goodman, but without success. This was essentially constructed on the same principle, but with a different and very disadvantageous application.—*Berliner Berichte,* April 1865.

ON THE OPTICAL PROPERTIES DEVELOPED IN DIFFERENT KINDS OF GLASS BY THE PASSAGE OF AN ELECTRIC DISCHARGE. BY M. A. DE LA RIVE.

In continuation of several experiments on the modifications which the passage of an electric current, either continuous or discontinuous, might produce in the rotatory magnetic power of various liquid conductors, I was led to submit to experiment a specimen of crown glass which had been perforated by the discharge of a large Ruhmkorff's coil. I found that this specimen had almost entirely lost rotatory magnetic power, and, on the contrary, had acquired the properties of a crystallized body and of annealed glass.

Since then, thanks to M. Ruhmkorff, who has kindly placed his large apparatus at my disposal, I have made similar trials on another specimen of crown glass, on one of flint glass, and on one of Faraday's heavy glass.

All have given the same result as the first specimen which I tried, as I ascertained by comparing them with similar specimens which had not been submitted to the action of the discharge.

Hence the passage of a strong electric discharge through transparent insulating bodies imparts to these bodies a permanent molecular modification of the same kind as that which is produced, but only momentarily, by heat and pressure. The curious circumstance is, that this modification is not limited merely to the parts traversed by the discharge, but extends to the entire mass, provided this mass do not exceed certain limits; its texture undergoes thus a permanent change resembling that produced by tempering, and for this the simple shock imparted to the glass by the passage of the discharge is sufficient.—*Comptes Rendus,* May 15, 1865.

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

[FOURTH SERIES.]

SEPTEMBER 1865.

XXI. *Observations on the Polarization of the Atmosphere, made at St. Andrews in* 1841, 1842, 1843, 1844, *and* 1845. *By* Sir David Brewster, *K.H., D.C.L., F.R.S., &c.*

[Concluded from p. 129.]

IN the year 1840, when M. Babinet had occasion to visit the sea coast, he proposed to observe if the neutral point of Arago varied in its height as the sun rose or set, and to observe it also when the sun was beneath the horizon; but he was allured from these observations by a circumstance which he had never even suspected, namely, the existence of a *second neutral point* above the setting sun, and nearly as high in the atmosphere as the neutral point of Arago. "I afterwards"*, he says, "determined a great number of times the position of this new neutral point, which appeared in the west even when the sun was just on the horizon before setting, and in the east when he had risen only a few degrees. A very imperfect estimate made me sometimes believe that this new neutral point was a little less high than that of Arago."

Observations on Babinet's *Neutral Point.*

The following observations on this neutral point were made generally on the same day, and even in the same hour, as those on Arago's neutral point, and therefore under the same atmospherical influences.

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* *Comptes Rendus,* &c. 1840, vol. xi. p. 619.

1841, May 12.—Wind west in the evening.

Mean time.	R=maximum polarization. In zenith.	 In horizon.	Height of neutral point above the sun*.
h m		° '	° '
8 10 P.M.	..	..	16 4

1841, May 16.—Barom. 29·24 in. Windy.

7 0	..	..	8½

1841, June 10.—Barom. 29·27 in. Very fine sky.

10 10 A.M.	Sun's altitude about 50°.		2° or 3°
7 17 P.M.	..	..	13 15
7 35 „	..	..	14 55
7 52 „	..	..	14 5
8 8 „	..	..	16 37
8 21 „	Sun sets...	..	15 27
8 39 „	..	..	17 15
8 52 „	..	..	16 25
9 3 „	..	..	16 42
9 15 „	..	..	16 20

1841, September 6.—Barom. 29·55 in.

			° '
6 30	..	..	16 15
6 45	..	..	14 45
6 58	..	..	12 55
7 6	..	..	13 8

1841, September 12.—Barom. 29·75 in. Sky clear.

6 54	..	..	18 53
7 0	..	..	18 20

1841, September 29.—Barom. 28·73 in. After rain, sky clear.

5 46	..	..	12 53
5 57	..	..	14 55
6 7	..	..	16 15

1841, October 23.—Rainy day; cleared up at 3^h P.M.

4 21	..	..	17 33
27	27½	25½	15 35
30	..	..	14 30
39	..	..	15 13
45	26	25½	14 22
53	..	..	15 53

1841, November 2.

4 50	..	..	14 51

1841, November 4.—Barom. 30·2 in. Foggy day; sky tolerably free from clouds.

3 13	..	..	14 4
4 9	..	..	19 0
4 14	..	..	17 48

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* In the Edinburgh Transactions, vol. xxiii. p. 223–228, the words *antisolar point* are by mistake used in place of *the sun*.

1841, November 25.—Barom. 29·63 in. Splendid day.

Apparent time.	R=maximum polarization.		Height of neutral point above the sun.
	In zenith.	In horizon.	
h m	°	°	° ′
10 31 P M.	..	..	3 20
12 9 „	27½	25½	6 24
12 26 „	..	..	12 5
2 24 „	28½	26½	13 47
3 10 „	..	..	16 35

1842, January 29.—Barom. 29·93 in. Fine day; clear sky; snow covers the ground partially.

3 32	27	21½	16 35
3 53	..	..	17 38
4 8	..	..	17 51

1842, February 15.—Rain in morning, then a fine day.

4 25	..	..	21 58
4 44	..	..	20 24
4 55	27½	22½ in S.E. hor.	20 30

At this hour clouds rose in the S., N., and N.E. horizon; a dark band of distant haze 6° or 8° high rose above the sea horizon.

1842, February 21.—Fine day; wind west.

4 35	..	..	18 57
5 8	..	..	18 32

1842, March 2.—A wet day; the place of the sun was seen only as a white spot. The polarization everywhere feeble.

$2^h\ 20^m$.—The neutral point was 75° above the horizon, or about 54° above the sun. The polarization was negative from the neutral point to the horizon beneath the sun, and positive to the horizon opposite to the sun.

1842, March 16.—Barom. 29·96 in. The sun occasionally shining through a thickish haze in a China-ink sky without any blue; the wind in the south-west, and slight.

$10^h\ 45^m$.—Sun's altitude about 30½°; the polarization below the sun was negative down to the horizon; the neutral point was 30° above the sun, or more than 60° high!

1842, April 5.—Barom. 30·07 in. Splendid sky.

6 23	30	26½	19 17
6 58	..	..	18 46
7 7	30½	..	15 45

The polarization was unusually great, equivalent to a rotation of the plane of polarization of about 29°; it became stronger as it grew darker.

N.B.—This day and April 8th were the only days on which I was able to observe the three neutral points and determine their place. (See p. 168.)

1842, April 8.

Apparent time.	R=maximum polarization. In zenith.	R=maximum polarization. In horizon.	Height of neutral point above the sun.
h m	°	°	° ′
6 0	24½	18½	17 15
6 25	..	..	18 20
6 45	27½	..	19 11
7 7	28¾	..	21 5
7 20	..	..	19 31

1842, April 13.—Barom. 30·12. in. Fine day.

5 51	..	..	18 50
6 23	29	22½	18 0
6 58	29½	..	18 40
7 12	..	..	17 35
7 21	30½	..	17 57

At 7^h 32^m the maximum polarization or rotation was 32½°, the greatest ever observed. (See p. 174.)

1842, May 15.—A haze.

6 34	15	15	18 10
6 48	..	..	19 55
7 28	20½	..	17 44
8 29	23½	..	18 1

1842, November 14.—Barom. 29·6 in. Fine, frosty and clear morning.

8 58 A.M.	25½	19½	14 7

1842, November 21.—Barom. 29·77 in. Frosty morning. Thermometer 31°.

9 4 A.M.	..	..	18 36

1842, December 28.—Barom. 29·56 in.

11 40 A.M.	29	..	13 41
12 3 P.M.	27	18½	13 37
1 4 ,,	..	..	25 25!
1 23 ,,	..	..	27 30!
2 31 ,,	14½	..	27 48!

These remarkable results were doubtless owing to the causes which produced the following phenomenon:—At 1^h 4^m, when the neutral point started away from the sun, a white halo of 45° appeared round the sun and continued till 2^h 31^m. It was slightly brown on its inner rim. At 1^h 23^m the altitude of the halo was 32° 10′, and the sun's altitude 8° 30′, so that the radius of its outer rim was 23° 40′.

When the vertical bands of the polariscope passed over the apex of the halo *their intensity* was increased, and when they passed over the halo in a direction parallel to its horizontal diameter their intensity was diminished. As the crystals of ice, by which the halo was produced, are doubly refracting, one of the pencils must have been weaker than the other, an effect which would be produced if the surfaces of the crystals were not perfectly smooth*.

The polarization of the sky was greatly reduced by the crystals of ice floating in the air which produced the halo.

1843, March 25.—Barom. 29·97 in.

Apparent time.	R=maximum polarization. In zenith.	In horizon.	Height of neutral point above the sun.
h m			° '
5 46	28	..	18 25
6 11	29½	..	17 41

1843, April 17.—Barom. 29·84 in. Fine day; wind east.

6 33	..	..	17 32
7 7	..	..	18 4
7 33	..	..	18 44

1843, April 29.—Barom. 29·63 in. Wind east. The heat of the rising sun very great, and the vapour rising copiously from the ground. Everything more than a quarter of a mile distant invisible. The sun shone occasionally, showing his pale white disk.

10 20 A.M. The bands were negative from the zenith through the sun to the horizon, and positive to the opposite horizon. The maximum polarization was 90° from the sun, but was so weak that it was compensated by the refraction of a plate of ground glass, at an angle of incidence of about 30°, the negative bands being then scarcely visible.

2 55 P.M. The vapour still rising copiously, and the place of the sun not visible. The bands were hardly visible, the polarization, when a maximum, being compensated by the refraction of a plate of glass at an angle of 5°.

1843, May 3.—Barom. 29·65 in. Easterly haur.

4 25 Mist flying before the sun, and the neutral point oscillating from near the sun to the zenith as the mist thickens.

1843, June 21.—Barom. 29·75 in. Fine day; wind west.

7 46	..	..	15 55
8 33	29	..	17 57
9 14	30	..	16 28

1844, February 3.—Snow covering the ground.

4 11	26	23	17 55

1844, April 15.

5 54	..	..	25 6
6 50	..	..	18 10

1844, June 10.—Barom. 29·70 in.

8 20	24¾	..	18 14

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[*] See Phil. Trans. 1819, p. 146.

In a normal state of the sky, when the sun is rising or setting in a fine day without clouds, the neutral point of Babinet is situated about 18° 30′ above the sun. Owing to the great quantity of light in the neighbourhood of the sun, this neutral point is not so easily seen as that of Arago, and escaped the scrutiny of that distinguished observer. In high latitudes it is above the horizon the greater part of the year, and, being above the sun, it is of course always visible in a clear sky, when he is above the horizon. When the sun is in the zenith this neutral point coincides with the sun's centre—its distance from the sun gradually increasing till it becomes 18° 30′ at sunrise or sunset, when the sun's altitude is nothing.

Like that of Arago, the neutral point of Babinet *must* be accompanied, in certain states of the horizontal sky, with a secondary neutral point, but I have never had an opportunity of observing it.

Observations on Brewster's *Neutral Point.*

This neutral point, which I discovered in 1842, is much more difficult to be seen than that of Babinet. In November, December, and January it cannot be seen in our latitude, unless when, early in November and late in January, a high degree of polarization in the sky brings it above the horizon at noon.

The following interesting remarks of M. Babinet on his successful attempt to confirm the existence of this neutral point, explain in the clearest manner the causes of the difficulties which he experienced (and which every future observer will experience) in observing this remarkable spot with its surrounding polarizations:—

"On the 23rd July," says M. Babinet, "after having observed from half-hour to half-hour the polarization of the sky below the sun, the regularity of this polarization appeared to change after 4^{h}; and from $4\frac{3}{4}^{h}$ to $5\frac{1}{4}^{h}$ I observed, in placing the bands horizontally—

"1st. A space without polarization below the sun;

"2nd. Below this space a second space, where the bands were certainly seen.

"3rd. Lower still a neutral space, where no bands were seen; and,

"4th. In approaching to the horizon, a fourth space, where the bands were very visible. The phenomenon is therefore no longer doubtful; but the immense brightness of the sun in a clear day, the intense illumination of the atmosphere in the region immediately below him, and the reflexion from the strongly illuminated earth, all concur in rendering this observation difficult to make, and very painful to the eyes, even if we take the precaution of shielding the head and the polariscope from the direct rays of the sun, and the reflexion from the earth. . . . . . M. Brewster was doubtless guided in his research by theoretical views; otherwise it appears to me very improbable that, by merely observing the atmospherical polarization, he could have made the remarkable discovery of this neutral point, so difficult to see, and which, after him, I have several times tried in vain to rediscover. . . . . . The small quantity of polarized light which is observed between the neutral point of Brewster and the sun, seems to me to reach the limit which it is possible to observe, and perhaps to exceed the limit which it is possible to measure"*.

1841, November 17.—Barom. 29·43 in.

Apparent time.
h m
12 0. The polarization between the sun and the horizon decidedly *negative*, but no neutral point there.

1842, February 16.—Barom. 30·16 in.

12 0. Sun's altitude about 21° 13′; a China-ink muddy sky. There is clearly a neutral space below the sun and a little above the horizon. The bands on the sun and below him are negative; but as the negative polarization becomes very weak, it must pass into a neutral point.

1842, February 18.—Cold, wind west, and rather stormy.

12 0 The neutral point below the sun was distinctly seen, the polarized bands being negative over and below the sun, and below the neutral point positive down to the horizon. The sun's altitude was about 22°, and the distance of the neutral point from the sun about 15°.

1842, February 21.—Barom. 20·44 in. Fine, dry day; wind west.

12 39 Neutral point 6½° above the horizon, and its distance from the sun 15° 0′.

The positive polarization between the neutral point and the horizon was compensated by the refraction of *one* plate of glass, at 20° of incidence. The neutral line was convex towards the sun in the west horizon.

1842, March 10.—Sky clear in zenith; wind west.

11 15 The neutral point distinctly seen below the sun.

1842, April 3.—Barom. 29·8 in. Fine, clear sky; cold; hail in the afternoon.

11 45 Neutral point below the sun very distinctly seen, the sun's altitude being about 39½°, and the height of the neutral point 26½°. The distance of the neutral point from the sun was 13°. The bands over the sun down to the neutral point were negative, and those below it down to the horizon positive.

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* *Comptes Rendus*, &c. 1846, vol. xxiii. p. 234.

1842, April 5.—Barom. 30·07 in. Splendid sky.

Apparent time. h m		Distance of neutral point from sun.
12 27	Neutral point seen distinctly below the sun.	
12 27	.. ..	15°25′
12 53	.. ..	14 40
12 56	.. ..	15 45
4 13	.. ..	15 35
4 33	.. ..	15 22

N.B. All the three neutral points were observed and their places ascertained this day, and also on April 8.

1842, April 6.—Barom. 30·05 in. Considerable haze.

8 51	Neutral point distinctly seen below the sun.	
11 6	.. ..	12 0

1842, April 8.

2 7	The neutral point below the sun beautifully seen. Estimated distance from the sun..................	16 0

1842, April 15 and 17.

3 0	Neutral point below the sun distinctly seen.	
3 30	.. ..	

1842, April 20.—Barom. 30·02 in. Very fine day; wind west.

12 10	.. ..	11 20
12 37	.. ..	10 40
2 21	.. ..	12 0
3 45	.. ..	12 35

The maximum polarization was very great.

1842, April 25.—Thin hazy clouds before the sun.

1 11 Sun's alt. 45½°. Alt. neutral point below sun 34°. Distance below sun 11½°.

1842, April 26.—Barom. 30·08 in. Not a cloud in the sky from morning till night.

11 1	.. ..	12 15
11 46	.. ..	12 30
3 30	.. ..	14 35
3 35	.. ..	15 5
4 10	.. ..	17 45

1842, April 27.—Barom. 30·04 in. Singularly fine sky.

10 45	.. ..	12 5
12 12	A fog from the sea has just come on, and has driven the neutral point beneath the horizon, the bands being negative all the way to the horizon.	
1 20	The fog has diminished. The neutral point is now seen near the horizon, playing up and down from 4° to 6° above the horizon, as the fog becomes denser or rarer!	

1842, April 28.—Barom. 30 in. Fine sky; wind east.

Apparent time. h m			Distance of neutral point from sun. ° ′
11 24	..	..	10 55

1842, April 29.

11 4	..	..	11 20

1842, May 3.—China-ink sky; wind east.

11 29	..	..	10 15
12 15	..	..	9 25

1842, May 16.—Barom. 30·3 in. Thick haze; sun faintly seen.

8 49 A.M. Neutral point below the horizon. The sun now quite hid, and no bands seen below him. A great glare of light, and a sort of *quaquaversus* polarization.

N.B. When the neutral point is out of the plane passing through the sun, the zenith, and the observer, it arises in certain cases from there being a greater haze on one side of the plane than on the other.

1842, May 17.—Barom. 30·22 in. Haze all the day.

11 0 The neutral point beneath the horizon.

12 30 *Several neutral points, three at least below the sun, as the haze flies past in different thicknesses.*

1842, August 17.—Barom. 28·8 in. Fine day; sky clear at 2h.

2 9 Neutral point seen both below and above the sun.

1842, August 28.—A diffused haze came over a bright blue sky.

3 49 The neutral point below the sun was almost in the horizon, and Babinet's neutral point near the zenith.

5 30 Sun invisible; mist thick; and the polarization everywhere positive, and very feeble.

1843, February 13.—Barom. 29·7 in. Fine sky.

1 10	..	..	15 0

1843, February 16.—Barom. 29·15 in. Fine sky.

12 57	..	..	11 25

1843, April 30.—Barom. 30·07 in. Not a cloud. Neutral points distinctly seen, both above and below the sun.

4 15 Neutral point under the sun still above the horizon.

1843, June 15.—Barom. 30·03 in. Splendid day; wind east.

12 13	..	..	8 10
7 0	..	..	9 49

1844, May 3.—Barom. 30·15 in.

11 3 The neutral point below the sun distinctly seen. In order to see it well, I look at it perpendicularly through a plate of glass. The bands on each side of it are increased in intensity, the bands above being reversed.

When the sky is clear the neutral point under the sun approaches to the sun as his altitude increases, and coincides with the sun's centre when he is in the zenith.

As the neutral points of Arago and Babinet may be seen before the sun has risen, and after he has set, they are comparatively distinct and limited in their area; but as the neutral point below the sun never can be seen unless when the sun is shining, it has a less defined boundary and a wider area, owing to the flood of light in which it is generally enveloped. Hence arises the great difficulty of seeing it, and of detecting the form of the lines of equal polarization which surround it. For the same reason, we can hardly expect to see the secondary neutral point, which must accompany it, when it rises or sets on a sea horizon in a condition to produce that phenomenon.

On the Place of Maximum Polarization, and its Intensity.

Next in importance to the determination of the place and movements of the three neutral points is the determination of the place and intensity of the maximum polarization of the atmosphere.

In order to obtain these elements, a *polarimeter*, or instrument for measuring degrees of polarization, is required. M. Arago constructed a very ingenious polarimeter, and I have described two forms of a polarimeter in the 'Transactions of the Royal Irish Academy for 1841*; but these instruments are too complex to be used from hour to hour during transient conditions of the atmosphere, when observations must be made with great facility and quickness. I was therefore obliged to use the following instruments.

1. Into one end of a tube 5 or 6 inches long and 1¼ inch wide I inserted a band polariscope, and in the axis of the tube I placed in a trough several (six to twelve) well annealed thin glass plates with their surfaces inclined to the axis of the tube at such an angle as to equal or compensate the average maximum polarization to be measured. This compensation was effected more simply by adding or removing one or more plates when those in the trough had been previously placed at a fixed angle to the axis of the tube. It is obvious that, by giving the pile of plates a motion in one plane so as to vary the angle of refraction of the incident light, we should have an instrument for measuring all degrees of polarization. I preferred, however, to use a polarimeter all the parts of which were absolutely fixed.

In looking through this instrument we have a circular field

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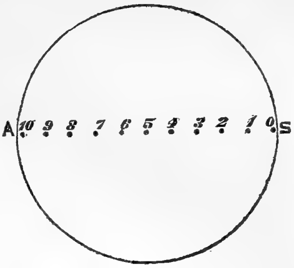
* Vol xix. part 2.

Fig. 4.

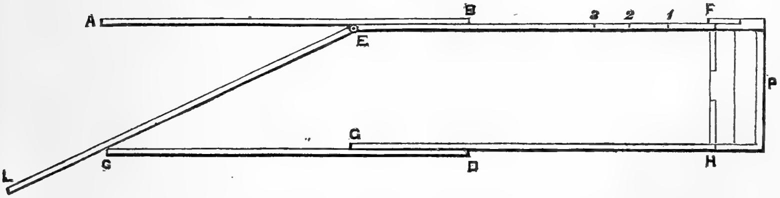


S A, and when we direct it to the region of maximum polarization, with the polarized bands parallel to S A, S being towards the sun, we shall see an interruption in the bands somewhere between S and A. If this interruption, or point of compensation, is at the point 2 in S A, I call S 2 the measure of the maximum degree of polarization at the time of observation. After some practice I had no difficulty in estimating by the eye when the neutral line was at 1 or $1\frac{1}{2}$, or 2 or $2\frac{1}{2}$, without placing marks at 1, 2, 3, &c. on a plate of glass at the end of the tube.

Having found that S 0, S 1, S 2, &c. corresponded with degrees of polarization, measured by the rotation of the plane of polarization, I thus had a measure of the maximum polarization of the atmosphere at the time of observation*.

2. When it was necessary to measure very small degrees of polarization, I preferred using a polarimeter with a single plate to one with a pile of plates receiving the light at very small angles of incidence. This instrument, shown in the annexed figure, consists of two tubes of the same length, one of which, E F H G, moves within the other, A B D C. One plate of glass, E L, longer than A B, moves round a joint at E the end of the tube E F H G, resting upon C (the end of the tube A B D C), so that when the tube E F H G is pushed in, the plate E L forms a greater angle with the axis of the tube, and when it is pulled out a smaller angle.

Fig. 5.



Now, if A B = E F, B F will be = A E, the tangent of the angle A C E, A C being radius, or of the angle of incidence of rays that fall upon E L parallel to the axis of the tube. The

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* See Phil. Trans. 1830, pp. 69, 133, 145, 287; and Trans. Royal Irish Acad. vol. xix. part 2.

degrees of rotation, R, therefore, of the planes of polarization produced by the refraction of the plate EL at different inclinations to the axis of the tube may be calculated from the formulæ

$$R=\phi-45^\circ,$$

and

$$\cot\phi=\cos^2.(i-i'),$$

i being the angle of incidence, and i' the angle of refraction. The values of R (the measures of the degrees of polarization) being laid down on the tube E F, we have a polarimeter which gives us direct measures of the polarization of the atmosphere, or any other kind of polarization, when it does not exceed the maximum polarization produced by the refraction of one plate. In this instrument the *zero* of the scale is at F, and B is the index.

In place of one plate, E L, we may use two, three, or more plates, and thus obtain a measure of all degrees of polarization. If n is the number of plates, the value of R will be as follows:—

$$R=\phi-45^\circ,$$

$$\cot\phi=\cos^n(i-i').$$

In the instrument which I constructed, the radius A C was 1·13 inch, and the scale on F B was laid down from the following Table, the index of refraction of the plate E L being 1·51.

Angles of incidence i.		Angles of refraction i'.		Rotation R.	Length of tangent, in inches.
°	′	°	′	°	
0	0	0	0	0	
30	0	19	20	1	0·655
40	34	25	31	2	0·968
47	46	29	22	3	1·244
53	17	32	4	4	1·515
57	39	34	1	5	1·784
61	25	35	3	6	2·074
64	40	36	46	7	2·386
67	29	37	43	8	2·726
70	0	38	29	9	3·104
72	19	39	7	10	3·544

By setting off the tangent 0·655 inch from the zero at F, we obtain the place of 1° of rotation, and so on with the rest. The polariscope P is placed at the end F H of the tube.

Having observed the maximum polarization of the atmosphere by the polarimeter, and its place, we take its altitude A, and by means of the sun's altitude A′, observed or computed, we obtain the distance D of the place of maximum polarization from the sun,

$$D=180^\circ-\overline{A+A'}.$$

The following are all the values of D which I obtained. I have added the values of R or the degrees of polarization, in the zenith and in the horizon, when they happen to have been measured :—

Observations in 1841.

		Apparent time.	Values of D.	Rotation in zenith.	Rotation in horizon.
		h m	° ′	°	°
April	28.	3 0	88 16	..	..
	30.	2 5	79 15	..	..
May	17.	1 20	99	18½	15½
June	6.	4 45	+90	16½	22½
	9.	4 30	+90	25½	24½
	10.	10 39	92 35	..	..
	10.	12 0	88 26	30 15	29
Sept.	15.	10 18	88 4	27 0	26½
Oct.	26.	4 30	93	28¼	27½
Dec.	17.	9 7 A.M.	Appt. +90	27	24½
	18.	9 0 ,,	time. +90	28	27

Observations in 1842.

Jan.	7.	9 0 A.M.	120	Haze.	24½
April	5.	6 58	+90	30½	26½
	20.	12 49 P.M.	84	25	17
	20.	2 25	86	25	19
	20.	3 35	88 20	25	
	20.	4 32	90	27	17
	21.	11 0 A.M.	90	23	
	21.	12 0	90	23	
	21.	1 10	90	..	25
	25.	1 17	90	26½	
	26.	10 53 A.M.	90	29	
	26.	11 46	88	29	
	26.	3 42 P.M.	88	29	
	27.	10 41 A.M.	87	29	
	28.	11 34 A.M.	87	29	
	28.	1 50 P.M.	88	29	
	29.	1 34	87½	29	
May	3.	11 29 A.M.	89	20	

Omitting the three extreme values of D, viz. 79° 15′, 99° and 120°, the mean of all the other values is 89°; but considering that five of the values of D are marked as more than 90°, we may conclude that 90° is, in the normal state of the atmosphere, the distance from the sun of the place of maximum polarization, and 45° the corresponding angle of incidence.

This determination of the place and angle of maximum polarization affords a highly probable explanation of the *azure* colour of the sky. Sir Isaac Newton considers this colour to be a "*Blue of the first order,* though very faint and little; . . . . . for all vapours, when they begin to condense and coalesce into small parcels, become first of that bigness whereby such an azure must be reflected"*.

Professor Clausius considers the vapours to be vesicles or flattened bladders, and ascribes the *blue* colour of the first order to reflexion from the thin pellicle of water. In reference to these opinions the following facts are important:—

(1) The azure colour of the sky, though resembling the *blue* of the first order, when the sky is viewed from the earth's surface, becomes, as observed by Mr. Glaisher in his balloon ascents, an "exceedingly deep *Prussian blue*" as we ascend to the height of five or six miles, which is a *blue of the second or third order*.

(2) The maximum polarizing angle of the atmosphere being 45°, is that of air, and not that of water, which is 53°.

(3) At the greatest height to which Mr. Glaisher ascended, namely, at the height of five, six, or seven miles, where the blue is the brightest, "the air is almost deprived of moisture."

Hence it follows that the "exceedingly deep Prussian blue" cannot be produced by vesicles of water, but must be caused by reflexion from the molecules of air, whose polarizing angle is 45°. The faint blue which the sky exhibits at the earth's surface is therefore not the blue of the first order, and is merely the blue of the second or third order rendered paler by the light reflected from the aqueous vapour in the lower regions of the atmosphere.

Immediately after the values of D, I have placed the values of R, or the degree of maximum polarization, in order to show the relation between these two quantities; but as the values of D were taken only when it was convenient, the numbers R do not show the maximum intensity of the polarization of the atmosphere. I have therefore selected the following from several hundreds of observations recorded in my journal.

	Mean time.	Rotation in zenith.	Rotation in horizon.
	h m	°	°
1841, May 12.	4 30	30½	25½
12.	7 10	30½	28½
12.	7 35	30½	29½
1842, April 13.	7 32	32½	
16.	7 37	32	29
Sept 29.	4 37	30½	
Very frequently the value of R was		29	

The following observations show the changes which take place in the maximum polarization in a few hours:—

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* Newton's 'Optics,' 3rd edit. Book II. part 3. prop. vii. p. 232. See also prop. v. p. 228, from which it would appear that by "small parcels" Newton meant solid globules of water.

		Mean time.	Rotation in zenith.	Rotation in horizon.
		h m	°	°
1841, May	12.	4 12	30½	25
	12.	6 0	27	24½
	12.	7 10	30½	28½
	12.	7 35	30½	29
	12.	7 45	28½	28½
	Apparent time.			
1842, April	16.	6 0	23	Much less
	16.	6 33	24	21½
	16.	6 59	27½	20½
	16.	7 23	29½	24½
	16.	7 37	32	29½
	20.	4 44	27½	17½
	20.	5 54	28½	
	20.	6 53	28	
	20.	7 1	29½	
	20.	7 24	30	
Sept.	13.	5 28	29½	
	13.	5 31	29	
	13.	6 38	29½	
		Apparent time.	Rotation in zenith.	Rotation in horizon.
		h m	°	°
1842, Dec.	24.	12 3	26½	
	24.	12 44	28	
	24.	1 32	27	
	24.	2 24	29	
	24.	3 8	27½	
	24.	3 39	29½	
	27.	11 48	26½	
	27.	12 0	23½	16½
	27.	1 45	24	
	27.	2 35	27½	
	27.	3 15	29	
	28.	11 38	29	
	28.	11 48	27½	18½
	28.	11 58	27	18½
	28.	2 31	16½	Hazy
1843, Feb.	16.	3 28	29½	24½
	16.	4 3	29½	26½
	16.	5 4	30	

The great rotation, amounting to 32½° on the 13th of April, 1842, at 7^{h} 32^{m}, the greatest ever observed, was occasioned by an unusually favourable state of the sky. I consider 30° to be the maximum rotation in a normal state of the sky.

Having, in a normal state of the atmosphere, fixed the locality of the three neutral points, and determined the place and degree of maximum polarization, we have the means of ascertaining approximately the form of the lines of equal polarization, and of constructing a map of them when the sun is in the horizon.

In a paper published in Johnston's 'Physical Atlas,' in 1848*, I have shown how this may be done, and have given two projections of these lines—one on a plane passing through the zenith of the observer and perpendicular to the line joining the observer and the sun, and the other on the plane of the horizon. These two projections on a reduced scale, and without any of the numbers upon the larger ones, are given in Plate II. It will be seen from these projections, that the lines of equal polarization approximate to lemniscates, like the isochromatic lines in biaxal crystals.

On the Polarization of Clouds and Exhalations.

The polarization of clouds and other vapours presents some interesting phenomena, and should be studied in climates more genial than ours.

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* Reprinted, without the Plate, in the Philosophical Magazine, December 1847, vol. xxxi. p. 444.

On the 29th of June, 1850, at about 8^{h} 30^{m} P.M., several reddish-white clouds appeared in the south-west sky at different heights, and in the zone of maximum polarization. They were illuminated by the setting sun, and the sky around and, of course, behind them was of a deep blue. Upon looking at one of these clouds through a Nicol's prism, I found that its reddish-white light was polarized in a plane at right angles to that in which the light of the sky was polarized. When the sky was dark by the disappearance of the blue polarized light in one position of the prism, the cloud was bright; but when the sky was brightest in a rectangular position of the prism, the cloud was of a dark blue colour.

July 15, 1850, 9^{h} 12^{m} P.M.—All the clouds to the east of the plane passing through the sun and moon, between the south-west and south, are black seen against the sky; but when we view them with a Nicol's prism, so as to extinguish as much as possible the polarized light of the sky, the clouds are white seen against a dark sky. When the Nicol's prism is turned round 90°, they again become black.

July 1, 1850, 8^{h} 30^{m} P.M.—A fine rainbow, with the secondary and supernumerary bows, appeared in the south-east. When the bands of the polariscope crossed either of the two bows at right angles, the bands at the intersection were very brilliant. When the rainbows were invisible from the great faintness of their light, they became visible (that is, the invisible portion became visible) when crossed with the bands of the polariscope. This effect did not seem to be produced when the bands crossed the supernumerary bows.

When the sun shines upon a light transparent vapour interposed between the observer and terrestrial objects, these objects are indistinctly seen, through the light reflected by the vapour. As this light is partially polarized, it may be extinguished by a Nicol's prism, or a pile of thin plates of glass, or by reflexion at the polarizing angle from a glass plate. The terrestrial objects are then seen with great distinctness. This mode of obtaining improved vision of objects imperfectly visible, or of seeing objects not otherwise visible, may occasionally be of great use at sea.

On the Theory of Atmospherical Polarization.

When the atmosphere is illuminated by the sun, his rays fall upon the aërial particles which compose it at all angles of incidence. In the immediate vicinity of the sun, where the angle of incidence is 180°, there is no polarization. The polarization increases with the angle of incidence, and becomes a maximum, as we have seen, at about 90° from the sun. It then diminishes with the angle of incidence, and becomes nothing at 180°, the point opposite to the sun.

At all these points the polarization is said to be *vertical,* being in the vertical plane passing through the sun and the observer.

In addition to the vertical polarization produced by the direct illumination of the aërial particles, there must be an opposite polarization by which the neutral points are produced. MM. Arago and Babinet, and, we believe, every other writer on the subject, have sought for this counterpolarization "in the secondary illumination which the same aërial particles receive from the reflexion of the rest of the atmosphere, which sends to them light polarized *horizontally*"*, or oppositely to the light polarized vertically. That is, all the phenomena of atmospherical polarization are produced by the opposite action of two lights polarized by reflexion, the one vertical, arising from the direct illumination of the aërial particles, and the other horizontal, produced by a secondary illumination of the same particles by the rest of the atmosphere.

This theory of atmospherical polarization, omitting all consideration of the light polarized by refraction, never appeared to me satisfactory. There is no evidence whatever that such a secondary reflexion exists, even in a perfectly cloudless sky, and still less evidence that, if it did exist, it would be capable of neutralizing the light polarized by reflexion at considerable distances from the antisolar point. It must be very feeble when the neutral point is about to disappear at the close of twilight; and as the polarization by direct reflexion must be visible when the secondary reflexion ceases to be visible, this cessation ought to be marked by a return of the neutral point to the antisolar point, the place which it would occupy were there no secondary reflexion.

Were the neutral points produced by a secondary reflexion, their distances from the antisolar point and from the sun ought to be affected when the sky is more or less covered with clouds; but though I have observed the neutral point of Arago in a clear part of the sky, I never observed that its distance from the antisolar point was changed when the rest of the atmosphere was obscured by clouds.

On these grounds I was led to the opinion that the neutral points must be produced by the opposite action of two polarized lights which had nearly the same relative intensity; and this opinion was strengthened by observations which I had made on the polarization of light by refraction and transmission through piles of glass plates. In these experiments, published in the Philosophical Transactions for 1814, I observed phenomena

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* Babinet, *Comptes Rendus,* &c. 1846, vol. xxiii. p. 233.

analogous to neutral points, that is, the coexistence in the transmitted light of rays polarized by reflexion and rays polarized by refraction; but I did not observe the effect where the intensities of these rays were such as to neutralize each other.

Guided by these views, I never doubted that the three neutral points in the atmosphere, and the partial polarization of the light which it reflects, are produced by the opposite action of lights polarized by reflexion and refraction; and, regarding the congeries of separate molecules which constitutes the atmosphere as a rough or dispersing surface, I made a series of experiments on such surfaces as were likely to act upon light in the same manner as the aërial molecules. In these experiments, which have been already communicated to the Society*, I found that such surfaces not only polarized partially the incident light, but produced neutral points, like those of the atmosphere, by the opposite action of the rays which they polarized by reflexion and refraction.

APPENDIX.

The partial polarization of the light reflected from the blue sky was first discovered by M. Arago in 1811†. In 1812, without knowing what had been done by M. Arago, I discovered "that the light reflected from the clouds—the blue light of the sky and the light which forms the rainbow—had experienced a partial polarization"‡. In 1825 M. Delezenne§ of Lille, and M. Quetelet‖ of Brussels, made the same observation without knowing what had been previously published. At a later date the polarization of the atmosphere was studied by A. B. Francesco Zantedeschi, who with a simple polariscope made several observations at Venice on the distribution of the polarization in different parts of the atmosphere¶.

In 1854 M. Félix Bernard made several observations at Bordeaux on the intensity of the maximum polarization at different hours of the day. Though made only on four days in the month of September, he found "that in proportion as the sun ap-

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* See Phil. Mag. S. 4. vol. xxv. p. 344, May 1863.

† *Œuvres*, vol. vii. p. 394.

‡ Treatise on New Philosophical Instruments. Edinburgh, March 1813, p. 349.

§ *Recueil des Travaux de la Société de Lille*, 1825,

‖ *Correspondance Mathématique et Physique*. Gand, 1825, vol. i. No. 5, p. 275.

¶ "Delle leggi della polarizzazione della luce solare nelle atmosphæra serena," comunicato con lettera al David Brewster, LL.D., in the *Raccolta Physico-Chemica Italiana*, vol. i. fascic. 10, 1846, p. 429.

proaches the meridian, the maximum polarization diminishes, that it increases gradually as the sun descends from the meridian, and reaches its maximum when the sun is very near the horizon, the amplitude of this variation being about 0·09"[*].

This interesting result was obtained by adopting my measure of the maximum polarization, namely, that when the sun's altitude was 20°, the intensity of the point of maximum polarization 90° from the sun was equivalent to that which would be produced by reflexion from the surface of glass whose index of refraction was 1·486 at an angle of 65° 30′[†]. From these data the formulæ of Fresnel give 0·64 as the measure of the intensity. M. Bernard, taking the mean of two observations, found the intensity to be 0·6523, differing only 0·012 from the measure which I had obtained.

In 1858, M. Liais, in crossing from France to Brazil, observed the polarization of the atmosphere at the commencement of the dawn (*l'aurore*) and at the end of twilight, in order to obtain a measure of the height of the atmosphere. From observations made at St. Domingo, and in the Bay of Rio Janeiro on the 1st, 2nd, and 3rd of December 1858, he found the height of the atmosphere to be 340 kilometres[‡]. During the same voyage, M. Liais made the important observation that the zodiacal light was not polarized[§].

In 1860, some interesting experiments on the polarization of a beam of light diffused or dispersed by the smoke of different substances were made by Professor M. G. Govi[||]. From their relation to atmospheric polarization, they were repeated by Dr. Rubenson, who extended them to the vapour of water. Professor Govi found, as Dr. Rubenson did, that the diffused light was polarized by reflexion or refraction, according as we viewed the luminous cylinder perpendicular to its axis or in a direction slightly inclined to it. The vapour of water, according to Dr. Rubenson, produces the opposite effect. In 1846 I made analogous experiments on the beam of light dispersed in passing through certain fluids and solids[¶].

In 1859, Dr. R. Rubenson, of the University of Upsal, undertook a series of observations on the polarization of the atmosphere. With the exception of a few made at Upsal in 1859, they were carried on at Rome between the 6th of June and the

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[*] *Comptes Rendus*, vol. xxxix. p. 775, October 1854.

[†] See Phil. Mag. S. 3. vol. xxxiv. p. 453, December 1847.

[‡] *Comptes Rendus*, vol. xliii. p. 109 (1854).

[§] Ibid. vol. xlvii. p. 450, September 1858.

[||] Ibid. vol. li. pp. 360 & 669.

[¶] Edinburgh Transactions, vol. xvi. p. 111

5th of August, 1861; at Segni* between the 5th and the 27th of August, 1861; and at Rome from the 5th of October, 1861, to the 27th of July, 1862, with the finest instruments and with a degree of accuracy which had not been attempted by previous observers. In his elaborate work, occupying 238 pages†, he has treated the subject in twelve sections, with copious tables of his observations, and illustrated with four plates.

In his first section, on the history and literature of the subject, he has done ample justice to his predecessors in this inquiry.

In his section on the Cause of Atmospherical Polarization, Dr. Rubenson is led to the same conclusion as that contained in the preceding pages, namely, that the light is polarized by reflexion from the particles of air, and not from vesicles of water with parallel faces, as supposed by Clausius, nor, as others have supposed, from extremely small and transparent drops of water, nor from molecules of aqueous vapour in an intermediate state between that of gas and that of vesicles.

The place of maximum polarization, according to Arago, was 89° 6′, the mean of six observations. I found 89° to be the mean of a great number of observations, but, like Arago, I consider 90° to be the nearest approximation to the place of maximum polarization. Dr. Rubenson found it to undergo, as I did, great variations, chiefly from 88° to 92°, the general mean of which was 90° 2′.

In observing the daily variation of the polarization of the maximum point, Dr. Rubenson found that it was subject to a diminution during the morning, and an augmentation during the evening, without being able to assign with certainty the precise hour of minimum polarization—a result which doubtless arises from the greater quantity of aqueous vapour in the air in the morning than in the evening.

In treating of the causes which disturb the polarization of the atmosphere, Dr. Rubenson found, as I did, that clouds and fogs and smoke were the most important. He found also, as I had done‡, that the halo formed by crystals of ice reduces the intensity of polarization.

Dr. Rubenson has not observed the secondary neutral point which I found to accompany sometimes that of Arago. He once observed, however, a neutral point accompanying that of Babinet, but arising apparently from a different cause from that which produces the secondary neutral point of Arago.

Dr. Rubenson has never been able to see, even under the fine

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* Long. 52^m East, and lat. 41° 41′·1.

† *Mémoire sur la polarisation de la lumière atmosphérique.* 4to. Upsal, 1864. Extracted from the 'Acts of the Royal Society of Sciences of Upsal,' vol. v.

‡ Treatise on Optics, p. 394.

sky of Italy, the neutral point which I discovered under the sun. He sought for it in vain on the 1st and 2nd of July 1862. We are not aware that it has been seen by any other observer than M. Babinet.

The observations published by Dr. Rubenson were made with a very fine polarimeter executed by M. Duboscq. It was composed, like the one which I used, of a pile of glass plates, and a Savart's polariscope, and was fitted up with graduated circles and other accessories, so as to give at once and very accurately without any calculation of the sun's motion, the position of the plane of polarization, the distance between the sun and the point observed, the altitude and the azimuth of this point, and, finally, the relative value of the polarization.

At Rome the observations were made upon a terrace on the top of the house No. 101 of the Via Sistina on Monte Vincio, one of the highest points of the city, and commanding an extensive view in all directions. At Segni the polarimeter was placed on the top of the mountain on the upper part of which the town is built.

The most recent observations on the polarization of the atmosphere were made between 1862 and 1864 by M. Andrès Poey, under the tropical sky of the Havana. He adopts my theory of the polarization by refraction, and admits the resemblance between the phenomena and those of biaxal doubly refracting crystals, and to those of uniaxal crystals when the sun is in the zenith. He seems also to have observed the neutral point below the sun. The general results of his observations will be found in a short paper of four pages in the *Comptes Rendus*, tom. lx. p. 781, Avril 17, 1865.

XXII. *On the Electrical Behaviour of Solid Insulators.*

By Dr. W. von Bezold*.

It is well known that bodies are divided, as to their behaviour in relation to electricity, into two classes—conductors and insulators. While the former have been the subject of frequent and searching investigations, the latter have of late attracted but little attention, notwithstanding that it was in them that electrical phenomena were first observed, and that consequently they were long called preeminently electric, and the conductors non-electric bodies. They have been regarded as almost perfectly indifferent in their relations to electricity, and were studied only so far as the practical requirements of the experimentalist demanded. A single phenomenon seemed continually to indicate that this indifference was nevertheless not so complete as

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* From Poggendorff's *Annalen*, vol. cxxv. p. 132.

had been supposed; I refer to the peculiar part played by the insulating medium in the electrical condenser, Leyden jar, or coated plate, which shows itself in the so-called residual effects—that is, in the diminution of the charge after it is first given, and in the reappearance of a charge after complete discharge.

These facts have called forth several investigations, a portion of which are referred to in a paper published by the author in Poggendorff's *Annalen,* vol. cxiv. p. 404. This communication was intended to serve as introduction to the researches of which he now takes leave to record briefly the principal results.

It was mentioned on the former occasion that Kohlrausch was the first, as well as the only person who had formed any precise conception as to the behaviour of insulators in the case in question. He assumed that the force with which the electricities distributed upon the coatings tend to separate the natural electricities of any portion of the insulator, either produced such separation in the smallest particles, or caused the particles which already contained the separate electricities to rotate so as to come into such a position as to exert an electrostatic moment upon the coatings, and so to affect the phenomena of tension.

It was shown in the paper referred to, that two consequences necessarily follow from this conception. If Kohlrausch's view is correct, it follows—

(1) That a small intervening layer, the connecting medium, for example, can exert no effect upon the disappearance of the charge—that is to say, upon the formation of a residual charge.

(2) That, provided the coatings are large enough in proportion to the distance between them, this phenomenon must remain precisely the same whether thick or thin plates are used as insulators, so long as they are all of the same material.

The author had ascertained on a previous occasion that the first of these consequences is not borne out by experiment, and he has more recently found the observation confirmed.

Having subsequently, by the kind intervention of Dr. Quincke, obtained eight very fine glass plates of various thicknesses, but each pair of equal thickness, all blown from the same pot and cooled in the same manner, he was in a position to investigate the second point also.

The experiments showed that the alterations of the charge took place at rates which were essentially different with different plates, in such wise that *the times which elapsed until the charge had sunk by the same proportion of its original amount were nearly proportional to the thickness of the plates*[*].

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[*] This, as well as all the subsequent comparisons, relates only to the first few minutes after the charge was given; and the numerical statements are here only approximate.

Kohlrausch's view of the behaviour of insulators is accordingly *completely untenable*; and the question therefore arises, what hypothesis can be set up in its stead?

It is well known that the theoretical investigations of Kirchhoff and the experimental researches of Kohlrausch have led to the following mode of regarding the movement of electricity in conductors. As soon as a current is established, no more free electricity exists in the interior of the conductor, but only at its surface, and at the points of contact of heterogeneous metals. These free electricities *act at a distance* upon the interior of the conductor so as to cause continual decompositions and recombinations of the electricities in each smallest particle, and thus produce a movement of one electricity in one direction, and of the other in the opposite direction.

Now can this conception be transferred to insulators also? can they be regarded simply as bad conductors, which differ from good ones only by the forces which are required to produce the separation of equal quantities of electricity being extraordinarily much greater?

In other words, Is it the action of the electricities distributed upon the coatings which occasions decompositions in the smallest particles, consequently setting the two electricities in motion towards the two sides, and thus giving rise to the phenomena of the residual charge?

A theoretical investigation shows that in this case also the influence of very thin intervening layers must be imperceptibly small, as it is in Kohlrausch's view, and that differences of thickness would be equally without effect. It is moreover easy, upon this hypothesis, to determine the form of the curve which represents the available charge (potential) as a function of the time; and this does not agree with that actually observed. We are thus brought to conclude that it is certainly not merely the action at a distance of the electricities on the coatings which brings about the movements of the electricities in the interior of insulators. But, on the other hand, it can also be proved by experiment that this action at a distance is not completely excluded. Thus if an uncoated glass plate is introduced between the plates of an air condenser, so as to be separated from them by spaces filled with air sufficiently great (according to special experiments) to make the passage of electricity between the plates impossible, we still find, on the one hand, after charging the air condenser, a greater diminution of the charge than can be explained by the mere loss of electricity into the air, and, on the other hand, after discharging the condenser, a reappearance of a residual charge.

Another essential difference between insulators and conductors manifests itself in the opposite ways in which changes of temperature affect the electrical behaviour of each.

Whereas a rise of temperature diminishes the conductivity of solid conductors, electrical movements take place in insulators much more quickly at high temperatures than at low ones, and indeed very small variations of temperature exert a most marked influence on the formation of the residual charge.

It had been previously observed that glass became capable of transmitting the galvanic current at 200° C.; but that changes of temperature, such as occur in our rooms, could affect in an important degree the formation of the residual charge is a thing which, so far as I am aware, no one had suspected.

Experiment has, however, demonstrated that an elevation of temperature from 10° to 20° C. lessens the time in which the same diminution of the charge takes place to *one-half* in the case of a glass plate, and to *one-tenth* in the case of wax.

As to the practical execution of these experiments, they were all made in the Physical Institute of the University of Munich, and by means of a Kohlrausch's sine-electrometer. Since, however, the alterations frequently took place so quickly* that it would have been impossible to make an observation according to Kohlrausch's method, it was needful to employ a slight (but, as the author thinks, not unimportant) modification of the instrument.

This consisted in a divided paper scale suspended in the inside of the case. By reducing the values of the scale-divisions by empirical comparison to the direct indications of the electrometer, it was possible to make observations without touching the instrument. This arrangement enabled me, under favourable circumstances, to make ten observations in a minute, while by the original method it was not possible, even in the most favourable case, to make more than four readings in the same time.

The results obtained may be briefly stated thus:—

(1) Electrical movements can take place in the interior of insulators.

(2) These movements are occasioned in part only by the action at a distance of the electricities collected upon external conductors.

(3) They take place much more quickly at high temperatures than at low ones.

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* With the thinnest plate (1·6 millim. thick) the charge sank in twenty seconds from 100 to 15, and in sixty seconds to 0·92.

XXIII. *Influence of Gravity on Magnetic Declination.*

By PLINY EARLE CHASE, *M.A., S.P.A.S.*[*]

IN my first communication on the diurnal variation of the barometer (Proceedings Amer. Phil. Soc. vol. ix. p. 284), I expressed the belief that a careful investigation would "show a mutual connexion through which all the secondary [disturbing] causes may be referred to a single force." In my various subsequent papers, and especially in the one to which the Magellanic Premium was awarded (Phil. Mag. July 1865), I pointed out various reasons for supposing that the primal unitary force is the same that controls the motions of the several stellar systems—in other words, the force of gravitation, or perhaps of simple undulation, which is manifested as heat in one of its subordinate forms, and as attraction in another. The numerical relations which I demonstrated between the disturbances of weight and of total magnetic force were certainly noteworthy, and, to my own mind, extremely satisfactory; and as further investigations have afforded additional confirmation of my views, I desire to put upon record a brief notice of the general harmony which mutually characterizes the gravitation-currents and the variations of magnetic declination.

Preliminary investigations showed, as might have been reasonably anticipated, that the best quantitative results can be obtained from the observations at stations near the equator, and I therefore based my reasoning in great measure upon the St. Helena records and Major General Sabine's discussions, confirming it by such incidental references to other observations as seemed available for the purpose. At the same time allusion was made to researches now in progress, which may probably enable us to discover numerical relations that will be equally satisfactory from an examination of the observations in higher latitudes. While patiently and confidently awaiting the completion of those researches, it may be well to present some considerations which will serve both as a corroboration of my own views and as a guide to the investigations of others.

The discussions of the magnetic and meteorological observations at Girard College (Coast Survey Reports, and Smithsonian Contributions) should be specially interesting to all Americans, and they are also among the most recent and valuable publications on terrestrial physics. From them I quote the following references to the most important and best-established normal disturbances of declination.

I. "The annual variation depends on the earth's position in

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[*] From Silliman's American Journal for July 1865.

its orbit, the diurnal variation being subject to an inequality depending on the sun's declination. The diurnal range is greater when the sun has north declination, and smaller when south declination, the phenomenon passing from one state to the other about the time of the equinoxes." (Part 2. p. 10. See also Toronto Observations, 2. xvii.; St. Helena Obs. 2. cxviii.)

II. "At the hour of 6 or 7 in the morning the annual variation is a maximum, disappearing at a quarter before 10 A.M., and reaching a second (secondary) maximum value at 1 P.M. It almost disappears soon after 5 P.M., and a third still smaller maximum is reached after 9 P.M. Half an hour before midnight, the annual variation again disappears. At (and before and after) the principal maximum between 6 and 7 in the morning, the annual variation causes the north end of the magnet to be deflected to the east in summer and to the west in winter; at 1 P.M. the deflections are to the west in summer and to the east in winter. The range of the diurnal motion is thus increased in summer and diminished in winter—the magnet being deflected in summer more to the east in the morning hours, and more to the west in the afternoon hours, or having greater elongations than it would have if the sun moved in the equator. In winter the converse is the case." (Ibid. p. 12. Compare St. Helena Obs. 2. cxviii.; Toronto Obs. 1. xiv. and 2. xvi.)

III. "According to the same authority [General Sabine], the *annual* variation is the *same* in both hemispheres, the north end of the magnet being deflected to the east in the forenoon, the sun having north declination; while in the *diurnal* variation the north end of the magnet at that time of the day is deflected to the east in the northern hemisphere, and to the west in the southern hemisphere; in other words, in regard to the direction the law of the annual variation is the same, and that of the diurnal variation the opposite in passing from the northern to the southern magnetic hemisphere." (Ibid. p. 13. Compare St. Helena Obs. 2. lxxx. cxviii.)

IV. "The regular progression of the monthly values is a feature of the annual variation deserving particular notice. There is no sudden transition from the positive to the negative side, or *vice versâ*, at or near the time of the equinoxes (certainly not at the vernal equinox); on the contrary, the annual variation seems to be regular in its progressive changes. The method here pursued is entirely different from that employed by General Sabine for the same end, but the results are, nevertheless, in close accordance." It has been found that the transition takes place "*ten* days after either equinox, and also that the turning-points occur ten days after the solstices." (Ibid. p. 14. Compare St. Helena Obs. 2. cxx.)

V. "The general character of the diurnal motion . . . . . is nearly the same throughout the year; the most eastern deflection is reached a quarter before 8 o'clock in the morning (about a quarter of an hour earlier in summer, and half an hour later in winter); . . . . . the north end of the magnet then begins to move westward, and reaches its western elongation about a quarter after one o'clock in the afternoon (a few minutes earlier in summer). . . . . . The diurnal curve presents but a single wave, slightly interrupted by a deviation occurring during the hours near midnight (from about 10 P.M. to 1 A.M.), when the magnet has a direct or westerly motion; shortly after 1 A.M. the magnet again assumes a retrograde motion, and completes the cycle by arriving at its eastern elongation shortly before 8 o'clock in the morning. This nocturnal deflection is well marked in winter, vanishes in the summer months, and is hardly perceptible in the annual curve. According to the investigations of General Sabine, it is probable that if we had the means of entirely obliterating the effect of disturbances, this small oscillation would almost disappear. In summer, when it has no existence, the magnet remains nearly stationary between the hours of 8 P.M. and 3 A.M., a feature which is also shown by the annual type curve." (Ibid. p. 20. Comp. Hobarton Obs. 2. vi.; St. Helena Obs. 2. cxi. cxix. cxx. Toronto Obs. 1. xiv. 2. xvi.)

VI. "The critical hours which vary least during the year are those of the western elongation and those of the morning mean declination. The extreme difference between the value for any month and the mean annual value, is 31 minutes in the former and 28 minutes in the latter." (Ibid. p. 21.)

VII. The curves of lunar-diurnal variation "show two east and two west deflections in a lunar day," the westerly maxima "occurring about the upper and lower culminations," and the easterly maxima "at the intermediate six hours. The total range hardly reaches 0'·5. These results agree generally with those obtained for Toronto and Prague." (Part. III. p. 8. Compare St. Helena Obs. 2. xxiii. lxxxii. cxliv. Toronto Obs. 3. lxxxv.).

VIII. "In comparing the easterly and westerly curves, the constant in Bessel's formula comes out zero, and hence it is inferred that the moon has no specific action in deflecting the magnet by a constant quantity." (Ibid. p. 10.)

IX. "If we take the four phases into account, the lunar action seems to be retarded ten minutes, which quantity may be termed the *lunar-magnetic* interval for the Philadelphia Station. At Toronto the intervals are not so regular." (Ibid. p. 11.)

X. "The characteristic feature of the annual inequality in the lunar-diurnal variation is a much smaller amplitude in winter than in summer. Kreil, indeed, inferred from the ten-year series of the Prague observations, that in winter the lunar-diurnal variation either disappears, or is entirely concealed by irregular fluctuations, requiring a long series for their diminution. The method of reduction which he employed was, however, less perfect than that now used. The second characteristic of the inequality consists in the earlier occurrence of the maxima and minima in winter than in summer. The winter curve precedes the summer curve by about one and three-quarter hours." (Ibid. pp. 12 & 13.)

XI. The curves of semiannual variation at all points where continuous observations have been made, present a striking similarity, the amount of deflection being nearly equal in all parts of the globe. (See diagram, Part II. p. 12. Compare St. Helena Obs. 2. cxix.; Toronto Obs. 2. xvi. xvii.)

Now it is evident that the mechanically-polarizing currents (Proc. Amer. Phil. Soc. vol. ix. p. 367 *et seq.*) must be variously deflected at different periods of the day, in consequence of gravitation-disturbances arising both from the varying distance of the sun and from solar heat. Although it may sometimes be desirable to consider the so-called thermal currents apart from those which would be produced independently of any change of temperature, such a distinction is unnecessary in a preliminary qualitative investigation like the present, because the periods both of maxima and of minima are precisely coincident in the two currents (except as they may be slightly modified by the earth's radiant heat), and because *even the thermal currents are occasioned simply and solely by the varying gravitation* of fluids of varying density.

Regarding, therefore, the air and æther over any given magnetic meridian, during the day-hours the intertropical and during the night the extratropical portions will be most drawn toward the sun, and the following deflections will be thus produced in the portions nearest the equator:—

	6 to 12 A.M.	12 to 6 P.M.	6 to 12 P.M.	12 to 6.A.M.
Northern zones.	S.E	S.W.	S.E.	S.W.
Southern zones.	N.E	N.W.	N.E.	N.W.

The night disturbances, whether from variations of temperature or from simple fall toward the sun (the distance fallen varying as the square of the time from midnight), will be very slight. The earth's rotation, centrifugal force, and the atmospheric inertia tend to throw each of the phases forward and to increase the magnitude of the westerly, while they diminish the easterly deflections. If these modifications were sufficient to override the slight easterly tendency at 6 to 12 P.M., and to advance the phases one hour, the disturbances would assume the following forms, the change between 7 P.M. and 1 A.M. being scarcely, if at all, perceptible:—

	7 A.M. to 1 P.M.	1 P.M. to 7 A.M.
Northern zones . . .	S.E.	S.W.
Southern zones . . .	N.E.	N.W.

At the equinoxes the amounts of deflection in the northern and southern magnetic hemispheres should be equal; at other seasons the shortest lines would suffer the greatest displacement, the deflections being greatest in the northern zones from April to September, when the sun is in the northern signs, and in the southern zones from October to March, when the sun is in the southern signs. My experiments have shown that the compass-needle sympathizes with, and is to some extent controlled by, purely mechanical vibrations; and if in obedience to such control it should tend to parallelism with the æthereal currents, a westerly disturbance of declination (the declination being always conventionally referred to the north pole of the needle) would correspond either to an equatorial south-easterly deflection of the southern, or a north-westerly deflection of the northern extremity of a half meridian; and an easterly disturbance to a south-westerly deflection of the southern, or a north-easterly deflection of the northern extremity.

Substituting these declination-values for the current-deviations to which they correspond, the almost *precise* accordance of theory and observation in the prominent features of the normal variations of declination may be seen by a reference to the following Table:—

Daily maximum* .... Half-yearly max. April to Sept.† Half-yearly max. Oct. to Mar.†	Easterly. Easterly. Westerly.	Westerly. Westerly. Easterly.	Morning Mean.	Evening Mean.	Stationary.
Theoretical	7 A.M.	1 P.M.	10 A.M.	4 P.M.	7 P.M. to 1 A.M.
Observed [I. to V.]‡	6–8 ,,	1 ,,	9¾ ,,	5 ,,	8 ,, ,, 3 ,,

General Sabine, in speaking of the opposition of the annual and semiannual curves (St. Helena Obs. 2. cxix.), says, "These remarkable systematic dissimilarities may be regarded as sufficient indications of a difference in the *mode of operation* of the solar influence in the two cases." I am not aware that any attempt has hitherto been made to explain this apparent difference, or to show that it is only apparent, and may result from the action of

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* In the northern zones. † Over the whole earth.

‡ The bracketed references are to the numbered quotations from the Girard College discussions.

a uniform law. I believe that I have now given the needed explanation; and since I have shown experimentally that the phenomena are such as *should* be produced by gravitation, it is reasonable to assume that they probably *are* so produced. The probability is increased by the disappearance of the night oscillation in summer (V.), the probability that when it is observed it results from thermal disturbances, and the greater stability of those critical hours which are nearest to the hours of maximum sunward gravitation (VI.). The precise coincidence both in time and direction of the lunar-diurnal declination and tidal curves (VII.), the unavoidable inference that the moon has no constant or specific magnetic action (VIII.), the "establishment" of ten minutes at the Philadelphia station (IX.), the correspondence of the lunar and solar curves in the diminished winter amplitude (X.), and the uniformity at all stations of the semiannual variation-curves (XI.), are all necessary corollaries of my hypotheses.

The demonstration of a connexion between the daily and annual variations of magnetism and gravitation would be incomplete if our theory could not be so extended as to explain the decennial and secular changes. For such an extension we are compelled to wait until further study and observation have more precisely defined the character and value of those changes, and suggested all the important gravitation-disturbances of long period to which they may be plausibly referred. We may find, however, in the attraction of Jupiter, one of the possible causes of the 10 to 11 year period, while nutation, precession, geological upheaval and depression, change of seasons, accumulation or diminution of polar ice, and the shifting position of the centre of gravity of our planetary system, must all necessarily contribute to the production of gradual changes in the terrestrial-gravitation currents. The belief does not, therefore, seem unreasonable that the feeble vibrations of the tremulous needle may not only furnish us with a delicate scale for weighing (as we have already approximately done) the huge mass of the sun, but may also aid us in the discovery and verification of other important cosmical phenomena, and the assignment of their appropriate laws.

Although it is probable that terrestrial magnetism is mainly owing to currents circulating above the surface, it is well known that there are also earth-currents which exert an appreciable modifying influence. I am confident that they will be found equally obedient to the laws of gravitation, which affect every particle of the earth's body, modifying the crystalline polarity and cohesion of solids as well as the flow of liquids, and producing internal tides, which may contribute largely to that metamorphism of stratified rocks which has been referred by geologists to the agency of heated fluids and vapours. (See Rogers, Pa. Report, vol. ii. p. 700. Lyell, Silliman's Journal, (2) vol. xxxix. p. 22.)

The inclination presents some anomalies that are difficult to explain; and whether we compare the solar-diurnal or the annual curves at the principal northern and southern stations, the "indications of a difference in the *mode of operation* of the solar influence in the two cases" seem as striking and perplexing as they did to Hudson and Herschel in their examination of the influence of heat on the barometer (Proc. Amer. Phil. Soc. vol. ix. p. 283), and to Sabine in his discussions of the semiannual declination curves (St. Helena Obs. 2. cxix.). But the disappearance, in the progress of our investigations, of these once seemingly insurmountable difficulties—the wonderful coincidence in the general features of the gravitation and magnetic currents—and the *à priori* probability that all disturbing forces, of whatever character or variety, will tend constantly to a mutual equilibrium—encourage the belief that this apparent paradox may be likewise susceptible of a simple interpretation which will drive it from its latest lurking-place.

I can think at this moment of no more probable causes of the want of symmetry here spoken of, than the different distribution of land and water in the two hemispheres, and the influence of powerful alternating land and sea breezes. A long series of connected observations at a number of new stations may perhaps be required before it can be satisfactorily ascertained whether the disturbances thus occasioned are sufficient to account for all the phenomena; but meanwhile it is interesting to observe the degree of accordance that exists at northern inland stations like Philadelphia and Toronto, between the curves of vertical force and force of wind, on the one hand, and those of horizontal force and barometric pressure on the other, as well as the agreement that has been pointed out by Dr. Lloyd and Mr. Homer between the annual curves of declination and of temperature.

XXIV. *On the Thermal Action in the Wire of a Voltaic Circuit as indicating that the current-force operates on the surface of a metallic conductor only, and not through its substance.* By JOHN JAMES WATERSTON, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

WHILE engaged on the subject of electric conduction, I have endeavoured in vain to discover if there is any experimental evidence on record of what seems generally taken for granted—viz. that in the voltaic circle electricity circulates in the mass of the wire through every part of its transverse section, and is not confined, as with common electricity, to the surface only. Professor Ohm's formula, however excellent as a guide in practice, is only empirical; and although one of the terms is the transverse section of the wire, the accuracy with which it applies to the various conditions of a circuit is no proof of the electric current acting directly upon the interior of the wire. Heat simply is produced in the wire, which may or may not be first wholly engendered upon its surface. If on the surface alone, conduction would immediately convey it into the interior; so that there is absolutely nothing to indicate the exact *locale* of the primary thermal action, whether it takes place on the surface alone, or whether in the interior alone, or in both.

Barlow and Harris have proved that the power of magnets resides on their surface, and, of course, inductively in the space exterior to that surface. What if this should also be the case with the current-force in a submarine cable? would it not lead to the adoption of flat conductors? would not a strip of silver-leaf or copper-foil $\frac{1}{16}$th of an inch in breadth conduct as well as the wires of the Atlantic cable? Theoretical considerations lead me to believe that it would, and that with a strip half an inch in breadth we should have a conductor equal to a wire of nearly half an inch diameter. Ample surface of conduction with corresponding low electromotive force (or, as it may otherwise be expressed, low *electric pressure*) seem to be essential to the permanent success of long submarine cables; and as strength of core is not required, it seems to me that the riband form of conductor might well deserve a trial by those practically interested in the question.

The calorific effects of discharges of static electricity and of dynamic or voltaic electricity through wires of various gauges have both been fully examined experimentally. It will not be disputed that in the former the primary thermal action takes place only on the surface of the metal. Now the law of this with reference to wires of different diameters has been deduced from Professor Riess's experiments, and is thus stated at page 224 of the second volume of De la Rive's Treatise:—"We may therefore consider the following law as being well established: *When the same quantity of electricity completely discharged in the same interval of time traverses wires of the same nature but of different diameters, each wire experiences an elevation of temperature independent of its length, and inversely proportional to the fourth power of its radius.*"

The quantity of material in an inch of the thicker wire is greater than that of an inch in length of the thinner in the ratio of the square of radius; so that if the rise of temperature is inversely proportional to the fourth power of diameter or radius, the heat-force engendered in an inch length of the wire is inversely proportional to the square of the diameter; and if the elimination takes place on the surface of the wire, *the force engendered on a square inch of that surface is inversely proportional to the cube of the diameter.*

Assuming that the heat evolved in the wire of a voltaic circuit is also produced on its surface only, we may compute from Ohm's formula the ratio between the diameter of the wires and the heat evolved on a square inch of their surfaces respectively. I have made this calculation (see the appended note), and find that the ratio is *precisely the same as the above.*

It seems difficult after this to believe that there can be any difference in the *modus operandi*: if it is the surface alone that is primarily affected in one case, it must be the same in the other.

I am, Gentlemen,
Your most obedient Servant,
JOHN JAMES WATERSTON.

Inverness, July 22, 1865.

Note as to the Heat-generating Faculty in the Voltaic Circuit.

W = heat-work performed in constant element of time.
E = electromotive power of battery.

Assume that the resistance in the metallic part of the circuit is incomparably greater than in the liquid portion.

C = conducting-power of circuit $=\frac{d^2}{l}$, in which d is diameter and l length of wire.

$$W=EC=E\frac{d^2}{l}.$$

Suppose W and E to be invariable, and the circuit to be composed first of a wire whose surface is s_1, diameter d_1, and length l_1, and afterwards of a wire whose surface is s_2, diameter d_2, and length l_2,

$$C=\frac{d_1^{\,2}}{l_1}=\frac{d_2^{\,2}}{l_2};\ \text{hence } l_2=l_1\frac{d_2^{\,2}}{d_1^{\,2}}.$$

To find the ratio of s_1 to s_2, we have $s_1=\pi d_1 l_1$ and $s_2=\pi d_2 l_2=\pi l_1\frac{d_2^{\,3}}{d_1^{\,2}}$; hence

$$s_1 : s_2 :: \pi l_1 d_1 : \pi l_1\frac{d_2^{\,3}}{d_1^{\,2}} :: d_1 : \frac{d_2^{\,3}}{d_1^{\,2}} :: d_1^{\,3} : d_2^{\,3}.$$

If the invariable work W of the circuit or heat-force is engendered on the surface of the wire in each case, it is obvious that the amount engendered on a square inch of s_1 is to the amount engendered on a square inch of s_2 as $d_2^3 : d_1^3$, that is, in *the inverse ratio of the cube of the diameter*,

$$\frac{W}{s_1} : \frac{W}{s_2} :: \frac{1}{d_1^3} : \frac{1}{d_2^3}.$$

XXV. *On the Determination of the Limit of Elasticity in Metals.* *By* Robert Thalén*.

[With a Plate.]

HODGKINSON and Morin have been led by their investigations to different results as regards the determination of the limit of elasticity. The former thought he had found that a body, when it has experienced any elongation or contraction, never completely assumed its original dimensions. Hence it follows that in stretching a metal, the least loading produces a permanent elongation, that is, *lowers* the limit of elasticity. Morin doubts the validity of this conclusion, and, in the circumstance that the bars used by Hodgkinson (15 metres in length) were composed of smaller pieces, thinks he finds an adequate explanation of the low position of the limit of elasticity. The permanent elongations observed, Morin thinks, need not necessarily depend on an actual elongation of the fibres, but might rather arise from an alteration of the connecting joint, or from a stretching of the bars themselves. Morin† found a confirmation in some new investigations which were made with metal wires 24 metres in length: it was observed then that the permanent elongation only occurred with greater loads, or, that the limit of elasticity had a *high* position. Even if Morin's explanation of Hodgkinson's lower position of the limit of elasticity is partly right, it cannot be extended to the conclusion that the permanent elongation observed with small loads *entirely* arises in this way. This is too much to object to so experienced a man as Hodgkinson. Nor does it seem necessary, on the basis of Morin's experiments, completely to reject Hodgkinson's statements; for the results of both experimenters, however discrepant they appear, need not necessarily exclude one another, but may be both true.

To show the possibility of this, and also to furnish a contribution to the important question of the elastic properties of bodies, I venture to give here part of the results which I have obtained in a similar investigation with iron or steel. And first,

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* From Poggendorff's *Annalen*, April 1865.

† *Résistance des matériaux*, 2nd edit. p. 10. *Comptes Rendus*, vol. liv. p. 235.

to enable the reader to judge of the trustworthiness of my observations, I will describe the apparatus and the mode of observation which I adopted, limiting myself to what is most essential.

§ 2. *Mode of Observation.*

The kinds of steel and iron which I used were prepared from Swedish ores, in the form of rolled, partly round and partly rectangular bars 6 feet in length; the round ones were half an inch in diameter, and the square ones half an inch in the side.

The stretching-experiments were made with the Fuller's machine* used by Lagerhjelm in the year 1828, in which the bars were fastened between an hydraulic press on the one hand, and a lever with unequal arms on the other; the latter increased the load on the scale twentyfold. To measure the lengthening of the bars tested, a couple of small finely graduated scales were used, the "Index" scale and the "Measuring" scale, which were screwed on the bars at a distance from each other of 5 Swedish feet. Above, on each scale, a powerful microscope was fixed on a large pair of wooden beam compasses, which by means of a fork was firmly fixed with its ends against the scales, and therefore against the bars under examination. Its weight was almost entirely got rid of by suitably adjusted compensatory weights. There was also an arrangement by which the compass was compelled to follow the index-scale, so that a small deviation was sufficient to place the index-microscope on the zero-point: the real measuring was effected by means of the micrometer-screw on the microscope above the measuring-scale.

A *division of the measuring-scale* was almost exactly 0·2 millim.; a turn of the *micrometer-screw* almost a quarter of a division, or 0·05 millim. The *direct* reading of the milled head went to hundredths of a turn, or 0·0005 millim. It is thus seen that the accuracy of the apparatus is far greater than what is required.

As far as the excellence of the arrangement is concerned, it may be mentioned that the distance of the parallel hairs in each microscrope is about 0·7 of a turn of the screw, or 0·04 millim.; according to which, the *sum* of the errors made in

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* These experiments were instituted in the summer of 1862, on behalf of the committee which had for its object to ascertain the applicability of Swedish iron and steel as material for railways.

The fitting up of this machine, and the essential arrangement of the measuring-instruments used, were effected by the care of M. C. A. Angström, who also began the breaking-experiments which I afterwards continued. The experiments on elasticity proper were mostly made by myself, but in part by Engineer K. W. Cronstrand.

fixing the microscope could not exceed 0·02 millim., or 0·1 of a division as *a maximum*. Notwithstanding the observations in the commonest case exhibited an agreement to hundredths of a division, in the following Tables only tenths of a division were taken, and for the following reasons.

As will subsequently be shown, the permanent elongations increase considerably with greater loads, and the bars become then extremely susceptible of the stretching forces. Provided now that in using Fuller's machine the tension of the bar has already been so increased that it is able to raise the lever with its load attached, there is yet at each stroke of the pump of the hydraulic press, united with a further raising of the lever, a *new* elongation of the bar under experiment. As such an elongation occurs, however carefully the pumping (that is, the stretching) may be effected, it is seen that, after all, any given permanent elongation may be obtained. In the agreement between the various experiments, however, it will be found that the scale was continually raised to a given distance above a light moveable index fixed on it. What has been said shows that in the question of great loads it cannot be of interest to investigate the course of the elongations otherwise than approximately, and an extreme fineness in the measurement would be here merely specious and therefore unnecessary.

§ 3. *Limit of Elasticity.*

In the question of stretching, the theoretically correct definition of the limit of elasticity would be this—the highest load for the unit of surface which the bar investigated can bear without its undergoing the slightest permanent elongation after removing the load. But it is clear that the determination of this highest load depends greatly on the goodness of the instruments used for measuring lengths, and can be lowered in the same degree as the accuracy and sharpness of the said measuring-apparatus is increased; for this reason it has been thought necessary to fix a certain permanent elongation as corresponding to the limit of elasticity, and to take this at 0·0005 of the unit of length*.

Against this new definition it has been justly urged that it is arbitrary. The point upon the curve representing the permanent elongation, which according to the said definition fixes the limit of elasticity, is not in the least distinguished from other adjacent points, and is therefore quite destitute of any physical significance. But even neglecting the arbitrary way of propounding the definition, and only looking at the possibility of

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* G. Wertheim, *Recherches sur l'élasticité*, premier mémoire, p. 55.

determining experimentally the limit of elasticity thus defined, there are in certain cases exceptionally great difficulties.

In his first paper on the elasticity of bodies, Wertheim* says that before beginning the actual observations a stretching of the bar must take place; but the stretching is never complete, he adds, for even in the first loads small permanent elongations are observed, which, as he then maintains, depend on the stretching. He therefore loaded the wire with a weight sufficiently great for the stretching, which he now allowed to serve as starting-point for the subsequent measurements, whereby the present length of the wire was considered the original length.

This method, applicable perhaps in wires, is inadmissible, it seems to me, in bars of greater section. A rolled bar is never completely straight, but has a number of bendings. If by loading the bar it is to be stretched straight, this cannot be done without the bar undergoing at the same time a permanent elongation; for it is evident that it is only when the bar is so considerably stretched that a permanent elongation results, and the particles assume a new position of equilibrium, that a return of the crooked part of the bar to its old form is no longer possible. But the elongation observed consists of the straightening of the bendings, as well as of the permanent elongation necessary for this stretching; and these cannot be separated. It is therefore impossible with perfect accuracy to determine the original position of the bars, and therefore also to find accurately the limit of elasticity. The error in the determination becomes greater in proportion as the load for stretching the bar is great; for then, as I shall show, the limit of elasticity can by stretching be raised not merely beyond its original value, but, in short, to any extent.

§ 4. *Raising the Limit of Elasticity by Stretching.*

The subsequently adduced observations show that a successive stretching of a bar can actually raise the limit of elasticity. In the Tables,

P = the load in Swedish pounds upon the unit of surface (a Swedish decimal line square).

ΔL = the permanent elongation produced (in a bar 5 feet in length), expressed in parts of the measuring-scale.

L_0 is the original length of the bar.

A its original section.

E the loading of the bar after passing the limit of elasticity.

Using now Wertheim's definition of the limit of elasticity (permanent elongation $=0{\cdot}0005$ of the unit of length), in the

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* *Recherches sur l'élasticité*, premier mémoire, p. 19.

5-foot bar it is a question of the permanent elongation here necessary, or 0·025 Swedish decimal line—that is, in round numbers, of 0·4 of the above-mentioned scale.

Table I.

Steel of medium hardness. A=17·33 Swedish square decimal lines.

No.	P.	ΔL.	No.	P.	ΔL.	No.	P.	ΔL.	No.	P.	ΔL.	No.	P.	ΔL.
1	179	0·0	20	503	0·9	39	723	12·2	57	469	41·1	75	469	68·6
2	238	0·0	21	515	1·0	40	734	15·0	58	585	41·2	76	585	68·6
3	261	0·0	22	526	1·2	41	746	17·8	59	700	41·4	77	700	68·7
4	284	0·1	23	538	1·3	42	757	20·8	60	757	41·4	78	757	68·9
5	307	0·1	24	549	1·4	43	769	23·7	61	780	41·5	79	792	69·0
6	331	0·1	25	561	1·6	44	780	26·7	62	792	41·6	80	815	69·1
7	353	0·2	26	573	1·6	45	792	29·7	63	803	41·8	81	838	69·3
8	365	0·2	27	585	1·6	46	803	32·7	64	815	42·0	82	862	69·5
9	377	0·3	28	596	1·9	47	815	35·6	65	826	42·5	83	885	69·8
10	388	0·4	29	608	2·2	48	,,	37·1	66	838	43·5	84	896	70·3
11	399	0·4	30	619	2·4	49	,,	38·4	67	850	45·3	85	919	71·5
12	411	0·4	31	631	2·7	50	,,	39·1	68	862	48·1	86	942	74·5
13	423	0·5	32	642	3·1	51	,,	39·7	69	885	54·6	87	966	80·5
14	434	0·5	33	654	3·7	52	,,	40·2	70	907	61·9	88	989	89·7
15	446	0·6	34	665	4·4	53	,,	40·6	71	930	68·6	89	1012	98·4
16	457	0·6	35	677	5·3	54	,,	41·0				90	1035	107·0
17	469	0·7	36	688	6·4				72	122	68·6	91	1058	117·8
18	480	0·8	37	700	8·0	55	238	41·1	73	238	68·6	92	1081	128·5
19	492	0·9	38	711	9·9	56	353	41·2	74	353	68·6	93	1139	156·8

From the foregoing Table and the corresponding figure (fig. 1, Plate III.), it is seen that after the loads were gradually raised to 815 pounds, the stretching-experiments began again with small loads, which were then successively raised to 930 pounds, and thereupon the same experiment was repeated.

If for these three series the limit of elasticity is defined in accordance with the definition given above, and taking as the original length,

In the first series L_0=500 decimal lines,
,, second ,, =500+41·0 divisions,
,, third ,, =500+68·6,

we get for the limit of elasticity the following approximate values:—

$$E_1 = 400,$$
$$E_2 = 700,$$
$$E_3 = 800.$$

A number of similar examples might be adduced; but the foregoing show sufficiently *that, by stretching the bars, a raising of the limit of elasticity is possible.*

§ 5. *On the Diminution of the Limit of Elasticity by Heat.*

By heating and by subsequent cooling, the limit of elasticity of metals can be very considerably diminished. This has been shown by Wertheim in his experiments on elasticity. For when he investigated the drawn wire without any previous heating, and at another time after heating, he found the limit of elasticity so changed that, after heating, it was

In iron	$\frac{1}{6}$
„ cast steel	$\frac{1}{11}$
„ English steel (pianoforte-wire) . .	$\frac{1}{3}$

of what it was *before* heating*.

Although Wertheim's experiments show completely the power of heat to lower the limit of elasticity, I may be allowed to adduce an example from my experiments which also affords an illustration.

A bar of hard steel was placed in a Fuller's machine and surrounded by a thick cast-iron tube (internal diameter =2·25 inches, length =4 feet) in which three thermometers were inserted, in order to indicate the temperature of the air in the tube which surrounded the bar. The cast-iron tube, suitably supported, was closed at its ends by covers provided with a sufficiently wide hole for the bar to move freely. Above the cast-iron tube was a gas-tube which served as heating-apparatus.

By carefully reading the thermometer and corresponding regulation of the gas-tap, the temperature could be kept tolerably constant. That the mean temperature of the mass of air, as indicated by the thermometers, was sufficiently near the temperature of the bar, is shown by the fact that, from the observations, the coefficient of expansion of steel was obtained as follows:—

I.	0·0000106,
II.	0·0000105,

which numbers differ little from those of Lavoisier and Laplace.

The permanent elongations obtained by stretching the bar in question are given in Table II. For the sake of comparison, the experiments are given which were made at the ordinary temperature with another bar of the *same* kind of steel. See Table III. and the corresponding figs. 2 and 3 on Plate III.

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* *Loc. cit.* pp. 40 & 55.

TABLE II.

Hard Steel.

A=17·19 square dec. lin.

No.	P.	ΔL.	Temp.
1	221	0·0	204°
2	280	0·1	
3	338	0·3	
4	396	0·5	
5	454	0·8	
6	512	1·7	205
7	571	3·5	
8	628	6·3	
9	687	13·0	
10	803	34·5	
11	920	62·0	

TABLE III.

Hard Steel.

A=17·25 square dec. lin.

No.	P.	ΔL.	Temp.
1	220	0·0	16°·3
2	278	0·0	
3	337	0·0	
4	395	0·0	
5	453	0·1	16·8
6	511	0·3	
7	569	0·8	
8	627	2·4	
9	685	19·1	
10	743	37·0	17·5
11	801	53·6	
12	859	79·4	

From the preceding Tables,

$$\text{E becomes} = 360 \text{ if } t = 200°,$$

but

$$\text{E becomes} = 520 \text{ if } t = 17°.$$

Hence both from Wertheim's and the above experiments, the following rule can be derived:—*That the limit of elasticity of a metal can be lowered by heating*[*].

§ 6. *Explanation of the Discrepancy between* Hodgkinson's *and* Morin's *results.*

With the aid of the two principles, that the limit of elasticity can be raised by stretching and lowered by heating, it may be explained how it is that Morin's results differ so materially from those of Hodgkinson, if it is considered that the former worked with *wires* of 2·5 to 0·2 millim. in diameter, and the latter with *bars*.

It is obvious that if the wire has gone through the screw-plate, it must be considerably stretched, and the limit of elasticity lie too high. If the wire had passed several times through the screw-plate, its limit of elasticity could easily be determined with accuracy. To illustrate this assertion, I will in the following consider the case more closely where the stretching took place several times successively with *the same* load.

Whether the bars used by Hodgkinson were rolled or forged

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[*] The truth of this follows also from the well-known fact, that the metals in a warm state can be forged more easily than when cold, and are also easier to work after they have once been heated.

is not stated. But doubtless neither rolling nor forging could produce in these bars a stretching at all comparable with that produced by drawing the wire. The limit of elasticity is therefore here quickly attained. As already remarked, it is not necessary with Morin to dispute entirely the permanent elongations observed by Hodgkinson.

Far from favouring one view, and rejecting the other, we may assume that both Morin and Hodgkinson have reason for their views—the former as regards the high position of the *limit of elasticity in wires,* the latter as regards its *low position in the case of bars.* The difference in the results, it must therefore be assumed, lies in the different treatment which the material of the two observers had previously undergone.

§ 7. *On the Permanent Elongations on repeated stretching with the same Load.*

In the foregoing it has been shown that the permanent elongations continually increase with increasing loads. But even when the load is several times successively the same, each time an increased permanent elongation is produced. The law for the relation between these elongations is certainly dependent on the position of the point of the curve at which the experiment is made; yet the following data, which are the means of a number of such cases observed, may render evident the phenomenon in question. If the following increments of the elongation be compared with that which the bar underwent when it was stretched for the first time with the given load, the following values are obtained:—

TABLE IV.

No. 1.	Permanent elongation . .	1·00
„ 2.	„ „ . .	0·55
„ 3.	„ „ . .	0·37
„ 4.	„ „ . .	0·28
„ 5.	„ „ . .	0·25
„ 6.	„ „ . .	0·18

In fig. 4, Plate III., the ordinates represent the order of the numbers of the stretchings, and the abscissæ the corresponding *total* elongations, thus 1·00, 1·55, 1·92, &c.

It is true the *new* permanent elongation which is here each time obtained is always smaller and smaller, and it would consequently be supposed that after a great number of loads no new permanent elongation would be observed. But even if such a *limit* could be attained, it is probable that after the lapse of a long time a new permanent elongation would arise notwithstanding that the load was not increased. For it may in general be noticed that in this kind of investigation, *time*, as Wertheim has already showed*, exercises an important influence, which Hodgkinson, indeed, endeavoured to determine quantitatively. In my experiments I have continually noted the influence of time, although the apparatus, on account of a leak in the hydraulic press, did not allow any measurements.

If the load be taken smaller than that which the bar has already borne, no permanent elongation is perceived before the load in magnitude is nearest the greatest which has already been applied. If the latter load is exceeded, increments occur in the permanent elongations, and increase in the same measure the more rapidly the more the bar had been previously stretched. (See Table I. and fig. 1.)

If this be applied to a metal wire which has been considerably stretched in drawing, the permanent elongations, according to the above, must at first be but slightly perceptible. But when the magnitude of the load approaches that at which it was drawn, permanent elongations must arise, which with greater loads are very perceptible†. It is thus easy to see that the limit of elasticity in wires must be capable of sharp definition.

If now the loads are taken several times successively *just as large as or less than* those which the bar has already borne, and they are then gradually increased, a peculiar phenomenon is observed, which deserves a closer attention. It is illustrated by fig. 5, to which the Table below belongs, and by fig. 1 with Table I.

Table V.

Hard Steel. A=17·13.

No.	P.	ΔL.	No.	P.	ΔL.	No.	P.	ΔL.
1	280	0·0	12	719	0·8	23	980	35·2
2	339	0·0	13	748	1·2	24	980	35·7
3	397	0·0	14	777	1·7	25	980	36·0
4	456	0·0	15	806	2·5	26	980	36·4
5	514	0·1	16	835	4·6	27	980	36·6
6	543	0·1	17	865	8·3	28	980	36·9
7	573	0·1	18	923	18·1	29	1040	44·3
8	602	0·2	19	980	30·7	30	1098	59·5
9	631	0·3	20	980	32·9	31	1156	76·9
10	660	0·4	21	980	34·0			
11	689	0·5	22	980	34·7			

According to this Table ten stretchings had successively taken

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* *Loc. cit.* p. 55.

† Morin, *Résistance des matériaux*, pp. 8 & 9.

place with 980 pounds in all. The load was now increased to 1040 pounds and upwards. The curve for the permanent elongations (fig. 5) has now the same direction which it had before using the equal loads of 980 pounds. Fig. 1 shows quite the same phenomenon with the loads 850 and 966 pounds. Before the curve again assumes its old direction the bar appears to give up as much to the permanent elongations as it had previously been stretched. Hence an increase of 50 pounds on the square dec. line in the load, immediately after repeated stretching-experiments with equally great or smaller loads, did not produce the same elongation as before. Thus it is seen that the form of the curve is ultimately quite independent of the manner in which the bar has been treated.

Such a property of the curve for the permanent elongations, that is, its independence of the treatment of the bar in the stretching-experiments, is worth particular attention; for it possibly justifies us in regarding the curve either wholly or partially as representative of the quality of the material.

§ 8. *On the Curve for Permanent Elongations.*

As a type of the curves which represent the permanent elongations of iron and of the softer kinds of steel, fig. 6, constructed according to the following Table, may be taken:—

Table VI.

Steel. $A=17\cdot41$.

No.	P.	ΔL.	No.	P.	ΔL.
1	237	0·0	20	536	2·0
2	260	0·0	21	547	2·3
3	283	0·0	22	559	3·2
4	306	0·0	23	570	4·8
5	329	0·0	24	582	8·8
6	352	0·1	25	593	18·1
7	375	0·3	26	605	45·3
8	398	0·5	27	616	53·4
9	409	0·7	28	639	63·3
10	421	0·8	29	662	72·3
11	432	0·9	30	685	81·4
12	444	1·0	31	708	91·4
13	455	1·1	32	731	102·2
14	467	1·2	33	754	111·8
15	478	1·3	34	766	120·0
16	490	1·3	35	789	132·6
17	501	1·4	36	812	147·0
18	513	1·6	37	835	161·6
19	524	1·8	38	858	180·6

The curve takes first an ascending direction, by which the permanent elongations are in general small, passes then through the point at which *the curvature attains its maximum*, and hereupon is almost horizontal. At this latter part of the curve the bar is most susceptible of loads. The previous tolerably stable position of equilibrium of the particles appears now to be quite displaced. If the bar has been so much lengthened that the curve is convex as regards the abscissa axis, the new positions of equilibrium are certainly more stable than they were before; but the smallest increase of load, even the same load, can disturb anew the relative position of the particles, and therefore produce permanent elongations. The curve which was first convex is again concave; it more and more endeavours to become horizontal, and thus indicates that the permanent elongations increase more and more until fracture ensues.

To this description, in its general features, of what takes place in stretching might be added, that if, after the bar has been torn, the stretching-experiments be continued with the pieces, the curve continues in the old direction, provided blisters in the bars did not cause the break*. If the curve on first breaking was as good as horizontal, the new tearing cannot be specially greater than that just observed, but the permanent elongation may of course increase so much the more; for it has been seen that if a bar has been torn twice successively, and both times with the *same* load, the addition in the permanent elongation can amount in the hardest steel to 1·5 decimal line for a Swedish foot, and in soft steel to 6 decimal lines. It is therefore clear how inappropriate it is to attach so much importance, as is often done in practice, to the elongation on tearing.

If it is a wire with which the stretching-experiment has been made, and if this wire has been greatly stretched on drawing, the limit of elasticity must have a high position, and likewise the point of curvature; it will then often be found that the curve bends quickly in a horizontal direction, upon which tearing asunder directly follows†.

Many ores exhibit a difference as regards the form of the curve, which moreover also depends on the degree of hardness (proportion of carbon ?). While iron and the softer steel from all ores have a strong curvature at the point of the greatest curve, and immediately after a distinctly convex part, in the hardest kinds of steel from certain ores the curvature is considerably smaller, and the convex part is quite wanting. Hence in them

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* From the irregularity in the form of the curve on tearing, it can be immediately seen whether it took place too soon owing to defective homogeneity in the bar.

† See Wertheim's experiments with wires of cast steel (*acier fondu étiré*) *loc. cit.* p. 41.

the curve has a more ascending direction than in the other kinds. See, for instance, figs. 1 and 4. In general it may be said that the radius of curvature is *smaller* the softer the material; hence it is *least* in iron.

§ 9. *On the Point of the Maximum of Curvature.*

It is perhaps superfluous to remark that the quality of a material may be judged, not merely by the position of the two points of the curve which indicate the limit of elasticity and the tear, but that the curve must be considered in its totality. But since the form of the curve is very complicated, and its equation cannot probably be determined without much trouble, it is only possible in each case to reproduce it graphically.

Yet if in comparative experiments of this kind, for ascertaining the greater or less applicability of different ores for a given purpose, it is necessary to choose a determinate point of the curve as point of comparison, I do not hesitate to propose that at which the greatest curvature takes place. In my experiments with rolled bars, those of the same kind have exhibited a special agreement precisely in reference to the position of this point, which cannot be said of the limit of elasticity.

To determine practically the position of greatest curvature from the figure is extremely easy, the more so since the curve has the remarkable property, that in the neighbourhood and on both sides of the said point it is symmetrical. On the axis about which symmetry takes place is the least radius of curvature.

The point in question has, too, over the so-called limit of elasticity, the advantage that it is indicated by the curve itself, and has, we may say, a physical significance, which is not the case with the point of the limit of elasticity. From what has been said, it is obvious that just after passing through the point of greatest curvature the permanent elongations are of real significance; and, it may be added, these elongations are so marked that, at any rate with bars 5 feet in length, they may be observed with the greatest ease with the naked eye; from which it is clear that at a time when the permanent elongation was indeed known, but the measuring-instruments had only a small degree of precision, the point in question at which the curvature is a maximum must have been designated as the point at which the permanent elongations *begin*—that is, as the limit of elasticity.

From the important significance which I have sought to attach to the point of maximum curvature, and from the preference which I would give to it above that of the limit of elasticity, the conclusion must not be drawn that I consider this point free from all the errors which are incidental to the limit of elasticity.

It has above been shown that the limit of elasticity can be displaced; that this is also the case with the point of maximum curvature is clear from fig. 1. Hence it cannot well be considered an *absolute* measure for the quality of the material; but it may be regarded as a *relatively* good one, provided the bars investigated have undergone exactly the same treatment.

§ 10. *On the Relation between the Loads at the Maximum Curvature and on Tearing.*

For the sake of its simplicity, a remarkable relation may here be adduced which is observed between the loads at the point of maximum curvature and on tearing; a knowledge of it may perhaps be of practical use.

If M is the load at the maximum curvature,
B the load on tearing,
n the number of the bars investigated,

we obtain the values of $\frac{B}{M}$ given in the following Table:—

Table VII.

Kinds of ore.	Iron.		Soft steel.		Steel of mean hardness.		Hard steel.	
	$\frac{B}{M}$.	n.	$\frac{B}{M}$.	n.	$\frac{B}{M}$.	n.	$\frac{B}{M}$.	n.
a	1·65	3	2·02	2	2·03	3	2·02	2
b	1·54	2	1·88	2	2·10	3	*	
c	1·63	3	1·90	3	2·12	3	2·07	2
d	1·54	3	2·09	3	2·00	2	2·03	3
e	1·59	4	1·80	2	2·01	3	2·05	3
f	1·60	2	2·00	3	1·90	1	2·00	2
Mean.	1·59	...	1·96	...	2·07	...	2·04	

Holding to the mean value for the different kinds of ore, and denoting by 10 the load of iron at the maximum curvature, and expressing in proportional numbers the loads at the maximum curvature and on tearing for the various kinds of steel, we get the following values:—

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* The hardest steel of the kind *b* has been excluded from the list because in this, as in many other respects, it materially deviated from the ordinary kind.

TABLE VIII.

Bar of	M.	B.
Iron	10	16
Soft steel	12	24
Medium hard steel	13	26
Hard steel	14	28

On the basis of the preceding mean values we can therefore give as a practical rule,—

"A rolled bar (of Swedish ores), whose curve has its greatest curvature under a load = M Swedish pounds on the square decimal line, tears when this load is increased 60 per cent. in the case of iron, and in the case of steel when it is *doubled.*"

This practical rule is still more appropriate (provided that only approximate determinations are concerned) if we remember that the scales with which every rolled bar is coated just fall off at the load which the bar bears in its passage through the point of greatest curvature. Taking notice, then, of the load at which scales fall off in quantity, we know that this load in the case of iron must be multiplied by 1·6, and in the case of steel by 2·0 to obtain the load at which it would break. Hence in such a case a direct measure of the permanent elongation is unnecessary.

This practical rule obtains in the case of the kinds of iron and steel of the most ordinary Swedish ores, and I have reason to think that it may be extended to some kinds, at any rate, of English iron.

XXVI. *Supplementary Researches in Hydrodynamics.*—Part I.

By Professor CHALLIS, *M.A., F.R.S., F.R.A.S.*[*]

IN three articles contained in the Numbers of the Philosophical Magazine for June, August, and October 1862—one on the general differential equations of Hydrodynamics, and the two others on the mathematical theory of the vibrations of an elastic fluid—I have gone through a revision of the proofs of the principal hydrodynamical theorems which I had given in previous communications. The theorems were reconsidered with reference more especially to their application in the Undulatory Theory of Light. Such application is made of them in the December Number of the same year, in an Article entitled "Explanations of phenomena of Light on the hypothesis of undulations." The phenomena there explained are restricted to

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[*] Communicated by the Author.

those which depend only on properties of the ætherial medium in which the undulations are supposed to be generated and propagated. In three subsequent articles, contained in the Numbers for December 1863, June 1864, and the Supplementary Number of December 1864, I have employed the same theorems in investigations relating to double refraction and dispersion. The theories of phenomena of this class require hypotheses to be made respecting the forms and properties of the ultimate constituents of visible and tangible substances, and are in that respect distinguished from those which refer exclusively to properties of the æther. Finally, in the Philosophical Magazine for May of the present year, I have discussed several points which, on a review of the arguments employed in the explanations of phenomena of both classes, appeared to require correction or confirmation. There was no occasion to indicate particularly the application of the hydrodynamical theorems to cases of diffraction, because the usual theoretical treatment of these phenomena accords well with the characteristics, as determined by my analysis, of the undulations of a continuous elastic fluid. I consider, therefore, that I have now sufficiently exhibited the principles of the application of hydrodynamics in the Undulatory Theory of Light.

But the propositions of hydrodynamics are, I maintain, required for ascertaining the laws of the other physical forces, as well as those of light. I have, in fact, applied them to some extent in theoretical investigations relative to heat, electricity, galvanism, magnetism, and gravity. With the view of carrying on these researches further, I recently submitted to another revision the principles on which the hydrodynamical propositions rest, and the processes of reasoning employed in proving them; and the object of the present communication is to give the results of this new inquiry. In treating of the dynamics of undulations, so far as relates to phenomena of light, it is only necessary to consider by what action they cause *vibrations* of the atoms of sensible bodies, or, conversely, the undulations which the vibrations of the atoms produce in the æther. But in order, on the same principles, to account for the other physical forces, it is required to investigate the dynamical action of the æther in producing not only vibrations, but also *permanent motions of translation*, of atoms. My previous researches pointed to two different modes in which the latter kind of motion might be impressed on spherical atoms. In the one the motions result from pressures of the æther accompanying its *vibrations*; in the other, from pressures accompanying its *steady motions*—that is, motions which are functions of space but not of time. The forces of heat and of aggregation, and the force of gravity, are, according to these views, referable to ætherial action of the first kind, and the forces of electricity, galvanism, magnetism, and diamagnetism to ætherial action of the second kind. I propose in this communication to treat only of motions of translation of a spherical solid produced by the *vibrations* of an elastic fluid.

Here it may be remarked that the hydrodynamical questions about to be discussed are such as can be answered by *mathematical reasoning alone.* I assume that there will be no dispute about the fundamental properties of the fluids; so that to answer the proposed questions it is only required to discover the appropriate mathematical processes. The same remark applies to the questions discussed in the Numbers of the Philosophical Magazine for June, August, and October 1862. What I have now to say will consist in great measure of a revision of the arguments contained in those communications, reference to which will be made by citing the numbers of the articles (from 1 to 48) into which their contents are divided. I beg, therefore, that the processes of reasoning employed in the present communication, as well as in the three just mentioned, may be regarded quite apart from the physical applications which I have specified above. It is very evident that the laws of the physical forces cannot be referred to the motions and pressures of an elastic medium, unless the principles and the methods of mathematically investigating such motions and pressures be well ascertained previously.

On reviewing the contents of the article in the Number for June 1862, I found nothing requiring notice before coming to art. 10. Towards the end of that article the assertion is made, as a consequence of antecedent reasoning, that "the propagation of a *solitary* wave is not possible." This conclusion is drawn from a method of solving the problem of motion propagated from a centre, to which objection cannot be taken by those, at least, who maintain that there are not more than two general hydrodynamical equations. Further, I have asserted that "this conclusion involves another, that the variation of the condensation from point to point at a given time cannot be expressed by a *discontinuous* function; for if that were the case, the possibility of the uniform propagation of a solitary wave would necessarily follow." And yet such discontinuity appears to be matter of fact, judging from what is known by experience to take place in air. The articulation of consonants and words is evidence that we can impress on the air forms of condensation not necessarily expressible by continuous functions. Moreover, the principle I have employed in proving the law of equal pressure, viz. that the parts of a perfect fluid are divisible without assignable force by an infinitely thin partition, leads to the same inference. For if such a partition be made to separate parts of the fluid in a state of condensation or rarefaction, and at the same time to partake of the motion of the fluid in contact with it, since no assignable force is thus introduced, it is evident that the condensation is only required to satisfy the condition of being equal on the opposite sides of the partition. It is not necessary that the changes of condensation from point to point at a given instant on the one side and on the other should be expressed by the same function. Consequently, the partition being conceived to be removed, ordinates drawn to represent the condensation will have consecutive values, but the directions of the tangents to the line joining their extremities may change *per saltum.* Hence the motion of a given element is not generally expressible by a single function, but by different functions, in such manner, however, that the velocity always changes *gradatim.* The motion is in fact analogous to that of a material particle acted upon by a central force, which from time to time changes abruptly both as to law and amount. The path of the particle would in that case consist of portions of different curves so joined together as to have common tangents at the points of junction. From the above considerations it will be seen that the theory of the solitary wave presents a difficulty which it is absolutely necessary to remove before advancing further in these researches. To do this is the object of the following argument.

For the purpose of conveniently indicating the course of the reasoning, I shall here cite the equation (5) in art. 21 (August Number), viz.

$$b^2\phi - a^2\frac{d^2\phi}{dz^2} + \frac{d^2\phi}{dt^2} + 2\frac{d\phi}{dz}\cdot\frac{d^2\phi}{dz\,dt} + \frac{d\phi^2}{dz^2}\cdot\frac{d^2\phi}{dz^2} = 0. \quad . \quad (\alpha)$$

It will not be necessary to repeat the process by which this equation was obtained, as sufficient details on this head are given in arts. 14–22. The first term and the factor b^2 are due entirely to the use that was made in that process of the third general equation, which is the equation (3) obtained in art. 7. It is known that the exact equation applicable to rectilinear motion perpendicular to a plane, as obtained by means of the first and second general equations, is the equation (a) deprived of its first term. If, therefore, an integral of that equation can be obtained, and be such as to admit of receiving an interpretation consistent with the motion of a fluid, that circumstance might be adduced as an argument against the necessity of a third general equation. An integral is, in fact, obtainable; but, as I have pointed out in arts. 17 and 47, and have proved elsewhere, it does not admit of being interpreted consistently with the necessary conditions of fluid in motion. This result is presumptive evidence that the third general equation cannot be dispensed with. Accordingly I now proceed to inquire how the problem of rectilinear motion perpendicular to a plane, as well as other problems, may be solved by means of the equation (α).

The steps of the reasoning by which that equation was shown to be applicable to motion along a rectilinear axis accompanied by motion transverse to the axis, are given in arts. 14–21. Also in arts. 22–24 a particular solution of it is derived from the indications of the analysis prior to the consideration of any case of motion, which solution is on that account to be regarded as expressing an independent law of vibratory motion due to the mutual action of the parts of the fluid. The integration is effected by successive approximations, and the result to the third approximation is

$$\phi = m' \cos q\mu - \frac{m'^2 q^3 a_1}{3b^2} \sin 2q\mu - \frac{m'^3 q^4}{4b^2}\left(\frac{q^2 a_1^2}{b^2} - \frac{1}{8}\right) \cos 3q\mu,$$

$$a_1^2 = a^2 + \frac{b^2}{q^2} + m'^2 q^2 \left(\frac{2q^2 a^2}{3b^2} + \frac{5}{12}\right),$$

μ being put for $z - a_1 t + c$, and q for $\frac{2\pi}{\lambda}$. If m be substituted for $-qm'$ and $\kappa' a$ for a_1, we shall have, to the same approximation,

$$\frac{d\phi}{dz} = m \sin q\mu - \frac{2m^2\kappa'}{3a(\kappa'^2-1)} \cos 2q\mu - \frac{3m^3(7\kappa'^2+1)}{32a^2(\kappa'^2-1)^2} \sin 3q\mu,$$

$$a_1^2 = a^2 + \frac{b^2}{q^2} + \frac{m^2}{12}\,\frac{5\kappa'^2+3}{\kappa'^2-1}.$$

These equations should be substituted for those in art. 31, the value of κ employed in obtaining the latter having been shown, by an argument in the communication to the Philosophical Magazine for last May, to be erroneous. In that communication the numerical value of κ', obtained on the supposition that $a_1^2 = a^2 + \frac{b^2}{q^2}$, is found to be 1·2106. If this constant be represented by κ, we shall have

$$\kappa'^2 = \kappa^2 + \frac{m^2}{12a^2} \cdot \frac{5\kappa^2+3}{\kappa^2-1}.$$

The expression for the condensation may now be derived from the equation

$$a^2\,\text{Nap.}\log\rho + \frac{d\phi}{dt} + \frac{d\phi^2}{2dz^2} = 0,$$

from which the arbitrary quantity F(t) has disappeared by supposing that $\frac{d\phi}{dz}=0$ and $\frac{d\phi}{dt}=0$ where $\rho=1$. Let $\rho=1+\sigma$. Then to the second order of approximation, which is all that is required for the present purpose, it will be found that

$$a\sigma=m\kappa\sin q\mu-\frac{2m^2\kappa^2}{3a(\kappa^2-1)}\cos 2q\mu+\frac{m^2}{2a}(\kappa^2-1)\sin^2 q\mu, \quad (\beta)$$

and, w being the velocity along the axis, to the same approximation,

$$w=m\sin q\mu-\frac{2m^2\kappa}{3a(\kappa^2-1)}\cos 2q\mu \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (\gamma)$$

As the above expressions for w and σ have been obtained antecedently to any arbitrary conditions of the motion, the *inferences* drawn from them must relate exclusively to circumstances of the motion that are not arbitrary. In the first place, since μ is equal to $z-\kappa at+c$, it follows that both the velocity and the condensation are propagated with the uniform velocity κa. As this is true however far the approximation be carried, the law of uniform propagation is independent of the magnitude of the motion, and holds good in every instance of propagated motion.

Again, from the equations (β) and (γ) the following equation, to the same approximation, is readily deduced:

$$a\sigma=\kappa w+(\kappa^2-1)\frac{w^2}{2a}.$$

This result informs us that the condensation corresponding to a positive value of w is greater than the rarefaction corresponding to an equal negative value by $(\kappa^2-1)\frac{w^2}{a^2}$. The reason for this law will be apparent by considering that, as the motion is wholly vibratory, the forward excursion of each particle must be equal to its excursion backward, and that this will be the case if at a given instant the variation of σ for a given variation of z be greater at a point of condensation than at the corresponding point of rarefaction in the proportion in which the density is greater at the former point than at the latter.

In the third place we may conclude that as the forms of the expressions for w and σ were obtained antecedently to the supposition of any arbitrary disturbance, they must be applicable generally to cases of disturbance producing vibratory motions. Also since the equation (α) is exact, and no limitation was made as to the magnitude of the motion in obtaining the particular solution now under consideration, it follows that that solution is applicable to motions of all magnitudes. But in proportion as the motions are larger, a greater number of terms of the series for w and σ will have to be taken into account. For the vibrations of a very elastic medium it is usually required to take account only of the first term of each series.

Another remark relating to the particular solution of (α) may be suitably added here. When the approximations to the values of w and σ are restricted to the *second* order, the value of a_1 is given with sufficient exactness by the equation $a_1{}^2=a^2+\frac{b^2}{q^2}$. Hence we have $a_1{}^2=\kappa^2a^2$ and $b^2=q^2a^2(\kappa^2-1)=\frac{4\pi^2a^2}{\lambda^2}(\kappa^2-1)$. Consequently, as the differential equation contains b, the solution is true only for a single value of λ. It is, however, not the less proper for making known those *laws* of the motion of the fluid which are independent of arbitrary circumstances. In fact, the foregoing values of w and σ not only indicate the law of uniformity of propagation, and the law of vibration expressed by a circular function, but also show that the principal vibrations are accompanied by others of less magnitude, severally expressed by the terms of a convergent series of circular functions. Although the mathematical theories of sound and light usually take account only of the first terms of the values of w and σ, the movements which the other terms represent coexist with those that are principal, and in the case of sound have been actually recognized as producing the *harmonics* accompanying a musical note. Also the proof that the rate of propagation is exactly uniform depends on the possibility of expressing w and σ by these series.

After the foregoing preliminaries, we may next consider in what manner the values of w and σ are applicable in given cases of motion. The reasoning will at first be confined to approximations of the first order; and the problem of rectilinear motion perpendicular to a plane, which has been already referred to, will be selected for a first example. I have given a solution of this problem in arts. 32–36; and the principles of *composite* motion, that is, of motion compounded of the motions defined by the first terms of the expressions for w and σ, and of accompanying transverse motions, are fully exhibited as far as regards this instance. On reviewing the reasoning, I find nothing that requires correction; it is for the purpose of adding a supplementary remark which is of considerable importance that I have occasion to advert to that solution. In art. 34 reasons are given for asserting that the successive values of the function $\mathrm{F}(z-\kappa at)$, the form of which is arbitrary, may be expressed as nearly as we please by the sum of such terms as $m\sin\frac{2\pi}{\lambda}(z-\kappa at+c)$, the number of terms and the values of m, λ, and c for each term being arbitrary. Now it has been argued, in the previous part of this communication, that, in consequence of the principle of the easy divisibility of the fluid, the function F may be discontinuous. To satisfy this condition by the composite motion, it is necessary to admit that the constants m, λ, c may vary *per saltum*. Nothing in the antecedent reasoning is opposed to this admission, if only the values of the composite velocity and condensation do not vary *per saltum*. From this argument we may draw the conclusion that a solitary wave, such, for instance, as that of which the condensation is represented by the values of the function $m \sin \frac{2\pi x}{\lambda}$ taken from $x=0$ to $x=\frac{\lambda}{2}$, or that of which the (negative) condensation is represented by the values of the same function from $x=\frac{\lambda}{2}$ to $x=\lambda$, may be propagated uniformly in the fluid without undergoing alteration.

I proceed now to the problem of motion tending to or from a centre, with respect to which I have recently seen reason to come to conclusions at variance with the views expressed in art. 10. Although the integrations required by this problem are effected without difficulty, the principles appropriate to the interpretation of them are not readily discoverable. For the purpose of elucidating the subsequent reasoning, I shall first take the simple example of the central motion of an incompressible fluid. The velocity (V) being a function of the distance from the centre, $udx+vdy+wdz$ is an exact differential, and may be supposed to be $(d\phi)$. Hence $\mathrm{V}=\frac{d\phi}{dr}$. Also the equation of constancy of mass becomes $\frac{d^2 . r\phi}{dr^2}=0$, the integration of which gives

$$\phi=-\frac{f(t)}{r}+\mathrm{F}(t),\quad \frac{d\phi}{dr}=\frac{f(t)}{r^2},\quad \frac{d\phi}{dt}=-\frac{f'(t)}{r}+\mathrm{F}'(t).$$

The second of these equations proves that the velocity at each instant varies inversely as the square of the distance from the centre. The equation that gives the pressure (p) is

$$p+\frac{d\phi}{dt}+\frac{d\phi^2}{2dr^2}=0.$$

Hence by substitution,

$$p=\frac{f'(t)}{r}+\frac{\mathrm{V}^2}{2}-\mathrm{F}'(t).$$

As this equation contains two arbitrary functions, two conditions of the motion may be satisfied. Let us first suppose that where r is indefinitely great, and consequently $\frac{f'(t)}{r}$ and V both vanish, the pressure p has the constant value Π. Then

$$p-\Pi=\frac{f'(t)}{r}-\frac{V^2}{2},$$

which shows that if the velocity either be a maximum, or be constant at a given distance from the centre, p is less than Π, and the difference depends only on the square of the velocity. Again, suppose the fluid to be put in motion by being continuously impressed at the distance c with a given velocity equal to $m\sin ht$. Then, by what is proved above, the velocity at any distance r is $\frac{mc^2}{r^2}\sin ht$, and $f(t)=mc^2\sin ht$. Hence $f'(t)=mc^2h\cos ht$, and

$$p-\Pi=\frac{mc^2h}{r}\cos ht-\frac{m^2c^4}{2r^4}\sin^2 ht.$$

Thus the motion and pressure are completely determined. At the distance c, when $t=0$, $p=\Pi+mch$. This result shows that the fluid, although of infinite extent, may be *started* at a finite distance from the centre by a pressure exceeding Π by a finite amount. When $ht=\frac{\pi}{2}$, and consequently the velocity is a maximum, $V=\frac{mc^2}{r^2}$, and $p=\Pi-\frac{m^2c^4}{2r^4}$, showing that the pressure is greater as r is greater. This is also true if the velocity be constant at a given distance, and evidently explains why in that case the velocity of a *given* particle diminishes as its distance from the centre increases. When $V=\frac{mc^2}{r^2}$, the total momentum of the fluid is $4\pi mc^2(r-c)$, which is an infinite quantity, r being infinite. Consequently an infinite amount of momentum may be generated in a finite interval. This peculiarity of incompressible fluid in motion is analogous to what is called "the hydrostatic paradox." The solution of the problem of the central motion of an incompressible fluid presents no difficulty.

Let us now treat in a similar manner the central motion of a compressible fluid. It will suffice for the present purpose to restrict the reasoning to the first approximation, for which we have the known equations

$$\frac{d^2.r\phi}{dt^2}=a^2.\frac{d^2r\phi}{dr^2},\quad a^2.\text{Nap.}\log\rho+\frac{d\phi}{dt}=0,$$

it being supposed that $\frac{d\phi}{dt}=0$ where $\rho=1$. The general integral of the first equation is $r\phi=f(r-at)+\mathrm{F}(r+at)$. Now as the value of $\frac{d\phi}{dt}$ derived from this integral is the sum of those given by the arbitrary functions taken separately; and as this is true also of $\frac{d\phi}{dr}$, the total motion is the sum of two motions admitting of independent treatment. Let, therefore, $r\phi=f(r-at)$, and substitute $1+\sigma$ for ρ. Then, to the first approximation,

$$\frac{d\phi}{dr}=\frac{f'(r-at)}{r}-\frac{f(r-at)}{r^2}, \quad a\sigma=\frac{f'(r-at)}{r}.$$

Hence it appears that the condensation σ is propagated from the centre with the velocity a, varying at the same time inversely as the distance from the centre. But if $f'(r-at)$ can be a discontinuous function having positive values corresponding to the values of r between r_0 and r_0+h, and vanishing for all other values of r, and if h, the breadth of the wave of condensation, be constant, the principle of constancy of mass requires that σ should vary inversely as the *square* of the distance. What I have advanced respecting this difficulty in the Philosophical Magazine for January 1859, and in art. 10 in the number for June 1862, was written under the impression that the contradiction might be got rid of by taking account of the second term of the value of $\frac{d\phi}{dr}$. But I have since discovered that that explanation is wholly untenable, and that my original ideas on the question were correct. The reasons for this conclusion are the following. For the sake of distinctness let the function f' have the specific form $m \sin\frac{\pi}{h}(r-at+c)$, and, a certain time t_0 being fixed upon, let the constant c be equal to $-r_0+at_0$. Then by giving to r in the function $m\sin\frac{\pi}{h}(r-r_0)$ successive values from r_0 to r_0+h the condensation from point to point of the wave may be obtained. But since $f'(r-at)=m\sin\frac{\pi}{h}(r-at+c)$, it follows that

$$-\frac{f(r-at)}{r^2}=\frac{mh}{\pi r^2}\cos\frac{\pi}{h}(r-at+c).$$

Now it is true that the velocities derived from this expression by substituting for r as above indicated, exactly account for the change of the law of the condensation from the inverse square to the simple inverse of the distance, and also that the rate $\frac{mh}{\pi r_0{}^2}$, at which the fluid flows into the wave, exceeds the rate $-\frac{mh}{\pi(r_0+h)^2}$ of flowing out of it by just the quantity that this change of the law requires. This result is evidently a consequence of the principle of constancy of mass, which was taken account of at the beginning of the reasoning. Since also for all positions outside the wave $f'(r-at_0)=0$, it follows that for the same positions $f(r-at_0)$ is constant, and consequently that the velocity where there is no condensation varies inversely as the square of the distance; which, again, is consistent with the principle of constancy of mass. But if $f(r-at_0)$ be constant, $r-at_0$ must also be constant, although r is variable; which is an absurdity. We are thus brought to a conclusion which indicates that there is fault or defect in the premises of the reasoning, assuming the reasoning to be good. This contradiction is of like character with that which is met with in the solution, on the same principles, of the problem of motion perpendicular to a plane, which leads to the conclusion that at the same point of space there may be maximum velocity and no velocity at the same time. Defective premises would account for both contradictions.

I shall now explain in what manner the problem of central motion is solved after the reasoning is supplemented by employing the third general equation. It will be unnecessary to repeat here the argument, indicated in art. 37, by which the expressions for the velocity and condensation obtained after taking that equation into account are shown to be in no respect different from those discussed above, except in having κa in the place of a. Now if the new expressions be such, it might reasonably be supposed that they would conduct to the same contradictions as the others. The sequel of the argument will show why that is not the case. From the principle of constancy of mass, combined with that which is the foundation of the third general equation, namely, that the directions of motion in each element are always normals to a continuous surface, the following equation, cited in art. 10, is deduced:—

$$\frac{d\rho}{dt}+\frac{d\,.\,\rho\mathrm{V}}{dr}+\rho\mathrm{V}\left(\frac{1}{r}+\frac{1}{r'}\right)=0. \quad . \quad . \quad . \quad (\epsilon)$$

Here r and r' are the principle radii of curvature of the surface of displacement of an element whose velocity is V and density ρ at the time t; and, as was shown in the investigation of the equation, the focal lines through which the radii of curvature pass may either be fixed in space, or vary their positions with the time. Let us now introduce into this equation the law of rectilinear axes of motion, and the law of uniform propagation along such axes, both which, as well as the equation itself, are independent of arbitrary circumstances of the motion. From the first law we may infer that $dr=dr'$, the time being given. The other, after supposing the motion to be *composite* in the manner stated in art. 34, gives rise to a certain relation between V and ρ, which is investigated in an article "On some Hydrodynamical Theorems," in the Philosophical Magazine for November 1853, and also less completely in the October Number of 1862 (art. 35). The latter investigation only takes account of changes of velocity accompanied by changes of density, while the other includes changes of velocity due to divergencies of the lines of motion, and on that account is proper for our present purpose. The reasoning is briefly as follows. Conceive a slender four-sided tube to be formed by four planes, two passing through one of the focal lines and two through the other; and at the time t let V and ρ be the velocity and density of the fluid, and m the transverse section of the tube, at the distance z measured along its axis from an arbitrary origin; and let at the same time V', ρ', m' be corresponding quantities at the distance $z+\delta z$. Then the increment of fluid in the space between the sections in the small time δt is ultimately

$$(V\rho m-V'\rho'm')\delta t.$$

Also the difference between the condensations in two adjacent spaces, each of length δz, terminated towards the same parts by the sections m and m', is ultimately

$$(\rho-1)m\delta z-(\rho'-1)m'\delta z.$$

If the former of these quantities be equated to the latter, δt will be the time in which the condensation is propagated through δz. Hence $\frac{\delta z}{\delta t}$ is the rate of propagation; and as this, by the argument above, is equal to κa, we have, after passing from differences to differentials,

$$\frac{d\,.\,V\rho m}{dz}=\kappa a\,.\,\frac{d\,.\,(\rho-1)m}{dz}.$$

Hence, by integration,

$$V\rho=\kappa a(\rho-1)+\frac{\phi_1(t)}{m}.$$

Since m varies as the product rr', this equation may be changed to

$$V\rho=\kappa a(\rho-1)+\frac{\phi(t)}{rr'}.$$

Eliminating $V\rho$ from (ϵ) by this formula, the result is

$$\frac{d\rho}{dt}+\kappa a\,.\,\frac{d\rho}{dr}+\kappa a(\rho-1)\left(\frac{1}{r}+\frac{1}{r'}\right)=0,$$

which, it may be remarked, is the same that would be obtained if $\phi(t)$ be supposed to vanish. This equation gives by integration

$$\rho-1=\frac{\mathrm{F}(z-\kappa at)}{rr'},$$

so that

$$\mathrm{V}\rho=\kappa a\,.\frac{\mathrm{F}(z-\kappa at)}{rr'}+\frac{\phi(t)}{rr'}.$$

These general results being applied to the case of central motion for which $r=r'$, and r being substituted for z, we have to the first approximation

$$\rho=\frac{\mathrm{F}(r-\kappa at)}{r^2},\quad \mathrm{V}=\kappa a\sigma+\frac{\phi(t)}{r^2}.$$

The last equation shows that if V be supposed to consist of parts V_1 and V_2 such that $\mathrm{V}_1=\kappa a\sigma$ and $\mathrm{V}_2=\frac{\phi(t)}{r^2}$, these two motions may coexist, and that the former is propagated with the velocity κa and varies inversely as the square of the distance, while the latter, not being accompanied by condensation, obeys the law of the central motion of an incompressible fluid.

The laws of central motion that are independent of arbitrary circumstances having been thus obtained, we may now revert to the equations that apply to *given arbitrary disturbances* producing central motion. These equations for propagation from the centre are

$$\mathrm{V}=\frac{f'(r-\kappa at)}{r}-\frac{f(r-\kappa at)}{r^2},$$

$$\kappa a\sigma=\frac{f'(r-\kappa at)}{r}.$$

The difficulty that previously occurred in the application of these equations may now be met by the obvious inference from the foregoing argument that they are not proper for determining *laws* of central motion, but apply exclusively to the parts of the fluid which, both as regards time and space, are immediately acted upon by the arbitrary disturbance. For example, if the fluid be continuously impressed with a given velocity $m\phi(t)$ at the distance c from the centre, we shall have

$$m\phi(t)=\frac{f'(c-\kappa at)}{c}-\frac{f(c-\kappa at)}{c^2},$$

which equation, by putting T for $f(c-\kappa at)$, is convertible into this,

$$\frac{d\mathrm{T}}{dt}+\frac{\kappa a}{c}\mathrm{T}+\kappa acm\phi(t)=0.$$

Thus we have a differential equation of two variables, by the integration of which T may be found. The value of this function being ascertained, we can determine, first, the amount of that portion of the impressed velocity which, not being accompanied by condensation, at least so far as the first approximation indicates, takes effect as if the fluid were incompressible, and then the amount which, being accompanied by the condensation which the law of propagation requires, is propagated together with the condensation into space. [I have given instances of the determination of these two velocities for given disturbances in the Transactions of the Cambridge Philosophical Society, vol. vii. part iii. pp. 340–346.] The motion and condensation of all the elements not immediately impressed are to be inferred from the laws governing the mutual action of the parts of the fluid, which were demonstrated independently of arbitrary conditions. From this argument it follows that the condensation propagated from a centre does not vary inversely as the distance, as is inferred from hydrodynamical principles which I have proved to be defective, but inversely as the square of the distance. (To this point I shall have to refer again.) Also the argument by which it was shown that a solitary wave, either of condensation or rarefaction, may be propagated without undergoing change when the motion is perpendicular to a plane, equally applies to the condensation accompanying central propagated motion.

If, as a second example of central motion, we suppose a portion of the fluid, contained within certain limits of distance from the centre, to be condensed at a given instant in an arbitrary manner without initial velocity, it would be found on the same principles that two *equal* waves of condensation would be generated, and that one would be propagated towards, and the other from, the centre, the condensation and velocity in both varying inversely as the square of the distance. I consider that the mode of solving this problem exhibited towards the end of my paper on the principles of hydrodynamics, in the Philosophical Magazine for February 1853, is in accordance with the principles I am now explaining, and needs no correction.

It will probably be noticed that no difficulty like that which the foregoing argument is intended to meet, presented itself in the problem of the central motion of an incompressible fluid. The explanation of this difference may be readily given. For an incompressible fluid the equation (ϵ) becomes

$$\frac{dV}{dr}+V\left(\frac{1}{r}+\frac{1}{r'}\right)=0.$$

Now by the general law of rectilinear motion, which is deduced by the same reasoning for incompressible as for compressible fluids, we may assume that $dr=dr'$; and consequently the equation gives by integration $V=\frac{\phi(t)}{rr'}$. Hence when the motion is such that $r=r'$, we have $V=\frac{\phi(t)}{r^2}$, which is the same expression for the velocity as that which is obtained when the arbitrary disturbance is supposed to be a function of the distance from a centre. On account of this identity of the expression applicable to specific disturbances producing central motion, with that which, being derived from general antecedent considerations, gives the law of the variation of the velocity in such motion, no difficulty occurs like that met with in the application of analysis to problems relating to compressible fluids.

The foregoing arguments have been adduced on account of their having a direct bearing upon the principles to be applied in the solution of the problem which was stated at the beginning of this communication to be its ultimate object, viz. to investigate the motions of a spherical solid submitted to the action of the vibrations of an elastic fluid for the purpose of ascertaining whether a permanent motion of translation results from such action. The second Part of these researches will be devoted to the consideration of this problem.

Cambridge, August 19, 1865.

XXVII. *On a Method of Utilizing Compact Logarithmic Tables.* *By* Robert Templeton, *Esq.*[*]

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The fourth volume of the *Annales de l'Observatoire Impérial de Paris* came into my hands a few days ago; and in looking through it I found in the appendix a paper by M. Lefort, giving an account of the great Cadastral Tables, constructed under the superintendence of Prony, and a copy of which is preserved in manuscript in the library of the observatory. After describing in minute detail the contents of the seventeen volumes, he proceeds to remark that "Delambre, whose competence to form an opinion is unquestionable, has written, 'The great Cadastral Tables are not useful in common cases, but solely in extraordinary cases; like those of Briggs, they will be the source whence will be derived whatever extension will be given to the Tables in common use. They will likewise serve as a basis of comparison for everything that has been done or will be done.'"

[*] Communicated by the Author.

M. Lefort continues, "Under this double relation it would be undoubtedly of much interest to have these Tables published; but it would appear still more desirable to make them first serve for an edition to eight places of decimals, which would be of commodious form and not necessitate the employment of second differences. Such Tables would be of extreme value in certain calculations in geodesy, astronomy, and compound interest." M. Lefort would like to see more extensive tables of logarithms published, but is reasonably enough frightened at columns of differences. There are, no doubt, many persons who will sympathize in M. Lefort's desire to have the facilities which logarithms afford for verifying complex formulæ with a greater amount of accuracy than the common tables permit, or who may require logarithms to ten or twelve places for application to geodetic or other computations, a preference being given at the present day to exact operations rather than to the approximative which were so much in vogue at the early part of the century. It will be presently shown that these facilities are accessible without adopting the suggestion of M. Lefort. A very compact table of not greater bulk than would add three-tenths of an inch to the thickness of Hutton's volume would put it in the power of anyone accustomed to logarithmic calculations to find these numbers very readily, if only they would consent to use logarithms also for finding the corrections to be applied to the tabular numbers.

Let the common logarithmic formula be thus transformed:

$$\log\frac{x+n}{x}=\mathrm{M}\left\{\frac{n}{x}-\frac{1}{2}\frac{n^2}{x}+\&\text{c.}\right\}$$

$$=\mathrm{M}\frac{n}{x}\left\{1-\frac{1}{2}\frac{n}{x}+\&\text{c.}\right\}$$

$$=\log^{-1}\left\{\log\frac{\mathrm{M}n}{x}-\frac{1}{2}\frac{\mathrm{M}n}{x}+\frac{5}{24}\frac{\mathrm{M}n^2}{x^2}-\frac{1}{8}\frac{\mathrm{M}n^3}{x^3}+\&\text{c.}\right\}$$

$$=\log^{-1}\left\{\log\frac{\mathrm{M}n}{x}-\frac{1}{2}\frac{\mathrm{M}n}{x}\right\}\text{ very nearly.}$$

So, to find from $\log x$ the value of $\log(x+n)$, we have but to find the $\log\frac{\mathrm{M}n}{x}$, subtract from it one-half of the first few figures of the number answering to it, and at the same opening of the Tables find the number of which the remainder is the logarithm. This number will be the quantity to be added to $\log x$ to obtain the logarithm of $(x+n)$. Suppose the logarithm of 3141 5926535 to be required; here $x=3141$, and $n=592$ &c., which, until the required logarithm be found, is to be considered a decimal annexed to the whole number x, just as if the number were written 3141·592 &c.; it is plain that the right index can be supplied afterwards. Then the Tables give

	$\text{Log}\,\frac{\text{M}}{x}$.	x.	Log x.
	6·1407164	3141	3·49706 79364
log n	9·7728009		
	5·9135173		
$\frac{1}{2}\,\frac{\text{M}n}{x}$	−410		
	5·9134763	log	8 19363
			3·49714 98727

Here, adding to 6·140 &c. (which is found in the Tables) the logarithm of n, we obtain the logarithm 5·9135173, which being sought for in Hutton's Tables (p. 149), the first few figures are 8195, one-half to the nearest digit (the index 5 indicating its position) being subtracted leaves 5·9134763, which is the logarithm of ·00008 19363; this added to log 3141 gives the exact logarithm required*

This is very simple and easy, the only puzzle being the index and proper position of the figures to those who are not much in the habit of using these large numbers; but two examples at the bottom of each page of the Tables causes this difficulty to be soon surmounted. It may be remarked that when n is negative, the half is to be added, not subtracted.

The reverse operation is equally simple and effective. Let

$$a=\log\frac{x+n}{x};$$

then

$$\frac{n}{x}=\frac{a}{\text{M}}+\frac{1}{2}\,\frac{a^2}{\text{M}^2}+\frac{1}{6}\,\frac{a^3}{\text{M}^3}+\text{\&c.},$$

and

$$\log\frac{n}{x}=\log\frac{a}{\text{M}}+\frac{1}{2}\,a+\frac{1}{24}\,\frac{a^2}{\text{M}^2}+0-\frac{1}{2880}\,\frac{a^4}{\text{M}^3}+\text{\&c.},$$

whence

$$\log n=\log x+\frac{1}{2}\,a+\log\frac{1}{\text{M}}+\log a \text{ very nearly.}$$

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* If d be the number of digits, then the formula is correct to $3d\mp1$. In point of fact the $\log\frac{\text{M}}{x}$ is never copied out from the Tables, but a slip of paper with $\log n$ on its upper edge is brought up to it, and the final number only is written in under $\log x$.

In applying this formula, it is only necessary to replace whatever be the index proper to the given number by the temporary index 3, and to add in the constant $\log \frac{1}{M}$ (=·3622157).

Next let us take the case of such logarithmic sines as are not given to seconds of arch in Hutton's Tables, that is, of 2° and upwards. Here we have the following rule*:—To the constant 4·3233591 add the cotangent of the arch to the nearest tabular minute, also the logarithm of the given number of seconds; from the sum subtract half the number (in Hutton's Tables) answering to it; the remainder will be the logarithm of the differential quantity answering to the given seconds of arch, to be added to the tabular sine.

To test the accuracy of the rule, let it be required to find the sine of 2° 1′ from the sines of 2° 0′ and 2° 2′; it will be a pretty fair trial, as the first differences are very large and the second nearly equal to 300.

const. .	4·3233591	const. .	4·3233591
cotan 2° 0′	1·4569192	cotan 2° 2′	1·4497317
log 60″ .	1·7781513	log 60″ .	1·7781513
	7·5584266		7·5512421
	− 18088 … $\frac{1}{2}\frac{Mn}{x}$		+ 17791 … $\frac{1}{2}\frac{Mn}{x}$
	7·5566178		7·5530212
	·0036026		−·0035729
sin 2° .	8·5428192	sin 2° 2′	8·5499948
sin 2° 1′	8·5464218	sin 2° 1′	8·5464219

The correct number lies between these, the second being somewhat in excess. This method can be used until the differences become nearly constant, when its utility ceases.

The formula places within our reach great facilities for taking out sines from Tables, which, if tangents be omitted, may be made very compact, that is, at 10″ intervals in the earlier portions of the Table. The sines so computed are perfectly true to nine places, in the tenth place with a slight error, which, however, being constant over a great extent of Table, can be easily provided for. The following Table of sines to each second of arch computed as above will give an idea of its trustworthiness at certain points of the canon—its exactitude in other parts may be inferred.

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* Since $\delta \log \sin x = M \cot x \, \delta x$.

	3° 0′.		7° 33′.		15° 6′.	
″	Sine 8·71.	M sin 1″cot x.	Sine 9·118.	M sin 1″cot x.	Sine 9·415.	M sin 1″cot x.
0	880 01637	5·6039633	56 67460	5·2010109	81 52442	4·8922838
1	884 03375		58 26316		82 30475	
2	888 05076		59 85166		83 08507	
3	892 06740		61 44010		83 86537	Error.
4	896 08367		63 02848	Error.	84 64567	+1
5	900 09956	Error.	64 61681	+1	85 42594	1
6	904 11510	+2	66 20507	1	86 20621	2
7	908 13024	1	67 79328	2	86 98646	3
8	912 14503	2	69 38143	3	87 76669	3
9	916 15944	3	70 96952	3	88 54691	4
10	920 17347	3	72 55756	5	89 32712	5
11	924 18714	4	74 14554	6	90 10731	6
12	928 20042	3	75 73346	7	90 88749	7

It will be obvious from the examples given, that we can for any number, whose significant figures do not exceed seven more than the tabular number, find the quantity required to complete its logarithm. It hence follows that if we can by any particular process find a larger number of preceding figures, or larger value for x, we shall always have it in our power to extend the value so found to seven additional places, thereby advancing beyond the ten places to which we have hitherto limited ourselves. This may be done by determining factors whose product shall be nearly identical with the first eight digits of the given number.

Let this given number be 1535897923846. Point off the first eight significant figures, and subtract them from the next greater complete square number, and find the remainder; to this remainder add double the root of the square, increasing this double by a single unit; afterwards add continuously the series of odd numbers which next follow that double root so increased, until a succession of sums be obtained to such extent as may be desirable.

$3920^2 =$ 15366400 Next greater square.
15358979
$7396 = 86^2$ ... 7421
7841 ... $2r+1$, add
$15376 = 124^2$ .. 15262
7843 Next odd No.
$23104 = 152^2$ .. 23105
7845 ... &c.,
$30976 = 176^2$ .. 30950
7847 ... &c.,
$38809 = 197^2$ .. 38797

Seek for the first remainder and successive sums in a Table of squares, and take out such squares as correspond most closely with each, writing them alongside the particular sums to which they approximate most closely. Select such as have closest agreement, rejecting the others. In the example before us, 152^2 differs by a single unit from the sum 23105; add then 23105 to the given eight figures, and we have 15382084, a complete square, viz. 3922^2; and 3922 ± 152, or 4074 and 3770 are the factors required, their product (3922^2-152^2) being 15358980*.

Now to find the logarithm of 1535897932384626.

$4074 \times 3770 = 15358980$			
15358979·32384626			
−·67615374	n ...	$\log n$	9·83004 55
4074 ... 3·61002 10246 64145	...	$\log \frac{M}{x}$	6·02776 33
3770 ... 3·57634 13502 05793	...	A.C.	6·42365 86
7·18636 23748 69938			2·28146 74
		Correction	+ 0
−191 19100	No.		2·28146 74
7·18636 23557 50838			

Mark off the first eight figures, omitting for the time the number's proper decimal point if it have one, and regard these eight figures as a whole number, the remainder as decimal. Next take out the logarithms of the factors, prefixing to each the index 3, the proper index to the four digits of the factor; take out also the logarithm of $\frac{M}{x}$ for the first of the factors, writing it under $\log n$; also place beneath it the arithmetical complement of the other factor; add these three logarithms together, correct the sum†, and we have the logarithm 2·2814674, which is the logarithm of the quantity to be applied to the sum of the logarithms of the factors; it is in this instance subtractive, since n is negative. The proper index is now to be supplied.

Next let it be required to find the number answering to a given logarithm

2·32699 68049 73387.

Reject the index of the given logarithm and replace it with the index 7. Find from the common Tables, or by the method

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* There is no reason why the square should be limited to eight places other than that it would require an additional factor if they were extended to ten or twelve, and that primes beyond the limits of the present Tables inconveniently present themselves.

† *Vide* preceding Section.

in the preceding section, the first eight figures of the required number. Determine the factors.

As a matter of convenience write out the logarithms of the factors beneath the given logarithm, but separated from it by a score, when the product is less than the corresponding figures of the number to eight figures. If the product be greater, write them above; so that whichever be the less, the given logarithm or the sum of the logarithms of the factors, the less shall always be below, but separated from the other by a score*. Here the factors selected are 4645 and 4571, and their product 21232295; their logarithms are therefore placed above. Subtracting then the given logarithm with its index replaced by 7 from the sum of these, we have a, negative since the given logarithm is the less, and one half of it gives to the nearest unit the correction 1 in the 7th place, which is written beneath the given logarithm, and under these the constant and logarithm of a; the sum 0·8184048 is the logarithm of 6·582710, the number to be subtracted, since a is negative, from the product (21232295) of the factors, to have the number answering to the given logarithm, and it is to be pointed off into whole number and decimal in accordance with its own proper index. Had the factors 4924 and 4312 been adopted, the number would have been found to be 212 32884272897.

$$\begin{array}{rll}
 & \phantom{7{\cdot}32699\ }-1346\ 456 & \ldots\ a \\
4645\ldots & 3{\cdot}66698\ 57183\ 297 & \\
4571\ldots & 3{\cdot}66001\ 12212\ 893 & \\ \cline{2-2}
 & 7{\cdot}32699\ 69396\ 190 & \ldots\ \text{sum} \\ \cline{2-2}
 & 7{\cdot}32699\ 68049\ 73387 & \ldots\ \text{given logarithm} \\
 & \phantom{7{\cdot}32699\ 6804}1\ \ \ldots & \ldots\ \tfrac{1}{2}a \\
 & {\cdot}36221\ 57\ \ \ldots & \ldots\ \text{constant.} \\
 & 3{\cdot}12919\ 22\ \ \ldots & \ldots\ \log a \\ \cline{2-2}
 & 0{\cdot}81840\ 48\ \ \log &
\end{array}$$

$$\begin{array}{rl}
21232295 & \\
-6{\cdot}582710 & \ldots\ \text{No.} \\ \hline
21232288\ 427290 &
\end{array}$$

Lastly, to find a logarithm to twenty places of figures. Make the preceding processes subsidiary, in the accompanying example using the logarithm and numbers above found. The value of $\log\frac{M}{x}$ will be found by subtracting the logarithm of the first factor from 9·63778 43113 00536 78912.

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* This step is simply to make the correction $\frac{1}{2}a$ always additive.

Find log π to twenty places.

3·14159 26 535 89793 23846 264	… π	
3·14159 25		
1·535 89793 23846 264 n	… log …	0·18636 23557 50839
5615 . . 3·74934 97605 97476 58947	… log $\frac{M}{x}$	5·88843 45507 03060
5595 . . 3·74780 00908 64368 54759	… A. C.	6·25219 99091 35631
[212 32288]		2·32699 68155 89530
	$\frac{1}{2}\frac{Mn}{x}$	−106 16144
212 32288 42729	… log	2·32699 68049 73386
7·49714 98726 94133 86435		

Restoring the proper index, we have 0·49714, &c.

XXVIII. *On the Action of Nitric and Nitrous Acids on the Sulphocyanides.* *By* Edmund William Davy, *A.B., M.B., M.R.I.A., Professor of Agricultural Chemistry to the Royal Dublin Society, &c.*[*]

SOME time since, having had occasion to test a sample of carbonate of potash (which I believed to be very pure), to ascertain if it was perfectly free from iron, and suitable for the purpose required, I was surprised, on dissolving a little of it in water and supersaturating the solution with some carefully purified nitric acid, to find that the mixture, on the addition of a few drops of a solution of sulphocyanide of potassium, became of a reddish colour, indicating apparently the existence of a small quantity of iron in the mixture. Conceiving that the coloration observed was caused by the presence of that metal, I proceeded to ascertain whether it existed in the acid or in the carbonate. A few experiments showed me that the effect was due to the acid; for I found, on testing a diluted portion of it with sulphocyanide of potassium, that it soon acquired the same reddish tint; whereas some of the carbonate of potash, after supersaturation with pure hydrochloric acid, did not acquire the reddish tinge on the addition of the sulphocyanide, or give the slightest indication of iron when tested with either the ferrocyanide or ferridcyanide of potassium, clearly proving that the effect observed was alone attributable to the acid. This I thought was rather singular, as the nitric acid I employed was very carefully prepared by myself, and I believed that it was chemically pure. A considerable portion, however, of this acid being evaporated

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[*] Communicated by the Author.

down to dryness in a porcelain dish on a water-bath, and some pure hydrochloric acid added to the minute residue which remained, and the evaporation repeated to destroy any nitric acid existing in it, I found that this residue, being dissolved in water, gave, both with the sulphocyanide and ferrocyanide of potassium, indications of the presence of a very minute quantity of iron, which I conceived was not at all sufficient to account for the former very decided effects I had observed in treating the acid with sulphocyanide of potassium.

It thus remaining doubtful whether the effect observed was due to the action of the acid itself on the sulphocyanide or, at least in part, to the trace of iron which was found to exist in it, I very carefully redistilled some of this nitric acid; and having satisfied myself that it was then perfectly free from iron (by testing in the manner just described), I tried its action on the sulphocyanide, when I found that the same reddening effect as in the former case was produced, clearly showing that its production was independent of the presence of iron, and was caused by the action of the acid on this salt.

Not finding, in the chemical works I was able to refer to, any mention made of the development of this red compound under the particular circumstances which I have just described, I proceeded to examine the subject more minutely. I found that on adding a few drops of this pure nitric acid to a solution of the sulphocyanide of potassium the red coloration developed itself, and, having become more and more intense up to a certain point, it gradually disappeared, leaving the mixture perfectly transparent and colourless, the change being generally attended with the evolution of very minute globules of gas. The application of a gentle heat I found very quickly developed the red coloration, and of a much greater degree of intensity than when left to itself, so that the mixture acquired a dark blood-red colour (precisely similar to the sulphocyanide of iron in appearance); but when thus formed it very quickly again disappeared with the copious evolution of gaseous matter, unless the mixture was at once cooled; but even then it only remained a short time longer, becoming, as in the former cases, perfectly colourless. I ascertained also, where the solution of sulphocyanide had been heated with the nitric acid, and after developing the red coloration it had become colourless, that, if the mixture was then cooled to the ordinary temperature, it immediately acquired, on the addition of a few fresh drops of the solution of sulphocyanide, the same intense red colour without the assistance of any heat; and the same I afterwards found to be the case where these substances had more slowly reacted on each other at the ordinary temperature. These observations led me to infer that the coloration was immediately due to the action of nitrous acid, or at least to some lower oxide of nitrogen than nitric acid, produced by the deoxidation of that acid, from its giving oxygen to the constituents of a part of the sulphocyanide, whilst the reduced portion of the acid, reacting on the rest, gave rise to the formation of the red compound. That this inference was correct, appeared to be shown by the immediate and much more striking effect which was produced by a portion of the same pure and colourless nitric acid after it had suffered reduction to some extent by its being made to act on a little alcohol, sugar, or starch, or even by exposure to strong sunlight for a short time before the addition of the sulphocyanide. But what appeared to be still more conclusive as to nitrous acid being the chief agent in the formation of this peculiar red combination were the following facts:—Having prepared nitrite of potash by strongly heating some pure nitre, I found that a little of the salt so obtained, being dissolved in water and mixed with some sulphocyanide of potassium, developed instantly, and without the assistance of heat, an intense blood-red coloration, after the addition of a few drops of pure diluted hydrochloric or sulphuric acid had been added to the mixture to set free the nitrous acid combined with the potash.

That the production of this red compound, under the circumstances described, is not confined to the combination of sulphocyanogen with potassium, was shown by my obtaining it where I employed other sulphocyanides, as those of sodium, ammonium, and silver, as well as with the hydrosulphocyanic acid itself; so that it is probable that it belongs to all the sulphocyanides, which are either soluble, or the sulphocyanogen of which is capable of liberation when acted on in the manner described.

As to the reaction which takes place, or the exact nature of this red compound which is formed when nitrous acid thus acts on the sulphocyanides, it is very difficult to determine, owing chiefly to the great instability of this peculiar compound; for it is no sooner formed than it begins to decompose and disappear, evolving, amongst other gaseous products, the binoxide of nitrogen, which can be easily recognized by the usual tests for that substance; whilst the presence of sulphuric acid can be readily detected in the mixture. But though this part of the subject is surrounded with many difficulties, I purpose continuing its investigation in the hope of throwing some additional light on the nature of this interesting reaction. In the meantime, however, the observations I have made appear to me of sufficient interest and importance to be placed on record as having some practical bearings in an analytical point of view; for many cases may occur in different chemical researches where the production of this peculiar red compound may be mistaken for the presence of iron, as, for example, in testing sulphuric, hydrochloric, or nitric acid for iron, as well as in searching for the same metal in the salts of the alkalies, &c., wherein the sulphocyanide of potassium (one of our most delicate tests for iron) is used, either alone or in conjunction with hydrochloric, sulphuric, or nitric acid, as the means of detecting that metal; for the apparent indications of iron so obtained may be only caused by the action of the nitric or nitrous acid used, or present in the other acids employed, or even by the presence of some nitrates or nitrites in the compounds under examination, the nitrogen acids of which, being set free by the sulphuric or hydrochloric acid employed, reacting on the sulphocyanide, may be the sole cause of the effect which might be so naturally mistaken for iron. I have ascertained, however, that there are several points of distinction between the red compound produced by the action of the nitrogen acids on the sulphocyanides, and the sulphocyanide of iron, resembling it so closely in appearance, which may be easily taken advantage of in distinguishing one from the other. Amongst these the following may be mentioned:—

1st. The former is very unstable, and completely disappears on standing a short time, or immediately on heating it; whereas the latter possesses considerable permanence at the ordinary temperature, and even only very slowly disappears by the continued application of heat.

2nd. The smallest portion of a solution of the ferrocyanide of of potassium added to a mixture coloured red by the sulphocyanide of iron instantly changes its colour to a deep blue; whereas, in the other case, that reagent appears at first to have very little effect on its colour, till it has been added in considerable quantity, when its dark red coloration gradually passes into a bright yellow, owing apparently to the conversion of the ferrocyanide into ferridcyanide of potassium; whilst each drop of the reagent added is attended with the copious evolution of small globules of gas.

3rd. The alkalies (potash, soda, and ammonia) added in excess, decolorize both red compounds; but this in the case of the metal iron is attended with the precipitation of its oxide, whereas there is no precipitation whatever produced with the other compound. On the addition, however, of an excess of either hydrochloric or sulphuric acid the red coloration is restored in both cases. The above differences will easily enable us to distinguish between these two compounds; but, in addition to them, I may mention another interesting peculiarity I have observed in the case of the compound formed by the nitrogen acids and their salts—that it is very quickly taken up and dissolved by chloroform, ether, benzole, or bisulphide of carbon when those liquids are agitated with mixtures containing it, leaving them nearly colourless, whilst those substances themselves acquire an intense blood-red colour, which, however, soon again disappears, evolving globules of gas. Solutions containing the sulphocyanide of iron, on the other hand, when agitated with those liquids, do not impart to them any colour (except in the case of ether, which acquires a very faint pinkish tint), whilst the solutions themselves remain of the same intensity of colour as they possessed before the agitation with those substances. So that in these, and many other characters I have observed, there exists a wide difference in the properties of these two compounds, and a further study of them may lead to some new and interesting results.

XXIX. *Note to the Editors of the Philosophical Magazine and Journal. By* Professor SYLVESTER, *F.R.S.*, *&c.*

K, Woolwich Common,
August 9, 1865.

GENTLEMEN,

IN answer to Prof. Young's remarks contained in the last Number of your valuable Journal, I have only to observe that I abide by the statement therein recorded as put forth by me when invited by Prof. Young to express an opinion on his supposed demonstration of Newton's rule, viz. that, "like all which had gone before, it was a failure, and no proof at all." This opinion is in no degree shaken or modified by the ingenious rhetoric of the additional "two notes enclosed within brackets" of his recent article.

It was "*not* hastily formed," for I was in possession of Prof. Young's pamphlet on the subject, dated 1844, lent to me by Professor De Morgan, at or about the time (now a year or two ago) when he suggested to me to seek for a proof of Newton's rule, and have consequently reason to presume that that eminent mathematician's opinion of the soundness of Prof. Young's logic coincides with my own. I caused to be forwarded to Prof. Young an invitation to attend my lecture, under the impression that, after an interval of twenty years, his mind would have awakened to a sense of the unsubstantial nature of his fancied proof (as imaginary as the roots to which it refers), and that he would listen with some pleasure to a sober exposition of a subject which had so deeply engaged his thoughts*. I am sorry that a dif-

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* The delusion appears to have assumed the chronic form which is usually confined to quadraturists of the circle, inventors of perpetual motion, and the oppugners of the received laws of falling bodies. It is my rule

ferent result was produced, but await without anxiety "the judgment and decision of mathematicians" which Prof. Young invokes, and have the honour to remain,

Gentlemen,
Your obedient humble Servant,
J. J. SYLVESTER.

XXX. *Notices respecting New Books.*

Saturn and its System. By RICHARD A. PROCTOR, *B.A., late Scholar of St. John's College, Cambridge, and King's College.* 8vo. pp. 252. London: Longman and Co. 1865.

THIS work contains a popular account of all the points of interest to be met with in the course of a detailed study of the motions and appearances of the planet Saturn. Of course, when any astronomical work is termed popular, no more is intended than that it can be understood by persons acquainted with the elements of geometry and mechanics. In this sense a work is called popular when it expounds methods and states results, but does not aim at applying the methods to the determination of the results. To read such a work requires a considerable expenditure of thought and attention, but not a special scientific training, it is therefore addressed to that large and intelligent class which has been not inaptly termed the "scientific laity." Perhaps the best example of such a popular work is Sir J. Herschel's 'Outlines of Astronomy'—a work which, as is well known, makes serious demands on the reader's attention, though it can be read by any ordinary well-educated man who will take the necessary trouble. To the same class belongs the work before us, and a very favourable specimen of the class it is. Mr. Proctor first considers the simpler elements of Saturn's orbit, and explains how, from its observed motions, these could be determined by astronomers furnished with imperfect instruments, but acquainted with the fact that the Sun is the centre of the planetary world. He then proceeds to consider the determination of Saturn's orbit considered as an ellipse, and to trace historically the course of the discovery of the ring, the satellites, and the other phenomena disclosed by telescopic inspection. He next investigates with considerable minuteness the changes in the planet's appearance as it describes its orbit, in consequence of the inclination of the plane of its rings to that of the ecliptic. These form the subject of the first four of the seven chap-

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never to argue (in the hope of convincing them of their error) with those suffering under any of these forms of mental hallucination, for I know it would be labour thrown away; nor do I see reason to follow a different course on the present occasion. I shall be happy to forward a copy of the syllabus of the lecture which has drawn forth Prof. Young's ire to any one taking an interest in the subject.

ters into which the book is divided. The last three chapters are taken up with discussions as to the physical constitution of the rings (which Mr. Proctor uncompromisingly regards as a congeries of minute satellites), of the great inequality, and of the habitability of Saturn.

It is plain, from the above brief summary, that a great many things contained in the book would apply with slight variation to other planets; but even in these respects there is a considerable gain in the way of distinctness when properties common to several planets are discussed with reference to one. Saturn, however, possesses numerous striking peculiarities which render it well worthy of separate treatment. These are enumerated by Sir W. Herschel in an admirable passage quoted by Mr. Proctor (pp. 60 & 61):—"There is not another object in the heavens that presents us with such a variety of extraordinary phenomena as the planet Saturn; a magnificent globe encompassed by a stupendous double ring; attended by seven satellites; ornamented with equatorial belts; compressed at the poles; turning on its axis; mutually eclipsing its rings and satellites, and eclipsed by them; the most distant of the rings also turning on its axis, and the same taking place with the furthest of its satellites; all the parts of the system of Saturn occasionally reflecting light to each other—the rings and moons illuminating the nights of the Saturnian, the globe and moons illuminating the dark parts of the rings, and the planet and rings throwing back the sun's beams upon the moons when they are deprived of them at the time of their conjunctions."

We venture to think that the seventh chapter has been unfortunately named. When a chapter is headed the "Habitability of Saturn," we are led to expect a discussion of the question whether the conditions requisite for the support of life exist in Saturn; but instead we are presented with an account of what would be more fittingly termed the physical aspects presented to an imaginary inhabitant of Saturn—such as the variations of the force of gravity, the amount of heat and light received from the sun, the lengths of the Saturnian day and year, the appearance of the satellites and rings, &c.: these and kindred topics form a very proper conclusion to the work, but it is not quite fair to give them as a discussion of the habitability of Saturn. The fact is, that the data for such a discussion do not really exist; *e. g.* nothing, we believe, is known as to the composition of Saturn's atmosphere. It might, for aught we know, consist of nitrogen saturated with aqueous vapour; and if by any chance it did so, this, we suppose, would be conclusive as to the question of Saturn's habitability.

What sort of positive argument can be alleged to justify the prevalent belief in "Saturnians"? Mr. Proctor answers, "when we consider the analogy of our own planet, it seems impossible to doubt that Saturn is inhabited by living creatures of some sort." We think, on the contrary, that it is very possible to entertain doubts on the subject, and, to tell the truth, we are inclined to regard the prevalent belief as little better than a prejudice. But putting that on one side, let us consider "the analogy of our own planet." There are two heavenly bodies in regard to the habitability of which we can speak with tolerable certainty; the one is the Earth, the other is the Moon. If we knew the Earth alone, the argument of analogy would lead to the conclusion that all the planets are inhabited; if we knew the Moon alone, the same argument would lead to the conclusion that none are inhabited: two considerations which show that the argument of analogy, when thus employed, proves nothing.

When we descend to details, the negative argument becomes lamentably strong. Let us put the case thus:—Suppose the Earth, peopled as at present, to have the conditions of life on its surface brought into conformity with those known to exist on the surface of Saturn; viz., suppose the length of the day to be reduced by more than one-half, the length of the year increased thirtyfold, the heat and light of the sun reduced to one-ninetieth of its present amount, and extensive tracts of the regions on either side of the equator to be exposed to eclipses lasting for several years (as men count years)—suppose this to be done, and we may be pretty certain that all the higher forms of life would be destroyed by the change, and this irrespectively of any of the numerous adverse possibilities of which we gave a specimen above. Of course it is possible that the laws of the variability of species, and of natural selection (supposing such laws to have any real existence) acting on the remaining lower forms, might in time give rise to races of highly organized beings, totally different from anything which we have seen or can conceive; but though this is possible, there is nothing to show that it would be so in fact.

Above we took it for granted that there is conclusive evidence that the Moon is uninhabitable. Mr. Proctor has an elaborate note of six closely printed pages on the Habitability of the Moon, which in its way is really very curious. He very properly regards the question as involved in the secondary question whether the Moon has an atmosphere. To all appearance he would answer this question in the negative, were his judgment not oppressed by "the analogy of our planet." Accordingly, as the evidence will not allow him to believe that there exists a gaseous atmosphere, he suggests that it may have become solid. His notion is that the Moon has become intensely cold, and that her surface is covered with glaciers—not glaciers here and there, but that what we see on her surface is a frozen ocean, hemmed in by frozen continents covered with glaciers—ice everywhere—all her pleasant seas and rivers fast bound in iron misery. And as though this were not enough, he goes on to tell us that "it is conceivable that the Moon's mass may have become so intensely cold that the atmospheric envelope, once clothing it, has been condensed into the liquid, and thence into the solid form" (p. 212). We have tried, but cannot conceive it. When we remember that the temperature of the Earth's surface is determined by the solar radiation, and not by the interior heat or cold, we cannot help thinking that the same must be true of the Moon, and therefore cannot conceive how the gases can ever have become liquid. And when we further consider that her surface undergoes for a fortnight at a time the uninterrupted blaze of the sun unmodified by air or vapour, we feel pretty certain that if her surface were covered with ice, a large portion of it would be turned into vapour long before the fortnight was over. What would become of a lump of solid oxygen under the same treatment we do not know.

It is much to be regretted that Mr. Proctor should have disfigured a very meritorious book by these crude speculations. We trust that a second edition will be soon called for, and that he will take that opportunity of correcting his errors by means of a little judicious omission.

XXXI. *Intelligence and Miscellaneous Articles.*

RESULTS OF BLASTING-EXPERIMENTS MADE WITH NITROGLYCERINE AT VIEILLE-MONTAGNE MINE. NOTE BY M. NABEL.

THE greatest advantage of nitroglycerine consists in the fact that when it is used a force can be introduced into the blast-hole of a mine ten times as great as when powder is used. Hence arises a great economy in manual labour, the importance of which is understood when it is remembered that the labour of the miner represents, according to the hardness of the rock, from five to twenty times the value of the powder required—a saving therefore which will often amount to 50 per cent.

The use of this substance is very simple. If the blast-hole of the mine is fissured, it must be lined with clay in order to render it tight. Nitroglycerine is then poured in, and the upper part of the hole is filled with water; in the nitroglycerine is then introduced a safety-match of suitable length, at the end of which is pressed a strong percussion-cap. The operation is finished, and it is only necessary to put fire to the match. Sand may also be used to close the hole of the mine above the charge; but the operation is then a little more complicated. In all cases ramming is useless.

The following are the results of three experiments made with nitroglycerine in the open works at the mine of Altenberg in the presence of M. von Decken, M. Noeggerath, and several German and Belgian mining engineers.

The rock in which the blast-holes were placed is stratified dolomite, a stratum internally hard and sound, but traversed by numerous fissures, and merely superficially decomposed at the points of contact with the stratification.

First experiment.—A blast-hole of $\frac{5}{4}$ths of an inch (34 millimetres) was bored perpendicularly in a dolomite rock, forming one of the sides (60 feet in length) of an excavation (crater-shaped) of 17 feet in depth. The hole was placed at a distance of 14 feet from the edge of the almost vertical side of the rock. At 8 feet deep it would have traversed a fault filled with clay about $1\frac{1}{2}$ feet deep. To prevent the injurious effect of this fault, the hole was only bored to 7 feet in depth, then charged with $1\frac{1}{2}$ litre of nitroglycerine, corresponding to about 5 feet of the hole; after placing the patent plug and the fuse, the mine was filled with sand and then fired. The mass to be removed was not thrown out, but simply cracked; for on the one hand the volume was too great, and on the other the hole was not deep enough, and, lastly, the quantity of nitroglycerine was insufficient. Yet the effect was enormous. A crater of an elliptic section was formed around the mine, filled by little fragments. After having cleared them away, it was found that the rock was strongly cracked, and, so to say, powdered even below the bottom of the hole; a crack 50 feet in length on the surface divided the rock in the direction of the major axis of the crater; another 20 feet in length in that of the minor axis.

The total effect can only be judged of when all the mass has been cleared away little by little by small mines.

Second experiment.—The mine was sunk at an angle of 50 degrees in a dolomite rock exposed on three sides; it was 34 millims. in diameter and 7 feet in depth; at 5 feet a fault 6 inches thick was traversed. The aperture of the hole was 15 feet above the upper level of the quarry; the distance to the sides was 10 feet, to the top of the rock 10 feet also.

The charge consisted of $\frac{3}{4}$ of a litre of nitroglycerine, corresponding to about 2 feet of hole. After inserting the plug and filling the hole with sand, the match was lit. The sound was dead, and the effect complete and immense. If powder had been used, the gases would doubtless have been lost in the fissures, and only a very feeble effect would have been obtained even with a maximum charge.

The explosion produced its effect on all sides; at more than 10 feet distance the rock was fissured and split; a quarter of the mass was thrown out, and all the rest so smashed that, by means of crowbars and three small mines, a total volume of 100 cubic metres were removed.

The cost of this second trial was 94·10 francs.

For this cost 100 cubic metres of rubble stone were obtained, for which labourers are paid at the rate of 1 franc 30 cent. per cubic metre; so that besides their day's wages, at the rate of 2 francs 50 per diem, they would still have earned 35 francs 90 cent. if they had had to *pay for the nitroglycerine.* If the same amount had been blasted by powder, at least twenty ordinary mines of 20 to 36 inches in depth must have been used, and the cost would have been in this case 125 francs.

Third experiment.—The third trial was made with a block of cast iron 40 inches long, 20 inches broad, and 11 inches thick, weighing 1000 kilogrammes; a hole 8 inches deep by $\frac{5}{8}$ths of an inch in diameter was bored in the middle of one of its two largest faces; at a depth of 6 inches the borer had traversed a bar of iron which was placed in the iron at the time of casting: hence the hole was not water-tight. After having lost about 2 cubic inches of nitroglycerine in these fissures, the hole was lined with clay to make it tight, and was then charged with nitroglycerine to a height of $4\frac{1}{2}$ inches. It was then closed for a distance of $1\frac{1}{2}$ inch by means of an iron screw plug, having in its axis a tube, which received on the one side the powder, on the other the fuse. The effect was complete, the block broke into four large and ten to twelve small pieces, and the carriage on which it rested was broken.—*Comptes Rendus*, July 17, 1865.

ON THE NATURE OF THE INVISIBLE PHOTOGRAPHIC IMAGE.

BY M. CAREY LEA, PHILADELPHIA.

Some experiments in which I have lately been engaged seem to me to finally settle the long-contested question as to the nature of the invisible photographic image, and I hasten to send a very brief description of them, hoping it will be in time for your July number.

The view that the change which takes place in an iodo-bromized plate in the camera is a purely physical one, that no chemical decomposition takes place, and neither liberation of iodine nor reduction of silver, has obtained a pretty general acceptance. But latterly it has been opposed by two distinguished photographers, Dr. Vogel and Major Russel. The former affirms that iodide of silver is never sensitive unless there is a body present capable of taking iodine from it under the influence of light. And Russel believes that the developed image is chiefly produced at the expense of the silver haloid in the film.

The following experiments seem to me to decisively close this controversy in *favour of the physical theory*.

Experiment 1.—If the iodide or bromide of silver in the film undergoes decomposition in the camera, and still more, if the developed image is formed at its expense, the film of iodo-bromide must necessarily be greatly consumed in the development under the dense portions of the negative which it has contributed to form.

To settle this point, I exposed and developed an iodo-bromized plate in the ordinary manner. Then, instead of removing the unchanged iodide and bromide by fixing in the ordinary manner, I took measures to *remove the developed image without affecting the iodide and bromide*. This I succeeded in doing with the aid of a very weak solution of acid pernitrate of mercury. Now if the iodide, or bromide, or both had been in any way decomposed, to form, or aid in forming the developed negative image, when this came to be removed there should have been left a *more or less distinct positive image*, depending upon varying thicknesses of iodide and bromide in the film, much like a fixed negative that has been completely iodized. *Nothing of this sort* was visible, the film was perfectly uniform, just as dense where an intense sky had been as in those parts which had scarcely received any actinic impression, and looking exactly as it did when it first left the camera and before any developer had been applied.

This experiment seems sufficiently decisive. But the following is far stronger.

Experiment 2.—A plate was treated in all respects as in No. 1, except that the application of the nitrate of mercury for removing the developed image was made by yellow light. The plate, now showing nothing but a uniform yellow film, was carefully washed, and an iron developer, to which nitrate of silver and citric acid had been added, was applied. *In this way the original image was reproduced,* and came out quite clearly with all its details.

Now as every trace of a picture and all reduced silver had been removed by the nitrate of mercury, it is by this experiment absolutely demonstrated that the image is a purely physical one, and that, after having served to produce one picture, that picture may be dissolved off, and the same physical impression may be made to produce a second picture by a simple application of a developing agent.

P.S.—Since the above was written, I have repeated the experiment with a pyrogallic development, with similar results. Both the first and second developments may be made with an iron developer, or both with a pyrogallic. The experiment succeeds without the least difficulty in either way.—Silliman's *American Journal* for July 1865.

EXPERIMENTS ON THE PRODUCTION OF CYLINDERS OF ICE BY PRESSURE THROUGH ORIFICES. BY M. FRESCA.

These experiments were made by acting on ice contained in a cylinder 0·16 metre (about 6 inches) in diameter, with the requisite pressure to drive it through a central orifice in its base 0·05 metre (nearly 2 inches) in diameter. These plates, prepared by Tyndall's method, in some cases coloured at the joints and in others not, acted like plates of lead or of porcelain paste, as before explained by the author to the Academy. The surfaces of the planes of division or joints, originally flat, are transformed by the process, as with other substances, into tubes concentric and perfectly distinct from one another, thus indicating the movement of each point of the mass in the course of the transformation. The cylinders of ice are longitudinally furrowed; and the furrows appeared to proceed from fractures produced at the moment when a portion of the cylindrical block leaves the orifice, and when, as a consequence, it ceases to be subjected to pressure at the outer extremity. The porcelain paste afforded the author delicate furrows or fractures of a similar kind, and in some cases the material was divided into small separate lamellæ. The evenly-bedded structure of the cylinder of ice shows that the origin of these fractures is subsequent to the first formation of the cylinder.

For a block of the dimensions employed in the experiments the pressure required for the flow of the ice is 10,000 kilogs., while for lead it is 50,000 kilogs. These pressures correspond, for the square centimetre, to 126 and 630 kilogs. This pressure of 126 kilogs. corresponds to a column of water 1300 metres in height. It is evident that if the orifice were smaller in proportion to the diameter of the mass under pressure, the force requisite would be notably less.

M. Fresca observes that the circumstances attending the formation of these jets of ice appear to throw light on the controverted question as to the movement of glaciers. The relative displacement of the layers of ice in the process, the change of form in the faces originally flat, the curved form of the layers at the extremity of each partial tube, the large cavities formed toward these extremities, and even the fissures or fractures at the moment of escape from the pressure, are so many points of resemblance to the phenomena of glaciers. There is not the mass of material constituting moraines; but the traces of colouring-matter which are deposited in parallel threads, and which are reunited towards the axis, complete to a degree the analogy.

Tyndall had before shown that ice could be moulded to any shape by pressure in a mould. M. Fresca observes that his trials prove, in addition, that it may be pushed into a thread, in accordance with the geometrical law of this kind of flow. The facts help to bring the explanations of Tyndall and Forbes into accord, since they indicate that the viscosity, more or less great, of the material does not necessarily play any important part in the phenomenon. They exhibit the material conditions of the flow: and the transparency of the jet after its escape from the orifice shows besides, that, under a comparatively feeble pressure, ice may be changed in form without ceasing to be glassy in texture or aspect.—*Les Mondes*, February 23 (Silliman's *American Journal* for July 1865.

NOTE ON THE REFLEXION OF SOUND. BY M. VIONNOIS.

Being on the manœuvring-ground at Nancy, in an extensive plain, the echoes of the troops firing were very distinctly heard; yet instead of being sharp, they were slightly confused and prolonged. The trees of an English garden, separated from the manœuvring-field, could alone have produced this phenomenon; the leaves were thus reflecting surfaces. This fact enables us to understand how the explosion of the electric spark may be reflected by the vesicular vapour of clouds, and the sound be softened and considerably prolonged, owing to the inequality of the distances and of the successive reflexions.—*Comptes Rendus*, February 27, 1865.

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XXXII. *On the Second Law of Thermodynamics.* *By* W. J. Macquorn Rankine, *C.E., LL.D., F.R.SS.L. & E. &c.*[*]

1. It has long been established that all the known relations between heat and mechanical energy are summed up in two laws, called respectively the *first law* and the *second law* of thermodynamics: viz.,

First Law.—Quantities of heat and of mechanical energy are convertible at the rate very nearly of 772 foot-pounds to the British (or Fahrenheit-avoirdupois) unit, or 424 kilogrammetres to the French (or Centigrade-metrical) unit of heat.

Second Law.—The quantity of energy which is converted from one of those forms to the other during a given change of dimensions and condition in a given body, is the product of the absolute temperature into a function of that change, and of the kind and condition of the matter of the body.

By *absolute temperature* is here to be understood temperature measured according to a scale so graduated that the temperature of a homogeneous body shall vary in the simple proportion of the quantity of energy it possesses in the form of sensible or thermometric heat.

2. The laws of thermodynamics, as here stated, are simply the condensed expression of the facts of experiment. But they are also capable of being viewed as the consequence of the supposition that the condition of bodies which accompanies the phenomena of sensible heat consists in some kind of motion amongst their particles.

3. The first law would obviously follow from the supposition

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[*] Communicated by the Author, having been read to the British Association, Section A, at Birmingham, September 1865.

of any kind of molecular motion whatsoever, and it therefore affords of itself no reason for preferring one supposition as to the kind of molecular motion which constitutes sensible heat to another.

4. But if there be molecular motions in bodies, it is certain that, although all such motions are capable of conversion into that which constitutes sensible heat, some of them are not accompanied by sensible heat. For example, the motion (supposed to be vibratory and wave-like) which constitutes radiance, whether visible or invisible, is not accompanied by sensible heat, and only produces sensible heat by its absorption; that is, in the language of hypothesis, by its conversion into some other kind of motion; while, on the other hand, in the production of radiance, sensible heat disappears.

5. The object of the present paper is to give an elementary proof of the proposition, *that the second law of thermodynamics follows from the supposition that sensible heat consists in any kind of steady molecular motion within limited spaces.*

6. The term "steady motion" is here used in the same sense as in hydrodynamics, to denote motion, whether of a continuous fluid or of a system of detached molecules, in which the velocity and direction of motion of a particle depend on its position only; so that each particle of the series of particles which successively pass through a given position, assumes in its turn the velocity and direction proper to that position. In other words, steady motion may be defined as motion in a set of streams of invariable figure.

When steady motion takes place in matter that is confined within a limited space, the streams in which the particles move must necessarily return into themselves, and be *circulating streams*, being in that respect of the nature of whirls, eddies, or vortices.

7. Steady motion keeps unaltered the distribution of the density of the moving matter; and it therefore keeps unaltered the forces depending on such distribution, whether of the nature of pressure or of attraction. In that respect it differs from unsteady motion, such as vibratory and wave-like motion.

8. Conceive a limited space of any figure whatsoever to be filled with matter in a state of steady motion. The actual energy of any particle of that matter is the product of its mass into the half-square of its velocity; and the actual energy of the whole mass of matter is the sum of all those products; and because of the steadiness of the motion, the actual energy of the particle which at any instant whatsoever occupies a given position is some definite fraction of the whole actual energy, depending upon that position, and upon the distribution of matter within the

space; but the scale of absolute temperature is defined as being so graduated that the whole actual energy of the matter within the space is the product of the absolute temperature, the mass of matter, and some function of the sort and distribution of the matter; therefore *the half-square of the velocity of the particle which at any instant occupies a given position in the space considered is equal to the absolute temperature multiplied by some function of that position, and of the sort and distribution of the matter.*

9. Suppose now that the dimensions of the limited space in which the moving matter is enclosed, and the distribution of that matter, undergo an indefinitely small change by the application of suitable forces, and that after that process the motion becomes steady as it was before. Then the dimensions and position of each circulating stream will have been altered; and the work done in effecting that alteration will consist of energy converted between the forms of potential energy of the applied forces, and actual energy of the molecular motions—that is, between the forms of mechanical energy and of heat. Consider now a point in one of the circulating streams before the change, and let fall from it a perpendicular upon the same stream after the change. The work done in shifting the path of the particle which at any instant occupies that point, is the product of the perpendicular displacement of the stream into the force exerted along that perpendicular. But the perpendicular displacement of the stream is a function of the position of the point shifted, the distribution of matter in the space, and the change of dimensions and distribution; and the force is equal and opposite either to the centrifugal force of the particle or to one of its components, and is therefore proportional to the square of the velocity of the particle, and to some function of its position and of the sort and distribution of matter in the body. Therefore *the energy transformed in shifting the path of any particle is proportional to the square of its velocity, and to some function of its position, of the sort and distribution of matter in the space considered, and of the change in dimensions of that space and in the distribution of the matter.*

10. But the square of the velocity of the particle which at any instant occupies a given position has already been shown to be proportional to the absolute temperature, and to some function of that position and of the sort and distribution of the matter; therefore, *if sensible heat consists in any kind of steady molecular motion within limited spaces, the conversion of energy during any change in the dimensions of such spaces, and in the distribution of matter in them, is the product of the absolute temperature into some function of that change and of the sort and distribution of the matter.*

11. In a paper "On the Mechanical Action of Heat," published in the Transactions of the Royal Society of Edinburgh for 1850, the author deduced the Second Law of Thermodynamics, in the form above stated, from the hypothesis of a particular sort of steady molecular motion, viz. revolution in circular streams or vortices. In a paper "On the Centrifugal Theory of Elasticity," published in the same Transactions for 1851, he deduced the same law from the hypothesis of steady molecular motion in circulating streams of any figure whatsoever, being a proposition substantially identical with that set forth in the present paper; but as the demonstration in the paper of 1851 involved tedious and intricate symbolical processes, he has written the present paper in order to show that the demonstration can be effected very simply.

12. It is obvious that the *steadiness* of the supposed molecular motions is the essential condition which makes the Second Law of Thermodynamics deducible from a mechanical hypothesis; and that no kind of unsteady motion, such as vibratory or wave-like motion, would lead to the same results. If, then, it be admitted as probable that the phenomena of heat are due to unseen molecular motions, it must also be admitted, that while the motions which constitute radiance may be vibratory and wave-like, the motions which constitute sensible or thermometric heat must be steady and like those of circulating streams.

13. The function by which the absolute temperature is multiplied in calculating the conversion of energy between the mechanical and the thermic forms, is the variation of what the author has called the *metamorphic function**, being one term of the *thermodynamic function*† which corresponds to what Professor Clausius calls *entropie*‡.

Glasgow University,
September 4, 1865.

APPENDIX.

The following is the symbolical expression of the demonstration given in the paper.

Let m stand for the specific properties of the sort of matter which is in a state of steady motion within a limited space.

f for the figures and dimensions of that space, and of the paths described by the particles contained in it, and δf for any indefinitely small change of such figures and dimensions.

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* See "On the Science of Energetics," Edinburgh Philosophical Journal, 1855.

† Philosophical Transactions, 1854.

‡ *Ueber verschiedene für die Anwendung bequeme Formen der Hauptgleichungen der mechanischen Wärmetheorie*, April 1865.

p for the position, relatively to the centre of the matter contained in the space, of a point which is fixed so long as $\delta f=0$. Because the motion is steady, each particle of matter which successively arrives at the point p assumes the velocity, direction, and curvature of motion proper to that point. Let v be that velocity, and r the radius of that curvature; then for a particle of mass unity, in the act of traversing p,

$$\text{actual energy of mass } 1=\frac{v^2}{2}=k\tau, \quad . \quad . \quad . \quad (1)$$

where τ is a quantity upon whose uniformity throughout the space the steadiness of the motion depends, and k a function of (m, f, p); and

$$\text{centrifugal force of mass } 1=\frac{v^2}{r}=\frac{2k\tau}{r}; \quad . \quad . \quad (2)$$

in which r, and consequently $\frac{2k}{r}$, are functions of (m, f, p).

Now let the change denoted by δf take place, and let the steadiness of the motion be restored: let δn be the length of a line drawn through the original position of the point p, so as to be perpendicular to the path of the stream of particles which formerly traversed p; and let $\widehat{rn}$ be the angle made by δn with r. Then δn and $\widehat{rn}$ are both functions of $(m, f, p, \delta f)$. Also the work done, or energy converted, for a unit of mass at the point p, while the path of the particles that traverse p is shifted through δn, is as follows:

$$\frac{v^2}{r}.\,\delta n\,.\cos\widehat{rn}=\frac{2k\tau\,.\,\delta n\,.\cos\widehat{rn}}{r}=\tau\times\text{function of }(m,f,p,\delta f). \quad (3)$$

The energy converted during the change δf, throughout the whole space considered, is the sum of the quantities of energy converted for each unit of mass within the space. But τ by definition is uniform; and the sum of a set of functions of p is a function of f and m; therefore the *whole energy converted* is

$$\tau\,.\,\Sigma\,.\,\frac{2k\,.\,\delta n\,.\cos\widehat{rn}}{r}=\times\text{ function of }(m, f, \delta f); \quad . \quad (4)$$

and because δf is indefinitely small, the preceding expression is equivalent to the following:

$$\text{energy converted }=\tau\,.\,\text{function }(m, f)\,.\,\delta f=\tau\,.\,\delta\mathrm{F}(m, f). \quad (5)$$

Let τ be called *absolute temperature*, and this is the *second law of thermodynamics*. It is to be observed that f may be, and often is, a function of τ.

September 6, 1865.

XXXIII. *On the Mechanical Equivalent of Light.*

By Professor JULIUS THOMSEN *of Copenhagen*[*].

THAT the luminous ray exerts a mechanical action cannot be doubted; but as to its magnitude, there are at present no determinations. I have proposed to myself to determine this, at all events approximately. Since light can change into heat when it is absorbed by a black and non-luminous surface, we may, from the heating-effect of luminous rays freed from thermal rays, draw conclusions as to the mechanical activity of light. The production of heat by luminous rays was detected by Melloni's apparatus; but as this apparatus has only been used for relative measurements, it was necessary to reduce the results to absolute measurement.

Hence the indications of the thermo-multiplier were compared with the absolute radiation of a known source of heat. As source of heat, a glass bulb was used filled with warm water, and placed at various distances from the thermo-pile. The bulb contained (including the reduced value in water of the glass) 1351 grms. water; at a temperature of 50° C. the cooling of the bulb was $0^\circ{\cdot}185$ a minute, and the bulb experienced therefore a loss of $1351 \text{ gr.} \times 0^\circ{\cdot}185 = 250c$ (c = caloric or thermal unit) in a minute.

But this loss arises partly from radiation, and partly from cooling by contact with the air. If by Dulong's formula that part be calculated which is due to radiation, it is found to be $102c$ for one minute, the temperature of the air being 17°.

This source of heat was placed at a distance of 0·8 metre from the thermo-pile; and the multiplier indicated then a constant deflection of $17^\circ{\cdot}8$. From this it may be inferred that a source of light or of heat which, placed at a distance of 0·8 metre from the thermo-pile, produces a deflection of $17^\circ{\cdot}8$, radiates in a minute 102 thermal units. But as the indications of the multiplier are, within certain limits, proportional to the radiation, under these circumstances a deflection of 1° corresponds to a radiation of $5{\cdot}76c$ in a minute.

By placing the bulb successively at different distances from the pile, and observing the position of the needle when the temperature was only 50°, similar factors were determined for a few other distances, in which the different sources of light should subsequently be placed.

I then placed a candle at a distance of 0·8 metre from the pile, and obtained a constant deflection of the needle of $36^\circ{\cdot}5$[†].

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[*] From Poggendorff's *Annalen*, June 1865.

[†] In this, as in other cases in which the deflection was large, the experiment was made so that the needle before the experiment was deflected o the opposite side, and that the actual deflection from 0° did not exceed 30°.

Hence *the entire radiation of the candle, which burnt* 8·2 *grms. spermaceti in an hour, was* 5·75c . 36°·5 = 210c *in each minute.* As the corresponding quantity of spermaceti disengages about 1400c in a minute, it follows from this observation that only one-seventh of the entire heat disengaged leaves the flame as radiant heat and light, while six-sevenths of the entire quantity are carried away by the heated air.

Experiments were then made with more powerful flames. A moderator lamp, whose luminous intensity was 8·6, and a gas-flame of 7·7 luminous intensity, were placed at a distance of 2 metres, and the results referred to the spermaceti candle as unit. The results were as follows:—

Nature of the flame.	Luminous intensity.	Radiation for the unit of light per minute.
		c
Spermaceti candle......	1	210
Gas-flame	1·2	201
Gas-flame	7·7	199
Moderator lamp	8·6	199

It follows from these results *that the radiation of a flame is proportional to the intensity of light, and that for the unit of light,* 8·2 *grms. of spermaceti in an hour, it is about* 200c *in a minute.*

But this is the action of the entire radiation; in order to determine that part which is due to the luminous rays, the thermal rays had to be eliminated. Probably it is not possible entirely to separate the thermal from the luminous rays; but it may be done approximately. It seemed best to effect the absorption of the thermal rays by water; and by various experiments I convinced myself that the invisible rays cannot traverse a layer of water 0·2 metre in thickness, or at all events traverse it to so small an extent that they exercise no appreciable influence on the result. The flame of a Bunsen's lamp emits, as is well known, a very feeble quantity of light; its thermal radiation is nevertheless very great. I observed, for example, that a Bunsen's lamp which, without access of air, had a luminous intensity of 1·2 unit of light and radiated 255 thermal units, when air was admitted and the flame was non-luminous exhibited a radiation of 196c in a minute. Of these non-luminous rays not a trace passed through 0·2 metre water; but as soon as the access of air ceased and the flame became luminous, the needle was deflected, and indicated that 4·3c were transmitted. Whereas the needle of the multiplier remained unchanged at 0° so long as the radiation of the non-luminous flame took place through water, a deflection of the needle was immediately exhibited as soon as the flame was made luminous, for instance, by chloride of sodium, or other volatile compounds, or by a platinum wire.

It was further seen that the rays which had passed from a non-luminous flame through the layer of water, were absorbed in very various degrees by coloured glasses, while this was only to a small extent the case with the rays before they had been separated by the water.

From these various investigations the conclusion may be drawn that a *layer of water of* 20 *centims. completely absorbs the heat-rays and only transmits light-rays,* or that the absorption of thermal rays is so complete that the error is imperceptible. By experiment, the loss in luminous intensity experienced by radiation through such a layer in water which was enclosed between parallel glass sides, was determined, and the result reduced to the luminous intensity of the light which had so traversed the water. The absorption was found to be 0·13.

Hence between the flame and the thermo-pile was placed a box with parallel sides of plate glass enclosing a layer of water 0·2 metre in thickness, and the flame was then brought to the same distance as in the first experiment. All the circumstances were the same, except that now the radiation had to traverse the water, so that the deflection of the multiplier would only depend on the heating-effect which the light-rays produced. The results were as follows:—

Nature of the flame.	Intensity of light.	Thermal and luminous radiation per minute for the unit of light.	Luminous radiation per minute for the unit of light.
		c	c
Spermaceti candle......	1	210	4·4
Moderator lamp.........	6·25	...	3·9
Moderator lamp.........	8·6	199	4·1
Gas-flame	7·7	199	4·2
Gas-flame	1·2	20	3·7

The mean of these experiments is 4·1*c* in a minute; or the result may be expressed:—

A flame whose luminous intensity is equal to that of a candle which burns 8·2 *grms. in an hour, radiates in the form of light, in each minute, a quantity of heat which would raise the temperature of* 4·1 *grms. water one degree Centigrade.*

But the entire radiation of the flame, inclusive of the heat-rays, amounts to about 200*c*, or is 50 times as great as the luminous rays; and the entire development of heat in the chemical process of the flame is, in a candle and in a lamp, about

350, and in a gas-flame about 1000 times as great as that which appears in the luminous rays.

Reduced to mechanical measure, the mechanical equivalent of light is as follows:

The unit of the quantity of work in a second, that is, 1 *kil. raised to a height of* 1 *metre in a second, is equal to that which the luminous rays contain which issue from a source of light whose luminous intensity is* 34·9 *times as great as that developed by a candle which burns* 8·2 *grms. of spermaceti in an hour.*

This is the maximum of the mechanical equivalent of light; it may possibly still be somewhat reduced by subsequent experiments, but the equivalent is in any case very small. I shall subsequently continue the investigation with more intense light, as solar and the electric light.

XXXIV. *Chemical Notices from Foreign Journals.*
By E. Atkinson, *Ph.D., F.C.S.*

[Continued from vol. xxix. p. 538.]

FROM numerous investigations on nitride of iron, it would appear that iron is capable of uniting with nitrogen in numerous proportions, and thus can alloy itself with a nitride of iron of definite composition. Stahlschmidt* has recently investigated the preparation and properties of nitride of iron, and finds that nitrogen unites with iron in a perfectly definite proportion, and that all the different nitrides of iron, the existence of which has been assumed, must be regarded as mixtures of this definite nitride with pure iron.

The mode of preparation he adopted was that of Regnault and Fremy, which consists in passing ammonia over pure protochloride of iron. The temperature at which the action takes place may be very low—far under the red heat; and that chloride which is exposed in a thin layer passes almost immediately into nitride, while thicker masses require a longer time. Nitride of iron prepared at different temperatures contains different proportions of nitrogen; and the proportion is in general smaller the lower the temperature. The best results are obtained when the chloride is exposed in a very thin layer to the action of ammoniacal gas at a temperature at which the chloride of ammonium formed is just volatilized. The nitride of iron obtained thus, partly in thin laminæ and partly as a grey powder, has exactly the composition $N\,Fe^4$, and is thus an ammonium in which all hydrogen is replaced by iron. It contains 11·1 per cent. of nitrogen.

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* Poggendorff's *Annalen,* May 1865.

If nitride of iron is heated to a high temperature in hydrogen, or even in ammonia, it loses nitrogen. The percentage of one specimen thus heated in ammonia was reduced from 9·3 to 4·9 per cent. The discordant results which various experimenters have arrived at as regards the composition of nitride of iron are explained by the remarkable property it has, that its decomposition takes place at a temperature close to that at which it is formed.

In its formation ammonia is first decomposed,

$$NH^3 = N + H^3,$$

while the hydrogen decomposes protochloride of iron,

$$3FeCl + 3H = 3Fe + 3HCl.$$

The hydrochloric acid unites with a corresponding quantity of ammonia to form sal-ammoniac, while the porous iron unites with free nitrogen to form nitride of iron.

Nitride of iron was also prepared by heating iron in ammoniacal gas at just that temperature at which the gas is decomposed; the iron used was some which had been prepared from nitride of iron by heating in hydrogen.

According to Fremy, nitrogen gas unites directly with iron, though with difficulty; more readily when nitrogen is passed over a mixture of oxide of iron and charcoal. The results of Stahlschmidt's experiments lead him to the conclusion that free nitrogen does not unite directly with iron.

Nitride of iron, according to the mode of its preparation, appears either as a grey powder, or as a more or less compact silver-white mass; it is extremely brittle, and easily pulverizable. When projected into a gas- or spirit-flame, it burns with brilliant sparks. When heated, it gives off its nitrogen; iron which still contained 0·9 per cent. of nitrogen was not in the least brittle, but had the properties of iron free from nitrogen.

Nitride of iron dissolves in hydrochloric and in sulphuric acids with liberation of hydrogen and formation of ammoniacal salts. Iodine placed in contact with nitride of iron in the presence of water does not combine with it. The same is the case with bromine; but when heat is applied, bromine combines with it, disengaging hydrogen, while the nitrogen can be detected in the solution as ammonia. Chlorine acts in a similar manner.

The iron prepared by heating nitride of iron in hydrogen is of a silver-white colour, lustrous, and so soft that in thick pieces it can be cut with an ordinary knife without much trouble. It is soluble in acids without any residue, and is probably the purest iron which has yet been obtained.

Stahlschmidt finally discusses the theory of the formation of steel, with especial reference to the properties of nitride of iron.

He draws the conclusion that proofs are wanting of the presence of nitrogen in steel, and still more so of the necessity of nitrogen as a steel-forming body. It follows from various investigations, that iron can pass into steel when placed in contact at a high temperature with bodies free from nitrogen, but that, on the other hand, the volatile cyanogen compounds are to be regarded as bodies specially suited for the formation of steel.

Rudorff* describes a method for determining the quantity of carbonic acid in coal-gas, which in the absence of the usual gasometric apparatus may be used with advantage. The principle of the method consists in absorbing the carbonic acid by concentrated potash, and replacing the carbonic acid which disappears by an equal volume of caustic potash; but without a figure a description would not be easily intelligible.

Hautefeuille describes the artificial formation of sphene and Perowskite†. To prepare the former, three parts of silicic acid and four of titanic acid are introduced into a large platinum crucible, which after being filled with chloride of calcium and suitably protected, is rapidly raised to a high temperature. The mass when cold is treated with acidulated water, which dissolves the excess of chloride of calcium and a little silicate of lime. In all cases under these circumstances crystals of sphene are formed; but their development is favoured by all the circumstances which prolong the action of the chloride of calcium. It is probable that the crystallization is not produced by simple fusion in the gangue, but arises from the action of chloride of titanium on a silicate of lime. A mixture of coarsely powdered rutile, silica, and chloride of calcium gives, with a day's heating, a beautiful crystallization. The pieces of rutile are covered with crystals of sphene, just like the specimens from St. Gothard.

Analysis of the artificial mineral gave numbers agreeing with the formula SiO^2, TiO^2, CaO.

At a red heat sphene in contact with chloride of calcium is rapidly decomposed by aqueous vapour. The decomposition is best effected by heating to bright redness in a platinum dish a mixture of titanic and silicic acids with chloride of calcium, and exposing it to the action of a stream of carbonic acid saturated at 50° with aqueous vapour, or to that of a current of air saturated with hydrochloric acid and moisture by passing through hydrochloric acid. By repeated treatment of the residual mass with hydrochloric acid and aqueous potash, crystals are obtained which have all the physical, crystallographical, and optical properties of natural titanate of lime, Perowskite, $CaO\,TiO^2$.

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* Poggendorff's *Annalen,* May 1865.

† *Comptes Rendus,* vol. lix. p. 68.

Fleitmann describes[*] a method of preparing oxygen gas, to which he was led by the observation that a concentrated solution of bleaching-powder in contact with a trace of freshly precipitated moist peroxide of cobalt is *completely* decomposed into chloride of calcium and oxygen. It is known that there are several oxides of cobalt; and the explanation of the above process is doubtless that the lower oxide continually takes oxygen from the hypochlorite of lime, passes into a higher oxide, and then is resolved into oxygen and a lower oxide. The action of the peroxide is thus quite analogous to that of binoxide of nitrogen in the sulphuric acid manufacture.

The same quantity of peroxide serves for the decomposition of fresh quantities of hypochlorite of lime; and the quantity required is extremely small, one-half to one-tenth per cent. of the chloride is sufficient. It is not even necessary to have freshly precipitated peroxide; a few drops of any soluble cobalt-salt added to a solution of hypochlorite of lime produces at once a corresponding quantity of hydrated peroxide.

Unfortunately it is essential to have a clear solution of hypochlorite; for otherwise it froths up. The clear solution is best obtained by decantation, the liquid used for exhausting a first portion being applied to a second, and so on. The vessels used may be filled with the liquid to the extent of $\frac{7}{8}$ of their capacity.

Ste.-Claire Deville has given the name of *dissociation* to the phenomenon which many bodies exhibit of partially decomposing into their constituents when heated at a considerably lower temperature than that at which they are completely decomposed, or at which their constituents unite[†]. In two recent communications[‡] he has furnished additional instances of this phenomenon.

The electric spark decomposes many substances, doubtless in consequence of the great heat which it disengages; and if a reunion of the separated constituents does not ensue, this is probably due to the circumstance that on their decomposition they come into contact with a relatively cold atmosphere; for the path of the spark is very short as compared with the mass of the gas, whose temperature is scarcely changed.

Deville has devised a plan by which the same conditions are attained without the aid of electricity. A porcelain tube is laid through a furnace in which a very high temperature can be produced; its ends are provided with corks in which are two perforations. In each cork is fitted a small tube, serving at the one end to

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[*] Liebig's *Annalen*, April 1865.

[†] Phil. Mag. S. 4. vol. xx. p. 448; vol. xxv. p. 536.

[‡] *Comptes Rendus*, vol. lix. p. 873; vol. lx. p. 317.

admit a gas, and at the other to allow its exit. A thin brass tube three-tenths of an inch in diameter passes through the entire length of the tube and through the other hole in each of the corks; through this tube a rapid current of cold water constantly passes. Two small screens of unglazed porcelain in the interior of the tube separate those parts which are to be heated from those which, projecting from the furnace, remain quite cold.

A current of pure and dry carbonic oxide was admitted into the tube, and the issuing gas passed into baryta-water. When the temperature attained a bright redness, the baryta-water indicated that some carbonic acid was formed. The carbonic oxide had thus been decomposed into carbonic oxide and carbon; the latter was found deposited as fine lampblack in the brass tube, whose temperature even in the hottest part of the apparatus never exceeds 10°. The mass of the water was so great that its temperature was not perceptibly raised by passing through the tube.

In this experiment there is a strongly heated cylindrical porcelain surface, and a very cold concentric brass one. The particles of carbonic oxide heated in the lower part of the tube ascend, after partial decomposition into carbon and oxygen; but this current meets the cold and rough side of the brass tube, and the carbon particles are deposited on it mechanically, and, being kept cold, escape the influence of the oxygen. In all cases the carbon was actually found deposited on the lower surface of the tube.

If through the above apparatus, which may be spoken of as that of the hot and cold tube, a current of pure dry sulphurous acid be passed while the tube is at a temperature of about 1200° C., the gas is partially decomposed into sulphur and anhydrous sulphuric acid. The metal tube was made of galvanoplastic copper thickly coated with silver. Silver, it is known, has no action on sulphurous acid at 300°, and far less therefore at 10°. At the close of the experiment the silver tube was found coated with sulphide of silver and with a layer of anhydrous sulphuric acid.

Sulphurous acid can also be decomposed by the electric spark of a Ruhmkorff's coil. Two small graduated eudiometers are filled with pure sulphurous acid gas; into one of them some solution of chloride of barium in saturated aqueous sulphurous acid is introduced; into the other some hydrated sulphuric acid. After continuing the action of the spark for some days, the gas entirely disappears and the mercury rises in its place. Much sulphur is deposited, and the tube in which is the barium solution shows a precipitate of sulphate of baryta; while in the other some fuming sulphuric acid is contained.

Hydrochloric acid has hitherto resisted all attempts to decompose it; and the reason is not difficult to explain; the tension of dissociation at high temperatures is very small, and the tendency to unite so powerful that it is not easy to see how the momentarily separated substances cannot but reunite. But by the following device Deville has succeeded in showing that there is a slight dissociation. The silver tube in the above experiment is amalgamated with a very thin coating of mercury, which is not attacked by hydrochloric acid gas at a temperature of 300°. Pure hydrochloric acid gas was admitted into the above apparatus, a difference in temperature of about 1500° being maintained between the porcelain tube and the amalgamated silver. At the close of the experiment, on moistening the tube with ammonia, some of it was blackened, and a small portion of chloride of silver dissolved. The gas, carefully collected, was found to contain a distinct quantity of hydrogen.

In a repetition of the carbonic oxide experiment, Deville found that when some purified lampblack was placed in the apparatus and heated to nearly the fusing-point of silver, and carbonic oxide passed through, a considerable proportion of carbonic acid was even then formed.

By passing the electrical spark of a Ruhmkorff's coil through carbonic oxide placed in a eudiometer containing some caustic potash, it was completely decomposed into carbon and carbonic acid, which was absorbed by the potash. For complete decomposition, however, five or six days were required.

Carbonic acid is decomposed by the electric spark into carbonic oxide and oxygen; and if some phosphorus be placed in the eudiometer so as to absorb the latter, the decomposition is nearly complete in the course of twenty-four hours.

Laspeyres* gives a complete account of the various substances in which, up to the present time, the new alkaline metals cæsium and rubidium have been found, whether separately or together. He also describes the occurrence of these two metals in a plutonic silicate in the Rhine province. According to his analyses, the rock appears to contain about 0·00038 per cent. of cæsia and about 0·0003 per cent. of rubidia. He is still engaged on the investigation of the occurrence of these minerals in rocks of this origin.

Engelbach has examined† a basalt from Upper Hesse, and has found that it contains *rubidium*, apparently in larger quantity than was found by Laspeyres. The rock contained no cæsium, but traces of the other comparatively rare substances, *titanium*, *chromium*, and *vanadium*.

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[*] Liebig's *Annalen*, June 1865.

[†] Ibid. July 1865.

In the Giessen laboratory, in which this investigation was made, chloride of rubidium was prepared from a large quantity of ashes from beech wood which had grown on a basaltic soil.

Debray has investigated the chlorides of tungsten*. When pure and dry chlorine is passed over tungsten heated to dull redness in a tube of hard glass, intensely red vapours are produced which condense to a liquid of a deep grey colour. This is a mixture of perchloride of tungsten, WCl^3, and of subchloride, $W^2 Cl^5$ ($W=92$, $Cl=35{\cdot}5$). To obtain the perchloride as pure as possible, it must be repeatedly distilled in a current of chlorine; and though it can never be obtained quite free from subchloride, the quantity of the latter is made so small as to have no appreciable effect on the composition.

There are also two oxychlorides of tungsten; one, which is red, has the formula $WO\,Cl^2$, and the other, which is of a yellowish white, the formula $WO^2\,Cl$. They are obtained mixed with tungstic acid by passing chlorine over dry oxide of tungsten, WO^2, and are separated by their varying volatility, the red being most volatile.

They may also be obtained by distilling perchloride of tungsten with oxalic acid in suitable proportions. The red is pure; but however the yellow chloride is procured it is always mixed with red oxychloride or with tungstic acid, for when distilled it decomposes into these two substances:

$$2WO^2\,Cl = WOCl^2 + WO^3.$$

When perchloride of tungsten is mixed with anhydrous tungstic acid, the two combine with disengagement of heat, forming the red oxychloride:

$$2WCl^3 + WO^3 = 3WO\,Cl^2.$$

This reaction is analogous to that observed by Persoz in distilling perchloride of phosphorus and anhydrous phosphoric acid.

Debray was only able to determine with certainty the vapour-density of the yellow oxychloride and of the red chloride. The theoretical density of the chloride, WCl^3, corresponding to two volumes, is 13·75, and 6·875 if it correspond to four volumes; and in like manner the density of the oxychloride, $WO\,Cl^2$, for a two-volume vapour is 11·86, and for a four-volume 5·93. Now the mean number for the density of WCl^3 is 11·72, and for the oxychloride, $WO\,Cl^2$, is 10·58; which are therefore materially different from the numbers required by theory.

But if the equivalent of tungsten be altered, and it be supposed that the perchloride has the formula WCl^5, this would require

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[*] *Comptes Rendus*, April 24, 1865.

for a four-volume density 11·46, which agrees sufficiently with observation. The formula of the oxychloride must then be written $WO^{\frac{5}{3}}Cl^{\frac{10}{3}}$, according to which its vapour-density is 9·871 on the four-volume hypothesis; but if the fractions are made to disappear and its formula be called $W^3O^5Cl^{10}$, we must assume that its vapour-density then corresponds to twelve volumes.

Strecker describes the preparation of several salts of peroxide of thallium*. Crude chloride of thallium is easily purified by crystallization from solution of carbonate of soda or of caustic soda. The chloride is added to solution of soda, which is raised to boiling as long as any dissolves; on cooling, it crystallizes out in white feathery crystals. If the solvent contained any organic matter, the chloride of thallium is somewhat grey in colour, owing to a partial reduction. In fact if an alkaline solution of chloride of thallium is heated with grape-sugar or with protochloride of tin, thallium is deposited in a grey powder, though it is impossible in this way to precipitate it entirely.

Peroxide of thallium is prepared by adding to a boiling saturated solution of chloride of thallium in carbonate of soda hypochlorite of soda, on which a dark brown precipitate resembling peroxide of lead separates. This peroxide dissolves in warm dilute sulphuric acid; on concentration, colourless thin laminæ separate, which are the sulphate of peroxide of thallium,

$$TlO^3\,3SO^3+7HO;$$

on drying they lose 6 atoms of water. The double salts, *sulphate of peroxide of thallium and soda*, $TlO^3\,3SO^3+NaO\,SO^3$, *and sulphate of peroxide of thallium and potash*,

$$TlO^3\,3SO^3+KO\,SO^3,$$

are also crystallized salts. Besides these, several other salts were examined.

As not only the ordinary oxide TlO, but also the so-called peroxide TlO^3 have decidedly basic properties, Strecker suggests that to the former the name *thallous oxide* (*thalliumoxydul*), and to the latter the name *thallic oxide* (*thalliumoxyd*) might be assigned. And he points out that in its combinations with oxygen and the halogens, thallium agrees with gold, inasmuch as in both cases the quantities of oxygen or chlorine united with the same weight of metal are as 1 : 3. But seeing, on the other hand, its analogies with, yet divergencies from lead, and also with the alkaline metals, it must be confessed that as yet it can be included in none of the families assumed.

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* Liebig's *Annalen*, August 1865.

Hebberling has also made a communication* on thallium. Besides a careful determination of the atomic weight of the metal, in which he obtained numbers not at all materially differing from those obtained by Lamy and by Werther, he examined the action of the most ordinary chemical reagents on the salts of thallium, and has also made careful determinations of the solubility of the chloride, the iodide, and the platinochloride.

Troost has communicated the results of an investigation† on metallic zirconium. He obtained it crystallized by heating, in a crucible of gas-graphite at the fusing-point of iron, one part of double fluoride of potassium and zirconium with one and a half part of aluminium. When the crucible was cold, the surface of the aluminium was found coated with crystalline laminæ pressed against each other like the leaves of a book. On treating the regulus with dilute hydrochloric acid, at first laminæ of zirconium were separated; and when all the aluminium was dissolved, an alloy was left of aluminium and zirconium; which two metals, from the analogy in their properties, appear to dissolve each other in all proportions.

The above reaction takes place at a lower temperature; but then the alloy of zirconium and aluminium is almost exclusively obtained.

The zirconium, obtained as above, contained 1·28 per cent. of aluminium and 0·55 per cent. of silicon.

Besides the above method, Troost also obtained zirconium in small microscopic crystals by passing vapour of chloride of zirconium over aluminium heated to redness in a porcelain tube traversed by a current of hydrogen. It was also obtained in small crystalline plates on decomposing, by the battery, fused double fluoride of zirconium and potassium. These crystals decompose water in the cold.

Graphitoidal zirconium does not appear to exist, or if so, only under special conditions. Amorphous zirconium, with all the properties described by Berzelius, wâs prepared by passing chloride of zirconium in vapour over sodium heated to redness in a porcelain tube.

Crystallized zirconium is a very hard lustrous substance, resembling antimony in its colour, brilliancy, and brittleness. It is obtained in very thin plates, about half an inch in length and apparently forming an oblique prism. Its density is 4·15, differing little from that of zircon. It is certainly less fusible than silicon, and is so difficult of fusion that no certain opinion can be expressed regarding its fusing-heat.

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* Liebig's *Annalen*, April 1865.

† *Comptes Rendus*, July 17, 1865.

Crystallized zirconium resists the action of oxygen at a red heat; at a white heat it becomes covered with a thin iridescent layer which protects the rest of the metal; it only burns in the flame of the oxyhydrogen blowpipe. In chlorine, combustion takes place at a red heat with incandescence, forming chloride of zirconium. Hydrate of potash in fusion is decomposed by zirconium, with liberation of hydrogen, which ceases when the potash is quite dehydrated. Heated to redness with silica it is reduced, forming amorphous silicon and zirconia.

Nitric and sulphuric acids have no action on zirconium in the cold, and scarcely any when they are hot and concentrated. Gaseous hydrochloric acid is decomposed by zirconium at a red heat, forming chloride of zirconium but no subchloride.

Hydrochloric acid in the cold does not dissolve zirconium, by which this body is distinguished from aluminium. The true solvent is hydrofluoric acid, in which, whether strong or not, zirconium dissolves—a deportment in which it differs from silicon.

Troost says in conclusion, zirconium plays, in the carbon family, a part analogous to that of antimony in the nitrogen family. It forms a passage between metallic silicon and metallic aluminium, and justifies Sainte-Claire Deville's classification, who has placed carbon, boron, silicon, zirconium, and aluminium in one natural group.

XXXV. *Supplementary Researches in Hydrodynamics.*—Part II.

By Professor CHALLIS, *M.A., F.R.S., F.R.A.S.*[*]

THE first Part of these Researches was devoted to considerations preliminary to the investigation of the motions of a spherical solid submitted to the action of the vibrations of an elastic fluid. I proceed now to give a solution of this important problem in accordance with the principles there advocated. The conditions of the problem will be assumed to be such that both the motion and the condensation of the fluid are symmetrical with respect to an axis passing through the centre of the sphere, and parallel to the direction of the incidence of the waves. The equation (ϵ), obtained in the Supplementary Number of the Philosophical Magazine for December 1864 (p. 493), is applicable to such motion, and will be employed in the present investigation. For greater distinctness in carrying on the subsequent argument, the reasoning by which that equation was arrived at will be here reproduced.

In Part I. (September Number, p. 211) I have shown how to obtain the following expressions to the second approximation for

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[*] Communicated by the Author.

the condensation (σ') and velocity (w') along a rectilinear axis of motion:—

$$a\sigma' = m\kappa \sin b\mu - \frac{2m^2\kappa^2}{3a(\kappa^2-1)}\cos 2b\mu + \frac{m^2}{2a}(\kappa^2-1)\sin^2 b\mu, \quad (\alpha)$$

$$w' = m \sin b\mu - \frac{2m^2\kappa}{3a(\kappa^2-1)}\cos 2b\mu, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (\beta)$$

μ being put for $z-\kappa at+c$, and b for $\frac{2\pi}{\lambda}$, and the other letters having their usual significations. The first terms of these expressions are much more considerable than the others, on account of the factor $\frac{m}{a}$ contained in the latter, which is supposed to be a very small quantity. It was argued (p. 212) that the last term of the value of $a\sigma'$ necessarily coexists with the first, otherwise the movement of a *given* particle could not be wholly vibratory. Further, it was argued that as the equations (α) and (β) were obtained without reference to any specific case of motion, they must be applicable to all cases of vibratory motion; and that since the antecedent reasoning showed that motion along an axis is always accompanied by motion transverse to the axis, such general application requires that the total motion in a given instance should be composed of an unlimited number of longitudinal motions, such and so disposed that the transverse motions destroy each other. This being understood, we may at present omit the consideration of the terms involving m^2, and confine ourselves to quantities of the first order. In that case the differential equations from which the values of σ' and w' are derived are linear with constant coefficients, and we may consequently assume that any arbitrarily imposed velocity is the *sum* of an unlimited number of velocities defined by the expression $m \sin \frac{2\pi}{\lambda}(z-\kappa at+c)$, m, λ, and c being at disposal, and the axes of motion being all parallel; and similarly that any condensation resulting from arbitrary conditions is compounded of an unlimited number of condensations accompanying the velocities and severally defined by circular functions. But if V_1 be the composite velocity and σ_1 the corresponding composite condensation, the relation between V_1 and σ_1 will not be the same as that between σ' and w' in each component, although the rate of propagation of velocity and condensation will be the same in the compound waves as in the components, namely, the velocity κa. The reason for the different relation will be seen by considering that in an uncompounded series of vibrations the condensation is due both to longitudinal and to transverse vibrations, and is therefore greater for a given longitudinal velocity than it would be if the effect of the transverse vibrations were neutralized, as is the case in the composite motion. In order to find the relation between V_1 and σ_1, recourse must be had to the abstract relation between velocity and condensation uniformly propagated in parallel lines, which is investigated in Part I. (p. 218). It is there shown that to the first approximation $V_1 = \kappa a \sigma_1$. Hence, if

$$V_1 = \Sigma \,.\, m \sin \frac{2\pi}{\lambda} \,.\, (z - \kappa a t + c) = f(z - \kappa a t + c_1),$$

we shall have $\sigma_1 = \frac{1}{\kappa a} \,.\, f(z - \kappa a t + c_1)$. On the principle of the coexistence of small vibrations, contemporaneously with the propagation of V_1 and σ_1 in the positive direction, there may be propagated in the contrary direction the velocity V_2 and condensation σ_2 such that $V_2 = -\kappa a \sigma_2 = F(z + \kappa a t + c_2)$. Hence if $V = V_1 + V_2$, and $\sigma = \sigma_1 + \sigma_2$, we have

$$V = f(z - \kappa a t + c_1) + F(z + \kappa a t + c_2),$$

$$\kappa a \sigma = f(z - \kappa a t + c_1) - F(z + \kappa a t + c_2).$$

It follows from these two equations that

$$\kappa^2 a^2 \,.\, \frac{d\sigma}{dz} + \frac{dV}{dt} = 0 \,.\,.\quad . \quad . \quad . \quad . \quad . \quad . \qquad (\gamma)$$

This differential equation takes account of the composite character of the motion, and, as far as regards the first order of approximation, is of perfectly general application. For although the axes of the component motions were supposed in the investigation to be rectilinear and parallel, since this is a *differential* equation, by a well-known principle of analytical reasoning, it is ultimately applicable to curvilinear and inclined lines of motion, provided they are everywhere inclined to each other by indefinitely small angles. Hence dz may be assumed to be the same as ds, the increment of a line of motion, and the equation will thus agree in form with the general dynamical equation of the first order applicable to uncompounded motion, only having $\kappa^2 a^2$ in the place of a^2.

Conceive now waves to impinge on a sphere at rest, and let the centre of the sphere be the origin of the polar coordinates r and θ, the angle θ being measured from the part of the axis directed *towards* the origin of the waves. Also let U and W be the resolved parts of the velocity respectively along and perpendicular to the radius vector, so that $V^2 = U^2 + W^2$. Consequently

$$\frac{d\sigma}{ds}=\frac{d\sigma}{dr}\cdot\frac{dr}{ds}+\frac{d\sigma}{rd\theta}\cdot\frac{rd\theta}{ds}$$

$$=\frac{d\sigma}{dr}\cdot\frac{\mathrm{U}}{\mathrm{V}}+\frac{d\sigma}{rd\theta}\cdot\frac{\mathrm{W}}{\mathrm{V}},$$

and

$$\frac{d\mathrm{V}}{dt}=\frac{d\mathrm{U}}{dt}\cdot\frac{\mathrm{U}}{\mathrm{V}}+\frac{d\mathrm{W}}{dt}\cdot\frac{\mathrm{W}}{\mathrm{V}}.$$

Hence, by substitution in the equation (γ),

$$\left(\kappa^2a^2\cdot\frac{d\sigma}{dr}+\frac{d\mathrm{U}}{dt}\right)\mathrm{U}+\kappa^2a^2\cdot\left(\frac{d\sigma}{rd\theta}+\frac{d\mathrm{W}}{dt}\right)\mathrm{W}=0. \quad . \quad (\delta)$$

The equation of constancy of mass to the same approximation is

$$\frac{d\sigma}{dt}+\frac{d\mathrm{U}}{dr}+\frac{2\mathrm{U}}{r}+\frac{d\mathrm{W}}{rd\theta}+\frac{\mathrm{W}}{r}\cot\theta=0. \quad . \quad . \quad . \quad . \quad (\epsilon)$$

Now let it be required to ascertain from these two equations values of U and W that are not related in any manner indicated by the given conditions of the problem, but in a manner which depends only on the mutual action of the parts of the fluid. Such values must be found by integration, and it is therefore necessary to obtain a differential equation proper for this purpose, which may be done as follows. Since by hypothesis U and W have no *assignable* relation, the factors by which they are multiplied in equation (δ) must be separately equated to zero, and then from the two resulting equations and the equation (ϵ) U and W are to be eliminated. The equation which this process gives is

$$\frac{1}{\kappa^2a^2}\cdot\frac{d^2\cdot\sigma r}{dt^2}=\frac{d^2\cdot\sigma r}{dr^2}+\frac{1}{r^2}\cdot\left(\frac{d^2\cdot\sigma r}{d\theta^2}+\frac{d\cdot\sigma r}{d\theta}\cot\theta\right). \quad . \quad (\zeta)$$

But for the solution of our problem this equation does not possess the requisite generality. For not only may the values of U and W depend on the mutual action of the parts of the fluid, but also *changes* of these values from point to point accompanying changes of condensation may be determined solely by the same action. On this account I differentiate the above equation with respect to θ in order to obtain an equation in which $\frac{d\sigma}{d\theta}$ is the principal variable. (It should be noticed that differentiation with respect to r does not give in a similar manner an equation in which $\frac{d\sigma}{dr}$ is the principal variable.) After substituting q for $\frac{d\sigma}{d\theta}$, the result of the differentiation is the equation which it was proposed to obtain, viz.

$$\frac{1}{\kappa^2a^2}\cdot\frac{d^2qr}{dt^2}=\frac{d^2\,.\,qr}{dr^2}+\frac{1}{r^2}\left(\frac{d^2\,.\,qr}{d\theta^2}+\frac{d\,.\,qr}{d\theta}\cot\theta-\frac{qr}{\sin^2\theta}\right). \quad (\eta)$$

Now this equation admits of being exactly integrated by supposing that $qr=\phi_1\sin\theta+\phi_2\sin\theta\cos\theta$, and that ϕ_1 and ϕ_2 are functions of r and t only. For on substituting this value of qr in the equation, the result is

$$\left(\frac{1}{\kappa^2a^2}\cdot\frac{d^2\phi_1}{dt^2}-\frac{d^2\phi_1}{dr^2}+\frac{2\phi_1}{r^2}\right)\sin\theta$$

$$+\left(\frac{1}{\kappa^2a^2}\cdot\frac{d^2\phi_2}{dt^2}-\frac{d^2\phi_2}{dr^2}+\frac{6\phi_2}{r^2}\right)\sin\theta\cos\theta=0,$$

which shows that the equation (η) is satisfied by the assumed value of qr, if the quantities ϕ_1 and ϕ_2 be determined by integrating the equations

$$\frac{1}{\kappa^2a^2}\cdot\frac{d^2\phi_1}{dt^2}-\frac{d^2\phi_1}{dr^2}+\frac{2\phi_1}{r^2}=0, \quad \ldots \quad (\theta)$$

$$\frac{1}{\kappa^2a^2}\cdot\frac{d^2\phi_2}{dt^2}-\frac{d^2\phi_2}{dr^2}+\frac{6\phi_2}{r^2}=0. \quad \ldots \quad (\iota)$$

Although the integral thus obtained may not be the most general that the equation (η) admits of, yet if it can be shown to satisfy the conditions of the proposed problem, it must be the particular integral which is alone appropriate to the question. I propose, therefore, to inquire next what is indicated by this integral.

In the first place it may be remarked that whether qr be supposed equal to $\phi_1\sin\theta$ or to $\phi_2\sin\theta\cos\theta$, or to the sum of these two quantities, the equation (η) is satisfied. This analytical circumstance shows that the motions indicated by the two functions are independent of each other, and may exist either singly or conjointly. Let us first suppose that $qr=\phi_1\sin\theta$. The solution thence derived, which satisfies also the equation (ζ), has been discussed in the communication entitled "Researches in Hydrodynamics," contained in the Philosophical Magazine for June 1864. The case there considered is that of a sphere of radius c vibrating in the fluid with the velocity T, and the results obtained are

$$\mathrm{U}=\frac{\mathrm{T}c^3}{r^3}\cos\theta,\quad \mathrm{W}=\frac{\mathrm{T}c^3}{2r^3}\sin\theta,\quad a^2\sigma=\frac{d\mathrm{T}}{\kappa^2dt}\cdot\frac{c^3}{2r^2}\cos\theta.$$

The investigation also showed that if the velocity of the sphere be supposed to be impressed on the sphere and the whole of the fluid, in the direction contrary to that of the sphere's actual motion, the above expressions give, to the first approximation, the velocity and condensation due to the reaction of the sphere. We have thus the case of waves incident on a sphere at rest, but *unaccompanied by condensation,* the whole mass of fluid moving with the same velocity. The motion of the fluid being by hypothesis vibratory, the expression for $\frac{d\mathrm{T}}{dt}$ will be a periodic function, even if T be supposed to be identical with w', and consequently to include a term of the second order. Hence by such action there will be no tendency to produce a permanent motion of translation of the sphere. The case will, however, be different if the impinging waves be accompanied by condensation, and to this circumstance we have now to direct our attention. But a preliminary remark of considerable importance must first be made.

The above expressions show that if the velocity T be constant, and consequently either the sphere have a given velocity, or a uniform stream impinge upon it, the condensation σ vanishes, so that there is no dynamical action between the sphere and the fluid. At the same time the velocities U and W are such that in the case of the moving sphere as much fluid passes it as it displaces; and in the case of the fixed sphere, the quantity of fluid which passes the plane through its centre perpendicular to the stream is the same as if the stream had not been interrupted by the sphere. The circumstances will be very nearly the same if T represent the velocity in waves of which the breadth and maximum velocity are such that the excursion of a given particle much exceeds the diameter of the sphere. In fact I have only recently recognized that the part of the integral of (η) now under consideration applies especially to motions of this magnitude, and that it is distinguished in that respect from the other part, which applies to motions that are small compared to dimensions of the sphere. This will more fully appear in the sequel: at present it is to be understood that we are investigating the dynamical action, on a small sphere, of waves of alternate condensation and rarefaction, the excursions of whose particles are large compared to the sphere's diameter.

We have, first, to obtain ϕ_1 by integrating the equation (θ). The known integral of this equation is

$$\phi_1=\frac{1}{r}\cdot\{f(r-\kappa at)+\mathrm{F}(r+\kappa at)\}-f'(r-\kappa at)-\mathrm{F}'(r+\kappa at),$$

and for the present purpose it will be proper to retain both the arbitrary functions. Also for the sake of brevity these functions will be represented by f and F. Hence, since $qr=\phi_1\sin\theta$, and $q = \frac{d\sigma}{d\theta}$, it follows that

$$\frac{d\sigma}{d\theta} = \left(\frac{f+\mathrm{F}}{r^2} - \frac{f'+\mathrm{F}'}{r}\right)\sin\theta.$$

Consequently by integration,

$$\sigma = \psi(r, t) - \left(\frac{f+\mathrm{F}}{r^2} - \frac{f'+\mathrm{F}'}{r}\right)\cos\theta.$$

As the sphere is supposed to be very small and to disturb the fluid through a very small space compared to the magnitude of the waves, it will be assumed that wherever r is very large σ is equal to the condensation σ_1 of the waves at incidence. Consequently $\psi(r, t)$ is equal to σ_1, and does not contain r, σ_1 being supposed to be a function of the time. Again, from the equations

$$\kappa^2 a^2 . \frac{d\sigma}{dr} + \frac{d\mathrm{U}}{dt} = 0, \quad \kappa^2 a^2 . \frac{d\sigma}{r d\theta} + \frac{d\mathrm{W}}{dt} = 0,$$

it will be found, assuming U and W to be wholly periodic, that

$$\frac{\mathrm{W}}{\kappa a} = \left(\frac{f_1 - \mathrm{F}_1}{r^3} - \frac{f-\mathrm{F}}{r^2}\right)\sin\theta,$$

$$\frac{\mathrm{U}}{\kappa a} = \left(\frac{2(f_1 - \mathrm{F}_1)}{r^3} - \frac{2(f-\mathrm{F})}{r^2} + \frac{f'-\mathrm{F}'}{r}\right)\cos\theta,$$

f_1 and F_1 being respectively put for $\int f\,dr$ and $\int \mathrm{F}dr$. It will now be supposed that the incident waves are defined by the equations $w_1 = \kappa a \sigma_1 = m \sin\frac{2\pi\kappa at}{\lambda}$, w_1 and σ_1 being considered uniform at each instant through the small extent of the influence of the sphere's reaction. Also since by hypothesis the functions f and F are periodic, and must have their period determined by that of the incident waves, it will be further assumed that

$$f = m_1 \sin\frac{2\pi}{\lambda}(r - \kappa at + c_1) \text{ and } \mathrm{F} = m_2 \sin\frac{2\pi}{\lambda}(r + \kappa at + c_2).$$

Now if c be the radius of the sphere, the condition must be satisfied that $\mathrm{U} = 0$, where $r = c$ for all values of θ and at all times. Hence, substituting c for r in the expression for U, we shall have, independently of the time,

$$\frac{2(f_1 - \mathrm{F}_1)}{c^2} - \frac{2(f-\mathrm{F})}{c} + f' - \mathrm{F}' = 0, \quad . \quad . \quad . \quad (\kappa)$$

which equation furnishes two relations between the arbitrary constants m_1, m_2, c_1, c_2.

I shall now introduce a condition which has special reference to the applications proposed to be eventually made of these researches, and by means of which the relations between the unknown constants may be still further determined. It will be supposed that the radius c of the sphere is so extremely small compared to λ, that the distance from the centre at which the sphere has any sensible influence in disturbing the incident waves is very small compared to the same quantity; so that if r_1 represent that distance, the ratios $\frac{c}{r_1}$ and $\frac{r_1}{\lambda}$ are both very small. On this supposition it may be assumed without sensible error that, within the limits of the sphere's influence, the ratio of the condensation at each point to the condensation of the incident waves is a function of coordinates only. This condition, which, in fact, is involved in the assumed values of f and F, is equivalent to supposing that

$$-\frac{f+\mathrm{F}}{r^2}+\frac{f'+\mathrm{F}'}{r}=\frac{m}{\kappa a}\sin\frac{2\pi\kappa a t}{\lambda}\times\text{ a function of } r.$$

By substituting the values of f and F, it will be found that the condition is satisfied if $m_2=-m_1$ and $c_2=c_1$. Hence, eliminating by these equalities m_2 and c_2, and substituting for f and F in the foregoing expression for σ, the result is

$$\frac{\sigma}{\sigma_1}=1+\frac{m_1\kappa a}{m}\left(\frac{2}{r^2}\cos\frac{2\pi}{\lambda}(r+c_1)+\frac{4\pi}{r\lambda}\sin\frac{2\pi}{\lambda}(r+c_1)\right)\cos\theta.$$

Reverting now to the equation (κ), and substituting in it $-m_1$ for m_2, and c_1 for c_2, we obtain for determining c_1 the equation

$$\tan\frac{2\pi}{\lambda}(c+c_1)=-\frac{\lambda}{2\pi c}+\frac{\pi c}{\lambda}.$$

Substituting c for r in the above value of σ, and eliminating c_1 by means of the last equation, the exact result is

$$\frac{\sigma}{\sigma_1}=1-\frac{m_1\kappa a}{m}\cdot\frac{8\pi^3 c\cos\theta}{\lambda^3\left(1+\frac{4\pi^4c^4}{\lambda^4}\right)^{\frac{1}{2}}},$$

which, on account of the small ratio $\frac{c}{\lambda}$, is very nearly

$$\frac{\sigma}{\sigma_1}=1-\frac{m_1}{m}\cdot\frac{8\pi^3\kappa a c\cos\theta}{\lambda^3}.$$

This equation gives the condensation at any point of the *surface* of the sphere due to the condensation of the waves, and therefore additional to that which was before found to be due to the incidence of waves without condensation. It is evident that, since the investigation has been restricted to quantities of the first order, and σ_1 is consequently expressed by a circular function having as many *plus* as *minus* values, the pressure accompanying the condensation σ has no tendency to produce a permanent motion of translation of the sphere. Let us next inquire whether such motion is indicated when terms of the second order are taken into account.

As we have hitherto employed only the first terms of the values of $a\sigma'$ and w', the differential equations we have been concerned with are linear with constant coefficients, and from such equations the coexistence of small vibrations has been inferred. But it is particularly to be observed that the general *composite character* of the vibrations of an elastic fluid is not demonstrated by means of these approximate equations, inasmuch as the proper proof consists in finding antecedently, and without reference to arbitrary conditions, expressions giving the laws of simple vibrations. On this account it is allowable to assert generally that the motion is compounded of motions defined by any number of terms of the series for $a\sigma'$ and w'. Since m is supposed to be very small compared with a, the convergence of each series is very rapid, and the terms following the first are collectively very small compared with the first. Also, as has been already urged, the vibrations, whether great or small, of a given particle through a *mean* position necessarily require that the condensations should be greater than the rarefactions, which accords with what is indicated by the equations (α) and (β). For these reasons I conclude that the motions and condensations represented by the additional terms always accompany those expressed by the first; and that in composite motion each component is in strictness defined by all the terms of the series for $a\sigma'$ and w', excepting so far as modifications are produced by the transverse vibrations.

It is, however, true that after advancing beyond terms of the first order, the reasoning no longer rests exclusively on inferences drawn from linear differential equations with constant coefficients. Yet, as I now proceed to show, certain results, sufficient for the present purpose, may be obtained when terms of the second order are included in the reasoning. First, it is to be noticed that w' is periodic in such manner as to have as many *plus* as *minus* values, whatever number of terms be taken, although this is not the case with respect to the corresponding value of σ'. Also as the composite velocity is by supposition wholly periodic, it, like the components, must have equal amounts of *plus* and *minus* values. To satisfy this condition, it clearly suffices to suppose that the composite velocity is the *sum*, or proportional to the sum, of the individual velocities. Let, therefore, the total motion be composed of an unlimited number of separate vibrations, direct and transverse, having their axes perpendicular to a plane; and let the direct vibrations be expressed by two terms of the series for w', and their phases be such that the transverse vibrations destroy each other. The total motion is consequently in parallel straight lines; and the rate of propagation being κa, the composite velocity (V) and density (ρ) are related to each other by the general formula $V\rho=\kappa a(\rho-1)$, obtained in Part I. (p. 218), which must now be used to terms of the second order. Hence if S represent the composite condensation, we shall have

$$\kappa a S = V + \frac{V^2}{\kappa a},$$

in which, from what is said above, V is either $\Sigma . w'$ or proportional to this sum. From the form of the expression for w', it follows that $(\Sigma . w')^2$ is equal to the sum of the squares of periodic terms together with periodic terms having equal amounts of *plus* and *minus* values. Hence it appears that part of the value of S *is always positive.*

Reverting now to our problem of waves impinging on a sphere at rest, to proceed to the second approximation we have to employ S in the place of σ_1 for the condensation of the waves. But since S differs from σ_1 by a very small quantity, if we call $f(\sigma_1)$ the first approximation to σ, we shall have very nearly for the second approximation,

$$\sigma = f(\sigma_1) + \frac{d.f(\sigma_1)}{d\sigma_1}(S-\sigma_1).$$

Now the value of $f(\sigma_1)$ found above is of the form $\sigma_1(1-m_1Q)$, Q being independent of σ_1. Hence by the above formula $\sigma=S(1-m_1Q)$; that is, to advance to the second approximation, it suffices to substitute S for σ_1 in the value of σ. Consequently we have

$$\sigma - S = -\frac{8\pi^3 m_1 \kappa a c}{m\lambda^3} . S\cos\theta.$$

The total pressure on the sphere tending to produce motion in the direction of the propagation of the incident waves is

$$2\pi\int a^2(\sigma-S)c^2\sin\theta\cos\theta\,d\theta,\ \text{from } \theta=0 \text{ to } \theta=\pi.$$

This quantity will be found to be

$$-\frac{32\pi^4\kappa a^3 c^3}{3\lambda^3} . \frac{Sm_1}{m}.$$

Hence if Δ be the ratio of the density of the sphere to the density of the fluid, the accelerative force due to the action of the fluid on the sphere is

$$-\frac{8\pi^3\kappa a^3}{\lambda^3\Delta}\cdot\frac{\mathrm{S}m_1}{m},$$

which is independent of the magnitude of the sphere. Now since it has been shown that the function S contains, together with periodic terms, others which are always positive, it follows that the sphere, if free to move, would not only vibrate, but also permanently change position by the action of an accelerative force. If the unknown factor m_1 be positive, the acceleration would be *towards* the origin of the waves, and the movement would resemble that produced by an *attractive force.* Supposing that the axes of the component vibrations all pass through a central point, the magnitude of the force due to the *same* composite waves would, at different distances from the centre, be proportional to the number of the axes included within a given transverse area, and consequently would *vary inversely as the square of the distance from the centre.*

It remains to discuss the other part of the particular integral of the equation (η), namely that obtained by supposing that $qr=\phi_2 \sin\theta\cos\theta$. Putting, as before, f for $f(r-\kappa at)$, and F for $\mathrm{F}(r+\kappa at)$, the equation (ι) for determining ϕ_2 admits of the following exact integral,

$$\phi_2=\frac{f+\mathrm{F}}{r^2}-\frac{f'+\mathrm{F}'}{r}+\frac{f''+\mathrm{F}''}{3}$$

(See Peacock's 'Examples,' pp. 469–473. In the course of obtaining this integral, it appeared that f could not be an arbitrary function of $\kappa at-r$.) Retaining both functions, the following results may be obtained by processes analogous to those applied to the first part of the value of qr, viz.

$$\sigma=\sigma_1-\left(\frac{f+\mathrm{F}}{r^3}-\frac{f'+\mathrm{F}'}{r^2}+\frac{f''+\mathrm{F}''}{3r}\right)\frac{\cos^2\theta}{2},$$

$$\frac{\mathrm{W}}{\kappa a}=\left(\frac{f_1-\mathrm{F}_1}{r^4}-\frac{f-\mathrm{F}}{r^3}+\frac{f'-\mathrm{F}'}{3r^2}\right)\sin\theta\cos\theta,$$

$$\frac{\mathrm{U}}{\kappa a}=\left(\frac{3(f_1-\mathrm{F}_1)}{r^4}-\frac{3(f-\mathrm{F})}{r^3}+\frac{4(f'-\mathrm{F}')}{3r^2}-\frac{f''-\mathrm{F}''}{3r}\right)\frac{\cos^2\theta}{2}.$$

These equations show that $\mathrm{U}=0$ and $\mathrm{W}=0$ where $\theta=\frac{\pi}{2}$, for all values of r at all times, and that in the same case σ is equal to the condensation of the incident waves. In fact the excursions of the particles of the fluid must now be regarded as small compared to the dimensions of the sphere, and in this respect the above equations are quite distinct from those given by the first part of the value of qr. In this case, as I have on several occasions argued, the reaction of the hemispherical surface on which the waves are immediately incident is such that the condensation at every point of it is *quam proxime* equal to the condensation (σ_1) of the waves at incidence. The equations apply exclusively to the condensations at points for which the angle θ is greater than $\frac{\pi}{2}$, and to those parts of the velocities at such points which depend on the variations of condensation due to the mutual action of the parts of the fluid. The following process of reasoning is the same, *mutatis mutandis,* as that employed with respect to the former set of equations. In the first place, since $\mathrm{U}=0$ where $r=c$ for all values of θ and at all times, by putting c for r in the general value of U, we have

$$\frac{3(f_1-\mathrm{F}_1)}{c^3}-\frac{3(f-\mathrm{F})}{c^2}+\frac{4(f'-\mathrm{F}')}{3c}-\frac{f''-\mathrm{F}''}{3}=0. \quad . \quad (\lambda)$$

Again, introducing, for the same reasons as before, the condition that the ratio of σ to σ_1 is at each point a function of r and θ, and giving to f, F, and σ_1 like expressions to those in the first case, the ratio of σ to σ_1 will be found to be

$$1+\frac{\kappa a m'_1}{m}\left\{\left(\frac{1}{r^3}-\frac{b^2}{3r}\right)\cos b(r+c'_1)+\frac{b}{r^2}\sin b(r+c'_1)\right\}\cos^2\theta,$$

b being put, for brevity, for $\frac{2\pi}{\lambda}$. To obtain this result, $-m'_1$ was substituted for m'_2, and c'_1 for c'_2. The same substitutions being made in the equation (λ), there results for determining c'_1,

$$\tan b(c+c'_1)=-\frac{1}{bc}\cdot\frac{9-4b^2c^2}{9-b^2c^2}.$$

The condensation at any point of the second hemispherical surface can now be found by putting c for r in the general value σ, and eliminating c'_1 by means of the last equation. These operations give the following exact result:

$$\frac{\sigma}{\sigma_1}=1-\frac{m'_1\kappa a b^5c^2\cos^2\theta}{3m(81+9b^2c^2-2b^4c^4+b^6c^6)^{\frac{1}{2}}}.$$

Since bc is by hypothesis a very small quantity, we have very nearly

$$\frac{\sigma}{\sigma_1}=1-\frac{32\pi^5m'_1\kappa a c^2\cos^2\theta}{27m\lambda^5}.$$

By the same argument as before, in order to include terms of the second order, S must be put in the place of σ_1. Then the total pressure on the second hemispherical surface not counteracted by pressure on the opposite surface, and estimated in the direction of the incidence of the waves, is

$$2\pi\int a^2(\sigma-\mathrm{S})c^2\sin\theta\cos\theta\,d\theta,\text{ from }\theta=\frac{\pi}{2}\text{ to }\theta=\pi.$$

This pressure is found to be

$$\frac{16\pi^6\kappa a^3c^4}{27\lambda^5}\cdot\frac{\mathrm{S}m'_1}{m};$$

and the mass of the sphere being $\frac{4\pi c^3\Delta}{3}$, the accelerative force is

$$\frac{4\pi^5\kappa a^3c}{9\lambda^5\Delta}\cdot\frac{\mathrm{S}m'_1}{m}.$$

By the considerations already applied to the condensation S, it follows that in this case also the waves tend to produce a permanent motion of translation of the sphere, and that the acceleration from a centre varies inversely as the square of the distance. Since the above expression contains c, the acceleration is not independent of the magnitude of the sphere. Also the direction of the motion of translation is *from* the origin of the waves if m'_1 be a positive quantity, and the movement is like that produced by a *repulsive force*.

I have now reached the point to which I proposed to carry these researches in the present communication. It was required to ascertain whether or not the undulations of an elastic medium are capable of causing permanent motions of translation of a small sphere; and the question has been answered affirmatively. The investigation has shown that the effect is due to the circumstance that, on expressing to terms of the second order the relation between the condensation and velocity of a wave, the amount of condensation is found to be greater than that of rarefaction, and that, while the excess of condensation causes the excursions backwards and forwards of a given particle of the *fluid* to be exactly equal, the excursions of a spherical *solid* submitted to the action of the waves are by that very excess made unequal. Although the mathematical process by which this conclusion has been arrived at is intricate, the *rationale* of the result may be understood from common mechanical principles—it being evident that as the solid does not change its density like the fluid, the action upon it of the greater condensation cannot be counteracted by that of the smaller rarefaction.

The foregoing mathematical reasoning is incomplete, inasmuch as it does not determine the composition of the constants m_1 and m'_1, and the relation of the one to the other in a given case of motion. For the present I reserve the consideration of these points. In the meanwhile the reasoning, as far as it has gone, points to the inference that large waves act on a small sphere in the manner of an attractive force by the accumulation of pressure on the further side of the sphere, and that small waves act in the manner of a repulsive force by reason of a defect of pressure on that side. The abstract demonstration of these effects of the vibrations of an elastic fluid, besides being interesting as a matter of pure reasoning, appears to have an extensive physical bearing, as suggesting that the æthereal medium in which the waves of light are known to be generated and transmitted, may be the source from which all attractions and repulsions derive their energy. I consider that in this communication a more secure basis is laid for a hydrodynamical theory of the physical forces than in any previous attempt that I have made. The former researches are vitiated by the adoption of the commonly received law, that the condensation in central motion varies inversely as the distance, which I now consider to be an error. This is more especially the case with respect to the Theory of Gravity proposed in the Philosophical Magazine for December 1859, for which the present researches supply a substitute.

Cambridge, September 11, 1865.

XXXVI. *On Moving Photographic Figures, illustrating some Phenomena of Vision connected with the combination of the Stereoscope and the Phenakistoscope by means of Photography.* By A. Claudet, *F.R.S.*[*]

FROM the beginning of photography it must have struck many of those who were acquainted with the phenomenon illustrated by the phenakistoscope invented by Plateau, that photography could produce with advantage the series of pictures used in that instrument, on account of their possessing a greater degree of accuracy than when made by hand. At a later period, when the stereoscope had become popular from its application to photography, there must have been a still stronger incitement to make use of that process to produce binocular pictures for the phenakistoscope, in order to combine the stereoscopic effect with the illusion of moving figures elicited in the phenakistoscope. For example, if a number of binocular photographic pictures were taken of a machine in various consecutive stages of its motion, these pictures, applied to a phenakistoscope, would give a complete illusion of the machine in perfect relief and in its full action.

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[*] Communicated by the Author, having been read, the 7th of September, 1865, before the British Association at Birmingham.

Binocular pictures of persons dancing, fencing, or boxing, of acrobats at their wonderful feats, of boys playing at different games, all in the various stages of the action of each sport, representing consecutively the whole performance—such pictures might have been supposed to be invaluable as calculated to exhibit the stereoscopic illusion of persons in the real action of life. Therefore the solution of such an interesting problem was capable of exciting the emulation and the ambition of many ingenious and scientific minds.

Among those who undertook the task, M. Duboscq, the eminent optician of Paris, was the one who attained the greatest success. He had fixed the two series of binocular photographs on two zones of the revolving disk of the phenakistoscope, one above the other; and by means of two small mirrors placed each respectively at the inclination capable of reflecting the two zones on the same horizontal line, from thence the images could each separately meet the axis of each of the two prismatic lenses of the stereoscope. In this manner, during the revolution of the disk each eye had separately the perception of one of the series of photographs, each showing the perspective of one eye, and the stereoscopic effect of figures in motion was consequent.

M. Duboscq gave another form to the phenakistoscope. Instead of the vertical original revolving disk of Plateau, he employed a cylinder revolving on its vertical axis; and he placed on two inside zones of that cylinder, one above the other, the two series of photographic pictures, between the slits through which the eyes can see the pictures; and by means of two mirrors, as in the other apparatus, each series was reflected on its respective lens through the cylinder, and the stereoscopic effect was produced in combination with the phenakistoscopic effect.

However, these two attempts of M. Duboscq present a few imperfections, which we are going to explain. In the revolving disk the two series of pictures do not move with the same velocity, on account of their being placed on two zones of different peripheries; and this produces a sort of confusion and distortion in the representation of the object during its movement. The same defect exists in Plateau's phenakistoscope in the perception of its single series; for the top and bottom parts of the figures, owing to the cause explained, revolving with different velocities, are not impressed on the retinæ during the same time; and the blank spaces between the pictures, being larger for the top than for the bottom part, give a stronger sensation of void during the visual perception of the pictures.

In the revolving cylinder this defect does not occur; but the pictures being considerably curved, like the cylinder, is a most unfavourable disposition for examining them in the stereoscope;

and if it were flat, it would not have its various parts revolving in the same peripheries. However, M. Duboscq's contrivances are very ingenious, and in his attempt he has succeeded at all events in proving the possibility of solving the problem.

About that time I had also turned my attention to the subject, and found some difficulty, in obtaining at once the phenakistoscopic and the stereoscopic effects, in avoiding the defects I have alluded to. However, as I am far from considering the case to be quite desperate, I intend to resume my researches as soon as I have leisure to do so. What gives me some hopes of success, is the attempt I made years ago. The result has been incomplete and imperfect; but if I have not succeeded in obtaining at once the motion and relief, I have gone so far as to show figures which appear really to be in motion, and preserving all the correctness and distinctness of the photographs.

My ambition to obtain the stereoscopic effect with the movement of the figures having (I hoped only momentarily) been foiled, and not being satisfied with partial success, I did not like to publish an incomplete attempt, and for this reason, till now, kept it only for the curiosity of a few intimate scientific friends. But years are passing away swiftly; and as I do not feel at my period of life that I can reckon upon endless time and inexhaustible activity to complete many labours, I did not like to let another Meeting of the British Association pass without availing myself of the opportunity of bringing before this annual scientific congress a very simple contrivance which, if I do not later succeed in solving the whole problem, will at all events, perhaps, call the attention of others to the subject, and induce them to try their hands and brains at its solution.

This is one of the many instances of the advantages of the British Association. Once a year all the branches of science of the world are brought together to show the progress made, and point out the progress to be made. All the devoted followers of those sciences consider that they are obliged to contribute their mite, however small it may be, to increase the general interest of the Meeting, and to awaken the desire for further improvements and new discoveries. In this way very often questions are brought forward which otherwise would have been neglected or forgotten.

As will be seen by the instrument I submit to the Meeting, it is very easy to obtain the illusion of moving figures, but without stereoscopic effect. In this instrument, my first object having been only to try the principle, I have constructed it in the simplest form capable of showing the motion of the figure; and I have found that only two pictures are sufficient to elicit the phenomenon, although the illusion of reality suffers from the abruptness of two extreme movements, and from the deficiency of intermediate positions.

But nothing is so easy as to employ eight different pictures in as many different stages of the action, and with this number of pictures the effect will be sufficiently complete. For this, having placed in the stereoscope two separate cubic frames, revolving independently on the same horizontal axis, I have only to fix on their four sides at right angles two sets of four pictures, making eight pictures, which are made to pass in consecutive order one after the other before the lenses of the instrument, and the figure will appear to assume consecutively eight different stages of the whole action. The cubic frames are made to revolve by the motion applied to the slide which, as is to be explained, transfers the sight alternately from one eye to the other. The instrument in its simple state, with only two pictures, will suffice to illustrate the principle, and at the same time to elicit some curious phenomena of the perception of vision.

It is known that the retina has the power of retaining for a short time the impression or the sensation of the image which has struck it. Now, availing myself of this property, I have constructed the instrument in such a manner that, by means of a slide with one hole, I can, by moving it rapidly in a reciprocating horizontal direction, shut one lens while the other remains open; and in continuing that motion, while one eye sees one of the two pictures, the second eye cannot see the other picture.

Now, if before the sensation of one eye is exhausted the slide shuts the lens and opens the other, a new impression is produced on the retina, and we have an uninterrupted sensation of vision, as if the object had moved before us; and if a sufficient number of pictures represent that object in the various consecutive positions it has assumed during several stages of its motion, we experience on the retina the same sensation we have when we see the object itself while it is moving. For it must be observed that although the pictures in their limited number do not, and cannot, show all the intermediate positions of all the stages of a continuing action, still the mind has the power of filling up the deficiency, as it does if, when looking at a real object in motion, we accidentally wink the eyes, or an obstacle happens to pass between us and the object. Although during that short interval we have lost the perception of a certain progress of the action, the mind has, as it were, guessed and represented to itself what ought to have taken place during the winking of the eyes, or during the intervention of the passing obstacle, and by that power of the mind there has been no interruption in the whole perception.

This is exemplified in the most forcible manner when we have only two pictures to look at alternately, one with the right and the other with the left eye, as with the instrument I have constructed for my experiment. One of the pictures represents the beginning of an action, and the other the end of the same action. By moving the slide one way the right eye can see the picture representing the figure in one position, and the picture showing the other position is invisible to the left eye. Now by moving the slide the other way the left eye sees the figure in the second position, and the first position is invisible to the right eye. Now, although we have only seen the figure in two extreme positions, one showing the beginning of the action and the other its end, still we have had the illusion of having seen the intermediate positions.

This is fully illustrated by the pictures representing two boxers. In one picture the arms and fists of one of the pugilists are near his body, as if he were preparing to hit his opponent; and in the other picture they are extended in the act of striking the blow. We have not seen the intermediate positions which the boxer ought to have gradually assumed during the whole act, but we know that they must have taken place, and our mind completes the action. This mental perception is due to the sensation which we expect from habit and judgment, and we feel it as if it had been truly realized.

Another curious phenomenon is elicited by the alternate vision of the two eyes consecutively. We see the object without any difference or interruption, whether it is by the right or by the left eye. The ultimate sensation on the mind is the same from whatever eye it has been carried to the sensorium of vision. Whether the object be seen by the right or by the left eye, the sensation is exactly the same, and we cannot even distinguish which is the eye that has had the perception; so that if, while we have both eyes open, an object is passing before us, or if we move the hand before the eyes in such a manner that it hides alternately the vision of one and of the other eye, we do not feel that the vision is passing from one eye to the other, and it is impossible to know by which eye we have had the perception.

This explains the reason why in the instrument I have constructed, which alternately shows a picture in one position to one eye, and a different picture in another position to the other eye, we have a single perception of an object in motion without being conscious that the two actions have been consecutively and separately perceived only by one eye at a time, and in turn by one and then by the other. The result is an uninterrupted perception of an object in motion.

Our sensation of vision is not in the eyes, but only in the single sensorium of vision, to which both eyes convey their separate perceptions. We have an example of a similar physiological fact in the sense of hearing. Although we receive the sound from two organs in opposite directions, only one perception is felt by the mind; the two sensations, like two drops of water the moment they reach each other, are resolved into one.

If, for such a trifling subject, I have encroached so much on the time of the Meeting, I have to offer an excuse which, I hope, will be kindly accepted. How is it possible to be short when one principle of science irresistibly leads us to another? and how can we stop when we begin to unfold the marvellous and innumerable expedients which the Creator has employed to make our senses perfect and to help our intellect?

XXXVII. *On Molecular Physics.* By Prof. W. A. NORTON*.

[Continued from p. 113.]

THERMO-ELECTRICITY.—The key to thermo-electric phenomena should be found in the effect produced by heat on the electric condition of molecules. Now when heat is applied to a surface, the molecules at the surface first receive the ethereal pulses of which the force of heat consists. These pulses passing on to the central atom of each molecule, or the condensed universal æther at the centre of the molecule, are there partially expended in expanding its electric atmosphere on the other side, and are partially propagated on. Upon reaching the inner side of the atom they will again be partially consumed in expanding the atmosphere on that side, while a certain portion will be transmitted to the next atom. It is easy to see that this second expansion should be less than the first. Under these circumstances two important electrical effects will be produced. (1) By reason of the greater expansion of the atmosphere on the outer than on the inner side, its density will be diminished on the outer side, and hence electric æther will flow around to that side. *The molecular atmosphere will therefore become polarized positively on the outer side.* (2) As a consequence of this polarization, the molecular atmosphere *will urge away from it a portion of the electric æther posited near its surface, and tend to develope a negative polarization in the particles of contiguous surfaces.* The surface receiving the heat will then become positively polarized, and there will be at the same time an electric movement outward from the surface. Cold, or the abstraction of heat, will have precisely the opposite tendency; that is, a surface in the act of cooling will become negatively polarized, and this change will be attended with a flow of electricity toward the surface. Such movements of the electric æther will be in

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* From Silliman's Journal for July 1865.

waves of translation, or in currents of free electricity, or both combined, according to the conducting-power of the medium exterior to the surface (p. 111). Substances may differ in the effects thus produced under similar circumstances, from two causes—a difference in their conducting-power for heat, and a difference in the degree of expansion, or in the effect of the expansion produced by the same amount of heat.

Now let a plate of bismuth be placed in contact with a plate of antimony, and let the junction be heated and the other ends be brought into good conducting communication. If the above-mentioned effects of heat be different from these two metals, a current should set, at the junction, from the one which experiences the greatest effect to that which experiences the least, and pass through the circuit. Bismuth is a poorer conductor of heat than antimony, and in fact than most other metals; and hence its surface-molecules should imbibe and retain more heat than those of the antimony. The diamagnetic properties of bismuth also indicate, as will be seen hereafter, that its molecular atmospheres are remarkably expansible. If these peculiarities of bismuth be admitted, we have an explanation of the fact that bismuth is positive* to other metals in its thermo-electric relations. The bismuth and antimony in the thermo-electric pair, it will be observed, hold the same relation to each other as the zinc and copper in the galvanic pair, and the heat does the same electrical duty in the one that the oxygen does in the other.

If the other ends of the two metals be brought together and cooled, the current will be reinforced, since the molecular atmospheres of the bismuth will contract more than those of the antimony. (See effect of cold, p. 276.)

It has been ascertained as the result of numerous experiments, that "increasing the temperature of the negative metal generally increases the amount of deflection of the galvanometer-needle produced by heating the junction; while if the higher heat is applied to the metal which is positive at moderate temperatures, a current in the opposite direction is established." To get at the explanation of these curious effects, we must observe that the "higher heat" spoken of is applied at a certain distance from the junction, and hence it is the inner sides of the surface-molecules which first receive the heat from this second source, and become positively polarized. Accordingly the current should be strengthened in the first case above mentioned, and weakened in the second. It has also been observed that "a current

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* By the positive metal is here meant that which imparts, at the heated junction, positive electricity to the other metal. This is often termed the negative metal. Its polarization is positive, but its electrical state is negative (p. 104).

may be excited with two wires of the same metal, by heating the end of one and bringing it in contact with the other; and that the direction of the current at the junction is from the cold to the hot wire." In this case it is to be remarked that the hot wire is in the act of cooling; and hence there should be an electric movement toward its surface (p. 276), or from the cold to the hot wire through the junction.

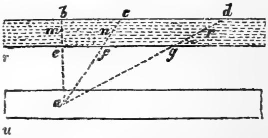
Pyro-electric Crystals.—Tourmaline is the most conspicuous crystal belonging to this class. "A prism of tourmaline has different secondary planes at its two extremities, or, as it is expressed, is hemihedrally modified." This peculiarity of crystallization, which also obtains in the other pyro-electric crystals, indicates that in the direction of the axis the molecules of the crystal have different mechanical properties on opposite sides. In this condition of things we may reasonably suppose that the molecular atmospheres would expand unequally on opposite sides, under the influence of the same amount of heat. If this be admitted, we have a complete explanation of the electric phenomena exhibited by the tourmaline when heated, in accordance with the principles already laid down. Thus let a tourmaline be heated regularly—that is, so that all points of its surface shall receive equal increments of heat; at all points of the surface the unequal expansive action of the heat upon the two sides of the molecular atmospheres in the axial direction will determine their polarization and an attendant electric movement from the positively polarized side of one molecule to the negatively polarized side of the next. There should accordingly be opposite electrical states manifested at the ends of the crystal. This state of things should continue so long as the temperature is rising. But it is to be observed that the effective polarization determined in each molecule by the heat is weakened by the discharge that takes place from one molecule to the next, and that from this cause the signs of electrical excitement at the ends of the crystal will be much feebler when the temperature becomes uniform than they would otherwise be. Now if the heated tourmaline be cooled regularly, the process that attended upon the heating will be reversed, and the electrical states, or effective poles, will be reversed. This reversal of the poles may occur soon after the temperature begins to fall, because of the above-mentioned weakening of the effective polarization, since this remaining polarization, when the temperature becomes uniform, which is really *effective* upon bodies placed near the ends of the tourmaline, may be no greater than the opposite *transient* polarization that may arise from a small loss of temperature.

Mutual Attractions and Repulsions of Electric Currents.—We have seen that in an electric current there is a flow or onward movement of electric æther throughout the entire route of the current. Now the rapidly moving molecules of electric æther must act impulsively against the universal æther in their path; and every such impulse must be propagated indefinitely outward through this æther. The innumerable impulses thus originating in a current are parallel to the direction of the current, and must take effect upon all surrounding molecules in this direction.

They constitute what may be called the *external impulsive force of the current.* The well-known phenomena under immediate consideration, as well as all the other phenomena of the external action of currents to be discussed, are to be ascribed to the operation of this external impulsive force. Let us first take the case of two parallel wires conveying currents in the same direction. The impulses propagated from these wires will tend to weaken the tension of the universal æther lying between the wires. A similar effect, but less in amount, will be produced upon the æther lying on the outer sides of the wires. The æthereal tension should therefore be in excess upon the outer sides of the wires, and hence they should be urged toward each other, or there should be an apparent attraction between the wires conveying the currents. The excess of æthereal tension here alluded to is attributable to the fact that the impulses proceeding from the one wire, in being propagated through the other, are materially reduced in intensity. This effect results mainly from the dispersion produced by the interstitial æther, which is brought into a very disturbed state of density by the swiftly moving atoms of the electric æther in the current. When only one of the wires conveys a current, no attraction or repulsion is observed, because the dispersion just mentioned is wanting.

If the currents be supposed to traverse the wires in opposite directions, then the same operative cause, the external impulsive forces of the currents, will compress the æther between the wires to a greater degree than beyond them, and thus there will be an effective force urging them further apart.

Fig. 8.



Let *a*, fig. 8, be a point of one of the currents from which an impulse is propagated, and *ab*, *ac*, *ad* lines radiating from it and crossing the second wire, *v*, under different degrees of obliquity. The dotted lines drawn between *bd* and *eg* represent the lines of moving electrical atoms. Each of the lines *ab*, *ac*, *ad* will cross the same number of such lines, and therefore impulses propagated along them will encounter the same number of moving atoms, and experience the same proportional diminution. This diminution should be a certain constant fractional part of the impulse that reaches any point of the nearer side *e g* of the wire. The diminution of the propagated impulse in traversing the wire, simply by reason of the increased length of the passage, as the line is more oblique, should also be a constant fractional part of the impulse; since *f c* is the same proportional part of *a f*, or *a n* for each point of the wire. Hence the action of any point *a* of the first wire upon any point *f* of the second should be inversely proportional to the square of the distance; and the entire force of action of one indefinite wire upon another should be inversely proportional to the distance between the two. (See Lamé, *Cours de Physique*, vol. iii. p. 236.)

If the currents cross each other under a certain angle instead of being parallel, it may be seen, by attending to the mutual actions of the separate points of the two currents, that there will be attractions or repulsions according to the relative directions of the currents at the points; and that the entire action will tend to bring the two currents into the same direction, in which the attraction will be a maximum.

External Action of an Electric Current upon bodies in their natural state.—In undertaking to deduce from our fundamental principles the varied phenomena of the action of a current upon bodies in its vicinity, we have to consider that there are *two modes* in which the external impulsive force of a current may act upon such bodies and develope currents,—the one *direct*, and the other *indirect*. (1) The propagated impulses may take effect directly upon the atoms of the electric atmospheres of the molecules, impelling them in the same direction that the primary current is moving, and so tend to generate a current similar to the primary. (2) Or these same æthereal impulses may fall upon the central atoms of the molecules, force up the atmospheres on the side of the atoms upon which they fall, and so develope a current opposite to the original one. We have to consider also that theoretically the external current-force may operate in both these ways, either upon the *simple molecules* which are grouped together into compound molecules, or upon *compound molecules* as a whole. The action upon the constituents of the compound molecules tends to develope currents within the mass of these molecules. Among the variety of especial currents which may thus be excited, we have particularly to note those which may be developed in the surface of each group, and circulate around it from particle to particle. If the direct mode of operation of the primary current predominates, such circular currents are *magnetic*; if the indirect prevail, they are *diamagnetic*. If the two tendencies countervail each other, the substance is in a *neutral* magnetic condition. The neutral magnetic state may also result from the absence of groups of particles, in the substance, within which circular currents can be established.

To the action of the current upon each compound molecule, with its own proper atmosphere (p. 99), as a whole, are to be ascribed the remarkable phenomena of *Induced Currents,* especially so called. Experiment has hitherto failed to detect the existence of any current, from one such molecule to another, or through the mass of a body, produced by the uniform action of an established current. All such induced currents result from either an increase or a decrease in the effective action of the inducing current, and are wholly due to the indirect mode of operation of this current. This fact indicates, in accordance with the views that have been advanced upon electric polarization (p. 100), that the current developes a polarization in the compound molecules of bodies which increases or decreases as the external impulsive action of the current increases or decreases, but remains constant so long as this action continues the same. This polarization is Faraday's "electrotonic state" of bodies.

Magnetism.—The general nature of the magnetic currents, as distinguished from other electric currents, has just been indicated. The existence of such currents in the surface of a compound molecule seems to imply that there is a virtual chain of particles extending circularly around it, which there should be if the number of particles in each group be large. Ampère's researches have completely established that the idea of circular molecular currents is the key to all magnetic phenomena. It suffices therefore, for our present purpose, to show, as has been done, that such currents are legitimately deducible from the fundamental conceptions laid down at the outset, and that the mutually attractive and repulsive actions of currents may also be derived from the same mechanical ideas (p. 278). We adopt, then, definitively, Ampère's theory of the constitution of a magnet.

The immediate cause of the development of magnetic currents is the direct impulsive action of an established current taking effect upon the particles in the surface of compound molecules.

In respect to magnetic properties, we have specially to distinguish soft iron and steel. The cause of their difference of property seems to lie simply in a difference in the conducting-power of the groups of particles into which they are aggregated. If in soft iron these groups are good conductors, the electric æther set in motion should pass freely around them, unattended with any material polarization of the particles, and unresisted by the force that results from such polarization (p. 103). When the exciting cause ceases to operate, there is no force remaining to counteract the resistance of the universal æther to the flow of the electricity and the retarding operation of contiguous opposing currents. But if we suppose that, in the case of steel, the molecular groups are imperfect conductors, the magnetizing force will develope a polarization of the separate molecules; and, as a consequence, the currents may pass chiefly through their atmospheres, as in the case of electric induction (p. 101). In this state of things, when the force is withdrawn, it does not follow that the magnetic currents will entirely pass off; for there would apparently be a force in operation which would tend to retain currents of a certain intensity in circulation. This is the mutual action of the contiguous positive and negative sides of two molecules throughout the chain. It would seem that such a circular chain of particles would not, of necessity, attain to a condition of statical polarization, but might reach a dynamical equilibrium in which the impulsive force, from each discharge brought about by the action of contiguous molecules, should restore the polarization lost by the discharge to the next preceding molecule*.

According to this view, permanent magnetization consists in polarizing the molecules; and it is this induced state which determines and maintains, in opposition to all resistances, currents of a certain intensity. A certain amount of force ("coercive force") and a certain interval of time are expended in developing this polarization. At the same time it is only when this force is exerted with this result that permanent magnetism can exist. Heat, or a blow, or an electric discharge, by destroying the molecular polarization, may demagnetize the bar. It is also to be observed that intense heat may demagnetize a bar, and destroy the susceptibility to magnetism in iron or steel, by breaking up the groups of particles into which the mass is aggregated.

Diamagnetism.—It has already been intimated that the diamagnetic state into which certain substances, as bismuth, phosphorus, antimony, &c., are brought by the action of a powerful magnet, probably consists in the circulation of currents around the compound molecules of the mass in the opposite direction to the inducing current, and that these currents result from the second mode of operation of the external force of the primary current (p. 280). Accordingly, the susceptibility to diamagnetism must arise from a special liability to expansion on the part of the electric atmospheres of the simple molecules, when urged upward by the æthereal pulses that fall upon their central atoms.

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* It is worthy of consideration, whether certain phenomena of luminosity as phosphorescence, heat-lightning, &c., may not have a similar origin, viz. in recurring discharges resulting from a previous molecular polarization, established by an electric discharge through the mass—or from a similar effect produced in a feebler degree by heat or light.

Electro-magnetism.—The essential theory of the development of magnetism by electric currents, and of electro-magnetic phenomena generally, is embraced in what has already been stated (pp. 278–281).

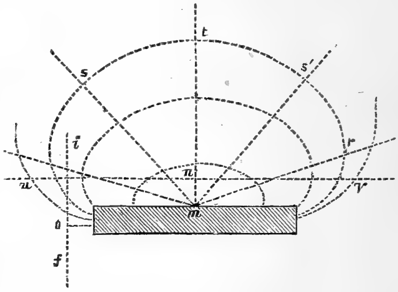
Magneto-electricity.—The excitation of electric currents by magnetic action is a phenomenon of pure induction, and will be included under the next head.

Induction of Electric Currents.—The term induction, as ordinarily used in dynamical electricity, has reference only to the development of currents, through wires, or upon the surfaces, or within the mass of bodies. It will suffice to confine our attention to the origination of currents in wires forming a closed circuit. The general idea of the process, as contemplated from our theoretical point of view, has already been indicated. The fundamental principle which covers the whole ground is, that whenever the action of the impulsive force of any current begins or is on the increase, the induced current sets in the opposite direction to the inducing current; and when the same action terminates or is on the decrease, the induced flows in the same direction as the inducing current. The *rationale* is, that the æthereal pulses of the originating force fall upon the condensed æther at the central portion of each group of molecules, and urge outward the interstitial electric æther on that side of the group. This determines a flow of a certain portion of the electric æther around to that side, and a consequent positive polarization on that side of the group or compound molecule. While this process of polarization is going on, there must then be a flow of electricity from one compound molecule to the next, in the same direction in which the transfer of æther occurs within the molecules themselves, as an inevitable attendant upon the increasing polarization (p. 101). When the primary action becomes constant, there simply abides a state of static polarization, an "electrotonic state," in which an equilibrium prevails throughout the circuit. But when this action falls off, the polarization maintained by it must decline, and a current arises setting in the opposite direction to the former one. The inducing current may be either electric or magnetic, so called. The increase of its action upon the adjacent wire may result either from the closing of the circuit in the case of a galvanic current, or the magnetization of the iron when a magnet is employed; or from the approach of the current to the wire; or from a change in the position of the wire in the magnetic field, attended with an augmentation in the action of the external impulsive force exerted by all the circular currents of the magnet. A decrease in the polarizing effect of the inducing current may result from an interruption of the circuit, or a diminution in the force of the magnet, or a recess of the wire from the current, or a movement of the wire in the magnetic field attended with a diminished action of the entire impulsive force of the magnet.

The oppositely directed currents induced by alternately closing and breaking the galvanic circuit, when the two contiguous wires are formed into a hollow coil, are greatly augmented by inserting rods or bundles of iron wire within the coil. The explanation of this is, doubtless, that the direct action of the galvanic current is reinforced by the magnetic currents which it developes in the iron. The other cases of induction above referred to need not be dwelt upon, with the exception of that resulting from the movement of a wire to different points of the magnetic field.

This case has been subjected to a rigorous experimental analysis by Faraday, who has deduced from his researches the following general results. If a wire, forming part of a closed circuit, be held in a direction transverse to a magnet, and moved in the magnetic field, across the "lines of force," or magnetic curves, a current will be induced in the wire. If the wire be moved in one direction across these lines of force, a current in a certain direction will arise, and if it be moved across them in the other direction, a current in the opposite direction will be induced. The quantity of electricity set in motion will depend upon the number of lines of force crossed by the moving wire, and not upon the obliquity of the direction of the motion to these lines. Thus if the wire be placed over the middle of the magnet (*m*, fig. 9) and moved in any direction from this position across the lines of force as shown in the figure, a current will be induced in it, running in the same direction as the circular currents on the upper side of the magnet; and if the wire be moved across these lines toward the middle of the magnet, a current will traverse the wire in the opposite direction. In each case the comparative quantity of electricity set in motion will depend solely upon the comparative number of magnetic curves crossed, and not upon the line *mr*, *ms*, or *mt*, along which the movement takes place.

Fig. 9.

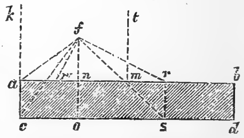


These facts are in entire accordance with the fundamental principle of induction stated on page 283. For (1) the impulsive force of the magnet at any point of the field is the resultant of the special impulsive forces due to all the circular currents of the magnet, and has the same direction as these currents in the upper face of the magnet. (2) This resultant will have the greatest value at the middle of the magnet *m*, and decrease in all directions from this point. (3) The curves of equal impulsive force correspond very closely to Faraday's "lines of force." For it will be seen on a little reflection, that the force will, beyond a certain distance from the middle, if not from the very middle, decrease most rapidly parallel to the surface of the magnet; and that the curves of equal impulsive force, proceeding from points on the ends of the magnet, will recede over the middle of the magnet to a greater distance from it than half its length. This is strikingly true of the curves that originate from near the centre of each end; for at the very centre the force in question vanishes entirely, and therefore the curves for that point would be thrown to an infinite distance from the middle of the magnet.

In making a comparative estimate of the impulsive force of the magnet in different parts of the field, it should be observed that in receding from the magnet the force that results from any one molecular current is the resultant of the opposing impulses propagated from the oppositely directed currents on the nearer and further sides of the molecule, and that this not only depends upon the distance of the point from the molecule, but also upon the obliquity of the line connecting the two, to the plane of the circular current. For the same distance the resultant will be greatest when the point lies in this plane, and zero when the point is opposite the centre of the current. Bearing this in mind, it may readily be seen that if a line *u v* be drawn parallel to the axis of the magnet at any distance from it, the impulsive force of the magnet along this line will decrease from the point *n*, opposite the middle of the magnet, indefinitely in both directions; also that the force will decrease along a line, as *i f*, parallel to the end, and vanish at *o* in the prolongation of the axis.

To obtain a general expression for the intensity of the external impulsive force of the magnet, let *a b c d*, fig. 10, be a magnet, and let us regard its effective action upon any molecule at *f* as the result of the joint action of two sets of opposing currents, the one lying in the upper face *a b*, and the other in the lower face *cd*. Let $nf=y$, $nr=z$, $nr'=z'$, $nb=u$, $na=v$, $ac=d$, and $m=$ coefficient of the impulsive force of an individual current. Then for the action of *r* upon *f* we

Fig. 10.



have $\frac{mdz}{y^2+z^2}$; and for the action of nr,

$$\int \frac{mdz}{y^2+z^2} = \frac{m}{y}\tan^{-1}\frac{z}{y} + \text{C}.$$

For the entire action of nb, we have the definite integral $\frac{m}{y}\tan^{-1}\frac{u}{y}$. In a similar manner we obtain for the opposing action of $o\,d$,

$$\frac{m}{y+d}\tan^{-1}\frac{u}{y+d}.$$

The effective impulsive action of the portion $n\,o\,d\,b$ of the magnet will then be

$$\frac{m}{y}\tan^{-1}\frac{u}{y} - \frac{m}{y+d}\tan^{-1}\frac{u}{y+d}.$$

The effective action of the other portion, $n\,a\,o\,c$, of the magnet will be

$$\frac{m}{y}\tan^{-1}\frac{v}{y} - \frac{m}{y+d}\tan^{-1}\frac{v}{y+d}.$$

We therefore have for the entire action of the magnet

$$w = \frac{m}{y}\left(\tan^{-1}\frac{u}{y} + \tan^{-1}\frac{v}{y}\right) - \frac{m}{y+d}\left(\tan^{-1}\frac{u}{y+d} + \tan^{-1}\frac{v}{y+d}\right),$$

or

$$w = \frac{m}{y}(\text{arc } afb) - \frac{m}{y+d}(\text{arc } cfd). \quad . \quad . \quad \text{(a)}$$

When y is large as compared with d, we have approximately

$$w = \frac{m}{y}(\text{arc } afc + \text{arc } bfd). \quad . \quad . \quad . \quad . \quad \text{(b)}$$

To obtain the equation of the curve of equal impulsive force, let $mn = x$, and $rm = t$. Then $nr = z = x + t$, and

$$nb = u = x + mb = x + a.$$

Also $na = v = a - x$. Hence

$$\frac{m}{y}\left(\tan^{-1}\frac{x+a}{y} + \tan^{-1}\frac{a-x}{y}\right) - \frac{m}{y+d}\left(\tan^{-1}\frac{x+a}{y+d} + \tan^{-1}\frac{a-x}{y-d}\right) = \text{C}. \text{(c)}$$

C here represents the constant intensity of the impulsive force of the magnet for one curve. The value of C decreases as the distance of the curve from m (fig. 9) increases. The equation (b) shows that for the larger curves, except near the magnet, $afc + bfd$ must vary in nearly the same ratio with the ordinate y, from one point to another of the curve. To the left of the line $a\,k$ the arc *a f c* becomes negative in equation (b). Equations (a) and (c) fail for $y=0$.

The investigation here made proceeds upon the supposition that the breadth of the magnet is indefinitely small. If we suppose it to be indefinitely great, the action of each individual transverse current upon any point *f* (fig. 10) would be inversely proportional to the distance of the current from this point*, and it will be readily seen that the amount of force propagated to *f*, within any angle, as *m f' r*, will be the same whatever may be the value of *f n*.

The equation for the value of the effective impulsive force will be approximately of the form

$$w=k\left(\frac{l}{n}+\frac{l'}{n'}\right),$$

k being a constant coefficient dependent upon the strength of the magnet, *l* and *l'* the parts of the length *a b* of the magnet comprised between the angles *a f c* and *b f d* subtended by the two ends, and *n n'* the mean distances of these parts from *f*. We approximate to this state of things in proportion as the magnet is supposed to be broader, and shorter and thinner, or in proportion as, with a magnet of given dimensions, the point *f* is taken nearer to the magnet.

Let us now replace Faraday's lines of force by the curves of equal impulsive force of the magnet, and consider what should be the effect of moving a wire across them, along any line *m r*, *m s*, *m t*, &c. (fig. 9).

It is obvious that if the movement be outward, the impulsive force taking effect upon the wire will decrease; and that if it be inward, the force will increase. Hence, agreeably to the fundamental principle before alluded to (p. 283), in the first case there should be an induced current having the same direction as the currents of the upper face of the magnet, and in the second case a current pursuing the opposite direction. Again, the amount of change of force which results from the displacement of the wire, and therefore the quantity of electricity which this change sets in motion, should depend solely upon the number of curves traversed. We may add that in whatever part of the magnetic field, and in whatever direction the wire be supposed to move, the theoretical result is in perfect accordance with the facts as experimentally established by Faraday.

In the foregoing we have supposed the wire, transverse to the magnet, to be moved parallel to itself to various points of the

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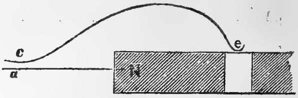
* The individual molecular currents lying in a transverse section of the magnet are here supposed to be replaced by two linear currents transverse to the magnet, one in the upper and the other in the lower surface.

Fig. 11.



magnetic field; but Faraday has shown that a current may also be induced in the wire, by bending it into a curve and causing it to revolve around the magnet, after one end has been brought into contact with the equatorial part of the magnet, and the other with a wire or rod leading out from the pole, as shown in fig. 11. "A copper ring was fixed round and in contact with the equatorial part, and the wire *e* made to bear by spring pressure against this ring, and also against a ring on the axis." The direction of the current changed with the direction of revolution. Corresponding currents were also obtained by rotating the magnet in the opposite directions, the wire remaining fixed. To explain these currents upon the principles now developed, we must first observe that the impulsive force of the magnet will impart a transverse polarization to the molecules of the wire. Now let a motion of revolution be imparted to the wire in a direction opposite to that of the circulation of the magnetic currents, and the relative velocity with which the æthereal impulses will fall upon the molecules will be the sum of the velocity due to the impulse, and that of the molecules themselves in the opposite direction. The molecules at the end *e* of the wire will therefore take on a higher polarization than is induced in the copper ring by the magnet simply. This polarization should be attended with a disturbance of the electric condition of the molecules in the direction of the length of the wire. There should then be an inequality in this disturbance at the point of contact *e*. This inequality should originate a current that would pass around the circuit. Let v denote the velocity answering to the magnetic impulse, and v' the velocity of revolution of the molecule at *e*. Then the effect due to the polarizing force at the end *e* of the wire may be represented by $m(v+v')^2$, and that induced in the contiguous particles of the copper ring by mv^2. The difference is $mv'(2v+v')$, which represents the electromotive force of the current. If the wire were made to revolve around an unmagnetized bar, the originating force of the current would be mv'^2. The electromotive force just mentioned would exceed this nearly in the ratio of $2v$ to v'. A very high velocity of revolution of the wire would therefore be required to develope a sensible current if the bar were unmagnetized. The above expression for the electromotive force, viz. $mv'(2v+v')$, which is nearly equal to $2mvv'$, shows that this force is proportional to the velocity v' of revolution of the wire. The entire force developed in ten revolutions of the wire should then remain the same if the velocity of revolution should be changed (as determined by Faraday). If the magnet rotates in the opposite direction, its impulses against the æther will be correspondingly increased, and the result will be the same.

Faraday, in certain papers originally published in the Philosophical Magazine and the Philosophical Transactions, has indulged in ingenious speculations upon the probable physical character of the lines of magnetic force, and distinctly intimates that he inclines to the opinion that they have in reality a physical existence, correspondent to their analogues the electric lines, instead of being simply "representants of magnetic power," or lines of resultant magnetic action. In speculating upon the question in what this physical existence may consist, he remarks that "it may be a vibration of the hypothetical æther" (along the lines), "or a state or tension of that æther equivalent to either a dynamic or a static condition, or it may be some other state." The results arrived at in the present paper are opposed to these speculative ideas of the great English physicist, for our conclusions are that the lines upon which the phenomena of induction by a magnet depend are merely lines of equal magnetic action; but the action is that of a force whose existence has not heretofore been recognized, viz. the so-called impulsive force of the magnet.

[To be continued.]

XXXVIII. *On* Newton's *Rule for Imaginary Roots. By* J. R. Young, *formerly Professor of Mathematics in Belfast College*[*].

IT is not the object of the investigation at page 114 of this Journal to determine the exact number of imaginary roots in a given numerical equation: Newton's Rule does not undertake so comprehensive an office. All that is aimed at in the investigation referred to, is the discovery of the number of independent imaginary pairs; all of which have this peculiarity, namely, they necessitate the entrance of imaginary roots, fulfilling the first or second of the conditions at p. 114, into the limiting cubics at p. 115.

As a distinctive epithet, we may call those imaginaries in the primitive equation which have this peculiar character *primary pairs*; and the object is to ascertain how many of *such* pairs enter the equation.

The desired information, as appears from what has just been said, is to be derived exclusively from the cubic equations alluded to: these are each to be submitted to the first and second of the tests for imaginary roots given at p. 114, the first test being employed for the leading three terms of each cubic, and the

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[*] Communicated by the Author.

second test for the final three. Since, however, the value of n is 3 for every cubic equation, it follows that in the conditions referred to, $2n$ is the same as $3(n-1)$, and $(n-1)$ the same as $2(n-2)$; so that, A'_{k+1} being put for the middle one of either of the three consecutive coefficients in any cubic, only one condition need be appealed to in reference to the character of the roots of that cubic, namely, the condition

$$3A'_k A'_{k+2} > A'^2_{k+1}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

Now in prosecuting the search after primary pairs, we proceed (mentally at least) as follows:—Taking the three leading coefficients of the proposed equation, we form from them the first three terms of the commencing cubic at p. 115. From this incomplete cubic we determine, by an appeal to the condition (1), whether or not a primary pair of imaginaries enters the proposed equation *solely as a consequence of the relative values of the first three coefficients of that equation.*

If a pair be seen to enter, a pair would still enter, alter the subsequent coefficients as we may; but if the terms of the incomplete cubic do not satisfy the condition (1), then a primary pair cannot enter the primitive equation solely as a consequence of the relative values of these three coefficients.

We now pass on to the fourth of the given coefficients, by the introduction of which we complete the cubic, and then examine its last three terms. If these satisfy (1), we infer one primary pair, and one pair only, whether the condition for the first three terms hold or fail. In the former case there is a double indication of the same thing; in the latter case but a single indication.

Whatever be the character of the final three terms in reference to the condition (1), I have proved that that character will of necessity be transmitted to the leading three of the cubic next following: these therefore need not be submitted to the test (1); and the second cubic being completed by aid of the fifth coefficient of the proposed equation, we then examine, as before, the final three terms. And in this way we proceed, from cubic to cubic, till the last of the given coefficients is brought down.

I have demonstrated that in this series of cubics consecutive fulfilments of the condition (1) merely repeat the information conveyed by the first fulfilment: they concur in testifying to one and the same thing, namely, the *necessary* entrance of but a single pair of imaginaries into the proposed equation. For aught we know to the contrary, other pairs *may* enter, and enter, too, like the primary pair, quite independently of subsequent coefficients; but we can pronounce with certainty only as to the number of *primary* pairs that enter.

If after a fulfilment of the condition (1), or after a series of fulfilments, there occur a failure, that failure must take place in the *final* triad of the cubic into which it first enters. Should this be the terminal cubic, the examination ends, and nothing can be inferred beyond what was previously known. But if other cubics still remain, and the failure or succession of failures be followed by a fulfilment, this fulfilment must occur in the *final* triad of the cubic where it first appears. And *it must indicate a new and independent primary pair.*

This last conclusion I regard as an axiomatic truth—as a principle of which, though no special proof is given, no special proof is needed by a mind clearly apprehending the antecedent reasoning. To such a mind the following obvious considerations can scarcely fail to suggest themselves.

1. The condition (1) is applied exclusively to the triads of the successive *cubics*, and not to those of the primitive equation at all. Whatever knowledge we gain of the character of the roots of this primitive, we derive from the character of these triads as tested, set after set, by the condition (1), our inferences, however, being always controlled by the fact that each triad consists of three terms of a *cubic* equation, so that the two triads furnished by every such cubic can never imply more than one imaginary pair. And however many of the successive triads fulfil the condition (1) of primary pairs, sufficient reason is given *why* the inference of multiple pairs is forbidden: every final triad transmits its character, in reference to the condition (1), to the leading triad of the next cubic.

Since a fulfilment after a failure (as also a failure after a fulfilment) always takes place in a final triad, it is plain that such a fulfilment is entirely owing to the coefficient last brought down from the primitive, to complete the cubic, being of a certain suitable value. By simply altering the value of this coefficient, we could expel the imaginary pair from the cubic, replacing it not only by a real, but even by an equal pair, all the other coefficients remaining untouched. And consequently, by this change of a single coefficient of the primitive equation, a pair of primary roots would disappear. In a cubic not so constituted, that is, in one not consisting of a failure followed by a fulfilment, such conversion of imaginary roots into real could never be brought about by any alteration in the coefficient brought down from the primitive to complete that cubic.

Suppose that from a preceding cubic one primary pair I_1 in the primitive has been detected, and let the primary pair now indicated be denoted by I_2. This latter primary pair is wholly dependent for its existence, as such, upon an advanced coefficient of the primitive equation, of which coefficient the primary pair I_1 is wholly independent. It is impossible, therefore, that I_1 and I_2 can be one and the same pair. And I submit that, to a careful reader of my demonstration, all this must be abundantly apparent.

2. If, however, from having but an infirm grasp of the subject, anyone should demur to the above conclusion, or to that marked 3 at page 116 of my former paper, let him further consider that if only the three leading terms of an equation of the nth degree be written down, that equation will necessarily have two significant roots, the following coefficients being merely zeros; that the introduction of another significant term will cause the entrance of another significant root, of two significant terms, *two* more significant roots, and so on; that is, that for $k+1$ terms there must be k significant roots in the equation, and that, too, how many soever of the terms between the first and last (these themselves being significant terms) are replaced by zeros. But if the pairs I_1, I_2, noted above, could be but one and the same pair, then for $k+1$ terms there would be fewer than k significant roots, which is impossible.

I here take my leave of this subject—a subject on which quite enough has, I think, now been said, and about which a fuss, certainly *more* than enough, has been made. Professor Sylvester's demonstration will very likely be preferable to mine —preferable on the score of brevity, or of elementary simplicity, or of lucid exposition. But whatever may be the merit of his "discovery" in these respects, the merit (such as it is) of priority belongs to me.

Note.—In Professor Sylvester's communication in the last Number of this Journal, there are two mistakes which ought to be corrected.

(1) It is said that I invited Professor Sylvester to express an opinion on my supposed demonstration. I did nothing of the kind.

(2) Professor Sylvester says, "I caused to be forwarded to Professor Young an invitation to attend my lecture." The only invitation that reached me was through 'The Times' newspaper, on the afternoon of the day on which the lecture was to be delivered. The former invitation was never given, the latter was never received *.

I crave permission to add to this paper two theorems, not generally known, which may be found useful for determining the character of the roots of equations of the third and fourth degrees; they were, I believe, first investigated in my 'Course of Mathematics,' and are as follows:—

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* I have been informed, however, that a copy of the Syllabus of the lecture was left for me at the office of this Journal.

I. Let $x^3+px+q=0$ be any cubic equation when deprived of its second term; then whatever be the signs of p and q, the following are sure and complete tests of the character of the roots:

1. If $\left(-\frac{p}{3}\right)^3 > \left(\frac{q}{2}\right)^2$, the roots are all real and unequal.

2. If $\left(-\frac{p}{3}\right)^3 < \left(\frac{q}{2}\right)^2$, two of the roots are imaginary.

3. If $\left(-\frac{p}{3}\right)^3 = \left(\frac{q}{2}\right)^2$, the roots are real, and two are equal.

II. Let $A_4x^4+A_3x^3+A_2x^2+A_1x+N=0$ be any equation of the fourth degree, with its last term positive; then all its roots will be imaginary if the coefficients satisfy the condition

$$(4A_4A_2-A_3^2)N>A_4A_1^2;$$

which condition becomes considerably simplified when $A_3=0$, and $A_4=1$.

September 4, 1865.

XXXIX. *On some Products derived from Indigo-blue.*
By Edward Schunck, *Ph.D., F.R.S.*[*]

My experiments on the formation of indigo-blue, an account of which I had the honour of presenting to this Society several years ago, led me to make some inquiries regarding the processes employed in tropical countries for the production of indigo from the various plants yielding that dye-stuff. I found that all the authors who have written on the subject agree in affirming that the process of fermentation, which is the one usually adopted for the purpose of extracting the colour from the plant, requires to be conducted with the greatest care, in order to yield a successful result. Unless certain precautions are adopted, a product of very inferior quality will be obtained; in some cases, indeed, the colouring-matter is entirely lost. This will not be surprising to any one who considers that though indigo-blue, when once formed, is a very stable compound, the substance existing in the cells of the plant from which it originates, and which I have named *indican*, is decomposed with the greatest facility in various ways, that indigo-blue is only one of its products of decomposition, and may be formed or not, according to the nature of the process to which it is submitted. With this sufficiently obvious expla-

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[*] From the Memoirs of the Literary and Philosophical Society of Manchester, S. 3. vol. iii. Session 1864–65.

nation I should have been inclined to rest contented, had I not acquired a knowledge of some other facts relating to indigo-blue, to which the same explanation cannot be applied, but which evidently belong to the same class.

It is well known to those dyers who employ the so-called woad-vat, in which the reduction of the indigo-blue is effected by the action of various organic matters, such as woad, madder, and bran, together with lime, that if the process be not carefully managed it may change its character entirely, the contents of the vat entering into a state of complete putrefaction—a change which results in the total destruction, or at least disappearance, of the colouring-matter. Now this phenomenon, the reality of which cannot be doubted, though its nature has never been subjected to scientific scrutiny, cannot be explained in accordance with what is at present known regarding indigo-blue, which is considered by chemists to be a body of such a stable character as not to be decomposed by any except very potent agents, such as chlorine, bromine, and nitric acid. In no work on scientific chemistry is it stated that indigo-blue may be decomposed by any process of fermentation or putrefaction, in the same way as sugar or albumen.

In my experiments on indigo-blue I have generally employed for its reduction and purification the process of Fritzsche, which consists in acting on it with a mixture of alcohol, grape-sugar, and caustic soda. The colouring-matter dissolves when the mixture is heated, and is again deposited on exposure to the atmosphere, in crystalline needles. Now in performing this operation with very small quantities of indigo-blue and an excess of alcohol and grape-sugar, I found that the colouring-matter did not make its appearance again on agitating the solution with air. The yellow colour of the liquid passed as usual through red to green; but, instead of the indigo-blue being precipitated, the whole became yellow or brownish-yellow, and the colouring matter disappeared entirely. In this way I had the mortification of losing a quantity of indigo-blue which I had prepared with much labour from human urine, though the loss resulted, as it afterwards turned out, in some gain of information.

This fact was also difficult to account for, since it is usually supposed that by the combined action of reducing-agents and alkalies indigo-blue merely takes up an atom of hydrogen and then dissolves, and, by the action of the atmospheric oxygen is again precipitated, unchanged and undiminished in quantity.

In order to ascertain on what the disappearance of the colouring-matter in this case depends, I first dissolved a small quantity of indigo-blue by means of grape-sugar and caustic soda, using water as a solvent instead of alcohol; but though the indigo-blue was kept for a long time in solution, and heat was applied at the same time to assist the action, it made its appearance again on exposure to the air, apparently undiminished in quantity. In another experiment, in which alcohol was used as the menstruum and protoxide of tin as the reducing-agent, the same result was arrived at. It was therefore apparent that the disappearance of the colouring-matter was due to the combined action of the alcohol and the grape-sugar, not to the separate action of either. By the use of a great excess of these two agents, together with caustic soda and the long-continued application of heat to the solution, I succeeded in causing several grammes of indigo-blue to disappear entirely. I avoid the word *decompose*, because, as I shall show, the colouring-matter is not decomposed, but enters into new forms of combination.

It now occurred to me that, since by the action of caustic alkalies on sugar acetic and formic acids are formed, the effect produced by the grape-sugar in this process might in reality be due to the presence of one or both of these acids rather than to that of the sugar itself. My supposition was completely verified by experiment. On treating some pure indigo-blue with alcohol, to which an alkaline solution of protoxide of tin was added until it dissolved, then adding acetate of soda and digesting at a moderate heat, the indigo-blue after some time ceased to be deposited on exposure to the air, or even agitation; it had entirely disappeared. The same thing occurred when formiate of soda was employed in the place of acetate. It was evident, therefore, that in this process acetic or formic acid was capable of playing the same part as grape-sugar; and as the use of the latter might have tended to introduce complications, in consequence of the formation of secondary products, I ceased to employ it in my subsequent experiments. The object of the present communication is to give an account of the combined action of alcohol, acetate of soda, and caustic alkali on indigo-blue, and the products thereby formed.

At the commencement of the investigation I imagined that it was an essential condition that the indigo-blue should be in a state of solution; but I soon found that this was not necessary. The operation succeeds equally well if indigo-blue freshly precipitated or in fine powder be employed. The plan which I adopted was quite simple. Pure indigo-blue was introduced into a large quantity of ordinary spirits of wine, and, after being well agitated, the mixture was raised to the boiling-point. A quantity of pure acetate of soda, previously deprived of its water of crystallization, and a little solid caustic soda were then added, and the boiling was continued for several hours. A reduction of a portion of the indigo-blue took place in the first instance, as was evident from the deep red colour of the liquid. On agitating with air, this red colour disappeared for a moment, the indigo-blue being precipitated in powder, to be again dissolved on boiling the liquid; but after some time the liquid acquired a dark-brown colour, and deposited nothing on exposure or agitation. The process was then completed. There sometimes remained a residue of indigo-blue, which obstinately resisted the action of the boiling liquid; but, on pouring off the latter, and adding fresh materials, it generally disappeared rapidly. I found it advisable to employ only a small quantity of indigo-blue at a time, as the process is a slow one and requires a great excess of alcohol and acetate of soda. The presence of caustic alkali I found to be quite essential, as no perceptible action took place without it; but the quantity required was not large. The stronger the alcohol, and, generally speaking, the freer from water all the substances employed were, the more rapidly was the process completed.

In order to obtain the products resulting from this process, I proceeded as follows:—The dark-brown alcoholic liquid containing them was first mixed with sulphuric acid until it had acquired a slightly acid reaction, and it was then evaporated. During evaporation, brown resinous masses were deposited; and on adding water when the evaporation was nearly completed, a fresh quantity of resin-like matter was thrown down. The liquid filtered from this matter was still brown. It was evaporated to a syrup, which, after standing some time, became solid from the formation of crystals, consisting chiefly of acetate of soda. The whole mass of crystals was then dissolved in boiling alcohol, and tolerably strong sulphuric acid was added to the solution, until no more sulphate of soda was precipitated, care being taken to avoid an excess of the acid. The liquid, after standing some time, was filtered and evaporated, so as to drive off the acetic acid as well as the alcohol. When the evaporation was nearly completed, water was added, which threw down a large quantity of a brown pulverulent substance, as well as a little brown resin, which, after filtration, were added to the resinous matter previously obtained. The filtered liquid had lost much of its brown colour. I shall return to it presently.

The products insoluble in water obtained in this manner consist partly of resinous, partly of pulverulent substances.

Among these products there are at least five distinct substances, which I have succeeded in separating from one another by the use of various solvents; but it is probable that small quantities of other substances closely resembling them are also formed at the same time. These bodies are all unfortunately amorphous, and possess very few characteristic properties. It is indeed only their origin and mode of formation which impart to them any interest; and I shall therefore refrain from adding to the already cumbrous mass of terms with which organic chemistry has to deal by inventing names for them, but shall simply distinguish them by the letters of the alphabet.

The process adopted for the separation of these substances from one another was as follows:—The whole of the mass insoluble in water was first treated with boiling water in order to remove all the sulphate and acetate of soda. It was then dried, finely pounded, and treated with successive doses of ether, as long as anything dissolved. The ethereal liquid, which had a rich reddish-brown colour, was filtered and evaporated, when it left a resin-like residue of the same colour. This residue was digested with weak caustic ammonia, which dissolved a great portion of it. The portion insoluble in ammonia was filtered off, washed, dried, and then treated with ether, which generally left a small quantity of brown powder undissolved. The filtered ethereal solution was evaporated, and the residue was dissolved in cold alcohol, which left behind a little resinous matter. The filtered liquid left on evaporation a brittle, brownish-yellow resin, which I assume to be an unmixed substance, and shall distinguish by the letter A. The matter dissolved by the ammonia was precipitated by acid in thick flocks, which, after being filtered off, washed, and dried, were treated with ether. The ether left some brown powder undissolved, which was separated by filtration. The liquid was evaporated, and the residue was treated again with ether, in order to separate a little more of the brown powder. The substance was then introduced into a hot solution of carbonate of ammonia, which, if not too concentrated, dissolved the greatest part of it, leaving only some brown powder behind. If, as sometimes happened, the solution of carbonate of ammonia was not sufficiently dilute, very little was dissolved by it, the greatest part of the substance sinking to the bottom of the vessel as a viscid resinous mass, which dissolved, however, almost entirely on pouring off the liquid and adding pure water. The addition of acid to the filtered solution produced a brown flocculent precipitate, which was filtered off, washed with water, and treated with cold alcohol. The filtered alcoholic solution left, on evaporation, a resinous body hardly to be distinguished in appearance from the preceding, and which I will denote by the letter B.

The matter insoluble in ether, constituting by far the larger part of the whole mass, was first treated with a little cold alcohol, to which it communicated a dark-brown colour. The filtered alcoholic liquid left, on evaporation, a brown resinous residue, which was not further examined, since it was sure to contain some of that well-known product of decomposition which is formed by the action of caustic fixed alkalies on alcohol, and which, being also resinous, I saw no prospect of being able to separate from any product derived from indigo-blue which might be mixed with it. The portion left undissolved by the cold alcohol was, after being dried, a brown powder, which consisted of three substances. In order to separate these from one another, the mixture was first subjected to the action of boiling dilute caustic soda-lye, in which one of the three was found to be insoluble. The alkaline liquid, which was of a dark-brown colour, was filtered, and the residue left undissolved was again treated with alkali in order to remove the whole of the soluble portion, and it was then treated with a boiling alcoholic solution of caustic soda, in which the greatest part dissolved with ease. The dark-brown solution was filtered and then mixed with an excess of hydrochloric acid, which precipitated the greatest part of the substance as a dark-brown powder. This was collected on a filter, washed with alcohol until all the acid and chloride of sodium were removed, and dried. This body I will distinguish by the letter C. The caustic soda-lye contained the two other substances in solution; and it was accordingly mixed with an excess of acid, which produced an abundant brown flocculent precipitate. This was collected on a filter, well washed with water, and then treated with a boiling solution of acetate of soda, which dissolved part of it, thereby acquiring a brown colour. The liquid was filtered boiling hot, and the residue was treated with fresh solution of acetate of soda, the process being repeated as long as the boiling liquid acquired any colour. The residue left undissolved by the acetate of soda was treated with boiling alcohol containing a little ammonia, in which it dissolved with ease, forming a dark-brown solution, from which the greatest part was again precipitated on the addition of an excess of hydrochloric acid as a brown powder. This was filtered off, well washed with alcohol, and dried. This body may be denoted by the letter D. The substance held in solution by the acetate of soda was precipitated by sulphuric acid in brown flocks, which were filtered off, well washed with water, and then treated with boiling alcohol, in which they dissolved completely. The alcoholic solution deposited, on cooling, a brown powder, which was collected on a filter, washed with a little cold alcohol, and dried. To this product I apply, for the sake of distinction, the letter E.

The acid liquid filtered from the mixture of substances insoluble in water still contained in solution a product of decomposition derived from the indigo-blue. It was evaporated until crystals began to appear on its surface, and it was then set aside and allowed to stand for some time, when a large quantity of crystals was gradually deposited. After separation from the mother-liquor, these crystals appeared of a brown colour; but by recrystallization from boiling water and decolorization with animal charcoal, they were rendered white and pure. They were then found to have the properties and composition of anthranilic acid, the well-known product formed by the action of caustic alkalies on indigo-blue. The mother-liquor of the crystals left, on evaporation, a thick brown syrup, which seemed to be a compound of anthranilic acid and acetic acid. On dissolving it in water, adding sulphuric acid to the solution and evaporating, I obtained a quantity of crystals, which were purified by crystallization, first from water and then from boiling alcohol. They differed in appearance from anthranilic acid, and consisted indeed of a compound of the latter with sulphuric acid. The same compound is obtained in place of uncombined anthranilic acid, if a great excess of sulphuric acid beyond what is required to unite with the free soda and that combined with acetic acid and the various products yielded by the process has been employed in the first instance. The sulphate, being more soluble in water than the free acid, does not crystallize so easily from the brown syrup which the liquid always leaves on evaporation; and hence it is advisable not to use an excess of sulphuric acid in the process above described for the separation of the anthranilic acid.

As regards their properties, the products insoluble in water present very little that is of interest. The body A is a brittle, amorphous, brownish-yellow resin, transparent in thin layers. At a temperature of 100° C. it becomes soft and semiliquid. When heated on platinum foil, it burns with a bright flame, leaving much charcoal, which, on being heated, disappears without leaving any ash. It is decomposed by boiling nitric acid, yielding a product of decomposition in crystalline needles. It is quite insoluble in alkaline liquids, such as caustic potash, soda, and ammonia, even when a reducing-agent, such as protoxide of tin, is added; but it is decomposed on being heated with dry soda-lime, giving off alkaline fumes having a peculiar penetrating odour. The body B can hardly be distinguished by its external appearance from A, with which it has also many properties in common; but it is easily soluble in caustic and carbonated alkalies, yielding yellow solutions, from which it is precipitated by acids in brown flocks. The compounds with baryta, lime, lead, silver, and copper prepared by double decomposition are brown or yellow, and insoluble in water. When treated with boiling nitric acid it behaves like A, yielding also a product of decomposition crystallizing in needles. The body C is a brown powder, which, on being heated, burns without previously melting; it is insoluble, like A, in watery solutions of alkalies, and very little soluble in alcohol alone, but easily soluble in an alcoholic solution of soda. D resembles C in most of its properties, but differs from it by its solubility in caustic and carbonated alkalies. E is a reddish-brown powder, soluble in alkalies, and more easily soluble in alcohol than C and D, but distinguished from the others chiefly by its solubility in acetate of soda.

The composition, however, of these bodies is a matter of some interest, since it is only from a knowledge of their composition that any light can be thrown on the nature of this curious process. I shall therefore proceed to give a short account of the results yielded by the analysis of these products, which will lead to a few remarks regarding their mode of formation and probable constitution.

A.

Of this body I made two series of analyses, the specimens being prepared on different occasions. Unfortunately the results to which they led did not harmonize, though no difference could be detected in the external properties of the two specimens.

I. 0·3275 grm. dried at 100° C., and burnt with oxide of copper and oxygen, gave 0·9135 grm. carbonic acid and 0·2360 grm. water.

0·5390 grm., burnt with soda-lime, gave 0·2470 grm. chloride of platinum and ammonium.

II. 0·3290 grm. of the same gave 0·9165 grm. carbonic acid and 0·2335 grm. water.

These numbers lead to the formula $C_{62}H_{39}NO_8$, which requires

	Calculation.		Experiment. I.	Experiment. II.
C_{62} …………	372	76·07	76·07	75·97
H_{39} …………	39	7·97	8·00	7·88
N …………	14	2·86	2·87	
O_8 …………	64	13·10	13·06	
	489	100·00	100·00	

In order to explain the formation of a body of this composition in this process, it is necessary to assume that 1 atom of indigo-blue has combined with 8 atoms of alcohol, 3 ats. of acetic acid, and 2 ats. of carbonic acid, the whole losing 26 ats. of water, and forming 1 at. of the substance, since

$$C_{62}H_{39}NO_8+26\,HO=\overset{\text{Indigo-blue.}}{C_{16}H_5NO_2}+8\,\overset{\text{Alcohol.}}{(C_4H_6O_2)}+3\,\overset{\text{Acetic acid.}}{(C_4H_4O_4)}+2CO_2.$$

On the next occasion, though the method of preparation was exactly the same as that above described, the analysis of the substance led to different results, as the following details will show :—

I. 0·3810 grm. gave 1·0600 grm. carbonic acid and 0·2385 grm. water.

0·7280 grm. gave 0·5000 grm. chloride of platinum and ammonium.

II. 0·4015 grm. gave 1·1200 grm. carbonic acid and 0·2500 grm. water.

0·6235 grm. gave 0·4265 grm. chloride of platinum and ammonium.

These numbers lead to the formula $C_{80}H_{44}N_2O_{10}$, which requires

	Calculation.		Experiment. I.	Experiment. II.
C_{80} …………	480	75·94	75·87	76·07
H_{44} …………	44	6·96	6·95	6·91
N_2 …………	28	4·43	4·31	4·29
O_{10} …………	80	12·67	12·87	12·73
	632	100·00	100·00	100·00

Though this formula differs widely from the first, it presupposes a similar mode of formation for the substance, the only difference consisting in the relative quantities of the elements uniting to produce it, as will be seen from the following equation :—

$$C_{80}H_{44}N_2O_{10}+28\,HO=2\overset{\text{Indigo-blue.}}{(C_{16}H_5NO_2)}+9\overset{\text{Alcohol.}}{(C_4H_6O_2)}+2\overset{\text{Acetic acid.}}{(C_4H_4O_4)}+4CO_2.$$

It appears, therefore, that in both cases its formation was due to the union of indigo-blue with alcohol, acetic acid, and carbonic acid, accompanied by the loss of a certain proportion of water. The alcohol and acetic acid are ingredients employed in the process; but it is not easy to see whence the carbonic acid is derived. I think, however, it may originate in the formation of anthranilic acid. This acid, as is well known, is produced by the action of caustic alkalies on indigo-blue, which, taking up water and oxygen, yields anthranilic acid and carbonic acid, in accordance with the following equation:—

$$\overset{\text{Indigo-blue.}}{C_{16}H_5NO_2}+2HO+4O=\overset{\text{Anthranilic acid.}}{C_{14}H_7NO_4}+2CO_2.$$

The oxygen in this case must be derived from water, the hydrogen of which, instead of being set at liberty, probably unites with a portion of the indigo-blue, forming reduced indigo, which dissolves in the caustic alkali. Hence the partial reduction and solution of the indigo-blue, which, as mentioned above, is observed at the commencement of the process. The carbonic acid does not, as might naturally be supposed, combine with the alkali, but unites *in statu nascenti* with alcohol, acetic acid, and a portion of the indigo-blue to form the body A. That it should do so in the presence of an excess of alkali is not more surprising than that acetic acid should, under the same circumstances, leave the base with which it is combined in order to form a perfectly neutral body —a fact of which there can be no doubt.

B.

Of this body also two series of analyses were made, the material being obtained at the same time as that of the two series of A. The first series yielded the following results:—

I. 0·3315 grm. gave 0·8925 grm. carbonic acid and 0·2465 grm. water.

0·5270 grm. gave 0·2645 grm. chloride of platinum and ammonium.

II. 0·3330 grm. gave 0·9015 grm. carbonic acid and 0·2445 grm. water.

0·5420 grm. gave 0·3190 grm. chloride of platinum and ammonium.

III. 0·3390 grm. gave 0·9195 grm. carbonic acid and 0·2530 grm. water.

0·5230 grm. gave 0·2625 grm. chloride of platinum and ammonium.

These numbers correspond with the formula $C_{52}H_{35}NO_8$, which requires

			Experiment.		
	Calculation.		I.	II.	III.
C_{52}	312	73·41	73·42	73·83	73·97
H_{35}	35	8·23	8·26	8·15	8·29
N	14	3·29	3·15	3·69	3·15
O_8	64	15·07	15·17	14·33	14·59
	425	100·00	100·00	100·00	100·00

This body is therefore formed by the union of 1 at. of indigo-blue, 6 ats. of alcohol, and 3 ats. of acetic acid, 18 ats. of water being eliminated, as will be seen by the following equation :—

$$C_{52}H_{35}NO_8+18HO=\overset{\text{Indigo-blue.}}{C_{16}H_5NO_2}+\overset{\text{Alcohol.}}{6(C_4H_6O_2)}+\overset{\text{Acetic acid.}}{3(C_4H_4O_4)}.$$

The second series of analyses made of this body gave the following results :—

I. 0·4420 grm. gave 1·0810 grm. carbonic acid and 0·2515 grm. water.

0·6550 grm. gave 0·3865 grm. chloride of platinum and ammonium.

II. 0·4610 grm. gave 1·1820 grm. carbonic acid and 0·2745 grm. water.

0·5085 grm. gave 0·3325 grm. chloride of platinum and ammonium.

These numbers lead to the formula $C_{40}H_{23}NO_8$, which requires

	Calculation.		Experiment. I.	Experiment. II.
C_{40} ……………	240	70·38	69·86	69·92
H_{23}……………	23	6·74	6·62	6·61
N ……………	14	4·10	3·70	4·10
O_8 ……………	64	18·78	19·82	19·37
	341	100·00	100·00	100·00

In this case the composition is to be explained by supposing that 3 ats. of alcohol and 3 ats. of acetic acid have combined with 1 at. of indigo-blue to form 1 at. of the substance, since

$$C_{40}H_{23}NO_8+12HO=\overset{\text{Indigo-blue.}}{C_{16}H_5NO_2}+\overset{\text{Alcohol.}}{3(C_4H_6O_2)}+\overset{\text{Acetic acid.}}{3(C_4H_4O_4)}.$$

It appears, therefore, that in the case of this body, as in that of A, the composition may vary extremely, without any corresponding difference in external appearance and properties. The difference in composition in both cases is owing to the different proportion between the elements—indigo-blue, alcohol, acetic acid, and carbonic acid—of which they are composed. The two formulæ to which the analyses of B led, viz. $C_{52}H_{35}NO_8$ and $C_{40}H_{23}NO_8$, differ from one another by a multiple of CH, and they therefore represent homologous bodies.

C.

This body is formed in relatively small quantities, and I only obtained sufficient for one analysis, which yielded the following results :—

0·3600 grm. gave 0·9790 grm. carbonic acid and 0·1605 grm. water.

0·5435 grm. gave 0·5175 grm. chloride of platinum and ammonium.

Hence it is to be inferred that the formula is $C_{28}H_{11}NO_4$, which requires

	Calculation.		Experiment.
C_{28}	168	74·66	74·16
H_{11}	11	4·88	4·95
N	14	6·22	5·98
O_4	32	14·24	14·91
	225	100·00	100·00

This formula leads to the conclusion that the formation of the compound is due to the union of 1 at. of indigo-blue with 1 at. of alcohol and 2 ats. of acetic acid, and the elimination of 8 ats. of water; for

$$C_{28}H_{11}NO_4+8HO=\overset{\text{Indigo-blue.}}{C_{16}H_5NO_2}+\overset{\text{Alcohol.}}{C_4H_6O_2}+\overset{\text{Acetic acid.}}{2(C_4H_4O_4)}.$$

D.

This body is formed in abundance during this process; and I think it probable that its composition is always the same, as the following analytical results will tend to show:—

I. 0·3540 grm. of the substance, dried at 100° C., and burnt as usual, gave 0·9360 grm. carbonic acid and 0·1635 grm. water.

0·5630 grm. gave 0·5540 grm. chloride of platinum and ammonium.

II. 0·2325 grm., prepared on another occasion, gave 0·6030 grm. carbonic acid and 0·0985 grm. water.

0·5700 grm. gave 0·5435 grm. chloride of platinum and ammonium.

The formula with which these numbers most closely correspond is $C_{56}H_{24}N_2O_{10}$, which requires the following values—

	Calculation.		Experiment.	
			I.	II.
C_{56}	336	71·79	72·11	70·73
H_{24}	24	5·12	5·13	4·70
N_2	28	5·98	6·18	5·98
O_{10}	80	17·11	16·58	18·59
	468	100·00	100·00	100·00

The deficiency of carbon in the second analysis may be partly due to the extreme difficulty with which the combustion of the substance is effected—a difficulty which is always experienced in the case of such bodies as become charred when heated without previously melting.

If the formula of C, $C_{28}H_{11}NO_4$, be doubled, it will be found to differ from that of D merely by $2HO$ less. The formation of both bodies is therefore to be explained in the same manner. The body C may, indeed, be regarded as the anhydride of D, the resemblance between the two substances, in their appearance and properties, being so great that it is only by their behaviour to caustic alkalies that they can be distinguished.

E.

Of this body I only obtained a quantity sufficient for two analyses, and I must, therefore, leave it doubtful whether its composition is uniform or not.

I. 0·4355 grm. gave 1·1050 grm. carbonic acid and 0·1770 grm. water.

0·5995 grm. gave 0·5740 grm. chloride of platinum and ammonium.

II. 0·4685 grm. gave 1·1890 grm. carbonic acid and 0·1930 grm. water.

0·5615 grm. gave 0·5560 grm. chloride of platinum and ammonium.

These numbers lead to the formula $C_{28}H_{11}NO_6$, which requires

	Calculation.		Experiment.	
			I.	II.
C_{28}	168	69·70	69·20	69·21
H_{11}	11	4·56	4·51	4·57
N	14	5·80	6·01	6·21
O_6	48	19·94	20·28	20·01
	241	100·00	100·00	100·00

If this formula be correct, it follows that 2 ats. of indigo-blue combine with 1 at. of alcohol and 5 ats. of acetic acid in order to form, after elimination of 14 ats. of water, 2 ats. of the body E, since

$$2(C_{28}H_{11}NO_6)+14HO=2\overset{\text{Indigo-blue.}}{(C_{16}H_5NO_2)}+\overset{\text{Alcohol.}}{C_4H_6O_2}+5\overset{\text{Acetic acid.}}{(C_4H_4O_4)}.$$

On comparing the formula of E with that of C, it will be seen that the former merely differs from the latter by 2 ats. oxygen, so that $C+2O=E$.

Anthranilic Acid.

Though there could be no doubt, after an examination of the properties of the crystallized acid formed in this process, of its identity with anthranilic acid, still I conceived that its analysis, if not altogether indispensable, might prove of some interest. The results obtained were as follows:—

I. 0·3135 grm., dried at 100° C., gave 0·7035 grm. carbonic acid and 0·1500 grm. water.

0·4050 grm., burnt with soda-lime, gave 0·2890 grm. metallic platinum.

II. 0·1664 grm. gave 0·3720 grm. carbonic acid and 0·0780 grm. water.

0·5590 grm. gave 49 cc. of moist nitrogen at 7° C. and 759·2 millims. pressure, equivalent to 47·25 cc. dry nitrogen at 0° C. and 760 millims. pressure, or 0·0591 grm.

These numbers correspond with the formula $C_{14} H_7 NO_4$, which is that of anthranilic acid, as the following comparison of the composition with that required by theory will show:—

	Calculation.		Experiment.	
			I.	II.
C_{14} …………	84	61·31	61·19	60·97
H_7 …………	7	5·10	5·31	5·20
N …………	14	10·21	10·13	10·58
O_4 …………	32	23·38	23·37	23·25
	137	100·00	100·00	100·00

Since under ordinary circumstances this acid can only be obtained by the long-continued action of boiling concentrated alkaline lye on indigo-blue, its formation in this process, in which only a small quantity of caustic soda dissolved in a large quantity of alcohol was employed, is remarkable. There can be little doubt that its formation in this case is connected in some way with that of the other substances, and could not be effected by the mere action of a dilute alcoholic solution of caustic alkali on indigo-blue.

The experiments just described suggest a few general remarks on this process and the products to which it gives rise.

1. Though I have no doubt that the products, of the properties and composition of which I have just given an account, are distinct chemical compounds, still it might be objected that some of them were not free from an admixture of products of decomposition derived from alcohol alone, the action of caustic alkali on alcohol being a process not very well understood. In order to satisfy myself on this point, I took an alcoholic solution of caustic soda, boiled it for some time, and then evaporated it in contact with the air. The solution became brown; and on adding water and an excess of acid, after evaporation of the alcohol, I obtained a brown flocculent precipitate, which, being filtered off and washed, was dissolved in alcohol. The solution left, on evaporation, a dark brown resinous residue, which I found to be quite insoluble in ether. That portion of the products obtained in this process which was insoluble in water and ether, but easily soluble in alcohol and alkalies, was therefore certain to contain some of this resinous matter; and I therefore laid the whole of it aside, and gave up all further examination of it. It is certainly true that by the action of alkali on alcohol in closed vessels a totally

different product is obtained—a product which differs from the other by its solubility in ether, and its total insolubility in alkalies, and shows a striking resemblance to the body A, which is also soluble in ether and insoluble in alkalies. Still, as my process was conducted in open vessels and not under pressure, I think it is not probable that any of this substance was formed*.

2. From what has been stated above, it follows that all the products, except anthranilic acid, are formed by a very simple process, which consists merely in indigo-blue combining with alcohol and acetic acid in various proportions, and yielding compounds in which none of the constituents as such can be detected. It is, therefore, not a process of decomposition, but rather a synthetical process, a building up of complex bodies from others of a simpler constitution. This is proved by the fact of water being eliminated during the process, whereas in all cases in which complex organic substances are decomposed into simpler ones water is absorbed. This elimination of water proceeds so far, that some of the products, notwithstanding that they are formed by the addition to indigo-blue of many atoms of alcohol and acetic acid (bodies having much less carbon and more oxygen), are found to contain even more carbon than indigo-blue itself, a great proportion of the water both of the alcohol and the acetic acid having been separated. Is it not possible that processes of a similar nature may go on within the cells of plants, the chief function of which is known to consist, chemically speaking, in the construction of complex bodies from others of a simpler composition? Is not the power residing in the vegetable cell which enables it to neutralize very potent chemical affinities somewhat of the same nature as that which, in this process, causes the acetic acid to leave the strong base with which it is combined in order to

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* According to Liebig, the colour which an alcoholic solution of caustic potash assumes in contact with the air is due to aldehyde-resin, the product of decomposition formed by the action of caustic alkalies on aldehyde. Weidenbusch (Annalen der Chemie u. Pharmacie, B. lxvi. S. 153), however, states that the true aldehyde-resin is almost insoluble in alkalies; and in consequence of the discrepancy in the accounts of this body, I requested Mr. A. Mylius to make some experiments on the action of caustic alkalies on alcohol in sealed tubes. He obtained by this action a resin of a fine reddish-yellow colour, soluble in ether, but totally insoluble in watery solutions of alkalies. Its properties so nearly resemble those of the true aldehyde-resin, as described by Weidenbusch, and its composition differs so little from that of the latter, that it seems very probable that the two resins may be identical. If so, it follows that aldehyde-resin is certainly formed by the action of caustic alkalies on alcohol, but only under pressure in sealed tubes. The resin formed in open vessels in contact with the air is totally different. For further particulars regarding this peculiar action I must refer to the account of Mr. Mylius's experiments contained in the Proceedings of the Society, February 21st, 1865.

unite with alcohol and indigo-blue, for which it cannot be supposed to have any strong chemical affinity?

3. The physical properties of these compounds do not seem to depend in any way on those of their constituents. Nevertheless it is to be observed that those containing the largest proportion of alcohol are insoluble in alkalies, whilst those in which the indigo-blue preponderates are the least soluble in alcohol and ether.

4. No law or rule can be detected determining the number of atoms of alcohol and acetic acid which are capable of uniting with the indigo-blue. Were the series more extensive, it is probable that some such law might be found to prevail. It may be remarked, however, that all the products insoluble in water, with one exception, contain either 8 or 10 equivalents of oxygen (assuming the formula of C to be doubled), as will be seen from the following tabular view of their formulæ:—

$$\begin{array}{ll}
\text{A} \ldots\ldots & \begin{cases} C_{62}\,H_{39}\,N\,\,O_{8} \\ C_{80}\,H_{44}\,N_{2}\,O_{10} \end{cases} \\
\text{B} \ldots\ldots & \begin{cases} C_{52}\,H_{35}\,N\,\,O_{8} \\ C_{40}\,H_{23}\,N\,\,O_{8} \end{cases} \\
\text{C} \ldots\ldots\ldots & C_{56}\,H_{22}\,N_{2}\,O_{8} \\
\text{D} \ldots\ldots\ldots & C_{56}\,H_{24}\,N_{2}\,O_{10} \\
\text{E} \ldots\ldots\ldots & C_{28}\,H_{11}\,N\,\,O_{6}
\end{array}$$

5. Regarding the rational formulæ or probable internal constitution of these compounds I hardly venture to indulge in any speculations. They might be considered as conjugated compounds—compounds of which organic chemistry affords so many examples; and it might consequently be possible to obtain from them, by decomposition, some of the simpler bodies which are known to have entered into their composition. I have, however, been unable to discover any facts in favour of this view. Neither indigo-blue nor any of its products of decomposition can be obtained from them by any means which I have tried. In one experiment which I made for this purpose, and which consisted in subjecting the body D to the action of caustic soda, I obtained neither anthranilic acid nor acetic acid, as might have been expected. By evaporating the alkaline solution to dryness, and heating the residue to incipient fusion, the substance was partly converted into a black humus-like matter, insoluble not only in water and alcohol, but also in alkalies. The alkali was supersaturated with sulphuric acid, and the liquid was distilled, when a trace of what I suppose to be formic acid passed over. The liquid yielded no anthranilic acid.

In this respect these compounds resemble some of the secondary products which are formed during the decomposition of indican by acids, and from which no indigo-blue can be obtained, though they must be supposed to contain the elements of that body and of various organic acids, such as formic, acetic and propionic acids. Indeed the resemblance between the two series of compounds extends also to their physical properties. For instance, the body A resembles indifulvine, one of the products derived from indican, both being brownish-yellow resins insoluble in alkalies. B is very similar to indiretine; and D is so like indifuscine, that the two can hardly be distinguished from one another. There may, in fact, be some analogy in the composition of the two last-named bodies. Indifuscine may, as I have shown on a former occasion, be considered as a compound of indigo-blue and propionic acid minus water, as may be seen by the following equation:—

$$\overset{\text{Indifuscine.}}{C_{44}H_{20}N_2O_{10}}+2\,HO=\overset{\text{Indigo-blue.}}{2(C_{16}H_5NO_2)}+\overset{\text{Propionic acid.}}{2(C_6H_6O_4)}.$$

In like manner D may be supposed to contain the same elements combined in a different proportion, since

$$\overset{\text{D.}}{C_{56}H_{24}N_2O_{10}}+10\,HO=\overset{\text{Indigo-blue.}}{2(C_{16}H_5NO_2)}+\overset{\text{Propionic acid.}}{4(C_6H_6O_4)}.$$

Analogies such as these, unsupported by experimental proof, may be only fanciful. Nevertheless they may prove of some use in facilitating the classification of facts. At all events, the circumstance of indigo-blue yielding, by the combined action of alcohol, acetic acid, and alkalies, bodies so closely resembling the products obtained along with indigo-blue in the decomposition of indican seems to afford a striking confirmation of the view which I have taken regarding the composition of these products.

There is another point of view from which these bodies may be considered. They may be represented as substitution products of indigo-blue, one or more of the atoms of hydrogen in the latter being replaced by one or more organic radicals. For instance, the body C may be looked upon as the hydrate of a compound in which one atom of the hydrogen of indigo-blue is replaced by phenyle ($C_{12}H_5$), since

$$C_{28}H_{11}NO_4=C_{16}H_4(C_{12}H_5)NO_2+2\,HO.$$

In order to obtain some confirmation for this hypothesis I took some of the body D, of which I had a considerable quantity, and which only differs from C by containing more water, and subjected it to the action of hydriodic acid and phosphorus in a sealed tube. By the action of the nascent hydrogen I expected that indigo-blue might possibly be regenerated, but the experiment led only to a negative result; for though the tube was heated in the water-bath for several days, the substance, on its being opened, was found to be almost unchanged, a small part only having been converted into a resinous matter easily soluble in alcohol. A similar negative result was obtained when an amalgam of sodium was employed as a source of hydrogen. After these failures I felt but little encouragement to make further experiments in this direction; and this part of the subject must therefore be left in its present state of obscurity.

6. The occasional disappearance of the indigo-blue in the woad-vat, in consequence of mismanagement, now admits of an explanation, which will probably be allowed to be the correct one. By the fermentation of the sugar contained in the madder and other materials employed, alcohol is generated, which in its turn may yield some acetic acid; and alcohol, acetic acid, and a base (lime) being present, nothing further is required for the development of the process above described. By neutralizing a portion of the lime when necessary, the danger of losing colouring-matter is to some extent obviated; but I would venture to suggest, as a means of rendering it still less, the avoiding all materials containing much sugar or starch—substances which might, by their decomposition, lead to the formation of alcohol.

When, in the process above described, formiate of soda is employed instead of acetate of soda, exactly the same phenomena are observed. The indigo-blue gradually disappears, and a dark brown alcoholic liquid is obtained, which is found to contain bodies closely resembling those formed by means of acetate of soda. By operating on a tolerably large quantity of material, I was enabled to ascertain the presence in this liquid of anthranilic acid, and of three products corresponding to, and having the same physical properties as, the bodies B, D, and E. They were separated from one another by the same means as the latter,—the first being a brownish-yellow resin, easily soluble in alcohol and ether, as well as in alkalies; the second a brown powder, soluble in alkalies, but soluble with difficulty in alcohol and ether; whilst the third was a reddish-brown powder, distinguished by its solubility in a boiling solution of acetate of soda—a property which afforded a ready means of separating it from the others. No compounds insoluble in alkalies, and corresponding to the bodies A and C, were formed with formiate of soda. The analysis of the compound resembling B yielded the following results:—

0·2960 grm. gave 0·7565 grm. carbonic acid and 0·1920 grm. water.

0·3615 grm. gave 0·1915 grm. chloride of platinum and ammonium.

These numbers lead to the formula $C_{48}H_{29}NO_{10}$, which requires

	Calculation.		Experiment.
C_{48}	288	70·07	69·70
H_{29}	29	7·05	7·20
N	14	3·40	3·32
O_{10}	80	19·48	19·78
	411	100·00	100·00

The substance, it will be seen, is formed by the union of 6 ats. of alcohol, 4 ats. of formic acid, and 1 at. of indigo-blue, accompanied by the separation of 20 ats. of water, since

$$C_{48}H_{29}NO_{10}+20\,HO=\overset{\text{Indigo-blue.}}{C_{16}H_5NO_2}+\overset{\text{Alcohol.}}{6(C_4H_6O_2)}+\overset{\text{Formic acid.}}{4(C_2H_2O_4)}.$$

The composition of the substance corresponding to E was also determined, the results being as follows:—

0·1960 grm. gave 0·4500 grm. carbonic acid and 0·0820 grm. water.

0·3635 grm. gave 0·3250 grm. chloride of platinum and ammonium.

These numbers correspond with the formula $C_{26}H_{11}NO_8$; but the only formula which can lead to an explanation of the manner in which the substance is formed, though it gives a calculated composition not agreeing quite so well with the experiment, is $C_{26}H_{13}NO_8$, which requires

	Calculation.		Experiment.
C_{26}	156	63·15	62·61
H_{13}	13	5·26	4·64
N	14	5·66	5·61
O_8	64	25·93	27·14
	247	100·00	100·00

Assuming this to be the correct formula, its formation would take place in accordance with the following equation:—

$$2(C_{26}H_{13}NO_8)+10HO=\overset{\text{Indigo-blue.}}{2(C_{16}H_5NO_2)}+\overset{\text{Alcohol.}}{3(C_4H_6O_2)}+\overset{\text{Formic acid.}}{4(C_2H_2O_4)}.$$

It will be seen that the same law regarding the number of atoms of oxygen prevails here as in the case of the bodies before described, this number being either 8 or 10.

If in this process ordinary alcohol is replaced by methylic alcohol, the same effect is produced, provided acetate of soda is employed; but a mixture of methylic alcohol, formiate of soda, and caustic soda does not act in the same manner on indigo-blue, which remains unchanged, however long it may be left in contact with the boiling liquid. It appears, therefore, that one of the two agents, ethylic alcohol or acetic acid, is quite essential. One of the two may be replaced by an homologous body; but when both are so replaced, the indigo blue remains intact.

XL. *Notices respecting New Books.*

Table of the Reciprocals of Numbers, from 1 *to* 100,000, *with their differences, by which the reciprocals of numbers may be obtained up to* 10,000,000. *By* Lieut.-Colonel W. H. Oakes, *A.I.A.* London: C. and E. Layton. 1865.

In most, if not all, tables of the reciprocals of numbers, the arrangement hitherto adopted has been to give integral numbers from 1 to n, and the corresponding reciprocals as decimals. In the present Table both the numbers and their reciprocals are given without decimal points, which are to be supplied according to circumstances. Advantage is thus taken of the fact that the significant digits of the reciprocals of, for example, 3·7256, 37·256, 372·56, &c. are 2684131; the reciprocals being respectively 0·2684131, 0·02684131, 0·002684131, &c. In other words, Colonel Oakes has arranged his Table with a view to the fact that if r is the reciprocal of n, then will $r\times 10^p$ be the reciprocal of $n\div 10^p$. In consequence he has been able to render the arrangement of the Table almost identical with that of an ordinary Table of Logarithms. In fact the only exception is that the differences reckoned in order of the numbers are all negative; as they must obviously be, since, the numbers increasing, their reciprocals will decrease.

Prefixed to the Table are two notices, one describing the arrangement of the Table and exemplifying its uses, the other giving a short account of the manner in which the reciprocals of the larger numbers were calculated. A word or two may be said on the latter point.

Let n denote any large number, K the sum of the arithmetical complements of the logarithms of n and $\overline{n+1}$, or

$$K=\log\frac{1}{n\,.\,(n+1)}.$$

Also let d_1, d_2, d_3 ... denote respectively

$\log(n+1)-\log(n-1), \log n-\log(n-2), \log(n-1)-\log(n-3)$,&c.

Now

$$\frac{1}{n-1}-\frac{1}{n}=\frac{1}{n\,.\,(n-1)}=\frac{1}{n(n+1)}\,.\,\frac{n+1}{n-1}.$$

Therefore

$$\log\left(\frac{1}{n-1}-\frac{1}{n}\right)=\log\frac{1}{n(n+1)}+\log(n+1)-\log(n-1)=K+d_1.$$

Similarly

$$\log\left(\frac{1}{n-2}-\frac{1}{n-1}\right)=K+d_1+d_2,$$

$$\log\left(\frac{1}{n-3}-\frac{1}{n-2}\right)=K+d_1+d_2+d_3,$$

and so on. Of course d_1, d_2, d_3 ... are given by a Table of logarithms, and are equal for a considerable series of numbers. Hence it is plain that the logarithms of the differences of the successive reciprocals can be obtained by addition, and the calculation conducted in a tabular form. Suppose *n* to equal 62500, then will K equal 408 2330, and the calculation will stand thus :—

Numbers.		Logs. of diff. of reciprocals.	Diff. of reciprocals.	Reciprocals.
	K=	408 2330		
62500		......	......	·00001600 0000000
	Diff. of logs.			
62499	139	408 2469	256004	0256004
98	139	2608	12	0512016
97	139	2747	20	0768036
96	139	2886	29	1024065
95	139	3025	37	1280102

The reciprocals entered in the Table are, of course,

1600 000, 1600 026, 1600 051, 1600 077,
1600 102, 1600 128, &c.

In the same tabular calculation the reciprocals of the half and quarter numbers are found by simple multiplication.

The chief merit of a work of this kind is, of course, accuracy. To secure this, every precaution seems to have been taken. "To prevent error," says Colonel Oakes, "the co. logarithms were checked independently at each 50th term. In taking out the numbers, the progression of their differences was kept in view, so that no material error could occur. The summation of the differences was checked at every 10th term by a subordinate summation, and by comparison with Barlow's Tables; and wherever the seventh figure could be uncertain, it was determined by actual division. Finally, every hundredth term was computed by division. The whole of the calculations were performed in duplicate, and when the proofs were set up from one manuscript they were read with the other; and second and third proofs were also each examined by addition of the printed differences, and by comparison with Barlow's Table at each 10th term. Lastly, the proofs were again examined, and the whole Table virtually recomputed by summation on the Arithmomètre of M. Thomas de Colmar."

It is proper to add that the work was undertaken at the suggestion of Professor De Morgan, who says that it is, as far as he knows, "the largest which has ever been attempted," and that "it is a very useful Table, and that its applications are far too little known and thought of."

XLI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 157.]

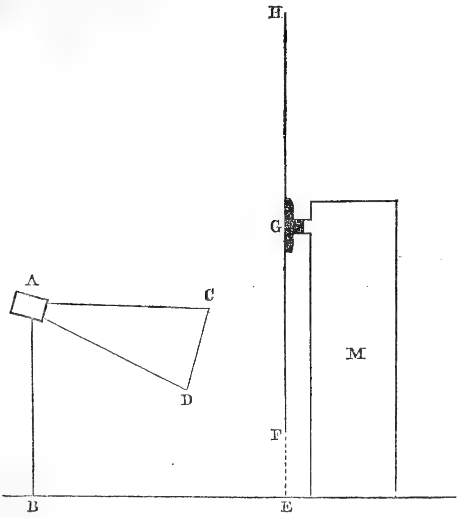
June 15, 1865.—Major General Sabine, President, in the Chair.

THE following communication was read:—

"On the Heating of a Disk by rapid Rotation *in vacuo*." By Balfour Stewart, M.A., F.R.S., and P. G. Tait, M.A.

1. The authors were led, by certain views which they entertain regarding the loss of energy by a body, to make experiments in order to test these views, and about the end of 1863 they obtained results in air, which encouraged them to have constructed an apparatus wherewith to procure rotation *in vacuo*.

2. The apparatus for this purpose was devised and executed by Mr. Beckley, mechanician at the Kew Observatory, at which place the experiments about to be described were made. In this apparatus a slowly revolving shaft is carried up through a barometer tube, having at its top the receiver which it is wished to exhaust. When the exhaustion has taken place, it is evident that this shaft will revolve in mercury. In the receiver the shaft is connected with a train of toothed wheels, and ultimately causes a circular disk to revolve 125 times for each revolution of the shaft. The disks used have a diameter of 13 inches, and their plane is vertical. Two insulated wires, connected with a Thomson's reflecting galvanometer, are carried through two holes in the bed-plate of the receiver, and are then connected with a thermo-electric pile, having the usual reflecting cone attached to it. The outside of the pile, and of its attached cone, is wrapped round with wadding and cloth, so as to be entirely out of the reach of currents of air. The vacuum-gauge is on the siphon principle; it was constructed by P. Adie, and there is every reason to believe that it is perfectly deprived of air. The following figure will



A B= 6 inches.
B E= 8 „
E G= $8\frac{1}{2}$ „
F H=13 „ (diam. of disk.)
A C= $5\frac{1}{4}$ „
C D= $2\frac{1}{2}$ „
M. Multiplying gear.

render the arrangements clear: and it is only necessary to add that the whole is covered over with an air-tight glass shade, 15 inches in diameter and 16 inches high.

3. The deflection of the galvanometer-needle, produced by heating the pile, is recorded by means of a small mirror attached to the needle, which, as it moves, causes the reflected image of a line of light to travel over a graduated scale. The galvanometer-needle is rendered very nearly astatic by means of an auxiliary magnet; and this arrangement can be made so sensitive that if the temperature of the disk, exposed to the cone as in the figure, were to rise 1° Fahr., this would be denoted by a change in the position of the line of light equal to fifty divisions of the scale.

4. In these experiments the disk was rotated rapidly for half a minute, and a heating effect was, in consequence of this rotation, recorded by the pile. The object of this paper is to investigate the origin of this heating effect.

5. In this investigation the authors prefer discussing the effect produced on a metallic disk. The metal aluminium was chosen, from its lightness, so as to diminish the weight upon the bearings as much as possible. The reason for preferring a metallic disk is that the heat produced in this case affects the whole substance of the disk, and can thus be approximately measured. The disk of this metal employed was $\frac{1}{20}$th of an inch thick and 13 inches in diameter; it weighed ten ounces, and in most of the experiments it was covered with a coating of lampblack, applied by means of negative photographic varnish. In some of the experiments a plate of rock-salt, tightly secured in a brass fitting, was screwed upon the mouth of the cone. When this was done, a small piece of anhydrous baryta was placed within the cone to keep the inner surface of the salt dry, and a dish containing strong sulphuric acid was likewise placed in the receiver. Indeed the latter was always used; so that in the results obtained the residual air may be considered as nearly dry, and the surface of the disk, as well as that of the rock-salt, when this was used, nearly free from moisture.

Furthermore, in order to obviate the objection that the electric currents which take place in a revolving metallic disk might alter the zero of the galvanometer, the position of the line of light was read before the motion began, and immediately after it ceased, the difference being taken to denote the heating effect produced by the rotation. The turning was made in this way:—As soon as full speed was obtained, which might be about 10 seconds after beginning the motion, a chronometer was noted, and the handle was turned at a uniform rate for 30 seconds, and thereafter stopped as soon as possible. The most convenient speed *in vacuo* was found to be 20 revolutions of the handle, or 2500 of the disk, in 30 seconds. It is believed that the heating effect recorded may be considered as due to about 40 seconds at full speed.

6. The *thermometric* value of the indication given by the galvanometer was found in this way:—The disk was removed from its attachment and laid upon a mercury-bath of known temperature. It was

then attached to its spindle again, being in this position exposed to the pile, and having a temperature higher than that of the pile by a known amount. The deflection produced by this exposure being divided by the number of degrees by which the disk was hotter than the pile, we have at once the value in terms of the galvanometric scale of a heating of the disk equal to 1° Fahr.

7. The following sets of experiments were made with blackened aluminium disk and rock-salt in the cone.

No. of set.	No. of observations in each set.	Time at full speed.	No. of turns of handle at full speed.	Heat indication. ° Fahr.	Tension of air in inches.
I.	3	30	20	0·85	0·3
II.	4	30	20·5	0·87	0·3
III.	4	30	20	0·81	0·3
IV.	3	30	20	0·75	0·65

8. A piece of wood precisely similar to the rock-salt plate was next inserted into the fitting of the latter, and after rotation there was no indication whatever. Hence the above effect (art. 7) is due to radiant heat, and not to currents of heated air reaching the pile. Again retaining the rock-salt, the interior of the cone was covered by black paper, and the effect upon the pile was very much diminished: this also goes to prove that the effect (art. 7) is due to radiant heat; and it now remains to discover whether this radiant heat comes from the rock-salt or from heated air, or from the surface of the disk.

9. The following sets of experiments were made with blackened aluminium disk, but without rock-salt.

No. of set.	No. of observations in each set.	Time at full speed.	No. of turns of handle at full speed.	Heat indication.	Tension of air in inches.
V.	3	30	20	0°·92	0·37
VI.	3	30	20	0·93	0·60

And when a black paper cover was introduced into the cone, other things remaining as before, the indications of the galvanometer were greatly diminished. The effect produced without rock-salt is therefore also a radiant heat effect; and as the indications (in terms of temperature) are as large as when rock-salt was used, we may conclude that the effect of art. 7 was to no perceptible extent due to heating of the rock-salt, otherwise it would have been diminished when the plate of rock-salt was taken away. Besides, as rock-salt is a bad radiator and a good absorber of its own heat, the plate would have had to be heated perhaps as much as 15° or 20°, in order to furnish a radiation equal to 0°·8 from the disk. On both these accounts it is impossible to believe that the effect was due to heating of the rock-salt.

10. Nor is it probable that the heating effect is due to radiation from heated air, since in order that nearly dry air of such a tenuity might give such a radiation, it would require to be heated enormously. But another proof that the effect is not due to air is afforded

by removing the black from the aluminium disk and leaving it a rough metallic surface, when the indication afforded by the galvanometer is reduced to about one-fourth of the amount with the blackened disk.

11. It only remains that the heating effect proceeds from the disk, and since the heat-indication afforded by the galvanometer-needle remains nearly constant for some time, this effect must be due to the heating of the whole substance of the disk.

12. Presuming, therefore, that the entire substance of the disk is heated, the next point is to ascertain the cause of this heating effect.

Now, in the first place, it cannot be due to conduction of heat from the bearings, for in some of the experiments the disk was insulated from its bearings by means of a plate of ebonite, and the result was the same.

Again, it is not due to revolution under the earth's magnetic force, for Professor Maxwell has kindly calculated the effect due to this cause under the conditions of the experiment, and he finds it infinitesimally small. Nor is the effect due to the condensation of vapour of water upon the surface of the disk. In some of the experiments, when the vacuum was newly made, there appeared to be a strictly temporary effect, due probably to moisture, which *increased* the range of the needle, but only during the time when the motion was taking place, for it very soon assumed its permanent position. In other experiments, when the air was very dry, there appeared to be a temporary cold effect of a similar description; but in all cases when the vacuum was kept long enough for the sulphuric acid to act, the only effect was a permanent one in the direction of heat, and this is that which has been described in these experiments. This permanent heating effect cannot, therefore, be due to the condensation of aqueous vapour, and indeed it is impossible to suppose that in the presence of sulphuric acid so much vapour should remain suspended in air of so low a tension as to produce a permanent effect so very considerable by its deposition.

13. In this endeavour to account for the heating effect observed, it would appear that we are reduced to choose between one of two causes, or to a mixture of the two.

(1) It may be due to the air which cannot be entirely got rid of.

(2) It is possible that visible motion becomes dissipated by an ethereal medium in the same manner, and possibly to nearly the same extent, as molecular motion, or that motion which constitutes heat.

(3) Or the effect may be due partly to air and partly to ether.

14. Now, if it be an air effect, it is not one which depends upon the mass of air. For (art. 7) the effect for a vacuum of 0·3 in. is as large as for one of 0·65 in.; and also (art. 9) the effect for a vacuum of 0·37 in. is as large as for one of 0·60 in.; and further, in some approximate experiments, the effect produced upon a wooden disk, in a vacuum of 4·0 in. and 2·0 in., was found to be the same as in one of 0·5 in., or very nearly so. It may therefore be presumed that only a very inconsiderable portion of the effect observed depends upon the mass of air left behind. It would, however, appear, from the views

of Professor Maxwell and Mr. Graham, that there is another effect of air, namely, fluid friction, the coefficient for which they believe to be independant of the tension; and as far, therefore, as this effect is concerned, little is gained by diminishing the amount of the residual air. It would appear, however, that the fluid friction of hydrogen is much less than that of atmospheric air; so that, were the heating effect due to fluid friction, it ought to be less in a hydrogen vacuum. An experiment was made with this purpose; and, other circumstances being precisely similar, it was found that in a hydrogen vacuum the heating effect due to rotation was 22·5, while in an air vacuum it was 23·5. These numbers may probably be considered as sensibly the same, and this experiment would therefore appear to denote that the effect is not due to fluid friction.

15. The authors, in submitting these remarks to the Royal Society, do not suppose that their experiments have yet conclusively decided the origin of this heating effect, but they hope by this means to elicit the opinions of those interested in the subject, which may serve to direct their future research.

XLII. *Intelligence and Miscellaneous Articles.*

CONTRIBUTIONS TO THE KNOWLEDGE OF INDIUM.

BY PROFESSOR SCHRÖTTER.

PURE metallic indium has the colour and lustre of cadmium; it is very soft, and marks paper easily, giving a bright streak with only a slight shade of grey.

The positions of the two characteristic lines of the indium spectrum were determined by direct comparison with the solar spectrum. The spectrum of the metal was obtained in the usual manner by means of a Ruhmkorff's induction-coil used in connexion with a Leyden jar. The comparison showed that the magnificent blue indium-line does not coincide with any of the dark lines of the solar spectrum, and hence, according to Kirchhoff's acute deduction, that no indium exists in the sun's atmosphere. This blue line corresponds exactly with the number 2523 upon Kirchhoff's scale. The second indium-line lies in the violet, between G and H, a part of the spectrum which was only partially measured by Kirchhoff: it coincides with a strong Fraunhofer's line, which is very sharply defined on Rutherfurd's photograph of the solar spectrum. In order to determine the position of this line with greater precision, the three 60° prisms of the apparatus were first placed in the position of minimum deviation for the line G (2854·8), and then the distances of the three known lines, 2721, 2670, and 2574, were measured by means of the micrometer-screw. In this way the value of a revolution of the micrometer-screw was obtained in millimetres. On now measuring in the same way the distances from G to the violet indium-line, and to the lines H and H′, keeping the prisms always in the position of least deviation, it was found that upon Kirchhoff's scale the number 3265·8 nearly corresponded to the violet indium-line, the number 3582 to the line H, and the number 3677 to the line H′. A measurement of the positions of these lines on Rutherford's photograph of the solar spectrum gave nearly the same relative distances.

In addition to the lines above mentioned, several other lines were visible in the indium spectrum; but these belonged in part to the gases of the atmosphere, and in part to the iron and zinc of which the indium employed appears still to have contained traces,—a point upon which light will be thrown by future experiments.

Although the most recent determination of the equivalent of indium by C. Winkler is probably tolerably near the truth, it did not seem superfluous to ascertain its value by some other method. I selected for this purpose the sulphide of indium, which I have found possesses the very valuable property, for the object in view, of being completely converted by hydrochloric acid at the common temperature into chloride of indium, with separation of sulphuretted hydrogen. Chloride of indium, although extremely hygroscopic, can be accurately weighed if certain precautions are observed; and, in addition, the proportion of sulphur can be accurately determined as a control by leading the sulphuretted hydrogen that escapes into a solution of a ferric salt, and determining volumetrically the quantity of ferrous salt that is thereby produced. Contrary to what has been stated hitherto, sulphide of indium is completely precipitated by sulphuretted hydrogen from any sufficiently dilute and not too acid solution. In colour, it cannot be distinguished from sulphide of cadmium; in fact indium in all respects exhibits the greatest similarity to cadmium, in relation to which its electrical behaviour requires it to be classed as electro-negative. The results of the numerical determinations have not yet assumed a final shape; they will be contained in the more detailed communication that is to follow.—*Anzeiger d. Kaiserl. Akad. d. Wissenschaft. in Wien,* 1865, p. 139 (July 27).

ON THE VISUAL SENSATIONS PRODUCED BY INTERMITTENT EXCITATIONS OF THE RETINA.

Professor E. Mach of Graz communicated to the Section of Natural Sciences of the Imperial Academy of Vienna, at its session on July 20, a preliminary note on the effect upon the retina of the division of the luminous stimulus with regard to time and with regard to space.

I. The luminous stimulus was examined when intermittent with regard to time, and its useful effect for exciting sensation was *measured.* Both the intervals during which the light acted and during which it was absent were varied in their absolute as well as relative lengths; and the results of the experiments permit of conclusions being drawn as to the manner in which the vibrations of the retina are excited and die out.

II. If a rectangular strip of paper be painted with Indian ink so that the depth of shade varies from one end to the other, a remarkable phenomenon is presented by it, the law of which may be expressed as follows:—Let the distances of the various points of the strip from one end be laid down as abscissæ, and the intensity of the light at the respective points as ordinates; then at every part of the strip at which the curve formed by joining the extremities of the ordinates is concave towards the axis of abscissæ the strip appears lighter than it is in reality, and wherever the curve is convex the strip appears darker. The sensation of light, d, produced on any portion of the retina by an illumination of the intensity i, may accordingly be thus expressed,

$$d=f(i)-f_1\left(\frac{d^2i}{dx^2}\right),$$

where x is the distance of the point in question measured longitudinally along the strip, and $f_1(z)$ is to be taken as representing a direct function of z, and a function of the same denomination as z.

With the help of this law it is easy to produce such a distribution of the illumination that a surface which is objectively *lighter* shall appear *darker*, while a neighbouring surface which is objectively *darker* shall appear *lighter*.

III. Experiments analogous to those mentioned under I. were made with intermittent sounds, but the results were not such as can be briefly stated.—*Sitzungsber. d. K.-k. Akad. d. Wissensch. in Wien*, 1865, No. xix. p. 123.

INVERSION OF THE ABSORPTION-BANDS IN THE SPECTRUM OF ERBIUM. BY PROF. BAHR OF UPSALA.

I have succeeded in a very simple manner in inverting the absorption-spectrum of erbia (and of terbia?), and in changing the absorption-bands into bright lines, which, as regards brilliancy and distinctness, are scarcely inferior to the lines of a gas-spectrum.

If the bent end of a fine platinum wire is dipped into a syrupy solution of the nitrate or hydrochlorate of this earth, and the adherent salt brought into the flame of a Bunsen's burner, a spongy mass of erbia is formed which shines with an intense greenish light, and still greater lustre than zirconia. Placed before the slit of the spectroscope, it gives a continuous spectrum in which bright lines appear. These correspond, with the exception of a few, with the absorption-bands which the solutions of erbia give.

I have not succeded in obtaining in the same way such a spectrum with oxide of didymium,—which appears to prove that the dark absorption-band which erbia-solutions give near the Fraunhofer line D does not arise, as we might be tempted to believe, from an admixture of didymium.—Liebig's *Annalen*, September 1865.

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

[FOURTH SERIES.]

NOVEMBER 1865.

XLIII. *On some Physical Effects produced by the contact of a Hydrogen-flame with various Bodies.*

By W. F. Barrett, *Assistant in the Physical Laboratory of the Royal Institution**.

IN the spring of this year, when conducting a series of experiments for Professor Tyndall, on the character of the radiation emitted by various solids heated by means of a hydrogen-flame, I noticed that on the surface of several of the substances examined a deep blue colour arose as soon as the flame was brought into contact with the body. Other engagements prevented a further investigation of the matter until some time afterwards, when, again observing the same phenomenon, I drew Professor Tyndall's attention to it, and he advised my pursuing the subject. For this purpose he kindly placed sufficient time at my disposal, for which, and other marks of kindness, I may here perhaps be allowed to express my sincerest thanks.

A careful examination of this blue coloration showed that it occurred only when the flame came into actual contact with the substance, the colour being restricted with great precision to the place where the hydrogen was in combustion; accordingly it gave in section a faithful image of the construction of the flame. This singular appearance was invariably of a rich blue tint; its production was instantaneous, but its duration was generally short, for it disappeared after a few seconds' exposure when the flame continued to play upon one spot, although it was immediately restored by shifting the position of the jet to an untried portion. The hydrogen in some experiments burnt from a small orifice in a brass tube, and in others from a platinum jet;

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* Communicated by the Author.

the gas was made in the usual way, and was washed before passing into the reservoir which held it. The blueness was not due to any foreign element in the gas; for when chemically pure hydrogen was used, the colour appeared as bright as with the impure gas.

I now examined a number of bodies taken at random from various places in the laboratory, and found the blueness was to be seen on most of them; it appeared with extreme brightness on some pieces of granite and glass tubing; but was not at all confined to non-metallic bodies, as the majority of the metals presented the same appearance. Still there were several substances which showed no blueness whatever, even when subjected to the most careful and repeated examinations; and there were others on which the blueness was only faintly and momentarily seen. This latter occurred with many objects taken from the cases of the mineralogical collection in the museum of the Institution. A classification of the substances experimented upon, according to the effects they gave, yielded no information as to the cause to which the phenomenon might be ascribed. The results obtained were in many cases so utterly anomalous, and the blueness appeared so capriciously, it at once became evident that only by varying the experiments in every way could I hope to discover the origin of the colour. Nevertheless, from the nature of the appearance, I imagined it might probably have to be attributed to a new kind of fluorescence, differing from that investigated by Professor Stokes in requiring contact of the source of rays, and in its disappearance after a short exposure to the flame—therefore in some respects resembling phosphorescence, yet quite distinct from it in the sharpness of its outline, the rapidity of its appearance, and its non-persistence when the source was removed even to the smallest distance.

The examination of the various bodies demonstrated one fact of some importance—namely, that in every case where the blueness appeared it was always of the same colour and shape, the area over which it extended being, as already mentioned, strictly determined by the size of the hydrogen-flame. Heating the solid to redness, by any means, sufficed to destroy its power of showing the colour: for example, a piece of granite was taken which gave the blueness in every part; one spot on it was then exposed to the oxyhydrogen-flame: the granite when cold was again examined, when the blue appeared distinctly on every part except that portion which had been heated; here the blueness was suddenly extinguished. This spot, which by heating had been deprived of its power to give the blue colour, and which it is evident did not recover the property on cooling, had its power restored after a few days' exposure in the laboratory, the blueness then being seen with a brilliancy which seemed to vary according to the length of time it had been exposed. Moreover, if a piece of granite, brick, &c., which showed the blueness vividly, was broken in two, the colour was invariably absent on the newly-fractured surfaces.

Upon trying a block of ice, I found the blue coloration was produced here also, as soon as the hydrogen-flame touched its surface; but it was not so certain in its appearance as on other bodies: it was best seen around the bottom of the block, or between two pieces of ice placed nearly in contact. Some liquids as well as solids showed the blue colour when the hydrogen-flame was brought down upon them. Occasionally it was to be seen on the surface of water; but invariably it appeared with great brilliancy on sulphuric acid. With regard to the latter, a mere trace of the liquid could be detected by the sudden appearance of the blue: what, however, was most remarkable, and seemed extremely suggestive, was that the blueness did not disappear even after a prolonged exposure of the acid to the flame, the colour remaining undimmed throughout. As soon as the flame was brought to bear upon the surface of the acid, contact with the liquid was immediately announced by a vivid blue light, of a splendour only comparable to the blue line observed by Professor Tyndall in the spectrum of highly heated lithium vapour. The blueness here, as elsewhere, shone from the body itself, and did not in any way tinge the flame with its colour. It appeared like a sudden luminosity emitted from the surface of the liquid wherever the actual flame came in contact with it.

The next inquiry which suggests itself is, whether the blueness was peculiar to the contact of a hydrogen-flame, or could it be excited by other sources? To answer this, I tried a luminous jet of coal-gas, the non-luminous flame issuing from a Bunsen's burner, and flames of alcohol, bisulphide of carbon, carbonic oxide, olefiant and marsh-gas—each flame being reduced to about the size of the jet of hydrogen employed. No trace of the blue luminosity was seen with any of these flames. Bodies which showed the colour strongly with burning hydrogen, when transferred to any of the foregoing flames were perfectly inert. Was the blueness, then, to be attributed to the high temperature of the hydrogen-flame, to its richness in chemical rays, or to its possession of some other property? I endeavoured to solve the first part of this question by urging the different flames with the blowpipe, and also by holding a piece of granite, which showed the blueness vividly with hydrogen, in the still hotter oxyhydrogen-flame; but in no case was the blue colour produced. I obtained, however, a very slight coloration by using oxygen and coal-gas, allowing the latter to be rather in excess. Finally, on account of the extreme richness of this source in rays of high refrangibility, I made use of the secondary spark from a powerful Ruhmkorff's coil. But even here no blueness was evident along the line of the spark. Moreover the passage of the discharge over a piece of granite was unable to bestow or restore its power of showing the blue radiance when the solid was afterwards exposed to the hydrogen-flame *.

An accidental experiment revealed the source of this curious appearance. Amongst the various substances I examined, I found that vulcanized india-rubber, when exposed to the flame, exhibited the blue colour with great brilliancy. But here I had reason to believe the blue colour was occasioned by the combustion of the sulphur with which the india-rubber is associated. To see if such were the case, a plate of platinum had its surface rubbed with a stick of sulphur, and was then exposed to the flame. Although no blueness was visible before, yet after the rubbing the coloration shone out brightly for a short time, having in every respect the same appearance as the luminosity seen on granite, &c. Afterwards I found that the slightest contact of sulphur, or a body containing it in the free state, was sufficient to impart to any substance the power of showing the blue coloration beneath the hydrogen-flame. When a plate of platinum which showed the blue colour from being wiped over with a piece of vulcanized rubber, was examined with a high power under the microscope, some spots were seen which were unaffected by a drop of water, but which readily dissolved in bisulphide of carbon. These and other experiments convinced me that, at any rate, the chief part of the phenomenon must be attributed to the presence and combustion of minute quantities of sulphur. It may be urged that if the blueness be simply due to the combustion of sulphur, the different flames tried ought, equally well with the hydrogen, to show the luminosity. But such is not the case, and as yet I have no experimental grounds for explaining why this difference in action should exist. It cannot be wholly due to the high

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* On granite and some other bodies, the spark, when caused to pass through the substance, ramified for a short distance, more especially from one pole, into beautifully sinuous lines of light, the current splitting itself up as it struck the block, in order to pass through the paths of least resistance. This occurred when the secondary terminals, separated beyond the striking-distance, were caused almost to touch the surface of the granite. When the terminals were approximated to each other until the spark crossed the interval, grazing over the surface of the granite, it was found that the striking-distance was considerably greater than in air alone. This may be accounted for by the fact that the discharge deflagrated a portion of the granite in its passage, rendering the spark not only longer, but far more brilliant than when the granite was absent.

temperature of the hydrogen-flame, but may probably have some connexion with the chemical nature of the gas.

But how came the sulphur on the surface of those bodies which had not been purposely placed in contact with that element? It was extremely unlikely that some sulphurous substance had accidentally touched all the different solids on which the blueness was distinctly seen. Could the sulphur proceed from the atmospheric dust deposited on the surface of these solids? This explanation was established by the following experiment. An old block of granite was taken and examined by the hydrogen-flame; it showed the blue coloration vividly on every part of its surface. The block was now split in halves and again searched by the flame: on the newly fractured surfaces not the faintest trace of colour was seen even in a darkened room. The two fragments were now placed on an upper shelf in a store-room adjoining the laboratory, in which no fire was ever made. Over one of the pieces a glass vessel was inverted, dust being thus excluded from it, whilst the other fragment was left uncovered; both had their fractured surfaces turned uppermost. After three days they were again tried with the hydrogen-flame. The upper side of the portion which had been under the glass showed, as before, no blueness; whereas the other exposed fragment was now found to give a well-marked blue colour on its upper and previously inert surface. A piece of white paper had been placed by the side of the granite, and from this it was seen that dust had settled on the exposed fragment. Two plates of chemically clean platinum similarly exposed gave corresponding results; the same was true of marble and other substances; and similar effects were obtained when the bodies were exposed in other places away from the laboratory, in private dwellings, or in the open air. An instructive result was given by the examination of a flask which, having remained in the laboratory undisturbed for some time, had become thickly coated with dust on its upturned parts. After trying the under surface of the flask, which showed no blueness whatever, the hydrogen-flame was caused to pass slowly up the surface of the flask. As the upper portions were approached, a slight coloration made its appearance; and when the flame came into contact with the dust on the upturned surface, an extremely bright but momentary blueness was seen on the spot examined. The duration of the colour could easily be prolonged by moving the flame to other dusty portions of the flask.

These experiments, as far as they go, show, I believe, that among the other foreign ingredients of our atmosphere, sulphur, either free or in a state of combination, must be regarded as occupying not a very unimportant place—derived, it may be, from our coal fires. From investigations that have recently been made in France, it has been stated that traces of sulphate of soda are often present in the air: it may be objected that under the intense heat of the hydrogen-flame, it is this body which has been decomposed and burnt, and thus the sulphur of the compound mistaken for the free element. This conclusion is incorrect; for sulphate of soda does not apparently decompose in this way, no blue coloration having ever been produced by contact of a hydrogen-flame with this salt. On the other hand, the various specimens of alum which were tried, both on old and new surfaces, showed a vivid blue figure of the flame. On the sulphides generally, and on some of the sulphates, the blue was well seen, owing to their decomposition by the burning hydrogen. The singular persistence of the blue appearance on sulphuric acid points to the probability that the liquid was instantaneously decomposed by the contact of the flame*.

The explanation of the *capricious* appearance of the blue coloration seen on the surface of many bodies is now easy. Those which showed the luminosity had either been long exposed to the air, or had in some way come into contact with sulphur. The latter will account for the blueness seen on the surface of a block of ice. In order to convey the gas from its reservoir to the jet from which it was burnt, a piece of vulcanized india-rubber tubing had been used, in handling which the fingers had become coated with a film of sulphur. This was readily proved to be the case by exposing the fingers to the burning hydrogen; on the fore finger and thumb the blue section of the flame was seen to perfection, which was not the case after the hands had been thoroughly washed. Having grasped the ice with unwashed hands, the sulphur on the fingers had been partially transferred to its surface. By the liquefaction of the ice the particles of sulphur were carried down to the base of the block; hence, as already noticed, the blueness was always seen brightest around the lower part of the ice. But the fingers had not thus come into contact with the water on which the blueness occasionally flashed out. Here the coloration was found to be due to particles of sulphur carried over, by the current of hydrogen, from the interior of the tube, on to the surface of the water: there they were burnt; and when observed through a lens, their presence could be verified by the discontinuity of the blue colour. If absent before, a slight shake of the tubing was generally sufficient to cause the appearance of the blueness on water or other liquids†.

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* Subsequent experiments render it probable that the sulphur detected in dust is chiefly derived from a decomposition of sulphate of ammonia.

† This is in harmony with a fact observed by Professor Tyndall in his researches on radiant heat: the passage of a gas through a new piece of

In subsequent experiments care was taken to avoid the use of any vulcanized india-rubber tubing. This destroyed the blue seen on water and ice, but did not affect the other results which have been enumerated.

The sudden appearance of a blue colour when a hydrogen-flame is brought into contact with a body, thus becomes a test for the presence of sulphur, and as such it is one of great delicacy. For example, after the fingers had touched a piece of vulcanized india-rubber, the passage of them through a small quantity of water caused the blueness to be seen on its surface. What weight of matter was here employed in the production of a distinct colour I will not venture to suggest. The best mode of observing the blue coloration on water, and indeed elsewhere, is to bring the flame vertically down on to the surface of the body examined: when a certain part of the flame reaches the body, if sulphur be present, a blue ring of light is seen.

I have to thank my friend Mr. Broughton for enabling me to compare the delicacy of this physical test with the best chemical tests for sulphur. To accomplish this, milk of sulphur was mixed with precipitated silica in varying proportions until a point was reached at which the chemical reagent (nitroprusside of sodium) failed to indicate the presence of sulphur. With the hydrogen-flame, the silica alone gave no blue colour, but it immediately appeared on the admixture of sulphur, and could easily be recognized, not only in part of the very mixture where the chemical test failed, but far beyond this degree. It is almost needless to state that whatever was employed to support the powder was carefully examined by the flame just before the experiment was made. A mixture of silica and sulphur was made containing the $\frac{1}{500}$th of a grain of the latter. Of this, the minutest quantity that could be removed, less than $\frac{1}{100}$th of a grain, was placed on a piece of platinum or thrown on the surface of water; in either case, when contact with the hydrogen-flame was made, the blue colour shone for a moment. By the chemical test, the sulphur in a less minute quantity of the powder remained undetected. If it could be uniformly mingled, I have reason to believe that certainly the 100,000th part of a grain of sulphur could readily be detected by means of the hydrogen-flame. The presence of sulphur in the homœopathic powders which are said to contain a trace of that substance was easily verified by moistening the powder (so as to prevent the combustion of the sugar of milk) before it was examined by the hydrogen-flame: as the water evaporated, the blue was seen for an

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vulcanized india-rubber tubing was sufficient to raise its absorption several degrees, solely from the impalpable dust removed from the tubing.

instant. But, from the brightness of the colour in the two cases, it would appear that the homœopathic doses of sulphur are exceeded by the quantity we daily inhale with the atmospheric dust.

I must now briefly allude to another phenomenon which was observed early in the foregoing investigation. In addition to the blue colour seen on so many bodies, I found that contact of a hydrogen-flame with some metallic surfaces gave rise to a second appearance succeeding or sometimes accompanying the blue coloration. This was a bright scarlet spot encircled by a green ring of light, forming together a section of the flame. The coloration now mentioned was not nearly so general in its appearance as the sulphur-blue. On tin, or any of its alloys, it was seen brightly, and it also momentarily appeared on any surface which had been rubbed by these bodies. With a piece of thick and tarnished tin-foil a beautiful experiment may be made. Scraping the foil in places, and bringing it into contact with the hydrogen-flame, the blue colour is seen on the dull parts, and the scarlet and green light appears on the bright surfaces, the latter remaining until the foil melts. The colours may sometimes be made to commingle, and a lovely purple is the result. On some clean pieces of lead the same appearance was noticed; but here it soon faded away, and was probably only caused by the impurity of the metal. On clean zinc it could not be seen; but after a piece of tin plate had been drawn over its surface it appeared for an instant. Like the blue coloration, this scarlet appearance could not be obtained by exposing tin to a coal-gas or alcohol-flame, nor to that of the oxyhydrogen blowpipe. An examination of this colour through a spectroscope will, I trust, in a future investigation, throw more light on its nature.

I may mention in passing, that I found many of the solids examined phosphoresced brilliantly when held for a short time in contact with the hydrogen-flame. This was the case with marble, lime, selenite after it had been calcined by the flame or with plaster of Paris, granite, sand-paper, and with different kinds of writing-paper. On sand-paper, if the hydrogen be suddenly extinguished, a phosphorescent image of the flame is observed. Chalk gives a brilliant greenish light, which changes into a still brighter yellow as lime is formed, the mass glowing throughout on the continuance of the flame.

The combustion of the hydrogen itself offers some points worthy of further investigation. When burnt in an inverted vessel, it is generally accompanied by the blue colour, which is seen around the upper part of the flame where this is in contact with the sides of the vessel. If, however, the hydrogen be mixed with about a third its bulk of air, the blue colour becomes much fainter, and a dark space is observed above the burning hydrogen surmounted by a flame of a light green colour. The greenness is seen very well by employing a long test-tube, and slowly bringing the mouth upwards as the hydrogen burns. Mr. Waldie, in a paper "On Combustion and Flame," published in the Philosophical Magazine for August 1838, remarks that "one peculiarity in the flame of hydrogen must have been frequently observed, namely, a green jet inside of the usual dark central portion; and when the flame is full, a dark central part even in this." No explanation of this green colour is given by the writer; but it is, I believe, quite distinct from that which I remarked. The appearance seen by Mr. Waldie I have frequently noticed, and is probably caused by impurities in the gas. In connexion with the hydrogen-flame, however, there is another curious appearance, which possibly has often been observed—namely, a kind of luminous stream to be seen stretching itself some distance above the tip of the flame when the gas burns in a darkened room.

After the results detailed in the foregoing paper had been obtained, it occurred to me that it would be well to try the effect of bringing gases as well as solids and liquids into contact with the hydrogen-flame. I tried carbonic acid, and found that the presence of this gas instantly changed the colour of burning hydrogen from a yellowish brown to a pale lilac, the whole of the hydrogen-flame being here tinged. The same change in colour may be seen by allowing the products of combustion from a small and lightless gas- or alcohol-flame to pass through a jet of burning hydrogen. A very small quantity of carbonic acid imparted this colour to the hydrogen; for it may be produced by merely breathing on the flame. Beyond the fact just noticed, I have as yet made no additional experiments: the explanation of the phenomenon is therefore merely conjecture; but it is possible this lilac-blue colour may be due to the hydrogen-flame rendering incandescent the molecules of carbonic acid—or, more probably, to a partial decomposition of the gas, and combustion of the carbonic oxide.

XLIV. *On Gravity and Magnetic Inclination.*
By PLINY EARLE CHASE, *M.A., S.P.A.S.*[*]

A SOMEWHAT critical survey of the anomalies presented by the magnetic inclination, to which I referred in a former article (Phil. Mag. S. 4. vol. xxx. p. 191), has given me

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[*] From Silliman's American Journal for September 1865.

renewed reasons for regretting the want of a complete record of the investigations on which Professor Secchi based his conclusion, that *all* "the phenomena hitherto known of the diurnal magnetic variations may be explained by supposing that the sun acts upon the earth as a very powerful magnet at a great distance"[*]. As I know of no magnetic law which will account for those anomalies, I propose briefly to describe them, and to point out some relations between the gravitation-currents and the *dip* of the needle, as a sequel to my papers on the influence of gravity upon the total magnetic force and the magnetic declination.

General Sabine's discussions have shown some important points of difference between the magnetic disturbances at intertropical and extratropical stations, the Cape of Good Hope being magnetically, though not geographically, intertropical. In the third volume of the 'Toronto Observations,' and in Professor Bache's discussions of the observations at Girard College, projections of the daily and semiannual inclination-curves are given[†]; and plate 5 of the second volume of the 'Hobarton Observations' contains a graphical representation of the diurnal variations of the inclination at the different observation-hours in the four seasons. If we also project, from General Sabine's Tables of the mean results, the daily and semiannual curves at St. Helena and Cape Colony, and compare the curves at the five stations, it will be found that,

1. The greatest daily disturbance of inclination occurs about noon.
2. At (magnetically) intertropical stations the dip is diminished, but at extratropical stations it is increased in the middle of the day.

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[*] Phil. Mag. S. 4. vol. ix. p. 452. Faraday (Experimental Researches, vol. iii. p. 493) states "that the celebrated Professor Gazzaniga, starting from his numerous experiments, which demonstrate the influence of magnetism upon the same aërial fluids in a manner therefore different from that of Bancalari, was induced to consider the sun and all the other celestial bodies as so many enormous magnets; by which he established that attraction is merely one effect of the magnetism of the great celestial masses placed at an enormous distance—an idea which reappeared in 1846 in Prussia, and in 1847 in France."

While admitting the intimate relationship of magnetism and gravity, I must dissent from the learned Professor's inference. For the evidence appears irresistible, that the earth's magnetism is directly dependent on the terrestrial gravitation of the thermally disturbed aërial currents, and that it is only slightly affected by the perturbations of solar and lunar gravitation; so that if we regard the relationship as a causal one, magnetism rather than attraction should be considered the effect. (See Gauss, "General Theory of Terrestrial Magnetism," §§ 30, 40, Taylor's Scientific Memoirs, vol. ii. p. 232.)

[†] By the kindness of Professor Henry I have been permitted to refer to the proof-sheets of the Fourth Section of Professor Bache's 'Discussions.'

3. Increasing temperature and increasing solar altitude augment the inclination-disturbance. This is shown both by the diurnal and the semidiurnal curves.

4. As a corollary of propositions 2 and 3, at St. Helena and Cape of Good Hope the inclination-disturbance is opposed to, and subtracted from the normal dip; but at Philadelphia, Toronto, and Hobarton the disturbance is added to the dip. Thus the inclination is—

A minimum at	St. Helena . . .	at 22–23^{h}.
,,	Cape of Good Hope	,, 0– 1^{h}.
A maximum at	Philadelphia . .	,, 22– 0^{h}.
,,	Toronto	,, 22–23^{h}.
,,	Hobarton	,, 23– 1^{h}.

Reasoning either *à priori,* or from Secchi's postulates, we might naturally infer that the ellipticity of the atmosphere would be increased by the direct action of the sun, and that consequently, from the tendency of magnetic parallelism to the gravitation-currents, the dip would be augmented at noon in all places between the magnetic equator and the magnetic poles.

Mr. William Ferrel, in his paper* which furnished the first satisfactory explanation of the barometric depression at the equator and at the poles, shows that, in consequence of the earth's motion, fluids tend to assume a form similar to his fig. 1 (Math. Monthly, vol. i. p. 215), "*the surface of the fluid being slightly depressed at the equator, having its maximum height about the parallel of* 35°, *and meeting the surface of the earth towards the poles.*" The direct action of the sun, in increasing the equatorial ellipticity of the air, may also increase the tendency to equatorial and polar depression; and the magnetic parallelism may therefore be manifested in the solar-diurnal inclination-disturbance *precisely as it is manifested* at St. Helena and Cape of Good Hope, by a diminution of dip between the parallels of 35°, and an increase in higher latitudes.

These coincidences may well suggest the need of more extensive observations in different parts of South America, Northern Africa, Southern Asia, the Pacific Ocean, and the Frigid Zones, to furnish the data for determining to what extent mountain-ranges, coast-lines, land and water radiation, winds, and ocean-currents modify the theoretical phenomena of dip and declination.

By projecting on isoclinal and isogonic charts† the magnetic currents as indicated by the position of the needle in different

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* "The Motion of Fluids and Solids relative to the Earth's Surface." See 'Nashville Journal of Medicine and Surgery' for 1856, and 'Mathematical Monthly' for 1859, vol. i. p. 140 *et seq.*

† The charts that I used were the polyconic projection of the "Lines of equal Magnetic Variation for the year 1858," which was constructed from

portions of the globe, I have obtained the following results, which serve to show the character of some of these modifications:—

I. *Declination.*

1. The currents manifest a tendency to follow the lines of most direct ocean communication between the warmest and the coldest portions of the globe, the general declination being westerly in the Atlantic, and easterly in the Pacific Ocean.

2. The lines of no variation are apparently determined in part by the contours of the land* that divides the waters of the globe into two great bodies.

3. The currents are deflected by the southern pointed extremities of the several continents toward the east on the eastern shores, and toward the west on the western shores, of New Holland, Africa, and South America.

4. The magnetic currents, in the three respects above enumerated, exhibit *a precise parallelism to the ocean tidal flow.*

5. In the comparatively narrow belt of the Atlantic Ocean, the declination between the parallels of 35° reaches 30°; in the broad expanse of the Pacific, the maximum within the same limits of latitude is 15°.

6. Between the parallels of 70° and 80° the declination becomes very great, and the currents tend to a gyratory or cyclonic motion, which appears to be modified by glaciers or local poles of great cold.

7. The cyclonic tendency is most marked in the southern hemisphere, where the ocean waters experience the least interruption to their normal motions. Mr. Ferrel's computation gives "28° 30′ for the polar distance of the parallel where the surface of the fluid or the stratum of equal pressure meets the surface of the earth"†. It will be readily seen that at that distance the south pole is entirely surrounded by water; and it seems therefore highly probable that, if there were no land in the frigid zones, there would be no definite magnetic poles, but a simple polar belt toward which the magnetic currents would flow in parallel spirals‡, except when deflected by land radiation, or tidal or other gravitation-currents.

Evans's English Admiralty Chart for the United States Coast Survey Report of 1859, and Nos. 1 and 2 of the 'Admiralty Manual for ascertaining and applying the deviations of the Compass caused by the iron in a ship,' second edition, 1863.

[*] The influence of coast-lines in producing a tendency to equality of declination is beautifully shown in the United States Coast Survey Chart of "Lines of equal Magnetic Declination on the Gulf of Mexico for the epoch 1860·0."

[†] *Loc. cit.*

[‡] This inference accords with Barlow's conclusion, that every place has its particular polarizing axis.

II. *Dip.*

8. The lines of equal dip are arranged in approximate parallels around the two (principal) magnetic poles.

9. In consequence of this parallelism they are convex toward the north in the Pacific Ocean, and toward the south in the Atlantic Ocean.

10. The magnetic parallels also approximate to the isothermal parallels, both in direction and in position, but with some important departures.

11. In South America the magnetic equator is depressed nearly 30° south of the isothermal equator; it is, however, nearly equidistant from the (principal) north and south magnetic poles.

12. The magnetic parallels near the magnetic poles are more convex than the isothermal lines, but they present some interesting instances of parallelism to the ocean-currents, which are indisputably gravitation-currents.

13. This parallelism is specially observable in the regions of the equatorial currents, the Gulf-stream, and the North Pacific and Japan currents.

14. If a series of waves were propagated through the air from the magnetic poles to the equator, with slight deflections by the continental contours and ocean currents, they could be represented with great accuracy by the magnetic parallels.

All of the foregoing modifications, as well as the theoretical phenomena previously referred to, have their exact counterparts in the currents which tend to restore the gravitation-equilibrium of the air and water. It may be unwise, ignorant as we are of the true nature of causal efficiency, to assert that one form of force is produced by another, but it is one of the most important duties of true philosophy to observe sequences and correlations. It has long been known that magnetic action may be so directed as either to assist or counteract the attractions of cohesion, chemical affinity, and gravitation: it has also been known that, under certain circumstances, disturbances of chemical or of cohesive attraction are accompanied by magnetieal disturbances*; but I have now shown for the first time, by independent examinations of the total force, declination, and dip, that disturbances of gravitation are similarly attended.

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* "A few years ago magnetism was to us an occult power, affecting only a few bodies; now it is found to influence all bodies, and to possess the most intimate relations with electricity, heat, chemical action, light, crystallization, and, through it, with the forces concerned in cohesion; and we may, in the present state of things, well feel urged to continue our labours, encouraged by the hope of bringing it into a bond of union with gravity itself." (Faraday, 'Experimental Researches,' 2614.)

It would certainly be very satisfactory, if it were possible, to have some means of exhibiting, by simple laboratory experiments, the direct and mutual convertibility of gravitation and magnetism; but I fear the attempt to reproduce, in any appreciable mechanical form, the magnificent and daily repeated operations in the laboratory of nature which I have feebly endeavoured to interpret must always be futile. In order to obtain even the small amount of disturbance (·00134) which I have noted in the half-daily variation of atmospheric weight (Trans. Amer. Phil. Soc., vol. xiii. p. 121), it would be necessary to take observations at two stations, one of which should be 2·655* miles more distant from the earth's centre than the other. If the differences of vapour, temperature, barometric pressure, force and direction of wind, atmospheric electricity, &c. did not so complicate the problem as to discourage even the most sanguine experimenter from any attempt at solution, any result that could be obtained under such circumstances would give little general satisfaction.

It is possible, however, that the end, which we should vainly strive to reach directly, may be indirectly attained. Indeed the various stages of an indirect road have long been known, but we have not been able to compare them by any common measure. The motion of gravity, by percussion or the obstruction of simple fall, has been repeatedly converted into the motion of heat; and the motion of heat by the thermo-electric pile has been converted into the motion of magnetism. The experiments of Barlow, Coulomb, Kupffer, and Christie† on the influence of heat upon the magnet, furnish data that may lend some aid to any investigator who seeks to ascertain the precise value and modification of each force, in these successive conversions.

But I look most hopefully to researches that are based upon

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[*] $R\times(\sqrt{D'}-\sqrt{D})=3963\times(\sqrt{1\cdot00134}-1)=2\cdot655$. At Singapore the daily disturbance of total force is only $\frac{31}{43}$ as great as it should be theoretically. I suspect that the discrepancy is owing mainly to the monsoons and other great temperature-disturbances of the station, which shift the lines of force by a kind of conduction polarity. (Faraday, 'Experimental Researches,' 3279.) In other important respects there is a satisfactory correspondence between Singapore and St. Helena. *E. g.*:—

	Mean fall.	Mean tide (Theoretical $\times\frac{31}{43}$).		
		0 h.	6 h.	12 h.
	h m			
Theoretical	8 29	−·00031	−·000173	+·00064
Observed	8 23	−·00031	−·000180	+·00064

(See 'Numerical Relations of Gravity and Magnetism,' Sections V., IX., and Faraday's Experimental Researches, III. 321–2.)

[†] See *Encyclopædia Britannica*, eighth edition, vol. xiv. pp. 35–39.

differences of specific gravity. Even the experiments of Barlow and others, to which I have just referred, as well as the electro-magnetic currents which are generated by chemical solution, involve such differences; the thermal aërial currents which harmonize with and increase the effects of simple gravitation toward the sun, are caused solely by the greater centripetal tendency of the cold dense air, which has the greatest specific gravity; and the recent investigations in thermodynamics, together with the experiments of Fusinieri and Peltier[*], confirm the natural conviction that the imponderable agents can only be manifested through their influence on ponderable matter, and therefore under tendencies to equilibrium with the force of gravitation. I already find a curious approximate coincidence, to which I attach little importance so long as it is unsupported by corroborative evidence; but I refer to it as an indication of the very character that we might reasonably expect, and one that may possibly become valuable in the course of future research. The last edition of the *Encyclopædia Britannica,* article "Heat," gives for the expansion and consequent diminution of specific gravity between 32° and 212° Fahr., of

Iron	$\frac{1}{819}$
Air	$\frac{3}{8}$

If we suppose their specific magnetisms to be inversely proportioned to the disturbance of their specific gravities, we have, assuming the specific magnetism of iron as the unit,

$$\tfrac{3}{8} : \tfrac{1}{819} :: 1 : \tfrac{1}{307},$$

a value which is intermediate between those given by MM. Becquerel ($\frac{1}{382}$) and Plücker ($\frac{1}{287}$)[†]. This result would be somewhat modified by an accurate determination of the ratios of the linear to the cubic expansions of iron in its several forms.

Faraday disclaims the assumption of any other than a conduction polarity of oxygen (2933, 2934); but that polarity is conveyed in lines strikingly analogous to the thermal gravitation-currents (see Experimental Researches, 2787, and III. plate 4, fig. 6), which, in their turn, accurately represent the hypothetical indirect action of the sun on the needle, through the atmospheric affection of the lines of force (2936).

I know of no physicist who has given so lucid a theoretical explanation of the various magnetic perturbations as the illustrious Fullerian Professor; and as his hypotheses appear to me

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[*] Taylor's 'Scientific Memoirs,' vol. iii. p. 394.

[†] Professor Frazer has kindly referred me to the coefficients of dilatation for iron, in the 'Artizan' of December 1, 1860, and to the experiments of Regnault on the dilatation of air (see Journ. of Frankl. Instit. S. 3. vol. xv. p. 281). According to these data, the theoretical specific magnetism of oxygen would be between $\frac{1}{294}$ and $\frac{1}{374}$.

still more satisfactory when viewed in connexion with the gravitation-disturbances, I will briefly refer to some of the more important points that have helped to confirm me in the various views that I have hitherto advanced. Such are, *e. g.*, the evidences of the identity of helices and magnets (2239); the existence of magnetic repulsion without polarity (2274); the relative magnetic position of gases and vapours (2416); the relations of the magnecrystallic, cohesive, and magnetic forces (2479, 2562, 2578); the probable dependence of the magnetic motions of fluids upon their mass and density (2768, 2769, 2781, 2863); the magnetic influence of winds and varying atmospheric pressure (2952, 2954)*; the supposed velocity of magnetic transmission in space or æther (2958); the daily bipolar minima of cold (3006); the closed circuits and prominent characteristics of the magnetic lines of force (3117, 3278, 3279, 3284); the tendency of all bodies to evolve electric currents when moving in a magnetic field (3337); the relative diamagnetic and magnetic effects of heat and cold upon air and the diamagnetic gases, as well as upon iron, nickel, and cobalt (2861, and III. pp. 446, 460, 464, 472, 473, 489, 490).

It should not be forgotten that there is no such thing within the compass of our observation as "potential" gravity, no instance of matter in absolute rest, and just beginning or tending to move under a gravitating pull. Every particle of the earth, independently of the action of heat, chemical affinity, and cohesion, is at every instant subjected to four principal and important impulses—two toward the centres of the sun and earth respectively, and two tangential to the earth's orbit and to its circumference. Of the several motions, the orbital one is by far the most important. Next in point of *velocity* is the one tangential to the circumference; in point of *intensity*, the one toward the centre of the sun. Since the solar central and tangential motions are *in æquilibrio*, it seems eminently proper that the others should be regarded as disturbances, which tend, as I have elsewhere shown, to give a daily ellipticity to each section of air parallel to the equator. Not only are the barometric daily tides a necessary consequence of such ellipticity; owing to the difference of specific gravity, the cold air, in addition to the proper motion of convection, is alternately drawn toward and repelled from the earth's surface; and I am inclined to believe that many of the phenomena of the deposition of dew and the magnetic perturbations, which cannot be explained by other more important gravitation-currents, may be thus accounted for.

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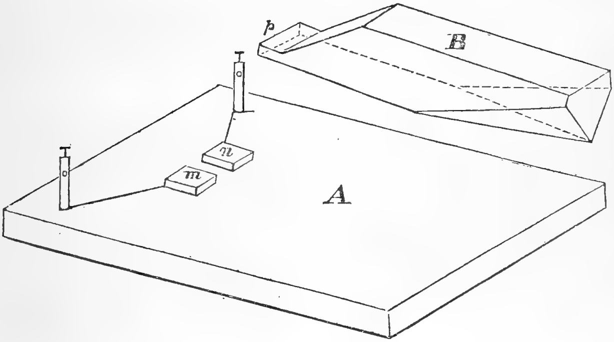
* Humboldt speaks of the accumulation of electricity in the lower equinoctial regions, "at the maximum of heat, and when the barometric tides are near their minimum." (Taylor's 'Scientific Memoirs,' vol. iii. p. 398.)

XLV. *On the Thermo-electric Tension of Minerals.*

By WALTER FLIGHT, *D.Sc.*[*]

PROFESSOR BUNSEN[†] has already called attention to the strongly electro-negative properties of copper pyrites and pyrolusite, with respect to copper or an alloy of antimony and zinc, and to the consequent value of these minerals for the construction of a sensitive thermo-electric pile. In order to complete these investigations, experiments were made with all procurable minerals which conduct electricity, for the purpose of ascertaining how they comport themselves in this respect.

For this purpose the following apparatus was used:—Two square pieces of thick platinum-foil, *m* and *n*, 10 millims. in length, were firmly nailed with sprigs made of platinum wire to the board A, leaving a groove between them 1 millim. in width.



Each of these plates pressed tightly against a platinum wire, the other ends of which communicated with their respective binding-screws. To the binding-screws were fastened the wires of the galvanometer.

A piece of firebrick was sawn into the shape B, 21 millims. wide and about 120 millims. long. Round its narrow end (*p*) was bound a strip of platinum-foil 10 millims. in width.

To test any given mineral by means of this apparatus, a piece about the size of a small pea, thoroughly cleaned, was placed on the platinum plate *n*, which we will call the test-plate, and a piece of antimony of the same size on the other plate *m*. The firebrick holder was then taken in the hand, and its platinum end *p* heated in the Bunsen flame; it was then rested on the two sub-

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[*] Communicated by the Author.

[†] Poggendorff's *Annalen*, vol. cxxiii. p. 505 (1864).

stances, and the *first* instantaneous deflection of the needle observed. A piece of bismuth was then substituted for the antimony, and the experiment repeated. We ascertain from the direction of the needle in this second experiment whether the mineral in question lies between bismuth and antimony, or beyond one of these metals in the series. Each mineral, in its turn, was placed on the test-plate and treated in like manner with the above two metals and the minerals which had been previously examined till its position in the series was discovered.

In this manner the substances mentioned in the following Table were treated; each one is electro-negative towards those below it:—

Red iron ore (two specimens from the same locality examined).
Copper pyrites, No. 1 (locality unknown).
Copper pyrites, No. 2 (Worthing copper-mine, South Australia).
Galena.
Iron pyrites, No. 1 (an irregularly shaped piece).
Selenide of lead.
Pyrolusite.
Copper pyrites, No. 3 (Rammelsberg near Gosslar. Very pure).
Psilomelane.
Pitchblende.
Manganite.
Braunite.
Copper pyrites, No. 4 (Dillenburg. Contains 27 per cent. iron pyrites).
Telluride of bismuth (Schemnitz, Hungary).
Iron pyrites, No. 2 (a small cube).
Melted copper pyrites, No. 4.
Peacock copper ore.
Arsenical iron.
Tinstone.
Magnetic iron ore.
Melted copper pyrites, No. 1.
Melted copper pyrites, No. 3.
Bismuth (artificially prepared).
Melted copper pyrites, No. 2.
Cobalt speiss.
Meteoric iron (Atacama).
Kupfernickel, No. 1.
Nickel-antimony glance (Müssen near Siegen).
Melted kupfernickel, No. 1.
Kupfernickel, No. 2.
Silver (artificially prepared. Chemically pure).

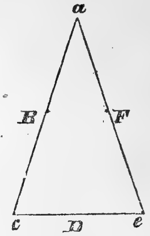
Melted galena.
"Spiegeleisen."
Meteoric iron (Aix-la-Chapelle).
Alloy of antimony and copper.
Graphite, No. 1 (very compact; used for the electric light).
Melted magnetic pyrites.
Cobalt pyrites (Müssen near Siegen).
Native arsenic.
Nickel glance (Ems, Nassau).
Antimony.
Magnetic pyrites.
Melted copper glance, No. 1.
Sulphuret of iron (for the preparation of sulphuretted hydrogen).
Melted iron pyrites, No. 1.
Copper glance, No. 1 (two specimens from the same locality).
Alloy of two parts antimony and one part zinc.
Graphite, No. 2 (native).
Subsulphide of copper, No. 1.
Copper glance, No. 2 (Cornwall).
Subsulphide of copper, No. 2.
Cobalt glance.
Tellurium, No. 1 (chemically pure).
Tellurium, No. 2 (prepared by Professor von Liebig).
Arsenical pyrites.
Fahlun ore, No. 1.
Fahlun ore, No. 2 (small specimen).
Copper glance, No. 3 (Bristol County, America. Four specimens from the same locality examined).
Fahlun ore, No. 2 (larger piece from the same locality).
Melted copper glance, No. 3.

For those substances which lie beyond bismuth on the one side and antimony on the other, I employed, with scarcely an exception, a galvanometer with a great number of coils of thin wire; whilst for those minerals which are situated between the above-mentioned metals, it was necessary to use an instrument with only a few coils of thicker wire.

Besides those mentioned in the above series, many other minerals were tested without yielding any noteworthy results. Although pyrolusite, manganite, and Braunite are mentioned above, Hausmannite gave no current. Amongst ores containing iron, specular iron ore, brown iron ore, chrome iron ore, spathic iron ore, and Lievrite caused no deflection of the needle. Zinc-blende was also tested, but it did not conduct electricity. Other ores, again, such as those containing the sulphides of antimony and silver, were precluded by reason of their easy fusibility.

XLVI. *On a Triangle in-and-circumscribed to a Quartic Curve.* *By* Professor CAYLEY, *F.R.S.*[*]

THE quartic curve $(x^2-a^2)^2+(y^2-b^2)^2=c^4$ presents a simple example of a triangle in- and-circumscribed to a single curve, viz. such that each angle of the triangle is situate on, and each side touches, the curve. Assuming that the triangle is symmetrically situate in regard to the axis of y, viz. if it be the isosceles triangle ace, the sides whereof touch the curve in the points B, D, F respectively, then we must have a single relation between the constants a, b, c of the curve; or if (as may be done without loss of generality) we write $a=1$, then there must be a single relation between b and c. The relation in question is most conveniently expressed by putting b and c equal to certain functions of a parameter ϕ, which is in fact $=\tan^2\theta$, if θ be the angle at the base of the triangle; the equation of the curve is thus obtained in the form



$$(x^2-1)^2+\left(y^2-\frac{\phi^4+4\phi^2-1}{4\phi\,(\phi^2+1)}\right)^2=1+\frac{(\phi^2-1)^4}{16\phi^2(\phi^2+1)^2};$$

and the coordinates of a, c, e, B, D, F are as follows:—

Coordinates of	a are	$0,$	$\sqrt{\tfrac{1}{2}\phi},$
,,	c, e, ,,	$\pm\sqrt{2},$	$-\sqrt{\tfrac{1}{2}\phi},$
,,	D, ,,	$0,$	$-\sqrt{\tfrac{1}{2}\phi},$
,,	B, F ,,	$\pm\dfrac{\phi^2-1}{\sqrt{2}(\phi^2+1)},$	$\dfrac{2\sqrt{\phi}}{\sqrt{2}(\phi^2+1)}.$

It is easy to verify that the points a, c, e, D are points of the curve, and it is obvious that the tangent at D is the horizontal line ce. It only remains to be shown that B and F are points of the curve, and that the tangents at these points are the lines ac and ea respectively. It is sufficient to consider one of the two points, say the point F; and taking its coordinates to be

$$\xi=\frac{\phi^2-1}{\sqrt{2}(\phi^2+1)},\quad \eta=\frac{2\sqrt{\phi}}{\sqrt{2}(\phi^2+1)},$$

we have to show that (ξ, η) is a point of the curve, and that the equation of the tangent at this point is $X\sqrt{\phi}+Y=\sqrt{\tfrac{1}{2}\phi}$, where (X, Y) are current coordinates.

First, to show that (ξ, η) is a point of the curve, the equation

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[*] Communicated by the Author.

to be verified may be written

$$(\xi^2-1)^2+\left(\eta^2-\frac{\phi^4+4\phi^2-1}{4\phi(\phi^2+1)}\right)^2=\frac{(\phi^4+6\phi^2+1)^2}{16\phi^2(\phi^2+1)^2},$$

and we have

$$\xi^2-1=-\frac{\phi^4+6\phi^2+1}{2(\phi^2+1)^2},\quad \eta^2-\frac{\phi^4+4\phi^2-1}{4\phi(\phi^2+1)}=-\frac{(\phi^2-1)(\phi^4+6\phi^2+1)}{4\phi(\phi^2+1)^2},$$

so that the equation becomes

$$\frac{(\phi^4+6\phi^2+1)^2}{4(\phi^2+1)^4}+\frac{(\phi^2-1)^2(\phi^4+6\phi^2+1)^2}{16\phi^2(\phi^2+1)^4}=\frac{(\phi^4+6\phi^2+1)^2}{16\phi^2(\phi^2+1)^2};$$

that is

$$4\phi^2+(\phi^2-1)^2=(\phi^2+1)^2,$$

which is right.

Next, the equation of the tangent at the point (ξ, η) is

$$(\xi^2-a^2)(\xi X-a^2)+(\eta^2-b^2)(\eta Y-b^2)-c^4=0;$$

that is

$$(\xi^2-1)(\xi X-1)+\left(\eta^2-\frac{\phi^4+4\phi^2-1}{4\phi(\phi^2+1)}\right)\left(\eta Y-\frac{\phi^4+4\phi^2-1}{4\phi(\phi^2+1)}\right)$$
$$=\frac{(\phi^4+6\phi^2+1)^2}{16\phi^2(\phi^2+1)^2};$$

or, substituting for $\xi, \eta,$ $\xi^2-1,$ and $\eta^2-\frac{\phi^4+4\phi^2-1}{4\phi(\phi^2+1)}$, their values, and throwing out a factor $\frac{\phi^4+6\phi^2+1}{16\phi^2(\phi^2+1)^2}$, the equation becomes

$$-8\phi^2\left(X\frac{\phi^2-1}{\sqrt{2}(\phi^2+1)}-1\right)-4\phi(\phi^2-1)\left(Y\frac{2\sqrt{\phi}}{\sqrt{2}(\phi^2+1)}-\frac{\phi^4+4\phi^2-1}{4\phi(\phi^2+1)}\right)$$
$$=\phi^4+6\phi^2+1,$$

or, what is the same thing,

$$-8\phi^2\{X(\phi^2-1)-\sqrt{2}(\phi^2+1)\}$$
$$-(\phi^2-1)\{Y.8\phi\sqrt{\phi}-\sqrt{2}(\phi^4+4\phi^2-1)\}=\sqrt{2}(\phi^2+1)(\phi^4+6\phi^2+1);$$

that is,

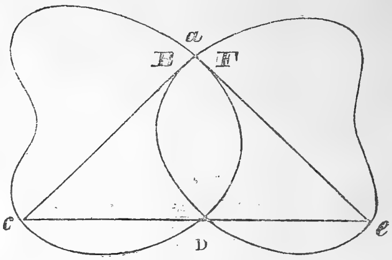
$$(\phi^2-1)(-8\phi^2X-8\phi\sqrt{\phi}Y)$$
$$=\sqrt{2}(\phi^2+1)(\phi^4+6\phi^2+1)-\sqrt{2}(\phi^2+1)\,.\,8\phi^2-\sqrt{2}(\phi^2-1)(\phi^4+4\phi^2-1)$$
$$=\sqrt{2}(\phi^2+1)(\phi^2-1)^2-\sqrt{2}(\phi^2-1)(\phi^4+4\phi^2-1)$$
$$=\sqrt{2}(\phi^2-1)\{(\phi^4-1)-(\phi^4+4\phi^2-1)\}$$
$$=-4\sqrt{2}(\phi^2-1)\phi^2,$$

whence, finally,

$$X\sqrt{\phi}+Y=\sqrt{\tfrac{1}{2}}\phi,$$

which is the required equation.

It may be remarked that for $\phi=1$, the equation of the curve is $(x^2-1)^2+(y^2-\frac{1}{2})^2=1$, which is the binodal form $a^2>b^2$, $c^4=a^4$. We have in this case $\xi=0$, $\eta=\sqrt{\frac{1}{2}}$, and the curve and triangle are as shown in the figure, viz. the base $c\,e$ of the triangle, instead of being a proper tangent, is a line through the node D. For any other value of ϕ, the curve consists of an exterior oval (pinched in at the sides and the top and bottom) and of an interior oval; the angles a, c, e lie in the exterior oval, the sides ac, ea touch the interior oval, and the base $c\,e$ touches the exterior oval.



If, to fix the ideas, we assume $\phi>1$, then we have always $c^4>a^4<a^4+b^4$: for $\phi=1$ we have, as appears above, $b^2=\frac{1}{2}$, which is $<a^2$; but for a certain value of ϕ between 3 and 4, b^2 becomes $=a^2$, and for any greater value of ϕ we have $b^2>a^2$. The condition for the equality of a^2 and b^2 is

$$\phi^4+4\phi^2-1=4\phi(\phi^2+1), \text{ or } \phi^4-4\phi^3+4\phi^2-4\phi-1=0;$$

this equation may be written $2\phi(\phi-2)(\phi^2+1)=(\phi^2-1)^2$, and we thence obtain

$$\frac{(\phi^2-1)^4}{16\phi^2(\phi^2+1)^2}=\tfrac{1}{4}(\phi-2)^2;$$

or the equation of the curve is $(x^2-1)^2+(y^2-1)^2=1+\frac{1}{4}(\phi-2)^2$, where ϕ is determined by the equation just referred to. The curve is in this case symmetrical in regard to the two axes; and there are in fact four triangles, each in- and-circumscribed to the curve.

Cambridge, June 16, 1865.

XLVII. *On the Conditions of Stability of thin Films of Liquids; a Report by* Professor PLATEAU *on a Memoir by* Professor LAMARLE *of Ghent.*—Second Part*.

IN the first part of this investigation†, M. Lamarle considered more particularly the theoretical questions connected with the stability of systems of films; in the present part he examines the matter experimentally, by means of skeleton figures of iron wire and the glycerized liquid, or a simple solution of soap. He studies first of all the systems of the seven typical polyhedra, which he had discussed in the First Part,—those, namely, which, disregarding for the moment the question of stability, would be composed of plane films, all meeting in a single point at the centre of the figure, and conforming to the laws I have established. With regard to these systems, he arrives at the same results as those which I have described in my Sixth Series, and shows their connexion with the formulæ deduced in his First Part; but he has obtained in addition, partly by direct experiment, and partly by the help of reasoning and calculation, a series of new results of which I am about to give an outline.

But, to avoid repetitions, it may be pointed out previously that, according to an observation communicated to me by M. van Rees, and mentioned in my Sixth Series, when, after obtaining upon a skeleton its ordinary system of films, the base of the skeleton is again plunged into the liquid and then withdrawn, there is formed in the base a film which afterwards ascends between the films of the system, shutting in a certain quantity of air, and thus causing a closed laminar polyhedron with curved faces to be generated in the middle of the figure. We now proceed to the results obtained by M. Lamarle.

I. *System of the Regular Tetrahedron.*

In the laminar tetrahedron with convex faces, produced in the middle of the figure by the above process: (1) The curvature of the faces is spherical, and consequently the curvature of the edges is circular. (2) The centre of the sphere to which any one of these faces belongs is situated at the opposite summit. (3) The centre of the circumference to which any one of the edges belongs is situated at the middle of the chord of the opposite edge.

II. *System of the Right Triangular Prism with Equilateral Base.*

1. If the side of the base is denoted by a, and the height of

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* From the *Bulletin de l'Académie Royale de Belgique,* S. 2. vol. xx. No. 7.

† See Professor Plateau's Report thereon, *Bull. de l'Acad. Roy. Belgique*, S. 2. vol. xvii. p. 591; and Phil. Mag. Ser. 4. vol. xxviii. p. 206.

the prism is comprised between the typical value $\frac{a}{\sqrt{6}}$ and a value which is either exactly or very nearly indeed equal to $\frac{a}{2}$, it is possible to obtain at will, by proper manipulation, at the middle of the figure resulting from a single immersion, a triangular film parallel to the bases, or a liquid edge parallel to the lateral edges. I had pointed out these two forms, but as belonging respectively to limits of height differing more widely from each other.

2. In order to be able to obtain a triangular laminar prism at the middle of the figure, it is necessary that the ratio between the height of the skeleton and the side of the base should not exceed a certain limit.

3. When this laminar prism is obtained, its faces never have a spherical curvature: for them to assume such a curvature, it would be needful for the lateral edges of this same laminar prism to be diminished so far as to disappear. It is possible to approach as nearly as we choose to this condition; and it may even be attained, but not permanently, for in that case six liquid edges meet at each of the summits of the laminar polyhedron; and this, as I tried to show by experiments, and as M. Lamarle has demonstrated, in the first part of this investigation, involves the instability of the system.

III. *System of the Cube.*

1. The faces of the laminar hexahedron formed at the middle of the figure have a spherical curvature, and the radius of the sphere to which they belong is half as long again as the straight line joining two summits opposed to one of the faces.

2. If the height of the skeleton be made greater than the side of the base, so as to convert it into a right square prism, M. van Rees's process still gives an internal laminar prism with curved faces, provided the height of the prism is not too great; but the faces of this prism never have a spherical curvature.

3. If the height of the skeleton notably exceeds the side of the base, the central film of the ordinary system always presents itself parallel to two of the lateral faces; but if the height exceeds the side of the base by a sufficiently small quantity, this film can be transformed by an appropriate method into one parallel to the base—a transformation analogous to what we have seen above in the case of the system of the triangular prism.

IV. *System of the Right Pentagonal Prism with Regular Base.*

1. Denoting by r the radius of the circle inscribed in the base, I had given, in my Sixth Series, for the typical height of

the prism the value

$$2r \,.\, \sqrt{3} = 2r \times 1{\cdot}732.$$

This value, which I had arrived at by neglecting the very slight curvature of the films composing the system, is not quite accurate; the true value is

$$\frac{2r \,.\, \sqrt{2}}{\sqrt{3-\sqrt{5}}} = 2r \times 1{\cdot}618.$$

2. Here again, for it to be possible to produce a laminar pentagonal prism at the middle of the figure, the skeleton must not have too great a height in comparison with the dimensions of the base. Under these conditions, and provided also that the height exceeds the typical height, the laminar prism will have faces of spherical curvature, if it has a certain determinate volume relatively to the size of the skeleton. This laminar prism will then have a height equal to 21·853 times the chord of the side of its base, and the radius of the sphere to which its faces belong will be equal to 23·072 times the same chord.

3. When the height of the skeleton is only very little less than the typical height, it is possible, by certain manipulations, to obtain at pleasure, in the system of films resulting from a single immersion, either a very small pentagonal film at the middle of the figure, or the other system—that is to say, the one corresponding to the typical height or to a greater height.

V. *System of the Regular Dodecahedron.*

The faces of the laminar dodecahedron produced at the middle of the system have a spherical curvature; the radius of the spheres to which they belong is equal to about 23 times the chord of their side.

In all the above systems with an internal laminar polyhedron, when the faces of the latter have a spherical curvature, all the films which extend from the edges of the skeleton to the edges of this laminar polyhedron are plane; and consequently all the liquid edges which join the angles of the skeleton with those of the same polyhedron are straight.

VI. *Systems of two Special Polyhedra.*

These polyhedra, as well as their systems of films, would require for their proper comprehension either a long description or figures. I will therefore confine myself to saying only a few words about them. The first is composed of two equal and parallel squares, one of which is turned through a quarter of a revolution in relation to the other, and which are connected together by eight half-regular pentagons. In each of these pentagons, the plane containing the side common to one of the squares, and the two adjacent sides, makes a reentering angle of about 168° with the plane which contains the other two sides. This skeleton affords five different systems of films which can be obtained at pleasure; and, in addition, eight different laminar polyhedra can be produced at the middle of them.

The second polyhedric skeleton is still more difficult to describe. It consists of four rectangles and four half-regular pentagons; each of these pentagons is also formed of two parts, of which the planes make a reentering angle with each other. This skeleton gives two different systems of films; and four internal laminar polyhedra can be produced upon it.

M. Lamarle then leaves the typical polyhedra, and developes some general considerations upon the systems of films belonging to other skeletons.

He takes, as an example for investigation in detail, the regular octahedron. By aid of an artifice of reasoning, he arrives *à priori*, and solely by theory, at five different systems of films, which are very probably the only systems possible in the skeleton in question. The first is formed entirely of plane films, and its centre is occupied by the common summit of six spearhead-shaped quadrilateral films, having their sharp points respectively at the six summits of the skeleton; the second is formed by curved films, and has an hexagonal film at the middle. I had previously indicated these two systems: the three new systems found by M. Lamarle, and which are formed of curved films, contain at the middle, one of them a pentagonal film, another a trapezoid four-sided film, and the last an equilateral four-sided film. M. Lamarle is able to obtain these systems at pleasure, and also to cause them to change at pleasure one into another.

He dwells specially upon the first of these five systems—that is to say, upon the one which has plane films only—and shows that the dimensions of its parts are related to each other and to the dimensions of the skeleton in very simple numerical ratios.

As to the internal laminar polyhedra, M. Lamarle obtains fifteen different ones in this same system. He has succeeded in deducing all of them from the theory, and in effecting their derivation, both theoretically and experimentally, from one another.

Lastly, M. Lamarle discusses, still in reference to the same skeleton, systems of a kind which he calls incomplete, inasmuch as they are formed by breaking certain films in a system already formed, and they consequently present vacant spaces. When one of the above-mentioned laminar polyhedra is obtained in the middle of a system of plane films, six small triangular films are formed abutting on this polyhedron; and it is by breaking either two or four of these small films that M. Lamarle obtains the incomplete systems in question. In the case of three of them, resulting from the disappearance of two opposite films, the internal polyhedra which were originally octahedric, are converted into hexahedra of very elegant forms; the disappearance of two more of the opposed small films converts one of these hexahedra into a tetrahedron of a remarkable appearance, on account of the outlines of its faces.

XLVIII. *On Quantoids. By the* Honourable Chief Justice Cockle, *President of the Queensland Philosophical Society**.

1. USING a convenient terminology and notation, we may say that the quantoid

$$a_0(1,\ a_1,\ a_2,\ \ldots\ a_n \between \tfrac{d}{dx},\ 1)^n y = y_n \quad . \quad . \quad . \quad (1)$$

has the quadricriticoid

$$a_1^2 - a_2 + \frac{da_1}{dx} = \Box, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

and the cubicriticoid

$$2a_1^3 - 3a_1a_2 + a_3 - \frac{d^2a_1}{dx^2} = \Delta; \quad . \quad . \quad . \quad . \quad (3)$$

and that if y_n be transformed by the factorial substitution

$$y = u\text{Y} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

into the quantoid

$$\text{A}_0(1,\ \text{A}_1,\ \text{A}_2,\ \ldots\ \text{A}_n \between \tfrac{d}{dx},\ 1)^n \text{Y} = \text{Y}_n, \quad . \quad . \quad (5)$$

there subsist the relations

$$\text{A}_0 = a_0 u, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

and, generally,

$$\text{A}_0\text{A}_m = a_0(1,\ a_1,\ a_2,\ \ldots\ a_m \between \tfrac{d}{dx},\ 1)^m u, \quad . \quad . \quad (7)$$

or

$$\text{A}_0\text{A}_m = a_0 u_m. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

2. These criticoids are, as I have shown (Phil. Mag. S. 4. vol. xxiv. pp. 532 & 533), invariable under the factorial substitution, and we have

$$\text{A}_1^2 - \text{A}_2 + \frac{d\text{A}_1}{dx} = a_1^2 - a_2 + \frac{da_1}{dx} = \Box \quad . \quad . \quad . \quad . \quad . \quad (9)$$

$$2\text{A}_1^3 - 3\text{A}_1\text{A}_2 + \text{A}_3 - \frac{d^2\text{A}_1}{dx^2} = 2a_1^3 - \&\text{c.} = \Delta. \quad . \quad (10)$$

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* Communicated by the Author.

3. Now, designating the differential covariants of which I have already spoken (in the March and September Numbers of this Journal for 1864) as covaroids, the functions y_n, y_m, and $\frac{d^p y_q}{dx^p}$ are covaroids for all values of m, n, p and q, and consequently any functions of $\Box$, Δ, and the higher criticoids, and also of any number of values of y_m and of $\frac{d^p y_q}{dx^p}$ are covaroids, and *vice versâ.* Accordingly the covaroid which I have called the differential Hessian may, after a slight modification, be put under the form

$$\Box y_2 - \left(\Delta + \frac{d\Box}{dx}\right) y_1,$$

and is only one of an infinite number of quadricovaroids. For this particular case (the differential Hessian), if we so determine u as to cause the middle term of the quadricovaroid to disappear, we shall have transformed the quantoid y_3 into a quantoid Y_3, whereof the coefficients will satisfy the relation

$$A_1A_2 - A_3 + \frac{dA_2}{dx} = 0. \quad . \quad . \quad . \quad . \quad . \quad . \quad (11)$$

4. This transformation, the possibility of which is thus manifested *à priori,* leads, when discussed by change of the dependent variable, after expulsion of superfluous terms, to the result

$$\left(a_1a_2 - a_3 + \frac{da_2}{dx}\right) u + 2\Box \frac{du}{dx} = 0, \quad . \quad . \quad (12)$$

which is linear in u. A variety of other possible transformations is thus indicated *à priori.*

5. The symbolical decomposition

$$\left(\frac{d}{dx} + \frac{1}{x}\right)\left(\frac{d}{dx} - \frac{1}{x}\right) = \frac{d^2}{dx^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad (13)$$

is remarkable, and leads to the conclusion that if $a_1, a_2, a_3, \ldots a_{n-2}$ be constant, and

$$a_n = \frac{na_{n-1}}{x} + \frac{nda_{n-1}}{dx}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (14)$$

the equation $y_n = 0$ is depressible by one order. By reversing the symbolical factors on the sinister of (13) we obtain a result which, though slightly more complicated than (14), is still comparatively general.

Brisbane, Queensland, Australia,
August 16, 1865.

XLIX. *An Account of the Water-Barometer constructed and erected by* ALFRED BIRD, *Experimental Chemist, Birmingham.*

[With a Plate.]

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

AT the Meeting of the British Association which has just been held at Birmingham, I had the pleasure to show a water-barometer which has been in perfect action for six years. A general desire having been expressed that some account of the instrument should appear, I have the pleasure to send you the following particulars and drawings.

In the construction of a water-barometer four things have to be attended to:—

1st. The water must be deprived of air;

2nd. The air must not again enter the water;

3rd. The water must go into the barometer, to the exclusion of the air; and

4th. The instrument must be so constructed that, while the atmospheric pressure within the instrument shall be uninterrupted, no air shall penetrate into the vacuum-chamber.

I begin by describing the material. The tube is composed of metal and glass, and the three taps are those which go by the name of "Lambert taps." The size of the metal part is half an inch internal diameter, and is that sort of white-metal tube which is in universal use by gas-fitters, called "compo." I believe it is an alloy of lead and zinc.

I recommend that which is made by Messrs. Stock Brothers and Co., in Birmingham, as their compo tube will stand an internal pressure of fifty pounds of air to the inch without leaking: it is also very cheap. The glass tube to show the "readings" is 1 inch internal diameter and 6 feet long. The brass Lambert taps are half an inch internal diameter. These taps are constructed internally with a cushion of india-rubber, pressed down by means of a brass plate acted upon by a screw, which makes them absolutely secure.

I now proceed to describe the upper and lower parts of the barometer in reference to the drawings. Plate IV. A A is the compo tube, having two enlarged sockets B, B, 1½ inch in diameter and 3 inches deep. These sockets were made of brass, and their office is to receive the ends of the glass tube. To fix the glass tube C, about six inches of the compo tube was soldered to the bottom of the socket, and being inverted and fixed very steady, enough dry sand was poured into the compo tube to fill it up to the *bottom* of the socket B. The using of the sand was to prevent the cement from running into and stopping up the compo tube. The glass tube C, perfectly clean inside, was now placed in the socket; and being most carefully steadied to keep it upright, six inches of dry sand were poured down to keep the cement from rising up the glass tube C.

The cement was composed of two parts of gutta percha and one part of common black pitch. These two substances were heated in an iron ladle with a lip, till they became perfectly fluid and quite free from froth. A "copper bit" used by plumbers having been heated to low soldering-heat, a small quantity of the cement was poured into the socket. The copper bit was then applied to the outside, the effect being to perfectly liquefy the cement *in situ.* A little more of the hot cement was then poured in, and again the heated copper bit was applied till the socket was quite full of very fluid cement without any air-cavities therein. As the cement cooled, it clung to the glass and metal, and became absolutely solid and air-tight. If the cement is poured in *all at once,* it is impossible to prevent crevices, which will let in air when the barometer is filled, causing the water gradually to descend till it falls out of the instrument.

A place being chosen on the staircase of my house, a flat board, 7 feet long and 1 foot wide, was fastened to the wall, upon which board was fixed the socketed glass tube C, and graduated scale F, from the top of which 422 inches were most carefully measured down to the "zero"-point E beside the cistern.

The scale F is to the right of the glass tube. It is made of well-seasoned boxwood, and is graduated to inches and tenths. The sliding-tube G, with the vernier H, is between the glass tube and the boxwood scale F. On the left side of the glass tube C is another sliding-tube *g,* with a vernier *h,* to record position of top of tidal column of water at 9 A.M. the morning previously.

The glass tube, scale, and verniers having been securely placed on the board and perfectly upright, the gas-fitter proceeded to connect, by soldering, the remainder of the compo tube *above* the glass tube C, which was continued upwards till it entered nearly at the bottom into a round vessel K, made of zinc, 4 inches in diameter and 18 inches high. Inside the vessel the tube coils round in a spiral, like the worm of a still. This vessel and spiral are not necessary to the action of the barometer; but as the spiral is in the part of the tube in which is the vacuum-chamber, it gives the opportunity of artificially cooling with ice or snow the included aqueous vapour, and thus determining by actual experiment the amount of correction required.

If the experiment of cooling the included vapour to 32° be tried in summer, when the external temperature is 70° or 80°, the sudden cooling causes so great an evaporation from the surface of the water, and condensation in the upper part of the barometer, that a real rain-shower is produced, the condensed water running down the glass tube in innumerable pellucid drops in the most beautiful manner, thus perfectly imitating the condensation of invisible watery vapour in the higher regions of the atmosphere. When the compo tube leaves the zinc vessel, it is led up perpendicular to the Lambert tap L. Above the tap L the tube still rises perpendicular, when it suddenly bends down, leaving the end open at M.

I now describe the part of the barometer *below* the glass tube.

The compo tube being soldered on, was carried down to the cistern, not necessarily perpendicular; for instance, the tube may descend at an angle of 30° or 40°, and may twist in any convenient direction. The entire instrument erected by me is in the house, to escape a freezing temperature. At the lowest bend of the compo tube is a short upright tube, having at the end a Lambert tap N, to which is soldered a male screw of a $\frac{3}{8}$th-inch gas union-joint O, the use of which will be understood further on. The compo tube now begins to ascend; and at the top of the bend is another Lambert tap P. Beyond this the compo tube bends down and reaches nearly to the bottom of the cistern, which is a one-gallon white-glass narrow-mouth upright bottle R. The bottle rests upon a stand S, which moves up and down by means of a set screw T, acting through a stout shelf U U; and the bottle is kept steady by means of the two uprights W, W, upon one of which is fixed the zero-point E.

I shall now describe the method of filling the barometer, which was as follows.

Four gallons of water were carefully distilled, and being put into *a perfectly clean and new* tin oil-can with a narrow mouth, the water was boiled for one hour over a bright fire, the object being to drive out the air. While still boiling, two quarts of olive oil were poured in. This slightly increased the pressure in the water underneath, causing the last remains of the air to rise with the steam in jets or spirts through the stratum of oil. The instant ebullition was stopped, the oil closed over the boiled water, and it became hermetically sealed from the atmosphere. The contents of the tin can were now cooled, and the can X was placed above the top of the water-barometer. A piece of $\frac{3}{8}$th-inch gutta-percha tube Y Y, sufficiently long to reach from the can X above to below the very bottom of the barometer, was procured, and one end of the tube was put into the mouth of the can X, the end passing through the supernatant stratum of oil down to the bottom of the water underneath. At the other end of the gutta-percha pipe Y is a $\frac{3}{8}$th-inch tap, terminating with a $\frac{3}{8}$th-inch female screw union-joint Z. The gutta-percha pipe being in position, and hanging down as seen in the drawing, became a siphon; and the air being sucked out, the water at once came over, and was stopped from running away by turning the small tap Z. The female union-screw at Z being tightly screwed on to the male screw-joint O, the water was ready to enter the barometer.

The first thing to be done was to displace the air in the bend of the tube, reaching from the tap N at the bottom, to the extreme end of the compo tube in the cistern R. This was done in the following manner:—The cistern or bottle was taken clean away and filled quite full to the very brim with best olive oil; the three Lambert taps being all open, and the bottom end of the "compo" tube hanging down, the small gas-tap Z was opened; the water then began to ascend both legs of the barometer, and when it reached the tap P, it passed over and ran out of the end of the tube which was hanging down. At that instant the stream was stopped with the thumb, and, the tap Z being turned off, the bottle full of oil was brought to the thumb which stopped the end of the compo tube and kept-in the water. The thumb supporting the tube was now put into the oil, and the end of the tube slipped down to the bottom of the oil. The bottle was then put into its place on the stand S, and the surplus oil being siphoned out, there remained in the cistern R about 3 inches in depth of olive oil, the compo pipe dipping into it *nearly* to the bottom.

The next thing was to fill the longer part of the barometer, which was accomplished as follows:—The tap P being closed and the small tap Z opened, the water rapidly rose in the barometer; when the water had reached the opening M at the top, it was allowed to run a minute or two to carry any traces of air away which might have lingered in the tube. Tap L at the top and tap N at the bottom being then securely closed, tap P was opened, and the column of water began to descend and to accumulate in the cistern R *under* the stratum of olive oil. As the column fell it was narrowly watched in the glass tube, but not a bubble of gaseous matter was observed. On examining the cistern R, it was found that the oil did not quite reach the zero-point E, more oil therefore was poured in till the zero-point E and the *level* of the oil were coincident. The graduated scale was now looked at, and it showed that the column of water was 400 inches high, the mercurial barometer being 30·4 inches, and the temperature 67°.

In order to test if gaseous matter would accumulate in the vacuum-chamber, the gutta-percha siphon was allowed to remain in its place for some weeks, and four different times tap P was closed, tap N opened, with tap Z, thus filling the barometer up to tap L at top, which being opened allowed the water and gaseous matter, if there had been any, to flow out at M. On closing tap L and tap N and opening tap P, the column of water again fell; and after siphoning out the surplus water from *under the oil* in the cistern till the oil was level with the zero-point E, the column of water was found on the four different trials to be exactly the same height on the scale after each trial as before. It was therefore plain that no gaseous matter had accumulated above the water, and that, with the exception of the vapour of water, it was a perfect vacuum.

I will now mention one or two precautions which are required in order to ensure success. In the first place the water must be distilled—for this reason, amongst others, that if the water contains "earthy salines" or colouring-matter, it is certain, by the constant evaporation and precipitation in the working part of the glass tube, to crust it over so completely, that in a few months the water becomes invisible; pure distilled water is therefore indispensable. Then, if the slightest leak in the barometer exists, it will infallibly bring the instrument to grief. In order, therefore, to be sure that the barometer was sound (before the water deprived of air was put in), I closed tap L at top and tap P; then, connecting the gutta-percha tube with the "street waterworks" pressure, I allowed it to enter the barometer till the included air was contracted to one-fourth of its length, having a pressure of water under it of between 40 and 50 lbs. to the inch.

The barometer stood this internal pressure for ten hours without the air being forced out. I therefore concluded that if the barometer would stand this great pressure *inside*, it would stand 14 lbs. to the inch pressure on the outside, and without hesitation I filled it with the prepared water.

As the instrument is made by a gas-fitter, it would be easy to put the whole of it together, Lambert taps included, and to *prove it* with some powerful water-pressure *before* the instrument is taken to the place where it is to be erected. Also the water deprived of air and covered with the stratum of olive oil in the tin can could be sent, if necessary, 100 miles away without the possibility of any air getting into it. If a gutta-percha pipe is not to be had to fill the barometer, a piece of compo tube will answer every purpose, which, when done with, is none the worse for gas-fitting purposes.

I shall conclude with some account of the action of the water-barometer. In the Philosophical Transactions for 1832 is a description by Mr. Daniell of a water-barometer which he erected at the "Royal Society's Rooms," at Somerset House, which was in action for two years, but afterwards got out of order. In describing the action, Mr. Daniell states that "the water appears to be in perpetual motion, resembling the slow action of respiration."

I can fully corroborate Mr. Daniell in this particular, and from careful and continued observation am able to state that the times of the oscillations are about every four minutes and twenty seconds. It is requisite to watch the oscillations with a magnifier, as they vary from the twentieth to the thirtieth part of an inch, which distance can be well observed when it is slightly magnified. But the most surprising oscillations in the water-barometer are during a thunder-storm accompanied with great falls of hail and heavy rain-drops. I have given a chart of five minutes' readings for one hour and five minutes during a heavy thunder-storm from the north-east, which passed over Birmingham July 20, 1859. The upper curved line shows the water-oscillations, and the lower curved line shows the oscillations in the mercurial barometer. The temperature is recorded at the foot. It will be observed that while the water-column rose and fell in a most surprising manner, the mercurial column showed hardly any motion, which was of a laggard character.

At 4.20 P.M. the storm reached its climax, the heavens grew darker overhead, and the water rapidly descended, causing a most impressive feeling on the mind, when suddenly came a terrific blaze of lightning instantly followed by the "thunder cannonade" (if I may so call it); then down came the hail and heavy rain, and as the sky began to brighten the water commenced to rise, and in the next five minutes it had risen more than four-tenths of an inch.

Since Sir John Herschel proposed his new theory—that the disruptive electric discharge *is the result*, and not the cause of the sudden condensation of cloud into "rain-drops," in consequence of the cloud coming in contact with an extremely cold and dry current of air—it has occurred to me that the sudden increase in the density of the air, as shown by the rise of the water-column, *may* be due to the *sudden* precipitation of rain-drops of unusual size, leaving the atmosphere drier and consequently denser; it being well established that the mercurial column is always high when the air is dry, and *vice versâ*.

I conclude this account of the water-barometer by stating that the object with which it is written is to give practical directions for its construction with a view to their becoming common for the furtherance of meteorological science.

I just add that the total cost of the materials need not exceed £3, exclusive of gas-fitter's time.

I am, &c.,

Birmingham, October 14, 1865. ALFRED BIRD.

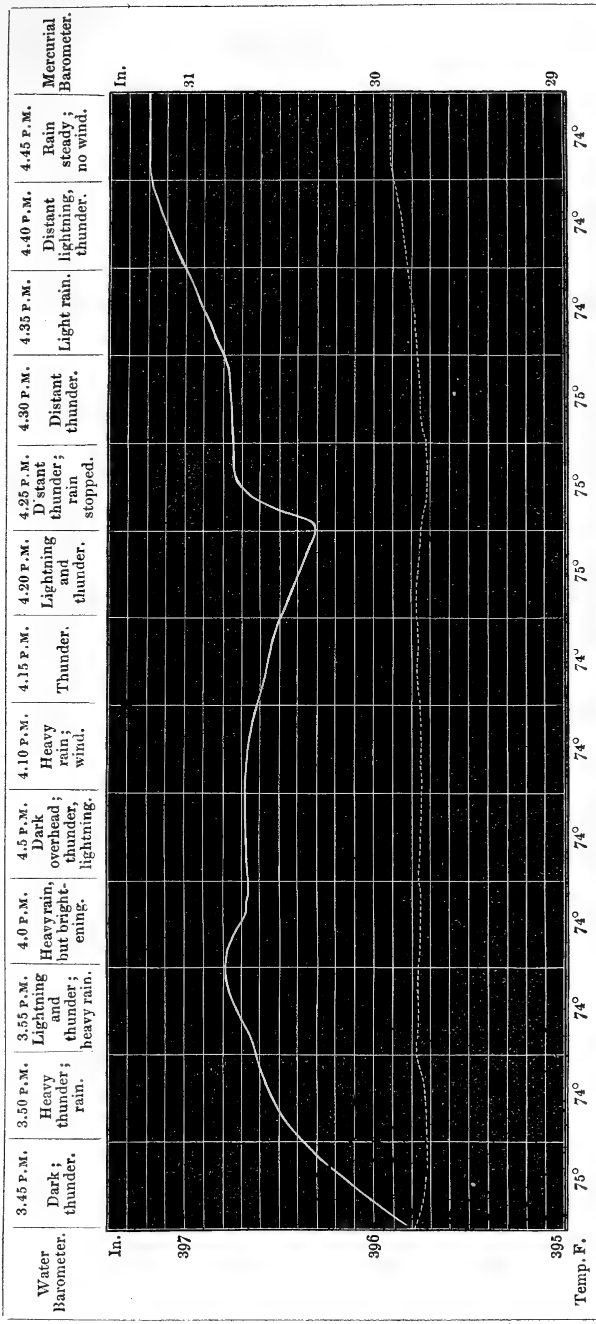

Chart of one hour and five minutes' readings of the Tidal oscillations in the Water-Barometer, during Thunder Storm, July 20th, 1859.

L. *Chemical Notices from Foreign Journals.*

By E. Atkinson, *Ph.D., F.C.S.*

[Continued from p. 258.]

In the expectation of preparing brominated diamylene, $C^{10}H^{19}Br$, Bauer* studied the action of alcoholic soda on bromide of diamylene, $C^{10}H^{20}Br^{2}$. The reaction is very energetic and results in the removal of both atoms of bromine and the formation of a new hydrocarbon, $C^{10}H^{18}$. Thus:

$$\underset{\text{Bromide of diamylene.}}{C^{10}H^{20}Br^{2}} + 2NaH\Theta = 2NaBr + 2H^{2}\Theta + \underset{\text{New hydrocarbon.}}{C^{10}H^{18}}$$

The new body belongs to the acetylene series, and stands in the same relation to diamylene that valerylene† does to amylene. And it is related to rutic acid in the same way that acetylene is to acetic, and valerylene to valerianic acid; hence Bauer proposes to call the new hydrocarbon *Rutylene.*

$C^{2}H^{2}$	$C^{2}H^{4}$	$C^{2}H^{4}\Theta^{2}$
Acetylene.	Ethylene.	Acetic acid.
$C^{5}H^{8}$	$C^{5}H^{10}$	$C^{5}H^{10}\Theta^{2}$
Valerylene.	Amylene.	Valerianic acid.
$C^{10}H^{18}$	$C^{10}H^{20}$	$C^{10}H^{20}\Theta^{2}$.
Rutylene.	Diamylene.	Rutic acid.

It is a colourless liquid, lighter than water, and with a pleasant odour. It boils at about 150° C. By carefully keeping down the temperature it can be made to unite with bromine and form a new compound, $C^{10}H^{18}Br^{2}$, *bromide of rutylene.* This cannot be kept without decomposition. It acts with great energy on alcoholic solution of soda, and on dry acetate of silver; in both cases a hydrocarbon of the formula $C^{10}H^{16}$ is probably formed.

With a view of extending our knowledge of xylole, and of clearing up the very discrepant statements of its properties which exist, Beilstein‡ has undertaken an investigation of this body. By subjecting a large quantity of coal-tar to fractional distillation over metallic sodium, he obtained constant boiling-points at 82°, 111°, and 141°, corresponding respectively to benzole, toluole, and xylole. He thus confirms H. Müller's observations, who finds that xylole boils at 140°.

By oxidation of toluole with chromic acid Hofmann obtained *benzoic acid*; and it was natural to expect that by oxidation of

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* Liebig's *Annalen*, September 1865.

† Phil. Mag. S. 4. vol. xxvii. p. 501.

‡ Liebig's *Annalen*, January 1865.

xylole the homologous *toluylic acid*, $Ꞓ^8 H^8 Ɵ^2$, would be formed. This, however, is not the case: when xylole is oxidized by chromic acid it yields the bibasic *terephthalic acid*, according to the equation

$$Ꞓ^8 H^{10} + Ɵ^6 = Ꞓ^8 H^6 Ɵ^4 + 2 H^2 Ɵ.$$

Fittig and Tollens made the interesting observation* that the mixed radical *methyl-phenyle*, $\left.\begin{matrix} Ꞓ^6 H^5 \\ Ꞓ\ H^3 \end{matrix}\right\}$, is in all respects identical with toluole, $Ꞓ^7 H^8$; and it was natural to expect that *ethyl-phenyle*, $\left.\begin{matrix} Ꞓ^6 H^5 \\ Ꞓ^2 H^5 \end{matrix}\right\}$, which has the same composition as xylole, would be identical with it. But Fittig's observations show that this is by no means the case; there are material differences between the two substances.

Fittig, in conjunction with Glinzer, has recently prepared† another body which has the composition of xylole; it is the mixed radical *methyl-benzyle*, $\left.\begin{matrix} Ꞓ^7 H^9 \\ Ꞓ\ H^3 \end{matrix}\right\}$, and is procured by the action of sodium on a mixture of brominated toluole, $Ꞓ^7 H^7 Br$, and iodide of methyle. Methyl-benzyle boils constantly at 139°, and, both in its physical properties and in the chemical changes which it experiences under the influence of various reagents, it is identical with xylole.

Brigel‡ has prepared benzoyle, the radical of benzoic acid, by treating chloride of benzoyle with sodium. In order to moderate the action, the chloride of benzoyle was dissolved in anhydrous ether, and the sodium used in the form of amalgam. It was ultimately necessary to apply heat to complete the reaction.

The ethereal solution was filtered off from the chloride of sodium and mercury, washed with water to decompose some of the undecomposed chloride, and then, after distilling off the ether, the liquid, being left to itself, deposited some crystals. On analysis these were found to have the composition $C^{14} H^5 O^2$, or rather $\left.\begin{matrix} C^{14} H^5 O^2 \\ C^{14} H^5 O^2 \end{matrix}\right\}$, as its molecular weight must be doubled: the formation of benzoyle is a simple case of the withdrawal of chlorine by sodium:

$$2 C^{14} H^5 O^2 Cl + 2 Na = \left.\begin{matrix} C^{14} H^5 O^2 \\ C^{14} H^5 O^2 \end{matrix}\right\} + 2 NaCl.$$

By acting with an excess of protochloride of iodine on phenylic acid, Schützenberger§ obtains *teriodized phenylic acid*,

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* Phil. Mag. S. 4. vol. xxix. p. 311.

† Liebig's *Annalen*, January 1865.

‡ Ibid. August 1865.

§ *Bulletin de la Societé Chimique*, August 1865.

$Ꞓ^3 H^3 I^3 Θ$. The crude product of the action of the protochloride, treated with soda, is precipitated by dilute sulphuric acid; this precipitate is exhausted by weak alcohol at the boiling-point, which dissolves out the *biniodized* phenylic acid, $Ꞓ^3 H^4 I^2 Θ$, and the residue is then crystallized from strong boiling alcohol. The crystals are yellow, easily soluble in ether, and decompose, when heated, with disengagement of much iodine.

In the hope of getting a more highly iodized compound, the author acted with protochloride of iodine on teriodized phenylic acid. Under these conditions all the iodine became free, and quintichlorophenic acid, $Ꞓ^6 HCl^5 Θ$, was obtained, subliming in magnificent white needles 3 to 4 centimetres in length.

Seekamp* found that when a solution of oxalic acid containing some uranium-salt was exposed to the sun's rays it was decomposed into carbonic acid, carbonic oxide, and formic acid.

Succinic and pyrotartaric acids, which are homologous with oxalic acid, experience an analogous decomposition under the same circumstances. A 5 per cent. solution of succinic acid, to which 1 per cent. of uranium-salt was added, exposed to the sun's rays soon became green; succinate of uranium was deposited as a green powder, carbonic acid was given off, and the completely decolorized liquid furnished propionic acid. Pyrotartaric acid in like manner was decomposed with formation of butyric acid. The decompositions which take place are as follows:—

$$\underset{\text{Succinic acid.}}{Ꞓ^4 H^6 Θ^4} = Ꞓ Θ^2 + \underset{\text{Propionic acid.}}{Ꞓ^3 H^6 Θ^2}$$

$$\underset{\text{Pyrotartaric acid.}}{Ꞓ^5 H^8 Θ^4} = Ꞓ Θ^2 + \underset{\text{Butyric acid.}}{Ꞓ^4 H^8 Θ^2}.$$

Chloroxynaphthalic acid, $Ꞓ^{10} H^5 ClΘ^3$, differs from alizarine, $Ꞓ^{10} H^6 Θ^3$, in containing an atom of chlorine in the place of hydrogen; and many years ago Wolff and Strecker endeavoured, but without success, to replace the chlorine in the above compound by hydrogen, and thus produce alizarine from naphthaline.

Martius and Griess† have succeeded in producing from naphthaline a body of the same composition as alizarine, but entirely different in properties. The starting-point for their investigation was *binitronaphthylic alcohol*, $Ꞓ^{10} H^6 (NΘ^2)^2 Θ$, a substance recently introduced into manufactures, and a description of which is promised. When this body is acted upon by tin and hydrochloric acid, as a direct product of the action a beautiful tin double salt of a new base is produced,

$$Ꞓ^{10} H^6 (NH^2)^2 Θ, HCl\, Sn^2 Cl^2.$$

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* Liebig's *Annalen*, February 1865.

† Ibid. June 1865.

The base appears to be of easily alterable nature, and has not yet been obtained in the free state. If an aqueous solution of the tin-salt is decomposed by sulphuretted hydrogen, and the acid filtrate from the sulphide of tin saturated with potash, while air is excluded no precipitate is formed; but as soon as air is allowed access, oxygen is greedily absorbed and the formation of a yellow crystalline substance takes place. This body is a base having the formula $Ꞓ^{10}H^8N^2Θ$, and forms with acids very well-defined salts. Curiously enough, it is decomposed when boiled with either alcohol or water; the change in the case of water is as follows:—

$$Ꞓ^{10}H^8N^2Θ + H^2Θ = Ꞓ^{10}H^7NΘ^2 + NH^3.$$

The new body crystallizes in beautiful yellowish-red needles; it is an indifferent body, but is decomposed by boiling hydrochloric acid in the following manner:—

$$Ꞓ^{10}H^7NO^2 + H^2O = Ꞓ^{10}H^6Θ^3 + NH^3.$$

This new body crystallizes in yellow needles which are easily soluble in ether. Like alizarine it is sublimed when heated, and gives oxalic and phthalic acids when oxidized, but it differs from alizarine in giving no precipitate with chloride of barium; and cotton with a mordant of alum is not coloured; wool and silk are coloured yellow. Like chloroxynaphthalic acid it forms a series of well-defined salts.

The authors have observed analogous reactions in the phenylic acid group, which they propose following up.

Schützenberger describes* the following process of obtaining methyle-gas. The method of Sir B. Brodie for the preparation of peroxide of acetyle consists in allowing pure precipitated binoxide of barium to act upon cooled acetic anhydride. By using an excess of binoxide and warming the mixture, a large quantity of gas is disengaged with regularity, and at the same time some acetate of barium is formed. The operation may be effected in a 4- or 6-ounce flask provided with a delivery-tube. The gas is a mixture in exact proportions of two volumes of carbonic acid and one volume of a carburetted hydrogen. The former is absorbed by potash, and the carburetted hydrogen left undissolved is found to have the composition and all the properties of the radical methyle, $Ꞓ^2H^6$, the methylide of methyle, $\begin{matrix}ꞒH^3\\ ꞒH^3\end{matrix}$, the preparation of which thus becomes very easy. The reaction, in virtue of which it is formed, is thus written:—

$$\underset{\text{Peroxide of acetyle.}}{2Ꞓ^4H^6Θ^3} + \underset{\text{Binoxide of barium.}}{2BaO^2} = Ꞓ^2Θ^4 + \underset{\text{Methyle.}}{Ꞓ^2H^6} + \underset{\text{Acetate of barium.}}{2Ꞓ^2H^3BaΘ^2}.$$

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* *Comptes Rendus*, September 18, 1865.

Gautier has found* that when gaseous hydriodic acid is brought into contact with gaseous hydrocyanic acid, or is passed into an ethereal solution of this acid, the two substances combine, forming a white amorphous powder, which crystallizes from alcohol or water in transparent needles. This body has the formula $\text{Ꞓ}\,\text{H}^{2}\,\text{N}\,\text{I}$, and the author considers that its formula is $\text{N}\left\{\begin{array}{l}\text{ꞒH}\\ \text{H}\\ \text{I}\end{array}\right.$, that it is an iodide of ammonium in which the triatomic radical ꞒH replaces H^{3}.

Maly† gives the following two methods for the synthetical preparation of formic acid. Sodium-amalgam, besides decomposition of water, acts upon aqueous solution of carbonate of ammonia with formation of ammonium-amalgam; this decomposes with liberation of hydrogen, while at the same time the carbonic acid is in the act of being transferred from the ammonia to soda. To a concentrated aqueous solution of carbonate of ammonia a weak sodium-amalgam was gradually added; and when all the ammonia-amalgam that formed at first was again decomposed, the liquid was poured off from the mercury and distilled with sulphuric acid. The acid distillate thus obtained was found to contain formic acid, the identity of which was established by analysis and by the usual tests.

When a mixture of powdered metallic zinc and of carbonate of zinc is added to hot potash-lye, the nascent hydrogen adds itself to the carbonate of potash which is in course of formation, and formic acid is also obtained,

$$\text{ꞒΘ}^{2}+\text{H}^{2}+\text{KHΘ}=\underset{\substack{\text{Formiate of}\\ \text{potash.}}}{\text{Ꞓ H K Θ}^{2}}+\text{H}^{2}\,\text{Θ}.$$

The conditions for the synthesis of formic acid are thus (1) hydrogen in the nascent state, (2) carbonic acid at the moment of transference, and (3) the presence of a powerful base.

Furfurol exhibits in almost all respects a complete analogy with oil of bitter almonds; by alcoholic potash it is converted into pyromucic acid, as Ulrich has shown. It is probable that in this reaction the corresponding alcohol is formed; and, with a view to the direct preparation of this substance, Schmelz and Beilstein ‡ allowed sodium-amalgam to act upon furfurol, which it does with great energy, and without the disengagement of hydrogen. When the action was over, the reddish-brown liquid was poured

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* *Comptes Rendus*, August 28, 1865.

† Liebig's *Annalen*, July 1865.

‡ Liebig's *Annalen* (Supplement), vol. iii. p. 275.

off from the mercury and treated with sulphuric acid, by which the alcohol in question was deposited as a reddish-brown oil. To purify this oil it was dissolved in alcohol, the alcoholic solution mixed with ether, water gradually added, and the whole well shaken, by which two layers were obtained, one containing a solution of the alcohol of pyromucic acid in ether, and the lower one an aqueous solution of pyromucate of soda. On evaporating the ethereal solution, a reddish liquid was obtained which decomposed on evaporation. By the action of alkalies this alcohol appears readily to pass into pyromucic acid.

When bromine is added to pyromucic acid a violent reaction is set up, accompanied by disengagement of carbonic acid. This reaction is at first moderated by cooling, but its completion requires the aid of heat. It is also necessary that an excess of bromine be added, and that the solution be of the proper degree of concentration. When these conditions are complied with, and the solution is concentrated in the water-bath, it yields crystalline laminæ of a new body, *mucobromic acid*. This is a decidedly acid substance, and forms salts, which, however, are extremely unstable, and have hence not been further examined. Its formation is expressed as follows:—

$$\underbrace{\text{Ꞓ}^5\,\text{H}^4\,\Theta^3}_{\text{Pyromucic acid.}} + 8\text{Br} + 2\text{H}^2\,\Theta = \underbrace{\text{Ꞓ}^4\,\text{H}^2\,\text{Br}^2\,\Theta^3}_{\text{Mucobromic acid.}} + \text{Ꞓ}\Theta^2 + 6\text{HBr}.$$

This reaction only expresses the final decomposition of pyromucic acid; and an oily body is first formed which is perhaps $\text{Ꞓ}^4\,\text{H}^2\,\Theta^3$. The action of excess of bromine on the soda-salt of pyromucic acid is the same as on the free acid.

The decomposition of pyromucic acid by chlorine is quite analogous to that of bromine; it gives rise to *mucochloric acid*, $\text{Ꞓ}^4\,\text{H}^2\,\text{Cl}^2\,\Theta^3$. Iodine is without action upon pyromucic acid.

By comparing the formula of mucobromic acid, $\text{Ꞓ}^4\,\text{H}^2\,\text{Br}^2\,\Theta^3$, with that of mellithic acid, $\text{Ꞓ}^4\,\text{H}^2\,\Theta^4$, it is seen that the latter acid might be formed by the simple replacement of the bromine of the former by oxygen.

By treating mucobromic acid with baryta the bromine was removed, but the decomposition in other respects was different. When the acid is boiled with excess of baryta, air being excluded, carbonate of baryta is deposited, and a gas is liberated which spontaneously inflames in the air, and is *brominated acetylene*, $\text{Ꞓ}^2\,\text{HBr}$; and the solution contains the baryta-salt of a new acid which the authors call *muconic acid*. The formula of this salt is $\text{Ꞓ}^4\,\text{Ba}^2\,\Theta^3 + \frac{1}{2}\text{H}^2\,\Theta$, and the free acid is obtained in crystals from its lead-salt by treatment with sulphuretted hydrogen.

In the formation of this acid the elements of water are concerned, as is expressed in the following reaction:—

$$2C^4H^2Br^2O^3 + H^2O = C^4H^2O^3 + C^2HBr + 2CO^2 + 3HBr.$$

Mucobromic acid. Muconic acid. Brominated acetylene.

In the decomposition of mucrobromic acid by oxide of silver, the final products are the same, but intermediate products are formed which ultimately decompose.

The authors have described several salts of pyromucic acid, and they propose to continue the study of the products of decomposition of mucobromic acid.

When atropine is boiled with baryta-water it is decomposed, as Kraut has found*, into a new acid, *atropic acid,* and a new base, *tropine,* in the manner expressed by the following equation:—

$$C^{34}NH^{23}O^6 = C^{18}H^8O^4 + C^{16}NH^{15}O^2$$

Atropine. Atropic acid. Tropine.

The two new bodies remain in solution as atropate of tropine. After removing the baryta by carbonic acid, hydrochloric acid and ether are added, by which the tropine is dissolved out as hydrochlorate, the acid being left. The free base is obtained by removing the hydrochloric acid by oxide of silver, and the small trace of silver by sulphuretted hydrogen. The aqueous solution evaporated and left over sulphuric acid solidifies to a mass of crystals which have the formula $C^{16}H^{15}NO^2 + HO$. The crystals which form from an ethereal solution have the formula $C^{16}H^{15}NO^2$.

Tropine does not take up carbonic acid when exposed in the air; but it forms with acids a series of beautifully crystallizing salts. When distilled with baryta it is decomposed, forming an oily liquid which contains, besides ammonia, probably methylamine. When tropine is treated with iodide of ethyle in a sealed tube, one atom of hydrogen is replaced by ethyle, and a base, ethyltropine, is formed, $C^{16}H^{14}(C^4H^5)NO^2$.

Atropic acid, $C^{18}H^8O^4$, is isomeric with cinnamic acid, from which, however, it materially differs in physical properties, and in the circumstance that its neutral salts do not precipitate salts of manganese. The atropate of lime is a salt which crystallizes in large plates with a vitreous lustre. By treatment with chromic acid, atropic acid yields benzoic acid; it likewise resembles cinnamic acid in assimilating 2 atoms of bromine.

Lossen has found† that atropine is decomposed by fuming

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[*] Liebig's *Annalen*, vol. cxxviii. p. 280; and vol. cxxxiii. p. 187.

[†] Liebig's *Annalen*, vol. cxxxi. p. 43.

hydrochloric acid, and yields the same products as with baryta-water.

Husemann and Marme* have described the preparation of a new base from the leaves and stems of the *Lycium barbarum*, L. (Chinese Box-thorn). The aqueous decoction of the plant preferably of the leaves) was treated with acetate of lead, and the filtrate from the precipitate treated with sulphuric acid to remove the lead, then neutralized with soda and evaporated. The salts of the new base are so soluble that its precipitation was impossible by the ordinary methods, and a new one was made use of. This consisted in the addition of phosphomolybdate of soda, with which the base forms an insoluble compound. The precipitate produced in this way was decomposed with moist levigated chalk, and the evaporated mass exhausted with boiling alcohol.

This alcoholic extract left, on evaporation, a deliquescent crystalline mass. To obtain the base in the free state, its sulphate is decomposed with carbonate of baryta. The free base crystallizes from water and from alcohol in small plates and prisms; in ether it is almost insoluble. Analyses of its salts give for it the formula $\text{Ꞓ}^5\text{H}^4\text{N}\Theta^2$; it is isomeric with butalanine, the base found by Gorup-Besanez in the salivary glands of the ox; but it differs from it materially; and it most resembles sarcosine, with which it is also homologous. Its poisonous action is inconsiderable. It forms a series of beautifully crystallizing salts and double salts.

LI. *On* Newton's *Rule for Imaginary Roots.*

To the Editors of the Philosophical Magazine and Journal.

Priory Cottage, Peckham, S.E.,
October 14, 1865.

GENTLEMEN,

AT the top of page 291 of my paper in the last Number of this Journal there is printed in italics a statement which the remarks that follow were intended to show to be an admissible truth. I now perceive, however, that the inference drawn from those remarks at the bottom of that page does really assume the principle contended for, and that that principle has not the axiomatic character I had considered it to have. All therefore that I have satisfactorily done, in reference to Newton's theorem, is to show that the condition (1) at page 290 always implies a pair of imaginary roots, and that, however numerous be the con-

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* Liebig's *Annalen* (Supplement), vol. ii. p. 383; and vol. iii. p. 245.

secutive fulfilments of that condition, only one pair of imaginary roots can with certainty be inferred.

I had always regarded this first part of Newton's theorem as the part which it was the more difficult to prove; and as to the second part, namely, that a fulfilment after a failure always implied a distinct imaginary pair, I conceived that a few general considerations, such as those I have given, would suffice to establish it. In this I was mistaken; and I hasten to acknowledge that my investigation is incomplete, and to at once withdraw all claim to having furnished a complete demonstration of Newton's Rule.

Professor Sylvester's demonstration (*in extenso*) has not yet appeared; but I have no doubt that it will be free from the defect which attaches to mine. To Professor Sylvester alone therefore the claim of priority will be due; and I much regret that by disputing this claim I should have put him to any trouble. I cannot atone for it in any other way but this. He will see that I have not allowed any considerations whatever to prevail with me against my own conscientious convictions—against a clear sense of what is just and right; and I trust that this *amende*, which is entirely spontaneous, and not the result of any external influence, will be accepted in the same spirit in which it is tendered.

I remain, Gentlemen,
Your obedient Servant,
J. R. YOUNG.

P.S.—This letter would no doubt have been forwarded earlier, but you are aware that my copy of the Magazine was sent by post from your office (at my request) to another person: it did not reach *me* till the 10th inst.

[Mr. Sylvester, to whom, at Professor Young's request, the above has been shown, has requested insertion of the following observations.—ED.]

There can be but one opinion of the course adopted by Professor Young in avowing his mistake; it can only tend to raise him in the estimation of all right-thinking persons. Most writers in the course of their lives have published some erroneous results, but comparatively few have had the magnanimity to make so prompt an avowal. I wish it were not indispensable for me to observe on the two charges of inaccurate statement of matters of fact brought against me in the last Number of this

Magazine*. As Professor Young was informed by the publishers of this Journal that I directed an invitation to be forwarded to him through their agency, he should, I think, have confined his statement (if he conceived that anything turned upon a point apparently so immaterial) to a denial of his having received it. I was not informed that it had not been transmitted. As to the other charge, Professor Young's memory must certainly be in default, as my observations on his "supposed demonstration" formed no part of my lecture, but were called forth by his interpellations addressed to me at the close of it, and his express "invitation" to state wherein the same was inaccurate. Surely then "I was invited by Professor Young to express an opinion on his supposed demonstration."

Whilst guarding myself against being understood to acquiesce in the correctness of the general impression which Professor Young's last statement (however honestly meant) tends to convey on the matter late in controversy, I think it right, under the circumstances, to refrain from making any comment upon it. A syllabus of my lecture, filling many pages of print, was directed to be put into the hands of all who did me the honour of attending it; and if Professor Young received one he will find, if he can spare time to consult it, that it does contain a proof *in extenso* of Newton's theorem—not merely that which commonly goes by the name, but of the complete theorem in the form given to it by Newton himself, which had been lost sight of, without exception, so far as I know, by all who have since treated of the subject, in which form it is a refinement upon Descartes's rule of signs, and admits of being generalized still further into a theorem which bears to itself the same kind of relation that Fourier's theorem bears to Descartes's,—this generalized theorem, which is of a geminate character, being in its turn included in one still more general, containing an arbitrary parameter (*limited*). The late deeply lamented Mr. Purkiss has drawn up a very full account of these investigations for the Number of the Cambridge, Oxford, and Dublin Mathematical Messenger which has just made its appearance. I do not like to conclude without expressing my sincere regret if I have been unguardedly

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* Imagine the case that A. B., having boxed C. D.'s ears, immediately afterwards offers a handsome apology for having just before unintentionally trodden on his toes. This will afford a clue to the conflict of feeling occasioned in my mind by Professor Young's concession as regards the right of discovery, following an unredressed imputation upon my accuracy. In a world and in a country where the experience of life has taught me that the love of truth so loudly professed is so rarely acted upon as a rule of conduct, I shall perhaps hardly be believed when I assert that I prize a character for truthfulness far above any merit that may be supposed to attach to originality.

drawn into the use of any seeming asperity in my mode of alluding to so eminent a teacher and meritorious and veteran an author as Professor Young. The sole imputation which I meant to convey when I hastily compared him with a class of individuals proverbially difficult to convince, was that of obstinacy in his convictions,—a charge which his present honourable course of proceeding triumphantly refutes.

K, Woolwich Common,
October 20, 1865.

LII. *On the Magnetism of Salts of the Magnetic Metals.*

By Professor Wiedemann *of Brunswick*[*].

In a series of earlier investigations the author endeavoured to establish the properties of temporary and permanent magnets, in so far especially as they can be explained on the assumption of moveable molecular magnets. By this assumption the increase of the temporary and permanent magnetic momentum with increasing magnetizing power, its change by alternating action of the latter in an opposite direction, and the mechanical and thermal deportment of the various magnetized bodies can be explained, at any rate qualitatively. For a further establishment of this theory, as well as from a chemical point of view, it appeared interesting to consider more closely the properties of the feebly magnetized bodies, the salts of the magnetic metals, and their solutions. Hence the magnitude of their temporary magnetic momentum under various circumstances was determined.

The measurements of this momentum were made with the aid of a special torsion-apparatus. To a vertical brass pin, moveable in a sheath, a German-silver wire was suspended, to which was attached a long vertical brass bar, the lower end of which was loaded with weights, and provided with brass paddles which were immersed in a vessel full of oil. At the upper end of the brass rod a mirror was fastened, by means of which the rotation of the bar about its vertical axis could be read off by a scale and telescope in the usual manner. On the bar, below the mirror, and in a north and south direction, was a horizontal brass arm about 20 millims. in length, to which was attached a small glass flask filled with the substance to be investigated. In front of this, and in an east and west direction, was a straight horizontal soft iron bar rounded in front and surrounded by a magnetizing spiral, the magnetic momentum of which could be read off in a mirror galvanometer placed in the direction of its axis. After the magnetizing current was closed, by turning the pin

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[*] From the *Berliner Berichte*, June 1865.

to which the German-silver wire was attached, a definite direction could be assigned to the glass flask, which was read off by means of the mirror fixed to the brass rod. The current was then opened, and the position of the mirror again observed. The rotation of the mirror measures the magnitude of the torsion in the German-silver wire which supports the apparatus, and thus the force of magnetic attraction.

By numerous experiments it was shown that, within the limits of observation, this attraction is proportional to the square of the magnetic momentum of the attracting electromagnet—that thus,

1. *The temporary magnetic momentum of the vessel filled with various saline solutions is directly proportional to the magnetizing power.*

An approximation to the maximum of magnetizing could not be observed with the magnetizing forces used.

If the glass vessel was filled with solutions of the same salt of different degrees of concentration, and with solutions in different solvents (perchloride of iron in alcohol, ether, water), and, from the temporary momentum imparted to the solution by the magnetizing power, unit of the momentum subtracted which was produced by the same force when the vessel was filled with the solvent alone, values were obtained which are proportional to the weights of the salt contained in the unit of volume.

2. *The magnetic momentum of the salts dissolved in different solvents is directly proportional to the weights contained in the unit of volume, and independent of the solvent.*

If in the same way the temporary momentum of the glass vessel when filled with various aqueous saline solutions (protosulphate of iron, protochloride of iron, perchloride of iron, sulphate of nickel, sulphate of cobalt, ferricyanide of potassium) was investigated at different temperatures, and in each case the magnetism was subtracted of the vessel filled with water for the same temperature and the same magnetizing force, it was found that,

3. *The temporary magnetic momentum of salts decreases with increasing temperature, and in the same ratio in all salts investigated. If* t *is the temperature in degrees Centigrade,* m_0 *the temporary momentum at* 0°, m_t *at* t°, *then approximately*

$$m_t = m_0(1 - 0{\cdot}00325t).$$

A fact interesting in relation to the theory of magnetism is, that this decrease of magnetic momentum, whose magnitude corresponds to the intensity of the Ampèrian molecular currents circulating round the magnetic molecules, is not very different from the decrease of the conductibility of the metals for the voltaic current for the same changes of temperature.

If the glass vessel was filled with solutions of different salts, and, as above, the magnetic momentum produced by the unit of magnetizing force determined, and then the value obtained divided by the weight of the salt in the unit of volume of the solution, the values given in the following Table under *m* were found.

	m.	μ.
Sulphate of nickel	18·40	1426
Nitrate of nickel	15·67	1433
Chloride of nickel	21·54	1400
Sulphate of cobalt	41·52	3218
Nitrate of cobalt	33·98	3109
Chloride of cobalt	47·08	3058
Ferrous sulphate, I.	52·05	3956
Ferrous sulphate, II. ..	51·07	3882
Ferrous ulphate, III. ..	50·95	3873
Ferrous nitrate, I.	43·28	3895
Ferrous nitrate, II.	42·54	3828
Ferrous chloride	60·78	3858
Ammonio-ferrous sulphate.	19·35	3795
Manganous sulphate	62·18	4695
Manganous nitrate	52·46	4693
Manganous acetate	53·03	4586
Chloride of manganese, I.	75·34	4742
Chloride of manganese, II.	74·15	4669
Chromic sulphate, I.	19·65	3863
Chromic sulphate, II.	19·15	3761
Chromic nitrate, I.	15·34	3659
Chromic nitrate, II.	15·38	3716
Chromic nitrate, III.	15·68	3739
Chromic chloride, I.......	25·27	4017
Chromic chloride, II	25·04	3959
Ferric sulphate, I.	46·63	9326
Ferric sulphate, II.	46·34	9267
Ferric nitrate	38·89	9410
Ferric chloride, I.........	59·57	9661
Ferric chloride, II.	58·95	9572
Ferric chloride, III.	51·51	9605

The values *m*, the temporary magnetic moments excited in the unit of weight of the salts by the unit of magnetizing force, may be designated as the *specific magnetism of the salts*.

The values μ in the preceding Table are obtained by multiplying the values *m* by the atomic weight of the corresponding salt.

4. *Hence, in salts of the same metal of analogous composition,*

the product of the specific magnetism into the atomic weight is constant, or the magnetism of each atom of these compounds is the same. In this respect the oxygen salts and haloid salts corresponding to one another show the same deportment.

The mean magnetic moments of an atom of a protosalt of nickel, cobalt, iron, and manganese are as 142 : 313 : 387 : 468.

5. *Hence the magnetism of an atom of the cobalt salt is very nearly the mean between the magnetisms of the atoms of salts of nickel and manganese,* $\frac{1}{2}(142+468)=305$. *In like manner the magnetism of the atom of ferrous salts is the mean between the magnetisms of the atoms of cobalt and manganese salts,*

$$\tfrac{1}{2}(313+468)=390{\cdot}5.$$

The atoms of salts of different oxides of the same metal have, on the other hand, very different magnetism—thus, for instance, the atoms of ferrous and ferric salts, whose magnetisms with the same weight of iron are as 1 : 1·24. Ferric oxide obtained in the soluble form by dialysis, as well as basic compounds of ferric oxide with acids in which a part of it is in the same condition, behave like the soluble ferric oxide.

Finally a number of solid salts were investigated. The magnetic momenta m of the same weights, as well as the products μ of the values m into the atomic weights, were

	m*.	μ.
Ferrous sulphate (crystallized)	46·87	3183
Ferrous chloride (anhydrous)	57·71	3660
Ammonio-ferrous sulphate (crystallized)	20·81	4078
Sulphate of manganese (crystallized)..	60·92	4600
Sulphate of cobalt (crystallized)......	38·09	2950
Sulphate of cobalt, I. (anhydrous)	38·13	2954
Sulphate of cobalt, II. (anhydrous) ..	39·19	3038
Ferric chloride	55·41	9000

6. Hence the magnetism of the dry salts is almost the same as the magnetism of the *same salts when dissolved; the combination of anhydrous salts with water of crystallization does not materially alter their magnetism.*

7. *From a few further experiments the insoluble salts show the same deportment* (phosphate and carbonate of cobalt, manganous phosphate); *so that in these salts also the product of the specific magnetism into the atomic weight has the same value as in the soluble salts.*

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* Except in the case of ammonio-ferrous sulphate, the values m refer to the proportion of anhydrous salt.

Only when the insoluble salts (as, for instance, carbonate of manganese by washing with hot water) alter their constitution and become basic, is the magnetism of their atom smaller, as in the case of the dissolved basic ferric salts.

As regards theoretical conclusions from the present results of observation, a paper to be forthwith published in Poggendorff's *Annalen* must be consulted.

LIII. *On a Problem of Geometrical Permutation.*

By Professor CAYLEY, *F.R.S.**

IT is required to find in how many modes the nine points of inflexion of a cubic curve can be denoted by the figures 1, 2, 3, 4, 5, 6, 7, 8, 9, in such wise that the twelve lines, each containing three points of inflexions, shall be in every case denoted by the same triads of figures, say by the triads

123, 147, 159, 168,
456, 258, 267, 249,
789, 369, 348, 357.

We may imagine the inflexions so denoted in one particular way, which may be called the primitive denotation; then in any other mode of denotation, a figure, for example 1, is either affixed to the inflexion to which it originally belonged, and it is then said to be *in loco*, or it is affixed to some other point of inflexion. This being so, the total number of modes is $=432$; viz. this number is made up as follows:—

9	figures	*in loco*	1
3	„	„	60
1	figure	„	243
0	„	„	128
			432

There is of course only one mode wherein the nine figures remain *in loco*. It may be seen without much difficulty that there is not any mode in which 8, 7, 6, 5, or 4 figures remain *in loco*. There is no mode in which only 2 figures remain *in loco*; for any two inflexions are in a line with a third inflexion; and if the figures which belong to the first two inflexions are *in loco*, then the figure belonging to the third inflexion will be *in loco*; that is, there will be 3 figures *in loco*. The only remaining modes are therefore those which have 3 figures, 1 figure, or 0 figure *in loco*.

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* Communicated by the Author.

First, if three figures are *in loco,* these, as just seen, will be the figures which belong to three inflexions in a line. Suppose the figures are 1, 2, 3; then the inflexion originally denoted, say, by the figure 4 may be denoted by any one of the remaining figures 5, 6, 7, 8, 9; but when the figure is once fixed upon, then the remaining inflexions can be denoted only in one manner. Hence when the figures 1, 2, 3 remain *in loco* there are 5 modes; and consequently the number of modes wherein 3 figures remain *in loco* is $5 \times 12, = 60$.

Next, if only a single figure, suppose 1, remains *in loco,* the triads which belong to the figure 1 are 123, 147, 159, 168; and there is 1 mode in which we simultaneously interchange all the pairs (2, 3), (4, 7), (5, 9), (6, 8). (Observe that the triads 123, 147, 159, 168 here denote the same lines respectively as in the primitive denotation, the figure 1 remains *in loco,* but the figures belonging to the other two inflexions on each of the four lines are interchanged.) There are, besides this, 2 modes in which the figures (2,3), but not any other two figures, are interchanged; similarly 2 modes in which the figures (4, 7), 2 modes in which the figures (5, 9), 2 modes in which the figures (6, 8), but in each case no other two figures, are interchanged; this gives in all $1+2+2+2+2, =9$ modes. There are besides, the figure 1 still remaining *in loco,* 18 modes where there are no two figures (2, 3), (4, 7), (5, 9), or (6, 8) which are interchanged: viz., the figure 2 may be made to denote any one of the inflexions originally denoted by 4, 5, 6, 7, 8, or 9. Suppose the inflexion originally denoted by 4; 3 will then denote the inflexion originally denoted by 7: it will be found that of three of the remaining six inflexions, any one may be denoted by the figure 4, and that the scheme of denotation can then in each case be completed in one way only. This gives $6 \times 3, =18$, as above, for the number of the modes in question; and we have then $9+18, =27$, for the number of the modes in which the figure 1 remains *in loco*; and $9 \times 27, =243$ for the number of modes in which some one figure remains *in loco.*

Finally, if no figure remains *in loco,* the figure 1 will then denote some one of the inflexions originally denoted by 2, 3, 4, 5, 6, 7, 8, 9. Suppose that originally denoted by 2; 2 cannot then denote the inflexion originally denoted by 1, for if it did, 3 would remain *in loco*: 2 must therefore denote the inflexion originally denoted by 3, or else some one of the inflexions originally denoted by 4, 5, 6, 7, 8, 9. It appears, on examination, that in the first case there are 4 ways of completing the scheme, and in each of the latter cases 2 ways; there are therefore in all $1 \times 4 + 6 \times 2, =16$ ways; that is, 16 modes in which (no figure remaining *in loco*) the figure 1 is used to denote the inflexion originally denoted by 2; and therefore $8\times16, =128$ modes, for which no figure remains *in loco*. This completes the investigation of the numbers 1, 60, 243, and 128, which together make up the total number 432 of the modes of denotation of the nine inflexions.

LIV. *Proceedings of Learned Societies.*

CAMBRIDGE PHILOSOPHICAL SOCIETY.

October 26, 1863.—A communication was made by Mr. H. D. Macleod "On the Theory of Banking."

November 9.—Communications were made by Dr. Humphry, "The Results of Experiments on the Growth of the Jaw."

By Mr. Todhunter "On a Question in the Theory of Probabilities."

November 23.—A communication was made by Professor Challis "On the Meteor of August 10, 1863."

December 7.—A communication was made by Dr. Akin "On the Origin of Electricity."

February 8, 1864.—A communication was made by Professor Liveing "On the new Metal, Thallium."

February 22.—Communications were made by Professor Cayley "On a Case of the Involution of Cubic Curves," and "On the Theory of Involution."

By Mr. Spencer "On Vitality."

March 7.—Communications were made by Mr. Harry Seeley—

1. "On the Significance of the Succession of Rocks and Fossils."

2. "On Pterodactyles, and on a new Species of *Pterodactylus machærorhynchus*."

April 18.—Communications were made by Mr. Röhrs "On the Strains and Vibrations of Ordnance under the Action of Gunpowder."

By Professor Cayley "On the Classification of Cubic Curves," and "On Cubic Cones and Curves."

May 2.—Communications were made by Mr. Harry Seeley—

1. "Note on *Palæobalæna Sedgwicki* (nob.), a Whale from the Oolite."

2. "On the Fossil Birds of the Upper Greensand, *Palæocolyntus Barretti* (nob.) and *Pelargonis Sedgwicki* (nob.)."

3. "On the Osteology and Classification of Pterodactyles, Part II., with descriptions of the new species *P. Hopkinsi* and *P. Oweni*."

May 16.—Papers were read by Professor De Morgan—

1. "A Theorem relating to Neutral Series."

The theorem is as follows. If $a_0-a_1+a_2-\ldots$ be a convergent series which has the limiting form $1-1+1-\ldots$, and if a_z be of continuous law, so that $a_{z+1} : a_z$ finally approaches towards a limit, the limit towards which the series approaches as its form approaches neutrality is $a_z - a_{z+1}$ divided by $a_z - a_{z+2}$. And this limit is always $\frac{1}{2}$. If there be in the series a cycle of laws involving an even number of terms, so that

$$a_{2nz}-a_{2nz+1},\ a_{2nz+1}-a_{2nz+2},\ \dots\ a_{2nz+2n-1}-a_{2nz+2n}$$

approach in ratio to $k_0, k_1, \dots k_{2n-1}$, then the two series $a_0 - a_1 + a_2 - \dots$ and $a_1 - a_2 + a_3 - \dots$, which have unity for their sum, have the ratio of $k_0 + k_2 + \dots + k_{2n-2}$ to $k_1 + k_3 + \dots + k_{2n-1}$. But if the cycle have an odd number of terms, each of these series is $\frac{1}{2}$, just as if the law had been continuous. The demonstration is founded upon the following theorem:—If $P_0 + P_1 + \dots$ and $Q_0 + Q_1 + \dots$ be diverging series, whether of increasing or decreasing terms, their two infinite sums are in the final ratio of P_z to Q_z. Applications of this theorem are given to the determination of a large number of terms of $1^n + 2^n + \dots$ when n is -1 or greater, and to the determination of the usual approximation to $1 . 2 . 3 \dots n$ when n is great.]

2. "On Infinity, and on the Sign of Equality."

The author professes himself satisfied of the *subjective* reality of the notions of infinitely great and infinitely small. His paper, as far as it deals with various objections by various modes of answer, is not capable of abstract; but four points, on which he especially relies, may be stated as follows:—

1. The concepts of the mind are divisible into imageable and unimageable: the first can be pictured and placed before the mind's eye; the second cannot. The mathematician, dealing in great part with imaged concepts, is apt to repel the unimageable, as if it could not be a legitimate object of mathematical reasoning. But all that is necessary to reasoning is knowledge of the connexion of subjects and predicates. Infinite quantity is unimageable in its relation to finite quantity, but not therefore inconceivable, nor destitute of known attributes. A million of cubic miles is as destitute of image as infinite space; nevertheless it is a conception the attributes of which give known propositions.

2. Number, or enumeration as distinguished from multitude, is a concept from which no notion of infinity can be gained; but much perplexity has arisen from the attempt to make it a teacher of this subject. Abstract number has more than one affection which is derived from the concrete in such manner that the two abstractions, number and its affection, cannot have their function explained excepting by return to the concrete. Such affections are the divisible unit, on which the doctrine of fractions is founded, and the opposition of positive and negative. The representation of infinite and of infinitesimal numbers is a third affection of the numerical, which cannot be explained on purely numerical notions.

3. The infinite is not a kind of terminus to the finite, but another *status* of magnitude, such that no finite, however great, is anything but an infinitesimal of the infinite. And the same may be said of each order of infinity with reference to the one below it.

4. The symbol $\frac{1}{0}$, the infinite of common algebra, represents an extreme of infinite which can no more be attained by passage through orders of infinity, than any infinite by passage from finite to finite.

Each of these positions comes into conflict with some of the usual arguments for or against the introduction of infinites.

The second part of the paper is on the meaning of the sign of equality. Mr. De Morgan contends for the ultimate attainment of a purely formal algebra, in which every transformation shall have meaning and validity in every possible case. He points out certain difficulties and inconsistencies in the ordinary use of the sign of equality, which can, he affirms, receive a consistent explanation on the extension which he proposes, and which, to some extent, he considers as virtually adopted.

His notion is that *equality*, strictly so called, is but a species of the genus *undistinguishable*; and that the actual use of the sign (=) shows a leaning to the generic definition. Every order of infinites or infinitesimals has its own metre, and the sign (=) indicates undistinguishability with reference to the metre, which is often in thought, but for which no symbol is employed. Algebraical changes may or may not demand or permit changes in the metre. It would be impossible to give any further account, with justice to the subject, in a short abstract.

3. By Mr. Harry Seeley "On Saurornia, and the Classification of Pterodactyles, Part III."

October 31, 1864.—Papers were read by Professor Selwyn "On Autographs of the Sun."

By A. R. Catton, B.A., St. John's College, "On the Constitution of Chemical Compounds."

November 14.—A communication was made by G. F. Browne, M.A., St. Catharine's College, "On certain Ice-Caverns."

November 28.—Papers were read by Dr. Humphry on the question, "Is the Vertebral Theory of the Skull to be abandoned?"

This communication was intended partly as a reply to the opinion expressed by Professor Huxley in his lectures on comparative anatomy, that the vertebral hypothesis of the skull has been abolished by the recent discoveries in development. Dr. Humphry commenced by calling attention to the Laws of Uniformity of Plan, and Variety in Detail, which prevail throughout the animal kingdom, and, indeed, throughout the material system, and which the recent discoveries by the microscope have shown to rule over the ultimate structure and formation of all the tissues of the body. The discovery of the illustration of these laws in the plan of cell-formation of the tissues, and in the development of all animal and vegetable structure from the simple cell-form, he regarded as the grandest discovery in physical science that has taken place in our time. Of late years, the attention of anatomists has been much directed to the exemplification of these laws in the vertebrate classes, to tracing the uniformity of plan, especially in the skeleton, through the variety in detail which the members of these classes exhibit. This constitutes the branch of anatomy called "Homology." The general features of the plan upon which vertebrate animals are constructed are clear enough in all of them. Osseous segments, or vertebræ, with neural and visceral processes, enclosing respectively the neural and visceral centres, constitute the trunk, including neck, chest, loins, &c. Probability is in favour of the view propounded by Goethe and Oken, and worked out by Oken and Owen, that the skull falls in with the Law of Uniformity, and corresponds with the rest of the frame in having a vertebral composition. It is by all anatomists admitted to be segmentally constructed. Most anatomists are agreed as to the number of segments. Ought not, therefore, these segments to be described by the same name as those of which they form a continuation, especially as they bear the same relations to the neural and visceral centres, and the same or nearly similar relations to the nerves and blood-vessel? In their mode of development, too, the segments of the skull show a marked general correspondence with those of the trunk. The chorda dorsalis, around which the vertebral centres are formed, extends at any rate halfway along the base of the skull; and the bodies and arches of the cranial segments are evolved from a continuation of the same embryonic structure (the "vertebral plates") as the trunk segments—the chief difference being that in the trunk segmentation takes place at an earlier period than in the head. In the trunk, it is observed in the vertebral plates; and these primitive segments are called "protovertebræ." They appear not to exist in the head. The segmentation, however, takes place in the cranium as soon as ossification begins, even if it does not do so before; and the significance of the protovertebræ as distinctive features between the skull and the trunk is diminished, first, by their being related to the formation of the nerves as much or more than to that of the vertebræ; and secondly, by their not really corresponding with the vertebræ, each permanent vertebra being formed by a half of two protovertebræ. Dr. Humphry expatiated on this and other points in the development of the skull, and expressed his decided opinion that the differences between the development of it and of the trunk vertebræ were by no means sufficient to controvert the view—which coincides with the Law of Uniformity, and which is confirmed by the segmental construction of the skull, by the relation of its components to surrounding parts, and by so many fundamental resemblances in development—that the same name may be applied to the segments of the skull and of the trunk, and that the one, as well as the other, consists of vertebræ modified to meet the requirements of the parts in which they are found. He concluded by stating that the greater number of those anatomists to whose observations we are indebted for most of our knowledge of the development of the skull and of the trunk, are agreed that the differences between the mode of formation of the segments in the two form no real argument against the vertebral character of either; and he thought stronger reasons must be adduced than had yet been shown before the anatomists could be called upon to abandon the vertebral theory of the skull.

By Professor De Morgan "On the Early History of the Signs + and —."

An account is given of the work on arithmetic of John Widman, printed in 1489, in which the signs + and — are used to denote *more* and *less*. The use made is twofold: $a+b$ signifying that b more than a is wanted, infers a direction to add b to a. But $a+b$ in the old rule of *false position* is used to signify that the assumption of a for the answer gives b too much in the solution. This last usage was continued by many writers through the greater part of the sixteenth century.

Some account is given of the *Die Coss* of Chr. Rudolf, which passes for the first work in which + and — are used. The first edition of his work being lost, a question is raised as to how far the second edition, edited by Stifel, is a fair reprint of the first. A Latin translation of this first edition is said to be in the Imperial Library at Paris.

From the mannner in which Widman introduces his signs, Mr. De Morgan thinks there is some ground to suspect that they were originally *warehouse marks*, indicating the scale into which smaller weights were introduced to make the balance, when the nearest number of larger weights had been put in. This point and others require the examination of older works, print and manuscript.

February 13, 1865.—Communications were made by Professor Cayley "On Abstract Geometry."

By Professor Clifton, "Note on the Early History of the Signs + and —."

February 27.—Mr. Alfred Newton, M.A., F.L.S., communicated some "Notes on Spitzbergen," of which the following is an abstract.

The author stated that last summer he accompanied Mr. Edward Birkbeck on a voyage to Spitzbergen, in that gentleman's yacht, the 'Sultana,' R.S.Y.C. After giving a slight sketch of some of the principal voyages which had been made to that country, he proceeded to say that the 'Sultana' left Hammerfest on the 3rd of July, in company with a Norwegian sloop which had been specially fitted to encounter the ice, and chartered to attend upon the yacht. On the 6th they arrived at the entrance of Stor Fjord, which was found to be entirely blocked by the ice. Horn Sound and Bell Sound were subsequently discovered to be in the same condition. They then made for Ice Sound, and anchored in Safe Haven on the 9th. Ice Sound was described as a very much larger inlet than it is represented in the charts to be, extending at least fifty miles into the interior. All the valleys on the north side, and consequently having a southern aspect, are completely occupied by large glaciers, which, with one exception, are only terminated by the sea. The single exception consists of a small but remarkable glacier suspended on a hill-side, some 360 yards from the beach, resting conformably on its own moraine, and having no apparent means of discharge. The author supposes this last was effected by filtration through its bed. The south side of Ice Sound contains several bays of considerable size; and the valleys opening upon it, and therefore having a northern aspect, were entirely free from glaciers; the observation being directly opposed to the account given of the Spitzbergen glaciers by Sir John Richardson in his 'Polar Regions.' A great many reindeer frequent this part of the country, and countless numbers of sea-fowl breed on such of the high cliffs around the Sound as are inaccessible to the Arctic foxes. In Ice Sound Mr. Birkbeck's party had the pleasure of meeting the Swedish Scientific Expedition under Professor Nordenskjold, who are engaged in measuring an arc of the meridian. On the 4th of August the party separated, some going to the eastward in the Norwegian sloop, while the yacht made another ineffectual attempt to ascend the Stor Fjord. The sloop sailed as far as Ryklis Islands, but was stopped by the ice. She then proceeded further east in the hope of getting round the pack, and came in sight of "Commander Gile's land," the existence of which had been so long doubted, it having been ignored by Sir John Richardson in his work, and in the Admiralty Chart of Spitzbergen. It appears to lie about sixty miles east of the entrance to Walter Thymen's Strait, and its flat or round-topped hills (so different from those of Spitzbergen) were very plainly seen. The author stated that in 1859 the master of a Norwegian vessel landed upon it, and he produced a pebble which had then been brought thence, in proof of the reality of its existence as land, and not either ice or fog-bank. He then proceeded to remark on the driftwood with which the shores of the "Thousand Islands" are strewn, which he believed to be certainly of Siberian origin, and not brought, as sometimes imagined, by the Gulf-stream,—stating that though often worm-eaten, he had never observed any signs of barnacles upon it. He then commented on the discovery of the passage from the top of Stor Fjord to Hinlopen Strait, of which there had long been a traditionary knowledge, though it was not effected till 1859; and showed, from the Swedish surveys in 1861, that this passage must lie some thirty miles further south than the position assigned to it on the Admiralty charts, thus affording another instance of our imperfect knowledge of the geography of Spitzbergen. In conclusion, the author stated that he left Spitzbergen on the 21st of August, the sun having set a night or two previously for the first time, and the salt water begun to freeze; and he warmly urged his audience to support the further circumpolar exploration which has been lately proposed by Captain Sherard Osborne; and that, as a zoologist, he could declare there were many questions of the very highest interest which could only be solved by a new Arctic expedition.

Professor Babington and Mr. Harry Seeley made communications on the plants brought by Mr. Newton from Spitzbergen.

March 13.—A communication was made by Professor Liveing "On Gun-cotton."

March 27.—A communication was made by Professor Miller "On the Crystallographic Methods of Grassman, Hessel, Frankenheim, and Uhale, and on their employment in the investigation of the general geometrical properties of Crystals."

May 1.—Communications were made by Mr. Harry Seeley—

1. "On the Cambridge Greensand.—Part I. The Rock and its Origin."

2. "On the Gravel and Drift of the Fenland.—Part II. Theory."

May 15.—A communication was made by Professor Churchill Babington "On the Coinage of England before the Norman Conquest."

May 29.—A paper was read by Mr. Todhunter "On the Method of Least Squares."

The object of this communication is principally to demonstrate a very remarkable result which Laplace enunciated, without demonstration, in the first Supplement to his work on 'Probabilities.' An exposition is also given of the process adopted by Laplace for investigating the method of least squares. Laplace's process is generalized and extended; and results which he obtained for the case of two elements are shown to hold for any number of elements. The mathematical part of the investigation consists chiefly in the evolution of certain definite multiple integrals.

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May 24, 1865.—Dr. E. Meryon, Vice-President, in the Chair.

The following communications were read:—

1. "Additional Observations on the Raised Beach of Sangatte, with reference to the date of the English Channel, and the presence of Loess in the Cliff-section." By Joseph Prestwich, Esq., F.R.S., Treas. G.S.

In his paper on the Loess and Quaternary beds of the North of France and South-east of England, Mr. Prestwich expressed an opinion that the break in the land between France and England was not the result of the last geological change, but that the channel existed at the period of the formation of the Low-level gravels of the Somme and Thames Valleys, and probably at that of the High-level gravels. During a recent visit to the Sangatte Raised Beach, the author recognized fragments of chert in the shingle and associated sands, which he inferred were derived from the Lower Cretaceous strata; associated with them were fragments from the Oolitic series of the Boulonnais and two pebbles of red granite, probably from the Cotentin.

These facts seemed to the author to add much probability to the existence of a channel open to the westward, and extending between France and England, anterior to the Low- and possibly to the High-level Valley-gravel period. Above the raised beach occurs a mass of chalk- and flint-rubble, with beds of loam, from 20 to 80 feet thick, and containing Land-shells. Mr. Prestwich considered this accumulation analogous to the Loess, which it resembles in general character, while the shells found in it belong to species common in that deposit.

2. "On the Superficial Deposits of the Valley of the Medway, with Remarks on the Denudation of the Weald." By C. Le Neve Foster, Esq., B.A., B.Sc., F.G.S., and William Topley, Esq., F.G.S., of the Geological Survey of Great Britain.

In the first part of the paper the authors gave a description of the superficial deposits of the valley of the Medway. They showed that deposits of river-gravel and brick-earth (loess) occur at various heights up to 300 feet above the level of the river. A detailed account was given of the "pipes" at Maidstone, where brick-earth (loess), containing land and freshwater shells and mammalian remains, has been let down into deep cavities in the Kentish Rag, probably by the gradual dissolving away of the limestone by the action of rain-water containing carbonic acid. Several interesting cases of disturbed gravel were mentioned.

The second part of the paper was intended mainly to show what light is thrown upon the theory of the denudation of the Weald by a study of the superficial deposits. After a brief account of previous theories, with objections to the theory of fracture and the marine theory, the authors endeavoured to prove that the gravel and brick-earth (loess) occurring at a very great height above the level of the Medway are old alluvia of that river. If this point be granted, it follows that so large a denudation has been effected by atmospheric agencies, *i.e.* rain and rivers, that, in the opinion of the authors, there will be little difficulty in supposing the present inequalities of surface in the Weald to have been produced by these agents acting on a comparatively plane surface of marine denudation. A discussion as to the origin of Escarpments then follows. The authors considered that the Escarpments of the Chalk and Lower Greensand which surround the Weald are not sea-cliffs, but are due to the difference of waste of the hard and soft formations under atmospheric denudation.

June 7, 1865.—W. J. Hamilton, Esq., President, in the Chair.

The following communications were read:—

1. "Note on *Ovibos moschatus*, Blainville." By M. E. Lartêt, For. Mem. G.S. Translated by the late H. Christy, Esq., F.R.S., F.G.S.

A hoof phalange found by Mr. Christy and the author at one of their stations in the Gorge d'Enfer was stated to be identical in form and dimensions with the corresponding bone of the existing *Ovibos moschatus*, to which species M. Lartêt therefore referred it. With it were found remains of *Ursus spelæus*, *Felis spelæa*, Wolf, Reindeer, and Aurochs, as well as worked flints differing from those found in any other of the Dordogne caves. The author remarked that the Gorge d'Enfer is the most southern locality at which remains of *Ovibos moschatus* have yet been found, and is 15° south of its most southern limit at the present day; but the Reindeer has been found by Mr. Christy and himself further south still—on the northern slope of the Pyrenees.

2. "On some Additional Fossils from the Lingula-flags." By J. W. Salter, Esq., F.G.S. With a Note on the Genus *Anopolenus*; by Henry Hicks, Esq., M.R.C.S.

In a recent paper Mr. Salter described the new genus *Anopolenus* as a blind Trilobite allied to *Paradoxides*, without facial sutures or head-spines, and with truncate body-segments not produced into spinous appendages, as in most of its congeners. The remains of a new species, provided with extraordinary free cheeks, have proved that this conclusion was founded upon a part only of the head and of the body of the animal, which now appears to be more truly intermediate between *Paradoxides* and *Olenus* than was before supposed, while at the same time it presents characters opposed to those of either genus.

Mr. Hicks gave a full description of the genus as now known, and of the new species, which he called *Anopolenus Salteri*. From his description it appears that *Anopolenus* possessed minute eyes, a facial suture, and expanded pleuræ, but that their arrangement was abnormal.

In conclusion Mr. Salter compared the two species of *Anopolenus* now known, stating that the one first described, without the more anterior of the two segments which compose the head, was to all appearance a perfect Trilobite. He also gave a figure of a new species of *Olenus*—*O. pecten.*

3. "On the Discovery of a New Genus of *Cirripedia* in the 'Wenlock Shale of Dudley.'" By Henry Woodward, Esq., F.G.S.

The attention of the author having been called to two species of *Chiton* described M. de Koninck from the Wenlock Shale, he found one of them (*Chiton Wrightianus*) to be a Cirripede. He stated that the distinctive characters of Chiton are:—(1) The valves never exceed eight in number; (2) the series is always unilinear; (3) the valves are always symmetrical, and divided into three areas. The species mentioned does not, however, conform to any of these characters, as it had probably as many as four rows of unsymmetrical plates, having no apophyses, a uniformly sculptured surface, and not divided into three areas; each series exceeded eight in number.

Mr. Woodward then endeavoured to show that *Chiton Wrightianus* was really a Cirripede, and formed the type of a new genus, to which he gave the name *Turrilepas*.

4. "On some new species of *Euterypрrida*." By Henry Woodward, Esq., F.G.S.

In his Advanced Text-book of Geology, Mr. Page figured and named the only known species (*S. Powriei*) of his new genus *Stylonurus*, but gave no description of it. Mr. Woodward now described the species in detail, from specimens found near Pitscandly, in the Turin Hill Range, Forfarshire; he also gave a description of a new species (*S. Scoticus*) found in an Old Red Sandstone quarry in Montroman Muir, near the Forfar and Montrose Pike. Mr. Salter has expressed an opinion that *S. Powriei* is a full-grown male, and *S. Scoticus* a young female of the same species; but Mr. Woodward observed that if the sexes are not to be determined by the thoracic plates, but by more general characters, then the two forms of plates in *Slimonia* indicate two species of females, and the two forms in *Pterygotus minor* ought to indicate two species of males.

5. "On a new genus of *Eurypterida* from the Lower Ludlow Rocks of Leintwardine, Shropshire." By Henry Woodward, Esq., F.G.S.

In this paper Mr. Woodward described a Crustacean alluded to by Mr. Salter in the 'Annals and Magazine of Natural History' for 1857, under the MS. name of *Limuloides*. It appears to form a connecting link between the *Xiphosura* and the *Eurypterida*, but it differs from the former in not having a cephalothorax—the cephalic, thoracic, and abdominal divisions being distinct, and apparently capable of separate flexure; and from the latter in having only three thoracic segments, &c. The name *Limuloides* not being allowable as a generic appellation, the author applied it to the species, using the generic term *Hemiaspis*.

June 21, 1865.—W. J. Hamilton, Esq., President, in the Chair.

The following communications were read:—

1. "On the Carboniferous Rocks of the Valley of Kashmere." By Capt. H. Godwin-Austen. With Notes on the Carboniferous Brachiopoda, by T. Davidson, Esq., F.R.S., F.G.S.; and an Introduction and Résumé, by R. A. C. Godwin-Austen, Esq., F.R.S., F.G.S. Communicated by R. A. C. Godwin-Austen, Esq., F.R.S., F.G.S.

This paper was a continuation of one read before the Society last year, in which the Carboniferous, Jurassic, and Post-tertiary deposits and fossils were described by Capt. Godwin-Austen, Mr. Davidson, and Mr. Etheridge. In this communication Capt. Godwin-Austen confined himself to the Carboniferous formation, which was shown by him to have, in the Valley of Kashmere, a thickness of more than 1500 feet. The upper portion of this mass contained but few fossils, except in one particular bed near the entrance of the ravine above the village of Khoonmoo; but the lowest portion, or Zéwan bed, is made up chiefly of the remains of Brachiopoda and Bryozoa; and a higher stage, though still near the base of the formation, contains abundant remains of *Producti*. The position of a limestone containing *Goniatites* is not very clearly determined, but it is probably a member of the Zéwan series.

The sections in which the relative positions of the different beds were exhibited were described in detail, and plans and a map were given showing their geographical relation.

Mr. Davidson described the Brachiopoda forwarded with the paper, stating that they abound particularly at Barus and Khoonmoo, but are rarely in a very good state of preservation. Among them are several common and wide-spread European and American species, with a few that have not hitherto been noticed. They appear to be of Lower Carboniferous age.

In the Introduction Mr. Godwin-Austen gave a synopsis of the more remarkable facts brought forward in the paper, and in a Résumé he gave lists of the fossils which had as yet been determined. These were forty-seven in number, forty-two of which had specific names, and twenty-two of which are well-known forms; eight are common to the Punjaub and Kashmere, seven of them being also European species. Of the Kashmere list, full half the species are found in British Carboniferous beds; and Mr. Godwin-Austen remarked on the support given to the notion of the approximate contemporaneity of distant formations containing the same fossils by the occurrence of these European Lower Carboniferous species near the base of the Carboniferous formation of Kashmere.

2. "On the Mammalian Remains found by E. Wood, Esq., near Richmond, Yorkshire." By W. Boyd Dawkins, Esq., B.A., F.G.S. With an Introductory Note on the Deposit in which they were found. By E. Wood, Esq., F.G.S., and G. E. Roberts, Esq., F.G.S.

These mammalian remains were discovered last autumn on a terrace of blue clay, mixed with limestone débris, about 130 feet above the north bank of the River Swale, during excavations for a new sewer. The deposit was stated by Mr. Dawkins to be a heap of kitchen-refuse; and the great majority of the bones, except the solid and marrowless, are consequently broken, while not one of the numerous skulls is perfect. The collection contained bones of the following species:—*Ursus arctos, Canis familiaris, Sus scrofa,* Horse, *Cervus Elaphus, Cervus dama, Bos longifrons, Bos brachyceros, Ovis Aries, Capra ægagrus,* and the horn-cores of a third form of goat, which appeared to be the *Ægoceros Caucasica,* which had also been found by Mr. Dawkins and Mr. Sanford in a bone-cavern explored by them in 1863. In a note to Mr. Dawkins, M. Lartêt expressed his opinion that these horn-cores belonged to some of the diversified forms that are the result of hybridity, and stated that they resembled some found in a bone-cave in the Pyrenees, which appeared to belong to a hybrid between the goat and the Bouquetin.

LV. *Intelligence and Miscellaneous Articles.*

ON THE DEPORTMENT OF TWO SALTS IN SOLUTION.

BY E. GERLAND.

ACCORDING to Berthollet, whenever two salts are dissolved, four salts are formed out of them, inasmuch as each acid unites with each base. This law has recently received an important confirmation from Graham's researches on liquid diffusion. He found in the case of mixtures of chloride of potassium and sulphate of soda, and of sulphate of potash and chloride of sodium, that the *diffusion of the metals* is not limited by the acids with which they are united.

The validity of Berthollet's law may be further tested by investigating the tension of the vapour of solutions of such mixtures of salts as Graham used. For Wüllner has found that the diminution of the tension of the vapour for each salt and each mixture of two salts, even when, from being salts of the same acid or the same base, they do not act chemically upon each other, is a special function of the temperature. Hence if, in the solution of a mixture, four salts are formed, the diminutions of the tensions must be the same, whatever be the compound in which the two acids and bases are brought into solution.

The author has undertaken the solution of these questions. He worked with an apparatus used by Wüllner in his experiments. It consists essentially of Magnus's modification of the shortened siphon-barometer, in the closed limb of which is the liquid to be investigated, while the open one is connected with a manometer and an air-pump, by which the mercury in both limbs can be kept at the same level. From the position of the manometer, and the difference in the levels of the mercury in the two legs of the barometer, the tension of the vapour can be determined. The heating of the barometer was effected in a transparent water-bath.

The author used two solutions of salts mixed in equivalent quantities (the salts being dry); one of these solutions was as saturated as possible (in the case of sulphate of potash not above 10 per cent.), and the second half as saturated. The salts used were

(1) $KSO^4 + NaCl$ and $Na\,SO^4 + KCl$;
(2) $KNO^6 + NaCl$ and $Na\,NO^6 + KCl$; and
(3) $KSO^4 + Na\,NO^6$ and $Na\,SO^4 + KNO^6$.

Of the numerous results obtained, the following Table furnishes an example. Columns 3, 4, 5, and 6 give the diminution of tension by the quantity of salt. Column 7 the corresponding diminution when the solution was not obtained by dissolving dry mixtures of salts, but was prepared by dissolving one salt in the solution of another.

1.	2.	3.	4.	5.	6.	7.	8.	9.
Temperature.	Tension of aqueous vapour.	$5KSO^4$ + 3·015 NaCl.	4·175 $Na\,SO^4$ + 3·84 KCl.	$10KSO^4$ + 6·031 NaCl.	8·351 $Na\,SO^4$ + 7·680 KCl.	$10KSO^4$ dissolved in 6·031 NaCl.	Mean for 1 per cent. salt.	Calculated.
12·86	11·036	0·39	0·35	0·70	0·75	0·70	0·0722	0·0651
15·24	12·900	0·49	0·50	0·94	0·85	1·10	0·0993	0·0803
16·47	13·952	0·49	0·50	0·89	0·90	0·92	0·0935	0·0816

The author concludes from the values which he obtained, about thirty in each case, between 12° and 96°,—

1. That the tension of the vapour of solutions which are prepared with salts mixed in equivalent proportions is the same, provided the constituents of the salts are the same.

2. That the diminutions of tensions in the mixtures belonging to each other, as also in the case of simple salts, increase proportionally to the percentage amount calculated according to the first salt-mixture.

In order to obtain an interpolation formula for the diminutions observed, and to represent it in its dependence on the temperature, the diminutions of the tension was considered to be dependent on the latter. It was found that the three mixtures of salts, at all degrees of temperature, diminish the tension of the vapour in the same proportion, so that if V= the diminution, T= the tension of vapour in pressures of millimetres of mercury, $v=a\mathrm{T}$, and $a=\frac{\mathrm{V}}{\mathrm{T}}$ = invariable (as Wüllner had already found). Thus, for instance, for

$1\,\mathrm{KSO}^4+0{\cdot}6031\,\mathrm{NaCl}$ and $0{\cdot}8351\,\mathrm{NaSO}^4+0{\cdot}768\,\mathrm{KCl}$, $\mathrm{V}=0{\cdot}00574\,\mathrm{T}$.

The values given above are calculated by these formulæ ; they do not diverge from the observed ones by more than the ordinary errors of experiment.

Two salt-mixtures of the same constituents, which contain them in equivalent proportions, diminish the tension in quite the same manner; hence the correctness of Berthollet's law is proved. For if the four salts were not formed, the diminution could not be the same, seeing that different salts act differently on the tension. But if they are formed, each two of the four salts have the same acids in the same metals, and in each mixture a composition of the two salts is possible in four ways. But the diminution by two such salts is not equal to the sum of the diminutions by the individual salts. Hence equality of diminution can only occur if the substances, of which there are equal quantities in both solutions, are present in the solution in the form prescribed by Berthollet's law.

The law propounded by Wüllner, that the diminution by two salts in solution is not equal to the sum of the diminutions of the individual salts, is confirmed by the experiments adduced. For if the diminutions for the individual salts of the four in solution are calculated, there is obtained from the diminutions given by Wüllner for 1 per cent of the salt taken,

$$\mathrm{V}=0{\cdot}062\,\mathrm{T}-0{\cdot}00000075\,\mathrm{T}^2, \text{ \&c.},$$

numbers which show no agreement with those given above.—Poggendorff's *Annalen*, January 1865 ; *Zeitschrift für Chemie*, part 6, 1865.

ON THE GENERAL EXISTENCE OF COPPER IN THE ANIMAL KINGDOM. BY G. L. ULEX*.

In the blood of the lower animals, especially the Mollusca, the existence of copper has been an established fact for some years. In the higher animals it has been but little sought for; and when it has, mostly none has been found. Thus in 1853, Wackenroder was unable to detect either copper or lead in the blood of the ox, the sheep, or the fowl; but he found it in the blood of the duck.

In the human blood and muscles the existence of copper has been as often asserted as denied : the details of this question are given in Liebig and Kopp's *Jahresbericht* for 1847 and 1848.

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* From a separate Paper, communicated by the Author.

Hence, when I commenced my investigations, the existence of copper in the blood appeared to be an *accidental* occurrence, void of physiological importance; which view derived support from the fact that copper had been repeatedly found in the human bile, in gall-stones, and in the liver, which especially accumulates metals foreign and injurious to the body.

In my experiments, about 800 to 1500 grains of the muscular substances and other soft parts of vertebrate animals, and 15 to 250 grains of those of the invertebrate animals were used. The substances, which were coarsely minced when necessary, were incinerated in a platinum dish over a Bunsen's gas-burner. The reduction to ash is difficult and tedious, but indispensable for the certain detection of the copper; for so long as unburnt carbon is present, this retains not only the copper but also the iron, so that these metals are not acted upon by the muriatic acid for instance. The combustion of the carbon may, however, be promoted by exhaustion with boiling water, which removes the soluble salts. The filtered solution contains phosphoric acid, alkaline chlorides and sulphurets, but no copper, and may therefore be disregarded. The filter with the carbon is dried, the latter separated from the filter, and perfectly incinerated. The copper exists in the ash as oxide. It is moistened with a little hydrochloric acid, gently warmed, a little water added, filtered, and washed. The perfectly clear liquid is treated with solution of sulphuretted hydrogen. If it becomes brownish, or even yellowish, we may be sure that copper is present; but it frequently becomes blackish brown or even brownish black. The liquid is set aside in a warm place for some hours, when a brown or black deposit is formed, which is filtered and washed. The very small filter with the precipitate is dried and incinerated. The ash warmed with a drop or two of hydrochloric acid gives with ammonia a blue solution. More conclusive evidence is afforded by reducing the ash on charcoal with soda by the blowpipe, triturating the incinerated mass in an agate mortar with water, and pouring off the carbonaceous particles, when spangles of copper are left behind. Not unfrequently the spangles are metallic grey or yellowish grey instead of red; in this case lead is precipitated and reduced with the copper, and obscures its red colour.

Copper was detected in the following classes of animals:—

Mammalia.—In the human urine and fæces with lead—and hence, as was found by experiment, in the flesh and blood. In the stomach and intestines of a European and of a Canadian lynx copper was found. Also in those of the Lesser and Red Coatimondi (*Nasua mexicana* and *rufa*); in those of the Chetah (*Cynailurus guttata*); those of *Canis cancrivorus*; repeatedly in horseflesh, and the flesh of the ox.

To determine whether the copper existed in the soluble portions of ox-flesh or in the insoluble tissues, Liebig's extract of flesh was used; and in this it was detected, as also in boiled ox-flesh.

Birds.—Copper was found in the flesh of the breast of the teal; in the yelk of a hen's egg, as also in the white, but in far less quantity.

Amphibia.—In the geometric tortoise; in the common lizard, lead being also present in greater quantity than that of the copper; in that of the common adder; and in the brown frog.

Fishes.—In the eel and in the haddock, the former containing the largest quantity.

Crustacea.—In the common shrimp, copper was found in abundance. A *Scolopendra italica* left 1·52 gr. of ash, containing ·015 gr. of copper.

Insects.—231·4 grs. of Spanish flies left 12·65 grs. of ash, containing ·0092 gr. of copper.

Arachnida.—Copper was found in the ash of two South American bird-catching spiders.

Annulata.—1157 grs. of fresh earthworm gave 163·5 gr. of ash (with a considerable quantity of earth), containing ·003 gr. of copper.

Among the *Echinodermata*, copper was detected in two small starfishes (*Asterias rubens*); among the *Entozoa*, in two round worms; among the *Polypi*, in a sea-anemone (*Tealia crassicornis*), and in a sponge.

Copper was thus found in animals whenever it was searched for. As animals live in part directly and in part indirectly upon plants, it follows that it must occur in all plants; and as plants derive their contents from the soil or from sea-water, copper must be generally diffused through both of these media. John and Meissner proved the existence of copper in the ashes of plants more than fifty years ago, while Sarzeau detected it in more than two hundred plants, and searched in vain for plants free from it. This result has been more recently confirmed and extended by Commaille. Copper has been repeatedly detected in soils, while Durocher and Malaguti, as also Field and Piesse, have found it in sea-water.

SPECULATIONS UPON A POSSIBLE METHOD OF DETERMINING THE DISTANCE OF CERTAIN VARIABLY COLOURED STARS. BY JOSEPH WHARTON.

Arago conceived the idea of testing the correctness of the corpuscular or emission theory of light by subjecting two rays of different velocities to the same refracting influence; for, as that theory explained the refraction to be due to retardation of velocity caused by the molecular attraction of the refracting medium, rays entering the medium at different velocities should be differently refracted. To get rays of different velocities, he took light from a star toward which the earth in its orbit was moving, and from another star which the earth was moving away from. Supposing the ray to strike the earth from the first of these stars at the real velocity of light plus the velocity of the earth, and that from the other at the real velocity of light minus that of the earth, Arago had two rays entering his refractor at velocities differing by about $\frac{1}{5000}$. Arago found no difference in the refraction of his two rays, and his conclusion thence derived that the corpuscular theory of light is untenable, has been generally accepted, this experiment having in fact been frequently quoted as one of the props of the adverse or undulatory theory.

It seems, however, rather surprising that any great weight should be attached to an apparent disproval, by a single test, of one merely imaginary function of corpuscular light, especially as the test itself is utterly fallacious; for who shall say that retardation by attraction is the only possible means by which emitted light could be refracted? and how can we know that the two stars selected by Arago had either no proper motion of their own, or none of a sort to affect his result?

Perhaps the only cases in which we can be sure of receiving starlight of absolutely different velocities are those of such binary stars the plane of whose orbit is not at right angles with the line from thence to the earth. When that line lies in the plane of such an orbit, and the two stars are situated at right angles to that line, it is clear that the velocity of the rays reaching us from one star exceeds that of the rays from the other by double the speed of those stars in their orbit. That light reaches us at various velocities from all the various stars is of course as certain as that they have proper motions, or that our solar system is moving through space; but the difficulties in the way of gaining any accurate comparison of those velocities are very great.

In reflecting upon the undulatory theory of light, I have been quite unable to conceive how the luminiferous æther could "tremble laterally," as the phrase is, causing vibrations transverse to the line of propagation, without a direct relation existing between those lateral vibrations and the forward impulses by which the phenomena of light are translated. The æther being continuous and material and elastic, being, in a word, capable of sustaining a vibration, a vibratory impulse in it at right angles to the course of a ray of light seems fairly comparable to a lateral displacement of a point in a rope, or to the merely vertical vibration in a water-wave not of translation, or to the vibration of air by a sound, all of which vibrations produce (we may almost say *are*) longitudinal undulations.

But if the lateral vibrations of the æther produce the onward propagation of the ray, then the number of lateral impulses in a second equals the number of forward impulses arriving in a second at a relatively stationary point; and as it is established that the number of lateral impulses varies according to the colour of the light, so the number of forward impulses striking a relatively stationary point (say a retina) must vary with the colour of the light; and if the source of light, instead of being at a constant distance, should rapidly approach the retina, the latter must receive a greater number of impulses per second, and its impression of colour therefore must be correspondingly modified. If the retina and the source of light rapidly separate from each other, the number of impulses striking the retina must on the other hand be diminished, producing the corresponding change in the perceived colour.

Now if we imagine a star emitting white light to approach us in an orbital movement at a sufficient rate of speed, its light should appear to us reddish, changing at the perigee into white, changing again into bluish as the star departs, and again into white at the apogee. There are, however, variable stars whose colours undergo exactly those changes, viz. passing from one colour to its complementary, and back again, with periods of white light intervening. The binary stars, whose colours are frequently complementary to each other, should, under the proper circumstances, exhibit the same circuit of change; but I am not prepared to say that they do so in any case: it is in fact asserted that the larger star is usually red and the smaller one blue.

Supposing this train of thought to be sound, and that by extremely careful observation a difference could be detected in the position of a variably coloured star when it appears red, from its position when blue, we should then be upon the track to calculate its distance. We should need to know the differing rates of light-impulses required to produce the observed colours, the time occupied by the star in passing from one extremity of its course to the other, and the angular distance between those extreme positions. Let us assume that such a variably coloured star has been found, which has a measurable change of position in one line—in other words, the plane of whose orbit coincides with our line of vision; and let us assume that its extreme colours indicate a difference in the rate of arrival of light-impulses (or in other words, a difference in the velocity of the light arriving from that star at the two periods) equal to $2v$: then the actual speed of the star in its orbit $=v$; and as the orbital period of the star has been found $=t$, it follows that $\frac{v \times t}{3 \cdot 1416}$ is the real length of that diameter which is the measured angular distance between the two extreme positions of the star. Knowing the angle and the length of the base which subtends it, we have the distance of the star.

If a pair of binary stars could be found whose colours alternate, and which alternately eclipse each other, the matter would be simplified by so much as accurate measurement of the angle of parallax would be facilitated.

Should it be objected that no such binary stars have been observed, and that no change of position has been noticed in any single stars of variable colour, I can only reply that possibly it may be worth while to direct attention to those points.

Should it be said that this hypothesis of the cause of variable colour in stars would oblige us to believe that rays of different colours are propagated at different velocities, and that this whole suggestion is valueless until those velocities have been determined by direct experiment, I answer that very probably rays of different colours have different velocities, and that to determine them would be a most important achievement.

To conclude, I offer the analogy of the changing tone of a locomotive whistle as you rush past it on another train. Here, as you approach the sound, its impulses reach you more frequently than if its source and you were at rest. At the instant of passing you receive the normal number of impulses, and after passing, the impulses reach you less frequently. The shrill shriek, the real tone, and the low roar in this case are facts which I suppose to be parallel to the red, the white, and the blue light of a star moving swiftly, first toward, and then from us.—Silliman's *American Journal,* September 1865.

ON THE ACTION OF LIGHT UPON SULPHIDE OF LEAD, AND ITS BEARING UPON THE PRESERVATION OF PAINTINGS IN PICTURE-GALLERIES. BY DR. D. S. PRICE[*].

The author's attention was directed to this subject by observing that in the cases in the South Kensington Museum, which are painted with white lead, substances which emitted sulphurous vapours did not cause a darkening of the surface of the case, excepting where it was protected from the direct influence of light. A number of experiments were then tried as to the action of light upon sulphide of lead produced by the action of sulphuretted hydrogen upon lead paint. A board painted white with white-lead was exposed for several hours to the action of sulphuretted hydrogen, until the surface had acquired a uniform brown colour. Plates of glass of different colours were then placed upon the painted surface, one portion being at the same time covered with an opake medium, and another left entirely exposed. The board was then placed facing the light. The glasses employed were red, blue, yellow (silver), violet, and smoke-colour glass. The results exhibited were after an exposure of eight days, and showed that the parts of the board directly exposed to light were bleached; those protected by an opake medium were not acted upon; while with the glasses of different colours intermediate effects were produced, those of the violet glass being most decided. Drying-oils in conjunction with light rapidly bleach sulphide of lead, and boiled oil effects the bleaching still more rapidly. When water-colour is used bleaching takes place, but much more slowly than in the case of oil. After quoting authorities, stating that generally light was advantageous to the preservation of pictures, Dr. Price showed a striking illustration of this fact. He had a picture painted, and then exposed it to the action of sulphuretted hydrogen, until it became sadly discoloured, and to all appearance destroyed. Some strips of paper were laid across the picture so as to cover some parts. The picture, thus partially covered, was exposed to light for a long time. The result, as shown at the Meeting, was very curious indeed—the parts of the picture exposed being perfectly restored, while those protected by the paper remained still discoloured. From his experiments he came to the conclusion that it was advantageous to have picture-galleries well lighted, especially where, as in towns, the atmosphere was charged with sulphur compounds, and that it was quite a mistake to have curtains placed in front of pictures with a view to their protection. In the course of his communication, Dr. Price referred to the use of zinc paint for

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[*] From a communication read at the Meeting of the British Association, Birmingham, September 1865.

houses, and considered it likely to be acted upon, as the paint was rendered soluble by the acids contained in the atmosphere of towns. —*Chemical News*, October 13, 1865.

USE OF MAGNESIUM AS ELECTROMOTOR METAL IN VOLTAIC ELEMENTS. BY M. BULTINCK.

Having observed the great oxidability of magnesium as compared with other electromotor metals, I supposed it might be used advantageously in place of a less oxidizable metal. I made the experiment with a multiplier, taking as standard of comparison copper and zinc in pure distilled water, and I have had the following result.

Taking on the one hand two wires, one of zinc and the other of copper, and connecting both with a multiplier, and then immersing them simultaneously in distilled water, I obtained at the moment of immersion a deflection of 30°, and after five minutes the needle stopped at 10°. On the other hand, with two wires of magnesium and silver respectively, and of absolutely the same dimensions as those of the wires of copper and zinc, I obtained at the moment of immersion a deflection of 90°, and after five minutes' immersion the needle stopped at 28°, showing a difference of 60° in favour of magnesium and silver over zinc and copper.

This first result inspired the desire of having a voltaic element with magnesium instead of zinc; but as I had only a little of this metal in the shape of fine wire, it was limited to making a voltaic chain, which, spite of its smallness, has given surprising results. It is constructed as follows:—

It consists of three pieces—a small piece of caoutchouc 14 millims. in length and 4 millims. broad by 3 millims. thick, a magnesium wire 35 millims. in length, and a silver wire of the same dimensions as the magnesium wire. The magnesium and the silver wire are folded upon themselves in two equal parts; then each wire thus folded is put astride one of the ends of the piece of caoutchouc, but so that one (the magnesium, for instance) is across the breadth and the other across the thickness, and that the part which forms the fold projects sufficiently to serve as ring. The whole is well fixed by means of a double knot of silk thread.

Twenty such elements joined form the chain. As regards the effects, by placing the chain in water without any addition of salt or acid, all the effects possible with a Pulvermacher's chain are obtained—the only difference being that Pulvermacher's chain must be immersed in acidulated or saline water. By the use of magnesium a galvanic current can thus be formed without the use of any acid or salt.—*Comptes Rendus*, October 9, 1865.

ON THE INVERSION OF THE SPECTRA OF METALLIC VAPOURS. BY LOTHAR MEYER.

The author describes the following simplified method of Madan's experiment[*] for showing the inversion of the spectra of metallic

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[*] Phil. Mag. S. 4. vol. xxix. p. 338.

vapours. It is simply necessary to melt sodium in the open air in a flat iron dish over a small gas-flame, and, the spectrum apparatus being placed on a somewhat inclined plane, the slit of the telescope is directed obliquely on the ignited metallic surface. If the metal becomes covered with a coating, it is heated by a Bunsen's flame directed downwards upon it. The surface is thus kept clean.

If the flame under the dish is very small, a continuous spectrum only is seen. With stronger heating, white vapours are disengaged and the black lines occur. If the flame is allowed to surround the small dish, the line is observed to be yellow. It is easy also to regulate the flame so that the line is black with yellow edges.—*Zeitschrift für Chemie*, vol. i. part 15.

NOTE ON THE VELOCITY OF SOUND, AND ON THE MECHANICAL ENERGY OF CHEMICAL ACTIONS. BY DR. SCHRÖDER VAN DER KOLK.

Dear Sir, Zutphen, August 17, 1865.

I see from the July Number of the Philosophical Magazine that you have done me the honour to translate into English my paper on the Velocity of Sound, for which I beg to thank you. Since, however, a few misprints occur in the German version, in the formulæ as well as in the text, page 3, I take the liberty of sending you a corrected copy. In the case of the three mistakes which you have yourself noticed, you are perfectly right: they were clerical errors in the manuscript, but fortunately they do not affect the result. I sent the calculation of the Mechanical Equivalent to Poggendorff as early as February, but it has not yet appeared. The value found is 422·10, which agrees pretty well with Joule's number.

I am also greatly indebted to you for your remarks upon my paper "On the Mechanical Energy of Chemical Actions." Your remarks on page 273 in regard to phosphorus, and on page 276 about the oxide of copper, are correct. With regard to oil of turpentine, I must, however, observe that I did not by any means assert that it remains unaltered; but, as I had read somewhere or other that such was the case, I was wishful to anticipate the objection that in this instance a body passed of itself into a higher state of energy, and left it an open question whether it was so or not. If it is altered, so much the better. On page 278 you ask why should silver impart more heat to the water than porcelain would do? I never said that it would; porcelain, however, would not fulfil the second condition, namely, acting differently on the constituents of the water. On the other hand, a body must satisfy both conditions in order to decompose the water. Your remark on page 280 is quite correct; I ought to have added that I here used the expressions stronger and weaker base, as well as greater and less affinity, in the sense which they bear in common language; for in what follows I point out the inadmissible character of these distinctions, as well as the difficulty there is in giving a correct definition of them.

As you have studied my paper so accurately, I was unwilling to omit communicating to you the above, from which you will have seen that in reality I am perfectly at one with you.

I have since found some additional applications of this theorem which I hope shortly to publish. Amongst others, Rose observes (Pogg. *Ann.*, vol. ciii. p. 311) that some minerals, for instance Gadolinite, when heated to a certain temperature, begin suddenly to glow in consequence of heat being set free. When they are heated again, after being cooled, this glowing is not repeated. He discusses the question whether this could be connected with a change of specific heat; but evidently it is nothing but a transition from a state of more to a state of less energy. Perhaps, however, we may conclude from this that this substance could not have been formed by fire, at least not if it occurs in nature at the surface, but that it is rather of Neptunian origin.

* * * *

Yours, &c.,
H. W. Schröder v. d. Kolk.

To Professor G. C. Foster.

The following are the additional errata indicated by Dr. Schröder van der Kolk in his paper "On the Velocity of Sound" (Phil. Mag. S. 4. vol. xxx. p. 34, July 1865).

Page 35, line 11 from bottom, *for* I was nevertheless convinced *read* I was nevertheless not convinced.

— 35, — 9 from bottom, *dele* not.

— 38, — 8 from bottom,

$$for\ s=\sqrt{\frac{gH\beta}{b}}\left\{1+\frac{\gamma(\gamma+1)-2}{4\sqrt{\gamma}}\cdot\frac{\Delta V}{V_0}\right\}\ .\ .\ .\quad (2)$$

$$read\ s=\sqrt{\frac{gH\beta}{b}}\gamma\left\{1+\frac{\gamma(\gamma+1)-2}{4\gamma}\cdot\frac{\Delta V}{V_0}\right\}\ .\ .\quad (2)$$

— 39, line 9 from bottom,

$$for\ s=\sqrt{\frac{gH\beta}{b}}\gamma\left\{1+\frac{\gamma(\gamma+1)-2}{4\sqrt{\gamma}}\cdot\frac{\Delta V R^2}{V_0\rho^2}\right\},$$

$$read\ s=\sqrt{\frac{gH\beta}{b}}\gamma\left\{1+\frac{\gamma(\gamma+1)-2}{4\gamma}\cdot\frac{\Delta V R^2}{V_0\rho^2}\right\}.$$

— 39, line 5 from bottom,

$$for\ \frac{\gamma(\gamma+1)-2}{4\sqrt{\gamma}}\cdot\frac{\Delta V}{V_0}=\lambda^2,$$

$$read\ \frac{\gamma(\gamma+1)-2}{4\gamma}\cdot\frac{\Delta V}{V_0}=\lambda^2.$$

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

[FOURTH SERIES.]

DECEMBER 1865.

LVI. Professor HELMHOLTZ *on Ice and Glaciers.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

I HAVE been recently favoured by Professor Helmholtz with a *brochure* entitled "Popular Scientific Lectures," from the preface of which I learn with great satisfaction that other discourses, of a cast similar to those here placed before the public, may be expected to follow. The present pamphlet contains four lectures,—the first, "On the Relation of the Natural Sciences to Science in general;" the second, "On Goethe's labours in Natural Science;" the third, "On the Physiological Origin of Musical Harmony;" and the fourth, "On Ice and Glaciers." The pamphlet is in German, and it is much to be desired that some competent person should undertake its translation into English.

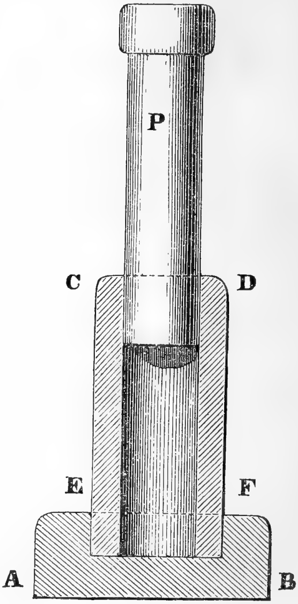
I turned with natural interest to the last-mentioned discourse to see how my notions and experiments on the formation and motion of glaciers were regarded by so eminent a man. And as it can hardly be doubted that the subject will also interest many of the readers of the Philosophical Magazine, I will here endeavour to give a summary of the scientific portion of the lecture.

Professor Helmholtz refers the cold of the upper regions of the atmosphere to the causes generally assigned; but he adds a remark important at the present moment, when the origin of the hot wind called Föhn in Switzerland is the subject of so much discussion. This wind, as Helmholtz justly observes, may not only be a cold wind upon the mountain summits, but a *wet* one. It is deprived of its moisture upon the heights, and it is warmed by its subsequent fall into the valleys. The heat and dryness of the Föhn are therefore no proof that its origin is Sahara, and not the Atlantic Ocean.

It will probably be remembered that I deduced the formation of glaciers, and their subsequent motion through valleys of varying width and flexure, from the fact that when two pieces of ice are pressed together they freeze together at their places of contact. This fact was first mentioned to me verbally by its discoverer, Mr. Faraday. Soon afterwards, and long before I had occasion to reflect upon its cause, the application of the fact to the formation and motion of glaciers flashed upon me. Snow was in the yard of the Royal Institution at the time; stuffing a quantity of it into one of the steel moulds which I had previously employed to demonstrate the influence of pressure on magnetic phenomena, I squeezed the snow, and had the pleasure of seeing it turn out from the mould as a cylinder of translucent ice. I immediately joined Mr. Faraday, and expressed the conviction that his little outlying experiment would be found to constitute the basis of a true theory of glaciers. It became subsequently known to me that the Messrs. Schlagintweit had made a similar experiment with snow; but they did not connect with it the applications which suggested themselves to me, and which have since been developed into a theory of glacier-motion.

A section of the mould used in the experiment above referred to is given in fig. 1. A B is the solid base of the mould; C D E F a hollow cylinder let into the base; P is the solid plug used to compress the snow. When sufficiently squeezed, the bottom, A B, is removed, and the cylinder of ice is pushed out by the plug. The mould closely resembles one of those employed by Professor Helmholtz.

Fig. 1.



The subsequent development of the subject by the moulding of ice into various forms by pressure is too well known to need dwelling upon here. In applying these results to glaciers, I dwelt with especial emphasis upon the fact that while the power of being moulded by *pressure* belonged in an eminent degree to ice, the power of yielding by stretching to a force of *tension* was wanting. On this point Prof. Helmholtz speaks as follows:—"Tyndall in particular maintained, and proved by calculation and measurement, that the ice of a glacier does not stretch in the smallest degree when subjected to tension—that when sufficiently strained it always breaks;" and he adds in another place, that the property thus revealed establishes "an essential difference between a stream of ice, and one of lava, tar, honey, or mud."

In the beautiful experiments of M. Tresca recently executed, the power of ice to mould itself under pressure has been very strikingly illustrated. Professor Helmholtz also, in the presence of his audiences at Heidelberg and Frankfort, illustrated this property in various ways. From snow and broken fragments of ice he formed cakes and cylinders; and uniting the latter, end to end, he permitted them to freeze together to long sticks of ice. Placing, moreover, in a suitable mould a cylinder of ice of the shape represented in fig. 2, he squeezed it into the cake represented in fig. 3. In fact he corroborated, by a series of striking experimental devices of his own, the results previously obtained by myself.

Fig. 2.

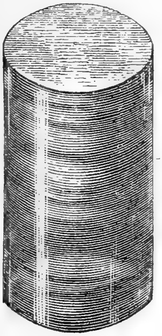
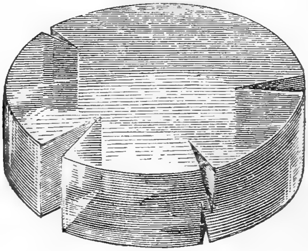


Fig. 3.



With regard to the application of these results to the phenomena of glaciers, Professor Helmholtz, after satisfying himself of the insufficiency of other hypotheses, thus finally expresses his conviction:—"I do not doubt that Tyndall has assigned the essential and principal cause of glacier-motion, in referring it to fracture and regelation."

It is perhaps worth stating that the term "regelation" was first introduced in a paper published by Mr. Huxley and myself more than seven years after the discovery of the fact by Faraday, and that it was suggested to us by Dr. Hooker, now Director of the Royal Gardens at Kew. As already remarked, the formation and motion of glaciers, and other points of a kindred nature, had been referred to regelation long before I occupied myself with the cause of regelation itself. This latter question is not once referred to in the memoir in which the regelation theory was first developed[*]. The inquiries, though related, were different. In referring the motion of glaciers to a fact experimentally demonstrated, I referred it to its *proximate* cause. To refer that cause to *its* physical antecedents formed the subject of a distinct inquiry, in which, because of my belief in the substantial correctness of Mr. Faraday's explanation, I took comparatively little part.

Five persons, however, mingled more or less in the inquiry—viz. Professor Faraday, Principal Forbes, Professor James Thomson, Professor William Thomson, and myself[†]. Professor James Thomson explained regelation by reference to an important deduction, first drawn by him[‡], and almost simultaneously by Professor Clausius[§], from the mechanical theory of heat. He had shown it to be a consequence of this theory that the freezing-point of water must be lowered by pressure; that is to say, water when subjected to pressure will remain liquid at a temperature below that at which it would freeze if the pressure were removed. This theoretic deduction was confirmed in a remarkable manner by the experiments of his brother, Professor William Thomson[||]. Regelation, according to Professor Thomson's theory, was thus accounted for:—"When two pieces of ice are pressed together, or laid the one upon the other, their compressed parts liquefy. The water thus produced has rendered latent a portion of the heat of the surrounding ice, and must therefore be lower than 0° C. in temperature. On escaping from the pressure this water refreezes and cements the pieces of ice together." I always admitted that this explanation dealt with a "true cause." But considering the infinitesimal magnitude of the pressure sufficient to produce regelation, in common with Professor Faraday and Principal Forbes I deemed the cause an insufficient one. Professor James Thomson, moreover, grounded upon the foregoing theory of regelation a theory of glacier-motion, in which he ascribed the changes of form which a glacier undergoes to the incessant liquefaction of the ice at places where the pressure is intense, and the refreezing, in other positions, of the water thus produced[¶]. I endeavoured to show at the time that this theory was inapplicable to the facts. Professor Helmholtz has recently

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[*] Phil. Trans. vol. cxlvii. p. 327.

[†] Proc. Roy. Soc. vol. ix. p. 141; and vol. x. p. 152. Phil. Mag. S. 4. vol. xvi. pp. 347 & 544; and vol. xvii. p. 162.

[‡] Proc. Roy. Soc. Edinb. February 1850.

[§] Pogg. *Ann.* vol. lxxxi. p. 168.

[||] Phil. Mag. August 1850.

[¶] Proc. Roy. Soc. vol. viii. p. 455.

subjected it to the test of experiment, and the conclusions which he draws from his researches are substantially the same as mine.

Thus, then, as regards the incapacity of ice to stretch in obedience to tension, and its capacity to be moulded to any extent by pressure—as regards the essential difference between a glacier, and a stream of lava, honey, or tar—as regards the sufficiency of pressure and regelation to account for the formation of glaciers, and of fracture and regelation to account for their motion—as regards, finally, the insufficiency of the theory which refers the motion to liquefaction by pressure, and refreezing, the views of Professor Hemholtz and myself appear to be identical.

But the case is different with regard to the cause of regelation itself. Here Professor Helmholtz, like M. Jamin*, accepts the clear and definite explanation of Professor James Thomson as the most satisfactory that has been advanced; and he supports this view by an experiment so beautiful that it cannot fail to give pleasure even to those against whose opinions it is adduced. But before passing to the experiment, which is described in the Appendix to the lecture, it will be well to give in the words of Professor Helmholtz the views which he expresses in the body of his discourse.

"You will now ask with surprise," he says, "how it is that ice, the most fragile and brittle of all known solid substances, can flow in a glacier like a viscous mass; and you may perhaps be inclined to regard this as one of the most unnatural and paradoxical assertions that ever was made by a natural philosopher. I will at once admit that the inquirers themselves were in no small degree perplexed by the results of their investigations. But the facts were there, and could not be dissipated by denial. How this kind of motion on the part of ice was possible remained long an enigma—the more so as the known brittleness of ice also manifested itself in glaciers by the formation of numerous fissures. This, as Tyndall rightly maintained, constituted an essential difference between the ice-stream, and a stream of lava, tar, honey, or mud.

"The solution of this wonderful enigma was found—as is often the case in natural science—in an apparently remote investigation on the nature of heat, which forms one of the most important conquests of modern physics, and which is known under the name of the mechanical theory of heat. Among a great number of deductions as to the relations of the most diverse natural forces to each other, the principles of the mechanical theory of heat enable us to draw certain conclusions regarding the dependence of the freezing-point of water on the pressure to which the ice and water are subjected."

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* *Traité de Physique,* vol. ii. p. 105.

Professor Helmholtz then explains to his audience what is meant by latent heat, and points out that, through the circulation of water in the fissures and capillaries of a glacier, its interior temperature must remain constantly at the freezing-point of water.

"But," he continues, "the temperature of the freezing-point of water can be altered by pressure. This was first deduced by James Thomson, and almost simultaneously by Clausius, from the mechanical theory of heat; and by the same deductions even the magnitude of the change may be predicted. For the pressure of every additional atmosphere, the freezing-point sinks 0°·0075 C. The brother of the gentleman first named, William Thomson, the celebrated Glasgow physicist, verified experimentally the theoretic deduction by compressing a mixture of ice and water in a suitable vessel. The mixture became colder and colder as the pressure was augmented, and by the exact amount which the mechanical theory of heat required.

"If, then, by pressure a mixture of ice and water can be rendered colder without the actual abstraction of heat, this can only occur by the liquefaction of the ice and the rendering of heat latent. And this is the reason why pressure can alter the point of congelation * * * *.

"In the experiment of William Thomson just referred to, ice and water were enclosed in a solid vessel from which nothing could escape. The case is somewhat different when, as in the case of a glacier, the water of the compressed ice can escape through fissures. In this case the ice is compressed, but not the water which escapes. The pressed ice will become colder by a quantity corresponding to the lowering of its freezing-point by the pressure. But the freezing-point of the uncompressed water is not lowered. Here, then, we have ice colder than 0° C. in contact with water at 0° C. The consequence is, that round the place of pressure the water will freeze and form new ice, while, on the other hand, a portion of the compressed ice continues to be melted (während dafür ein Theil des gepressten Eises fortschmilzt).

"This occurs, for instance, when two pieces of ice are simply pressed together. By the water which freezes at the points of contact they are firmly united to a continuous mass. When the pressure is considerable and the chilling consequently great, the union occurs quickly, but it may also be effected by a very slight pressure if sufficient time be afforded. Faraday, who discovered this phenomenon, named it the *regelation of ice*[*]. Its explanation has given rise to considerable controversy: I have laid that

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[*] I have already corrected this slight inadvertence.

explanation before you which I consider to be the most satisfactory."

In the Appendix, Professor Helmholtz returns to the subject thus handled in the body of his discourse. "The theory of the regelation of ice," he observes, "has given rise to a scientific discussion between Faraday and Tyndall on the one hand, and James and William Thomson on the other. In the text of this lecture I have adopted the theory of the latter, and have therefore to justify myself for so doing." He then analyzes the reasonings on both sides, points out the theoretic difficulties of Faraday's explanation, shows what a small pressure can accomplish if only sufficient time be granted to it, draws attention to the fact that when one piece of ice is placed upon another the pressure is not distributed over the whole of the two appressed surfaces, but is concentrated on a few points of contact. He also holds with Professor James Thomson, that in an experiment devised by Principal Forbes even the capillary attraction exerted between two plates of ice is sufficient, in due time, to produce regelation. To illustrate the slow action of the small differences of temperature which here come into play Professor Helmholtz made the following experiment, to which reference has been already made.

"A glass flask with a drawn-out neck was half filled with water, which was boiled until all the air above it was driven out. The flask was then hermetically sealed. When cooled, the flask was void of air, and the water within it freed from the pressure of the atmosphere. As the water thus prepared can be cooled considerably below 0° C. before the first ice is formed, while when ice is in the flask it freezes at 0° C. [why ?], the flask was in the first instance placed in a freezing-mixture until the water was changed into ice. It was afterwards permitted to melt slowly in a place the temperature of which was +2° C., until the half of it was liquefied.

"The flask thus half filled with water with a disk of ice swimming on it was placed in a mixture of ice and water, being quite surrounded by the mixture. After an hour the disk within the flask was frozen to the glass. By shaking the flask the disk was liberated, but it froze again as often as the shaking was repeated. The flask was permitted to remain for eight days in the mixture, which was preserved throughout at a temperature of 0° C. During this time a number of very regular and sharply-defined ice-crystals were formed, and augmented very slowly in size. This is perhaps the best method of obtaining beautifully formed crystals of ice.

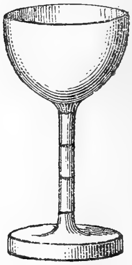
"While, therefore, the outer ice which had to support the pressure of the atmosphere slowly melted, the water within the flask whose freezing-point, on account of a defect of pressure, was 0°·0075 C. higher, deposited crystals of ice. The heat abstracted from the water in this operation had, moreover, to pass through the glass of the flask, which, together with the small difference of temperature, explains the slowness of the freezing-process."

A single additional condition in connexion with this beautiful experiment I should like to have seen fulfilled—namely, that the water in which the flask was immersed, as well as that within it, should be purged of its air by boiling. It is just possible that the point of congelation may not be entirely independent of the presence of air in the water.

The revival of this subject by Professor Helmholtz has caused me to make a few experiments on the moulding and regelation of ice. The following experiment illustrates both:—A quantity of snowy powder was scraped from a block of clear ice and placed in a boxwood mould having a shape like the foot of a claret-glass. The ice-powder was compressed by an hydraulic press, and a clear mass of ice of the shape shown in section at the bottom of fig. 4 was the result. In another mould the same powder was squeezed so as to form small cylinders, three of which are shown separate in fig. 4. A third mould was then employed to form a cup of ice, which is shown at the top of fig. 4. Bringing all the parts into contact, they were cemented through regelation to form the claret-glass sketched in fig. 5, from which several draughts of wine might be taken if the liquid were cooled sufficiently before pouring it into the cup of ice.

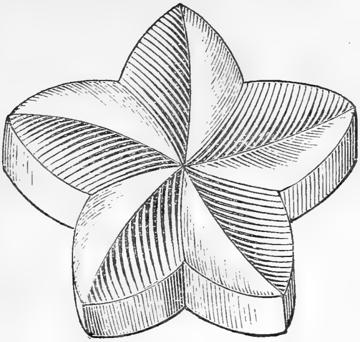
Fig. 4.

Fig. 5.



There are brass shapes used for the casting of flowers and other objects which answer admirably for experiments on the regelation of ice. One of them was purchased for me by Mr. Becker. Ice-powder squeezed into it, regelated to a solid mass and came from the mould in the sharply defined form sketched in fig. 6.

Fig. 6.



I placed a small piece of ice in warm water and forced it underneath the water by a second piece. The submerged morsel was so small that the vertical pressure was almost infinitesimal. It froze, notwithstanding, to the under surface of the superior piece of ice. Two pieces of ice were placed in a basin of warm water, and allowed to come together. They froze as soon as they touched each other. The parts surrounding the place of contact rapidly melted away, but the two pieces continued for a time united by a narrow bridge of ice. The bridge finally melted away, and the pieces were for a moment separated. But it is well known that bodies which water wets, and against which it rises by capillary attraction, move together when caused to swim upon water. The ice morsels did so, and immediately regelation set in. A new bridge was formed, which in its turn was dissolved, and the pieces closed up as before. Thus a kind of pulsation was kept up by the two pieces of ice. They touched, froze; a bridge was formed and melted, leaving an interval between the pieces. Across this interval they moved, touched, froze, the same process being repeated over and over again. We have here the explanation of the curious fact, that when several large lumps of ice are placed in warm water and allowed to touch each other, regelation is maintained among them as long as they remain undissolved. The final fragments may not be the one-hundredth part of the original ones in size; through the process of closing up just described, they incessantly lock themselves together until they finally disappear. What is true of ice-fragments in a basin of water, is also true of the ice-blocks in the Märgelin See; and probably, if attention were directed to it, the play of regelation might be observed in the icebergs of the ocean.

With a certain amount of skill, numerous interesting observations may be made on the regelation of floating fragments of ice.

According to Professor James Thomson's theory, to produce regelation the pieces of ice have to exercise pressure, to draw from the surrounding ice the heat necessary for the liquefaction of the compressed part; and then this water must escape and be refrozen. All this requires time. In the foregoing experiments, moreover, the water liquefied by the pressure issued into the surrounding warm water, but notwithstanding this the floating fragments regelated in a moment. It is not necessary that the touching surfaces should be flat; for in this case a film of water might be supposed to exist between them of the temperature 0° C. The surfaces in contact may be convex: they may be virtual points that touch each other, clasped all round by the warm liquid, which is rapidly dissolving them as they approach each other. Still they freeze immediately when they touch.

There are two points urged by Helmholtz—one in favour of the view he has adopted, and the other showing a difficulty associated with the view of Faraday—on which a few words may be said. "I found," says Helmholtz, "the strength and rapidity of the union of the pieces of ice in such complete correspondence with the amount of pressure employed, that I cannot doubt that the pressure is actually the sufficient cause of the union."

But, according to Mr. Faraday's explanation, the strength and quickness of the regelation must also go hand in hand with the magnitude of the pressure employed. Helmholtz rightly dwells upon the fact that the appressed surfaces are usually not perfectly congruent—that they really touch each other in a few points only, the pressure being thus concentrated. Now the effect of pressure exerted on two pieces of ice at a temperature of 0° C. is not only to lessen the thickness of the liquid film between the pieces, but also to flatten out the appressed points, and thus to spread the film over a greater space. On both theories, therefore, the strength and quickness of the regelation ought to correspond to the magnitude of the pressure.

The difficulty referred to above is thus stated by Helmholtz:—"In the explanation given by Faraday, according to which the regelation is caused by a contact action of ice and water, I find a theoretic difficulty. By the freezing of the water a very sensible quantity of heat would be set free; and it does not appear how this is to be disposed of."

On the part of those who accept Faraday's explanation, the answer here must be that the free heat is diffused through the adjacent ice. But against this it will doubtless be urged that ice already at a temperature of 0° C. cannot take up more heat without liquefaction. If this be true under all circumstances, Faraday's explanation must undoubtedly be given up. But the essence of that explanation seems to be that the interior portions of a mass of ice require a higher temperature to dissolve them than that sufficient to cause fusion at the surface. To be perfectly distinct, let a beam of solar heat, or a beam from the electric lamp, be sent through a mass of ice. The substance breaks up into those six-petaled liquid flowers which have been described elsewhere. The flowers expand as the beam continues to act, the energy of the absorbed portion of the beam being almost wholly expended in enlarging the flowers formed during the first few seconds of exposure, and not in forming new flowers. Now the assumption involved in Faraday's theory is, that, before a flower makes its appearance in the interior, the ice there must have been raised to a temperature higher than 0° C., while at the surface the ice fuses at this temperature. When therefore two moist surfaces of ice at the temperature 0° are pressed together, and when, in virtue of the contact action assumed by Faraday, the film of water between them is frozen, the adjacent ice (which is now in the interior, and not at the surface as at first) is in a condition to withdraw by conduction, and without prejudice to its own solidity, the small amount of heat set free. Once granting the contact action claimed by Faraday, there seems to be no difficulty in disposing of the heat rendered sensible by the freezing of the film.

When the year is advanced, and after the ice imported into London has remained a long time in store, if closely examined, parcels of liquid water will be found in the interior of the mass. I enveloped ice containing such water-parcels in tinfoil, and placed it in a freezing-mixture until the liquid parcels were perfectly congealed. Removing the ice from the freezing-mixture, I placed it, covered by its envelope, in a dark room, and found, after a couple of hours' exposure to a temperature somewhat over 0° C., the frozen parcels again liquid. The heat which was sufficient to fuse this interior ice, passed through the firmer surrounding ice without the slightest visible prejudice to its solidity. But if the freezing temperature of the ice-parcels be 0° C., then the freezing temperature of the mass surrounding them must be higher than 0° C., which is what the explanation of Faraday requires.

In a former paragraph I have attached to the description of a precaution used by Professor Helmholtz the query "why?" He states that water freed of its air sinks, without freezing, to a temperature far below 0° C.; while when a piece of ice is in the same water it cannot so sink in temperature, but is invariably deposited in the solid form at 0° C. This surely proves ice to possess a special power of solidification over water. It is needless to say that the fact is general—that a crystal of any salt placed in a saturated solution of the salt always provokes crystallization. Applying this fact to the minute film of water enclosed between two appressed surfaces of ice, it seems to me in the highest degree probable that the contact action of Faraday will set in, that the film will freeze and cement the pieces of ice together.

Apart from the present discussion, the following observation is perhaps worth recording. It is well known that ice during a thaw disintegrates so as to form rude prisms whose axes are at right angles to the planes of freezing. I have often observed this action on a large scale, during the winters that I spent as a student on the banks of the Lahn. The manner in which these prisms are in some cases formed is extremely interesting. On close inspection, a kind of cloudiness is observed in the interior of a mass of apparently perfect ice. On closer inspection, this cloudiness appears arranged in striæ at right angles to the planes of freezing, and when the direction of vision is across these planes the ends of the striæ are apparent. The spaces between the striæ are composed of clear unclouded ice. The objects which produce this cloudiness are exceedingly small, but when duly examined they turn out to be piles of minute liquid flowers, whose planes are at right angles to the direction of the striæ.

JOHN TYNDALL.

Royal Institution,
November 1865.

Since writing the above, I have been favoured with a copy of a Discourse delivered by Professor De la Rive, at the opening of the forty-ninth meeting of the Société Helvétique, which assembled this year at Geneva. From this admirable *résumé* of our present knowledge regarding glaciers I make the following extract, which, together with those from the lecture of Helmholtz, will show sufficiently how the subject is now regarded by competent men:—"Such, gentlemen," says M. De la Rive, "is a description of the phenomena of glaciers, and it now remains to explain them, to consult observation, and deduce from it the fundamental character of the phenomena. Observation teaches us that gravity is the motive force, and that this force acts upon a solid body —ice—imparting to it a slow and continuous motion. What are we to conclude from this? That ice is a solid which possesses the property of flowing like a viscous body—a conclusion which appears very simple, but which was nevertheless announced for the first time hardly five and twenty years ago by one of the most distinguished philosophers of Scotland, Professor James D.

Forbes. This theory, for it truly is a theory, basing itself on facts as numerous as they are well observed, enunciates the principle that ice possesses the characteristic properties which belong to plastic bodies. Although he did not directly prove it, to Professor Forbes belongs not the less the great merit of insisting on the plasticity of ice, before Faraday, in discovering the phenomenon of regelation, enabled Tyndall to prove that the plasticity was real, at least partially.

"The experiment of Faraday is classical in connexion with our subject. It consists, as you know, in this, that if two morsels of ice be brought into contact in water, which may be even warm, they freeze together. Tyndall saw very quickly the application of Faraday's experiment to the theory of glaciers; he comprehended that, since pieces of ice could thus solder themselves together, the substance might be broken, placed in a mould, compressed, and thus compelled to take the form of the cavity which contained it. A wooden mould, for example, embraces a spherical cavity; placing in it fragments of ice and squeezing them, we obtain an ice sphere; placing this sphere in a second mould with a lenticular cavity and pressing it, we transform the sphere into a lens. In this way we can impart any form whatever to ice.

"Such is the discovery of Tyndall, which may well be thus named, particularly in view of its consequences. For all these moulds magnified become the borders of the valley in which a glacier flows. Here the action of the hydraulic press which has served for the experiments of the laboratory is replaced by the weight of the masses of snow and ice collected on the summits, and exerting their pressure on the ice which descends into the valley. Supposing, for example, between the spherical mould and the lenticular one, a graduated series of other moulds to exist, each of which differs very little from the one which precedes and from that which follows it, and that a mass of ice could be made to pass through all these moulds in succession, the phenomenon would then become continuous. Instead of rudely breaking, the ice would be compelled to change by insensible degrees from the spherical to the lenticular form. It would thus exhibit a plasticity which might be compared to that of soft wax. But ice is only plastic under *pressure*; it is not plastic under *tension*: and this is the important point which the vague theory of plasticity was unable to explain. While a viscous body, like bitumen or honey, may be drawn out in filaments by tension, ice, far from stretching in this way, breaks like glass under this action. These points well established by Tyndall, it became easy for him to explain the mechanism of glaciers, and by the aid of an English geometer, Mr. William Hopkins, to show how the direction of the crevasses of a glacier are the necessary consequences of its motion."

I have quite recently had a mould constructed for me by Mr. Becker*, and yesterday (November 16th) made with it an experiment which, on account of the ease with which it may be performed, will interest all those who care about exhibiting in a striking and instructive manner the effects of regelation. The mould is shown in fig. 7. It consists of two pieces of cast iron, A B C and D F G, slightly wedge-shaped and held together by the iron rectangle R E which is slipped over them. The inner face of A B C is shown in fig. 8. In it is hollowed out a semi-ring M N, with a semicylindrical passage O leading into it. The inner face of D F G is similarly hollowed out, so that when both faces are placed together, as in fig. 7, they enclose a ring 4 inches in external diameter, from M to N, and $\frac{3}{4}$ of an inch in thickness, with the passage O, 1 inch in diameter, into which fits the polished iron plug P leading into it. At *q* and *r*, fig. 8, are little pins which, fitting into holes corresponding to them, keep the slabs A B C and D F G from sliding over each other.

Fig. 7.

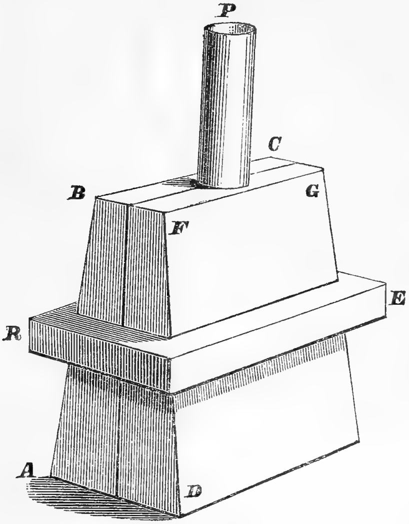
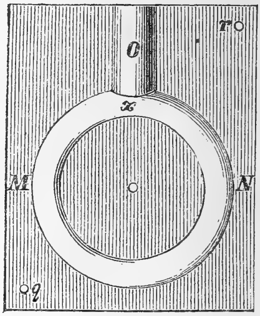


Fig. 8.



The mould being first cooled by placing it for a short time in a mixture of ice and water, fragments of ice are stuffed into the

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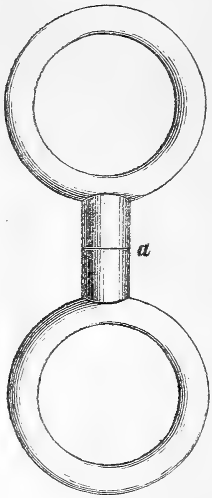
* I am continually indebted to this able mechanician for prompt and intelligent aid in the carrying out of my ideas.

orifice O and driven down with a hammer by means of the plug P. The bruised and broken ice separates at *x*, one portion going to the right, the other to the left. Driving the ice thus into the mould, piece after piece, it is finally filled. By removing the rectangle R E, the two halves of the mould are then separated, and a perfect ring of ice is found within. Two such rings soldered by regelation at *a* are shown in fig. 9. It would be easy thus to construct a chain of ice. An hydraulic press may of course be employed in this experiment, but it is not necessary; with the hammer and plug beautiful rings of ice are easily obtained by the regelation of the crushed fragments.

Fig. 9.



I have now to add the description of an experiment which suggested itself to my ingenious friend Mr. Duppa when he saw the ice-rings just referred to, and which was actually executed by him yesterday (the 16th) in the laboratory of the Royal Institution. Pouring a quantity of plaster of paris into a proper vessel, an ice-ring was laid upon the substance, an additional quantity of the cement being then poured over the ring. The plaster "set," enclosing the ring within it: the ring soon melted, leaving its perfect matrix behind. The mould was permitted to dry, and, molten lead being poured into the space previously occupied by the ice, a leaden ring was produced. Now ice can be moulded into any shape: statuettes, vases, flowers, and innumerable other ornaments can be formed from it. These enclosed in cement, in the manner suggested by Mr. Duppa, remain intact sufficiently long to enable the cement to set around them; they afterwards melt and disappear, leaving behind them perfect plaster moulds, from which casts can be taken.—J. T.

LVII. *On Thermodynamic and Metamorphic Functions, Disgregation, and Real Specific Heat.* *By* W. J. MACQUORN RANKINE, *C.E., LL.D., F.R.SS.L. & E.*[*]

1. THE following paper relates to certain differences which Professor Clausius considers to exist between his views and mine of some questions in thermodynamics—differences

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[*] Communicated by the Author.

relating, not to the two fundamental laws of thermodynamics, but to some of their applications. (See in particular the papers of Professor Clausius in Poggendorff's *Annalen*, vol. xcvi. p. 73, in the 'Proceedings' of the Swiss "Naturforschender Gesellschaft" for 1863 and 1865, and in the *Bibliothèque Universelle* (Geneva) for October 1865.)

2. According to the Second Law of Thermodynamics, the quantity of heat which a body receives or gives out during any given indefinitely small change of figure and dimensions is expressed in every case as follows,

$$\tau d\phi,$$

in which τ is the absolute temperature and ϕ the "*Thermodynamic Function.*"

The thermodynamic function is made up of two parts, as follows,

$$\phi = \mathfrak{k} \text{ hyp.} \log \tau + \mathrm{F};$$

so that

$$\tau d\phi = \mathfrak{k} d\tau + \tau d\mathrm{F}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

In these expressions $\mathfrak{k}$ is the *real specific heat* of the substance, being the part of the specific heat due to the energy of molecular motions alone; and F is what I have proposed to call the "*metamorphic function*;" so that $\tau d\mathrm{F}$ is the quantity of heat which is transformed into mechanical work, whether external or internal, during any indefinitely small change in the condition of the body. In all those expressions quantities of heat are supposed to be expressed in units of mechanical work.

3. Let x, y, z, &c. denote changes of different kinds in the dimensions and figure of a substance, and X, Y, Z, &c. the forces of the nature of elastic stress by which such changes are promoted; so that $\mathrm{X}dx + \mathrm{Y}dy + \mathrm{Z}dz +$ &c. is the external work performed during a given indefinitely small change of figure and dimensions. Then the metamorphic function F is the integral of the following set of differential equations*:—

$$\frac{d\mathrm{F}}{dx} = \frac{d\mathrm{X}}{d\tau}; \quad \frac{d\mathrm{F}}{dy} = \frac{d\mathrm{Y}}{d\tau}; \quad \frac{d\mathrm{F}}{dz} = \frac{d\mathrm{Z}}{d\tau}; \quad \text{\&c.}$$

When, as is the case in the most frequent class of questions in thermodynamics, the change under consideration is simply a change of volume, the preceding set of differential equations are reduced to the following single equation,

$$\frac{d\mathrm{F}}{dv} = \frac{dp}{d\tau}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

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* See Proceedings of the Philosophical Society of Glasgow for 1853; Edinburgh Philosophical Journal for 1855.

in which v is the volume of the substance, and p its elastic pressure.

4. In the value of the metamorphic function F which I gave in the Transactions of the Royal Society of Edinburgh for 1850–51, I proceeded on the supposition that every substance is capable of passing into the state of perfect gas at every temperature. If such is the fact, the value of F is as follows:—

$$\mathrm{F}=\int\frac{dp}{d\tau}\,dv. \qquad \ldots\ldots \qquad (3)$$

5. Professor Clausius points out that, if the substance under consideration is at any temperature incapable of passing into the state of perfect gas, the preceding value of the integral of equation (2) is incomplete, and requires the addition of a function of the temperature to complete it. The value which he gives in units of mechanical work for the function which he calls "disgregation" (being that which corresponds, in his researches, to the "Metamorphic Function" in mine) is as follows,

$$\frac{\mathrm{Z}}{\mathrm{A}}=\int\frac{d\mathrm{I}}{\tau d\tau}\,.\,d\tau+\int\frac{dp}{d\tau}\,dv\,; \qquad \ldots\ldots \qquad (4)$$

in which A is the thermic equivalent of a unit of work (or reciprocal of Joule's mechanical equivalent of a unit of heat), and I denotes the *internal work* done during change of temperature, at the volume corresponding to the lower limit of integration with respect to dv.

6. The first term of equation (4) is what constitutes the difference between my value of the "metamorphic function" F and Professor Clausius's value of the "disgregation" $\frac{\mathrm{Z}}{\mathrm{A}}$. Professor Clausius observes that, for substances which at all temperatures are capable of passing into the perfectly gaseous state, that term vanishes, and the two functions become identical; but that for substances which at any temperature are incapable of passing into the perfectly gaseous state, the term in question has some value, and the two functions are no longer absolutely identical. I admit that Professor Clausius is right in both those assertions; and that, in assigning the value to F which is given by equation (3), I did not contemplate the existence of substances which at any temperature are incapable of passing into the perfectly gaseous condition.

7. Whether such substances exist or not is a question which experiment alone can decide. I cannot agree, however, with Professor Clausius in thinking it difficult to conceive that such substances as iron can pass into the perfectly gaseous state at low temperatures. Iron, copper, tin, zinc, and lead at low tem-

peratures have distinct smells by which they can be recognized; therefore they exist at low temperatures in the gaseous condition: and why, then, not in the perfectly gaseous condition? And if those metals exist in the perfectly gaseous condition at low temperatures, may not other substances chiefly known to us as solids do so, although we cannot smell them?

8. Professor Clausius dissents from a statement of mine, that the real specific heat of a substance can be different in the three different states of aggregation—solid, liquid, and gaseous. When differences occur between the apparent values of the specific heat at constant volume of the same substance in those three conditions, he ascribes them to internal work.

I admit that it is difficult to conceive how the same substance can alter its real specific heat in changing its state of aggregation. But it is also difficult to conceive how the elevation of temperature of liquid water, for example, can be accompanied by internal work to an amount sufficient to account for the excess of the specific heat of liquid water at constant volume above that of steam and above that of ice, those three quantities being nearly as follows:—

Specific heat at constant volume of ice, about . . .	0·5
„ „ liquid water, about	1·0
„ „ steam	0·37

It appears to me that both difficulties are diminished, if not removed, by supposing that in some cases the same substance, when in different states of aggregation, is not absolutely identical, but *isomeric*, and may so have different values for its real specific heat. For example, we may suppose that one atom of ice or of steam is composed of two atoms of liquid water; just as it has been conjectured that an atom of common oxygen may consist of two atoms of ozone; and then a change in the real specific heat becomes a natural result.

I do not, however, propose that supposition as more than a conjecture; and for the present I am content to regard as certain merely the fact that the *minimum* specific heat of the same substance in different states of aggregation is in many cases different, leaving the relation between that minimum specific heat and the real specific heat to be ascertained by further investigation.

Glasgow University,
November 15, 1865.

LVIII. *On a Property of Commutants.*

By Professor Cayley, *F.R.S.*[*]

I CALL to mind the definition of a commutant, viz., if in the symbol

$$\left[\begin{array}{cccc} \dagger & & & \\ 1 & 1 & 1 & (\theta) \\ 2 & 2 & 2 & \\ \vdots & \vdots & \vdots & \\ p & p & p & \end{array}\right]$$

we permute independently in every possible manner the numbers $1, 2, \ldots p$ of each of the θ columns except the column marked $(\dagger)$, giving to each permutation its proper sign, $+$ or $-$, according as the number of inversions is even or odd, thus

$$\pm_s \pm_t \ldots \mathrm{A}_{\begin{array}{llll} 1 & s_1 & t_1 \ldots & (\theta) \\ 2 & s_2 & t_2 & \\ \vdots & \vdots & \vdots & \\ p & s_p & t_p & \end{array}}$$

which is to be read as meaning

$$\pm_s \pm_t \ldots \mathrm{A}_{1\, s_1\, t_1 \ldots}\ \mathrm{A}_{2\, s_2\, t_2 \ldots}\ \ldots \mathrm{A}_{p\, s_p\, t_p \ldots}\,,$$

the sum of all the $(1 \,.\, 2 \,.\, 3 \ldots p)^{\theta-1}$ terms so obtained is the commutant denoted by the above-mentioned symbol. In the particular case $\theta = 2$, the commutant is of course a determinant: in this case, and generally if θ be even, it is immaterial which of the columns is left unpermuted, so that the $(\dagger)$ instead of being placed over any column may be placed on the left hand of the A; but when θ is odd, the function has different values according as one or another column is left unpermuted, and the position of the $(\dagger)$ is therefore material. It may be added that if *all* the columns are permuted, then, if θ be even, the sum is $1 \,.\, 2 \ldots p$ into the commutant obtained by leaving any one column unpermuted; but if θ is odd, then the sum is $=0$.

The property in question is a generalization of a property of determinants, viz. we have

$$\left|\begin{array}{llll} 2\lambda\lambda' \,, & \lambda\mu' + \lambda'\mu, & \lambda\nu' + \lambda'\nu, & \ldots \\ \lambda\mu' + \lambda'\mu, & 2\mu\mu' \,, & \mu\nu' + \mu'\nu, & \ldots \\ \lambda\nu' + \lambda'\nu, & \mu\nu' + \mu'\nu, & 2\nu\nu' \,, & \\ \vdots & & & \end{array}\right| = 0$$

whenever the order of the determinant is greater than 2.

To enunciate the coresponding property of commutants, let

$$\left\{\begin{array}{ll} \lambda_{11}, & \lambda_{12} \ldots \\ \lambda_{21}, & \lambda_{22} \\ \vdots & \end{array}\right\}$$

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[*] Communicated by the Author.

or, in a notation analogous to that of a commutant,

$$\left[\begin{array}{c} {}^{\dagger}\lambda^{\;\;+}_{1\;1} \\ 2\;2 \\ \vdots\;\vdots \\ p\;p \end{array}\right]$$

denote a function formed precisely in the manner of a determinant (or commutant of two columns), except that the several terms (instead of being taken with a sign + or − as above) are taken with the sign +: thus

$$\left\{\begin{array}{ll} \lambda_{11} & \lambda_{12} \\ \lambda_{21} & \lambda_{22} \end{array}\right\} \text{ or } \left[\begin{array}{c} {}^{\dagger}\lambda^{\;\;+}_{1\;1} \\ 2\;2 \end{array}\right]$$

each denote

$$\lambda_{11}\lambda_{22}+\lambda_{12}\lambda_{21}.$$

This being so, the theorem is that the commutant

$$\left[\begin{array}{l} A_{1\;1\;1\,..\,(\theta)} \\ \;\;2\;2\;2 \\ \;\;\vdots\;\;\vdots\;\;\vdots \\ \;\;p\;p\;p \end{array}\right]$$

where

$$A_{rst\,..\,(\theta)}=\left\{\begin{array}{llc} \lambda_{1r}, & \lambda_{1s} & ..\;(\theta) \\ \lambda_{2r}, & \lambda_{2s} & . \\ \vdots & & \\ \lambda_{\vartheta r}, & \lambda_{\vartheta s} & . \end{array}\right\}=\left[\begin{array}{c} {}^{\dagger}\lambda^{\;\;+}_{r\;1} \\ s\;2 \\ t\;3 \\ \vdots\;\vdots \\ \;\;p \end{array}\right]$$

whenever $p>\theta$, is $=0$.

To prove this, consider the general term of the commutant, viz. this is

$$\pm_s\pm_t\ldots A_{1s't'..}\,A_{2s''t''..}\ldots A_{ps_pt_p..}$$

the general term of $A_{rst..}$ is $\lambda_{ar}\lambda_{bs}\lambda_{ct}..$, where a, b, c.. represent some permutation of the numbers 1, 2, 3 .. θ. Substituting the like values for each of the factors $A_{1s't'..}$, $A_{2s''t''..}$, &c., the general term of the commutant is

$$=\pm_s\pm_t\ldots\lambda_{a'1}\lambda_{b's'}\lambda_{c't'}\,..\,\lambda_{a''2}\lambda_{b''s''}\lambda_{c''t''}\,..\,\lambda_{a_pp}\lambda_{b_ps_p}\lambda_{c_pt_p}\ldots$$

Taking the sum of this term with respect to the quantities s', s'', .. s_p, which denote any possible permutation of the numbers 1, 2 ... p; again, with respect to the quantities t', t''.. t_p, which denote any possible permutation of the numbers 1, 2, ... p; and the like for each of the $(\theta-1)$ series of quantities, the sum in question is

$$\lambda_{a'1}\lambda_{a''2}\ldots\lambda_{a_pp}\,\Sigma\pm_s\lambda_{b's'}\lambda_{b''s''}\,..\,\lambda_{b_ps_p}\,\Sigma\pm_t\lambda_{c't'}\lambda_{c''t''}\ldots\lambda_{c_pt_p}..,$$

which is

$$=\lambda_{a'\,1}\lambda_{a''\,2}\dots\lambda_{a_p\,p}\begin{bmatrix}\lambda^{\dagger}_{b'\;1}\\ b''\;2\\ \vdots\;\;\vdots\\ b_p\;p\end{bmatrix}\begin{bmatrix}\lambda^{\dagger}_{c'\;1}\\ c''\;2\\ \vdots\;\;\vdots\\ c_p\;p\end{bmatrix}\cdots$$

But p being greater than θ, since the numbers $b', b'', \dots b_p$ are all of them taken out of the series $1, 2, \dots \theta$, some of these numbers must necessarily be equal to each other, and we have therefore

$$\begin{bmatrix}\lambda^{\dagger}_{b'\,1}\\ b''2\\ \vdots\;\;\vdots\\ b_p\,p\end{bmatrix}=0;$$

whence finally the commutant is $=0$.

In the case where $p=\theta=2$, we have for a determinant of the order 2 the theorem

$$\begin{vmatrix}2\lambda\lambda' & , \lambda\mu'+\lambda'\mu\\ \lambda\mu'+\lambda'\mu, & 2\mu\mu'\end{vmatrix}=-\begin{vmatrix}\lambda, & \mu\\ \lambda', & \mu'\end{vmatrix}^2;$$

and it is probable that there exists a corresponding theorem for the commutant

$$\begin{bmatrix}A^{\dagger}_{1\;1\;1\,..(p)}\\ 2\;2\;2\\ \vdots\;\vdots\;\vdots\\ p\;p\;p\end{bmatrix},$$

where

$$A_{rst..(p)}=\begin{Bmatrix}\lambda_{1r}, & \lambda_{1s} & \dots(p)\\ \lambda_{2r}, & \lambda_{2s} & .\\ \vdots & & \\ \lambda_{pr}, & \lambda_{ps} & .\end{Bmatrix}=\begin{bmatrix}{}^{\dagger}\lambda^{\dagger}_{r\,1}\\ s\,2\\ t\,3\\ \vdots\;p\end{bmatrix},$$

but I have not ascertained what this theorem is.

Cambridge, October 26, 1865.

LIX. *On the Signification of an Elementary Formula of Solid Geometry.* *By* Professor Cayley, *F.R.S.*[*]

THE expression for the perpendicular distance of a point (x, y, z) from a line through the origin inclined at the angles (α, β, γ) to the three axes respectively, is

$$\begin{aligned}p^2&=x^2+y^2+z^2-(x\cos\alpha+y\cos\beta+z\cos\gamma)^2\\ &=(y\cos\gamma-z\cos\beta)^2\\ &\quad+(z\cos\alpha-x\cos\gamma)^2\\ &\quad+(x\cos\beta-y\cos\alpha)^2;\end{aligned}$$

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[*] Communicated by the Author.

and the remark in reference to it is, that if at the given point P we draw, perpendicular to the plane through P and the given line, a distance PK equal to the distance of P from the given line, then the expressions

$$y\cos\gamma-z\cos\beta,\quad z\cos\alpha-x\cos\gamma,\quad x\cos\beta-y\cos\alpha,$$

which enter into the preceding formula, denote respectively the coordinates of the point K referred to P as origin.

If the given line instead of passing through the origin pass through the point x_0, y_0, z_0, then the corresponding expressions are of course

$$(y-y_0)\cos\gamma-(z-z_0)\cos\beta,\quad (z-z_0)\cos\alpha-(x-x_0)\cos\gamma,$$
$$(x-x_0)\cos\beta-(y-y_0)\cos\gamma,$$

and if we denote the "six coordinates" of the given line, viz.

$$\cos\alpha,\ \cos\beta,\ \cos\gamma,\ y_0\cos\gamma-z_0\cos\beta,\ z_0\cos\alpha-x_0\cos\gamma,\ x_0\cos\beta-y_0\cos\gamma,$$

by

$$a\ ,\quad b\ ,\quad c\ ,\qquad f\qquad ,\qquad g\qquad ,\qquad h$$

respectively (so that $af+bg+ch=0$), then the three expressions become

$$cy-bz-f,\quad az-cx-g,\quad bx-ay-h$$

respectively.

It is moreover clear that if the point P be moved to P′ by an infinitesimal rotation ω about the given line, then P′ lies on the line Pk at a distance PP′, $=\omega$Pk, from the point P, and the displacements of P in the directions of the axes are consequently equal to

$$\omega(cy-bz-f),\quad \omega(az-cx-g),\quad \omega(bx-ay-h)$$

respectively, which is a fundamental formula in the theory of the infinitesimal rotations of a solid body.

Cambridge, October 26, 1865.

LX. *On Chemical Reactions obtained by employing Anhydrous Liquids as Solvents.* By G. Gore, *F.R.S.*[*]

WATER has long been used as a solvent to bring a large number of substances into a liquid state suitable for the exercise of chemical affinity; and a very large class of chemical reactions is due to the liquefying power of water upon one or both of the two substances which are brought into contact for

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[*] Communicated by the Author.

the purpose of chemically acting upon each other; but we have comparatively neglected the solvent power of other liquids which might be employed for the purpose. Numerous reactions may be obtained by dissolving one or both of the acting substances in non-aqueous liquids, such as bisulphide of carbon, chloride of sulphur, chloride of arsenic, bichloride of tin, ether, &c. Various substances which are insoluble in water, such as phosphorus, sulphur, selenium, iodine, &c., and various compounds also which are decomposable by water, such as the chlorides of phosphorus, antimony, &c., may thus be brought into a liquid state and rendered fit for chemical reaction; and I propose in the following communication to illustrate this by various experiments which I have made with bisulphide of carbon as the dissolving agent. The bisulphide employed in these experiments had been previously redistilled from fragments of fused chloride of calcium mixed with carbonate of lead in powder.

It is well known that bisulphide of carbon dissolves phosphorus, sulphur, and iodine, and mixes freely with bromine and with chloride of sulphur; and sulphide of phosphorus and iodide of sulphur have each been formed by dissolving their ingredients in that liquid and removing the solvent by evaporation.

In examining the behaviour of bisulphide of carbon with numerous inorganic substances, I have obtained the following positive results: the substances were generally in the driest state, and were in most cases digested in the liquid in closed bottles during several weeks at the ordinary atmospheric temperature:—

Anhydrous sulphuric acid dissolved slightly and formed a fuming liquid. Persulphide of hydrogen containing an excess of sulphur dissolved instantly and copiously, and formed a yellow liquid. (Bisulphide of carbon also absorbs sulphuretted hydrogen gas.) Selenium in powder dissolved slowly and sparingly, and formed a rich yellow liquid; the solution when evaporated left a residue of red powder intermingled with brilliant golden scales; white phosphorus added to the solution produced no visible change. Phosphide of selenium dissolved slowly and sparingly, and formed a yellow solution. Sulphide of selenium dissolved slowly and sparingly, and formed a very rich yellow liquid. Iodide of selenium dissolved sparingly, and formed a purple solution. Bromide of selenium formed a red liquid. Pentachloride of phosphorus dissolved and formed a colourless liquid. Bisulphide of carbon, digested with potassium and sodium, slowly acquired in the former case a faint yellow colour, and in the latter case a deeper yellow colour; the metals acquired (as is already well known) thick coatings of reddish-brown matter. Hydrate of soda slowly acquired an orange-red colour.

Caustic baryta, lime, and soda-lime slowly acquired a lemon-yellow colour. Bisulphide of carbon that had been saturated with gaseous fluoride of silicon fumed in the air strongly. Bichloride of titanium (freed from excess of chlorine by agitation with mercury) mixed instantly with bisulphide of carbon, and formed a yellow and strongly fuming liquid.

Metallic arsenic in contact with platinum slowly imparted a brownish-yellow tint to bisulphide of carbon, and appeared to dissolve in very minute amount. Iodide of arsenic formed a dark-red solution. Bromide of arsenic formed a colourless solution. Terchloride of arsenic mixed freely. Iodide of antimony dissolved sparingly, and formed a red solution. Bromide of antimony dissolved to a colourless liquid. Terchloride of antimony dissolved freely, and formed a colourless liquid. Pentachloride of antimony produced decomposition, with a light yellow precipitate, and two liquid layers of reddish-yellow colour.

Carbonate of cadmium slowly became reddish yellow. Bichloride of tin mixed freely, and formed a colourless solution. Thallium slowly blackened. Carbonate of cobalt became of a dark red-brown colour. Copper reduced by hydrogen slowly became black. Protochloride of mercury dissolved to a small extent. Silver in contact with platinum slowly blackened. Crystals of nitrate of silver blackened superficially. Benzoic acid dissolved moderately, and formed beautifully flat colourless crystalline plates. The proportion of instances of the foregoing class in which no apparent effects occurred, and which are not specified, amounted to about 75 or 80 per cent.

Bisulphide of carbon added to a saturated solution of protochloride of mercury in ether precipitated the mercury-salt undecomposed.

With a solution of phosphorus in bisulphide of carbon the following positive results occurred (the liquids were all kept in a dark place):—Anhydrous boracic acid slowly became of a lemon-yellow colour. Anhydrous sulphuric acid was decomposed, and sulphurous acid gas set free. Iodine dissolved and formed a fuming liquid, which was of a yellow colour, unless the iodine was in excess; if the proportion of the solvent was small, a mass of crystals was produced. Iodic acid slowly decomposed with formation of a copious scarlet-red powder, and the liquid became yellow. Bromine dissolved with violent action, and produced a nearly colourless fuming liquid; if the proportion of the solvent was small, the addition of an excess of bromine caused a precipitate. Iodide of ammonium became lemon-yellow.

Iodate of potash became yellowish red, and the supernatant liquid became yellow. Nitrate of potash became lemon-yellow. Hydrate of soda slowly became red. Iodide and bromide of barium slowly caused a yellow deposit. The iodides of calcium and magnesium produced a copious yellow deposit slowly. Molybdic acid became of a dark-blue colour, almost black, being evidently reduced to a lower oxide by the phosphorus. Molybdate of ammonia became partly green. Chromic acid was also reduced, and became dark green. The iodides of arsenic and antimony formed rich-yellow solutions, and, like the bromides of those metals, gave reddish-yellow deposits. Teroxide of bismuth slowly became brown.

Iodide of zinc, the hyponitrite, and yellow chromate of lead produced yellow deposits. Carbonate of cobalt quickly became of a brown-black colour. Hydrated oxide of nickel and carbonate of nickel slowly blackened. Copper reduced by hydrogen, and suboxide of copper, soon became black. Dry carbonate of copper immediately blackened and *evolved gas.* Phosphate, anhydrous sulphate, ammoniuret, and arsenite of copper soon became black.

The iodides of mercury and of silver, and both the chlorides of mercury, produced yellow deposits. Dry crystals of nitrate of silver became black, and gave a lemon-yellow deposit. Green cyanide of copper and white cyanide of silver became black, and the liquid smelt strongly of hydrocyanic acid. Cyanide of mercury became red-brown. Nitrocyanide of sodium produced a lemon-yellow deposit.

Salts of copper appear to be particularly liable to be decomposed by this liquid. The results obtained with iodides were probably due in part to the presence of free iodine, which most iodides are liable to contain, as may easily be proved by digesting them with bisulphide of carbon. The percentage of negative results obtained with this liquid was about eighty.

The purer variety of precipitated sulphur was dissolved in bisulphide of carbon, and the yellow solution filtered. This liquid was singularly inactive; upwards of one hundred inorganic substances were submitted to its action; it only *slowly* blackened the salts of lead, copper, mercury, and silver; the carbonate and fluoride of lead, however, were rapidly blackened by bisulphide of carbon containing free sulphide of hydrogen in solution; agitation, therefore, of bisulphide of carbon with carbonate of lead will not remove free sulphur, but will probably remove any sulphuretted hydrogen it may contain. The solution of sulphur in bisulphide of carbon blackened metallic mercury and copper reduced by hydrogen when agitated with them. Sulphur, when dissolving in bisulphide of carbon, absorbs heat to a small extent. Carbonate of lead in a solution of sulphur in bisulphide of carbon did not become blackened by passing hydrogen gas through the liquid.

With a solution of iodine in bisulphide of carbon the following positive results occurred:—A stream of hydrogen converted it into a clear yellow liquid. Agitation with dilute sulphuric acid and zinc decolorized it. White phosphorus converted it into a pale yellow fuming liquid. It was quickly decolorized by agitation with a strong aqueous solution of sulphurous acid. Pentachloride of phosphorus removed its red colour. It was also decolorized with varied degrees of rapidity by sesquicarbonate of ammonia, potassium, hydrate of potash, hypophosphite of potash, sulphide of potassium, and sulphite of potash. Sodium converted it into a pale yellow liquid. It was decolorized by hydrate of soda, sulphide of sodium, hyposulphite of soda (especially by an aqueous solution), peroxide of barium, baryta-water, sulphide of barium, lime-water, sulphide of calcium, soda-lime, slowly by magnesium or aluminium—also by arsenic or bisulphide of arsenic, each forming a yellow solution—by tersulphide of arsenic, arsenite of soda, antimony in powder or tersulphide of antimony (each left the liquid pale yellow), slowly by teroxide of bismuth, tersulphide of bismuth (which became a dark-red powder), zinc, an alloy of zinc and sodium, sulphide of cadmium, protoxide of tin, sulphide of lead, iron reduced by hydrogen, slowly by hydrated oxide of nickel, carbonate of nickel, copper reduced by hydrogen, suboxide of copper, slowly by carbonate of copper, also by arsenite of copper, mercury, oxide, sulphide, and sulphate of mercury, subchloride of mercury, oxide of silver, and slowly by nitrate of silver. The percentage of negative results with this liquid amounted to about sixty-six.

A weak solution of bromine in bisulphide of carbon behaved similarly to that of iodine. It was slowly decolorized by contact with water, also by a stream of hydrogen or by agitation with zinc and dilute sulphuric acid. White phosphorus decolorized it rapidly with violent action, the product of decomposition dissolving to a colourless liquid. Agitation with aqueous solutions of sulphurous acid, hyposulphite, or bisulphite of soda instantly removed its colour. Dry sulphite of potash or of soda or bisulphite of soda, also decolorized it, less rapidly. Potassium, sodium, magnesium, and aluminium were very slowly corroded. The sulphides of sodium, barium, and calcium slowly removed its colour. Metallic arsenic decolorized it with strong action. Metallic antimony and the two sulphides of arsenic decolorized it quickly. Teroxide of arsenic, teroxide of antimony, tersulphide of antimony, and metallic tellurium gradually removed its colour.

Sulphide of cadmium, metallic tin, sulphide of lead, metallic iron, copper reduced by hydrogen, carbonate of copper, metallic mercury, and oxide of mercury decolorized it slowly. Silver was slowly corroded, and iodide of silver was decomposed and its iodine set free.

Anhydrous carbonate of copper was slowly blackened by contact with metallic arsenic in powder beneath bisulphide of carbon.

Remarks.

Bisulphide of carbon dissolves metalloids only, viz. phosphorus, sulphur, selenium, bromine, and iodine; metals tend only to remove its sulphur. It dissolves the chlorides, bromides, and iodides of phosphorus, arsenic, and antimony, generally with facility, those of phosphorus and arsenic (especially the chlorides and bromides) the most readily. It dissolves also, with varied degrees of facility, the chlorides, bromides, and iodides of sulphur and selenium. Persulphide of hydrogen is freely soluble in it. It also mixes freely with the bichlorides of titanium and tin. Metallic oxides, fluorides, chlorides, bromides, iodides, and sulphides, and the whole series of oxygen salts, including carbonates, borates, hypophosphites, phosphites, phosphates, sulphates, sulphites, hyposulphites, chlorates, bromates, iodates, and nitrates, are insoluble in it.

The solution of phosphorus in bisulphide of carbon may be considered as a deoxidizing liquid, as shown by its action upon sulphuric acid, molybdic acid, and chromic acid, and especially by its behaviour with the various oxygen salts of copper. And the solutions of bromine and iodine may be viewed as oxidizing liquids, being decolorized chiefly by deoxidizing substances, such as metals, sulphides, sulphites, hyposulphites, &c. By means of these solutions of opposite chemical character, numerous chemical reactions may be obtained, as already shown. Various sulphides might also probably be formed by means of the solution of sulphuretted hydrogen or persulphide of hydrogen in bisulphide of carbon.

My intention in the foregoing paper being merely to show that numerous chemical reactions might be obtained by employing anhydrous liquids as solvents, I have refrained from further examining the special reactions obtained.

LXI. *On Twilight.* *By* Dr. WILHELM VON BEZOLD*.

I TAKE the liberty of communicating in the following pages the principal results of an investigation which I published in detail some little time ago in Poggendorff's *Annalen*†. It

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* Communicated by the Author.

† Vol. cxviii. pp. 240 & 276.

relates to a phenomenon which, though of daily occurrence, has been hitherto very imperfectly examined. Notwithstanding the extensive literature* which we possess on the subject of Twilight, there is not in the whole of it a single correct and complete description of the phenomenon, to say nothing of anything like a satisfactory theory. I will therefore, in the first place, give a short description of the occurrence of twilight, based on my own observations, made on cloudless evenings, and continued for almost a whole year, in Münden and in the Alps, and will then compare the results with those of the theory which has hitherto been generally received, so as to show the incorrectness of the latter.

I. A short time before sunset the lower part of the sky becomes covered with a yellowish-white tint, which towards the east passes into a dull purple. This purple zone, the *countertwilight* (*Gegendämmerung*) as it is called, increases in intensity about the moment of sunset, and reaches, according to the condition of the atmosphere, from 6° to 12° above the horizon. As the sun sinks, the ashy-grey shadow of the earth rises in the form of a segment, in consequence of which Le Mairan called it the *dark segment*. As soon as the limb of this segment, which, for reasons afterwards stated, I will call the *first* dark segment, has attained an elevation of more than 1° or 2°, it is tolerably sharply defined. It rapidly pursues its course towards the zenith, seeming to stretch itself in the manner of a dark veil over the countertwilight. As it approaches the upper edge of the latter, its own limit becomes more and more indistinct, until at last it is impossible to recognize it any longer. At this time the sun is from 3° to 4° below the horizon.

Whilst this is taking place in the east, a very different spectacle is displayed in the western heavens. The western horizon is coloured, at sunset and afterwards, orange-yellow or even red, and this yellowish coloration is defined from the upper blue portions of the sky, at an elevation of from 9° to 12°, by a very transparent, shining, whitish zone, which gradually spreads out in a horizontal direction. I will call it, with Brandes, the *twilight-sheen* (*Dämmerungsschein*).

While the twilight-sheen sinks towards the horizon, very delicate purple tints appear at an elevation of about 25°. They increase rapidly in intensity, and form a circular disk with constantly augmenting radius, whose lower part seems to be covered or cut off by the yellow tint. This phenomenon, which I will

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* Nearly the whole of the earlier literature is quoted in Kämtz, *Meteorologie*, vol. iii. pp. 50 *et seq.*, and in a paper by Forbes in the Edinburgh Transactions, vol. xiv. The more recent literature is referred to in my paper above-mentioned.

call the *first purple light,* attains a maximum of intensity when the sun is 4° 20′ below the horizon. This value was the mean of twenty observations varying between 3° 40′ and 4° 50′ as extreme limits. The radius of the disk has at this moment attained a value of about 30°, and its centre is about 15° above the horizon. The centre sinks very quickly, so that the purple-light finally contracts to a narrow girdle of dull purple colour, very sharply marking off the yellow portion, which, from the form it thus assumes, may be called the *first bright segment.* This limitation retains its sharpness, even after the complete disappearance of the purple light, which occurs when the sun is about 6° below the horizon. Measurements of the height θ of the limb of this segment showed that on each day the sum $\omega+\theta=\gamma$ is nearly constant, ω denoting the depression of the sun. The value of γ varied on different days from 8° to 12°. This law extends even to the twilight-sheen, so that we are justified in denoting the yellow portion in the western heavens as belonging to the bright segment from the very beginning of twilight.

While the purple light, which, it may be remarked in passing, shows scarcely any trace of polarization, attains its greatest intensity, westerly objects which immediately after sunset were quite pale and colourless, appear again illuminated with a tolerably bright rose tint. This illumination is often sufficiently intense to be remarked in narrow town streets where the sky is scarcely visible. The Alps afford at this time the glorious spectacle of the *afterglow* (*Nachglühen*). In the vale of Chamouni each of these appearances has its proper name: a distinction is drawn between the "coloration brillante du Mont-Blanc," the "teinte cadavéreuse," the "coloration seconde, ou la résurrection," and lastly the "extinction."

The whole of the phenomena are, however, to be seen in the plain as well; so that there is no ground whatever for seeking the cause of the afterglow on Mont Blanc, as De la Rive does, in the extraordinary height of the mountain. That the phenomenon does not depend upon any mere effect of contrast, as Necker supposes, is proved by the fact that it may be observed even in places whence the sky is not seen, but only the illuminated objects, such, for example, as a house on the opposite side of the way seen from a room.

The moment of complete disappearance of the purple light is accompanied by a remarkable diminution of daylight. For this reason, doubtless, the termination of twilight, popularly so called, that is, of the portion of twilight during which common occupations can be pursued without artificial light, is fixed at about the same moment.

While the first bright segment continues its course towards the horizon, a repetition of the appearances already described is preparing itself in the east. The eastern horizon again displays a faint purple colour, or a *second countertwilight,* over which a second (but very imperfectly defined) dark segment rises up. In the west there appears over the first bright segment a *second twilight-sheen,* but not reaching to quite the same elevation as the first; it is separated from the bright segment by a dirty yellowish-green stratum. It moves downwards in the same way as the first, and a *second purple light* is gradually developed, exactly like the first, except that it is much less intense and has somewhat more of a reddish tint. On many days it is as intense as the first is on others, though for the most part it is so faint that it is impossible to observe it with any certainty. Out of twenty-four evenings, I saw it only eight times, and I was able to determine the moment of its maximum intensity with tolerable accuracy only on four occasions. From these observations, it resulted that the depression of the sun corresponding to the maximum of the first purple light was to the depression corresponding to the maximum of the second in a nearly constant ratio of 1 : 2·2. It will of course be understood that the second purple light is accompanied by a second illumination of favourably-situated objects; and thus in the Alps a *second afterglow* may often be observed.

The second purple light undergoes the same changes as the first, and consequently occasions at last the formation of a *second bright segment.* It was the height of this segment of which Lambert* made a series of measurements one evening, but erroneously regarded as the continuation of what he called the first twilight-bow (*Dämmerungsbogen*), that is, the limit of the first dark segment. When the second bright segment sinks below the horizon, the last brightness disappears, and twilight is at an end. This corresponds on an average to a depression of the sun of about 17°.

The course of the phenomena, which are observable in exactly the inverse order, in the morning remains in general the same. The intensity and extent of the individual appearances, however, are very different on different days; nevertheless twilight always retains its definite character, so that its course can be foreseen from the first; or, to borrow the language of mathematics, the law remains always the same, but the constants which enter into its expression vary from day to day.

My measurements were made with an apparatus which consisted simply of a strip of wood, one foot in length, which carried

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* *Photometria,* p. 448.

a pin at one end and a notched piece of metal plate at the other, the whole being attached to a graduated arc. Telescopes, or strictly dioptric apparatus, are of no use, inasmuch as they limit the view too much, and introduce disturbing effects of contrast. For the results of the measurements I must refer to my before-mentioned paper.

II. All the theoretical investigations which have hitherto been made upon twilight are fundamentally nothing more than further developments of the theory promulgated by the Arabic astronomer Alhazen. According to this view, the part of the sky from which light can reach us after a single reflexion, is separated from the part which cannot give us light except by at least two reflexions by a bow, which was called the first twilight-bow, and whose height above the horizon depended only, according to this view, upon the position of the sun, and upon the height of the atmosphere. Hence the phenomena of twilight were regarded as affording a proper basis for determining the height of the atmosphere; but it is well known that no serviceable results were thus arrived at. A definition, similar to that of the first, was given of a supposed second twilight-bow; and places were said to be in the first twilight when the first twilight-bow was above their horizon, and in the second twilight when the second bow was above their horizon.

A glance is enough to show the insufficiency of this theory. The dark segment, of which, according to this supposition, the first twilight-bow marks the limit, and which ought therefore to advance over the whole sky according to a perfectly definite law, disappears at a very moderate elevation, and follows in other respects quite a different course. If, for instance, the height of the atmosphere is calculated on the basis of this theory from the measurements of this phenomenon, each observation gives a greater value than the preceding one. The results, indeed, vary from 0·05 to 2 geographical miles. The bright segment and that very remarkable phenomenon the purple light, are left entirely out of consideration in the old theory.

Moreover the observed division into the first and second twilight does not by any means answer to what the theory requires. For, according to this, the end of the first twilight ought to coincide with the beginning of the second, whereas in reality the two encroach upon each other. The course of the second is, indeed, completely analogous to that of the first, if only we assume that the first purple light plays exactly the same part in relation to the production of the second twilight that the sun does in the case of the first. That is to say, the moment of the complete disappearance of the first purple light must be taken as the beginning of the second twilight; and this takes place nearly at the middle of the first, for the first bright segment does not set until the sun is from about 9 to 12 degrees below the horizon.

I will content myself with having, on the one hand, established the course of the phenomena, and having, on the other hand, demonstrated that the theory hitherto accepted is altogether incorrect and insufficient, without venturing to try to replace it by a better. I believe, nevertheless, that the behaviour of the first dark and bright segments is quite capable of explanation when the problem is regarded from the photometric point of view. The brightness of any portion of the sky must in fact depend, first, upon the number of illuminated particles which lie upon the straight line drawn from the observer's eye to the point in question; secondly, upon the absorption of light by the atmosphere; and lastly, upon various other conditions of more or less importance. If, in the meantime, we consider only the two first-named conditions, it is easy to see that, after sunset, there must be two zones with a maximum of brightness, the lower limit of the countertwilight and the twilight-sheen. According to this, the latter would mark the limit between those points of the sky which send us chiefly transmitted light and those which send us chiefly reflected light.

The difference of colour which may be remarked on the two sides of this zone is a confirmation of the truth of this view. In like manner the course of the phenomena, as it results from measurements, accords in general entirely with the conception here indicated.

The above are the most important results of an investigation which I am unfortunately unable to pursue, from want of a proper place for making observations.

Briefly stated, they are as follows:—

Twilight consists of a first and a second period, each of which exhibits the dark and the bright segment with the twilight-sheen and the purple light.

These two periods of twilight overlap each other, so that the beginning of the second coincides approximately with the end of the first half of the first period.

In the morning the same phenomena may be observed, but they follow each other in the inverse order.

LXII. *On a new Electrical Machine.*
By W. Holtz *of Berlin*[*].

[With a Plate.]

EXPERIMENTS with the electrophorus led me to the idea of otherwise turning to account the theory of this apparatus. As this theory contains a very rational principle of developing electricity, I proposed to myself to apply the same principle to the construction of an electrical machine. It has been found not only that such machines are possible, but that, with a moderate expenditure of force, they far exceed ordinary machines in their quantitative effects. It is the object of this paper to describe accurately the construction and mode of action of a simple apparatus of this kind.

A steel shaft 9 inches in length (fig. 1, Plate V.) is supported at its ends in a horizontal position, and can be set in rapid rotation by means of a cord and a pulley which is turned by a winch[†]. In the middle of the axis, and fastened exactly at right angles to it in a mounting of vulcanite, is a round glass disk of 15 inches diameter. The mounting is best made of two smaller but thick disks, of which one is fixed on the axis, and the other can be screwed on it. The glass disk must be accurately centred, and chosen of very thin and flat plate glass.

Another round disk, but 2 inches larger in diameter, which may consist of flat window-glass, is provided in the middle with such an aperture that it can be fastened parallel to the first at about the eighth of an inch distance. This is effected by means of four horizontal bars of vulcanite provided with small moveable rings, which touch the edge of the glass at about equal intervals. In this disk are two deep notches of peculiar shape, and it is coated in two places with paper; the two notches, as well as the two coatings, are exactly half a turn apart and so placed that in each case a notch directly precedes a coating. The form of the notch is best seen from the drawing: its greatest breadth and depth amount to 4 inches. The coatings, which are on both sides of the glass disk, are of the same length, but do not pass beyond the edge of the rotating disk. The breadth of the outside part is 2 inches, that of the inside is about half as great. From the latter, two pointed pieces of drawing-paper project to about the middle of the notch.

In front of the rotating disk, parallel to the axle, and also

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[*] Translated from Poggendorff's *Annalen*, No. 9, 1865. A notice of an imperfect form of the instrument, communicated by Professor Poggendorff, has already appeared at page 159 of this volume.

[†] It is very convenient to have an arrangement for treading, so as to leave the hands free.

about half a turn distant, are two metal rods, *e* and *f*, which I will call conductors, held by insulating supports. At their free ends they are provided with binding-screws for fastening wires; at the other, with which they approach the glass disk, with radial cross bars: these latter in turn are provided with a great number of fine points ½ an inch long, which stand quite near the disk, but without touching it. These series of points are opposite the external parts of the coatings.

For the sake of greater insulation, it is best to coat the disks with a solution of shellac. By the working of the apparatus, the rotating disk especially becomes a conductor; for it gradually gets covered with an exceedingly fine dust, which appears to consist of carbon, and to be formed from the decomposition of the resin. After three or four hours' continuous activity the action perceptibly decreases; and then, if a further decrease is injurious, the disk is taken out and the dust removed by a moistened pad. From time to time, a new coating of shellac is desirable, to keep the action at the same level.

The principle on which the apparatus works presupposes a certain electrical excitation; and as this is not effected by the rotation of the disk alone, it must be given by a previously electrified body. For this a thin plate of vulcanite, about 4 inches broad and twice as long, is well adapted, especially when the surface, which is sometimes a good conductor, has been removed by scraping with a knife. This becomes electrical when it is laid on a table and rubbed with fur, and more easily if this is done alternately on both sides. The electrified surface is brought near to one of the coatings, while the disk rotates like the hand of a watch, and the conductors are connected with each other or with the earth. The two coatings then immediately take opposite electrical charges, whose intensity increases, accompanied by a crackling noise, till after a few minutes a definite and provisionally constant maximum value is obtained. At the same time a continuous electrical current is established, with which, as long as it is not completely broken, experiments can be made.

For this purpose a discharger is very convenient, which consists of three insulated brass supports. If the two outside ones, *a* and *c* (fig. 2, Plate V.), are connected with the conductors, or one of them with the earth, the striking-distance between *a* and *b* can be varied by means of the moveable wires *m* and *n*; while between *b* and *c*, instead of the wire *l*, the body which is to be exposed to the action of the current can be inserted.

The current loses its continuity if the wires *m* and *n* are not in perfect contact; for the tone which accompanies the luminous phenomenon shows that it consists of a great number of small discharges. If these discharges are to be intensified at the expense of their number, either the striking-distance, the electrodes, or the surface of the conductors must be increased: the latter is best effected by a Leyden jar, the coatings of which are connected with *a* and *c*.

By increasing the striking-distance, the efficiency of the machine is not in general diminished. This is best seen in charging a jar, for the time then varies in the same ratio as the intensity of the charge. Beyond a certain point, the striking-distance can be no more increased. If it is, the current of sparks begins gradually to disappear, and, indeed, most rapidly if both conductors are insulated. A complete connexion must then be quickly reestablished for a short time, if the electricity is not to be quite lost from the coatings. If it is increased, when a jar is in the circuit, the direction of the current changes when a certain intensity is exceeded: the jar is discharged by the apparatus itself, to be recharged in the opposite direction, and this is continued until the striking-distance is lessened. But the striking-distance which can be attained is in each case somewhat greater if both conductors are insulated.

The resistance of the body interposed between *b* and *c* must not exceed a certain limit; for instance, it must not be a semiconductor, if the machine is to work. On the other hand, the conductors, if they are to fulfil their object, must not be connected with semiconductors alone, as these act, as regards the intensity of the current, more or less like insulators.

If the machine is to cease working for a short time, the current is closed and the disk then allowed to come to rest. The glass surfaces remain then (usually for half an hour) sufficiently electrical for its working to be reestablished by rotation. In dry air the apparatus can be left for hours without the electricity being quite removed from the glass.

The connexion of these phenomena may be seen from the following considerations.

Assuming that the coating denoted + is charged feebly with positive electricity, it acts inductively on the rotating insulator. The repelled +E passes into the conductor *e*, while −E simultaneously collects on the glass disk. The latter becomes partially free as soon as it passes beyond the limits of the coating; but it still remains partially bound, as it acts inductively on the outside of the fixed disk, causing here a gradual accumulation of +E. The binding influence of the latter is interrupted by the notch in the glass. The −E still on the insulator, and which has now become free, then enters at the points of the second coating, and the part which is not taken up by it disappears by the conductor *f*. In the degree in which the second coating is charged, it exerts an inducing action; negative electricity is repelled, while + E collects on the disk, and the latter must again be partially bound, during half a turn, until it comes to a notch and the points of the first coating. Thus it is seen that the one coating charges the other, and it is a question whether these charges will gradually decrease or increase.

Under the influence of the charge which the rotating disk acquires, the quantity of the electricity on the coatings is resolved into that which is bound and that which is free. If the inducing action is very slight, the charge can be so small that its intensity is at first less than the intensity of the latter. If the inducing action is greater, the losses which occur during rotation may become so considerable that after half a turn this is still the case. If the glass disk has become more feebly electrical than the points of the coatings, the former are not electrified by the latter, but the latter by the former. With the disappearance of the free E the inducing action is smaller, the charge which the rotating disk assumes, and its inducing action, become less and less; the induced electricity becomes more and more free, and that which has become free will again disappear: the charges of the coatings must therefore be continually lessened. But if the glass surface at the notches is more strongly electrical, it must inversely gradually acquire higher and higher charges as long as the insulation permits an increase. The latter is limited by the position of the conductors, and by the attraction which is simultaneously exerted by them on the electrical surface. This attraction is strongest if the circuit is not open, for then it is to be regarded as a body in connexion with the ground. The more completely the circuit is opened, the more will the attraction diminish, since there is on the conductors a continually higher tension which is of the same kind as that of the glass. The tension on the coatings will increase as long as their mutual distance, or the distance from the notches opposite them, permits. From a certain point a discharge takes place through the air. But this point need not be reached within the limits of the striking-distance.

If we consider more closely the current in the circuit, we shall see that it consists of two currents which are different but in the same direction. One is formed by induction, by the glass disk being continually charged afresh; the other is formed by the liberation of this charge, of which only a small part is necessary to retain the tension on the coatings. If we suppose these currents, of which I will call the former the primary, and the latter the secondary, to be separated, they must be different, both quantitatively and in their maximum striking-distance. In the first respect the primary will exceed the secondary, because the charge of the disk is materially weakened during half a turn;

in the latter, on the other hand, an inverse deportment prevails, for the tension of the free electricity on the glass surface is necessarily higher than that which results from its inducing action. But since both currents traverse the same conductor, and one is only a consequence of the other, the cessation of the one must follow the cessation of the other, and the greatest striking-distance of the primary current is at the same time that of the apparatus.

If one of the conductors is connected with the earth, it is only after exceeding this striking-distance that the secondary current can disappear, since the coating opposite to it can still electrify the glass disk. In fact, without interrupting this current the electrodes may be removed to more than double the distance; but this will very soon diminish with the charge of the coating, since no more new electricity is added.

If there is a jar in the circuit, both currents must simultaneously disappear when the charge has attained a definite amount. The intensity on the coatings of the fixed disk sinks; the jar allows part of its charge to pass on to the rotating disk; and as this, after half a turn, passes to the opposite electrical coatings, in a short time the machine begins to work in the opposite direction. But the jar must in this way continually be charged and again discharged.

Without a jar, too, inversions of the current may occur, if the maximum striking-distance is near, and the fixed disk is only coated on its outside. The opposite electricity then collects on the inside, which remains bound so long as there is a portion of free electricity on the outside. If this latter is lost, either intentionally or unintentionally, the latter begins to act inductively in the opposite direction. It is very inconvenient, that under these conditions the rotation can scarcely be interrupted for a few seconds without, on beginning it again, the current being found already in the opposite direction.

By two simple arrangements the machine can be made still more suitable for each special object.

To keep up the action independently of the opening of the circuit, it is necessary, as I have stated, to keep the currents separate. The construction might easily be so altered that those currents could only move in separate conductors; but this would do away with the possibility of using them together where it was desirable to do so. It is rather advantageous to avoid the separation until the primary current ceases; and this is effected as follows.

Imagine a third conductor, which may be called *g*, parallel to the others and at about a quarter of a turn from them, fastened on the corresponding supports, and the coating marked — prolonged to its points and permanently joined with the conductor *e*. As long as the striking-distance is within definite limits, both currents will, as before, traverse *e* and *f*. For even if two conductors are now opposite the lengthened coating, the disk will only be charged by that which precedes it in the direction of the rotation. It is only if the resistance between *e* and *f* becomes greater that gradually begins to act, and the primary current will finally only pass between *e* and *g*. If now the electrodes are removed, the action of the machine undergoes thereby no interruption. From a certain point the secondary current will indeed disappear, but on the conductors a constant quantity of free electricity will be retained. It is, moreover, not even necessary to prolong the coating, if only that point of the glass opposite the conductor *g* is touched as often as the machine is set in motion. In this case, or when the rotation is interrupted for a short time, *e* and *f* need not be joined; the electrified object must then be approached to the coating marked + in order that the current may be set up immediately between *e* and *g*. The machine gives in this form the longest sparks if *e* and *g* are in connexion with the ground and *f* is connected with a large conductor.

In order to increase the quantity, without at the same time heightening the velocity of rotation, the points at which the disk can charge and discharge itself must be increased. If we call the entire arrangement by which this is effected an element, the quantitative performance will be proportional in general to the number of such elements. It is here presupposed that they do not disturb one another, which in general will be the sooner the case the greater the intensity of the free electricity, the greater therefore at the same time the striking-distance in the circuit. The maximum will therefore more or less diminish with the number of elements.

Fig. 3 shows how four such elements can be arranged on the machine; the conductor *s* is supposed to be fastened in the place of the conductor *g*, while *p* is supported by a prolongation of the same frame. To work the machine in this form, all the conductors must first be connected with each other. If then one coating is electrical, all of them are alternately oppositely electrical. To have the greatest quantitative effect, *p* and *s* must be connected with one, and *q* and *r* with the other support of the discharging-arrangement. *r* and *s* may, however, be left connected, and only *p* and *q* used; a smaller quantity is thus obtained, but a greater tension. Or *p* and *s* may be connected together, and *q* and *r* connected with the coatings; free electricity is then soon obtained of considerable tension, which cannot, however, be conducted away without impairing the activity of the apparatus. In regard to the first two connexions, what I said as to the treatment in the first mode holds good; I must only remark that if the rotation is interrupted, the electricity escapes much more readily from the glass surfaces. In moist air this loss, after one or two minutes, is so great that the action cannot be reproduced by rotation alone.

To pass now to the action of the machine. I give, in the first place, a few determinations of the maximum striking-distance, and its quantitative effect.

Between circular electrodes of $\frac{3}{4}$ inch diameter, the striking-distance which could not be exceeded was with two elements 1 inch, with four elements in cross contact $\frac{3}{4}$ inch; with two elements and separated currents a striking-distance of 2 inches, and with a larger conductor one of 3 to 4 inches could be obtained. When a Leyden jar was used, in the first and second case the maximum striking-distance was $\frac{1}{8}$ inch less.

Between the same electrodes, and with twelve turns in a second, a jar with a square foot of coating, and $\frac{1}{12}$th of an inch thick, could be charged to a striking-distance of $\frac{5}{8}$ inch by two elements in two seconds, and by four elements in one second.

By diminishing the electrodes, the striking-distance could be increased to a certain extent. With four elements, knobs half an inch in diameter could be removed to $\frac{5}{4}$ths of an inch distance, and the jar discharged itself, regularly at this distance in one second. Points, on the contrary, on account of the brush formed, could only be brought to a distance of $\frac{5}{8}$ths of an inch, at which about three or four discharges took place in a second.

The expenditure of force which corresponded to this effect was, as compared with other electrical machines, very small; for if the disk was suddenly left to itself, it generally continued to rotate eight seconds with four elements, and fourteen with two elements.

The current of sparks took very different shapes, according to the form and distance of the electrodes. Between points it formed a small bundle, wider towards the middle, and consisting of innumerable little sparks (fig. 4). Between knobs, at a greater distance, it formed usually a thicker sinuous thread, of small brightness and roseate colour. At a smaller distance it divides, without losing its colour, into several separate threads, which, for the most part parallel and lying in one plane, diverge in continually greater arcs from the straight middle line. By connecting a conductor with the ground, or by enlarging the surface, the dark colour can be easily changed into a light one. A very intensely luminous current of sparks, which is surrounded by a bluish sheath, is obtained, between points, with a jar of 3 to 4 square inches of coating.

In order to vary most conveniently the luminous phenomena in a Geissler's tube, it is interposed in the circuit instead of the wire *l*. As long as *m* and *n* are in complete contact, it shines constantly with a feeble bluish light. Stratification is obtained when *a* and *c* are simultaneously connected with a larger jar. It is also obtained if the electrodes are pointed and at a very small distance. With a greater striking-distance, and especially with the use of the above-mentioned small jar, the tube is so intensely luminous that the phenomenon is quite visible even by broad daylight.

Physiological actions only occur if there is at the same time a layer of air in the circuit. If the current of sparks is taken directly on the hand, it produces an extremely smart pricking sensation. If the human body be interposed between *b* and *c*, distinct shocks are felt even without a jar. But if *a* and *c* are simultaneously in connexion with the small jar, if the electrodes are points and so near that they all but touch, the sensation of a tolerably powerful induction-apparatus is felt. By gradually increasing the striking-distance, this action may be increased to any extent; but at a distance of $\frac{1}{32}''$, where there are still more than fifty discharges in a second, it is unendurable.

To exhibit the heating effect which the current produces in a thin wire, I interposed a Riess's air-thermometer in the circuit. With a slight inclination of the instrument the liquid flowed out of the tube; with a greater inclination it assumed a more or less constant position. To test the heat of the sparks, I allowed them to strike through a narrow glass tube into which the electrodes were led. Within a short time it was so warm that I could light a match at it. A direct lighting by the current was produced by giving the electrodes the form of points and bringing them as near as possible. Phosphorus and gun-cotton were immediately inflamed between such points, and finely-divided charcoal became incandescent. Tinder was only ignited with great difficulty, and gunpowder not at all.

Of chemical actions, the decomposition of water may be mentioned; but I may remark that I have only succeeded in it with wires melted into glass tubes. The little bubbles ascend in fine but continuous streams, and at one electrode apparently in double the quantity at the other. By interposing a layer of air, this action was only enfeebled.

To confirm the magnetic actions, I used the extra coil of a Stöhrer's induction-apparatus. In the inside, near the open end, a magnetic needle was placed, and the current led continuously through the spiral; I obtained thus a pretty constant deflection of 35° to 40°.

From further experiments it will be seen to what degree these actions can be increased. In order to ascertain the influence of the magnitude of the disk, I am having machines made of various sizes, and hope soon to be in a position to report on their action*. At the same time I am engaged on the construction of complex machines, which consist of a small machine of the above construction, and a larger apparatus, whose coatings are not kept in electrical tension by its own action, but by that of the electrical machine.

Fig. 5, Plate V. shows such a one, as especially fitted for high tension, and as constructed some time ago, with disks of 30 inches diameter. The coating marked + must be supposed to be constantly electrified, the conductor *e*, opposite to it, in contact with the earth, and the conductor *f* with a larger conductor. Although the insulation of this apparatus was extremely imperfect, I could obtain a striking-distance of 9 inches. For greater quantity, instead of one, a larger number of elements had to be used. In order to have as many as possible, I was at last obliged to try to avoid the notches by alternately electrifying the coatings negatively and positively. The liberation of electricity in this case was effected by the tension of the coatings†.

I have, however, already remarked that, by multiplying the elements on a disk, the quantity is always increased only at the expense of the striking-distance. If this is to be retained, the disks must be increased in number, but removed so far that they do not mutually disturb one another; and this is more difficult of attainment with a high degree of tension than with a low one.

In any case it is to be remembered that the velocity of rotation which can be attained with such a machine is materially diminished by the increase of its dimensions, and that therefore the quantitative effect cannot be increased in the same degree as the tension.

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* I have entrusted their construction to M. W. Schulz, mechanician, Berlin. A machine like the above, but with better insulation, and more convenient arrangement for experimenting, will cost about 25 to 30 thalers. A larger table with wheel and treadle would increase the price about 15 to 20 thalers.

† Last summer I had the opportunity of showing to Dr. Paalzow the action of an apparatus which depended on the application of this principle. Both disks were coated on their outside with tinfoil, and this coating was divided by insulating spaces into a greater or smaller number of sectors alternately joined with each other. Those of the fixed disk were positively and negatively electrified by an ordinary electrical machine. To two succeeding ones conductors were opposite, which touched the rotating disk with fine wires. If these were in connexion, and the disk rotated, all the sectors when they passed those of the solid ones were discharged and again charged. I have subsequently placed sixty such elements, in which the coatings of the fixed disk were replaced by wires enclosed in glass, on a smaller apparatus, and thus rendered it possible to electrify the disk, which rotates twenty times in a second, 800 times. It is a defect in this and other apparatus, that by their means only a comparatively small striking-distance can be attained.

LXIII. *On Fractions for the Value of* M.

By R. Templeton, *Esq.*[*]

Having had occasion to make use of the modulus of common logarithms carried to a considerable number of figures, I attempted the verification of the figures given in Hutton's Tables (Introduction, Hyperbolic Logarithms) by the methods to be met with in ordinary works, but soon gave it up, embarrassed by the endless rows of figures which the modes of procedure required. After a little consideration I was led to have recourse to an expedient which had already been resorted to[†] for avoiding this irksome calculation—that of deducing the logarithm of 10 from the logarithms of a moderate number of small fractions, the denominators of which differ by unity from the numerators, and are composed of a sufficient number of figures to ensure rapid convergence, and, if practicable, of digits easily worked with. The following consideration indicates the mode by which these small fractions may be found. If by any process the denominator of a fraction can be doubled, the series into which that fraction is introduced will converge with more than twice the rapidity, since the higher powers will be proportionally of larger dimensions. Now the square of the fraction $\frac{x+1}{x}$ will be, when x exceeds unity, very nearly equal to $\frac{x^2+2x}{x^2}$ or $\frac{x+2}{x}$. Let y be the small fraction required to restore equality, then

$$\frac{x+2}{x}=\left(\frac{x+1}{x}\right)^2.y;$$

whence

$$y=\frac{x^2+2x}{x^2+2x+1},$$

and

$$\frac{x+2}{x}=\left(\frac{x+1}{x}\right)^2.\frac{x^2+2x}{x^2+2x+1}.$$

As an example, let $x=48$; then

$$\frac{48+2}{48}=\frac{25}{24}=\left(\frac{49}{48}\right)^2.\frac{2400}{2401}.$$

We have here the denominator of the fraction $\frac{25}{24}$ doubled, thus making the convergence much more rapid at the expense of the labour imposed by the introduction of another fraction, of, how-

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[*] Communicated by the Author.

[†] *Vide* paper by the Rev. John Hellins in the Phil. Trans. 1796, p. 135.

ever, very swift convergence; but the work for both is far less onerous than the computation of the terms of the original fraction. The above may be put under a slightly different form:

$$\frac{x+1}{x}=\left(\frac{2x+1}{2x}\right)^2\cdot\frac{(2x+1)^2-1}{(2x+1)^2};\quad\ldots\ldots\ldots\quad A_{I}.$$

and by substitution,

$$=\left(\frac{4x+1}{4x}\right)^4\cdot\left(\frac{(4x+1)^2-1}{(4x+1)^2}\right)^2\cdot\frac{(2x+1)^2-1}{(2x+1)^2},\quad\ldots\ldots\quad A_{II}$$

$$=\left(\frac{8x+1}{8x}\right)^8\cdot\left(\frac{(8x+1)^4-1}{(8x+1)^4}\right)^4\cdot\left(\frac{(4x+1)^2-1}{(4x+1)^2}\right)^2\cdot\frac{(2x+1)^2-1}{(2x+1)^2}\,A_{III}$$

&c. &c. &c.

The equation A_{I} is only a particular value of a more general expression, in which x receives a small multiplier n (positive and integer),—practically, however, with this disadvantage, that every unit of which n is composed indicates the requirement of another fractional factor: some such expressions are

$$\frac{x+1}{x}=\left(\frac{nx+1}{nx}\right)^n\cdot\left(\frac{(nx+1)^2-1}{(nx+1)^2}\right)^{n-1}\cdot\left(\frac{(nx+2)^2-1}{(nx+2)^2}\right)^{n-2}\cdot\left(\frac{(nx+3)^2-1}{(nx+3)^2}\right)^{n-3}.\,\&c.$$

$$=\frac{(nx+1)^2}{(nx+1)^2-1}\cdot\left(\frac{nx+2}{nx+1}\right)^n\cdot\left(\frac{(nx+2)^2-1}{(nx+2)^2}\right)^{n-2}\cdot\left(\frac{(nx+3)^2-1}{(nx+3)^2}\right)^{n-3}.\,\&c.$$

$$=\frac{(nx+1)^2}{(nx+1)^2-1}\cdot\left(\frac{(nx+2)^2}{(nx+2)^2-1}\right)^2\cdot\left(\frac{nx+3}{nx+2}\right)^4\cdot\left(\frac{(nx+3)^2-1}{(nx+3)^2}\right)^{n-3}.\,\&c.$$

$$=\frac{(nx+1)^2}{(nx+1)^2-1}\cdot\left(\frac{(nx+2)^2}{(nx+2)^2-1}\right)^2\cdot\left(\frac{(nx+3)^2}{(nx+3)^2-1}\right)^3\cdot\left(\frac{nx+4}{nx+3}\right)^n.\,\&c.$$

&c. &c. &c.

It is sometimes advantageous to change a fraction into another differing little from it, but whose denominator shall be increased or diminished by an aliquot part of the denominator of the original fraction. The form for this is

$$\frac{nx+1}{nx}=\frac{nx+n+1}{nx+n}\cdot\frac{nx^2+nx+x+1}{nx^2+nx+x}\quad\ldots\quad B_{I}.$$

$$=\frac{nx-n+1}{nx-n}\cdot\frac{nx^2-nx+x+1}{nx^2-nx+x}\quad\ldots\quad B_{II}.$$

$$\frac{nx-1}{nx}=\frac{nx+n-1}{nx+n}\cdot\frac{nx^2+nx-x-1}{nx^2+nx-x}.\quad\ldots\quad B_{III}.$$

Useful particular values for more immediate reference may be selected from the above, and will be made use of in the examples which follow.

$$\frac{x\pm 1}{x}=\frac{x}{x\mp 1}\cdot\frac{x^2-1}{x^2} \quad \ldots\ldots\ldots\ldots \quad \text{C.}$$

$$\frac{x+1}{x}=\left(\frac{2x+1}{2x}\right)^2\cdot\frac{(2x+1)^2-1}{(2x+1)^2} \quad \ldots\ldots\ldots \quad \text{D}_{I}.$$

$$=\left(\frac{2x+2}{2x+1}\right)^2\cdot\frac{(2x+1)^2}{(2x+1)^2-1} \quad \ldots\ldots\ldots \quad \text{D}_{II}.$$

$$=\left(\frac{2x+3}{2x+2}\right)^2\cdot\left(\frac{(2x+2)^2}{(2x+2)^2-1}\right)^2\cdot\frac{(2x+1)^2}{(2x+1)^2-1}\cdot \quad . \quad \text{D}_{III}.$$

$$=\left(\frac{3x+1}{3x}\right)^3\cdot\left(\frac{(3x+1)^2-1}{(3x+1)^2}\right)^2\cdot\frac{(3x+2)^2-1}{(3x+1)^2} \quad . \quad \text{D}_{\text{iv}}.$$

$$=\left(\frac{3x+2}{3x+1}\right)^3\cdot\frac{(3x+1)^2}{(3x+1)^2-1}\cdot\frac{(3x+2)^2-1}{(3x+2)^2} \quad . \quad . \quad \text{D}_{\text{v}}.$$

$$.=\left(\frac{3x+3}{3x+2}\right)^3\cdot\frac{(3x+1)^2}{(3x+1)^2-1}\cdot\left(\frac{(3x+2)^2}{(3x+2)^2-1}\right)^2 \quad . \quad \text{D}_{\text{vi}}.$$

$$\frac{x-1}{x}=\left(\frac{2x-1}{2x}\right)^2\cdot\frac{(2x-1)^2-1}{(2x-1)^2} \quad \ldots\ldots\ldots \quad \text{D}_{\text{vii}}.$$

$$\frac{x}{x-1}=\left(\frac{2x+1}{2x}\right)^2\cdot\left(\frac{4x^2}{4x^2-1}\right)^2\cdot\frac{(2x-1)^2}{(2x-1)^2-1} \quad . \quad . \quad \text{D}_{\text{viii}}.$$

&c. &c. &c.

In the examples the mode of procedure has been to make use of one or other of these expressions, as it may happen to suit itself to our immediate purpose,—that purpose being to select, with careful discrimination in the selection, a promising set of primary equations to develope; developing these with a view to coalescing as many fractions as possible into others with larger denominators, at the same time trying to secure as many small digits or ciphers as possible in the denominators, so as to render them easy divisors, and, where we fail in this, so large as to have but few terms to compute. We may then proceed as follows:

$$2=\frac{6}{3}=\frac{6}{5}\cdot\frac{5}{4}\cdot\frac{4}{3};$$

and since $2^3.\frac{5}{4}=10$,

$$10=\left(\frac{6}{5}\right)^3\cdot\left(\frac{5}{4}\right)^4\cdot\left(\frac{4}{3}\right)^3.$$

Let $x=3$ in equation D_{vi}, then

$$\frac{4}{3}=\left(\frac{11}{10}\right)^3\cdot\frac{10^2}{10^2-1}\cdot\frac{11^2-1}{11^2};$$

and if $x=4$ in equation $D_{\prime\prime\prime}$, we have

$$\frac{5}{4}=\left(\frac{11}{10}\right)^3\cdot\left(\frac{10^2}{10^2-1}\right)^2\cdot\frac{9^2}{9^2-1};$$

so also if $x=5$ in equation $D_{\prime\prime}$,

$$\frac{6}{5}=\left(\frac{11}{10}\right)^2\cdot\frac{11^2-1}{11^2}.$$

Substituting these values, we find

$$10=\left(\frac{11}{10}\right)^{23}\cdot\left(\frac{100}{99}\right)^{11}\cdot\left(\frac{81}{80}\right)^4\cdot\left(\frac{120}{121}\right)^6.$$

In equation $A_{\prime\prime\prime}$ let $x=10$, and we have

$$\frac{11}{10}=\left(\frac{81}{80}\right)^8\cdot\left(\frac{6560}{6561}\right)^4\cdot\left(\frac{1680}{1681}\right)^2\cdot\frac{440}{441}.$$

Also in equation $B_{\prime}$ let $nx=80$, $n=40$, $x=2$, and we get

$$\frac{81}{80}=\frac{121}{120}\cdot\frac{243}{242}.$$

Applying these values, we have finally

$$10=\left(\frac{121}{120}\right)^{182}\cdot\left(\frac{243}{242}\right)^{188}\cdot\left(\frac{6560}{6561}\right)^{92}\cdot\left(\frac{1680}{1681}\right)^{46}\cdot\left(\frac{440}{441}\right)^{23}\cdot\left(\frac{100}{99}\right)^{11}.$$

The number 10 is thus resolved into six fractions excellently suited for computation.

Another set may be evolved as follows.

In equation $A_{\prime\prime\prime}$ let $x=1$, then

$$\frac{1+1}{1}=2=\left(\frac{9}{8}\right)^8\cdot\left(\frac{80}{81}\right)^4\cdot\left(\frac{25}{24}\right)^2\cdot\frac{8}{9};$$

and therefore

$$2^3=\left(\frac{9}{8}\right)^{21}\cdot\left(\frac{80}{81}\right)^{12}\cdot\left(\frac{25}{24}\right)^6.$$

But by equation $D_{\prime}$,

$$\frac{5}{4}=\left(\frac{9}{8}\right)^2\cdot\frac{80}{81};$$

consequently

$$2^3\cdot\frac{5}{4}=10=\left(\frac{9}{8}\right)^{23}\cdot\left(\frac{80}{81}\right)\cdot\left(\frac{25}{24}\right)^6.$$

But by equation C,

$$\frac{9}{8}=\frac{10}{9}\cdot\frac{81}{80} \text{ and } \frac{10}{9}=\frac{5}{3}\cdot\frac{2}{3}=\frac{5.5}{3.5}\cdot\frac{2.8}{3.8}=\frac{5.5}{3.8}\cdot\frac{2.8}{3.5}=\frac{25}{24}\cdot\frac{16}{15}$$

Substituting these, we have

$$\frac{9}{8}=\frac{25}{24}\cdot\frac{16}{15}\cdot\frac{81}{80},$$

and therefore

$$10=\left(\frac{16}{15}\right)^{23}\cdot\left(\frac{25}{24}\right)^{17}\cdot\left(\frac{80}{81}\right)^{10}.$$

Now by equation C,

$$\frac{16}{15}=\frac{17}{16}\cdot\frac{256}{255}=\frac{25}{24}\cdot\frac{51}{50}\cdot\frac{256}{255};$$

by substituting in equation $B_{\prime}$,

$$nx=16,\ n=8,\ x=2.$$

In equation $D_{\prime\prime\prime}$ let $nx=48$, $x=24$, $n=2$; then

$$\frac{25}{24}=\left(\frac{51}{50}\right)^{2}\cdot\left(\frac{50^2}{50^2-1}\right)^{2}\cdot\frac{49^2}{49^2-1}=\left(\frac{51}{50}\right)^{2}\cdot\left(\frac{2500}{2499}\right)^{2}\cdot\frac{2401}{2400}.$$

But by equation $D_{\prime}$,

$$\frac{51}{50}=\left(\frac{101}{100}\right)^{2}\cdot\frac{10200}{10201};$$

and if $nx=80$, $n=20$, $x=4$ in equation $B_{\prime}$,

$$\frac{81}{80}=\frac{101}{100}\cdot\frac{405}{404}.$$

Effecting the substitution of these values,

$$10=\left(\frac{101}{100}\right)^{216}\cdot\left(\frac{256}{255}\right)^{23}\cdot\left(\frac{405}{404}\right)^{10}\cdot\left(\frac{2401}{2400}\right)^{40}\cdot\left(\frac{2500}{2499}\right)^{80}\cdot\left(\frac{10200}{10201}\right)^{103}.$$

In equation $B_{\prime\prime\prime}$ let $x=51$, $n=5$, $nx=255$; then

$$\frac{256}{255}=\frac{251}{250}\cdot\frac{12800}{12801};$$

also equation $D_{\prime}$,

$$\frac{251}{250}=\left(\frac{501}{500}\right)^{2}.\ \frac{251000}{251001}.$$

And if $nx=404$, $n=101$, $x=4$,

$$\frac{405}{404}=\frac{401}{400}\cdot\frac{40500}{40501};$$

and from equation $A_{\prime\prime}$,

$$\frac{101}{100}=\left(\frac{401}{400}\right)^4\cdot\left(\frac{160800}{160801}\right)^2\cdot\frac{40400}{40401}.$$

But

$$\frac{401}{400}=\frac{501}{500}\cdot\frac{2005}{2004};$$

and if $nx=2004$, $x=501$, and $n=4$ in equation $B_{\prime\prime\prime}$,

$$\frac{2005}{2004}=\frac{2001}{2000}\cdot\frac{1002500}{1002501}.$$

Finally, from equation $A_{\prime\prime}$,

$$\frac{501}{500}=\left(\frac{2001}{2000}\right)^4\cdot\left(\frac{4004000}{4004001}\right)^2\cdot\frac{1002000}{1002001}.$$

Substituting these values,

$$10=\left(\frac{2001}{2000}\right)^{4554}\cdot\left(\frac{2500}{2499}\right)\cdot\left(\frac{2401}{2400}\right)^{40}\cdot\left(\frac{4004000}{4004001}\right)^{1840}$$
$$\cdot\left(\frac{1002000}{1002001}\right)^{920}\cdot\left(\frac{1002500}{1002501}\right)^{874}\cdot\left(\frac{160800}{160801}\right)^{432}\cdot\left(\frac{40400}{40401}\right)^{216}$$
$$\cdot\left(\frac{10200}{10201}\right)^{103}\cdot\left(\frac{251000}{251001}\right)^{23}\cdot\left(\frac{12800}{12801}\right)^{23}\cdot\left(\frac{40500}{40501}\right)^{10}.$$

We thus see that by assuming different forms of fractions in the preliminary steps, and carefully examining the results, very superior sets of fractions may be hit on, and the modulus computed, to any amount of precision that may be fixed on, with ease, not perhaps so much from saving of figures as from the vast amount of relief from mental labour that such efficient divisors secure.

LXIV. *On Electric Conduction, and the possibility of curing the retardation of Electric Waves in Submarine Telegraph Lines.* *By* JOHN JAMES WATERSTON, *Esq.*[*]

IN the September Number of the Philosophical Magazine I endeavoured to show that the thermal action in the metallic part of the voltaic circuit took place on the surface only, and not through the substance of the conductor. I took for granted that there was no question as to this being the case in the conduction of common electricity, since all are agreed that, as charge, it exists only on the external surface of conducting-matter.

It appears I have been too hasty in assuming this to be an established point. In the evidence before the committee on the Atlantic cable, an experienced electrician holds that common electricity is conducted through the substance of the wire, the same as galvanic (Report, Appendix II. p. 327, § 208); and this is more distinctly affirmed in a class-book on chemical physics, which there is little doubt represents what is generally held and taught by professors.

At p. 459, § 276, of Dr. Miller's 'Chemical Physics,' the process of conduction is described in the following words:—"In all cases where electricity is in motion, whether it be exerted by chemical action as in the voltaic pile, or by friction as in the common electrical machine, the force *is conveyed by the entire thickness of the conductor*; the charge is not confined to the surface, as occurs when the power is stationary, and produces effects by induction only." This is a distinct statement; but the evidence upon which so important a point may be supposed to rest is not given, nor is any reference made to where it may be found. A little further on (p. 461) a figure is given of the apparatus used by M. E. Becquerel in measuring the conducting-power of wires of different metals for voltaic electricity. If by means of such an apparatus we find that *an iron wire plated with copper conducts the same as a copper wire of the same diameter, and that a copper wire plated with tin conducts no better than a tin wire of the same diameter,* we may at once conclude by such *crucial experiment* that voltaic electric conduction is a surface phenomenon; and *e. g.* in designing a submarine telegraph-cable with its strength in the core, we may be convinced of the propriety of rather having a steel wire coated with copper than, as in Allan's system, a copper central wire with steel wires enclosing it.

That common electric conduction is a surface phenomenon may, I think, be held to be a direct consequence of its existing

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[*] Communicated by the Author.

only on the outer surface of a conductor when in its normal static condition of charge. Thus let A, B, C, D, fig. 1, be four metallic plates parallel and equal to each other. Let A and D be connected to earth or by return wire, and let B C be joined by a long metallic cylinder B L L C. Let there be interposed between A and B a thin dielectric plate, and another between C and D.

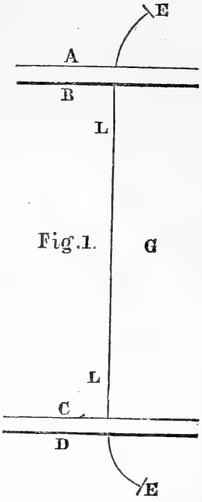

Fig. 1.

Let B and C be equidistant from A and D respectively, and the whole BLLC charged with positive electricity. The greater part of the charge will be located on the sides of B and C that are adjacent to A and D, being inductively retained there as in the magic pane. Let this charged system B L L C be supposed to move closer to A, the surface B draws nearer to A, and simultaneously the surface C withdraws from D (the parallelism being strictly maintained). The attractive force between B and A now becomes greater than between C and D. Also the quantity of electricity engaged between B and A becomes greater than between C and D. Electricity leaves C and travels to B; and this takes place more and more as B gets closer to A. Thus we have a current of positive electricity travelling from C to B and conducted by means of L L. If the motion of the system is towards D, a positive current travels back from B to C. Thus a current may be made to flow in either direction by mechanical force applied to the system, and it may be made to flow with greater or less speed according as the movement is made more or less promptly.

Let an electroscope be placed at G: the leaves will show maximum divergence when the system is exactly in the middle between A and D, and will close as C approximates D or B approximates A. We may assume the distance between B and A to be less than any given magnitude, in which case the tension of the electricity becomes indefinitely small (although discharge has been prevented by the interposed dielectric). By applying mechanical force and expending work, we separate the plates and raise the tension of the electricity, which consequently moves along the conductor; and this may be supposed to be effected with an indefinitely great quickness, so as to represent the discharge of a Leyden jar. Those who affirm that electricity in motion "*is conveyed by the entire thickness of the conductor* L L," call upon us to believe that, while at the beginning and end of any minute interval in the above motion the charge is confined to the surface, yet during the minute interval in which the electricity is in motion it burrows below the surface and moves through the body of the conductor.

That the actual *potentiality* of the electric force resides, not upon the charged surface, but in the space adjacent and exterior to it, has, I think, been experimentally established (see § 54, "Electrical Experiments," Phil. Mag. February 1865). That such is also the case with the electricity of the galvanic current we have some proof in the phenomena of induced currents. It is true that a current of this kind (M), while the circuit is closed, does not induce a current in an adjacent wire circuit (N) at rest, but the inductive influence is manifested when N approaches or recedes from M; therefore that very potential influence emanating from M must exist in the space occupied by N before N begins to move, and before the inductive current consequent to that motion makes its appearance. That change of distance between N and M should be accompanied by change of electric tension in N is a direct consequence of electricity being an emanative force. That change of tension accompanied with transference of dynamic or work force is the primary condition of a current we may clearly recognize in the example, fig. 1. The difference of tension in a current giving rise to a gradient of tension has been experimentally established in a happy manner by Mr. Latimer Clark (Report on the Atlantic Cable, Appendix II. §§ 23, 25).

Should the crucial experiment above suggested prove that conduction in metal conductors is a surface phenomenon, I think it will be found possible to avoid the retarding effect of induction in submarine cables. The mere covering of gutta percha upon a wire does not cause retardation (Atlantic Cable Report, Appendix II. § 69). The presence of a conducting medium on the outside of covered wire is necessary. Hitherto experiments seem to have been confined to iron and water as the outside conductors. So far as I know, the effect of copper as outside conductor has not yet been tried. Iron has one-sixth or one-seventh the conducting-power of copper; water only about 1000 millionth. Electric conduction as a whole depends on the freedom of motion allowed to the force as a whole. *The induced current should thus have the same freedom to move as the primary.*

Practical considerations would probably require that the outside copper conductor should be insulated; so that, to make the trial effectively, it would be necessary to connect the outside coating (of woven thin copper wire?) with one pole of the battery, while the other pole led to the central wire. At the signal end, of course, a similar arrangement (fig. 2). This would give to the induced current the same propelling force as the primary, and utilize the half of the power of the battery that is now lost by putting to earth. That putting to earth is discharge of half the force of the current is an obvious deduction from theory (see Phil. Mag. for March 1865), but it has also been experimentally proved by Wheatstone (Atlantic Cable Report, p. 292, Exp. 5). At p. 103, § 30, Atlantic Cable Report, a similar arrangement to fig. 2 is suggested by Mr. C. V. Walker; but the necessity of having the outside and inside conductor of the same metal—the vital point in my opinion—is not insisted upon. This arrangement would exclude earth-currents (§§ 21, 25, p. 102, Atlantic Cable Report); and if the signal-house, as well as the shore end of the cable, be iron-covered, it is difficult to see how atmospheric electricity could intrude upon the signals.

There are other considerations with respect to conduction that press upon the attention as suggested by theoretical considerations, but it would be presuming too much to expect they would receive attention before the fundamental point of surface conduction has been experimentally established.

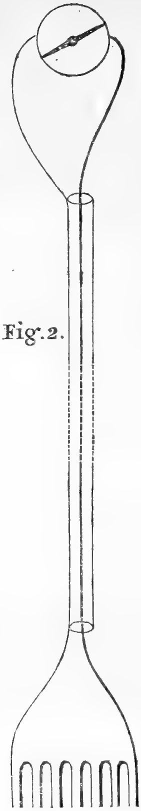

Fig. 2.

Inverness, November 20, 1865.

LXV. *Chemical Notices from Foreign Journals.*

By E. Atkinson, *Ph.D., F.C.S.*

[Continued from p. 363.]

WINKLER has communicated* a series of observations on indium. Hitherto the Freiberg zincblende has been the only material generally accessible; the zinc from this source is

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* *Journ. für Prakt. Chemie*, xciv. 1. *Zeitschrift für Chemie*, vol. i. p. 228.

far more convenient for the production of the new metal. Besides small quantities of lead, iron, arsenic, and cadmium, it contains 0·0448 per cent. of indium.

The method given by the discoverers, of precipitating by ammonia*, in which oxide of indium is absolutely insoluble, may be used; and besides the method of separating given by Weselsky†, the following may be used:—

Granulated zinc is dissolved in dilute acid, care being taken that a small quantity remains undissolved, and the liquid boiled until there is no more disengagement of gas. In the precipitate formed, which mainly consists of lead, all the indium is contained, together with some arsenic, iron, and cadmium.

Or to the solution some sulphuric acid is added, and it is then neutralized with carbonate of soda until a feeble turbidity ensues; acetate of soda being added, the whole is then boiled for a time. A ferruginous basic sulphate of indium is thus precipitated, together with some oxide of zinc, which is washed by decantation.

Either of the precipitates obtained by the above methods is dissolved in nitric acid, the lead precipitated by sulphuric acid, and the other foreign metals by sulphuretted hydrogen; the latter removed by warming, the iron oxidized by chlorate of potash, and precipitated by ammonia. The precipitate contains indium, together with iron and some zinc. This is dissolved in acetic acid and heated with sulphuretted hydrogen, which precipitates all the indium, with some iron and zinc, which adhere to it very pertinaciously. They are best removed by dissolving the sulphide in hydrochloric acid, boiling well to expel sulphuretted hydrogen, and allowing the mixture to stand for twelve to twenty-four hours with excess of carbonate of baryta. This process is well adapted for the quantitative determination of indium.

The oxide is best reduced by heating it in a very slow current of hydrogen. The metal is thus obtained in several small globules, which can be melted under cyanide of potassium to a regulus.

Indium in its colour resembles platinum; it is softer than lead, can be cut with a knife, and rolled out in thin sheets by very slight pressure. It colours paper, and takes the brightest polish with a fine cloth. It retains its lustre in the air. It dissolves in dilute hydrochloric and sulphuric acids with disengagement of hydrogen, gives sulphurous acid with concentrated sulphuric acid, and is quickly oxidized by nitric acid. Heated in a crucible to bright redness, it evaporates and burns with a violet light to a yellow oxide. Its specific gravity is 7·362 at 15°.

The oxide InO appears to be the only one. It is of a straw-

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* Phil. Mag. S. 4. vol. xxvii. p. 199 *et seq.*

† Ibid. vol. xxix. p. 328.

yellow colour; obtained in pieces, by igniting the hydrate, it is of a honey-yellow colour and transparent. Its salts are white. From their solutions zinc precipitates the metal in white lustrous laminæ. The hydrated oxide is a white voluminous precipitate like alumina, quite insoluble in ammonia and the fixed alkalies.

Sulphate of indium was obtained in indistinct crystals. It is more soluble and more difficult to crystallize than the zinc sulphate. *Nitrate of indium* readily crystallizes from an acid solution, in stellate groups of prisms.

Sulphide of indium is precipitated by sulphuretted hydrogen from an acetic acid solution as a yellow slimy precipitate, which dries up to reddish-brown brittle pieces. The presence of strong acids prevents the precipitation. The clear yellow precipitate produced by sulphide of ammonium is dissolved in an excess of the precipitant when warm. On cooling, the sulphide is again deposited, probably as hydrate.

Winkler found the atomic weight of the metal to be somewhat less than that found by the discoverers (37·128). He determined it by converting the nitrate into oxide by ignition. He found the number 35·918 as the mean of three determinations.

Dr. Böttger describes* the following simple method of obtaining selenium from the residue of the sulphuric-acid works. The mass is freed from acid by repeated washings with water; it is then boiled with a concentrated solution of sulphite of soda until it has become quite black in colour, owing to the presence of lead. The solution is then filtered through a double filter-paper, and the filtrate is allowed to drop into dilute hydrochloric acid. The selenium, which has been dissolved up by the sulphite, is then immediately precipitated in thick vermillion-coloured flakes, which, in case any foreign matters adhere to them, may be perfectly purified by being again subjected to the same treatment.

Marignac has investigated a number of niobium compounds†, both analytically and crystallographically. Rose described a fluoride of niobium which he called *Unterniobfluorid.* He considered it to be the fluoride of an allotropic modification of the metal niobium, *Unterniobium,* $\mathrm{Hnb\,Fl^3}$; Marignac has investigated a number of double salts of this body, all of which crystallize well, and exhibit simple ratios of composition.

The analysis shows that this fluoride contains three atoms of fluorine. The double salts are isomorphous with the correspond-

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* *Zeitschrift für Chemie,* vol. i. p. 528. *Journal für prakt. Chemie,* vol. xciv. p. 439.

† Liebig's *Annalen,* July 1865.

ing double fluorides of tin and titanium. In all cases the group $\mathrm{Hnb\,Fl^3}$ (if Hnb represents the atomic weight of niobium in the hyponiobic compounds) is exactly equivalent to a molecule of fluoride of tin, $\mathrm{Sn\,Fl^4}$, or fluoride of titanium, $\mathrm{Ti\,Fl^4}$.

To bring this into accordance with the doctrine of isomorphism, two suppositions are possible. If hyponiobium is an element, then it must be assumed that hitherto unknown fluorides of tin and titanium, $\mathrm{Sn\,Fl}$ and $\mathrm{Ti\,Fl}$, are compound radicals which play the part of metallic elements, just as ammonium replaces potassium.

Or hyponiobium is not a simple body as Rose had supposed, but contains one atom of a metalloid which is isomorphous with fluorine.

Now not only are the fluoride-of-hyponiobium double salts isomorphous with those of fluoride of titanium, but also with those of oxyfluoride of tungsten. All these compounds acquire analogous formulæ, if it is assumed that hyponiobium is an oxide, $\mathrm{Nb}\Theta$. These fluorine double salts stand then, as regards their composition, intermediate between the potassium fluoride of titanium and the potassium oxyfluoride of tungsten, thus—

$$\begin{array}{ll} \mathrm{Ti\ K^2\,Fl^6}, & \mathrm{H^2}\,\Theta, \\ \mathrm{Nb\,K^2\,Fl^5}\,\Theta, & \mathrm{H^2}\,\Theta, \\ \mathrm{W\ K^2\,Fl^4}\,\Theta^2, & \mathrm{H^2}\,\Theta. \end{array}$$

On this view the chloride of hyponiobium becomes an oxychloride, $\mathrm{Nb}\Theta\mathrm{Cl^3}$; hyponiobic acid, or properly *oxyniobic* acid, has the formula $\mathrm{Nb^2\,O^5}$, or perhaps $(\mathrm{Nb}\Theta)^2\,\Theta^3$.

Marignac found for the equivalent of niobium, on this view, the number 93 ($\mathrm{H}=1$, $\Theta=16$) (Rose found 97·6). If hyponiobium is an oxide, several peculiarities in Rose's investigation are cleared up. In his chloride of hyponiobium he always found oxygen, which he ascribed to an admixture of a certain quantity of an oxychloride. But on determining the amount of oxygen, he obtained three times as much as he could have done on this supposition, and he ascribed the result to an error of experiment. His analyses agree, however, better with the formula $\mathrm{Nb}\Theta\mathrm{Cl^3}$ than with $\mathrm{NbCl^3}$. On the new supposition, too, it is easy to see why Rose failed in converting the chloride of hyponiobium into chloride of niobium by excess of chlorine.

Marignac's formulæ have acquired a confirmation in the experiments of Deville and Troost, who have determined the vapour-density of oxychloride of niobium, as they had previously done that of the chloride.

They found that of the oxychloride of niobium to be 7·87 at 440° and 7·89 at 860°. The formula $\mathrm{Nb}\Theta\mathrm{Cl^3}$ requires 7·5.

To demonstrate directly the presence of oxygen in oxychloride of niobium, it was heated with magnesium. The reduction takes place easily, provided the temperature at which oxide of niobium is reduced is not exceeded. The magnesium and the platinum boat in which the reduction took place were covered with apparently regular lustrous crystals, which were not altered by water, and when heated in the air were changed into hyponiobic acid. Crystals of the same kind were obtained when hyponiobate of potash was heated with sodium. Probably this body stands to niobium in the same relation as Peligot's uranyle to uranium.

Von Hauer has made a communication* on the easily fusible alloys produced by the addition of a little cadmium to the easily fusible mixture of tin, lead, and bismuth. As the alloys described by Wood approached very near to mixtures in equivalent proportions, v. Hauer fused substances together in definite proportions and determined their fusing-point and density. The specific gravity and the combining-proportion of the metals taken were—

Cd,	spec. grav.	8·572	equiv.	56
Sn	„	7·625	„	58
Pb	„	11·350	„	103·7
Bi	„	9·708	„	210

The results of his experiments are given in the following Table:—

Formula.	Specific gravity. Found.	Specific gravity. Calculated.	Difference.	Fusing-point.
$Cd\,Sn\,Pb\,Bi$	9·765	9·624	+0·141	68°·5 C.
$Cd\,Sn^2\,Pb^2\,Bi^2$	9·784	9·698	+0·086	68·5 „
$Cd^3\,Sn^4\,Pb^4\,Bi^4$	9·725	9·666	+0·059	67·5 „
$Cd^4\,Sn^5\,Pb^5\,Bi^5$	9·685	9·652	+0·033	66·5 „

In all these cases there is a contraction, but it seems that the smallest contraction accompanies the lowest melting-point. All these alloys are pasty below their proper melting-points, and only solidify a few degrees lower. The melting-point of an alloy of 3 *parts by weight* of Cd, 4 Sn, 8 Pb, and 15 Bi, the author found to be 70°, and not, as Lipowitz had observed, 60°. He found the melting-point of an alloy of 2 parts by weight of Cd, 3 Sn, 11 Pb, and 16 of Bi to be 76°·5.

If 8 to 10 per cent. of cadmium are added to Rose's fusible metal, its melting-point sinks to 75°. If, in the same alloy, lead

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* *Journal für praktische Chemie,* vol. xciv. p. 436. *Zeitschrift für Chemie,* vol. i. p. 475.

is replaced by cadmium, the fusing-point is not altered. An alloy of 50 parts of solder ($PbSn^3$) and 50 parts Bi is liquid at 92–93°. The author has further determined the fusing-point of the following alloys:—

	Specific gravity.		Differ-ence.	Fusing-point.
	Found.	Calculated.		
1 Cd 6 Pb 7 Bi .	10·529	10·330	0·199	88°
Cd Bi^2 Pb^3 .	10·563	10·275	0·288	85·5
Cd^2 Bi^4 Pb^7 .	10·732	10·341	0·391	95

The first alloy was prepared according to the proportions given by Wood, who found 82° as its melting-point. The others are prepared in equivalent proportions.

According to a statement of Troost*, a magnesium wire of 97 millims. in length and 0·33 millim. in thickness, gives by its combustion as much light as sixty-four candles; and this intensity is increased to 110 when the combustion takes place in oxygen. He also mentions that an equally intense and more economical light is obtained if, instead of pure magnesium, an alloy of magnesium and zinc is burned.

Kraut describes the following lecture experiment†. A platinum wire 0·5 millim. in thickness is coiled fifteen to twenty times round a lead pencil so as to form a spiral, the end of which is passed through and round a longish cork crosswise. This spiral is hung in a wide-necked flask of about 450 cubic centims. capacity, standing on wire gauze over a lamp: ammonia-water of 20 per cent. is poured in this flask so that the point of the spiral just stands above the liquid. On the other hand, a gas-holder of oxygen is connected with a bent glass tube (20 millims. in the clear) the end of which just dips in the liquid. If the spiral is now heated to redness, and oxygen is allowed to enter, the flask at first fills with white vapours of nitrite of ammonia, and then with intense red ones of nitrous acid; and in general the glass tube which conveys the oxygen is covered with a crust of nitrite of ammonia. If now the ammonia-water is warmed, the mixture of oxygen and ammonia-vapour explodes with a loud but perfectly safe explosion, by which the platinum wire is cooled down below the temperature at which it visibly glows. In a few moments the spiral is heated to redness, the gas mixture is again exploded, and so on; thus the experiment can be repeated as often as is liked. With a rapid current of oxygen it burns for some time under the liquid; and if the end of the tube is placed

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* *Cosmos*, 1865, p. 533.

† Liebig's *Annalen*, October 1865.

directly over the ammonia and quite near the spiral, the oxygen burns, producing the long-drawn sound of the chemical harmonicon. The oxygen-ammonia flame hangs then as a greenish-yellow bubble at the aperture of the glass tube, and, without being extinguished, moves up and down. The cork which supports the platinum spiral can only be laid crosswise over the aperture of the boiling flask, as it is often projected by the explosions.

Mitscherlich[*] describes a method according to which, as he states, chlorine, iodine, and bromine compounds can be discovered by the spectrum-apparatus, and even the smallest quantities of these halogens detected.

The method depends upon the property which copper has of forming compounds with the halogens, which are very hard to decompose by the influence of other substances, and are only resolved into their constituents at the highest attainable temperature. The spectra of these compounds are admirably adapted for recognizing the smallest quantities of the halogens. In this respect copper has a material advantage over other metals, inasmuch as their haloid compounds are decomposed into their constituents at lower temperatures, and the spectra either of the oxide or of the metal are obtained.

The application of the new method is as follows. The dried solid substance is mixed with half its weight of sulphate of ammonia, and a tenth its weight of oxide of copper, and heated according to method 3 of Mitscherlich's paper[†]—that is, is placed in the bulb of a glass tube and slowly heated in a current of hydrogen gas. The hydrogen being inflamed, is at first coloured by some lower degree of oxidation of copper until the oxide is reduced: this is too undefined to give a definite spectrum; subsequently the spectra of the haloid salts of copper occur. With very feeble reactions the chlorine compound is best recognized by the lines at 105 and 109, and by the brightnesses 85 to 87 of the scale; the bromine compound by the brightnesses 85, $88\frac{1}{2}$, and 92; and the iodide of copper by the brightnesses 96, 99, and $102\frac{1}{2}$. These numbers refer to spectra of the salts which appeared in Poggendorff's *Annalen* of last year, and of which improved copies in colour are promised by the author.

By the above method, $\frac{1}{4}$ per cent. of chlorine, $\frac{1}{2}$ per cent. of bromine, and 1 per cent. of iodine can be detected without much practice. A skilful observer can detect far smaller quantities. A disadvantage of the method is that the sulphate of ammonia, which decomposes at a high temperature, gives the ammonium spectrum and obscures the reaction. In order, then, to detect the

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[*] Poggendorff's *Annalen*, August 1865.

[†] Phil. Mag. S. 4. vol. xxviii. p. 169.

halogens, they are precipitated with a silver-salt, the dried precipitate mixed with double its weight of oxide of copper, and investigated as above stated. In this manner less than $\frac{1}{10}$ per cent. chlorine, $\frac{1}{9}$ bromine, and $\frac{1}{8}$ iodine can be found in the precipitate produced by silver. The spectra appear in the order of their respective volatilities—first that of the chloride of copper, then that of the bromide, and finally that of the iodide.

If small quantities of bromine or iodine are to be detected in the presence of large quantities of chlorine, a small quantity only of nitrate of silver is added; the bromide and iodide are in that case precipitated first. Thus in one case 5 milligrammes of bromide of sodium were mixed with a pound of common salt, and to the solution a decigramme of nitrate of silver added and the precipitate obtained treated in the above manner. After the spectrum of chloride of copper was observed for some time, the spectrum of bromide of copper could be easily seen for five minutes. Corresponding experiments made with iodine gave similarly sharp results.

It has usually been considered that nascent hydrogen acted on nitric acid in two ways: either there was a partial removal of oxygen with production of lower oxides of nitrogen, or there was a total removal of the oxygen, while at the same time hydrogen was taken up, producing therefore ammonia. Lossen, however, has shown* that under certain circumstances a body is obtained which stands between the oxygen compounds of nitrogen and its hydrogen compound ammonia—a product of the reduction of nitric acid which has parted with some of its oxygen and has taken up hydrogen. This body, whose composition is represented by the formula $NH^3\Theta$, Lossen calls *hydroxylamine.*

It is produced by the action of tin and hydrochloric acid on nitric ether. The mixture soon becomes heated, but without any considerable disengagement of hydrogen. When the action is complete, the tin is removed by sulphuretted hydrogen, and after concentration, at first large quantities of sal-ammoniac crystallize out, and afterwards the hydrochloric compound of the new body. This is separated from the sal-ammoniac by treatment with absolute alcohol, and then precipitating the sal-ammoniac with bichloride of platinum, which does not precipitate the hydrochlorate of hydroxylamine.

Leaving out of view the ethyle of the nitric ether, which is simply exchanged for hydrogen, the formation of hydroxylamine is thus expressed,

$$\underset{\text{Nitric acid.}}{NH\Theta^3} + 6H = \underset{\text{Hydroxyl-amine.}}{NH^3\Theta} + 2H^2\Theta.$$

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* *Berichte der Berliner Academie,* July 1865.

The hydrochlorate of hydroxylamine has the formula $\mathrm{NH^3\Theta, HCl}$. It crystallizes in distinct prisms. The sulphate, which also crystallizes, has the formula $\mathrm{2NH^3\Theta, H^2S\Theta^4}$. The nitrate is extremely soluble, and could not be obtained in crystals. The oxalate crystallizes from a hot saturated solution in beautiful prisms. It has the formula $\mathrm{2NH^3\Theta, \text{Ȼ}^2H^2\Theta^4}$.

Hitherto the body has not been obtained pure in the free state. When a strong solution of one of its salts is treated with caustic potash, a disengagement of nitrogen takes place with the simultaneous formation of ammonia. The decomposition is essentially expressed by the equation

$$\mathrm{3NH^3\Theta = NH^3 + 2N + 3H^2\Theta}.$$

If in a solution of this sulphate the sulphuric acid is exactly precipitated by baryta-water, a solution of hydroxylamine is obtained which is somewhat more stable. It can be boiled without any material decomposition taking place; and a part of the hydroxylamine passes off with the aqueous vapour.

Hydroxylamine is an oxygen base which differs from the metallic oxides in the mode in which it unites with acids. While the latter unite with acids with liberation of water, hydroxylamine, like ammonia, unites directly with the acids. Hydroxylamine is an ammonia in which an atom of hydrogen is replaced by the radical $\mathrm{H\Theta}$. To this radical the name hydroxyle has long been given; and the compound $\mathrm{NH^3\Theta}$ is as much entitled to the name hydroxylamine as the compound $\mathrm{N\text{Ȼ}H^5}$ to the name methylamine.

Berlandt* describes the following method of giving a conducting surface to a body which is to be electrotyped. The impression is wetted with dilute alcohol to avoid the formation of air-bubbles, then washed with distilled water, and while still wet a solution of nitrate of silver (one in four of water) is poured over it; in a few minutes the excess of liquid is poured off, and a solution of green vitriol (one in three) poured over it. In five minutes this is allowed to drain off; the impression is again treated with solution of silver, and then with vitriol solution three or four times, till the surface of reduced silver has a whitish-grey colour. The impression is then washed with pure water and subjected to the action of the electric current. At the first moment the entire cast is covered with a thin layer of copper of dark brown colour, and in a few minutes the layer, which firmly adheres, acquires a natural copper-colour.

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* *Archiv der Pharm.* vol. cxxi. p. 54.

LXVI. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 382.]

Nov. 8, 1865.—W. J. Hamilton, Esq., President, in the Chair.

THE following communications were read:—

1. "On the Submarine Forest-beds in Porlock Bay." By R. A. C. Godwin-Austen, Esq., F.R.S., For. Sec. G.S.

The submerged forest of Porlock Bay has been briefly noticed by Sir H. De la Beche, but has not been sufficiently described, inasmuch as it illustrates more clearly than the larger forests of Bridgewater Level or Swansea Bay the nature and order of the oscillations of small amount which have taken place at times shortly antecedent to the present, and which were classified by the author in this paper as follows:—(1) The formation of an angular detritus and its accumulation at lower levels; at this period the land was at its highest relative elevation. (2) Forest-growths established on the detrital beds just mentioned; the stumps of these forest-trees, some of which are of large size and great age, at present constitute the *Submerged Forest*. (3) Accumulation of freshwater mud (resulting probably from a depression of the land), by which the trees were killed. (4) Surface of water-plant growths on the mud-deposit, or nearly dry surface, on which the trees fell. (5) Area depressed below the sea-level, and sea-mud with *Scrobiculariæ* deposited. (6) Conversion of the surface into meadow-land at the level of the highest springs of the present day, caused by re-elevation. (7) Shingle, heaped up on the meadow-land, and showing that the most recent change has been one of slight depression.

This succession of changes corresponds with that of numerous other localities in the West of England; and as Mr. Godwin-Austen considered the angular detritus (1) to be the result of subaërial weathering, which took place over this area approximately contemporaneously with the deposition of the Boulder-formation in more northern districts, this Porlock submerged forest seems to fix a relative date for the rest—namely, as being subsequent to the Glacial Period. By this means, also, a relative date is arrived at for many of the raised beaches in the West of England, as of that near Baggy Point, where the old sea-bed, at an elevation of 60 feet, is covered by an enormous accumulation of angular débris; these raised beaches may therefore correspond in time to certain marine deposits, which, in Norfolk and Suffolk, next underlie the great "Boulder Formation."

2. "On the Marine Origin of the 'Parallel Roads' of Glen Roy." By the Rev. R. Boog Watson, B.A., F.G.S.

After a brief description of these well-known "Roads," the author gave an analysis of the two principal theories that have been started to account for their formation—namely, the Ice-dam theory and the Marine theory. With regard to the first theory, Mr. Boog Watson stated his opinion that, although it has some strong points, especially in respect of the coincidence between the levels of the "Cols" at the glen-heads and those of the "Roads," yet on the other hand it is weak inasmuch as the cause assigned is extremely local in its action, while the phenomena to be explained are very general and have a wide range—terraces similar to those of Glen Roy occurring in Scandinavia and elsewhere. In the author's opinion, also, the ice-dam is impossible, and would be inefficient if possible ; it would not be watertight, and there is no place for it in the history of the Postpliocene changes in Scotland. But he remarked that objections like these cannot be urged against the Marine theory, as the sea has been on the spot, and is able to perform the work required of it. At the same time the author admitted that the Marine theory is not free from difficulties, the chief being the perfection and horizontality of the "Roads," and their barrenness in marine organisms; and he concluded by suggesting some explanations of these apparent anomalies.

LXVII. *Intelligence and Miscellaneous Articles.*

ON THE ELECTRICITY OF THE TORPEDO.

BY M. CH. MATTEUCCI.

In the structure and electric function of the organ of the torpedo, as far as we are at present acquainted with it, there is evidently still a very obscure but very important problem, which we shall sooner or later solve: I mean that of the relation existing between the function of the nerves and electricity. Being convinced that my later experiments upon the electromotor power of the organ of the torpedo *in a state of repose* are calculated to throw great light upon this problem, I was unwilling to lose the opportunity offered by a very brief stay at Via Reggio, a seaport from which I procured a large number of torpedoes when I was at Pisa, to repeat the most interesting of these experiments.

There is no difficulty in discovering the electromotor power of the organ of the torpedo independently of the discharge, in the state which I have called that *of repose* : it is only requisite to possess a galvanometer sensitive to the muscular current of the frog, and to close the ends of the instrument with two plates of amalgamated zinc plunged in sulphate of zinc, and communicating with each other by pads of flannel or filtering-paper. The galvanometer which I used was not provided with a good astatic system ; so that I only obtained a deviation of from 40 to 45 degrees with the gastrocnemius of a not very lively frog. With this instrument, a piece of the organ cut from a small torpedo which had ceased to give discharges, produced a deviation of 14 or 15 degrees in the same direction of the current as that obtained at the moment of the discharge. This result is constant. The following are the principal results at which I arrived, and which confirm my former experiments.

1. A piece of the electric organ, cut from a torpedo which gave no sensible discharge to the galvanoscopic frog when its skin was irritated, gives a *constant current* between the dorsal and abdominal surfaces in the same direction as the discharge obtained on pulling

or cutting the nerves of this piece. With a delicate galvanometer, the needle of the galvanometer settles at 40 or 50 degrees, and remains so for a long time. I obtained a persistent and well-marked deviation with some portions of the organ which had been kept for five or six days in a cavity made in a piece of ice. It has been thought (I believe, in Germany) that this electromotor power of the organ of the torpedo in a state of repose was analogous to that of living muscles; but experiment does not appear to me to support this hypothesis.

2. In fact, the electromotor power of the organ of the torpedo in a state of repose increases markedly after the portion of the organ has been made to give the discharge by the irritation of its nerves. This increase also remains for a certain time, and diminishes but very slowly. I may relate in regard to this point, an experiment which never fails to succeed, and which consists in opposing two portions of the organ cut from the same torpedo, in such way that no sign of the differential current is given. If the nerves of one of the portions be then irritated, and the circuit of the galvanometer be re-established, the needle will immediately be seen to deviate, from the existence of a strong differential current, which is persistent, and is owing to the irritated organ. We know that on repeating the same experiment with two pieces of muscle, the *very opposite* result is obtained.

3. During the very hot season especially, I have met with some torpedoes which, when out of the water, rapidly lost their electric function, and in which the electromotor power in a state of repose was none, or almost none. On irritating the nerves of the organ of these torpedoes, or on wounding the fourth lobe, the electromotor power reappeared immediately and remained for a certain time. I am thus more than ever induced to believe that, instead of interposing the chemical action of the muscular respiration, as is done with good foundation to account for the electric power of the living muscles, the electricity of the torpedo and other electric fishes ought to be attributed to kinds of secondary piles formed in the cells of the electric organs by the action of the nerves. Just as nervous action intervenes to cause in the organs of secretion the production of liquids of different chemical nature, so we may conceive similar effects to be produced in the elementary cells of the electric organs. We know that the organ of the torpedo, in repose or activity, does not exert an action upon the atmospheric air analogous to that which is produced by muscle in a state of repose or contraction. So also the influence of nervous irritation in increasing in a double manner the electromotor power of the organ of the torpedo cannot be conceived without imagining that it is due to an unceasingly acting cause, such as the presence of two substances capable of acting chemically upon each other, and constantly reproduced by the action of the nerves. I consider that this hypothesis merely indicates the direction in which we may be led to make new experiments with the hope of some success.

I profited by this opportunity to try upon the rays some beautiful experiments recently performed by M. Robin. Several years ago, after this skilful anatomist had announced the discovery of an organ in the rays analogous to that of the torpedo, I tried in vain to obtain signs of electricity in the organ of the ray. In regard to this point, I must own that I was only able to operate upon small and not very lively rays, and that very probably I did not succeed in satisfactorily detecting the organ found by M. Robin. M. Schiff has lately helped me in the preparation, and has taught me to recognize the organ of the ray. Operating upon a very lively and tolerably large ray, and inducing in this fish a number of very strong contractions, I obtained with the galvanoscopic frog, the nerve of which was laid upon the organ, evident signs of electric discharges. I wished to vary this experiment and to repeat it upon other rays, but I was unable to procure them. As the electric organ of the ray is enveloped in a large quantity of muscular substance, I would suggest to M. Robin to repeat and vary his beautiful experiment by operating upon the isolated organ of the fish, so that no doubt may remain that the electric effects obtained are not attributable to the muscular fibre. The difference in the dimensions and the number of the elementary cells, and the nerves, of the electric organ of the ray and that of the other electric fishes, gives great importance to the complete study of this function of the ray; and this study should explain the *peculiar* electric phenomena discovered by M. Robin in the electric function of the ray, and which cannot be verified in the other electric fishes.—*Comptes Rendus*, Oct. 16, 1865.

ON A NEW AVENTURINE, WITH CHROME AS A BASE.

BY M. J. PELOUZE.

It is well known that the sesquioxide of chrome imparts a green colour to fluxes, and especially to glass. The bichromate of potash possesses the same property, as it is decomposed by heat into oxide of chrome and neutral chromate of potash. The latter salt is in its turn decomposed by the silica, whence result oxygen, silicate of potash, and oxide of chrome.

Hence, in the presence of silica, all the chromic acid of the alkaline bichromate passes into the state of oxide of chrome, which remains in the glass. If the proportion of the salt is small, the glass is transparent, perfectly homogeneous, and of a green colour with a yellowish tinge. But if it is larger in a certain proportion, spangles of the sesquioxide of chrome are found in the glass.

My experiments were made with the following proportions, which have always been the same, the proportion of the chromate alone being variable:—

Sand	250 parts,
Carbonate of soda	100 „
Calcareous spar	50 „

First Experiment.—With 10 grms. of the bichromate the glass melts, refines, and anneals well. It is homogeneous, transparent, and green with a tinge of yellow.

Second Experiment—With 20 grms. of the bichromate the glass is worked and annealed as easily as in the preceding case; its colour is very dark green, and minute spangles of the sesquioxide of chrome are easily recognized in it.

Third Experiment.—With 40 grms. of the bichromate, the fusion is decidedly more difficult, and the glass is filled with extremely brilliant crystals. Those persons who saw specimens of this glass, at once compared it to Venetian aventurine, and called it chrome aventurine, which name I propose to retain.

FourthExperiment.—With 50 grms. of the bichromate, fusion is extremely difficult. The glass is filled with a confused mass of spangles, and has no longer the lustre and beauty of the preceding.

Hence the best proportions for the preparation of the chrome aventurine are these:—

Sand	250	parts,
Carbonate of soda......	100	,,
Carbonate of lime	50	,,
Bichromate of potash ..	40	,,

The resulting glass contains from 6 to 7 per cent. of oxide of chrome, nearly half of which is combined with the glass, the other half remaining free in the form of brilliant scales or spangles.

The green aventurine is much more easily prepared than the Venetian. It exists during the fusion of the glass, while the latter is only formed during the annealing. I may refer those who are interested in this question to Fremy and Clémandot's paper upon artificial aventurine in the *Comptes Rendus* for 1846, p. 341*.

Chrome aventurine sparkles in the sun and in strongly lighted places; in this respect it is surpassed by the diamond alone. It is harder than common glass, which it scratches and cuts easily, and is especially harder than the Venetian aventurine; hence its greater value.

Among the irregularities presented by the outlines of the spangles disseminated in the chrome aventurine, M. Daubrée has recognized with certainty the form of the regular hexagon, which belongs to the crystalline system of the sesquioxide of chrome. Moreover the spangles have the greatest resemblance to certain varieties of laminated oligistic iron ore, such as are met with in certain lodes, especially the specular variety; and it is well known that oligiste iron is isomorphous with the sesquioxide of chrome.

The colour of the new aventurine is that of the third yellowish green, the thirteenth shade in M. Chevreul's chromatic circle.

The lapidaries who saw my first specimens of the new aventurine, and who cut some of them, agree in stating that it forms an important acquisition to their branch of manufacture, for which reason I have thought it worth making public.—*Comptes Rendus,* October 16, 1865.

ON THE DETECTION OF POISONING BY CARBONIC OXIDE.

BY M. HOPPE-SEYLER.

Blood charged with carbonic oxide, investigated in the solar spectrum after suitable dilution, exhibits almost exactly the same absorption-bands as oxygenated blood. If sulphide of ammonium be added, the bands do not disappear for some days; while blood free from

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* See also Chem. Gazette for April 1846, p. 141.

carbonic oxide, a few minutes after it has been treated with sulphide of ammonium, shows only one absorption-band between the spectrum-lines D and E. From this unalterability in the bands, the presence of carbonic oxide in the blood may be recognized; in the case of animals which have been gradually poisoned by a small quantity of carbonic oxide, the gas can in this manner be detected. It is desirable in such cases greatly to dilute the blood Numerous cases have shown that after animals have been poisoned with carbonic oxide, recovery takes place in the open air if the poisoning has not gone too far. And Pokrowsky has shown that in this reconvalescence no carbonic oxide, but more carbonic acid than usual is exhaled. Defibrinated blood gradually loses its carbonic oxide on standing in the air; hence in forensic cases rapid investigation is desirable: yet several days may elapse without any appreciable diminution in the quantity of carbonic oxide.—*Zeitschrift für analyt. Chemie*, vol. iii. p. 439.

RELATION OF THE WEIGHT OF A MAGNETIZING SPIRAL TO THE MAGNETIZING FORCE. BY DR. MENZZER.

If an electro-magnet is to be constructed, the current for which is to be produced by a given battery, it is of practical importance to consider, first, what thickness and length of wire must be chosen for the coil, and what combination must be once for all taken in order to obtain the most powerful electro-magnet for the given cost. It is assumed that the maximum of intensity is in all cases obtained when the essential resistance, or that offered by the battery, is equal to the external resistance in the interpolar, *i. e.* in this case the magnetizing coil.

The author gives an investigation in a special case, from which he arrives at the result that "*the magnetizing powers of two coils which give the maximum of intensity are as the square roots of their weights*; and he then states the question in a general manner as follows:—

If e is the electromotive force of one cell, t the number of cells which are united in one element, and s the number of such elements, then $st=n$ is the number of available cells; if, moreover, λ is the internal resistance of one cell, m the length in metres of the copper wire used for the coil, and d the diameter in millimetres of this wire, the maximum intensity of the current is

$$S=\frac{se}{s\frac{\lambda}{t}+\frac{m}{d^2}}, \quad \dots\dots\dots\dots \quad (1)$$

in which

$$\frac{s\lambda}{t} \text{ must } =\frac{m}{d^2}. \quad \dots\dots\dots\dots \quad (2)$$

If, further, m' is the length in metres of the copper wire which is used for one turn, then

$$W=\frac{m}{m'} \quad \dots\dots\dots\dots\dots \quad (3)$$

is the number of turns, and the magnetizing power M of the coil is

$$M=SW=\frac{se}{s\frac{\lambda}{t}+\frac{m}{d^2}}\cdot\frac{m}{m'}\cdot \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

If, again, σ is the specific gravity of the copper, and G the absolute weight in grammes of the wire used, then

$$G=\frac{md^2\pi s\sigma}{2}; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

and therefore

$$\frac{M}{\sqrt{G}}=\frac{\frac{se}{s\frac{\lambda}{t}+\frac{m}{d^2}}\cdot\frac{m}{m'}}{\sqrt{\frac{md^2\pi\sigma}{2}}}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

The expression is constant so long as the number, the electromotive force, and the internal resistance of the cells, as well as the width of the coils and the specific gravity of the wire, remain the same.

For

$$n=st, \text{ and } \frac{m}{d^2}=s\frac{\lambda}{t},$$

from which there is easily obtained

$$t=\sqrt{\frac{n\lambda d^2}{m}}, \text{ and } s=\sqrt{\frac{mn}{\lambda d^2}}.$$

By substituting these values in equation (6), and simplification, we have

$$\frac{M}{\sqrt{G}}=\frac{e}{m'}\sqrt{\frac{2n}{\pi\lambda s}}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

an expression which is quite independent of the thickness of the wire and the mode of combination of the battery, and which is constant as long as the width of the turns, the number, electromotive force, and internal resistance of the cells, and the specific gravity of the wire are unchanged.

Hence generally, the circumstances remaining constant, the magnetizing powers of coils of one and the same metal, with a maximum intensity of the current in each case, are as the square roots of the weights of the wire used.—Poggendorff's *Annalen*, November 1865.

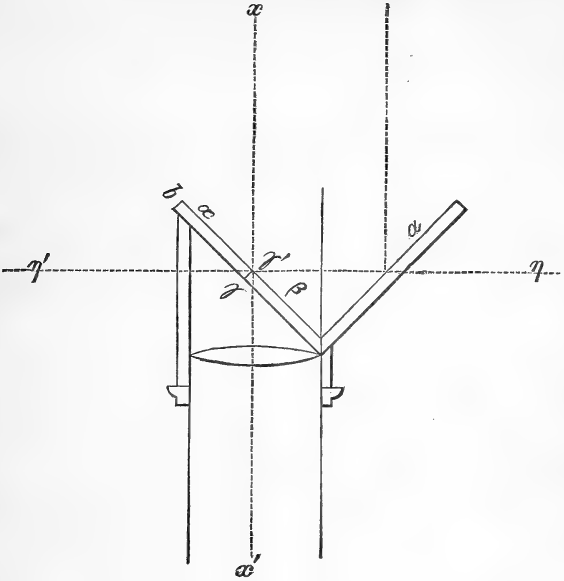
ANOTHER NEW FORM OF THE HELIOTROPE.

BY K. A. STEINHEIL.

The interesting paper of Professor Miller of Cambridge (Phil. Mag. S. 4. vol. xxx. p. 71), on two new forms of the heliotrope, reminds me of one which some thirty years ago I had made, and which since then has been kept in the physical collection of our State, but has never been described. A knowledge of it may contribute to complete the literature on this subject.

In determining heights by triangulation, it is desirable, in order to get rid of part of the uncertainty of the path of a luminous ray that the determinations of two points take place not only mutually but simultaneously. To make this possible, I have devised a heliotrope which is placed upon the tube of a theodolite telescope, and reflects the sunlight along the axis of the telescope. By means of it the axes of the two telescopes are brought into the same straight line when each observer sees the light of the sun reflected from the other's instrument.

The instrument (see adjacent figure) consists of two mirrors, *a* and *b*, which form angles of 45° with the axes x and η. The mirror *a* can rotate about the axis of η, *b* with the entire piece about the axis of x. By these two rotations it can be arranged that rays which fall from any point of the sphere on *a* are reflected at *b* at an angle of 45°, and in the direction towards x. But in order to see in the telescope whether the sun's rays are reflected towards x, an image of the sun is needed which shall be 180° from the point that is to receive light from the heliotrope. This is effected when the mirror *b* consists of two halves, α and β, which are in the same plane, but fit against each other with vertically ground edges. This line $\gamma\gamma'$ must make with the surface of the mirror exactly an angle of 90°: it must not be polished, but must only be rubbed smooth, otherwise it produces too intense an image of the sun in the telescope; for the rays which proceed from η to η' are reflected from the covered mirror *b* towards x. In the middle of the mirror, however, the coating is taken off, so that a small portion of the rays from η which goes through the plate glass towards η' meets the edge $\gamma\gamma$ of the mirror *b*, and being reflected towards x', the image of the sun is formed in the focus of the object-glass.—Poggendorff's *Annalen*, September 1865.



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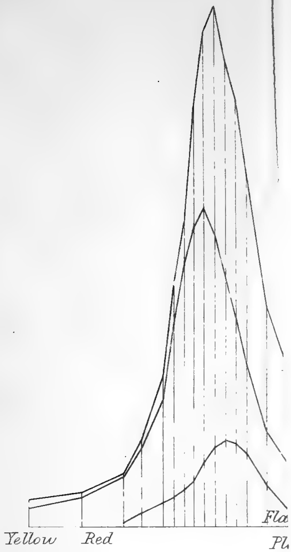

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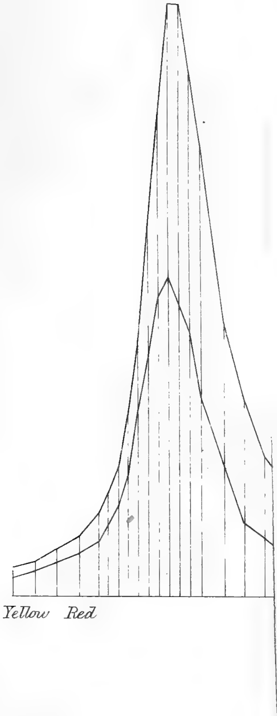

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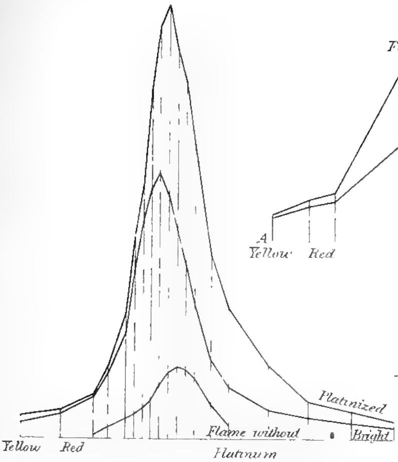

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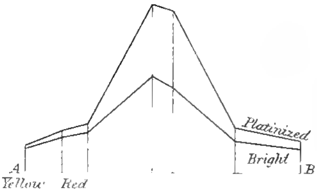

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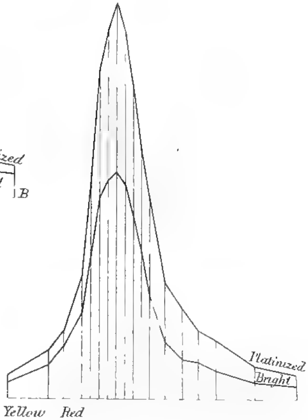

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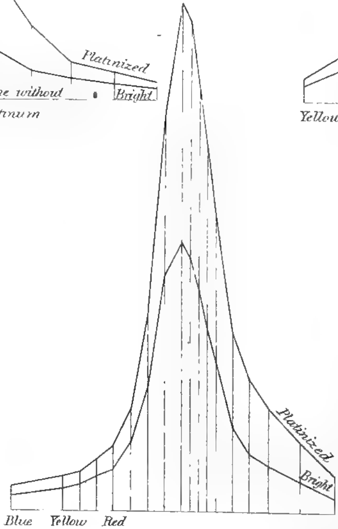

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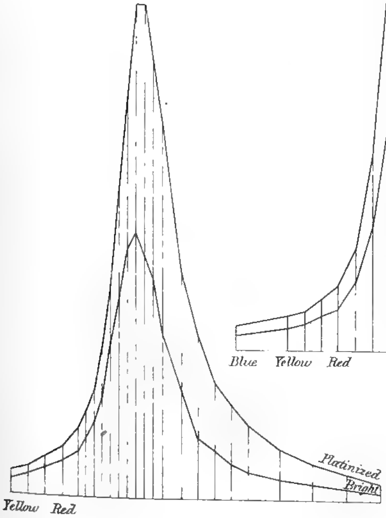

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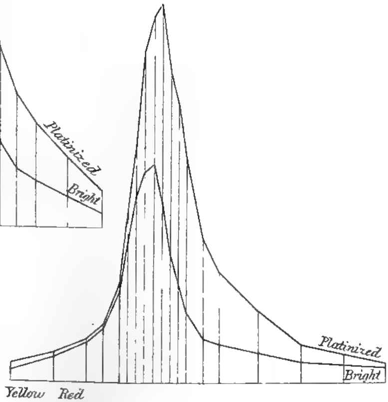

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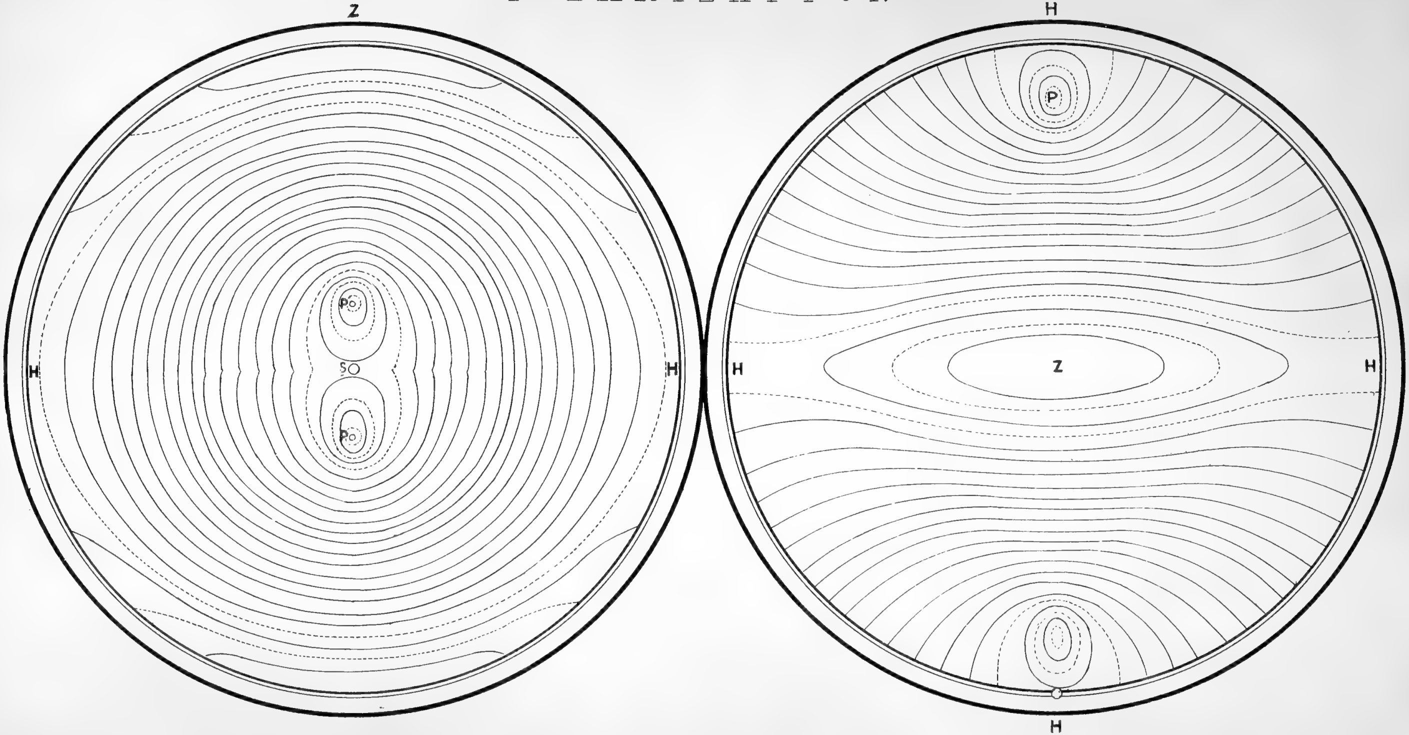

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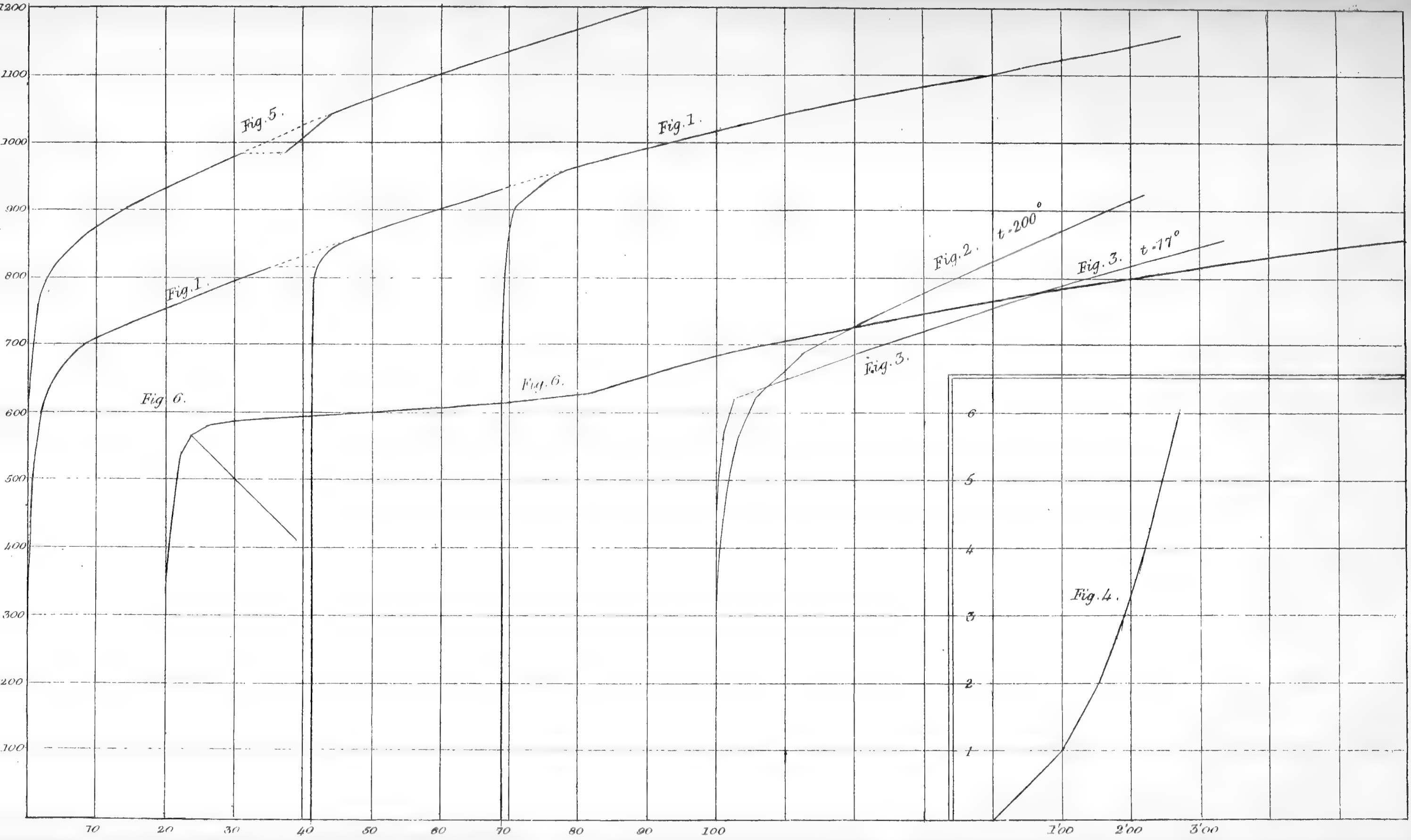
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Fig. 1.
Fig. 2.
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Fig. 6.

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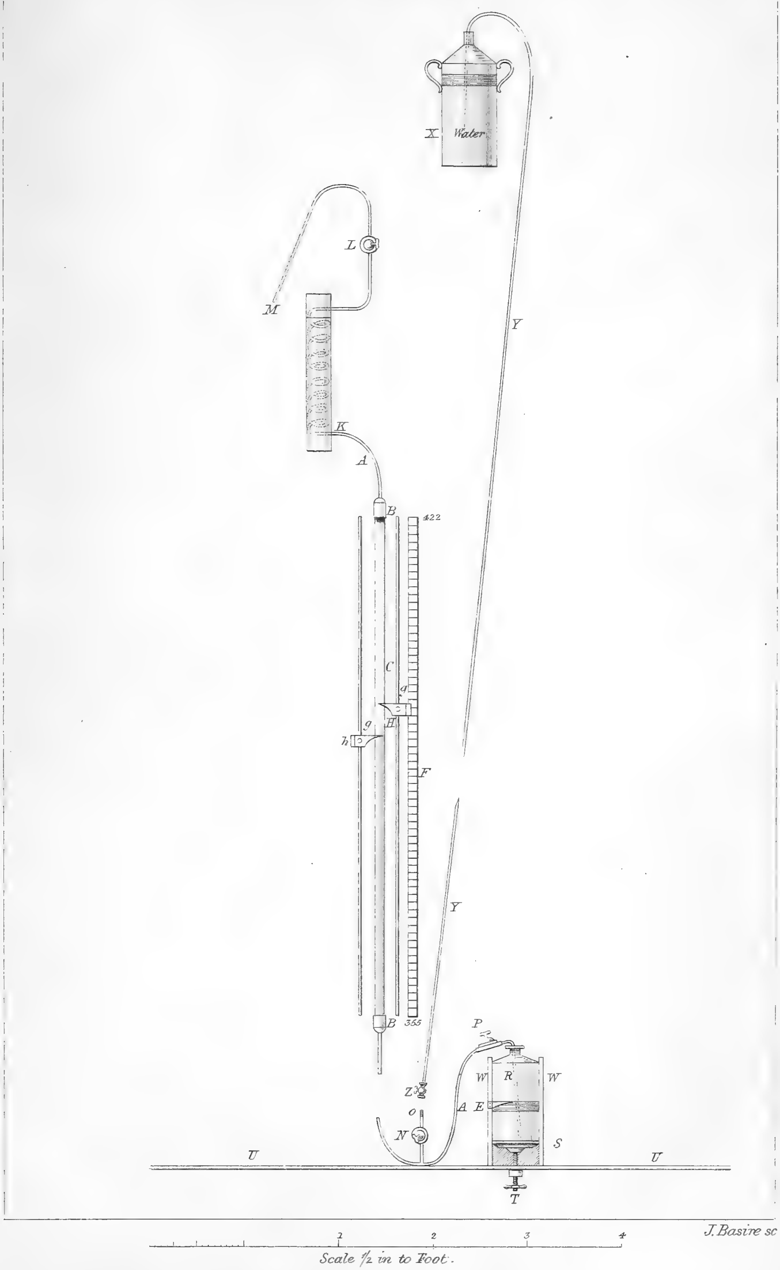

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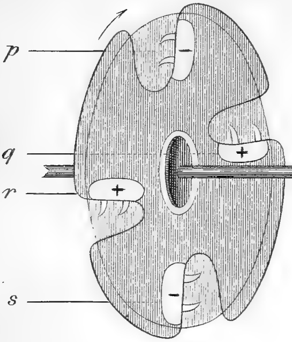

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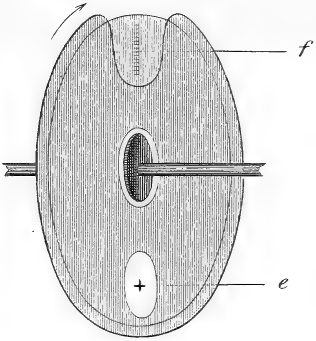

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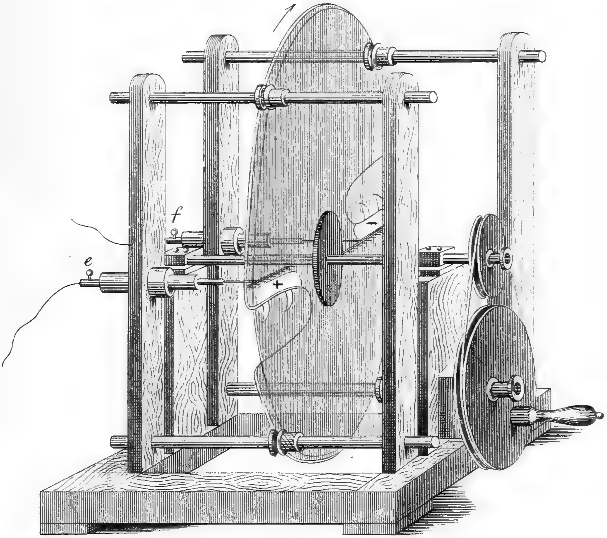

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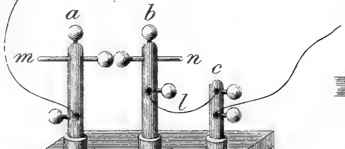

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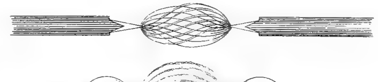

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