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

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

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NUMBER CCXXXII.—SUPPLEMENT TO VOL. XXXIV.

PLATE,

Illustrative of Mr. E. J. Lowe's Remarkable Solar Halos and Mock Suns.

Erratum in Mr. J. PHILLIPS's paper.

Page 250. In the note *read* for the shield of Achilles ῞ΗΦΑΙΣΤΟΣ throws into his crucibles brass unconquered, *κασσίτερος*, &c., *not* —brass, unconquered *κασσίτερος*, &c.

Errata in Mr. J. COCKLE's paper.

Page 43, line 12, *for* must *read* may.
— 43, — 12–13, *for* for otherwise the rectangle would obviously be greater than the square, *read* and then ascertain the consequences of such a supposition.

Supra, p. 42, note *, line 6, *for* "and then *n*" *read* when the index.

Erratum in Prof. PLÜCKER's paper.

Page 451, line 30, *for* obtain *read* command over, M. Plucker's meaning is that "you may give to it any declination you like, from about 25° to the east to 65° to the west."

THE
LONDON, EDINBURGH AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
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[THIRD SERIES.]

JANUARY 1849.

I. *On a new Empirical Formula for ascertaining the Tension of Vapour of Water at any Temperature.* By J. H. Alexander, *Esq.**

THE formula which the following memoir is intended to expose, is called *new*; because, to the best of my knowledge, it has never been used or suggested hitherto. It is also rightly termed *empirical*, in so far as it is not susceptible of geometrical demonstration, but thus far only; since in point of fact, it was derived entirely from considerations *à priori* and independent of any experiment or interpolation. Of course, it was compared as soon as possible with the temperature corresponding to the ordinary atmospheric pressure; and after a satisfactory agreement had been found at this point, the accord of the formula with observations at other points, above and below, was regarded as neither accidental nor surprising. The extent and nearness of this accord through a range of experiment more extensive than has hitherto been included in one and the same table, it is the principal aim of the present paper to exhibit, after having shown in few words the reasonableness of the formula and its limits (or rather want of limits) in application; a comparison, then, of the errors existing and admitted in several of the experimental series of the highest authority, with the differences developed at the same epochs by the formula, will indicate the probabilities in favour of the latter, and the nature and amount of its reliability.

It is obvious that the pressure of vapour or steam must be always in proportion to the absolute temperature at which it is produced. But as this temperature is only observable relatively and upon an arbitrary scale, it is necessary, in order to obtain anything like a measure of the quantity of heat existing, to use the ratio of the whole extent of the scale assumed between the two epochs where the liquid changes its state respectively, to that portion of it (*i. e.* the number of its de-

* From Silliman's Journal for Sept. 1848.

grees) which expresses the existing temperature. Or, what amounts to the same thing, the pressure of steam whose temperature is observed on any scale, is directly as the number of degrees read for the temperature; and inversely, as the whole number of degrees on the same scale between the melting of ice and the boiling of water. With Fahrenheit's scale, calling t the number of degrees at any temperature, the pressure of steam at that temperature must be proportionate to $\frac{t}{180}$. Again, the pressure of steam must be always directly as the absolute heat of conversion, or, as it is otherwise termed, the *latent* heat; expressed, of course, in degrees of the scale assumed. For, the greater the number of degrees for such latent heat, the greater also will be the repulsive force of the heat existing in the steam; which repulsive force may be assumed to be at least a function of the elasticity of the vapour. And as such repulsive force takes effect in part by expanding the medium vaporized, and the greater such expansion, the less will be the remaining elasticity, it follows that the pressure of steam in the ordinary state of an atmosphere must be also inversely as its increased volume. This increased volume may be taken, from the experiments of Guy-Lussac, at 1695 times that of water at its greatest density. And the latent heat of steam is generally admitted as 990° F.; which number results from the experiments of Clement and Desormes, is not far from the mean of several other observers, and will probably require a very small modification only to be identical with the deduction from a strict theory of volumes applied to vapours generally, in the mechanical relation of the observed effects of heat upon such vapours and the liquids producing them.

So far as we have gone, then, the pressure of steam must be as $\left(\frac{t}{180}+\frac{990}{1695}\right)$. It is here that the empiricism comes in, and dictates the numerical power to which this ratio must be involved, in order to harmonize the progression of the elasticities with the series of the temperature. That the simple ratio will not present such harmony is manifest; for that would be the measurement, by a lineal scale of equal parts, of central forces, which, acting upon *volumes*, might rather be supposed to be in the duplicate ratio of the cube. Such a ratio, equivalent to the sixth power, is in fact what has been taken; and the complete equation becomes then, calling p the pressure,

$$p=\left(\frac{t}{180}+\frac{990}{1695}\right)^6.$$

This index, however plausible upon reflection the reason to justify its adoption might have appeared, was no doubt suggested too, even unconsciously, from the recollection that it had before (though with different factors) served both Young and Tredgold, and perhaps others, in approximating the results of experiments. Moreover, it was apparent that the numerical results in English inches from the formula were altogether an accidental coincidence, which, dependent upon the properties of numbers, could not be expected to occur upon the use of any other scale than that of Fahrenheit; unless, indeed, one should adopt the fancy of Mr. Woolf, who supposed that he had discovered an immediate numerical relation between the atmospheric pressure and the pound avoirdupois, in which case the English inch might also claim to be among natural dimensions. It is, of course, quite possible, by a little artifice among the terms of a formula of this shape and retaining the same index, to produce a series of numbers corresponding to any linear scale. Thus, for instance, substituting Centigrade degrees for those of Fahrenheit, but carrying the denominator of the first term down to the degree at which the pressure becomes by the formula zero (which may be presumed to correspond to an absolute negation of heat, and which in fact has to be used with the present formula when it is intended to give the pressure in atmospheres), we obtain pressures expressed in French metres, and through a range of several atmospheres, but little discordant with the results of experiment. In using Centigrade degrees, however, and transforming the equation so as to express atmospheres, the index (6·) gradually diverges from regular multiples; serving to show what we might otherwise conjecture, that such index is not based upon any general relation in nature. It was, therefore, of less interest for me to weary myself with comparisons of other thermometric and linear scales; it is enough that the formula affords a remarkable coincidence in its own terms with the measures recognized among ourselves.

It is readily seen that the first term is positive as far as 0° of Fahrenheit; for temperatures lower than that it becomes negative; and there is a point, of course, where the negative value of the first term equals the constant positive value of the second, and the pressure, therefore, as was said just now, becomes itself zero. This point occurs at $-105^{\circ}{\cdot}13$; of which it is enough to say, that it is not very far from the lowest degree of heat yet produced, and that long before it is reached the mercurial thermometer becomes useless. Whether there is in the theory of nature (for it is admitted that there is not in fact) such a point as that of the absolute privation of heat, and if so, how it should be reckoned and where placed,—are questions which, although kindred to the matter in hand, are not necessary to the elucidation purposed. Whatever the answers might be, they would not affect the working of a rule which is intended for practicable temperatures; and if it should be objected to the present formula (as it was to the method of interpolation of Dr. Dalton) that it determines a limit which does not exist in nature, or places it where it should not stand, it may be very well replied, that the objection may hold good against the factors without prejudice to the form. It is quite likely that, below 32° F., it is theoretically no longer proper to use in behalf of chilled and freezing vapour, the number (990°) which belongs to boiling steam. But as even here and for nearly 60° below the melting-point of ice, the actual formula was not so very discordant from the results of experiment, I had the less motive for modifying or transforming it into a nearer agreement at this unusual temperature. The determination of a limit of this sort, whether real or assumed, is necessary in converting easily a formula like the present, into one which will show the pressure in atmospheres;—a method which, as it extricates the results from any dependence on a particular system of weights and measures and thus makes them generally applicable, is of course in any purely scientific investigation much to be preferred. It is obvious that such a scale of atmospheres or volumes must start from the degree where the expansions and the tendency to expand (which is *elasticity*), so far as they are due to temperature, are null. The limit that we have found, then, of 105°·13 below 0° Fahr., is such a term; and the distance between that and the other extremity of the scale, or 317°·13 (which is the measure on the thermometer of one atmosphere), becomes the new denominator to replace the 180° actually used for the pressure in inches of mercury.

In fact, I had expected, in advance, to find the present manometric formula (as it may be termed) becoming a barometric one, by putting it into this shape;

$$p=\left(\frac{t}{317\cdot13}+\left(\frac{990}{1695}\right)^2\right)^6.$$

But this did not hold good. Applying it numerically, it results in giving,

For 212°,	a pressure of 1·059 atmospheres,	equal to	31·68 inches.
And for 322°·38,	... 6·263	... ...	187·37	...

In both these instances, to agree with the original formula, the numbers representing the atmospheres should have been without fractions. The excess, however (as is visible), goes on in a converging series, and by and by disappears altogether, the difference then changing its sign. Even then it is not much; and at high temperatures the equation corresponds very nearly with the actual observations. For instance, comparing it with the experiments of Dulong and Arago,

Temp. 335°·3 F.	gave a pressure of	7·391 atmos.;	by formula	7·478 atm.
... 371°·3 F.	... ...	11·660 ...	...	11·958 ...

Nevertheless, as the object I had in view was not to find an equation that merely fits any particular series of observations, or is exact only for the higher ranges of temperature, I abandoned this theoretical expression and preferred to deduce a formula for pressure in atmospheres from the original one, in the ordinary analytic way. This results in the alternative expressions—

$$p' = \left(\frac{t}{317\cdot13} + \frac{561\cdot91}{1695}\right)^6 = \left(\frac{t}{317\cdot13} + \frac{990}{2986\cdot33}\right)^6;$$

either of which may be adopted, according as we prefer to retain in view the factor of the latent heat, or that of the expansion at the unitary atmospheric pressure. In practice, the constant fraction may be substituted by the number 0·33151. For the original formula, the similar constant was carried no further than *four* places of decimals; in this, where the unit of pressure is thirty times larger, both the attainment of equal precision requires, and the facility of calculation allows, another decimal place to be taken.

These decimal constants might, indeed, have been given in both formulæ at once, instead of the fractional expression from which they originate, were it not that I thought it desirable to preserve those factors which, besides solving the equation, indicate also, in part accidentally and in part essentially, certain elementary relations between pressure and temperature (or rather certain epochs in those relations), which are important in the future complete theory of the subject. For example, the denominator (1695) which expresses the number of volumes of steam under the atmospheric pressure and at a temperature of 212°, developed from an unitary volume of water at its maximum density, shows also the number of atmospheres, the equivalent of whose pressure will, below a certain temperature, prohibit the development of steam beyond the sphere of said unitary volume. On the other hand, the numerator (990°) gives this limiting temperature; and shows the degree on Fahrenheit's scale, where the force of steam becomes equivalent to 1695 atmospheres. Its density, therefore, would be equal to that of water; if its behaviour in other regards were like that of water too, this temperature would be the limit to its useful effect. But as there would most probably still remain the elasticity due to its expansion at that temperature, it does not appear that we are warranted in supposing any such limit.

At this point, 990°, the ratio of the latent and sensible heats has just inverted itself from what it was under the unitary atmospheric pressure. Then the former, which went altogether towards maintaining the state of vapour, without increasing its apparent temperature, was 5·5 times the latter; now the latter is 5·5 times the former. And from this consideration flows an easy manner of connecting proportionate volumes and pressure with simple multiples of the increments of heat. But to develope this here, would be to wander from the present aim.

If we go on to suppose the temperature increased until the whole of the latent heat is utilized and becomes sensible (which occurs at 1170° F.), we should then have a condition in which steam under a pressure of 4225 atmospheres (by the formula) is reduced to very nearly $\frac{4}{10}$ths of the unitary volume of water to produce it, and has a density (without regard to the expansion from temperature) 2·5 times that of water at its maximum. The expansion due to the temperature is (with Gay-Lussac's factor) 0·975 of the unitary volume; the expansion of water due to the same temperature (taking its maximum density as occurring at 39°·4 F., its actual expansion as 0·04333 at 212°· F., and its rational expansion as the cube-root of the fifth power of the temperature above the maximum) is 0·99944 of the same unitary volume—an accord which, considering the possible errors of the experiments, appears to me sufficiently satisfactory.

The equations already given serve to find the pressures in inches of mercury and in atmospheres respectively, when the temperatures are given: to find the temperatures corresponding to given pressure, they become as follows:

$$t = 180 \sqrt[6]{p} - 105°{\cdot}13;$$

where p represents the number of inches of mercury; and

$$t = 307{\cdot}13 \sqrt[6]{p'} - 105°{\cdot}13;$$

where p' stands for the number of atmospheres.

After these preliminaries, may now be presented the comparison of results by the formula and by the experiments of various observers; as is done in the following table. This table is quite extensive; and, for my own sake, rather more so than I desired. But it will be considered that it comprehends the assemblage of the observations of many persons through many years; and it was, besides, to me of an interest that I think will be partaken of by others, to have in one view and without interpolation nearly all the determinations that have been made by actual experiment. In limiting it to statements of this character, I confess I thought at first to restrict its extent, though it appears that there are in fact more experimental data than is generally supposed; and I desired besides to increase its utility beyond the mere comparison with the present formula, by fitting it as a depository for general reference. In this last regard, it may be considered as authentic and reliable.

And yet, after all, it does not include the whole of our experimental data. Those of John Henry Ziegler, of Winterthur in the Canton of Zurich (whose name I give at length because he may be regarded as having led the way in this research), of Achard, of Schmidt, of Magnus, and of some others whose names escape me at the moment or have failed to come to my knowledge at any time,—either were not accessible to me at all, or only in statements at second or third hand, whose accuracy I had not the means of verifying.

Nor does it contain quite all the results of those observers to whose experiments I had access. To have given every one of each, would have rendered the table, in respect to the present aim, quite enormous. For instance, the several series of M. Regnault, the latest experimentalist, exhibit such a luxury of repetition, at temperatures varying very slightly—sometimes only by small fractions of a degree—as almost to outnumber, in themselves alone, all the quotations I have made from others. I exercised, therefore, the discretion of using only those instances which, at reasonable degrees apart, rested upon a number of concurrent observations. In general, such concurrences of the same mean temperature give the mean of *three* observations of pressure; the result for 32° F. is the mean of *forty seven* recorded pressures. For the experiments earlier than Mr. Southern's, I have usually taken only those whose recorded temperatures were already otherwise in the table. I shall have occasion, however, to speak of each one more particularly, presently.

The difference of apparatus and manipulations necessary for experimenting upon the elasticity of vapour above and below the boiling temperature, has led several of the observers, for convenience or other motives, to confine themselves to one or the other side of this limit; and would render proper, in any discussion of the value and reliability of such observations, an arrangement of them in two distinct tables. But as I have no such aim at present, and as the exemplification of the formula belongs equally to the lowest and highest temperatures, there is no reason for breaking the continuity of the comparison or for presenting the results in more than one table.

The order of the different columns is chronological, according to the dates of execution; or, when that was not known, of publication of the experiments. As in fact each successive observer had, or might have had, the benefit of the experience and precautions of his predecessor, it may be presumed that this order represents too, in a measure, the respective reliabilities of the results. Only the two French series, while they are evidently unrivalled in the extent to which, in opposite directions, they have been carried, are similarly distinguished by the refined elaboration which characterizes every part of the research.

Table of the Pressure of Steam at different Temperatures, calculated and variously observed through a Range of 462 degrees of Fahrenheit's thermometer.

Temp. in deg. of Fahrenheit.	Pressure in inches by Formula.	Pressure in inches of Mercury observed by											
		Regnault, 1844.	Frankl. Inst. 1836.	French Acad. 1829.	Taylor, 1822.	Arzberger, 1819.	Ure, 1818.	Dalton. 1801.	Dalton. 1820.	Southern. 1797—1803.	Betancourt, 1790.	Robison, 1778.	Watt, 1774.
−27·112	0·0066	0·0106	Temperatures read to 0·25 deg.							Extremes of thermometer do not vary more than 0·5 deg.			Supposes, in 1814, that his barometer may have been 0·2 in. *too low.*
−13·	0·0180	0·0205											
− 4·504	0·0305	0·0284											
0·	0·0400												
+ 1·706	0·0437	0·0457											
9·41	0·0664	0·0638											
17·402	0·0911	0·0941											
23·702	0·1345	0·1256											
24·	0·1363						0·170						
27·626	0·1611	0·1500											
32·	0·1956	0·1811					0·200	0·200		0·16		0·0	
36·	0·232							*0·229	0·29				
40·	0 275						0·250					0·1	
42·	0·298									0·23			
43·25	0·314							0·297					
49·514	0·402	0·366											
50·	0·410						0·360				0·1856	0·2	
52·	0·443									0·41			
54·5	0·487							0·435					
55·	0·496						0·416						0·15
60·	0·596						0·516					0·35	
62·	0·641									0·52			
64·	0·689							*0·597	0·75				
64·976	0·713	0·616											
65·							0·630						
65·75	0·732							0·630					
70·	0·849						0·726					0·55	
72·	0·908									0·73			
75·	1·001						0·860						
77·	1·074							0·910			0·7326		
80·	1·184						1·010					0·82	
80·762	1·214	1·055											
82·	1·263									1·02			
85·	1·389						1·170						
88·25	1·538							1·290					
90·	1·623						1·360					1·18	
92·	1·726									1·42			
95·234	1·903	1·673											
96·	1·947							*1·63	1·95				
99·5	2·159							1·820					
100·	2·192						1·860					1·6	
102·	2·323									1·95			
105·	2·531						2·100						
110·	2·915						2·456					2·25	
110·75	2·977							2·540					
112·	3·081									2·65			
115·	3·346						2·820						
120·	3·829						3·300					3·0	
121·244	3·958	3·562											
122·	4·038							3·50		3·57	3·17		
125·	4·367						3·830						
130·	4·969						4·366					3·95	
132·	5·228							*4·60	5·07	4·68			
133·25	5·397							4·76					

Table of the Pressure of Steam, &c. (*continued*).

Temp. in deg. of Fahrenheit.	Pressure in inches by Formula.	Pressure in inches of Mercury observed by: Regnault, 1844.	Frankl. Inst. 1836.	French Acad. 1829.	Taylor, 1822.	Arzberger, 1819.	Ure, 1818.	Dalton, 1801.	Dalton, 1820.	Southern, 1797—1803.	Betancourt, 1790.	Robison, 1778.	Watt, 1774.
135·	5·638	...	...	...	...	...	5·070	...	...	...	...	...	4·53
140·	6·381	...	...	...	...	...	5·770	...	...	...	5·16	5·15	
142·	6·699	...	...	...	...	...	...	...	...	6·06	...	...	5·46
144·5	7·117	...	...	...	...	...	...	6·45					
145·	7·203	...	...	...	...	...	6·600						
150·	8·109	...	...	...	...	...	7·530	...	...	...	...	6·72	
151·124	8·327	7·698											
152·	8·497	...	...	...	...	...	...	...	...	7·85			
155·75	9·271	...	...	...	...	...	...	8·55					
160·	10·214	...	...	...	...	...	9·600	...	...	...	...	8·65	
162·	10·685	...	...	...	...	...	...	...	...	9·99			
164·	11·174	...	...	...	...	...	...	...	...	...	...	...	10·10
165·	11·427	...	...	...	...	...	10·800						
167·	11·944	...	...	...	...	...	...	11·25	...	...	10·27	...	11·07
170·	12·752	...	...	...	...	...	12·050	...	...	...	...	11·05	
172·	13·323	...	...	...	...	...	...	...	...	12·64	...	...	11·95
173·	13·612	...	...	...	...	...	...	*13·02	13·18				
175·	14·209	...	...	...	...	...	13·550	...	...	...	...	...	12·88
176·416	14·647	14·081											
178·25	15·230	...	...	...	...	...	...	14·60					
180·	15·801	...	...	...	...	...	15·160	...	...	...	...	14·05	14·73
182·	16·477	...	...	...	...	...	...	...	...	15·91			
185·	17·540	...	...	...	...	...	16·900	...	...	...	15·88	...	16·58
189·5	19·237	...	...	...	...	...	...	18·80					
190·	19·435	...	...	...	...	...	19·0	...	...	...	...	17·85	
195·	21·491	...	...	...	...	...	21·100						
198·05	22·839	22·538											
200·	23·731	...	...	...	...	...	23·600	...	...	...	...	22·62	
200·75	24·087	...	...	...	...	...	...	24·0					
202·	24·679												
205·	26·164	...	...	...	...	...	25·900						
210·	28·802	...	...	...	...	...	28·880	...	...	...	...	28·65	
211·27	29·500	29·621											
212·	29·915		...	*29·922	...	...	30·	30·	...	29·8	29·87		
213·	30·48	...	...	...	...	...	...	...	...	...	...	...	30·
216·6	32·61	...	...	...	...	...	33·40						
220·	34·73	...	...	...	34·95	...	35·54	*34·99	34·20	...	...	35·8	
221·6	35·77	...	...	...	...	...	36·70						
222·44	36·33	35·78											
225·	38·07	...	...	...	38·0	...	39·11	...	...	...	...	...	37·
226·3	38·97	...	...	...	...	...	40·10						
226·5	...	...	...	...	...	...	...	...	...	...	...	...	38·
230·	41·67	...	...	...	41·51	...	43·10	...	...	...	44·38	44·5	
230·5	42·04	...	...	...	...	...	43·50						
232·	43·17	...	...	...	43·0	44·40							
233·132	44·05	44·75											
234·	...	...	43·05										
234·5	45·13	...	...	...	...	...	46·80						
235·	45·54	...	...	...	45·50	...	47·22	...	...	...	...	...	44·
238·5	48·41	...	...	...	...	...	50·30	...	...	...	...	...	47·
240·	49·69	...	...	...	50·0	...	51·70	...	...	...	...	54·9	49·
242·	51·44	...	...	...	51·75	...	53·60						
245·	54·17	...	...	...	54·40	...	56·34						
245·25	...	...	52·12										
245·8	54·93	...	...	...	...	...	57·10						
248·25	...	...	59·08										
248·5	57·51	...	...	...	...	...	60·40	...	...	...	...	...	56·
249·	57·99	...	...	...	58·20	59·10							

Table of the Pressure of Steam, &c. (*continued*).

Temp. in deg. of Fahrenheit.	Pressure in inches by Formula.	Pressure in inches of Mercury observed by: Regnault. 1844.	Franklin Inst. 1836.	French Acad. 1829.	Taylor, 1822.	Arzberger, 1819.	Ure, 1818.	Dalton. 1801.	Dalton. 1820.	Southern, 1797—1803.	Betancourt, 1790.	Robison, 1778.	Watt, 1774.
250·	58·99	......	......	......	59·12	......	61·90	......	......	......	......	66·8	
250·25	......	......	58·14										
250·3	59·29	......	......	......	......	......	......	......	......	59·6			
250·5	......	......	......	......	......	......	......	......	......	......	......	......	58·
251·6	60·60	......	......	......	......	......	63·50						
252·662	61·69	63·33											
254·003	63·08	......	......	64·14	63·20								
254·5	63·62	......	......	......	......	......	66·70						
255·	64·15	......	......	......	64·40	......	67·25	......	......	......	......	......	62·
257·5	66·88	......	......	......	......	......	69·80						
260·	69·67	......	......	......	70·12	......	72·30	......	......	......	......	80·3	
260·4	70·15	......	......	......	......	......	72·80						
262·5	......	......	......	......	......	......	......	......	......	......	......	......	70·
262·75	72·88	......	73·27										
262·8	......	......	......	......	......	......	75·9						
263·3	73·55	76·15											
264·9	75·47	......	......	......	......	......	77·90						
265·	76·44	......	......	......	76	......	78·04						
267·	78·08	......	......	......	78·50	......	81·90						
269·	80·63	......	......	......	81·14	......	84·90						
269·5	......	......	81·51	......	......	......	......	......	......	......	......	......	78·
270·	81·94	......	......	......	82·50	......	86·30	......	......	......	......	94·1	
270·86	83·08	......	......	85·70									
271·	83·27	......	82·56	......	......	......	......	......	......	......	......	......	80·
271·2	83·54	......	......	......	......	......	88·						
271·346	83·73	......	......	85·89									
272·	84·60	......	......	......	85·45	......	......	*79·94	88·90				
272·5	85·28	......	......	......	......	......	......	......	......	......	......	......	82·
273·7	86·92	......	......	......	......	......	91·20						
274·	87·32	......	......	......	88·50	88·8							
275·	88·72	......	......	......	90·	......	93·48	......	......	......	100·07		
275·5	......	......	92·42										
275·7	89·71	......	......	......	......	......	94·60						
276·224	90·45	93·86											
277·9	92·86	......	......	......	......	......	97·80						
278·	......	......	93·81										
278·4	93·60	......	......	99·95									
279·5	95·22	......	......	......	......	......	101·60	......	......	......	104·91		
280·	95·97	......	......	......	97·75	......	101·90	......	......	......	......	105·9	
281·8	98·69	......	......	......	......	......	104·40						
283·8	101·79	......	......	......	......	......	107·70						
284·5	102·75	......	110·30										
285·2	103·98	......	......	......	......	......	112·20						
287·2	107·25	......	......	......	......	......	114·80						
289·	110·23	......	......	......	112·65	......	118·20						
289·5	111·05	......	119·02										
290·	111·90	......	......	......	114·50	......	120·15						
292·3	115·88	......	......	......	......	......	123·10						
292·5	116·22	......	120·60										
293·4	117·81	......	......	......	......	......	......	......	......	119·2			
294·	118·87	......	......	......	122·20	......	126·70						
294·5	119·77	......	129·11										
295·	120·68	......	......	......	124·15	......	129·						
295·6	121·76	......	......	......	......	......	130·40						
297·1	124·54	......	......	......	......	......	133·90						
297·464	125·21	130·79											
298·5	......	......	135·8										

Table of the Pressure of Steam; &c. (continued).

Temp. in deg. of Fahrenheit.	Pressure in inches by Formula.	Pressure in inches of Mercury observed by								
		Regnault, 1844.	Franklin Inst. 1836.	French Acad. 1829.	Taylor, 1822.	Arzberger, 1819.	Ure, 1818.	Dalton. 1801.	Dalton. 1820.	Southern, 1797—1803.
298·8	127·72	....	....	....	....	......	137·40			
299·5	129·06	....	142·62							
300·	130·02	....	136·36	....	133·75	......	139·70			
300·6	131·17	....	....	....	....	......	140·90			
301·316	132·57	....	....	136·85						
302·	133·91	....	144·33	....	137·55	......	144·30			
303·8	137·51	....	....	....	....	......	147·70			
304·5	138·93	....	151·92							
305·	139·92	....	....	....	144·05	......	150·56			
305·393	140·75	....	....	145·15						
305·5	....	....	154·28							
306·8	143·65	....	....	....	....	......	154·40			
308·	146·19	....	....	....	150·65	......	157·70			
308·606	147·50	....	....	152·80						
310·	150·49	....	....	....	155·	......	161·30			
310·25	151·03	....	163·51							
311·4	153·57	....	....	....	....	......	164·80			
312·	154·88	....	....	....	159·45	... {	165·50 or 167·			
314·75	161·12	....	181·23							
319·75	172·99	....	197·13							
320·	173·61	....	....	....	179·40					
322·	178·56	....	....	....	....	176·				
325·76	188·21	....	....	194·42						
334·50	212·28	....	240·48							
335·21	214·37	....	....	220·69						
337·073	219·84	....	....	227·32						
338·75	224·88	....	248·92							
340·	228·74	....	....	....	....	......	....	......	231·	
341·798	234·35	....	....	242·17						
343·6	240·07	....	....	....	....	......	....	......	......	238 4
345·	244·61	....	267·62							
346·	247·86	....	274·							
348·	254·51	....	278·33							
350·	261·32	....	290·35							
352·	268·35	....	297·36							
357·269	287·40	....	....	295·28						
362·66	308·13	....	....	316·35						
368·51	331·99	....	....	†342·51						
371·12	343·12	....	....	348·04						
372·	346·95	....	....	....	....	325·				
380·66	386·47	....	....	393·66						
389·345	429·78	....	....	433·83						
395·375	462·20	....	....	467·02						
398·813	481·58	....	....	483·88						
403·043	506·35	....	....	511·31						
403·88	511·43	....	....	514·22						
405·041	518·41	....	....	?516·84						
407·615	534·30	....	....	538·76						
408·407	539·28	....	....	540·85						
410·873	557·56	....	....	553·69						
419·333	611·88	....	....	610·23						
423·257	639·87	....	....	635·95						
425·03	652·93	....	....	644·96						
429·08	683·43	....	....	676·49						
432·	706·12	....	....	....	....	620·				
435·227	731·92	....	....	716·13						

Of the earlier observations in the preceding table I need not say much. Those of Mr. Watt, which he suffered to lie by him for forty years, and, in the caustic phrase of Tredgold, only produced when they had become unnecessary, he was himself dissatisfied with, but, as appears upon comparison, with more modesty than reason. I have specially calculated but two or three of his temperatures; and of the whole sixty-two experiments, have inserted but twenty-two; among which, however, both the limits are to be found. Of his friend Robison's, I have had to calculate none specially; but all happened to find a place in the table. Of M. Betancourt's numerous observations, which were reported originally in degrees of Reaumur and French inches, I have inserted only those which have been reduced to English scales by Sir D. Brewster for the Edinburgh Encyclopædia.

The experiments of Mr. Southern are, in fact, the supplement of those of Mr. Watt; having been made and reported at the desire of the latter. The numbers will be found to differ somewhat from those generally found in professed treatises on the steam-engine; they are in fact the mean of the actual *observations*; while those usually given have been selected now from one and now from the other set, and reduced (by himself) to what they might have been, had the pressure at 212° been thirty inches. For the present purpose it seemed to me proper to state the real, not the possible result.

Mr. Dalton's experiments were distinct, and are therefore given in distinct columns. The numbers in the earlier column, marked with an asterisk, were not from actual experiment, but by interpolation, according to the method he has himself explained. I have inserted them opposite to experimental numbers in the adjoining column, for the sake of comparison and the benefit of the inference which may flow from the variations. The numbers in the later column were not in every case given by his own experiments; but they were accepted by him as authentic, and the most reliable he knew. It is more complimentary to his reputation than to their own research, that compilers of chemical manuals, even down to the present time, retain among their tables his ancient results whose inaccuracy he himself has recognised. All of his experiments, of Southern's, and of Dr. Ure's, are in the table. To the originals of M. Arzberger I have not had access; but I have found these quoted in so many authorities and so uniformly accordant that I have not hesitated in recording them. Of the extensive table of Mr. Philip Taylor, whose remarkable accord with the results from the formula I may be allowed to call attention to, I have taken only those epochs of temperature which were already in my table.

The experiments of the French Academy have been already signalized. It is enough to establish their claim to distinction, to say that they were executed by Dulong and Arago; names that have been long since inscribed in the very highest rank of physical philosophers. The numbers found in the appropriate column, are, agreeably to what I have already mentioned as governing me throughout, quantities actually *observed*. The temperatures and pressures generally quoted in the text-books on steam, as of the French Academy, are not, in fact, what they observed, but what they deduced (in part, by a formula of their own, and in part by Tredgold's) from the present experimental series. The pressure 29·92 inches corresponding to the temperature 212°, is marked with an asterisk, because it is not expressly declared to have been *observed*. It is the height which is constantly taken in France for the barometric standard, as thirty inches are in England: in the latter assumption, the temperature is rated at 60° F.,—in the former, at 32° F.; and the difference of heights is nearly identical with the difference of expansion at the respective temperatures.

The pressure in this series corresponding to the temperature 368°·51, is also noted with a dagger; it may be presumed to be erroneous, not only because it differs so much from the result by my formula, but because it varies so much and so suddenly from the rate accused by the pressure on either side of it. Nor does it correspond at all with their own formula; calculated by that, the pressure will be 335·87 inches. The error is not, in this instance, of the press; since it makes its appearance in both ways of reckoning, by atmospheres and by metres.

I do not know how to account for another discrepant pressure, corresponding to the temperature 405°·04; which has been indicated by a note of interrogation. On both sides, above and below, the observed pressures are higher than the calculated one; in this instance it is suddenly lower. It agrees, to be sure, with an independent calculation by the formula of Dulong and Arago, at the temperature; but very manifestly breaks the uniformity or any regular progression of the series. What adds to the difficulty, is that the same observation is given again in another part of the Memoir of the Academicians; but the ciphers do not agree. I have neither altered nor omitted either of these instances; it is obvious that they are not to be used in comparison with the present formula.

The temperatures of the Franklin Institute, which were taken for the composition of the table, come from the second series of their reported experiments. Pressures have been also taken from both the other series, when their temperatures were already in the table; and, adopting this method as a uniform system, I did not allow myself to exclude the anomaly which shows itself between the different series at the temperature of 300°.

Of the experiments of M. Regnault, I have already spoken sufficiently.

It is apparent, upon a slight examination of the table, that the calculated pressures do not differ more from the average of the experimental ones, than these experimental ones do among themselves; which is about as much as could be desired to show the validity of the formula, and the reasonableness of its application, instead of others which are in general merely means of interpolating a particular experimental series. But in order to establish this more clearly, it will be necessary to ascertain more distinctly what the difference is between the results of the formula and those of observation. This difference is, of course, best expressed in the arithmetical scale of temperatures; as I have tabulated it here, upon the maximum deviation in each instance.

	Temperature.		Greatest Differences.	
	Observed.	Calculated.	+	−
Southern's experiments	343°·6	343°·09	....	0°·51
Dalton's of 1820	340·	340·74	0°·74	
Arzberger's experiments	322·	320·98	....	1 ·02
Taylor's experiments	320·	322·34	2 ·34	
French Academy's experiments	362·66	364·73	2 ·07	
Regnault's experiments	297·46	300·41	2 ·95	

The mean of the sum of these differences is +1°·09 Fahrenheit; which is the maximum error of the formula, compared with these six series.

It will be observed that I have left out of this comparison the last observation of the Academy; because it was the very utmost point which the apparatus could carry, and because it might therefore be expected to be affected by the untrustworthiness which forbade the series from being extended further. I have also neglected the last observation of Arzberger, which, compared with the Academy's, is in error more than 10°,—a deviation sufficient to discredit it entirely. Ure's experiments I have not compared at all; because, if we admit the series just now tabulated, his results are altogether too high. He may, however, be compared with himself, in the two results he has recorded for his last observation. These two different pressures accuse a corresponding difference of temperature of $0^{\circ}{\cdot}63$ F.; a possible error, not so materially less than what we have found as the maximum that can attach to the formula. The Franklin Institute experiments, which correspond closely with Ure's, I have omitted for a similar reason; they do not profess even to read nearer than $0^{\circ}{\cdot}25$ F. They may, however, for illustration be compared with those of the Academy, as under :—

	Pressure (inches)	Temp. observed	Temp. calculated
Academy	145·15	305°·39	307°·51
Institute	154·28	305 ·50	311 ·73
Differences	9·13	0°·11	4°·22

Discounting the *observed* difference from the calculated one, we have left $4^{\circ}{\cdot}11$ F. as the error of one or the other series; an amount nearly four times that of the formula.

It is manifest that the comparative error of the formula is only approximate; because it is based in each case upon only *one* observation instead of upon the combined mean of all the observations, or, rather, the mean of the differences at every epoch observed. Also, it can only be called an *error*, upon the assumption of the mean of all the experiments resulting in absolute accuracy; an assumption by no means to be made; for in general the utmost that can be done for any experimental series, is to determine the limits of its necessary or accidental errors. Such a research and determination I have thought the present formula of sufficient interest to warrant. The account, which is in fact the promised and proper conclusion of the present paper, will appear in a future number of this journal.

[On this subject, see the following papers in the Philosophical Magazine: first series, Philip Taylor, vol. lx. for 1822, p. 452, with engraved scale for each degree of temperature from 212° to 320° Fahr. Mr. Ivory, vol. i. second series, for 1827. Mr. Farey, S. 3. vol. xxx. for 1847.—Also the papers by Holtzmann, Magnus, and Regnault, in Taylor's Scientific Memoirs, vol. iv.]

II. *On the Products of the Soda Manufacture.* *By* John Brown, *Esq.**

GLAUBER first showed in 1658 that common salt could be decomposed by sulphuric acid. In the year 1736, Du Hamel proved the base of common salt to be soda. Previous to this, however, Cohausen, in 1717, had mentioned that salt might possibly be decomposed by means of lime; but as this observation was associated with numerous errors, it was entirely overlooked. In 1737 Du Hamel succeeded in obtaining

* Communicated to the Philosophical Society of Glasgow by Dr. R. D. Thomson, and read April 12, 1848.

the alkali from sulphate of soda by fusing with charcoal, and digesting the fused mass in acetic acid, evaporating the acetate of soda thus formed to dryness, and calcining the residue.

Margraff endeavoured to decompose sulphate of soda by limestone, but without success. In 1768 Hagen showed that salt might be decomposed by means of *potash*, chloride of potassium and caustic soda being formed.

Bergmann succeeded in decomposing salt by *caustic barytes*.

In 1775 it was shown by Scheele that salt was partially decomposed by *oxide of lead*.

In 1782 Guyton and Carny decomposed salt by fusion with *felspar*.

In 1781 Constantini succeeded in decomposing salt by means of *alum*.

The sulphates of *lime*, *magnesia*, *ammonia*, *potash*, *&c.* decompose salt, as also iron pyrites.

To convert the sulphate of soda into caustic or carbonated alkali was, however, the process of greatest importance. The first step, viz. the conversion of soda into sulphuret of sodium, was known to Glauber, Stahl, Du Hamel, Margraff and others. The difficulty was to get rid of the sulphur. Du Hamel effected this by means of acetic acid. But in the year 1784 the present process was discovered by Le Blanc and Dizé, and in the beginning of 1791 it was patented by Le Blanc. He used carbonate of lime to convert the sulphuret of sodium into carbonate of soda.

The proportions used by him were—

2 parts dry sulphate of soda.
2 ... carbonate of lime.
1 ... ground charcoal.

These were intimately mixed and introduced into a reverberatory furnace, where a strong heat was applied to it. After this had been continued for about an hour, the fused mass was raked out of the furnace and allowed to solidify. When this cooled, it was broken up and exposed to the action of moist air, which caused it to crumble down. In this way the caustic soda was converted into carbonate of soda, the carbonic acid being derived from the atmosphere. After being ground it was ready for use.

The soda process, as at present carried on, will be best considered under the four following heads:—

1st. The production of sulphate of soda from salt and sulphuric acid.

2nd. The conversion of sulphate of soda into crude carbonate of soda or British barilla.

3rd. The soda-ash process.

4th. The carbonate of soda process.

I. The first stage which thus comes under our consideration is—*The decomposition of common salt by sulphuric acid, causing the formation of sulphate of soda and muriatic acid.*

The salt used in this process is obtained from the brine-springs of Cheshire, which exist abundantly in the new red sandstone of that county. The solution is evaporated till it reaches a certain strength, when all the salt precipitates; it is then raked out into wicker baskets and allowed to drain. The mother-liquor is used for the manufacture of the salts of magnesia. The salt thus obtained contains, as might be expected, numerous impurities, the principal of which are lime, sulphuric acid and magnesia.

To estimate the lime, a portion of the salt was dissolved in water, and after separating the insoluble matter by filtration, the lime was precipitated by ammonia and oxalic acid, a large quantity of muriate of ammonia being added to retain the magnesia in solution.

	CaO, CO_2.	CaO.	CaO per 1000 grs.
2000 grains of salt gave	15·10	8·456	4·228
... ...	14·60	8·176	4·088
		Average	4·158

The sulphuric acid was precipitated by the addition of nitric acid and nitrate of barytes.

	BaO, SO_3.	SO_3.	SO_3 per 1000 grs.
2000 grains of salt gave	39·85	13·738	6·869
... ...	39·50	13·620	6·810
		Average	6·839

The quantity of magnesia was ascertained by precipitation by ammonia and phosphate of soda, the lime having been previously separated.

	$2MgO, P_2O_5$.	MgO.	MgO per 1000 grs.
2000 grains of salt gave	4·65	1·660	0·830

The carbonate of lime remained as insoluble matter when the salt was digested in water, and was separated by filtration.

	CaO, CO_2.	CaO, CO_2 per 1000 grs.
2000 grains of salt gave	3·000	1·50

By estimating the amount lost by drying the salt at 212° the quantity of water was ascertained.

		Water per 1000 grs.
330·2 grains of salt lost	17·96	54·373

In order to estimate the quantity of iodide of potassium and bromide of magnesium, $1\frac{1}{2}$ lb. of salt was put into a funnel, the lower end of which was closed with filtering-paper. The salt was then repeatedly washed with boiling water. The iodide and bromide were thus taken up by the water along with a large quantity of common salt. This solution was evaporated to dryness and the residue digested in alcohol, which dissolved the iodide and bromide along with a little of the salt, leaving, however, the greater part of it, which was afterwards separated by filtration. The filtered solution was again evaporated to dryness and the residue digested in water. Chloride of palladium was then added, but no precipitation of iodide of palladium took place. The palladium was precipitated by sulphuretted hydrogen, and the sulphuret of palladium thus formed separated by filtration. Upon testing the filtered solution with ammonia and nitrate of silver, no precipitate was obtained. Had bromine been present, it would have been precipitated in combination with the silver, bromide of silver being insoluble in caustic ammonia. It is therefore evident that the common salt manufactured, as previously mentioned, does not contain iodine or bromine, although it is highly probable that these bodies are present in small quantity in rock salt; and we might therefore be able to detect them in the brine from which the magnesia salts are manufactured.

Upon treating the salt with bichloride of platinum a slight precipitate of potassio-chloride of platinum was obtained.

Composition of Commercial Salt.

		Magnesia.	Lime.	Sulphuric acid.
Chloride of sodium	931·615			
Chloride of potassium	trace.			
Chloride of magnesium	1·066	0·381		
Sulphate of lime	10·098	....	4·158	5·940
Sulphate of magnesia	1·348	0·449	....	0·899
Carbonate of lime	1·500			
Water	54·373			
	1000·000	0·830	4·158	6·839

About 6 cwt. of this salt is introduced into an iron pot, and upon this is run, by a siphon, about $5\frac{1}{2}$ cwt. of sulphuric acid of about 1·750 specific gravity (150° Twaddell). A violent action immediately takes place, and large quantities of muriatic acid gas are evolved, which pass off by a chimney. If, however, the muriatic acid can be made use of, the gas is absorbed either by passing it through water contained in large cylindrical vessels or through a column of coke, which retains the gas until a considerable quantity of it is collected; a stream of water is then allowed to trickle through the coke, and in this manner all the gas is absorbed. At the expiration of about two hours the evolution of gas ceases; and the sulphate, which is in a semifluid state, is removed to another chamber, where it is strongly heated in order to drive off the whole of the acid. The whole operation takes about four hours.

The foreign matters contained in the sulphate of soda thus obtained are sand, peroxide of iron, magnesia and undecomposed salt.

To estimate the sand. This remained as insoluble matter when the sulphate was digested in water containing muriatic acid, and was separated by filtration.

1000 grains of sulphate of soda gave	2·82	grains of sand.
... ...	3·38	...
Average	3·10	...

From the solution filtered from the sand the peroxide of iron was precipitated by ammonia, muriate of ammonia having been previously added to retain the magnesia in solution.

1000 grs. of sulphate of soda gave	2·15	grs. peroxide of iron.
... ...	2·45	...
Average	2·30	

After separating the sand and peroxide of iron, as mentioned above, the lime was precipitated by oxalic acid and caustic ammonia.

	CaO, CO_2.	CaO, SO_3.
1000 grains of sulphate of soda gave	7·000	9·656
... ...	7·367	10·019
... ...	7·100	9·520
Average		9·731

The solution thus freed from lime, &c. was treated with ammonia and phosphate of soda. The magnesia was thus separated as ammonio-phosphate.

	$2MgO, P^2O_5$.	MgO, SO
1000 grains of sulphate of soda gave	2·70	2·893

The quantity of chloride of sodium was ascertained by precipitating the chlorine by nitrate of silver and nitric acid.

	Ag Cl.	Na Cl per 1000 grs.
200 grains of sulphate of soda gave	4·30	8·995
1000	29·70	12·373
500	13·80	11·500
Average		10·956

The sulphate of soda always contains a small quantity of free acid, the amount of which was ascertained by determining the weight lost by heating to redness.

		Per 1000 grs.	
200 grs. of sulphate of soda lost	1·70	8·50	grs. free acid.
... ...	1·84	9·20	...
Average		8·85	

Composition of Crude Sulphate of Soda.

Sulphate of soda	962·170
Sulphate of lime	9·731
Sulphate of magnesia . .	2·893
Chloride of sodium . .	10·956
Iron peroxide	2·300
Sand	3·100
Free acid	8·850
	1000·000

II. This brings us to the consideration of the second part of the process, namely,—*the conversion of sulphate of soda into crude carbonate of soda or British barilla.*

This is effected by the combined action of coal and carbonate of lime. The following table shows the proportions commonly used:—

	cwt.	qrs.	Per cent. lbs.	Theoretical quantity. lbs.
Sulphate of soda . . .	2	2	100	100
Ground limestone . . .	2	$2\frac{1}{2}$	102·9	105·3
Coal dross	1	3	61·7	33·6

These, after being intimately mixed, are introduced into a reverberatory furnace and strongly heated. The mass soon becomes soft, when care must be taken to stir it frequently in order to expose a fresh surface to the heat. When it becomes of the consistence of dough the chemical action commences, and jets of inflamed carbonic oxide begin to issue from it. The evolution of gas soon becomes very rapid, so much so, that the whole mass appears to be in a state of ebullition. When this ceases the operation is completed, and the fused mass is raked out of the furnace and allowed to solidify. The cake thus obtained is the crude carbonate of soda, or, as it is technically called, "soda ball" or "black ash."

This process consists of two subprocesses, which might be carried on in separate furnaces. 1. The coal is consumed at the expense of the oxygen of the sulphate of soda, causing the formation of sulphuret of sodium and carbonic oxide—

$$NaO, SO_3+4C=NaS+4CO.$$

2. The sulphuret of sodium thus formed is decomposed by

the carbonate of lime, with the formation of sulphuret of calcium and carbonate of soda—

$$NaS + CaO, CO_2 = NaO, CO_2 + CaS.$$

But if this compound were digested in water, a reverse action would immediately take place, sulphuret of sodium and carbonate of lime being again formed. To obviate this difficulty, a large excess of lime is used in the process, nearly twice as much as would otherwise be absolutely necessary. This excess of lime causes the formation of a compound insoluble in water, the composition of which is 3CaS, CaO. This substance has no effect on a solution of carbonate or caustic soda.

Analysis of Soda Ball, or Crude Carbonate of Soda.

An average sample was obtained by pounding a large quantity of the soda ball, and from this the specimens analysed were taken.

1. To estimate the amount of soluble and insoluble salts.—A portion of the substance was thrown on a weighed filter and washed with water at about 120° F., until a portion of the filtered liquor left no residue on evaporation; the filter and insoluble matter were then dried in a water-bath and weighed.

Soda ball.	Insoluble matter.	Soluble matter.
100 gave . .	59·87	40·13
... ...	58·92	41·08
... ...	59·90	40·10
Average	59·56	40·43

2. Sulphate of soda.—After saturating the soda ball with pure muriatic acid, and separating the insoluble matter by filtration, the sulphuric acid was precipitated by chloride of barium.

Soda ball.	BaO, SO_3.	BaO, SO_3 per cent.	NaO, SO_3 per cent.
245·20 gave	8·50	3·466	2·147
110·00 ...	1·30	1·181	0·733
78·36 ...	0·76	0·969	0·601
Average		1·872	1·160

3. Chloride of sodium.—The soda ball was digested with nitric acid and filtered, and from the filtered solution the chlorine was precipitated by nitrate of silver.

Soda ball.	Ag Cl.	Cl.	Na Cl.	Na Cl per cent
98 gave	5·400	1·350	2·250	2·295
100 ...	3·679	0·912	1·532	1·532
			Average	1·913

4. Soda.—The total quantity of available soda, that is, soda existing as carbonate, sulphuret and hydrate, was determined in the following manner:—A portion of the soda ball was thrown on a filter and washed with warm water until all the soluble matter was taken up; the filtered solution was then exactly neutralized by dilute sulphuric acid, which was afterwards precipitated by chloride of barium. From the quantity of sulphate of barytes thus obtained, the amount formerly got from the sulphate of soda was deducted, and from the remainder the per-centage of alkali was calculated.

Soda ball.		BaO, SO_3.	BaO, SO_3.	BaO, SO_3 p. c.	Soda p. c.
44·60	gave	40·60	91·031 − 1·872	= 89·159	24·593
100	...	88·96	88·960 − 1·872	= 87·088	24·024
48·50	...	42·76	88·164 − 1·872	= 86·292	23·800
				Average	24·138

5. Sulphur.—The amount of sulphur was determined in two different ways:—1st. The soda ball, after being very carefully pulverized, was intimately mixed with about four times its weight of nitrate of potash and heated in a covered platinum crucible. The nitrate of potash was thus decomposed, and the sulphur converted into sulphuric acid by the oxygen of the nitric acid.

$$KO, NO_2\, O_3 + S = SO_3 + KO + NO_2.$$

The fused mass was dissolved by muriatic acid, and after filtering the solution, the sulphuric acid was precipitated by chloride of barium. 2. The soda ball, moistened with a small quantity of water, was intimately mixed with a quantity of finely pulverized chlorate of potash, and to this muriatic acid was added, drop by drop, until upon a fresh addition of acid no more gas was evolved. The flask containing the substance was then gently heated by means of a water-bath, care being taken to keep the temperature below 180° F., as chlorous acid explodes with great violence at about 200° F. When all action had ceased, the solution was filtered, and the sulphuric acid precipitated by chloride of barium. From the weight of the sulphate of barytes thus obtained, the former quantity, 1·872, was deducted, and from the number thus found the amount of sulphur was calculated.

	Soda ball.		BaO, SO_3.		BaO, SO_3 per cent.	Sulphur per cent.
By 1st method.	19·34	gave	17·90	92·554 − 1·872	= 90·628	12·507
	19·53	...	18·20	93·189 − 1·872	= 91·317	12·595
By 2nd method.	28·90	...	27·00	93·425 − 1·872	= 91·553	12·627
	29·60	...	27·20	91·891 − 1·872	= 90·019	12·416
					Average	12·536

6. Magnesia.—This was precipitated by ammonia and phosphate of soda.

Ball soda.	$2MgO, P^2O_5$.	MgO per cent.
100 gave .	0·980	0·350

7. Silica and sand.—The soda ball was dissolved in muriatic acid and the solution evaporated to dryness. The residue was then digested with strong muriatic acid, and the insoluble matter separated by filtration.

Ball soda.	Silica and sand.	Silica and sand per cent.
56·00 gave .	4·30	7·679

The silica was separated from the sand by strong caustic potash.

Ball soda.	Sand.	Sand per cent.	Silica per cent.
56·00 gave	2·40	4·285	3·394

8. Iron and alumina.—A portion of the soda ball was dissolved in muriatic acid, and after separating the insoluble matter, the iron and alumina were precipitated by caustic ammonia.

Ball soda.	Al_2O_3 & Fe_2O_3.	Al_2O_3 & Fe_2O_3 per cent.
61·20 gave	3·45	5·637
19·53 ...	1·15	5·888
29·10 ...	1·45	4·982
	Average	5·502

The peroxide of iron was separated from the alumina by caustic potash.

Ball soda.	Fe_2O_3.	Fe_2O_3 per cent.	Fe per cent.	Al_2O_3 per cent.
61·20 gave	2·94	4·804	3·363	0·833
29·10 ...	1·20	4·123	2·886	0·859
		Average	3·129	0·846

9. Lime.—From the solution filtered from the alumina and iron, the lime was precipitated by oxalate of ammonia.

Ball soda.	CaO, CO_2.	CaO.	CaO per cent.
61·20 gave	33·00	18·480	30·194
29·10 ...	15·50	8·680	29·828
21·80 ...	12·05	6·748	30·954
		Average	30·325

10. Carbonic acid.—By the addition of muriatic acid to the ball soda, sulphuretted hydrogen and carbonic acid gases were evolved, which were passed through a strong solution of caustic barytes. The precipitated carbonate of barytes was filtered as rapidly as possible, care being taken to keep it covered with a plate of glass during the process.

Ball soda.		BaO, CO_2.	CO_2.	CO_2 per cent.
45·35	gave	28·90	6·487	14·304
90·18	...	59·20	13·289	14·736
			Average	14·620

11. Carbon.—To determine the amount of carbon, a portion of the ball was treated with muriatic acid and the solution evaporated to dryness; dilute acid was then added, and the insoluble matter thrown on a filter which had been previously dried at 212° and weighed. The total amount of carbon, silica and sand, was thus ascertained. The whole was then ignited and weighed, and from the loss the per-centage of carbon was calculated.

Ball soda.	Insoluble matter.		Carbon per cent.
100 gave	15·941,	which lost on ignition	7·998

12. Water.—The soda ball was dried at 212°, and the amount lost estimated.

Ball soda.		Water.	Water per cent.
50·00	lost . .	0·35	0·700

Whilst washing out the soluble salts, it was observed that the filtered solution was of a greenish colour; and upon boiling it a green-coloured substance was deposited, after which the supernatant liquor became perfectly colourless. Upon examining this precipitate, it was found to consist principally of silica and alumina with a little lime. From this it was concluded to be artificial ultramarine, which is frequently found in the crevices of the ball furnaces, and which, when dissolved in caustic soda, yields a green-coloured solution, precisely the same as that mentioned above.

Ball soda.		Ultramarine.	Ultramarine per cent.
200	gave . .	0·46	0·23
100	...	0·36	0·36
		Average	0·295

Sulphate of soda . . .	1·160
Chloride of sodium . .	1·913
Soda	24·138
Lime	30·325
Sulphur	12·536
Carbonic acid	14·520
Sand	4·285
Silica	3·394
Magnesia	0·350
Alumina	0·846
Iron	3·129
Water	0·700
Carbon	7·998
Ultramarine	0·295

		Soda.	Lime.	Carbonic acid.	Sulphur.
Carbonate of soda	35·640	21·120	....	14·520	
Caustic soda	0·609	0·609			
Aluminate of soda............	2·350	1·504			
Sulphate of soda	1·160				
Sulphuret of sodium	1·130	0·905	....	....	0·454
Chloride of sodium	1·913				
Ultramarine	0·295				
3CaS+CaO	29·172	....	24·024	....	10·296
Caustic lime	6·301	....	6·301		
Sand	4·285				
Sulphuret of iron	4·917	....	....	....	1·786
Silicate of magnesia	3·744				
Carbon	7·998				
Water (hygroscopic).........	0·700				
	100·214	24·138	30·325	14·520	12·536

It will be seen that in the above analysis I consider almost all the soda to be united with carbonic acid, there being very little caustic soda. Unger and others who have examined the soda balls, fall into the error of supposing a large quantity of the alkali to exist as hydrate, and also of always finding carbonate of lime; but if a portion of the ball soda be digested in alcohol, and the alcoholic liquor carefully examined, it will be found that it holds in solution a very small quantity of alkali, which I consider to be as sulphuret. If, on the contrary, the soda balls contained caustic soda, it would be immediately dissolved by the alcohol, and we should obtain a *strongly* alkaline solution. This, however, is not the case. But if the ball soda be digested in water, the liquid will be found to contain a large quantity of caustic soda, which, however, can easily be accounted for in the following way. There exists in the ball soda a large quantity of caustic lime; and whenever water is added to it a decomposition takes place, carbonate of soda and caustic lime becoming carbonate of lime and caustic soda,—

$$NaO, CO_2 + CaO = CaO, CO_2 + NaO.$$

Some analysts have also found water of combination in ball soda, that is, water united to soda or lime. But this is impossible, for where does the water come from? The materials contain none. A small quantity of water is certainly formed in the combustion of coal, but this is not sufficient to account for it. The method of analysis pursued in the determination of the amount of water combined with soda or lime was, I think, very incorrect: it was to burn the ball soda with chromate of lead, and determine the weight of the water given off.

Had any undecomposed coal existed in the waste, it would have contained hydrogen, and water would consequently have been formed, the oxygen being derived from the chromic acid of the chromate of lead.

As might be expected, I found upon trying samples taken from different furnaces, that the constituents were subject to great variations. Thus the lime varied from 27 per cent. to 34 per cent.; the soda from 22 per cent. to 26·5 per cent.; the sulphur from 10 per cent. to 16 per cent. But they always stood in a certain fixed relation to one another; for when the quantity of lime was large the amount of sulphur was proportionally increased, and the per-centage of soda consequently diminished. The following table will suffice to show this:—

	I.	II.	III.
Soda . . .	26·480	22·000	24·138
Lime . . .	26·959	33·807	30·324
Sulphur . .	10·527	13·820	12·436

I insert here two analyses of soda balls; the one from Cassel by Unger, the other from Newcastle by Richardson. They both get hydrate of soda and carbonate of lime, and are, I think, wrong in both of these, although the other parts of the analysis are probably quite correct.

The manufacture in Cassel and Newcastle is carried on almost exactly in the same way as here.

	From Cassel.	From Newcastle.
Sulphate of soda	1·99	3·64
Chloride of sodium . .	2·54	0·60
Carbonate of soda . .	23·57	9·89
Hydrate of soda	11·12	25·64
Carbonate of lime . .	12·90	15·67
3CaS, CaO	34·76	35·57
Sulphuret of iron . .	2·45	1·22
Silicate of magnesia . .	4·74	0·88
Charcoal	1·59	4·28
Sand	2·02	0·44
Water (hygroscopic) .	2·10	2·17
	99·78	100·00

III. This brings us to the consideration of the third division of the soda process, viz. *the manufacture of soda-ash from ball soda.*

The first point is to extract all the soluble matter from the balls. This is done by digestion in warm water. The vessels used for this purpose are large square iron pans, five or six of which are usually worked together. They are so contrived that the water which runs into the first pan passes through the whole six in succession. In this way a very saturated solution is obtained. From the last digester the liquor is run into a large iron vessel, where it is allowed to settle: the insoluble matter which remains in the pans is of no use and is therefore thrown away. It is a source of great annoyance to the manufacturer, as also to the whole neighbourhood of the place where it is deposited, large quantities of sulphuretted hydrogen being evolved from it. Numerous attempts have been made to recover the sulphur from it, but without success.

Analysis of Soda Waste.

The following analysis of fresh soda waste was made in the same way as that of the ball soda.

1. Sulphuric acid.—The waste was digested in pure muriatic acid, and after separating the insoluble matter by filtration, the sulphuric acid was precipitated by chloride of barium.

Waste.	BaO, SO_3.	BaO, SO_3 per cent.	CaO, SO_3 per cent.
28·00	2·10	7·500	4·396
30·95	2·20	7·108	4·166
		Average	4·281

2. Sulphur.—The sulphur was oxidized by chlorate of potash and muriatic acid, and the sulphuric acid thus formed precipitated by chloride of barium.

Waste.		BaO, SO .		BaO, SO_3 p. c.	Sulphur p. c.
27·75	gave	27·56	99·315 − 7·304 =	92·011	12·689
30·90	...	32·40	104·854 − 7·304 =	97·550	13·455
26·95	...	27·80	103·154 − 7·304 =	95·850	13·220
					13·182

3. Silica and sand.—By dissolving the waste in strong muriatic acid, evaporating to dryness, and dissolving the residue, the silica, sand and carbon remained as insoluble matter, the last of which was destroyed by ignition. The silica and sand were then separated by caustic potash.

Waste.		SiO & sand.		SiO.		Sand.	SiO p. c.	Sand p.c.
50·00	gave	5·513	containing	2·640	and	2·873	5·280	5·746

4. Peroxide of iron.—After separating the silica and sand, the iron was precipitated by caustic ammonia. It contained a very small quantity of alumina.

Waste.	Fe_2O_3.	Fe_2O_3 per cent.
20·00 gave .	1·10	5·500
50·00 ...	2·46	4·920
21·40 ...	1·44	6·729
	Average	5·716

5. Lime.—After the iron had been precipitated by ammonia, the lime was thrown down by oxalic acid.

Waste.	CaO, CO_2.	CaO.	CaO per cent.
21·40 gave	17·10	9·576	44·747
48·90 ...	39·10	21·896	44·777
		Average	44·762

6. Magnesia.—After separating the lime the magnesia was precipitated by phosphate of soda and ammonia.

Waste.	2MgO, P^2O_5.	MgO.	MgO per cent.
48·90 gave	0 970	0·346	0·707

7. Carbonic acid.—A quantity of the waste was put into a flask and dilute acid slowly added to it. The carbonic acid thus disengaged was passed through a solution of caustic barytes, and from the quantity of carbonate of barytes thus precipitated the amount of carbonic acid was calculated.

Waste.	BaO, CO_2.	CO_2.	CO_2 per cent.
30·80 gave	15·65	3·513	11·406
27·20 ...	13·30	2·985	10·974
		Average	11·190

8. Soluble and insoluble salts.—The whole of the soluble matter was extracted by water, and the residue dried at 212° and weighed.

Waste.	Insol. matter.	Insol. matter p. c.	Sol. matter p. c.
71·2 gave .	52·50	73·736	26·264

9. Carbon.—The amount of carbon was determined in the same way as in the ball soda.

Waste.	SiO sand and carbon.	Carbon.	Carbon per cent
50 gave .	11·552, lost on ignition	6·039	12·078
	Insoluble salts per cent.		61·658

10. Carbonic acid in insoluble salts.

Waste.	BaO, CO_2.	CO_2.	CO_2 in insol. salts.
20·30 gave	15·70	3·525	10·657

11. Lime in insoluble salts.

Waste.	CaO, CO_2.	CaO.	CaO in insol. salts.
23·80 gave	20·90	11·704	30·448

12. Bisulphuret of calcium.—A quantity of the waste was digested with muriatic acid and a large quantity of water, and heated till the whole of the sulphuretted hydrogen was dissipated. The sulphur which remained was then oxidized by chlorate of potash and muriatic acid, and the sulphuric acid thus formed precipitated by chloride of barium. But as this method does not yield very accurate results, the amount of bisulphuret of calcium given below can only be considered as an approximation.

Waste.	BaO, SO_3.	Sulphur.	Sulphur per cent.	CaS_2 per cent.
35·8 gave	11·45	1·579	2·205	3·583

13. Hyposulphite of lime.—About 100 grains of the waste were digested for twenty-four hours with a solution of oxalate of potash; a salt of the oxide of copper was then added, by which all the sulphur was thrown down. The precipitated sulphuret of copper was then separated by filtration, and to the filtered solution sulphuric acid was added. At first no precipitation took place; but after standing for one or two hours, the solution became slightly turbid. The quantity of sulphur was, however, too small for estimation.

14. Water.

Waste.	Water per cent.
100 grains lost by drying at 212°	2·10

Soluble salts . .	26·264
Insoluble salts .	73·736
	100·000

Sulphate of lime	4·281
Sulphur	13·182
Silica	5·280
Sand	5·746
Peroxide of iron	5·716
Lime	44·762
Magnesia	0·707
Carbonic acid	11·190
Carbon	12·078
Carbonic acid in insoluble salts .	10·657
Lime in insoluble salts . . .	30·448
Bisulphuret of calcium . . .	3·583
Hyposulphite of lime	trace
Water	2·10

			Lime.	Sulphur.	Carbonic acid.
Insoluble salts.	Carbonate of lime	24·220	13·563	......	10·657
	3CaS CaO	20·363	16·769	7·187	
	Carbon	12·709			
	Silicate of magnesia	5·987			
	Sand	5·746			
	Peroxide of iron	5·716			
Soluble salts.	Sulphate of lime	4·281	1·645		
	Hyposulphite of lime	trace			
	Bisulphuret of calcium	3·583	1·929	2·205	
	Sulphuret of calcium	8·527	6·631	3·790	
	Hydrate of lime	5·583	4·225		
	Carbonate of soda	1·309	......	......	0·533
	Water (hygroscopic)	2·100			
		100·124	44·762	13·182	11·190

As might be expected, the quantities of lime, sulphur and carbonic acid, are subject to great variations, every sample varying to a considerable extent.

Upon examining a sample of waste three or four weeks old, I found the quantity of hyposulphite of lime to be much greater than in perfectly fresh waste. Another specimen, which had been partially exposed to the action of the atmosphere for three years, was entirely converted into sulphate of lime, sulphite of lime and carbonate of lime, and hyposulphate of lime. Some specimens were obtained which consisted entirely of sulphate of lime, carbonate of lime and caustic lime. These experiments are very interesting from their showing the gradual oxidation of the sulphur which the waste contains.

The waste in the soda ball consists entirely of oxysulphuret of lime (3CaS, CaO) and caustic lime. The 3CaS, CaO soon, however, decomposes, giving rise to sulphuret and bisulphuret of calcium and caustic lime. The bisulphuret of calcium being very efflorescent, forms on the waste heap a yellow coating of small prismatic crystals. The sulphur is then further oxidized, the first products being hyposulphite and sulphite of lime: the process still continuing, hyposulphate and sulphate of lime are formed; and this oxidation goes on till sulphate of lime remains. The caustic lime is also for the most part converted into carbonate.

It would be very interesting to ascertain the exact amount of each of these substances present in waste in different stages of decomposition; but there are as yet no methods known by which sulphurous, hyposulphurous, and hyposulphuric acid can be accurately determined, especially when existing along with sulphuric acid and sulphurets, as in soda waste. Under these circumstances, it would be impossible to make a series of analyses of the waste in its different stages of decomposition, upon which perfect dependence could be placed; but it is to be hoped that as the science advances these at present insuperable obstacles may be entirely removed.

The following is an analysis by Unger of a sample of waste from Cassel.

Carbonate of lime . .	19·56
$3CaS+CaO$	32·80
Carbon	2·60
Silicate of magnesia . .	6·91
Sand	3·09
Iron peroxide	3·70
Sulphate of lime . . .	3·69
Hyposulphite of lime .	4·12
Hydrate of lime . . .	11·79
Bisulphuret of calcium .	4·67
Sulphuret of calcium .	3·25
Sulphuret of sodium .	1·78
Water	3·45
	100·31

The soda waste thus affords ample room for further researches, which if carefully prosecuted might yield very interesting results. But without dwelling any longer on this subject, I pass on to the consideration of the remaining part of this division of the process, viz. the manufacture of soda-ash from the liquor containing the soluble matter extracted from the ball soda.

This liquor contains carbonate of soda, caustic soda, sulphuret of sodium, sulphate of soda, and chloride of sodium, with a little aluminate of soda, the greater part of which is, however, soon decomposed by the action of the carbonic acid of the atmosphere, carbonate of soda being formed whilst the alumina precipitates. This solution is boiled down in an iron pan until it is nearly dry.

Analysis of Soda-ash.

The analysis of this and the remaining salts were made in the following way:—

1. Carbonate of soda.—The amount of carbonate of soda was determined by ascertaining the weight of the carbonic acid which was evolved on the addition of muriatic or sulphuric acid to the salt.

2. Sulphuret of sodium.—The amount of sulphuret of sodium was ascertained by passing the gases, evolved on the addition of muriatic acid to the salt, through a solution of arsenious acid in caustic potash. The sulphuret of arsenic thus formed was precipitated by neutralizing the potash with nitric

acid; it was then thrown on a filter, dried at 212° and weighed. From its weight the quantity of sulphuret of sodium was calculated.

3. Hydrate of soda.—To ascertain the quantity of hydrate of soda, a portion of the substance was heated strongly with carbonate of ammonia in order to convert the hydrate and sulphuret into carbonate. The amount of carbonic acid was then determined as formerly, and the difference between the results of the two experiments gave the amount of carbonic acid equivalent to the quantity of soda existing as hydrate and sulphuret in the sample. The amount united to sulphur was then deducted, and the remainder gave the per-centage of hydrate.

4. Sulphate of soda.—A portion of the salt was dissolved in a pretty large quantity of water, and nitric acid added to expel the carbonic acid. The sulphuric acid was then precipitated by chloride of barium.

5. Sulphite of soda.—The salt was boiled with strong nitric acid in order to oxidize the whole of the sulphite of soda and sulphuret of sodium. Water was then added, and the sulphuric acid precipitated by a salt of barytes. From the quantity of sulphate of barytes thus obtained, the amount got by the former experiment was deducted, and the remainder showed the quantity of sulphate of barytes equivalent to the amount of sulphite of soda and sulphuret of sodium. The per-centage of sulphuret of sodium being known, the sulphite of soda was easily determined.

6. Chloride of sodium.—After expelling the carbonic acid by nitric acid, the chlorine was precipitated by nitrate of silver.

7. Aluminate of soda and insoluble matter.—A solution of the salt was acidified by muriatic acid, and the insoluble matter (principally sand) separated by filtration. From the filtered solution the alumina was precipitated by caustic ammonia.

The salt obtained by evaporation from the liquor from the keaves, after drying at 212°, yielded on analysis,—

	I.	II.
Carbonate of soda	68·907	65·513
Hydrate of soda	14·433	16·072
Sulphate of soda	7·018	7·812
Sulphite of soda	2·231	2·134
Hyposulphite of soda	trace	trace
Sulphuret of sodium	1·314	1·542
Chloride of sodium	3·972	3·862
Aluminate of soda	1·016	1·232
Silicate of soda	1·030	0·800
Insoluble matter	0·814	0·974
	100·735	99·941

This salt is then introduced into a reverberatory or *carbonating* furnace, where it is strongly heated. In this process the sulphuret of sodium is converted into sulphate of soda, and part of the hydrate of soda into carbonate. The salt when removed from the furnace is ready for the market. In Newcastle and some other places it is dissolved and carbonated again, and when thus manufactured it contains less caustic soda.

Soda-ash thus prepared contains from 48 to 53 per cent. of available alkali, that is, alkali combined with carbonic acid and water, and yielded on analysis,—

	I.	II.	Analysis of ash from Germany by Unger.
Carbonate of soda	71·614	70·461	62·13
Hydrate of soda	11·231	13·132	17·20
Sulphate of soda	10·202	9·149	8·66
Chloride of sodium	3·051	4·279	3·41
Sulphite of soda	1·117	1·136	0·35
Aluminate of soda	0·923	0·734	1·11
Silicate of soda	1·042	0·986	2·56
Sand	0·316	0·464	0·62
Water	...	...	3·96
	99·496	100·341	100·00

IV. *Formation and Analysis of Carbonate of Soda.*

The next stage of the process which comes under our consideration is the carbonate of soda process.

The carbonate of soda balls are lixiviated with water in the same way as in the manufacture of soda-ash. The liquor from the settler is pumped up into a pan, where it is evaporated till it becomes nearly dry; it is then taken out of the pan in colanders, thrown up in a heap and allowed to drain. The sulphuret of sodium and caustic soda soon deliquesce and drain out from the salt.

This salt, after drying at 212°, gave when analysed,—

	I.	II.
Carbonate of soda .	79·641	80·918
Hydrate of soda . .	2·712	3·924
Sulphate of soda . .	8·641	7·431
Sulphite of soda . .	1·238	1·110
Sulphuret of sodium	trace	0·230
Hyposulphite of soda	trace	trace
Chloride of sodium .	4·128	3·142
Aluminate of soda .	1·176	1·014
Silicate of soda . .	1·234	1·317
Insoluble matter . .	0·972	0·768
	99·742	99·854

This salt is then introduced into a reverberatory furnace and carbonated. The last traces of sulphur are thus oxidized, and almost the whole of the hydrate is converted into carbonate.

This salt yielded on analysis,—

	I.	II.
Carbonate of soda .	84·002	83·761
Hydrate of soda . .	1·060	0·734
Sulphate of soda . .	8·560	9·495
Sulphite of soda . .	trace	0·386
Chloride of sodium .	3·222	3·287
Aluminate of soda .	1·013	0·620
Silicate of soda . .	0·984	0·780
Insoluble matter . .	0·716	0·846
	99·557	99·909

A finer kind of soda-ash is frequently made from this salt by dissolving it in water, evaporating to dryness, and carbonating. It contains very little caustic soda, and should average about 50 per cent. of alkali.

It yielded on analysis,—

	I.	II.
Carbonate of soda .	84·314	84·721
Hydrate of soda . .	trace	0·280
Sulphate of soda . .	10·260	9·764
Sulphite of soda . .	trace	
Chloride of sodium .	3·480	3·140
Aluminate of soda .	0·632	0·716
Silicate of soda . .	0·414	0·318
Insoluble matter . .	0·250	0·498
	99·350	99·437

It is from this salt that the crystallized carbonate of soda is manufactured. The ash is dissolved in boiling water until the solution attains a specific gravity of 1·250 (50° Twaddell); it is then run into a cistern, where it is mixed with sufficient cold water to reduce the specific gravity to 1·21 (42° Twaddell). This occasions the deposition of a quantity of earthy matter. A little bleaching-powder is then added to the liquid, which causes another deposition. After this has been allowed to settle, the solution is carefully decanted into another pan, and evaporated till it attains a specific gravity of 1·27 (54° T.). From this it is run into another cistern, from which it passes into the crystallizing pans. The average time taken in crystallization is eight days; but it of course varies very much with the season of the year and the state of the atmosphere. It is greatly assisted by placing a few bars of wood, two or three inches broad, on the top of the liquor.

The crystallized carbonate of soda thus obtained yielded on analysis,—

	I.	II.
Carbonate of soda	36·476	36·931
Sulphate of soda	0·943	0·542
Chloride of sodium	0·424	0·314
Water	62·157	62·213
	100·000	100·000

As it contains ten atoms of water of crystallization, its formula is NaO, CO_2+10HO, and the per-centage composition calculated from this formula is—

Carbonate of soda	37·500
Water	62·500
	100·000

By driving off the water from these crystals by heat, a very pure carbonate of soda is obtained, which is used in the manufacture of glass.

It yielded on analysis—

Carbonate of soda	98·120	97·984
Sulphate of soda	1·076	1·124
Chloride of sodium	0·742	0·563
	99·938	99·671

Table exhibiting the Composition of Common Salt and Products of the Soda Manufacture.

	Salt.	Sulphate of soda.	Soda ball.	Waste.	Soda-ash before carbonating.	Soda-ash.	Carbonate of soda before carbonating.	Carbonate of soda.	Refined ash.	Crystallized carbonate of soda.	Carbonate of soda theory.	Ash made from crystallized carbonate of soda.
Chloride of sodium	931·615	10·956	1·913	......	3·917	3·665	3·635	3·254	3·310	0·369	......	0·652
Chloride of potassium	trace											
Chloride of magnesium	1·066											
Sulphate of lime	10·098	9·731	......	4·281								
Sulphate of magnesia	1·348	2·893										
Carbonate of lime	1·500	......	......	24·220								
Water	54·373	......	0·700	2·100	......	......	......	......	......	62·135	62·500	
Sulphate of soda	......	962·170	1·160	......	7·415	9·676	8·036	9·027	10·012	0·742	......	1·100
Sulphite of soda	......	......	......	......	2·182	1·126	1·174	0·386				
Iron peroxide	......	2·300	......	5·716								
Free acid	......	8·850										
Sand	......	3·100	4·285	5·746								
Carbonate of soda	......	......	35·640	1·309	67·210	71·037	80·279	83·881	84·517	36·703	37·500	98·052
Caustic soda	......	......	0·609									
Caustic lime	......	......	6·301									
Hydrate of soda	......	......	......	......	15·252	12·181	3·318	0·897	0·280			
Aluminate of soda	......	......	2·350	......	1·124	0·828	1·095	0·816	0·674			
Sulphuret of sodium	......	......	1·130	......	1·428	......	0·230					
Hyposulphite of soda	......	......	......	......	trace							
Silicate of soda	......	......	......	......	0·915	1·014	1·275	0·882	0·366			
Ultramarine	......	......	0·295									
3CaS, CaO	......	......	29·172	20·363								
Sulphuret of iron	......	......	4·917									
Silicate of magnesia	......	......	3·744									
Carbon	......	......	7·998	12·709								
Hyposulphite of lime	......	......	......	trace								
Hydrate of lime	......	......	......	5·583								
Bisulphuret of calcium	......	......	......	3·583								
Sulphuret of calcium	......	......	......	8·527								
Insoluble matter	......	......	......	......	0·894	0·390	0·870	0·781	0·374			

III. *On a new Imaginary in Algebra. By* James Cockle, *Esq., M.A., of Trinity College, Cambridge, and Barrister-at-Law of the Middle Temple**.

ALGEBRA may be regarded under the triple aspect presented by the words Identity, Equivalence, and Impossibility †; but the latter view will fall more particularly within the scope of the present observations. The ordinary algebra, it is true, takes cognizance, not only of negative and unreal quantities, but sometimes of questions involving impossibility. This impossibility is indicated either by contradictory arithmetical results or, occasionally, by the symbol *infinity* ‡. But, neither in the one case nor the other, does the indication of impossibility furnish us with the elements of a calculus. Unreal results, on the contrary, although not subjects of conception, like number, nor directly interpretable, like negative quantities, are yet not only indirectly interpretable, but also important instruments of investigation. Why is this? My answer is,—because impossibility has never yet been symbolized. And I would add that, before this is done, we ought to

* Communicated by T. S. Davies, Esq., F.R.S.L. & Ed., &c., who requests us to annex the following note.

[It will of course be understood that I do not pledge myself to an agreement with Mr. Cockle's views on the *geometrical* signification of his i, j, k. With respect to them as algebraical symbols, I would not here offer an opinion: but with respect to the geometrical interpretation, I take a totally different view, as will be inferred from a short paper of mine printed in vol. xxix. (pp. 171–175) of the Philosophical Magazine, under the signature "Shadow."

Mr. Cockle has undoubtedly the weight of cotemporary scientific authority on his side of the question; and, indeed, I believe I stand nearly alone in the view I take of these questions. It would, however, be as unphilosophical a mode of searching for the truth as it would be disingenuous in the discussion, to suppress the expression of all views which differ from those which I may happen to entertain. It is always a matter of far less moment WHO *is right* than WHAT *is true*.

Mr. Cockle very kindly put his paper into my hands for perusal before he printed it; and I have much pleasure in fulfilling his request by forwarding it to you.

I may add, that it is much to be desired that the *history* of the attempts that have been made to give an explanation of the *symbols of incongruity* should be published. A strict discrimination between the views of different algebraists and geometers might prevent the waste of much valuable time and power; for there can be no doubt that a large portion of the speculations which have been put forward in recent times are essentially identical with much earlier ones.

Little Heath, Charlton, T. S. D.]
Nov. 25, 1848.

† Phil. Mag. S. 3, vol. xxxii. p. 352.

‡ Peacock, Report on Analysis (Third Report of British Association), pp. 237–238.

hesitate in saying, either that impossibility is incapable of being rendered subservient to the purposes of algebra, or even that it is absolutely uninterpretable. A symbol for impossibility is not only desirable, but actually necessary, provided that we wish to classify with accuracy the various subjects of algebraic research, and to distinguish those which are unreal from those which are impossible. We might adopt an arbitrary symbol to denote impossibility; but a deduced symbol is preferable, for it gives to our investigations the character of true developments of ordinary algebra. Such a deduced symbol I have obtained by means of a surd equation *. The next step is, to ascertain the fundamental properties of the new symbol, its origin and nature being duly considered. As we might expect, *à priori*, anomalous results offer themselves in the course of our progress with these inquiries; such results require at least an attempt at explanation. The geometrical interpretation (or rather capability of interpretation) of the new symbol is another point not unworthy of consideration, and the same may be said of the employment of that symbol in analytical discussions. I purpose, then, in this paper, to treat,—1, of the Utility of the new Symbol; 2, of the Value of its Square; 3, of a certain Anomalous Result; 4, of the Interpretation of the Symbol in Geometry; and, lastly, of its proposed Employment in Analysis.

1. *Of the Utility of the new Symbol.*

I shall show that there are relations, which cannot be expressed by means of the ordinary algebraic symbols. Hence, if it be of any importance to express such relations, a new notation must be adopted. That it is of importance, those who value logical precision and accurate classification will, if I am not mistaken, be disposed to admit, on such grounds alone, and quite irrespectively of any ulterior applications of which the new symbol may be susceptible. It will be observed, in what follows, that I have in certain cases employed an accented zero ($0'$). I have done this in order to distinguish what is, in fact, an absolute negation of existence, from the

* This symbol possesses the character which we might, almost, have anticipated for it *à priori*. In Universal Arithmetic, a negative quantity is *impossible* (Peacock, Report, p. 189), but its square is possible. So, even when negative quantities are recognized, a negative square is contradictory, and unreal quantities are *impossible*, but their squares are possible. In like manner, the square of a Pure Impossible—of a quantity taken as simultaneously positive and negative—is to be treated as possible. The contradiction vanishes on squaring, and there is a striking analogy with the other cases.

ordinary (unaccented) zero which represents a certain state of quantity*.

Let

$$0' = 1 + \sqrt{j}\,; \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

multiply both sides of this equation by $1 - \sqrt{j}$, and we have

$$0' \times (1 - \sqrt{j}) = (1 + \sqrt{j})(1 - \sqrt{j}); \quad . \quad . \quad . \quad (2.)$$

but

$$1 - \sqrt{j} = 2 - (1 + \sqrt{j}) = 2 - 0'\,;$$

hence, substituting on the left-hand side of (2.), and multiplying together the factors which compose its right, we obtain

$$0' \times (2 - 0') = 1 - j. \quad . \quad . \quad . \quad . \quad . \quad (3.)\dagger$$

In (1.) substitute unity for j; the result is

$$0' = 1 + 1\,;$$

hence j is not equal to unity, and consequently $1 - j$ is not equal to zero. Acting upon this, let us proceed to obtain, from the symbols of ordinary algebra, the most general expression for a quantity different from zero. We may attain our end as follows. Let a, w, and x be any real quantities whatever, positive, negative or zero, and let $i = \sqrt{-1}$; also suppose that

$$W = w - a + \left(\frac{a - a}{w - a}\right)\left(\frac{a - a}{x - a}\right),$$

and

$$X = x - a + \left(\frac{a - a}{w - a}\right)\left(\frac{a - a}{x - a}\right);$$

then

$$W + iX$$

is the most general expression of ordinary algebra for a quantity different from zero, and may be made to take any given value whatever, excepting zero. Now, as we have seen,

$$1 - j = \textit{some quantity other than zero}\,;$$

hence, those who would maintain the adequacy of the ordinary notation to express any relation whatever, possible or impossible, must sustain the equation

$$1 - j = W + iX\,;$$

* Peacock (Third Report of British Association), pp. 232, 233, &c., and also p. 268. The accented zero is discontinuous; and a remark of Professor J. R. Young, on impossible equations, in the Mechanics' Magazine (vol. xlix. p. 463) suggests the characteristic that, to 0′, the right-hand side of (1.) can make *no approximation* in terms of the ordinary symbols of algebra.

† The product $\sqrt{j} \times \sqrt{j}$ has the double form j and $\sqrt{j^2}$; I here take the former value, on the principle that $\sqrt{-1} \times \sqrt{-1}$ is equal to -1.

but this equation cannot be sustained; for, if we deduce from it the value of j, and substitute that value in (1.), we arrive at

$$0' = 1 + \sqrt{1 - W - iX},$$

in place of which we may write

$$0' = 1 + \alpha + i\beta;$$

this last equation gives $\beta = 0$, whence we infer that $X = 0$; consequently, α being real, and the positive square root being taken, we arrive at

$$0' = 1 + \alpha = \textit{unity together with a positive number};$$

which cannot be. Hence j is not of the form $W + iX$, and yet it is different from zero, as will be seen on substituting zero for j in (1.). In short, j is a quantity *sui generis*—an impossible quantity—a quantity the very conception of the existence of which involves the equation (3.). And, of this last-mentioned equation, it may be added, that it is to be regarded as one of the principal symbolic decompositions which the theory of j involves, and is not to be confounded with the apparently similar equation

$$0 \times (2 - 0) = W + iX.$$

Thus there are impossibilities not capable of being expressed either by zero, or by quantities of the form $W + iX$, unlimited as are the values which may be given to W and X. And, although infinity be among such values, it becomes necessary to have recourse to the new symbol j to indicate the impossibility implied in (1.). And j would be useful, were it only to indicate such impossibilities.

2. *Of the Value of its Square.*

We are now arrived at a topic, the discussion of which will, perhaps, assist in showing that the apparent difficulties, connected with the theory of j, are not such as to justify us in rejecting it as unsuited to the purposes of analysis, or as incapable of becoming the symbol of a calculus. In the fact, that j has a real square, we have something like a key to the method of rendering available this anomalous symbol. Starting from our fundamental equation, slightly changed in its form, we have

$$+\sqrt{j} = -1;$$

on cubing both sides of this equation, we obtain

$$+\sqrt{j^3} = -1,$$

whence* we infer that

$$j^2 = 1.$$

* The present case is distinguishable from that alluded to in the last note.

Is j then identical with either of the ordinary square roots of unity? No. In what respect does it differ from those roots? This I proceed to show, and as follows. Unity may be regarded as the square, either of positive, or of negative unity: and, as we regard it from one or the other point of view, we must write its square roots thus:—

$$+\sqrt{(+1)^2}, \text{ or } -\sqrt{(-1)^2},$$

both of which are included in the expression

$$\pm\sqrt{(\pm 1)^2}.$$

Let us now reverse the signs under the radical; then the last expression becomes

$$\pm\sqrt{(\mp 1)^2}.$$

This last is a contradictory and, consequently, impossible expression, which takes the following two impossible forms,

$$+\sqrt{(-1)^2} \text{ and } -\sqrt{(+1)^2}$$

which I shall represent by j' and j'' respectively. Now, if we square $\pm\sqrt{(\mp 1)^2}$, it becomes $(\mp 1)^2$, the contradiction being eliminated by involution. Hence we infer that

$$j'^2 = j''^2 = 1 = j^2;$$

and it is not difficult to see that j, j' and j'' are values of impossible square roots of unity*. To indicate, however, that the discrepancy between the signs without and within the radical cannot be eliminated by merely changing the sign prefixed to the radical, I shall use additional brackets, and suppose that the following equations hold; viz.

$$\pm j' = \pm[+\sqrt{(-1)^2}], \text{ and } \pm j'' = \pm[-\sqrt{(+1)^2}].$$

Seeing thus the contradictory nature of the symbols j, j', j'', we must not be surprised at finding ourselves, very early in our inquiries on the subject, face to face with such a result as the equation (3.) given above. We see, however, that contradictions may vanish and available results follow.

3. *Of a certain Anomalous Result.*

In the case of the equation (3.), we have seen that there is an anomaly, inherent in the very supposition of impossible quantity, which does not occur in treating of real or unreal quan-

* I think that the following relations hold, viz.—

$$j' = -j'', \quad j'j'' = -1.$$

I shall not here attempt to discuss the relation of j' and j'' to j. The former quantities are only introduced here to illustrate what is meant by an *impossible* square-root of unity.

tity. That equation is to be considered, rather as an evidence of the *nature* of the quantity which I am discussing, than as a guide (or impediment) to us in its symbolic application; and although it merits further consideration, yet I do not feel called upon to bestow that consideration here*, inasmuch as in the theory of tessarines j is not affected with a radical sign, and it consequently becomes unnecessary, for the purpose which I have in view, to enter upon the subject of equations expressed by means of radicals. But I am about to point out another anomaly, the reverse of that which occurs in (3.): it is that on the supposition that j^2 is equal to unity,

$$(1+j)(1-j)=1-1=0;$$

that is to say, the (unaccented) zero may be considered as the product of two impossible factors, neither† of which vanishes. It can, however, be at once shown that this anomaly cannot lead us into error; for assuming the equation

$$a^2-b^2=(a+jb)(a-jb), \quad . \quad . \quad . \quad . \quad (4.)$$

and bearing in mind that a tessarine cannot vanish unless all its constituents are zero, we see that neither $a+jb$ nor $a-jb$ can vanish, unless $a=0$ and $b=0$. Suppose that $a=0=b$, then the equation (4.) becomes an identical one, and no error is introduced. On the other hand, imagine that a^2-b^2 should vanish from a becoming equal to b (both a and b being different from zero), then the right-hand side of (4.) would become

$$(a+ja)(a-ja);$$

but, bearing in mind the fundamental property of tessarines, we should be in no danger of inferring that one of these factors must be zero, and consequently we should introduce no error into our investigations. It may be said, Is zero, then, decomposable into non-vanishing factors? Impossible. I reply, true, the factors *are* impossible: they are so by their origin and nature.

4. *Of the Interpretation of the Symbol in Geometry.*

In this field, I am about to indicate what (I hope) will be my

* If for no other purpose, the accent on the zero is useful for the purpose of denoting an impossible equation. If the accented zero is different from the arithmetical zero (and there are indications of a difference), what is the square $(0'^2)$? a *negation of a negation*? I think that we must regard $0'^n$ as *identical* with $0'$, when n is positive, and, consequently, $0'^{-n}$ as identical with $0'^{-1}$, and then n is negative. Zero is not supposed to be included in these values of n.

† If one such factor be zero, the other must be infinite. But this is inconsistent with the forms of the factors. It is on a consideration of this kind that my theory of congeneric surd equations is based. (See Mechanics' Magazine, vols. xlvii., xlviii. and xlix.)

future course, rather than actually to commence it. But I will not disguise the end which I am desirous of attaining—that of vindicating, for ordinary algebra, a claim to the power of representing space of three dimensions, as completely as, by the aid of unreal quantities, it can denote any conditions whatever *in plano*, and any modification of such conditions. By way of illustration, let there be given two points, A and B, and let it be required to find a third point, C, such, that the rectangle AC × CB may be equal to half the square described upon AB. Now, if we proceed on the supposition that C is somewhere in the straight line which passes through A and B, we must suppose that C lies between A and B, for otherwise the rectangle would obviously be greater than the square. Bearing this in mind, let $AB=2a$, and $AC=x$; then the *quæsitum* of the problem gives the equation

$$x(2a-x)=2a^2, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5.)$$

or

$$x^2-2ax=-2a^2,$$

whence

$$x=a\pm a\sqrt{-1}.$$

Hence, the problem is an impossible one, if we regard C as lying in AB. But, if we interpret the symbol $\sqrt{-1}$ as meaning perpendicularity*—in which case we must regard (5.) as the representation of the problem in its most general form—we have the following construction. Along AB take $AD=\frac{1}{2}AB$; from D draw DC perpendicular to AB, and equal to DA; then, C is the point required. The point C, so obtained, evidently fulfils the condition of the question. In fact, any line in a plane being given, as an axis, we may represent any point whatever, in the plane, by the formula $p+q\sqrt{-1}$, p and q being real. It is to be remarked, however, that, a line being given in space, when the symbol $\sqrt{-1}$ occurs in our researches, we may (as in the above problem) draw our perpendicular in whatever direction we please, provided only it be in a plane perpendicular to the primitive axis. But, the perpendicular once drawn, we have a *determined plane* to which, I apprehend, all our interpretations of $\sqrt{-1}$ are to be confined; for I conceive that, consistently with ordinary algebra, we cannot, on $\sqrt{-1}$ occurring a second time in our investigations, take that symbol to denote a line perpendicular to, or making any angle with, the former perpendicular. Perhaps I have said enough to show what my own views are, as to the applications of the symbol $\sqrt{-1}$, and the limitations to be imposed on its inter-

* This has been done by Hamilton, Warren, and others.

pretation. Supposing, then, i and its interpretation to be admitted into geometrical inquiries, the question comes, how can j enter into such inquiries when it can never enter into the rational equations in which such inquiries usually result? The answer is, that geometrical conditions are not necessarily reducible to the form of rational equations. Consider, by way of example, the equation (5.). This equation, after reduction to the usual form, may be resolved into congeneric surd factors; and, the geometrical meanings of a and x being lines, we may express the evanescence of those factors in geometrical language. Such evanescence may be possible or impossible; if the latter, then j forces itself into our investigations, and the next point is, how to interpret its occurrence. I think that the following are the considerations which ought to guide us in this interpretation. Imagine three points A, B, and C. If these points are in a straight line, their relations may be represented by real quantities, and the only *determined* space before us is a *line*. But suppose that these points are required to fulfill conditions inconsistent with the hypothesis of their being in the same straight line; then (as will be clearly seen on referring to the above problem) a *plane* is determined, and unreal quantities introduced; and, supposing that a problem respecting three points admits of solution, the most general geometrical entity that can be determined by it is a plane. If then we arrive at an *impossible* quantity, as the result of our geometrical inquiries respecting the possibility of a supposed relation between three points, we may be sure that the relation cannot exist. Were the relation possible, it would be possible in a plane; and i is quite adequate to express (in combination with real quantities) any possible relative position of three points. Let us take a step further, and suppose that we are discussing the position of four points A, B, C, D. Take AB as the primitive axis, and let $AB=2a$; then the expression $p+iq$ may be made to represent either C or D (or both, provided that all the four points are in the same plane). Thus we shall have A represented by zero, B by $2a$, C by $c+ie$, and D by $d+if$. But, suppose that D is out of the plane of A, B and C, then, from what has been before observed, $d+if$ will cease to represent D, for i means perpendicularity to AB in the plane of A, B and C. Hence, in any inquiry respecting four points, the occurrence of j would not be conclusive as to the impossibility of the problem, but only as to the fact that the points cannot be in the same plane. (I assume, of course, that, in forming the condition or conditions of the problem, all the points are represented by different values of the expression $p+iq$.) In such a case, then, j would (provided the problem were possible) be the sign of perpendicularity to the plane ABC, and the transition, from regarding j as the sign of impossibility, to viewing it as the symbol of perpendicularity, is by no means difficult*. As may be inferred, from what I said in opening the question of interpretation, I am not prepared to complete this view of the question in the present paper; but I cannot refrain from remarking, that j is not an unreal root of unity, and that, although it may indicate perpendicularity, yet that we must *envisage* it in a manner different from that in which we regard i. In fact j indicates perpendicularity *to a plane* as i does *to a line*; $j^2 = 1$ and $i^2 = -1$. We may realize the distinction, geometrically, as follows. On the semidiameter of a sphere conceive another sphere described. Let the point of contact of the spheres be considered as the pole of both. Conceive two points, one at the centre of the larger sphere, and the other situate anywhere on its equator; let the first point revolve in a meridian of the smaller sphere, and the second in the equator of the greater, and let the angular velocity of the first point be double that of the second. We need only consider the relative positions of the points when the first point is either at the centre of the greater sphere, or at the common pole of both the spheres. It will be seen that the phases (so to say) of the points correspond to those of the above quantities j^2 and i^2; and also that the first point represents direction perpendicular to a plane, and the second, direction perpendicular to a line†. I mention this because it might be supposed that j is only a second perpendicular to the primitive axis, and, consequently, that it is only a second unreal root of unity‡.

5. *Of its proposed Employment in Analysis.*

Should the admissibility of the new symbol j be established

* The transition from unreal to impossible quantities will, perhaps, be best exemplified by the two following problems:—(1.) Find three equidistant points, and, (2.), Find four equidistant points. The first may be expressed by means of a quadratic with unreal roots; the unreal quantities arising from one of the points being out of the *line* joining the other two. So, unless I am mistaken, the solution of the last may be exhibited by means of impossible quantities, which take their origin from the fourth point being out of the *plane* of the other three.

† This remark will perhaps be better expressed when we have substituted, for the lesser sphere, a prolate spheroid, and then diminished indefinitely the minor axis of the spheroid. Its major axis is, of course, to remain unaltered, and equal to, and coincident with, the axial semidiameter of the larger sphere.

‡ The symbol j will enter into geometrical inquiries in the following manner. Suppose that we arrive at such an equation as

$$\sqrt{r(+1)^2} + \sqrt{s(+1)^2} = 0;$$

then, $s = jr$ is the solution.

its use would not be confined to the discussion of the functions which I have proposed to call tessarines. It would, as it seems to me, be capable of other applications, and would tend to generalize all processes to which the imaginary symbol $\sqrt{-1}$ has been yet applied. Thus, by way of illustration, consider the expression

$$\varepsilon^{w \pm ix \pm jy \pm kz}.$$

If we vary, in every possible way, the order of the signs in the above; add the different values, so obtained, together; and expand the sum; it will be seen that the result is free from imaginaries. Hence, that sum may, in all cases, be used instead of the resulting series. The finite expression for the series, so obtained, would in some instances be found useful.

It now only remains to make one remark respecting the notation which I have adopted. In taking i to represent $\sqrt{-1}$, I think that I acted under an impression that it had been so used anterior to the quaternion theory*. The use of j and k followed that of i, and seemed to offer an easier and better mode of comparing results with that theory than I should otherwise have had. In order, however, in future to avoid confusion, and the misapprehensions which may arise from employing like symbols for unlike purposes, I shall use α, β, and γ in place of i, j and k respectively. Under this notation, a tessarine (t) will be written

$$w + \alpha x + \beta y + \gamma z,$$

where

$$\alpha^2 = -1, \quad \alpha\beta = \gamma, \quad -\beta^2 = \gamma^2, \quad \gamma\alpha = -\beta;$$

and also, if the view taken in this paper be correct,

$$\beta^2 = 1, \text{ and } \beta\gamma = \alpha.$$

In my endeavours to bring space of three dimensions under the dominion of a new species of ordinary algebra, I may perhaps be permitted to disclaim anything like dogmatism. I should wish all my views respecting the new symbol to be regarded in the light of suggestions. And if, regarded as suggestions, they should have the effect of directing attention to the theory of congeneric surd equations, they will not be without their utility.

2 Church-Yard Court, Temple,
November 23, 1848.

Postscript, 7th December 1848. Perhaps I shall be permitted to add the following few lines on the subject:—

* Sir W. R. Hamilton has noticed this in the Phil. Mag. S. 3, vol. xxv.

6. *Of the Modular Relations of Tessarines.*

Let θ be the *amplitude*, ϕ the *colatitude*, ψ the *longitude*, and μ the *modulus* of a tessarine (t). To these quantities (which are identical with the corresponding ones in the quaternion theory, and which may, without confusion, be adopted from that theory) must be added another, which I propose to call the *submodulus*, and to denote by ν. The submodulus is defined by the equation

$$\nu^2 = wy + xz,$$

and the submodulus of the product (t'') of two tessarine factors (t and t') is determined by the relation

$$\nu''^2 = (\mu\nu')^2 + (\mu'\nu)^2,$$

and the modulus of that product by

$$\mu''^2 = (\mu\mu')^2 - (2\nu\nu')^2.$$

We have, further,

$$ww'' + xx'' + yy'' + zz'' = w'\mu^2 + 2y'\nu^2,$$

and

$$w'w'' + xx'' + yy'' + zz'' = w\mu'^2 + 2y\nu'^2.$$

The equation for the submodulus may also be expressed as follows:—

$$\nu^2 = \mu^2 \sin\theta \sin\phi \,(\cos\theta \cos\psi + \sin\theta \sin\psi \cos\phi).$$

The construction of this last equation, and of the preceding ones, I hope to discuss on another occasion. And there is a surface—that defined by the equation

$$\mu y \cos\chi + xz = 0$$

(where χ is supposed constant),—which appears to merit attention in connexion with the theory of tessarines.

Second Postscript, 14th December 1848. I have the satisfaction of adding, that the hope above expressed has not been disappointed, and that I have solved the problem of the *four equidistant points* in the manner I proposed. I have determined the position of the fourth point by an expression of the form

$$A + iB + jC;$$

where C is to be measured in a direction perpendicular to the *plane* in which the first, second, and third points are situate. The four points are, of course, at the angles of a regular tetrahedron. I hope to obtain for the solution a place in this Journal.

IV. *On the Continuance of a Solar Spot.* *By* W. Pringle, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

IN the notice last communicated respecting the duration of a solar spot from August to November (Phil. Mag. Dec. 1848), I stated that I should not perhaps have it in my power to observe its re-appearance in December, should it then return. I have had, however, the opportunity of making two observations; one on the 8th, and the other on the 13th of December.

On the 8th there was a succession of spots, or clusters, following each other at intervals in a straight line, and cutting down in an oblique direction, as usual at this period. They were all in the northern hemisphere, except one dispersed group of small spots which appeared in the south-western portion of the sun.

The first spot or group next the eastern verge, which consisted of two rather large spots with some minor ones attached, appeared too little advanced to have come on upon the 4th or 5th—the time that the spot of November might be expected—being only about two digits from the circumference. The second spot, which was about three digits and a half advanced, seemed to me to correspond to the place the November spot ought to have occupied; and upon examining it attentively, it presented such features as might result from the further decay of the spot since it was last seen and sketched by the writer on the 19th of November. At that time it was nearly of a triangular form, the longest side measuring about 45,000 miles; the interior space exhibiting a mottled ground, or shallow speckled with small dark spots. It was then large enough to be very plainly perceptible to the naked eye, as it had been at each period of its appearance.

If I have rightly recognised it in the spot of the 8th of December, it has dwindled into a comparatively small and unnoticeable cluster, of an irregular outline, still preserving the mottled penumbral interior studded with minute nuclei; and this was one of the distinguishing characteristics of its past appearance, that it uniformly presented a dusky ground inlaid with *lateral* nuclei, never once resolving itself into anything like a great *central* nucleus.

The nucleus of September 21st, which Mr. Weld estimated at $1' \; 7''{\cdot}2$ in its longest diameter (Phil. Mag. Dec. 1848. p. 480), lay imbedded in the western portion of the spot, and might be considered as the nucleal remains of one of the two separate spots of August, and which I conceive had been conjoined. On the eastern margin of this nucleus, or rather on the interior edge of the penumbra trenching on it, I may mention, a singular patch of a half-burnished colour which attracted my attention after the spot had passed the sun's centre, and for several succeeding days. It was not like those white or bright stellar appearances sometimes seen, and of which there were also at the same time several scattered over the general shallow or shaded space; it rather resembled some dim, distant fire, seen through a foggy atmosphere at night; and I thought at times that I could detect a gradation of tints from reddish-orange to yellowish-green. It partook so much of a certain lustre, that I imagined at first that it was in the glass. But it was not. Could the electric principle have had any part there? or was the lurid reflexion merely the result of a particular fusion of the luminous and nebulous matter; different in degree though not in kind to that which is thought to occasion the stellate specks? The large nucleus alluded to became latterly enveloped in a partial penumbra of its own, of the usual smooth and comparatively clear surface, though still involved in and connected with the darker discoloured base, or ground of the entire spot.

Another large round nucleus became formed by the combination of the small ones on the eastern side of the spot before its disappearance at the end of September, which circumstance at the time gave some reason to suspect the possibility of the united group breaking up into two distinct spots, as in August. Nor was such a metamorphosis even improbable; and from some observations made in July I am much disposed to consider that it had previously undergone a similar one. Among some memoranda made during the latter month, I find a notice of a spot or cluster which was seen by the naked eye on the 29th and 31st of July, consisting of one large spot, with well-defined nucleus and umbra, closely connected with an extensive compact group of minute nuclei imbedded in a dusky penumbra. A rough detached draft represents this spot somewhat advanced past the centre of the sun, going westwards, though the exact date of the drawing is not given underneath. As it was evidently somewhat past the centre, it could not have been more than six days from its disappearing. Taking the time from the 30th of July, the intermediate day, this would give the 6th of August for its passing the western edge of the sun, and the 19th of August for its arrival at the eastern limb, or reappearance. The supplied data must, of course, render this hypothetical, though it is far from being improbable.

But I am retrograding to the detail of former appearances, which would have been more appropriate in the previous notice, had not I feared intruding on space, as I may even now.

The second view I had of the spot of this month was on the 13th, when it was within little more than four days of its disappearance. Its general aspect was still in favour of its identity with the preceding.

From what has been observed, I am inclined to conclude that the solar spots may last much longer than we are yet aware of; and that the want of sufficient observation alone has restricted our knowledge of the true extent of their duration. No doubt it would occupy almost the entire time and undivided attention of any individual to follow out their phases and developments so as to satisfy the objects required: but an association, I am informed, has recently been instituted for the express purpose of observing and studying the spots in a more systematic manner than has hitherto been attempted, and to whose united labours, therefore, if published, we may look for a mass of new matter and interesting information on the subject, such as could not be expected from mere individual and isolated observation.

The spots seem almost the only inlets whereby to penetrate, if possible, into the sun's physical character, with the exception of the phænomena connected with eclipses; and however volatile and changeable they may appear, there is no good reason to despair of yet reducing their evolutions to a system, and even thus detecting laws which may bear upon the solution of the general physical organization of the sun.

Submitting these somewhat desultory notices to your consideration,

I remain, Gentlemen,

Your most obedient Servant,

Edinburgh, Dec. 15, 1848. W. PRINGLE.

P.S. Permit me to take this opportunity to state, what it may perhaps interest Mr. Glaisher or others to know, that the same *distorted* image of Mercury on entering the sun's disc which one out of eight telescopes presented at the Greenwich Observatory, was also observable here. The planet appeared to me to enter like a black wedge, with undulating edges like a Malay *Krees* or dagger, an appearance which I ascribed at the time to the tremors of intervening smoke and vapours. This could scarcely have been the cause at Greenwich, as the seven other telescopes showed the planet round and clear. Dr. Dick, of Broughty Ferry near Dundee, also described the appearance as an "indentation" on the sun's limb.—W. P.

V. *A simple Rule for converting intervals of Sidereal into intervals of Mean Solar Time, and* vice versâ, *without the help of Tables. By the* Rev. J. M. Heath*.

I. WRITE down the given time.

II. Underneath its minutes, seconds, and decimals of second, write its hours, minutes, and decimals of minute, respectively, retaining three decimal places.

III. Subtract II. from I.

IV. Subtract II. from III., retaining the first decimals of hours only.

V. Write the hours, minutes, and decimals of minute in III. as minutes, seconds, and decimals of second respectively.

VI. Divide V. by VI., and place the *first decimal of hour* in IV. under the third decimal place of the quotient, writing the integral number of hours to the left, and add.

VII. To I. add its third part, and the hundredth part of their sum, retaining only the first decimal place of the hours.

VIII. As in VI., place the decimal of hour of VII. under the third decimal of second in I., writing the entire hours to the left, and add.

Then, the sum or difference of VIII. and VI. gives the sidereal, or the mean solar time, of which I. is solar or the sidereal time respectively, accurately to the third decimal of seconds.

		h	m	s		
Example.	I.	17	15	47·24		
	II.		17	15·787		
	III.	16	58	31·453		
	IV.	16	41	15·666		= 16h·7 nearly.
	V.		16	58·524		
	VI.		2	49·754		
				·167		
			2	49·921		
	VII.	17	15	47·24	0	
				17·	25	= I.
				5·	75	= $\frac{1}{3}$ of I.
					22	= $\frac{1}{100}$ of sum.
	VIII.	17	15	47·472		
Then adding or subtracting VI.			2	49·921		
Sidereal time for I. . =		17	18	37·393		
Mean solar time for I. =		17	12	57·551		

* Communicated by the Author.

VI. *On some Points in the received Theory of Sound.* *By* G. G. Stokes, *M.A., Fellow of Pembroke College, Cambridge**.

BEFORE entering on the main subject of this communication, I will make a few remarks with reference to Professor Challis's last paper in this Magazine. (Vol. xxxiii, p. 462.)

I have no intention of rendering my reasoning liable to the epithet "illogical," by attempting to explain away admitted contradictions. It was my endeavour in a former communication, and will be my endeavour in this, to show that the contradictions in question have no real existence. Since it would seem from Professor Challis's words, near the bottom of page 463, that he does not perceive that any step of the mathematical reasoning by which the contradiction in the case of plane waves was arrived at has been called in question, I beg to state that I do not admit the validity of any mathematical results obtained by a process which is equivalent to integrating over an infinite ordinate, without inquiring whether the passage be legitimate or not. Such a proceeding would lead to contradictions in other subjects as well as in hydrodynamics; for example, in central forces. It was not until after I had pointed out, at page 352, what I conceived to be the flaw in Professor Challis's reasoning, that I entered on the physical considerations alluded to by Professor Challis at page 462: and when I did so, it was not in the slightest degree with any intention of explaining away an admitted contradiction (for I have already stated that I do not admit the validity of the contradiction), but simply because those considerations seemed to be of some interest on their own account. I was particularly careful (see page 353) to keep the purely mathematical question quite distinct from the physical considerations which followed.

It will be necessary, in order to prevent confusion, that I should say a few words with reference to the admission that "plane waves are wholly incompatible with the transmission of articulate and musical sounds." What signifies it if the *ideal* elastic fluid which forms the basis of our mathematical reasoning be wholly incompetent to transmit such sounds unchanged? The purely mathematical question of contradiction or no contradiction is not in the slightest degree affected, although it forms an interesting subject of physical inquiry how far air, as agreeing approximately with our elastic fluid, may be incapable of transmitting musical sounds without modification.

To turn now for a moment to the physical question, I would observe that a fluid in which $p \propto \rho$ *would* be capable of trans-

* Communicated by the Author.

mitting such waves as would produce musical sounds to a considerable distance without important alteration, even if the waves were plane; although sooner or later a wave of this kind would be converted into what Professor Challis calls a *breaker*, but which I think is more nearly analogous to a *bore*. An integral of the exact equation which applies to spherical waves has never been obtained; but it is evident that the divergence of such a wave must tend to counteract the formation of a bore. In fact, if we suppose the velocity v represented, as it would be approximately, by $\frac{1}{r}.f(r-at)$, we have, neglecting the term involving $\frac{1}{r^2}$, $\frac{dv}{dr}=\frac{1}{r}f'(r-at)$; so that for a given phase of the wave, that is for a given value of $r-at$, the tangent to the velocity curve (vol. xxxiii. p. 350) would vary inversely as r; and therefore, as far as depended only on divergence, the inclination of the curve would become more and more gentle in the anterior, as well as the posterior portion. I feel however almost certain, in consequence of an investigation in which the effect of divergence was very approximately taken account of, that the formation of what I have called a bore, although much retarded by divergence, is not ultimately prevented. I speak of course with reference to the ideal fluid in which $p\alpha\rho$. I see no reason for supposing that the development of heat and cold by sudden condensation and rarefaction would have any tendency to prevent the formation of a bore. I have already alluded to one cause which would have such a tendency (vol. xxxiii. p. 356), namely the internal friction of the fluid. If, during the rapid condensation and rarefaction of the fluid, there should be time for any sensible quantity of heat to pass off or be received by way of radiation, that would apparently have much the same effect as internal friction. The effect of distance upon the quality of sound, and the causes why sounds are mellowed by distance in air, whereas under water the sound of a distant bell is heard as a crash, would form an interesting field of inquiry.

I proceed now to notice the apparent contradiction at which Professor Challis has arrived by considering spherical waves, a contradiction which it is the chief object of this communication to consider. The only reason why I took no notice of it in a former communication was, that it was expressed with such brevity by Professor Challis (vol. xxxii. p. 497), that I did not perceive how the conclusion that the condensation varies inversely as the square of the distance was arrived at. On mentioning this circumstance to Professor Challis, he kindly explained to me his reasoning, which he has since stated in detail (vol. xxxiii. p. 463).

The whole force of the reasoning rests on the tacit supposition that when a wave is propagated from the centre outwards, any arbitrary portion of the wave, bounded by spherical surfaces concentric with the bounding surfaces of the wave, may be isolated, the rest of the wave being replaced by quiescent fluid; and that being so isolated, it will continue to be propagated outwards as before, all the fluid except the successive portions which form the wave in its successive positions being at rest. At first sight it might seem as if this assumption were merely an application of the principle of the coexistence of small motions, but it is in reality extremely different. The equations are competent to decide whether the isolation be possible or not. The subject may be considered in different ways; they will all be found to lead to the same result.

1. We may evidently without absurdity conceive an outward travelling wave to exist already, without entering into the question of its original generation; and we may suppose the condensation to be given arbitrarily throughout this wave. By an outward travelling wave, I mean one for which the quantity usually denoted by ϕ contains a function of $r-at$, unaccompanied by a function of $r+at$, in which case the expressions for v and s will likewise contain functions of $r-at$ only. Let

$$as=\frac{f'(r-at)}{r}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

We are at liberty to suppose $f'(z)=0$, except from $z=b$ to $z=c$, where b and c are supposed positive; and we may take $f'(z)$ to denote any arbitrary function for which the portion from $z=b$ to $z=c$ has been isolated, the rest having been suppressed. Equation (1.) gives

$$\phi=-a^2\int s dt=\frac{f(r-at)}{r}+\psi(r), \quad . \quad . \quad . \quad (2.)$$

$\psi(r)$ being an arbitrary function of r, to determine which we must substitute the value of ϕ given by (2.) in the equation which ϕ has to satisfy, namely

$$\frac{d^2.r\phi}{dt^2}=a^2\frac{d^2.r\phi}{dr^2}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (3.)$$

This equation gives $\psi(r)=\mathrm{C}+\frac{\mathrm{D}}{r}$, C and D being arbitrary constants, whence

$$v=\frac{d\phi}{dr}=\frac{f'(r-at)}{r}-\frac{f(r-at)}{r^2}-\frac{\mathrm{D}}{r^2}. \quad . \quad . \quad (4.)$$

Now the function $f(z)$ is merely defined as an integral of $f'(z)dz$, and we may suppose the integral so chosen as to vanish when $z=b$, and therefore when z has any smaller value. Consequently we get from (4.), for every point within the sphere which forms the inner boundary of the wave of condensation,

$$v=-\frac{D}{r^2}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5.)$$

Again, if we put $f(c)=A$, so that $f(z)=A$ when $z>c$, we have for any point outside the wave of condensation,

$$v=-\frac{A+D}{r^2}. \quad . \quad . \quad . \quad . \quad . \quad (6.)$$

The velocities expressed by (5.) and (6.) are evidently such as could take place in an incompressible fluid. Now Professor Challis's reasoning requires that the fluid be at rest beyond the limits of the wave of condensation, since otherwise the conclusion cannot be drawn that the matter increases with the time. Consequently we must have $D=0$, $A=0$; but if $A=0$ the reasoning at p. 463 evidently falls to the ground.

2. We may if we please consider an outward travelling wave which arose from a disturbance originally confined to a sphere of radius ε. At p. 463 Professor Challis has referred to Poisson's expressions relating to this case. It should be observed that Poisson's expressions at page 706 of the *Traité de Mécanique* (second edition) do not apply to the whole wave from $r=at-\varepsilon$ to $r=at+\varepsilon$, but only to the portion from $r=at-\varepsilon$ to $r=at$; the expressions which apply to the remainder are those given near the bottom of page 705. We may of course represent the condensation s by a single function $\frac{1}{ar}\chi(r-at)$, where

$$\chi(-z)=f'(z), \quad \chi(z)=F'(z),$$

z being positive; and we shall have

$$A=\int_{-\varepsilon}^{\varepsilon}\chi(z)dz=f(\varepsilon)-f(0)+F(\varepsilon)-F(0).$$

Now Poisson has proved, and moreover expressly stated at page 706, that the functions F, f vanish at the limits of the wave; so that $f(\varepsilon)=0$, $F(\varepsilon)=0$. Also Poisson's equations (6.) give in the limiting case for which $z=0$, $f(0)+F(0)=0$, so that $A=0$ as before.

3. We may evidently without absurdity conceive the velocity and condensation to be both given arbitrarily for the instant at which we begin to consider the motion; but then we

must take the *complete* integral of (3.), and determine the two arbitrary functions which it contains. We are at liberty, for example, to suppose the condensation and velocity when $t=0$ given by the equations

$$as=\frac{f'(r)}{r}, \quad v=\frac{f'(r)}{r}-\frac{f(r)}{r^2},$$

from $r=b$ to $r=c$, and to suppose them equal to zero for all other values of r; but we are not therefore at liberty to suppress the second arbitrary function in the integral of (3.) The problem is only a particular case of that considered by Poisson, and the arbitrary functions are determined by his equations (6.) and (8.), where, however, it must be observed, that the arbitrary functions which Poisson denotes by f, F must not be confounded with the given function here denoted by f, which latter will appear at the *right-hand* side of equations (8.). The solution presents no difficulty in principle, but it is tedious from the great number of cases to be considered, since the form of one of the functions which enter into the result changes whenever the value of $r+at$ or of $r-at$ passes through either b or c, or when that of $r-at$ passes through zero. It would be found that unless $f(b)=0$, a backward wave sets out from the inner surface of the spherical shell containing the disturbed portion of the fluid; and unless $f(c)=0$, a similar wave starts from the outer surface. Hence, whenever the disturbance can be propagated in the positive direction only, we must have A, or $f(c)-f(b)$, equal to zero. When a backward wave is formed, it first approaches the centre, which in due time it reaches, and then begins to diverge outwards, so that after the time $\frac{c}{a}$ there is nothing left but an outward travelling wave, of breadth $2c$, in which the fluid is partly rarefied and partly condensed, in such a manner that $\int rs\,dr$ taken throughout the wave, or A, is equal to zero.

It appears, then, that for any outward travelling wave, or for any portion of such a wave which can be isolated, the quantity A is necessarily equal to zero. Consequently the conclusion arrived at, that the mean condensation in such a wave or portion of a wave varies ultimately inversely as the distance from the centre, proves not to be true. It is true, as commonly stated, that the condensation at corresponding points in such a wave in its successive positions varies ultimately inversely as the distance from the centre; it is likewise true, as Professor Challis has argued, that the mean condensation in any portion of the wave which may be isolated varies ultimately inversely as the square of the distance; but these conclusions do not in the slightest degree militate against each other.

If we suppose b to increase indefinitely, the condensation or rarefaction in the wave which travels towards the centre will be a small quantity, of the order b^{-1}, compared with that in the shell. In the limiting case, in which $b=\infty$, the condensation or rarefaction in the backward travelling wave vanishes. If in the equations of paragraph 3 we write $b+x$ for r, $b\sigma(x)$ for $f'(r)$, and then suppose b to become infinite, we shall get $as=\sigma(x)$, $v=\sigma(x)$. Consequently a plane wave in which the relation $v=as$ is satisfied will be propagated in the positive direction only, no matter whether $\int \sigma(x)dx$ taken from the beginning to the end of the wave be or be not equal to zero; and therefore any arbitrary portion of such a wave may be conceived to be isolated, and being isolated, will continue to travel in the positive direction only, without sending back any wave which will be propagated in the negative direction. This result follows at once from the equations which apply directly to plane waves; I mean, of course, the approximate equations obtained by neglecting the squares of small quantities. It may be observed, however, that it appears from what has been proved, that it is a property of every plane wave which is the limit of a spherical wave, to have its mean condensation equal to zero; although there is no absurdity in conceiving a plane wave in which that is not the case as already existing, and inquiring in what manner such a wave will be propagated.

There is another way of putting the apparent contradiction arrived at in the case of spherical waves, which Professor Challis has mentioned to me, and has given me permission to publish. Conceive an elastic spherical envelope to exist in an infinite mass of air which is at rest, and conceive it to expand for a certain time, and then to come to rest again, preserving its spherical form and the position of its centre during expansion. We should apparently have a wave consisting of condensation only, without rarefaction, travelling outwards, in which case the conclusion would follow, that the quantity of matter altered with the time.

Now in this or any similar case we have a perfectly definite problem, and our equations are competent to lead to the complete solution, and so make known whether or not a wave will be propagated outwards leaving the fluid about the envelope at rest, and if such a wave be formed, whether it will consist of condensation only, or of condensation accompanied by rarefaction: that condensation will on the whole prevail is evident beforehand, because a certain portion of space which was occupied by the fluid is now occupied by the envelope.

In order to simplify as much as possible the analysis, instead of an expanding envelope, suppose that we have a sphere, of a constant radius b, at the surface of which fluid is supplied in such a manner as to produce a constant velocity V from the centre outwards, the supply lasting from the time 0 to the time τ, and then ceasing. This problem is evidently just as good as the former for the purpose intended, and it has the advantage of leading to a result which may be more easily worked out. On account of the length to which the present article has already run, I am unwilling to go into the detail of the solution; I will merely indicate the process, and state the nature of the result.

Since we have no reason to suspect the existence of a function of the form F $(r+at)$ in the value of ϕ which belongs to the present case, we need not burden our equations with this function, but we may assume as the expression for ϕ

$$\phi=\frac{f(r-at)}{r} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7.)$$

For we can always, if need be, fall back on the complete integral of (3); and if we find that the particular integral (7.) enables us to satisfy all the conditions of the problem, we are certain that we should have arrived at the same result had we used the complete integral all along. These conditions are

$$\phi=0 \text{ when } t=0, \text{ from } r=b \text{ to } r=\infty\ ; \quad . \quad . \quad (8.)$$

for ϕ must be equal to a constant, since there is neither condensation nor velocity, and that constant we are at liberty to suppose equal to zero;

$$\frac{d\phi}{dr}=\text{V when } r=b, \text{ from } t=0 \text{ to } t=\tau\ ; \quad . \quad . \quad (9.)$$

$$\frac{d\phi}{dr}=0 \text{ when } r=b, \text{ from } t=\tau \text{ to } t=\infty \ldots \quad . \quad (10.)$$

(8.) determines $f(z)$ from $z=b$ to $z=\infty$; (9.) determines $f(z)$ from $z=b$ to $z=b-a\tau$; and (10.) determines $f(z)$ from $z=b-a\tau$ to $z=-\infty$, and thus the motion is completely determined.

It appears from the result that if we consider any particular value of r there is no condensation till $at=r-b$, when it suddenly commences. The condensation lasts during the time τ, when it is suddenly exchanged for rarefaction, which decreases indefinitely, tending to 0 as its limit as t tends to ∞. The sudden commencement of the condensation, and its sudden change into rarefaction, depend of course on the sudden commencement and cessation of the supply of fluid at the surface of the sphere, and have nothing to do with the object for which the problem was investigated. Since there is no isolated wave of condensation travelling outwards, the complete solution of the problem leads to no contradiction, as might have been confidently anticipated.

How then stands the theory of sound as usually received? So long as we confine our attention to the first order of small quantities, which is a perfectly legitimate mode of approximation, there is neither contradiction nor difficulty; for Professor Challis's difficulty with respect to the effect of the development of heat by sudden compression, in the altered form in which he has now put it, has nothing to do with the first order of small quantities. On employing exact equations, it is true that a very remarkable kind of motion has been brought to light in the course of the discussion, and shown to be possible, if not in air, at least in an ideal fluid in which the pressure is equal in all directions, and varies as the density. The precise nature of this motion I do not pretend to describe. I have already stated (vol. xxxiii. p. 352) that I had grounds for believing that a sort of reflexion would take place; though whether this reflexion would or would not prevent the formation of what I have called a surface of discontinuity I am unable to say, although I am inclined to think that it would. To prevent misapprehension, I will observe that it is the motion, whatever be the nature of it, which takes place after the quantity denoted at page 351 by A becomes infinite, that I have referred to in using the word *bore*: I did not mean, in using that term, to assert that a surface of discontinuity was certainly formed. An interesting field of inquiry lies open with reference to the possibility of an actual approximation to a bore in the case of fluids such as they exist in nature, and generally with reference to the modification of sound by distance. It is to such an inquiry that the consideration of the effect of different functional relations between p and ρ, when the changes of p are too great to be considered proportional to those of ρ, properly belongs, and in particular the functional relation which connects p and ρ in the case of air, in consequence of the heat developed by sudden compression. But as regards the purely mathematical question of the treatment and interpretation of our equations, no contradiction arises when the restrictions which the occurrence of infinite quantities imposes on *all* mathematical reasoning are attended to.

In conformity with an intention which I have already expressed (vol. xxxiii. p. 349), and with the title which I have chosen for this article, I have confined myself to the defence of the theory of sound as usually received. I have refrained from calling in question the new and startling conclusions at which Professor Challis has arrived with reference to ray vibrations. I have done so partly because the subject has been taken up by the Astronomer Royal, partly because I have not leisure for the discussion at present, partly because the points which would have to be noticed, and which would be likely to arise in the course of the discussion, are so numerous, that I think it hardly fair to take up the pages of a Magazine like the present with the controversy. I cannot however conclude without recording my protest, first against equation (8.) (vol. xxxii. p. 282), and secondly against equations (B.) and (C.) (vol. xxxiii. pp. 99 and 100), by which an attempt is made to satisfy equation (A.).

Pembroke College, Cambridge,
Dec. 23rd, 1848.

VII. *On the Calculus of Operations. By the* Rev. Charles Graves, *M.A., Professor of Mathematics in Trinity College, Dublin**.

PROFESSOR YOUNG, objecting to the method by which the theorem of Leibnitz is usually extended to successive integration, has lately proved its applicability in that case by means of repeated "integrations by parts:" and he has shown how to obtain in this way a series of supplementary integrals, without the addition of which the theorem is not generally true, though they are commonly suppressed in the statement of it. Professor Young seems to impute this omission to the nature of the Calculus of Operations, by means of which the theorem is usually treated; as though that method necessarily gave the theorem in the imperfect form, and made no provision for the correction which he suggests.

It is my purpose here to show that the omission of the supplementary integrals has been caused by the use of an incomplete form of the binomial theorem, rather than by any inherent deficiency in the Calculus of Operations, which, if applied to this problem with proper caution, will furnish the desired result in a direct and elegant manner.

If we take the identity

$$(1+x)^{-1}=1-x+x^2-\&c.\dots.+(-1)^{m-1}x^{m-1}$$
$$+(-1)^m(1+x)^{-1}x^m,$$

and differentiate it $(n-1)$ times; we shall obtain a development, which coincides for its first m terms with that obtained by the use of the binomial theorem; and furnishes moreover

* Communicated by the Author.

the supplemental terms necessary to make the equation identically true, viz.

$$(1+x)^{-n}=1-nx+\frac{n(n+1)}{1.2}-\&c.\ldots.+(-1)^{m-1}A_n\,x^{m-1}$$
$$+(-1)^m\{A_n(1+x)^{-1}+A_{n-1}(1+x)^{-2}+\&c.\ldots.$$
$$+\frac{m(m+1)}{1.2}(1+x)^{-n+2}+m(1+x)^{-n+1}+(1+x)^{-n}\}x^m.$$

A_n being used to stand for

$$\frac{n(n+1)\ldots.(n+m-2)}{1.2\ldots\ldots.(m-1)},$$

or its equal

$$\frac{m(m+1)\ldots.(m+n-2)}{1.2\ldots\ldots.(n-1)};$$

and so on for A_{n-1}, A_{n-2}, &c.

And, if we write $\frac{D'}{D''}$ in place of x, we have the identity

$$(D''+D')^{-n}=D''^{-n}-nD''^{-n-1}D'+\frac{n(n+1)}{1.2}D''^{-n-2}D'^{2}-\&c.\ldots$$
$$+(-1)^{m-1}A_nD''^{-n-m+1}D'^{m-1}+(-1)^m\{A_n(D''+D')^{-1}$$
$$D''^{-n-m+1}+A_{n-1}(D''+D')^{-2}D''^{-n-m+2}+\&c.\ldots$$
$$+\frac{m(m+1)}{1.2}(D''+D')^{-n+2}D''^{-m-2}+m(D''+D')^{-n+1}$$
$$D''^{-m-1}+(D''+D')^{-n}D''^{-m}\}D'^{m};$$

which continues to hold good when D' and D'' are any two symbols of commutative and distributive operation, just as much as if they denoted quantities.

Using D to denote the operation of taking the differential coefficient with respect to x, we have

$$D(vu)=vDu+uDv;$$

from which we derive the symbolical equation

$$D=D''+D':$$

understanding that D'' shall operate exclusively on u, and D exclusively on v. The symbol D being thus absolutely equivalent to $D''+D'$, we are entitled to operate on uv with the symbol $(D''+D')^{-n}$, or its expansion as given above, for the purpose of effecting an n-fold integration. Thus we obtain the complete formula of which we are in search:

$$\int^n vudx^n=vu_n-n\frac{dv}{dx}u_{n+1}+\frac{n(n+1)}{1.2}\frac{d^2v}{dx^2}u_{n+2}-\&c.\ldots$$

$$+(-1)^{m-1}A_n\frac{d^{m-1}v}{dx^{m-1}}u_{m+n-1}+(-1)^m\Big\{A_n\int\frac{d^mv}{dx^m}u_{m+n-1}dx$$

$$+A_{n-1}\int^2\frac{d^mv}{dx^m}u_{m+n-2}dx^2+\ldots\ldots+\frac{m(m+1)}{1.2}$$

$$\int^{n-2}\frac{d^mv}{dx^m}u_{m+2}dx^{n-2}+m\int^{n-1}\frac{d^mv}{dx^m}u_{m+1}dx^{n-1}$$

$$+\int^n\frac{d^mv}{dx^m}u_m\,dx^n\Big\},$$

u_r being used for brevity instead of $\int^r udx^r$.

The series of supplemental integrals here given differs from that exhibited by Professor Young only as regards the signs of its terms, which by an oversight he has made alternately positive and negative.

The reasoning remaining the same, it is enough merely to indicate the corresponding mode of obtaining the general formula for the finite integration of a product. Sir John Herschel has given this formula in his excellent article on differences and series, appended to the Cambridge translation of Lacroix's treatise on the Differential and Integral Calculus: and it is to be observed that he has taken care to supply those supplementary integrals which are necessary to its correctness.

Since

$$\Delta(u_xv_x)=u_x\Delta v_x+v_{x+1}\Delta u_x=u_x\Delta v_x+e^{D}v_x.\Delta u_x,$$

we have the symbolical equation

$$\Delta=\Delta''+e^{D}\Delta';$$

it being understood that Δ'' and e^{D} operate only on v_x, and Δ' only upon u_x. And if we put Δ'' and $e^{D}\Delta'$ in place of D'' and D' in the formula given above for $(D'+D'')^{-n}$, we shall at once obtain Sir John Herschel's formula.

The examples here discussed by the Calculus of Operations are certainly instructive. Whilst they manifest the danger, noticed by Professor Young, of substituting symbols of operation for those of quantity in divergent infinite series, they indicate that, wherever we know how to express in a finite form the value of the remainder after any given number of terms of an infinite series, there is a safe way of effecting such a substitution. It must be made in the expression for the *remainder*, as well as in the terms of the series.

Dublin, November 30, 1848.

VIII. *Proceedings of Learned Societies.*

ROYAL ASTRONOMICAL SOCIETY.

[Continued from vol. xxxiii. p. 480.]

June 9, 1848. NOTICE of the principal English Observatories. Extracted from official or direct sources.

Two British observatories only are, properly speaking, public,—those of Greenwich and Edinburgh. The Observatory of Oxford was built and is supported by a bequest under Dr. Radcliffe's will: it is under the control of private trustees. The Cambridge Observatory was erected principally by private subscription, and is supported in part by the funds of a professorship founded by Dr. Plume, but mainly out of the University chest. The Radcliffe observer makes an annual report to his trustees; and Visitors appointed by the Cambridge Senate draw up yearly a statement to be laid before that body. Since the appointment of Mr. Airy to the Royal Observatory, a minute report of all proceedings and changes in that establishment is read to the Board of Visitors at the Annual Visitation in June.

Greenwich.

It has been mentioned in former numbers that the meridional instruments at Greenwich are deficient in optical power, and that the Astronomer Royal proposed to replace the mural circle and transit by a single instrument, viz. a transit circle, which is to be erected on the site of the present circle-room. In his report to the Visitors, Mr. Airy says,—

"An object-glass of eight inches clear aperture and eleven feet six inches focal length having been placed in my hands by Mr. Simms, I carefully examined it. I found that it showed some objects not of the closest class (as ε Boötis and ζ Cancri) better, I think, than I had seen them before; that it separated η Coronæ; that it did not separate γ Coronæ (which, having witnessed the difficulty of that star in the great Pulkowa refractor, I was prepared to expect); and that it dispersed light no more than the best object-glasses usually do. At my recommendation, therefore, this object-glass was purchased by the Lords Commissioners of the Admiralty, at the price of 275*l.* I have now to explain the form in which I propose to mount it. No verbal description, probably, can dispense with reference to the model*, and I will therefore confine myself to the leading points. I propose to mount it as a transit circle, its Ys bearing solidly on the piers far from their edges, and having no adjustments; the axis carrying two nearly similar circles on the east and west arms, one for clamping, the other for the divisions. I propose that the clamps

* A small model of the proposed transit circle was exhibited at the Visitation in June; and the Astronomer Royal, having obtained the consent of the government, is proceeding with the construction of the instrument. A full-sized model has been made and approved. The circle-room is rebuilding.

have no tangent-screw, the bisection being in all cases effected by the micrometer in the field of view of the telescope. I propose that the divisions be illuminated by a single lamp in the prolongation of the axis, without reflectors; and that the microscopes be in a conical surface, passing through one pier, the eye-ends being in a circle of two feet diameter; and that the divisions be cut upon a limb of metal which is so bevelled on the circle that the light of the lamp will be reflected up the microscopes. Several microscopes to be permanently mounted, in positions proper for ascertaining with the utmost exactness the errors of division. Microscopes to be mounted for ascertaining the laws of movement of points on the ends of the pivots. The instrument never to be reversed; but an apparatus to be provided for raising it so far that a collimating telescope firmly fixed on a solid pier on the north side, and one on the south side, can be adjusted on each other; then when the instrument is dropped into its usual place, the error of collimation and the flexure will be determined without reversion, by observation of the two collimators. No spirit-level or equivalent instrument to be used, but the error of level to be determined by observation of the image of the wires by reflexion in a trough of mercury. A parallel-motion apparatus to be used for carrying the trough, and a peculiar arrangement for facilitating the process of cleaning the mercury. In regard to the material, I propose that the whole be made of cast-iron, the axis being in two parts (which enables the founder to make the pivots of hard chilled iron, while the rest is of soft iron), each end of the telescope being in one part, and each of the two circles being cast in one piece. An instrument thus constructed would probably be more accurate for right ascensions than the present transit, in so far as the frequent observation of the well-mounted collimators would add to the knowledge of its azimuthal error; and perhaps more accurate for zenith distances than Troughton's circle, in so far as the circle is in a state of less strain, while its construction possesses greater firmness. But the reasons for recommending it, as is known to the Visitors, are the power of carrying a larger object-glass, and the enabling one observer to complete the observation of the two elements."

The observations in polar distance were made with Jones's Cape circle, until Troughton's circle was erected in another apartment, where it is and will be used until the circle-room is rebuilt and furnished. The transit will not be disturbed till the new instrument is at work.

The zenith tube has been taken down. It was proposed by some of the Visitors that it should be erected in another and less objectionable position than that which it formerly occupied, and a new site was pointed out by the Astronomer Royal. Mr. Airy, however, greatly prefers a different construction, if a continuous series of observations of γ Draconis be required. The principle of this construction, which is singularly simple, is thus described by Mr. Airy:—

"Let the micrometer be placed close to the object-glass, the frame of the micrometer being firmly connected with the object-glass cell, and a reflecting eye-piece being used with no material tube passing over the object-glass; and let a basin of quicksilver be placed below the object-glass, but in no mechanical connexion with it, at a distance rather greater than half the focal length of the object-glass, so that an image of the star is formed on the wires after the rays are reflected from the mercury. Such an instrument would at least be free from all uncertainties of twist of plumb-line, viscosity of water, attachment of upper plumb-line microscope, attachment of lower plumb-line microscope, and the observations connected with them; and might be expected, as a result of this extreme simplicity, to give accurate results."

The Astronomer Royal was recommended by the Board of Visitors to take the necessary steps for procuring a zenith instrument on the principle described, and he has already printed and distributed an account of the construction which he proposes to adopt, with explanatory drawings. There seems scarcely any limit to the power and probable accuracy of such a zenith tube; and as the mounting is exceedingly cheap and simple, it will most likely come into general use, especially for nice determinations of latitude.

It will be remembered that an altitude and azimuth instrument, made after Mr. Airy's designs by Messrs. Ransome and May, and Messrs. Troughton and Simms, has recently been added to the Royal Observatory, for the express purpose of observing the moon. Mr. Airy says:—

"The altitude and azimuth instrument having now been in use for an entire year, I am able to give some account of its success or failure as a mechanical arrangement. The first subject for remark is the steadiness of support of the upper pivot, which is held in its place, as the Visitors will remember, by a frame of bars whose arrangement in every part is triangular. The steadiness is perfect. In the first observations, the levels were read before and after the telescopic observation, but it was very soon found that this caution was entirely unnecessary. The next point is the steadiness in the position of the horizontal axis of the vertical circle relatively to the vertical revolving-frame, and generally the steadiness of the constants of instrumental errors. For some time the constants were so unsteady as to give me great trouble. Several observations of stars were absolutely rejected. In the month of July, after careful consideration of the discordances, I came to the conclusion that there was a wandering of the horizontal pivots in their Ys, caused probably by the counterpoises: the counterpoises were therefore diminished by one-third part, and since that time the constants of instrumental error have been steady, and not a single observation has been rejected. The next circumstance which gave me trouble was the uncertainty in the scale-values of some of the long levels. The singular good fortune of having four parallel levels upon the instrument, which are always read, enabled me to compare the proportion of the scale-values in actual use to the scale-values determined before mounting. These were very discordant. I became at length persuaded that this was caused by the very defective construction usually adopted in the mounting of English levels; and in the autumn I applied to the two longest levels the construction with which I had become familiar in Germany and Russia, in which the glass tube of the level is supported in Ys: since that time the levels have been fairly accordant. Another contrivance extensively applied to German levels, namely the covering by glass shades, has also been applied here. A difficulty which can be surmounted only by constant care has sometimes presented itself, namely, that when the dome is opened very shortly before observation, the changes of readings of the upper and lower levels do not exactly correspond. Lastly, when the best values of instrumental errors of every kind are applied, the accuracy of every part of observation, of calculation and application of instrumental errors, and of tabular calculation, is checked by the determinations of the zero of azimuth. These determinations are sufficiently steady in any one evening, or perhaps in groups of several evenings; but they are not steady from time to time, the variation amounting to three or four seconds of arc. Whether this arises from a twist of the brick pier, or from a twist of the piers of the transit instrument (the times being obtained from the transit-clock), or from a change in the observer's personal equation, I cannot tell. The substitution of improved meridional instruments for those now in use will enable me to remove one of these conjectural causes.

"I have spoken hitherto solely with respect to the azimuthal observations, in which alone, from the first, I anticipated any difficulty. The zenith distance observations have never given the smallest trouble.

"The accuracy of the results, as estimated by the observation of stars, is somewhat less than that of the mural circle, perhaps nearly in the degree which might be expected in circles whose diameter is half that of the mural circle.

"For observations with the altitude and azimuth instrument, the following rule is laid down. The moon is to be observed if visible, and the observer is bound to watch if necessary while the moon is above the horizon, and the sun is not more than an hour above the horizon. One azimuth and one altitude are to be observed, and if possible, two azimuths and two altitudes in reversed positions of the instrument: and if the night is fine, a low star and a high star are to be observed in azimuth, both in reversed positions of the instrument, and one star in altitude, in reversed positions. Thus a complete set includes ten observations. These rules have been followed carefully during the thirteen lunations intervening between 1847, May 15, and 1848, May 30; and I am now able to give a comparison of the number of days of observation of the moon with the altitude and azimuth instrument and with the meridional instruments. With the altitude and azimuth instrument no days are included except both altitudes and azimuths are observed; and with the meridional instruments, no days except both right ascensions and polar distances are observed."

The number of days during the last year in which the moon has been observed at Greenwich are—

With the altitude and azimuth circle =203
... meridional instruments ... =111

This statement gives only an imperfect idea of the value of the instrument. When the moon passes from one to four hours before or after the sun, there are thirty-four observations with the altitude and azimuth, and not one with the transit and circle*. It is not necessary to point out the immense utility of these results in the lunar theory, or to geography and navigation, which depend on lunar observations for their fundamental determinations†. The results of the observations, as reduced to the state of apparent errors of tables in R.A. and N.P.D., appear very good; perhaps a little, and but a little, inferior to those of the meridional instruments.

Throughout the year 1847, the new form of star-reduction proposed by Mr. Airy as a substitute for Bessel's (see M. Notices, vol. vii. p. 189) has been used, and it has been found convenient. At present the assistants are employed in collecting all the star-observations in 1842–47, for the purpose of forming another grand catalogue reduced to the epoch 1845. The Astronomer Royal proposes to give in this catalogue the star-constants e, f, g, h, l; e', f', g', h', l', and also, for a few years, the day-constants E, F, G, H, L, which are required by his method.

The reduction of Fallow's Cape Observations was commenced some time ago under the direction of the Astronomer Royal. This was interrupted by the work incident to the completion of the lunar reductions, but it will be resumed in a short time.

The ledgers of star-observations and occasional star-catalogues found in Maskelyne's observations have been fairly written out. Mr. Airy submitted to the Visitors the propriety of printing these reductions, and also suggested whether it might not be advisable to take some steps of calculation with respect to Bradley's observations anterior to 1750.

In June last, the printing of the volume for 1846 was nearly finished, and the volume for 1847 was commenced.

Edinburgh.

Professor C. P. Smyth has been hitherto engaged in reducing and editing the observations of his lamented predecessor, and in examining and repairing the defects of his instruments and observatory. The meridian buildings are reported to be now in perfect order, and the instruments in a satisfactory state. It will be remembered that Professor Smyth has undertaken to determine the places of stars compared with the small planets and comets in extra-meridian observations, and when he is fully prepared (of which due notice will be given), it will be desirable that he should have early information of

* The working of this instrument is considered to absorb one assistant and one additional computer.

† When the lunar tables are made to satisfy the places of the moon given in the 'Reduction of the Greenwich Lunar Observations,' and are further corrected by observations made with the azimuth and altitude instrument, well-observed moon-culminations will not require *corresponding observations*, and occultations will yield trustworthy results *wherever* they may be observed.

the approximate places of those stars, on which accurate and important comparisons depend.

The Astronomer Royal has communicated, and doubtless will continue to communicate, the mean right ascensions of the stars employed at Greenwich, so that the Edinburgh places will harmonize with those of Greenwich; or, as Professor Smyth remarks, "it will not work against but co-operate with Greenwich."

There seems reason to complain of the smoke of the city, but probably this will not very materially injure the great mass of observations. The transit has a noble object-glass $6\frac{1}{2}$ inches aperture, and the Professor proposes to use the circle with illuminated wires on a dark field.

The instability of the Edinburgh transit was suspected by Professor Henderson to arise from the effect of temperature on the foundation. Professor Smyth has traced it to a much simpler cause, a defective original construction of the Ys. He thus describes the construction of the new Ys:—

"They are large slabs of cast-iron, covering the whole area of the top of the pier, and weighing several hundredweight; there are no adjusting-screws, but the sides of the angles in which the pivots rest have been filed away, until the instrument is made to move as nearly in the plane of the meridian as could, perhaps, have been managed with screws. One good result has been certainly proved to have followed, viz. that the reversing of the instrument to obtain the error of collimation does *not* now sensibly throw it out in azimuth, which Professor Henderson used to complain of with the old Ys. Touching the fears that the new Ys might split the stone piers, and the hopes that they might correct the temperature fluctuations, there has not been sufficient time yet to settle that question through the medium of the large transit; it may, however, be considered to be pretty well set at rest by the experiments on the 30-inch transit. This was mounted on a similar huge block of cast-iron screwed and cemented down to its pier; on this it has now been sticking for a year, as firmly but as innocently as could be desired.

"The following is a list of azimuth errors of the 30-inch transit in its cast-iron block, as determined by *all* the transits of α Ursæ Minoris, observed on all five wires, during the period which elapsed from the final filing of the Ys to a snapping of two of the wires, which took place, it was supposed, from moisture: the clock-star used on each occasion was θ' Ceti:—

			s
1847.	Nov.	1	−0·091
		2	−0·003
		15	−0·058
		16	−0·040
		30	+0·043
	Dec.	14	+0·006
1848.	Jan.	7	+0·020
		13	−0·024

"Now these apparent fluctuations of the Ys in azimuth, which are very small, include the probable error with which each observation may be affected, by reason of the small optical power of the telescope and other matters inseparable from an inferior instrument. Hence they may be considered to be quite insignificant: taking them, however, as they are, and comparing them with the azimuth errors of the large transit in the corresponding period of the years 1841–42, the fluctuations of the large instrument turn out to be *five* times as great as those of the small one; a convincing proof that the cause of the changes hitherto remarked is not in the 'hill on which the observatory is built.' The above list of errors in azimuth may also convince observers that they may themselves rub down unadjustable Ys to limits which will be abundantly within easy calculation (and with transits, too, without micrometer wires).

"The Ys of the large transit having been erected, every screw about the instrument was tried, to make sure that it was doing its duty: a number of the smaller ones (which seemed to be made of brass *wire*—drawn brass, not cast brass) were found quite rotten; these were replaced, and a good many new ones introduced about the sliding tubes at the eye end; handles for moving the instrument, and acting *only in the plane of the meridian,* were added; and then, as the line of soldering of the telescope was beginning to show symptoms of oxidation, the instrument was painted. A nadir-pier and mercury-trough have been established, and a collimating eye-piece of peculiar construction, which for perfect vision seems to leave little to be desired, and reveals almost every affection of the instrument. On account of some of its revelations, the fixed wires have been removed, and five in their place mounted on the micrometer-frame. I propose to examine the errors of collimation and level every night, before and after the observations, as shall be found necessary, and am now engaged in trying to cure the reversing-carriage of a trick it has got of throwing the instrument to the west during reversal. Collimating lenses, of the full aperture of the object-glass, for marks on the boundary wall, are also being put up, as the old semi-collimating semi-meridian marks are now seldom seen, on account of the increased smoke of the city; and when they are, the 6·5-inch aperture of the transit must be reduced to 2 inches, and the eye-piece pulled out so far as to make the wires very indistinct and unsteady."

Oxford.

The Radcliffe observer has lately published his seventh annual volume. This consists, like the preceding volumes, chiefly of observations of circumpolar stars contained in Groombridge's Catalogue.

It is not necessary to dwell upon the merits of Groombridge's Catalogue, one of the most laborious tasks ever undertaken by an amateur, as well as one of the most useful. His transit circle, though perhaps rather weak as a right ascension instrument, was at the time of its construction, and for many years after, the most perfect instrument in existence for determinations in north polar distance. On this account, and considering that the time elapsed since Groombridge made his observations is sufficient to detect and exhibit proper motions, Mr. Johnson undertook to re-observe the Catalogue with great care, and has now nearly completed the task.

The north polar distances have almost all been redetermined, and a large portion of the right ascensions; there are, however, several gaps, occasioned by the necessity of observing certain circumpolar stars for meridian error, and fundamental stars for clock error. The increased number of well-determined stars will now allow the observer more liberty in this respect, and the blanks are rapidly filling up.

Besides the general advantage of a full standard catalogue of stars within 50° of the north pole (which by the aid of Groombridge's determinations may be carried forwards for some years), and the materials thus afforded for investigating precession, proper motion, &c., a special advantage will be found in geodesical operations from this large supply of accurate places for the zenith sector*.

Mr. Johnson expects very soon to receive the heliometer by Repsold, when his attention will be directed to another department of practical astronomy. Thus limited in time, and having the aid of only one assistant, he has been induced to confine himself in most cases to *two* observations of a star in the same year, and occasionally to *one*. The star has, however, been observed in *different* years, so that there is a considerable check on errors of computation and on casual fluctuations in the instruments.

Care has generally been taken to note circumstances of interest connected with stars, which have come under observation. Among others, their magnitudes have been watched with much attention. The method adopted has been simply to estimate the apparent magnitudes in reference to *ideal* standards; and pending the discovery of some more accurate photometric measure, Mr. Johnson has instituted an inquiry as to the degree of reliance which may be placed on the method he has pursued. This inquiry has not yet been fully followed out; but the results, as far as they go, are given in the preface to the present volume. From what is there said, it may be inferred that Mr. Johnson would recommend that at every observation of a star, not distinctly visible to the naked eye, an estimate of its magnitude should be noted, unless there is some obvious impediment to a correct determination; and that a mean of such estimates should be taken as the magnitude of any given star, just as the mean of a number of observations in right ascension or north polar distance is considered as the correct right ascension or north polar distance.

Mr. Johnson acknowledges the great services which he has hi-

* It is proper to remind observers who possess instruments not of the highest class, or who cannot afford the time for deducing fundamental places, that the partial catalogues in the Radcliffe Observations will supply them abundantly with zone stars, from the pole to 50° of north polar distance, and that the volumes of the Edinburgh Observations will afford a sufficient number of similar stars for a complete zodiacal catalogue. With these and the Greenwich catalogues there can be no want of a sound base of operations. Professor Argelander has made excellent use of the Radcliffe Observations in his admirable Zone Observations.

therto received in the voluntary revision of his work, first from Mr. Harris, our late assistant-secretary, and latterly from Mr. William Luff, of Oxford. Mr. Luff has most kindly undertaken to read the proof-sheets and to revise the additions*; and from the great care employed, it is hoped few typographical errors escape. When any errors are detected, Mr. Johnson hopes that they will be communicated to him.

The entire expense of printing these observations is borne by the Radcliffe trustees. The beautiful typography, and the convenient size of the volumes, enhance their value, and it is gratefully acknowledged that the trustees distribute them liberally and judiciously.

Cambridge.

The Syndicate appointed to visit the Cambridge Observatory made a report to the Senate, of which the following is the substance:—

The total number of observations in 1847 were,—

With the transit		2540
...	circle	2285
...	Northumberland equatoreal	1400

The observations with the meridional instruments are chiefly of the sun (of which there is a very extensive series), moon, Jupiter, Saturn, Uranus and Neptune, with a good series of Astræa, Flora, and Iris. About 300 stars have been also observed.

The equatoreal observations are for the most part of the minor planets and comets, which could not be seen on the meridian. These are Neptune, Astræa, Hebe, Iris, Flora, and the following comets:— Hind's, Feb. 6; Mauvais', 3rd; Miss Mitchell's; Colla's.

Professor Challis finds himself so much oppressed with unreduced and unpublished observations, that he has discontinued observations of the sun, moon, and the older planets since the beginning of this year†. The recently-discovered planets are observed on the meridian and with the Northumberland equatoreal, and the results com-

* "The process of revision is as follows:—Mr. Luff receives the proof sheet as soon as it comes from the printer. He goes over all the additions, without having the copy by him; he notes all the mistakes he finds; then the proof is collated with the copy, and it is seen which are the mistakes of the printer and which of the copy. All being corrected, the proof is returned to the printer. The *revise* is carefully read over again, and no sheet is marked for *press* till it is clear of mistakes."

† It is perhaps proper to inform those who are not acquainted with the University of Cambridge, that Professor Challis gives lectures during one term on physics, and that he is largely engaged in the university examinations. His duties as lecturer and examiner *must* be attended to in the *first* place, whatever the observatory business may be. The university cannot afford to give such a salary as will secure persons competent to carry on the computations without constant superintendence; and when an assistant has obtained the necessary acquirements, he is naturally and properly on the look-out for a better place. It is not generally known how much mere heavy labour has been actually performed by the late and present professor.

municated to the Royal Astronomical Society and to foreign astronomers.

The meridional observations of 1847 are completely reduced. The equatoreal observations are less forward.

The volume for 1843 is nearly ready for publication. It does not contain the equatoreal observations, which are reserved for separate publication. Two appendices are added; one containing so many of the observations made in search of the planet Neptune as are required to substantiate the statements given in the special report of Dec. 12, 1846; and the other a description of the Northumberland telescope and dome, drawn up by the Astronomer Royal.

Liverpool.

A very fine equatoreal has recently been erected at the Liverpool observatory. The general form of the instrument has been mentioned in former Notices; and it promises, so far as we have heard, to be the most accurate and most convenient instrument of its size now existing. The object-glass is by Merz of Munich, of eight inches aperture; and as it has been approved of by Messrs. Dawes and Lassell, most capable and somewhat fastidious judges, there can be no doubt of its superior excellence. In firmness and steadiness the equatoreal is reported to resemble a meridian instrument. The hour-circle is carried, as in the Northumberland telescope, by clock-work, and the right ascension is read off at once by the verniers. The Astronomer Royal, under whose direction the instrument was constructed, has given a perpetual motion to this hour-circle by clock-work moved by a water-wheel, to which a regulator is applied. The variation of the clock does not exceed 1^s per hour. The declination and hour-circles are sufficiently good to give excellent results when objects are compared beyond the limits of the micrometer, an immense advantage when time is wanting and the weather is uncertain, and in all cases a great comfort, as it secures perfect identification.

We do not know certainly what line of astronomical research Mr. Hartnup will take up. He will do most wisely to follow his own inclination; but such an instrument would be very well employed in observing the planets, for instance, especially the smaller planets, when they cannot be observed on the meridian at Greenwich. This would not only complete the series of the Greenwich observations, but would greatly relieve the Cambridge observatory, on which this branch of observing has of late pressed heavily*.

* It is desirable that a semi-public observatory like Liverpool should take a determinate line. We have every reason to admire the zeal and steadiness of our amateur observers, many of whom might be cited as models in these respects; but they ought not to be tied down to a strictness and continuity of research which must often be inconvenient and sometimes impossible.

ROYAL SOCIETY.

[Continued from vol. xxxiii. p. 551.]

Anniversary Meeting, November 30, 1848.

The Marquis of Northampton, President, in the Chair.

The President, after returning thanks to the Royal Society for the honour conferred on him for ten years, delivered the Medals with the following words:—

Mr. Galloway,

I deliver this Royal Medal to you with great satisfaction, for your communication on one of the most interesting and difficult problems in Astronomy, the proper motion in space of our system; speculations which may almost seem too mighty and daring for the human intellect.

One who, like yourself, has entered on such a path of discovery, is not likely to turn from it. In further pursuing it, I feel assured that your zeal for the prosperity of the Royal Society will induce you to enrich our Transactions with other communications. Should my hopes prove well-founded, though my successor will, from his own pursuits, be much better able than myself to appreciate your labours, he will not be able to hail them with greater pleasure than myself.

Mr. Hargreave,

I am glad to deliver into your hands this Royal Medal for the mathematical paper with which you have enabled the Council to adorn the Philosophical Transactions.

It is a paper, from its nature indeed, more suited for the attentive study of the closet, than for reading before an audience, however scientific, but it is not on that account less valuable.

Mathematical analysis is doubly important: important in itself, and important as one of the great instruments of philosophical investigation. Every extension of it must then be at all times most highly welcome to a Society founded for the advancement of natural knowledge, and I, therefore, in its name, tender its thanks and an expression of the hope that it will not be the last communication that we shall receive at your hands.

Mr. Adams,

It is a great pleasure to me to be the channel by which the Council of the Royal Society gives you this Copley Medal.

In their award, I am sure that they have not done more than justice to the scientific zeal, industry, and skill exerted by you in the search of the great and distant body that caused the perturbations of the planet Uranus, a search crowned with success, both in your case and in that of your illustrious friend Le Verrier.

If he be an honour to his nation, not the less so are you to England; if he is a worthy follower of La Place, not less so are you of Newton. His name and yours will remain imperishably united in the annals of the glorious science which you both cultivate with so much zeal and so much success.

Lieut.-Col. SABINE,

I have to request of you, when transmitting to M. Regnault this Rumford Medal, to state to him the importance which the Royal Society attaches to his researches, determining with a degree of accuracy hitherto unobtained, the laws which govern the connexion between the temperature and elasticity of saturated steam, and the quantity of heat absorbed by a given weight of water under different densities and pressures.

The laws which govern the expansion of atmospheric air, under different pressures, and the expansion and densities of different gases and mercury, and the measurement of temperatures by these means, form in a series of memoirs altogether the most important investigations hitherto made on this subject.

Had the philosophical and philanthropical founder of this Medal been now living, I am sure that he would have cordially approved of the award of it to inquiries connected with the most important power that Providence has, as yet, given to man for lightening and assisting his industry, and for giving him speed for crossing sea and land, compared with which, the fabled wings of Dædalus would have been comparatively useless. My only regret on the present occasion is, that M. Regnault is not here himself to receive this Medal.

The Statutes relating to the election of Council and Officers having been read by the Secretary, and Dr. Royle and Mr. Bennett having, with the consent of the Society, been nominated Scrutators to assist the Secretaries in examining the lists, the votes of the Fellows present were collected.

Mr. Bennett reported the following Noblemen and Gentlemen as being duly elected Officers and Council for the ensuing year:—

President.—The Earl of Rosse.
Treasurer.—George Rennie, Esq.
Secretaries. { S. Hunter Christie, Esq. / Thomas Bell, Esq.
Foreign Secretary.—Lieut.-Col. Edward Sabine, R.A.

Other Members of the Council.—George Biddell Airy, Esq., M.A.; Sir James Clark, Bart., M.D.; John P. Gassiot, Esq.; Thomas Graham, Esq., M.A.; William Robert Grove, Esq., M.A.; Leonard Horner, Esq.; Sir Robert H. Inglis, Bart., LL.D.; John George Shaw Lefevre, Esq., M.A.; Sir Charles Lyell, M.A.; William Allen Miller, M.D.; The Marquis of Northampton; Richard Owen, Esq.; John Phillips, Esq.; Peter Mark Roget, M.D.; the Dean of Westminster; Charles Wheatstone, Esq.

It was moved by Sir Robert Harry Inglis, Bart., seconded by Mr. Broughton, and resolved unanimously:—

That on this the last occasion of the Marquis of Northampton occupying the Chair of the Royal Society as its President, the special thanks of the Society be cordially tendered to his Lordship, for his able, zealous, and efficient discharge of the duties of that office for ten years.

On the motion of Dr. Paris, seconded by Professor Baden Powell, it was resolved unanimously :—

That the best thanks of the Royal Society be, and they are hereby given, to Dr. Roget for his continued and valuable services during a period of twenty-one years, in the office of Secretary to the Society.

Dec. 7.—" Experimental Researches in Electricity." By Michael Faraday, Esq., F.R.S. Twenty-second Series. § 28. On the Crystalline Polarity of Bismuth and other bodies, and on its relation to the magnetic form of force.

The author states that in preparing small cylinders of bismuth by casting them in glass tubes, he had often been embarrassed by the anomalous magnetic results which they gave, and that having determined to investigate the matter closely, it ended in a reference of the effects to the crystalline condition of the bismuth, which may be thus briefly stated. If bismuth be crystallized in the ordinary way, and then a crystal, or a group of symmetric crystals, be selected and suspended in the magnetic field between horizontal poles, it immediately either points in a given direction, or vibrates about a given position, as a small magnetic needle would do, and if disturbed from this position it returns to it. On resuspending the crystal so that the horizontal line which is transverse to the magnetic axis shall become the vertical line, the crystal then points with its maximum degree of force. If it be again resuspended so that the line parallel to the magnetic axis be rendered vertical, the crystal loses all directive force. This line of direction therefore, which tends to place itself parallel to the magnetic axis, the author calls the *Magnecrystallic axis* of the crystal. It is perpendicular, or nearly so, to the brightest and most perfect of the four cleavage planes of the crystal. It is the same for all crystals of bismuth. Whether this magnecrystallic axis is parallel or transverse to the magnetic axis, the bismuth is in both cases repelled from a single, or the stronger, pole; its diamagnetic relations being in no way affected. If the crystal be broken up, or if it be fused and resolidified, and the metal then subjected to the action of the magnet, the diamagnetic phenomena remain, but the magnecrystallic results disappear, because of the confused and opposing crystalline condition of the various parts. If an ingot of bismuth be broken up and fragmentary plates selected which are crystallized uniformly throughout, these also point; the magnecrystallic axis being, as before, perpendicular to the chief plane of cleavage, and the external form, in this respect, of no consequence.

The effect takes place when the crystal is surrounded by masses of bismuth, or when it is immersed in water, or solution of sulphate of iron, and with as much force, apparently, as if nothing intervened.

The position of the crystal in the magnetic field is affected by the approximation of extra magnets or of soft iron; but the author does not believe that this results from any attractive or repulsive force exerted on the bismuth, but only from the disturbance of the lines of force or resultants of magnetic action, by which they acquire as it were new directions; and, as the law of action which he gives, is, that *the line or axis of magnecrystallic force tends to place itself parallel,* *or as a tangent, to the magnetic curve or line of magnetic force, passing through the place where the crystal is situated,* so the crystal changes its position with any change of direction in these lines.

A common horse-shoe magnet exhibits these phenomena very well: the author worked much with one lifting 30lbs. by the keeper; but one that can raise a pound or two only, is sufficient for many of the actions. When using the electro-magnet, the advantage of employing poles with large plane opposed faces is mentioned as being considerable, for then diamagnetic phenomena are almost or entirely avoided and the peculiar magnecrystallic relations then appear.

The peculiar force exerted in these phenomena is not either attractive or repulsive, but has for its distinctive character the tendency to place the crystal in a definite position or direction. The author further distinguishes it from that described by M. Plücker in his interesting memoir upon the repulsion of the optic axes of crystals by the poles of a magnet*, in that, *that* is an equatorial force, whereas this is an axial force.

Crystals of *antimony* were then submitted to a similar magnetic examination, and with the same results. But there were also certain other effects produced of arrest and revulsion, the same in kind as those described in a former series of the 'Experimental Researches' (par. 2309, &c.); these are wrought out and eliminated, and the results described.

Arsenic also proved to be a body capable of pointing in the magnetic field, like bismuth and antimony.

The paper describing the foregoing results is dated 23rd of September, 1848. In a later paper of the date of 20th October, 1848, the author continues his researches. Native crystals of iridium and osmium, and also crystallized titanium and tellurium, appeared to be magnecrystallic: crystals of zinc, copper, tin, lead, gold, gave no signs of this condition. Crystals of sulphate of iron are very strongly affected by the magnet according to this new condition, and the magnecrystallic axis is perpendicular to two of the planes of the rhomboidal prism; so that when a long crystal is employed, it will not, as a mass, point between the poles, but across the line joining them. On the other hand, the sulphate of nickel has its magnecrystallic axis parallel, or nearly so, to the length of the ordinary prism. Hence bodies, both magnetic and diamagnetic, are, by their crystalline condition, subject to the magnetic force, according to the law already laid down. Diamond, rock-salt, fluor spar, boracite, red oxide of copper, oxide of tin, cinnabar, galena, and many other bodies, presented no evidence of the magnecrystallic condition.

The author then enters upon a consideration *of the nature of the magnecrystallic force.* In the first place he examines closely whether a crystal of bismuth has exactly the same amount of repulsion, diamagnetic or other, when presenting its magnecrystallic axis *parallel* or *transverse* to the lines of magnetic force acting on it. For this purpose the crystal was suspended either from a torsion ba-

* Poggendorff's Annalen, lxxii. Oct. 1847; or Taylor's Scientific Memoirs, vol. v. p. 353.

lance, or as a pendulum thirty feet in length; but whatever the position of the magnecrystallic axis, the amount of repulsion was the same.

In other experiments a vertical axis was constructed of cocoon silk, and the body to be examined was attached at right angles to it as radius; a prismatic crystal of sulphate of iron, for instance, whose length was four times its breadth, was fixed on the axis with its length as radius and its magnecrystallic axis horizontal, and therefore as tangent; then, when this crystal was at rest under the torsion force of the silken axis, an electro-magnetic pole was so placed, that the axial line of magnetic force should be, when exerted, oblique to both the length and the magnecrystallic axis of the crystal; and the consequence was, that, when the electric current circulated round the magnet, the crystal actually *receded* from the magnet under the influence of the force, which tended to place the magnecrystallic axis and the magnetic axis parallel. Employing a crystal or plate of bismuth, that body could be made to *approach* the magnetic pole under the influence of the magnecrystallic force; and this force is so strong as to counteract either the tendency of the magnetic body to approach or of the diamagnetic body to retreat, when it is exerted in the contrary direction. Hence the author concludes that it is neither attraction nor repulsion which causes the set or determines the final position of a magnecrystallic body.

He next considers it as a force dependent upon the crystalline condition of the body, and therefore associated with the original molecular forces of the matter. He shows experimentally, that, as the magnet can move a crystal, so also a crystal can move a magnet. Also, that heat takes away this power just before the crystal fuses, and that cooling restores it in its original direction. He next considers whether the effects are due to a force altogether original and inherent in the crystal, or whether that which appears in it, is not partly induced by the magnetic and electric forces; and he concludes, that the force manifested in the magnetic field, which appears by external actions and causes the motion of the mass, is chiefly, and almost entirely *induced*, in a manner subject indeed to the crystalline force and additive to it; but at the same time exalting the force and the effects to a degree which they could not have approached without the induction. To this part of the force he applies the word *magneto-crystallic*, in contradistinction to the word magnecrystallic, which is employed to express the condition, or quality, or power, which belongs essentially to the crystal.

The author then remarks upon the extraordinary character of the power, which he cannot refer to polarity; and gives expression to certain considerations and views which will be best learned from the paper itself. After this, he resumes the consideration of Plücker's results "*upon the repulsion of the optic axes of crystals*" already referred to, and arrives at the conclusion that his results and those now described have one common origin and cause. He then considers Plücker's results in relation to those which he formerly obtained with heavy optical glass and many other bodies. In conclusion he remarks, "How rapidly the knowledge of molecular forces grows upon us, and how strikingly every investigation tends to develope more and more their importance and their extreme attraction as an object of study! 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 in our labours, encouraged by the hope of bringing it into a bond of union with gravity itself."

IX. *Intelligence and Miscellaneous Articles.*

ON A NEW MODIFICATION OF PHOSPHORUS.

M. SCHROETTER has stated, that the red substance which forms on the surface of phosphorus exposed to the light, is entirely an isomeric formation of phosphorus. It takes place in various gases, such as hydrogen, azote and carbonic acid, when the phosphorus is perfectly dry; it is therefore impossible to attribute this effect to the oxidizement of the phosphorus.

The transformation is rapid in direct light, but it is observable even in diffused feeble light. Heat effects the same change. When phosphorus which has been thoroughly dried is exposed for forty or sixty hours to a temperature of 464° to 482° F., a great part of it becomes of a carmine-red. A red opake powder is first detached, which is soon uniformly generated in every portion of the mass, and it eventually falls to the bottom of the vessel.

By operating on small quantities in close vessels, and by continuing the action in the mode described, M. Schroetter has succeeded in converting the whole of the phosphorus into the red modification.

In order to isolate the amorphous phosphorus prepared in rather large quantity, M. Schroetter employed sulphuret of carbon, which is an excellent solvent of common phosphorus, but dissolves amorphous phosphorus with difficulty; filtration is performed with peculiar precautions; the residue is afterwards boiled in a solution of potash of 1·3 density; it is then to be washed with pure water, afterwards with water acidified with nitric acid, and again with pure water; the phosphorus thus obtained is a powder varying in colour from scarlet to deep carmine-red.

Under peculiar circumstances a blackish-brown modification of phosphorus may be obtained. The density of amorphous phosphorus at 50° F. is 1·964.

Amorphous phosphorus is unalterable by exposure to the air, insoluble in æther, alcohol, naphtha, or chloride of phosphorus; oil of turpentine dissolves a little at a high temperature; it is much less combustible than common phosphorus, it gives out no light in the dark, and does not burn till exposed to 500° F. This is the temperature at which amorphous phosphorus begins to return to the state of common phosphorus when heated in an inert gas.

Amorphous phosphorus does not combine with sulphur at 233° F., it must be heated to 446°. Chlorine combines with it without the disengagement of light, a boiling solution of potash acts upon it, evolving non-inflammable phosphuretted hydrogen, and the phosphorus becomes the black modification, described by M. Thenard; and according to M. Schroetter, phosphorus never becomes the black modification without having previously assumed the red one.—*Comptes Rendus*, Octobre 1848.

METEOROLOGICAL OBSERVATIONS FOR NOV. 1848.

Chiswick.—November 1. Rain, with fog. 2. Fine: cloudy. 3. Overcast: cloudy and fine. 4. Overcast. 5. Clear and frosty: overcast. 6. Overcast. 7. Clear and cold: sharp frost at night. 8. Frosty: bright sun: clear and frosty. 9, 10. Clear: slight frost at nights. 11. Overcast. 12. Slight rain. 13, 14. Very fine. 15. Clear: severe frost at night. 16. Frosty: clear and fine. 17. Densely clouded: rain: peculiar luminosity in the evening: overcast. 18. Densely clouded. 19. Very fine. 20. Densely clouded: rain: boisterous. 21. Clear and fine: peculiar aurora borealis half-past seven P.M. in N.W. 22. Overcast. 23. Rain. 24. Cloudy: clear and frosty. 25. Frosty: overcast: slight rain. 26. Cloudy. 27. Very fine. 28. Cloudy. 29. Densely overcast: boisterous. 30. Clear: cloudy: partially overcast.

Mean temperature of the month .... 41°·01
Mean temperature of Nov. 1847 .... 44 ·61
Mean temperature of Nov. for the last twenty years ...... 43 ·00
Average amount of rain in Nov. .... 2·56 inches.

Boston.—Nov. 1. Foggy. 2. Fine. 3. Rain: rain A.M. and P.M. 4. Fine: rain early A.M. and snow P.M. 5. Fine: rain P.M. 6. Cloudy. 7, 8. Fine. 9. Cloudy: snow A.M. 10—14. Fine. 15. Cloudy. 16. Fine. 17. Cloudy: rain A.M. 18. Cloudy. 19. Fine. 20. Cloudy: rain in evening. 21. Fine. 22. Cloudy: rain A.M. 23. Cloudy. 24. Fine. 25. Fine: rain P.M. 26—28. Fine. 29. Fine: rain P.M. 30. Fine.

Applegarth Manse, Dumfries-shire.—Nov. 1. Dull A.M.: soft rain P.M. 2. Fine generally: flying showers. 3. Rain A.M.: cleared: looking frosty. 4. Frost hard: hills covered with snow. 5. Frost hard: sprinkling of snow. 6. Thaw: showers: stormy. 7. Frost: fine clear day. 8. Frost: clear: snow P.M. 9. Fine winter day: frost: snow inch deep. 10. Frost: clear: snow melting. 11. Frost: dull and cloudy: snow gone. 12. Fine: no frost A.M.: gentle frost P.M. 13. Frost A.M.: a change of weather. 14. Frost A.M.: thaw: frost again. 15. Frost A.M.: thaw P.M. 16. Drops of rain occasionally. 17. Rain during night: aurora very splendid. 18. Heavy rain during night: ditto day. 19. Frost A.M.: thaw P.M. 20. Storm of rain and wind: flood. 21. Bleak and dull all day. 22. Rain, greater part of day. 23. Fair A.M.: rain P.M. 24. Frost again. 25, 26. Thaw: rain and high wind. 27. Fair and fine. 28. Wet nearly all day: high wind. 29. Frequent showers. 30. Fair, but cloudy.

Mean temperature of the month .... 39°·8
Mean temperature of Nov. 1847 .... 47 ·7
Mean temperature of Nov. for the last twenty-five years . 40 ·4
Rain in Nov. 1847 .... 3·79 inches.
Average amount of rain in Nov. for twenty years .... 3·60 „

Sandwick Manse, Orkney.—Nov. 1. Drops: rain: aurora. 2. Showers: hail-showers. 3. Snow: hail-showers. 4. Snow: clear. 5. Showers. 6. Cloudy: showers. 7. Showers: sleet. 8. Snow-showers: clear: frost. 9. Cloudy: rain. 10. Cloudy: clear: aurora. 11. Bright: drizzle: showers. 12. Fine: clear. 13. Cloudy: showers. 14. Cloudy: hail-showers. 15. Cloudy: showers. 16. Bright: cloudy. 17. Showers: aurora. 18. Damp: showers: aurora. 19. Hoar-frost: showers. 20. Rain. 21. Rain: cloudy: aurora. 22. Rain. 23. Showers: aurora. 24. Cloudy: clear. 25, 26. Cloudy: rain. 27, 28. Showers. 29. Cloudy: showers. 30. Showers.

Meteorological Observations made by Mr. Thompson *at the Garden of the Horticultural Society at* Chiswick, *near London; by* Mr. Veall, *at* Boston; *by the* Rev. W. Dunbar, *at Applegarth Manse,* Dumfries-shire; *and by the* Rev. C. Clouston, *at Sandwick Manse,* Orkney.

Days of Month.	Barometer.							Thermometer.							Wind.				Rain.			
	Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Dumfries-shire.	Orkney, Sandwick.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.
1848. Nov.	Max.	Min.		9 a.m.	9 p.m.	9½ a.m.	8½ p.m.	Max.	Min.		Max.	Min.	9½ a.m.	8½ p.m.								
1.	29·602	29·505	29·15	29·48	29·48	29·49	29·50	49	32	45	46½	37	42	46½	e.	calm	wsw.	ne.	·03	......	......	·05
2.	29·765	29·697	29·10	29·58	29·51	29·63	29·69	54	33	44	48½	35	41	36	nw.	ssw.	wsw.	ne.	......	......	......	·30
3.	29·614	29·446	29·16	29·40	29·56	29·76	29·78	55	32	44	41	36	36	33	sw.	sw.	n.	n.	......	·07	......	·17
☽ 4.	29·737	29·496	29·16	29·60	29·66	29·72	29·72	39	23	37	36	28	32½	33	n.	nw.	nnw.	calm	......	·20	......	......
5.	29·808	29·589	29·43	29·48	29·38	29·20	29·33	47	39	31·5	45½	24	46	44	sw.	sw.	nw.	nw.	·01	·06	......	·24
6.	29·645	29·538	29·27	29·38	29·24	29·18	29·14	50	38	42·5	50	39	44	39	w.	w.	wnw.	nnw.	......	·03	......	·02
7.	29·879	29·518	29·14	29·40	29·70	29·49	29·81	46	22	37	45	32	40	39½	nw.	w.	nw.	nnw.	......	·04	......	·39
8.	30·084	29·983	29·12	29·94	29·99	29·97	30·15	45	22	32	40	29	33	36	nw.	nw.	nw.	nnw.	......	......	0·40	·17
9.	30·319	30·169	29·90	30·24	30·30	30·28	30·20	43	28	31·5	38	28½	40	45	nw.	nw.	n.	nw.	......	......	......	......
10.	30·357	30·306	30·02	30·28	30·40	30·37	30·41	44	30	34	39	25	44	42	n.	nw.	n.	nw.	·01	·10	......	·09
○ 11.	30·334	30·295	30·00	30·39	30·40	30·44	30·45	46	39	40	43½	31½	44½	45	ne.	nw.	nnw.	nw.	·01	......	......	·07
12.	30·419	30·357	30·04	30·46	30·46	30·48	30·45	46	35	43	47	39½	46	46½	ne.	n.	nne.	wnw.	·04	......	......	·02
13.	30·429	30·323	30·07	30·38	30·21	30·25	30·17	49	27	35	46	24	49	44	nw.	nnw.	ne.	nnw.	......	......	......	·01
14.	30·351	30·281	29·88	30·24	30·28	30·25	30·31	48	24	39	44½	32½	39½	37	n.	nw.	nnw.	nnw.	......	......	......	......
15.	30·435	30·421	30·04	30·35	30·20	30·25	30·04	43	18	36	42	26½	41½	45	n.	nw.	w.	nw.	......	......	......	·02
16.	30·316	30·235	29·86	30·05	30·03	30·07	29·82	45	38	37·5	46	37½	39	40½	w.	w.	w.	s.	......	......	0·10	·25
☾ 17.	30·073	29·808	29·55	29·69	29·50	29·37	29·25	49	38	44	48½	39	45½	40	sw.	wsw.	nw.	nw.	·04	......	......	·50
18.	29·729	29·513	29·20	29·30	29·33	29·18	29·35	50	34	44	47	41	39	38	sw.	sw.	w.	n.	·03	·02	......	·26
19.	30·050	29·793	29·46	29·71	29·68	29·64	29·38	50	24	37	45	30	37	41	nw.	nnw.	w.	s.	......	......	......	......
20.	29·874	29·581	29·34	29·25	29·10	28·77	28·73	55	40	49	51½	43½	47	46	sw.	s.	sw-w.	w.	·09	......	......	1·04
21.	29·662	29·523	29·20	29·23	29·20	29·00	29·43	54	33	43	46½	42½	38½	35	sw.	sw.	w.	e.	·03	·14	......	·65
22.	29·258	29·168	28·93	29·00	28·87	29·20	28·95	55	43	48	48½	39	40	45	sw.	s.	e.	e.	·01	·10	......	·31
23.	29·207	29·151	28·80	28·95	29·18	28·87	29·18	52	39	45	47	38½	44½	41½	s.	s.	e.	wnw.	·04	......	1·53	·32
24.	29·763	29·391	29·33	29·56	29·80	29·46	29·81	45	22	38	43	31	42	39	n.	nw.	w.	nnw.	......	......	......	·05
● 25.	30·084	29·925	29·70	29·78	29·50	29·71	29·38	48	40	31	47	31	41	43	s.	sw.	ese.	sse.	·05	......	......	......
26.	29·869	29·826	29·40	29·48	29·45	29·21	29·06	55	43	48	50	44	45	47½	sw.	s.	w.	sw.	·38	·14	......	·18
27.	30·078	29·772	29·38	29·56	29·78	29·47	29·57	51	41	44	50½	43	46	45½	w.	ssw.	w.	sw.	......	......	......	·26
28.	30·090	29·962	29·56	29·60	29·39	29·28	28·90	54	46	46	53	43½	46½	47	sw.	sw.	sw.	ssw.	......	......	......	·27
29.	29·919	29·742	29·40	29·54	29·48	29·26	29·40	55	38	52	53	45	45	43	sw.	w.	sw.	nw.	·12	......	......	·34
30.	29·893	29·713	29·38	29·57	29·57	29·44	29·46	48	30	43	43½	39½	44	41	sw.	w.	w.	nnw.	·01	·12	0·70	·25
Mean.	29·954	29·800	29·46	29·695	29·687	29·623	29·627	49·00	33·03	40·7	45·7	35·1	41·96	41·48					0·90	1·02	2·73	6·23

THE
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[THIRD SERIES.]

FEBRUARY 1849.

X. *Experiments on Diamagnetism.* *By* H. C. Œrsted*.

At a meeting of the Royal Society of Sciences at Copenhagen on the 30th of June, I communicated the results of some researches I had made upon diamagnetism, an abstract of which appeared in the proceedings of the Society. During the vacation I have continued my experiments, and have obtained several new results. As the memoir will not appear for many months, I have decided on giving an abstract which may be communicated to my distant friends.

My researches relate to the celebrated diamagnetic discoveries of Mr. Faraday, and to the extensions they have received from some learned Germans.

Mr. Faraday, in the experiments with his great electro-magnet, met with a class of bodies which are repelled by the two poles of the magnet. One or two examples of this repulsion had been known for some time; but the researches of the illustrious Englishman has rendered this fact general, and of such importance as to become the object of attention of all philosophers. Brugmanns had already discovered in 1778 that bismuth is repelled by the two poles of a magnet. Becquerel, sen., again met with this repulsion both in bismuth and in antimony. Mr. Faraday found that his great electro-magnet produced this repulsion in almost all the bodies it did not attract. He discovered at the same time that long pieces thus repelled, assume, under the influence of the electro-magnet, a position perpendicular to that which an attracted body would take under the same circumstances. This property he called *diamagnetism*.

M. Reich of Freiburg†, so well known from his beautiful experiments on the deviation in the fall of bodies from a great

* From the *Annales de Chimie et de Physique* for December 1848.
† See p. 127 of the present Number.

height, applied to the discovery of diamagnetism the observation, overlooked by other philosophers, that the two poles of the magnet employed together do not produce on these bodies a repulsion equal to the sum of the repulsions produced by each of them, but equal to their difference; so that their joint effect is null when their forces are equal. At the same time he made some experiments which seemed to indicate that the pole which repulsed the diamagnetic body produced in the parts adjacent to it a magnetic force similar to its own, but not an opposite force, as occurs in attracted bodies. Prof. Weber* confirmed the opinion of M. Reich by some very elaborate researches, and showed that diamagnetic bodies acquire, under the influence of the electro-magnet, a transversal magnetism having two poles, but so placed that each of them has the same kind of magnetism as the pole nearest to the electro-magnet.

M. Poggendorff conceived some very decisive experiments, which have the advantage of proving the new view in an easy manner; and M. Plücker contributed a further experiment, which, if it did not tend to establish the view, rendered the possibility of proving it more easy.

In my experiments I made use of the large U-shaped electro-magnet of the Polytechnic School of Copenhagen, which is capable of carrying 1400 kilogrammes†. It must, however, be remarked, that it was not necessary to put all its force in activity for these experiments; but there was rarely less

* See Taylor's Scientific Memoirs, vol. v. Part 19, p. 477.

† I have here conformed to the usual way of indicating the power of the magnet, although there is much uncertainty, as is proved by my experiments on this electro-magnet, communicated to the Society in December 1847. The object of these experiments was to ascertain the weight the electro-magnet was capable of carrying when its poles were furnished with different pieces of iron. Within certain limits the carrying force increased nearly in proportion to the mass of the keeper; but what merits most attention is, that the force of the electro-magnet expressed in weight does not follow the same relation to the electro-motive power of the galvanic apparatus, when the keeper is in contact with the electro-magnet, and when it is at a certain distance. In contact, the mean effect of each galvanic element was 712·5 kilogrammes. But two elements combined gave but 0·72 of the sum of the individual effects of the elements; three elements combined gave but 0·48; eight, 0·26; sixteen, 0·125; so that the effect of sixteen elements was only twice that of one. At the distance 1·33 millim. the effect of one element was only 0·178 of that of the same element in the case of contact, but the effect increased in a very different ratio with the number of the elements; in this case sixteen elements gave four times the effect of a single one. At 2·225 millims. distance the effect of one element was only 0·051 of that produced in case of contact; but sixteen elements gave 9·4 times the effect which a single one gave. These experiments, which require much time, I intend to continue as soon as my other engagements will allow.

than half of this power made use of, although the greater part of them might have been executed with a much weaker force, even with a single element. Each extremity of the electro-magnet supports a horizontal piece of iron, which we shall call a polar piece. These polar pieces serve to give a horizontal direction to the action of the electro-magnet. It is between the two perpendicular surfaces, which face one another, that the diamagnetic body is made to oscillate. These are called polar faces. In every case, excepting those indicated, I have made use of rectangular pieces. At the commencement of my experiments I employed cylindrical pieces; but this form is less suited for discovering all the circumstances which should be taken into consideration in these investigations.

A diamagnetic needle suspended horizontally between the polar faces, assumes, as is well known, an *equatorial* position, which is parallel to the polar faces; but if it is raised a little above the edges of the polar faces, it takes a perpendicular direction to the prolonged polar faces. This position is at the same time axial; but it will subsequently be seen that the question here is as to its perpendicularity to the polar faces. This phænomenon is exhibited with remarkable quickness, which renders the experiment very convenient for many diamagnetic investigations. When the needle is turned from its perpendicular position, it reacquires it by oscillation. Its directing force diminishes with its elevation above the polar pieces. The experiment was made with several diamagnetic bodies, with bismuth, amber, mother-of-pearl, tortoise-shell, alabaster, quills of feathers, sulphur, coal, &c.

The change of direction observed in these experiments vanishes as the polar faces are separated. At the distance of 17 millims. the effect was still well-marked; but it is very much stronger at short distances. When the distance was diminished so that the diamagnetic body could not be inserted between the polar faces, that is to say, perpendicular to them, the part of the effect which takes place above the polar faces was exhibited with considerable energy. When the diamagnetic needle was suspended above the upper edge of one of the polar faces, it equally assumed the position called *axial*, perpendicular to that edge, but with less force than under the influence of the two faces. On examining the position which the needle takes above the other edges of the polar piece, it is found that it everywhere assumes the position perpendicular to the edge to the influence of which it is exposed. In those cases where it is exposed to the action of two edges at once, it takes the intermediary position. Above the edge of a wedge-shaped piece of iron, placed with its base on one of the poles of the electro-magnet, the needle likewise assumes a position perpendicular to this edge. On a cylindrical polar piece, the needle placed with its centre above the edge of the polar face, arranges itself perpendicular to it; but at some distance from the edge it turns and takes a perpendicular position to the line which may be drawn parallel to the axis in the more elevated part of the cylindrical surface. When a perforated cylinder is employed as a polar piece, and the diamagnetic needle is made to descend and rise alternately, and parallel to the polar face, the needle is found to leave the position parallel to the polar faces, and to assume the position called axial as soon as it is opposite the perforations. I employed for this experiment a needle of bismuth 16 millims. in length. With two similar polar pieces the same effect is obtained, but it is much greater.

When the diamagnetic needle is suspended between the polar faces, it has, in accordance with the experiments of the German philosophers above-mentioned, magnetic poles in the transversal direction, arranged in such a manner that the magnetism of each side is of the same nature as that of the pole nearest to the electro-magnet. The easiest method of observing this is that described by M. Plücker, who introduces between the polar faces, and parallel to them, a small bar of iron separated from the faces by some non-magnetic substance. As the sides of this bar acquire by the influence magnetism the reverse of that of the nearest face, but each side of the needle has the same magnetism as the nearest face, the needle now retained by two forces oscillates with much greater velocity than when under the influence of the polar faces only. When the diamagnetic needle is raised above a polar piece, and its direction is changed, its magnetic poles change their places at the same time.

I had been led into error at the commencement by several phænomena, which in the novelty of the investigation seemed very complicated, but which nevertheless appeared very simple when the law had been found for them. At first I thought that the diamagnetic needle above the polar pieces had in each extremity the opposite magnetism to that of the adjoining polar piece; for the lower part of a bar of iron, influenced by the piece, repelled the extremity of the needle which was above that piece. This effect takes place not only on placing the repelling pole of iron near to each side, but equally above and below. Nevertheless later experiments have led me to reject the conclusion which I had previously drawn. I found that a piece of iron of moderate size receives from the polar piece acting upon it, a magnetic force sufficient to repel the diamagnetic matter of the needle, notwithstanding the poles which it had received under the influence of the electro-magnet. To discover the diamagnetic poles in this case, it is necessary to employ very small pieces or blades of iron; they should not weigh more than two or three grammes. To experiment with them the easier, I attached slips of zinc or pieces of wood to them. By this means I at last arrived at the conviction, that the lower part of the diamagnetic needle suspended above a polar piece has the same magnetism, and that its upper portion has the opposite. In experimenting on this subject, I finally made use of a thin blade of iron, shaped thus ☾, and fixed to a piece of wood. When this blade is placed on the polar piece, it has in its upper part the same magnetism as the polar piece, and its lower part the opposite. When the opening of the curve faces the needle, it attracts it; but when the upper part is beneath, or its lower part above the needle, it repels it.

When the needle is suspended over one of the polar pieces, so that the prolongation of one of the perpendicular faces of the piece divides the needle into two parts, it is found that the diamagnetic poles produced by the electro-magnet extend beyond the part which is above or correspondent to the upper surface of the piece. In some experiments made with a needle of bismuth of 56 millims., this effect extended to nearly 14 millims.

When the needle was divided into two equal parts by the prolonged perpendicular faces, the extremity of the needle the more distant from the polar piece was not polarized.

When the electro-magnet was furnished with two polar pieces placed at a distance of 48 millims., I found that the same needle had diamagnetic poles in all its parts. The half of the needle which was turned towards the north pole had north magnetism at its lower edge, and south magnetism at its upper edge; the other portion of the needle had, under the influence of the south pole, the magnetism of this pole at the lower edge, and north magnetism at its upper edge. There is, therefore, opposite magnetism in the two halves of each edge, taken separately, and in each half between the two edges, the upper and lower one.

When the diamagnetic body is made to oscillate between the polar faces, it does so with greater velocity the nearer it is to one of the edges of this face. In one experiment in which the electro-magnet was excited by sixteen of Bunsen's galvanic elements, and where the distance of the polar faces was 6 millims., a needle of bismuth at an equal distance from the upper and lower edges of these faces made twenty-five oscillations in 30 seconds; but level with the edges, it made

100 oscillations in the same time. Above the polar pieces, in the axial position, the needle made only nineteen oscillations in the same time. These experiments have been sufficiently repeated and varied to afford the most perfect certainty as to what has been stated; but the investigations have not yet been carried far enough to admit of an accurate numerical law being deduced from them.

When a horizontal needle of bismuth is suspended by a fibre of silk to the extremity of a balance, so that the balance can be raised or lowered, the needle is found to be repelled with so much more force the nearer it is situated to one of the edges of the polar faces. It will be understood that this repelling power causes the needle to ascend when it is near the upper edges, and to descend when near the lower edges; in the intermediate position it neither ascends nor descends. When the needle is suspended above the polar pieces, and consequently in the direction perpendicular to the edges of the polar faces, it is again repelled, but much more feebly than when in the equatorial position.

Hitherto diamagnetic effects had only been observed in bodies which are repelled by the two poles of the magnet. My experiments have shown that a similar effect may be produced in most bodies which are attracted by the two magnetic poles; so that these bodies constitute a new kind of diamagnetic substances. These two classes may be distinguished by the names of *repulsive* and *attractive* diamagnetic bodies.

A needle made of a substance that is attracted by the magnet, but of which the magnetism is not of the same nature as that of iron and nickel, when suspended between the two polar faces of the electro-magnet, acquires, as is well known, the position called axial by Mr. Faraday; but if it is raised above the upper edges, or lowered beneath the lower edges of the polar faces, it assumes the equatorial position. This property has hitherto been found in the following substances; viz. platina, palladium, iridium, titanium, an alloy of 0·825 of tin, 0·024 of bismuth, 0·108 of iron, in brass, German silver, wood-charcoal, coke (fossil coal belongs to the repulsive diamagnetic bodies), obsidian, native carbonate of iron, attractive glass, prussian blue, and solutions of iron.

In the majority of these substances, the magnetic poles which they obtain during the influence of the electro-magnet disappear nearly as soon as this influence is removed; however, their existence is betrayed when the poles of the electro-magnet are suddenly changed, for then many of these bodies turn half round, as is the case with a magnetic needle; others do not exactly turn, but oscillate, thus indicating their tendency to a change of position. But we find some attractive diamagnetic bodies, such as a piece of iridium in my possession, wood-charcoal and coke, which retain the poles they have acquired by the influence much longer, of which it is easily to be convinced by experiments on the mariner's compass. The experimental investigation of the phænomena exhibited by these bodies is complicated by this duration of the polarity; but they will probably lead us to discover the connexion which exists between magnetism and diamagnetism.

When a needle constructed of an attractive diamagnetic substance is suspended above the upper or beneath the lower edge of a polar piece, it assumes a position parallel to this edge. In this parallel position, which might either be perpendicular to the magnetic axis of the polar piece, or parallel to it, or have quite another position than the figure of the polar piece requires, the disposition of the magnetic forces in the needle is transversal, as in a repellent diamagnetic body; but with this difference, that its lower part has the opposite magnetism to that of the polar piece, and the upper part that of the same nature.

I have not succeeded in causing iron to assume the diamagnetic state. An iron wire, of which the diameter is but $\frac{1}{10}$th of a millimetre, still takes the axial position just as well above the polar faces as between them, and with a force nearly sufficient to break the fibre of silk. This experiment has been varied by placing in a quill of a feather, which is repellent, a fragment of the same iron wire, merely 2 millims. in length; but this arrangement still exhibited the same effects as the iron separately. The same result was also obtained on substituting for the bit of iron wire a very small particle of iron filings; but on introducing, in the place of iron, a piece of straw which had been immersed in a solution of iron, the diamagnetic effects of attractive bodies was obtained. Nickel gives the same results as iron. Thus nickel and iron ought properly to be called magnetic in the strict sense. This probably applies to some other substances, perhaps to cobalt.

There is consequently a decreasing magnetic progression which comprises the properly so-called magnetic substances, the attractive diamagnetic substances, and the repulsive diamagnetic substances. The magnetism of these last may be considered as negative, if the magnetism of iron and the attractive diamagnetic substances is regarded as positive.

The effect which the polar faces exert upon the attractive diamagnetic bodies is like that which takes place with the repulsive diamagnetic bodies,—stronger when the body is placed nearer the upper or lower edges than their intermediate parts. A piece of attractive glass, 27 millims. in length, which was suspended between the polar faces, 29 millims. apart, in such a manner that the extremities of this needle were not more than 1 millim. distant from the polar faces, was made to oscillate each time during 30 seconds. At an equal distance from the upper and lower edges it made only 4·5 oscillations in the 30 seconds, but level with the polar faces it made nineteen.

When the polar faces are at this distance, the needle does not assume the equatorial position when it is suspended above their edges. At the distance of 4·5 millims., it made 5·5 oscillations; 13·5 millims. distant it made only 2·5 oscillations. The polar faces were approached to within 3 millims. The needle, which now could not assume the axial position between the faces, nevertheless showed its tendency to take that position; but raised 2 millims. above their edges, it assumed the equatorial position, and made eighteen oscillations in 30 seconds. At a distance of $\frac{3}{10}$ths of a millim., it made thirty-five oscillations. At the least possible distance, so as just to avoid contact with the polar pieces, it made forty-five oscillations.

It is seen that both the repulsive as well as the attractive diamagnetic bodies make more numerous oscillations in a parallel position to the polar faces than in the perpendicular position. It must however be observed, as has already been done, that the determinations of the numbers have not yet been carried sufficiently far to serve for the calculation of their laws.

I lately made some experiments on the influence which heat exercises on diamagnetic substances. These experiments are not yet numerous enough; they however prove to me that some attractive diamagnetic substances pass into the class of repulsive diamagnetic substances by an increased temperature. The only substance which exhibits this effect in a high degree is brass. My analogous experiments on other bodies are not yet sufficiently decisive to warrant their publication.

XI. *Continuation of Researches in the Mathematical Theory of Aërial Vibrations. By the* Rev. J. Challis, *M.A., F.R.S., F.R.A.S, Plumian Professor of Astronomy and Experimental Philosophy in the University of Cambridge**.

In replying to arguments against my conclusion that plane-waves are physically impossible, I am required by the rules of logic to notice those only which profess to invalidate any

* Communicated by the Author.

step of the reasoning by which the conclusion was arrived at, the proof being a *reductio ad absurdum.* I shall therefore confine myself to the single objection of this nature brought forward by Mr. Stokes in his communication to the Philosophical Magazine for January, viz. that which he expresses by saying that I "integrate through an infinite ordinate." Now the very terms of this objection preclude the necessity of answering it. For it admits the existence of an infinite ordinate, and consequently of some valid reasoning by which it is shown to exist. I contend for nothing more. The expression "infinite ordinate" is a symbolical term to indicate that a finite change of velocity or density occurs in an infinitely small space. I have expressed the same thing in plainer terms by saying, that the same point of the same wave is at the same time a position of maximum velocity and of no velocity. It is clear, therefore, that there is no question as to the mathematical reasoning, but as to the interpretation of the result. I consider that the infinite ordinate is condemnatory of the hypothesis of plane-waves: Mr. Stokes says that it is indicative of certain physical circumstances analogous to a *bore.* As this difference is not likely to be removed by further mathematical discussion, I pass on to the case of spherical waves.

First, I propose to exhibit as distinctly as possible the reasoning which Mr. Stokes has undertaken to call in question. For this purpose I shall commence with the usual hydrodynamical equations applicable to small disturbances, and proceed through the different steps in order, numbering the paragraphs for the sake of reference.

(1.) Formation of a general differential equation applicable to small condensations.

Let $p=a^2(1+s)$, p being the pressure and s the small condensation at the time t at a point whose co-ordinates are x, y, z. Then if u, v, w be the resolved parts of the velocity at the same point and at the same time in the directions of the axes of co-ordinates, we have the known approximate equations,

$$a^2.\frac{ds}{dx}+\frac{du}{dt}=0,\quad a^2.\frac{ds}{dy}+\frac{dv}{dt}=0,\quad a^2.\frac{ds}{dz}+\frac{dw}{dt}=0,$$

$$\frac{ds}{dt}+\frac{du}{dx}+\frac{dv}{dy}+\frac{dw}{dz}=0.$$

By differentiating the last equation with respect to t, we obtain

$$\frac{d^2s}{dt^2}+\frac{d^2u}{dt\,dx}+\frac{d^2v}{dt\,dy}+\frac{d^2w}{dt\,dz}=0;$$

whence, by substituting for $\frac{du}{dt}, \frac{dv}{dt}, \frac{dw}{dt}$, from the three first equations, we have the equation sought, viz.

$$\frac{d^2s}{dt^2}=a^2.\left(\frac{d^2s}{dx^2}+\frac{d^2s}{dy^2}+\frac{d^2s}{dz^2}\right).$$

(2.) The hypothesis of spherical waves.

In consequence of this hypothesis s is a function of the distance (r) from the origin of co-ordinates, and the above equation becomes

$$\frac{d^2.sr}{dt^2}=a^2.\frac{d^2.sr}{dr^2}.$$

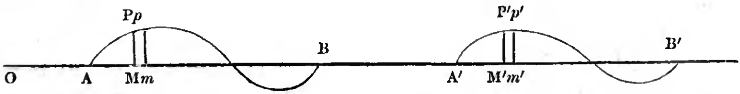
(3.) Integral of the last obtained equation.

The general integral contains two arbitrary functions, each of which separately satisfies the equation. I need only write down the solution which has been employed in the present discussion, viz.

$$s=\frac{1}{r}\cdot \text{F}(r-at).$$

(4.) Interpretation of the above integral.

The meaning of this integral may be exhibited as follows. Draw any straight line OABA$'$B$'$ from O the origin of co-ordinates, and putting z for $r-at$, describe a curve APB such that any ordinate MP shall represent the value of $\frac{\text{F}(z)}{r}$, that is, of the condensation s, corresponding to the value OM of r. Because the function F is arbitrary, the form of the



curve is arbitrary. It is admitted that the function F may be discontinuous, and accordingly that at a given time t_1, $\text{F}(r-at_1)$ has real values from $r=\text{OA}$ to $r=\text{OB}$, and that for values of r less than OA and greater than OB, $\text{F}(r-at_1)=0$. The state of condensation which this curve represents is propagated with the uniform velocity a, so that at a subsequent epoch t_2 it has the position of the curve A$'$P$'$B$'$. The change which the curve has undergone is such that, A$'$B$'$ remaining equal to AB, any ordinate PM corresponding to an abscissa AM has to the ordinate P$'$M$'$ corresponding to an equal abscissa A$'$M$'$, the ratio of OM$'$ to OM. Or if PM, P$'$M$'$

represent respectively the condensations s_1, s_2, and $OM = r_1$, $OM' = r_2$, we have $\frac{s_2}{s_1} = \frac{r_1}{r_2}$.

(5.) THE CONTRADICTION.

Draw an ordinate pm indefinitely near to PM, and $p'm'$ indefinitely near to P′M′, and let the interval $Mm = M'm' = \alpha$. Then the quantity of matter contained between the spherical surfaces of which the radii are r_1 and $r_1 + \alpha$, beyond what would exist in the same space in the quiescent state of the fluid, is $4\pi r_1^2 s_1 \alpha$; and similarly the quantity of condensed matter between the surfaces of which the radii are r_2 and $r_2 + \alpha$ is $4\pi r_2^2 s_2 \alpha$. The same reasoning applies to every set of corresponding ordinates of the two curves. Hence by the principle of constancy of mass employed in investigating one of the general hydrodynamical equations, those two quantities must be equal to each other; that is, $4\pi r_1^2 s_1 \alpha = 4\pi r_2^2 s_2 \alpha$. Consequently $\frac{s_2}{s_1} = \frac{r_1^2}{r_2^2}$. This result, which is incontrovertible, is at variance with the conclusion in (4.). I infer, therefore, that the hypothesis of (2.) is inadmissible.

I have been thus explicit for the purpose of stating distinctly the course which this discussion must take. It will be observed that subsequently to making the hypothesis of (2.), the *velocity* of the fluid is nowhere introduced. The rules of right reasoning absolutely forbid the introduction of any expression for the velocity by an opponent, simply because such expression cannot be obtained without adopting the very hypothesis the legitimacy of which is the point in dispute. Mr. Stokes from beginning to end has argued from a value of the velocity derived from the disputed hypothesis. To all his argument I have therefore this one answer: the truth of the expression for the velocity is not *proved*. It is quite necessary that the discussion should turn on the reasons which I have alleged for the different steps of my argument, *without taking for granted the legitimacy of the hypothesis of spherical waves.* I am discharged from the necessity of dwelling on the details of Mr. Stokes's reasoning, because the whole of it contains a *petitio principii.* Indeed I should desert the position which the acknowledged rules of right reasoning compel me to take, if I made a single remark which implied an admission that that reasoning required from me any answer. I proceed, therefore, with the subject which more strictly accords with the title of this communication.

I shall commence by saying that I regret Mr. Stokes does not feel himself at liberty to give me the benefit of his strictures on my mathematical theory of ray-vibrations, and to state his reasons for objecting to equations (8.), (B.) and (C.). The subject, being entirely new, offers a fair field for discussion; and I am quite prepared to find that I have been mistaken on some points. In particular, I have recently discovered that the equations just named are not generally true beyond the first order of approximation, as I shall presently show. From the nature of the problem the processes applied to it must be in a great measure *tentative*, and can only be tested by the results. The equation (A.), for instance (Phil. Mag., vol. xxxiii. p. 99), as far as I know, is the first example of an application of analysis to a question in physics, which presents for solution a partial differential equation containing two principal variables and two independent sets of variables mixed up with each other. I have proceeded on the principle that if particular and consistent values of one set of variables be substituted in the equation, the resulting equation will be true for *general*, if not the most general, values of the other set. But the application of this principle is restricted by any limitation to which the supposition which conducted to the equation (A.) is subject, viz. the supposition by which $udx+vdy+wdz$ was made an exact differential. Now it is well known that for small vibrations, the equations

$$\frac{du}{dy}=\frac{dv}{dx}, \quad \frac{du}{dz}=\frac{dw}{dx}, \quad \frac{dv}{dz}=\frac{dw}{dy},$$

must be at least approximately verified. This will be the case for approximate values of u, v, w, if the complete values make $udx+vdy+wdz$ an exact differential. On this account, for the purpose of verifying those three equations approximately, it was assumed (Phil. Mag., vol. xxxiii. p. 99) that $udx+vdy+wdz$ was integrable for the complete values of u, v, and w. But it is equally possible that the same three equations may be *exactly* verified by approximate values of u, v, and w, in which case the condition that $udx+vdy+wdz$ be integrable, may be only satisfied by approximate values of u, v, w. It does not seem possible to decide which of these is the true state of the case but by trial. I supposed the former to be true, and obtained equations applicable to ray-vibrations on this presumption. It turns out on further investigation that the latter is the true theorem, when the integrability depends on the supposition that $(d.f\varphi)=udx+vdy+wdz$, φ being a function of z and t, and f a function of x and y. For this reason those equations require certain modifications which I now proceed to develope.

The equation (B.) in page 99 of the Phil. Mag. for last August, viz.

$$0=-b^2\varphi+a^2.\frac{d^2\varphi}{dz^2}-\frac{d^2\varphi}{dt^2}-2\frac{d\varphi}{dz}.\frac{d^2\varphi}{dzdt}-\frac{d\varphi^2}{dz^2}.\frac{d^2\varphi}{dz^2} \quad . \quad (B.)$$

was derived from the equation (A.) in the same page, by assuming, since the motion is by hypothesis vibratory, that f has a maximum value equal to unity, and that the values of x and y which satisfy $\frac{df}{dx}=0$ and $\frac{df}{dy}=0$, make

$$\frac{d^2f}{dx^2}+\frac{d^2f}{dy^2}=-\frac{b^2}{a^2}.$$

On the same suppositions respecting f I have shown in the Phil. Mag. for last December (p. 465) that the equation (B.) is satisfied by an equation of this form,

$$\frac{d\varphi}{dt}+a_1\frac{d\varphi}{dz}=0, \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

a_1 being a certain constant. This may be regarded as a particular integral applying to propagation in a single direction, and is all that is required for the present investigation. An integral of (B.) satisfying (1.) was also obtained (vol. xxxiii. p. 363) by successive approximations. By differentiating (1.) and substituting the resulting values of $\frac{d^2\varphi}{dt^2}$ and $\frac{d^2\varphi}{dzdt}$ in (B.), we obtain

$$\frac{d^2\varphi}{dz^2}=\frac{b^2\varphi}{a^2-\left(a_1-\frac{d\varphi}{dz}\right)^2}, \quad \frac{d^2\varphi}{dt^2}=\frac{a_1{}^2b^2\varphi}{a^2-\left(a_1-\frac{d\varphi}{dz}\right)^2},$$

$$\frac{d^2\varphi}{dzdt}=-\frac{a_1b^2\varphi}{a^2-\left(a_1-\frac{d\varphi}{dz}\right)^2}.$$

Now substituting these values in (A.) (which equation being long I dispense with inserting here), the result is

$$\left.\begin{aligned}0=a^2\varphi\left(\frac{d^2f}{dx^2}+\frac{d^2f}{dy^2}\right)+b^2f\varphi\cdot\frac{a^2-\left(a_1-f\frac{d\varphi}{dz}\right)^2}{a^2-\left(a_1-\frac{d\varphi}{dz}\right)^2}\\ +2\varphi\frac{d\varphi}{dz}\left(a_1-f\frac{d\varphi}{dz}\right)\left(\frac{df^2}{dx^2}+\frac{df^2}{dy^2}\right)\\ -\varphi^3\cdot\left(\frac{df^2}{dx^2}\cdot\frac{d^2f}{dx^2}+2\frac{df}{dx}\cdot\frac{df}{dy}\cdot\frac{d^2f}{dxdy}+\frac{df^2}{dy^2}\cdot\frac{d^2f}{dy^2}\right)\end{aligned}\right\} \quad (A'.)$$

It thus appears that the result is not independent of φ unless powers of this quantity above the first be neglected. In this case the equation becomes

$$0=\frac{d^2f}{dx^2}+\frac{d^2f}{dy^2}+\frac{b^2}{a^2}f, \quad . \quad . \quad . \quad . \quad . \quad . \quad (2.)$$

and at the same time the equation (B.) becomes

$$0=\frac{d^2\phi}{dt^2}-a^2.\frac{d^2\phi}{dz^2}+b^2\phi. \quad . \quad . \quad . \quad . \quad . \quad (3.)$$

It is, however, important to remark, that the equation (A′.) is satisfied without any restriction of the value of ϕ the more exactly in proportion as the value of f approaches more nearly to unity. We may hence infer that the equation (B.) is strictly true for points on and immediately contiguous to the axis of the ray, but not more generally. Thus, with the exception just named, I have not hitherto succeeded in obtaining equations applicable to ray-vibrations beyond the first order of approximation, which probably is all that will be required in application. In equation (3.) of my communication to the Philosophical Magazine for last November (p. 364), the terms involving $\frac{df^2}{dx^2}$, $\frac{df^2}{dy^2}$, and m, which arose from relying too implicitly on the hypothesis by which $udx+vdy+wdz$ was made integrable, must be rejected. The equations (1.) and (2.) in p. 363 hold good. It may also be stated, that the reasoning in the December Number, by which it was shown that along an axis of ray-vibrations a given state of density and velocity may be propagated uniformly and without alteration, remains untouched.

Having stated all the modifications which, according to the present investigation, the previous results require, I beg to add a few general remarks. It is well known that on first applying the integrals of partial differential equations to physical questions, a dispute arose between Euler and D'Alembert as to the arbitrariness of the forms of the functions. The discussion issued in the establishment of the principle of discontinuity by Lagrange. Yet where two mathematicians of so great eminence differ, it may generally be concluded that a share of truth is on each side. The singular contradictions which I have pointed out as resulting from the suppositions of plane-waves and spherical waves, must, I think, raise the question whether, as D'Alembert appears to have supposed, forms of solution are not discoverable which indicate motions independent of the particular disturbance, and whether these must not be discovered before proceeding to consider cases of arbitrary disturbance. The explanation I give of those contradictions is as follows. I regard them as proving, not that cases of plane-waves and spherical waves cannot exist, but that they cannot exist *spontaneously*. Having been led deductively from the general equations to those which define ray-vibrations, without meeting with any similar contradiction, and anterior to any supposed case of motion, I conclude that ray-vibrations are common to all instances of vibratory motion. Any instance of such motion may be conceived to be composed of the ray-vibrations defined by the equations (2.) and (3.), the number, magnitude, and directions of the rays being unlimited. Accordingly if a plane-wave were generated, it would be composed of an unlimited number of ray-vibrations having their axes all parallel to the direction of propagation; and from what has been proved of these vibrations, the resultant wave might be propagated to any distance without undergoing any change. So if a spherical wave were generated, it would be composed of an unlimited number of ray-vibrations having their axes diverging from a centre, and the change of condensation with the change of distance from the centre would be according to the inverse *square* of the distance, since it would depend only on the divergence of the rays. This result is in accordance with the principle of the constancy of mass.

Cambridge Observatory,
January 9, 1849.

Postscript, Jan. 11, 1849.—After despatching the foregoing communication, I deduced from my theory of ray-vibrations a result which ought, I think, to command the attention of mathematicians.

I have already exhibited the course of reasoning which finally led to the conclusion, that the transverse motion in ray-vibrations is defined, to the first order of approximation, by the foregoing equation (2.). To this equation is next to be applied the process by which, from the equation (3.) of analogous form, which defines the motion along the axis of the ray, a *unique* integral expressed in finite terms was obtained, viz.

$$\varphi = m \cos \frac{2\pi}{\lambda}\left(z - at\sqrt{1 + \frac{e\lambda^2}{\pi^2}}\right).$$

(See Phil. Mag. for April 1848, p. 279.) On so doing, the result is

$$f = \alpha \cos (gx + hy),$$

the quantities g and h being subject to the condition,

$$g^2+h^2=\frac{b^2}{a^2}=4e.$$

Hence if $g=2\sqrt{e}\cos\theta$, it follows that $h=2\sqrt{e}\sin\theta$; so that

$$f=\alpha\cos(2\sqrt{e}(x\cos\theta+y\sin\theta)).$$

This result is not definite, because the angle θ is indeterminate. But a result which is definite may be obtained by satisfying the given equation so as to embrace all possible values of θ. This may be done by taking account of the analytical circumstance, that the value of f may be expressed by an unlimited number of terms like that above. Let, therefore, $\alpha\delta\theta$ be a given indefinitely small quantity. Then we may have

$$f=\Sigma\alpha\delta\theta\cos(2\sqrt{e}(x\cos\theta+y\sin\theta)),$$

θ having all values from $\theta=0$ to $\theta=2\pi$. By performing the summation, substituting r^2 for x^2+y^2, and determining α so as to satisfy the condition that $f=1$ where $r=0$, the result is

$$f=1-er^2+\frac{e^2r^4}{1^2.2^2}-\frac{e^3r^6}{1^2.2^2.3^2}+\&c. \quad . \quad . \quad . \quad (4.)$$

It seems, therefore, that we have arrived at an expression for f which involves no indeterminate quantity, and which defines precisely the transverse motion. Making $f=0$, the resulting equation contains an unlimited number of possible positive roots, and there are consequently an unlimited number of positions for which $f.\frac{d\phi}{dt}=0$, or the condensation vanishes, on any given radius. Similarly the equation resulting from making $\frac{df}{dr}=0$, contains an unlimited number of intermediate possible positive roots; and there are consequently an unlimited number of values of r, which are radii of cylindrical surfaces situated in positions where there is no transverse velocity. Since no fluid passes these surfaces, there is no propagation of motion in directions transverse to the axis of the ray. The intervals between the surfaces approach to a certain limiting value in proportion as we recede from the axis, and the maximum values of f go on continually decreasing. The limiting value of the interval between two consecutive surfaces of no condensation may be obtained as follows.

Let n be an indeterminate infinite whole number. Then making $f=0$, the equation (4.) may be expressed in the following form:

$$\frac{1}{1^2.2^2.3^2\dots n^2}.\Big\{\dots-(n+1)^{-2}e^{n+1}r^{2n+2}+e^n r^{2n}-n^2.e^{n-1}r^{2n-2}$$
$$+n^2(n-1)^2e^{n-2}r^{2n-4}-n^2(n-1)^2(n-2)^2e^{n-3}r^{2n-6}+\&c.\Big\}=0.$$

Or thus:

$$\dots.+e^n r^{2n}-n^2e^{n-1}r^{2n-2}+n^4\left(1-\frac{1}{n}\right)^2e^{n-2}r^{2n-4}$$
$$-n^6\left(1-\frac{1}{n}\right)^2\left(1-\frac{2}{n}\right)^2e^{n-3}r^{2n-6}+\&c.=0;$$

whence it will be seen that the terms at an infinite distance either way will become insignificant on account of the factors

$$\left(1+\frac{1}{n}\right)^{-2},\ \&c.,\ \left(1-\frac{1}{n}\right)^2,\ \&c.$$

Hence retaining only terms of the highest order of infinity, we have,

$$\dots.+(er^2-n^2)e^{n-1}r^{2n-2}+(er^2-n^2)n^4e^{n-3}r^{2n-6}+\&c.=0.$$

This equation is satisfied if $er^2-n^2=0$; or

$$r_1\sqrt{e}=n.$$

As no condition has been imposed on the quantity n, except that it be an infinite whole number, the radius r_2 of the next surface of no condensation will be given by the equation

$$r_2\sqrt{e}=n+1.$$

In fact, by the same process as that by which the equation $(er^2-n^2)Q=0$ was obtained, we might obtain

$$(er^2-(n+1)^2)Q'=0,\quad (er^2-(n+2)^2)Q''=0,\ \&c.,$$

all the terms of Q, Q', Q'', &c. being essentially positive. Hence the infinite roots of the equation

$$0=1-er^2+\frac{e^2r^4}{1^2.2^2}-\&c.\pm\frac{e^nr^{2n}}{1^2.2^2.3^2\dots.n^2}\mp\&c.,$$

are contained in the equation

$$(er^2-n^2)(er^2-(n+1)^2)(er^2-(n+2)^2)\&c.=0.$$

Consequently if Δ be the interval between two consecutive surfaces of no condensation, we have

$$\Delta\sqrt{e}=1.$$

It is now to be remarked, that the transverse motion between the two consecutive surfaces either of no condensation or of no velocity, is exactly the same as that which would take place along the axis of the ray between two consecutive positions of no condensation or of no velocity, if two series of waves for which λ is the same were propagated along that axis in *opposite* directions. The time in which each particle executes a vibration is the same in the two cases. We must consequently have

$$\Delta = \frac{\lambda}{2}.$$

Hence

$$\frac{\lambda\sqrt{e}}{2} = 1, \text{ and } \frac{e\lambda^2}{\pi^2} = \frac{4}{\pi^2}.$$

Recurring now to the expression for the velocity (a') of propagation of a ray, obtained in the Phil. Mag. vol. xxxiii. p. 363, and neglecting the term involving m^2, it appears that

$$a' = a\sqrt{1 + \frac{b^2}{q^2a^2}} = a\sqrt{1 + \frac{e\lambda^2}{\pi^2}} = a\sqrt{1 + \frac{4}{\pi^2}}.$$

Hence if we take for a the value in art. 66 of Sir John Herschel's Treatise on Sound in the *Encyclopædia Metropolitana*, we shall obtain

$$a' = \overset{\text{ft.}}{916{,}322}\sqrt{1 + \frac{4}{\pi^2}}$$
$$= 1086{,}25 \text{ feet.}$$

The value of a' obtained by experiment is 1089,42 feet, as given in the same work. The slight excess may be owing to the neglect of the term involving m^2.

I have thus obtained a value of the velocity of sound, closely agreeing with experiment, *on purely hydrodynamical principles.* As this result is not in accordance with received ideas on this subject, I shall at a future opportunity give a careful *résumé* of the course of reasoning by which it has been arrived at.

J. Challis.

XII. *On a new Empirical Formula for ascertaining the Tension of Vapour of Water at any Temperature.* By J. H. Alexander, *Esq.**

[Concluded from p. 15.]

IN the last number of this Journal, I gave the formula itself, the principles from which it was deduced, and a comparison of results by it, with those by experiment at numerous identical temperatures. Want of room excluded then what

* From Silliman's Journal for Nov. 1848.

remained to complete this memoir, in showing the probable errors of the formula as compared with the principal experiments, and with the probable errors affecting too those different series of experiments themselves. Such a discussion is the object of the present paper.

It was already said in the preceding part, that the most proper mode of expressing these errors is by the linear scale of temperature; which both in theory is the most important, and in practice is the most accessible and usual. In this last aspect, it is on this scale, too, where errors of observation are the most easy to be made, and likely to occur. With this view the formula need be repeated here only in its converse form (*i. e.* for ascertaining temperatures from given pressures), as under:—

$$t^{\circ}\text{Fahr.} = 180\sqrt[6]{p} - 105^{\circ}\cdot 13;$$

p being in inches of mercury; and

$$t^{\circ}\text{Fahr.} = 317\cdot 13\sqrt[6]{p'} - 105^{\circ}\cdot 13;$$

p' being in atmospheres at 32°.

As this will have to be frequently applied for interpolation throughout the following discussion, it may be as well to remark here, once for all, in justification of such application, that there need be no apprehension of its affecting the results; for it is easy to see, by inspecting a few instances taken at random from the table, that the rational deviation of the formula (*i. e.* the difference between calculated and observed pressures) is, for small differences of temperature, either null, or so remote a fraction as to be inappreciable in the calculation.

In applying this formula, I shall take up the principal series of experiments separately, beginning with the most recent, and shall then make assemblage of the mean results.

1. *Experiments of M. Regnault.*—To deduce the absolute mean error of the numerous quantities of this observer, it would be obviously requisite to take up each experiment; a labour of which I am by no means ambitious, and which would be disproportionate at once to what is admissible in the other series presently to be noticed, and to the present aim. I shall, therefore, in all only make use of short general methods, which, without laying claim to the accuracy of geometrical refinements, will yet be recognized as having foundation in the theory of mathematical probabilities; and will, by their popular form, recommend themselves the more readily to the convictions of those who are chiefly conversant with steam in

practice, and for whose benefit the whole of the present discussion is mainly intended.

It is obvious, then, in the first place, that the idea of freedom from error is associated with symmetry in the results. Such symmetry will always be observable in quantities that progress (as natural quantities may be assumed to do) according to some constant law; and as, in our ignorance of what the true law is in this case, all that we can deal with is *relative* symmetry, it is of no importance what law or formula we take as the other term of comparison, provided there be no material difference between the origin and termination of the two. I shall therefore compare a few of M. Regnault's observations at the lower temperatures with the results of the present formula, as under:—

Temp. (Fahr.).	Pressure in inches of mercury.		Differences.
	Observed means.	Calculated.	
°	in.	in.	
−27·112	0·01063	0·00664	+0·00399
−13·	0·02047	0·01799	+0·00248
− 4·504	0·02835	0·03055	−0·00220
+ 1·706	0·04567	0·04375	+0·00192
+ 9·41	0·06378	0·06643	−0·00265
17·402	0·09410	0·09111	+0·00299
23·702	0·12559	0·13451	−0·00892
27·626	0·15000	0·16106	−0·01106
32·	0·18111	0·19561	−0·01450

It is apparent, then, that so far these observations do not follow any uniform or symmetrical progression; and without pretending to criticise the experiments themselves, which doubtless have as much accuracy as the nature of the research admitted, it follows that, in spite of all the extraordinary tact and skill of the observer, there is yet *primâ facie* evidence against the absolute accuracy of the results. It is to be remarked upon the column of temperatures, both here and hereafter, that the remote decimals result from the reduction of Centigrade degrees to those of Fahrenheit, and are preserved because they added to the accuracy, while they did not increase the labour of the calculation. Nevertheless the thermometer of M. Regnault could be read directly to the $\frac{1}{50}$th of a degree Centigrade, corresponding very nearly to $\frac{1}{28}$th of a degree Fahrenheit; and by estimation, to the next decimal place.

The temperatures of this table under 32° F. are lower than pressures have ever been observed at before, and rest upon single observations. They do not admit, therefore, of a comparison other than has been instituted. But the observations at 32° F., a temperature especially disengaged from instrumental errors, are, as has been already said, very numerous, and allow of being compared among themselves. Of the forty-seven observations whose arithmetical mean pressure is given in the table, the

	in.		
maximum was	0·18485;	corresponding to a temp. by formula of	30°·72 F.
and *minimum* was	0·17717	....	29·77 ...
and the difference	0·00768	corresponding difference of temp.	0·95

This difference shows a mean error in temperature, unaccounted for, of 0°·425 F.; and a limiting error in pressure rather more than half the difference between the formula and the mean of all the observations.

In the various series of M. Regnault, the temperature is given sometimes by one thermometer only, and sometimes by two, and even four. Of these latter classes, I have taken out of each series the observation where the difference of reading of the several thermometers is the greatest, to serve for another comparison, as follows:—

Series.	Thermometer.		Differences.
	Maximum.	Minimum.	
A.	33°·61 C.	33°·49 C.	0°·12 C.
B.	42·63	42·56	0·07
N.	43·64	42·84	0 81
O.	47·84	47·14	0·70
P.	47·87	47·	0·87
Q.	91·25	91·06	0·19
R.	122·72	122·50	0·22
S.	110·72	110·64	0·08
T.	137·75	137·52	0·23

Mean temp. 75°·154 C; mean difference 0°·366; corresponding with
... 167°·277 F. ... 0°·659 F.

This difference is that of the extremes; and as the mean error of any number of observations is as likely to be *plus* as *minus*, it is equivalent in this example to an absolute error of 0°·33 F.

This error manifests itself in a series where the thermometric variations are the greatest. I shall now present another where these same differences, although not perhaps the lowest of all, are yet very much less than in the last. At least, this series (which in fact forms part of the comparative table in the preceding memoir) was selected without any reference to the present investigation, and with a view to the introduction of the greatest number of accordant observations, and may be considered, therefore, as offering an impartial, if not favourable, term of comparison.

The mean temperatures are given here, as in our former table, in degrees of Fahrenheit; the individual differences between the thermometer readings are, to save calculation, retained in Centigrade degrees.

Mean temperatures. Fahrenheit.	Differences of thermometer readings. Centigrade.
151°·124	0°·52
176·416	0·30
198·05	0·13
211·27	0·06
222·44	0·08
233·132	0·16
252·662	0·22
263·3	0·14
276·224	0·20
297·464	0·19
Mean difference	0·20

corresponding with mean temperature 228°·2 F.; mean difference 0°·36 F., which is equivalent to an absolute mean error of 0°·18 F.

We have, then, for the mean error at	32° F.	0°·42
... ...	167	0·33
... ...	228	0·18
the average of which, or		0·31

is the probable amount of error, *plus* or *minus*, with which the various series of M. Regnault are still to be considered as affected.

Such being the error of the experiments, I shall now show, by the following table, the comparative error of the formula. The quantities in the column of differences are considered as on the same side of the equation with the results from the formula; those marked + indicate, therefore, the *default*, while the sign – indicates the *excess* of the calculated temperatures.

Pressures in inches.	Temperatures.		Differences.
	Observed.	Calculated.	
in. 0·01063	−27°·112 F.	−20°·73 F.	+7°·38 F.
0·02047	−13·	−10·98	+2·02
0·02835	− 4·504	− 5·74	−1·24
0·04567	+ 1·706	+ 2·49	−0·78
0·06738	+ 9·410	+ 8·65	+0·76
0·09410	+17·402	+16·27	+1·13
0·12559	+23·702	+22·25	+1·45
0·15000	+27·626	+26·08	+1·55...+1°·53
0·18111	32·	30·26	+1·74
7·6977	151·124	147·80	+3·32
14·081	176·416	174·58	+1·84
22·538	198·050	197·38	+0·67
29·620	211·27	211·49	−0·22...+1·47
35·779	224·44	221·62	+2·82
44·752	233·132	234·03	−0·90
63·333	252·662	254·24	−1·58
76·152	263·3	265·45	−2·15
93·859	276·224	278·59	−2·37
130·790	297·464	300·42	2·96...−1·19
		Mean difference......	+0·605

This mean difference (say 0°·61 F.) is the error of the formula as compared with the observations above, which are themselves the mean of more than twice as many experiments, and which may be taken as representing impartially the whole range of M. Regnault's results. In arriving at this mean difference, I have arranged the several instances into groups, whose individual means furnish the definitive general one. This is proper, in view of the different methods of experiment which the different relations of temperature in the respective groups rendered necessary. The indiscriminate mean of all, however (0°·657 F.), is not materially variant. It will be seen that, up to the point of boiling water, the formula-temperatures are generally *lower* than experiment; above that point, they are in general *higher*. I believe that such a change of sign accords with what might be anticipated, and in so far does not diminish the reliability of the formula.

The difference (+0°·61 F.) would be the absolute error of the formula, were we to assume the experiments as perfectly accurate. But they have been already shown to be themselves affected by an error of ±0°·31 F.; and the absolute error of the formula, then, may be either 0°·30 or 0°·92 F., according as the equation is made of the sum or the difference. Either of these quantities may in the theory represent the true error; and we have, therefore, in fine, the case of an even chance for accuracy with the formula or with the observation.

Such are the conclusions that arise from the comparison with M. Regnault's experiments.

2. *Experiments of the Franklin Institute.*—The temperatures were read by these observers to only the nearest quarter of a degree of Fahrenheit; they are therefore not comparable in precision, whatever they may be in accuracy, to those that have just been considered. And as but one reading either of temperature or pressure is given in each instance, they do not allow of being treated in the method that has just now been applied. I can only then compare them as in the following table:—

Pressures in atmospheres.	Temperatures.				Differences of Franklin Institution in formula and experiment.
	Calculated by my formula.	Observed by French Academy.	Observed by Franklin Institute.	Calculated by formula of Frank. Instit.	
1	212° F.	212° F.	212° F.	212° F.	°
2	250·84	250·52	250	248·8	+1·2
3	275·73	275·18	275	272·3	+2·7
4	294·43	293·72	291·5	290·1	+1·4
5	309·57	307·54	304·5	304·4	+0·1
6	322·36	320·36	315·5	316·5	−1·0
7	333·49	331·70	326	327·3	−1·3
8	343·36	341·78	336	336·4	−0·4
9	352·25	350·78	345	344·8	+0·2
10	360·36	358·88	352·5	352·5	
Mean differences	−1°·20	−3°·45	+0°·36		

The temperatures of the Academy in this table were not, as has been said already, from experiments at the precise epochs of pressure, but were interpolated from experimental terms not remote. Under a general principle, I excluded them from the comparative table in the preceding memoir; but they satisfied even the fastidiousness of M. Dulong, as representing accurately the results of observation, and are therefore fit to be compared as they are here. The last line of *mean differences* shows the excess of the formula-temperatures above those of the Academy to be not much more than *one-third* of the excess of the latter above those of the Franklin Institute; the probability of accuracy of these last, then, at most cannot be more than in the same ratio. It also shows a mean error between the formula adopted by these observers and their observations, of 0°·36; by which deviation the former, with the advantage of having the two extremes arbitrarily to coincide, yet fails to adjust itself to the latter.

3. *Experiments of the French Academy of Sciences.*—Out of the whole of this series M. Dulong has himself selected *eleven* as the most unexceptionable, and has used them for a standard

whereby to compare the merits of divers proposed formulæ as well as his own. It is true this group contains, among others, one of the very experiments which I have in the preceding memoir noted as faulty, and as being differently recorded in the two tables; but I have not allowed myself to exclude it here any more than before. Also for his comparison, M. Dulong has taken the reading of only the maximum thermometer, which represented the actual temperature of the *water* in the boiler. For the present purpose, however, as in the case of M. Regnault's experiments, it is necessary to take the other thermometer-reading also and register the variation of the extremes, as under. I have entered the pressures here in English inches, since they have already been reduced for the comparative table; but to save unnecessary calculations, I retain the temperatures in Centigrade degrees.

Number of experiment.	Pressures in inches.	Temperatures.		Difference in degrees Cent.
		Maximum.	Minimum.	
	in.			
1	64·14	123°·7 C.	122°·97 C.	0°·73
3	85·70	133·3	132·64	0·66
5	136·85	149·7	149·54	0·16
8	194·42	163·4	163·00	0·40
9	220·69	168·5	168·40	0·10
15	348·04	188·5	188·30	0·20
21	514·22	206·8	206·40	0·40
22	516·84	207·4	207·09	0·31
25	553·69	210·5	210·47	0·03
28	644·96	218·4	218·30	0·10
30	716·13	224·15	223·88	0·27
		Mean difference......	0°·3055 C. = 0°·55 F.	

equivalent to a mean error of ±0°·28 F., arising from the uncertainties of temperature.

To contrast this with the formula, we have from the same experiments as under:—

Number of experiment.	Temperatures.		Differences.
	Observed.	By the formula.	
1	123°·7 C.	123°·9 C.	−0°·2 C.
3	133·3	133·8	−0·5
5	149·7	150·8	−1·1
8	163·4	164·5	−1·1
9	168·5	169·5	−1
15	188·5	189	−0·5
21	206·8	206·8	0
22	207·4	207·2	+0·2
25	210·5	210·4	+0·1
28	218·4	217·7	+0·7
30	224·15	222·9	+1·2
	Mean difference......	−0°·2 C. = −0°·36 F.	

a deviation between the formula and the experiment but little more than the admitted error of the thermometric readings. The mean error of observation from this last source was found just now to be $\pm 0^{\circ}\cdot 28$ F., and the mean error of the formula then may be either $0^{\circ}\cdot 08$ or $0^{\circ}\cdot 64$ of Fahrenheit. These quantities equally satisfy the equation, and the probabilities in favour of each are even.

It is observable that errors in this series come out with a different sign from those of M. Regnault, though the errors of observation in the two experimental series are nearly identical, as might be expected in advance from the great skill and probably equal tact of the two observers. Such a difference of sign is favourable to the character of the formula, which will be seen by combining the two results, as under:—

	Error of formula.	
	Maximum.	Minimum.
From experiments of Academy of Science...	$-\overset{\circ}{0}\cdot 64$ F.	$-\overset{\circ}{0}\cdot 08$ F.
From experiments of M. Regnault	$+0\cdot 92$	$+0\cdot 30$
Mean error by both	$+0\cdot 14$	$+0\cdot 11$

The nearness of these limits, and the smallness of the number inclosed by them, warrant, I think, a sole and entire reliance upon the formula in the present state of experimental knowledge on the subject. I do not introduce into combination any of the other and earlier series of observations; because, from the way in which they have been reported by their respective authors, they do not admit the application of the same methods of comparison; and because it may justly be supposed that the apparatus, intellectual and mechanical, resorted to in 1829 and since, is paramount in accuracy to what had been at disposal in preceding researches.

I shall only, therefore, in further illustration of the present formula, compare its results with those of expressions that have been proposed by other mathematicians; only extending, in point of fact, for this purpose, a similar comparison which MM. Dulong and Arago have already instituted; and using, except for the last column, quantities from the calculation of these philosophers. Their table is founded upon the same eleven observations of their own, just now quoted; and they have given for each instance the individual deviations of the several formulæ from the result of experiment. Not to employ so much room, I have thought it equally satisfactory to give the general results and inferences, as under. The deviations are given in Centigrade degrees, and belong to the same side of the equation with the temperatures given by the respective formulæ.

	Formulæ proposed by				
	Tredgold.	Roche.	Coriolis.	Dulong.	J. H. A.
Maximum deviation in excess	$0^{\circ}.69$	$0^{\circ}.53$	$0^{\circ}.80$	$0^{\circ}.40$	$1^{\circ}.10$
Maximum deviation in defect	2·11	0·75	0·25	0·73	1·20
Mean deviation without regard to signs	0·79	0·37	0·44	0·24	0·60
Mean deviation with regard to signs...	+0·338	−0·001	−0·363	−0·007	−0·20

The last formula, as is seen, although the sum of its deviations is greater than two or three of the others, lies yet more symmetrically with the curve of the experiments than any.

The three first are given by M. Dulong, after a copious enumeration of different formulæ, as agreeing the best with observation. Of these, in that of Tredgold and of Coriolis, the elasticity is a function of the temperature; but M. Coriolis uses, instead of an integral, a mixed fractional index. His exponent, instead of 7 as Dr. Young took, or 6 as Creighton and Tredgold preferred, or 5·13 as Southern chose, or 5 as Dulong adopted, is 5·335; whose coincidence with the natural law is only empirical, and can be but accidental. In the formula of M. Roche (which he offers, not as a means of interpolation, but as the expression of a general physical law), the temperature is itself an element of the index by which certain constant quantities are to be involved. The principles, however, upon which he has founded the expression, are disapproved both by M. Dulong and M. Regnault. The formula of M. Dulong presents a smaller aggregate deviation than any of the others; and it would be singular if it did not, seeing that it was derived from a constant furnished by his own experiments. But as might also be anticipated, this constant, taken (to four places of decimals) from the result of the highest experimental temperature, fails to apply in the lower ones. The maximum deviation under his formula, given in the last table, occurs at the lowest experimental temperature; and in fact in his final table of atmospheric pressures and corresponding temperatures, he has preferred, below the limit of four atmospheres (145°·4 C. or 293°·72 F.), to abandon his own formula and use that of Tredgold. Below the ordinary atmospheric pressure his quantities are utterly inapplicable, as will be seen by the following statement:—

Pressures in atmospheres.	Temperatures (Centigrade).			
	Observed by Regnault.	Calculated by formulæ of		
		Dulong.	Franklin Instit.	Alexander.
0·047368	32°·38	36°·16	33°·52	29°·80
0·006	0·00	10·45	4·28	− 1·08
0·000684	−25·00	− 7·25	−17·31	−23·89

The last two columns are added here for illustration; and show, among other things, that the formula of the Franklin Institute is, like that of the French Academy, inapplicable to low temperatures and pressures.

Later than these, M. Biot, in 1839, proposed another formula, and in 1841 published a table calculated by it, in which the pressures are given in metres and for every degree Centigrade from −20° to 220° C., corresponding to the limits of −4° and 428° F. The patient labour requisite for this task has not been overrated by its distinguished performer; as can be readily appreciated, when it is known that part of the calculations actually were, and it was apprehended that even the whole might require to be, executed with logarithms of eleven decimals, and that the constants reach even the twelfth decimal place. These constants were derived, for the higher temperatures, from the already quoted experiments of Dulong and Arago and of Taylor; and for the lower, from unpublished experiments of M. Gay-Lussac. The temperatures are throughout given in terms of an *air*-thermometer instead of a mercurial one, a modification which undoubtedly impresses a more systematic accuracy upon the method; but yet, in spite of the aid afforded by tabular corrections for reduction, appears to diminish materially the chances of practical resort to the table itself. These temperatures M. Biot, in the form first proposed by Prony, (the same which Dr. Young, with more emphasis than reflection, has called "ridiculously complicated,") employs as the exponents of a series; the peculiarity of the method, however, is in that the direct numerical result of the equation gives, instead of the pressure itself, the tabular logarithm of the pressure. It is therefore essentially a logarithmic formula.

I present the following comparison between it and the present formula, applied to the same instances of experiment, which have been already signalised by M. Dulong himself, and already quoted here. To save both the tedium and hazard of a reduction to English measures, I leave the quantities under their original denominations; and, in so far varying from the preceding instances, I give the deviation of the formulæ in terms of the pressure instead of the temperature. This method enables me, by an easy and safe interpolation, to extract the proper quantities from M. Biot's table, and thus to avoid the portentous labour of working out the numerical transformation of his theorem.

Number of experiment.	Temperatures (Centigrade).		Pressures in metres.		
	Mercurial thermometer.	Air-thermometer.	By Biot's formula.	Observed by Dulong and Arago.	By present form.
1	123°·7	123°·13	1·65020	1·62916	1·62022
3	133·3	132·47	2·26396	2·1816	2·14687
5	149·7	148·41	3·47146	3·4759	3·37449
8	163·4	161·69	4·94220	4·9383	4·80439
9	168·5	166·63	5·60263	5·6054	5·45121
15	188·5	185·96	8·89046	8·840	8·73476
21	206·8	203·60	13·05578	13·061	13·04525
22	207·4	204·18	13·21410	13·137	13·21202
25	210·5	207·16	14·05179	14·0634	14·10266
28	218·4	214·75	16·36717	16·3816	16·60100
30	224·15	220·28	18·18048	18·1894	18·64254

Not to embarrass this table with so many columns, I omit the individual deviations of the two formulæ, and present the general result as under.

	Biot.	Alexander.
Mean deviation from experiment without regard to signs . .	0·02566	0·12191
Mean deviation from experiment with signs	−0·01704	−0·02114

It is hardly necessary to repeat that the first of these formulæ is founded in part upon the very experiments with which it accords so well, and that the other was not.

The table of M. Biot goes up as far as 220° C.; but he supposes that his formula is applicable much further; and in fact he has given results, in a small supplementary table, as high as 300° C. or 572° F., at which temperature it makes the pressure equal to almost exactly eighty-five atmospheres. The present formula would make, corresponding to this pressure, a temperature of 559°·92 F. or 293°·3 C., differing from the other within the correction between a mercurial and an air-thermometer.

It is at the other extremity, where we still have opportunity of referring to experiment, that the difference between the two formulæ becomes more marked; and where that of M. Biot, neither in its terms nor its progression, can be considered applicable. This may be seen as under:—

Pressures in inches of mercury.	Temperatures (Centigrade).		
	Observed by Regnault.	B.	A.
in.			
0·024	$-23\overset{\circ}{}\cdot83$	$-2\overset{\circ}{0}$	$-22\overset{\circ}{}\cdot46$
0·033	−20	−17	−19·52

About the same time with M. Biot, other formulæ claiming (like that of M. Roche) a foundation on abstract theoretical principles were proposed by Mr. Russell, who has also applied their somewhat extensive logarithmic apparatus to the calculation of a table of pressures for each degree from 32° to 250° F., and then for intervals of one or more atmospheres up to fifty. This does not properly come into this discussion, because the author has found it necessary to employ different terms above and below the point of boiling water, and in point of fact to have *two* formulæ; an inconvenience, the same in kind though not in degree, with what the object of the very research is to avoid. Nor do they counterbalance this by a proportionate accuracy which would warrant their results to be substituted for those of experiment. On the contrary, starting from their common zero, 212°, they both deviate in their respective directions from the curve given by observation; the pressures calculated by them are, at the two extremities, very much above any experimental ones. Not to trouble ourselves with the part of the scale below the boiling temperature, where the errors are not of so much practical importance, I give a few instances in the higher degrees, contrasted with the results of the French Academy.

Pressures in atmospheres.	Temperatures (Fahrenheit).		Differences.
	French Academy.	Russell's table.	
1	212°	212°	
5	307·5*	306·8	0°·7
10	358·9	355·6	3·3
20	418·5	410	8·5
30	457·2	444·6	12·6
50	510·6	491·4	19·2

The formulæ of M. Regnault, to whose experimental researches such resort has been had, are in one respect in the same category as those of Mr. Russell: they are *three*; one adapted to pressures below the melting of ice, the second

* Mr. Russell, in his comparison, as well as the Franklin Institute in theirs, give this temperature at 308°·8; an error which has arisen from hastily reducing the actual Centigrade temperature of 153°·08, as if it were 153°·80.

reaching from that point to the ordinary atmospheric pressure, and the last proper for high temperatures only. He promises, when his experiments in this upper part of the thermometric scale shall have been sufficiently extended and accumulated, to apply himself to the grouping of all three divisions in one comprehensive expression; and from his well-known character much may be expected, original and appropriate. In the mean time it would be premature to enter here upon any discussion of what is only provisional.

To resume, then, in conclusion of this rather protracted memoir; it seems to me that in the various combinations and comparisons that have been given, the claim of the formula I propose is reasonably well-established, not to be an expression of a law of nature, for to this much it makes no pretension, but to represent the phænomena of reciprocal pressures and temperatures more exactly and with a more extensive scope, than any that has yet been offered; and that in so far it is worthy of being taken as paramount to all that have preceded it. How far, in view of the discord yet existing between experimental results of the most recent and reliable observers, it is fit to come in as a substitute for any and all of those results themselves, is not of course for me to determine. I shall only allow myself to notice, then, its remarkable simplicity, and the consequent facility with which it adapts itself to calculation, either with or without logarithms; as well as the readiness with which, from its elements and form, it suggests itself at all times to the memory. One important use of a formula, it is to be observed, is in enabling an inquirer in any emergent case, away from books and tables, to extemporise an accurate result; in proportion to its complexity and arbitrariness, then, it becomes a question of individual strength of memory, and its resort more and more limited. In the present instance all its terms are either given in the very case to be solved, or are physical constants at the foundation of the theory of heat, which I may even say it is impossible for one ordinarily well-informed to forget. And the composition of these terms, thus susceptible of instant recall to the mind, is so plain and necessary even, that it is equally impossible, with a moment's reflection, for one to go wrong. I believe I am only stating the simple fact when I say that, in these respects, the present formula stands alone.

Finally, then, I offer for general practical use and reference the following table of temperatures corresponding to pressures in atmospheres and parts through the whole range of experiment hitherto. If my labour in so far shall be fortunate enough to meet with the approval of the learned, it will be but an inconsiderable task hereafter to complete its scope by furnishing a table of pressures at all useful temperatures for each degree of Fahrenheit's thermometer, to whose arbitrary and otherwise inconvenient scale the present investigation has served not a little to reconcile me.

Table of Temperatures corresponding to the Pressures of Steam in Atmospheres.

Pressures in atmospheres and parts.	Temperatures in degrees of Fahrenheit.	Pressures in inches of mercury at 32° F.	Pressures in lbs. avdp. per square inch, mercury sp. gr. 13·6.
	°	in.	lbs.
0·00025	−25·53	0·0075	0·0037
0·0005	−15·78	0·015	0·0074
0·001	− 4·84	0·030	0·0147
0·005	+26·01	0·15	0·0735
0·01	42·41	0·30	0·1470
0·05	87·36	1·50	0·7352
0·10	110·93	2·99	1·4704
0·20	137·39	5·98	2·9407
0·25	146·58	7·48	3·6759
0·50	177·40	14·96	7·3518
1·	212·00	29·92	14·7036
2·	250·84	59·84	29·41
3·	275·73	89·76	44·11
4·	294·43	119·68	58·81
5·	309·57	149·60	73·52
6·	322·36	179·52	88·22
7·	333·49	209·44	102·93
8·	343·36	239·36	117·63
9·	352·25	269·28	132·33
10·	360·36	299·20	147·04
11·	367·81	329·12	161·74
12·	374·72	359·04	176·44
13·	381·16	388·96	191·15
14·	387·20	418·88	205·85
15·	392·90	448·80	220·55
16·	398·28	478·72	235·26
17·	403·40	508·64	249·96
18·	408·27	538·56	264·66
19·	412·91	568·48	279·37
20·	417·36	598·40	294·07
21·	421·62	628·32	308·78
22·	425·73	658·24	323·48
23·	429·67	688·16	338·18
24·	433·48	718·08	352·89

XIII. *On the Remainder of the Series in the development of* $(1+x)^{-n}$, *and on a Theorem respecting the products of Squares.* By J. R. Young, *Professor of Mathematics, Belfast*.*

IN the last Number of the Philosophical Magazine, there is a very interesting paper, by Professor Graves, On the Calculus of Operations, in which he has communicated a valuable theorem in that important department of analysis, which I believe has not hitherto appeared in a complete form.

Professor Graves has been enabled to deduce this theorem from the previous development of $(1+x)^{-n}$; which, by means of the differential calculus, he has exhibited in connexion with the *remainder* of the series.

This completed form of the expansion may be readily obtained by a process imitative of that employed in my paper published in the November Number of this Journal, and without involving any operation of a more advanced character than that of common algebraical division. It is as follows:—

As in Professor Graves's notation, let

$$A_n = \frac{n(n+1)\ldots\ldots(n+m-2)}{1.2\ldots\ldots\ldots(m-1)}$$

$$= \frac{m(m+1)\ldots\ldots m+n-2}{1.2\ldots\ldots\ldots(n-1)}.$$

Put also

$$(1+x)^{-1}(-x)^m = R_1,\ (1+x)^{-2}(-x)^m = R_2,$$
$$(1+x)^{-3}(-x)^m = R_3,\ \&c.;$$

then, since

$$(1+x)^{-1} = 1 - x + x^2 - \&c.\ldots\ldots(-x)^{m-1} + R_1,$$

we shall have, by dividing the terms on the right severally by $(1+x)$, the following rows of results, namely,

$$\begin{aligned}(1+x)^{-2} = 1 - x + x^2 - x^3 + \ldots\ldots(-x)^{m-1} + R_1\\ -x + x^2 - x^3 + \ldots\ldots(-x)^{m-1} + R_1\\ +x^2 - x^3 + \ldots\ldots(-x)^{m-1} + R_1\\ -x^3 + \ldots\ldots(-x)^{m-1} + R_1\\ \&c. \qquad \&c. \qquad + R_2;\end{aligned}$$

that is,

$$(1+x)^{-2} = 1 - 2x + 3x^2 - 4x^3 + \ldots\ldots A_2(-x)^{m-1} + A_2R_1 + R_2.$$

Similarly,

* Communicated by the Author.

$$(1+x)^{-3}=1-3x+6x^2-10x^3+\ldots A_3(-x)^{m-1}+A_3R_1$$
$$\vdots \qquad +A_2R_2+R_3$$
$$\vdots$$
$$(1+x)^{-n}=1-nx+\frac{n(n+1)}{1.2}x^2-\&c.\ldots A_n(-x)^{m-1}+A_nR_1$$
$$+A_{n-1}R_2+A_{n-2}R_3+\ldots+A_1R_n,$$

the A_1 being introduced before R_n for the sake of uniformity of notation: its value is evidently unit. When R_r is replaced by $(1+x)^{-r}(-x)^m$, this result becomes the same as that in Professor Graves's paper; and it must certainly strike a reader as a circumstance worthy of notice, that an expression thus obtained by aid of only the first principles of algebra, should virtually involve a theorem of such interest in the higher researches of analysis as that given in the paper alluded to.

I fear, from the remark in the first paragraph of that paper, that I must have expressed myself somewhat obscurely in reference to the Calculus of Operations making "no provision for the correction" I had adverted to as necessary. I think I ought to have added, that this provision should always be furnished by the theorem for quantity whence that for operations is derived.

I take this opportunity of mentioning that the general form of the theorem respecting squares, namely,

$$\Sigma_{16}q'\Sigma_{16}q''=\Sigma_{32}q^{\backprime},$$

which the Rev. Mr. Kirkman has done me the favour to insert at page 500 of the last volume of this Journal, I should prefer to have appeared in the following more comprehensive shape:

$$\Sigma_{8m}q'\Sigma_{8n}q''=\Sigma_{8mn}q^{\backprime},$$

to which may be added the analogous theorems

$$\Sigma_{4m}q'\Sigma_{4n}q''=\Sigma_{4mn}q^{\backprime}$$
$$\Sigma_{2m}q'\Sigma_{2n}q''=\Sigma_{2mn}q^{\backprime},$$

where m and n are any positive whole numbers whatever.

Belfast, Jan. 13, 1849.

Note. I submitted the substance of the foregoing investigation to Professor Graves, who, in reply, did me the favour to communicate to me a sketch of two other methods of arriving, algebraically, at the same result: this I here give in his own words:—

* * * "I indicated the method of obtaining the remainder by differentiation, because that process admits of being described in the fewest words, though it is far from being the simplest. I know of two algebraical methods by which the result is obtained more easily.

"One follows the ordinary track; showing that, if the theorem holds for $(1+x)^{-n+1}$, it will hold likewise for $(1+x)^{-n}$. And this is readily proved by means of the fundamental property of the binomial coefficients; viz. that the algebraical sum of the coefficients of x^{r-1} and x^r in the development of $(1+x)^p$ is equal to the coefficient of x^r in the development of $(1+x)^{p+1}$.

"My other method having something peculiar in it, I shall give it in full. Using S to denote the sum of the first m terms in the development of $(1-x)^{-n}$, and R the remainder after S, we shall have

$$R=\frac{1-(1-x)^n.S}{(1-x)^n}.$$

Now, when we come to examine the numerator in this value of R, we find that it contains only powers of x, from x^m up to x^{m+n-1}. R may therefore be put into the form $x^m.f(1-x)^{-1}$, $f(x)$ being used to denote a series of n terms proceeding according to positive integer powers of x, from x up to x^n.

"We have now ascertained the *form* of the remainder about which we are inquiring, and it will be easy to determine the coefficients in f. For this purpose let us take the equation

$$(1-x)^{-n}=1+nx+\frac{n(n+1)}{1.2}x^2+\ldots+A_nx^{m-1}+x^m.f(1-x)^{-1},$$

and in it interchange x with $1-x$, and m with n. Then we shall have

$$x^{-m}=1+m(1-x)+\frac{m(m+1)}{1.2}(1-x)^2+\ldots+A_n(1-x)^{n-1}$$
$$+(1-x)^n f'x^{-1};$$

where $f'x^{-1}$ stands for a series of powers of x^{-1}, from x^{-1} up to x^{-m}. Multiply the last equation by $x^m(1-x)^{-n}$, and it will become

$$(1-x)^{-n}=x^m\left\{(1-x)^{-n}+m(1-x)^{-n+1}+\frac{m(m+1)}{1.2}(1-x)^{-n+2}\right.$$
$$\left.+\ldots+A_n(1-x)^{-1}\right\}+x^mf'x^{-1}.$$

On comparing the several terms of the two finite developments thus given for $(1-x)^{-n}$, it is obvious that we must have

$$x^mf'x^{-1}=S,$$

and

$$R=x^m\left\{(1-x)^{-n}+m(1-x)^{-n+1}+\frac{m(m+1)}{1.2}(1-x)^{n+2}\right.$$
$$\left.+\ldots+A_n(1-x)^{-1}\right\}."$$

XIV. *On a Mode of rendering Substances incombustible. By* Robert Angus Smith, *Ph.D., Manchester**.

I HAVE often been surprised that, considering the number of materials which will not burn and the small number which do burn, we should be compelled to build houses so liable without constant watchfulness to instantaneous destruction; that we should go also to sea in vessels made of a most combustible substance filled with enormous fires, frequently under the care of ignorant men. I think, therefore, I may be excused when I endeavour to add to a knowledge of the mode of rendering substances incombustible, or the theory of the mode to be sought after, even if the addition which I make be but a very small one.

Silicate of potash has been considered good. It is a soluble glass which was expected to cover the fibre of cloth or wood, and so protect it from heat. This does act to some extent, probably in the same manner as stones do when put into a fire of wood or coal; they take heat but give none, and are also bad conductors. If silicate of potash remained as a glass, it would act also by keeping out the air; but this does not seem to be the case, as it falls after a time to a powder.

It struck me that the mode of preventing combustion was not by protecting the wood from the fire merely, as heat must cause combustible gases to rise from wood, whether there be incombustible substances mixed with it or not, and these gases will force their way to the surface where there is no longer any preventive to burning. My object then was to find a substance which would render the wood unfit to burn, and would cause it to give out gases which would not burn; so that whilst the wood itself was being preserved, except where in contact with the fire, the gases would assist in extinguishing the fire.

I first tried phosphate of magnesia and ammonia, thinking the ammonia given out would be of use in extinguishing the fire; but this was of no value, as a piece of calico required to be made quite stiff with it before it was rendered incombustible. The calico was prepared by dipping in a solution of phosphate of magnesia in muriatic acid and then in ammonia. It seemed to me that the earthy salts are of little use for the purpose required, and that the amount of solid matter incapable of evaporation left on the cloth, assists in a very small degree.

Sulphuric acid, however, seemed to present the most promising characteristics of a substance incapable of burning, and of acting so strongly on vegetable substances as to make them

* Communicated by the Author, having been read before the Manchester Literary and Philosophical Society, January 9, 1849.

incapable of burning. Sulphuric acid itself is a body perfectly burnt, or we may say overburnt, having an atom of oxygen given to it by artificial means, so to speak, which atom is difficult to separate, and therefore not resembling the oxygen of many highly oxidized bodies. It requires a high degree of heat to raise it to vapour; and the vapour formed is sluggish and heavy, remaining long where formed, and quenching flame wherever it is. It destroys the texture of wood also and other vegetable substances, causing them to give out after a time gases which do not burn, mixed with some which do burn; but if there be enough of acid, forming a mixture which does not burn. The wood also cannot be again induced to become combustible until it be heated to redness, so as to remove all the sulphuric acid, leaving only charcoal.

If sulphuric acid then could be introduced into wood just at the time that the fire was going to take place, the fire would cease to take place; and this we can do easily by saturating the wood with sulphate of ammonia. When there is no fire present there is no sulphuric acid present, as such; but as soon as the heat rises, ammonia goes off, and sulphuric acid is instantly presented to the wood. The ammonia does not come off quite pure, it is mixed with nitrogen and sulphurous acid; and this disengagement of gases is of advantage in extinguishing fire; when the heat rises to 536°, the sulphuric acid is then left to act on the wood in part and to volatilize in part, and that which I have mentioned takes place. The outside of course would first undergo the change, and the inside would be protected by the incombustible outer part; if the fire continued to act long, the inner layer would undergo a similar change. I imagine, then, the acid acts in a double manner; it makes the wood refuse to burn and it puts out fire. As sulphurous acid is given off in this process, the action is also similar in one point of view to that of sulphur, which has long been used for putting out fire in chimneys.

I have no doubt that a house built of wood prepared in this manner might have a fire lighted on the wooden floor without danger, burning only on the spot to which the fire was limited. A ship also would be safe, even if the cinders did fall from the grate in stormy weather.

I know that muriate of ammonia has been used, and that it acts very well; but I think the sulphuric acid is superior, the ammonia being merely to keep it innocent; any other volatile base might do. I am sorry, however, that this is not perfect; its solubility in water is a great disadvantage, as it cannot be applied to clothes to be frequently washed. True, it is so cheap that it might be applied every washing where there are peculiar dangers; but if a person were standing very near the fire, the ammonia would in part be evaporated, and the acid remaining would be enough to injure the fabric. There are however cases, such as curtains, to which this could not apply, and where it would be valuable.

Sir William Burnet's liquid is chloride of zinc: he uses it for preserving wood and canvas, and also for preventing fire. I am certainly surprised that more use has not been made of it, being, as far as I have seen it, so efficient. I believe the manner in which the chloride of zinc acts is very similar to that of the sulphuric acid, destroying the organic matter on the approach of heat, and rendering it incombustible. It can be introduced into wood at a specific gravity of 2000, I believe; sulphate of ammonia cannot easily be used above 1200. By heating the solution more may be attained. Sulphate of ammonia is cheap and easily procured and used, not hurting anything with which it may come in contact, and therefore more easily managed in households.

The chloride of zinc is said to unite with the fibre. This cannot be said for the sulphate of ammonia. It would not, however, come from the centre of a beam of wood, even if immersed in water, as the water enters with great difficulty into wood; and the solution itself cannot be introduced without forming a vacuum in the saturating vessel, and so removing all the air from the wood.

The first time I used this solution I found a large quantity of mould formed, and indeed it contains all the elements to increase its growth. The second time the solution was boiled in an iron vessel, and no mould formed on it; on the contrary, mould was destroyed by it. The sulphate of ammonia dissolves iron rapidly, and forms a double salt which is deleterious to such growths. I imagined any other metallic salt would do, and used ordinary chloride of manganese prepared in the laboratory, which killed all such fungi rapidly, and no more have grown after standing eleven months in contact with organic matter.

I believe there are many ways in which this may be used. My wish was to find a substance suited for building fire-proof ships, and I believe this would do; at any rate the ships would be fire-proof, experience could alone tell if any other objection followed. It does not render the wood hard, heavy or brittle.

I believe it would be of the greatest advantage in mills, which now suffer so much from fire, diminishing or rather entirely removing the expense of insurance. It does not hurt colours; so that even coloured goods might be dipped when kept long in one place, or when sent in vessels abroad. Possibly some delicate colours may be attacked, but this must be a rare case.

I am more desirous of seeing ships built of an incombustible material, the means of escape at sea being few, and confined to few; and whilst there is any hope of doing it easily, I scarcely think it proper for any one to neglect what information may exist on the subject.

XV. *On a system of Triple Algebra, and its application to the Geometry of three Dimensions. By the* Rev. CHARLES GRAVES, *A.M., Professor of Mathematics in Trinity College, Dublin*.*

WHETHER we can directly evolve out of the ordinary double algebra such a more general algebra as will answer all the requisitions of the geometry of three dimensions, is a question into which I am not about to enter at present. But Mr. Cockle's discussion of it in the last Number of the Philosophical Magazine, and his use of an imaginary whose square is equal to positive unity, have reminded me of a system of triple algebra which I devised some time ago, and which, I am inclined to believe, will find more favour with others than it has done with myself. The outline of it was stated in a letter addressed by me to the late Professor MacCullagh in April 1845, since which time I have suffered it to lie without further development or application. For my own part, I commend the superior power, symmetry, and flexibility of Sir William Hamilton's quaternion theory, of the excellence of which use has only more and more convinced me. I find, however, that there are mathematicians who continue to object to it on the ground that they cannot conceive the nature of the operations by which his symbols i, j, k transform extra-spatial, scalar quantity into directed linear magnitude; and who protest against the sacrifice of the commutative character of multiplication. To those who cannot bring themselves to waive these objections, I submit the following system. It has the advantage of being easily and completely interpreted; and I may add that, in the treatment of some geometrical questions, it has proved itself not inferior even to the quaternions.

1. Imagine an operation denoted by the symbol (s), and of such a nature that—

1. $s(1)$ is a quantity not homogeneous with the real unit, so that no equation of the form $s(a)+b=0$, where a and b are real, can subsist, unless a and b be separately equal to zero.

* Communicated by the Author.

2. It is distributive; therefore

$$s(a)+s(b)=s(a+b)=(a+b)s(1).$$

3. It is a periodic operation of the second order; that is to say, $s^2(a)=a$.

2. The nature of the operation being so far defined, let us take two mixed binomials, $x+s(y)$ and $x'+s(y')$, and operate with either upon the other; for it matters not which is operator and which operand; the result of the operation will be

$$(x+s(y))(x'+s(y'))=xx'+yy'+s(xy'+yx');$$

and putting

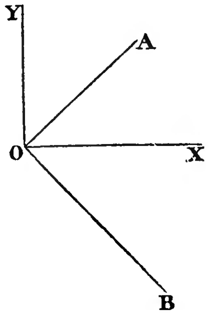
$$x''=xx'+yy', \text{ and } y''=xy'+yx',$$

it is easy to see that we shall have

$$\left.\begin{array}{l}(x+y)(x'+y')=x''+y'' \\ (x-y)(x'-y')=x''-y''\end{array}\right\} \quad . \quad . \quad . \quad . \quad (1.)$$

These are the *modular* equations of multiplication in the system of double algebra, with which we are at present concerned.

3. There is no difficulty in assigning a geometrical interpretation to the symbol s.



Draw in a plane the two rectangular axes of co-ordinates OX and OY, and bisect the angle between them by the right line OA. Then the symbol s may be taken to express *rotation from left to right through an angle of* 180° *round the axis* OA.

Supposing that the real unit be placed upon the axis of x, it is evident that this geometrical representation fulfils the conditions imposed upon s. For, in the first place, $s(1)$ lies upon the axis of y; and being at right angles with the real unit, is as much distinct from it as $\sqrt{-1}$ is. Next, this rotation is plainly a distributive operation. Lastly, it complies with the third condition; seeing that a repetition of the rotation through 180° brings the real unit back again into its original position.

4. We may now agree to represent the line drawn from the origin to the point whose rectangular co-ordinates are x and y, by the binomial $x+s(y)$. The first consequence of this will be, that the sum of two lines will be represented by the diagonal of the parallelogram whose sides are the lines to be added. In fact, analogy so strongly demands this, that it is a

part of almost all modern systems of symbolic geometry. If we next proceed to inquire how we may represent the product of two lines (xy) and $(x'y')$, we shall find the following rule for constructing it: "*The projections of the real unit line, the factor lines, and the product line, either upon the axis* OA, *or upon* OB, *a right line perpendicular to it, form an algebraic proportion.*" This is the geometrical interpretation of the equations (1.).

5. So far we have been geometrizing only *in plano*; but we can pass readily into the geometry of three dimensions, since in the course of its rotation round OA the real unit quits the plane of xy.

As s denotes the rotation of 180° round OA, $s^{\frac{1}{2}}$ may be taken to denote half that rotation.

Again, the unit of length in the direction of OA is

$$\frac{1+s(1)}{\sqrt{2}};$$

and the unit of length in the perpendicular direction is evidently

$$\frac{1-s(1)}{\pm\sqrt{2}}.$$

Now if this latter unit be operated upon by $s^{\frac{1}{2}}$, it will be brought into the position of the axis of z, perpendicular to the axes both of x and y; that is to say,

$$\frac{s^{\frac{1}{2}}(1-s(1))}{\sqrt{2}}$$

is the positive z-unit, which we shall henceforth denote by $n(1)$.

For the square of this z-unit we shall find a simpler expression. Squaring the equation

$$\frac{s^{\frac{1}{2}}(1-s(1))}{\sqrt{2}}=n(1),$$

we have

$$s(1)-1=n^2(1);$$

and operating upon the preceding equation with s, we find

$$sn(1)=-n(1).$$

6. Having now ascertained the laws of the combination of s and n, we may proceed to deal with trinomials of the form $x+s(y)+n(z)$, which we shall take to represent the line drawn from the origin of rectangular co-ordinates to the point (xyz).

As before, the sum of two lines is their *resultant*. If

$$x''+s(y'')+n(z'')=(x+s(y)+n(z))(x'+s(y')+n(z')), \quad (2.)$$

we shall have

$$\left.\begin{aligned} x'' &= xx' + yy' - zz' \\ y'' &= xy' + yx' + zz' \\ z'' &= (x-y)z' + z(x'-y') \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad (3.)$$

From these relations it would not be difficult to determine directly the mode of constructing the product line; but the result will be more readily arrived at by means of the following method.

Let ξ, η, ζ be the projections of the line (xyz) upon OA, OB, and OZ the axis of Z. Then l, m, n, the unit lines upon these axes, are respectively expressed in terms of s and $s^{\frac{1}{2}}$ by means of the equations

$$l = \frac{1+s(1)}{\sqrt{2}}, \quad m = \frac{1-s(1)}{\sqrt{2}}, \quad n = \frac{s^{\frac{1}{2}}(1-s(1))}{\sqrt{2}};$$

and we may write

$$l\xi + m\eta + n\zeta$$

instead of

$$x + s(y) + n(z).$$

And for the laws of combination of the imaginaries l, m, n, we have the equations

$$\left.\begin{aligned} l^2 &= \sqrt{2}.l, & m^2 &= \sqrt{2}.m, & n^2 &= -\sqrt{2}.m, \\ lm &= 0, & ln &= 0, & mn &= \sqrt{2}.n \end{aligned}\right\}. \quad (4.)$$

Hence the product of two lines $(\xi\eta\zeta)$ and $(\xi'\eta'\zeta')$ is

$$\sqrt{2}.l\xi\xi' + \sqrt{2}\{m(\eta\eta' - \zeta\zeta') + n(\eta\zeta' + \zeta\eta')\}.$$

Now the part of this which is on the axis OA, viz. $\sqrt{2}.l\zeta\xi'$, is in length a fourth proportional to the projections of the real x-unit and the two factor lines upon that axis.

So again the part

$$\sqrt{2}\{m(\eta\eta' - \zeta\zeta') + n(\eta\zeta' + \eta'\zeta)\},$$

which lies in the plane perpendicular to OA, is, in Mr. Warren's sense of the word, a fourth proportional to the projections upon that *plane* of the same three lines.

7. From the geometrical interpretation which has been assigned to the symbol s, we can derive what seems to me to be a satisfactory explanation of the vanishing of the product of the factors $1-s(1)$ and $1+s(1)$, although these factors are each different from zero.

In consequence of s being distributive, the expression $(1-s(1))(1+s(1))$ means the difference between $1+s(1)$ and $s(1+s(1))$. Now, as the line $1+s(1)$ coincides with the axis OA, round which rotation takes place, it is unaffected by any amount of such rotation, and so may be considered equal to $s^p(1+s)$, where p is any real quantity whatsoever. There is therefore no difference in either magnitude or direction between the lines $1+s(1)$ and $s(1+s(1))$; and we are entitled to put

$$(1-s(1))(1+s(1))=0.$$

8. I think the following will be found to be the true theory of the vanishing of factors and products.

In a system of algebra in which there is but one real modulus of multiplication, a factor and its modulus vanish together. The vanishing of a factor will cause the vanishing of a product into which it enters along with other finite factors; and conversely, the vanishing of a product indicates the evanescence of one of the factors. This rule applies to the ordinary double algebra, whose units are 1 and $(-1)^{\frac{1}{2}}$, and also to Sir William Hamilton's quaternion algebra of four units, 1, i, j, k.

But systems of algebra may be constructed in which the case is different. The process of operating with one mixed quantity upon another—that process, in fact, which, on the score of analogy, seems to claim the title of multiplication—may lead us to regard a mixed quantity as having more than one real modulus: and these moduli, suppose n in number, need not all vanish simultaneously. When they do, the quantity to which they belong vanishes likewise; but we cannot say that it does so unless its different moduli of multiplication are separately equal to zero. Such a factor entering, along with other finite factors, into a product, annihilates it by annihilating all the n real moduli of the product. On the other hand, the product is not reduced to zero unless all its real moduli vanish; and this cannot take place unless, amongst all the moduli of all the factors, there be n, *of different kinds*, separately equal to zero. These n vanishing moduli may in general be distributed amongst the factors in various ways without annihilating any one of them. What has been last said applies to the systems of double and triple algebra discussed in the present paper.

Operating with $x+sy$ upon $x'+sy'$, we found that the mixed quantity $x+s(y)$ had two real moduli of multiplication, $x+y$ and $x-y$. It vanishes if both these are equal to 0, but not otherwise. And $x''+s(y'')$, the product of these two factors, will not vanish unless both its real moduli likewise vanish. Now it appears from equations (1.) that this may happen in four different ways.

1. We may have $x=0$ and $y=0$; 2, $x'=0$ and $y'=0$; 3, $x+y=0$ and $x'-y'=0$; 4, $x-y=0$ and $x'+y'=0$. In

the first and second cases, a factor actually vanishes; but not so in the third and fourth. In these, two real moduli, of different kinds and belonging to different factors, becoming equal to zero, annihilate the two real moduli of the product, and so cause itself to vanish.

So again in the case of the trinomial $x+s(y)+n(z)$. It appears from the equations (3.) that it has two real moduli of multiplication, $x+y$, and $(x-y)^2+2z^2$. That is to say, if

$$(x+s(y)+n(z))(x'+s(y')+n(z'))=x''+s(y'')+n(z'');$$

we shall have

$$(x+y)(x'+y')=x''+y'',$$

and

$$((x-y)^2+2z^2)((x'-y')^2+2z'^2)=(x''-y'')^2+2z''^2.$$

The factor $x+s(y)+n(z)$ vanishes if both its real moduli vanish, but not otherwise. And the product $x''+s(y'')+n(z'')$ will not vanish unless both its real moduli likewise vanish. But this may happen in different ways.

1. We may have $x+y=0$, and $(x-y)^2+2z^2=0$; conditions equivalent to $x=y=z=0$, and therefore involving the annihilation of a factor.

2. $x'+y'=0$, and $(x'-y')^2+2z'^2=0$; conditions which, in like manner, annihilate the other factor.

3. $x+y=0$, and $(x'-y')^2+2z'^2=0$; equivalent to $x+y=0$, $x'-y'=0$, and $z'=0$.

4. $(x-y)^2+2z^2=0$, and $x'+y'=0$; equivalent to $x-y=0$, $z=0$, and $x'+y'=0$.

Here, as before, we see that the complete annihilation of the product may be brought about either by the complete annihilation of either factor, or by the vanishing of two moduli, of different kinds, occurring one in each of the two factors.

Similar observations apply to the system of triple algebra discussed by Professor De Morgan in the Transactions of the Cambridge Philosophical Society, vol. viii.; and to the closely related system, of which I have given a brief account in the Proceedings of the Royal Irish Academy, vol. iii. pp. 51 and 57.

In both the systems just referred to there are two real moduli of multiplication; and in both a product of two factors may vanish without the vanishing of either of the factors themselves.

9. It is not only when the two factors $1+s(1)$ and $1-s(1)$ come together that we meet with a singular result. The occurrence of either of them in a product affects it in a remarkable manner.

If we consider the two right lines which represent the quantities $x+s(y)+n(z)$ and $s(x+s(y)+n(z))$, we shall see that either of them is obtained from the other by causing it to turn round the axis OA through an angle of 180°. The sum of the two, therefore, compounds a line in the direction of that axis; and their difference is a right line in the plane perpendicular to OA. Thus it appears that the product $(1+s(1))(x+s(y)+n(z))$ denotes a right line, which, as well as the factor $1+s(1)$, coincides with the axis of rotation; whilst the line denoted by the product $(1-s(1))(x+s(y)+n(z))$ lies, like the factor $1-s(1)$, in the plane perpendicular to that axis. Here we have a geometrical interpretation of the strange-looking results,

$$(1+s(1))(x+s(y)+n(z))=(1+s(1))(x+y)$$
$$(1-s(1))(x+s(y))=(1-s(1))(x-y)$$

10. Lest it should be supposed, however, that the explanations just given of these seemingly anomalous results have arisen accidentally out of the geometrical interpretation assigned to the symbol *s*, I proceed to show that these results admit of being explained by reference to the analytical conditions imposed upon *s* at the outset. I do so, because in the course of the inquiry we shall be led to notice some general principles useful in other systems of algebra as well as in the one before us.

On reviewing the operation which conducted to the result

$$(x+s(y))(x'+s(y'))=x''+s(y''),$$

we observe that this equation holds good, whatever distributive operation *s* may be, provided it satisfies the equation $s^2(a)=a$. Now this equation has two purely algebraic solutions $s=+1$ and $s=-1$. It follows, then, that we may write successively $+1$ and -1 instead of *s* in it, and so we obtain at once the two modular equations (1.). And further, whatever equation we find subsisting, in which *s* appears along with real quantities, it must continue to hold good for $s=+1$ and $s=-1$. Thus, for instance, from the equation

$$e^{s(x)}=\text{Hyp. cos } x+s \text{ Hyp. sin } x$$

we derive both

$$e^{x}=\text{Hyp. cos } x+ \text{ Hyp. sin } x$$

and

$$e^{-x}=\text{Hyp. cos } x- \text{ Hyp. sin } x.$$

Suppose now that we had before us some expression of the form $(1\mp s(1))\varphi(s, x, y, z)$; we might expect to find in all transformations of it the operator $(1\mp s(1))$ still remaining, or capable of being readily put, in evidence, because the expression vanishes for $s=\pm 1$. In other words, as s partakes of the character of ± 1, these two factors are of such a nature that either of them, like zero, assimilates the product into which it enters to itself. If both enter into the same product, they must make it like their own product $1-s^2(1)$, which, by the definition of s, is equal to zero.

11. The same kind of reasoning admits of a more interesting application to the problem of finding the moduli of multiplication in the triple algebra whose units are 1, s, and n; or in that other whose units are l, m, and n.

As the operation n is defined by the equation

$$n=\frac{s^{\frac{1}{2}}(1-s(1))}{\sqrt{2}},$$

it appears that, when s degenerates into $+1$, n becomes 0; and when s is equal to -1, n is equal to $\pm\sqrt{-2}$. In the former case the equation (2.) is reduced to

$$(x+y)(x'+y')=x''+y''.$$

In the latter we shall have

$$(x-y\pm\sqrt{-2}.z)(x'-y'\pm\sqrt{-2}.z')=x''-y''\pm\sqrt{-2}.z''.$$

Thus we obtain the two real moduli of multiplication of the system whose units are 1, s, and n.

12. Let us next pass to the consideration of the triple system, whose three units are l, m, and n, as defined by the equations (4.).

According as $s=+1$, or -1, these latter become

$$l=\sqrt{2},\quad m=0,\quad n=0;$$

or

$$l=0,\quad m=\sqrt{2},\ n=\pm\sqrt{-2}.$$

And, if we introduce these two systems of values successively into the equation

$$(l\xi+m\eta+n\zeta)(l\xi'+m\eta'+n\zeta')=l\xi''+m\eta''+n\zeta'',$$

we shall derive from it the following:

$$\sqrt{2}.\xi\xi'=\xi'',$$

$$\sqrt{2}(\eta\pm\sqrt{-1}.\zeta)(\eta'\pm\sqrt{-1}.\zeta')=\eta''\pm\sqrt{-1}\zeta'',$$

which furnish the modular relations belonging to this system of triple algebra.

Dublin, Jan. 12, 1849.

[To be continued.]

XVI. *On the Repulsive Action of the Pole of a Magnet upon Non-magnetic Bodies.* *By* Professor Reich *of Freiberg**.

THE repulsion which takes place, according to Faraday's recent observations, between the pole of a magnet and every diamagnetic substance, apparently with the exception of the atmosphere, is to my mind so new and surprising an exhibition of force, that probably some observations on the subject will be considered worthy of attention, even though they merely confirm what Faraday has found, if they exhibit this repulsion in a more easy and direct manner.

For these observations I employed a torsion-balance which had been arranged to determine the mean density of the earth. A horizontal wooden rod two metres in length is suspended by means of a copper wire to a strong iron beam fastened into a massive wall, and at each of its extremities a metallic ball is suspended by a fine wire. The whole is inclosed in a wooden case, which, however, is nowhere in contact with the torsion-balance. The torsion-rod carries a mirror, in which the position of the rod is observed with a telescope upon a distant scale. The force required to deflect the rod a certain quantity from its position of rest results from the following expressions, which will likewise show the very great sensitiveness of the apparatus. The mass of the whole moveable portion of the torsion-balance reduced to the central point of one of the two balls is $=q=1031560$ milligrammes. The distance of the central point of either ball from the axis of rotation is $=r=10005$ millimetres. The horizontal distance of the mirror from the scale which is divided into millimetres is $=\mu=42827$ millimetres; if, therefore, we suppose the deflection of the ball from its position of rest to be $=$A millimetres, and the number of divisions of the scale corresponding to this deflection to be $=$B millimetres, then

$$A=\frac{r}{2\mu}B=\frac{10005}{85654}B,$$

and the force which deflects the ball A millimetres from the position of rest, with a time of vibration $=$ N seconds,

$$\frac{q.A}{N^2 l}=\frac{rq.B}{2\mu.N^2.l}.$$

l being the length of the seconds' pendulum in millimetres. When the torsion-rod vibrates without any external action upon the balls, N is very nearly $=350$ seconds, which gives a deflecting force of 0·00098956 B milligrammes. But B may

* From Poggendorff's *Annalen*, vol. lxxiii. p. 60.

be estimated to a tenth, and the force therefore to 0·0001 of a milligramme.

I first tried the effect of magnets upon one of the balls which had been employed in the determinations of density, and which consisted of tin mixed with 10 per cent. of bismuth and about 2 per cent. of lead. Magnet bars on being brought up in a horizontal direction to the case near a ball produced a very distinct repulsion, both when the north and the south pole were brought near. But when several similar bars were brought near, half with their north and the other half with their south poles, there was no effect perceptible, or merely a slight one, arising from the inequality of the magnets employed. A horse-shoe magnet with its two poles was just as ineffective. A four-pound magnet bar belonging to a magnetometer was brought as close to the south ball as the wooden case would permit, and in a direction perpendicular to the rod. The rod previously stood at 41·50 of the scale, the approach of the north pole rendered it 53·14; the south pole then raised it to 55·45, and the north pole again brought it to 54·05. After removing the magnets, the position of rest found was 42·80. If we take the mean from the first and last position of rest with the magnets removed, and also with the north pole brought near, we obtain—

Repulsion by the north pole 11·445 divisions of the scale,
... south pole 13·300 ...

The difference may be owing to unsymmetrical distribution of the magnetism in the bars.

As is well-known, the repulsive action of a magnet upon bismuth had been observed; I therefore had a ball made of pure bismuth of the same weight, and hung it in the place of the one hitherto used upon the south extremity of the rod of the torsion-balance.

The position of rest of the rod with the magnet at a distance was observed to be—

	Divisions of scale.	Time of vibration.
previously . .	11·200	
subsequently .	9·775	
mean . . .	10·488	350·5 seconds.
North pole close to the case . .	69·250	280·8
at 10 millims. distance	43·670	307·4
at 30 millims. distance	21·205	333·7

This gives—

Distance of the position of rest from 0.		Observed repulsion.		Distance of the pole of the magnet from the central point of action in the ball. Millims.	Time of vibration.	Repulsive force. milligram.	
Divisions of scale.	millims.	Divisions of scale. B.	millims. A.		Seconds.	Observed.	Calculated.
10·488	1·2223	0	0	∞	350·5	0	0
69·250	8·0889	58·762	6·8666	$x+$ 8·0889	280·8	0·09038	0·09038
43·670	5·1010	33·182	3·8787	$x+$15·1010	307·4	0·04260	0·04084
21·205	2·4769	10·717	1·2546	$x+$32·4769	333·7	0·01169	0·01042

The last column is calculated upon the assumption that x, *i. e.* the distance of the pole of the magnet when close to the case from the central point of action in the ball, is 15·04 millimetres for the position of the torsion-rod at 0, and that the repulsive force acts in an inverse ratio to the *third* power of the distance. The differences do not exceed the possible error of observation.

At the distance of 15 millimetres from the surface of the case, and with the position of the rod at 0 of the scale, scarcely the surface of the ball is attained, which would show that the principal action is upon the nearest surface of the ball.

A second observation gave the following measures of repulsion :—

	Millims.
For the magnet close to the case . . .	7·388
At a distance of 10 millims. from the case	4·365
... 20 ...	2·641
... 30 ...	1·628
... 40 ...	0·856

Since, however, the corresponding periods of vibration were not observed, the repulsive force exerted in each instance cannot be calculated from them. That the effect was found to be greater on this occasion, is explained by the circumstance of the position of rest of the rod, with the magnet removed, being on an average at 0·994 of the scale instead of, as in the first series of experiments, at 10·488 divisions, so that consequently the distances were less than in the first observation.

A third series of experiments were undertaken after the ball of bismuth had been suspended from the north end of the rod. In this instance it approached less close to the case than previously on the south side, as will be seen in the following from the determination of x.

The direct observations gave—

	Position of rest in divisions of the scale.	Duration of vibration in seconds.
With the magnet removed .	72·6229	349·46
Magnet close to the case .	33·2583	301·59
At 10 millims. distance . .	50·1167	323·75
At 20 ... distance . .	58·46875	336·56
At 30 ... distance . .	63·73125	342·44

Whence we obtain—

Distance of the position of rest from 100.		Observed repulsion.		Distance of the pole of the magnet from the central point of action in the ball. millims.	Time of vibration.	Repulsive force. milligrammes.	
Divisions of scale.	millims.	Divisions of scale. B.	millims. A.		Seconds.	Observed.	Calculated.
27·3771	3·1978	0	0	∞	349·46	0	0
66·7417	7·7959	39·5646	4·5981	$x+7·7959$	301·59	0·05246	0·05246
49·8833	5·8267	22·5062	2·6289	$x+15·8267$	323·75	0·02603	0·02719
41·53125	4·8511	14·15415	1·6533	$x+24·8511$	336·56	0·01515	0·01494
36·26875	4·2364	8·89165	1·0386	$x+34·2364$	342·44	0·00919	0·00899

The three last values of the last columns are calculated from the second, upon the assumption that $x=25$ millims., and that the action is inversely as the third power of the distance. With this assumption we again constantly arrive nearly at the surface of the ball of bismuth.

Although the calculated values agree sufficiently with observation, I by no means regard the experiments sufficient to deduce from them the two following positions, that—

I. The repulsive action acts principally upon the nearest surface of the diamagnetic body.

II. That this repulsion decreases as the third power of the distance of the pole of the magnet increases.

In the first place, the experiments are not sufficiently numerous, and require to be repeated with modifications; and secondly, it should be observed that the ball was contained in a cylindrical wooden case, the inside and outside of which was coated with tinfoil. Now if the cause of the repulsive action is owing to an induction, perhaps, of electric currents which the pole of the magnet excites in or upon the ball, it is highly probable that it would excite similar induction upon the coating of tinfoil, or even upon the wood of the case, which would react upon the ball and so complicate the total effect.

XVII. *On the Action of Chloroform on the Sensitive Plant* (Mimosa pudica). *By* Professor Marcet of Geneva*.

WHEN one or two drops of pure chloroform are placed on the top of the common petiole of a leaf of the sensitive plant, this petiole is seen almost immediately to droop, and an instant after the folioles close successively pair by pair, beginning with those which are situated at the extremity of

* Read before the *Société de Physique et d'Histoire Naturelle*, Oct. 19, 1848, and communicated by the Author.

each branch*. At the end of one or two minutes, sometimes more, according as the plant is more or less sensitive, most of the leaves next to the chloroformed leaf and situated beneath it on the same stalk, droop one after another, and their folioles contract, although generally in a less complete manner than those of the leaf placed in immediate contact with the chloroform. After a rather long time, varying according to the vigour of the plant, the leaves open again by degrees; but on trying to irritate them by the touch, it is seen that they have become nearly insensible to this kind of excitement, and no longer close as before. They thus remain as if torpid for some time, and generally do not recover their primitive sensitiveness till after some hours. If, however, when they are in this state of apparent torpidity, they are subjected again to the action of the chloroform, they close as they did the first time. It is not till after they have been chloroformed several times, that they lose all kind of sensitiveness, at least until the next day; sometimes they even fade completely at the end of too frequent repetitions of the experiment. In all cases the effects observed are the more marked in proportion to the purity of the chloroform employed and the degree of sensitiveness in the plant.

An analogous phænomenon is produced if, instead of placing the drop of chloroform on the base of the petiole, it is laid on the folioles situated at the extremity of a branch. The folioles of this branch immediately begin to close pair by pair, the common petiole droops, lastly the folioles of the other branches close in turn. At the end of two or three minutes, the nearest opposite leaf, and if the plant is vigorous, most of the other leaves situated below on the same stalk follow their example. When, after some time, the leaves open again, the same want of sensitiveness is manifested as in the preceding case.

A singular feature in this phænomenon is the manner in which the action of the chloroform is propagated from one branch to another, then from one leaf to another, even when the liquid disappears by evaporation almost as soon as it is deposited. This action, as we have just seen, appears to be communicated from the leaf to the stalk, following in the latter a descending direction; generally the leaves situated beneath the chloroformed leaf are not at all affected. DeCandolle, in making an analogous experiment on a sensitive plant with a drop of nitric or sulphuric acid, remarked, on the contrary, that it was the leaves above the leaf touched which closed, without

* I previously convinced myself by experiment that a drop of water, placed delicately on a leaf of the sensitive plant, caused no movement.

those situated beneath participating in this motion*. The observation of our learned countryman is quite naturally explained by attributing to the ascending sap the transport of the corrosive poison, a transport which, in this case, would take place in the direction from below upwards. But how to account for the apparent transmission of the effects of the chloroform in the contrary direction, from above downwards? Might the descending sap more peculiarly have the property of transmitting the narcotic effects of this singular compound from one part of the sensitive plant to the other; or might there exist in this plant some special organ susceptible of being affected by certain vegetable poisons in a manner analogous to the nervous system of animals? Notwithstanding the interesting investigations of Dutrochet and other physiologists, there still prevails too much obscurity on this subject to hazard an opinion. But in any case the fact is singular, and appears to me to merit the attention of persons accustomed to engage in questions of this nature.

Experiments of the same kind, made on the contractility of the sensitive plant with rectified æther, have furnished me with results nearly similar to the preceding; with this difference, however, that whilst one drop of chloroform placed on the common petiole of a leaf situated at the extremity of a branch of a sensitive plant suffices to cause most of the other leaves situated beneath on the same branch to close, æther in general produces an effect only on the leaf itself with which it is put in contact. The next leaves have generally appeared to me not affected. I must however add, that my experiments with æther having been made after the others, and at a time of year when the sensitiveness of the plant already began to diminish, it is possible that the intensity of the effects produced may have thereby been affected.

XVIII. *Solution of two Geometrical Problems. By* James Cockle, *Esq., M.A., of Trinity College, Cambridge, and Barrister-at-Law of the Middle Temple*†.

THE following solutions are effected by what may be termed a Uniaxal or an Imaginary Geometry. The equations of the problems are formed and treated as if the points which constitute the data and quæsita were in the same straight line. The sketch here given of such a geometry is necessarily short and confessedly imperfect. And yet, perhaps, it will be found

* DeCandolle, *Physiologie Végétale,* vol. ii. p. 866.

† Communicated by the Author.

sufficient for my purpose,—which is, to show the interpretability of impossible quantity.

DEFINITION. In the equation

$$v = A + iB + jC \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

let

$$A^2 + B^2 + C^2 = a^2;$$

then I propose to call (1.) a *virtual* solution of the equation

$$v = a.$$

RULE. To solve a problem by the imaginary geometry, let its conditions be expressed by the independent relations $U=0$, and $V=0$; form the equation

$$U + mV = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2.)$$

where m is a disposable multiplier: then, if a solution of (2.) is a virtual solution of

$$V = 0,$$

the thing required is done*.

PROBLEM I. Find three points equidistant from each other.

Let A be one of the points. Draw AB equal to any quantity a, and in any direction, and let B be another of the points. Let C be the third point. Then, since C is equidistant from A and B, we have $AC \times CB = AC^2$; and also, since A is equidistant from B and C, we obtain $AC^2 = AB^2$. These equations will, on putting $AC = x$, be expressed algebraically as follows:—

$$x(a-x) = x^2, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3.)$$

$$x^2 = a^2; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4.)$$

add (3.) and (4.) and we obtain

$$x(a-x) = a^2, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5.)$$

and hence

$$x = \frac{a}{2} \pm \frac{a\sqrt{-3}}{2},$$

$$= \frac{a}{2} \pm i\frac{a\sqrt{3}}{2};$$

and, the solution of (5.) being a virtual solution of (4.), the problem is solved. ABC is, of course, an equilateral triangle.

PROBLEM II. Find four points equidistant from each other.

Complete the rhombus ACDB. Then D is equidistant from B and C. And, by symbolical geometry, we have $AD = AB + BD$. But, we must also have, since A is equi-

* Observations on this Rule and its grounds are reserved for another opportunity.

distant from B and D, AD=AB. Let AD=z; then, we may express the two last conditions by

$$z=a+x, \quad \ldots \ldots \ldots \quad (6.)$$

and

$$z=a; \quad \ldots \ldots \ldots \quad (7.)$$

add (6.) and (7.) and we obtain, on reducing,

$$z=a+\frac{x}{2}, \quad \ldots \ldots \ldots \quad (8.)$$

or

$$z-\frac{5a}{4}-i\frac{a\sqrt{3}}{4}=0. \quad \ldots \ldots \quad (9.)$$

Now, in its present form, the last equation does not furnish us with any solution of the problem. But it may be rendered available by decomposing it into two congeneric surd equations and selecting the impossible congener. For, the left-hand side of (9.) may be resolved into two factors, both of which are included in the expression

$$\pm\sqrt{Z}+\sqrt{z-\frac{5a}{4}-i\frac{a\sqrt{3}}{4}+Z} \quad \ldots \quad (10.)$$

Assume that

$$z-\frac{5a}{4}-i\frac{a\sqrt{3}}{4}+Z=jZ, \quad \ldots \quad (11.)$$

then one of the values of (10.) takes the form

$$(1+\sqrt{j})\sqrt{Z},$$

and, consequently, vanishes. This, then, is the solution of (9.) which we are in quest of, and, further, this is a solution which must not be neglected supposing that we admit impossible quantities into algebra. We must now consider D as situate out of the plane of ABC.

But, the question occurs, which of the infinite number of values of Z are we to select? The answer is, that which satisfies the condition* that the orthographical projection of D on the plane of ABC shall be the centre of the circle inscribed in ABC. But, this condition gives,

$$-\frac{5a}{4}+Z=-\frac{a}{2},$$

whence,

$$Z=\frac{3a}{4}.$$

* The condition in question is, perhaps, the one best adapted to the problem before us; the determination of Z under its most general aspect will be discussed on a fitting occasion.

Let

$$A=\frac{a}{2},\quad B=\frac{a\sqrt{3}}{4},\quad C=(Z=)\frac{3a}{4},$$

then we have

$$z=A+iB+jC; \quad . \quad . \quad . \quad . \quad . \quad (12.)$$

and, since (12.) is a virtual solution of (7.), the problem is solved. ABCD is a regular tetrahedron.

Scholium. These solutions may be readily verified. The form, which imaginary geometry will finally take, may possibly be very different from that exhibited here, but I have endeavoured to show *à priori* that, under certain limitations, j indicates perpendicularity to a plane. As to those limitations the reader is referred to pp. 44, 45 of this volume. The geometrical illustration given at the latter of those pages will be more correct if we suppose the small sphere to be moved, parallel to itself and perpendicular to its axis, until its pole is at a distance unity from its former position. The reader who is interested in the subject of the impossible quantity j is referred to my papers at pp. 435–439 of the last, and pp. 37–47 of the present volume of this Journal. For distinctness of reference I have in this paper used i and j instead of α and β. I have not thought it necessary to refer to the case where k (or γ) enters into a geometrical problem, as it was beyond my present object.

Note. The value of $w\mu'^2+2y\nu'^2$ (*supra*, p. 47) should be

$$w'w''+x'x''+y'y''+z'z''.$$

The omission of accents has occasioned the error.

2 Church-Yard Court, Temple,
January 16, 1849.

Correction. Supra, p. 42, note*, line 6,
for " and then *n* " *read* when the index.

XIX. *Proceedings of Learned Societies.*

CAMBRIDGE PHILOSOPHICAL SOCIETY.

[Continued from vol. xxxiii. p. 394.]

Nov. 13, 1848. SECOND Memoir on the Fundamental Antithesis of Philosophy. By W. Whewell, D.D.

This memoir is a continuation of a former one in which the antithesis of thoughts and things, of ideas and facts, of subjective and objective, were shown to be at bottom the same antithesis, and to be a fundamental antithesis, the union of the two elements entering into all knowledge, and their separation being the test of all philosophy. The present memoir is employed in illustrating the proposition that the progress of science consists in the transfer of some truth from the factorial to the ideal side of the antithesis, or as it may be termed, in the *idealization of facts*. This is exemplified in mechanics, astronomy, botany and chemistry.

In a note, the author remarks on certain German systems of philosophy with reference to this antithesis. The Sensatorial school having reduced all knowledge to facts, Kant re-established the necessity of Ideas, which Fichte made almost the exclusive element. Schelling founded his philosophy upon the *absolute*, from which he derives both facts and ideas, but which a wiser philosophy shows us that we can never reach; and Hegel took the same foundation, but in a certain degree rightly pointed out that the progress towards the identity of fact and idea is to be traced in the history of science; which view, however, he has carried into detail by rash and blind conjecture.

Nov. 27.—On a Difficulty suggested by Professor Challis in the Theory of Sound. By Robert Moon.

In a paper by Professor Challis contained in the Supplementary Number of the 32nd Volume of the Philosophical Magazine, I find the following:—

"The difficulty respecting the augmentation of the velocity of sound by the development of heat, cannot be so summarily disposed of as Mr. Airy appears to imagine. I shall perhaps succeed better in conveying my meaning by using symbols. If θ be the temperature where the pressure is p and density ρ, and θ_1 the temperature in the quiescent state of the fluid, we have, by a known equation,

$$p=a^2\rho(1+\alpha.\overline{\theta-\theta_1}).$$

Hence

$$\frac{d^2z}{dt^2}=-\frac{dp}{\rho dz}=-\frac{a^2d\rho}{\rho dz}-a^2\alpha(\theta-\theta_1)\frac{d\rho}{\rho dz}-a^2\alpha\frac{d\theta}{dz}. \quad . \quad (1.)$$

"The usual theory explains how the third term of the right-hand side of this equation may be in a given ratio to the first; but my difficulty is to conceive how the same can be the case also with the *second* term, since it changes sign with the change of sign of $\theta-\theta_1$."

I conceive that the explanation, according to the usual theory to which Professor Challis here alludes, depends upon the principle, "that for very small condensations of air, the rise of temperature will be proportional to the increase of density." (Vide Herschel On Sound, *Encyc. Met.*, art. 72.) Thus we may put

$$\theta-\theta_1=k(1-\rho),$$

where k is a constant, and 1 is put for the density of equilibrium: on which hypothesis it is obvious that the third term of equation (1.) will be a multiple of the first, as described by Prof. Challis. It also follows that the second term vanishes, since it has $(1-\rho)$ for a factor, and in reducing (1.) to the ordinary form of the differential equation of sound the difference between ρ and 1 is neglected. It thus, I think, appears that the difficulty suggested by Prof. Challis has no real existence.

Dec. 11.—On the Formation of the Central Spot of Newton's Rings beyond the Critical Angle. By G. G. Stokes, M.A., Fellow of Pembroke College, Cambridge.

It has long been known that when Newton's rings are formed between the under surface of a prism and the upper surface of a lens, or of a second prism, so as to allow of increasing the angle of incidence at pleasure, the rings disappear when the critical angle is passed, but the central spot remains. The existence of the spot under these circumstances has even been attributed to the disturbance in the second medium, which, when the angle of incidence exceeds the critical angle, takes the place of that disturbance which at a smaller incidence constitutes the refracted light; but the expression for the intensity has not hitherto been given, so far as the author is aware. The object of the author in the present paper is to supply this deficiency.

The author has not adopted any particular dynamical theory, but has deduced his results from Fresnel's expressions for the intensities of reflected and refracted polarized light. When the angle of incidence becomes greater than the critical angle these expressions become imaginary. When the imaginary expressions are interpreted in the way in which physical considerations show that they must be interpreted, it becomes easy to obtain the expression for the intensity of the light, whether reflected or transmitted, in the neighbourhood of the spot. When the first and third media are of the same nature, the following expression is obtained for the intensity (I) of the reflected light, the incident light being polarized in the plane of incidence, and its intensity being taken for unity,

$$I = \frac{(1-q^2)^2}{(1-q^2)^2+4q^2\sin^2 2\theta}, \text{ where } q = \varepsilon^{-\frac{2\pi D}{\lambda}\sqrt{\mu^2\sin^2 i-1}}.$$

In this expression μ is the refractive index of the first medium, i the angle of incidence on the surface of the second medium, or interposed plate of air, D the thickness of that plate at the point considered, λ the length of a wave in air, 2θ the acceleration of phase due to total internal reflexion. When the light is polarized perpendicularly to the plane of incidence, it is only necessary to replace 2θ by 2ϕ, the angles θ, ϕ being those so denoted in Airy's Tract. The intensity of the transmitted light is obtained by subtracting that of the reflected light from unity.

From the expression for the intensity, the author has deduced the following results, all of which he has verified by observation.

The spot is comparatively large near the critical angle, and becomes smaller and smaller as the angle of incidence increases. Near the critical angle the fainter portion, or *ragged edge*, of the bright spot seen by transmission is broad; at considerable angles of incidence the light decreases with comparative abruptness. Towards the edge of the spot there is a predominance of the colours at the red end of the spectrum, causing the ragged edge to appear brown. Near the critical angle the spot is larger for light polarized perpendicularly to the plane of incidence than for light polarized in that plane: at considerable angles of incidence the order of magnitude is reversed. The difference is far more conspicuous in the former case than in the latter, and in that case consists principally in the greater extent of the ragged edge. When the incident light is polarized at an azimuth of 45°, or thereabouts, and the transmitted light is analysed so as to extinguish the light transmitted near the point of contact, there is seen a central dark patch surrounded by a luminous ring.

ROYAL ASTRONOMICAL SOCIETY.

[Continued from p. 72.]

Nov. 10, 1848.—The Method in Use at the Cambridge Observatory of Measuring Differences of Right Ascension and North Polar Distance by an Equatoreal provided with Clock-movement, and of Correcting the Observations for Refraction. By Professor Challis.

"Differences of north polar distance are usually measured by the Northumberland equatoreal, by means of a small sector of a large circle, on the limb of which are inscribed equidistant divisions, separated by an arbitrary but ascertained interval. A similar sector can be clamped to any part of the hour-circle, and differences of right ascension measured in an analogous manner. This is effected by an arrangement contrived by Mr. Airy (who contemplated the kind of observation here described), by which the instrument may be moved about its polar axis independently of the hour-circle, while the latter is carried nearly at the rate of sidereal time by a clock. The hour-circle sector has been substituted for the hour-circle itself, because the divisions of the latter are on brass, and not so well-adapted for accurate bisection as those of the sector, which are on white metal; and because the equidistance of the divisions, which is the essential condition, is more likely to be secured in a small portion of a circle than in a complete circle. The intervals of both sectors are subdivided by microscope-micrometers. The following is the method of taking the observations.

"It is generally required, and always desirable, to measure simultaneously differences of right ascension and north polar distance. Accordingly the object is bisected by the equatoreally adjusted wire, *very near* the transverse wire, so that the rate of the clock, gaining or losing as the case may be, soon brings it upon the latter wire, the observer taking care in the meantime that it remains bisected by the other. The instant of simultaneous bisection by the two wires is noted, and the microscope-micrometers of the two sectors are then read off in integral intervals and revolutions, and parts of a revolution. This process is *commenced with the star*, or point of reference; the object referred is next observed in the same manner, and so on alternately, the series *concluding with the reference star*. In case the compared object be too faint for observation with micrometer wires, the practice with the Cambridge equatoreal is to use a diaphragm bounded by straight edges at right angles to each other, and the object being placed near the angular point in the prolongation of the edge which is equatoreally adjusted, the instant at which its centre is brought into coincidence with the angular point by the clock's rate is noted. In other respects the operation is the same as that just described. The chronometer is compared with the transit-clock at the end of the series (sometimes, also, before its commencement), and finally the barometer and thermometer are read off.

"With respect to the reduction of the observations, the chief things to remark upon are the corrections for the clock's rate, and for refraction. The differences of the hour-circle sector-readings for the *star* are entirely due to these two causes, if the instrument be supposed to be in good adjustment. The star being known, and the times of bisection known, the effects of refraction on the hour-angles are calculated for each observation of the star, by a process which will be presently stated. Corrections for refraction being applied to the hour-circle sector-readings for the star, the remaining differences are due to the clock's rate, and by comparison with the times of bisection, determine the rate. The *correction for rate of hour-circle* is a part of the loss or gain in the interval between *consecutive* bisections of the star, which bears the same ratio to the whole, as the interval from either bisection of the star to the bisection of the planet or comet bears to the interval between the two bisections of the star. The following is the formula for this correction, the sidereal times of the three bisections, in the order of their occurrence, being s_1, σ, s_2; H being the excess of the hour-circle sector-reading for the star at s_1 above the reading at s_2 converted into time, and R the excess of the correction for refraction in hour-angle for the star at s_1 above that at s_2:—

$$\text{Correction for rate of hour-circle} = \frac{\sigma - s_1}{s_2 - s_1}(H+R).$$

This formula gives the quantity to be *added* to the algebraic excess of the sector-reading for the comet or planet, above that sector-reading for the star which was taken at the time s_1, and is sufficient for all cases.

"It is to be remarked, that if the difference of the sector-readings be affected by any other source of error acting proportionally to the time, as, for instance, want of adjustment of the instrument, such error is eliminated by the above calculation. For this reason, to ensure greater accuracy, the excess of the reading of the *declination* sector for the compared object, above that for the star at the time s_1, is also corrected by the process just indicated, although that excess is unaffected by the clock's rate. The formula for this purpose is precisely the same as that given above; H, in this case, representing the excess of the declination sector-reading for the star at s_1 above the reading at s_2, converted into arc; and R the excess of the correction for refraction in north polar distance for the star at s_1 above that at s_2.

"After applying the corrections now considered, it is presumed that the instrumental measures of differences of apparent right ascension and north polar distance are affected only by refraction. The total refractions for the star in R.A. and N.P.D. have been already required, and therefore the obvious course is to calculate also the total refractions for the planet or comet, and thence deduce the differences of refraction corresponding to the measured differences of R.A. and N.P.D. It may be questioned whether any approximate formulæ, requiring only the calculation of differences of refraction, would lead to a less amount of calculation in this kind of observation. If P be the pole of the heavens, Z the zenith of the observer, S the place of the object, and ZQ be drawn a perpendicular on PS, the formula used for the total corrections for refraction in R.A. and N.P.D. are the following:—

$$\text{Correction for refraction in N.P.D.} = \text{A.} \tan(\text{PS}-\text{PQ})$$

$$\text{Correction for refraction in R.A.} = \text{A.} \frac{\tan \text{ZQ}}{15}. \operatorname{cosec} \text{PS.} \sec(\text{PS}-\text{PQ}).$$

The factor A is given by the tables in Bessel's *Astronomische Untersuchungen*, vol. i. pp. 198, 199, the argument in the case of the star being the *true* zenith distance, which is obtained by the formula $\sec \text{ZS} = \sec \text{QZ} \sec(\text{PS}-\text{PQ})$. The argument in the case of the compared object is the *apparent* zenith distance, which is deduced from the same formula, the apparent N.P.D. and hour-angle being first obtained by applying the corrections for refraction in N.P.D. and R.A. of the star (with signs changed) to its true N.P.D. and R.A., together with the measured differences of N.P.D. and R.A. affected only by refraction.

"The above calculations will be much facilitated by two tables, one containing the values of PQ, log sec QZ, and log $\frac{\tan \text{QZ}}{15}$ (to five figures) for every minute of hour-angle from 0^h to 6^h, which will be found to require interpolations only to first differences, and which is, in fact, merely an expansion of the table mentioned in the Monthly Notices, vol. viii. No. 9, p. 210. The other is a table for obtaining the factor A. It will save much trouble, and be sufficiently accurate to take account of the barometer and thermometer by the empirical formula given in the Monthly Notice above cited, viz.

$$\log \text{A} = \log k + 0{\cdot}015\,\text{B} + 0{\cdot}001(100^\circ - \text{T}),$$

in which $\log k$ is $\log \alpha$ or $\log \alpha'$ of Bessel, according as the argument is the true or the apparent zenith distance, diminished by the constant 0·49572. Any error which the use of this formula induces, will very nearly disappear in the *differences* of the refractions. Thus the second table need merely consist of values of $\log \alpha - 0{\cdot}49572$, and $\log \alpha' - 0{\cdot}49572$; and the most convenient argument is log sec ZS, the consecutive logs differing by 0·01. This table would, therefore, very well range with the table of values of $\log \alpha'' - 0{\cdot}4957$, required in the computation of differential refractions."

The Astronomer Royal gave a description of the gigantic telescope erected by the Earl of Rosse, at Birr Castle, which he visited and carefully examined this autumn. The mode of grinding and polishing the speculum, the mounting, &c. were fully described and illustrated by models, and the residual difficulties stated. He also exhibited models of Mr. Lassell's grinding and polishing machine, and of the mounted instrument, dome, &c. It was clearly shown that, though pursuing different courses, the Earl of Rosse and Mr. Lassell had each attained almost *absolute perfection* in figuring and polishing their specula, and that the difficulties in mounting, &c. were gradually being overcome by Lord Rosse, while they were already nearly got rid of by Mr. Lassell in his comparatively small instrument.

Mr. Drew, who has lately built and furnished a very convenient observatory at Southampton, adopts a collimating telescope for getting rid of his error of collimation. To this latter telescope he has attached a wire micrometer, which supplies the object to be viewed by the transit. He also uses the wire-micrometer to measure the intervals of his wires. The results are more readily obtained than by slow moving stars, and he conceives with at least equal accuracy. Specimens of the determination of the intervals by both methods are given, which agree very nearly.

Dec. 8, 1848.—Transit of Mercury, Nov. 8–9, 1848. By the Rev. W. R. Dawes, at Cranbrook.

"My attention was directed principally to the appearance of the planet at its ingress, and to measurements of its diameter during the transit.

"The ingress was observed with my 8½-foot achromatic, the aperture being limited to 4 inches, the eye-piece magnifying eighty-seven times. So extremely undulating was the edge of the sun in general, that no advantage seemed to arise from an increase of power. Nothing remarkable was noticed till Mercury had advanced on the sun's disc to about three-quarters of its own diameter, when the cusps appeared much rounded off, giving a pear-shaped appearance to the planet. The *degree* of this deformity, however, *varied* with the steadiness and definition of the sun's edge, being *least* when the definition was *best*. A few seconds before the complete entrance of the planet, the sun's edge became much more steady, and the cusps sharper, though still occasionally a little broken towards their points by the undulations. At the instant of their junction the definition was pretty good, and they formed the finest conceivable line, Mercury appearing at the same time *perfectly round.*

"The impression upon my mind was, that the distortion of the planet arose entirely from the rounding off of the points of the cusps by the tremor and diffusion of the image. I have repeatedly observed precisely the same appearance at the ingress and egress of the shadow of a satellite of Jupiter, when the edge of the planet has been rather undulating and diffused.

"For the measurement of the diameter of Mercury I had prepared several different instruments. The filar micrometer was applied to the 8½ foot equatorially mounted achromatic, the clock motion being in use. A 5-foot achromatic by Dollond was furnished with one of his spherical crystal double-image micrometers, and mounted on a very stout floor-stand with an equatorial socket. An excellent Gregorian reflector, of 5 inches aperture and 20 inches focus (the large metal figured by Cuthbert), and furnished with its own divided object-glass heliometer, was also employed. And lastly, a spherico-prismatic crystal double-image micrometer was applied to the $8\frac{1}{2}$-foot equatoreal. Measurements were obtained with each of these instruments; but from the excessive tremor which usually affected the image, the results were not very satisfactory.

With the $8\frac{1}{2}$-foot equatoreal and filar micrometer, power 163, aperture reduced to 2·84 inches,
Polar diameter of Mercury $=9''\cdot3694$, six observations.

Same instrument and power, aperture 4·02 inches,
Polar diameter $=9''\cdot3890$, six observations.
The mean of the two sets $=9''\cdot393$.

Same telescope, and spherico-prismatic micrometer, power 184,
Polar diameter $=8''\cdot89$, three observations.

With the 5-foot achromatic and the spherical micrometer, power 117,
Polar diameter, by four observations $=9''\cdot02$ } diff. $=0''\cdot34$,
Equatoreal diameter, by two do. $=9''\cdot36$ }

With the heliometer on the 20-inch Gregorian, power 115,
Polar diameter, by four observations $=8''\cdot89$ } diff. $=0''\cdot31$.
Equatoreal diameter, by ten do. $=9''\cdot20$ }

"No difference is recognised in the Nautical Almanac between the polar and equatoreal diameters of this planet; yet my observations, both with the 5-foot achromatic and the Gregorian, show a perceptible difference, and nearly to the same amount. And it was noticed with each of the double-image micrometers that a satisfactory measure of the equatoreal diameter was always perceptibly too large for the polar diameter, the images appearing slightly separated; and that, on the contrary, with a good measure of the polar diameter, the images overlapped when placed in the direction of the equator. The change was repeatedly made from one to the other, and always with the same result. The compression would thus appear to be about $\frac{1}{29}$.

"It will be remarked that no sensible difference was produced in the apparent diameter by varying the aperture from 4·02 inches to 2·84 inches. The same darkening glass was employed with both apertures; and therefore, though the *telescopic* irradiation would be *least* with the *larger* aperture, yet, the image being brighter with that aperture, the *ocular* irradiation would be *greater*. Probably, therefore, the two effects might counteract each other.

"The measurements, though few, were taken with extreme care, each of them having been repeatedly examined under the best views before it was read off."

By Mr. T. Dell, at Dr. Lee's Observatory, Hartwell.

"The time was taken from the transit-clock, the error of which was well known from observations on the 7th and 8th. The first contact was not noted with any degree of certainty; the interior contact was well-observed.

Interior contact $14^h 18^m 55^s\cdot3$ sid. time, or $23^h 3^m 57^s$ mean time at Hartwell.

"My attention was directed by the Rev. Mr. Reade to a phænomenon described by the late Professor Moll (Mem. Ast. Soc. vol. vi. p. 116), a recurrence of which we all observed,—Mr. Reade and his assistant, with a Gregorian telescope, at Stone, and again with me here. This is a considerable grayish spot on the disc of Mercury, very indefinite, but gradually shading off from the brightest point in the centre to the blackness of the rest of the planet. I have attempted to give some idea of this appearance in the drawing annexed, as seen with a power of 240; with a less power we could not distinguish it."

By the Rev. Mr. Reade, at his observatory, Stone.

Mr. Reade has sent a drawing of the gray spot observed in Mercury, which agrees with Mr. Dell's. The observations consist of a numerous series of angles measured from Mercury to spots on the sun, from which M. Fazell has made an elaborate chart of the path of Mercury over the sun's disc.

By Mr. Hartnup, at the Observatory, Liverpool.

Equatoreal, $8\frac{1}{2}$ inch achromatic; power, 134.

Internal contact $23^h\ 6^m\ 54^s{\cdot}4$ Greenwich mean time.

"The instant is noted at which the sun's light was first seen to surround the planet completely."

Description of a Machine for Polishing Specula. By Mr. Lassell.

"The twelfth volume of the Memoirs of the Royal Astronomical Society contains a description of a Newtonian Reflecting Telescope, of 9 inches aperture and 112 inches focus, equatoreally mounted in a revolving dome of $14\frac{1}{2}$ feet diameter.

"Several years' experience in the use of this instrument so well convinced me of its general efficiency, and especially of the convenience and stability of its mounting, that I determined, two or three years ago, to carry out precisely the same principle on a much larger scale.

"With a view of informing myself what degree of perfection is attainable in figuring surfaces of larger mirrors than can be wrought by hand, and also of ascertaining the proportion of aperture to focus which it would be most desirable to adopt, I visited Birr Castle; and, by the kindness of the Earl of Rosse, enjoyed the opportunity of two nights' observations with the 3-foot telescope erected by his lordship.

"I was also favoured with an examination of the whole of the machinery employed in grinding and polishing the great speculum: and I returned so well satisfied with all I had seen, that I very shortly resolved to cast a speculum of 2 feet diameter and 20 feet focus.

"The mode of casting the large speculum which I employed involved the principle, discovered, I believe, and first published, by Lord Rosse, of casting the speculum on what is technically called a *chill, i. e.* an iron base, slightly warmed, which causes the speculum to cool upwards in horizontal strata.

"Principally, however, from the difficulty of forming it, I did not employ a base constructed with iron hoops placed edgewise, and turned to the gauge, as Lord Rosse recommends, but, instead of it, a *disc* of cast iron, with its upper surface convex, according to the required radius of curvature, and a rebate formed on the edge of its upper surface, which, receiving a stout iron hoop equal in breadth to the thickness of the speculum, formed an iron mould, and dispensed altogether with the use of sand in the casting. The disc does not require to be *turned,* but if cast from a well-made wooden pattern will be sufficiently true; neither do I think turning the hoop essential, though it might be well to turn the inside surface and the edges, if the means of doing so were at hand.

"As it is necessary that the pouring should be pretty quick, in order that there may not be time for the base to solidify any portion of the metal before it is completely covered, I inclined the base a little, pouring on the lowest side, in order that the fluid might rise in one compact wave; and when the disc was nearly covered, it was restored to a truly horizontal position, and the pouring continued, until the mould was sufficiently filled, namely, to the depth of about two inches and three quarters. The hoop was about three inches broad, and having been turned parallel, the mould was in the first instance placed horizontal, by a spirit-level being placed upon its edge. The inclination was produced by the application of a lever, which, when withdrawn, restored the base to its horizontal position, and ensured the equable thickness of the speculum at every part of its circumference."

Mr. Lassell then describes the very ingenious method which he adopted to procure the requisite quantity of metal in the proper state, and his mode of ascertaining that the dose of tin was sufficient. The final proportion which he used is 32 lbs. of copper to 15·09 lbs. of grain tin, and 18 lbs. of white arsenic were stirred up with 438 lbs. of the melted mixed metal.

"The speculum was ground and polished on a machine almost precisely the same as that described by Lord Rosse in his lordship's very interesting paper, published in the second part of the Philosophical Transactions for 1840.

"I found, however, the grinding process much facilitated by interposing a piece of sheet-lead, about a tenth or twelfth of an inch thick, between the speculum and the iron grinding-tool. This saved the rapid wearing down of the tool and also cut the metal much faster, as the softness of the lead suffered the particles of emery to imbed themselves into it, and thus to form a very keen grinding surface. When the lead, fully charged with the emery, had become smooth, it was exchanged for a fresh piece. When an entire surface had been obtained upon the speculum, the smoothing and perfecting of the surface previous to polishing was produced by the iron tool and the finest washed emery.

"The speculum was polished many times on the same machine, following as nearly as practicable the directions given by Lord Rosse; but, after several months' trial, I did not succeed in obtaining a figure which satisfied me, the best I got being very inferior to the surfaces I had obtained by hand on specula of various sizes, up to nine inches diameter. In despair of success by this process, I ultimately contrived a machine, in which I endeavoured to represent as closely as possible the evolutions of the hand, by which I had been accustomed to produce very satisfactory surfaces on smaller specula."

The machine invented by Mr. Lassell, and constructed by Mr. Nasmyth, for figuring and polishing specula, cannot be made intelligible without figures*. The speculum rests with its face uppermost in a horizontal position, and is carried slowly round by a vertical axis. The polisher rests, with its grinding and polishing surface, upon the speculum, and is moved by a pin which fits loosely into a hole in the centre, at the back of the polisher. The motion of the polisher is that of the driving-pin.

Now this, by a very ingenious and very compact mechanism, receives a compound motion which may be thus imagined. Conceive a circular motion given to a point round the centre of the speculum, and then conceive that the driving-pin has a circular motion round this point. The curve is an *epitrochoid*, and the adjustments of the mechanism enable the workman to give any radius to either circular motion, from 0 up to a certain number of inches. The proportions of these radii, in order to give a parabolic figure, are determined experimentally, in which the relation of aperture to focal length must be considered. The size of the polisher, and even the hardness of the pitch, must also be proportioned to the figure and aperture required. Mr. Lassell finds no difficulty in getting a true parabolic figure when the aperture is one-eighth of the focal length†. The speculum, while grinding and polishing, is supported in the same way as it is in the tube when in use. The principle of this mode of support is mentioned by Lord Rosse, Philosophical Transactions, 1840, p. 524.

"The polisher should possess as much stiffness as is compatible with the requisite lightness, and I have found these qualities best combined by making it of white American deal, in two strata, well-united by glue and a few screws, with the direction of the grain at right angles, the wood well-seasoned, and, if possible, cut out of the same board. The polisher for the 2-foot speculum is made out of $1\frac{1}{8}$ inch board, and has, for symmetry, both the upper and under surfaces convex, to fit the speculum. It is about 2 inches thick at the circumference, $20\frac{1}{2}$ inches diameter, and weighs about 12 lbs. with the pitch surface upon it."

Mr. Lassell then enters very minutely into the mode of coating the polisher with pitch evenly and to a proper thickness, of dividing

* A model to half the true size, and the drawing by Mr. Nasmyth, may be seen at the Society's apartments. A model of Lord Rosse's engine, and of the mounting, &c. of the 6-foot reflector at Birr Castle, may also be seen. These were made by Mr. Airy, and presented by him to the Society. Mr. Williams will explain the action and details of all the models to any fellow who wishes for information.

† Mr. Lassell has given so full an account of all his processes that we conceive any person of ordinary intelligence would be able to execute them; but they do not admit of compression, and extend beyond the limits of a Monthly Notice. It ought to be mentioned that the Earl of Rosse and Mr. Lassell have at all times freely communicated the steps of progress as soon as these became evident to themselves.

the surface into equal squares, and the various manipulations which are required to produce a perfect result. The grinding powder is known as *rouge*, and the best quality may be had from Mr. Fox of Saffron Hill.

"The whole time occupied in obtaining the requisite lustre varies from about one hour to three, and it ought to be steadily advancing throughout.

"A good idea may be formed of the quality of the operation as it proceeds by watching the motion of the tool. It should be regular and uniform, without any apparent labouring or inequality of speed, and the spontaneous motion which the tool has upon the pin as a centre should be slow and regular. No firm adhesion is ever to be allowed between the tool and speculum: this will take place if a due and regular supply of water be not afforded.

"A second application of powder will rarely be required, and never in any quantity, but many applications of water probably will, and the more rapidly the polish is advancing the more frequently will water be required. It is best applied through a hole in the back of the polisher as near the centre as is convenient, which may perhaps be at about the distance of one-third of the radius. But care should be taken not to give the water in excess. The speculum must never be *dry*, but there must be no superfluous water. It is very conveniently applied with a flat camel's-hair brush, half or three-quarters of an inch broad; but as much as the brush would take up would generally be too much for one application. Towards the end, the water should be added more sparingly, and if needful more frequently, going as near to dryness as may be but *never reaching it*.

"The lustre in this state of the process advances most rapidly. If the process has gone on well the powder will have become almost black at the close. The machine having been stopped, the tool is to be carefully taken off by a sliding motion, and the speculum may then be cleaned with a soft linen cloth or leather; or it may be washed with a soft sponge and water, and then dried, and ultimately rubbed lightly with some very soft wash-leather. If the polishing has apparently wrought smoothly, and the aspect of the tool when taken off, both during the process and at its close, is everywhere of even texture when viewed by an oblique light, the speculum will most likely have a *uniform* curve of some description, whether parabolic or not, for it is a characteristic quality of this machine generally to produce a uniform curve. The quality of the curve is best examined by placing the mirror in its tube, and, by means of diaphragms, exposing separate portions of the mirror of equal area from the centre to the circumference.

"I have been accustomed to produce by hand surfaces of, I believe, great excellence, on various sized specula up to nine inches diameter, of which I may instance my 9-foot equatoreal, which enabled me to discover independently (for I did not previously know of its existence) the sixth star in the trapezium of Orion, and with which also the observations of a second divison of the ring of Saturn were made, as described in the Astronomical Notices, vol. vi. p. 11. Such surfaces as these were, however, produced with some degree of anxiety, much manual labour, and perhaps some admixture of accident, especially in the union of a perfectly parabolic curve with regularity of surface. The superiority of the machine in these respects is so striking as almost to put comparison out of the question.

"If driven by a steam-engine the manual labour is of course annihilated. The control over the machine, by the setting of the cranks, is such, at least with all foci not less than eight diameters of the speculum, that the curve can be changed almost at pleasure from the spherical side to the hyperbolic side of the parabola, and *vice versâ*; the alterations of the curve being, *cæteris paribus*, almost exactly commensurate with the adjustments of the cranks. In fact, one of the most anxious and laborious operations is, by this machine, converted into an intensely interesting amusement. With moderate care and a little experience a *bad* figure never need to be feared, though it may require two or three successive trials to satisfy the fastidiousness of a cultivated and long-practised eye. The lustre of polish transcends even my best efforts by hand, and is the easiest quality of all to obtain; and however erroneous the figure may be after any unsuccessful effort, the proper curve may be recovered without resorting to the grinder, or indeed materially impairing the polish—at least, I have not found it needful, even when the difference of foci of the central and exterior portions of a mirror has amounted to fifteen hundredths of an inch. In 3 or $3\frac{1}{2}$ hours by the polisher alone, it is possible to annihilate an error even as enormous as this. I have a strong persuasion that this machine might prove eminently serviceable in working the curves of object-glasses of large dimensions, though of this I have no experience."

Mr. Lassell then briefly describes the mounting of the telescope, the form, weight, and dimensions of its component parts, and the covering dome. They are in *principle* almost the same as were used on a smaller scale in his 9-foot Newtonian. There is a very good model of the dome and mounting, presented by Mr. Lassell, at the apartments of the Society.

"To afford some notion of the degree of facility attained in the management of so large a dome and telescope, I may mention, that with an assistant I can, without hurry, place an object, invisible to the naked eye, within the field of the telescope in nine or ten minutes from leaving my house. This includes opening the dome, uncovering the large speculum, attaching the eye-piece, setting from the catalogue for the object, and turning the dome to the required azimuth. Without an assistant, I should require three or four minutes longer, which would be principally occupied in opening the shutters of the dome.

"One of the greatest difficulties I have encountered in supporting the speculum in its various positions equably, is to avoid the effects of the friction of its edge under considerable changes of altitude of the telescope.

"It is obvious, that when the altitude is low, the principal part of the weight of the speculum must be borne upon its edge, and the supporting plates being thus in a great measure relieved from the pressure of the speculum, must, by their elasticity, tend to distort the metal by pressure at its back; and when the telescope is moved towards the zenith, the plates yield again by the weight of the speculum, while the lower edge, still in hard contact at the points of support, is unduly borne up there, and the equilibrium is destroyed. To remedy this evil I have slung the speculum in a hoop of thin iron, equal in length to half its circumference, the ends of the hoop being attached to swivels fixed in each of the two horizontal brackets, and the lower part of the hoop being thus quite at liberty to rise and fall with the plates.

"This has nearly, if not entirely, removed all perceptible distortion; yet in some positions, and under some circumstances, vestiges of it are to be perceived. I have devised a plan of supporting the metal laterally by an equal tension on the several points of support, and think it may probably be useful; but I have not yet had leisure to carry it into effect.

"Instead of a plane speculum I usually employ a prism, which transmits a pencil of two inches in diameter, made for me by Messrs. Merz and son, of Munich. I am persuaded, from repeated experiments, that the prism has an obvious advantage in light over a speculum, and the material is so fine, and the surfaces so exquisitely wrought, that no perceptible injury of the image exists. The only care necessary in the use of the prism is to preserve it from dew, which it is extremely liable to collect; this I have remedied by having a chamber made in the mounting of the prism, which receives a cube of cast iron enveloped in thick *felt*: this, being moderately warmed and placed in the chamber, effectually prevents the deposition of dew for at least some hours, while the extremely slow radiation through the felt does not produce any sensible disturbance in the formation of the image. The prism is rather small; for though it transmits the entire pencil, there is scarcely anything to spare; and had it been easy to obtain a sufficiently good one half an inch larger, I should have procured it."

A short notice of the Equatoreal of the Liverpool Observatory. By Mr. Hartnup.

As the Astronomer Royal will probably give some account of this instrument, which has been constructed on his recommendation and entirely under his superintendence, Mr. Hartnup states, in a few words, that it is of the English construction; that is, the telescope is a transit supported at each end, between two long supports which form the polar axis. The telescope is by Merz of Munich, $8\frac{1}{2}$ inches in aperture, and 12 feet focal length. The circle and declination-circle are each 4 feet in diameter, divided by Mr. Simms upon his "self-acting circular dividing engine*." The hour-circle revolves independently of the instrument, and is carried

* Described in vol. xv. of the Memoirs. The new altitude and azimuth instrument at Greenwich, which was divided on the same engine, is considered by Mr. Airy to be exceedingly well divided.

by clock-work, the moving power of which is a water-mill, regulated by "Siemen's Chronometric Governor." This is so successfully applied, that the rate of the hour-circle is not sensibly altered by clamping the polar axis to it. When the hour-circle is properly adjusted, the instrument reads off right ascensions at once *. The polar axis, which is of wrought iron-plate, is very massive and stiff. The weight of the whole instrument is between 70 and 80 cwt. This keeps all steady, even in very hard gales. The instrument is abundantly supplied with eye-pieces and micrometers. The stiff frame and large circles were evidently designed by Mr. Airy to supply a peculiar power to the instrument. In ordinary mountings, great accuracy is not to be expected when the star of reference is more than a few minutes distant from the object compared. The screw of the micrometer is not to be relied upon for larger spaces, and the circles, though sufficient for finding and identifying, are seldom intended for accurate measures. Stars of comparison can, indeed, generally be found which are contained in some of the special and extended catalogues, but such stars can only be considered to be roughly known, and in many cases fail altogether. The Liverpool equatoreal is intended to measure *by its circles* intervals of a few degrees, with as much accuracy as the average stars of our extensive catalogues possess, and thus to give excellent places by reference to well-known stars.

Mr. Hartnup has made some observations to test the powers of his equatoreal in this respect. The observations of γ, α, β Aquilæ, of α and β Lyræ, of Castor and Pollux †, show satisfactorily, that within such limits as these the instrument will measure differences of right ascension and north polar distance almost, if not altogether, as well as can be expected from the best meridian instruments.

Mr. Hartnup further remarks, that the instrument keeps its adjustments steadily, which seems to show that it is not only firm in itself, but, also, that it rests on a sound foundation. The observations by Mr. Hartnup, of standard stars in all parts of the heavens, are not sufficiently numerous to yield a safe estimate of the probable error of a single independent determination, but it is evidently very small, even for stars at 6^h from the meridian.

Mr. Bishop's Ecliptic Charts, from Observations at the South Villa Observatory.

Our treasurer, Mr. Bishop, has lately published the first hour of an ecliptic chart for the epoch 1825. This contains all the stars to the 10 mag. inclusive in a zone of 6° of latitude, 3° on each side the ecliptic. The scale is 1·2 inch to 1°, which gives a clear and open map. The execution is very good.

In the notice which accompanies the chart Mr. Bishop says, "It is the first of a series of twenty-four charts, which I hope to publish.... The discovery of planets is materially facilitated by mapping down the stars within a few degrees on each side the ecliptic;

* This contrivance is peculiar to the equatoreals of Cambridge and Liverpool, and in some researches is of great convenience.

† These observations are given in detail in the accompanying memoir.

and it is for this purpose I have undertaken the present series of charts. . . . The stars included in Weisse's Catalogue from Bessel's Zones were first laid down for 1825 as points of reference. All other stars, to the tenth magnitude inclusive, were then entered by estimation of their positions with respect to the neighbouring members of Weisse's Catalogue. . . . The charts for the hours of right ascension in which the ecliptic falls beyond the declination limits of the Berlin maps (−15°) are in a state of forwardness, and will be published as soon as they are completed. They are regularly compared in their present state with the heavens, so that the search for planets and the formation of the charts are going on at the same time. . . . I take this opportunity of expressing my warmest thanks to Mr. J. R. Hind, for the great care and indefatigable zeal he has displayed in the formation of this chart, which, to my knowledge, he has examined with the heavens from fifty to sixty times; but the success of his research, as shown by the discovery of two planets, speaks for itself, and will, I am sure, dispose astronomers to receive these charts with confidence."

Extract of a Letter from Lieut. Gilliss*, U.S.N.

"The computations for the longitude of Washington, from corresponding moon-culminations observed by me between 1838–1842, are nearly completed. The results for 1839 and 1840 give the following corrections of the (hitherto received) longitude:—

1st Limb	$-5^{s}{\cdot}39$	by 182 comparisons.
2nd Limb	$-4{\cdot}41$	by 74 comparisons.
Mean . .	$-4{\cdot}84$	by 256 comparisons according to weight.

The European observatories with which the comparisons are made, are Edinburgh, Oxford, Greenwich, Cambridge, and Hamburg: the individual results very accordant; those from Cambridge strikingly so. Comparisons have also been made with the observations of Copenhagen, Kremsmunster, Cracow, and Wilna, which seem to show considerable errors in the longitudes assigned to those observatories."

XX. *Intelligence and Miscellaneous Articles.*

ON THE EQUIVALENT OF FLUORINE. BY M. LOUYET.

IN some previous experiments the author had deduced the equivalent of fluorine from the quantity of sulphate of lime yielded by a certain weight of the purest natural fluoride of calcium, and also by artificial fluoride. As the two series of experiments agreed perfectly, M. Louyet had presumed that the results to which they led were sufficiently correct. Nevertheless he decided with some reserve; for having demonstrated that sulphuric acid did not completely decompose fluoride of lead, it occurred to him that this acid might act in an analogous manner on fluoride of calcium. His doubts were

* Lieut. Gilliss became very favourably known to many members of this Society on his visit to England a few years ago. The observations made at Washington were published in 1846 by the order of the Senate, and have been very freely distributed here and on the continent. They are a proof of what may be done with moderate means by a skilful and conscientious observer.

the stronger, because he had found the equivalent of fluorine, deduced from the analysis of fluoride of lead, a higher number than that obtained by the fluoride of calcium. M. Louyet had also announced his intention of studying and consequently of analysing all the fluorides, in order to attempt a discovery of the cause of these differences; the present notice gives the additional researches on this subject, and the author states his belief that he has decided the question.

In a previous memoir M. Louyet had fixed the equivalent of fluorine at 239·81 ; but this number ought to be raised to 240 in making calculations with 250 for the equivalent of calcium and 200 for that of sulphur. Calculation then indicated, if this calculation was correct, that 1 gramme of fluoride of sodium should yield 1·680 grm. of anhydrous sulphate of soda. In decomposing this fluoride by sulphuric acid, it is extremely difficult to avoid loss. The vapours of sulphuric acid appear to carry off very small quantities of sulphate of soda; besides which it is requisite to expose the crucible for a long time to a strong red heat, in order to completely decompose the alkaline bisulphate formed. It is not useless to dwell on this point, in order to show that all the sources of error tend to produce loss; that is to say, to lessen the weight of the sulphate obtained. In three experiments, 1 gramme of fluoride of calcium gave successively 1·686, 1·685, 1·683 of anhydrous sulphate of soda. All these figures are higher than that which calculation indicates the equivalent of fluorine to be 240.

These observations induced M. Louyet to repeat the analysis of fluoride of calcium. A fresh series of experiments, which he considers as exact as possible, gave him the following results:—In his experiments 1 gramme of fluoride gave of sulphate of lime, 1·742, 1·744, 1·745, 1·744, 1·7435, 1·7435, the mean being 1·7436. If the equivalent of fluorine be taken as 237·50, that is to say, 19, calculation shows that 1 gramme of fluoride of calcium should yield 1·74358 of sulphate of lime; it results, therefore, from these experiments, that the equivalent of fluorine is 237·50.

M. Louyet has analysed other fluorides to verify this result. Fluoride of barium converted into sulphate in a manner to ensure its perfect decomposition, gave the following results:—1 gramme of the fluoride of barium gave of sulphate of barytes 1·332, 1·331, 1·330; the equivalent of fluorine being 237·50, the calculated amount would be 1·33090.

Lastly, the author repeated the examination of fluoride of lead, and he discovered the cause of the differences that had formerly resulted between the equivalent deduced from the analysis, and that obtained from the fluoride of calcium. He had not observed, that on account of the great difference existing between the equivalents of fluorine and lead, that the slightest error in the analysis would lead to great differences in the calculated results. This being stated, the following are the figures obtained in his last experiments:—5 grammes of fluoride of lead gave of sulphate, 6·179, 6·178, 6·178; the theoretical number is 6·1828. All these amounts are too small, a circumstance which might readily arise from manipulation or other causes which the author enumerates in his memoir.

In his first memoir on fluorine and the fluorides, M. Louyet, guided by various considerations, rejected the hypothesis of Ampère, in which fluorine is ranged with chlorine, bromine and iodine. The equivalent of chlorine, indicated by the later researches, strengthen this opinion; for whilst the equivalents of chlorine, bromine and iodine, are not exactly divisible by the equivalent of hydrogen, that of fluorine is, on the contrary, a multiple of this equivalent, which associates it with the series of oxygen, sulphur, nitrogen, phosphorus, arsenic and carbon.—*L'Institut*, Janvier 4, 1849.

SOLUBILITY OF CHLORIDE OF SILVER IN HYDROCHLORIC ACID.

M. J. Pierre states hydrochloric acid is capable of dissolving at least 1-200dth of its weight of chloride of silver; and when diluted with twice its weight of water, it is capable of holding 1·600dth of its weight of the chloride in solution.—*Journ. de Ch. Med.*, Jan. 1849.

PREPARATION OF IODIDE OF ARSENIC.

M. Meurer proposes the following method of obtaining this compound:—Pass arseniuretted hydrogen gas into a solution of 4 parts of iodine in 120 parts of alcohol until the liquid is decolorized; a fresh quantity of iodine is then to be added, and the current of arseniuretted hydrogen is to be again passed through the solution to the same point. The liquid ought not then to become turbid; but if a brown turbidness should be produced, it must be made to disappear by an addition of iodine.

By spontaneous evaporation the liquor deposits microscopic hexagonal tables, which, according to M. Kuhn's analysis, are iodide of arsenic.—*Journ. de Ph. et de Ch.*, Decembre 1848.

COMPOSITION OF THE BLACK YTTRO-COLUMBITE OF YTTERBY.

According to the analysis of M. Peretz, this mineral consists of—

Columbic acid	58·65
Yttria	21·25
Tungstic acid	0·60
Lime	7·55
Magnesia	1·40
Protoxide of uranium	3·94
Protoxide of iron	6·29
Oxide of copper	0·40
	100·08

The density of this mineral at an average temperature is 5·67; it becomes 6·40 by calcination.

M. Rose states, on the occasion of this analysis, that an orthite occurs at Ytterby so much resembling yttro-columbite, that it is impossible to distinguish these minerals from each other by appearance.

According to M. Rose, the columbite of Finland possesses the same composition, density, and metallic acids as the yttro-columbite of Ytterby.—*Ibid.*

ON LIQUID PROTOXIDE OF NITROGEN. BY M. DUMAS.

M. Natterer of Vienna has constructed a forcing-pump for the liquefaction of gases, by means of which carbonic acid and protoxide of nitrogen can readily be obtained in the liquid state. Having procured one of these instruments, and employed it more especially for the liquefaction of the protoxide of nitrogen, I soon perceived the necessity of using a series of indispensable precautions, but which, once adopted, have enabled me to effect with promptitude and security, as well as œconomy, the liquefaction of large quantities of protoxide of nitrogen.

As this liquid furnishes a means of producing an excessively low temperature, and is very easily operated with, I will here briefly point out the observations I have made. The first relates to the principal piece of the apparatus, that is to say the reservoir. In my opinion the Viennese manufacturer has not given it sufficient strength. I have had it surrounded with a belt of forged iron, capable of resisting 800 atmospheres, and very nicely made by M. Bianchi. Moreover, I arranged things so that the reservoir being surrounded by ice, the body of the pump was cooled uninterruptedly by a circulation of water around it, and that even the stem of the piston was always moistened by cold water; in this manner there is no danger of the valve of the piston being injured by the heat proceeding from the compressed gas, and by its special action as a combustible gas. With these precautions, we may compress into the reservoir in the course of two hours 200 litres of gas, of which 20 suffice to produce a pressure of 30 atmospheres, about which liquefaction commences. The remainder of the gas furnishes a liquid; 100 litres yield 200 grms., or very nearly. The gas should be absolutely dry in order to succeed, and likewise as pure as possible. I prepare it from the nitrate of ammonia as usual, and after having dried it, pass it into Macintosh bags; a couple of pounds of nitrate of ammonia suffices.

Once compressed, the liquid gas may be preserved for one or two days at least in the reservoir; the valve however is slightly injured by it. When the stopcock of the reservoir is opened, the gas escapes; a portion freezes at first, but it then flows liquid; the solid portion resembles a mass of snow; it melts upon the hand, and rapidly evaporates, leaving a severe burn. The liquid portion, which is by far the most abundant, and of which it is easy to obtain in one operation 40 to 50 grms., being received in a glass, keeps for half an hour, or even more, in the air.

In order to observe more readily its properties, I collected it in open tubes, contained in vessels at the bottom of which was placed some pumice-stone moistened with sulphuric acid. It then retains its transparency for a very long time.

The protoxide of nitrogen is liquid, colourless, very mobile and perfectly transparent; each drop that falls upon the skin produces a very painful burn. The gas, which is incessantly liberated by a slow ebullition, possesses all the properties of the protoxide of nitrogen. When metals are dropped into this liquid, they produce a noise like that of red-hot iron immersed in water. Quicksilver causes the same noise, instantly freezes, and affords a hard brittle mass, white like silver, which it perfectly resembles in appearance. Potassium floats upon the liquid, and experiences no change; the same is the case with charcoal, sulphur, phosphorus and iodine. Ignited charcoal floats upon the surface of the liquid, and burns with considerable brilliancy, and frequently until the whole is consumed. Ordinary sulphuric acid and concentrated nitric acid freeze immediately. Æther and alcohol mix with the liquid without freezing. Water is instantly converted into ice; but it produces such a sudden evaporation of a portion of the liquid, that it causes suddenly a kind of explosion, which would be dangerous if merely a few grammes of water were poured at once into the liquid.—*Comptes Rendus*, Nov. 6, 1848.

ON THE URATES.

MM. Allan and Bensch have examined several of these salts.

Neutral Urate of Potash is obtained with greater facility than has been supposed. It is prepared by saturating a cold dilute solution of potash, free from carbonate, with uric acid diffused in water, and then concentrating the solution by ebullition in a retort. At a certain point of concentration the salt separates in fine needles; the matter is allowed to remain a few minutes, the liquid is poured off and the crystals are washed, first with weak and afterwards with stronger alcohol.

The salt thus obtained is very soluble in water, has a strong caustic taste, attracts carbonic acid from the air quickly, and is gradually decomposed by boiling in water.

The crystals are anhydrous, and gave by analysis a composition corresponding to $C^5 N^4 H^2 O^2$, KO.

This salt is soluble in 44 parts of cold and 35 of boiling water.

Urate of Soda.—The preparation of this salt succeeded by adopting a process corresponding to the preceding. One part of it dissolved in 77 parts of cold and 75 parts of boiling water.

Neutral Urate of Ammonia and *Neutral Urate of Magnesia.*—Neither of these salts could be obtained. Attempts were made, but also in vain, to prepare double salts of magnesia and ammonia, potash or soda.

Neutral Urate of Lime.—This is readily obtained by adding, drop by drop, a neutral solution of urate of potash to a boiling solution of chloride of calcium, until the precipitate, which at first redissolves, begins to be permanent; the limpid liquid is then to be boiled for an hour; the neutral urate is then deposited in the state of anhydrous grains at 212°; they contain $C^5 N^4 H^2 O^2$, CaO. One part of this salt dissolves in 1500 of cold water and 1400 of boiling water.

The acid salt of lime is more soluble than the neutral salt; it requires only 603 parts of cold and 276 of boiling water for solution.

Urate of Strontia.—Uric acid diffused through water is to be added to a boiling and saturated solution of strontia, taking care that the acid is greatly in excess. The first portions of acid are entirely dissolved; but by the addition of successive portions a salt separated, which, examined by the microscope, appeared to be acicular and grouped in stars. This urate of strontia contains $C^5 N^4 H^2 O^2$, $Sr O^2 + 2aq$.

The two equivalents of water are expelled at 329° F. The salt attracts moisture rapidly from the air, and decomposes at 338°. One part of it is soluble in 4300 parts of cold, and 1790 parts of boiling water.

The acid salt is much more soluble: one part dissolves in 603 parts of cold, and 276 parts of boiling water.

Neutral Urate of Barytes is obtained similarly to the neutral salt of strontia. It contains $C^5 N^2 H^2 O^2$, BaO. One part of this salt requires 7900 parts of cold, and 1790 parts of boiling water for solution.

Urate of Lead.—When a dilute solution of neutral urate of potash is added, drop by drop, to a solution of nitrate of lead also dilute and boiling, a yellow precipitate is at first obtained; this is to be separated by filtration, and a fresh portion of urate of soda [potash ?] is to be added to the liquid. A heavy precipitate is thus obtained, which is perfectly white and is easily washed. It is quite insoluble in water and alcohol. It may be heated to 320° F. without decomposing; it appears to be an anhydrous salt composed of $C^5 N^4 H^2 O^2$ PbO.

The authors did not succeed in preparing any other neutral urates. —*Ibid.*

ON THE PRESENCE OF COPPER IN THE HUMAN BLOOD. BY M. DESCHAMPS.

The author observes, that when the numerous examinations of the question of the existence of copper in human blood which have been published are considered, it will be found that these experiments cannot be adduced either to oppose or support the existence of copper in organized beings, because many authors forget to describe their processes of analysis, neglect to examine the precipitate which is formed in a liquid by hydrosulphuric acid, either gaseous or liquid, do not state the length of time which the liquid, treated with sulphuretted hydrogen, is allowed to deposit the precipitate, and they do not state whether they have prepared their hydrochloric acid, whether they have analysed their distilled water and acids, and particularly the hydrochloric acid, for the *pure* hydrochloric acid of commerce almost always contains copper.

After considering the different processes which have been proposed for the detection of metallic substances in the blood, M. Deschamps followed a method analogous to that which he employed to extract copper from vegetables.

The acids and distilled water which he employed contained no metallic substance whatever. The hydrochloric acid was prepared expressly for the purpose; nitric acid only was sometimes employed; the filters were made of paper which was analysed and found to contain no copper, and they were washed with concentrated nitric acid diluted with an equal volume of distilled water. The capsules, crucibles, glass rods, bottles, funnels and glasses, were washed with aqua regia, with nitric acid, and in some cases with boiling nitric acid.

The blood employed in these experiments weighed 162 grs., 200 grs., 300 grs., 315 grs., 380 grs., 472 grs.; it was cautiously evaporated to dryness in a porcelain capsule, and burnt in a porcelain crucible; the ash was treated with aqua regia or nitric acid; the solution was evaporated to get rid of the greater part of the acid, then treated with water, filtered into a bottle, subjected to the action of hydrosulphuric acid, and allowed to stand at least eighteen hours that the precipitate might subside; the liquid was filtered to separate this: the filter after being washed with water containing a little hydrosulphuric acid, in a small porcelain capsule, treated with a few drops of aqua regia of nitric acid, allowed to stand, or slightly heated till the colour of the precipitate was so modified as to possess the colour of sulphur. The filter was washed, the liquid evaporated, and the residue calcined and treated, after cooling, with two drops of nitric acid; it had all the properties of a solution of a salt of copper, for ammonia rendered it blue, and the ferrocyanide of potassium gave a reddish-brown precipitate, and lastly it deposited copper on metallic iron.

From the facts above detailed, the author considers that the existence of copper in the blood cannot be questioned; and he is of opinion, as stated in a memoir presented to the Academy in 1848, that vegetables take from the soil part of the copper which they contain; that herbivorous animals receive it from plants, and man from plants and animals which serve him for food.—*Journ. de Ph. et de Ch.*, Decembre 1848.

FORMATION OF CARBONATE OF LIME FROM THE NEUTRAL MALATE OF LIME. BY M. DESSAIGNES.

The researches of M. Piria have proved that asparagine may be regarded as the amide of malic acid. When it is impure and dissolved in water, it soon ferments, and is converted into succinate of ammonia. It occurred to M. Dessaignes that if malic acid, or one of its salts, was susceptible of undergoing the same kind of fermentation, the relation discovered by M. Piria would receive from it a more complete demonstration.

Neutral malate of lime, such as obtained by M. Liebig's process from the berries of the mountain ash, was exposed to a somewhat deep stratum of water, in a vessel covered merely with paper. This was in the autumn of 1847; after three months, the supernatant water was partly filled with a mucilaginous and unquestionably organized product; on this and on the sides of the vessel there were formed abundance of fine crystals of hydrated carbonate of lime. The filtered water slightly precipitated acetate of lead. The formation of carbonate of lime and mucilage ceased as the spring advanced and in the summer. M. Dessaignes observed beneath the malate of lime, which diminished insensibly, the formation of a stratum of very fine and compact prismatic crystals. This stratum was raised by some large bubbles of gas which were given out by the malate of lime. This mass of crystals was dissolved in hot water, precipitated by carbonate of soda and filtered. By this there was obtained a slightly coloured solution, which, with the addition of alcohol and ammonia, precipitated nitrate of lead, nitrate of silver, neutral perchloride of iron, and chloride of barium. The liquor was concentrated, treated with a slight excess of hydrochloric acid, and evaporated to dryness, the residue being repeatedly treated with boiling æther. The æthereal solution gave by spontaneous evaporation fine crystals of an acid which was volatilized without decomposing, burnt without residue on platina foil, and was in fact succinic acid.

It appeared to be composed of—

Carbon	40·68
Hydrogen	5·08
Oxygen	54·24
	100·00

At the sitting of the Academy of Sciences on the 2nd inst. Sir David Brewster was elected one of the eight foreign associate members of the National Institute of France, vacant by the death of the celebrated chemist, M. Berzelius.

JOURNEY TO DISCOVER THE SOURCES OF THE NILE.

In our number for December last* we announced the arrival of Dr. Bialloblotzky at Alexandria. According to letters since received from him by Dr. Beke, he left Suez for Aden on the 22nd of November, by the East India Company's steam-packet "Adjdaha," by which a free passage had been granted him by the Court of Directors; and at the latter place he was awaiting (Dec. 11) the arrival from Djiddah of a small steam-vessel with pilgrims returning from Mecca to the Persian Gulf, by which he intended to proceed to Makulla, on the south coast of Arabia. He there expected to meet with an Arab vessel to take him to Mombás, on the east coast of Africa, from which place he would commence his journey into the interior.

Dr. Beke informs us that he has just received a letter from Captain Haines, I.N., Political Agent at Aden, dated Dec. 24, informing him that Dr. Bialloblotzky had left that place for Makulla by the steamer "Sir Charles Forbes," Capt. Lichfield, and it was expected that by the end of the month he would be able to sail from Makulla for Mombás.

* Vol. xxxiii. p. 481.

POST-OFFICE REGULATIONS.

The speedy and cheap transmission of intelligence is of the highest importance for the interest of science, and our attention has been directed to it as a subject of general complaint, as well as by the inconvenience and loss which we ourselves experience.

In the Advertisement prefixt to the eighth volume of the Monthly Notices of the Royal Astronomical Society, the Council regret the difficulty and delay in receiving scientific information. "With other countries," they observe, "and for larger parcels, the communication is most unsatisfactory. The expenses and extra charges at the English ports are equivalent to a negative upon direct intercourse, even where the freight is prepaid, and the duty trifling. The Post-office charges for pamphlets over-sea are the same as for letters. Until these matters are better regulated, a greater service can scarcely be rendered to scientific bodies than by facilitating the rapid transfer of international communications at a moderate cost. Any information on this subject will be attended to."

We are glad to find that the subject has at length received attention from the authorities of the Post-office, and that some important improvements have been lately introduced: and with a view to render these available for the interests of science, we are glad to be enabled to furnish the following particulars from the Post-office regulations of the most recent date.

Periodicals published as pamphlets, and parliamentary proceedings, provided they are made up in the same manner as newspapers, in covers open at the sides, so as to admit of examination, are forwarded to the countries mentioned below at the following rates, which must be prepaid either in stamps or money.

Rates of Postage chargeable upon Periodical Publications and Parliamentary Proceedings to Foreign Countries.

Weighing and	not exceeding	*s.*	*d.*	Weighing and	not exceeding	*s.*	*d.*
.... ,, ...	2 ozs. .	0	1	9 ozs. . ,, ...	10 ozs. .	1	8
2 ozs. . ,, ...	3	0	6	10 .... ,, ...	11	1	10
3 .... ,, ...	4	0	8	11 .... ,, ...	12	2	0
4 .... ,, ...	5	0	10	12 .... ,, ...	13	2	2
5 .... ,, ...	6	1	0	13 .... ,, ...	14	2	4
6 .... ,, ...	7	1	2	14 .... ,, ...	15	2	6
7 .... ,, ...	8	1	4	15 .... ,, ...	16	2	8
8 .... ,, ...	9	1	6				

Beyond the weight of 16 ounces, they can only be forwarded at letter rates of postage.

The countries to and from which the above rates are applicable are:—

Belgium,
Bremen,
France,
Holland,
Prussia { *via* Belgium*, *via* Holland, *via* Hamburg.

* Periodicals, &c., when sent to Prussia *via* Belgium, are subject to a Belgian transit rate of 2*d.* per quarter ounce, in addition to the above rates. In charging works of this description, when more than one copy is under the same band, each copy is weighed and charged separately.

The rates to which parliamentary proceedings are liable when sent to the colonies, the rates for letters, prices current, &c., to the colonies and foreign parts in detail by every route, and numerous other particulars as to the despatch and arrivals of mails, &c. &c., will be found in the 'Post-Office Official Monthly Director,' corrected and published on the 1st of every month by Letts, Son and Steer, 8 Cornhill, price 1*s*. per single copy, or 8*s*. per annum.

METEOROLOGICAL OBSERVATIONS FOR DEC. 1848.

Chiswick.—December 1. Foggy and drizzly: cloudy: rain, and boisterous at night. 2. Fine. 3. Clear: overcast: boisterous, with rain at night. 4. Boisterous, with heavy rain: clear at night. 5. Overcast: clear: slight rain. 6. Clear: heavy clouds. 7. Rain. 8. Slight rain. 9. Very fine. 10. Clear and very fine. 11. Foggy: cloudy. 12. Foggy: uniformly overcast. 13. Exceedingly fine. 14. Fine. 15. Hazy: rain. 16. Drizzly: constant heavy rain. 17. Cloudy: foggy. 18. Hazy: fine: densely overcast. 19. Foggy. 20. Hazy: clear and frosty at night. 21. Clear and frosty. 22. Frosty: clear: frosty. 23. Foggy: hazy: sharp frost. 24. Frosty: slight haze: overcast. 25. Hazy: cloudy. 26. Densely clouded. 27, 28. Fine. 29. Overcast. 30. Foggy: fine: foggy. 31. Foggy: hazy: foggy at night.

Mean temperature of the month	41°·75
Mean temperature of Dec. 1847	41 ·09
Mean temperature of Dec. for the last twenty years	39 ·66
Average amount of rain in Dec.	1·58 inch.

Boston.—Dec. 1. Cloudy: rain A.M. and P.M. 2. Fine. 3. Fine: rain P.M. 4. Cloudy: rain P.M. 5. Cloudy: rain A.M. 6. Fine: rain A.M. 7. Rain: rain A.M. and P.M. 8. Fine: rain P.M. 9—11. Fine. 12. Cloudy. 13. Fine. 14. Cloudy: rain P.M. 15. Cloudy: stormy, with rain from S.W. P.M. 16. Cloudy: rain P.M. 17, 18. Fine. 19. Rain: rain early A.M. 20. Cloudy. 21. Fine: plenty of ice this morning. 22—24. Fine. 25, 26. Rain. 27, 28. Cloudy. 29. Fine. 30. Cloudy. 31. Cloudy: a remarkable dark day.

Applegarth Manse, Dumfries-shire.—Dec. 1. Frost A.M.: rain and high wind P.M. 2. Rain: sleet: high wind: lightning. 3. Snow inch deep: heavy rain P.M. 4. Very high flood: heavy rain and high wind. 5. Fair, after very wet night: flood again. 6. Dull: drizzling: frost A.M. 7. Frost: damp and drizzly P.M. 8. Soft, moist and foggy. 9. Rain all day: high wind P.M. 10. Fair: high wind. 11. Fair and fine. 12. Dull and foggy A.M.: rain P.M. 13. Rain A.M.: showery all day. 14. Fair A.M.: rain and high wind P.M. 15. Fair A.M.: rain P.M., with storm of wind. 16. Fair and fine. 17. Frost A.M.: slight showers P.M. 18. Fair A.M.: cloudy: showery P.M. 19. Fair: fog: cleared P.M. 20. Frost: thaw P.M. 21. Frost, hard: clear and bracing. 22. Frost very hard: clear. 23. Frost keen: clear: wind rising. 24. Frost: high wind P.M. 25. Frost, slight: thaw P.M. 26. Rain very heavy: high wind. 27. Fair and clear: threatening frost. 28. Hard frost all day. 29. Hard frost. 30. Frost moderate: dull. 31. Frost moderate: cloudy.

Mean temperature of the month	39°·8
Mean temperature of Dec. 1847	40 ·2
Mean temperature of Dec. for the last twenty-five years .	38 ·2
Average amount of rain in Dec. for twenty years	2·94 inches.

Sandwick Manse, Orkney.—Dec. 1. Hoar-frost: rain. 2. Cloudy. 3. Rain: cloudy. 4. Showers: thunder: hail-showers. 5. Hoar-frost: showers. 6. Bright: showers. 7. Bright: clear. 8. Showers: rain. 9. Cloudy: rain. 10. Hazy: rain: clear. 11. Cloudy: clear. 12. Cloudy: rain. 13. Bright: showers. 14. Cloudy. 15. Bright: rain. 16. Showers: clear. 17. Showers: cloudy. 18. Cloudy. 19. Bright: clear. 20. Cloudy. 21. Bright: clear: aurora. 22. Clear: frost: clear: aurora. 23. Clear: frost: clear. 24, 25. Cloudy. 26. Rain: cloudy. 27. Showers: clear. 28, 29. Clear: frost: clear. 30, 31. Cloudy.

Meteorological Observations made by Mr. Thompson *at the Garden of the Horticultural Society at* Chiswick, *near London; by* Mr. Veall, *at* Boston; *by the* Rev. W. Dunbar, *at Applegarth Manse*, Dumfries-shire; *and by the* Rev. C. Clouston, *at Sandwick Manse*, Orkney.

Days of Month.	Barometer.							Thermometer.							Wind.				Rain.			
	Chiswick.		Boston.	Dumfries-shire.		Orkney, Sandwick.		Chiswick.		Boston.	Dumfries-shire.		Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Dumfries-shire.	Orkney, Sandwick.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.
1848. Dec.	Max.	Min.	8½ a.m.	9 a.m.	9 p.m.	9½ a.m.	8½ p.m.	Max.	Min.	8½ a.m.	Max.	Min.	9½ a.m.	8½ p.m.								
1.	29·644	29·355	29·36	29·42	28·93	29·42	28·82	48	34	37·5	43	34½	34	39	sw.	s.	sw.	s.	·19	·02	.....	·05
2.	29·612	29·362	28·96	28·87	29·12	28·69	28·94	45	29	37	41½	36½	37	37	sw.	sw.	sw.	sw.	·02	·22	.....	·50
3.	29·797	29·579	29·90	29·39	29·03	29·09	29·16	51	36	33	44½	31	40	39	sw.	w.	se.	se.	·01	.....	.....	·47
4.	29·268	29·116	28·80	28·81	28·58	28·74	28·72	53	38	50	48½	40	45	39	sw.	s.	s.	n.	·19	·04	.....	·55
5.	29·215	28·986	28·50	28·60	28·87	28·70	28·73	53	39	42	43½	36	38	40	sw.	wsw.	w.	nw.	·01	·04	.....	·61
6.	29·376	29·215	28·84	28·91	28·97	28·70	28·93	47	40	40	41	32	40	38	sw.	wsw.	se.	ne.	·05	·12	.....	·30
7.	29·670	29·505	29·17	29·25	29·39	29·33	28·51	55	50	42	40½	30	38	35	s.	s.	w.	n.	·15	·22	.....	·07
8.	30·015	29·775	29·30	29·31	29·61	29·51	29·56	57	48	55	49½	32½	38	41	sw.	ssw.	s.	w.	·01	·18	2·50	·01
9.	30·253	30·130	29·58	29·69	29·77	29·69	29·53	57	33	51	50½	46	42	47	sw.	ssw.	s–sw.	s.	.....	·14	.....	·20
10.	30·273	30·155	29·73	29·88	29·84	29·77	29·61	60	29	47	52½	48½	48½	51	sw.	s.	sw.	s.	·01	.....	.....	.....
11.	30·160	30·038	29·64	29·76	29·90	29·55	29·76	60	38	43	53	47½	49	47	sw.	s.	sw-w.	sw.	.....	.....	.....	·16
12.	30·164	30·097	29·70	29·90	29·70	29·80	29·56	55	37	51	51	43	45	49	sw.	w.	s.	s.	.....	.....	.....	.....
13.	30·029	29·978	29·55	29·57	29·67	29·41	29·59	58	34	45	52	49	49½	46½	s.	s.	ssw.	se.	.....	.....	.....	·19
14.	29·831	29·732	29·37	29·48	29·41	29·47	29·33	52	40	44	54	50	49	47	s.	s.	s.	sse.	·14	.....	.....	.....
15.	29·810	29·640	29·49	29·48	29·18	29·56	28·68	55	43	45·5	53½	42½	41	45	s.	calm	ssc.	s	·22	·05	.....	.....
16.	29·809	29·731	29·38	29·56	29·60	29·23	29·52	42	38	45	46	37	43½	41	ne.	s.	s.	ssw.	·68	·03	0·77	·35
17.	29·937	29·872	29·54	29·61	29·63	29·47	29·54	48	33	37	43	32	42	43	ne.	calm	s-sw.	se.	·01	·07	.....	·05
18.	29·856	29·771	29·48	29·58	29·48	29·55	29·55	52	43	40	44	37	43	44	s.	calm	sse.	se.	·01	.....	.....	·03
19.	30·094	29·808	29·50	29·75	29·97	29·81	30·05	48	40	46	44	37	41½	41	ne.	calm	e.	sse.	·01	·12	.....	.....
20.	30·301	30·172	29·93	30·10	30·20	30·15	30·19	40	24	40	43½	32	45	41	ne.	sw.	e.	sse.	.....	.....	.....	.....
21.	30·334	30·315	30·06	30·24	30·23	30·18	30·20	34	23	33·5	33	27	37	36	e.	e.	e.	sse.	.....	.....	.....	.....
22.	30·345	30·211	30·14	30·30	30·35	30·26	30·39	39	22	32	38	23½	37	35	e.	e.	ne.	s.	.....	.....	.....	.....
23.	30·343	30·204	30·13	30·34	30·18	30·42	30·32	33	21	35	37	28	39	38½	e.	e.	e.	se.	.....	.....	0·12	.....
24.	30·093	29·706	29·83	30·00	29·78	30·18	30·03	40	26	35	37	33½	40	40½	e.	ese.	e.	ese.	·02	.....	.....	.....
25.	30·046	29·836	29·60	29·70	29·80	29·90	29·90	49	35	37	42	34	42	44	ne.	se.	e.	se.	.....	.....	.....	.....
26.	30·024	29·975	29·70	29·54	29·38	29·51	29·42	51	40	42	49	39	45	43	s.	e.	ese.	se.	.....	·08	.....	·08
27.	30·103	29·985	29·15	29·74	29·97	29·64	30·00	49	39	48·5	48½	43	42	44	w.	sw.	w.	nw.	·30	.....	.....	·25
28.	30·166	29·985	29·76	30·04	30·09	30·10	30·13	44	39	48	36½	26½	36	36	ne.	w.	nw.	s.	.....	.....	.....	.....
29.	30·180	30·149	29·92	30·10	30·09	30·11	30·13	44	30	29	35	25	37	40	n.	calm	nw.	sse.	.....	.....	.....	.....
30.	30·170	30·084	29·30	30·06	30·02	30·15	30·13	40	29	37	36½	29½	39	39	ne.	calm	e.	sse.	.....	.....	0·50	.....
31.	30·126	30·110	29·90	30·05	30·10	30·13	30·20	40	30	37	35	32	39	37	ne.	calm	e.	s.	.....	.....	.....	.....
Mean.	29·968	29·824	29·52	29·646	29·643	29·620	29·584	48·67	34·83	41·4	44·0	36·3	41·35	41·40					2·03	1·33	3·89	3·87

THE
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[THIRD SERIES.]

MARCH 1849.

XXI. *Hints towards a Classification of Colours.* *By* Professor J. D. Forbes*.

To classify and describe colours is not so easy a matter as it might at first sight appear to be; and yet it is of very considerable interest, as well scientific as artistical. It is impossible that this shall be completely done until we understand far better than at present the *cause* of the colours of bodies, natural or artificial. Had Newton's explanation of these colours been as satisfactory as it was for the phænomena of the solar spectrum, the classification of colours would be more simple and obvious than it is. My present object is to treat the subject not at all as a matter of art, neither as to the effect which colours produce in painting as an imitative art, nor as to the chemical art of producing and combining pigments; but simply as a matter of description and nomenclature, so that the objective effects of varied tints and hues may be referred to some standard classification of colours and their modifications. I shall then state what progress I have made in forming such a standard suite of colours.

I am unable, and have not the intention, to give a complete history of the principles and methods adopted at different times for classifying and compounding colours for the use of the painter, and in imitation of *natural hues* which probably exhaust all which art can succeed in producing. For what I shall now state I am in part indebted to the ingenious essay of the profound Lambert, called *Beschreibung einer Ausgemahlten Farbenpyramide*†.

Pliny, in describing the pigments used by the most famous painters in their pictures, mentions four, which, according to

* Communicated by the Author having been read before the Royal Society of Edinburgh, December 4, 1848, and January 15, 1849.

† 4to. Berlin, 1772.

Lambert, were white, ochre-yellow*, red and black; and he supposes that a bluish tint may have been obtained by diluting the black with white. Leonardo da Vinci, himself a painter of the first order, appears to have had clear ideas on the subject of the formation of compound colours from their simplest elements; and although he reckons six, namely white, yellow, green, blue, red and black, yet it is scarcely to be supposed that he was not aware that green could be compounded of yellow and blue, and therefore we may probably admit that he regarded blue, red and yellow as the primary colours, to be mixed with white or black according to the degree of shadow in which they are to be represented.

Waller, in the Philosophical Transactions for 1686, attempts a classification of colours and pigments, proceeding upon the basis (which appears to be there assumed as generally admitted) that red, yellow and blue are the sole primary colours. This paper was originally accompanied by a diagram of hues, which is, however, wanting in the copies which I have examined.

Newton's discovery of the composition of white light was, of course, an important step in the theory of the composition of colours generally; although that apparent paradox did not fail to introduce some difficulties into the explanation of the action of pigments, which have not unnaturally affected the views of persons accustomed to regard the subject solely as one of art, and at the same time to complicate it somewhat by the introduction of seven colours in the complete spectrum, red, orange, yellow, green, blue, indigo and violet. Perhaps it is not too presumptuous to say, that but for some peculiar respect for the number seven, and more particularly from a fancied analogy between the spaces occupied by the colours and the musical intervals, Newton would not have classed blue and indigo as distinct colours; in which case we may consider that the Newtonian spectrum consists of the three primary colours, red, yellow and blue, and the three secondary, orange, green and purple. Newton was perfectly aware that by combining the primary colours, such as yellow and blue together, a green, not distinguishable from that of the spectrum *except by its refrangibility*†, will be formed; and he also observed the effect of combining three or more coloured rays, which generally tend to a more or less perfect whiteness, though it does not appear that Newton ever actually formed white light by the *partial* combination of certain rays of the spectrum.

* *Silaceus*, the word translated ochre-yellow, is of very doubtful signification.

† Optics, Book I. part 2, prop. iv.

For he states (Prop. VI. p. 136, edit. 1730) expressly, "I could never yet by mixing only two primary colours produce a perfect white. Whether it may be compounded of a mixture of three taken at equal distances in the circumference [of the figure of a circular spectrum which he is describing] I do not know; but of four or five I do not much question but it may. *But these are curiosities of little or no moment to the understanding of the phænomena of nature.* For in all whites produced by nature there uses to be a mixture of all sorts of rays and by consequence a composition of all colours*."

Every optician knows Newton's empirical rule for the estimation of the colour produced by the mixture of any number of the elementary colours of the spectrum and in any proportions. But it is necessary here to repeat it, because it would appear to simplify the scale of colours much indeed, were colours only such as the composition of the coloured rays of light present. Imagine a circle drawn whose centre is pure white, and its circumference presents in order all the colours of the spectrum in succession, and occupying arcs proportioned to their lengths in the true spectrum, the two ends of the spectrum or the extreme violet and red coalescing at one place†. Let these colours pass gradually, by the mixture of white light, from their intensest development at the circumference to perfect whiteness at the centre of the circle. Suppose it were required to find the effect of mixing two parts of green, one of red and one of violet. At the circumference of the circle and at the centre of the green space, describe a small circle whose area is 2; at the middle points of the red and violet describe circles whose areas are 1; find the centre of gravity of the three circles: it will be found on the diagram over the tint which the mixture of rays, if actually formed, will present.

Since the quantity of colour in any mixture is to be regarded in this arbitrary construction as applied at the centre of the arc appropriated to each colour, and since no two such points are directly opposed to one another in the circle, it follows by construction (as M. Biot has remarked‡) that a perfect white

* Optics, Book I. part 2, prop. iv.

† The limits of the several colours will occur, according to the Newtonian proportions, at the following angles in a continued graduation :—

Red begins at	0° 0′
Orange	60·46
Yellow	94·57
Green	149·38
Blue	210·23
Indigo	265·4
Violet..........................	299·15

‡ *Traité de Physique*, vol. iii. p. 449.

cannot be compounded out of only *two* colours in the spectrum. This corresponds with Newton's experience, that such a colour (the mixture of two opposites) "shall not be perfectly white, but some faint anonymous colour*." But these experiments merit well a careful repetition, which I am not indeed aware that they have ever received; and it is very probable that Newton never made them with a pure, or even an approximately pure spectrum†.

But Newton's celebrated experiment of mixing together coloured powders until he obtained a perfectly indefinite gray is most to our present purpose. He describes in the fifteenth experiment of the second part of his first book of Optics, the various dry pigments which he employed, the most effective of which was a mixture of orange, purple, green and blue, which "became of such a gray or pale white as verged to no one of the colours more than to another" (p. 131), which when powerfully illuminated by the sun was an exact match for a pure white paper less perfectly illuminated. The reason why it does not appear absolutely white under ordinary circumstances Newton thus explains:—"All coloured powders do suppress or stop in them a very considerable part of the light by which they are illuminated. For they become coloured by reflecting the light of their own colours more copiously and that of all other colours more sparingly, and yet they do not reflect the light of their own colours so copiously as white bodies do." [This he illustrates by illuminating red lead and white paper with the red ray; the white paper appears the more brilliantly red of the two.] "Therefore by mixing such powders [powders, namely, of various colours,] we are not to expect a strong and full white, such as is that of paper, but some dusky obscure one such as might arise from a mixture of light and darkness, or from white and black, that is, a gray, or dun, or russet-brown, such as are the colours of a man's nail, of a mouse, of ashes, of ordinary stones, of mortar, of dust and dirt in the highways and the like."—P. 130.

Whatever may be thought of Newton's theory of the colours

* Optics, ed. cit. p. 136.

† If Newton's circular figure was intended (which, however, M. Biot questions) to be divided according to the accurate proportions of the colours in the spectrum, it would be a matter of great difficulty to assign the due proportions to the extreme red and violet; any variation in this respect would alter the character of the diametrically opposed tints, and make a perfectly white compound of two tints possible, or the reverse. It is hardly necessary to add here, that the perfect white produced by the complementary tints which occur in experiments of depolarization, arises from the mixture of colours of a very complex constitution.

of natural bodies (which refers them to the colours of thin plates), the reasoning of the above paragraph will hardly be questioned. There will therefore be always this essential difference between compounding rays of the spectrum and compounding pigments; that in the former case, by throwing light of two or more colours upon a white screen, each of these colours being reflected with equal vividness, the brightness of the screen will be the *sum* of the brightnesses due to the several rays (and if a sufficient number of rays be combined, the result will be a dazzling white); but, on the other hand, by combining pigments we do not add together *lights*, but merely construct a ground or screen capable of scattering a greater number of the constituents of a beam of white light which falls upon it. Thus there will be an inevitable quantity of *darkness* or absorbent faculty in the constitution of every artificial colour, whatever be its predominant reflecting hue; and the mixture of pigments will not tend to increase the brightness, as the mixture of lights would do, but only to mix with the fundamental darkness of the surface a portion of light which shall be of a mixed instead of a simple hue.

Let us suppose for a moment a simple case. Let us admit that a paper thickly coated with ultramarine can reflect none but blue rays, and that a paper coated with chrome-yellow reflects only yellow rays. But further than this, a share of the blue and of the yellow light falling on each is absorbed; suppose one-half to be thus lost. If a compound pigment of blue and yellow be formed and exposed to the same white ray as before, we cannot expect that it should have more brilliancy than either one or other of the primitive colours, whilst it is evident that the union of rays of yellow and blue light upon a white screen would have a twofold splendour*. For we must admit the reflecting particles in each of the separate pigments to be so densely spread that a ray of light can fall nowhere, on the ultramarine for instance, but it finds a particle of colour ready to decompose and reflect it, and the same of the pure yellow pigment; in a mixture, therefore, of the two, the surface may be regarded as equally divided amongst an infinite number of the blue and the yellow reflecting points, so that the reflected light is half yellow, half blue, but altogether is no more than the amount which either pigment covering the whole surface would have reflected. We must not therefore suppose that by mixing pigments we render the surface on the whole more reflective, that is to say, more luminous, than before. Experience confirms this anticipation. On the

* What is meant here by speaking of rays of different colours having "equal" or "two-fold" brilliancy will be explained by and by.

whole, then, in this experiment, half the light is still absorbed and half of the remainder is yellow, the rest blue, constituting a green colour.

Similar reasoning will hold true of any number of separate colours combined into one; and as perhaps no pigment reflects only one pure colour of the spectrum, the mixtures will always be more compound than they are assumed to be, and give hues of always increasing impurity.

The process of mixture cannot in any case be expected to *improve* the power of reflecting the pure colours residing in the constituent pigments. It is much more likely to deteriorate it, which will tend to give a tone of always less absolute brightness to more complex colours.

The process of mixing pigments may often affect their relative strength, especially if moisture be used. There may be a mutual action, which will cause an undue preponderance of one or other constituent. These difficulties have constantly been felt by those who have endeavoured to compound colours from their elements.

We have assumed above that the reflective power of pigments of different colours is the same. But this does not appear to be the case. The ingenious experiments of Lambert*, which though probably but rough approximations are yet valuable as such, inform us that the whitest surface reflects $\frac{4}{10}$ths of the white incident rays, and king's yellow almost as much of rays of its own colour: but the brightest red (cinnabar) reflects but $\frac{1}{3}$rd of its own coloured light, and a blue surface (mountain Berlin blue) but $\frac{1}{7}$th of the blue rays.

On these principles we may expect that if the circumference of a wheel be painted with stripes of red, yellow and blue, alternating with one another, so that the extent occupied by these colours shall be in certain determinate proportions, the mixture shall appear white, or rather *neutral gray*, the wheel being put in rapid rotation. We can estimate the illumination of the surface compared to that of white paper in the following manner.

The proportional extent of the surfaces may be found by direct experiment, or otherwise thus. By Newton's rule of compounding colours (see p. 163), we may deduce that a white compounded of red, yellow and blue, must consist of 38·6, 19·6, and 41·8 rays out of 100 of these colours respectively; for the centres of gravity of the red, yellow and blue sectors make angles of 91° 54′·5 between the red and yellow, 115° 26′ between the yellow and blue, and 152° 39′·5 between the blue and red. In order that the centre of gravity of the whole shall

* *Photometria,* § 747; and *Farbenpyramide,* § 5.

coincide with the centre of the circle, the primary colours must be in proportion to the sines of those angles, which are ·9785, ·9031 and ·4593; the first being blue, the second red, and the third yellow, which give the proportions above stated. And there can be little doubt that this rule is sufficiently correct, though we restrict the colours of the spectrum to three only; for the centre of gravity of the blue (for example) may be regarded as the centre of gravity likewise of the blue contained in the green, and that in the indigo and violet; and so of the other colours.

Let then the proportions 0·386=R, 0·196=Y, 0·418=B represent the constituents of white light in the spectrum, their sum being =1. But by what has been said of Lambert's experiments, it appears that red, yellow, and blue pigments reflect but $\frac{1}{3}$rd, $\frac{4}{10}$ths, and $\frac{1}{7}$th of the rays of those respective colours which fell upon them. Therefore to have *reflected* light of the same composition with the white of our tricoloured spectrum, we must have the surfaces of the colours larger in proportion as their reflecting power is less. Hence the spaces in our coloured wheel must be

Red . .	3R or . .	$1·157=r$.
Yellow .	$\frac{10}{4}$Y or . .	$0·490=y$.
Blue . .	7B or . .	$2·927=b$.
The sum of these	. .	$4·574=n$.

Consequently, of all the red rays which fall upon our tricoloured surface only the fraction $\frac{1}{3}\cdot\frac{r}{n}$ are reflected (for of those which fall on the yellow and blue spaces, none are reflected, and but one-third of those falling on the red), that is,

$\frac{R}{n}$ or ·0843

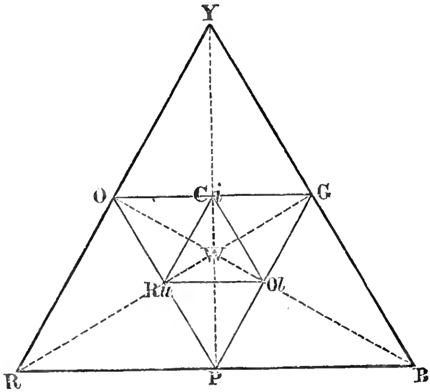
Of the whole yellow rays are reflected $\frac{Y}{n}$ or ·0429

Of the whole blue rays are reflected . $\frac{B}{n}$ or ·0914

In short, the reflected light is *white light*, whose intensity is attenuated by reflexion n or 4·57 times; whereas had it been incident on white paper, it would have still had $\frac{4}{10}$ths of its first brightness, or been attenuated only 2·50 times. This abundantly explains why the result is a *gray* colour, not a bright white*. The proportions of the *surfaces* of bright colours whose mixture produces white (r, y, b in the preceding notation) is 5, 3 and 8, as given by Field†.

It is to Mayer, the mathematician, that we owe a complete and perfect diagram of mixed colours, starting from red, yellow and blue, as constituents. Let the extreme corners of a triangle be painted of these colours, and let the periphery of the triangle be composed of graduating colours between each pair of these respectively; then the centres of the sides of the triangle will be occupied by perfect orange, perfect green and perfect purple, each of which will pass in each direction towards the predominating primary colour. The periphery of Mayer's triangle includes, therefore, all the colours of the spectrum, or primary colours mixed two and two. But combinations of three colours may be represented by selecting points in the interior of the triangle which shall be the centre of gravity of the constituent colours. Thus if the three colours, red, yellow and blue, be mixed in equal proportions, the resulting colour, which will be neutral gray, will be found at the centre of gravity of the triangle at W. But this would also result from the mixture of one portion of red and one of blue united at P to form two of purple, which then being compounded with one of yellow, Y, will give the centre of gravity at one-third of the distance from P towards Y. The

Fig. 1.



* Goethe, in his Theory of Colours, seems to think that he has overturned Newton's experimental demonstrations by calling the opinion that "all the colours mixed together produce white," "an absurdity which people have credulously been accustomed to repeat for a century in opposition to the evidence of their senses." (Eastlake's translation, p. 225.) The truth is, that "gray" is not an affection of Light at all, but of Surface merely. All Light combining the coloured elements in due proportion is essentially *white*, though more or less intense; but no Surface can be said to be *perfectly white* rather than *gray*, except by comparison with another. A surface of white paper illuminated by common daylight is gray relatively to a similar one placed in full sunshine.

† Field's Chromatography, p. 247.

same result will evidently flow from mixing two parts of orange with one of blue, or two of green with one of red.

It also clearly follows from this construction, that a point in the triangle may always be found which shall represent *any possible* proportional mixture of the three colours, because the centre of gravity of the three elements, however unequal, must of necessity be found within the triangle.

Also, a complex colour of three elements may be regarded as composed of primary colours and their binary compounds in an infinite variety of ways. Thus, the colour called *citrine* by some authors, and which is described as a compound of equal parts of orange and green, has its place in the triangle at C*i*, which shows that it is intermediate between pure yellow and neutral gray in the proportion of 1 of the first to 3 of the second; or it is a mixture of pure yellow and pure purple in equal proportions.

The annexed diagram shows the principle of Mayer's mixture of colours, the subscribed figures denoting the relative portions of each colour in any compound, the sum of the units making up 8 in every case. The same principle of numerical ratios may be extended to any degree of nicety; but it is soon found that the power of the eye in distinguishing hues is overpassed.

Fig. 2.

| y_8 |
$y_7 r_1$	$y_7 b_1$
$y_6 r_2$	$y_6 r_1 b_1$	$y_6 b_2$
$y_5 r_3$	$y_5 r_2 b_1$	$y_5 r_1 b_2$	$y_5 b_3$
$y_4 r_4$	$y_4 r_3 b_1$	$y_4 r_2 b_2$	$y_4 r_1 b_3$	$y_4 b_4$
$y_3 r_5$	$y_3 r_4 b_1$	$y_3 r_3 b_2$	$y_3 r_2 b_3$	$y_3 r_1 b_4$	$y_3 b_5$
$y_2 r_6$	$y_2 r_5 b_1$	$y_2 r_4 b_2$	$y_2 r_3 b_3$	$y_2 r_2 b_4$	$y_2 r_1 b_5$	$y_2 b_6$
$y_1 r_7$	$y_1 r_6 b_1$	$y_1 r_5 b_2$	$y_1 r_4 b_3$	$y_1 r_3 b_4$	$y_1 r_2 b_5$	$y_1 r_1 b_6$	$y_1 b_7$
r_8	$r_7 b_1$	$r_6 b_2$	$r_5 b_3$	$r_4 b_4$	$r_3 b_5$	$r_2 b_6$	$r_1 b_7$	b_8

The *unit of mass* for any primary colour or pigment is the proportion which, mixed with the other two primaries, forms a perfectly neutral gray. This must be found by experiment, and resembles the atomic weight or equivalent of the simple bodies of chemistry. Lambert found it by uniting carmine and gamboge until a perfect orange was formed, which (judging by the eye) inclined neither to red nor yellow; so with yellow and blue forming green, and with blue and red forming purple. The quantities being weighed in each case, two such experiments were sufficient to determine the relative powers of the colours, but the third was used to confirm them. He thus found the combining proportions by weight of carmine, gamboge and Berlin blue, to be 1, 10 and 3*.

We are now to consider how far this triangle carries us towards a complete scale of colours. It is manifest that the *intensity* of the colours depends upon the reflective power of the pigments used, and that this essentially varies for the different primary colours. In no sense, then, can we be said to have red, yellow and blue, of *equal* brightness at the corners of our triangle; for even if we assume as merely convenient definition, that by equal brightness of different colours we mean the proportions in which, when combined, white light results, we have already seen that the yellow pigment, being far most reflective, will be brightest, then the red, and after all the blue. But in fact we have no scale at all for comparative brightness of heterogeneous colours. We must take the pigments *purest* in *quality*, and most *lucid* or reflective as regards the *quantity* of light which they scatter, and consider these as the primary colours. The mixed colours also vary in their lucidity, according to the prevalence of a more or less lucid component; the yellow hues will be most lucid, the blue least.

When the triangle is exposed to a brighter light, the proportions of the colours remain unchanged, and the whole will be more *lucid*.

It is probable, however, that the decomposing action of all pigments upon light is limited; and that a coloured surface may be so *drowned* in white light, that much of the light is returned undecomposed, and the colour is thus *diluted*.

If less light fall on the triangle, a different kind of dilution occurs, only pure coloured light will be reflected, but so little of it as to affect the eye but slightly, or not at all with the sense of colour.

In the latter case all colours pass into indistinguishable blackness, in the former case into indistinguishable whiteness.

If we mix black and white pigments with coloured pigments, we may have both these variations exhibited at once under a common external illumination.

If we have a series of triangles thus constructed, they will embrace under one common illuminating influence (as ordinary daylight) all possible varieties of hue and shade under that illumination. Every conceivable natural or artificial object, such as a piece of stuff, a feather, or a flower, ought to be capable of being *matched* with one or other of the spaces in these triangles. This is all that we propose to accomplish. If we choose the most lucid known bodies for our primary

* *Farbenpyramide*, § 63.

colours, we shall be sure to have none to match which are not included in our suite.

The question now arises of the number of intermediate mixed colours which can be interpolated between any two primary or simpler colours, so that each may be distinguishable by the eye upon a close comparison. The number is much smaller than might be supposed. Lambert states, that from perfect *black* to perfect *white* he could only trace thirty intermediate shades distinguishable by the eye under *the most favourable circumstances possible**. The number of gradations of even the most positive colours is probably considerably less, and of the more neutral colours much less again; at least if we do not repeat those semi-neutral compounds which are indistinguishable from one another.

As to the transition from one *quality* of colour to another without regard to its dilution with light or shade, as, for instance, from red to blue, the sensible intermediate stages are also probably much fewer than might be suspected. Mayer affirms† that the distinction of mixed colours is evident so long as the sum of the component parts denoted, as in the diagram, fig. 2, does not exceed 12. Thus a bright yellow, such as king's yellow, being denoted by y_{12}, yellow ochre is $y_{10}\, r_2$, umber is $y_6\, r_3\, b_3$, ivory black $y_2\, r_3\, b_7$, in all which cases the sum of the parts is equal to 12. Upon this scale it is easy to show that the fundamental triangle, whose side is 13, will contain 91 coloured spaces. These embrace all possible combinations of colour, of the fundamentally greatest intensity which the imperfection of our pigments enable us to procure.

Allowing 4 gradations of each colour into *blackness* and 4 into *whiteness*, Mayer reckoned 819 colours in all; a number which will certainly appear small considering the apparent infinity of hues and shades. It is probably sufficient, however, for matching any colour by reference to two others, one above and the other below it, in any of the scales; and such subdivision may probably be carried by the eye to greater accuracy than one intermediate step. The gradation of nine steps from perfect black to perfect white through any colour is perhaps too small; but on the other hand, the neutral colours, as already observed, some of them at least, lose their distinguishable characters compared with one another, when diluted either with black or with white, but especially the former‡. Taking advantage of this consideration, Lambert modified Mayer's triangles by reducing them continually in

* Lambert, *Farbenpyramide*, § 10, 11. † Ibid. § 29.

‡ I suspect, indeed, that in some instances the dilution of the semineutrals with *white* renders them more easily distinguished, but only down to a certain point.

size and in the number of elements, as the standard colours approached white on the one hand and black on the other; forming thus a double pyramid, whose common base was Mayer's triangle, the colours vanishing into white at one apex and into black at the other*. A triangular pyramid or tetrahedron of 13 elements in the side would contain 455 elements, and the double pyramid or hexahedron 910 elements.

Other writers have attempted to adopt primary colours different from red, yellow and blue; and with this subject has been mixed up the inquiry into the actual composition of the solar spectrum, which though not immediately connected with it, may be mentioned in passing. Mayer maintained, not merely that all colours whatever may be formed by the combination of red, yellow and blue, but that in reality these colours alone exist in the solar light†, which he inferred by looking through a prism at a black spot on a white ground, an essentially faulty mode of operating. But later, Dr. Wollaston came to the conclusion that the solar spectrum is composed of four colours only, namely red, green, blue and violet, without any gradations in the quality of the colour. Dr. Young, finding experimentally that "the perfect sensations of yellow and of blue are produced respectively by mixtures of red and green, and of green and violet light‡," assumes that the primary colours are red, green and violet; a singular opinion, which appears to rest on no particular evidence further than the disjunction of the red and violet rays at the two ends of the spectrum, and which has met with no support any more than that of Abbé Nollet, who maintained the primary qualities of orange, green and purple. In truth no synthetical experiment can give any sure countenance to one or other of these views; for the fact that red and green combined in certain proportions produce yellow, admits of equally sound interpretation by supposing that the green, being a compound of yellow and blue, the whole of the blue and a part of the yellow combine with the red to produce a perfect white, which then dilutes the outstanding portion of the yellow; and in like manner a perfect purple mixed with perfect green must make a perfect blue diluted with a perfect white. *Analysis*, however, where possible, must lead to more conclusive results; and Sir David Brewster considers that the orange, green, and purple of the spectrum are really composed of two, if not three colours

* In the coloured plate accompanying Lambert's work we find only the pyramid of colour diluted with white, which he seems to have considered sufficient in practice, § 39. In reality, however, the *shades* or mixtures with black are indispensable components of such a system.

† Mayer, *Gottingischen Anzeigen*, quoted by Lambert, p. 30.

‡ Lecture XXXVII.

each*. The analysis he employs is the absorbing power of media, for these colours are (as is well known) undecomposable by refraction.

Various attempts have been made to carry out Mayer's principle of compounding colours from red, yellow and blue, and some elaborate attempts have been made to obtain model suites of colour. I shall at present only refer to Mr. D. R. Hay's ingenious work called Nomenclature of Colours, which he has illustrated by a very large number of selected hues and shades all compounded from Red, Yellow and Blue, variously diluted with Black and White, which, from Mr. Hay's skill in the choice and use of colours, are probably as pure and vivid as we can expect to produce in the present state of art. It is unnecessary here to speak of the taste and skill with which the harmony and contrasts of colour are used and illustrated in the plates to his work.

As a mere classification of colours, Mr. Hay's work does not adopt the simplest form; nor is the nomenclature, I conceive, by any means free from objection. It would be difficult, for instance, to refer any required colour to its place in a complete system of hues and shades by merely looking over Mr. Hay's plates. The specimens which have the closest affinity are often widely separated; but then the object, a purely artistic one, was different from ours. The mixtures used by Mr. Hay in his gradations of colour were made, I understand, by the eye, and not by weight; but an experienced eye will perhaps make a gradation at least as good as a quantitative one. The dilutions with white (or tints, as Mr. Hay calls them,) appear to be less perfect in this respect.

The primary colours in Mr. Hay's work are red, yellow and blue, or those which occupy the angles of Mayer's triangle, fig. 1. They are composed of carmine, chrome-yellow, and French ultramarine.

The secondary colours, or orange, green and purple, with their gradations into their component primaries, exhaust all the combinations two and two of the primaries, embracing all the colours of the spectrum, and are represented by the exterior row of colours in Mayer's triangle.

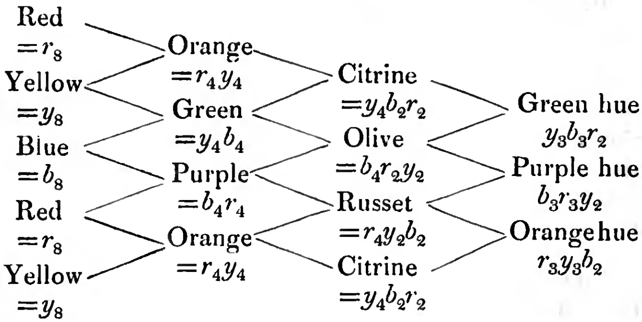
The combination of colours by three at a time leads to more complexity, and the advantage of Mayer's system is here most evident. Mr. Hay, following Field, calls *tertiary colours* those produced by a union of the secondaries: thus—

Orange and green form Citrine,
Orange ... purple ... Russet,
Purple ... green ... Olive.

* Edinburgh Transactions, vol. xii.

"Their distinctions arise from a double occurrence of yellow in the first, of red in the second, and of blue in the third*." In like manner, by the combination of the tertiary colours (or primary hues, as Mr. Hay calls them), he produced a system of quaternaries (or secondary hues), and so on until the small predominance of any one or two primary colours in the compound reduces the whole to a neutral gray. Now a simple inspection of Mayer's triangle, p. 168, shows that the confusion of the colours, by drawing a series of perpetually inscribed triangles, increases with great rapidity, and that consequently the gradations of shades will not be such as affect the eye most sensibly, but will be deficient in the brighter and redundant in the grayer colours.

The law of composition of the secondary and tertiary colours is however worthy of notice, and may be represented in the following diagram (employing the notation of page 169).



Hence *citrine*, for instance, is a mixture of four parts of yellow, with two of blue and two of red, which is equivalent to two parts of yellow and six of neutral gray (since gray $=y\,b\,r$); citrine is therefore yellow verging into gray, one-fourth of the mixture being yellow and three-fourths gray; its place in Mayer's triangle, fig. 1, will therefore be at *Ci* dividing the line YW in the ratio of 3 to 1; and so it does by construction, taking the centre of gravity of the orange and green, O, G.

Let us take an instance of a still more indefinite colour, a colour of the fourth order, which Mr. Hay calls a secondary hue, and to which he gives the distinguishing names of orange hue, green hue, and purple hue; we find these to be thus composed:

$$r_3y_3b_2, \quad y_3b_3r_2, \quad b_3r_3y_2;$$

or thus:

2 orange + 6 gray, 2 green + 6 gray, 2 purple + 6 gray.

* Hay, p. 18.

We therefore refer them at once to their places in Mayer's triangle as intermediate between the secondaries and neutrality, and dividing the interspace in the ratio of 3 to 1, as in the former case; only here the compound is more neutral, because the secondary colours are themselves one stage on the way to neutrality.

Thus we arrive at this conclusion, that *all combinations of three primary colours* (as far as difference of *quality* is concerned) *may be represented by transitions from the primary and secondary colours into gray*; and thence, though it may appear at first sight paradoxical, though the *quality* of a primary or secondary colour (such as red or green) is not changed by diluting it with white, it *is* changed by mixing it with gray, or by first mixing it with white and then diminishing the intensity of light in the mixture.

Hence a classification of colours may be made, which, although redundant in some parts, has the advantage of pointing out clearly the composition of each in this point, and also of suggesting a convenient nomenclature, which I propose to adopt in preference to Mr. Hay's (where they differ), as more expressive of the composition of each. This diagram, like Mayer's triangle, includes colours varying in *quality*, but of standard *intensity* and of the highest attainable *purity*. This diagram was obligingly arranged for me by Mr. Hay out of the coloured specimens in his work.

The places marked by asterisks will supply a sufficient relative number of intermediate hues, as these evidently approach the absolute uniformity of neutral gray in the last column, whilst the first contains the graduated colours of the spectrum.

All these colours may be varied by mixing them with white or black, forming what Mr. Hay judiciously calls tints or shades of any colour.

It is sometimes convenient to have these tints and shades arranged in immediate apposition for the purpose of comparison. This may be conveniently done for the principal colours by having two diagrams. In one, the colours of the spectrum form a circular ring, the colours passing through tints into perfect white at the centre, and the shades continued in outward radiating lines till they coalesce in a perfectly black circumference. In the other, the principal intermediates between the principal colours and gray may be exhibited with like transitions. These intermediates may conveniently be denominated by the following terms, sufficiently expressive and in common use.

Russet intermediate between red and gray.
Brown ... ... orange ...
Citrine ... ... yellow ...
Drab ... ... green ...
Olive ... ... blue ...
Slate ... ... purple ...

Red.	Grayish-red.	Gray-red.	Red-gray. [Russet.]	Reddish-gray.	Gray.
Orangish-red.	*	*			
Red-orange.	*	*	*		
Reddish-orange.	*	*			
Orange.	Grayish-orange.	Gray-orange.	Orange-gray. [Brown.]	Orangish-gray.	Gray.
Yellowish-orange	*	*			
Yellow-orange.	*	*	*		
Orangish-yellow.	*	*			
Yellow.	Grayish-yellow.	Gray-yellow.	Yellow-gray. [Citrine.]	Yellowish-gray.	Gray.
Greenish-yellow.	*	*			
Yellow-green.	*	*	*		
Yellowish-green.	*	*			
Green.	Grayish-green.	Gray-green.	Green-gray. [Drab.]	Greenish-gray.	Gray.
Bluish-green.	*	*			
Blue-green.	*	*	*		
Greenish-blue.	*	*			
Blue.	Grayish-blue.	Gray-blue.	Blue-gray. [Olive.]	Bluish-gray.	Gray.
Purplish-blue.	*	*			
Blue-purple.	*	*	*		
Bluish-purple.	*	*			
Purple.	Grayish-purple.	Gray-purple.	Purple-gray. [Slate.]	Purplish-gray.	Gray.
Reddish-purple.	*	*			
Red-purple.	*	*	*		
Purplish-red.	*	*			

Mr. Hay has been kind enough to arrange for me his extensive suite of artificial colours according to these diagrams.

But it must be owned to be highly desirable to possess such a suite of colours in more perfect and durable materials than any pigment as usually applied presents. Painted porcelain and coloured enamels alone appear to possess this valuable property. The immense collection of artificial enamels employed in the Vatican fabric of mosaic pictures seems to offer an unrivalled opportunity of forming such a classification.

This gigantic establishment was founded about two centuries ago for the express purpose of adorning the interior of St. Peter's with the elaborate mosaic pictures and ceilings which astonish every visitor. The whole interior of the stupendous dome is incrusted with mosaic patterns and pictures, of coarse execution indeed, but such as suits best the vast distance from which alone they can be properly viewed; whilst the finished mosaic works which adorn the altars reproduce in unfading colours and with consummate skill in shading the *chefs-d'œuvre* of Raphael, Domenichino, and other artists preserved in the Vatican gallery. The material is a soft and fusible enamel, and the formation of 18,000 tints was effected by an ingenious artist named Matteoli, at the time I have mentioned. The rough cakes of enamel are preserved in separate cupboards or pigeon-holes, surrounding a hall of great length appropriated to this purpose by Pope Pius VI. But the main intention of the work being completed with St. Peter's, it has not been thought worth while to preserve the integrity of the collection (which, indeed would be no easy matter); and it is certain that though still reputed to contain 18,000 modified colours, the effective number is vastly smaller.

Having been fortunate enough in 1844 to make the valuable acquaintance of Monsignore de' Medici Spada, an enlightened and influential prelate residing at Rome, I entreated his influence to procure a selection of specimens of the leading colours of the Vatican mosaics. For a long time official sluggishness rendered the application fruitless; at length the importunity of my friend overcame all difficulties, but not until I had long left Rome, and was therefore quite unable to superintend the selection. My instructions were therefore general, to prefer the most varied tints which the collection presented. At last an assortment of no less than 941 pieces of mosaic, classified in separate packets, arrived. A close examination rather disappointed me. They presented a great preponderance of *indefinite* colours, and a great deficiency of many of the livelier and brighter primary and secondary colours. But particularly whole packets were composed of specimens scarcely sensibly differing from each other. This last circumstance was probably occasioned by the carelessness and indolence of the workmen who selected them. The former circumstances might naturally be expected in a collection constructed for the purpose of imitating paintings, in which, as is well-known, optically pure colours are almost never used; but their effect is invariably produced by skilful contrasts. Many of the suites of indefinite colours are exquisitely beautiful. With Mr. Hay's assistance, I selected a sufficient number of distinct hues to represent tolerably Mayer's triangle of colours; but the great mass of colours being only detached suites, it was impossible to combine them into a connected whole. As I have no doubt, however, that the collection is one which faithfully represents the colours chiefly used by artists, it may not be uninteresting to copy the catalogue forwarded to me by Monsignore Spada, with the *local* names and the principal denominations on the scale of nomenclature proposed in this paper, which they include.

		Local Names.	Technical Names.
100	specimens.	Bigi.	Tints of yellow-gray, and tints of gray.
100	...	Carnagioni.	Tints of orange-gray [brown], reddish-yellow gray, reddish-purple gray, purple-gray.
60	...	Gialli.	Tints of yellow-gray and reddish-yellow gray.
20	...	Giugiolini.	Orange passing into red and yellow.
60	...	Lacche.	Grayish-red, reddish-gray, yellowish-gray, purplish-gray, *shades* of purplish-gray.
60	...	Leonati.	Yellow-gray passing into purple-gray, *shades* of purple-gray.
60	...	Pavonazzi.	Gray-purple and *tints* of ditto.
76	...	Porporini.	Red and grayish-red.
172	...	Scorzetti.	Tints of yellow, *tints* of orange, *tints* of yellow-gray and of red-gray.
91	...	Turchini.	Tints of blue, *tints* of purplish-blue, grayish-greenish blue.
142	...	Veroli.	Tints of green-gray, *tints* of blue-green.
941	Total of specimens received.		

This number of specimens would have been sufficient to make a complete series of colours; but, as has been said, they were very deficient in the more positive hues. I have still hopes, however, of being able to obtain a series of perfect matches for the whole series of Mr. Hay's pigments, specimens of which have indeed been already sent to Rome for the purpose.

Edinburgh, January 1849.

XXII. *On the Calculation of the Distance of a Shooting Star eclipsed in the Earth's Shadow.* By ARCHIBALD SMITH, *Esq., of Lincoln's-Inn, Barrister-at-Law, late Fellow of Trinity College, Cambridge**.

1. IN a paper in the Philosophical Magazine for February 1848, Sir John Lubbock has suggested that shooting stars may be small planetary bodies which shine by reflected light, and that their sudden disappearance may be occasioned by their immersion in the earth's shadow; and he has given a formula for calculating, on this hypothesis, the distance of a shooting star, at the moment of its disappearance, from a spectator on the earth's surface. The formula, however, as it is given in the paper in question, even when simplified by supposing the earth's shadow to be cylindrical instead of conical, is not well adapted for numerical calculation, and may repel some who would be inclined to pursue the investigation if the necessary calculations were less laborious.

The data assumed by Sir J. Lubbock are the zenith distances, and the difference of the azimuths of the sun and of the star at the moment of its disappearance. In repeating his calculations, I find that by introducing the angular distance between the shooting star and the sun, or rather the point opposite the sun, the formula is very much simplified, and thus the time requisite for calculating a distance does not exceed a very few minutes. As this is a point of some importance in furnishing a test for the theory, this communication may not be considered inappropriate to the Philosophical Magazine.

2. Let the centre of the earth be the origin of co-ordinates. Let the axis of Z be directed to the zenith, of x to the north, and of y to the east.

Let x, y, z be the rectangular co-ordinates of the shooting star at the moment when it enters the earth's shadow.
- ρ its distance from the spectator.
- ζ, α its zenith distance and azimuth.
- a, b, c the co-ordinates of the vertex of earth's shadow.
- S the length of the shadow $= \sqrt{a^2+b^2+c^2}$.
- Z, A the zenith distance and azimuth of the point in the heavens which is diametrically opposite to the sun.
- ϕ the angular distance of the shooting star from the point opposite the sun.
- R semidiameter of the earth.

* Communicated by the Author.

3. The equation to the cone of the earth's shadow is

$$\{x^2+y^2+z^2-R^2\}\{s^2-R^2\}=\{ax+by+cz-R^2\}^2 \quad . \quad (1.)$$

Also, since

$$\begin{aligned} x&=\rho\sin\zeta\cos\alpha & a&=S\sin Z\cos A \\ y&=\rho\sin\zeta\sin\alpha & b&=S\sin Z\sin A \\ z&=\rho\cos\zeta+R & c&=S\cos Z, \end{aligned}$$

it follows that

$$x^2+y^2+z^2=\rho^2+2R\rho\cos\zeta+R^2 \quad . \quad . \quad . \quad (2.)$$

$$ax+by+cz=S\rho\{\sin\zeta\sin Z\cos(A-\alpha)+\cos\zeta\cos Z\}+SR\cos Z$$

$$= \text{(by spherical trigonometry) } S\rho\cos\phi+SR\cos Z. \quad . \quad (3.)$$

It is the introduction of ϕ at this part of the process which so much simplifies the result.

Substituting in equation (1.) the values of $x^2+y^2+z^2$ and of $ax+by+cz$, given by equations (2.) and (3.), and putting q for $\frac{R}{S}$, q being a small quantity, the average value of which is about ·0046, we have

$$\left.\begin{aligned}\rho^2\{\sin^2\phi-q^2\}+2R\rho\{(1-q^2)\cos\zeta-\cos\phi(\cos Z-q)\} \\ -R^2\{\cos Z-q\}^2=0\end{aligned}\right\} \quad (4.)$$

In this formula $\sin\phi$ may be considered as always greater than q, or ϕ greater than 16′; since, if smaller, it would be impossible to make any approximation to the distance of the shooting star. This equation, therefore, will have two roots; one positive, the other negative. The positive root is of course the only admissible solution. Solving the equation and making $R=1$, we obtain

$$\rho=\sqrt{\left\{\frac{(1-q^2)\cos\zeta-\cos\phi(\cos Z-q)}{\sin^2\phi-q^2}\right\}^2+\frac{(\cos Z-q)^2}{\sin^2\phi-q^2}}$$
$$-\frac{(1-q^2)\cos\zeta-\cos\phi(\cos Z-q)}{\sin^2\phi-q^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5.)$$

The calculation of ρ from this formula will be facilitated by the use of a subsidiary angle ψ, such that

$$\cot\psi=\frac{(1-q^2)\cos\zeta-\cos\phi(\cos Z-q)}{\sqrt{\sin^2\phi-q^2}(\cos Z-q)}; \quad . \quad . \quad . \quad (6.)$$

we then have

$$\rho=\frac{\cos Z-q}{\sqrt{\sin^2\phi-q^2}}\cdot\tan\frac{\psi}{2} \quad . \quad . \quad . \quad . \quad . \quad (7.)$$

4. In almost all cases a sufficiently approximate result will

be obtained by supposing the earth's shadow to be cylindrical instead of conical; the formulæ then become extremely simple. This is the same thing as supposing $q=0$; we then have

$$\rho^2 \sin^2\varphi + 2R\rho(\cos\zeta - \cos\varphi\cos Z) - R^2\cos^2 Z = 0 \quad . \quad (8.)$$

$$\rho = \sqrt{\left\{\frac{\cos\zeta - \cos\varphi\cos Z}{\sin^2\varphi}\right\}^2 + \frac{\cos^2 Z}{\sin^2\varphi}} - \frac{\cos\zeta - \cos\varphi\cos Z}{\sin^2\varphi} \quad . \quad . \quad (9.)$$

and, as before, using a subsidiary angle ψ, but which is now determined by the equation

$$\cot\psi = \cos\zeta \sec Z \operatorname{cosec}\varphi - \cot\varphi, \quad . \quad . \quad (10.)$$

we have

$$\rho = \cos Z \operatorname{cosec}\varphi \tan\frac{\psi}{2}. \quad . \quad . \quad . \quad . \quad (11.)$$

These two formulæ furnish the means of calculating ρ in a very few minutes with the aid of a table of logarithms and of natural tangents. Such tables (Hutton's, for instance,) require to be opened seven times only.

5. The formulæ may be still further simplified by introducing, as one of the data, the angle contained between the great circles which pass through the shooting star and the sun, and the star and the zenith. As this angle may be directly obtained from a celestial globe without calculation, it may be worth while to exhibit the formulæ with this substitution. I believe, however, that although the formulæ thus become more simple in appearance, the calculation will be very little, if at all, facilitated thereby.

If we call this angle B, then by a well-known formula in spherical trigonometry we have

$$\cos\zeta - \cos\varphi\cos Z = \sin\varphi\sin Z\cos B;$$

and substituting this value in equation (8.), it becomes

$$\rho^2\sin^2\varphi + 2R\rho\sin\varphi\sin Z\cos B - \cos^2 Z = 0. \quad . \quad (12.)$$

Solving this equation, we have

$$\rho\frac{\sin\varphi}{\sin Z} = \sqrt{\cos^2 B + \cot^2 Z} - \cos B; \quad . \quad (13.)$$

and using, as before, a subsidiary angle ψ, such that

$$\cot\psi = \cos B . \tan Z, \quad . \quad . \quad . \quad . \quad . \quad . \quad (14.)$$

we have

$$\rho = \cos Z \operatorname{cosec}\varphi \tan\frac{\psi}{2}. \quad . \quad . \quad . \quad . \quad . \quad (15.)$$

The calculation by means of equations (14.) and (15.) does not require a table of natural tangents. It requires the logarithmic tables to be opened six times.

6. For the use of persons who may wish to make the calculations without being able to follow the steps of this investigation, it may be desirable to give the result separately; it is this.

Let ζ be the zenith distance of the shooting star at the moment of disappearance, Z the zenith distance of the point of the heavens diametrically opposite the sun, and ϕ the distance of this point from the shooting star. Find an angle ψ such that

$$\cot \psi = \cos \zeta \sec Z \operatorname{cosec} \phi - \cot \phi.$$

Then the distance ρ of the shooting star from the spectator in terms of the earth's semidiameter as the unit is

$$\rho = \cos Z \operatorname{cosec} \phi \tan \frac{1}{2} \psi.$$

3 Stone Buildings, Lincoln's-Inn,
February 14, 1849.

XXIII. *Remarks on the Weather during the Quarter ending December* 31, 1848. *By* James Glaisher, *Esq., of the Royal Observatory, Greenwich**.

THE meteorological returns for the past quarter furnished to the Registrar-General have been received from stations spread over the country. The observations have been made, for the most part by experienced observers, upon an uniform plan. The following remarks are based upon observations which have been furnished either to myself or to the Registrar-General, and drawn up to accompany the meteorological tables published by the Registrar-General, all of which have been examined by myself, and reduced under my direction.

The weather during the period has been variable. The changes of temperature have been frequent and great, there has been an unusually large number of exhibitions of the aurora borealis, and the magnetic instruments have been greatly disturbed. The amount of electricity in the atmosphere has been small, many days together having passed without the instruments at Greenwich being affected.

From the 1st of October to the 10th the excess of temperature above the average of seven years was $6^{\circ}\cdot6$; the greatest daily excess was $12^{\circ}\cdot3$ on the 7th. Between the 11th and

* Communicated by the Author.

21st the temperature was 4°·5 below the average; on the 18th it was 10° in defect. From October 22 to October 30 it was 5°·3 in excess; the greatest was 7°·7 on the 27th. From October 31 to November 16 the temperature was mostly below the average, its mean defect was 4°·2, its greatest within the period was 10°·2 on the 4th. From November 17 to December 19 the temperature exceeded the average by 4°·8. On December 7 the excess was 12°·4; on the 8th was 15°·7; on the 9th was 14°·4; and on the 10th was 10°·1. From December 20 to December 24 the defect was 6°·2; from December 25 to December 29 the excess was 5°·8; and it was 2°·3 below the average on December 30 and 31. The following are the particulars of each subject of investigation arranged as in the preceding quarters.

The mean temperature of the air at Greenwich—

For the month of October was 51°·6, which is 2°·5, 6°·2, 3°·6, 2°·1, 1°·4, and 1°·1 *above* those of the years 1841 to 1846 respectively, and 1°·3 *below* that in the year 1847; or it is 2°·3 *above* the average of these seven years;

For the month of November was 43°·8, which is 1°·1 and 1°·0 *above* those of the years 1841 and 1842, of the same value as that of 1843, 0°·2, 2°·0, 2°·2, and 3°·1 *below* those of the years 1844 to 1847 respectively; or it is 0°·7 *below* the average of these seven years;

For the month of December was 44°·0, which is 3°·5, 0°·1, 11°·0, 2°·3, 11°·1 and 1°·2 *above* those of the years 1841, 1843, 1844, 1845, 1846, and 1847 respectively; and 1°·0 *below* that of the year 1842, or it is 4°·1 *above* the average of these seven years.

The mean value for the quarter was 46°·5; that for 1841 was 44°·0; for 1842 was 44°·4; for 1843 was 45°·2; for 1844 was 42°·2; for 1845 was 45°·9; for 1846 was 43°·1; and for 1847 was 47°·5; so that the excess of temperature this quarter *above* the corresponding quarter in the years 1841 to 1846 was 2°·5, 2°·1, 1°·3, 4°·3, 0°·6, and 3°·4 respectively; the only year between 1841 and 1847 whose mean temperature for this period was *greater* than that for the present year was 1847, and the difference is 1°·0. The average value for this quarter from the seven preceding years was 44°·6; so that the mean temperature of the air for the quarter ending December 31, 1848, was *above* that of the corresponding quarter in the preceding seven years by 1°·9.

The mean temperature of evaporation at Greenwich—

For the month of October was 49°·3, which is 1°·5 *above* that for the preceding seven years;

For the month of November was 41°·7, which is 1°·7 *below* that for the preceding seven years;

For the month of December was 42°·3, which is 3°·5 *above* that for the preceding seven years.

The mean value for the quarter was 44°·4, which is 1°·1 *above* that for the preceding seven years.

The mean temperature of the dew-point at Greenwich—

For the month of October was 47°·4, which is 2°·3, 5°·0, 2°·7, 1°·4, 0°·9, and 0°·2 *above* those of the years 1841 to 1846 respectively, and 1°·7 *below* that of the year 1847; or it is 1°·6 *above* the average of these seven years;

For the month of November was 38°·8, which is 1°·0, 1°·6, 2°·1, 3°·1, 4°·0, 4°·3, and 5°·3 *below* those of the years 1841 to 1847; or it is 3°·0 *below* the average for these seven years;

For the month of December was 40°·1, which is 4°·9, 10°·1, 2°·4, 10°·7, and 0°·3 *above* those of the years 1841, 1844, 1845, 1846, and 1847 respectively, 3°·1 and 1°·9 *below* those of the years 1842 and 1843; or it is 3°·3 *above* the average of these seven years.

The mean value for the quarter was 42°·1, which is 0°·7 *above* the average for the corresponding period of the preceding seven years.

The mean weight of water in a cubic foot of air for the quarter was 3·3 grains, which is 0·1 grain *greater* than the average of the preceding seven years.

The additional weight of water required to saturate a cubic foot of air was 0·54 grain. This value from the preceding seven years was 0·38 grain.

The mean degree of humidity of the atmosphere for October was 0·853, for November was 0·848, and for December was 0·873. The averages for the seven preceding years were 0·888, 0·909, and 0·900 respectively. The value for the quarter was 0·858, which is 0·041 *less* than the average for these years.

The mean elastic force of vapour for the quarter was 0·285 inch, which is 0·008 *less* than the average for the preceding seven years.

The mean reading of the barometer at Greenwich for October was 29·646 inches, for November was 29·785 inches, and for December was 29·807 inches; these values are 0·014 inch *below*, 0·075 inch *above*, and 0·028 inch *below* the average for the same months from the preceding seven years. The mean value for the quarter was 29·746 inches, which is 0·011 *above* the average for these years.

The average weight of a cubic foot of air under the average temperature, humidity, and pressure, was 540·3 grains; the average for the seven preceding years was 542 grains.

The rain fallen at Greenwich in October was 3·50 inches; in November was 1·20 inch; and in December was 2·55 inches. In October, in the years 1841 to 1847, were 5·95, 1·41, 4·25, 4·03, 1·38, 5·13, and 2·00 inches respectively; the mean of these values is 3·45 inches. In November, in the years 1841 to 1847, were 3·70, 4·28, 2·30, 4·32, 2·40, 1·52, and 2·00 inches respectively; the mean of these values is 2·92 inches. In December, in the years 1841 to 1847, were 2·40, 0·74, 0·40, 0·42, 2·00, 1·13, and 2·00 respectively; and the mean of these values is 1·29 inches. The depth of rain in October this year was nearly the same as the average from the seven preceding years, the fall in three instances being less, and in four exceeding that of this year. In November the fall in this year was less than that in any corresponding period since the year 1828, its amount being 1·72 inch less than the average from the seven preceding years. In December the fall exceeded that in every December since 1833, the mean excess being 1·26 inch above the average since 1841. In October rain fell on twenty-four days, on fourteen of which the amount was less than 0·1 inch; on six it was between 0·1 inch and 0·2 inch; on three it was greater than 0·2 inch and less than 0·3 inch; there was one instance exceeding 0·3 inch, one exceeding 0·4 inch, and one between 0·5 inch and 0·6 inch. In November there were only two instances of the fall in one day exceeding 0·1 inch; on one of these it amounted to 0·390 inch. In December there were three instances exceeding 0·1 inch, five exceeding 0·2 inch, and one amounting to 0·685 inch; on all other days the fall was less than 0·1 inch. The amount for the quarter is 7·25 inches, and the average from the seven preceding years is 7·66 inches.

The fall of rain during the year 1848 *at Greenwich* was 31·9 inches; in 1841 it was 33·3 inches; in 1842 it was 22·6 inches; in 1843 it was 24·5 inches; in 1844 it was 25 inches; in 1845 it was 22·3 inches; in 1846 it was 25·3 inches; and in 1847 it was 17·6 inches. The mean of their values is 24·4 inches; so that the excess of the fall of rain this year over the average from the seven preceding years is 7·5 inches. At Beckington it was 43·16 inches; in 1845 it was 24·94 inches; in 1846 it was 32·30 inches; and in 1847 it was 28·74 inches. In 1845 it fell on 134 days; in 1846 on 168 days; in 1847 on 151 days; and in 1848 on 219 days, as registered by the Rev. Charles Blathwayt.

At St. John's Wood, London, the fall exceeded the average from ten years by 5·05 inches, as observed by George Leech, Esq.

At Aylesbury it fell on 174 days, amounting to 34·68 inches, exceeding the average from the preceding six years by 9·5 inches, as observed by Thomas Dell, Esq.

At Empingham it amounted to 30·36 inches, which is the largest fall since 1830, as observed by William Fancourt, Esq.

At Derby was 40·07 inches, exceeding the average from the preceding four years by more than 10 inches, and by 12 inches the average from twenty years, as observed by John Davis, Esq.

At Leeds was 37·86 inches, it having fallen on 244 days. In the year 1846 it fell on 218 days, and in 1847 it fell on 174 days; and the amount was 28·442 inches, as observed by Charles Charnock, Esq.

At Hereford was 46·41 inches; the average fall from a long series of years is rather more than 30 inches, as observed by James Pendergrass, Esq.

The fall of rain during the year 1848 all over the country was about one-third larger than the average fall, and this excess fell during the first three quarters. The fall in the last quarter was about its average value at most places.

The temperature of the water of the Thames was 47°·5 by day, and 45°·7 by night. The water, on an average, was of the same temperature as that of the air. During the quarter the temperature of the water has changed more than usual; the decrease of temperature from November 4 was rapid.

The direction of the wind at Greenwich—

From October 1 to 11 was chiefly S.W.; between October 11 and 20 was chiefly N.; and from October 20 to 31 was mostly S.;

From November 1 to 7 was variable, but was chiefly S.W. and N.W.; from the 7th to 15th was N.; from the 16th to the 21st was S.W.; from the 21st to the 23rd was S.E.; and was chiefly S.W. to the end of the month;

From December 1 to 9 was S.W.; from the 9th to the 15th was mostly S. by E., and was then N. and N.E. to the end of the month.

The daily horizontal movement of the air—

From October 1 to 11 was about 160 miles; the greatest value during the period was 300 miles, and the least was 80 miles; from October 11 to the 20th was 130 miles; the greatest was 270 miles, and the least was 30 miles; and from October 20 to the end of the month was 150 miles; the greatest being 240 miles, and the least 40 miles. The average for the month was 150 miles daily;

From November 1 to 7 was 150 miles, the greatest and least being 245 miles and 10 miles; from November 7 to 15 was 110, the extremes being 200 miles and 80 miles; from the 16th to the 21st was 250 miles, the extremes being 495 miles and 185 miles; from the 21st to the 23rd was 190 miles; and from the 24th to the end of the month was 230 miles, the extremes being 300 miles and 70 miles; the average for the month was 165 miles;

From December 1 to 9 was 290 miles daily; from the 9th to the 15th was 170 miles; and it was 94 miles from the 15th to the end of the quarter. The extremes in December were 320 miles and 10 miles. The average for the month was 170 miles, and that for the quarter was 160 miles daily.

In October the *readings of the thermometer on grass* were at and below 32° on four nights; between 32° and 40° on fourteen and above 40° on thirteen nights. In November the lowest reading was 21°·5; the readings were below 32° on eighteen nights, and above 32° on thirteen nights. In December the lowest reading was 18°, and the readings were below 32° on twelve nights, between 32° and 40° on fifteen nights, and above 40° on four nights.

The mean amount of clouds was 7·3 in October, and 6·7 both in November and December. The averages for the seven preceding years were 6·9, 7·2, and 7·2 respectively.

There were no less than twenty-four exhibitions of the *aurora borealis* during the quarter ending December 31, 1848, which occurred on October 18, 19, 20, 22, 24, 25, 27 and 30, both in the morning and in the evening of the 30th; November 13, 14, 17, 18, 21, 23, 24, 25, 26, 30; December 13, 17, 22, 27 and 29. At all these times the magnets were more or less disturbed. In the weekly reports it was stated that from October 17 to 30 the magnetic instruments were almost always under some cause of disturbance, and particularly on the 17th, 18th, 19th, 23rd and 24th, slightly on the 21st and 22nd, and moderate on the remaining days. The finest aurora was that on the 17th of November; this was best observed by Professor Challis, and described by him in the Cambridge Chronicle. The most important part of his communication was that relative to the varying position of the corona. Professor Challis says, "I took twenty-four observations of the position of the corona, partly by reference to stars, and partly by a small altitude and azimuth instrument expressly constructed for this kind of observation, which I call a meteoroscope. A comparison of the results of the several observations seemed to show that the central point has not a fixed altitude and azimuth, but oscillates in a capricious manner about a medium position, more especially in the azimuthal direction." Observations of this kind are of the highest importance for comparison with the varying positions of the corona with the simultaneous variations of the magnetic dip and positions of the magnets.

Thunder-storms occurred at Whitehaven on October 9, 23, 28, 29, November 22, December 1; at Preston on Dec. 1; at Stonyhurst on December 9, distant thunder and lightning were noticed. Thunder was heard at Exeter on October 22 and on December 1. Lightning was seen at Truro on October 16, at Stone on October 28, at Saffron Walden on December 1 and 6, at Durham on October 18 and 28, at Whitehaven on December 2, at Greenwich on October 25, and at Stone on October 6 and November 3.

Hail fell at Truro on October 18, November 4, 7 and 8, at Greenwich on December 1, at Exeter on December 23, at Whitehaven on October 23, 28, 29, December 1 and 4.

Snow fell at Exeter, Empingham, and Saffron Walden on October 18, at Truro, Southampton, Greenwich, and Empingham on November 4, at Truro on November 7 and 8, at Hartwell on November 23 and December 2, and at Exeter on December 23.

Solar halos were seen at Maidenstone Hill, Greenwich, on October 5, 24, 29, and November 25; at Stone on Nov. 30; at Greenwich on October 24; at Highfield House on Oct. 1, 4, 18, 29, and December 2.

On November 8 a *mock sun* was seen at Highfield House.

Lunar halos were seen on October 8, December 2, 4, 10 and 12.

Large and continuous falls of rain.—On October 23, at Latimer Rectory, rain to the depth of 1·7 inch fell in twenty-four hours following 9 A.M.

At Falmouth, on December 27, there was a heavy fall of rain; in a few hours 1·5 inch fell. At Truro, on December 27, rain fell to the depth of 2·1 inches. In some parts of the county of Cornwall the fall of rain on December 27 exceeded 2 inches; at Penzance more than 2 inches fell. Great damage was done by the consequent floods.

The mean monthly values of the several subjects of research for the times of observations are appended to the report of the Registrar-General.

The monthly mean temperatures in the counties of Cornwall and Devonshire exceeded those at other places; but there seems to have been a good deal of bad weather in these counties, and more snow, hail and sleet seems to have fallen in these counties than elsewhere.

The readings of the barometer till October 4 were between 29·5 inches and 29·7 inches; after October 4 it steadily increased, and passed the point 30 before noon on the 5th, and remained above this point until the 7th; the highest reading was 30·062, and took place at 9^h A.M. on the 6th. Between the 8th and the 20th the fluctuations were very frequent, with generally larger decreasing than increasing readings. On the 25th the reading was 29·111, and was the lowest in the month. On the 26th, at 6^h P.M., it had increased to 29·749, and after this the readings were low, and with slight variation to the end of the month. The extreme difference of the readings during the month was 0·953 inch.

From November 1 to 6 the readings were between 29·6 and 29·4; it then increased from the latter reading to 30·248 on the 10th at midnight. On the 15th the reading was 30·348, which was the highest during the month. On the 18th, at midnight, the reading was 29·417; on the 19th the increase was 0·520 inch, and on the 20th the decrease was 0·454. On the 23rd, at midnight, the reading was 29·048, which was the lowest in the month. On the 25th, at noon, the reading was 29·984; after this the changes were small till the end of the month. The range during the month was 1·300 inch.

On December 1 the reading decreased 0·436, and was 29·284 at midnight; on the 2nd it increased 0·253, and on the 3rd, at 10^h A.M., it was 29·730; it then decreased rapidly, and the lowest reading during the quarter took place on the 5th at 6^h A.M.; it increased slowly till the 7th, and then quickly from the 7th to the 10th. The reading was above 30 from the 10th to the 13th; it was between 29·5 and 30 from the 13th to the 18th. On this day, at 6^h P.M., it was 29·677, and on the 22nd, at midnight, the reading was 30·266, the highest during the month. The reading was generally high till the end of the month. The range during the month was 1·432 inches.

At Stonyhurst, from Nov. 1 to 6, the readings were between 29·098 and 29·355, it then increased to 30·150 at 11 P.M.; on the 12th it remained above 29·8 till November 17, when it decreased suddenly to 29·518, and gradually to 28·923 on the 20th; it increased to 29·110 on November 21, but decreased to 28·624 by 3^h P.M. on the 22nd; it then increased steadily till November 25 at 9 A.M., when it was 29·615, and the variations afterwards were small.

On December 5, at 9^h A.M., the reading was 28·421, the wind at the time blowing strongly from the west.

Charles Charnock, Esq., of Stourton Lodge, Leeds, has kindly furnished me with the following agricultural report for the North Riding of Yorkshire.

"The continued rain from the 20th of September to November 1 prevented any large quantity of wheat being sown, even on dry lands; and that which was sown was finished in a very unsatisfactory manner. The comparative dry weather from November 1 to 12 enabled the farmers to sow a great portion of their wheat. On strong wheat soils a large breadth remains for spring sowing with wheat or oats. The seed time upon an average was nearly a month later than usual, and the seed since has vegetated very slowly, owing to the wetness and coldness of the soil.

"The continued fall of rain in September completely destroyed the crops of corn in the backward situations, and large quantities of barley, oats and beans, in the straw have been carried into the yards for the cattle and pigs, as not worth the expense of thrashing.

"The disease among potatoes has not been found so destructive as was anticipated, and will be more injurious to the grower than to the consumer. In some situations the crops were totally, and in others partially destroyed; yet from the great extra breadth planted with this vegetable last spring, there will perhaps be no great scarcity felt. I was most surprised by seeing field potatoes taken up as late as the 18th of December.

"The crops of corn now thrashing are very deficient both in quantity and quality. Turnips are an indifferent crop, and do not bear much eating; the sheep folded upon them have been prevented from doing well by the wetness of the weather. Symptoms of rot are apparent among many flocks of sheep.

"From the open weather the grass land has been full of meat, and has kept cattle out of the straw yards longer than usual. The disease on the lungs of beasts and milch-cows has been prevalent and exceedingly fatal; the mortality is calculated to have been 95 per cent. of those attacked.

"Within the last few weeks the epidemic prevalent in the years 1839 and 1840 has appeared among lean stock; its symptoms are blisters on the tongue and lameness. It is not often fatal, but reduces the cattle attacked by it very much.

"Employment for agricultural labourers is scarce, and its ill effects are much augmented by the great number of men who have been discharged from the railways, whose intemperate and vicious habits tend greatly to demoralise the agricultural districts.

"Many of the low grounds have been flooded, and farming operations prevented in consequence."

The mean of the numbers in the first column is 29·608 inches, and this value may be considered as that of the pressure of dry air for England during the quarter ending December 31, 1848. The differences between this number and the separate results contained in the first column show the probable sums of the errors of observation and reduction; the latter arising partly from erroneously assumed altitudes, and partly from the index error of the instruments not having been determined. In most cases the sums of their errors are small.

The mean of the numbers in the second column, for those places situated in Cornwall and Devonshire, is 47°·9; for those places situated south of latitude 52°, including Chichester and Hartwell, is 44°·6; for those places situated between the latitudes of 52° and 53°, including Saffron Walden and Highfield House, is 44°·2; for those places situated between the latitudes of 53° and 54°, including Liverpool and Whitehaven, is 43°·3; and for Durham and Newcastle is 43°·0. These values may be considered as those of the mean temperature of the air for their parallels of latitude during the quarter ending December 31, 1848.

The average daily range of the temperature of the air in Cornwall and Devonshire was 9°·6; at Liverpool and Whitehaven was 6°·9; south of latitude 52° was 11°·6; between the latitude of 52° and 54° was 9°·6; and at Durham and Newcastle was 8°·9.

The greatest mean daily ranges of the temperature of the air took place at Greenwich, Hartwell, Latimer Rectory, and Aylesbury respectively; and the least occurred at Whitehaven, Guernsey, Torquay, Liverpool, and Truro respectively.

The highest thermometer readings during the quarter were 76° at Hartwell and Leicester, 74° at Greenwich and Aylesbury. The lowest thermometer reading was 20°·5 at Stonyhurst, and readings about 21° occurred at several places. The extreme range of temperature of the air during the quarter was therefore about 55°.

The average quarterly range of the reading of the thermometers in air in Cornwall and Devonshire was 37°·0; at Liverpool and Whitehaven was 36°·9; and the mean of the numbers at all the remaining places is 48°·7.

The mean temperature of the dew-point in Cornwall and Devonshire was 43°·5; at all places south of 53° was 41°·6; and it was 39°·6 at places north of 53°.

The great mass of air has passed from the south-west in all places except Exeter and Stonyhurst, at both of which places it seems to have passed from the north.

From the numbers in the tenth column the distribution of clouds has been the same at all places, and such as to have covered somewhat more than three-fifths of the whole sky.

Rain has fallen on the greatest number of days during the quarter at Highfield House, Stonyhurst, Derby, Leeds, Helston and Latimer, and the average number at those places was 63. It fell on the least number of days at Aylesbury,

Meteorological Table for the Quarter ending December 31, 1848.

Names of the places.	Mean pressure of the atmosphere of dry air reduced to the level of the sea.	Mean temperature of the air.	Highest reading of the thermometer.	Lowest reading of the thermometer.	Mean daily range of temperature.	Range of the thermometer.	Mean temperature of the dew-point.	Wind. Mean estimated strength 0—6.	Wind. General direction.	Mean amount of cloud 0—10.	Rain. Number of days on which it fell.	Rain. Amount collected.	Mean weight of vapour in a cubic foot of air.	Mean additional weight required to saturate a cubic foot of air.	Mean degree of humidity.	Mean whole amount of water in a vertical column of atmosphere.	Mean weight of a cubic foot of air.	Height of cistern of the barometer above the level of the sea.
	in.	°	°	°	°	°	°					in.	grs.	gr.		in.	grs.	feet.
Guernsey	29·701	51·4	70·5	30·0	7·0	40·0	42·5	......	w. & n.	......	59	15·7	......	......	......	......	......	123
Helston	29·592	48·3	65·0	31·0	9·7	34·0	45·3	1·7	s. & s.w.	6·6	61	12·5	3·7	0·5	0·868	4·4	537	106
Falmouth	......	48·7	69·0	30·0	10·7	39·0	......	1·4	s.s.w.	7·4	58	13·1						
Truro	......	47·7	66·0	32·0	8·7	34·0	......	0·9	s.w.	7·2	53	15·0						
Torquay	......	48·4	66·0	33·0	7·1	33·0	42·3	......	......	......	53	11·4	3·3	0·8	0·792	4·0	541	120
Exeter	29·736	46·6	69·0	24·2	12·1	44·8	42·9	......	n.	5·1	50	9·4	3·4	0·6	0·857	4·0	541	140
Chichester	......	44·2	67·0	25·0	10·3	42·0	......	......	s.w.	......	......	8·9						
Southampton	29·641	46·2	70·0	27·0	10·7	43·0	43·6	0·9	Variable.	6·5	52	11·8	3·4	0·3	0·935	4·1	543	55
Beckington	29·035	43·2	72·0	21·0	11·9	51·0	41·1	1·2	s.w.	6·3	56	11·7	3·2	0·3	0·858	3·8	542	265
Royal Observatory, Greenwich	29·653	45·9	74·0	21·8	14·9	52·2	42·1	......	s.w.	6·9	50	7·3	3·3	0·6	0·881	4·0	540	159
Maidenstone Hill, Greenwich	29·669	45·5	70·0	24·7	9·4	45·3	41·6	......	s.w. & n.	6·6	41	7·0	3·3	0·5	0·900	3·9	542	107
Lewisham	......	45·4	73·0	21·0	13·0	52·0	42·9	......	s.w.	6·5	......	......	3·4	0·4	......	3·4	......	40
Latimer Rectory	29·648	43·3	72·5	22·0	13·3	50·5	42·0	1·2	s.	7·2	60	10·1	3·3	0·2	0·933	3·9	540	335
Aylesbury	29·629	43·9	74·0	22·0	13·2	52·0	39·1	1·1	s.e.	6·8	39	7·6	3·1	0·5	0·836	3·6	538	280
Stone Observatory	29·625	44·0	69·9	23·0	10·1	46·9	39·0	1·2	s.w.	6·4	51	6·3	3·2	0·4	0·881	3·6	538	300
Hartwell House	29·672	45·0	76·0	22·0	14·5	54·0	42·3	1·2	s.	7·3	50	6·4	3·4	0·6	0·910	4·0	539	300
Saffron Walden	......	44·5	......	......	......	......	......	2·8	s.	5·5	44	6·3						
Pool Cottage, Hereford	......	45·8	......	......	......	......	......	......	s.w.	......	40	12·1						
Cardington	29·531	44·3	70·0	21·5	10·2	48·5	42·3	......	s.w.	6·4	52	7·3	3·3	0·3	0·914	4·0	543	70
Norwich	29·608	44·7	71·0	26·0	8·8	45·0	41·4	......	......	......	54	9·4	3·3	0·4	0·851	3·9	543	39
Leicester	29·711	44·2	76·0	22·0	11·0	54·0	......	......	s.w.	6·3	59	8·1	......	......	......	......	......	156
Derby	29·589	43·6	68·0	23·0	11·0	45·0	42·0	......	s.w.	......	64	8·1	3·2	0·3	0·930	3·9	540	39
Highfield House, Notts	29·595	44·6	71·0	22·7	9·0	48·3	40·4	......	s.w.	6·7	66	7·8	3·1	0·5	0·867	3·7	542	103
Liverpool Observatory	29·597	45·3	67·8	29·4	7·3	38·4	39·6	0·9	Variable.	7·0	51	7·0	3·1	0·5	0·890	3·6	544	37
Wakefield	29·626	43·5	72·5	19·0	11·7	53·5	39·5	......	s.w.	......	60	7·5	3·0	0·5	0·854	3·6	543	113
Stonyhurst Observatory	29·639	43·0	67·1	20·5	11·2	46·6	38·8	1·0	n.	7·2	64	14·1	3·0	0·5	0·860	3·5	534	381
Leeds	29·602	42·5	67·5	19·0	10·6	48·5	38·5	1·4	s.w. & n.w.	8·2	63	8·8	3·0	0·5	0·829	3·5	543	148
York	......	41·7	70·0	22·0	9·9	48·0	......	......	s.	......	47	7·7	......	......	......	......	......	50
Whitehaven	......	44·6	62·5	27·0	6·6	35·5	40·6	3·0	s.w.	......	58	14·1	3·2	0·5	0·847	3·7	541	
Durham	29·634	42·5	69·6	21·4	8·8	48·2	38·2	1·7	w.	6·1	53	7·2	2·9	0·4	0·864	3·4	540	340
Newcastle	29·552	43·5	68·0	95·0	11·0	43·0	41·1	......	s.w.	......	42	10·4	3·2	0·3	0·907	3·8	542	121
Number of columns	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18

Hereford, Newcastle and Saffron Walden, and the average number at these places was 41. The places at which the largest falls have taken place were Guernsey, Truro, Wakefield, Whitehaven, Falmouth and Helston. The falls were smallest in amount at Saffron Walden, Stone, Hartwell and Liverpool. The average fall in the counties of Cornwall and Devonshire was 12°·3; and at all other places except Southampton, Beckington, Hereford, Stonyhurst and York, was 8°·5.

The numbers in column 13 to 17 show the mean values of the hygrometrical results at every station; from which we find that—

The mean weight of vapour in a cubic foot of air for all places (excepting Cornwall and Devonshire) in the quarter ending December 31, 1848, was 3·2 grains.

The mean additional weight required to saturate a cubic foot of air in the quarter ending December 31, 1848, was 0·49 grain.

The mean degree of humidity (complete saturation = 1) in the quarter ending December 31, 1848, was 0·884.

The mean amount of vapour mixed with the air would have produced water, if all had been precipitated at one time on the surface of the earth to the depth of 3·7 inches.

The mean weight of a cubic foot of air at the level of the sea, under the mean pressure, temperature and humidity, at the mean height of 160 feet, was 541 grains.

And these values for Cornwall and Devonshire were 3·5 grains; 0·6 grain; 0·839; 4·1 inches, and 540 grains respectively.

XXIV. *On the Expression for the remaining roots of a complete Cubic, when one root is found.* By J. R. Young, *Professor of Mathematics, Belfast**.

THE following expression for two roots of an incomplete cubic equation, in terms of the third root, is given at page 216 of the *Analyse Algébrique* of Garnier, namely,

$$-\frac{x_1}{2} \pm \sqrt{\left\{-3\left(\frac{x_1}{2}\right)^2 - q\right\}};$$

where q represents the coefficient of x, the term in x^2 being absent, and where x_1 is a root already found.

This formula I obtained independently, and gave it at page 322 of the second edition of my book on Equations. I

* Communicated by the Author.

here propose to deduce from it the suitable expression when the equation is complete, as this is as likely to be useful as the other.

Let the equation be

$$x^3+px^2+qx+r=0,$$

then it is plain that we may accommodate the former expression to this case, provided we increase each of the roots of this by $\frac{1}{3}p$, and employ here $q-\frac{p^2}{3}$ for q above; so that, still representing a root by x_1, the foregoing expression under the radical will now become

$$-3\left(\frac{x_1+\frac{1}{3}p}{2}\right)^2-q+\frac{p^2}{3}$$

$$=\frac{-(3x_1{}^2+2px_1)+p^2}{4}-q$$

$$=\left(\frac{x_1+p}{2}\right)^2-2x_1\left(\frac{x_1+p}{2}\right)-q,$$

and consequently the expression for the remaining roots is

$$-\frac{x_1+p}{2}\pm\sqrt{\left\{\left(\frac{x_1+p}{2}-2x_1\right)\frac{x_1+p}{2}-q\right\}},$$

which may be stated in a rule, as follows:—

Add x_1 to the coefficient of x^2, and call half the sum, with changed sign, α.

Subtract α from $2x_1$, multiply the remainder by α.

Subtract the coefficient of x from the quotient, and call the result β.

Then $\alpha\pm\sqrt{\beta}$ will be the two remaining roots sought.

Dr. Rutherford of Woolwich has recently published a neat and ingenious method of attaining the above object when the root x_1 is developed by Horner's method, without contraction of the decimals; in which case it is remarkably easy when the coefficient of x^2 does not consist of many figures. The preceding rule involves nearly the same numerical work whether the coefficients be large or small, as x_1 will usually have several decimals; and its application does not preclude a free abridgement of Horner's columns.

It is plain that, by a similar process, rules for the higher equations, when all the roots but two are found, might be contrived: but they would, in general, be complicated when the equations are complete: when the second term of the equation is absent, formulas for two roots, in terms of the others, are given in the work before referred to.

Belfast, Feb. 15, 1849.

XXV. *On Single and Double Vision produced by viewing objects with both eyes; and on an Optical Illusion with regard to the distance of objects.* By John Locke*.

I HAD commenced the investigation of this subject so early as 1816, while I was a student of medicine in Yale College, and I have occasionally turned my attention to it up to the present time.

Although I have been fairly anticipated in the publication of some of my results, perhaps most of them, by the late investigations of Prof. Wheatstone † and Sir David Brewster ‡, yet I deem it not useless to give you an account of the history of my own experiments and conclusions, especially as some of them are not, so far as I know, contained in the publications of either of the distinguished philosophers who have just written upon the subject. It is a well-known phænomenon, that with both eyes open we can see a single object either single or double, according as the axes of the eyes are made to converge and meet either at the object, or at a point nearer than that object. Having acquired the power of voluntary convergence of the optical axes to an extreme degree without the aid of viewing near objects, such as the nose, or a finger held near to the eyes, I commenced my experiments as follows:—

Experiment I.—I viewed a burning candle at the distance of about eight feet, the axes of the eyes being "crossed" or extremely converged. Two images were of course seen, the distance between which could be varied at pleasure by the amount of that convergence. The two images of the candle being thus seen, I suddenly closed one of my eyes, when the image on the *same side* of the closed eye vanished. Thus on closing the right eye, the right image disappeared; and on closing the left eye, the left image became extinct.

Inferences.—1st. As the axis of the right eye was directed to the left of the object, and the image which disappeared on closing that eye was to the right of it, that image must have been an *oblique* one, seen as we see lateral objects to which the eyes are not directed. It appears, too, that while the axes were converged upon vacancy, the oblique image in the right eye took the place of an image formed directly in the axis of the left eye, and the same relatively of the left eye; thus each eye *appeared to have* an image in its axis, which image was really in the opposite eye §.

* From Silliman's Journal for January 1849.

† I have not seen his paper. ‡ Phil. Mag., May 1847.

§ This is not always the condition of strabismus, for one eye may be so directed that the axis shall be on the object while the other is oblique.

2nd. The two oblique images on the retina must have been formed on points nearer to the nose, or nearer to the medial line of the body than the principal axis of perfect vision.

3rd. The images appeared in such position as objects should have been to produce pictures on the same parts of the retina, *the axes being at the same time parallel*, or nearly so.

4th. As with both eyes we see a lateral object ordinarily single, especially when at the same distance as the principal object viewed, it is inferred that the two pictures, one in each eye, must fall on parts of the retina not correspondent to the medial line of the body in order to produce single vision. For example, on looking at a person standing ten yards in front, the image of a person standing two yards or more to the right will appear *single* though not well-defined. The picture of this second person must in such case be formed to the left side of the retina of both eyes, *towards* the nose in the right eye, and *from* it in the left eye. In these two situations on the retina, and in no other, will the two oblique pictures present a single image to the mind.

5th. All this establishes the principle, that *certain parts of the retina of one eye* CORRESPOND *to certain specific parts of the retina of the other eye, in such a manner that when identical pictures fall on those corresponding parts, single vision is the result.* Those corresponding parts lie inward in one eye and outward in the other, viz. both to the right or both to the left. From each of those corresponding parts of the retina it is probable that the fibres of the optic nerve proceed, and severally unite at the point of anatomical communication where the optic nerves cross before entering the brain; hence the single impression or single image.

It may be added to this experiment (I.), that if the finger be pushed against the under part of the eyeball so as to roll it upward, the image in that eye will appear to descend, and double vision will thus be the result. Here the eye being rolled upward, the image falls on the upper part of the retina; hence the impression of a lower object would form a picture on that part, *were the eye not distorted**.

Experiment II.—Two candles of equal size and height were placed side by side on the table, and by converging the axes of the eyes, four images were produced. As the convergence progressed, each pair of images receded gradually from the original place of the single image until the two contiguous ones, the second and third, approached, and finally coalesced

* In all cases the mind seems to make no allowance for distortion of the eye, but refers the image to its true place were the eye in its natural position.

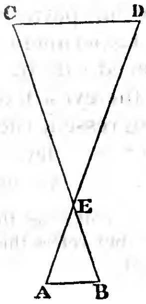
into one, when three images only were in view. The same experiment may be made with two letters, or any other figures or objects which are equal in size and form, as follows:—

1. A A Natural single vision.
2. AA AA View with axes slightly converged.
3. A A A { View with greater convergence of the optical axes and the two intermediate images coalesced into one.

On suddenly closing either eye, this middle or superimposed image did not disappear, and it was evidently made of two images from *two objects* formed on corresponding parts of the retinæ. Hence we have the converse of the case of double vision of a single object, for two objects are made to produce a single impression. Thus far I had proceeded in 1816, when I read a paper on this subject to a club of my fellow-students at Yale.

Experiment III.—In 1843 I made the experiment of converging the optical axes upon two contiguous figures on the wall-paper of my office, in the same manner as I had done with the two images of the two candles in Exp. II. When I had thus succeeded in taking up optically the two figures and superimposing them one upon the other, suddenly the whole wall appeared to leap out from a distance of ten feet to within half a yard of my eyes, where it remained in miniature beauty as palpable to vision as it had been in its original place. To this image, suspended as it were between the observer and the object, I shall in the subsequent part of my paper apply the term illusive image.

It then appeared that the right eye was directed to the left one of two contiguous figures, and the left eye to the right figure, which, being identical in form and size, gave the impression of a single object at the point of intersection of the optical axes. Here we have two triangles formed by the two optical axes intersecting each other and joined at their extremes; on one part by a line from one eye to the other, and on the other part by a line from one figure or object to the other. These last lines being parallel (see figure), where A and B represent the eyes, C and D the objects, or two figures on the wall, AD the axis of the eye A, BC the axis of the eye B, and E the point of intersection of the axes at the place of the illusive image. As these triangles are equiangular and similar, we can deduce from them all of the equations of such triangles and apply them to the optical phæ-



nomena. Thus the distance from the eye to the illusive image (AE) will be to the distance from the object to the same image (DE) as the distance between the eyes (AB) is to the distance between the objects (CD) the figures or pannels on the paper, &c.

It is not merely the two objects directly in the axes of the eyes which coincide, but every contiguous pair of objects seen obliquely will also coincide, and form the illusive picture *in extenso.* Indeed the optical operation of convergence seems like taking up a duplicate copy of the figures lying in the first place exactly over them, and slipping it gradually to the extent of one figure, until again the figures coincide in a new place.

Some of the phænomena of the Illusive Image.—It is quite perfect, and can be viewed deliberately and critically as if it were a real substance in place as it appears; the figures are smaller than the originals in proportion as they are nearer; as the outlines are a little blended by double pictures not exactly coincident, an elegant softening and a playful beauty exalts their effect above that of the original; as the head moves sideways, upward or downward, the illusive image moves, but with a diminished motion; as the head is inclined to the right or left, the superimposed pictures slide out from each other, the one ascending and the other descending to the extent of the inclination.

Optical Equivalency.—The illusive image and the erroneous distance at which it appears, show evidently that philosophically we do not *see an object,* but we contemplate an image on the retina. If this image can by any means be formed without the object, we still contemplate the substance such as would produce that image. Thus in Exp. III., and the figure illustrating that experiment, the two objects C and D produce each picture in the eyes at A and B, exactly as would be produced by a single object of smaller size at E. Thus the two objects, one at C and the other at D, "fulfill the conditions of the problem" of the images on the retina, exactly as it would be fulfilled by a single smaller object at E. In both cases identical pictures are formed on "corresponding" parts of the two retinæ. Hence the two objects produce the impression of a single image.

Directions how to make the experiment of the Illusive Image.—With two identical objects only, although it is easy to superimpose them as in Exp. II., yet the illusion of distance can scarcely be attained. But with a papered wall having a repetition of the same figure at equal distances, a person who has voluntary command of the optical axes will soon move the double images to coincide, when presently the illusion will be perfect. Persons who have not this command of their eyes may succeed in obtaining the proper convergence by looking at a finger held about fifteen inches from the face, while standing ten feet from a wall with figures twenty inches apart.

Apparent distances of objects.—It seems that we judge of moderate distances by a kind of triangulation, the distance between the eyes being a constant base-line. In order to put this to the test, I have several times made the actual measurements as in the following cases:—

Having measured the distance between my eyes, 2·6 inches, the distance between the figures on the wall 21 inches, the distance from the wall 10 feet, the distance of the illusive image was calculated to be 14·7 inches, when it had been measured as near as may be 14·5 inches. In a second experiment we endeavoured to ascertain the distance of the observer from the wall. The other data were—

Distance between the eyes . .	2·6	inches.
Distance between the figures .	21	inches.
Distance of illusive image . .	16·75	inches.
Calculated distance of the wall .	12·5	feet.
Measured distance of wall . .	13·15	feet.

When it is recollected that the observer is obliged to range lengthwise on his measure while he determines the distance of the aërial image, and that the base-line is only 2·6 inches, the above results appear quite as accurate as we ought to anticipate.

There is peculiar beauty and accuracy in some of the results of these experiments; and it had occurred both to Sir David Brewster and myself, that when a strip of wall-paper was placed at a greater or less distance from its fellow than others, the illusive image would not appear in the same place, some strips would advance a little and others would recede, so as to fulfill the conditions of the triangles above named; even the sixteenth of an inch would be appreciable.

In the history of my examination of this subject, I would observe that my friend Dr. D. S. C. H. Smith, of Sutton, was present when my paper was read at New Haven. In 1845, my assistant, Thomas K. Beecher, A.M., witnessed and repeated most of the experiments above named. Among other things we made the equations dependent upon the above triangles, and verified our calculations by actual admeasurement of the distances between the eyes, between the objects, and to the illusive image. I attempted a popular lecture on this topic, but found it difficult to interest an audience in a matter requiring so much previous optical knowledge. In the spring of 1846 I communicated the leading principles of what I thought then questionable discoveries, either to Prof. Bache or to Prof. Henry, and consulted him as to their originality. He gave his opinion that they were new. Without the least disposition to contest the point of originality, which I have failed to establish by neglecting to publish my results, I wish merely to inform my friends of what I have in fact done, and thus appear as a collateral witness to the truth and interest of Sir David Brewster's paper. He has brought forward some points which had never presented themselves to me. That figures less distant than the two eyes may be so viewed as to form an illusive image at a greater distance than the object itself, is evidently true, yet I had never made or anticipated the experiment. Two such small figures might occupy such a situation as to form the pictures on the retina due to a single larger object placed at a greater distance, and thus become an optical equivalent to that object. I am now experimenting on the subject of single vision produced by two identical figures of different colours. So far the results have not excited any very surprising interest. The illusive image, as would be anticipated, usually exhibits the effect of a commingling of the colours; but by directing the attention to one or the other eye, one or the other colour may be made to predominate. Thus a cameleon picture is formed, changing colour at the will of the inspector.

Sir David Brewster alludes in his paper to some discoveries made by Prof. Wheatstone, in reference to "binocular" vision of objects of three dimensions. I have not seen the paper on that subject, nor had I turned my attention in the least to its consideration; yet so intimately is it connected with the principles just laid down, that upon its being named certain important conclusions at once present themselves. Thus when the hand is held edgewise, within three inches of the nose, one eye will receive an image of the palm and the other of the opposite side; and the two pictures, being dissimilar, cannot fall on corresponding parts of the retina and produce a single perfect image. Let any one make the experiment, and he will perceive that Hogarth's caricature of bad perspective, in the figure of a barrel with both ends visible at the same time, was not altogether absurd; for if the barrel be shorter than the distance between the eyes, it is practicable. The same thing will occur with regard to any solid, as a cube, which has several aspects, and the imperfection will be evidently greater as the object is smaller and nearer the eye.

The experiments on this interesting subject can be extended and varied in many ways highly interesting and instructive;

and as no other apparatus is required than our eyes and the objects of our inspection, it would seem that they were easily made. But it requires rather an acquired power over the organs of vision to be readily successful. Sir Dávid Brewster applied "binocular" convergence upon two figures, drawn side by side to superimpose one upon the other, and compare their exactness in point of size and form. I have extended the same operation to figures of unequal size, though of the same form. My son had just completed a half-size copy of a drawing representing an Arab on horseback, the correctness of which had been questioned. It was evident that, being placed at distances proportionate to their size, the images of the original and copy on the retina would be equal when a consistent illusive image might be obtained by convergence. The original was hung on the wall, and the half-size copy suspended at about half the distance from the observer, at such an angle that one could be fully seen beside the other. I converged or superimposed the images, and found them so nearly to coincide, that the common outline was merely elegantly softened by the inequalities. In this experiment it appeared as if the eye, when the figures did not exactly coincide, had some power to complete the work or conceal the imperfections.

I have just succeeded in substituting a blank tablet for one of the pictures, and in tracing upon it with a pencil the illusive image converged from the other tablet. But this is not a very practicable method of copying pictures, requiring unusual command and steadiness of the optical axes for even the most moderate success in the operation.

XXVI. *Analytical Proof of the Parallelogram of Forces.* By T. H. Pratt.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

In my work on Mechanical Philosophy, I have given a proof of the Parallelogram of Forces which depends on the solution of the following functional equation,

$$\{f(\theta)\}^2+\left\{f\left(\frac{\pi}{2}-\theta\right)\right\}^2=1. \quad . \quad . \quad . \quad . \quad (1.)$$

I there solved this equation *indirectly*. The following direct solution may be acceptable to some of your readers.

Put

$$\{f(\theta)\}^2=\cos^2\theta+(\cos\theta-\sin\theta)\varphi(\theta),$$

and the equation (1.) becomes after reduction

$$\varphi(\theta)-\varphi\left(\frac{\pi}{2}-\theta\right)=0,$$

which is rendered *identical* by putting $\varphi(\theta)$ equal to any arbitrary function of $\sin 2\theta$, as $F(\sin 2\theta)$.

Hence the most general solution of equation (1.) is

$$\{f(\theta)\}^2=\cos^2\theta+(\cos\theta-\sin\theta)F(\sin 2\theta). \quad . \quad . \quad (2.)$$

In the application of this formula to the Parallelogram of Forces, we must make use of some condition that the function F may be determined; as mechanical experience shows it cannot remain arbitrary. Such a condition is the following: that if θ be increased by π, the resultant will obviously be the same as before in magnitude, but opposite in sign: this will therefore be the case with $f(\theta)$.

Put, therefore, $\pi+\theta$ for θ in (2.), and equate the two values of the square of the function (as they must be the same), and reduce, and we have

$$\cos^2\theta+(\cos\theta-\sin\theta)F(\sin 2\theta)=\cos^2\theta-(\cos\theta-\sin\theta)F(\sin 2\theta);$$

$$\therefore F(\sin 2\theta)=0$$

for all values of θ.

Hence in the case of the Parallelogram of Forces,

$$f(\theta)=\cos\theta \text{ or } -\cos\theta;$$

the latter is excluded, because when $\theta=0$, the resultant is the force itself, or $f(0)=1$.

Hence $f(\theta)=\cos\theta$ is the complete solution.

I am, Gentlemen,

Your obedient Servant,

Bombay, Dec. 14, 1848. T. H. PRATT.

XXVII. *Suggestions for rendering a Meridian mark visible at Night. By* N. S. HEINEKEN, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN, Sidmouth, Feb. 14, 1849.

SHOULD you deem the following suggestions likely to be of service, I shall be glad if you will give them a place in the Philosophical Magazine.

I am, Gentlemen,

Respectfully yours,

N. S. HEINEKEN.

It occurred to me eleven years since, that platinum wire, rendered incandescent by the galvanic battery, might be applied to the purpose of rendering a meridian mark visible at night; and that the same means might also be used for illuminating the wires of the transit instrument. I proposed that the meridian mark should consist of a hole in a plate of brass, adjustible in a vertical and horizontal direction by screws, and that behind this hole should be placed the incandescent wire in a glass tube for the purpose of illumination, the battery being of course at *any required distance.* For illuminating the wires of the telescope, I proposed that the platinum wire, protected by a glass tube, should be placed either within or at the side of the eye-piece, and thus obviate the necessity of piercing one of the transit-arms, as is usual. I was only able to try the above plans upon a small scale for want of a more powerful battery; but the experiments of Mr. Staite lead me to think that there may be cases in which his method of illumination by galvanism might be used with the greatest advantage for rendering a *very distant* meridian mark visible. I have found that even platinum wire, rendered incandescent *by alcohol,* may be distinguished by the telescope at a considerable distance; as may also the hydrogen and platinum lamp. By any of the above plans, the necessity of attention to the lamp itself by an assistant is done away.

While upon the subject of micrometer-wires, may I also be allowed to state, that so far back as 1831 I invented a substitute for them by lines drawn upon glass with a diamond, which lines were illuminated through the edge of the glass? but I was led to abandon the plan after trial, fearing to introduce the *errors of the two surfaces* of the glass, though I found that in other respects the plan fully answered. I am induced to mention this, having lately seen in the public prints that the same method has been since independently discovered, and I rejoice to find satisfactorily employed, by the Earl Rosse.

XXVIII. *On the Theory of Sound, in reply to* Professor Challis. *By* G. G. Stokes, *M.A., Fellow of Pembroke College, Cambridge*.*

As the subject of plane waves does not seem likely to be elucidated by further discussion, I pass on to spherical waves.

Professor Challis has divided his demonstration of the "contradiction" arrived at in this case into five heads. I entirely agree with the first four; the fifth I beg leave to dispute. The part to which I object is contained in the sentence,

* Communicated by the Author.

"Hence by the principle of the constancy of mass employed in investigating one of the hydrodynamical equations, those two quantities must be equal to each other." I deny that the equality follows in any manner from the principle in question. As the *onus probandi* evidently rests with Professor Challis, I might here stop; but to render everything as definite as possible, I will give a precise enunciation of the principle of the constancy of mass, at least as I myself regard it, and I have no reason to suppose that Professor Challis differs from me in this respect.

Let S be any closed surface, finite or infinitesimal, drawn in the fluid at the time t, τ any finite or infinitesimal interval of time; and at the time $t+\tau$ let the surface S′ be the locus of the particles which at the time t were situated in the surface S: then the mass contained within the surface S′ at the time $t+\tau$ is equal to the mass contained within the surface S at the time t.

The principle of the constancy of mass might have been enunciated somewhat differently, as follows; and it will be easily seen that the two enunciations come to the same thing.

Let S be any closed surface, finite or infinitesimal, drawn within the fluid and remaining fixed in space; and let M be the whole mass of fluid which flows across the surface S during the time τ, those portions being reckoned positive which flow from without to within S, and those negative which flow from within to without: then the mass contained within the surface S at the time $t+\tau$ exceeds the mass contained within the same surface at the time t by the quantity M.

I will not at present say anything about the paragraph which follows (5.); because if Professor Challis and I can agree about (5.), we shall probably have little difficulty in agreeing about the paragraph in question.

Neither will I pursue the discussion further in the present communication; because, in addition to the motives which I have already mentioned for declining to do so, it seems to me that too great discursiveness is an evil in controversy, especially in mathematics, where one false step may invalidate all that follows. I think it best to discuss only one fundamental point at a time.

Pembroke College,
Feb. 3, 1849.

XXIX. *On the Composition of the Gold from California.* *By* T. H. Henry, *Esq., F.R.S.**

Gold as found in nature is never chemically pure, being combined with variable proportions of silver and traces of iron and copper, and occasionally it occurs with palladium and also with tellurium.

The amount of silver was found by Boussingault in a series of analyses of the native gold of Columbia to vary between 2 and 35 per cent., from which he drew the conclusion that the gold and silver were combined in atomic proportions, 1 atom of silver being constantly combined with more than 1 atom of gold. The specimen containing 35 per cent. of silver he considered to be a compound of 1 atom of silver and 2 atoms of gold, $Ag\,Au^2$, and that containing 2 per cent. of silver 1 atom of silver and 12 gold, $Ag\,Au^{12}$.

This view of Boussingault was controverted by Gustav Rose on his return from his celebrated journey to the Ural Mountains theoretically, on the ground that gold and silver were isomorphous bodies, and such substances are not generally met with combined in atomic proportion. "It would be as remarkable as if antimony, arsenic and tellurium were found combined in atomic proportion," he remarks †; "but as isomorphous substances do sometimes occur combined in atomic proportion, as in bitterspar, diopside, &c., the only remarkable result of the analyses of Boussingault is, that the gold and silver should be *constantly* so combined;" and experimentally by the analyses of several specimens of native gold from the Ural Mountains, in the greater number of which no such definite composition existed.

The purest specimen analysed by Rose contained 98·96 per cent. of gold and 0·16 per cent. of silver; the others contained from 60 to 94 per cent. of gold.

The gold from California, a small quantity of which I obtained from Mr. Tennant of the Strand, was taken from a quantity of about 60 lbs. weight, and was considered a fair average sample of the whole: the greater part of it was in the form of flattened grains or spangles, varying from $\frac{1}{20}$th of a grain to 2 or 3 grains in weight; one piece however weighed upwards of 30 grains: the surface was rough and irregular, with minute portions of siliceous matter imbedded in it. The specific gravity of a number of the smaller grains taken together by the bottle was 15·96; an analysis was made of these

* Communicated by the Author.

† Poggendorff's *Annalen*, vol. xxiii. p. 164.

by treating them with aqua-regia, separating the chloride of silver, after dilution, by decantation of the solution of gold, and the chloride of silver, after having been well washed, dried and weighed, was dissolved in ammonia, leaving a white siliceous residue, but no gold. The solution of the gold was, after the destruction of the nitric acid by heat and hydrochloric acid, digested with oxalic acid until all the gold was precipitated; the acid solution from the precipitated gold was treated with sulphuretted hydrogen; the precipitate of sulphuret of copper produced was ignited strongly, the metal estimated from the oxide and a minute button of metallic copper was procured from this by the blowpipe. After the precipitation of the copper, the solution was evaporated to dryness, the oxalic acid was expelled by heat, leaving a minute quantity of chloride of iron *, which was dissolved in water acidulated by hydrochloric acid and precipitated by ammonia. The gold precipitated by the oxalic acid was entirely dissolved by aqua-regia. In this manner these grains were found to be composed of per cent.—

		Or after abstraction of siliceous matter.
Gold	88·75	90·01
Silver	8·88	9·01
Copper with trace of iron	0·85	0·86
Siliceous residue	1·40	
	99·88	99·88

The larger piece or "pépite" weighed 30·92 grains, and its specific gravity was 15·63. After being flattened out on a polished steel anvil until it appeared free from foreign matter and gently ignited, it weighed 30·24 grains, and the specific gravity was now found to be 16·48.

10·96 grains, mostly of this larger piece, were analysed in the manner described above, and were found to consist of in 100 parts,—

Gold	86·57
Silver	12·33
Copper	00·29
Iron	00·54
	99·73

0·688 grain of this larger mass, assayed by the blowpipe by the method described by Plattner †, gave 86·33 per cent. gold; and a very thin spangle, which weighed 0·483 gr., and after

* Unless the oxalic acid has been prepared by sublimation, a small quantity of carbonate of lime will be left after dissipation of the acid.

† Probirkunst mit dem Löthrohre. Leipzig, 1847.

fusion and separation of the siliceous matter ·461, gave 85·03 per cent.

I could detect no platinum, palladium, or any of the metals usually combined with them, such as osmium, iridium, &c., in this gold; but the small amount at my disposal would not allow me to employ a quantity sufficient to enable me to pronounce absolutely on the absence of any traces of them.

The remark of Dumas (*Traité de Chimie appliquée aux Arts*, tome iv. p. 434), that the proportions of gold and silver are so nearly constant in the mineral from the same locality (*gisement*) that the assayers know the composition when they have ascertained the precise locality which furnished it, is not confirmed by the above analyses, in which the gold varies from 85 to 90 per cent., nor indeed by those of G. Rose, of four specimens of gold from the same spot (Boruschka), which contained respectively 5·23, 8·35, 9·02 and 16·15 per cent. of silver.

This gold has very nearly the colour of the pure metal; after fusion however it becomes of a brass-yellow colour. This fact, together with the appearance of the grains under the microscope, would almost induce one to believe that the surface of the grains was purer or "*finer*" than the interior, and that a portion of the silver had been removed from the surface by some chemical agent in nature. Prof. G. Rose, at the end of his memoir already quoted, refers to the opinion prevalent both in the Ural and at St. Petersburg, that the gold from the washings is purer than that from the mines, and appears successfully to combat both this opinion and the speculations of Férussac, who would account for it by the action of sea-water, &c.; but I must refer the reader to his memoir for his arguments, which are of great interest.

XXX. *On the Geological Structure of the Alps, Carpathians and Apennines, more especially on the transition from Secondary to Tertiary Types and the existence of vast Eocene Deposits in Southern Europe. By Sir* RODERICK IMPEY MURCHISON, *F.R.S., V.P.G.S., &c.; Mem. Imp. Ac. Sciences of St. Petersburgh, Corresp. Member of the Academies of Paris, Berlin, Turin, &c.**

THIS memoir, chiefly the result of the author's last excursion on the Continent, consists of three parts, the first of which is an endeavour to bring up to the present standard of knowledge the work on the Eastern Alps, published long ago by Prof. Sedgwick and him-

* Abstract of a Memoir read before the Geological Society Dec. 13, 1848, and Jan. 17, 1849.

self*, and to extend the survey from that portion of the chain to Switzerland and Savoy. The second part is a brief explanation of his present views respecting the northern flank of the Carpathian mountains, and the third relates to Italy and the Apennines.

The Alps.—The central masses of the Eastern Alps, though in parts highly crystalline, contain recognizable remnants of Upper Silurian, Devonian, and carboniferous deposits, as proved by organic remains; but no traces of the Permian system† of the author, so abundant in Russia, Germany and England, have been found in them or in any part of Southern Europe. In the same regions, viz. in the South Tyrol and the Salzburg Alps, the above-mentioned palæozoic formations are succeeded by trias, with true "muschelkalk" fossils, as recently expounded by Von Buch, Emmerich, Von Hauer, and other geologists. But in following the central parts of the chain from Austria into Switzerland and Savoy, all fossil evidences of these palæozoic and triassic deposits cease; which, if ever they existed, have been obliterated by the very powerful action of metamorphism which has affected the Western Alps. The presence, however, of undoubted species of old coal plants in Savoy has led some geologists to believe that the carboniferous system had some representative there; whilst M. E. de Beaumont and M. Sismonda contend, that the association of such plants with belemnites proves that they occur in the lias of this part of the chain (Mont Blanc, Tarentaise, and Maurienne), so clearly recognized by its numerous animal organic remains. Sir R. Murchison allows, after personal inspection, that in the much-disputed locality of Petit Cœur, the coal-plants and anthracite really *appear* to lie in the same formation with the belemnites as described by M. E. de Beaumont.

After a notice of the better acquaintance of geologists at this day with the fossils of the secondary rocks of the Alps than when Prof. Sedgwick and himself described them, and after showing the great value of the Oxfordian group of Von Buch as the clear uppermost zone of the Jurassic limestones, the author goes to his chief point, and proves by a number of natural sections, that the opinion for which his colleague and himself formerly contended, and which met with so much opposition, is at length completely established;—that the flanks of the Alps exhibit a *true transition from the younger secondary into the older tertiary strata*. But whilst this principle was correct, the author allows that his friend and himself were in error in applying it to the Gosau deposits; all the lower and fossiliferous parts of which he now

* Trans. Geol. Soc. Lond. N. Ser. vol. iii. p. 301, and Phil. Mag. N. S. vol. viii. Aug. 1830.

† The term "Permian," derived from the vast region of Russia, where this uppermost Palæozoic system is more largely developed than in any part of the world hitherto examined (see Russia in Europe and the Ural Mountains), embraces in its meaning the Rothe Todt-liegende, Kupfer Schiefer and Zechstein of the German geologists, and among the latter, Professor Naumann, Dr. Geinitz and Capt. Gutbier have recently adopted the new term. In England the term includes the Lower Red sandstone, and the magnesian limestone. As far as researches have gone, it would appear that the Permian system is omitted in Southern Europe.

admits to be cretaceous. In common with all the geologists of their day, they also formed an erroneous opinion of the age of the "flysch" in viewing it as secondary greensand.

He now specially refers, as the base of all his subsequent results, to a memoir of his own, read before the Geological Society in 1829 (Annals of Phil. and Phil. Mag. June 1830), which proved that on the edge of the Venetian Alps, near Bassano and Asolo, the white and red scaglia, or chalk, is there conformably succeeded by the nummulitic and shelly deposits of the Vicentine, which are unquestionably of lower tertiary age, and graduate upwards through other shelly strata and sandstones into marls and conglomerates with sub-Apennine fossils. It has since been ascertained that deposits with the same shells, Echinidæ and nummulites of older tertiary age, enter far into the higher Alps of the South Tyrol, and are there elevated to great heights on the surface of limestones which represent the chalk. Natural sections are then described in Savoy, Switzerland, and Bavaria, which show a clear ascending order from the Neocomian limestone (a formation unknown when he formerly visited the Alps), or equivalent of the lowest greensand of England, through a zone charged with fossils characteristic of the gault and upper greensand into a limestone containing *Inocerami* and *Ananchytes ovata*, which, whether of white, gray or red colour, unquestionably stands in the exact place of the white chalk of Northern Europe. Certain conformable *transitions* from this inoceramus limestone up into shelly and nummulitic strata, like those of the Vicentine, are pointed out, particularly at Thones in Savoy, at the Hoher-Sentis in Appenzell and near Sonthofen in Bavaria, where these intermediate beds, partaking of all the mineral characters of the great supracretaceous groups, or "flysch," are still characterized by a Gryphæa, which is not to be distinguished from the *G. vesicularis* of the upper chalk. Above this zone (*i. e.* in tracts free from dislocation and inversion) no traces have been discovered of any one fossil referable to the cretaceous system; the overlying strata being unequivocally nummulitic and shelly rocks, which are linked together by position and fossils, and which on the north flank of the Alps (especially at Sonthofen and Kressenberg), as well as on the high summits of the Diableretz and Dent du Midi, represent the lower tertiary of the Vicentine. The upper portion of this group, so vastly expanded on the north flank of the Alps, is a collection of shale, impure limestone and sandstone, the "flysch" of the Swiss, to a great extent the "Wiener Sandstein," or fucoid grit of the Austrians*, and the "Macigno" of the Italians. The whole group of nummulite rocks and "flysch," much loaded with chlorite, pre-eminently a "greensand," and often assuming a very ancient lithological aspect, is not, as many geologists (including himself) sup-

* In an able map of the Northern Alps of Bavaria and Austria, M. Morlot had placed the nummulite and flysch rocks above the chalk. Now, however, great confusion prevails among the Austrian geologists respecting the position of the "Wiener Sandstein," which has recently been mapped as "Keuper."

posed, an upper member of the cretaceous rocks, but really represents the true eocene. The adoption of this view, which it is supposed all palæontologists must adhere to, seems already to be also in great part taken by M. Boué, in opposition to his former opinion. In reviewing the physical relations of the upper secondary and lower tertiary rocks of the Alps, it is made manifest, that the *independence* of any one member of this succession cannot be assumed from its *unconformability* to others in certain localities; inasmuch as such appearances are proved to be local phænomena only, by a more general survey which detects the order to be unbroken and continuous. In the Alps therefore, as in Russia, where deposits of several ages are conformable, the *limits of formations* can alone be defined by their imbedded organic remains.

The author next developed the true age of the " Molasse and Nagelflue" of the northern portion of the Alps. Citing the researches of Prof. Studer, M. Escher and others, he showed that the axis or older part of these tertiary deposits was usually removed to some distance from the higher ridges of cretaceous and eocene rocks, and consisted of freshwater strata; that the central or marine accumulations are from their fossils (as collected in the Cantons St. Gallen and Berne) of sub-Apennine or pliocene age, and that the great overlying portion of molasse and nagelflue, which frequently (owing to enormous dislocations) seems to *dip under the older rocks, out of which it has been formed*, is again, as far as can be ascertained, of terrestrial and freshwater origin. Following these deposits in ascending order to their outermost and superior zone, they are found to be surmounted by the well-known lacustrine formation of Œningen, formerly described in some detail by the author*. The remarkable feature of this deposit is, that although it has unquestionably been formed long after pliocene marine deposits (in which shells exist undistinguishable from those now living), its fauna and flora consist entirely of lost species. The examinations of its quadrupeds, chelonia and reptiles by Herman von Meyer and Owen, of its fishes by Agassiz, and of its plants by Göppert, all lead to this conclusion. Even in respect to the insects of Œningen, Prof. Heer of Zurich has recently satisfied himself that in a multitude of species which he is about to describe not one is identifiable with a living form. Hence Sir Roderick maintains, that the terms miocene and pliocene cannot be correlatively deduced from submarine and terrestrial formations; since if this be done in Switzerland, types of lost terrestrial species overlie existing marine forms.

In concluding his observations on the Alps, attention was called to the extraordinary contortions and convulsions they had undergone. By diagrams of various transverse natural sections it was shown that the Oxfordian, cretaceous, and eocene or nummulitic groups had conjointly undergone such great flexures as in many instances to produce absolute inversions, and in others great ruptures, both longitudinal and transverse. Whilst the direction of the sedimentary rocks is shown to conform to the axes of certain great ellipsoids

* See Trans. Geol. Soc. Lond. vol. iii. N. Ser. p. 277.

of crystalline rock, whether eruptive or purely metamorphic, the deviations from such conformity are very numerous, particularly where the strata wrap round the ends of each separate crystalline mass; in illustration of which a geological map of the Canton of Glarus, by M. Escher, was appealed to. Seeing that the forms of the anticlinal and synclinal folds exhibited in his sections coincided with the illustrations of the Appalachian mountains and other chains recently produced by Prof. H. Rogers, the author—without offering any opinion on the theory of that able geologist—pointed out that in the Alps, as in the United States, the long and slightly inclined slopes of each anticlinal face the great centre of disturbance, whilst the short and steep sides of the same dip away from the chain. In reference to the very frequent phænomenon of the younger strata (including the molasse) dipping under the older, particularly along the line of great longitudinal faults, Prof. Rogers presented diagrams explanatory of such overlaps in accordance with his theory.

Carpathians.—A brief sketch, the result of a survey in 1843, is then given of the northern flanks of the Carpathian mountains. Indicating the general succession northwards from the Tatra chain, the author points out how a mass of nummulitic limestone, overlying secondary rocks, dips under shale and sandstone like the flysch of the Alps, such deposits representing, as in those mountains, the eocene of geologists. An outer ridge (Zafflary and Rugosnik) of Oxfordian Jura and chiefly Lower Neocomian according to Zeuschner and Keyserling, rises abruptly through these superior deposits, and between it and Cracow are undulating hills, much broken up and dislocated, consisting of sandstones, shale, &c., in parts of which Prof. Zeuschner has discovered many secondary greensand or Neocomian fossils. These sandstones have a wide range, extending into Moravia, and doubtless constitute a large portion of what has been termed Carpathian grit. But the author observes that in tracts like this, where the cretaceous system assumes an arenaceous and earthy form, and particularly in those districts where the nummulitic limestones no longer exist, it is exceedingly difficult to draw any clearly defined line of separation between sandstones of secondary and tertiary age. He therefore believes that under the name of "Gres des Carpathes" rocks both of eocene and cretaceous age have hitherto been confounded, and that arguments concerning the age of any given portion of these sandstones in a country so constituted and so full of dislocations are valueless without the test of organic remains.

The Apennines.—A general view of the structure of Italy is then offered; and whilst on the authority of General della Marmora the existence of Silurian rocks in Sardinia is cited, it is shown that the lowest fossiliferous deposits of the Peninsula are liasso-jurassic, followed by limestones, often of red colours, of Oxfordian age (*ammonitico-rosso*). These constitute a number of parallel ridges of various altitudes, overlaid by or forming troughs with younger accumulations, and thus forming numerous backbones, of which the Apuan Alps and their crystalline marbles, the hills of La Spezia and Pisa, are the most prominent examples in the North.

Admirably exposed on the flanks of the Venetian Alps, and scarcely less so at Nice, the cretaceous system in all its members (from the Neocomian limestones of foreign geologists or equivalents of the English lowest greensand up to the white chalk inclusive) is surmounted by nummulitic eocene deposits, which near Asolo and Bassano are followed by miocene and pliocene shelly strata. After showing how they occupy a trough between such Alps and the Euganæans, the author explains how the latter hills have recently been described by M. de Zigno as composed of Oxfordian Jura and a full cretaceous system up to the white chalk inclusive, overlaid by the nummulitic group. In Liguria, Modena, Lucca and Tuscany, such clear evidences do not exist; for there the formations above the Oxfordian Jura are singularly devoid of fossils, and the series between that horizon and the deposits of miocene age, with the exception of certain flaggy limestones (*Alberese*), assumes an arenaceous type. At very rare intervals only, and chiefly to the south of Florence, are any bands of nummulites observable; but where they occur the author refers all the "macigno" sandstone which is associated with or overlies them to the eocene epoch; such rocks being perfectly undistinguishable from the "macigno Alpin," or flysch of the Alps. As, however, these rocks repose upon others, including vast thicknesses of the Alberese limestone, so largely seen in the Apennines between Bologna and Florence and in the northern part of the Tuscan Maremma, it is presumed that much of the latter *may* represent the chalk. For although these rocks contain *fucoids*, one or more of them being said to be similar in species to those which overlie the nummulite strata of the Alps, no sort of reliance can be placed on the presence of such marine vegetables, which in the Alps range from the lower chalk high into the eocene; whilst in Tuscany an ammonite and a hamite have actually been found in these infra-nummulitic masses. The inference of the author is, that Prof. Savi, though correct in viewing a portion of this series as cretaceous, has erred in including it in the nummulitic rocks.

In paying a just tribute to the talents, labours and character of the lamented Prof. Pilla, the author avows the impossibility of admitting his term of "Systema Etruriano" as an equivalent for any true geological division, as in it are comprehended strata which that writer had admitted to be cretaceous, with others which it is the chief object of this memoir to establish as lower tertiary.

In passing into the Papal States and Naples, the superposition of the nummulitic limestones with their usually associated fossils to hippuritic limestones, the equivalents of the chalk, is seen to be resumed; and thus the same general succession as in the Alps and Carpathians is maintained. Cases illustrative of this order, with much overlying macigno, are pointed out in the Sabine Hills and in the kingdom of Naples.

A transverse section of the Monferrato Hills (Superga) near Turin exposes a most instructive tertiary succession. A coralline concretionary limestone with small nummulites (Gassino), though described and mapped as cretaceous by Collegno and others, is shown to lie at the top of the eocene or bottom of the miocene, and to pass up through conglomerates, marls and sandstones replete with the well-known miocene types of the Superga into the blue marls and yellow sands of the Astesan, which are of sub-Apennine age. The great interest of this section lies in its exposure of a vast thickness of intermediate beds, in which the per-centage of recent and fossil species is of so mixed a character, that for a league across the inclined strata the able palæontologists, E. Sismonda and Bellardi, who made the section with the author, found it impossible to draw a defined line between miocene and pliocene accumulation, so completely do they inosculate.

After describing the relations of the miocene and pliocene formations near Bologna and in the Tuscan Maremma, including the great coal beds in the latter, which are believed to be of the older miocene date, the relations of all these marine tertiary deposits to younger terrestrial and freshwater travertines and limestones is traced; and reference is made to the more recent changes in the configuration of the Campagna di Roma and valley of the Tiber, with allusions to the labours of Monsignore Medici Spada and Prof. Ponzi, from whom he announced future communications; the one on the igneous rocks of Latium, the other on the sedimentary deposits of the Papal States.

After briefly recapitulating the principal phænomena in the Alps, Apennines and Carpathians, the author dwells in conclusion on the chief aim of his present communication, viz. the establishment of a true equivalent of the eocene in Southern Europe. He analyses the writings of the geologists who have recently described the nummulitic formations in the south of France, viz. Leymerie, Pratt, D'Archiac, Delbos, Raullin, Tallavignes, Rouant, &c., and indicates how their facts and his own are in harmony in showing the superposition of such deposits to the true cretaceous system, no *characteristic* fossil of which has been continued into the nummulitic group. Two or three species of Gryphææ are alone common to the upper beds of the one and the lower beds of the other. All the other fossils associated with the nummulites, whether from the Vicentine on the south or from Sonthofen and Kressenberg on the north of the Alps, are of tertiary forms, a certain number of them being absolutely identical with species of the London and Paris basins. Looking to the very great thicknesses and fine lamination of these accumulations, including the shale, sandstone and limestone above the nummulites in the Alps, it is contended that as all these surmount the white chalk, they must be an equivalent *in time* of what is legitimately eocene, and that they do not merely represent, as suggested by that eminent geologist M. E. de Beaumont, the interval which in the North of Europe has occurred between the termination of the chalk and the commencement of the plastic clay.

Extending the application of his view to still more southern and eastern regions, Sir Roderick Murchison is of opinion that the great masses of the nummulitic limestone of the Crimæa, Africa, Egypt and Hindostan are also of eocene age; or in other words, that from the Carpathians to Cutch at the mouth of the Indus, a space of not less than 25° lat. has been occupied by sea-basins in which creatures of this æra lived. In reference to Egypt, he cites copious collections of shells and nummulites, chiefly those at the Royal Museum of Turin, examined by M. Bellardi and himself; and in regard to Hindostan (after reverting to the Cutch fossils collected by Grant and described by Sowerby), he pointedly dwelt on the rich and instructive supplies of them recently sent home to him by Capt. Vicary from Scinde and Sabathoo, and examined by Mr. Morris, which not only demonstrate the existence of this same group in the Hala range, extending northwards towards Caubul, but also along the southern edge of the Himalaya mountains.

The inference then is, that it is necessary to separate the vast nummulitic formation, which the author believes to be eocene, from the cretaceous system with which it has hitherto been merged, and hence that a great change must be made in geological maps and in the classification of the rocks of this age in South Europe and other parts of the world. The union of the nummulitic and cretaceous groups in one system has been almost exclusively based upon the prevailing phænomenon of both having undergone the same movements, and having been often elevated into the same peaks and ridges. But such agreement in physical outline cannot be admitted as invalidating the clear testimony borne by organic remains, and from the study of which Brongniart, Deshayes, Agassiz, D'Orbigny and Bronn have all placed the nummulitic group as lower tertiary. Patient geological researches therefore at length prove that, when clear from obscurities and unbroken, the order of superposition is in harmony with the distribution of animal remains.

[P.S. In the course of the memoir, of which it is difficult to explain even the chief points in an abstract, the author particularly cites Professors Studer and Brunner, jun., and M. Arnold Escher von der Linth, as having rendered him very great services in his examination of the Swiss Alps. In reference to Savoy, he mentions the Canon Chamousset and M. Pillet; and respecting the Eastern Alps, he points out the assistance he received, first from the cooperation of his old associate M. de Verneuil in his re-examination of Styria, Gosau, &c., and afterwards from M. Leopold de Buch, to travel with whom through any part of that chain is to ensure good results. It was when with M. de Buch and M. de Verneuil that he explored the Triassic deposits of the South Tyrol. In attending the Venetian Meeting of the Italian "Scienziati" in the autumn of 1847, the author further necessarily acquired much additional knowledge there from intercourse with the geologists who have worked out the details of that region, including Pasini, Catullo and De Zigno, and he was then led to institute comparisons between some of the results of the Marquis Pareto in the western shores of the Southern Alps, and with those of the Austrian geologists, V. Hauer, Morlot, &c. in the East, as well as from that excellent palæontologist M. Ewald of Berlin. But as at that time Sir Roderick had not examined either the Swiss and Savoy Alps, the Monferrato, Apennines or Southern

Italy, any words he may be cited as having spoken at that Meeting are not to be taken as affecting his ultimate conclusions expressed in this memoir. Since it was read he has received a letter from M. Alcide d'Orbigny, which he willingly cites both as confirming his general conclusions and as bringing these deposits into close comparison with the lower tertiaries of Northern Europe. "For three years," M. d'Orbigny writes, "I have made the most extensive and most general researches on the strata containing nummulites; and in comparing all the stratigraphical and palæontological results, it is impossible not to recognize therein two distinct epochs superposed the one to the other, and having each its proper fauna. One of these epochs, which I have recognized in the French Alps, the Pyrenees and the Gironde, corresponds to the plastic clay of Paris and London, and which, belonging to the lower sands of Soissons, I have named 'Étage Suessonien'; the other, equally common in the Alps and the basins of the Gironde, and which includes the 'Calcaire Grossier' of Paris up to the gypsum of Montmartre and the London clay, &c., I designate 'Étage Parisien.' These divisions, based upon a considerable number of facts, are detailed in the work I am now printing, and the entire composition of their characteristic faunas is given in my 'Prodromus of Universal Palæontology.' The habit I have acquired of determining these fossils makes me regret that I cannot go to inspect your collections in London; but the portions of them I have seen in the hands of our friend M. de Verneuil has led me to recognize at once what I was already acquainted with in the Pyrenees and the French Alps. Again, the fossils I have examined in the collection of M. Tchichatcheff, confirm me in my opinion, and would lead me to extend the limits of these tertiary stages, as you have suggested, through Asia Minor and other tracts even to Hindostan."

It may be added, that in citing the able memoir of M. Coquand*, Sir Roderick has expressed his opinion, that the data, though construed differently by that author, may be so interpreted as to lead to the conclusion that the mass of the rocks containing nummulites in the Barbary states and the shores of the Mediterranean are, like those of the Alps and Apennines, supra-cretaceous; his own limited observations in the Neapolitan territories being confirmed by the local knowledge of Professor Savi. Similar conclusions are, he thinks, inevitable respecting the nummulitic rocks of Egypt, on the part of any one who has read Russegger's work on that country. At the same time, though well-assured of his own facts, he would not contend against the possible existence in certain southern regions, not examined by him, of some one species of nummulite in strata of the age of the uppermost chalk, as insisted upon for the Crimæa by M. Dubois de Montpereux, and for Cape Passaro in Sicily by M. Constant Prevost. All that he contends for is that the *great* nummulitic group, as characterized by a multitude of species of shells, Echino-

* Déscription géologique de la partie septentrionale de l'empire de Maroc, par H. Coquand.—*Bull. de la Soc. Géol. de France*, 2nd ser. vol. iv. p. 1188.

derms, nummulites, &c., is a formation superior to and distinct from the chalk; and if there be situations (which however he has never seen) in which a species of nummulite be common to the uppermost chalk and lowermost tertiary, they would only the more confirm his view of *transition from the one epoch to the other* in some regions of the surface of the globe. In the memoir about to be published, the author will give the result of the comparison of the species of the nummulites, whether collected in the Alps or Hindostan, with those of the south of France by M. le Vicomte d'Archiac, who has obligingly compared them.]

XXXI. *Proceedings of Learned Societies.*

ROYAL ASTRONOMICAL SOCIETY.

[Continued from p. 150.]

Dec. 8, 1848. EXTRACTS of a Letter from Dr. Forster, of Bruges. "I have long wished to call the attention of the Society to a very curious fact in the chronology of lunations, if I may so express myself; but I have always been deterred by an apprehension that it had so much the air of superstition about it, that it might, in many minds, rather excite ridicule than interest. Still, however, facts are not to be despised; and I have resolved to point out to you, that whenever the new moon has fallen on a Saturday, the following twenty days have been wet and windy. This must depend on some cycle of lunations whose influence on our atmosphere has hitherto escaped the notice of meteorologists. I first perceived the coincidence to which I allude in Sussex, in the years 1817–27, and at that time thought it accidental; but on accurately examining a journal of the weather kept in my family by my grandfather, my father, and myself, in succession, I find that in every twenty Saturday's new moons, nineteen have been actually stormy and the rest doubtful; and this has been the case ever since our journal began, A.D. 1767, up to the present time. I find, too, that the greatest storms of wind on record have been during the month following a *Saturday's moon*. It would be interesting to know whether this observation applies to other latitudes; and with a view of ascertaining the same, it is that I have thought it worth while to call the attention of the Society to the subject. For, during the last twenty-nine years, I have been enabled, in some measure, to predict the sort of weather that we should have for a long period, by examining the calculated times of new moon. It may here be observed, that stormy months, thus indicated, are characterised by the prevalence of S.W. and W. winds.

"*Periodical and other meteors.*—On the night of the 13th of November last, a clear interval occurring between 10^{h} and $13^{h}\ 50^{m}$, I observed the sky to be marked by numerous small meteors shooting, in general, towards some point in the heavens, as nearly as I could judge N.N.W.; but unfortunately I was not in a position to make any accurate observations. Several hundreds of meteors must have occurred during the three hours and a half to which I allude; the clouds then closing the sky, I gave up observation. The meteors were small and very white, and generally left long trains behind them: one meteor had a contrary direction, it was larger than the rest, and moved slowly across the zenith towards the S.E. I am most decidedly of opinion that this phænomenon is altogether atmospherical and connected with electrical changes; nor does their motion, in the apparent direction of the magnetic poles, at all militate against this hypothesis of their electrical origin. A few occurred last 10th of August, during a disturbed state of the atmospheric electricity; and I saw three on the 20th of December."

On the Variability of λ Tauri. By Mr. Baxendell.

"On the night of the 6th instant I observed that the star λ Tauri was decidedly less bright than usual, being barely equal to ν, a little less bright than γ, and decidedly below ο and ξ; whilst on the previous night I had noted it down as being a little brighter than ο and ξ, and decidedly above γ and ν, and in all my former observations I had invariably placed it above γ. On the following night (the 7th) it had nearly recovered its usual lustre, being decidedly brighter than ν, above γ, and equal to ο and ξ. A short time previous to the 6th instant I had remarked that my former observations of the stars ο, ξ, and λ Tauri exhibited discordances which rendered it impossible to fix with certainty the order in which these stars ought to be placed. After the observations on the nights of the 6th and 7th instant, there could be no doubt that these discordances were mainly, if not wholly, due to the variability of λ; and on carefully re-examining all my observations of this star, I was led to infer that its changes were accomplished in a period of only about four days. I therefore continued to watch it very closely, and on the night of the 10th instant had the satisfaction of again observing it reduced to an equality with ν. As, however, the presence of the moon on that night might be supposed to have interfered with the estimations, I have continued my observations regularly since; and having observed λ decidedly reduced in brightness on the nights of the 14th, 18th, and 22nd instant, I can no longer have any hesitation in concluding that this star belongs to the list of variable stars of short period, being, in fact, the next in order after β Persei, the period of which is the shortest yet known."

Dr. Gerling, of Marburg, published (*Astron. Nach.* 502) an account of a method for determining the parallax of the sun by observations on Venus and Mars when nearest the earth, and requested the co-operation of American astronomers. Lieut. Gilliss, having satisfied himself that the method was feasible, volunteered his services to the American government to carry Dr. Gerling's proposal into effect, and the expedition is now preparing.

Lieut. Gilliss is to place himself in the most suitable station he can find on the coast of Chili, where he is to make meridian and extra-meridian observations of both planets, at the proper times, in correspondence with other observers at home. He also proposes to observe an extensive catalogue of southern stars, and make various astronomical and magnetical observations. His instruments are a 3-foot meridian circle, with a telescope of 52 lines aperture, made by Pistor and Martius, of Berlin, under Professor Encke's direction; a 5-foot equatoreal, with clock-motion, by Fraunhofer; clock, chronometers, &c. Lieut. Gilliss expects to leave home in about six months, and to be absent two or three years.

At the close of the evening the chairman informed the meeting that the Astronomer Royal had presented the models of Lord Rosse's telescope and polishing machine to the Society. Thanks were returned to the Astronomer Royal for his present. They are now in the meeting-room for examination.

Feb. 9, 1849.—The Annual General Meeting of the Society, Sir John Frederick William Herschel, Bart., President, in the Chair.

Before commencing the usual business of the meeting, the President rose and said:—

Gentlemen,—Before the proper and formal business of this meeting begins, I must call your attention to the bust which you have seen in our entrance-hall;—it is that of our late beloved and respected president, Francis Baily, a name which will never be mentioned in this Society without calling up a lively recollection of all that is excellent in public, and amiable in private, character. When you trace, as you cannot but do, in that marble the faithful and charming reproduction of features we have so often seen in the place I now occupy, animated with the pure love of science, and with deep interest in the welfare of this Society, you will be surprised to learn that it is the production of an artist by whom these features had never been seen but in the faint reflection of an engraving from his portrait, and in that painful memento which preserves the impress of a form from which the spirit has departed. When I name the eminent artist, however, who has wrought this triumph over time and oblivion (our celebrated sculptor, Edward Hodges Baily), your surprise will cease. It is an achievement familiar to his chisel.

The bust is presented to this Society by Miss Baily, the surviving sister of our late president. She has judged rightly in supposing we shall value it. No possession we have will be more precious in our eyes. Nowhere could a memorial of the kind be more appropriately placed than in the meeting-place of a public body with which his name and his fame are so largely identified, and of which he was so distinguished an ornament. We have now his picture and his bust—both excellent. What art can do to keep his memory fresh is done. It remains for us to show that his spirit is not extinct among us.

I am sure you will enable me to respond as I ought to do to this touching and munificent gift of Miss Baily, who has requested me to be her spokesman on this occasion; and as there can be but one feeling on the subject, I shall call on the Astronomer Royal to embody that feeling in a motion of thanks.

Proposed by G. B. Airy, Esq., seconded by A. De Morgan, Esq.: —That the cordial thanks of the Society be given to Miss Baily for this valuable present.

Address delivered by the President (Sir J. F. W. Herschel, Bart.) on presenting the Honorary Medal of the Society to William Lassell, Esq., of Liverpool.

Gentlemen,—The Report of the Council having been read, in which the astronomical discoveries of the year, and especially that of the planet Metis, have been clearly and eloquently commemorated, it is now my pleasing duty to state to you the grounds on which it has been agreed by us to award the gold medal of the Society for this year to Mr. Lassell. And this duty, pleasing in itself, I execute with the greater satisfaction, because I have a sort of hereditary fellow-feeling with Mr. Lassell, seeing that he belongs to that class of observers who have created their own instrumental means—who have felt their own wants, and supplied them in their own way. I believe that this greatly enhances the pleasure of observing, especially when accompanied by discovery, and gives a double interest in the observer's eyes, and perhaps, too, in some degree, an increased one in those of the public, to every accession to the stock of our knowledge which his instruments have been the means of revealing: upon the same principle that the fruit which a man grows in his own garden, cultivated with his own hands, is enjoyed with a far higher zest than what he purchases in the market. Nor is this feeling by any means a selfish one. It arises from the natural and healthy excitement of successful exertion, and is part of that happy system of compensation by which Providence sweetens effort, and honours well-directed labour. If this be true of the labour of a man's hands in the mere production of material and perishable objects, it is so in a far superior sense, when the faculties of the intellect are called into exercise, and works elaborated with rare skill, and wrought to an extraordinary pitch of perfection, have yet a higher, ulterior, intellectual object, to which their existence is subordinate, as means to an end.

Mr. Lassell has long been advantageously known to us as an ardent lover of astronomy, and as a diligent and exact observer, in which capacity he has appeared before us, as a reference to our Memoirs and Notices will testify, on numerous other occasions besides those to which I shall more particularly call your attention presently. In the year 1840 he erected an observatory at his residence near Liverpool, bearing the appropriate name of Starfield, which has ever since been the scene of his astronomical labours. Even at its first erection this observatory presented features of novelty and interest. In addition to a good transit, it was furnished, instead of a meridian instrument or an ordinary equatorial achromatic, with a Newtonian reflecting telescope of nine inches aperture, and rather more than nine feet in focal length, equatorially mounted, the specula of which were of his own construction, and the mode of mounting devised by himself. This was already a considerable step, and forms an epoch in the history of the astronomical use of the reflecting telescope. Those only who have had experience of the annoyance of having to keep an object long in view, especially under high magnifying powers, and in micrometrical measurements, with a reflector mounted in the usual manner, having merely an altitude and azimuth motion, can duly feel and appreciate the advantage thus gained. But the difficulties to be surmounted in the execution of such a mode of mounting were very considerable—much more so than in the case of an achromatic,—owing partly to the non-coincidence of the centre of gravity of the telescope and mirror with the middle of the length of the tube, and partly to the necessity of supporting the mirror itself within the tube in a uniform bearing free from lateral constraint, and guaranteed against flexure and disturbance of its adjustment by alteration of its bearings. These difficulties, however, Mr. Lassell overcame: the latter, which is the most formidable, by an ingenious adaptation of the balancing principle first devised, if I am not mistaken, by Fraunhofer and Reichenbach for the prevention of flexure in the tubes of telescopes —a principle which has not received half the applications of which it is susceptible, and which, by throwing the whole strain of the weight of instruments on axes which may be made of unlimited strength, may be employed to destroy the distorting force of gravity on every other part*.

The success of this experiment was such, and the instrument was found to work so well, that Mr. Lassell conceived the bold idea of constructing a reflector of two feet in aperture and twenty feet in focal length, and mounting it upon the same principle. The circumstances of his local situation, in the centre of manufacturing industry and mechanical construction, were eminently favourable to the success of this undertaking; and in Mr. Nasmyth he was fortunate enough to find a mechanist capable of executing in the highest perfection all his conceptions, and prepared, by his own love of astronomy and practical acquaintance with astronomical observation and with the construction of specula, to give them their full effect. It was of course, however, the construction and polishing of the large reflector which constituted the chief difficulty of this enterprise. To ensure success, Mr. Lassell spared neither pains nor cost. As a preliminary step, he informs us that he visited the Earl of Rosse, at Birr Castle, and besides being favoured with more than one opportunity of satisfying himself of the excellent performance of that nobleman's three-foot telescope, enjoyed the high privilege of examining the whole machinery for grinding and polishing the large speculum, and returned so well satisfied as to resolve on the immediate execution of his own ideas.

* As, for example, the divided limbs of circles, and the spokes connecting them with their centres; an easy and simple mechanism, which, devised some time ago, and approved by the late M. Bessel, I may, perhaps, take some future opportunity to submit to the Society.—(*Note added in the Printing.*)

The mode of casting and grinding the mirror, differing in some of the details, though proceeding generally on the same principle as Lord Rosse's (*i. e.* by a chilled casting), has been described in a communication read to this Society on the 8th of December last. The polishing was performed on a machine almost precisely similar to that of his lordship. But finding after many months' trial that he could not succeed in obtaining a satisfactory figure, he was led to contrive a machine for imitating as closely as possible those evolutions of the hand by which he had been accustomed to produce perfect surfaces on smaller specula. This machine has been described (and a model of it, as well as Mr. Nasmyth's finished working drawings of it, exhibited) in a paper of great interest read at the last meeting of this Society, of which also an abstract has been printed in our Notices, and must by this time be in the hands of every fellow here present, so that it cannot be necessary for me to recapitulate its contents. Suffice it to say, that I have carefully examined both the drawings and the model, and having myself had some experience in the working and polishing of reflecting specula, approaching (though inferior) in magnitude to Mr. Lassell's, I am enabled to say that it seems to unite every requisite for obtaining a perfect command over the figure; and when executed with that finish which belongs to every work of Mr. Nasmyth, from the steam-hammer down to the most delicate product of engineering and mechanical skill, cannot fail to secure, by the oily smoothness and equability of its movements, the ultimate perfection of polish, and the most complete absence of local irregularities of surface. The only part which I do not quite like about it, or perhaps I should rather say which seems open to an *à priori* objection, refutable, and, in point of fact, refuted by the practical results of its operation, is the wooden polisher, owing to the possibility of warping should moisture penetrate the coating of pitch with which it is (I presume) enveloped on every side. Some unhygrometric, non-metallic substance, such as for instance earthenware, porcelain biscuit, or slate, would be free from this objection, though possibly open to others of more importance.

Both Mr. Lassell and Lord Rosse appear to be fully aware of the vital importance of supporting the metal, not only while in use, but also while in process of polishing, in a perfectly free and equable manner; but the former has adopted a mode of securing a free bearing on the supports, by suspending the mirror, which is a great and manifest improvement on the old practice of allowing it to rest on its lower edge, by which not only is the figure necessarily injured by direct pressure, but the metal is prevented from playing freely to and fro, and taking a fair bearing on its bed. As I have, however, on another occasion enlarged on the necessity of making provision against these evils, by a mechanism almost identical in principle, I need not dwell upon this point further than to recommend it to the particular attention of all who may engage in similar undertakings.

It is right that I should now say something of the performance of the nine-inch and two-feet reflectors. And first, as regards the success of the system of mounting adopted in securing the peculiar advantages of the equatorial movement. This appears to have been very complete. The measurements, both differential and micrometrical, made with them, and recorded in our Notices, show that in this respect they may be considered on a par with refractors, and in facility of setting and handling they appear nowise inferior. Of the optical power of the former, two facts will enable the meeting to form a sufficient judgement. With this instrument Mr. Lassell, independently and without previous knowledge of its existence, detected the sixth star of the trapezium of θ Orionis. And with this, under a magnifying power of 450, and in very unfavourable circumstances of altitude, both himself and Mr. Dawes became satisfied of the division of the exterior ring of Saturn into two distinct annuli, a perfectly clear and satisfactory view of the division being obtained.

The feats performed by the larger instrument have been much more remarkable and important. It has established the existence of at least one of the four satellites of Uranus, which since their announcement by Sir W. Herschel had been seen by no other observer, viz. the innermost of all the series, and afforded strong presumptive evidence of the reality of another, intermediate between the most conspicuous ones. The observations of M. Otto Struve, if they really refer to the same satellite, are of nearly a month later date.

To Mr. Lassell's observations with this telescope we also owe the discovery of a satellite of Neptune. The first occasion on which this body was seen was on the 10th of October, 1846, but owing to the then rapid approach of the planet to the end of its visibility for the season, it could not be satisfactorily followed until the next year, when, on the 8th and 9th of July, observations decisive as to its reality as a satellite were made, and in August and September full confirmation was obtained. This important discovery has since been verified both in Russia and in America. I call it so, because, in fact, the mass of Neptune is a point of such moment, that it is difficult to overrate the value of any means of definitively settling it. Unfortunately, the exact measurement of the satellite's distance from the planet is of such extreme difficulty, that up to the present time astronomers are still considerably at issue as to the result.

I come now to the most remarkable of Mr. Lassell's discoveries, one of the most remarkable, indeed, as an insulated fact, which has occurred in modern astronomy: though, indeed, it can hardly be regarded as an insulated fact, when considered in all its relations. I need hardly say that I allude to the discovery of an eighth satellite of Saturn, a discovery the history of which is in the highest degree creditable, not only to the increased power of the instruments with which observatories are furnished in these latter days of astronomy, but also to the vigilance of observers. If I am right in the principle that discovery consists in the certain knowledge of a new fact or a new truth, a knowledge grounded on positive and tangible evidence, as distinct from bare *suspicion* or *surmise* that such a fact exists, or that such a proposition is true—if I am right in assigning as the moment of discovery, that moment when the discoverer is first enabled to say to himself, or to a bystander, "I *am sure* that such is the fact,—and I am sure of it, *for such and such reasons,*" reasons subsequently acquiesced in as valid ones when the discovery comes to be known and acknowledged—if, I say, I am right in this principle (and I really can find no better), then I think the discovery of this satellite must be considered to date from the 19th of September last, and to have been made simultaneously, putting difference of longitude out of the question, on both sides of the Atlantic. In speaking thus, I desire, of course, to be understood as expressing only my own private opinion, and in no way as backing that opinion by the authority of the Society whose chair I for the moment occupy. The Astronomical Society receives with equal joy the intelligence of advances made in that science from whatever quarter emanating, and accords the meed of its approbation to diligence, devotion, and talent, with equal readiness wherever it finds them—but declines entering into *nice* questions of personal or national priority, and would, I am sure, emphatically disavow the assumption of any title to lay down authoritative rules for the guidance of men's judgements in such matters. The medal of this day is awarded to Mr. Lassell, not on account of this discovery alone, and as such, but as taken in conjunction with the many other striking proofs he has afforded of successful devotion to our science—both in the improvement and in the use of instruments. And among the motives which have induced your Council to place Professor Bond on the list of our Associates (I trust not long to be the only one of his countrymen by whom that honour is enjoyed), though this discovery has had its due and just weight, we have not been unheedful of his general merits, both as an observer and as a theoretical astronomer—merits of which the Memoirs which have recently reached us convey the most abundant evidence in both departments.

I have observed that, when taken in all its relations, the discovery of an eighth satellite of Saturn cannot be regarded as quite an insulated fact. Between Iapetus and Titan there existed a great gap unfilled, in which (as formerly between Mars and Jupiter) it was not in itself unlikely that some additional member of the Saturnian system might exist. The extreme minuteness of Hyperion forcibly recals the analogous features of the asteroids, and it would be very far from surprising if a further application of the same instrumental powers should carry out this analogy in a plurality of such minute attendants.

Mr. Lassell, as you are all well aware, is bound to astronomy by no other tie than the enjoyment he receives in its pursuit. But in *our* estimation of his position as an amateur astronomer it must not be left out of consideration, that his worldly avocations are such as most men consider of an engrossing nature, and which entitle them in their moments of relaxation, as they conceive, to enjoyments of a very different kind from those which call into fresh and energetic exertion all their faculties, intellectual and corporeal. It is no slight and desultory exercise of those faculties which will enable any man to carry into effect so much thoughtful combination, and to avail himself with so much consecutiveness of their results when produced. And however we may and must acknowledge that such a course of action is really calculated to confer a very high degree of enjoyment and happiness, we ought not to feel the less gratefully towards those who, by their personal example, press forward the advent of that higher phase of civilization which some fancy they see not indistinctly dawning around them; a civilization founded on the general and practical recognition of the superiority of the pleasures of mind over those of sense; a civilization which may dispense with luxury and splendour, but not with the continual and rapid progress of knowledge in science and excellence in art.

I think I should hardly be doing full justice to my subject or to the grounds taken by the Council in the award, if I were to conclude what I have to say otherwise than in the pointed and emphatic words of a report officially embodying the prominent features of the case. "The simple facts," says that document, "are, that Mr. Lassell cast his own mirror, polished it by machinery of his own contrivance, mounted it equatorially in his own fashion, and placed it in an observatory of his own engineering: that with this instrument he discovered the satellite of Neptune, the eighth satellite of Saturn, and re-observed the satellites of Uranus. A private man, of no large means, in a bad climate" (nothing, I understand, can be much worse), "and with little leisure, he has anticipated, or rivalled, by the work of his own hands, the contrivance of his own brain and the outlay of his own pocket, the magnificent refractors with which the Emperor of Russia and the citizens of Boston have endowed the observatories of Pulkowa and the Western Cambridge."

The President then, delivering the medal to Mr. Lassell, addressed him in the following terms:—

And now, Mr. Lassell, all that remains for me is to place the medal in your hands, and to congratulate you on your success and on the noble prospect of future discovery which lies before you, now that, free from the preliminary labour of construction, your whole attention can be devoted to using the powerful means you have created. In the examination of the nebulæ, in the measurement of the closest double stars, and the discovery of others which have hitherto defied separation—in the physical examination of the planets and comets of our own system, there is a wide field open and the sure promise of an ample harvest; and I can only add that we all heartily wish you health and long life to reap it.

The Meeting then proceeded to the election of the Council for the ensuing year, when the following Fellows were elected, viz.—

President.—G. B. Airy, Esq., M.A., F.R.S., Ast. Roy.

Vice-Presidents.—J. C. Adams, Esq., M.A.; Edward Riddle, Esq.; Rev. Richard Sheepshanks, M.A., F.R.S.; Lieut. William Stratford, R.N., F.R.S.

Treasurer.—George Bishop, Esq.

Secretaries.—Augustus De Morgan, Esq.; Captain R. H. Manners, R.N.

Foreign Secretary.—John Russell Hind, Esq.

Council.—George Dollond, Esq., F.R.S.; Rev. George Fisher, M.A., F.R.S.; Sir John F. W. Herschel, Bart., K.H., M.A., F.R.S.; John Lee, Esq., LL.D., F.R.S.; Rev. Robert Main, M.A.; Charles May, Esq.; Lieut. Henry Raper, R.N.; William Rutherford, Esq., LL.D.; Captain W. H. Smyth, R.N., K.S.F., D.C.L., F.R.S.; J. W. Woollgar, Esq.

CAMBRIDGE PHILOSOPHICAL SOCIETY.

[Continued from p. 138.]

May 17, 1847.—A Theory of the Transmission of Light through Transparent Media, and of Double Refraction, on the Hypothesis of Undulations. By Professor Challis.

The object of the author in this, as in two preceding communications on *Luminous Rays* and on the *Polarization of Light* *, is, to establish the undulatory theory of light on hydrodynamical principles, by means of a system of ray-vibrations, the motions in which are mathematically deduced from hydrodynamical equations. In applying these views to the transmission of light through transparent media, it is assumed that the æther is of the same uniform density and elasticity within any transparent medium as without; and that the diminished rate of propagation in the medium is owing to the obstacle which its atoms oppose to the free motion of the æthereal particles. Considering the proximity of the atoms to each other, and that the retarding effect of each atom at a given instant extends through many multiples of its linear dimensions, it is presumed that the mean retardation, though resulting from the presence of discrete atoms, may be regarded as continuous. It is also supposed that the mean effect of the presence of the atoms is to produce an *apparent* diminution of the elasticity of the æther, the motion in all other respects being the same as in free space. By the application of these principles, it is first shown that the *surface of elasticity*, that is, the surface whose radius vector drawn in any given direction represents the elasticity in that direction, is in general an *ellipsoid*. This being ascertained, the velocity of a *ray* in any given direction is investigated; and the result is, that the surface whose radius vectors drawn in any given direction represent the velocities of propagation of two oppositely polarized rays in that direction, is precisely the wave-surface in Fresnel's theory of double refraction.

March 6, 1848.—A Mathematical Theory of Luminous Vibrations. By Professor Challis.

This paper is intended to be supplementary to three former communications in which the undulatory theory is treated on hydrodynamical principles, and to elucidate or confirm results previously arrived at. In particular the author enters more at length into the mathematical theory of ray-vibrations, which, according to his views,

* Phil. Mag. vol. xxx. p. 365.

correspond to rays of light. The principal theoretical deductions are,—(1.) that the longitudinal vibrations of a ray are defined by a function of the form $\sin \frac{2\pi}{\lambda}\left(z - a\,t\sqrt{1+\frac{e\,\lambda^2}{\pi^2}}\right)$, λ being the breadth of the undulation, and a, e certain constants; (2.) that light from any source is in general *composed* of rays for which a and $\frac{e\lambda^2}{\pi^2}$ are the same and λ different; (3.) that light coming immediately from its origin is *common* light, whatever be the nature of the cause producing it, and that to become *polarized* light, it must be acted upon by reflexion, refraction, &c.; (4.) that light coming immediately from its origin is seen in all directions.

Nov. 27, 1848.—Observations of the Aurora Borealis of Nov. 17, 1848, made at the Cambridge Observatory. By Professor Challis.

These observations relate principally to the corona, or point of apparent convergence of the streamers, the remarkable display of Nov. 17 being peculiarly favourable for noting the position of this critical point. They were taken partly by estimation of distances from stars, and partly by a small altitude and azimuth instrument (called by the author a *meteoroscope*), which is furnished with a bar, eighteen inches long, carrying at one end a rectangular piece whose edges are horizontal and vertical, by looking at which through an eyelet-hole, about the size of the pupil of the eye, at the other end, the collimation is performed. Each observed position is compared with the point of the heavens to which the south end of the dipping-needle was directed at the time of observation. This point was ascertained by means of observations of declination, horizontal force, and vertical force, taken at the Greenwich Observatory during the prevalence of the aurora by Mr. Brooke's photographic process, the results of which were communicated to the author by the Astronomer Royal. It is assumed that the magnetic declination and dip at Cambridge differ from those at Greenwich at any given time by certain constant quantities, whether the magnet be disturbed or not. These constant differences were derived from the following formulæ:—

$$V - V_0 = 0{\cdot}142518\lambda + 0{\cdot}159548l$$
$$D - D_0 = 0{\cdot}027713\lambda + 0{\cdot}513523l,$$

in which V and D are the declination and dip at a place not very distant from Greenwich, V_0 and D_0 the contemporaneous declination and dip at Greenwich, λ the longitude of the place *west*, in *seconds of time*, and l the *excess* of its latitude in *minutes* above that of Greenwich. These are merely formulæ of interpolation by simple differences derived from the following data:—

	Lat.	Long. West.	Declination in 1843.	Dip in 1843.
	° ′	m. s.	° ′	° ′
Greenwich	51 28·6	0 0·0	23 17·59	69 1·9
Makerstoun....	55 34·7	10 3·5	25 22·85	71 25·0
Dublin........	53 21·0	25 4·0	27 9·87	70 41·3

The above are very accurate contemporaneous values of the declination and dip at the three places, and the formulæ derived from them will probably apply with considerable accuracy to any place in the United Kingdom at any date not very remote from 1843. For the Cambridge observatory $V-V_0 = +3'\cdot7$ and $D-D_0 = +22'\cdot0$.

The mean result from 24 observations of the position of the corona is, that it was situated 5′ further from the astronomical zenith, and 1° 14′ nearer to the meridian than the point of the heavens to which the south end of the dipping-needle was directed.

The places of the corona given by the different observations exhibit considerable discrepancies, which are accounted for by saying, that as the formation of the corona is merely an effect of *perspective*, its position varies, since the streamers are not exactly parallel, with the locality from which they rise; also with any variation of their direction at a given locality; and, supposing the course of the streamers to be somewhat curved in their ascent, it will vary with the height to which they rise. Accordingly, as appeared to be the fact, the corona would be continually shifting its position within certain limits.

Prof. Challis has made a similar comparison with observations of the position of the corona of the same aurora made at Haverhill, at Darlington, and at Bath; also with observations at Whitehaven of the aurora of Oct. 18, 1848, and of that of Oct. 24, 1847, at Cambridge. From a consideration of all the results derived from the discussion of observations made on different occasions and at different places, the following conclusions seem to be established:—

First, that the corona of an aurora borealis is formed near the point of the heavens to which the south end of the dipping-needle at the place of observation is directed.

Secondly, that the observations, while they indicate no decided difference of altitude between the two points, show with great probability that the corona is the more *westerly* by about $1\frac{1}{4}°$ measured on an arc perpendicular to the meridian.

The paper concludes with a particular description of the aurora borealis of Nov. 17 as observed at the Cambridge Observatory, and with three tables of the observations of declination, horizontal force, and vertical force, made at Greenwich, and used in the calculations. These observations present so striking an instance of great magnetic disturbances occurring simultaneously with an extraordinary display of the aurora borealis, that the connexion in some way of the two kinds of phænomena must be regarded as a physical fact.

XXXII. *Intelligence and Miscellaneous Articles.*

ON THE RATIONALE OF THE EXPLOSION CAUSING THE GREAT FIRE OF 1845 AT NEW YORK. BY DR. HARE*.

DR. HARE communicated to the meeting some inferences and facts, tending to explain the contradictory impressions which

* Communicated by the Author.

have existed respecting the competency of fused nitre to explode with water, or with aqueous, hydrogenous, and carbonaceous combustibles. This subject was treated of in reference to a series of detonations terminating in an explosion of tremendous force, by which, in July 1845, the intensely ignited contents of a store in Broad Street, New York, were thrown over an extensive district, involving the destruction of about 200 houses and property estimated at two millions of dollars. As far as the oaths of highly competent witnesses could avail, no gunpowder was present, so that the result could only be attributed to the reaction between an enormous quantity of nitre and combustible merchandize with which the store was promiscuously occupied. In all there were 300,000 lbs. of nitre in parcels of 180 lbs. (each secured by two bags, an additional bag having been put over that originally employed). About 30,000 lbs. was situated upon the first floor, 180,000 on the second floor, and 80,000 on the third floor.

Of the merchandize, the aggregate was more than double the weight of the nitre.

It was however the general opinion of those best acquainted with the subject, that when ignited with combustibles, nitre produces only that species of combustion which is called deflagration by chemists, without being capable of the more violent and instantaneous reaction designated by the word explosion. This impression was strengthened by the failure of every effort (made by several eminent chemists employed by the Corporation of New York) to explode nitre by ignition with combustibles.

Nevertheless, agreeably to Hays, of Massachusetts, an explosion was effected in his laboratory, by bringing water into contact with about 100 lbs. of incandescent nitre; also the accidental falling of a jet of melted nitre on some water in the laboratory of the University of Pennsylvania had been productive of a similar result.

The explosion of a vessel laden with nitre, which, while lying in Boston harbour, was burnt to the water's edge, and of others similarly laden and burnt, could only be explained by supposing that nitre, when sufficiently heated, will explode with water on due contact. Consistently, it might be inferred that this salt (well-known to be a compound of nitric acid and oxide of potassium or potash) would explode with any substance capable of yielding either or both of the elements of water or hydrogen. The presence of the latter would be equivalent to water, since it would, with the oxygen of the acid, form water.

In a letter addressed to the distinguished chemist above-mentioned, in July 1845, Dr. Hare had adverted to the explosion which succeeds the combustion of potassium upon water, as arising from the combination of one portion of the water with the resulting incandescent globule of oxide, while the heat of this globule uniting with another portion of the liquid, converts it into high steam. Moreover, it was suggested that in this instance chemical affinity between the water and the oxide, in causing the water and heated globule to coalesce, is equivalent in efficacy to the momentum of the hammer when a bar of iron, at a welding heat, is forced into contact with some moisture situated upon an anvil.

Dr. Hare presumes that no explosion can take place unless the reagents for producing it are held or brought together at the moment of reaction, by a certain force, either chemical or mechanical.

Some chemical compounds, such as are formed with fulminic acid or with ammonia, by metallic oxides; also the chloride of nitrogen and perchloric æther, explode violently without confinement, so as to fracture a plate or saucer, upon which a small quantity may be detonated; but pulverulent mixtures, such as gunpowder, however powerfully explosive when employed in gunnery or rock-blasting, in open vessels flash without fracturing them, or producing any report. In an exhausted receiver gunpowder is far less explosive than when subjected to atmospheric pressure in an open vessel. Nevertheless, when gunpowder is restrained until the temperature requisite for the appropriate reaction of its ingredients is attained, it exerts a force far exceeding that which the chamber confining it is capable of resisting. In this respect it differs from steam, of which, when the temperature of the fire applied is sufficiently high, the explosive force is directly as the pressure immediately before bursting, and this of course is commensurate with the strength of the confining boiler.

The ingredients of gunpowder, sulphur, charcoal and nitre, to produce the greatest effect, require extreme comminution and intimate intermixture by trituration, and to be so granulated that the flame of the portion first ignited may convey inflammation to the rest through the interstices between the grains. Its superiority over any other mixture of nitre with combustible matter destitute of sulphur, is conceived to be due not only to the pre-eminent susceptibility of this substance, of vaporization and inflammation, but likewise to its well-known ability to decompose metallic oxides by attracting both the metal and oxygen. Since an opinion was expressed in 1845, in the letter above-mentioned to Hays, that the formation of sulphide of potassium is the first step in the process of the explosive reaction of gunpowder, Faraday has alleged the flame of this compound to be, in the case in point, an important instrument in the propagation of fire throughout the mass.

The hepatic odour of the fumes consequent to the firing of cannon, and likewise of the washings of a gun after the customary service, demonstrate the production of a sulphide. It has been found that a filtered solution of the residue displays, when tested by iron, the red hue which indicates the presence of a sulphocyanide.

Agreeably however to a qualitative examination, the solid residue of exploded gunpowder consists mainly of nearly equal parts of carbonate and sulphate of potash, while the gaseous residue is constituted nearly of equal volumes of carbonic acid and nitrogen. Of course the sulphate may arise from the oxidation of sulphide formed at the outset. Notwithstanding that the ingredients of gunpowder are prepared as above stated, confinement is necessary to prevent the grains from being thrown apart and chilled, so as to prevent the propagation of the ignition, through the congeries forming a charge, by means of the flame of the first portions fired. This was fully demonstrated by the exposure of a pile of gunpowder, comprising enough for the charge of a musket, within an exhausted receiver, to a wire intensely ignited by a galvanic discharge. The grains did not take fire instantly, probably because the vapour evolved prevented actual contact; and when ignition did ensue, it extended only to the production of a feeble flash. On examination, it was found that a portion of the powder had escaped inflammation.

In the next place, a like weight of gunpowder was consolidated into a cylinder by intense pressure. Thus prepared and ignited, by contact with an incandescent wire in the exhausted receiver, more than half of the cylinder remained unconsumed.

A much larger cylinder of the same mixture, similarly consolidated, placed at the bottom of an iron pot, four inches in diameter and twelve inches in depth, on being touched by the end of an iron rod reddened in the fire, burnt at first like a squib, but towards the last was dissipated with an activity in some degree explosive, probably in consequence of the pressure created by the reaction of the gaseous current generated by its own deflagration.

The want of confinement, which is thus capable of lessening the explosiveness of gunpowder, of which the constituents are intimately intermingled, is still more enfeebling, where analogous reagents are ignited together without admixture or comminution. Under these circumstances, the reagents are made to recede from each other by the generation of that vapour or gas, to the evolution of which, under confinement, the capability of exploding is due. Thus sundered, they are chilled by radiation, so that the temperature requisite to sustain and communicate ignition is not supported. Moreover, the rapidity of reaction being as the multiplication of the points of contact, and these being fewer as the substances are less divided and intermingled, the deflagration takes place in detail, instead of having that simultaneousness which is indispensable to render it explosive.

In addition to the ideas above-mentioned as having been conveyed in Dr. Hare's letter to Hays, it was urged also that his inference as to the explosion of water with incandescent nitre being attributable to a reaction analogous to that represented as taking place when potassium is burnt with the oxide of hydrogen, was supported by the fact, that at a white heat the base of nitre spontaneously abandons its acid, while from water it cannot be separated by any temperature. Consequently, the presentation of substances, consisting of carbon, hydrogen, and oxygen, by yielding water to the base, could not but be productive of a result analogous to that which results from the presentation of sulphur and carbon.

The only obstacle is as follows:—Substances containing hydrogen and oxygen, whether in the proportion for forming water, like sugar, starch, gum and wood, or having an excess of hydrogen, like oils and resins; moreover, all the constituents of nitre, even the base, are susceptible of the aëriform state at the temperature producible by the reaction of nitre with them. But when kept together until that point is attained, the explosive power must be fully equivalent to that of gunpowder. The reagents are in a state analogous to that of two gases extremely condensed.

The explosibility of incandescent nitre with water was illustrated in the small way, by heating a portion in a platina capsule by the flame of a hydro-oxygen blowpipe, and sudden immersion in the liquid. So active was the explosion, that a portion of the resulting hydrate flew out upon the operator. Yet, when thrown in the same state upon molasses or sugar, no explosion ensued: nevertheless, when a capsule containing nitre heated to the point of volatilization was struck with the face of a hammer coated with sugar melted upon it and made to adhere by moisture, a detonation took place. A still more powerful detonation was produced as follows:—Upon an anvil a disc of paper of three inches in diameter was laid, covered with pulverized sugar: over the sugar was placed another similar disc, covered with pulverized nitre: a bar of iron rather wider than the discs at a welding heat was then held over them, and subjected to a blow from a sledge. An explosion, with a report like that of a cannon, ensued.

Instructed by the facts and considerations above stated, it is inferred that the explosions which contributed to extend the conflagration in New York, as above mentioned, arose from the reaction of the nitre with the combustible merchandize with which it was surrounded. It is presumed that as soon as the fire reached any of the gunny bags it must have run rapidly through the whole pile, by means of the interstices necessarily existing between them, the nitre with which they were imbued causing them to deflagrate. Much of the salt being thus brought to the temperature of fusion, it must have run about the floor, reached the combustibles, and soon found its way to the next story through the scuttles which were open. All the floors must have been rapidly destroyed by the consequent deflagration, far exceeding in activity any ordinary combustion. Meanwhile, the nitre being all liquefied and collected in the cellar in a state of incandescence, and the merchandize conglomerated by the fusion of sugar and shell-lac, aided by the molasses, the weight, the liquidity, and temperature, must have produced all the conditions requisite to intense detonations. The floors having been consumed, the store must have been equivalent to an enormous crucible of twenty feet by ninety, at the bottom of which were nearly 300,000 lbs. of nitre, superficially heated far above the temperature producible by any furnace, so as to convert the reagents into nascent aëriform matter under a pressure of half a million of pounds. The intense reaction, however, would not permit of durable contact. At each impact the whole mass must have been thrown up explosively, and hence the successive detonations. But the chemical reaction, the heat, and the height of the fall, growing with their growth, and strengthening with their strength, the last elevation was succeeded by the thundering report and stupendous explosion of which it has been an object to afford a satisfactory explanation.—*From the Journal of the Franklin Institute.*

PREPARATION OF IODIDE OF LEAD. BY M. T. HURAUT.

The author remarks that several processes are known for the preparation of iodide of lead; all of which give tolerably satisfactory results. When carefully employed they yield a pure product, and the quantity obtained differs but little from that indicated by theory; it is, therefore, of little consequence which of the processes is adopted in preparing small quantities of the iodide; the case is, however, different when considerable quantities of the ingredients are employed, as in this case the differences are too considerable to be neglected.

The author thinking that some experiments which he has made on the subject would not be uninteresting, has published them; and in every case such a quantity of iodine or iodide was employed as ought to yield, by theory, 18·20 grammes of iodide of lead.

Process by Iodide of Potassium.—This process is that originally employed; it consists in decomposing iodide of potassium by a salt of lead. The Codex prescribes the neutral acetate, but this salt has been generally abandoned since it was discovered by M. Depaire and Felix Boudet, that nearly one-tenth of the iodide of lead was dissolved by the acetate of potash formed; 13·10 grammes of iodide of potassium containing 10 grammes of iodine were treated with neutral acetate of lead; the weight of the iodide precipitated was 15·70 to 15·80 grammes.

To avoid the loss occasioned by the use of acetate of lead, M. Boudet proposed to substitute the nitrate for it; by this process M. Huraut obtained with 13·10 grammes of iodide of potassium from 17·50 to 17·55 of iodide of lead.

Iodide of lead prepared with iodide of potassium is of a fine lemon-yellow colour, and entirely soluble in boiling water.

Process by Iodide of Sodium.—Ten grammes of iodine converted into this salt gave with acetate of lead 15·90 to 16·10 of iodide, and with the nitrate 16·85 to 16·95. It resembled that obtained with iodide of potassium perfectly.

Process by Iodide of Calcium.—A quantity of this containing 10 grammes of iodine, gave 17·60 to 17·70 of iodide of lead, of a fine orange-yellow colour. In one experiment, so performed as to produce a crystalline iodide, the product was remarkably brilliant; with acetate of lead 17·25 to 17·40 of iodide were produced, also of a fine orange-yellow colour.

Process by Iodide of Iron.—Ten grammes of iodine converted into iodide of iron and treated with neutral acetate of lead, gave 16·70 to 16·75 grammes of iodide of lead; with nitrate the products were 17·50 grammes; they were orange-yellow, and totally soluble in boiling water in both cases.

Process by Iodide of Zinc.—This salt is now perhaps that most commonly employed in preparing iodide of lead; the preference given to it arises from the facility with which it is prepared, its great solubility and unalterability in the air; 10 grammes of iodine converted into this salt gave with acetate of lead 17·05 to 17·15 grammes of product, and with the nitrate 17·40 to 17·45. The colour is palish orange-yellow.

Process by the double Iodide of Potassium and Lead.—This is a complicated plan proposed by M. Thevenot; the author compared the product with that afforded by the above-described processes; the comparison was in favour of the latter. M. Huraut concludes from the above-described experiments, that in the preparation of iodide of lead the nitrate ought to be preferred to the acetate, on account of the greater quantity of product which it yields.

The process by iodide of calcium is the most advantageous both as to the quality and quantity of the product.

The two processes by iodide of iron and iodide of zinc yielding equally fine and abundant products, it is nearly indifferent which is employed.

The process by iodide of sodium offers no advantage, and that by iodide of potassium is the least economical.

There is a loss of nearly 10 per cent. in preparing iodide of lead on using iodide of potassium and acetate of lead; the greater part of which loss may, however, be avoided by substituting the nitrate, or by adding to the supernatant liquor a sufficient quantity of nitric acid to decompose the acetate of potash.—*Journ. de Pharm. et de Chem,* Janvier 1849.

ON THE PROTOGINE OF THE ALPS. BY M. DELESSE.

The author observes that protogine usually contains five different minerals, which are, quartz, orthose, oligoclase, mica with a base of iron, and a variety of talc: these may be seen in the protogine of Mont Blanc. These minerals are not however equally distributed, and one or more of them are frequently wanting; but then the minerals which remain have so preserved the same characters as those which they possessed when the five elements are present in the rock, that it is impossible not to consider them as formed under the same circumstances; they constitute therefore varieties of the original rock, into which they pass insensibly, both by their mineralogical characters and their geological relations.

Quartz.—Quartz forms one of the important elements of protogine as of all granitic rocks. When the rock has a well-characterized granitic structure, the quartz of the paste is sometimes confusedly crystallized; generally, however, this does not occur, and it is hyaline, gray or violet; when it is in crystals of several centimetres in thickness, as seen in some veins, instead of being reddish or violet, it generally has a deepish black colour, and is of the variety called smoky quartz.

It may be stated generally that in fracturing pieces having the usual thickness of the grains of quartz or the paste of the rock, the difference of colour is derived rather from the greater thickness of the quartz in the veins than from the presence of a greater quantity of colouring matter.

This colour of quartz, which is observable in many granitic rocks, is derived from organic matter, which is volatile without leaving any residue, and disappears completely by slight calcination, the quartz losing only twelve thousandths, and becoming white and transparent.

This organic matter is not volatile *in vacuo* at common temperatures, for it does not disappear by exposing the quartz for several days over sulphuric acid in the exhausted receiver; nor is it destroyed when the quartz is digested, either hot or cold, in hydrochloric acid or ammonia: this resistance to chemical and physical agents is probably derived, in part, from the intimate admixture of the organic matter in the pores of the quartz.

Orthose.—The colour of this is generally white or grayish-white, sometimes, however, it is fawn- or rose-coloured, or pale scarlet. When it has a tendency to a brownish-yellow or red colour, the mineral is altered by incipient decomposition: it has a brilliant pearly lustre.

According to Saussure, its density is 2·615. Its crystals are often several centimetres long, well-formed, and generally possess the characteristic macle which is common to them in granitic rocks.

The crystals of orthose, oligoclase and mica analysed by M. Delesse, were taken from an enormous block, well-known at Chamouni, and which has fallen from the needles above the Mer de Glace.

The following was found by the author to be the composition of the grayish-white orthose, with a tint of fawn colour:—

Silica	66·48
Alumina	19·06
Lime	0·63
Peroxide of iron	traces
Magnesia	traces
Potash	10·52
Soda	2·30
	98·99

Oligoclase.—In protogine, as in the greater number of granites, there is besides orthose a second felspar, which in this case is oligoclase. It is somewhat difficult to distinguish on account of its white colour, which is nearly the same as that of orthose: this however is translucent, whilst oligoclase is dull, or very slightly greenish; it is, moreover, characterized by parallel microscopic striæ, and the crystals are often complex and macled, like those of the albite of Carlsbad. Its density is 2·633.

The analysis of very pure crystals from the needles of the Mer de Glace, made by carbonate of soda and hydrofluoric acid, gave—

Silica	63·25
Alumina..............	23·92
Peroxide of iron	traces
Oxide of manganese	traces
Lime	3·23
Magnesia	0·32
Soda	6·88
Potash	2·31
	99·91

This composition is almost identical with that of the oligoclase of

Warmbrum in Silesia, analysed by MM. G. Rose and Rammelsberg.

Mica.—M. Beudant has already observed that protogine contains mica: it is of a more or less deep green colour, and has little or no lustre; by calcination in an open crucible it becomes of a reddish-bronze colour, with brilliant reflexions; in a close crucible it becomes blackish green. When it is in very thin laminæ the action of the air is sufficient to give it a bronze colour, which is a character that may serve to recognize it. Its density is 3·127, which is much greater than that of the micas of granites; this is unquestionably owing to the large quantity of oxide of iron which it contains.

It is not crystallized in small transparent scales; on the contrary, it has the form of small irregular hexagonal prisms, the edges of which are not perpendicular to the bases.

Before the blowpipe the edges are rounded with difficulty when in small laminæ; with fluxes it indicates iron and manganese, and it dissolves entirely in phosphate of soda.

It is perfectly acted upon even by hydrochloric acid, and the silica separates in the form of flocculi: the facility with which it is acted upon is probably owing to the great quantity of iron.

The analysis was performed on the mineral taken from the granite block already mentioned; it gave—

Silica	41·22
Alumina	13·92
Peroxide of iron	21·31
Protoxide of iron	5·03
Protoxide of manganese	1·09
Lime	2·58
Magnesia	4·70
Potash	6·05
Soda	1·40
Fluorine	1·58
Water and loss by heat	0·90
	99·78

Talc.—Protogine also contains a substance forming very small contorted laminæ inserted among its various minerals, and which is to be regarded as a variety of talc. It has a pearly lustre; its colour varies from celadon to emerald and pale grayish-green. By calcination it acquires sometimes a brownish tint and sometimes a bright wood-brown tint, with golden reflexions; pure talc becomes very slightly yellowish silver-white. It is not elastic; its hardness is rather greater than that of talc, even when unmixed with foreign matters; like talc, it scratches glass after calcination.

Very thin laminæ of this talc, extracted from various specimens of protogine, were tried by the blowpipe; at a very high temperature, as already remarked by Saussure, their edges were rounded without exfoliation, and the fused portion was coloured by iron. Talc, on the contrary, exfoliates without fusing; this fusibility of the substance and the brown colour which it acquires by calcination indicate that it is richer in iron than is the case with talc.

Independently of the minerals which have been described, protogine, as observed by MM. Dufrenoy and E. de Beaumont, may accidentally contain hornblende, sphène, iron pyrites, garnets and serpentine.

Some veins contain fluor spar, oligiste iron and sulphuret of molybdenum, &c. In l'Oisan there are albite, rutile, anatase, brookite, &c.; lastly, there are found in veins which appear to be contemporaneous with the rock, and usually formed of quartz, epidote and the variety of chlorite, to which M. G. Rose has given the name of ripidolite; it is also found in the paste of protogine.—*Ann. de Chim. et de Phys.*, Janvier 1849.

EXAMINATION OF MADDER. BY M. DEBUS.

In order to isolate the different colouring matters of Zealand madder, the author employs the following process: the root is exhausted by boiling water, and the decoction is boiled with excess of hydrate of lead. The colouring matters form with this oxide insoluble compounds of a reddish brown colour. The deposit is to be separated, washed, and decomposed with dilute sulphuric acid and heat. The colouring matters, which are slightly soluble in water, separate with the sulphate of lead. The precipitate is to be boiled in alcohol, which dissolves the greater part of the colouring matters. They may be separated into two groups by agitating the alcoholic solution with calcined oxide of zinc. Some of them precipitate in combination with the oxide of zinc, while others remain in solution.

The author has hitherto examined only the first group, that is to say, the colouring matters combined with the oxide of zinc. They are purified by decomposing them with weak sulphuric acid, and dissolving the precipitated colouring matter in æther; the solution obtained is again heated with oxide of zinc. The zinc compound, heated with dilute sulphuric acid, leaves as a residue a mixture of two colouring matters, both soluble in a boiling solution of alum, one of which precipitates on cooling and the other remains in solution; the first constitutes what the author calls *lizaric acid*; this substance is obtained in a state of purity by boiling it with a little dilute hydrochloric acid, to free it from the alumina which it retains, and by repeated solution in boiling alcohol.

The second colouring matter, which remains in solution in the aluminous liquors, may be precipitated by sulphuric acid. The separation is not completed in less than twenty-four hours; the precipitate, exhausted by hot dilute hydrochloric acid, which removes a little alumina, is afterwards dissolved in 150 to 200 times its weight of boiling alcohol. In two or three hours long red needles separate, which constitute what M. Debus calls *alizaric acid.*

Lizaric Acid.—It crystallizes from its alcoholic solution in long orange-red coloured needles: it is soluble in æther, in alcohol and in hot water, but dissolves with difficulty in a boiling solution of alum. Sulphuric acid dissolves it, and becomes of an intense red colour; on diluting the solution with water the colouring matter is precipitated unaltered. The salts formed with lizaric acid are of a red or violet colour, and, with the exception of the alkaline salts, are insoluble in water or in alcohol. The composition of the free acid is expressed by the formula $C^{30} H^{10} O^{9}$. The salt of lead, obtained by adding lizaric acid, dissolved in alcohol acidulated with acetic acid to an alcoholic solution of acetate of lead, is formed of $C^{30} H^{8} O^{7}, 2PbO$.

Oxylizaric Acid is distinguished from lizaric acid by the facility with which it dissolves in a solution of alum. It is slightly soluble in cold water, but dissolves more readily in boiling water, in alcohol, æther and the alkalies. Fuming sulphuric acid dissolves, and may be heated with it without altering it; the author gives $C^{15} H^{5} O^{5}$ as its composition; the salt of lead would have $C^{15} H^{4} O^{4}$, PbO for its formula. Hence it is evident that by adding one equivalent of oxygen to one equivalent of lizaric acid, two equivalents of oxylizaric acid will be obtained. It is this relation between the two acids which is expressed by the name of the latter.—*Journ. de Pharm. et de Chim.*, Janvier 1849.

ANALYSES OF FELSITE, OLIGOCLASE AND MUROMONTITE.

M. Kerndt has analysed the above-named minerals, with the annexed results :—

Felsite.—Crystallized green felsite, density 2·5465, from Bodenmais gave, taking the mean of two analyses,—

Silica	63·657
Alumina	17·271
Potash	10·659
Soda	5·134
Lime	0·394
Magnesia	2·281
Protoxide of iron	0·451
Protoxide of manganese	0·153
	100·00

Oligoclase.—This mineral from Boden near Marienberg, in the Erzgebirge, of density 2·66–2·68, gave—

Silica	61·958
Alumina	22·658
Potash	3·079
Soda	9·432
Lime	2·025
Magnesia	0·104
Peroxide of iron	0·348
Peroxide of manganese	0·396
	100·000

Muromontite.—By this name the author designates a ceriferous mineral met with in the environs of Mauersberg near Marienberg, in the Erzgebirge. It has the form of black grains, with a greenish reflexion. Density, 4·263–4·265.

This mineral contains—

Silica	31·089
Yttria	37·140
Glucina	5·510
Alumina	2·230
Oxide of lanthanium	3·530
Oxide of cerium	5·540
Protoxide of iron	11·230
Protoxide of manganese	0·900
Lime	0·710
Magnesia	0·420
Soda	0·650
Potash	0·170
Water	0·820
	99·939

Journ. de Ph. et de Ch., Novembre 1848.

ON THE FERROCYANIDES OF STRYCHNIA AND BRUCIA. BY M. D. BRANDES.

The author states that when a solution of ferrocyanide of potassium is added to one of a neutral salt of strychnia, an abundant precipitate is obtained, consisting of small and nearly colourless needles.

In operating on dilute solutions deprived of free acid, crystals of two to three centimetres in length and of a very bright yellow colour are obtained; they are four-sided prisms, terminated by dihedral summits: these crystals are ferrocyanuret of strychnia, represented by the formula 2(Str, H Cy)+Fe Cy+8HO. At 212° the salt loses 6·1 per cent., or six equivalents of water. If it be dissolved in hot water, or if the cold saturated solution be boiled, crystals of strychnia separate, and the liquor, which is of a deep yellow colour, holds ferrocyanide of strychnia in solution,

$$3(\mathrm{Str, HCy,}) + \mathrm{Fe^2\,Cy^3} + 12\mathrm{HO}.$$

This salt, which forms crystals of a golden-yellow colour, corresponds to the red prussiate of potash, and may also be obtained by mixing the cold saturated solutions of sulphate of strychnia and red ferrocyanide of potassium: according to the author, this salt loses three equivalents of water in a dry vacuum, six equivalents at 212°, and eight equivalents at 277° F. Above this temperature it decomposes. When an alcoholic solution of strychnia is mixed with a solution of hydroferrocyanic acid in alcohol, a white amorphous precipitate is obtained. This is nearly insoluble in water or alcohol, and has a distinct acid reaction. M. Brandes assigns to it the formula (Str, 2ACy+2Fe Cy)+5HO. He considers it as an acid analogous to the hydroferrocyanic, and expresses its constitution as follows, deducting the five equivalents of water it contains (Str, HCy

$+2FeCy)+HCy$; the author has not, however, as yet succeeded in preparing salts directly with this acid.

The ferrocyanurets of brucia are prepared by processes analogous to those above described; they resemble, both in their properties and composition, the corresponding salts of strychnia.—*Ibid. Janvier* 1849.

METEOROLOGICAL OBSERVATIONS FOR JAN. 1849.

Chiswick.—January 1. Overcast: hazy. 2. Clear and frosty. 3. Frosty: dry haze: overcast: frosty. 4. Uniformly densely overcast: rain. 5. Drizzly and foggy. 6. Overcast. 7. Overcast: rain at night. 8. Rain. 9. Very fine: slight rain. 10. Cloudy: boisterous: rain. 11. Rain: densely clouded. 12. Frosty: overcast: rain. 13. Densely clouded: rain. 14. Rain. 15. Clear. 16. Fine: rain. 17. Rain: densely overcast: clear. 18. Fine: boisterous at night. 19, 20. Very fine. 21. Very fine: overcast: boisterous. 22. Boisterous: fine: clear and boisterous. 23. Densely clouded: fine. 24. Cloudy: boisterous at night. 25. Densely clouded: boisterous. 26. Rain: exceedingly fine. 27. Slight frost: overcast: rain. 28. Cloudy: fine. 29. Rain: cloudy and cold: frosty at night. 30. Slight fog: drizzly. 31. Fine: clear and frosty at night.

Mean temperature of the month	39°·56
Mean temperature of Jan. 1848	33 ·62
Mean temperature of Jan. for the last twenty years	36 ·40
Average amount of rain in Jan.	1·59 inch.

Boston.—Jan. 1. Cloudy. 2—4. Fine. 5, 6. Cloudy. 7. Fine: rain early A.M. 8. Rain. 9. Fine: rain P.M. 10. Cloudy: stormy all day. 11. Cloudy: rain early A.M. 12. Fine. 13. Rain: rain early A.M. 14. Cloudy: rain early A.M. 15. Fine: rain A.M. and P.M. 16. Foggy. 17—20. Fine. 21. Cloudy. 22—24. Fine. 25. Cloudy. 26. Fine: rain early A.M. 27. Fine: rain P.M. 28. Fine. 29. Rain: rain A.M. 30. Cloudy: rain A.M. and P.M. 31. Fine.

Applegarth Manse, Dumfries-shire.—Jan. 1. Frost moderate. 2. Frost very hard: barometer falling. 3. Frost clear: fine. 4. Frost, but cloudy. 5. Frost: cloudy. 6. Frost: still cloudy. 7. Frost: still more overcast. 8. Thaw: rain: fog: rain again. 9. Frost again: clear A.M.: rain P.M. 10. Heavy rain during night: rivers flooded. 11. Frost A.M.: thaw at noon: rain. 12. Soft rain all day. 13. Soft rain: cleared: rain P.M. 14. Gentle frost: cloudy: wind rose. 15. Soft: cloudy. 16. Mild and clear after rain A.M. 17. Moist A.M.: rain and high wind P.M. 18. Very fine till noon: rained again. 19. Frost: getting cloudy P.M. 20. Heavy rain and high wind P.M.: thunder. 21. Storm of wind and rain. 22. Fair, but a storm of wind. 23. Fair A.M.: came on storm, wind and rain. 24. Rain nearly all day: wind high. 25. Fair and keen A.M.: wet P.M.: high wind. 26. Fair A.M.: rain P.M. 27. Snow: rain: wind high. 28. Frost: clear: dull P.M. 29. Frost and snow: thaw and rain. 30. Frost moderate. 31. Thaw and showery.

Mean temperature of the month	36°·35
Mean temperature of Jan. 1848	33 ·80
Mean temperature of Jan. for the last twenty-five years .	34 ·90
Rain	3·70 inches.
Rain in January 1848	2·34 ,,
Average amount of rain in Jan. for the last twenty years	2·60 ,,

Sandwick Manse, Orkney.—Jan. 1. Cloudy. 2. Bright: cloudy. 3. Cloudy. 4. Cloudy: frost: snow-showers. 5. Bright: cloudy. 6. Snow. 7. Thaw: clear. 8. Rain: showers. 9. Showers: cloudy. 10. Rain: snow. 11. Snow. 12. Rain: showers. 13. Showers. 14. Showers: sleet-showers. 15. Showers. 16. Showers: cloudy. 17, 18. Showers. 19. Showers: clear. 20. Cloudy. 21. Rain: showers. 22. Sleet-showers. 23. Sleet-showers: rain. 24. Rain*: sleet-showers: cloudy. 25. Sleet-showers: aurora. 26. Sleet-showers: cloudy. 27. Bright: sleet-showers. 28. Sleet-showers: clear. 29. Frost: cloudy. 30. Snow: sleet: showers. 31. Sleet-showers: showers.

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* From 9 P.M. on 23rd till 2 P.M. on 24th (about 17 hours) 2·08 inches of rain fell.

Meteorological Observations made by Mr. Thompson *at the Garden of the Horticultural Society at* Chiswick, *near London; by* Mr. Veall, *at* Boston; *by the* Rev. W. Dunbar, *at Applegarth Manse,* Dumfries-shire; *and by the* Rev. C. Clouston, *at Sandwick Manse,* Orkney.

Days of Month.	Barometer.							Thermometer.								Wind.				Rain.			
	Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Dumfries-shire.	Orkney, Sandwick.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.
1849. Jan.	Max.	Min.		9 a.m.	9 p.m.	9¼ a.m.	8½ p.m.	Max.	Min.		Max.	Min.	9¼ a.m.	8½ p.m.								
1.	30·179	30·144	30·00	30·12	30·12	30·22	30·19	33	19	33	36½	31	37½	38½	e.	calm	e.	s.	......	......	......	......
☽ 2.	30·124	29·833	29·93	30·00	29·86	30·02	29·89	29	20	27	33	24½	34	39	e.	calm	e.	wsw.	......	......	......	......
3.	29·744	29·696	29·59	29·70	29·72	29·83	29·89	32	23	27	29	15½	37½	36	e.	calm	e.	n.	......	......	......	......
4.	29·784	29·764	29·65	29·73	29·76	29·91	29·95	35	32	30	30	21	35	33	e.	calm	e.	nne.	·14	......	......	......
5.	29·908	29·760	29·58	29·72	29·89	29·95	29·98	34	26	35·5	35½	27½	34	35	ne.	calm	e.	w.	·01	......	......	......
6.	30·011	29·995	29·79	29·93	29·85	29·86	29·92	35	19	33	30	21½	33½	31	ne.	calm	ne.	e.	......	......	......	......
7.	30·061	29·894	29·78	29·88	29·61	29·88	29·62	38	35	30	33½	25½	36	37	nw.	calm	nne.	se.	·31	......	......	·66
○ 8.	29·589	29·553	29·38	29·35	29·25	29·26	29·24	42	34	37	39½	32	40	41	se.	sw.	ne.nw	sse.	·11	·10	0·00	·24
9.	29·591	29·483	29·15	29·31	29·14	29·28	29·17	47	37	38	42	33½	43	39	sw.	calm	e.	ese.	·08	·22	......	·06
10.	29·133	29·002	28·64	28·48	28·70	28·86	29·14	49	38	45	44	35	37	33½	sw.	sw.	se.	e.	·06	......	......	·20
11.	29·417	29·055	29·86	29·31	29·86	29·65	29·74	40	24	38	41½	34	34	34	nw.	n.	nw.	s.	·02	·08	......	·15
12.	30·136	29·879	29·85	29·75	29·55	29·37	29·31	38	36	32·5	47½	27½	40	42	sw.	calm	sw.	w.	·07	......	......	·18
13.	29·736	29·586	29·30	29·40	29·24	29·20	29·12	53	49	43	44	39	41½	40½	sw.	s.	s.	sw.	·05	·24	......	·12
14.	29·859	29·474	28·83	29·00	29·50	28·94	29·24	56	32	54	43	40	41	43	sw.	sw.	w.	wnw.	·08	·09	......	·23
15.	30·043	29·966	29·63	29·59	29·61	29·26	29·37	55	31	38	43	34½	44½	44	sw.	wsw.	sw.	w.	......	·07	......	·22
☾ 16.	29·954	29·720	29·60	29·65	29·42	29·44	29·30	52	42	34	...	40	46	42	s.	sw.	s.	s.	·02	......	1·50	·05
17.	29·945	29·708	29·26	29·27	29·65	29·27	29·44	54	30	45·5	...	...	41½	41½	sw.	ssw.	w.	wsw.	·05	......	......	·14
18.	30·005	29·934	29·57	29·46	29·40	28·91	29·02	50	46	40·5	...	...	48	45½	sw.	s.	s.	w.	......	......	......	·17
19.	29·983	29·909	29·46	29·60	29·73	29·50	29·77	53	42	49	...	...	41½	37½	sw.	sw.	sw.	calm	......	......	......	·08
20.	30·254	30·009	29·64	29·96	29·95	29·95	29·67	51	41	45·5	...	...	41	40	s.	sw.	ne.	sse.	......	......	......	......
21.	30·238	29·965	29·60	29·69	29·44	29·37	29·07	50	39	46	...	...	38½	43	sw.	s.	sw.	sw.	·16	......	......	·52
22.	30·214	29·955	29·46	29·50	29·74	28·99	29·33	50	48	42	...	...	42	41½	sw.	wsw.	sw.	w.	......	......	......	·32
23.	30·414	30·303	29·70	29·83	29·97	29·45	29·58	51	45	46	51	39	45	44	sw.	w.	wsw.	w.	......	......	1·40	·20
● 24.	30·374	30·200	29·80	29·82	29·70	29·36	29·49	50	47	47·5	53	46	43½	41½	sw.	w.	wsw.	w.	......	......	......	1·33
25.	30·089	29·907	29·52	29·56	29·44	29·14	28·99	51	44	50	50	47	43½	41½	sw.	wsw.	wsw.	wsw.	·04	......	......	1·38
26.	29·931	29·804	29·32	29·39	29·59	29·16	29·55	49	26	41·5	45½	36½	39	38	w.	wsw.	w.	nw.	·01	·18	......	·25
27.	29·948	29·385	29·05	29·67	29·29	29·65	29·50	45	34	34	40	31½	32	36	s.	wsw.	se.	ese.	·43	......	......	·03
28.	29·379	29·321	28·94	29·10	29·30	29·47	29·76	45	33	39	39	33	39	34½	w.	w.	sw.	nne.	·02	·11	......	·18
29.	30·079	30·571	29·25	29·79	29·93	29·98	29·81	42	24	37·5	37½	31½	34	36½	n.	nw.	ne.	s.	·01	·24	......	·02
30.	30·138	29·805	29·77	29·68	29·63	29·22	29·37	48	29	33	48	30	37	38½	s.	calm	se.	w.	·04	·15	0·80	·50
☽ 31.	30·242	30·131	29·74	29·88	30·03	29·65	30·01	47	22	35·5	45	34½	41	39	w.	w.	w.	nnw.	·02	·03	......	
Mean.	29·919	29·797	29·56	29·584	29·608	29·483	29·530	45·35	33·77	38·9	40·9	31·6	39·38	38·61					1·73	1·51	3·70	7·54

THE
LONDON, EDINBURGH AND DUBLIN
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AND
JOURNAL OF SCIENCE.

[THIRD SERIES.]

APRIL 1849.

XXXIII. *On the Existence and Effects of Allotropism in the constituent elements of Living Beings.* By JOHN WILLIAM DRAPER, *M.D., Professor of Chemistry in the University of New York**.

IT has been completely established for the majority of elementary substances, that there are several forms under which each may occur; forms which differ entirely both in their physical and chemical relations.

Thus, in the case of carbon, many such forms are known. To three of them M. Berzelius has directed attention:—1st, ordinary charcoal; 2nd, plumbago; 3rd, diamond. They are three distinct modifications of the same element. They differ in specific gravity, in specific heat, and in conducting power, both for electricity and caloric. In their relations to light, the first perfectly absorbs it and is black; the second reflects it like a metal; the third is transparent like glass. When crystallized, plumbago and diamond do not belong to the same system: their chemical relations are also strikingly different. Charcoal takes fire with facility, and some varieties of it are even spontaneously combustible in the air; but crucibles and furnaces are made of plumbago because of its incombustibility; and the diamond with difficulty is set on fire in pure oxygen gas.

It seems immaterial to what class elementary bodies belong, whether electro-negative or positive: they present analogous results. Silicon, sulphur, selenium, phosphorus, titanium, chromium, uranium, tin, iridium, osmium, copper, nickel, cobalt, iron, oxygen, chlorine, are cases in point; and the instances which appear as exceptions are rapidly diminishing in number.

* Communicated by the Author.

As is well known, to these singular modifications M. Berzelius gave the designation of allotropic forms, and the whole phænomenon passes conveniently under the designation of allotropism. He shows that the peculiarity assumed is often of such a persistent nature that it is not lost, even though the substance affected should go into combination with others. Thus there are two forms of silicon; one combustible, and the other remarkably incombustible. Each, by uniting with oxygen, gives rise to a silicic acid; the acid in one case being soluble in water and in hydrochloric acid, and in the other the reverse. And in like manner, metallic arsenic, which exhibits the same duality of condition, gives rise to two different arsenious acids. Of phosphorus there are at least two modifications; and accordingly we have two compounds of that body with hydrogen, one of which is spontaneously inflammable, and the other not; and at least two oxygen acids, the monobasic and tribasic, in which the essential difference rests in the state of the phosphorus they contain.

It is to be remarked, that, so far as observation extends, the most common cause of producing these singular differences is the action of that class of agents which we term imponderable substances. In very many cases change of temperature brings about allotropic change; in others it is the agency of light, as in chlorine and phosphorus; and again, in others, association with foreign bodies, which apparently establish new voltaic relations. Heat, light and electricity seem to be the general modifying agents.

M. Berzelius, following the suggestion of M. Frankenheim, proposes a nomenclature for pointing out the peculiar form referred to in any special case. It depends on the use of Greek letters. Thus we have the three forms of carbon just alluded to, designated on these principles by $C\alpha$, $C\beta$, $C\gamma$. But in a paper which I published in this Journal on the allotropism of chlorine (Nov. 1845, p. 327), it is remarked that we may often with greater convenience use the simple expressions "active" and "passive." Thus active chlorine is that which will decompose water in the dark, passive chlorine failing to do so. In this paper the same expressions will be employed.

Hitherto allotropism has only been considered as affecting inorganic states of matter, but its influence can be plainly traced in the far more interesting case of organic beings; and, when placed in a proper point of view, yields a remarkable explanation of some of the most obscure but important facts in physiology and pathology. These explanations I propose now to point out.

In the Philosophical Magazine (March 1846, p. 178) there is a paper by me explanatory of the causes of the circulation of the blood in the capillary vessels. It is merely an abridgement of a lecture which for eight years past has been delivered in this university. The doctrine there set forth has been generally received in America, and introduced into some of the standard works on physiology published in England. The principle on which it essentially depends, and which has been abundantly confirmed by direct experiment, is briefly this—that if there be two fluids occupying a capillary tube, or a porous structure of any kind, under the condition that one of them has a stronger chemical affinity for the substance of that tube or structure than the other, a movement of the liquids will at once ensue, that which has the stronger affinity driving the other before it. On this principle a clear account of the systemic circulation of animals may be given; for the arterial blood, an oxidizing liquid, having a stronger affinity for the soft tissues with which it is in contact than the venous blood, the affinities of which have been satisfied and therefore no longer exist, necessarily exerts such a pressure that motion must ensue, the arterial blood forcing the venous before it.

An application of the same principles shows that in the pulmonary circulation the motions must necessarily be in the opposite direction, or from the venous to the arterial side, as is actually the case. It also explains clearly the conditions of the portal circulation, in which the direct action of the heart could hardly be expected to be felt. With the generality which ought to belong to a true theory, it meets all the cases which occur in the lower orders of animal life, such as the greater circulation in fishes, in which there is no systemic heart; the movements which take place in the vascular system of insects; and even the extreme case of the rise and descent of sap in plants.

In this doctrine everything depends on the relationship between the nutritive fluid, or blood, and the solid parts with which it is brought in contact; and whatever changes that relationship must impress a corresponding change on the circulation itself.

From experiments which I made some time ago, I have been led to suppose that the arterialization of the blood, as it takes place on the cell-walls of the lungs, bears a strong analogy to the oxidation of white indigo. The loose hold which the colouring matter of the blood retains on the oxygen, coupled but not combined with it, is not unlike what is witnessed in other nitrogenized colouring matters, such as indigo, which oxidizes and deoxidizes with the utmost facility. Charged with the oxygen it has thus obtained, the arterial blood passes to all parts of the system; and now arises that striking but all-important physiological fact, that it does not attack indiscriminately all those parts of the soft solids which it first encounters, but proceeding in a measured way, exerts its action on such particles alone as have become effete, and accomplishing the great process of interstitial death, resolves those particles into other forms, so that they can be eliminated from the system by the lungs, the kidneys, and the skin.

Now why is it that things proceed in this way? What is it that regulates this interstitial death? Why is one atom preserved and another surrendered?

It is upon these obscure points that the recent discoveries in allotropism shed a flood of light.

The three leading neutral nitrogenized bodies, fibrine, albumen and caseine, are characterised by exhibiting allotropism in a most remarkable degree, and that in a double sense. 1st. Though so different from one another in their physical and chemical relations, it is admitted on all hands that they are mutually convertible; the albumen of the egg, during incubation, gives rise to fibrine and other allied bodies; from caseine, in the milk with which the young mammalia are nourished, the albuminous and fibrinous constituents of their systems arise; the nurse fed on fibrine and albumen secretes caseine from the mammary gland. Indeed there is no more reason to regard these three bodies as essentially distinct substances, than there is to apply the same conclusion to charcoal, plumbago and diamond. Between the two cases there is the most complete analogy; and if charcoal, plumbago and diamond, are merely allotropic forms of one substance, the same holds good for fibrine, albumen and caseine. But 2nd, each of these three compounds betrays a disposition under trivial causes to assume new forms; as with silicic acid so with fibrine, there are two varieties, one soluble in water, the other not. A difference of a few degrees of heat turns transparent albumen into the porcellanous variety, and analogous observations might be made respecting caseine.

It may therefore be asserted that these proteine bodies exhibit a propensity to allotropism in a far more remarkable manner than any other substances known; not only passing indiscriminately into one another, but also exhibiting special variations under the influence of the most trivial causes.

And now we may recall the fact, that of the agents which in the inorganic kingdom bring about these changes, the imponderable principles are pre-eminent. I transfer this observation to the case of organized beings, and infer that the nervous system has the power of throwing organized atoms into the active or passive state; that this is the fundamental fact on which all the laws of interstitial death depend; and that upon this principle—its existing allotropic condition—an organized molecule either submits to the oxidizing influence of arterial blood, or successfully resists that action.

But it has been stated that there are certain pathological conditions, which, upon these views, meet with a clear explanation; conditions, which, though long and laboriously studied by physicians, remain involved in contradictions and obscurity. The conditions to which I refer are those known as inflammation and congestion.

It is agreed among chemists, that during the prevalence of these conditions the urine assumes a peculiar constitution. In inflammatory actions the relative quantity of urea and sulphuric acid is much above the normal standard, whilst in congestive cases the reverse holds good, and the urea and sulphuric acid are below the standard. What is the interpretation of these remarkable facts? We shall find they are very significant.

The quantity of urea and sulphuric acid in the urine undoubtedly expresses the quantity of proteine matter that has undergone oxidation in the system. In all cases where that quantity is above the normal standard, the destruction of proteine matter has been correspondingly accelerated; and where it is deficient, the destruction has been reduced. The result of inflammations corresponds to the first of these cases, and of congestions to the second.

Recalling now what has been said respecting the cause of the capillary circulation, we see how all these apparently disconnected facts group themselves together in the attitude of dependent effects. In inflammation there has been that allotropic change in the soft solids involved, that they have assumed a disposition for rapid oxidation—they are active. Their relations with arterial blood have become highly exalted; and the blood flows, on the principles I have set forth, to the affected part with energy. Redness of that part and a higher temperature are the result. Oxidation goes on with promptitude, and urea and sulphuric acid begin to accumulate in the urine.

But in congestive cases it is the reverse; the parts affected are thrown into a more passive state. Oxidation goes on in a reluctant way, the amount of tissue metamorphosed diminishes, the urea and sulphuric acid diminish in the urine; and on the principles which I have endeavoured to explain respecting the capillary circulation, we perceive that an immediate action must be exerted on the flow of the blood, the passive condition of the tissues and diminished capacity for oxidation restrain the flow from the arteries, and there being now less pressure on the contents of the veins, engorgement of those vessels is the result, and this condition of things is what a physician designates as congestion.

In this manner, if we admit the existence of allotropism in organic atoms, we can give a very clear explanation of the condition of the circulation in the pathological states of inflammation and congestion, and also of the peculiarities which in those states belong to the constitution of the urine.

University of New York,
Feb. 17, 1849.

XXXIV. *On the discovery of the Chilling Process in the casting of the Specula for Reflecting Telescopes, &c.* By Professor Potter, *A.M., F.C.P.S., late Fellow of Queen's College, Cambridge.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

I PERCEIVE at page 143 of the February Number, that in an abstract of a paper by Mr. Lassell read at the Astronomical Society on the 8th of December 1848, there is the following passage:—" The mode of casting the large speculum which I employed involved the principle, discovered, I believe, and first published, by Lord Rosse, of casting the speculum on what is technically called a *chill*, *i. e.* an iron base, slightly warmed, which causes the speculum to cool upwards in horizontal strata."

Your readers will find in the fourth volume of the new series of the Edinburgh Journal of Science, namely for 1831, that in a paper on improvements in the casting, working, &c. of Specula for Reflecting Telescopes, I had discovered and there published (page 18) the improvement in speculum metal by casting upon a *chilling surface*.

All known substances which were affected in different manners by rapid and slow cooling, after being heated, had up to that time indicated that it was a general law, that sudden cooling induced the property of brittleness or loss of tenacity, cracking, and frequent falling into fragments of the substance suddenly cooled; and that the opposite procedure of very slow and gradual cooling, which was generally called annealing, induced toughness and tenacity in the substance. Glass, many minerals, and steel, were known to be subject to this law.

All writers on the casting of specula, including Lord Oxmantown in the second volume of the same series of the Edinburgh Journal of Science, had received or prescribed annealing as the course to be used in the casting of the metal; but in spite of all precautions, when the metal was of the best proportions of copper and tin for colour and polish, the castings were continually found to be cracked, and not unfrequently broken into fragments at the termination of the annealing process, whilst the metal was often so brittle as not to bear grinding with emery without tearing up.

In this state of the subject I undertook an experiment in the contrary direction (see page 18 of the paper referred to), and cast a metal upon a chilling surface, when I found that I had obtained by it the great desideratum, a hard compact metal, which bore grinding and polishing admirably. I repeated the experiment several times with small mirrors, and obtained always the same result. Having obtained full confidence in the method, I recommended it to Lord Oxmantown's notice in the before-named paper.

This chilling process in casting, and my method of making the polishing powder, detailed in the same paper, by precipitating with ammonia an oxide of iron from a solution of the sulphate and then calcining, will, I am sure, in future time be accounted amongst the most important discoveries connected with the construction of the reflecting telescope, and I claim the discovery and first publication of them.

I am, Gentlemen,

Your obedient Servant,

University College, March 13, 1849. RICHARD POTTER, A.M.

XXXV. *Thoughts on Ancient Metallurgy and Mining in Brigantia and other parts of Britain, suggested by a page of Pliny's Natural History. By* JOHN PHILLIPS, *Esq., F.R.S., F.G.S.**

TO one who meditates on the progress of natural knowledge, the difficulty of penetrating to a true estimate of its condition in past ages often appears unconquerable, except in cases which admit of the interpretation of ancient results by modern laws and theories. Once in firm possession of such laws, we enclose the old phænomena, so to speak, in a field to which are only such and such possible avenues, and

* From the Proceedings of the Yorkshire Philosophical Society for March 1848.

thus can sometimes declare the very mode by which the alchymist was led to his golden error, and the Chaldæan shepherds to brighter truths. Without this principle of interpretation, many almost modern writers, nay authors of this very century, can sometimes not be understood. The laws of modern geology and zoology, for such there are, and well-founded too, are as much required to put a true construction on some of the writings of Lister and Linnæus, as the methods of Ray, Linnæus, and Cuvier are required for the just estimation of Aristotle. We shall probably find the darkest pages of antiquity to be precisely those which refer to subjects where our own knowledge is least clear, least collected into laws of phænomena, and most removed from laws of causation. Ought we not, before declaiming on the ignorance of the ancients, to be careful to make allowance for the differences of form in which knowledge presents itself at different periods, as well as for the incompleteness of *their* records, and the imperfection of *our* interpretations?

Pliny's Natural History appears to me to be precisely in the position of difficulty which has been already alluded to. Its vastness, variety, and seeming disorder, may well deter the most comprehensive master of modern science from duly weighing its mass, or even measuring its surface; and the evident incompleteness and almost hap-hazard character of its chapters are apt to disgust the student of special branches of science and art. Yet, probably, if for each important branch of human knowledge handled by Pliny, a special editor were set to work, well-versed in the philosophy of his subject, Pliny would take a higher degree on examination, and the history of human knowledge be amended.

From the thirty-seven books of diffuse and erudite learning, the genuine work of Pliny the elder, let us fix on the part which treats of the nature of metals; and passing over his lamentations on the useless excess of gold and silver—which may be recommended to the Chancellor of the Exchequer—his accounts of the uses and properties of gold, electrum *, chrysocolla, silver, quicksilver, stibium, scoria argenti, spuma argenti, minium, cinnabar, brass, cadmium, iron, and many compounds of metals, let us pause at the 16th chapter of the 34th book, which treats of the metals of lead, white and black.

"The most precious of these, the white, is called by the Greeks *κασσίτερος*, and fabulously declared to be sought for in isles of the Atlantic, to which it is brought in wicker vessels, covered with leather (vitilibus navigiis corio circumsutis). But

* Gold with one-fifth of silver.

now it is ascertained to be indigenous in Lusitania and Gallicia, in sandy surface soil, of a black colour, and only distinguished by its weight. Small pebbles [of the ore] also occur principally in dried beds of streams. The miners [metallici] wash these sands, and what subsides they melt in furnaces.

"It is also found with the gold ores (aurariis metallis) which are called stream works (elutia), the stream of water washing out (eluente) black pebbles a little varied with white, and of the same weight as the gold. On this account, in the vessels in which the gold is collected, these pebbles remain with it; afterwards they are separated in the chimneys* (caminis separantur), and being melted are resolved into plumbum album.

"In Gallicia plumbum nigrum is not made, because the adjoining Cantabria [Asturias] so much abounds in that metal.

"Not out of white plumbum as out of the black can silver be extracted.

"To solder together [pieces of] plumbum nigrum is impracticable without [the use of] white plumbum, nor the white to the black without the addition of oil. Nor can [pieces of] white plumbum be soldered together without the aid of the black metal.

"That [plumbum] album was in esteem during the Trojan time Homer is witness, who calls it *κασσίτερος*.

"Of plumbum nigrum the source is double: either it comes from its own vein, without admixture, or grows with silver, and is melted while mixed with that metal. The part which is first liquid is called stannum†, that which flows next is silver, that which remains in the furnace galena‡, which is the third portion of the vein (or ore). This being again melted§ yields plumbum nigrum, [the other] two parts [of the ore] being deducted."

This chapter is a text on which a 38th book of Natural History might be written, embracing the history or fable of the *κασσιτέριδες*, the ancient arts of metallurgy, and the eager trade in metals which allured the Phœnician sailors on the Atlantic, and led the Roman armies to Britain.

What is *κασσίτερος*, for which plumbum album is the equi-

* What distinctive meaning should be attached to furnaces and camini is uncertain. It seems that the camini may indicate, if not what we call chimneys, at least cavities in or above the furnace.

† Analogous to this is the process of separating silvery lead from mere lead, invented by H. L. Pattison, Esq.

‡ Lib. xxxiv. cap. 18. Est et molybdæna, quam alibi galenam vocavimus, plumbi et argenti vena communis.

§ At the present day we should perform this melting of the residual 'galena' in the slag-hearth, with a flux.

valent? what is stannum, obtained from mixed ores of silver and lead? what is galena, elsewhere called molybdæna? (cap. 18.) We need not ask what is plumbum nigrum, for by that is clearly designated lead.

That *κασσίτερος* or *καττίτερος* was tin, appears to be generally allowed. The mineralogist and miner who know the mode of occurrence and character of tin ore, will have no doubt that plumbum album of Pliny is tin, and that author twice positively and expressly identifies this with *κασσίτερος*.

The uses to which Homer puts *κασσίτερος* in the thoraca and shields of Agamemnon, Achilles, and Asteropæus, and in the greaves of Achilles, are such as imply easy fusibility and ductility, and indicate that the metal was highly valued and almost precious*.

Virgil puts no tin into the arms of Æneas—perhaps the metal was then of too vulgar use—employed too much by tinkers—to be fit for a heroic shield. Electrum is substituted, and iron is the staple article in the Vulcanian workshop, as brass was in that of ῞ΗΦΑΙΣΤΟΣ, 1000 years before.

The picture of the great artist—the Tubal Cain of the west, the cunning worker in metal, who melted, alloyed, inlaid, carved, and polished his work—whose multiplied bellows breathed at the will of the god softly or fiercely—whose brass was hardened to wound, or tempered to bend,—is perfect, and might be paralleled on a small scale till a few hundred years in the famous smiths of Wales, who made their own iron, and were by the laws of that country, as renewed by Howell Dda, allowed to sit next the sacred priest.

* The following are the principal passages in the Iliad where *κασσίτερος* is mentioned:—

XI. 25. In the thorax of Agamemnon were ten plates (*οἶμοι*) *μέλανος κυάνοιο*, twelve of gold and twenty of *κασσίτερος*.

XI. 34. In the shield of Agamemnon were twenty white bosses (*ὀμφαλοὶ*) of tin, and in the middle one of *κύανος*.

XVIII. 474. For the shield of Achilles ῞ΗΦΑΙΣΤΟΣ throws into his crucibles brass, unconquered *κασσίτερος*, honoured gold, and silver.

XVIII. 564. He pours the tin round the border.

XX. 270. In this shield were five plates; the two exterior ones brass; within these, two of *κασσίτερος*; and in the middle of all, one of gold.

XVIII. 612. The greaves of Achilles are made of soft *κασσίτερος*.

XXII. 503. The chariot of Diomedes was adorned with gold, and *κασσίτερος*.

XXIII. 561. In the brazen thorax of Asteropæus the ornament was of glittering *κασσίτερος*.

What is here called *κύανος*, and is apparently a much-valued substance, is difficult to say. From its colour, lapis lazuli, turquois, and carbonate of copper have been suggested. As it is only mentioned in connexion with the arms of Agamemnon, which were the gift of Cinyras king of Cyprus, the latter mineral may be thought to have the best title, especially if, as at Chessy, it occurs blue in Cyprus.

Why Pliny treats as a fable the story of the Cassiterides yielding tin, is somewhat difficult to say. He classes the Cassiterides with Hispania, book iv. cap. xxii. (ex adverso sunt insulæ,—Cassiterides dictæ Græcis, a fertilitate plumbi), and speaks of Mictis (on the authority of Timæus the historian) as six days' sail from Britain, and as yielding candidum plumbum, iv. cap. 16. If the Cassiterides are the Ocrynian Promontory and the Scilly Isles, from which, as recorded by Strabo, the Phœnicians drew their tin (Ἴκτις of Diodorus, Μίκτις of Timæus, and Οὔηκτις of Ptolemy being Vectis or Wight, from which the tin was carried through France to Marseilles), we may suppose that in the early period the only route for the tin of Cornwall to the Mediterranean was by sea to the western parts of Spain; but that in the latter period the track by land through Gaul to Massilia was preferred, and the old trade had become a tradition which Pliny chose not to adopt from Strabo, who is never quoted on this subject by the author of the *Historia Naturalis*, but may be obliquely and slightingly alluded to. Whether tin occurs at all in any part of the Spanish Peninsula can hardly be doubtful after the assertion of Pliny. He had been procurator in Spain, and by his intimacy with Vespasian* must be supposed in position to learn much of Britain, from the despatches of Petilius Cerealis, Ostorius Scapula, and Agricola. But he was suffocated by the fumes of Vesuvius in 79, one year after the appointment of Agricola to Britain—and for the greater part of his literary life, Britain was a scene of never-ending war and confusion. Besides this, the Cornish promontory appears to have been at no time much occupied by Roman stations, or traversed by roads; and it may be thought to have had then, as afterwards in Saxon and Norman times, a history and commerce quite distinct from and little known to the Belgic settlers in Albion. He might be mistaken respecting Britain, of which perhaps he could know only Albion; but his positive assurance of the occurrence of tin in Spain is confirmed by a passage in Bowles's Natural History of Spain, and, as I hear from Mr. Kenrick, by a later German writer (Hopfensach); it occurs, in fact, according to one of our best books of mineralogy, in beds in the mica schist of Gallicia. (W. Phillips, 1823.) Oxide of tin has been found, besides, on both sides of the Erzegebirge in granite, at Puy de Vignes (Haute Vienne), also in granite in Wicklow (granite), on the east coast of Sumatra, Siam and Pegu, and in Banca and Malacca. It has been found in Mexico, Chili and Greenland, and mixed with other matters in Finland and Sweden.

* Accessit imp. A.D. 69.

Upon the whole, the case is probably thus. It is the old Phœnician trade, destroyed with Carthage, which Strabo describes, and Pub. Crassus went to explore in the *κασσιτέριδες*. Diodorus Siculus narrates the course of trade in the days of Augustus from Ictis, when Gaul offered an easy route to the Mediterranean; but 100 years of war and commotion interrupted this trade of Cornwall with the East, and Pliny was suspicious of the fables of Greece, and knew that tin was obtained in Spain. Notwithstanding this fact, it appears that Cornwall and the Asiatic Isles have been the principal, almost the only sources of the tin of the ancient world, that of Zinnwald being quite unknown till a much later date.

Stannum is evidently an alloy of an argentine or tin-like aspect—a variable pewter—a metal more easily melted than copper, for the lining of which it was much used in Pliny's days to obviate the danger of cupreous solutions. This process we now call tinning; and stannum*, with its variable meanings, is perhaps the common parent of the French *étain*, meaning as often pewter as tin; and of the German *zinn*, which like tin in the English workshops, is used sometimes for pewter when lining vessels, and solder when covering surfaces which are to be joined. Our German silver, Britannia metal, &c. belong to this class. The process of illination with stannum must have been well-executed to justify the exclamation of Pliny, that it did not augment the weight of the vessel to which it was applied. The Brundisian specula made of it yielded to silver, indeed, at last; but they are declared to have been of admirable efficiency.

Stannum, then, is an alloy of tin with lead, tin with brass, tin with antimony, lead with silver, or other variable mixtures of metals often associated in nature.

Pliny mentions adulterate or alloyed kinds of stannum, composed of one part *white brass* to three parts of candidum plumbum; of equal weights of candidum and nigrum (which is called argentarium); of two parts of nigrum and one of candidum (called tertiarium); with this last lead pipes are soldered†. Fraudulent dealers add to the tertiarium equal parts of album, call it *argentarium*, and with it plate or line other metals.

He gives the prices of these compounds and those of pure

* Pliny's notices of stannum are frequent. See Hardouin, vol. ii. 429, 22; 528, 7; 530, 30, 31, &c.
Stanno et ære mixtis, 627, 11—illitum æneis vasis saporem gratiorem facit, 669, 14—discerni vix possit ab argento, 669, 26—æramentis jungitur, 669, 11.

† Hoc fistulæ solidantur. This is the solder of our tinmen.

album and nigrum; the former twenty, the latter seven denarii for 100 lbs.

Plumbum album, he says, is rather of an arid nature; the nigrum is entirely humid; "therefore the white is of no use unless it be mixed with another metal. Silver cannot be leaded (lined) with it, it will be melted first."...."It is affirmed that if there be too little nigrum mixed with the album, the silver will be corroded by it. Album is melted into brass-work (inlaid, an invention of Gaul), so that it can hardly be known from silver—these works are called Incoctilia" (silvered). He then speaks of the application of this invention to the trappings of horses and carriages, and other curious productions of Alesia and the Bituriges, a subject which our esteemed Kenrick has lately handled with his usual felicity. One of Pliny's sentences is remarkable as narrating a class experiment fit for a chemical school: "Plumbi albi experimentum in charta est, ut liquefactum pondere videatur, non calore, rupisse."

The meaning seems to be, that the metal is fluid at so moderate a heat as when fused to break by its weight, not burn by its heat, the charta on which it is poured. Tin melts at 440° to 442°; lead at 612°.

What follows is a very important passage: "India neque æs neque plumbum habet, gemmisque suis ac margaritis hoc permutat."

May we be justified by this sentence in refusing to credit the supposition that tin (plumbum album) was brought overland or by other routes from the Asiatic Isles and shores towards Western Europe? If so, Cornwall chiefly, if not wholly, supplied the tin which entered so many ways into the comforts and necessities during peace and war of all the nations surrounding the Mediterranean and Euxine, Baltic and German Ocean; in fact, the world, as distinctly known to the Roman geographers.

Let us now inquire into the means whereby the ancient people reduced the metals which they were so earnest in seeking across mountains and oceans at the point of the sword. To confine the inquiry within reasonable limits, we shall speak chiefly of tin and lead, the only metallic products, as it appears, which were regarded by the ancients as abundant in Britain. [Iron is mentioned by Cæsar as of limited occurrence.]

Gold, the most widely if not most abundantly distributed metal—found near the surface of the earth, in a pure and malleable state, easily fused, uninjured by fusion—was probably the metallic substance on which the earliest processes of fire were tried, and they could not be tried unsuccessfully.

Tin, the ore of which has been found at the surface in many situations with auriferous sand and gravel, cannot have been long unknown to the gold-finders of the East and the West. Some one of the many accidents which may or rather must have accompanied the melting of gold would disclose the nature of the accompanying white metal, whose brilliance, ductility, and very easy fusibility, would soon give it value.

The melting of *tin ore* is, however, a step in advance of the fusion of *native gold*. The gold was fused in a crucible (xxxiii. p. 617, Hard.) made of white clay*, which only could stand the heat and the chemical actions which that generated: but tin ore would in this way of operation prove totally infusible. It must be exposed at once to heat and a free carbonaceous element. The easiest way of managing this is to try it on the open hearth. Perhaps some accidental fire in the half-buried bivouacs of the Damnonii may have yielded the precious secret. As to the fuel, we are told that pine-woods were best for brass and iron (Hard. xxxiii. p. 621); but the Egyptian papyrus was also used, and straw was the approved fuel for gold. In the metalliferous country of Cornwall and Devon, peat is plentiful; and an order of King John (1201) allows the miners to dig tin, and turves to melt the tin, anywhere in the moors, and in the fees of bishops, abbots and earls, as they had been used and accustomed. (Confirmed by Edward I., Richard II. and Henry IV.†)

These and other singular privileges, extending as far as the lands on which the crown claimed rights, are long anterior to the other rights of property in Cornwall, Mendip, Derbyshire and the Forest of Dean, and go far to justify the supposition of our modern mining laws being a relic of Roman, or perhaps of earlier than Roman times.

As the bellows was known at least 1000 years before Pliny, we have here all the materials for a successful tin smelter's hearth. If the smelting work was on waste land, and a little sunk in the ground, we recognize the old 'bole' or 'bloomery' of Derbyshire, now only a traditional furnace, but anciently the only one for the lead and iron of that country.

Pure tin once obtained, there must intervene a long series of trials and errors before its effect in combination with lead, brass, silver, &c. could be known; before the mode of conquering the tendency to rust in the act of *soldering* could be discovered; oil being in this respect as valuable to the tinner as artificial chrysocolla was to the jeweller and goldsmith (xxxiii. p. 621. Hard.). From all this it follows that the

* Such as now is called Cornish clay, for example.

† De la Beche, in Report on Geology of Cornwall.

smelting of tin might be, and probably was, performed by the inhabitants of the Cornish peninsula. This art they may have brought from the far east; Phœnicians may have taught it them; but all the accounts of the ancient tin trade represent the metal, and not the ore, as being carried away from the Cassiterides. Diodorus mentions the weight and cubical form of the tin in blocks, carried from Ictis to Marseilles and Narbonne; and Pliny says of the Gallician tin, that it was melted on the spot.

Did the Cornish or Gallician miners make bronze? For this is generally the compound indicated by the Roman *æris metalla*, though it is undoubted that they also knew of, and distinguished zinc brass. There is, I believe, no instance of a single bit of pure tin or pure copper being found with the numerous 'celts,' which occur in so many parts of England; nor is any other proof given that the direct union of tin and copper was effected by the natives of Britain. Copper is so abundant in Cornwall that it might tempt to the other hypothesis; but this copper is a sulphuret; it is found united to the sulphuret of iron, in deep veins, and in a matrix of quartz; and these are things which render the production of pure copper one of the most refined operations in smelting. Cæsar tells us the brass used by the natives of Britain was imported. Probably Cyprus,—colonized by the Phœnicians, to which old authors refer as the original source of brass—Cyprus with its ancient copper mines (Tamassus), which has given its name to the metal, might be one of the points from which bronze radiated over the Grecian, Roman and barbarian world. It was from Cinyras, the king of Cyprus, that Agamemnon received his splendid breastplate with twenty plates of tin, and its liberal additions of turquoise, lazulite, or rather malachite, obtained perhaps from the soil of the island. (Pliny, xxxiii. p. 633, Hard.)

The works of Ἥφαιστος, the Crawshay of antiquity, may have been fixed on Lemnos on account of some volcanic appearances there; but the tradition shows at least that the various operations of refined metallurgy were not strangers to the islands of the Mediterranean; and the uniformity of design and composition in the ancient celts, chisels, *μάκελλα*, and instruments of war, implies a common, and that not a barbarous origin. The perfection and variety and great proportions of the brass work executed in the Grecian states and colonies, may also be regarded as indicating the local seat of the early as well as the later art of working in bronze.

Lead was obtained in Spain and Gaul from deep and laborious mines (xxxiv. p. 669, Hard.), but so abundantly near the surface in Britain as to suggest a law for preventing more than a limited production—a Brigantian law of vend. The Romans employed lead in pipes (fistulæ) and sheets, which were soldered with alloys, as already mentioned. This lead was previously refined, and its silver removed; the silver indeed being often the object of the enterprise. How earnestly silver was sought—how well the mining operations were carried on by the 'old men'—appears from the notice of the Carthaginian mines in Spain, the pits and levels driven by Hannibal being mentioned as in wonderful preservation by Pliny. The same may be said of at least one set of mining works of Roman date, in the extreme parts of South Wales, viz. the Gogofau near Lampeter, where gold was extracted with much labour from broken and pounded quartz, of which enormous mounds remain. The adit still exists, and was lately entered by Sir H. T. De la Beche, who found in it a specimen of native gold. In the vicinity, tradition indicates a Roman settlement; and a massive chain of gold and other remains were found, and are now possessed by the family of Johnes of Abercothi *.

The districts in Britain, where lead veins coming to the surface in abundance might justify the praises of Pliny, are, in the south, Mendip; in the west, Flintshire, &c.; in the north, Derbyshire, Yorkshire and Cumberland, that is to say, the Brigantian territory; and it is to this last district that the descriptions apply most correctly. Lead cast in Roman moulds, pigs, in fact, of the age of Hadrian and other emperors, have been found in Flintshire, Derbyshire, Yorkshire, and some other counties. But few ancient mining instruments have ever been found in the lead-bearing districts of Britain †; and I am strongly of opinion that much of the lead ore was collected from the surface by aid of water, artificially directed. The process, in fact, is described by Pliny in terms so exactly applicable to the modern 'hushes' of Swaledale, that no doubt can remain of this custom, which is now esteemed rude and semi-barbarous, being of Roman or earlier date in Britain.

As thus from Roman or earlier times our lead-mining derives its 'hush,' its levels, and shafts, implements for washing, and other processes of the workmen, and the forms, weights, and marks of its melted metal, we may easily admit a similar origin for the melting processes. Lead mostly occurs in the sulphuret, which offers no particular difficulty in the fire. By cautious roasting, its excess of sulphur may be removed, and

* See Sir R. I. Murchison's remarks on Gogofau (Sil. Syst. p. 367, 368).

† Sir R. I. Murchison mentions Roman mining utensils at Shelve in Shropshire (Sil. Syst., p. 279).

the subsequent melting with charcoal, or a flux, be facilitated. Indeed without roasting, and without flux in many cases, the lead will flow out of the ore, if placed among flaming wood or peat, and subjected to a sufficient stream of air.

But the use of fluxes could not long remain unknown in the limestone districts of Northumbria, or amid the fluoric veins of Derbyshire—limestone and fluor being to this day valuable aids in the furnace. Peat was the fuel in Cornwall, and still is in Yorkshire; and perhaps the Roman smelters did really erect their furnaces on waste ground and heaths at Dacre and Matlock, far from the mines of Greenhow and Youlgreave, even as is done at present with the cupolas of Lee and Langley mills.

The use of crucibles (χόανοι), bellows, cavities of some peculiar sort (κάμινοι), perhaps chimneys, great variety of carbonaceous fuel, the power of purifying and alloying, and knowledge of the properties of alloys, appear quite conspicuous among the ancient arts.

The inscriptions * on these masses of lead are in the same general form as the 'marks' of the different mines now in work, and which, no doubt, are their literal and lineal descendants. Thus the Ald or Auld Gang mine of Swaledale, old in the days of the Saxons; the mines of Greenhow Hill, which

* The following inscriptions have been recorded on pigs of lead obtained from British mines during the Roman sway in Britain. It will be remarked that they belong to early imperial times.

IMP. CAES. DOMITIANO. AVG. C. C. S·VII. Found at Hagshaw Moor, Dacre Pasture, near Pately Bridge, Yorkshire, in 1734.

A Roman pig of lead, weighing 126 lbs., was found on Cromford Moor, near Matlock, in the year 1777, having the following inscription in raised letters on the top:

IMP. CAES. HADRIANI. AVG. MET. LVT.

A second was discovered near Matlock in 1783. It weighed 84 lbs., and was 19 inches long at the top, and 22 at the bottom. Its width at the top was 3½ inches, and at the bottom 4½. The inscription appears to contain these letters:

L. ARVCONI. VERECVND. METAL. LVTVD.

A third, with the inscription also in raised letters on the top, was found on Matlock Moor in the year 1787. It weighs 173 lbs., and was 17½ inches in length, and at bottom 20½.

TI. CL. TR. LVT. BR. EX. ARG.

Glover's Derbyshire, vol. i. p. 71, 72.

A fourth is stated to have been found at Castleton, on which only the letters IMP could be read distinctly. It was said by Mr. Mawe to be preserved in the museum of Mr. Green at Lichfield.

Sir R. I. Murchison records a Roman pig of lead (from the Shelve mines in Shropshire probably), bearing the inscription, IMP. ADRIANI. AVG. (Sil. Syst., p. 279.) This pig is said to be unlike the modern pig.

supplied sheet and pipe lead for our baths and coffins at York, as well as tribute to the imperial treasury; the mines of Middleton and Youlgreave (Aldgroove), from which the Lutudæ sent not only lead, but 'exargentate' (that is to say refined) lead from which the silver had been removed, use to this day the pig of the same weight of $1\frac{1}{2}$ cwt., of similar shape and similar mark to that of 1800 years antiquity*. And just as at the present day, the countryman whose galloway is tired drops the leaden load by the way side, for another day's work, so in the days of Rome, the Brigantian lead was thrown down from the tired caballus by the side of the ancient mining road, on Matlock Moor in Derbyshire, and Dacre Pasture in Yorkshire.

This fact of the discovery of the Roman lead, *not at the mines*, but at a distance of some miles from them on a track leading *towards* a Roman or rather a Pre-Roman station, is of much importance in archæology. For thus we arrive, in the first place, at the conviction of the existence of very ancient mining roads not of Roman work, nor probably of Roman but of earlier date, leading toward Cataractonium, Isurium, Eburacum, Mancunium, Derventio, or rather to the Brigantian towns or centres of trade, on which the Romans following their wont in Africa, Spain, and Gaul, fixed their attention and established their war camps and their colonies. The politic lords of the world broke up no national industry, set no legionaries to supplant the native miners, but stationing a few cohorts on the ancient roads, in or close to the mining district, as at Hope and Bainbridge, to control a rude population, received regularly the fruits of the industry which they might direct, but did not personally share. Viewed in this light, how complete appears the grasp of the Roman treasury on the mining fields of Britain! The Fossway from the Ocrynian promontory crosses the Mendip Hills—the road from Mancunium to Bremetonacum traverses the Calamine district of Bowland—the road from Derventio or Tutbury to Mancunium runs along the west of the great Derbyshire field, and the legionary path from Carlisle to York goes right across the metalliferous country of Yorkshire and Durham.

We may even ask, with some confidence, whether the line of the Hadrian wall, which cuts off from the north all the richest mines of the Derwent, the Allen, and the Tyne, but abandons the mossy dales of bleak Northumbria, was not

* The modern pig is made near to $\frac{2820}{16}$ of a fodder or $176\frac{1}{4}$ lbs. Three Roman pigs found near Matlock in 1777, 1783, 1787, weighed 173, 126, and 84 lbs., these being as 1, $\frac{3}{4}$, and $\frac{1}{2}$ of the modern pig.

drawn with especial reference to the mining wealth of the districts.

May we not regard, as a confirmation of all that has been advanced touching the antiquity of our mining processes, the fact of the existence to this day, though impaired by recent acts of parliament, of peculiar rights and privileges in the mining districts? These rights are sometimes guaranteed by and appear to emanate from royal charters, as in the stanneries of Cornwall and Devon, but they are probably of far earlier date, and have merely been confirmed as old customs by John and his successors. In Mendip, the Forest of Dean, and Derbyshire, the miners' rights were preserved by royal officers, but the rights themselves transcend all history and tradition. To sink a pit or drive a level in any field; to cover the rich herbage with barren ore-stuff; to cut a way to the public road; to divert, employ, and waste the running waters; and to do all this without consent of owner, and without compensation being so much as asked by lord or villein, landlord or tenant, implies in Derbyshire a settlement of mining rights long anterior to Domesday Book, the charters of Repton Abbey*, the neighing of the Saxon horse, and the flight of the Roman eagle. In connection with all that has been mentioned before,—the furnaces, the roads, the restricted vend, the foreign trade—they seem to me to indicate a people who came with many inventions from the metalliferous east to the metalliferous west, before the Athenians drew silver from Laurion, or the Carthaginians from Iberia.

To these ancient, these Semitic mining processes we have added perhaps steel instruments, and certainly explosive agents; the ore-hearth still remains, but it is generally yielding to the reverberatory furnace; silver is no longer obtained

* The mines in the neighbourhood of Wirksworth were wrought before the year 714; at which period that district belonged to the nunnery at Repton, over which Eadburga, the daughter of Adulph, king of the East Angles, presided as abbess. In that year the abbess sent to Croyland, in Lincolnshire, for the interment of St. Guthlac, who was originally a monk of Repton, a sarcophagus of lead lined with linen (plumbum lintheumque). This lead was obtained from the possessions of the old Saxon religious establishments at Repton, part of which were the mines near Wirksworth. In the year 835, Kenawara, then abbess of the same nunnery, made a grant to Humbert, the alderman, in which she surrenders that estate of mines, called Wircesworth, on condition that he gives annually, as a rent to Archbishop Ceolnoth, lead to the value of 300 shillings, for the use of Christ's Church, Canterbury. On the destruction of the religious houses by the Danes in 874, it is probable that the lead mines became the property of the Crown. As such they are mentioned in Domesday Book.—Glover's Derbyshire, vol. i. p. 73.

by oxidation of some thousand times its weight of lead; steam blows our furnace fires, rolls and pipes our metals, and flies with iron wings on roads more solid than the Appian Way. The world of George Stephenson is much different from that of Julius Agricola; but some features of the past remain to connect the earliest with the latest aspect of our country: and among these the least altered, and the most instructive, appear to be the mineral products and the mining processes. If by these we judge the great Brigantian tribes which surrounded Isurium, they must be placed far higher on the scale of civilization than the place usually accorded by the Saxon to the Celt.

I presume to think, indeed, that without full attention to the mining history of Britain, as indicated by fragments in classic authors, and illustrated by processes not yet extinct, the opinion which may be formed of the ancient British people would be altogether conjectural, derogatory, and erroneous.

XXXVI. *On the Determination of the Coefficients in any series of Sines and Cosines of Multiples of a variable angle from particular values of that series. By the* Rev. Brice Bronwin *.

M. LE VERRIER'S method of determining the coefficients of a series of sines and cosines of the multiples of a variable angle requires a very great amount of labour. Moreover his formulæ contain large factorials of sines in their denominators, which endangers the accuracy of the results. The method of Mechanical Quadratures, strictly so called, is attended with some uncertainty; because each term of the correction may vanish, while the sum of all of them to infinity may have a finite value, owing to the initial and terminal series of differences being divergent. Some more suitable method is therefore wanted. Sir J. W. Lubbock, in some tables which he very kindly sent me a short time ago, has inserted one given by Bessel and Hansen in the *Astronomische Nachrichten*, which is perhaps as good a method as we have reason to expect. In what way the authors arrived at it I do not know. As it is important, and may be found in a very simple manner, perhaps the investigation may not be unacceptable. Besides, the formulæ given by them, as printed in the tables above-mentioned, are only one out of several distinct sets, and are not well adapted to all cases; and moreover they may be modified and simplified.

* Communicated by the Author.

$$\left.\begin{aligned}&\cos\theta+\cos(\theta+h)+\cos(\theta+2h)+\dots+\cos(\theta+(n-1)h)\\&\quad=\cos\left(\theta+(n-1)\frac{h}{2}\right)\frac{\sin\frac{nh}{2}}{\sin\frac{h}{2}}\\&\sin\theta+\sin(\theta+h)+\sin(\theta+2h)+\dots+\sin(\theta+(n-1)h)\\&\quad=\sin\left(\theta+(n-1)\frac{h}{2}\right)\frac{\sin\frac{nh}{2}}{\sin\frac{h}{2}}\end{aligned}\right\}\quad\text{(a.)}$$

are formulæ which will be needed; and we will begin with

$$\varphi(x)=B_0+B_1\cos x+B_2\cos 2x+B_3\cos 3x+\quad . \quad . \quad \text{(A.)}$$

$$\varphi(\alpha)=B_0+B_1\cos\alpha+B_2\cos 2\alpha+B_3\cos 3\alpha+\dots\dots$$
$$\varphi(\alpha+k)=B_0+B_1\cos(\alpha+k)+B_2\cos 2(\alpha+k)+B_3\cos 3(\alpha+k)+\dots$$
$$\varphi(\alpha+2k)=B_0+B_1\cos(\alpha+2k)+B_2\cos 2(\alpha+2k)+B_3\cos 3(\alpha+2k)+\dots$$
$$. \quad . \quad . \quad . \quad . \quad .$$
$$\varphi(\alpha+(n-1)k)=B_0+B_1\cos(\alpha+(n-1)k)+B_2\cos 2(\alpha+(n-1)k)+B_3\cos 3(\alpha+(n-1)k)+\dots$$

Adding these together by summing the terms vertically by means of the first of (a.), putting $i\alpha$ for θ and ik for h, we have

$$\Sigma\varphi(\alpha+i'k)=nB_0+\Sigma B_i\cos i\left(\alpha+(n-1)\frac{k}{2}\right)\frac{\sin\frac{nik}{2}}{\sin\frac{ik}{2}}\quad . \quad . \quad \text{(1.)}$$

Let B_m be the last coefficient which is sensible, then i will represent all the numbers 1, 2, 3, ... m. In order that all the quantities B_i may vanish, we must have $\frac{nk}{2}=\pi$, or $k=\frac{2\pi}{n}$. Then

$$\sin\frac{nik}{2}=\sin i\pi=0;$$

but

$$\sin\frac{ik}{2}=\sin\frac{i\pi}{n}$$

is not nothing if $n>m$, or greater than the greatest value of i.

If $k=2\alpha$,

$$\cos i\left(\alpha+(n-1)\frac{k}{2}\right)=\cos in\alpha,\quad \sin\frac{ink}{2}=\sin in\alpha;$$

and (1.) will reduce to

$$\Sigma\phi\{(2i'+1)\alpha\}=n\mathrm{B}_0+\Sigma\mathrm{B}_i\frac{\sin 2in\alpha}{2\sin i\alpha}. \quad . \quad . \quad (2.)$$

If $\alpha=\frac{\pi}{2n}$ and $2n>m$, $\sin 2in\alpha=0$; but $\sin i\alpha$ is not nothing. Therefore

$$\mathrm{B}_0=\frac{1}{n}\Sigma\phi\left\{\frac{(2i'+1)\pi}{2n}\right\},\ 2n>m, \quad . \quad . \quad . \quad . \quad (3.)$$

where i' has all the values 0, 1, 2, ... $n-1$. This value of B_0 requires a value of n only half as large as the supposition $k=\frac{2\pi}{n}$ would require.

In order to find B_i, multiply the first of the assumed equations by $2\cos i\alpha$, the second by $2\cos i(\alpha+k)$, the third by $2\cos i(\alpha+2k)$, &c., and sum as before. Any coefficient B_p will now be multiplied by

$$2\cos p\alpha\cos i\alpha=\cos(p-i)\alpha+\cos(p+i)\alpha,\quad 2\cos p(\alpha+k)\cos i(\alpha+k)=\cos(p-i)(\alpha+k)+\cos(p+i)(\alpha+k),\ \&c.;$$

which will constitute two series, the sums of which must be separately taken. We shall thus have

$$2\Sigma\phi(\alpha+i'k)\cos i(\alpha+i'k)=2\mathrm{B}_0\cos i\left(\alpha+(n-1)\frac{k}{2}\right)\frac{\sin\frac{ink}{2}}{\sin\frac{ik}{2}}$$

$$+\Sigma\mathrm{B}_p\cos(p-i)\left(\alpha+(n-1)\frac{k}{2}\right)\frac{\sin(p-i)\frac{nk}{2}}{\sin(p-i)\frac{k}{2}}$$

$$+\Sigma\mathrm{B}_p\cos(p+i)\left(\alpha+(n-1)\frac{k}{2}\right)\frac{\sin(p+i)\frac{nk}{2}}{\sin(p+i)\frac{k}{2}} \quad . \quad . \quad (4.)$$

But when $p=i$, the coefficient of B_i will be

$$n+\cos i(2\alpha+(n-1)k)\frac{\sin ink}{\sin ik}.$$

We may give to p all the values 1, 2, ... m, except i; and if $k=\frac{2\pi}{n}$,

$$\sin(p-i)\frac{nk}{2}=0, \quad \sin(p+i)\frac{nk}{2}=0;$$

but

$$\sin(p-i)\frac{k}{2}, \quad \sin(p+i)\frac{k}{2}$$

are not nothing if $n>2m$. Thus the coefficients of B_0 and B_p will vanish. Also $\sin ink=0$; but $\sin ik$ is not nothing, and the coefficient of B_i reduces to n.

But make $k=2\alpha$, and (4.) becomes

$$\left.\begin{array}{l} 2\Sigma\varphi\{(2i'+1)\alpha\}\cos i(2i'+1)\alpha=B_0\dfrac{\sin 2in\alpha}{\sin i\alpha}+\Sigma B_p \\ \left\{\dfrac{\sin(p-i)2n\alpha}{2\sin(p-i)\alpha}+\dfrac{\sin(p+i)2n\alpha}{2\sin(p+i)\alpha}\right\} \end{array}\right\} \quad (5.)$$

When $p=i$, the coefficient of B_i is

$$n+\frac{\sin 4in\alpha}{2\sin 2i\alpha}.$$

Make $\alpha=\frac{\pi}{2n}$, $n>m$, and the coefficients of B_0 and B_p will vanish; and that of B_i reduces to n. Thus we shall have

$$n>m, \quad B_i=\frac{2}{n}\Sigma\varphi\left\{(2i'+1)\frac{\pi}{2n}\right\}\cos i(2i'+1)\frac{\pi}{2n}, \quad (6.)$$

where i' has all the values 0, 1, 2, ... $n-1$.

If we had not restricted the value of n, we should have had a series of terms, as B_{n-i}, B_n, B_{n+i}, &c. when $k=\frac{2\pi}{n}$; and the series B_{2n-i}, B_{2n}, B_{2n+i} when $k=2\alpha=\frac{\pi}{n}$.

The formulæ (3.) and (6.), making $n>m$ in both, enable us to determine all the coefficients of (A.) But

$$\cos(2n-1)\frac{i\pi}{2n}=\pm\cos\frac{i\pi}{2n}, \quad \cos(2n-3)\frac{i\pi}{2n}=\pm\cos\frac{3i\pi}{2n},$$

&c. to

$$\pm\cos\frac{(n-1)i\pi}{2n},$$

if n be even, which it will always be convenient to suppose. Therefore

$$\Sigma\varphi\left\{(2i'+1)\frac{\pi}{2n}\right\}\cos(2i'+1)\frac{i\pi}{2n}=\left\{\varphi\left(\frac{\pi}{2n}\right)\pm\varphi\left(\frac{(n-1)\pi}{2n}\right)\right\}$$

$$\cos\frac{i\pi}{2n}+\left\{\varphi\left(\frac{3\pi}{2n}\right)\pm\varphi\left(\frac{(2n-3)\pi}{2n}\right)\right\}\cos\frac{3i\pi}{2n}+\ldots.$$

$$+\left\{\varphi\left(\frac{(n-1)\pi}{2n}\right)\pm\varphi\left(\frac{(n+1)\pi}{2n}\right)\right\}\cos\frac{(n-1)i\pi}{2n}.$$

Hence if we put u_1, u_2, &c. for the sums of the first and last, of the second and last but one, &c. of the particular values of $\varphi(x)$, and in like manner v_1, v_2, &c. for the differences of the same, we may replace (3.) and (6.) by

$$\left.\begin{aligned} B_0 &= \frac{1}{n}\left(u_1 + u_2 + \dots + u_{\frac{n}{2}}\right), \\ B_i &= \frac{2}{n}\left(u_1 \cos\frac{i\pi}{2n} + u_2 \cos\frac{3i\pi}{2n} + \dots + u_{\frac{n}{2}} \cos\frac{(n-1)i\pi}{2n}\right), \\ &\quad \text{if } i \text{ be even;} \\ B_i &= \frac{2}{n}\left(v_1 \cos\frac{i\pi}{2n} + v_2 \cos\frac{3i\pi}{2n} + \dots + v_{\frac{n}{2}} \cos\frac{(n-1)i\pi}{2n}\right), \\ &\quad \text{if } i \text{ be odd.} \end{aligned}\right\} \quad (7.)$$

This will diminish the labour of numerical computation considerably; and when we know the value of n, we may for many values of i effect a further reduction.

Now let

$$\psi(x) = A_1 \sin x + A_2 \sin 2x + A_3 \sin 3x + \dots \quad . \quad . \quad (B.)$$

$$\psi(\alpha) = A_1 \sin \alpha + A_2 \sin 2\alpha + A_3 \sin 3\alpha + \dots$$

$$\psi(\alpha+k) = A_1 \sin(\alpha+k) + A_2 \sin 2(\alpha+k) + A_3 \sin 3(\alpha+k) + \dots$$

$$\psi(\alpha+2k) = A_1 \sin(\alpha+2k) + A_2 \sin 2(\alpha+2k) + A_3 \sin 3(\alpha+2k) + \dots$$

. . . . .

$$\psi(\alpha+(n-1)k) = A_1 \sin(\alpha+(n-1)k) + A_2 \sin 2(\alpha+(n-1)k) + A_3 \sin 3(\alpha+(n-1)k) + \dots$$

Multiply the first of these by $2 \sin i\alpha$, the second by $2 \sin i(\alpha+k)$, the third by $2 \sin i(\alpha+2k)$, &c. and sum as before. The coefficient of A_p will contain the terms

$$2 \sin p\alpha \sin i\alpha = \cos(p-i)\alpha - \cos(p+i)\alpha,$$

$$2 \sin p(\alpha+k) \sin i(\alpha+k) = \cos(p-i)(\alpha+k) - \cos(p+i)(\alpha+k),$$

&c., and therefore

$$2\Sigma\psi(\alpha+i'k) \sin i(\alpha+i'k) = \Sigma A_p \cos(p-i)\left(\alpha+(n-1)\frac{k}{2}\right) \frac{\sin(p-i)\frac{nk}{2}}{\sin(p-i)\frac{k}{2}} - \Sigma A_p \cos(p+i)\left(\alpha+(n-1)\frac{k}{2}\right) \frac{\sin(p+i)\frac{nk}{2}}{\sin(p+i)\frac{k}{2}} \quad (8.)$$

But when $p=i$, the coefficient of A_i is

$$n-\cos i\left(2\alpha+(n-1)k\right)\frac{\sin ink}{\sin ik}.$$

Suppose A_m the last coefficient which is sensible, and let p have all the values $1, 2, 3, \ldots m$ except i. As the coefficients of A_p and A_i differ from those found in (4.) only in some of their signs, the same conclusions result from them when we make $k=\frac{2\pi}{n}$, and $k=2\alpha$. Therefore, passing by the formulæ derived from $k=\frac{2\pi}{n}$, as we have before done, and taking only that which results from $\frac{k}{2}=\alpha=\frac{\pi}{2n}$, we have

$$A_i=\frac{2}{n}\,\Sigma\psi\left\{(2i'+1)\frac{\pi}{2n}\right\}\sin(2i'+1)\frac{i\pi}{2n},\ n>m, \quad . \quad (9.)$$

where i' has the same values as before.

This may be reduced as (6.) was. For

$$\sin(2n-1)\frac{i\pi}{2n}=\mp\sin\frac{i\pi}{2n},\quad \sin(2n-3)\frac{i\pi}{2n}=\mp\sin\frac{3i\pi}{2n}, \&c.$$

If therefore we make ϖ_1, ϖ_2, &c. the sums of the first and last, of the second and last but one, &c. of the particular values of $\psi(x)$, and t_1, t_2, &c. the differences of the same, (9.) may be replaced by

$$\left.\begin{aligned}
A_i&=\frac{2}{n}\left(\varpi_1\sin\frac{i\pi}{2n}+\varpi_2\sin\frac{3i\pi}{2n}+\ldots+\varpi_{\frac{n}{2}}\sin\frac{(n-1)i\pi}{2n}\right.\\
&\text{or}\\
A_i&=\frac{2}{n}\left(t_1\sin\frac{i\pi}{2n}+t_2\sin\frac{3i\pi}{2n}+\ldots+t_{\frac{n}{2}}\sin\frac{(n-1)i\pi}{2n}\right.
\end{aligned}\right\}(10.)$$

the first when i is odd, the second when it is even.

We now proceed to the more general form,

$$\left.\begin{aligned}f(x)=B_0+B_1\cos x+B_2\cos 2x+\ldots\\ +A_1\sin x+A_2\sin 2x+\ldots\end{aligned}\right\}\quad . \quad . \quad (C.)$$

The solution of this form might be derived from those of the two forms before treated, by taking that particular case in which $k=\frac{2\pi}{n}$; but I prefer treating it separately.

$$\begin{aligned}
f(\alpha)&=B_0+B_1\cos\alpha+B_2\cos 2a+\ldots\\
&\quad+A_1\sin\alpha+A_2\sin 2\alpha+\ldots\\
f(\alpha+k)&=B_0+B_1\cos(\alpha+k)+B_2\cos 2(\alpha+k)+\ldots\\
&\quad+A_1\sin(\alpha+k)+A_2\sin 2(\alpha+k)+\ldots
\end{aligned}$$

$$f(\alpha+2k)=B_0+B_1\cos(\alpha+2k)+B_2\cos 2(\alpha+2k)+\ldots$$
$$+A_1\sin(\alpha+2k)+A_2\sin 2(\alpha+2k)+\ldots$$

. . . . . .

$$f(\alpha+(n-1)k)=B_0+B_1\cos(\alpha+(n-1)k)+B_2\cos 2(\alpha+(n-1)k)+\ldots$$
$$+A_1\sin(\alpha+(n-1)k)+A_2\sin 2(\alpha+(n-1)k)+\ldots$$

Let B_m or A_m be the last of the coefficients which is sensible. Then if $k=\frac{2\pi}{n}$, taking the sum of these as before, the terms containing the cosines all vanish, as we have found from (1.), and the general term of the sines is

$$A_i\sin i\left(\alpha+(n-1)\frac{k}{2}\right)\frac{\sin\frac{ink}{2}}{\sin\frac{ik}{2}};$$

which vanishes for the same reason, having the same vanishing factor. Therefore

$$B_0=\frac{1}{n}\Sigma f\left(\alpha+\frac{2i'\pi}{n}\right),\ n>m. \quad . \quad . \quad . \quad (11.)$$

Now multiply the first by $2\cos i\alpha$, the second by $2\cos i(\alpha+k)$, the third by $2\cos i(\alpha+2k)$, &c., and sum. The part of this sum depending on the cosines is given in (4.); and the result, when $k=\frac{2\pi}{n}$, is the same as there found in the same case. The coefficients of A_p will be

$$\sin(p-i)\alpha+\sin(p+i)\alpha,$$
$$\sin(p-i)(\alpha+k)+\sin(p+i)(\alpha+k),\ \&c.;$$

and their sum

$$\sin(p-i)\left(\alpha+(n-1)\frac{k}{2}\right)\frac{\sin(p-i)\frac{nk}{2}}{\sin(p-i)\frac{k}{2}}+\sin(p+i)\left(\alpha+(n-1)\frac{k}{2}\right)\frac{\sin(p+i)\frac{nk}{2}}{\sin(p+i)\frac{k}{2}}.$$

The coefficient of A_i will be

$$\sin 2i\alpha+\sin 2i(\alpha+k)+\ldots=\sin i(2\alpha+(n-1)k)\frac{\sin ink}{\sin ik}.$$

As the vanishing factors here are the same as in (4.), these all vanish when $n > 2m$. Therefore

$$B_i = \frac{2}{n}\Sigma f\left(\alpha + \frac{2i'\pi}{n}\right)\cos i\left(\alpha + \frac{2i'\pi}{n}\right),\ n > 2m. \quad (12.)$$

Again, multiplying the first by $2\sin i\alpha$, the second by $2\sin i(\alpha+k)$, the third by $2\sin i(\alpha+2k)$, &c. and summing, it is obvious that the coefficient of B_p will be had by changing $p-i$ into $i-p$ in that of A_p last found, and therefore it will vanish under the circumstances supposed. The coefficient of B_i will be the same as that of A_i, and consequently will vanish also. It is to be remembered, that we do not give to p the value $p=i$, as we always find the terms depending on this value separately. The coefficient of B_0 will be

$$2\sin i\left(\alpha+(n-1)\frac{k}{2}\right)\frac{\sin\frac{ink}{2}}{\sin\frac{ik}{2}},$$

and will therefore vanish. And it is obvious that the coefficients of A_p and A_i will be the same as those found in (8.), and will be 0 and n respectively. Therefore

$$A_i = \frac{2}{n}\Sigma f\left(\alpha + \frac{2i'\pi}{n}\right)\sin i\left(\alpha + \frac{2i'\pi}{n}\right),\ n > 2m. \quad (13.)$$

In (11.), (12.) and (13.), i' has the same values as in the other forms, namely 0, 1, 2, ... $n-1$; and these three formulæ give all the coefficients of (C.). It will be evident that we may always suppose n as large as we please, and therefore that $n > 2m$ in (11.).

If $\alpha=0$, $i'=0, 1, \dots n-1$, or if $\alpha=\frac{2\pi}{n}$, $i'=1, 2, \dots n$, the two sets of formulæ coincide. It will be better to make the latter supposition, then we have

$$\left.\begin{aligned} B_0 &= \frac{1}{n}\Sigma f\left(\frac{2i'\pi}{n}\right),\quad B_i = \frac{2}{n}\Sigma f\left(\frac{2i'\pi}{n}\right)\cos i\left(\frac{2i'\pi}{n}\right), \\ A_i &= \frac{2}{n}\Sigma f\left(\frac{2i'\pi}{n}\right)\sin i\left(\frac{2i'\pi}{n}\right),\ i'=1, 2, \dots n \end{aligned}\right\}. \quad (14.)$$

We may derive from these others similar to (7.) and (10.).

If $\alpha=\frac{\pi}{n}$, $i'=0, 1, 2 \dots n-1$, $n > 2m$,

$$\left.\begin{array}{l} B_0=\frac{1}{n}\Sigma f\left(\frac{(2i'+1)\pi}{n}\right), \quad B_i=\frac{2}{n}\Sigma f\left(\frac{(2i'+1)\pi}{n}\right) \\ \qquad \cos i\left(\frac{(2i'+1)\pi}{n}\right), \quad A_i=\frac{2}{n}\Sigma f\left(\frac{(2i'+1)\pi}{n}\right) \\ \qquad \sin i\left(\frac{(2i'+1)\pi}{n}\right). \end{array}\right\} \quad (15.)$$

It will always be convenient to make n divisible by 2, or even by 4, 6, 8, &c. And thus we shall have

$$\cos(2n-1)\frac{i\pi}{n}=\cos\frac{i\pi}{n}, \quad \cos(2n-3)\frac{i\pi}{n}=\cos\frac{3i\pi}{n}, \text{ \&c.};$$

$$\sin(2n-1)\frac{i\pi}{n}=-\sin\frac{i\pi}{n}, \quad \sin(2n-3)\frac{i\pi}{n}=-\sin\frac{3i\pi}{n}, \text{ \&c.}$$

Make

$$u_1=f\left(\frac{\pi}{n}\right)+f\left(\frac{(2n-1)\pi}{n}\right), \quad u_2=f\left(\frac{3\pi}{n}\right)+f\left(\frac{(2n-3)\pi}{n}\right), \text{ \&c.};$$

$$v_1=f\left(\frac{\pi}{n}\right)-f\left(\frac{(2n-1)\pi}{n}\right), \quad v_2=f\left(\frac{3\pi}{n}\right)-f\left(\frac{(2n-3)\pi}{n}\right), \text{ \&c.}$$

We may therefore replace (15.) by

$$\left.\begin{array}{l} B_0=\frac{1}{n}\left(u_1+u_2+\ldots\ldots+u_{\frac{n}{2}}\right), \\ B_i=\frac{2}{n}\left\{u_1\cos\frac{i\pi}{n}+u_2\cos\frac{3i\pi}{n}+\ldots\ldots+u_{\frac{n}{2}}\cos\frac{(n-1)i\pi}{n}\right\}, \\ A_i=\frac{2}{n}\left\{v_1\sin\frac{i\pi}{n}+v_2\sin\frac{3i\pi}{n}+\ldots\ldots+v_{\frac{n}{2}}\sin\frac{(n-1)i\pi}{n}\right\}. \end{array}\right\} \quad (16.)$$

The only fault of this method of finding the coefficients is that it requires a large value of n, and consequently a large number of particular values of the functions $\phi(x)$, &c. But if we give it a smaller value, the quantities

$$\sin(p-i)\frac{k}{2}, \quad \sin(p+i)\frac{k}{2}, \quad \sin ik, \quad \sin 2i\alpha, \text{ \&c.},$$

would vanish for certain values of p and i; which would introduce the terms B_{n-i}, B_n, B_{n+i}, A_{n-i}, A_{n+i}, &c.; and we should have more unknown quantities than one in an equation, and could not determine them without employing some other means. In certain cases where the form of $\phi(x)$, &c. is given and simple, and we can ascertain with certainty the correction, the method of Mechanical Quadratures, properly so called, may require less labour, and may therefore be preferable.

Gunthwaite Hall, near Barnsley, Yorkshire,
March 7, 1849.

XXXVII. *On some phænomena of Binocular Vision.* By MM. L. Foucault *and* J. Regnault*.

IN a beautiful investigation on the vision of objects of three dimensions, Mr. Wheatstone† states that when two visual fields, or the corresponding elements of the two retinæ, simultaneously receive impressions from rays of different refrangibility, no perception of mixed colours is produced. The assertion of this able philosopher being opposed to the opinion of the majority of those who have attended to the same subject, we have thought it useful to repeat, modify, and extend these experiments. The stereoscope of Mr. Wheatstone offered a simple means of disentangling these delicate observations of all complication capable of injuriously affecting the accuracy of the physiological results.

The recomposition of mixed tints by means of vibrations produced on the retinæ by different coloured rays is beyond doubt. But the aptness varies in a remarkable manner in different individuals; it is possible that it may be exceedingly weak in some persons, and exceptionally null in others.

The tendency of one of the eyes to become inattentive in this kind of experiment is very remarkable when the whole extent of the visual fields is uniformly lighted up by different coloured rays. If we cause an impression to be made on limited and corresponding parts of the retinæ, the power of the attention constantly favours the recomposition. If two coloured rays susceptible, on reaching a white screen, of producing a mixed tint produce the same sensation when acting separately on the corresponding portions of the retinæ, it seems probable that two complementary rays will produce the sensation of white by affecting the corresponding elements of the sensitive membrane.

To prove this recomposition with respect to a great number of complementary tints, and present the phænomenon in all its clearness, we arranged the following experiment:—We affixed to the stereoscope two plane mirrors, forming a variable dihedral angle, the vertical edge of which is placed symmetrically in relation to that of the two glasses of the stereoscope. The vertical uprights bearing the grooves for the purpose of introducing the images are perforated by two large circular apertures. In the grooves are placed two glasses, on which are pasted two circular screens of white paper of the same size, and of a diameter less than that of the apertures. Two large luminous rays of complementary tints, obtained by chromatic polarization, are directed horizontally upon the plane mirrors

* From the *Comptes Rendus* for Jan. 15, 1849.

† Philosophical Transactions, part 2, 1838.

which reflect them; they traverse the glasses of the grooves which remain dark; but when reflected irregularly on the circular screens they give two coloured discs, exactly identical as to form and extent, which become the images conveyed by the stereoscope to the corresponding elements of the retinæ. We might, by means of an appropriate disposition of the polarizing apparatus, successively present numerous complementary tints, vary at the same time the intensity of the two coloured images, and modify the intensity of one or other of the images separately.

The following are the physiological results we have observed. When the corresponding elements of the retinæ receive an impression at the same time, the alternations of activity or inertness of one of the eyes is generally manifested at the commencement of the experiment, and sometimes one of the tints is perceived, and at other times its complementary one; but after a duration of observation, varying considerably according to the individuals, only a single white circle is seen.

When the eyes are in some degree accustomed to this unusual mode of impression, the tendency to recomposition becomes so energetic in some persons, that the screens might present successively all the complementary tints which the apparatus furnishes without there being any sensation corresponding to the colours; only white light is seen.

On diminishing the intensity of one of the colours, the other remaining constant, recomposition still takes place; but the white disc appears to become covered more or less strongly with the predominant tint.

If the intensity of the complementary rays is varied in the same manner for the two collections of rays, the recomposition is made with greater facility at the commencement of the observation, as their intensity is more moderated.

Of the complementary rays which we have examined, the sensible blue and the yellow tints are best adapted for the experiment, and immediately furnish the sensation of white.

We believe that this phænomenon is owing to the circumstance, that the accommodation of the eyes being the same for these groups of rays, according to the portions of the spectrum which they occupy, the efforts necessary to produce recomposition are on that account considerably less.

We find that, saving exceptional cases, the sensation of white light may be produced by any two complementary chromatic impressions in each of the eyes; that the sensation solely of white arising from two complementary rays is independent of any mutual action of these rays externally to the visual apparatus; that the luminous impressions produced on the retinæ retain their properties even to the innermost recesses of the brain.

XXXVIII. *On the Meteorology of England in the year* 1847. *By* James Glaisher, *Esq., of the Royal Observatory, Greenwich* *.

THE meteorological returns for the year 1847 furnished to the Registrar-General were from about thirty different stations, situated between the latitudes of $51\frac{1}{2}°$ and 55°, and between the longitudes of $5\frac{1}{4}°$ W. and $1\frac{1}{4}°$ E. of Greenwich. The elevations of the different places varied from 30 feet to 350 feet above the level of the sea.

The monthly returns in each quarter were published in their respective quarterly reports, but only the monthly values corresponding to the times of the observations, and not those showing the mean values for the month; these were reduced to mean values for the formation of the quarterly tables; but the corrections used in their reduction were those deduced from three years' observations only; since that time I have formed tables from the five years' Greenwich observations ending December 31, 1845†, and have reduced the observations again by these values. The true monthly values thus found will probably appear in a future Annual Report of the Registrar-General.

On discussing the true monthly values, it was found that the temperature of the air decreased from the month of January to that of February at all places south of the latitude 52°; that at places situated near this parallel the temperatures of these two months were nearly the same, and that north of this parallel there was an increase of temperature from January to February. Therefore the coldest month in the year, at places whose latitude was less than 52°, was February, and at places north of this parallel was January. The hottest month at all places was July. By taking the monthly means from the mean annual temperature, the annual variation is shown. It was found to be a single progression, having one ascending branch and one descending branch, being nearly identical with that shown in the tension of vapour. This evidence is conclusive upon the dependence of the monthly march of the vapour upon the temperature; each element has one ascending branch and one descending branch, and the march is harmonious.

The only other circumstance with respect to temperature to which I need allude, is the fact, that whilst the decrease of temperature month by month proceeded regularly in the

* Communicated by the Author.

† See Philosophical Transactions, part i. 1848.

counties of Cornwall and Devonshire, and also, though at a less rapid rate, in the northern latitudes, the temperatures of September and October at intermediate places were nearly of the same value.

It was found that the months from March to July were less humid than the average for the year, and that the remaining months were more humid than the yearly average. The months of March to July are those distinguished by the temperature of the air increasing, and those from August to January or February by a decreasing or stationary temperature. The places situated in the counties of Cornwall and Devonshire were less humid than elsewhere; for notwithstanding the greater amount of vapour contained in the same mass of air in those counties to that in other places, yet the temperature increased more rapidly than the air received the addition to its vapour necessary to keep an equal degree of humidity.

The following Table contains the mean of all the monthly values of each element:—

From the numbers in this table the following values have been deduced for the year 1847:—

The mean pressure of the atmosphere of dry air at the level of the sea was 29·641 inches. This value applies to all parts of the country.

The mean pressure of the atmosphere of vapour in latitude 51° 30′ was 0·319 inch; and this value seems to diminish 0·010 inch for an increase of 1° in latitude.

The mean temperature of the air at the level of the sea in latitude 51° 30′ was 50° 0′; and this value at a uniform level was found to vary 1° very nearly for a change of 1° of latitude.

No certain law can be deduced from the observations of 1847, representing the excess of the temperature of the air above those of evaporation and dew-point; depending upon the difference of latitude, it seems however that the excess becomes smaller as we proceed north; but the observations of the wet-bulb thermometer during the early part of the year 1847 were unsatisfactory in many places, and the following is all the information I can give in this respect:—

The mean excess of temperature of the air above that of evaporation was 3°·0, and above that of the dew-point was 5°·6, in the counties of Cornwall and Devonshire;

The mean excess of temperature of the air above that of evaporation was 2°·2, and above that of the dew-point was 4°·7, at places situated in the vicinity of the sea;

The mean excess of temperature of the air above that of

Meteorological Table for the Year 1847.

Name of place.	Reading of barometer.	Elastic force of vapour.	Pressure of dry air.	Temperature of air. From self-registering thermometer.	Temperature of air. From dry thermometer.	Mean temperature of Evaporation.	Mean temperature of Air above that of evaporation.	Mean temperature of Air above that of dew-point.	Mean temperature of Dew-point.	Temperature of air. Highest.	Temperature of air. Lowest.	Temperature of air. Range in year.	Temperature of air. Mean monthly range.	Temperature of air. Mean daily range.	Wind. Average strength.	Wind. General direction.	Amount of cloud.	Rain. Number of days it fell.	Rain. Amount fallen.	Deductions relative to humidity. Weight of vapour in a cubic foot of air.	Deductions relative to humidity. Additional weight to saturate a cubic foot of air.	Deductions relative to humidity. Degree of humidity.	Deductions relative to humidity. Weight of a cubic foot of air.	Height above the level of the sea.
	in.	in.	in.	°	°	°	°	°	°	°	°	°	°	°					in.	gr.	gr.		grs.	ft.
Helston	29·834	0·358	29·476	50·6	52·1	50·1	2·0	4·1	47·9	88·0	25·0	63·0	29·3	11·0	1·3	s.w.	5·9	152	38·7	4·1	0·6	0·874	533	106
Truro	30·002	...	...	49·9	...	...	...	...	...	73·0	27·0	46·0	22·7	8·2	1·1	s.w. & n.w.	...	185	46·5	...	...	...	...	...
Torquay	29·830	0·326	29·504	51·3	51·6	48·4	3·2	6·3	45·3	80·0	26·0	54·0	26·5	10·4	2·3	n.s. & s.w.	...	160	27·2	3·7	0·9	0·807	536	120
Exeter	29·695	0·311	29·384	49·5	51·2	47·4	3·8	7·5	43·7	77·5	18·0	59·5	31·9	13·5	...	n. & e.	...	150	30·6	3·5	1·1	0·793	533	140
Brighton	29·939	0·321	29·618	47·6	49·4	44·4	2·1	4·4	43·9	76·0	24·0	52·0	...	...	...	n.e. & s.w.	6·0	133	...	3·6	0·6	0·867	542	60
Chichester	29·935	0·313	29·622	47·8	...	...	...	4·0	43·8	82·0	18·0	64·0	32·0	13·6	...	...	...	...	20·8	...	...	...	...	...
Uckfield	29·876	0·320	29·556	49·2	51·3	47·5	3·6	7·2	44·3	98·0	1·0	97·0	47·7	18·7	...	...	...	121	17·6	3·6	1·1	0·795	537	180
Beckington	29·725	0·325	29·400	45·2	48·3	46·7	1·6	3·3	45·0	88·0	5·0	83·0	46·7	...	1·3	s.s.w.	4·9	151	28·7	3·7	0·5	0·894	538	265
Royal Observatory, Greenwich	29·813	0·319	29·494	49·7	49·7	46·9	2·8	5·7	44·0	86·0	10·2	75·8	39·4	16·1	...	s.s.w.	6·8	128	17·6	3·6	0·8	0·836	538	159
Lewisham	...	0·309	...	49·8	49·9	46·6	3·3	6·6	43·0	88·0	6·0	82·0	40·8	16·4	1·3	s.w.	4·9	140	18·7	3·5	0·9	0·811	...	40
Walworth	29·831	0·308	29·523	49·1	...	...	...	6·7	42·4	88·0	12·0	76·0	34·7	...	2·4	s.w.	6·3	149	14·7	...	...	...	...	32
Hereford	29·461	...	...	...	48·1	...	...	...	...	...	...	...	...	...	...	...	...	137	34·0	...	...	...	...	...
Cambridge	29·922	0·317	29·605	48·7	49·0	46·3	2·7	5·4	43·6	86·7	18·7	68·0	38·5	17·5	...	variable.	6·9	148	19·6	3·3	0·9	0·817	541	88
Derby	29·705	0·313	29·392	47·2	...	...	...	...	...	81·0	...	...	...	...	...	variable.	...	169	28·2	...	...	...	...	39
Highfield House	29·761	0·325	29·436	49·3	50·2	47·5	2·7	5·3	44·9	88·0	20·0	68·0	34·7	...	1·8	s.w.	6·1	178	25·0	3·7	0·8	0·841	539	103
Liverpool	29·811	0·289	29·592	49·6	46·9	44·5	2·4	5·0	41·9	76·8	26·8	50·0	25·1	8·5	1·9	n.w.	6·7	177	31·6	3·3	0·6	0·846	543	37
Whitehaven	29·722	0·299	29·423	47·6	...	45·1	2·1	4·5	42·7	79·0	24·5	54·5	28·6	10·3	...	s.w.	...	191	42·9	3·2	0·6	0·867	539	...
Durham	29·530	0·272	29·258	45·7	46·4	43·6	2·8	6·2	40·2	83·2	17·2	66·0	33·5	12·0	...	...	...	134	15·9	3·1	0·8	0·812	536	340
Newcastle	29·749	...	...	44·8	...	...	...	...	...	77·0	21·0	56·0	...	...	...	s.w.	...	130	24·7	...	...	...	...	121

evaporation was 2°·7, and above that of the dew-point was 6°·9, at places situated inland.

The highest readings of thermometer occurred at Uckfield, Helston, Beckington, Lewisham and Walworth respectively.

The mean yearly range of temperature in the counties of Cornwall and Devonshire was 55°·6, at places in the vicinity of the sea was 52°·3, and in latitude 51° 30′ was 78°·0. This value diminished by about 5° for an increase of 1° in latitude.

The places of greatest yearly range were Uckfield, Beckington, Lewisham, Walworth and Greenwich respectively.

The places of least yearly range were Truro, Liverpool, Brighton, Torquay and Whitehaven.

The mean monthly range of temperature in latitude 51° 30′ was 40°; and this value seemed to diminish 3° for every increase of 1° in latitude.

The places of greatest monthly ranges were Uckfield, Beckington, Lewisham, Greenwich and Cambridge respectively.

The places of least monthly ranges were Truro, Liverpool, Torquay and Whitehaven respectively.

The mean daily ranges were greatest at Uckfield, Cambridge, Lewisham and Greenwich respectively.

The places of least daily ranges were Truro, Liverpool, Whitehaven and Torquay respectively.

The mean daily ranges in latitude 51° 30′ were 16°·1; and this value diminished by about 1° for an increase of 1° of latitude.

With respect to the average strength of the wind, I can speak with no confidence. I believe no two observers have estimated its value upon the same scale.

The prevailing direction of the wind in the counties of Cornwall and Devonshire was north-east and south-west, at Liverpool it was north-west, and at all other places it was south-west.

The amount of cloud may be considered to have been equal at all places, and such as to cover about three-fifths of the sky.

The average number of days in which rain fell was 152; this number was greatly exceeded at Truro and at Whitehaven, and was much less between the latitudes of 51° and 52°.

The mean amount of rain fallen in the counties of Cornwall and Devonshire was 35·8 inches, at Liverpool and Whitehaven was 37·3 inches, and at all other places was 22·2 inches. This value was exceeded at Beckington, Hereford and Derby. The places at which the smallest amounts of rain fell are Walworth, Durham, Uckfield, Greenwich and Lewisham.

The mean weight of water mixed with a cubic foot of air was 3·8 grains in the counties of Cornwall and Devonshire, and 3·5 grains at other places.

The mean additional weight required to saturate a cubic foot of air was 0·9 grain in the counties of Cornwall and Devonshire, and 0·7 grain at other places.

The mean degree of humidity was 0·825 in the counties of Cornwall and Devonshire, and 0·839 at other places.

The mean weight of a cubic foot of air at all places at the level of the sea was 541 grains.

The preceding results are chiefly for one point only, viz. that of the Royal Observatory at Greenwich reduced to the level of the sea. To render the results most generally useful, I have deduced the following formulæ, applicable to any place in England, taking into account the irregularities of the formation of the surface of the soil, and the law of the decrement of heat with increasing elevation.

The pressure of dry air at any place in England in the year 1847 was

$$29^{\text{in.}}{\cdot}641 + \frac{\text{height of place in feet above the level of the sea}}{82 \text{ inches}}.$$

The pressure of water was $0^{\text{in.}}{\cdot}319 + (51^\circ\ 30' - \text{latitude}) \times 0{\cdot}010$ inch.

The sum of the two preceding formulæ gives the reading of the barometer.

The temperature of the air at any place in England may be calculated from the formula—

$50^\circ{\cdot}0 + (51^\circ\ 30' - \text{latitude}) \times 1^\circ - 0{\cdot}00345^\circ \times$ height of place in feet above the level of the sea.

The observations are not sufficient to deduce the term depending upon longitude; its coefficient however would be very small, and in the present state of our knowledge may be neglected.

The approximate yearly range of temperature may be calculated from the formula—

$$78^\circ + (51^\circ\ 30' - \text{latitude of place}) \times 5^\circ.$$

The mean monthly range of temperature may be calculated from the formula

$$40^\circ + (51^\circ\ 30' - \text{latitude of place}) \times 3^\circ.$$

The mean daily range of temperature may be computed from the formula

$$16^\circ{\cdot}2 + (51^\circ\ 30' - \text{latitude of place}) \times 1^\circ.$$

The approximate weight of a cubic foot of air may be calculated from the formula

$$541 \text{ grains} + \frac{\text{height of place in feet above the level of the sea}}{82 \text{ inches}}.$$

It is desirable to compare the results deduced by means of the preceding formulæ with the observed results, not only for the purpose of testifying their accuracy, but also to examine the accuracy of the several instruments which have been used at the different stations.

The annexed table shows the results of this comparison.

In viewing the differences shown in the fourth and tenth columns between the observed and calculated pressures, it is evident that at most places the instruments are good. When we view the differences which some of the places present, we shall readily see that the instruments at those places need correction. The barometer at Exeter seems to read too low by 0·103 inch; that at Uckfield to read too high by 0·131 inch; and that at Derby to read too low by 0·193 inch. These corrections should be applied to the readings of these instruments till the true values are determined by a direct comparison with a standard.

I proceed to consider the numbers in the thirteenth column, showing the differences between the observed and calculated annual temperatures of the air. These differences are generally small, and quite within the probable errors of the instruments themselves. This formula therefore may be considered to give the true temperature; and from it the mean temperature of any place in England may be calculated for the year 1847. The differences at Brighton and at Beckington are large, and the annual temperatures as determined at those places are either erroneous, or those places are subjected to a peculiar local influence. Let us turn for a moment to the annual temperature as determined from the corrected readings of the dry bulb thermometer in the yearly table at both these places. The annual temperatures, as thus deduced, are nearer the computed values than those determined from the maximum and minimum thermometers; and it would seem that the self-registering instruments are in error, and that the differences are not due to local disturbances. At Derby and at Nottingham the instruments require correction. At Liverpool the difference shown would seem to be due to locality.

The differences in the thirteenth column, at those places situated in the counties of Cornwall and Devonshire, are small; therefore the annual temperatures of these places are only those due to their latitude. In turning our attention to the 16th, 19th, and 22nd columns, we at once perceive the cause of the loveliness of the climate of those counties to be the uniformity of their temperature. Their yearly, monthly, and daily ranges are respectively 28°, 15°, and 7° less than those due to their latitude.

Comparison of the Computed Values of the Meteorological Elements for the Year 1847, with the Observed Values.

Name of place.	Pressure of dry air.			Pressure of vapour.			Reading of barometer.			Temperature of air.			Yearly range of temperature.			Monthly range of temperature.			Daily range of temperature.		
	Observed.	Computed.	Observed — Computed.	Observed.	Computed.	Observed — Computed.	Observed.	Computed.	Observed — Computed.	Observed.	Computed.	Observed — Computed.	Observed.	Computed.	Observed — Computed.	Observed.	Computed.	Observed — Computed.	Observed.	Computed.	Observed — Computed.
	in.	in.	in.	in.	in.	in.	in.	in.	in.	°	°	°	°	°	°	°	°	°	°	°	°
Helston	29·476	29·512	−0·036	0·358	0·333	+0·025	29·834	29·845	−0·011	50·6	51·0	−0·4	63·0	85·0	−22·0	29·3	44·2	−14·9	11·0	17·6	−6·6
Truro	....	....	....	......	......	....	....	....	....	49·9	51·0	−1·1	46·0	84·0	−38·0	22·7	43·6	−20·9	8·2	17·4	−9·2
Torquay	29·504	29·495	+0·009	0·326	0·331	−0·005	29·830	29·826	+0·004	51·3	50·8	+0·5	54·0	84·0	−30·0	26·5	43·6	−17·1	10·4	17·4	−7·0
Exeter	29·384	29·471	−0·087	0·311	0·327	−0·016	29·695	29·798	−0·103	49·5	50·4	−0·9	59·5	82·0	−22·5	31·9	42·4	−10·5	13·5	17·0	−3·5
Brighton	29·618	29·568	+0·050	0·321	0·326	−0·005	29·939	29·894	+0·045	47·6	50·2	−2·6	52·0	81·5	−29·5	......	41·2	....	......	16·9	
Chichester	29·622	....	....	0·313	0·326	−0·013	29·935	....	....	47·8	......	....	64·0	81·5	−17·5	32·0	41·2	− 9·2	13·6	16·9	−3·9
Uckfield	29·556	29·421	+0·135	0·320	0·324	−0·004	29·876	29·745	+0·131	49·2	49·9	−0·7	97·0	80·5	+16·5	47·7	41·5	+ 6·2	18·7	16·7	+2·0
Beckington	29·400	29·318	+0·082	0·325	0·320	+0·005	29·725	29·638	+0·087	45·2	49·3	−4·1	83·0	78·5	+ 4·5	46·7	40·3	+ 6·4	......	16·3	
Greenwich	29·494	29·447	+0·047	0·319	0·319	0·000	29·813	29·766	+0·047	49·7	49·5	+0·2	75·8	78·0	− 2·2	39·4	40·0	− 0·6	16·1	16·2	−0·1
Lewisham	....	....	....	0·309	0·319	−0·010	....	....	....	49·8	49·9	−0·1	82·0	78·0	+ 4·0	40·8	40·0	+ 0·8	16·4	16·2	+0·2
Walworth	29·523	29·601	−0·078	0·308	0·319	−0·011	29·831	29·920	−0·089	49·1	49·9	−0·8	76·0	78·0	− 2·0	34·7	40·0	− 5·3	......	16·2	
Hereford	....	....	....	......	......	....	29·461	....	....	48·1	......	....	......	75·5	....	......	38·5	....	......	15·7	
Cambridge	29·605	29·533	+0·072	0·317	0·312	+0·005	29·922	29·845	+0·077	48·7	49·1	−0·4	68·0	74·5	− 6·5	38·5	37·9	+ 0·6	17·5	15·5	+2·0
Derby	29·392	29·593	−0·201	0·313	0·305	+0·008	29·705	29·898	−0·193	47·2	48·5	−1·3	......	71·0	....	......	35·8	....	......	14·8	
Highfield House	29·436	29·518	−0·082	0·325	0·305	+0·020	29·761	29·823	−0·062	49·3	48·2	+1·1	68·0	70·5	− 2·5	34·7	35·5	− 0·8	......	14·7	
Liverpool........	29·592	29·596	−0·004	0·289	0·300	−0·011	29·881	29·896	−0·015	49·6	48·0	+1·6	50·0	69·0	−19·0	25·1	34·0	− 8·9	8·5	14·3	−5·8
Whitehaven.....	29·423	....	....	0·299	0·289	+0·010	29·722	....	....	47·6	......	....	54·5	63·0	− 8·5	28·6	31·0	− 2·4	10·3	13·2	−2·9
Durham	29·258	29·226	+0·032	0·272	0·287	−0·015	29·530	29·513	+0·017	45·7	45·8	−0·1	66·0	62·0	+ 4·0	33·5	30·4	+ 3·1	12·0	13·0	−1·0
Newcastle	29·469	29·495	−0·026	......	0·274	....	29·749	29·769	−0·020	44·8	46·1	−1·3	56·0	60·5	− 4·5	......	29·5	....	......	12·7	

I have already alluded to the influence of the sea in equalising the temperature of places in its vicinity in my remarks on the quarter ending June 30, 1847. It moderates the severity of winter and the heat of summer, but does not seem to exercise any influence over the mean annual temperature. Hence we perceive the same annual temperature may be distributed in various ways in the different seasons of the year.

At Uckfield, the yearly, monthly and daily ranges are in excess, which must be owing to local perturbations. By reference to column 13, it will be seen that the annual temperature has been uninfluenced by these large ranges.

The only other places at which considerable differences are shown between the calculated and observed ranges are Whitehaven and Liverpool; and the smallness of the ranges at those places are evidently to be attributed to the vicinity of the sea.

In conclusion, I have merely to remark, that I am persuaded the spirit of the above method of reducing meteorological observations, and deducing from them general formulæ, will some day lead to empirical laws, showing the reciprocal dependence of each subject of research. I would, however, impress upon observers generally the absolute necessity of using good instruments, and ascertaining their errors by comparison with standards; such would save me a great deal of labour and anxiety, which I have experienced in the past year. These exertions, it is evident, could not be long continued; and it must be borne in mind, that no system of calculation can deduce good results from imperfect observations. I must beg, however, to offer my sincere thanks to the gentlemen who have furnished the observations, for their ready acquiescence at all times to my wishes.

This is the first annual report upon the meteorology of England. May it be followed by many, more ably conducted and more valuable to meteorology!

XXXIX. *On the decomposition of Functions into Conjugate Factors; with some consequences deducible therefrom.* By J. R. Young, *Professor of Mathematics, Belfast College**.

I THINK that by decomposing a function of an even degree into its constituent conjugate factors, some interesting results in analysis may occasionally be deduced. It will be seen, from the expressions below, that this decomposition is very easy; and as the component factors involve an arbitrary function, common to each pair, we are at liberty to fix its value so as to fulfill certain prescribed conditions that may

* Communicated by the Author.

subserve the purpose of leading to general theorems. In the present communication, I shall confine myself entirely to the expression (A), which I propose to employ for the purpose of obtaining, in a more direct and simple manner, the formula investigated in the last Number of this Journal (p. 192), and of giving to that formula a greater degree of generality.

Let $\varphi(x)$ be any function of x; then since

$$\varphi(x) = f.\varphi(x)\frac{1}{f} = f_1.f.\varphi(x)\frac{1}{f.f_1}, \text{ \&c.}$$

we obviously have these identities, in which F, f, &c. are any functions whatever:

$$\varphi(x) = [\mathrm{F} + \surd\{\mathrm{F}^2 - \varphi(x)\}] \times [\mathrm{F} - \surd\{\mathrm{F}^2 - \varphi(x)\}] \quad . \quad . \quad \text{(A)}$$

$$= [\mathrm{F} + \surd\{\mathrm{F}^2 - f.\varphi(x)\}] \times [\mathrm{F} - \surd\{\mathrm{F}^2 - f.\varphi(x)\}]\frac{1}{f}$$

$$= [\mathrm{F} + \surd\{\mathrm{F}^2 - f.\varphi(x)\}] \times [\mathrm{F} - \surd\{\mathrm{F}^2 - f.\varphi(x)\}]$$

$$\times \left[\mathrm{F}' + \surd\left\{\mathrm{F}'^2 - \frac{1}{f}\right\}\right] \times \left[\mathrm{F}' - \surd\left\{\mathrm{F}'^2 - \frac{1}{f}\right\}\right]$$

and so on.

In the expression (A), let

$$-\varphi(x) = x^2 + px = -q;$$

then, as F is entirely arbitrary, it may be made to take such a value as to render the sum of the two conjugate factors in (A), that is to say 2F, equal to $-p$; which value is $\mathrm{F} = -\frac{1}{2}p$: and as the product of the same factors is q, it follows that these factors are the roots of the quadratic equation

$$x^2 + px + q = 0;$$

that is, the roots are

$$-\frac{1}{2}p \pm \sqrt{\left\{\frac{p^2}{4} - q\right\}}. \quad . \quad . \quad . \quad . \quad (1)$$

Again: let there be the cubic equation

$$x^3 + px^2 + qx + r = 0,$$

and, in this case, put

$$\varphi(x) = x^2 + px + q = -\frac{r}{x};$$

then, if x_1 be one of the roots of the cubic, the expression

$$x_1^2 + px_1 + q$$

will be the product of the two remaining roots; and therefore, that these roots may be represented by the factors in (A), we shall merely have to determine F so as to fulfill the condition

$$2\mathrm{F}+x_1=-p \therefore \mathrm{F}=-\frac{x_1+p}{2},$$

and, consequently, the two remaining roots are

$$-\frac{x_1+p}{2}\pm\sqrt{\left\{\left(\frac{x_1+p}{2}\right)^2-x_1^2-px_1-q\right\}}$$

$$=-\frac{x_1+p}{2}\pm\sqrt{\left\{\left(\frac{x_1+p}{2}-2x_1\right)\frac{x_1+p}{2}-q\right\}},$$

as in the paper referred to above

Let us now take a biquadratic equation

$$x^4+px^3+qx^2+rx+s=0;$$

then if x_1 and x_2 be two of its roots, we shall have

$$x_1^3+px_1^2+qx_1+r=-\frac{s}{x_1}$$

$$x_2^3+px_2^2+qx_2+r=-\frac{s}{x_2}$$

$$\therefore (x_1^3-x_2^3)+p(x_1^2-x_2^2)+q(x_1-x_2)=\frac{s}{x_2}-\frac{s}{x_1}=\frac{s(x_1-x_2)}{x_1x_2};$$

and, consequently, the expression

$$\left.\begin{array}{l} x_1^2+x_1x_2+x_2+p(x_1+x_2)+q=\frac{s}{x_1x_2}=(x_1+x_2)^2 \\ \quad +p(x_1+x_2)-x_1x_2+q \end{array}\right\} \quad \text{(B)}$$

is the product of the two remaining roots.

Let $\varphi(x)$ represent this product: then, as before, in order that the two factors in (A) may represent these roots, we shall merely have to determine F so as to fulfill the condition

$$2\mathrm{F}=-(x_1+x_2+p) \therefore \mathrm{F}=-\frac{x_1+x_2+p}{2};$$

so that the expressions for the two remaining roots are

$$-\frac{x_1+x_2+p}{2}$$

$$\pm\sqrt{\left\{\left(\frac{x_1+x_2+p}{2}\right)^2-(x_1+x_2)^2-p(x_1+x_2)+x_1x_2-q\right\}},$$

the irrational part of which may be put in the more convenient form

$$\pm\sqrt{\left\{\left(\frac{x_1+x_2+p}{2}-2(x_1+x_2)\right)\frac{x_1+x_2+p}{2}+x_1x_2-q\right\}},$$

which we see reduces to the preceding expression for the roots of a cubic when $x_2=0$.

If we were to take an equation of the fifth degree, and to proceed as above, we should obtain three expressions of the third degree, as the expression (B) of the second degree is obtained: and if from the double of one of these the sum of the other two be taken, and an obvious division performed, we shall get an expression for $\frac{-s}{x_1x_2x_3}$, the product of the remaining two roots x_4, x_5: and, determining F as before, the roots themselves are found to be

$$-\frac{x_1+x_2+x_3+p}{2}$$

$$\pm\sqrt{\left\{\left(\frac{x_1+x_2+x_3+p}{2}\right)^2-(x_1+x_2+x_3)^2-p(x_1+x_2+x_3)\right.}$$
$$\overline{\left.+x_1x_2+x_1x_3+x_2x_3-q\right\}},$$

of which the irrational part may be written

$$\pm\sqrt{\left\{\left(\frac{x_1+x_2+x_3+p}{2}-2(x_1+x_2+x_3)\right)\frac{x_1+x_2+x_3+p}{2}\right.}$$
$$\overline{\left.+x_1(x_2+x_3)+x_2x_3-q\right\}};$$

and thus the general form of the expressions for two roots of any equation, when the others are found, is sufficiently indicated. It is probable however that, beyond equations of the fifth degree, these formulas would not be much more commodious for actual numerical computation than those equivalent ones in which minus the product of the two sought roots is introduced under the radical, in the form in which it is immediately obtained from dividing the final term of the equation by the given roots with changed signs; the formula in this way being immediately suggested by the expression (1), from which indeed what is here done might have been derived. But my chief object has been to show how the conjugate factors (A) may be turned to account in a particular inquiry; as we see that, from these, the form (1) has been itself obtained. The reader will at once perceive how the term *conjugate* factors has been suggested; and I would here venture an opinion that the same term might with propriety be employed, instead of *congeneric,* in certain equations related to one another in a somewhat similar manner as these factors: and further, that the expression *conjugate roots* of equations seems to be unnecessarily restricted: all the roots of equations of an even degree may be expressed in pairs of the forms $\alpha+\beta$, $\alpha-\beta$; $\alpha_1+\beta_1$, $\alpha_1-\beta_1$, &c.; and these seem entitled to be called *conjugate* pairs. This way of pairing the roots of equations has already been distinctly noticed by Professor Davies in a former Number of this Journal. See vol. xxxiii. p. 366.

In reference to the formulas established in this paper, it may not be superfluous to observe that they will be found to be more especially useful in those cases in which all the roots but two are real; as they will enable us to exhibit the imaginary pair, by aid of the real roots, with comparatively little expense of calculation; and even when all the roots are real, a saving of figures is still effected by them. But, in comparing formulas of this kind with the numerical process of Horner, it must always be remembered that Horner's method supplies the roots in an explicit form; whereas, in expressions for them such as these, there yet remains an unperformed operation, indicated by the radical; which, however, in the case of imaginary roots, is of course impracticable; and therefore leaves nothing further to be done. But, in all formulas for imaginary roots, into which approximate values only of the real roots enter, it is necessary, in delicate cases, that is in those cases in which a very slight change in any of the coefficients would convert unequal into equal roots—it is necessary, in such cases, to push these approximations to a more than usual extent, in order to avoid the conversion of imaginary roots into real, and *vice versâ*; for there is no hope of attaining the imaginary forms accurately, when we employ approximations only to the real quantities which enter into the expression of them.

Although, as stated at the outset, it is not my intention at present to enter into any discussion of the forms which follow (A), yet I may perhaps be permitted briefly to notice here one or two obvious deductions from them.

By putting s for $\varphi(x)$, we at once see how easily the usual formulas for the solution of equations of the fourth degree may be obtained from those forms: we shall only have to multiply together the quadratic factors

$$x^2-2Fx+fs$$

$$x^2-2F'x+\frac{1}{f},$$

and to equate the resulting coefficients with those of the like powers of x in the proposed equation: and it may not be undeserving of notice, that when two roots are reciprocals, and two only, then $f=1$, and F, F′ may each be determined by a simple equation.

Again: if a pair of conjugate roots of a biquadratic equation be given in the form $\alpha \pm \sqrt{\beta}$, then it is plain, from the same expressions, that we must have

$$\beta = \alpha^2 - fs \therefore \frac{1}{f} = \frac{s}{\alpha^2 - \beta};$$

and consequently the values of the two remaining roots will be

$$-\frac{2\alpha + p}{2} \pm \sqrt{\left\{\left(\frac{2\alpha + p}{2}\right)^2 - \frac{s}{\alpha^2 - \beta}\right\}},$$

which might indeed have been deduced from the form (1), though I believe that form has not hitherto been employed for this purpose. The same values, as furnished by the expressions previously given, take the somewhat more convenient forms

$$-\frac{2\alpha + p}{2} \pm \sqrt{\left\{\left(\frac{2\alpha + p}{2} - 4\alpha\right)\frac{2\alpha + p}{2} + \alpha^2 - \beta - q\right\}}.$$

I shall merely add, in conclusion, that, as far as *equations* are concerned, the conjugate factors of $\varphi(x)$ do little more than express the fact that the roots of every equation of an even degree,

$$(x^2 + ax + b)(x^2 + a_1 x + b_1) \ldots\ldots = 0,$$

may be exhibited under the forms

$$\begin{array}{c} F \pm \sqrt{\{F^2 - b\}} \\ F' \pm \sqrt{\{F'^2 - b_1\}} \\ \&c. \qquad \&c.; \end{array}$$

a truth which, however obvious from the theory of common quadratic equations, has never, I believe, been turned to any account elsewhere. The conjugate factors here referred to express obvious identities: they do not presuppose the solution of a quadratic, but actually supply that solution, by aid of the fundamental property that the coefficient of the second term is the sum of the roots with changed signs, and the third term the product.

P.S. In my last paper (p. 194, line 18) *for* "quotient" *read* "product."

Belfast, March 8, 1849.

XL. *On the Theoretical Value of the Velocity of Sound, in reply to* Mr. Stokes. *By the* Rev. J. Challis, *M.A., F.R.S., F.R.A.S., Plumian Professor of Astronomy and Experimental Philosophy in the University of Cambridge*.*

THE velocity of propagation of waves in an elastic medium so constituted that the pressure varies in the same proportion as the density, is usually deduced from the hydrodynamical equations by assuming, either that the motion of the vibrating particles is a function of the distance from a fixed plane, or that it is a function of the distance from a fixed centre. On the former assumption, an exact integral, applicable to propagation in a single direction, may be obtained, which conducts to the inference that a point of maximum velocity of a given wave travels at a rate different from that of a point of no velocity, so that, however large the maximum velocity may be, one of these points may overtake the other, without any indication on the part of the analysis of the physical impossibility of such an occurrence. The inevitable conclusion from this result is, that the integral admits of no interpretation compatible with fluid motion, and that the assumption of plane-waves is inadmissible.

The assumption of spherical waves is shown to be inadmissible by conducting to an incompatibility of another kind, as I have proved by an argument contained in the Number of the Philosophical Magazine for last February. The argument is divided into five heads; the four first of which include the proof of incompatibility, and the fifth is merely an appeal to an admitted principle in physics, viz. that of constancy of mass, to which the result of the previous reasoning is opposed. Mr. Stokes, in the March Number, after assenting to the four first heads, meets the fifth by a simple denial for which he gives no reason. But surely the weight of this denial falls very harmlessly on a part of the argument which admits of no dispute; for I presume that Mr. Stokes does not intend to maintain that in physics there is such a thing as generation or annihilation of matter.

My argument put in syllogistic form is as follows:—

Let the waves be supposed to be spherical.

Then, as the analysis shows, the *same* portion of matter has a different value (expressed, for instance, in cubic feet of the matter in a given state of density) at one time from that which it has at another time.

But by the principle of constancy of mass the same portion of matter has the same value at all times.

* Communicated by the Author.

Therefore the waves cannot be supposed to be spherical.

That there may be no excuse for misapprehension as to the result attributed to the analysis in the second member of the syllogism, I proceed to exemplify that result by a numerical instance. The pressure being $a^2(1+s)$, the value of the condensation s at any distance r from the centre, and at any time t, is admitted to be given by the equation

$$s=\frac{F(r-at)}{r}.$$

Since the function F is arbitrary, it may be supposed that

$$s=\frac{\mu}{r}\sin\frac{2\pi}{\lambda}(r-at+c),$$

μ, λ, and c being certain constants. It is also admitted that the function F may be taken discontinuously, that is, from one zero value to another zero value; and that all other values of s not included between those limiting values may be zero. Let therefore the values of the circular function be taken from $r-at_1+c=0$ to $r-at_1+c=\frac{\lambda}{2}$. Then, the mean density of the medium being unity, the quantity of condensed matter in the space occupied by the wave above matter of mean density occupying the same space, is the integral of $4\pi r^2 sdr$ taken between the limits just mentioned. Call this quantity α, and for the sake of definiteness of conception, let the fluid under consideration be contained at the time t_1 between two rigid spherical surfaces, the radius of one of which is 1000 feet, and that of the other 1,000,000 feet. There is nothing in the antecedent investigation to exclude such a supposition, and for the purpose of the argument these numbers will serve as well as any others. Let the fluid of mean density which would fill the space between these surfaces be A in cubic feet, which of course is a constant quantity. Then α being expressed in cubic feet, the whole quantity of matter at the time t_1 is $A+\alpha$. To express α numerically let $\lambda=1$ foot, and let the constant $\mu=1$; which amounts to supposing that the maximum condensation at a distance of 10,000 feet is 0,0001. Consequently

$$\alpha=4\pi\int \delta r\sin\frac{2\pi}{\lambda}(r-at_1+c)dr$$

the exact value of which integral between the limits $r-at_1+c=0$ and $r-at_1+c=\frac{\lambda}{2}$ is $4\lambda r_1$, r_1 being the distance of the maximum condensation from the centre at the time t_1. Hence when $r_1=10{,}000$ feet, the whole quantity of matter is $A+40{,}000$

cubic feet. But since the wave is propagated from the centre with a velocity a, the distance of the maximum condensation from the centre may at certain time t_2 become 100,000 feet, in which case the whole quantity of matter is A + 400,000 cubic feet. Thus in the interval from t_1 to t_2 the analysis has generated 360,000 cubic feet of matter! After obtaining such a result from the part of the argument to which Mr. Stokes has expressed his assent, I am at a loss to conceive for what reason he asserts that any *onus probandi* rests with me.

I have no doubt whatever that I have pointed out *real* contradictions resulting from the suppositions of plane-waves and spherical waves, of the utmost importance in hydrodynamics, since they prove that the true theoretical value of the velocity of sound cannot be deduced from those suppositions. By another supposition which conducted to ray-vibrations, I obtained in the Philosophical Magazine for February a value of the velocity of sound very nearly agreeing with observation, without meeting with any similar contradiction. To this subject, however, I hope to find time to recur on a future occasion.

Cambridge Observatory,
March 22, 1849.

XLI. *On the Diurnal Variations of the Magnet Needle, and on Auroræ Boreales. By* Auguste de la Rive, *being an Extract from a Letter to* M. Arago*.

ALLOW me to communicate to you, with the request that you will make it known to the Académie des Sciences, an extract of a memoir recently read before our Société de Physique et d'Histoire Naturelle, on the cause of the diurnal variations of the magnet needle and of auroræ boreales. In assigning successfully these two classes of phænomena to the same origin, I have but followed the path you have pointed out; for more than thirty years ago you established with indefatigable perseverance, by your numerous observations, the remarkable agreement which prevails between the appearances of the aurora borealis and the disturbance of the magnet needle.

The following is my theory. You will observe that it rests solely upon well-ascertained facts and on principles of physics positively established.

I had already, in 1836, in a notice upon hail†, attempted to show that the atmospheric electricity owes its origin to the

* From the *Annales de Chimie et de Physique* for March 1849.

† *Bibliothèque Universelle*, vol. iii. p. 217, nouvelle série.

unequal distribution of temperature in the strata of the atmosphere. It is well known that, in a body of any nature whatsoever heated at one of its extremities and cooled at the other, the positive electricity proceeds from the hot part to the cold, and the negative electricity in the contrary direction; it thence results that the lower extremity of an atmospheric column is constantly negative and the upper one constantly positive. This difference of opposite electric conditions must be so much the greater the more considerable is the difference of temperature; consequently more marked in our latitudes in summer than in winter, more striking in general in the equatorial than in the polar regions. It must be observed that the negative state of the lower portions of the atmospheric columns must be communicated to the surface of the earth on which they repose, whilst the positive state of the upper portions is diffused more or less, from above downwards, through nearly the whole of each of the columns, according to the facilities offered by the greater or less degree of humidity of the air to the propagation of the electricity. An atmospheric column therefore resembles a high-pressure battery on account of the imperfect conductibility of the elements of which it is composed,—a battery the negative pole of which is in constant and direct communication with the terrestrial globe, discharges itself upon the globe, whilst it becomes itself charged with the electricity of its positive pole, which is distributed over it with an intensity decreasing with the distance from this pole; —this explains why the positive electricity increases with the height of the atmosphere.

The causes which determine the accumulation of negative electricity at the surface of the earth and of positive electricity in the upper regions of the atmosphere, act in a continuous manner: there should thence result an unlimited tension of the two opposite electric states, if, having attained a certain degree of energy, they did not neutralize each other by the aid of different circumstances. In other words, having reached a certain limit of tension which varies with the state of the atmosphere and the surface of the earth, the two electricities cannot go beyond it, and unite or neutralize each other as regards the excess over that limit. This neutralization is effected in two ways, in a normal or constant manner, and in an irregular and accidental manner.

This second mode is exhibited under a variety of forms; sometimes it is simply the humidity of the air, and better still the rain or snow, which re-establish the electrical equilibrium between the earth and the atmosphere; in some cases waterspouts manifest in an energetic form the mutual action of the two electricities, which tend to unite. Sometimes the winds, by mixing the air in contact with the surface of the earth, and like it negative, with the positive air of the more elevated regions, give rise to sheet-lightning, or to storms, when there is at the same time a formation of clouds and condensation of aqueous vapours, owing to the humidity and different temperature of the strata of air which become mixed. The attraction of clouds by mountains, the luminous phænomena exhibited at the extremity of elevated points, are likewise due to the same cause. But I will not stop to discuss further all these natural and intelligible consequences of the theory which I expound. I shall confine myself to one single remark, which is, that we must bear in mind that in observations of atmospheric electricity the intensity of the electric signs perceived is not always a proof of the intensity of the electricity itself; for the humidity of the atmosphere, by favouring the propagation of the electricity of the upper strata, may give rise, as is frequently seen in winter, to very powerful electrical manifestations even when the cause producing them is not very powerful. The contrary is frequently seen in summer.

I now pass to the regular and normal mode of neutralization of the two electricities. I had already suspected the existence of this mode in my notice of 1836; but I did not announce it positively, because there was then wanting a fact which science now possesses, viz. the perfect conductibility of the terrestrial globe with which the employment of the electric telegraph has made us acquainted.

To make it understood how I conceive this mode of neutralization, I divide the atmosphere into annular strata parallel with the equator; the positive electricity accumulated at the external portion of this layer cannot exceed a certain degree of tension without traversing rarefied and more or less humid air until it reaches the polar regions, where, finding an atmosphere saturated with humidity, it will combine readily with the negative electricity accumulated on the earth. We have thus the circuit formed; each annular stratum of the atmosphere gives rise to a current which proceeds in the elevated regions from the upper portion of the stratum towards the pole, redescends to the earth through the atmosphere surrounding the poles, and returns by the surface of the globe from the pole to the lower part of the stratum from which it started. These currents will consequently be the more numerous and the more concentrated the nearer we approach the pole; and as they all proceed in the same direction, that is to say from south to north in the upper portion of the atmosphere and from north to south on the surface of the earth, their effect will become the more perceptible in proportion as we leave the equator and approach the pole. But as the currents produced by the equatorial strata are individually stronger than those proceeding from more northerly strata, the difference, although real, will notwithstanding be less than would be believed. What passes in our northern hemisphere must occur in exactly the same manner in the southern hemisphere; the currents proceed equally from the equator to the pole in the upper regions of the air, and from the pole to the equator on the surface of the earth; consequently, for an observer travelling from the north pole to the south, the current would proceed in the same direction from the northern pole to the equator, and in a contrary direction from the equator to the southern pole: I speak here of the current circulating on the surface of the earth. I ought moreover to observe, that the limit which separates the regions occupied by each of these two great currents is not the equator properly so called, for it must be variable; it is, according to my theory, the parallel between the tropics which has the sun at its zenith; it changes consequently each day.

Now it is easy to conceive the cause of the diurnal variations of the magnetic needle. In conformity with the laws established by Ampère, the current which proceeds from the northern pole to the equator ought to cause the north pole of the needle to deviate to the west, which is what takes place in our hemisphere; and the current which proceeds from the southern pole to the equator should cause the north pole of the needle to deviate to the east, which is precisely what occurs in the southern hemisphere. The deviation should be, in one and the same place, the more considerable the greater the difference of temperature, and consequently of the electric conditions between the lower and the upper stratum of the atmosphere; thus the deviation increases from the morning to $1^h\ 30^m$ P.M. It is more considerable in those months during which the sun is longer above the horizon; it is at its minimum in the winter months. Lastly, these diurnal variations increase in magnitude in proportion as we recede from the equator and approach the pole, a result which again perfectly agrees with what I have stated respecting the increase in number of the currents towards the polar regions. In these regions themselves the variations may be very irregular, and may be entirely absent if the magnetic needle happens to be placed in those very localities where the electric currents traverse the atmosphere to reach the earth; in fact, a needle surrounded thus on all sides by currents is no longer affected by them, or at least is no longer affected in a regular manner. This remark may explain certain observations, especially those made at Port Bowen, which appeared rather exceptional.

On examining carefully all the magnetic observations I was able to consult, and in particular those of Colonel Sabine, I was especially struck by the remarkable manner in which they agreed with my theory. I will cite but one example—the observations recently made at St. Helena, and just published, by Colonel Sabine. At St. Helena the diurnal variation occurs to the west as long as the sun is to the south of the island, and to the east as soon as the sun is to the north. In fact, in the first case, as I have previously observed, St. Helena must form part of the region in which the electric currents proceed on the surface of the earth from the north pole to the equatorial regions; and, in the second case, it forms part of the region in which these currents pass from the south pole to the equator. The hour of the maximum of the diurnal variation is not the same at the island of St. Helena as in the continental countries, which is owing to the temperature of the surface of the ocean not following the same laws in its diurnal variations as the temperature of the surface of the earth. Now the temperature of the lower stratum of the atmospheric column is always that of the surface of the ocean, or of the soil on which it rests. This same circumstance explains certain apparent anomalies exhibited by the diurnal variations in some parts of the globe, as for instance at the Cape of Good Hope, which is surrounded almost on every side by a vast extent of ocean.

I wish it to be understood that in the preceding I have only taken notice of the causes disturbing the direction of the magnetic needle, and not of the cause of this direction itself, that is to say of terrestrial magnetism—a cause which I do not at all believe to be of the same nature, but upon which I at present express no opinion. I am content to consider the terrestrial globe as a large spherical magnet, and to study the external causes capable of modifying the direction which it tends to impart in its quality of magnet to magnetic needles.

Now what is the aurora borealis according to the theory which I have just expounded? It is the luminous effect of electric currents travelling in the high regions of the atmosphere towards the north pole—an effect due to the combination of certain conditions, which are not always exhibited in the same manner, nor at all seasons of the year.

It is now well proved that the aurora borealis is an atmospheric phænomenon, as you long ago suspected. The name of *magnetic storm*, by which Von Humboldt designates it in his Cosmos, implies the same idea, which is moreover confirmed by the interesting details which he gives of this meteor. The observations of Parry, Franklin, and especially those of MM. Bravais and Lottin, so numerous and carefully made, are likewise quite favourable to this opinion, which followed equally from the observations of M. Biot at the Shetland Isles.

Admitting this point, I explain the production of the aurora borealis in the following manner:—When the sun, having passed into the southern hemisphere, no longer heats so much our hemisphere, the aqueous vapours which have accumulated during the summer in this part of the atmosphere begin to condense, the kind of humid cap enveloping the polar regions extends more and more, and facilitates the passage of the electricity accumulated in the upper portions of the air. But in these elevated regions, and especially at this period of the year, the aqueous vapours must most frequently pass into the state of minute particles of ice or snow floating in the air, similar to those which give rise to the halos; they form, as it were, a kind of semitransparent mist. Now these half-frozen fogs conduct the electricity to the surface of the earth near the pole, and are at the same time illumined by these currents or electric discharges. In fact, all observers agree in asserting that the aurora borealis is constantly preceded by a mist which rises from the pole, and the margins of which, less dense than the remainder, are coloured the first; and indeed it is very frequent near the pole in the winter months, and especially in those where there is abundance of vapour in the air. For it to be visible at great distances from the pole, it is necessary that these clouds, composed of frozen particles, extend in an almost uninterrupted manner from the polar regions to somewhat southern latitudes, which must be of rare occurrence. These same clouds, when they are partial, which is frequently the case, produce the halos.

Now the analogy pointed out by nearly all observers between the mists which accompany the aurora borealis and those which produce the halos, is a somewhat remarkable circumstance. It is easy to verify by direct experiment the identity which exists between the light of the aurora borealis and that obtained by passing a series of electric discharges into rarefied air containing a large quantity of aqueous vapour, and especially through a very thin layer of snow or a slight layer of hoar frost deposited on the glass. I have ascertained that highly rarefied but perfectly dry air gives but a very faint light, and that in the experiment of the vacuum-tube it is essentially the moisture adhering to the inner sides of the tube which, by conducting the electric discharges, gives rise to the luminous effects. It will be conceived that the electric discharges transmitted by this kind of network of ice must, on becoming concentrated near the pole, produce there a far more brilliant light than they develope when they are distributed over a much greater extent.

But why does the magnetic pole, and not the terrestrial pole, appear to be the cause of the phænomenon? Here is my answer. Place the pole of a powerful electro-magnet beneath a large surface of mercury; let this surface communicate with the negative pole of a powerful battery; bring near to it the point of a piece of charcoal communicating with the positive pole of the battery; immediately the voltaic arc is formed, and the mercury is seen to become agitated above the electro-magnet; and wherever this is placed, luminous currents are observed to rotate around this pole, and throw out from time to time some very brilliant rays. There is always, as in the case of the aurora borealis, a dark portion in the form of a circular point over the pole of the magnet; this peculiar effect disappears without the voltaic light being interrupted when the electro-magnet ceases to be magnetized. With a continuous current of ordinary electricity arriving at the pole of a powerful electro-magnet in rarefied and moist air, luminous effects, still more similar in appearance to those of the aurora borealis, are obtained.

These phænomena result from the action of magnets on currents; now the same should apply to the action of the magnetic pole of the earth; the neutralization of the two electricities probably takes place over a somewhat large extent of the polar regions; but the action of the magnetic pole causes the conducting mists to rotate around it, sending forth those brilliant rays which by an effect of perspective appear to us to form the corona of the aurora. The sulphurous odour, and the noise which is said sometimes to accompany the appearance of the aurora, would not be inexplicable; for the odour would be due, like that which accompanies lightning, to that modification which the passage of electric discharges produces upon the oxygen of the air which M. Schönbein has called ozone; while, as regards the noise, it would be analogous to that which, as I have shown, the voltaic arc produces when it is under the influence of a very near magnet. If it seldom occurs in the case of the aurora, it is owing to its being very rare that the luminous arch is sufficiently near the earth, and consequently to the pole. However, the description which has been given of this noise by those who have heard it, is perfectly identical with that which I have given, without suspecting the analogy, of the noise which the voltaic arc produces in the action of the magnetism.

The magnetic disturbances which always accompany the appearance of an aurora borealis are now easily explained. This accidental union of a greater proportion of the accumulated electricities must derange the normal action of the regular current; with respect to the directions of the disturbance, it will depend on the portion of the current acting upon the needle, and consequently on circumstances impossible to foresee, since they depend on the extent of the phænomenon and the position of the needle in relation to it. In fact, according as the horizontal plane in which the declination needle moves comprises above or below some of the region in which the greatest activity of the phænomenon takes place, it will be either the current circulating on the earth or that travelling in the air (currents which proceed in a contrary direction) which will act upon the needle; even during the same aurora, it may be sometimes one sometimes the other of these two currents which will act. The variable directions in which the needle is deflected during an aurora borealis agree very well with this explanation, at least as far as I have been able to judge from the different observations published in the *Annales de Chimie et de Physique* and in several scientific voyages. The remarkable effect observed by M. Matteucci in the apparatus of the electric telegraph between Ravenna and Pisa, during the magnificent aurora of the 17th of last November, fully proves the existence of a current circulating on the surface of the earth, and which, ascending the wire of the telegraph, passed in part through this better conductor. The sounds which long iron wires strung in the direction of north to south give out under certain meteorological circumstances, are undoubtedly a proof that they are traversed by a current which is probably derived from the currents circulating on the surface of the earth from north to south in our hemisphere.

It would be highly interesting and important to profit by those telegraphic wires, which are found to have a direction more or less approaching to that of the declination needle, in order to make with them, when they are not in use for ordinary purposes, some observations which would enable us to demonstrate and to measure the electric currents which probably traverse them; it would be easily accomplished by means of a multiplying galvanometer, by completing the communication of these wires with the earth at one of their extremities. The comparison of the results obtained in this manner with those furnished by the simultaneous observation of the diurnal variations of the needle, would certainly present considerable interest, and might lead to meteorological results of a remarkable nature.

I cannot conclude this abstract without drawing attention to the circumstance, that M. Arago had already pointed out in 1820, shortly after Œrsted's discovery, the possibility of acting upon the voltaic arc by this magnet, and the analogy which might result between this phænomenon and that of the aurora borealis.

XLII. *On Quaternions; or on a New System of Imaginaries in Algebra.* By Sir William Rowan Hamilton, *LL.D., M.R.I.A., F.R.A.S., Corresponding Member of the Institute of France, &c., Andrews' Professor of Astronomy in the University of Dublin, and Royal Astronomer of Ireland.*

[Continued from vol. xxxiii. p. 60.]

65. If we make

$$\rho-\lambda=\lambda_{\prime};\ \rho-\mu=\mu_{\prime};\ \rho-\lambda'=\lambda'_{\prime};\ \rho-\mu'=\mu'_{\prime};\quad . \quad (115.)$$

and in like manner, (see (106.),)

$$\rho-\xi=-b^2\nu=\xi_{\prime};\quad . \quad . \quad . \quad . \quad . \quad (116.)$$

and if we regard these five new vectors, $\lambda_{\prime}, \mu_{\prime}, \lambda'_{\prime}, \mu'_{\prime}$, and $\xi_{\prime}$, as lines which, being drawn from the centre A, terminate respectively in five new points, $L_{\prime}$, $M_{\prime}$, $L'_{\prime}$, $M'_{\prime}$, and H; while the vector ρ, drawn from the same centre A, still terminates in the point E, upon the surface of the ellipsoid; then the equations (113.), (114.), of art. 62, will give:

$$T\lambda_{\prime}=T\mu_{\prime}=T\lambda'_{\prime}=T\mu'=b;\quad . \quad . \quad . \quad (117.)$$

while the equations (101.) will enable us to write

$$\frac{\lambda_{\prime}-\xi_{\prime}}{\varkappa}=\frac{\mu_{\prime}-\xi_{\prime}}{\iota}=\frac{\mu_{\prime}-\lambda_{\prime}}{\iota-\varkappa}=V^{-1}0;\quad . \quad . \quad (118.)$$

and in like manner, (see (112.),)

$$\frac{\lambda'_{\prime}-\xi_{\prime}}{\varkappa'}=\frac{\mu'_{\prime}-\xi_{\prime}}{\iota'}=\frac{\mu'_{\prime}-\lambda'_{\prime}}{\iota'-\varkappa'}=V^{-1}0;\quad . \quad (119.)$$

this symbol $V^{-1}0$ denoting (as already explained) a *scalar.* We shall have also, by (84.), (89.),

$$\frac{\rho-\lambda_{\prime}}{\iota-\varkappa}=\frac{\lambda}{\iota-\varkappa}=V^{-1}0;\ \frac{\rho-\mu_{\prime}}{\varkappa-\iota}=\frac{\mu}{\varkappa-\iota}=V^{-1}0;\quad (120.)$$

the scalars denoted by the symbol $V^{-1}0$ being not generally obliged to be equal to each other, and being, in these last equations (120.), respectively equal, by (86.), (91.), to those which have been denoted above by h and h'. In like manner, by (110.),

$$\frac{\rho-\lambda_{\prime}'}{\iota'-\kappa'}=\frac{\lambda'}{\iota'-\kappa'}=\mathrm{V}^{-1}0;\quad \frac{\rho-\mu_{\prime}'}{\kappa'-\iota'}=\frac{\mu'}{\kappa'-\iota'}=\mathrm{V}^{-1}0. \quad (121.)$$

And because, by (107.), ι' has a scalar ratio to κ, and κ' has a scalar ratio to ι, we may infer, from (118.), (119.), the existence of the two following other scalar ratios:

$$\frac{\mu_{\prime}'-\xi_{\prime}}{\lambda_{\prime}-\xi_{\prime}}=\mathrm{V}^{-1}0;\quad \frac{\lambda_{\prime}'-\xi_{\prime}}{\mu_{\prime}-\xi_{\prime}}=\mathrm{V}^{-1}0. \quad . \quad . \quad (122.)$$

Finally we may observe that, by (120.), (121.), there exist scalar ratios between certain others also of the foregoing vector-differences, and especially the following:

$$\frac{\rho-\lambda_{\prime}}{\rho-\mu_{\prime}}=\mathrm{V}^{-1}0;\quad \frac{\rho-\lambda_{\prime}'}{\rho-\mu_{\prime}'}=\mathrm{V}^{-1}0. \quad . \quad . \quad . \quad (123.)$$

66. Proceeding now to consider the geometrical signification of the equations in the last article, we see first, from the equations (117.), that the four new points, $\mathrm{L}_{\prime}$, $\mathrm{M}_{\prime}$, $\mathrm{L}_{\prime}'$, $\mathrm{M}_{\prime}'$, are all situated upon the surface of that *mean sphere*, which is described on the mean axis of the ellipsoid as a diameter; because the equation of that mean sphere has been already seen to be

$$\rho^2+b^2=0* \text{ equation (100.), article 58;}$$

which may also be thus written, by the principles and notations of the calculus of quaternions:

$$\mathrm{T}\rho=b. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (124.)$$

From the relations (122.) it follows that the two chords $\mathrm{L}_{\prime}\mathrm{M}_{\prime}'$ and $\mathrm{L}_{\prime}'\mathrm{M}_{\prime}$, of this mean sphere, both pass through the point H, of which the vector $\xi_{\prime}$ is assigned by the formula (116.); for

* This *form* of the equation of the *sphere* was published in the Philosophical Magazine for July 1846; and it is an immediate and a very easy consequence of that fundamental formula of the whole theory of Quaternions, namely

$$i^2=j^2=k^2=ijk=-1,$$

which was communicated under a slightly more developed form, to the Royal Irish Academy, on the 13th of November 1843. (See Phil. Mag. for July 1844.)

It may perhaps be thought not unworthy of curious notice hereafter, that *after* the publication of this form of the equation of the *sphere*, there should have been found in England, and in 1846, a person with any mathematical character to lose, who could profess publicly his inability to distinguish the method of *quaternions* from that of *couples*; and who could thus confound the system of the present writer with those of Argand and of Français, of Mourey and of Warren.

the first equation (122.) shows that the three vectors $\lambda_{,}$, $\mu_{,}'$, $\xi_{,}$, which are all drawn from one common point, namely the centre A of the ellipsoid, all terminate on one straight line; since otherwise the quotient of their differences, $\mu_{,}'-\xi_{,}$ and $\lambda_{,}-\xi_{,}$, would be a *quaternion**, of which the vector part would not be equal to zero: and in like manner, the second equation (122.) expresses that the three lines $\lambda_{,}'$, $\mu_{,}$, $\xi_{,}$, all terminate on another straight line. The four-sided figure $\text{L}_{,}\text{M}_{,}\text{L}_{,}'\text{M}_{,}'$ is therefore a *plane quadrilateral, inscribed* (generally) *in a small circle of the mean sphere,* and having the point H for the intersection of its second and fourth sides, $\text{M}_{,}\text{L}_{,}'$ and $\text{M}_{,}'\text{L}_{,}$, or of those two sides prolonged. And these two sides, having respectively the directions of $\text{HM}_{,}$ and $\text{HL}_{,}$, or of the vector-differences $\mu_{,}-\xi_{,}$ and $\lambda_{,}-\xi_{,}$, are respectively parallel, by (118.), to the two fixed vectors, ι and κ; or (by what was shown in former articles), to the two cyclic normals, AC$'$ and AC, of the original ellipsoid. The plane of the quadrilateral inscribed in the mean sphere is therefore constantly parallel to the *principal plane* CAC$'$ of that ellipsoid, namely to the plane of the greatest and least axes, which contains those two cyclic normals. The first and third sides, $\text{L}_{,}\text{M}_{,}$ and $\text{L}_{,}'\text{M}_{,}'$, of the same inscribed quadrilateral, being in the directions of $\mu_{,}-\lambda_{,}$ and $\mu_{,}'-\lambda_{,}'$, are parallel, by (118.), (119.), to two other constant vectors, namely $\iota-\kappa$ and $\iota'-\kappa'$, or to the axes AB, AB$'$, of the two cylinders of revolution which can be circumscribed about the same ellipsoid. And the point of intersection of this other pair of opposite sides of the same inscribed quadrilateral is, by (123.), the extremity of the vector ρ, or the point E on the surface of the original ellipsoid; while the point H, which has been already seen to be the intersection of the former pair of opposite sides of the quadrilateral, since it has, by (116.), its vector $\xi_{,}=-b^2\nu$, is the *reciprocal point,* on the surface of that *other* and *reciprocal ellipsoid,* which was considered in article 61; namely the point which is, on that reciprocal ellipsoid, diametrically *opposite* to the point which was named F in that article, and had its vector $=b^2\nu$.

67. Conversely it is easy to see, that the foregoing analysis by quaternions conducts to the following mode of *constructing*†, *or generating, geometrically,* and by a *graphic* rather than by

* A Quaternion, *geometrically* considered, is the *product, or the quotient, of any two directed lines in space.*

† This construction, of two reciprocal ellipsoids from one sphere, was communicated to the Royal Irish Academy in June 1848; together with an extension of it to a mode of generating two reciprocal cones of the second degree from one rectangular cone of revolution; and also to a construction of two reciprocal hyperboloids, whether of one sheet, or of two sheets, from one equilateral hyperboloid of revolution, of one or of two sheets.

a *metric* process, a *system of two reciprocal ellipsoids, derived from one fixed sphere*; and of determining, also *graphically*, for each point on either ellipsoid, the *reciprocal point* on the other.

Inscribe in the fixed sphere a plane quadrilateral $(L_{,}M_{,}L_{,}'M_{,}')$, of which the four sides $(L_{,}M_{,}$, $M_{,}L_{,}'$, $L_{,}'M_{,}'$, $M_{,}'L_{,})$ shall be respectively parallel to four fixed right lines (AB, AC$'$, AB$'$, AC), diverging from the centre (A) of the sphere; and prolong (if necessary) the first and third sides of this inscribed quadrilateral, till they meet in a point E; and the second and fourth sides of the same quadrilateral, till they intersect in another point H. Then *these two points, of intersection* E and H, *thus found from two pairs of opposite sides of this inscribed quadrilateral, will be two reciprocal points on two reciprocal ellipsoids*; which ellipsoids will have a common mean axis, namely that diameter of the fixed sphere which is perpendicular to the plane of the four fixed lines: and those lines, AB, AC$'$, AB$'$, AC, will be related to the two ellipsoids which are thus the loci of the two points E and H, according to the laws enunciated in article 61, in connexion with a different construction of a system of two reciprocal ellipsoids (derived there from one common *moving sphere*); which former construction *also* was obtained by the aid of the calculus of quaternions. Thus the lines AC, AC$'$ will be the two cyclic normals of the ellipsoid which is the locus of E, but will be the axes of circumscribed cylinders of revolution, for that reciprocal ellipsoid which is the locus of H; and conversely, the lines AB, AB$'$ will be the axes of the two cylinders of revolution circumscribed about the ellipsoid (E), but will be the cyclic normals, or the perpendiculars to the cyclic planes, for the reciprocal ellipsoid (H).

[To be continued.]

XLIII. *Notices respecting New Books.*

Letters addressed to H.R.H. the Grand Duke of Saxe Coburg and Gotha, *on the Theory of Probabilities, as applied to the Moral and Political Sciences.* By M. A. QUETELET, *Astronomer Royal of Belgium, Corresponding Member of the Institute of France, &c. &c. Translated from the French by* OLINTHUS GREGORY DOWNES, *of the Economic Life Assurance Society.*

OF this work, which was begun by M. Quetelet in 1837, and published at Brussels, we believe, early in 1845, the author thus describes the object in his preface.

"Certain circumstances, which have left me many pleasant reminiscences, made it necessary for me nearly ten years since to devote my whole attention to the application of the Theory of Probabilities to the study of the moral and political sciences. I then felt how desirable it was that this science should be rendered more *elementary*, and that it should be brought down from the higher regions of analysis, and placed within the reach of those who have most frequently to make use of it. It links itself to numerous questions which interest the legislator and the statesman,—both are often obliged to infer from the statistics of the past what it may be useful to do for the future, and they feel the want of means to enable them to judge of the results produced by modifications of the laws which connect events with each other, and to assign the weight to be assigned to symptoms which announce the adversity or prosperity of a country."

The subject is discussed under several general heads—the Theory of Probabilities—Means and Limits—the Study of Causes—and Statistics—each branching out into subordinate departments. The whole discussion, though perfectly elementary and practical, is a masterly performance. The illustrations are selected with singular judgement; and, for such a subject, the work is a book of very pleasant reading.

We have no doubt that the work will be eminently useful in this country, and that it will make the name of its author known to many who have never before heard of the astronomer of Brussels.

The translation is executed with faithfulness, and is creditable to the taste and judgement of the translator; and we shall rejoice if this is only a foretaste of what we may expect from the same hand.

First Steps to Zoology. By ROBERT PATTERSON. Simms and M^c^Intyre. London, 1849.

In this little volume, Mr. Patterson has presented to the young naturalists of this country an abridgement of his recently published Zoology for Schools. His object in so doing (as appears from the preface) is to convey some knowledge of the natural history and classification of the various animals which inhabit our globe, to a younger class of readers than would easily understand his more extended work above-mentioned. In most instances, accordingly, he has confined himself to giving short notices of the different orders of animals, selecting as individual examples of each, when such could be done conveniently, those which inhabit our own islands and the seas surrounding them. On the whole, the entire range of animated nature is very fairly represented; the vertebrated animals, the birds, beasts and fishes of the old natural history books, preponderating, as indeed, from the fact of their exhibiting the largest amount of intelligence and the greatest number of individual traits of character, must almost necessarily be the case in a popular book.

A large number of woodcuts are inserted, illustrating the different subjects treated of; but the impressions are by no means so good as one could wish; and we think that, if Mr. Patterson would have a little more care bestowed upon the getting up of these illustrations, in case of the appearance of a second edition, his work would be greatly improved. On the whole, however, we can safely recommend it to the notice of our readers.

XLIV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 78.]

Dec. 14, 1848.—"ON the effect of surrounding Media on Voltaic Ignition." By W. R. Grove, Esq., M.A., F.R.S.

The author refers to some experiments of his published in the Philosophical Magazine for December 1845, and in the Bakerian Lecture for 1847, relating to the difference of ignition generated in a platinum wire heated by the voltaic current, when the wire is immersed in atmospheres of different gases. In the present paper these experiments are continued, the current being passed through two platinum wires both in the same voltaic circuit, but immersed in atmospheres of different gases.

It appears from these experiments that the heat generated in the wire is less in hydrogen and its compounds than in other gases; and that when the wires and their atmospheres of gas are immersed in given quantities of water, the water surrounding the hydrogenous gases is less heated than that surrounding those which contain no hydrogen.

Similar experiments, in which the wires are immersed in different liquids, are then given; the heat developed appears not to depend on the specific heat of either the gases or the liquids.

The two series of experiments are brought into relation by one wire being immersed in hydrogen and the other in water, by which it appears that the cooling effect of the hydrogen nearly equals that of water.

Further experiments are then given, in order to ascertain, if possible, to what chemical or physical peculiarity these cooling effects are due; and from them it appears that they are not due to the specific gravity, specific heat, or to any conducting power of the gases for electricity; and that they do not follow the same law as that by which gases escape from minute apertures. They apparently depend upon some molecular character of the gases, by which either the interchange of hot and cold particles is facilitated, or a superficial action takes place, the surface of the hydrogenous gases presenting a more ready escape to the heat, similarly to that which has been long observed with regard to the different molecular constitutions of solid bodies, such for instance as the more rapid radiation or absorption of heat by black than by white surfaces, in the present case the epipolic action being dependent on the surface of the aëriform medium, and not on that of the solid substances.

Jan. 11, 1849.—"Contributions to the Physiology of the Alimentary Canal." By W. Brinton, Esq., M.B. Communicated by R. Bentley Todd, M.D., F.R.S.

The paper consists of two parts, having a real relation to each other, though apparently little connected.

I. *On the Movements of the Stomach.*—The anatomy of its muscular coat is first briefly mentioned, and the so-called oblique fibres of some authors stated to be really transverse, *i. e.* at right angles to the altered direction of the canal.

The muscular actions of the digesting stomach are then considered.

These Haller regarded as alternate contractions in two directions, now forwards, now backwards, forcing the contained food in correspondingly reversed directions, and rested this conclusion on experiment and argument; but the author believes the experiment to be solitary, and not parallel with the fact sought to be established, and the argument to be inconclusive.

Beaumont's views are cited as analogous to Haller's, but are considered as having been by no means clearly stated.

The author indicates an argument from analogy, but chiefly bases his conclusion on the observations of Owen and others on Fishes, and his own observations in animals immediately after death:—in the empty or non-digesting stomach; and in the stomach which contains food; first, in the early stage of digestion; and, secondly, at a later period.

From a contrast of these three states it is found, that in the first there is no movement; in the second and third a considerable one; that in the latter, the opening of the pylorus, and the preponderance of the contractions of the pyloric half of the viscus, constitute its chief *distinction* from the second. The two latter movements are both peristaltic, or *in one direction only*—being *never* reversed, so far as the author has seen.

The movement impressed on the food is next considered. According to the observations of Beaumont and others, the food passes in two directions or streams, forwards and backwards. These observations the author has been unable to repeat, but regards them as established.

Assuming the truth of these observations, and contrasting them with the muscular actions previously stated, it appears that the latter are uniformly in *one* direction, the former in *two*,—an apparent incongruity, which the author next seeks to explain.

By experiment he attempts to imitate the natural conditions, and with the production of the like result. He next offers an explanation and illustration of the fact (which might almost be predicated, *à priori*), and adduces some (possible) analogues from the animal kingdom.

He then seeks to establish a general law—that transverse contractions, occurring in a closed tube filled with fluid, and proceeding in *one* direction only, imply *two* currents; a peripheral of advance, taking the same course as the peripheral contractions; and an axial of return, in the opposite direction.

He next points out the modification of this law for stomachs of human shape, and shows how compatible this is with the careful observations of Beaumont, none of which are essentially opposed to it.

The author indicates a probable modification correlative with the *consistence* of the food in some animals, and thus shows a dependence of this physical process on a previous one.

A solitary experiment is adduced to show that, as in the healthy movement, so also in vomiting, no backward or antiperistaltic contraction necessarily occurs.

A conjecture concerning regurgitation of fluid from the stomach concludes this part of the paper.

II. *On the Physiology of Intestinal Obstructions.*—In the preceding part of the paper it has been stated, that two currents probably obtain in the liquid contents of the stomach. Many of the conditions of the intestinal tube approximate to those of the stomach; and if disease or experiment add to these occlusion and distension, the analogy of the two organs is rendered tolerably complete, and the results will hence probably be referrible to the same general principle.

The most remarkable and constant symptom of this state of obstruction is the occurrence of fæcal vomiting.

The author briefly states the theory of an antiperistalsis by which this phenomenon is ordinarily explained: and from an inquiry into its experimental basis he deduces this general result, that an antiperistaltic movement has never yet been observed in any part of the alimentary canal. He regards the irregular actions seen on laying open the bellies of *healthy* animals recently killed, as not definedly peristaltic or the reverse, but as dependent on the irritation produced by the admitted air. So also, in the case of the *occluded* intestine, an inverted movement likewise fails to be recognized. In general, the vermicular actions are more energetic, and more peristaltic, than in the healthy bowel.

He next adduces the following arguments:—

1. The antiperistalsis is usually attributed to irritation; but irritation is present in almost every disease of the tube, while fæcal vomiting is limited to cases of obstruction. This renders it probable that the latter is the cause, and that the process of causation is, like the cause, *physical.*

2. The starting-point of the supposed inverted movement is the fullest part of the bowel, while the place towards which it has set is the emptiest. This condition is inconsistent with the supposition of an antiperistalsis, yet perfectly consistent with a forward movement, and analogous to the obstructions of other tubes conveying fluids.

3. Intus-susception is often the cause of obstruction. But, both from experiment and argument, it appears probable, that an antiperistalsis would at once remove this condition, and would therefore be incompatible with it.

4. The supposed inverted movement is continuous, while the vomiting is occasional. Hence a theory which showed the essential independency of the return of fæcal matters to the stomach, and their ejection thence, would be, so far, preferable.

5. Experiment and observation agree in showing that the ordinary peristalsis obtains immediately below the strangulation. And it is difficult to imagine how or why the same irritation should produce *two opposite* movements in *reversed* directions.

6. The general and comparative date of accession of the vomiting is scarcely compatible with the antiperistaltic theory.

The author next adduces experiments in which the intestine of animals was artificially occluded by a ligature. In exceptional cases, the ligature sloughed into the canal, and the obstruction was thus destroyed. In all others, the tube was distended *above* the stricture to a variable extent. *Below* the stricture, the intestine was usually empty and contracted for an inch or two. The *contents* of the tube varied both in quality and quantity; uniform fluidity being associated with a large quantity of contents, while their smaller amount was often attended with differences of consistence. The date at which the vomiting acceded varied considerably. In one or two instances this symptom did not occur at all. These differences appeared mainly dependent on—

1. The amount of fluid ingesta,
2. The distance of the stricture from the stomach.

The date of death seemed to vary chiefly with the degree of distension.

He therefore deduces the theory,—That, in an obstructed intestine, a movement of the ordinary (and probably peristaltic) character propels the contents onwards to the seat of occlusion; that a continuance of the process distends, first this part of the tube, and next, those portions above it; that, if the contents are fluid, the ordinary peristalsis tends to develope an axial and reversed current, which returns matter from a lower to a higher point of the intestine;—often from the obstruction to the stomach, whence they are ejected by vomiting.

That in some cases, however, the action is probably much less perfect than this; the consistence of the contents preventing the perfection of these currents throughout the whole course of the tube. But still a mixture results, although a less intimate one.

The author next glances at the mode in which obstruction appears to affect peristalsis, and the nature of the distending fluid. He compares the obstructed intestine to the healthy stomach; to the obstructed artery and duct; referring its peculiar appearances to the dilatable yet muscular structure of its coats.

In conclusion, he indicates the possible result of this theory on practical medicine.

"On the Determination of the Difference of Longitude, by means of the Magnetic Telegraph." By Elias Loomis, Esq., in a Letter to Lieut.-Col. Sabine, R.A., For. Sec. R.S. Communicated by Lieut.-Col. Sabine, R.A., For. Sec. R.S.

The writer first refers to a series of experiments made under the direction of Professor Bache, for the determination of the difference of longitude between New York, Philadelphia and Washington, by means of the magnetic telegraph. By this series of experiments he considers it established that, by means of Morse's telegraph, two clocks distant from each other 200 miles, can be compared together with the same precision as if they were placed side by side; and that the difference of longitude of two places can be determined with the same precision as the relative error of the clocks. These results were so satisfactory that Professor Bache determined to prosecute them more extensively, and during the past summer comparisons have been made between New York and Cambridge observatory near Boston. The plan of operation this season was more matured than during the former. The comparisons were all made between a solar chronometer at Cambridge and a sidereal clock at New York. At ten o'clock in the evening, the two observatories having been put in telegraphic communication, when the seconds hand of the solar chronometer came round to 60^s, a signal was given at Cambridge, by pressing the key of the telegraph-register; at the same instant a click was heard at New York, and the time was recorded according to the sidereal clock. At the end of 10^s a second signal was given, which was also recorded at New York; at the end of another 10^s a third signal was given, and so on for sixty seconds. The Cambridge astronomer then commenced beating seconds by striking the key of the telegraph-register in coincidence with the beats of his chronometer. The New York astronomer compared the signals received with the beats of his clock, and waited for a coincidence. When the beats were sensibly synchronous the time was recorded, and the astronomer waited six minutes for another coincidence of beats. The Cambridge astronomer continued beating seconds for *fifteen minutes*, during which time the New York observer was sure of two coincidences, and might obtain three. When these were concluded, the New York astronomer in the same manner gave signals for one minute at intervals of 10^s, and then beat seconds for fifteen minutes, during which time the Cambridge astronomer obtained four or five coincidences upon his chronometer. This mode of comparison was practised every night, and it is considered that the uncertainty in the comparison of the time-pieces cannot exceed two or three hundredths of a second on any night; and in a series of comparisons the error may be regarded as entirely eliminated.

Another mode of comparison which was practised is that of telegraphing star transits. A list of stars which culminate near our zenith at intervals of five or six minutes was prepared, and the observers, both at New York and Cambridge, were furnished with a copy. They then proceeded as follows: Cambridge selected two stars from the list, which we will call A and B, and struck the key of his register at the instant when the star A passed each of the seven wires of his transit. These signals were heard at New York, and the times recorded. Cambridge then observed the transit of star B in the ordinary manner without telegraphing. New York then observed the transit of star A on his meridian in the usual manner; and struck his key at the instant the star B passed each of the seven wires of his transit, which signals were heard and recorded at Cambridge. The difference of longitude between New York and Cambridge is nearly twelve minutes, affording ample time for all these observations. Thus New York obtained upon his own clock the times of transit of star A over the meridians of Cambridge and New York; and Cambridge obtained upon his chronometer the times of transit of star B over the same meridians. The difference of these times gives the difference of longitude independent of the right ascension of the stars. Both observers then reversed the axis of their transit instruments; Cambridge selected a second pair of stars from the list, and the same series of observations was repeated as with the first pair. The error of collimation was thus eliminated, and by confining the observations to stars within about five degrees of the zenith, the influence of azimuthal error was avoided. The level being read at every reversal, the correction for it was applied by computation. In this manner it is hoped to eliminate every possible source of error, except that which arises from the personal habits of the observers. In order to eliminate this error, a *travelling* observer worked for a time at Cambridge and compared with the Cambridge astronomer; then came to New York and compared with the New York astronomer; then returned to Cambridge again, and so on as often as was thought necessary. Finally, at the conclusion of the campaign all the observers were to meet at Cambridge and make a general comparison of their modes of observation.

On one or two nights the preceding programme was changed, and each observer telegraphed both star A and star B.

"On the peculiar cooling effects of Hydrogen and its compounds in cases of Voltaic Ignition." By W. F. Stevenson, Esq., F.R.S.

In this communication the author gives several theorems which he considers to be established by the experiments cited in a pamphlet which he published, entitled "The Non-decomposition of Water distinctly proved." He then states, that when we apply the principle of these theorems to Mr. Grove's discovery of the cooling properties of hydrogen, it will be found to admit of a most simple solution: "for instance, when the coil of platinum wire is connected with the poles of the electric battery, and the current is established, it is evident that the electric matter thus passed through the wire must escape at the contrary end (the air with which the wire is surrounded not being a conductor of electricity), and as the quantity of electric matter thus transmitted is considerable, and its exit from the wire confined but rapid, that commotion before noticed (in one of the author's theorems) necessarily ensues and causes the ignition of the wire; but when the coil of wire is immersed in hydrogen, which is a conductor of electricity, it is evident that the electric matter must be, at the same moment, abstracted or conducted from every portion of the wire, and consequently the commotion or rush of the electric matter at the extremity of the wire, which causes the ignition, is suspended and the comparative coolness of the wire is the necessary result."

Postscript to a paper "On the Ganglia and Nerves of the Heart," with two drawings. By Robert Lee, M.D., F.R.S.

The author states that since his former communication was presented to the Royal Society he has made a very minute dissection in alcohol of the whole nervous system of the young heifer's heart.

In this preparation the distribution of the ganglia and nerves over the entire surface of the heart, and the relations of these structures to the blood-vessels and muscular substance, are considered by the author to be far more fully displayed than in any of his former dissections. He states, that on the anterior surface there are distinctly visible to the naked eye, ninety ganglia or ganglionic enlargements on the nerves, which pass obliquely across the arteries and the muscular fibres of the ventricles from their base to the apex; that these ganglionic enlargements are observed on the nerves, not only where they are crossing the arteries, but where they are ramifying on the muscular substance without the blood-vessels; that on the posterior surface the principal branches of the coronary arteries plunge into the muscular substance of the heart near the base, and many nerves with ganglia accompany them throughout the walls to the lining membrane and columnæ carneæ.

The author considers that, in the accompanying beautiful drawings, Mr. West has depicted with the greatest accuracy and minuteness the whole nervous structures demonstrable in this preparation on the surface of the heart; but that the ganglia and nerves represented in these drawings constitute only a small portion of the nervous system of the heart, numerous ganglia being formed in the walls of the heart which no artist can represent.

"On the Aurora Borealis which occurred on the evening of Friday, the 17th of November, 1848." By Mr. R. Smith, Blackford, Perthshire. Communicated by P. M. Roget, M.D., F.R.S.

The author states that the 17th of November was a fine day with a clear sky and bright sunshine: towards evening the sky became cloudy and a few drops of rain fell, but it soon again became clear, with the exception of a few fleecy clouds that here and there dimmed its brightness. At 6^{h} 45^{m} a soft and gentle light began to illumine the northern region of the sky; and at 7 o'clock a considerable portion of it was covered with dark-red streams of light towards the east; while streamers moving to and fro, arrayed in colours of golden and silvery hues, overspread the south and north. About 8 o'clock there appeared near the zenith, and upon the magnetic meridian, a ring of an elliptical form, from which proceeded in all directions towards the horizon, beams or columns of light, giving to the heavens the appearance of a splendid vault, with its top adorned with a crown or wreath; while around and within the vault were to be seen clouds of brilliant light flashing towards and from the crown or central circle of the aurora, sometimes tinged with prismatic rays, at other times intensely white and lucid. About half-past nine nearly the whole of the aërial canopy was clad with clouds of a bright red colour, casting a curious reddened hue over the objects on the surface of the earth. After a short period of time had elapsed, the red colour began to diminish in intensity, and was again replaced by the white dome. However, in various parts of the sky the red colour still remained, principally in the north-west, south-west, and north-east. Between the hours of twelve and one beams of brilliant white light commenced shooting up in the south from the horizon to the central ring or pole. The beams appeared to be at nearly equal distances from each other, the entire column of them stretching over a space equal to about one-fifth part of the visible horizon, in the form of a fan. The whole figure rapidly changed from a pure white light into a glow of brilliant colours of every tint, variegating the undulating waves as they rolled on their way to the pole of the aurora. In the course of three minutes these gave place to the white flashing radiations.

During the time of the aurora there were a great number of small meteors, the direction of whose motion was from east to west, and which appeared to be considerably below the sphere of the aurora.

A box containing a delicately balanced needle, was exposed upon the ground during the display of the aurora, but did not appear to be affected in the slightest degree till about one o'clock, when it was observed to be considerably deflected. At the time when the needle was disturbed, there was a dense column of radiating light in the north-west and south-east. The reflexion from the north-west was so clear, that when made to fall upon the polarizing plate of M. Biot's polarizing apparatus, and a film of mica was placed upon the stage of the instrument, the various colours produced by the mica were beautifully clear and distinctly seen in the analysing glass.

The author considers that the phenomenon of the colours which were noticed, was probably caused by exhalations or vapour floating in the atmosphere, betwixt the light of the aurora and the observer, causing a refraction of the rays transmitted to the eye, analogous to that which produces the phenomenon of halos. The continued undulations of the auroral light, and also the passing of the rays through thick and thin portions of the vapour, may, he considers, have produced the great variety of colours. During the time of the exhibition of this phenomenon, a thin fog or vapour was observed on both sides of the auroral fan. The author is of opinion that the cause which produced the variety of tints, is different from that which occasioned the red-coloured auroral clouds. At the time of the latter phenomenon the moon's position was nearly due east, and a cloud moved from the west towards the east, which in its course passed between the moon and the observer; as soon as the cloud obscured the light of the moon, the red colour to the north-west disappeared, but became visible when an opening in the cloud allowed the rays to pass through, and again vanished when another portion of the cloud cut off the light; and when the cloud had finally passed over, the red colour in the different parts of the sky resumed the same tint that it possessed before the moonlight was obscured by the cloud. The author states that it would thus appear, that when the light of the moon was incident at a certain angle upon the white light, or some kind of vapour that surrounded it, a red colour was produced; and hence that the moon is in some way or other connected with the phenomenon. He remarks, that the red colour was first observed in the east, and the moon being in that quarter of the heavens, the rays proceeding from it would first come in contact with that part of the aurora towards the east. When the aurora commenced, the moon was considerably below the horizon; but this, it is considered, does not form any serious objection to what has been stated, since the aurora soared to so great a height, that the rays of light proceeding from her would strike the aurora a considerable time before she arose above the horizon.

The aurora continued for upwards of six hours, and during that time the thermometer stood at 34°.

Jan. 18.—"On the Development and Homologies of the Carapace and Plastron of the Chelonian Reptiles." By Professor Owen, F.R.S.

The author commences by defining the several parts of which the osseous thoracic-abdominal case of the Chelonian Reptiles is composed, and briefly discusses the several opinions that have been published with regard to their nature and homologies, dwelling chiefly on that recently proposed by Prof. Rathké, in his work on the Development of the *Chelonia*, in which it is contended that the carapace consists exclusively of the development of parts of the endo-skeleton, viz. the neural spines and vertebral ribs (*pleurapophyses*), agreeably with the opinion of Cuvier and Bojanus, and that the remainder of the thoracic-abdominal case, consisting of the "marginal pieces" and "plastron," are formed entirely from bones of the dermal system.

Adverting to the hypotheses of Cuvier, Geoffroy and Meckel, that the thoracic-abdominal case is a modification of parts of the endo-skeleton exclusively, the author tests their determinations by comparisons with the corresponding parts of the bird and crocodile, and infers, from the latter animal, that the hyosternal, hyposternal and xiphisternal bones are not parts of the sternum, but are homologous with the hæmapophyses (sternal ribs and abdominal ribs); those in the *Plesiosaurus* making the nearest approach to the peculiar development of the parts in the *Chelonia*, especially as they appear in the plastron of the immature Terrapenes and Sea-turtles.

Admitting that any hypothesis framed from the comparison of the completed structures in the adult Vertebrata requires for confirmation its agreement with the important phenomena of the development of those structures, the author proceeds to apply that test.

He details his observations on the development of the skeleton, and especially of the thoracic-abdominal case, in the embryos and young of different genera of *Chelonia*. The chief facts that have governed his conclusions are the following:—

With respect to the carapace. The cartilaginous basis of the neural plates is developed in the substance of the derm; and of these, the 9th, 10th, 11th, and the 'nuchal' plate are ossified from independent centres, and remain permanently free from anchylosis with the subjacent spines of the vertebræ: they are, therefore, "dermal bones," homologous with those that overlie the vertebræ of the crocodile. But the first to the eighth neural plates inclusive are serial homologues with the foregoing, and must, therefore, have the same general homology. The objection that ossification extends into their dermal cartilaginous basis from the neural spines is met by the remark, that other parts, *e.g.* the radius and ulna of the frog, are ossified from a common centre, without their homological distinctness being thereby masked or destroyed. The course or starting-point of ossification does not determine the nature and homology of parts, and the author refers what he believes to be an erroneous conclusion of Prof. Rathké to undue value being given to the character of connation.

The cartilaginous basis of the costal plates is developed in the substance of the derm; the subjacent ribs are previously ossified and present the normal slender form. But ossification extends from near the head of each of the eight pairs of dorsal ribs, from the second to the ninth pair inclusive, into the superincumbent dermal cartilages. This had been described as the development of the tubercle of the rib. But Prof. Owen observes that, in the development of the carapace of the young of the *Testudo indica*, the connation of the costal plate with the rib commences at a different point in each rib alternately, and appears to be governed by the arrangement of the horny scutes above. Another objection to these ossific expansions being the tubercles of the ribs is presented by their abutment mesially against the neural plates, not against the vertebral diapophyses, as in the bird and crocodile.

In regard to the development of the plastron, the author describes two situations in which the primitive cartilages are developed, corresponding with those in the embryo-carapace, viz. one belonging to the endo-skeleton, the other in the derm. The first form under which the endo-skeletal parts of the plastron appear agrees with the evidence afforded by the comparison of the fully-developed parts with those of the crocodile, and proves the hyosternals, hyposternals and xiphisternals to be 'hæmapophyses' or abdominal ribs: the hyosternals and hyposternals are primitively long, slender, transverse bars, which join the vertebral ribs in the Tortoises and Terrapenes, without the intervention of any marginal pieces. The ossification of the superadded dermal portions proceeds from the previously ossified endo-skeletal elements.

The author concurs with M. Rathké in regarding the marginal pieces as 'dermal bones,' and concludes by a full discussion of the facts and arguments which have led him to a different conclusion respecting the nature and homologies of the carapace and plastron.

The memoir is illustrated by figures of the carapace and plastron, and of the corresponding segments of the skeleton in the bird and crocodile, and of the development of the thoracic-abdominal case in land- and sea-chelonians.

Jan. 25.—Some remarks on a paper entitled "On the Depth of Rain which falls in the same localities at different Altitudes in the Hilly districts of Lancashire, Cheshire, &c., by S. C. Homersham, C.E." By John Fletcher Miller, Esq. Communicated by Lieut.-Col. Sabine, R.A., For. Sec. R.S.

The author, after alluding to the discordance between the conclusions at which he had arrived from a discussion of his meteorological observations in the lake district of Cumberland and Westmoreland, described in a former paper, and those drawn from the same facts by Mr. Homersham, in a paper read before the Society on the 25th of May last, states that the results for the year 1848 show a precisely similar gradation to those of the two preceding years; and that the whole of the observations appear to warrant the conclusion which he had ventured to draw from those detailed in his former paper.

He remarks that, as the rain-gauges are, with one exception, situated on the high mountains surrounding the head of the Vale of Wastdale, this valley is the only one which can fairly be selected as a standard in comparing the quantities of rain obtained at the different mountain stations. The discordance between his conclusions and those arrived at by Mr. Homersham, he considers, has arisen from that gentleman having selected the distant and excessively wet locality of Seathwaite at the head of the southern fork of Borrowdale, as a representative of the quantity of water deposited in the valleys generally.

If the receipts of the mountain gauges, he observes, be compared with the rain-fall at Wastdale Head, or in any of the other valleys except Seathwaite, it will be found that the quantity *increases* considerably up to 1900 feet, where it reaches a maximum; and that above this elevation it rapidly decreases, until at 2800 feet above the sea the amount is very much *less* than in the surrounding valleys.

In conclusion, the author states that it appears to him, that much of the discordance in the results obtained at various elevations amongst the mountains has arisen from the circumstance of the instruments having been placed on the slope or breast of the hill nearly in a line with each other; in which positions, he is convinced from experience, that when strong winds prevail, the gauges are exposed to eddies or counter-currents, which prevent a portion of the water from entering the funnel, and thus a less depth of rain is obtained than is due to the elevation.

The gauges under his superintendence being all stationed either on the top or shoulder of the mountain, and exposed to the wind from every point of the compass, are not, he observes, open to this objection.

Supplement to a paper "On the Theory of certain Bands seen in the Spectrum." By G. G. Stokes, Esq., M.A., Fellow of Pembroke College, Cambridge. Communicated by the Rev. Baden Powell, M.A., F.R.S.

The principal object of the author in this communication is to point out some practical applications of the interference bands recently discovered by Professor Powell, the theory of which was considered by the author in the paper to which the present is a supplement. The bands seem specially adapted to the determination of the dispersion in media which cannot be procured in sufficient purity to exhibit the fixed lines of the spectrum. The ordinary experiments of interference allow of the determination of refractive indices with great precision; but in attempting to determine in this way the dispersion of the retarding plate employed, there is the want of a definite object to observe in connection with the different parts of the spectrum. In Professor Powell's experiment, the wire of the telescope, placed in coincidence with one of the fixed lines of the spectrum previously to the insertion of the retarding plate into the fluid, marks the place of the fixed line, and so affords a definite object to observe when the retarding plate is inserted into the fluid, and the spectrum is consequently traversed by bands of interference.

The practical applications considered by the author are principally four. In the first, the variation of the refractive index of the plate in passing from one fixed line to another is determined, the absolute refractive index for some one fixed line being supposed accurately known. The observation consists in counting the number of bands seen between two fixed lines of the spectrum, the fractions of a band-interval at the two extremities being measured or estimated.

In the second application, the absolute refractive index of the plate is determined for some one fixed line of the spectrum. The observation consists in counting the number of bands which move across the wire of the telescope, previously placed in coincidence with the fixed line in question, when the plate is inclined to the incident light.

The third application is to the determination of the change in the refractive index of the fluid, for any fixed line of the spectrum, produced by a change in the temperature. The observation consists in counting the number of bands which move across the wire of the telescope while the temperature sinks from one observed value to another, the temperature being noted by means of a delicate thermometer which remains in the fluid. For this observation a knowledge of the refractive index of the retarding plate is not required.

The fourth application is to the determination of the change of velocity of the light corresponding to any fixed line of the spectrum, when the direction of the refracted wave changes with reference to certain fixed lines in the plate, which is here supposed to belong to a doubly refracting crystal. The observation consists in counting the bands as they pass the wire when the plate is inclined. It requires that the plate should be mounted on a graduated instrument. It would be possible in this way to determine, by observation alone, the wave surface belonging to each fixed line of the spectrum.

While considering the theory of Professor Powell's bands, the author was led to perceive the explanation of certain bands, described by Professor Powell, which are seen in the secondary spectrum formed by two prisms which produce a partial achromatism. Although the account of these bands has been published many years, they do not seem hitherto to have attracted attention. It is easily shown by common optics that when two colours are united by means of two prisms, the deviation, regarded as a function of the refractive index, the angle of incidence being given, is a maximum or minimum for some intermediate colour. For the latter colour, two portions of light of consecutive degrees of refrangibility come out parallel; and therefore the diffraction bands belonging to different kinds of light, of very nearly the same refrangibility with the one in question, are superposed in such a manner that the dark and bright bands respectively coincide. Thus distinct bands are visible in the secondary spectrum, although none would be seen in the spectrum formed by a single prism, in consequence of the mixture of the bright and dark bands belonging to different kinds of light of nearly the same degree of refrangibility. The diffraction bands here spoken of are of very sensible breadth, in consequence of the small width of the aperture employed in the actual experiment.

When a spectrum is viewed through a narrow slit half covered by a plate of mica, the edge of which bisects the slit longitudinally, and is held parallel to the fixed lines of the spectrum, the bands described by Sir David Brewster are seen, provided the mica plate lie at the side at which the blue end of the spectrum is seen, and provided the thickness of the plate and the breadth of the slit lie within certain limits. When these bands are invisible in consequence of the slit being too narrow, or the spectrum too broad, it follows from theory that the bands ought to appear when the slit and plate are turned round the axis of the eye, so that the edge of the plate is no longer parallel to the fixed lines of the spectrum. The author has verified this conclusion by experiment, employing plates adapted to observations with the naked eye, which are best suited to the purpose.

Feb. 1.—"On the Chemistry of the Urine;" in three Parts. By H. Bence Jones, M.D., M.A., F.R.S.

Part I. *On the variations of the Acidity of the Urine in Health.*

The mode of examination adopted by the author was the following: Two test solutions were made; the one with carbonate of soda; the other with dilute sulphuric acid, of such strength that each measure of a graduated tube, when filled with either solution, was equivalent to one-twelfth of a grain of dry and pure carbonate of soda.

A weighed quantity of urine was neutralized by one or other of the test solutions, and thus the degree of acidity or alkalescence was determined.

Diurnal variations in the acidity of the urine were observed. The acidity of the urine was found to ebb and flow; it was greatest a short time before food was taken, and was least about three hours after breakfast, and five or six hours after dinner, when it reached the minimum point; after which it again increased, and attained the maximum point previous to food being again taken.

If no food was taken, the acidity varied but slightly for twelve hours.

By comparing the effect of vegetable food with animal food, it appeared that the food which irritated the stomach most and caused most secretion of acid in the stomach, caused the greatest oscillations in the urine.

Dilute sulphuric acid taken in large doses produced but little effect on the variations of the acidity of the urine; but it was proved to increase the acidity of the urine.

Part II. *On the simultaneous variations of the amount of Uric Acid and the Acidity of the Urine in a healthy state.*

The result of these experiments is, that there is no relation between the acidity of the urine and the amount of uric acid in it. The urine that was most acid contained least uric acid; that which contained most uric acid was not most acid. All food causes an increase in the amount of uric acid in the urine; and there is no decided difference between vegetable and animal food, either as to the increase or diminution of the amount of uric acid in the urine.

Part III. *Variations of the Sulphates in the Urine in the healthy state, and on the influence of Sulphuric Acid, Sulphur and the Sulphates, on the Sulphates in the Urine.*

The result of these experiments is, that the sulphates in the urine are much increased by food, whether it be vegetable or animal. Exercise does not produce a marked increase in the sulphates. Sulphuric acid, when taken in large quantity, increases the sulphates in the urine. In small quantity, even when long-continued, no effect on the amount of sulphates is manifest.

Sulphur taken as a medicine increases the sulphates in the urine. Sulphate of soda and sulphate of magnesia produce the most marked increase in the sulphates in the urine.

Feb. 8.—"On the application of the Theory of Elliptic Functions to the Rotation of a Rigid Body round a Fixed Point." By James Booth, L.L.D., F.R.S.

In the introduction to his investigation, the author, after noticing the investigations of D'Alembert and Euler, and the solution of this problem by Lagrange, refers more particularly to the memoir of Poinsot, in which the motion of a body round a fixed point, and free from the action of accelerating forces, is reduced to the motion of a certain ellipsoid whose centre is fixed, and which rolls without sliding on a plane fixed in space; and likewise to the researches of Maccullagh, in which, by adopting an ellipsoid the reciprocal of that chosen by Poinsot, he deduced those results which long before had been arrived at by the more operose methods of Euler and Lagrange; observing, however, that it is to Legendre that we are indebted for the happy conception of substituting, as a means of investigation, an ideal ellipsoid having certain relations with the actually revolving body. He then states, that several years ago he was led to somewhat similar views, from remarking the identity which exists between the formulæ for finding the position of the principal axes of a body and those for determining the symmetrical diameters of an ellipsoid; and further observing that the expression for the perpendicular from the centre on a tangent plane to an ellipsoid, in terms of the cosines of the angles which it makes with the axes, is precisely the same in form as that which gives the value of the moment of inertia round a line passing through the origin. Guided by this analogy, he was led to assume an ellipsoid the squares of whose axes should be directly proportional to the moments of inertia round the coinciding principal axes of the body. This is also the ellipsoid chosen by Maccullagh. Although it may at first sight appear of little importance which of the ellipsoids—the *inverse* of Poinsot, or the *direct* of Maccullagh and the author—is chosen as the geometrical substitute for the revolving body, it is by no means a matter of indifference when we come to treat of the properties of the integrals which determine the motion. Generally those integrals depend on the properties of those curves of double flexure in which cones of the second degree are generally intersected by concentric spheres; and it so happens that the direct ellipsoid of moments is intersected by a concentric sphere in one of these curves. By means of the properties of these curves a complete solution may be obtained even in the most general cases, to which only an approximation has hitherto been made.

In the first section of the paper, the author establishes such properties as he has subsequently occasion to refer to, of cones of the second degree, and of the curves of double curvature in which these surfaces may be intersected by concentric spheres, some of which he believes will not be found in any published treatise on the subject. He considers that he has been so fortunate as to be the first to obtain the true representative curve of elliptic functions of the first order. It is shown that any spherical conic section, the tangents of whose principal semiarcs are the ordinates of an equilateral hyperbola whose transverse semiaxis is 1, may be rectified by an elliptic function of the first order; and the quadrature of such a curve may be effected by a function of the same order, when the cotangents of the halves of the principal arcs are the ordinates of the same equilateral hyperbola.

This particular species of spherical ellipse the author has called the "Parabolic Ellipse," because, as is shown in the course of the investigation, it is the gnomonic projection, on the surface of a sphere, of the common parabola whose plane touches the sphere at the focus. As in this species of spherical ellipse either the focus or the centre may be taken as the origin of the spherical radii vectores, in effecting the process of rectification, we are unexpectedly presented with Lagrange's scale of modular transformations, as also with the other equally well-known theorem by which the successive amplitudes are connected. Among other peculiar properties of the spherical parabolic ellipse established in this paper, it is shown that the portion of a great circle touching the curve, and intercepted between the perpendicular arcs on it from the foci, is always equal to a quadrant.

In the second and following sections, the author proceeds to discuss the problem which is the immediate subject of the paper. Having established the ordinary equations of motion, he shows that, if the direct ellipsoid of moments be constructed, the motion of a rigid body acted on solely by primitive impulses may be represented by this ellipsoid moving round its centre, in such a manner that its surface shall always pass through a point fixed in space. This point, so fixed, is the extremity of the axis of the plane of the impressed couple, or of the plane known as the invariable plane of the motion.

But a still clearer idea of the motion of such a body is presented in the subsequent investigations, it being there shown, that the most general motion of a body round a fixed point may be represented by a cone rolling with a certain variable velocity on a plane whose axis is fixed, while this plane revolves about its own axis with a certain uniform velocity. This cone may always be determined. For the circular sections of the invariable cone coincide with the circular sections of the ellipsoid of moments; whence the cyclic axes of the ellipsoid, or the diameters perpendicular to the planes of these sections, will be the focal lines of the supplemental cone; and as the invariable plane is always a tangent plane to this cone, we have sufficient elements given to determine it.

From these considerations it appears that we may dispense altogether with the ellipsoid of moments, and say that if two right lines be drawn through the fixed point of the body in the plane of the greatest and least moments of inertia, making angles with the axis of greatest moment, the cosines of which shall be equal to the square root of the expression

$$\frac{L(M-N)}{M(L-N)},$$

(L, M, N being the symmetrical moments of inertia round the principal axes) and a cone be conceived having those lines as focals, and touching moreover the invariable plane, the motion of the body will consist in the rotation of this cone on the invariable plane with a variable velocity, while the plane revolves round its own axis with an uniform velocity.

Although it is very satisfactory, the author remarks, in this way to be enabled to place before our eyes, so to speak, the actual motion of the revolving body, yet it is not on such grounds that the paper is presented to this Society. It is as a method of investigation that it must rest its claims to the notice of mathematicians; as a means of giving simple and elegant interpretations of those definite integrals on the evaluation of which the dynamic state of a body at any epoch can alone be ascertained.

In these applications of the theory of elliptic functions, the author has been led to the remarkable theorem, that the length of the spiral, between two of its successive apsides, described in absolute space on the surface of a fixed concentric sphere, by the instantaneous axis of rotation, is equal to a quadrant of the spherical ellipse described on an equal sphere moving with the body, by the same instantaneous axis of rotation.

The last section of the paper is devoted to the discussion of that particular case in which the axis of the invariable plane is equal to the mean semiaxis of the ellipsoid of moments.

XLV. *Intelligence and Miscellaneous Articles.*

ON ANHYDROUS NITRIC ACID. BY M. DEVILLE.

M. DUMAS presented to the Academy in the name of M. Deville, Professor at the Faculty of Sciences of Besançon, the first results of his researches on the action of chlorine on the anhydrous salts which oxide of silver forms both with organic and inorganic acids.

By treating nitrate of silver with perfectly dry chlorine, M. Deville has succeeded in isolating anhydrous nitric acid, the existence of which was demonstrated by numerous analyses. This beautiful substance is obtained in colourless crystals, which are perfectly brilliant and limpid, and may be procured of considerable size; when they are slowly deposited in a current of gas rendered very cold, their edges are a centimetre in length. These crystals are prisms of six faces, which appear to be derived from a right prism with a rhombic base. They melt at a temperature not much exceeding 85°·5 F.; their boiling-point is about 113°; at 50° the tension of this substance is very considerable. In contact with water it becomes very hot, and dissolves in it without imparting colour, and without disengaging any gas; it then produces with barytes the nitrate of that base. When heated its decomposition appears to commence nearly at its boiling-point; this circumstance is an obstacle to the determination of the density of its vapour by the process of M. Dumas.

The process by which M. Deville obtained anhydrous nitric acid is very simple; but the readiness with which it penetrates tubes of caoutchouc renders it necessary to unite all the pieces of the apparatus by melting them. The following is the process:—the author employs a U-shaped tube capable of containing 500 grs. of nitrate of silver, dried in the apparatus at 356° F. in a current of dry carbonic acid gas. Another very large U-tube is connected with this, and to its lower part is attached a small spherical reservoir; it is in this reservoir that a liquid is deposited which always forms during the operation, and which is excessively volatile (nitrous acid ?). The tube containing the nitrate of silver is immersed in water covered with a thin stratum of oil, and heated by means of a spirit-lamp communicating with a reservoir at a constant level. The chlorine issues from a glass gasometer, and its displacement is effected by a slow and constant flow of concentrated sulphuric acid. The chlorine must afterwards pass over chloride of lime, and then over pumice-stone moistened with sulphuric acid. At common temperatures no effect appears to be produced. The nitrate of silver must be heated to 203° F., the temperature being then quickly reduced to 136° or 154°, but not lower. At the commencement, hyponitrous acid, distinguishable by its colour and ready condensation, is produced; and when the temperature has reached its lowest point, the production of crystals begins, and they soon choke the receiver cooled to 6° below zero; they are always deposited upon that part of the receiver which is not immersed in the freezing mixture, and M. Deville states that ice alone is sufficient to occasion their formation.

The gases are coloured, and the small sphere of the cooled tube contains a small quantity of liquid, which must be taken from the apparatus before the nitric acid is removed to another vessel; this latter operation is readily effected by replacing the current of chlorine by one of carbonic acid. The condenser is then to be no longer cooled, and the vessel for receiving the crystals is to be immersed in a freezing mixture; this is fastened to the producing apparatus by means of a caoutchouc tube furnished with amianthus. The chlorine should pass very slowly, at the rate of about three or four litres in twenty-four hours. All the gas however is not absorbed by the nitrate of silver. Oxygen is evolved, the volume of which appears to be equal to that of the chlorine employed. An apparatus thus constructed operates day and night without watching, care being however taken to renew the sulphuric acid which displaces the chlorine, the spirit of the lamp, and the ingredients of the freezing mixture.

The author states that he shall forward hereafter a more complete memoir, in which he will describe the chemical properties of the anhydrous nitric acid, and detail the results of his researches on the action of chlorine and hypochlorous acid on the salts of silver.—*L'Institut,* Fevrier 21, 1849.

ON A SERIES OF ORGANIC ALKALIES HOMOLOGOUS WITH AMMONIA. BY A. WURTZ.

The history of the ammoniacal compounds, so complete and so important in a theoretical point of view, forms in some measure a transition between inorganic and organic chemistry. Ammonia should decidedly be regarded as the most simple and the most powerful of the organic bases; and it would be for all chemists the type of that numerous class of bodies, did it not differ in one undoubtedly important character, but to which an exaggerated value has been attributed. Ammonia contains no carbon. This difference however of composition does not suffice, in my opinion, to separate ammonia from the organic bases; I have succeeded, in fact, in converting this alkali into a true organic compound, by adding to it the elements of the hydrocarbon $C^2 H^2$ or $C^4 H^4$, without depriving it of its characters of a powerful base, or of its most striking properties, for instance its odour. By adding to the elements of ammonia, NH^3, the elements of 1 equiv. of methylene, $C^2 H^2$, the compound $C^2 H^5 N$, which may be called methylic ammonia, is obtained. By adding to ammonia the elements of ethylene, $C^4 H^4$, ethylic ammonia, $C^4 H^7 N$, is obtained.

The compounds $C^2 H^5 N$ and $C^4 H^7 N$ may be viewed as methylic æther, $C^2 H^3 O$, and ordinary æther, $C^4 H^5 O$, in which the equivalent of oxygen is replaced by 1 equiv. of amidogen, NH^2; or as ammonia in which 1 equiv. hydrogen is replaced by methylium, $C^2 H^3$, or ethylium, $C^4 H^5$. The following formulæ will exhibit the relations which exist between these substances and ammonia:—

$H^3 N$, ammonia.	NH^2, H, *hydramide.*
$C^2 H^5 N$, methylic ammonia.	$NH^2, C^2 H^3$, *methylamide.*
$C^4 H^7 N$, ethylic ammonia.	$NH^2, C^4 H^5$, *ethylamide.*

I shall employ in preference the names methylamide and ethylamide to designate these new bases.

In the present communication I shall restrict myself merely to communicating the circumstances under which these substances are produced, and to communicating the results of some analyses which establish their composition. They are produced under three different circumstances,—1st, by the action of potash on *cyanic æthers*; 2nd, by the action of potash upon *cyanuric æthers*; and 3rd, by the action of potash upon the *ureas*. These reactions will be best exhibited by a few formulæ:—

$$C^2 NO, HO + 2KO + 2HO = 2CO^2, KO + H^3 N$$
Cyanic acid. Ammonia.

$$C^2 NO, C^2 H^3 O + 2KO + 2HO = 2CO^2, KO + C^2 H^5 N$$
Cyanate of methylene. Methylamide.

$$C^2 NO, C^4 H^5 O + 2KO + 2HO = 2CO^2, KO + C^4 H^7 N$$
Cyanic æther. Ethylamide.

Cyanuric acid and the cyanuric æthers being isomeric with the cyanic compounds, it will suffice to multiply the preceding formulæ by 3 to explain the second mode of formation. With respect to the ureas, the following equations will show how they give rise to these bases:—

$$C^2 H^4 N^2 O^2 + 2KO + 2HO = 2CO^2, KO + H^3 N + H^3 N$$
Urea.

$$C^4 H^6 N^2 O^2 + 2KO + 2HO = 2CO^2, KO + H^3 N + C^2 H^5 N$$
Acetic urea.

$$C^6 H^8 N^2 O^2 + 2KO + 2HO = 2CO^2, KO + H^3 N + C^4 H^7 N$$
Metacetic urea.

Hydrochlorate of Methylamide.—I obtained this salt by boiling cyanurate of methylene with an excess of potash in an apparatus arranged so that the vapours of methylamide, after having passed through a refrigerator, were condensed in a receiver containing a little pure water. The excessively caustic liquid thus obtained has a strong odour of ammonia, but does not contain a trace of that alkali; for if saturated with hydrochloric acid and evaporated to dryness, the residue, consisting of hydrochlorate of methylamide, dissolves very readily in hot absolute alcohol. The salt crystallizes on cooling in beautiful laminæ, which are iridescent so long as they float in the liquid, and assume a nacreous appearance when dry. Analysis gave—

Carbon	17·4	2 =	12	17·7
Hydrogen	8·7	6	6	8·8
Chlorine	52·2	1	35·5	52·5
Nitrogen	21·7	1	14	21·0

Hydrochlorate of Methylamide and Chloride of Platinum.—Beautiful golden scales, which are soluble in hot water, and contain $ClH, C^2 H^5 N PtCl^2$. Analysis gave—

Carbon	5·3	2 =	12	5·0
Hydrogen	2·8	6	6	2·5
Chlorine	44·4	3	106·5	44·9
Platinum	41·4	1	98·6	41·5
Nitrogen	..	1	14	

Nitrate of Methylamide—beautiful transparent prisms, which are soluble in alcohol.

Hydrochlorate of Ethylamide.—I have prepared this substance both with cyanic and with cyanuric æther. It dissolves readily in absolute alcohol, and crystallizes in laminæ; it melts below 212°, and solidifies on cooling into a crystalline mass. When distilled with burnt lime, it gives off ethylamide in the form of an excessively caustic liquid, which diffuses a strong odour of ammonia. This liquid precipitates all the metallic salts, and even the salts of magnesia. In solutions of salts of copper it at first forms a blue precipitate, which it afterwards redissolves, forming an azure-blue liquid; it produces a green precipitate in salts of nickel, which however is not redissolved, as is the case with ammonia. I ascertained that the liquid did not contain a trace of ammonia, by saturating it with hydrochloric acid; the residue, evaporated to dryness, dissolved entirely in absolute alcohol, and formed with chloride of platinum a double salt, the analysis of which will be found below.

The composition of the hydrochlorate of ethylamide is represented by the formula ClH, $C^4 H^7 N$. Analysis gave—

Carbon	28·9	29·4	4 =	24	29·4
Hydrogen	9·9	9·9	8	8	9·8
Chlorine........	43·7	..	1	35·5	43·6
Nitrogen........	17·5	..	1	14	17·2

Hydrochlorate of Ethylamide and Chloride of Platinum—golden scales, soluble in water. They gave on analysis—

Carbon	9·5	4 =	24	9·5
Hydrogen	3·2	8	8	3·2
Chlorine	42·0	3	106·5	42·4
Nitrogen	..	1	14	
Platinum	39·0	1	98·6	39·2

I hope soon to give a complete history of these alkalies.—*Comptes Rendus*, Feb. 12.

ON THE EXISTENCE OF MERCURY IN THE TYROL. BY M. H. ROSE.

M. Weidenbusch, in analysing in the author's laboratory a specimen of tender gray copper ore, stated to be from Schwarz in the Tyrol, found it to contain a notable quantity of mercury, amounting to 15·5 per cent. This gray copper is mixed with quartz and sulphuret of copper. Its powder is almost black, and has a specific gravity of 5·1075; when heated in a flask, it yields a little metallic mercury with a light reddish-brown sublimate. If it be mixed with carbonate of soda and heated, a larger quantity of mercury is obtained. It contains also zinc, iron, antimony and sulphur, and traces of arsenic and silver. These substances exist in it in the same proportions as in other gray copper ores. A crystallized gray copper, also stated to be from Schwarz in the Tyrol, did not contain any mercury.—*L'Institut*, Fevrier 21, 1849.

RECTIFICATION OF SPIRIT OF NITROUS ÆTHER.

M. Klauer, pharmacien of Mulhouse, states it to be well known, that when spirit of nitrous æther has become acid, it is usual to rectify it from calcined magnesia, which, however, does not prevent this preparation from becoming again acid in a few weeks. The author observes that this is not the case when it is rectified from neutral tartrate of potash instead of magnesia; it may then be kept for several months without exhibiting any sign of acidity.—*Journ. de Ph. et de Ch.*, Fevrier 1849.

[The rationale of this operation is not evident.—Edit. Phil. Mag.]

METEOROLOGICAL OBSERVATIONS FOR FEB. 1849.

Chiswick.—February 1. Frosty: foggy: rain. 2. Drizzly: hazy: rain. 3. Hazy and damp: densely overcast. 4. Overcast. 5. Very fine: overcast. 6. Hazy: densely overcast. 7. Overcast. 8. Very fine: clear. 9. Fine: overcast. 10. Overcast: clear at night. 11. Clear: very fine: barometer unusually high: clear and frosty at night. 12. Frosty and foggy: fine: clear and frosty. 13. Dense fog: fine at noon: foggy. 14. Foggy: fine. 15. Very fine. 16. Foggy: clear at night. 17. Frosty: exceedingly fine. 18. Overcast. 19. Overcast: fine. 20. Slightly overcast: cloudy: rain. 21. Cloudy and fine: rain. 22, 23. Fine. 24. Drizzly: rain: lightning in the evening: densely overcast. 25. Hazy: boisterous, with rain and thunder: constant heavy rain at night. 26. Cloudy and fine: frosty. 27. Frosty: cloudy and fine: clear. 28. Boisterous, with heavy rain.—On the 11th the barometer was higher than it has ever been observed in this locality.

Mean temperature of the month	41°·35
Mean temperature of Feb. 1848	43 ·06
Mean temperature of Feb. for the last twenty years	40 ·36
Average amount of rain in Feb.	1·61 inch.

Boston.—Feb. 1. Fine. 2, 3. Foggy. 4, 5. Fine. 6, 7. Cloudy. 8. Fine: rain P.M. 9—12. Fine. 13. Foggy. 14—17. Fine. 18. Cloudy. 19. Cloudy: rain P.M. 20. Fine: rain P.M. 21. Cloudy. 22, 23. Fine. 24, 25. Cloudy. 26, 27. Fine. 28. Rain: snow A.M. and P.M.

Applegarth Manse, Dumfries-shire.—Feb. 1. Frost and snow: looking moist P.M. 2. Fog and drizzling all day. 3. Fog and drizzling. 4. Dull A.M.: drizzling rain P.M. 5. Still dull, but fair: cloudy and moist P.M. 6. Mild: cloudy: high wind P.M. 7. Rain during night: fair and clear. 8. Fair, but dull A.M.: rain P.M. 9. Fair early A.M.: rain at noon: rain P.M. 10. Fine morning: one shower: clear P.M. 11. Frost: fog: cleared P.M. 12. Fair: slight shower: cleared. 13. Frost A.M.: clear: rain P.M. 14. Fine spring day: dry throughout. 15. Fine: drying wind. 16. Frost: clear and fine: high wind P.M. 17. Fair and clear: storm of wind. 18, 19. Rain and wind. 20. Dull and moist. 21. Frost: rain and wind P.M. 22. Dull A.M.: came on storm, wind and rain. 23. Fair: slight frost: snow on hills. 24. Frost and snow: clear: freezing. 25. Hard frost: fine. 26. Very hard frost: hail-shower. 27. Hard frost. 28. Rain heavy: wind high.

Mean temperature of the month	41°·2
Mean temperature of Feb. 1848	40 ·1
Mean temperature of Feb. for the last twenty-five years .	37 ·3
Rain in Feb. 1848	5·53 inches.
Average amount of rain in Feb. for the last twenty years	2·04 „

Sandwick Manse, Orkney.—Feb. 1. Clear: frost: cloudy. 2. Cloudy. 3. Bright: drizzle. 4. Bright: clear: hoar-frost. 5. Drizzle: rain. 6. Clear: cloudy. 7. Drizzle: cloudy. 8. Bright: hail, thunder and lightning. 9. Cloudy: damp. 10. Thunder and lightning: sleet-showers. 11. Bright: cloudy. 12. Fine: clear: aurora. 13. Showers. 14. Rain. 15. Rain: drizzle. 16. Hazy: showers. 17. Cloudy: showers. 18. Showers. 19. Showers: aurora. 20. Snow-showers. 21. Frost: snow-showers. 22, 23. Snow: frost: aurora. 24. Snow-showers. 25. Snow-showers: clear: frost. 26. Snow-showers: clear: aurora. 27. Snow: fine: clear: aurora. 28. Rain: sleet-showers.

Meteorological Observations made by Mr. Thompson *at the Garden of the Horticultural Society at* Chiswick, *near London; by* Mr. Veall, *at* Boston; *by the* Rev. W. Dunbar, *at Applegarth Manse*, Dumfries-shire; *and by the* Rev. C. Clouston, *at Sandwick Manse*, Orkney.

Days of Month.	Barometer.							Thermometer.								Wind.				Rain.			
	Chiswick.		Boston.	Dumfries-shire.		Orkney, Sandwick.		Chiswick.		Boston.	Dumfries-shire.		Orkney, Sandwick.		Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.
1849. Feb.	Max.	Min.	8½ a.m.	9 a.m.	9 p.m.	9½ a.m.	8½ p.m.	Max.	Min.	8½ a.m.	Max.	Min.	9½ a.m.	8½ p.m.	1 p.m.							
1.	30·294	30·230	29·94	30·09	30·04	30·08	29·97	44	35	32·5	36½	27½	37	40	sw.	w.	nne.	se.	·02	......	......	·05
2.	30·348	30·286	29·93	30·07	30·06	29·88	29·79	49	42	41	45	33½	42½	48	sw.	s.	sse.	sw.	·04	......	......	......
3.	30·408	30·358	29·93	30·09	30·06	29·96	29·99	50	44	45	47½	42½	43	42	sw.	ssw.	se.	sw.	·01	......	......	·05
4.	30·456	30·424	29·93	30·10	30·19	30·06	30·14	52	37	47	50	44	43	37	sw.	w.	sw.	e.	......	......	......	·09
5.	30·454	30·428	29·94	30·22	30·22	30·05	30·05	52	40	45	49	43	47	47½	w.	sw.	sw.	sw.	......	......	......	·14
6.	30·412	30·388	29·93	30·20	30·15	30·03	29·95	45	41	46	46½	43½	45½	47	sw.	wsw.	sw.	sw.	......	......	......	·35
7.	30·396	30·308	29·92	30·09	29·88	29·91	29·54	48	38	44	48	41½	43	48	sw.	wsw.	s.	sw.	......	......	......	·05
8.	30·299	30·085	29·66	29·65	29·90	29·40	29·50	51	32	41·5	46½	42½	44	43	sw.	sw.	w.	sw.	·06	......	0·13	·38
9.	30·463	30·380	29·97	30·10	29·96	29·87	29·60	49	33	40	45½	36½	44	49	sw.	w.	sw.	ssw.	......	·02	......	·30
10.	30·572	30·343	29·60	29·94	30·24	29·50	29·82	52	25	49	49	44	42½	46	sw.	sw.	w.	wnw.	......	......	......	·11
11.	30·880	30·773	30·34	30·57	30·61	30·38	30·45	51	22	43	47½	35	44	45½	n.	w.	wsw.	w.	......	......	......	·08
12.	30·755	30·575	30·31	30·54	30·38	30·42	30·23	44	21	33	43	30½	43	35	sw.	w.	wsw.	s.	......	......	......	......
13.	30·530	30·510	30·12	30·26	30·40	30·09	30·08	42	24	31	50	39½	43	43	w.	w.	w.	wsw.	......	......	......	·05
14.	30·671	30·528	30·22	30·37	30·22	29·87	29·82	51	41	36	47½	35½	46½	48	sw.	w.	sw.	w.	......	......	......	·34
15.	30·519	30·491	30·00	30·31	30·33	30·00	30·07	57	32	46	52	45	47	47	w.	w.	w.	wsw.	......	......	......	·45
16.	30·521	30·464	30·08	30·31	30·20	30·01	29·91	45	23	38	48	41½	45	44	w.	w.	sw.	w.	......	......	0·30	·10
17.	30·552	30·487	30·10	30·35	30·20	29·92	29·73	52	28	39	49	34	46	46½	sw.	w.	sw.	w.	......	......	......	·11
18.	30·459	30·396	29·95	30·10	30·04	29·80	29·54	52	35	43	53	43	43½	47	sw.	wsw.	w.	sw.	......	......	......	·36
19.	30·182	30·007	29·62	29·59	29·66	29·06	29·36	50	38	46	51	46½	45	39½	sw.	sw.	sw.	nw.	......	......	......	·62
20.	29·957	29·642	29·58	29·58	29·50	29·32	29·46	52	33	37	43	36½	34	33	sw.	w.	sw.	nw.	·28	·08	......	·10
21.	29·998	29·786	29·23	29·75	29·39	29·62	29·33	51	43	50	48½	31½	37	33	w.	w.	wsw.	e.	·06	·07	......	·03
22.	29·828	29·693	29·47	29·35	29·31	29·12	29·38	52	35	41	48	41½	35	35½	w.	wnw.	wnw.	n.	·01	......	0·10	·27
23.	29·996	29·839	29·57	29·70	29·59	29·61	29·52	50	34	41	44	33½	38	33½	w.	w.	w.	w.	......	......	......	·13
24.	29·657	29·613	29·22	29·51	29·47	29·36	29·43	47	37	42	42½	32½	35½	34	nw.	w.	w.	sw.	·28	......	......	·33
25.	29·493	29·326	29·27	29·38	29·40	29·50	29·58	51	34	35	42½	25½	35	32	nw.	se.	ene.	n.	·92	......	......	·10
26.	29·853	29·435	29·22	29·56	29·74	29·68	29·75	47	22	38	41	28½	36	33	n.	n.	nw.	n.	......	......	......	·08
27.	30·034	29·978	29·20	29·82	29·67	29·84	29·69	50	30	32·5	41½	23½	39	32	s.	nw.	nw.	e.	......	......	......	·07
28.	29·624	29·331	29·10	28·94	28·88	29·04	29·05	50	31	44	42	33	37	35	sw.	s.	w.	nne.	·84	·05	1·00	·18
Mean.	30·236	30·146	29·75	29·622	29·560	29·763	29·740	49·50	33·21	40·9	46·3	36·8	41·46	40·85					2·52	0·22	1·53	4·92

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XLVI. *On the Isomeric Modifications of Phosphoric Acid. By* H. Rose, *Professor of Chemistry in the University of Berlin**.

IT has frequently been remarked, that no substance presents greater difficulties in every respect to the chemist in its examination than phosphoric acid; whilst the longer and more constantly we are engaged in studying the reactions of this acid, the greater do the difficulties become. Every time the investigation is renewed, the chemist meets with new anomalies, and numerous new obscure phænomena are exhibited, whilst even those previously known are far from being satisfactorily explained.

Some time since, in an investigation upon the isomeric modifications of the peroxide of tin, I drew attention to the fact, that such modifications are principally met with in those metallic oxides which possess acid properties; they are almost entirely absent in those which possess strong basic properties. The latter certainly frequently alter in density to a considerable extent when exposed to various elevated temperatures, becoming at the same time soluble with difficulty or insoluble in acids; when, however, they are once dissolved, they always exhibit in solution the same reactions.

Phosphoric acid exhibits paradoxical isomeric properties still more than any metallic acid. After the important discovery of Clark, the ingenious investigations of Graham did much towards the elucidation of the difficulties; but by new investigations new facts have been discovered, part of which cannot be satisfactorily explained by Graham's views.

Assuming, in accordance with Graham's views, the existence of three modifications of phosphoric acid, viz. metaphos-

* Translated from Poggendorff's *Annalen der Physik und Chemie*, vol. lxxvi. p. 1.

phoric, pyrophosphoric, and ordinary phosphoric acid, which with Berzelius we may denominate *a*-,*b*-, and *c*-phosphoric acid, it is the metaphosphoric acid which especially exhibits the greatest anomalies. The pyrophosphoric acid also presents some; and the ordinary phosphoric acid, the *c*-phosphoric acid, is that which has most analogy with other oxy-acids in its behaviour towards reagents. This is the one which can be best separated and determined quantitatively. Fortunately the other modifications can be more or less easily converted into the *c*-phosphoric acid; hence in their quantitative determination this conversion must in most cases be previously effected.

I shall now make some remarks upon each of these three modifications of phosphoric acid.

a-Phosphoric Acid (*Metaphosphoric Acid*).

For the purpose of elucidating to a certain extent, if only for the present, the chaos of anomalous phænomena which this acid exhibits, we are compelled to admit the existence of several submodifications of it: at least three of these are now distinguishable.

1. One of these submodifications consists of the acid existing in Graham's metaphosphate of soda, which is obtained by fusing the acid phosphate of soda with the phosphate of soda and ammonia (microcosmic salt). After fusion, the mass must be cooled rapidly, and not slowly. The solution of this salt, as is well known, possesses a neutral or very slightly acid reaction, and is especially characterized by giving precipitates with neutral solutions of several salts of the earths and metallic oxides, which are generally soluble in an excess of the salt of soda, and possess the remarkable property of forming a heavy, thick, oleaginous mass when shaken. The solution of the salt alone does not yield any precipitate with a dilute filtered solution of albumen, but a copious dense precipitate immediately appears on the addition of acetic acid.

The following are the special reactions of a solution of the metaphosphate of soda:—

Chloride of barium produces a voluminous precipitate, the supernatant liquid reddening litmus-paper; it is entirely soluble in excess of the salt of soda; ammonia does not produce a precipitate in this solution. The precipitate is not oleaginous, nor does it become so, either when set aside for some time, or on ebullition.

Chloride of calcium produces a voluminous precipitate, which on agitation, even in the cold, collects at the bottom of the vessel in the form of a thick, oily or turpentine-like mass.

The supernatant fluid reddens litmus-paper. The mass is not affected by ebullition, but when heated with muriatic acid, it is dissolved. The precipitate is entirely dissolved by excess of the soda salt; ammonia does not produce a precipitate in the solution.

A solution of *sulphate of magnesia* does not produce any precipitate, even on ebullition. If much of the soda salt has been added, no precipitate is produced in the solution by ammonia; in the opposite case, ammonia produces a precipitate which is soluble in chloride of ammonium.

Nitrate of silver produces a dense, voluminous, white precipitate, which is soluble in ammonia and nitric acid. It is also perfectly soluble in a large excess of the salt of soda. The supernatant liquid reddens litmus-paper. The precipitate does not become oleaginous when shaken in the cold, but when boiled it contracts, occupying a smaller volume, and becomes perfectly resinous. Heat renders it tenacious, so that it can be drawn into threads; and on cooling, it solidifies to a brittle mass.

A solution of *pernitrate of mercury*, which necessarily contains free acid, produces a white precipitate; this, when shaken, even in the cold, subsides to the bottom of the vessel as a dense oleaginous mass.

Solution of *bichloride of mercury* produces no change.

Solution of *protonitrate of mercury* produces a dense white precipitate, which is soluble in an excess of the soda salt; on boiling it becomes resinous, like the salt of silver.

Solution of *sulphate of copper* produces no change; but chloride of copper produces a bluish-white precipitate, which is soluble both in excess of the soda salt and of the chloride of copper.

Solution of *acetate of lead* produces a dense voluminous precipitate, which is soluble in excess of the soda salt, and when shaken forms a coherent mass, but does not become oleaginous; when set aside, however, it becomes somewhat resinous.

Solution of *protosulphate of manganese* produces a white precipitate, which when shaken becomes an oily mass. The precipitate is soluble in an excess of the soda salt; sulphuret of ammonium throws down sulphuret of manganese from this solution.

Solution of *protosulphate of iron* does not produce a precipitate. Nor is any precipitation caused by the addition of ammonia to this solution, but it renders it dark green.

Solution of *sulphate of zinc* produces no change.

Solutions of the *sulphates of cobalt and nickel* also produce no change. However, solutions of the chlorides of cobalt and nickel produce red and greenish-white precipitates, which on agitation subside in the form of heavy oily drops of a red and greenish-white colour. The precipitates are soluble in excess of the soda salt.

Solution of the *nitrate of bismuth*, although it contains free acid, produces a white precipitate, which on agitation becomes somewhat resinous, but not oily. The precipitate is soluble in the soda salt.

If the acid contained in the salt of soda be separated from the base, its aqueous solution exhibits somewhat different properties from those of the aqueous solution of the metaphosphoric acid produced by combustion in oxygen gas. The solution of the soda salt was precipitated with nitrate of silver, and the precipitate allowed to remain in the liquid during one night; it was then washed with cold water, and after suspension in water, was decomposed by a current of sulphuretted hydrogen. The sulphuret of silver formed remains suspended in the free acid for a long time, and is separated by filtration with extreme difficulty.

The aqueous solution of the acid does not at first produce any precipitate in *chloride of barium*; after a considerable time merely some flakes subside. Barytic water however produces a precipitate in it, even when not added in excess, and whilst the solution is still acid.

Chloride of calcium produces no precipitate. Lime-water only produces a precipitate when added in excess.

A solution of *sulphate of magnesia* mixed with chloride of ammonium, only produces a precipitate in the solution of the acid saturated with ammonia, when the solutions are concentrated. This is, however, soluble in a considerable quantity of water, and hence does not appear in dilute solutions.

Solution of *nitrate of silver* produces a white precipitate, which on the saturation of the solution with ammonia becomes more considerable.

In a solution of *albumen* a copious white precipitate is immediately formed.

The reactions of a solution of metaphosphoric acid produced by the combustion of phosphorus in oxygen gas, differ somewhat from those of the acid as procured from the salt of soda.

Thus, a solution of *chloride of barium* immediately produces a copious precipitate in it. A very large excess of the acid is requisite to redissolve the precipitate; ammonia does not produce any precipitate in this solution. Barytic water produces a precipitate in the acid, even when the latter is in great excess; an excess of barytic water, however, renders it more copious.

Solution of *chloride of calcium* produces an extremely small precipitate, which is perfectly soluble in excess of the acid. Ammonia produces a dense voluminous precipitate in this solution. Lime-water does not cause any precipitate until added in excess. Both acids exhibit the same reactions with sulphate of magnesia, nitrate of silver and albumen.

These two acids therefore differ principally in their reaction with chloride of barium. But as solutions of Graham's metaphosphate of soda, as stated above, when decomposed by solutions of neutral salts, yield liquids which possess an acid reaction, the acid separated from the precipitates must be different from that in the soda salt used.

As the composition of the oily and resinous precipitates produced by Graham's salt was entirely unknown, the salt of silver was analysed by M. Weber. It was prepared in the same manner as the salt precipitated for the purpose of obtaining the free acid. After having been dried for a long time at 212° F., 0·930 grm. lost at a red heat 0·019 grm., or 2·04 per cent. of water. It fused into a mass of a yellowish colour, which, when dissolved in nitric acid, yielded on the addition of hydrochloric acid 0·815 grm. of chloride of silver, corresponding to 70·09 oxide of silver. The phosphoric acid in the filtered liquid was precipitated in the form of the phosphate of ammonia and magnesia; 0·257 grm. of pyrophosphate of magnesia was obtained after heating to redness, corresponding to 27·87 per cent. of phosphoric acid.

These results correspond to the formula $3AgO, 2P^2O^5 + HO$. Berzelius examined this salt long ago, but he obtained it in a different manner*. He precipitated a solution of phosphoric acid which had been recently heated to redness, with nitrate of silver; he then obtained a precipitate, the composition of which but little resembled that of $AgO\ P^2O^5$, the amount of oxide of silver differing nearly 3 per cent. from that calculated. This salt was placed in boiling water; in a few minutes it fused into a viscid mass resembling turpentine, and had the same composition as the compound mentioned above.

The composition found above explains the acid reaction of the liquid, when the neutral solution of Graham's salt is precipitated with solution of nitrate of silver. It also renders evident why the acid separated from the silver salt may possess different properties from metaphosphoric acid as obtained by the combustion of phosphorus. The question, whether the other oleaginous salts, which a solution of Graham's salt forms with other neutral salts, possess an analogous composition to

* Poggendorff's *Annalen*, xix. p. 333.

the silver salt, as might be expected from the circumstance that on their formation the solutions which are originally neutral acquire an acid reaction, well deserves investigation.

A salt of silver having an analogous composition can be prepared from Graham's salt. M. Fleitmann decomposed Graham's salt with nitrate of silver, filtered the precipitate immediately, washed it slightly with cold water, and then pressed it strongly between blotting-paper. The water used in the washing possessed a barely acid reaction. The fused compound gave the following per-centage composition:—

Phosphoric acid . . .	37·62
Oxide of silver . . .	61·18
	98·80

which closely approximates the composition calculated according to the formula AgO, $P^2 O^5$, which requires 61·89 oxide of silver and 38·11 phosphoric acid. The loss probably arises from its containing a little soda, which was not removed in consequence of the rapidity with which the compound was washed. It is thus evident, that when the silver salt is precipitated and separated as quickly as possible from the liquid, it corresponds in composition with Graham's salt; but that, on prolonged contact with the liquid, even when cold, it becomes decomposed and loses acid.

Whether anything similar occurs in the case of the other precipitates has not been determined; but it is not improbable, as at first they are precipitated in a pulverulent form, and only acquire the oily appearance when powerfully shaken, by which they probably lose a portion of the phosphoric acid.

2. The acid existing in those remarkable salts which Fleitmann and Henneberg obtained from the acid phosphate of soda, or rather from the microcosmic salt, by fusion and very gradual cooling*, may be regarded as a second submodification of metaphosphoric acid. This salt has exactly the same composition as the metaphosphates; it contains the same number of atoms of base and acid, and has therefore the composition of Graham's salt; but differs from it in its opacity and crystalline structure, whilst Graham's salt is transparent and amorphous. It crystallizes from its solution with 4 atoms of water, whilst Graham's salt cannot be made to crystallize. Its solution, like that of Graham's salt, exerts a neutral reaction.

The most remarkable property of the acid of this salt is, that it forms with all bases compounds soluble in water, in which respect it differs essentially from all the modifications of phosphoric acid. The salts, even the silver salt, can be

* Chem. Gaz., vol. vi. p. 289.

obtained in a crystalline state. According to Fleitmann, the solution of the soda salt produces no precipitates with solutions of nitrate of silver, nitrate of lead, chloride of barium, chloride of calcium, chloride of strontium, sulphate of magnesia, protosulphate of manganese, protosulphate of iron, the sulphates of zinc, cobalt or nickel. In the solutions of the proto- and pernitrates of mercury, the solution of the soda salt at first produces no turbidity, but after a considerable time a precipitate is formed. It also produces a precipitate in a solution of acetate of lead.

The solution of the soda salt, as also that of Graham's salt, produces no precipitate with *albumen*; but this is the case when acetic acid is added. According to Fleitmann, the acid may be readily isolated from the solution of the crystallized silver salt by a current of sulphuretted hydrogen. The solution of the free acid immediately produces a copious precipitate with albumen. When saturated with carbonate of soda, the original soda salt is again obtained; and if its solution, after neutralization with ammonia, is treated with nitrate of silver, the crystallized silver salt can be procured from the solution.

3. The acid contained in those salts which were formerly called acid phosphates and have long been known, may be regarded as forming the third modification of metaphosphoric acid. These salts have recently been examined by Maddrell*. In obtaining them, Maddrell made use of salts of the most different kinds—metallic chlorides, sulphates, nitrates, carbonates and chlorates, which were heated to + 600° F. with free phosphoric acid. I shall presently show, that an insoluble pyrophosphate, possessing similar properties to those presented by Maddrell's metaphosphates, may be simultaneously formed. The heating with free phosphoric acid must therefore be continued until a portion of the heated mass, when removed, is found to precipitate a solution of albumen.

The insoluble metaphosphates dissolve when heated with concentrated sulphuric acid. According to Fleitmann, the acid cannot be isolated, or at least only very imperfectly, by transmitting a current of sulphuretted hydrogen through the copper salt suspended in water. The decomposition of it is best effected by treating the above salt with sulphuret of sodium. A soluble soda salt is then obtained, which somewhat resembles Fleitmann's metaphosphate of soda, but differs from it in many respects, and can only be procured with half its water of crystallization.

* Phil. Mag., S. 3, vol. xxx. p. 322.

The various sub-modifications of metaphosphoric acid all agree in having the same capacity of saturation; one atom of acid saturates one atom of a powerful base. Graham supposes that the different capacity of saturation of the various modifications of phosphoric acid is the cause of their different reactions. This difference in the capacity of saturation of the various phosphoric acids, however, is indisputably a result of their isomeric state, and, as I have remarked on a former occasion, cannot be the cause of them*.

A second general property of all the varieties of metaphosphoric acid is, that their aqueous solutions precipitate a solution of albumen. This is almost the only property by which the various kinds of metaphosphoric acid can be recognised in qualitative examinations, and unequivocally distinguished from the other modifications of phosphoric acid; for neither pyrophosphoric acid nor the ordinary *c*-acid precipitates albumen. The soluble salts of metaphosphoric acid do not precipitate albumen until acetic acid has been added to their solutions.

The property of metaphosphoric acid to produce a copious precipitate in the solution of chloride of barium is especially peculiar to the acid produced by the combustion of phosphorus only.

When a concentrated solution of *c*-phosphoric acid is heated very gently for several hours, so that none of it volatilizes, an acid is obtained, the aqueous solution of which does not produce any precipitate with albumen; nor does it produce a precipitate with chloride of barium, or merely an inconsiderable troubling after a long time. Nitrate of silver however produces a white precipitate. These are the properties of pyrophosphoric acid. When the same acid is heated in a platinum crucible longer and more strongly, so that it commences to be copiously volatilized, its aqueous solution then immediately produces a copious precipitate with albumen and chloride of barium, and a white precipitate with nitrate of silver, which when shaken becomes resinous. Thus by strongly heating it, metaphosphoric acid is formed, and apparently the same modification as that obtained by the combustion of phosphorus.

By the rapid application of a certain degree of heat, however, an acid can be obtained, the aqueous solution of which affords with albumen a copious precipitate, but none with chloride of barium, and which, after saturation with ammonia, yields a white precipitate with nitrate of silver, in which, after some time, an admixture of yellow can be distinctly perceived. In this case, the same acid as that which I separated from the

* Chem. Gaz. vol. vi. p. 383.

metaphosphate of silver appeared to have been formed, in admixture with a little undecomposed *c*-phosphoric acid.

Some uncertainty still continues regarding the composition of fused phosphoric acid. A very long time since I made several experiments upon this point*, and found that the acid, fused for a considerable time over a spirit-lamp, in three experiments, contained a slightly less amount of water than is required by the compound P^2O^5+HO. In another experiment, probably with some acid which had been heated more strongly, and for a longer time, the amount of water was still less, and nearly corresponded to the compound $3P^2O^5+5(P^2O^5+HO)$: hence it is thus rendered probable that phosphoric acid would be obtained in a perfectly anhydrous state by a very long and continuous application of heat.

My experiments have been recently confirmed by M. Weber, who examined an acid which had been exposed for a considerable time to a temperature at which it began to be slightly volatilized; 3·127 grms. of this acid, when treated with 16·891 grms. of oxide of lead, left, after having been heated to redness, a residue of 19·700 grms. The per-centage composition of the acid was therefore—

Phosphoric acid	89·84
Water	10·16
	100·00

In this case also the quantity of water is slightly less than the composition P^2O^5+HO requires. The quantities of oxygen are in the proportion of 50·34 : 9·03.

In addition to these three submodifications of metaphosphoric acid, there are undoubtedly others. That acid which is formed on burning phosphorus in dry atmospheric air or oxygen gas, may be considered as a fourth submodification, for, as has been stated above, the reactions of its solution are different from those of the other modifications. The salts which it forms with bases have not been prepared and examined. I shall merely remark here, that anhydrous phosphoric acid does not exhibit any affinity towards dry ammoniacal gas, nor does it absorb it; hence it differs in this respect from anhydrous sulphuric acid. Probably the various submodifications of metaphosphoric acid should be considered as conjugate acids, as the difference in their reactions would then be more satisfactorily explained. Anhydrous phosphoric acid may constitute the conjunct, which is capable of combining in different proportions with pyrophosphoric acid or with *c*-phosphoric

* Poggendorff's *Annalen*, vol. viii. p. 203.

acid, giving rise to the numerous modifications of metaphosphoric acid. This conjunct *per se* probably alone possesses the property of precipitating albumen, and thus this property is communicated to all the varieties of metaphosphoric acid.

b-Phosphoric Acid (Pyrophosphoric Acid).

At least two submodifications of this modification of phosphoric acid must also be admitted; for there are two different kinds of pyrophosphates. One of these consists of the well-known pyrophosphate of soda, which is obtained by heating the *c*-phosphate of soda, $2NaO, P^2O^5, HO$, to redness, and the salts which are formed from this soda salt by decomposition. The second variety is produced in the same manner as Maddrell's insoluble metaphosphates, that is, by heating the salts with excess of phosphoric acid, the heat not being so great as to cause the production of metaphosphates. Thus by treating nitrate of copper with phosphoric acid, a salt of copper is formed which resembles the insoluble metaphosphate of copper, especially as regards insolubility. But the acid existing in it may be readily isolated by a current of sulphuretted hydrogen, and its aqueous solution possesses the same properties as the solution of ordinary pyrophosphoric acid. As this modification of the pyrophosphates has not been sufficiently examined, a more detailed notice of it cannot now be given.

As is well known, the pyrophosphates are formed when the *c*-phosphates, which contain two atoms of a fixed and one atom of a volatile base (oxide of ammonium or water), are heated to redness. The usual process is the conversion of the ordinary phosphate of soda ($2NaO, P^2O^5, HO$) into pyrophosphate of soda ($2NaO, P^2O^5$).

Graham ascribes the difference in the properties of the pyrophosphates from the phosphates, to the difference in the capacity of saturation of the two acids contained in the two kinds of salts. It cannot be denied that pyrophosphoric acid especially saturates two atoms of a base, and thus differs characteristically from the *c*-phosphoric acid, which requires three for its saturation. But I have remarked above that this is a consequence of the isomerism of the two acids; hence it must appear as not improbable, that an atom of water may be expelled from the ordinary phosphate of soda without converting it into a pyrophosphate.

Some experiments which were made with this point in view, have not however yielded a favourable result. The ordinary phosphate of soda was exposed to a very gentle heat, so that it still contained more than one atom of water; 3·0635 grms. of it, when heated to redness, gave 2·7900 grms. of pyrophosphate of soda. This phosphate of soda, therefore, still contained 0·2735 grm., or 8·92 per cent. of water. 3·126 grms. of the same salt were exposed for a considerable time to definite temperatures; it was found that by gradually heating the salt, almost the whole of the water it contained could be expelled at a temperature of 464° F. The following are the details of this experiment:—

The above quantity of the salt weighed after exposure to a heat of

320° F.	during	11	hours	3·054	grms.
320	...	6	...	3·053	...
320	...	8	...	3·043	...
446	...	4	...	3·015	...
446	...	6	...	2·967	...
464	...	2	...	2·920	...
464	...	2	...	2·894	...
464	...	2	...	2·887	...
464	...	2	...	2·883	...

Had the salt been kept for a still longer time at a temperature of 464° F., it would undoubtedly have lost the whole of the water it contained. The quantity used would then have weighed 2·846 grms.

But when the salt was examined, it was found that even at the above temperature it had become almost completely converted into pyrophosphate of soda. The solution gave a white precipitate with nitrate of silver, which was mixed with as much of a yellow one as would have been expected from the quantity of water still existing in the heated salt.

Solution of pyrophosphate of soda yields, with very many neutral salts of metallic oxides, precipitates which are partly soluble in excess of the pyrophosphate of soda. The peculiarity of the pyrophosphate of soda in readily forming double salts, has already been very distinctly pointed out by Stromeyer. Persoz has recently studied this point, without alluding to Stromeyer's memoir; he has however confirmed all the facts given by him. Schwarzenberg has recently examined most of the pyrophosphates quantitatively*, and Baer has made the interesting discovery, that those insoluble precipitates which are produced by a solution of pyrophosphate of soda, and are not soluble in excess of it, are frequently insoluble double salts of the soda salt with the pyrophosphates formed, in which the soda replaced the other base, apparently without the two existing in the double salt in a definite simple propor-

* Chem. Gaz., vol. vi. pp. 181, 196.

tion*. Even the silver salt contains some, although a small quantity of soda. Persoz and Fleitmann have also procured and examined insoluble double salts of the pyrophosphate of soda with the pyrophosphate of copper.

The following are the special reactions of a solution of pyrophosphate of soda with salts of the metallic oxides:—

Solution of *chloride of barium* produces a precipitate, which is insoluble in excess of the soda salt; at least the filtered liquid either yields no precipitate, or at most a very slight troubling with dilute sulphuric acid.

Solution of *chloride of calcium* produces a precipitate, which is soluble in a very large quantity of the pyrophosphate of soda. The clear liquid however becomes spontaneously turbid when set aside, and in 24 hours a very slight precipitate only of oxalate of lime is caused by a solution of oxalate of potash.

Solution of *sulphate of magnesia* produces a precipitate, which is soluble in excess of the pyrophosphate of soda; but on ebullition a copious precipitate is produced in this solution, which does not disappear as the liquid cools. Ammonia does not cause a precipitate in a solution of the pyrophosphate of magnesia in pyrophosphate of soda, even when set aside for a long time. The precipitate of the pyrophosphate of magnesia is also readily soluble in excess of sulphate of magnesia. Ebullition causes a precipitate in this solution, which does not disappear on cooling.

Solution of *nitrate of silver* produces the well-known white precipitate. It is not wholly insoluble in a very large excess of the pyrophosphate of soda. The supernatant liquid does not affect litmus paper, and only renders it blue when excess of the soda salt has been added.

Solution of *pernitrate of mercury*, although it necessarily contains free nitric acid, produces a copious white precipitate, which becomes basic and of a reddish-yellow colour on the addition of excess of the pyrophosphate of soda.

Solution of the *protonitrate of mercury* produces a white precipitate, which is soluble in excess of the pyrophosphate of soda. In this solution, ammonia causes a blackish gray, sulphuret of ammonium a black, and hydrochloric acid a white precipitate; the latter consists of chloride of mercury.

Solution of *bichloride of mercury* does not immediately produce any precipitate. After a considerable time, a dense red precipitate is formed, which is still more rapidly thrown down when heat is applied.

Solution of *sulphate of copper* produces a bluish-white precipitate, which is readily soluble in excess of the pyro-

* Poggendorff's *Annalen*, vol. lxxv. p. 152.

phosphate of soda. The solution is of a blue colour; on the addition of ammonia it becomes of a darker blue, sulphuret of ammonium immediately produces in it a brown precipitate of sulphuret of copper. The pyrophosphate of copper is also soluble in a very large excess of sulphate of copper. Heat produces a precipitate in the solution which does not disappear on cooling.

Solution of *acetate of lead* causes a white gelatinous precipitate, readily soluble in the pyrophosphate of soda. Sulphuret of lead is immediately thrown down from this solution by sulphuretted hydrogen.

Solution of *protosulphate of manganese* produces a white precipitate, which is insoluble in excess of the protosalt of manganese, but is soluble in the pyrophosphate of soda. This solution is not troubled by ammonia; sulphuret of ammonium does not produce a precipitate of sulphuret of manganese in it (which certainly deserves special notice), even when set aside for a considerable time.

Solution of *protosulphate of iron* produces a white precipitate, soluble in excess of the pyrophosphate of soda. A black precipitate of sulphuret of iron is immediately formed in the solution on the addition of sulphuret of ammonium; the solution is not however rendered turbid by ammonia; it merely renders it of a dark colour. The pyrophosphate of iron is also soluble in excess of the solution of the protosulphate of iron.

Solution of *perchloride of iron* causes a white precipitate, readily soluble in excess of the pyrophosphate of soda. Sulphuret of ammonium immediately produces in the almost colourless solution, a black precipitate of sulphuret of iron, which deserves particular notice, because Persoz denies the production of sulphuret of iron in the solution by sulphuret of ammonium. Ammonia however does not render the solution turbid; it immediately turns it of a blood-red colour.

Solution of *sulphate of zinc* produces a white precipitate, soluble in the pyrophosphate of soda. The solution is neither precipitated by ammonia nor by boiling, but sulphuret of ammonium throws down sulphuret of zinc. The precipitate is also soluble in excess of the solution of sulphate of zinc. On boiling, the solution becomes turbid; the turbidity does not disappear on cooling.

Solution of *sulphate of cadmium* produces a precipitate which is soluble in pyrophosphate of soda. The solution becomes turbid when heated, the turbidity not disappearing as the solution cools. Sulphuret of cadmium is immediately precipitated from it by sulphuret of ammonium.

In a solution of *sulphate of nickel* a greenish-white precipitate is produced, which is readily soluble in the soda salt. The solution is not rendered turbid by heat. With chloride of nickel the same reaction occurs, except that the solution of the precipitate in excess of the soda salt is rendered turbid when heated and does not become clear on cooling. Sulphuret of nickel is immediately thrown down from this solution by sulphuret of ammonium.

Solution of *sulphate of cobalt* is precipitated of a pale red colour; the precipitate is readily soluble in the soda salt. The solution is red, and when heated becomes perfectly blue, but not turbid; on cooling it reacquires the red colour. Sulphuret of ammonium immediately causes the formation of sulphuret of cobalt in it.

Solution of *alum* produces a white precipitate, soluble in the soda salt. No precipitate is caused in this solution either by ammonia or sulphuret of ammonium. But the precipitate is soluble in excess of the solution of alum.

Solution of *nitrate of bismuth*, although it contains free acid, produces a white precipitate, soluble in the soda salt. On applying heat a precipitate is formed. Sulphuret of ammonium produces sulphuret of bismuth in it.

No precipitate is caused in a dilute filtered solution of *albumen* by solution of pyrophosphate of soda, even after the addition of acetic acid.

As is well known, aqueous solution of pyrophosphoric acid is best obtained by decomposing the pyrophosphate of lead, suspended in water, by a current of sulphuretted hydrogen. In this manner the conversion of the pyrophosphoric acid into the *c*-phosphoric acid is avoided, which, as we know, ensues after the lapse of some time even by repose, but much more rapidly when the acid is heated. If however the solution of pyrophosphoric acid has been saturated or supersaturated by a strong base, it may be kept in it without undergoing any change. Neither by ebullition nor by long repose is the *b*-phosphoric acid converted into the *c*, and a solution of the pyrophosphate of soda may be preserved for many years without change. With excess of alkali, the solution of pyrophosphoric acid is not converted into the *c*-phosphoric acid, until the mass, after evaporation to dryness, has been completely fused. But even in this case, according to Weber, the entire conversion into the *c*-phosphoric acid does not take place, until the pyrophosphate has been completely decomposed by fusion with excess of alkali, especially an alkaline carbonate. As this does not occur completely on fusing pyrophosphate of lime with excess of an alkaline carbonate, the *b*-phosphoric acid of the undecomposed portion of the lime-salt does not therefore become converted into the *c*-phosphoric acid. With the application of a high temperature, the conversion can be better effected in this manner in the case of the pyrophosphate of strontia, and still better of the pyrophosphate of baryta; pyrophosphate of magnesia may be completely decomposed, and the pyrophosphoric acid contained in it completely converted into *c*-phosphoric acid, even by fusion over a spirit-lamp with a mixture of carbonate of potash and carbonate of soda in atomic proportions.

It is well known that the conversion of pyrophosphoric acid is perfectly effected by acids, especially when it is heated with them. The stronger the acid, the more completely the conversion is effected; it succeeds best when concentrated sulphuric acid is used.

An aqueous solution of pyrophosphoric acid, immediately after its preparation, exhibits the following reactions:—

Solution of *chloride of barium* produces no precipitate; after a long time a very inconsiderable troubling occurs. A precipitate is produced in the liquid by ammonia.

Solution of *chloride of calcium* produces no precipitate, even after long repose. Ammonia causes a precipitate, although it is not very copious. Lime-water immediately causes a precipitate, even when the solution is slightly acid; if however excess of pyrophosphoric acid is added, the precipitate is redissolved; ammonia does not then cause a precipitate in this solution.

If chloride of ammonium be added to pyrophosphoric acid, and it then be supersaturated with ammonia, solution of *sulphate of magnesia* produces a precipitate, which is however soluble in a very large quantity of water. If a solution of *c*-phosphate of soda be added to this liquid, a precipitate is immediately thrown down.

Solution of *nitrate of silver* usually causes no precipitate. On saturation with ammonia, a white precipitate is produced, which, if the solution of pyrophosphoric acid has stood for some time, is of a yellowish tint. Solution of acetate of silver produces a white precipitate, which is soluble in a large quantity of pyrophosphoric acid.

A dilute filtered solution of *albumen* causes no precipitate. It is a remarkable fact, that even in the last edition of Berzelius's *Lehrbuch*, he ascribes to pyrophosphoric acid the property of precipitating albumen, and thus distinguishes it from the *c*-phosphoric acid. Also, according to his statement, solutions of the pyrophosphates after the addition of acetic acid precipitate albumen. It is however a very characteristic property of pyrophosphoric acid, that it does not precipitate albumen, and by this very property it differs most essentially from metaphosphoric acid, all the submodifications of which possess the property of precipitating albumen in a remarkable degree.

I have already remarked above, that the aqueous solution of pyrophosphoric acid obtained from the insoluble pyrophosphate of copper by sulphuretted hydrogen gas, exhibits the same reactions as the acid obtained from the lead salt.

Pyrophosphoric acid differs from metaphosphoric acid, in addition to its characteristic reaction with albumen, also in that with a solution of chloride of barium, although, as I have remarked above, all the modifications of metaphosphoric acid are not alike in this respect; also in the different properties of the precipitate produced by a solution of silver, regarding which, it must be observed, that one of the modifications of metaphosphoric acid (the submodification described at p. 326) forms a soluble salt with oxide of silver. The difference between the reaction of metaphosphoric acid and pyrophosphoric acid with albumen therefore forms the most important distinction between these two acids.

c-Phosphoric Acid (*ordinary Phosphoric Acid*).

This modification of phosphoric acid is the one most commonly occurring in analytical investigations, because the other modifications are converted into it by the action of acids.

Its salts have been so frequently examined, that most of their properties are known. One property however, which especially characterizes the *c*-phosphates, appears to have been hitherto overlooked. This consists in the solubility of a large number of the insoluble phosphates in excess of the saline solution from which they have been precipitated by means of the phosphate of soda. The solution generally possesses the property of producing a copious precipitate when heat is applied, which disappears however on cooling. Hence double salts are formed, which are decomposed by a high temperature. Many of the precipitates thrown down by pyrophosphate of soda, frequently dissolve, as has already been mentioned, even in excess of the solution of the salt; this solution is also troubled by heat, but the precipitate is not dissolved on cooling. As in chemical investigations the *c*-phosphate of soda is so frequently used to precipitate oxides from the solutions of their salts, and as it appeared to me important to be accurately acquainted with the properties of the precipitates, I shall offer no excuse for stating here the reactions of the most important salts of the metallic oxides with a solution of *c*-phosphate of soda.

Solution of *chloride of barium* produces a copious precipitate, which is neither soluble in excess of the phosphate of soda nor of the chloride of barium.

Solution of *chloride of calcium* reacts in the same manner. Traces of the precipitate are soluble in excess of the chloride of calcium, and may be precipitated from the filtered solution by ammonia.

Its reaction with a solution of *sulphate of magnesia* is known generally, but not perfectly in detail. The sulphate produces a precipitate in a solution of the phosphate of soda, which is insoluble in the latter, but soluble in excess of the solution of sulphate of magnesia. If this clear solution be treated with ammonia, a copious precipitate falls, part of which consists of hydrate of magnesia and is soluble in chloride of ammonium; another portion, which is composed of the phosphate of magnesia and ammonia, is insoluble in it. The clear solution of the phosphate of magnesia in the sulphate of magnesia, when boiled, yields a copious precipitate, which however completely disappears as the liquid cools, reappearing if the ebullition be repeated. If however this experiment be repeated many times, the precipitate thrown down on ebullition at last ceases to disappear entirely on cooling.

The precipitate produced by *nitrate of silver* is insoluble both in excess of the phosphate of soda and in the salt of silver.

Solution of *pernitrate of mercury* produces a white precipitate, which is not insoluble in excess of the solution of the mercurial salt; but as this always contains free acid, the solubility of the precipitate may arise from this.

Solution of the *protonitrate of mercury* causes a white precipitate, insoluble in excess of the mercurial solution.

Solution of the *bichloride of mercury* at first produces no change. After standing for a long period, a slight red deposit subsides, which is produced sooner and in greater abundance by heat. The reaction is the same as with the pyrophosphate of soda.

Solution of *sulphate of copper* produces a bluish-white precipitate, soluble in a large quantity of the cupreous solution. A copious precipitate is produced by heat in the clear solution, which completely disappears on cooling.

Solution of the *protosulphate of manganese* produces a white precipitate, which is only soluble in a very large excess of the solution of manganese. A precipitate is caused in this solution by ebullition, which completely disappears on cooling.

Solution of *protosulphate of iron* produces a white precipitate, which is readily soluble in excess of the solution of the protosalt. A copious precipitate is thrown down by heat, and does not completely disappear on cooling.

In a solution of the *perchloride of iron* a white precipitate is formed, which is readily soluble in excess of the solution of the perchloride.

Solution of the *sulphate of zinc* produces a white precipitate, which is readily soluble in excess of the solution of zinc. When the solution is heated, a troubling ensues; it is but inconsiderable, and does not completely disappear on cooling.

Solution of *sulphate of cadmium* causes a white precipitate, readily soluble in excess of the solution of cadmium. The solution yields a copious precipitate when heated, but this completely disappears on cooling.

Solution of *chloride of nickel* yields a greenish-white precipitate, soluble in excess of the solution of nickel. The solution, which yields a precipitate when boiled, becomes perfectly clear on cooling.

Solution of the *sulphate of cobalt* produces a blue precipitate, soluble in excess of the solution of cobalt. The solution is red. On ebullition, a red precipitate is produced in it, which completely dissolves on cooling.

Solution of *alum* gives a white precipitate, soluble in a considerable excess of the solution of alum. When heated, the solution yields a copious precipitate, which partly, but not entirely, disappears on cooling.

Solution of *nitrate of bismuth* gives a white precipitate, insoluble in excess of the solution of bismuth.

Phosphate of soda does not cause a precipitate in a dilute filtered solution of *albumen*, even when acetic acid is added.

The aqueous solution of the *c*-phosphoric acid differs from pyrophosphoric and metaphosphoric acids, as is well known, by its reaction with a solution of silver.

Solution of *chloride of barium* produces only an inconsiderable turbidness; but on the addition of ammonia a copious precipitate is immediately formed.

Barytic water causes a precipitate, even when the liquid is acid. The phosphoric acid is not completely separated by carbonate of baryta in the cold. The liquid, filtered at the end of several days, still yields a precipitate on the addition of sulphuric acid.

Solution of *chloride of calcium* gives no precipitate, even after standing for a considerable time; but ammonia immediately causes a copious precipitate. Lime-water produces a precipitate, even when the liquid is somewhat acid.

A dilute filtered solution of *albumen,* as we know, gives no precipitate with the solution of phosphoric acid.

A short time since we were made acquainted, by Svanberg and Struve, with an excellent reagent for phosphoric acid* in the *molybdate of ammonia.* This is so delicate in the detection of the smallest trace of phosphoric acid, and is capable of showing its presence even in those compounds in which the acid is discovered with difficulty or cannot be so at all, that an important service has been rendered to analytical chemistry by the recommendation of this reagent.

If a solution of the molybdate of ammonia be added to a solution of any phosphate, and then so much muriatic, or what is better nitric acid, that the precipitate which is formed at first disappears again, the liquid immediately becomes yellow, and deposits, even when the smallest quantity of phosphoric acid is present, a yellow precipitate, which consists of molybdic acid, but which is a different modification, and possesses different properties from the molybdic acid which is obtained when phosphoric acid is not present. If the phosphoric compound to be examined is insoluble in water, it is used in solution in acids, especially nitric acid. The precipitation is accelerated by heat. The yellow precipitate is soluble in ammonia, as also in an excess of the phosphate. Hence only very small quantities of phosphoric acid are most easily detected in this manner; and it is quite possible that a larger quantity might be overlooked, because in this case a very large quantity of the molybdate is requisite to produce the precipitate after saturation with nitric acid.

The yellow precipitate can be readily recognised, even when it is precipitated from a coloured liquid, as from a nitric solution of phosphate of copper, or from acid solutions of other coloured phosphates.

It must however be remarked, that *c*-phosphoric acid and its salts only are able to produce this reaction. The other modifications of phosphoric acid do not give the yellow precipitate with the molybdate of ammonia, unless they have been converted into the *c*-phosphoric acid by the nitric acid added. As is well known, this often takes place slowly and incompletely in the cold. Hence the pyrophosphate of soda may be allowed to remain for a very long time in dilute solutions with the molybdate of ammonia and free nitric acid, without any effect being perceptible. But if the whole be made to boil, a yellow liquid is instantly produced, and soon afterwards a yellow precipitate.

* Phil. Mag., S. 3, vol. xxiii. p. 524.

Experiment to separate Phosphoric from Pyrophosphoric Acid.

The different reaction of the phosphate and pyrophosphate of soda towards a solution of sulphate of magnesia and ammonia, led me to hope that it might form the basis of a method of separating these two modifications of phosphoric acid.

When pyrophosphate of soda is dissolved in a large amount of water, and the solution is mixed with a very large quantity of chloride of ammonium, no precipitate is produced on the addition of sulphate of magnesia and solution of ammonia. But at the end of a considerable period a precipitate falls, and is deposited firmly upon the sides of the vessel. If, however, the quantity of chloride of ammonium is very considerable, it frequently does not appear for several days.

1·828 grm. of hydrated *c*-phosphate of soda, which had lost a small quantity of its water of crystallization by efflorescence, was dissolved in water with 1·521 grm. of the same salt, which had been previously heated to redness and furnished 0·611 grm. of pyrophosphate of soda. The solution was mixed with 100 grms. of chloride of ammonium, then diluted with 1600 grms. of water, and sulphate of magnesia and solution of ammonia added. The precipitate was filtered off after an interval of two hours, then washed, first with water containing chloride of ammonium, afterwards with water containing ammonia. 0·814 grm. of calcined phosphate of magnesia was obtained, which contains 0·516 grm. of phosphoric acid. But the 1·828 grm. of phosphate of soda contains only 0·391 grm. of phosphoric acid: hence a considerable amount of pyrophosphoric acid was precipitated with the phosphate of ammonia and magnesia. This method of separation is consequently inapplicable.

XLVII. *On Quaternions; or on a New System of Imaginaries in Algebra.* *By* Sir William Rowan Hamilton, *LL.D., M.R.I.A., F.R.A.S., Corresponding Member of the Institute of France, &c., Andrews' Professor of Astronomy in the University of Dublin, and Royal Astronomer of Ireland.*

[Continued from p. 297.]

68. THE equation of the ellipsoid (see Philosophical Magazine for October 1847, or Proceedings of the Royal Irish Academy for July 1846),

$$\mathrm{T}(\iota\rho+\rho\kappa)=\kappa^2-\iota^2, \text{ eq. (9.), art. 38,}$$

which has so often presented itself in these researches, may be anew transformed as follows. Writing it thus,

$$\mathrm{T}\frac{(\iota\rho+\rho\kappa)(\iota-\kappa)}{\kappa^2-\iota^2}=\mathrm{T}(\iota-\kappa), \quad . \quad . \quad . \quad (125.)$$

which we are allowed to do, because the tensor of a product is equal to the product of the tensors, we may observe that while the denominator of the fraction in the first member is a pure scalar, the numerator is a pure vector; for the identity,

$$\iota\rho+\rho\kappa=\mathrm{S}.(\iota+\kappa)\rho+\mathrm{V}.(\iota-\kappa)\rho, \quad . \quad . \quad . \quad (126.)$$

gives

$$\mathrm{S}.(\iota\rho+\rho\kappa)(\iota-\kappa)=0: \quad . \quad . \quad . \quad . \quad (127.)$$

the fraction itself is therefore a pure vector, and the sign T, of the operation of taking the tensor of a quaternion, may be changed to the sign TV, of the generally distinct but in this case equivalent operation, of taking the tensor of the vector part. But, under the sign V, we may reverse the order of any *odd* number of vector factors (see article 20 in the Philosophical Magazine for July 1846); and thus may change, in the numerator of the fraction in (125.), the partial product $\iota\rho(\iota-\kappa)$ to $(\iota-\kappa)\rho\iota$. Again, it is always allowed to *divide* (though *not*, generally, in this calculus, to *multiply*) *both* the numerator and denominator of a quaternion fraction, *by* any *common* quaternion, or by any common vector; that is, to multiply both numerator and denominator *into the reciprocal* of such common quaternion or vector: namely by writing the symbol of this new factor to the *right* (but not generally to the left) of both the symbols of numerator and denominator, above and below the fractional bar. *Dividing* therefore thus above and below *by* ι, or *multiplying into* ι^{-1}, after that permitted transposition of factors which was just now specified, and after the change of T to TV, we find that the equation (125.) of the ellipsoid assumes the following form:

$$\mathrm{TV}\frac{(\iota-\kappa)\rho+\rho(\kappa-\kappa^2\iota^{-1})}{(\iota-\kappa)+(\kappa-\kappa^2\iota^{-1})}=\mathrm{T}(\iota-\kappa); \quad . \quad . \quad (128.)$$

the new denominator first presenting itself under the form $\kappa^2\iota^{-1}-\iota$, but being changed for greater symmetry to that written in (128.), which it is allowed to do, because, under the sign T, or under the sign TV, we may multiply by negative unity.

69. In the last equation of the ellipsoid, since

$$\kappa-\kappa^2\iota^{-1}=\kappa(\iota-\kappa)\iota^{-1},$$

we have

$$\mathrm{T}(\kappa-\kappa^2\iota^{-1})=\mathrm{T}\kappa\mathrm{T}(\iota-\kappa)\mathrm{T}\iota^{-1}; \quad . \quad . \quad (129.)$$

and under the characteristic U, of the operation of taking the versor of a quaternion, we may multiply by any positive scalar, such as $-\kappa^{-2}$ is, because κ^2 and κ^{-2} are negative*

* By this, which is one of the earliest and most fundamental principles of the whole quaternion theory (see the author's letter to John T. Graves,

scalars; whereas to multiply by a negative scalar, under the same sign U, is equivalent to multiplying the versor itself by -1: hence,

$$U(\kappa-\kappa^2\iota^{-1})=-U(\kappa^2\iota^{-1}-\kappa)=-U(\kappa^{-1}-\iota^{-1}). \quad . \quad (130.)$$

If then we introduce two new fixed vectors, η and θ, defined by the equations,

$$\eta=T\iota U(\iota-\kappa);\ \theta=T\kappa U(\kappa^{-1}-\iota^{-1});\ . \quad . \quad (131.)$$

and if we remember that any quaternion is equal to the product of its own tensor and versor (Phil. Mag. for July 1846); we shall obtain the transformations,

$$\iota-\kappa=\eta T\frac{\iota-\kappa}{\iota};\ \kappa-\kappa^2\iota^{-1}=-\theta T\frac{\iota-\kappa}{\iota};\ . \quad (132.)$$

which will change the equation of the ellipsoid (128.) to the following:

$$TV\frac{\eta\rho-\rho\theta}{\eta-\theta}=T(\iota-\kappa). \quad . \quad . \quad . \quad . \quad (133.)$$

70. To complete the elimination of the two old fixed vectors, ι, κ, and the introduction, in their stead, of the two new fixed vectors, η, θ, we may observe that the two equations (132.) give, by addition,

$$\iota-\kappa^2\iota^{-1}=(\eta-\theta)T\frac{\iota-\kappa}{\iota};\ . \quad . \quad . \quad . \quad (134.)$$

taking then the tensors of both members, dividing by $T\frac{\iota-\kappa}{\iota}$, and attending to the expression (81.) in article 56, (Phil. Mag. for May 1848,) for the mean semiaxis b of the ellipsoid, we find this new expression for that semiaxis:

$$T(\eta-\theta)=\frac{\kappa^2-\iota^2}{T(\iota-\kappa)}=b. \quad . \quad . \quad . \quad . \quad (135.)$$

Esq., of October 17th, 1843, printed in the Supplementary Number of the Philosophical Magazine for December 1844), namely by the principle that *the square of* EVERY VECTOR (or directed straight line in tridimensional space) is to to be regarded as a NEGATIVE NUMBER, this theory is not merely *distinguished from,* but sharply CONTRASTED *with, every other system* of algebraic geometry of which the present writer has hitherto acquired any knowledge, or received any intimation. In saying this, he hopes that he will not be supposed to desire to depreciate the labours of any other past or present inquirer into the properties of that important and precious Symbol in Geometry, $\sqrt{-1}$. And he gladly takes occasion to repeat the expression of his sense of the assistance which he received, in the progress of his own speculations, from the study of Mr. Warren's work, before he was able to examine any of those earlier essays referred to in Dr. Peacock's Report: however *distinct,* and even *contrasted,* on several *fundamental* points, may be (as was above observed) the methods of the CALCULUS OF QUATERNIONS from those of what Professor De Morgan has happily named DOUBLE ALGEBRA.

But also, by (131.), or by (132.),

$$\mathrm{T}\eta=\mathrm{T}\iota;\ \mathrm{T}\theta=\mathrm{T}\varkappa; \quad \ldots\ldots \quad (136.)$$

and therefore,

$$\theta^2-\eta^2=\varkappa^2-\iota^2. \quad \ldots\ldots \quad (137.)$$

Hence, by (135.), we obtain the expression,

$$\mathrm{T}(\iota-\varkappa)=\frac{\theta^2-\eta^2}{\mathrm{T}(\eta-\theta)}; \quad \ldots\ldots \quad (138.)$$

which may be substituted for the second member of the equation (133.), so as to complete the required elimination of ι and $\varkappa$. And if we then multiply on both sides by $\mathrm{T}(\eta-\theta)$, we obtain this new form* of the equation of the ellipsoid:

$$\mathrm{TV}\frac{\eta\rho-\rho\theta}{\mathrm{U}(\eta-\theta)}=\theta^2-\eta^2; \quad \ldots\ldots \quad (139.)$$

which will be found to include several interesting geometrical significations.

* This form was communicated to the Royal Irish Academy, at the stated meeting of that body on March 16th, 1849, in a note addressed by the present writer to the Rev. Charles Graves. It was remarked, in that note, that the *directions* of the two fixed vectors η, θ, are those of the two *asymptotes* to the focal hyperbola; while their *lengths* are such that the two extreme *semiaxes* of the ellipsoid may be expressed as follows:

$$a=\mathrm{T}\eta+\mathrm{T}\theta;\ c=\mathrm{T}\eta-\mathrm{T}\theta;$$

the *mean* semiaxis being, at the same time, expressed (as in the text of the present paper) by the formula

$$b=\mathrm{T}(\eta-\theta).$$

It was observed, further, that $\eta-\theta$ has the direction of *one cyclic normal* of the ellipsoid, and that $\eta^{-1}-\theta^{-1}$ has the direction of the *other* cyclic normal; that $\eta+\theta$ is the vector of *one umbilic*, and that $\eta^{-1}+\theta^{-1}$ has the direction of *another* umbilicar vector, or umbilicar semidiameter of the ellipsoid; that the *focal ellipse* is represented by the system of the two equations

$$\mathrm{S}.\varrho\mathrm{U}\eta=\mathrm{S}.\varrho\mathrm{U}\theta,$$

and

$$\mathrm{TV}.\varrho\mathrm{U}\eta=2\mathrm{S}\sqrt{\eta\theta},$$

of which the first represents its *plane*, while the second, which (it was remarked) might also be thus written,

$$\mathrm{TV}.\varrho\mathrm{U}\theta=2\mathrm{S}\sqrt{\eta\theta},$$

represents a *cylinder of revolution* (or, under the latter form, a *second* cylinder of the same kind), whereon the focal ellipse is situated; and that the *focal hyperbola* is adequately expressed or represented by the *single* equation,

$$\mathrm{V}.\eta\varrho.\mathrm{V}.\varrho\theta=(\mathrm{V}.\eta\theta)^2.$$

To which it may be added, that by changing the two fixed vectors η and θ to others of the forms $t^{-1}\eta$ and $t\theta$, we pass to a *confocal* surface.

[To be continued.]

XLVIII. *On the Cause of the Diurnal Variations of the Magnetic Needle.* *By* W. H. Barlow, *Esq., M.I.C.E.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

In the Number of your Journal for April, an extract from a letter from M. de la Rive to M. Arago is published, in which the author attributes the diurnal variations of the magnetic needle and the auroræ boreales to the effect of electric currents at the surface of the earth and in the atmosphere.

In confirmation of this theory, mention is made of a remarkable effect observed by M. Mattencci in the apparatus of the electric telegraph between Ravenna and Pisa during the magnificent aurora on the 17th of last November; and the author concludes by observing that "it would be highly interesting and important to profit by those telegraph wires, which are found to have a direction more or less approaching to that of the declination needle, in order to make with them, when they are not in use for ordinary purposes, some observations which would enable us to demonstrate and to measure the electric currents which probably traverse them."

My object in addressing you is to state, that in the early part of 1847 I was led to undertake extensive observations on this subject, in consequence of the peculiar disturbances occasionally visible on the telegraph instruments of the Midland Railway (on which line the telegraph was erected under my superintendence as the company's engineer).

These disturbances were at first attributed to atmospheric electricity passing to the earth by means of the wires; but from certain effects observed, I was led to infer that they were due to other causes; and in order to explain these effects, it is necessary to state that the Midland system of telegraphs consists of four principal lines centring in Derby, as follows:—

1st. From Derby northwards to Leeds.
2nd. From Derby north-east to Lincoln.
3rd. From Derby southwards to Rugby.
4th. From Derby south-west to Birmingham.

The disturbances on these four telegraphs were observed to occur simultaneously, with rare exceptions; and the direction of the current in the two telegraphs proceeding northerly and north-easterly was always contrary to those proceeding southerly and south-westerly; that is to say, when the deflection was such as to indicate that the current was towards Derby on the first two, it was from Derby on the last two; and when it changed in one, it changed in all. It was also observed that on the 19th of March 1847 there was an unusual degree of disturbance during the presence of auroræ boreales.

As these effects could not be attributed to the transit of ordinary atmospheric electricity along the wires to the earth, I determined to make a set of experiments on the subject.

Having obtained delicate galvanometers, I first ascertained that currents are at all times perceptible in the telegraph wires to a greater or less extent when the galvanometer is applied on a sufficient length of wire, and between two earth connections; but that wires having no earth connexion, or only one, exhibited no currents.

I also found by simultaneous observations on two galvanometers, applied one at each extremity of a wire forty-one miles long, that the changes of force and direction of the currents were simultaneous at both ends; the current passing direct from one earth connexion to the other.

But the most interesting fact which appeared during these observations, and that which bears immediately on the remarks contained in the letter of M. de la Rive, is that there is a daily movement of the galvanometer needle, similar to that of the horizontal magnetic needle, produced by the electric currents travelling in one direction from about 8 A.M. to 8 P.M., and returning in the opposite direction during the remainder of the twenty-four hours. The times of zero are not regularly maintained, and vary from 7 to 10 o'clock both in the morning and evening; but the greatest regularity is observable in the morning, and the mean result of numerous observations is as above stated.

This regular diurnal movement of the galvanometer needle is subject to disturbances of greater or less force and duration, which are found to be of greatest energy during magnetic storms, and when aurora is visible; and in these cases the currents are so strong as to affect the ordinary telegraph instruments, and sometimes prevent altogether the transmission of messages.

The next experiments were made with a view to ascertain the direction in which these currents alternate; and the result, as determined from numerous observations, denotes it to be from north-east to south-west. The nearer this line is approached, the more decided is the effect on the galvanometer; but between east and south, and between north and west, the effect is smaller; and in approaching north-west and south-east, it becomes indefinite and irregular, but never ceases entirely.

It also appeared that the effect depended, not on the direction of the wire itself, but on the relative directions of the two earth connexions; that is, the points where the wire was connected with the earth. I next made simultaneous observations with the galvanometers and a declinometer needle; from which it appeared, taking the mean of numerous observations, that that part of the day in which the currents flow southwards (that is, from 8 or 9 A.M. until the evening), the variation of the declinometer needle is westerly; and that during the night and early part of the morning (at which time the currents travel northwards) the variation is easterly; also, that the large disturbances called magnetic storms are simultaneous on both instruments.

But although there is this resemblance in the general features of the movements of both needles, the paths described are not similar. The movements of the galvanometer needle are more frequent and rapid than the declinometer, and the deflection frequently changes over from right to left without a corresponding movement of the declinometer.

The observations thus briefly recorded formed the subject of a paper which was read at the Royal Society on the 17th of June 1847; and I have thought it desirable to make this communication to your Journal on reading M. de la Rive's letter, because it rather curiously happens, that the unusual delay which has arisen in the publication of my paper by the Royal Society is attributable to the fact, that I arrived from these experiments at the same conclusion as M. de la Rive, as to the electric origin of the diurnal variation of the magnetic needle, which I considered to be the effect of the alternating electric currents exhibited by the telegraph wires.

The Royal Society were unwilling to give their sanction to this view of the case, and only consented to the publication of the observations above described on my omitting that portion of the paper.

The paper is, however, now in the hands of the printers, and will, I hope, be shortly before the public.

I ought to state in conclusion, that my idea of the origin of the currents differs in one respect from the theory of M. de la Rive; inasmuch as he considers them to arise in the atmosphere, whereas I have attributed them to thermo-electric action in the crust of the earth. I speak of course with great deference on a subject of this kind; but there is an important fact tending to this conclusion which is now well-ascertained, namely, that in the telegraphs which are laid entirely under ground, deflections occur similar to those before described; while wires suspended in the air exhibit no deflections, unless they are connected with the earth in two places, and then the direction in which the current travels depends on the relative positions of the earth connexions, however circuitous may be the route of the wire itself.

I am, Gentlemen,

Your obedient Servant,

Derby, April 12, 1849. W. H. Barlow, *M.I.C.E.*

XLIX. *Note on Numerical Transformation.* *By* T. S. Davies, *Esq., F.R.S. L. & E.**

In my notes on Mr. Cockle's paper (Phil. Mag., S. 3. vol. xxxii. p. 351), it was incidentally suggested to express the conjugate pair of roots of one of the quadratic factors of an algebraic equation by $\alpha+\beta$ and $\alpha-\beta$, without assigning the *algebraical form of* β. It was, moreover, proposed to form the two subordinate equations which contained relations between α and β, as is now generally done, after the example of Lagrange.

It has been objected by analysts with whom I have corresponded or conversed—analysts who would not have raised frivolous objections to any proposition whatever—that by the assumption of these forms I deprived myself of the means of forming those two equations upon any legitimate principle; inasmuch as their derivation is founded entirely on the application of the principle of *incongruity*, by showing that in

$$X+Y\beta\sqrt{-1}=0$$

we must have simultaneously

$$X=0, \text{ and } Y=0.$$

I may remark in the first place, that though I do not question the legitimacy of this argument when all the roots are imaginary, I still think it ambiguous when some of the roots are real, and altogether fallacious when there are no imaginary roots at all.

And, in the second place, that any process founded on this for the determination of the real roots of an equation, is totally deficient of all legitimate foundation.

I proceed, however, to the object of this note, which is to form the equations $X'=0$, $Y'=0$ on the unrestricted forms of the pair of conjugate roots, $\alpha+\beta$ and $\alpha-\beta$. I use accented letters, because the quantities represented by X' and Y' differ in the signs of the alternate terms from X and Y.

If $f(x)=0$ be an algebraic equation of an even degree, and $\alpha+\beta$, $\alpha-\beta$ two of its roots, we have

* Communicated by James Cockle, Esq., M.A., Barrister-at-Law.

$$0=f(\alpha+\beta)=X'+Y'\beta \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

$$0=f(\alpha-\beta)=X'-Y'\beta \quad . \quad . \quad . \quad . \quad . \quad (2.)$$

where X' and Y' contain only even powers of $\pm\beta$ and rational functions of α; and these powers and functions are the same in (1.) and (2.).

When β is different from zero, we get, by addition and subtraction of (1, 2),

$$X'=0, \text{ and } Y'=0,$$

precisely as in Lagrange's method, where $\beta\sqrt{-1}$ is put for β. The separation into two subordinate equations is therefore as legitimate in the general case as in Lagrange's restricted one.

When $\beta=0$, then Y' is indeterminate: but this does not seem to be of much consequence, since the original equation is then reduced, as it should be, to

$$f(\alpha)=0, \text{ and } f_{\prime}\frac{(\alpha)}{1.2}=0,$$

indicative of a common measure $x-\alpha$ between the equation and its *dérivée*. This we already know to be the consequence of the given equation having equal roots.

I shall shortly send you for insertion some extracts from Mr. Horner's rough notes on the effect of the transformation of $f(x)=0$ by means of the quadratic divisor

$$x^2+2\alpha x+\alpha^2+\beta^2,$$

employing his synthetic method of transformation.

Royal Military Academy, Woolwich,
April 6, 1849.

L. *On the Theory of Sound, in reply to* Professor Challis. *By* G. G. Stokes, *M.A., Fellow of Pembroke College, Cambridge**.

IN my last communication I contented myself with a simple denial in the case of the fifth head of Prof. Challis's demonstration, having nothing, as I conceived, to meet but a simple assertion. I am unable to perceive the slightest connexion between the conclusion that $4\pi r_1^2 s_1 \alpha = 4\pi r_2^2 s_2 \alpha$ and the principle of the constancy of mass, irrespective of some *tacitly assumed* step of reasoning. Without noticing this connexion explicitly, Prof. Challis has certainly rendered his reasoning more definite by the introduction of the supposition of rigid envelopes.

* Communicated by the Author.

Prof. Challis's argument, as it now stands, may be divided into the following heads:—

1. The condensation s may be expressed by $r^{-1}F(r-at)$.
2. The function F may be given arbitrarily from $r-at=b$ to $r-at=b+\lambda$, and may be supposed to vanish beyond those limits, so that the wave of condensation is comprised between the spheres whose radii are $at+b$ and $at+b+\lambda$.
3. The fluid may be supposed at rest beyond the outer, and within the inner boundary of the wave of condensation.
4. Therefore we may introduce two rigid envelopes, &c.

The third head I wholly deny, and have already, as I conceive, disproved. (Phil. Mag., vol. xxxiv. p. 54.) I have shown that unless $\int_b^{b+\lambda} F(r)dr=0$, the fluid *cannot* be at rest both outside and inside the wave of condensation.

It is vain to reply (as at p. 91) that the expression for the velocity is not proved. It *is* proved *on the hypothesis* (I do not myself regard it as an hypothesis) of spherical waves, as Prof. Challis does not seem to deny. It is plainly illogical reasoning to make the hypothesis of spherical waves, obtain *a part* of the results to which the analysis leads on that hypothesis, refuse to attend to other results to which the analysis equally leads, and then, *on the gratuitous assumption* of the possibility (according to the hypothesis under trial) of a state of things which does not result from the partial solution already obtained, and which the analysis, if wholly carried out, would have proved to be impossible, to argue that the hypothesis is absurd.

In conclusion, I will merely explain what I meant by saying that I did not regard the hypothesis of spherical waves, as an hypothesis. I consider it axiomatic that the *initial* condensation and *initial* velocity may be conceived to be given arbitrarily in a mass of elastic fluid; at least if no abrupt variations be supposed to exist in the initial condensation or velocity: such abrupt variations, in so far as they are admissible, may be afterwards considered as limiting cases of continuous variations. Consequently we may without absurdity conceive the initial condensation and initial velocity to be arranged symmetrically about a centre. But in this case the condensation and velocity must be symmetrical with respect to the centre during the whole motion; because if we draw any two radii vectores from the centre, whatever we can say of the one we can say of the other; and therefore the velocity will be directed to or from the centre, and the condensation and velocity will be functions of r and of t. This will be equally true whether we neglect or take account of the development of heat, whether we suppose the pressure equal in all directions, or adopt any other hypothesis. Accordingly, when we adopt the equations which are obtained on the usual theory, and suppose the initial condensation and initial velocity given arbitrarily as functions of r, no contradiction is arrived at, either in the general case (see Phil. Mag., vol. xxxiv. p. 55, paragraph marked 2), or in the particular case which might seem beforehand most favourable to the contradiction (see paragraph 3), as might have been confidently anticipated, inasmuch as one truth cannot contradict another.

Pembroke College, April 12, 1849.

LI. *Note on the Composition of Shea Butter and Chinese Vegetable Tallow.* *By* Dr. R. D. Thomson *and* Mr. Edward T. Wood*.

SHEA Butter.—This substance is a vegetable product of Western Africa, and was brought into notice by the celebrated Mungo Park during his journey in 1796. The tree from which it is procured he describes as very much resembling the American oak, and the fruit (from the kernel of which, being first dried in the sun, the butter is prepared by boiling the kernel in water) has somewhat the appearance of a Spanish olive. The kernel is enveloped in a sweet pulp under a thin green rind, and the butter produced from it, besides the advantage of keeping the whole year without salt, is whiter, firmer, and, according to Park, of a richer flavour than the best butter he ever tasted made from cows' milk. The growth and preparation of this commodity seem to be among the first objects of African industry, and it constitutes a main article of their inland commerce. This butter is abundantly produced, not only towards the Gambia, but also in the countries adjoining the Niger, as it is mentioned by the Landers and other recent travellers. Mr. John Duncan, who penetrated by Dahomey, describes the tree as resembling a laurel, and growing to the height of eighteen or twenty feet. The leaf is somewhat longer than the laurel and a little broader at the point. The nut is of the size and form of a pigeon's egg, and of a light brown colour; the substance of the shell about that of an egg. The kernel when new is nearly all butter. The shell is crushed from the kernel, which is also crushed and boiled with a little water in a pot for half an hour; it is then strained through a mat, when it is placed in a grass

* Read before the Philosophical Society of Glasgow, April 26, 1848.

bag and pressed. A good sized tree will yield a bushel of nuts.

Shea butter appears to be the same as that which is called Galam butter, and is derived from a species of Bassia; but the species has not yet been made out, as no specimens of the flower and fruit have reached botanists. The oil upon which the following experiments were made was obtained through the kindness of Dr. Carson of Liverpool, from Mr. Jameson, formerly of this city and now of Liverpool, whose benevolent exertions for the improvement of Africa are so well known. The colour of the oil is white with a shade of green. It is solid at the usual temperature in this country; at 95° it assumes the consistence of soft butter, and at 110° is a clear and liquid oil. When boiled in alcohol the greater part is dissolved, and crystallizes on cooling in needles: it dissolves in cold æther, and separates in needles by evaporation. The oil was saponified by means of caustic potash in a silver basin; the soap separated from its solution by common salt and decomposed by tartaric acid. After being crystallized out from alcohol five or six times, and freed by pressure from adhering oleic acid, the acid was obtained in fine pearly scales fusing at 142°: it was united with soda, and yielded a salt in fine pearly scales. Its atomic weight was estimated by means of the silver salt. In the first, second and third experiments, the silver salt was formed by precipitating an aqueous solution of nitrate of silver by an aqueous solution of the fatty acid united to soda. In the fourth and fifth experiments, an alcoholic solution of the acid was precipitated by a solution of nitrate of silver in alcohol, and hence the excess of acid.

I. 3·73 grains of silver salt gave 1·05 metallic silver = 1·126 oxide of silver = 30·19 per cent. AgO.

II. 10·65 grains of silver salt gave 3·01 silver = 3·221 oxide of silver = 30·23 per cent. AgO.

III. 2·85 grains gave ·861 AgO = 30·21 per cent.

IV. 4·71 grains gave 1·30 silver = 1·394 AgO = 29·53 per cent.

V. 2·72 grains gave ·743 silver = ·797 AgO = 29·30 per cent.

The following table will express the per-centage composition of the silver salt by these five experiments:—

	I.	II.	III.	IV.	V.
Acid	69·81	69·77	69·79	70·41	70·70
Oxide of silver .	30·19	30·23	30·21	29·59	29·30

Taking the mean of all these experiments, the constitution of the silver salt will be—

Acid	70·10
Oxide of silver .	29·90

and the atomic weight of the anhydrous salt is—

Acid	33·97
Oxide of silver .	14·50
	48·47

or leaving out the two last determinations, we shall have as a mean for the three higher results the atomic weight of the acid equal to 33·82. To determine the composition of the anhydrous acid, the three following analyses were made by means of oxide of copper and chlorate of potash. :—

I. 2·85 grs. of silver salt gave $HO=2·30$ grs. and $CO_2=5·73$ grs.
II. 3·91 ... ... =3·39 ... =7·87 ...
III. 3·667 ... ... =3·058 ... =7·334 ...

The following table gives the composition of the above salt in 100 parts :—

	I.	II.	III.	Mean.	Anhydrous acid.
C	54·73	54·88	54·54	54·71	77·83
H	8·94	8·78	9·22	8·98	12·77
O	6·12	6·75	6·94	6·60	9·40
AgO	30·21	29·59	29·30	29·71	

From the facts which have been stated in reference to the acid contained in the shea butter, it is obvious it is margaric acid, the same substance which is found in the human fat and in butter. There is little doubt that on examination this acid will be found extensively distributed in the vegetable kingdom: its presence in the shea butter may assist in explaining the statement of Park, that this substance when fresh is equal in taste to butter.

Chinese Vegetable Tallow.—This is a solid oil, long known to those who are acquainted with China, where it is extensively used for making candles. It is derived from the seeds of *Stillingia sebifera*, which, according to Fortune (Wanderings in China, p. 65), are pulled in November and December. They are placed in a wooden cylinder with a perforated bottom over an iron vessel filled with water, which is boiled and the seeds well-steamed to soften the tallow; in ten minutes they are thrown into a large stone mortar, and beat with stone mallets to separate the tallow from the other parts of the seed: the tallow is thrown on a sieve heated over the fire and sifted, and is then squeezed out by a peculiar process. As imported it is a hard, white, solid oil, with a green shade. It fuses at about 80°. The oil was saponified, and the acid separated and purified according to the method already noticed. A soda salt was formed, and from this a silver salt was precipitated. 14·38 grains of this salt when burned left 4·03 grains of metallic silver, which gives the following for the composition of the salt:—

		Atomic weight.	Per cent.
Oxide of silver .	4·328	14·50	30·03
Acid	10·052	33·67	69·97

The acid was not quite pure; for when heated it softened at 143°, became very soft at 149°, of the consistence of cream at 150°, and quite fluid at 154°; it obviously therefore retained some stearic acid, but must have consisted principally of margaric acid, as stearic acid fuses at 167°. There is no doubt that both of these oils might be advantageously employed in soap-making, the supply apparently, from the statements of the traders, being unlimited.

LII. *Determination of the Velocity of Sound on the principles of Hydrodynamics. By the* Rev. J. Challis, *M.A., F.R.S., F.R.A.S., Plumian Professor of Astronomy and Experimental Philosophy in the University of Cambridge**.

In conformity with the intention expressed at the close of my communication to the Number of the Philosophical Magazine for last February, I propose now to exhibit collectively the whole course of the mathematical reasoning by which I obtain, entirely on hydrodynamical principles, a value of the velocity of sound closely agreeing with that found by observation. The importance of the result, and the novelty of the considerations on which it depends, will be my excuse for going through the reasoning somewhat in detail, and for repeating some parts of previous communications. It may be proper to state at once, that I do not regard as defensible, or pertinent, all that I have written in the course of this difficult investigation; for instance, I have found that the new hydrodynamical equation, the necessity of which I have elsewhere insisted upon, is not, as I supposed, essential to the present inquiry. My immediate object is to extract and put in logical order what is really legitimate and essential.

The problem to be solved is, the numerical determination of the velocity of sound from the equations of hydrodynamics. As this may be considered to be a case of small vibrations, powers of the velocities and condensations above the first will be neglected. The pressure (p) being such that $p=a^2(1+s)$, and u, v, w being the resolved velocities at the point xyz and at the time t, the equations applicable are the following:—

* Communicated by the Author.

$$a^2\frac{ds}{dx}+\frac{du}{dt}=0,\quad a^2\frac{ds}{dy}+\frac{dv}{dt}=0,\quad a^2\frac{ds}{dz}+\frac{dw}{dt}=0,$$

$$\frac{ds}{dt}+\frac{du}{dx}+\frac{dv}{dy}+\frac{dw}{dz}=0,$$

all the differential coefficients being partial. Hence by integration,

$$u=-\int a^2\frac{ds}{dx}dt+c=-\frac{d.\int a^2 sdt}{dx}+c$$

$$v=-\int a^2\frac{ds}{dy}dt+c'=-\frac{d.\int a^2 sdt}{dy}+c'$$

$$w=-\int a^2\frac{ds}{dz}dt+c''=-\frac{d.\int a^2 sdt}{dz}+c'',$$

the arbitrary quantities c, c', c'' being functions of x, y and z. As the motion is by supposition vibratory, it will be assumed that $c=0$, $c'=0$, $c''=0$, which is to assume that no part of the velocity is independent of the time. Now substitute ψ for $-\int a^2 sdt$. Then

$$u=\frac{d\psi}{dx},\quad v=\frac{d\psi}{dy},\quad w=\frac{d\psi}{dz}.$$

It follows that $udx+vdy+wdz$ is an exact differential.

Here it must be particularly remarked that the above result has been arrived at *prior* to the consideration of any particular case of disturbance. Consequently $udx+vdy+wdz$ is an exact differential for some reason which applies equally to all cases of small vibrations. Such a reason would be given if it were proved, that the motion in every case is composed of motions in *plane-waves*, that is, waves in which the motion of each particle is perpendicular to a fixed plane and a function of the distance from the plane, the number of such waves and the directions of motion being taken arbitrarily. The consequences of the general supposition of plane-waves have been traced in an ingenious paper by Mr. Earnshaw, contained in the Transactions of the Cambridge Philosophical Society (vol. vi. part 2, p. 203). It is there shown that the velocity of propagation on that supposition is the constant a, which, as will afterwards appear, is a first approximation to the true theoretical velocity of sound. Mr. Earnshaw finds also that "a plane-wave cannot be transmitted through any fluid unless it extend completely across the medium from boundary to boundary." This result makes it impossible to conceive how the motion can in *every* case be composed of motions in plane-waves. There is, moreover, an antecedent objection to the supposition of plane-waves. For on making this supposition, a particular and exact integral of the resulting differential equation may be obtained, which, as I have recently shown in this Magazine by arguments that need not now be repeated, admits of no interpretation consistent with fluid motion. Such inconsistency must necessarily be *significant.* The reasoning by which it was arrived at being good, it clearly means that the general supposition of plane-waves is not legitimate.

Again, a general reason for the integrability of $udx+vdy+wdz$ is given, if it be proved that in every instance of small vibrations the motion is composed of motions in *spherical waves*, that is, waves in each of which the motion is directed to or from a fixed centre, and is a function of the distance from the centre. But such composition of the motion does not admit of being proved, because the hypothesis of spherical waves is liable to an antecedent objection. For on making this hypothesis, a result is arrived at inconsistent with the principle of constancy of mass, on which one of the general hydrodynamical equations rests. This I conceive that I have shown in my communication to the Number of the Philosophical Magazine for last February.

It appears, therefore, that vibratory motion is not generally compounded of motion either in plane-waves or spherical waves, and that the integrability of $udx+vdy+wdz$ is not accounted for on either of those suppositions. At this stage of the argument it is important to remark, that although inconsistencies have resulted from the *general* suppositions of plane-waves and spherical waves, it does not thence follow that these are not possible cases of *arbitrary* disturbance. As such, however, they must plainly be treated by a different process. Before treating these, or any other instances of arbitrary disturbance, it is absolutely necessary to assign a general reason for the integrability of $udx+vdy+wdz$. I proceed, therefore, to make another supposition.

Let the function which we have called ψ be composed of two factors, f and ϕ, such that f is a function of x and y only, and ϕ a function of z and t only. Then

$$u=\frac{d\psi}{dx}=\phi\frac{df}{dx}$$

$$v=\frac{d\psi}{dy}=\phi\frac{df}{dy}$$

$$w=\frac{d\psi}{dz}=f\frac{d\phi}{dz},$$

and

$$udx+vdy+wdz=\varphi\left(\frac{df}{dx}dx+\frac{df}{dy}dy\right)+f\frac{d\varphi}{dz}dz.$$

It hence follows that the left-hand side of the last equality is an exact differential. We have now to trace the kind of motion resulting from this general hypothesis.

For this purpose I shall assume, for the present, that the supposition by which $udx+vdy+wdz$ was made integrable, holds good for exact values of u, v and w, although the proof of the integrability of that quantity extended only to terms of the first order of approximation. Thus we shall have the following seven exact equations:—

$$u=\varphi\frac{df}{dx} \quad \ldots \quad (1.)$$

$$v=\varphi\frac{df}{dy} \quad \ldots \quad (2.)$$

$$w=f\frac{d\varphi}{dz} \quad \ldots \quad (3.)$$

$$\frac{d\rho}{dt}+\frac{d.\rho u}{dx}+\frac{d.\rho v}{dy}+\frac{d.\rho w}{dz}=0 \quad \ldots \quad (4.)$$

$$\frac{a^2d\rho}{\rho dx}+\left(\frac{du}{dt}\right)=0 \quad \ldots \quad (5.)$$

$$\frac{a^2d\rho}{\rho dy}+\left(\frac{dv}{dt}\right)=0 \quad \ldots \quad (6.)$$

$$\frac{a^2d\rho}{\rho dz}+\left(\frac{dw}{dt}\right)=0, \quad \ldots \quad (7.)$$

with an eighth deducible from the first three and the last three by integration, viz.

$$a^2\,\text{Nap.}\log\rho+f\frac{d\varphi}{dt}+\frac{1}{2}\left(\varphi^2\frac{df^2}{dx^2}+\varphi^2\frac{df^2}{dy^2}+f^2\frac{d\varphi^2}{dz^2}\right)=\text{F}(t). \quad (8.)$$

By means of equations (1.), (2.), (3.) and (8.), u, v, w, $\frac{d\rho}{\rho dt}$, $\frac{d\rho}{\rho dx}$, $\frac{d\rho}{\rho dy}$, and $\frac{d\rho}{\rho dz}$, may be eliminated from (4.), and the resulting equation is

$$\left.\begin{aligned}F'(t)=a^2.&\left\{\phi\left(\frac{d^2f}{dx^2}+\frac{d^2f}{dy^2}\right)+f\frac{d^2\phi}{dz^2}\right\}-f\frac{d^2\phi}{dt^2}\\ &-2\phi\left(\frac{d\phi}{dt}+f\frac{d\phi^2}{dz^2}\right)\left(\frac{df^2}{dx^2}+\frac{df^2}{dy^2}\right)\\ &-\phi^3\left(\frac{df^2}{dx^2}\cdot\frac{d^2f}{dx^2}+2\frac{df}{dx}\cdot\frac{df}{dy}\cdot\frac{d^2f}{dxdy}+\frac{df^2}{dy^2}\cdot\frac{d^2f}{dy^2}\right)\\ &-2f^2\frac{d\phi}{dz}\cdot\frac{d^2\phi}{dzdt}-f^3\frac{d\phi^2}{dz^2}\cdot\frac{d^2\phi}{dz^2}\end{aligned}\right\}\quad \text{(A.)}$$

In treating this equation, it may be assumed, since f and ϕ contain no variables in common, that if particular and consistent values be substituted for f, x and y, the resulting equation is true for general values of ϕ, z and t. Now since, by hypothesis, the motion is vibratory, the function f must have a maximum value. This value may be assumed to be unity, because f may be regarded as a numerical quantity, and one value of it may be taken arbitrarily. Let therefore the values of x and y given by the equations

$$\frac{df}{dx}=0, \quad \frac{df}{dy}=0,$$

satisfy the equations

$$f=1, \quad \frac{d^2f}{dx^2}+\frac{d^2f}{dy^2}=-\frac{b^2}{a^2},$$

the negative sign in the latter being a consequence of the supposition of a maximum. Then by substitution in (A.) we obtain the following equation for determining ϕ:

$$F'(t)=-b^2\phi+a^2\frac{d^2\phi}{dz^2}-\frac{d^2\phi}{dt^2}-2\frac{d\phi}{dz}\cdot\frac{d^2\phi}{dzdt}-\frac{d\phi^2}{dz^2}\cdot\frac{d^2\phi}{dz^2}. \quad \text{(B.)}$$

It does not appear that this equation is generally integrable: but an integral applicable to the present inquiry may be obtained by successive approximations, the terms involving the first power of ϕ being first considered. The arbitrary function of the time may be supposed to be included in ϕ. Thus to the first approximation we have

$$\frac{d^2\phi}{dt^2}-a^2\frac{d^2\phi}{dz^2}+b^2\phi=0.$$

The integral of this equation, as I have shown in the Philosophical Magazine for April 1848 (p. 278), may be obtained in an infinite series involving two arbitrary functions. In investigating the velocity of propagation, one of the arbitrary functions must be made to vanish. This having been done, and the following substitutions made, viz.

$$e=\frac{b^2}{4a^2},\quad \mu=z+at,\quad \nu=z-at,\quad G_1(\nu)=\int G(\nu)d\nu,$$

$$G_2(\nu)=\int G_1(\nu)d\nu, \text{ \&c.,}$$

it was found that

$$\phi=G(\nu)+e\mu G_1(\nu)+\frac{e^2\mu^2}{1.2}G_2(\nu)+\frac{e^3\mu^3}{1.2.3}G_3(\nu)+\text{\&c.}$$

No inference respecting the propagation of the motion can be drawn from this result, unless ϕ be expressed in exact terms. The form of the series shows that this can be done only by satisfying the following equations:

$$n^2.G_1(\nu)=\pm\frac{d.G(\nu)}{d\nu}$$

$$n^2.G_2(\nu)=\pm\frac{d.G_1(\nu)}{d\nu}=+\frac{1}{n^2}.\frac{d^2.G(\nu)}{d\nu^2}$$

$$n^2.G_3(\nu)=\pm\frac{d.G^2(\nu)}{d\nu}=\pm\frac{1}{n^4}.\frac{d^3.G(\nu)}{d\nu^3}$$

$$\text{\&c.}= \qquad\qquad \text{\&c.}$$

Now since $G_1(\nu)=\int G(\nu)d\nu$, it follows that

$$\frac{d.G_1(\nu)}{d\nu}=G(\nu)$$

$$\frac{d^2.G_1(\nu)}{d\nu^2}=\frac{d.G(\nu)}{d\nu}=\pm n^2.G_1(\nu),$$

or

$$\frac{d^2.G_1(\nu)}{d\nu^2}\mp n^2.G_1(\nu)=0.$$

The upper sign would give to the function G a logarithmic form, which is clearly excluded by the nature of vibratory motion. Taking, therefore, the lower sign, the integral of the last equation gives

$$G_1(\nu)=-\frac{m}{n}\sin(n\nu+c).$$

Consequently

$$G(\nu)=m\cos(n\nu+c).$$

This form of G satisfies all the other equations. Hence

$$\varphi = G(\nu) - \frac{e\mu}{n^2} \cdot \frac{d.G(\nu)}{d\nu} + \frac{e^2\mu^2}{1.2.n^4} \cdot \frac{d^2.G(\nu)}{d\nu^2} - \&c.$$

$$= G\left(\nu - \frac{e\mu}{n^2}\right)$$

$$= m \cos\left\{n\left(\nu - \frac{e\mu}{n^2}\right) + c\right\}$$

$$= m \cos\left\{n(z-at) - \frac{e}{n}(z+at) + c\right\}.$$

Let, now,

$$n - \frac{e}{n} = \frac{2\pi}{\lambda},$$

so that

$$n + \frac{e}{n} = \left(\frac{4\pi^2}{\lambda^2} + 4e\right)^{\frac{1}{2}}.$$

Then

$$\varphi = m \cos \frac{2\pi}{\lambda}\left(z - at\sqrt{1 + \frac{e\lambda^2}{\pi^2}} + c'\right).$$

Hence it appears that the velocity of propagation of the wave, or series of waves, defined by the above form of φ, is

$$a\sqrt{1 + \frac{e\lambda^2}{\pi^2}},$$

the propagation taking place along a straight line parallel to the axis of z, or, if we please, along the axis of z itself. Here it is important to remark, that the particular expression obtained for φ, and the consequent velocity of propagation, have been arrived at by a strict induction. The course of the investigation leads to these results and to no others of a like kind. Hence as there is at present no case of disturbance under consideration, these results have, with regard to vibratory motion, a general significance. The inference from them is, that whatever be the disturbance, the motion consists of vibrations defined by a circular function of the above form, and that the velocity of propagation exceeds the value a by a quantity depending on the numerical value of $\frac{e\lambda^2}{\pi^2}$.

If the investigation be conducted on the hypothesis of plane-waves, the solution to the first order of approximation is $\psi = G(z - at)$, and the velocity of propagation is a, the form of G remaining arbitrary. I have already argued that these results have no significance, because the exact integral, of which this is the first approximation, conducts to results incompatible with fluid motion. The same objection might be raised against the results of the hypothesis now under consideration, if, on carrying the investigation to higher degrees of approximation, any similar incompatibility appeared. To determine whether this be the case, it is now required to integrate equation (B.), taking account of the two last terms.

This may be done by successive approximations, beginning with the value of ϕ already obtained. The result to three terms, as given in my communication to the Philosophical Magazine for November 1848 (p. 363), is

$$\begin{aligned}\phi = {} & m \cos q(z - a't + c) \\ & - \frac{m^2 q^3 a'}{3b^2} \sin 2q(z - a't + c) \\ & - \frac{m^3 q^4}{4b^2}\left(\frac{q^2 a^2}{b^2} + \frac{7}{8}\right) \cos 3q(z - a't + c)\end{aligned}$$

and

$$a'^2 = a^2 + \frac{b^2}{q^2} + m^2 q^2 \left(\frac{2q^2 a^2}{3b^2} + \frac{5}{12}\right),$$

q being substituted for $\frac{2\pi}{\lambda}$.

So far as this result indicates, ϕ is a function of $z - a't$, and the velocity of propagation at all points of a wave is the constant a'. To ascertain whether constant propagation, the same for all points of the same wave, accords exactly with equation (B.), let us introduce into this equation the condition $\phi = \mathrm{F}(z - a_1 t)$. For the sake of brevity write ν for $z - a_1 t$, and F for $\mathrm{F}(\nu)$. Then, supposing a_1 constant while z and t vary,

$$\frac{d^2\phi}{dt^2} = a_1^2 . \frac{d^2\mathrm{F}}{d\nu^2}, \quad \frac{d\phi}{dz} = \frac{d\mathrm{F}}{d\nu}, \quad \frac{d^2\phi}{dz\,dt} = -a_1 \frac{d^2\mathrm{F}}{d\nu^2}.$$

Consequently

$$\frac{d^2\mathrm{F}}{d\nu^2}\left(a_1^2 - a^2 - 2a_1 \frac{d\mathrm{F}}{d\nu} + \frac{d\mathrm{F}^2}{d\nu^2}\right) + b^2 \mathrm{F} = 0. \quad . \quad . \quad (\alpha.)$$

It may be here observed, that if the term involving b^2 be omitted, the resulting equation, which applies exactly to the case of plane-waves, is satisfied by either of the two equations,

$$\frac{d^2\mathrm{F}}{d\nu^2} = 0, \quad \left(a_1 - \frac{d\mathrm{F}}{d\nu}\right)^2 - a^2 = 0.$$

Neither of these equations gives a form of F compatible with vibratory motion: whence we may infer that, on the hypothesis of plane-waves, all the parts of a wave are not propagated with the same velocity. This result leads to the inconsistency already spoken of.

Retaining now the term involving b^2 in equation (α.), this equation gives by integration a certain form of F, which, if it be compatible with vibratory motion, is the particular form that satisfies the condition of constant and identical propagation of all parts of the same wave. A first integral of the equation is readily obtained; but the complete integration can probably be effected only by successive approximations. To the first approximation we have

$$\frac{d^2F}{d\nu^2} + \frac{b^2}{a_1^2 - a^2} \cdot F = 0.$$

Whence

$$F = m \cos\left(\frac{b\nu}{\sqrt{a_1^2 - a^2}} + c\right);$$

consequently, putting

$$\frac{2\pi}{\lambda} \text{ for } \frac{b}{\sqrt{a_1^2 - a^2}},$$

we obtain

$$a_1^2 = a^2 + \frac{b^2\lambda^2}{4\pi^2} = a^2\left(1 + \frac{e\lambda^2}{\pi^2}\right)$$

and

$$\varphi = F(\nu) = m \cos \frac{2\pi}{\lambda}\left(z - at\sqrt{1 + \frac{e\lambda^2}{\pi^2}} + c\right).$$

This is the first approximate value of φ already obtained by the integration of the partial differential equation (B.). By continuing the solution of equation (α.) to the third approximation, I find precisely the same expression for φ as that above given for the solution of equation (B.) to the same degree of approximation. We may hence conclude that the solution of (B.) carried to an unlimited number of terms, has the property of satisfying exactly the condition of uniform and identical rate of propagation of all the parts of a wave. I have proved this proposition in another way in the Philosophical Magazine for December 1848, p. 465.

The next step is to introduce into the equation (A.) the condition which it has just been shown that the function φ must satisfy, viz. the condition expressed analytically by the equation

$$\frac{d\varphi}{dt} + a_1 \frac{d\varphi}{dz} = 0.$$

When by means of this equation the differential coefficients of the second order are eliminated from (A.), the result is

$$\left.\begin{aligned} 0=a^2\varphi\left(\frac{d^2f}{dx^2}+\frac{d^2f}{dy^2}\right)+b^2f\varphi.\frac{a^2-\left(a_1-f\frac{d\varphi}{dz}\right)^2}{a^2-\left(a_1-\frac{d\varphi}{dz}\right)^2} \\ +2\varphi\frac{d\varphi}{dz}\left(a_1-f\frac{d\varphi}{dz}\right)\left(\frac{df^2}{dx^2}+\frac{df^2}{dy^2}\right) \\ -\varphi^3\left(\frac{df^2}{dx^2}.\frac{d^2f}{dx^2}+2\frac{df}{dx}.\frac{df}{dy}.\frac{d^2f}{dxdy}+\frac{df^2}{dy^2}.\frac{d^2f}{dy^2}\right). \end{aligned}\right\} \quad (A'.)$$

Hence it appears that an equation altogether independent of φ is obtained by neglecting powers of this quantity above the first. When this is done, the equation becomes

$$0=\frac{d^2f}{dx^2}+\frac{d^2f}{dy^2}+\frac{b^2f}{a^2}. \quad . \quad . \quad . \quad . \quad (\beta.)$$

At the same time equation (B.) becomes

$$0=\frac{d^2\varphi}{dt^2}-a^2\frac{d^2\varphi}{dz^2}+b^2\varphi. \quad . \quad . \quad . \quad . \quad (\gamma.)$$

We have thus arrived at two equations, one of which shows that f is a function of x and y only, and the other that φ is a function of z and t only. These results are in accordance with the original suppositions respecting these quantities, by which $udx+vdy+wdz$ was made integrable. The condition of integrability of that quantity has therefore now been satisfied in a manner consistent with the hydrodynamical equations. It is to be remarked that the equations (β.) and (γ.) contain only the first powers of f and φ. Now since the reasoning given at the commencement of this communication, by which it was shown that $udx+vdy+wdz$ must be integrable for vibratory motion, extended only to the first power of the velocity, there was no reason to expect greater generality in the equations which determine f and φ. It may, however, be remarked, that the equation (β.), as equation (A$'$.) shows, is satisfied without any restriction of the value of φ, the more exactly in proportion as the value of f approaches more nearly to unity. We may hence conclude that for points on and immediately contiguous to the axis of z, the motion is exactly determined by the equations (β.) and (B.), and that for these points $udx+vdy+wdz$ is integrable for exact values of u, v, and w. This result is important, because it enables us to infer from the reasoning that has preceded, that along the axis of z waves are propagated without undergoing any change whatever, all the parts of a given wave being propagated with precisely the same velocity.

To obtain a complete idea of the nature of the motion, it is now required to treat equation (β.) by the same process as that applied to equation (γ.), for the purpose of deducing the particular form of the integral which defines the motion transverse to the axis of z independently of any arbitrary disturbance. It may be presumed that such a form exists, because the motion along the axis is already so defined. I have exhibited the mathematical reasoning by which an integral of this nature is obtained, in the *Postscript* to my communication to the Philosophical Magazine for last February (p. 96). The following is another process which conducts to the same result. If $\mu=x+y\sqrt{-1}$, and $\nu=x-y\sqrt{-1}$, the general integral of (β.) is

$$f=\mathrm{F}(\mu)+\mathrm{G}(\nu)-e\{\nu\mathrm{F}_1(\mu)+\mu\mathrm{G}_1(\nu)\}$$
$$+\frac{e^2}{1.2}\{\nu^2\mathrm{F}_2(\mu)+\mu^2\mathrm{G}_2(\nu)\}$$
$$-\&c.,$$

where

$$\mathrm{F}_1(\mu)=\int\mathrm{F}(\mu)d\mu,\quad \mathrm{F}_2(\mu)=\int\mathrm{F}_1(\mu)d\mu,\ \&c.,\quad \mathrm{G}_1(\nu)=\int\mathrm{G}(\nu)d\nu,$$
$$\mathrm{G}_2(\nu)=\int\mathrm{G}_1(\nu)d\nu,\ \&c.$$

Now a specific form may be given to f by supposing the arbitrary functions to be arbitrary *constants*. Let, therefore, $\mathrm{F}(\mu)=c$, and $\mathrm{G}(\nu)=c'$. Then

$$\mathrm{F}_1(\mu)=c\mu,\quad \mathrm{F}_2(\mu)=\frac{c\mu^2}{2},\quad \mathrm{F}_3(\mu)=\frac{c\mu^3}{1.2.3},\ \&c.$$

$$\mathrm{G}_1(\nu)=c'\nu,\quad \mathrm{G}_2(\nu)=\frac{c'\nu^2}{2},\quad \mathrm{G}_3(\nu)=\frac{c'\nu^3}{1.2.3},\ \&c.$$

Hence

$$f=(c+c')\left(1-euv+\frac{e^2u^2v^2}{1^2.2^2}-\frac{e^3u^3v^3}{1^2.2^2.3^2}+\&c.\right),$$
$$=(c+c')\left(1-er^2+\frac{e^2r^4}{1^2.2^2}-\frac{e^3r^6}{1^2.2^2.3^2}+\&c.\right),$$

by putting r^2 for x^2+y^2. But f has already been required to satisfy the condition, $f=1$ when $r=0$. Consequently $c+c'=1$, and the arbitrary constants disappear of themselves. Thus we obtain

$$f=1-er^2+\frac{e^2r^4}{1^2.2^2}-\frac{e^3r^6}{1^2.2^2.3^2}+\&c.,\quad .\ .\ (\delta.)$$

a result independent of all that is arbitrary. This form of f indicates that the motion is the same in all directions transverse to the axis of z.

By thus obtaining, prior to any supposed case of disturbance, particular forms of ϕ and f, which define a particular kind of vibrations, it is shown that in all cases of small disturbances the motion is composed of such vibrations. As the vibrations are symmetrically disposed about an axis, I have called them *ray-vibrations.* The number of the rays (the equations defining them being linear), the directions of their axes, and the values of m and λ, may be assumed so as to satisfy the conditions of given disturbances.

This theory is not complete without obtaining a numerical value of the velocity of propagation. It is unnecessary to repeat here in detail the mathematical reasoning by which I succeeded in doing this in the *Postscript* to my communication to the February Number of this Journal. It will suffice to say that by making $f=0$ in the equation (δ.), an equation results, which is satisfied by an infinite number of values of r, such that the difference between two consecutive values approaches continually, as r increases, to the limit $\frac{1}{\sqrt{e}}$. These values of r correspond to positions of no condensation, since to the first approximation we have

$$a^2 s + f\frac{d\phi}{dt} = 0.$$

The values of r which satisfy the equation $\frac{df}{dr}=0$, are also unlimited in number, being intermediate to those which satisfy $f=0$, and correspond to positions of no transverse velocity, since the velocity transverse to the axis of the vibrations is $\phi\,\frac{df}{dr}$. Between two consecutive cylindrical surfaces of no velocity at an infinite distance from the axis, the transverse vibrations are the same in kind as those which would take place along the axis between two points of no velocity, supposing two series of waves exactly equal were propagated along that axis in opposite directions. The effect of the simultaneous propagation of two such series would be, to produce vibrations along the axis like the transverse vibrations at an infinite distance; and as the time of a vibration would be the same in the two cases, it follows that

$$\frac{1}{\sqrt{e}} = \frac{\lambda}{2}, \text{ and } \frac{e\lambda^2}{\pi^2} = \frac{4}{\pi^2}.$$

Hence, from what has already been shown, the velocity of propagation is

$$a\sqrt{1+\frac{4}{\pi^2}}.$$

The numerical value of this quantity is less by only 3·17 feet than the mean result (1089·42 feet) of a large number of determinations of the velocity of sound by direct experiment.

The course of this investigation has shown that the velocity of propagation (a) resulting from the hypothesis of plane-waves, is not the correct value deducible from hydrodynamical principles. The only true value given by hydrodynamics is that I have exhibited above. The difference between the observed velocity of sound and the value a, has been attributed, according to a well-known theory, to the effect of the development of heat accompanying sudden compressions of the air, and of absorption of heat accompanying sudden dilatations. The fact of such development and absorption is established by experiments made on air in *enclosed* spaces. The walls of the enclosure, by preventing the immediate escape of the developed heat, and the immediate restitution of the absorbed heat, allow of ascertaining the fact by the thermometer. The same development and absorption of heat must accompany the condensations and rarefactions of aërial vibrations. But the absence of enclosing walls makes an essential difference between this case and that of the experiment just mentioned. It may not unreasonably be supposed that the thermometric effect in the experiment is wholly due to reflexions at the walls of the enclosure, by which the developed heat or cold is made to traverse the enclosed air numberless times in an inappreciable interval, and thus produces a sudden change of temperature. In a similar manner, when heat is radiating from the earth's surface into a clear sky, as soon as a cloud comes over, the air between the cloud and the earth, becoming in some degree enclosed, and being traversed by the heat reflected from the one to the other, has its temperature immediately raised. As the heat developed or absorbed by aërial vibrations cannot be supposed to produce a difference of temperature by any analogous operation, a particular hypothesis is required to account for an analogous effect in this case. It is assumed that the air possesses the property of detaining the heat or cold set free by sudden compression or dilatation, and that as the development or absorption is greater the greater the condensation or rarefaction, there is always by this detention an excess of temperature in the denser of two contiguous elements of the vibrating air, and consequently an effective accelerative force from the denser to the rarer portion, which produces an apparent increase of the elasticity of the medium.

Respecting this theory, it must be said that it cannot be considered as fully established, unless the property which it ascribes to the air of preventing the immediate escape of the developed heat or cold be demonstrated. At present this property is hypothetical. The argument contained in this communication would rather point to the inference, that the developed heat or cold instantly passes off in a radiant form, without producing any very appreciable alteration of the state of temperature of the air where it is generated.

The mathematical investigation of the velocity of sound which I have now expounded, originated in an attempt to explain theoretically the polarization of light exclusively on hydrodynamical principles, the æther being treated as a continuous medium, the pressure of which, as in air of given temperature, varies in the same proportion as the density. My views on this subject are contained in several recent communications to the Cambridge Philosophical Society. In the course of the inquiry I found that the velocity of propagation in such a medium was not the value a, as generally supposed, but a certain greater quantity $a\sqrt{1+k}$, and that the explanation of the phænomena of polarization essentially depended on this result. It was clearly, therefore, important to obtain a numerical value of $a\sqrt{1+k}$ which could be tested by actual observation of the velocity of sound. This I consider that I have now done, and that I have thus removed an objection which otherwise might have been urged against the proposed theory of the polarization of light.

Cambridge Observatory,
April 18, 1849.

LIII. *Remarks on the Weather during the Quarter ending March* 31, 1849. *By* James Glaisher, *Esq., of the Royal Observatory, Greenwich**.

THE meteorological returns for the past quarter furnished to the Registrar-General and myself have been received from thirty-four different places, whose returns have passed the necessary examination. The observations generally indicate a decided improvement, having been made for the most part by experienced observers, who have generally paid more attention to their instruments than hitherto. The results are therefore found to be more accordant with each other than any previously received.

Till January 7 and after March 18, the temperature of the air was below its average value; the mean amount of the deficiency of daily temperature in the former period was 6°·9, and in the latter it was 3°·7.

* Communicated by the Author.

The interval of time between January 8 and March 17 was distinguished by very unusual warmth for the season. The average daily excess of temperature within this period was 6°·1; on four of the days this exceeded 12°, on three days it exceeded 13°, and on two days it was greater than 14°.

The mean temperature of the three months ending February, constituting, in fact, the three winter months, was 42°·5, being no less than 4°·7 above the average temperature of the same time for seventy years. The warmest winters within this period were those ending February 1796, 1822, 1834 and 1846, and which were 43°·2, 42°·4, 43°·0 and 43°·2 respectively.

The pressure of the atmosphere during the month of February was very unusual. The average reading of the barometer from the 1st of February till the 18th was 30·36 inches at the height of 160 feet: this was fully half an inch above its average value. This denotes an increase in the volume of air of about one-sixtieth part above the usual quantity. On the 11th day the very unusual reading of 30·715 inches took place. The true reading for the whole day, reduced and corrected to 32° Fahrenheit, was 30·695 inches, showing that about one-thirtieth more than the usual quantity of air was over England on this day. The reading of the barometer on the 11th day, reduced to the level of the sea, was 30·91 inches. In December 1778 the reduced reading was 30·90 inches; in January 1825 it was 30·92 inches ±.

The condition of the atmosphere, therefore, during the greater part of the past quarter, both with respect to pressure and heat, has been very unusual.

From the discussion of the observations which have been made at the Apartments of the Royal Society since 1774, there appears to be no foundation for the opinion that a hot summer either precedes or follows a cold winter; on the contrary, the hot summers have for the most part been accompanied by warm winters.

From the long continuance of high temperatures, it would seem that for some time past causes have been in operation which have raised the temperature: these causes probably still exist, and therefore there seems to be every probability of a fine and warm summer.

I proceed now to detail the results of the several subjects of research in the past quarter.

The mean temperature of the air—

For the month of January was 40°·1, *exceeding* the average of seventy years by 4°·3. The temperatures in this month in the years 1775, 1796, 1804, 1806, 1819 and 1834, were those only which exceeded that of this year. In the year 1796 it was 45°·4, being the warmest on record;

For the month of February was 43°·2, *exceeding* the average of the preceding seventy years by 4°·7. The temperature of this month in the year 1779 was 45°·2, being the only instance within the period of seventy years in which the temperature exceeded that of this year;

For the month of March was 42°·5, *exceeding* the average of seventy years by 1°·2.

The mean for the quarter was 41°·9. The average value for seventy years is 38°·6. In the year 1779 the mean was 42°·0; in 1822 it was 43°·4; in 1834 it was 42°·8; and in the year 1846 it was 43°·6: in all the remaining years it was less than 42°·0.

The *excess* of temperature above the average of the preceding eight years was in January 3°·0; in February was 5°·4; in March was 0°·1; and for the quarter was 2°·9.

The mean temperature of evaporation at Greenwich—

For the month of January was 38°·6; for February was 41°·4; and for March was 39°·8. These values are 2°·0 *above*, 6°·2 *above*, and 0°·3 *below*, respectively, the averages of the preceding eight years.

The mean value for the quarter was 39°·9, which is 2°·6 *above* that of the average of eight years.

The mean temperature of the dew-point at Greenwich—

For the months of January, February and March, were 36°·4, 38°·8, and 36°·5. The average values for the preceding eight years were 35°·1, 34°·5, and 36°·6.

The mean value for the quarter was 37°·2, which is 1°·8 *above* the average for the preceding eight years.

The mean elastic force of vapour for the quarter was 0·239 inch, which is 0·012 inch *greater* than the average for the preceding seven years.

The mean weight of water in a cubic foot of air for the quarter was 2·8 grains, which is 0·1 grain *greater* than the average of the preceding seven years.

The mean additional weight of water required to saturate a cubic foot of air was 0·6 grain. This value for the preceding seven years was 0·38 grain.

The mean degree of humidity in January was 0·883, in February was 0·863, and in March was 0·801. The averages for the seven preceding years were 0·903, 0·888, and 0·841. The mean value for the quarter was 0·849, which is 0·028 *less* than the average for these years. These values denote a considerable degree of dryness in these months.

The mean reading of the barometer at Greenwich in January was 29·771 inches, in February was 30·106 inches, and in March was 29·915 inches; these values are 0·005 inch *above*, 0·415 inch *above*, and 0·186 inch *above* respectively the averages of the same months for the preceding eight years.

The reading for the month of February, exceeding 30·1 inches at the height of 160 feet, is very remarkable. Since the year 1774 there have been eight such instances only: these occurred in July 1800, April 1801, November 1805, April 1817, February 1821, January 1825, December 1834, and December 1843.

The average weight of a cubic foot of air under the average temperature, humidity, and pressure, was 549 grains; the average for the seven preceding years was 546 grains.

The rain fallen at Greenwich in January was 1·6 inch; in February was 2·2 inches; and in March was 0·5 inch. The amount for the quarter was 4·24 inches. The average amount for the preceding eight years was 5·14 inches.

The temperature of the water of the Thames was 43°·8 by day, and 42°·1 by night. The water, on an average, was 1° warmer than the air.

The direction of the wind at Greenwich from January 1 to 6 was N.E.; from January 7 to 28 was S.W.; on January 29 was N.N.W.; from January 30 to March 7 was at times variable, but chiefly S.W.; from March 8 to 17 was mostly N.W.; from March 18 to 28 was chiefly N.E.; and afterwards it was mostly S.S.W. to the end of the month.

At Leicester the direction of the wind was S.W. during seventy-six days within the quarter.

The daily horizontal movement of the air from January 1 to 6 was about 90 miles; the greatest value was 200 miles; from January 7 to 28 was 240 miles; the greatest was 500 miles; and from January 30 to March 28 it was 110 miles; the greatest was 320 miles. The movement of the air in the month of March was small.

The average daily ranges of the thermometer in air at the height of four feet, were 10°·8 in January, 12°·9 in February, and 13°·8 in March. The average ranges for these three months, from the observations of the eight preceding years, were 8°·1, 9°·6, and 13°·4 respectively.

The readings of the thermometer on grass in January were below 20° on three nights, the lowest was 17°; at and below 32° on fourteen nights; between 32° and 40° on six nights; above 40° on six nights; and on one night it was 50°.

In February the lowest reading was 20°, and the readings were below 32° on eleven nights; between 32° and 40° on eight nights, and above 40° on seven nights. In March the lowest reading was 21°; and the readings were below 32° on sixteen nights; between 32° and 40° on twelve nights; and above 40° on three nights.

The mean amount of cloud was 7·5, being the same as the average for the preceding eight years.

There were nine exhibitions of the *aurora borealis* during the quarter ending March 31, 1849, which occurred on January 14, and were seen at Aylesbury, Whitehaven and Maidenstone Hill; on January 15 at Hartwell; on February 18 at Wakefield; on the 19th at Stone, Whitehaven and Wakefield; on the 20th at Whitehaven, Hartwell and Greenwich; on the 21st at Hartwell; on the 22nd at Holkham, Aylesbury, Stone, Norwich, Newcastle and Greenwich; on the 23rd at Whitehaven and Hartwell; and on March 18 at Stone.

Thunder-storms occurred on January 10 at Whitehaven; on January 14 at Norwich; on February 25 at Truro; and on March 31 at Uckfield.

Hail fell at Norwich and Hartwell on January 14; at Newcastle on February 22; at Wakefield on February 23; at Saffron Walden on March 8.

Snow fell at Saffron Walden on January 4; at Leicester, Saffron Walden, Highfield House, and Southampton on January 5; at Saffron Walden on January 29; at Stone, Hartwell, Norwich, and Saffron Walden on February 28; at Stone and Saffron Walden on March 24 and 25; at Stone on the 28th; and at Norwich on the 31st.

Solar halos were seen at Greenwich on February 26 and on March 30.

Lunar halos were seen at Greenwich on January 6, and at Stone on February 27, March 7, 8 and 31.

Zodiacal light was seen at Whitehaven on February 11.

The reading of the barometer was above 30 inches on the 1st of January; it decreased to 29·58 by the 3rd, and increased to 29·93 by the 6th. During the evening of the 7th it decreased quickly, and was 29·4 on the 8th and 9th. On the 10th the lowest reading in the month took place, and was 28·83; on the 11th it increased rapidly, and was 29·85 at midnight, and passed the point 30 early on the morning of the 12th. On the 13th and 14th the readings decreased, and were 29·31 on the latter day, and then it increased with slight exceptions till the evening of the 23rd, when the reading was 30·328, being the highest during the month; it then decreased, slowly at first, and rapidly during the afternoon of the 27th. The reading was 29·26 on the evening of the 28th, after which it increased, and was 30·16 at the end of the month. In February the average reading from the 1st to the 18th was 30·36. On the 11th, in the evening, the very extraordinary reading of 30·715 took place. On the 19th the reading descended below 30 inches; on the 20th the decrease was 0·4; on the 21st and 22nd the reading was about 29·7; on the 24th and 25th the decrease both days was about 0·25, and the reading was 29·21 on the evening of the 25th; it increased 0·5 on the 26th, and still further increased 0·25 on the 27th. The reading at this time was 29·91 at midnight. On the 28th it decreased rapidly, and was 29·20 at midnight, being the lowest in the month. The range within this month was 2·52 inches.

On the 2nd of March the reading passed the point 30 inches, and on the 6th the highest reading in the month took place, *viz.* 30·48; after this the changes were small till the 26th, the reading being above its average value. On the 27th the reading decreased half an inch, and on the 28th the lowest reading took place in the month, 29·18, and it remained low till the end of the month.

The reading of the barometer on February 11 at Aylesbury was 30·369; at Leicester was 30·800; at Durham was 30·440; at Whitehaven 30·62; at Newcastle was 30·764; at Exeter was 30·838; at Liverpool was 30·861; at Truro was 30·74; at Norwich was 30·910; and at Cardington was 30·846.

The monthly mean values of the several subjects of investigation are shown in the Registrar-General's report.

The observations have been corrected for diurnal ranges, and the results are all comparable with each other.

The observer at Southampton has kindly furnished me with the following agricultural report for Hampshire, the particulars having been supplied by John Clark, Esq., of Finsbury Farm, near Romsey.

"The weather during the quarter has been most propitious for cropping. The fine dry March, followed by the gentle showers of April, have benefited to a great degree both the soil and cattle. Sowing is in a forward state, and young wheat looks well.

"The lambing season is over, and it is believed will prove to be an average. Some strange anomalies have been prevalent. On adjacent farms no loss has been experienced in one, whilst the loss both of ewes and lambs have been great in the other."

The mean of the numbers in the first column of the subjoined table is 29·837 inches, and this value may be considered as the pressure of dry air for England during the quarter ending March 31, 1849. The differences between this number and the separate results contained in the first column show the probable sums of the errors of observation and reduction; the latter arising partly from erroneously assumed altitudes, and partly from the index errors of the instruments not having been determined. In most cases the sums of these errors are small.

The mean of the numbers in the second column, for Guernsey and those places situated in the counties of Cornwall and Devonshire, is 45°·2; for those places situated south of latitude

Meteorological Table for the Quarter ending March 31, 1849.

Names of the places.	Mean pressure of the atmosphere of dry air reduced to the level of the sea.	Mean temperature of the air.	Highest reading of the thermometer.	Lowest reading of the thermometer.	Mean daily range of temperature.	Mean monthly range.	Range of temperature in the quarter.	Mean temperature of the dew-point.	Wind. Mean estimated strength.	Wind. General direction.	Mean amount of cloud.	Rain. Number of days on which it fell.	Rain. Amount collected.	Mean weight of vapour in a cubic foot of air.	Mean additional weight of vapour required to saturate a cubic foot of air.	Mean degree of humidity.	Mean whole amount of water in a vertical column of atmosphere.	Mean weight of a cubic foot of air.	Height of cistern of the barometer above the level of the sea.
	in.	°	°	°	°	°	°	°					in.	grs.	gr.			grs.	feet.
Guernsey	29·937	45·6	58·0	29·5	8·2	20·0	28·5	40·1	......	w. & n.	......	41	7·2	3·1	0·5	0·854	3·7	549	123
Helston	29·863	45·7	58·0	30·0	10·5	24·3	28·0	43·0	1·6	s.w.	6·7	48	7·7	3·4	0·4	0·926	4·1	545	106
Falmouth	......	46·0	60·0	31·0	11·0	25·3	29·0	......	1·4	w.s.w.	7·3	54	8·6						
Truro	......	44·7	55·0	33·0	9·1	19·7	22·0	......	0·7	w.	7·6	51	8·0						
Exeter	29·868	43·9	58·0	25·0	13·2	29·8	33·0	40·2	1·7	w.	5·9	39	4·6	3·1	0·5	0·854	3·7	545	140
Chichester	......	41·0	59·0	26·0	10·2	26·7	33·0	......		s.w.	......	......	5·6	......					
Uckfield	29·875	41·2	61·0	21·0	13·0	33·3	40·0	35·4	......	w.	......	39	5·3	2·6	0·4	0·783	3·1	551	180
Southampton	29·814	42·8	61·6	27·5	11·2	30·1	35·1	38·4	0·6	w.	7·3	36	5·9	2·9	0·4	0·869	3·5	549	55
Beckington	29·934	40·3	61·0	18·0	12·9	33·7	43·0	35·9	1·1	w.	6·5	37	4·8	2·7	0·4	0·865	3·2	542	265
Maidenstone Hill, Greenwich	29·885	41·7	57·0	20·6	9·6	30·3	36·5	38·1	......	s.w.	7·4	34	4·4	2·9	0·6	0·883	3·4	551	107
Royal Observatory, Greenwich	29·888	42·0	60·0	19·9	12·5	34·4	40·1	37·1	......	s.w.	7·5	35	4·2	2·8	0·5	0·836	3·3	549	159
Lewisham	......	42·0	60·0	20·0	13·2	34·0	40·0	38·2	......	......	......	......	......	2·9	0·4	0·876	3·4	......	40
St. John's Wood	......	42·2	58·0	21·0	13·2	32·3	37·0	38·7	2·8	s.w.	7·3	38	5·3	2·9	......	0·875	3·5	548	
Walworth	......	41·5	61·5	22·0	13·3	33·8	39·5	40·6	1·5	......	8·3	41	5·5	......	......	......	3·5	545	32
Latimer Rectory	29·868	40·7	63·5	19·5	13·4	34·7	44·0	37·3	1·6	......	7·9	36	......	2·8	0·3	0·895	3·4	543	335
Aylesbury	29·735	40·9	63·0	19·0	13·8	35·7	44·0	36·4	0·7	s. & w.	7·0	31	4·9	2·7	0·5	0·849	3·2	546	280
Hartwell House	29·841	41·1	63·7	12·0	15·8	39·9	51·7	37·7	0·5	s.s.w.	7·3	46	......	2·9	0·4	0·885	3·4	546	260
Saffron Walden	......	......	59·0	10·0	11·3	37·0	49·0	......	2·5	n.w.	5·7	32	3·4						
Oxford	......	41·4	......	......	......	......	......	......	1·9	s.w.	7·8	24	3·4	2·9	0·4	0·876	3·4	......	250
Hereford	......	41·5	......	......	......	......	......	......		s.w.		20	4·9						
Cardington	29·815	40·8	57·0	16·8	12·5	33·9	40·2	37·5	......	w.s.w.	7·1	39	4·2	2·8	0·4	0·883	3·4	550	70
Norwich	29·797	40·9	59·0	20·0	11·5	32·0	39·0	36·3	1·0	w.s.w.	......	37	4·1	2·7	0·6	0·847	3·2	551	39
Holkham	29·718	40·3	57·5	16·2	11·5	31·6	41·3	36·3	1·0	s.w.	7·0	37	3·3	2·8	0·4	0·866	3·2	550	31
Leicester	29·904	40·2	62·0	17·0	12·5	35·7	45·0	36·1	1·8	w.	6·1	30	4·8	2·7	0·5	0·842	3·2	550	156
Derby	29·831	40·3	58·0	20·0	12·2	34·7	38·0	37·1	2·7	w.	......	39	4·3	2·8	0·4	0·864	3·3	548	39
Highfield House, Notts	29·863	40·3	65·0	20·5	9·5	36·5	44·5	36·9	0·9	s.w.	7·2	38	4·0	2·8	0·3	0·724	3·3	549	103
Liverpool Observatory	29·830	41·9	55·5	23·8	7·0	25·9	31·7	35·3	1·6	n.w.	6·8	45	3·9	2·6	0·6	0·834	3·1	542	37
Leeds	29·908	39·8	59·0	11·0	11·6	35·0	48·0	36·8		n.w.	6·2	42	8·2	2·7	0·4	0·889	3·3	541	148
Wakefield Prison	29·826	40·2	63·0	14·5	11·3	36·8	48·5	37·2	......	w.	......	46	3·7	2·8	0·5	0·865	3·3	550	113
Stonyhurst Observatory	29·830	39·7	54·8	20·1	10·1	28·8	34·7	36·1	1·4	w.s.w.	7·7	60	11·5	2·7	0·4	0·875	3·2	547	381
York	29·778	39·3	59·0	18·0	12·1	33·0	41·0	40·9	......	w.	......	34	3·2	3·2	0·5	0·861	3·8	546	50
Whitehaven	......	40·5	54·0	18·7	6·3	26·3	35·3	37·9	2·5	s.w.	......	45	8·5	2·9	0·2	0·940	3·4	549	
Durham	29·752	40·0	56·0	15·0	10·5	33·9	41·0	35·5	1·9	s.w.	5·7	25	1·5	2·7	0·5	0·848	3·1	544	340
Newcastle	29·718	41·0	56·5	19·0	12·4	32·0	37·5	37·5	......	w.s.w.	......	30	5·3	2·8	0·4	0·872	3·3	547	121
Number of columns	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19

of 52°, including Chichester and Hartwell, is 41°·4; for those places situated between the latitudes of 52° and 53°, including Saffron Walden and Leicester, is 40°·7; for those places situated between the latitudes of 53° and 54°, including Derby and York, is 40°·2; and for Whitehaven, Durham and Newcastle is 40°·5. These values may be considered as those of the mean temperatures of the air for those parallels of latitude during the quarter ending March 31, 1849.

The average daily range of temperature in Cornwall and Devonshire was 10°·4; at Liverpool and Whitehaven was 6°·7; south of latitude 52° was 12°·6; between the latitudes of 52° and 54° was 11°·5; and at Durham and Newcastle was 11°·5.

The greatest mean daily ranges of the temperature of the air took place at Hartwell, Aylesbury, and Latimer; in fact, in and near the vale of Aylesbury; and the least occurred at Whitehaven, Liverpool and Guernsey.

The highest thermometer readings during the quarter were 65° at Highfield House, 63°·7 at Hartwell, and 63°·5 at Latimer. The lowest thermometer readings were 10°·0 at Saffron Walden, 11°·0 at Leeds, and 12°·0 at Hartwell. The extreme range of temperature of the air during the quarter in England was therefore about 55°, most likely somewhat less than this value.

The average quarterly range of the reading of the thermometer in Cornwall and Devonshire was 28°·1; at Liverpool and Whitehaven was 33°·5; south of latitude 52° was 40°·3; and north of 52° was 42°·1.

The mean temperature of the dew-point in Cornwall and Devonshire was 41°·1; south of latitude 52° was 37°·6; between the latitudes of 52° and 53° was 36°·5, and north of 53° was 37°·2.

The direction of the wind has been mostly south-west; at some few places it seems to have prevailed for some time from the north-west.

From the numbers in the tenth column the distribution of clouds has been such as to cover about three-fifths of the whole sky.

Rain has fallen on the greatest number of days at Wakefield, Falmouth, Truro and Helston. The average number at these places was 53. It fell on the least number of days at Oxford, Saffron Walden, Durham and Leicester, and the average number at these places was 35. The stations at which the largest falls have taken place were Stonyhurst, Falmouth, Whitehaven and Leeds. The falls were smallest in amount at Durham particularly, York, Holkham and Oxford. The average fall in the counties of Cornwall and Devonshire was 7·2 inches; south of latitude 52° was 5·1 inches; between latitudes 52° and 53° was 4 inches; and south of 53°, omitting Stonyhurst, was 4·3 inches.

The smallness of the fall at Durham is remarkable; between January 31 and March 28 only 0·14 inch fell.

The numbers in column 14 to 18 show the mean values of the hygrometrical results at every station; from which we find that—

The mean weight of vapour in a cubic foot of air for all places (excepting Cornwall and Devonshire) in the quarter ending March 31, 1849, was 2·8 grains.

The mean additional weight required to saturate a cubic foot of air in the quarter ending March 31, 1849, was 0·4 grain.

The mean degree of humidity (complete saturation = 1) in the quarter ending March 31, 1849, was 0·860.

The mean amount of vapour mixed with the air would have produced water, if all had been precipitated at one time on the surface of the earth, to the depth of 3·3 inches.

The mean weight of a cubic foot of air at the mean height of 160 feet under the mean pressure, temperature and humidity, was 547 grains.

And these values for Cornwall and Devonshire were 3·2 grains; 0·5 grain; 0·878; 3·8 inches; 547 grains, at the mean height of 120 feet.

Errata.—In the formula for calculating the pressure of dry air, in the last Number of the Magazine, *for* + *read* —; and *for* 82 inches *read* 820 feet.

For the formula for calculating the weight of a cubic foot of air, substitute the following:

$$541 \text{ grains} - \left(\frac{\text{height of place in feet above the level of the sea}}{820 \text{ feet}} \times 18\right).$$

LIV. *On the Determination of the Coefficients in any series of Sines and Cosines of Multiples of a variable angle from particular values of that series. By the* Rev. BRICE BRONWIN*.

MY last paper in this Journal having been terminated rather in haste, I did not observe that the step contained in (16.) might be repeated. Thus

$$\cos(n-1)\frac{i\pi}{n} = \pm\cos\frac{i\pi}{n}, \qquad \cos(n-3)\frac{i\pi}{n} = \pm\cos\frac{3i\pi}{n}, \text{ \&c.};$$

$$\sin(n-1)\frac{i\pi}{n} = \mp\sin\frac{i\pi}{n}, \qquad \sin(n-3)\frac{i\pi}{n} = \mp\sin\frac{3i\pi}{n}, \text{ \&c.}$$

Therefore if we make

$$u_1 \pm u_{\frac{n}{2}} = w_1, \quad u_2 \pm u_{\frac{n}{2}-1} = w_2, \text{ \&c.};$$

$$v_1 \mp v_{\frac{n}{2}} = x_1, \quad v_2 \mp v_{\frac{n}{2}-1} = x_2, \text{ \&c.};$$

* Communicated by the Author.

we may replace the values of B_i and A_i in (16.) by

$$\left.\begin{aligned} B_i &= \frac{2}{n}\left\{w_1 \cos\frac{i\pi}{n} + w_2 \cos\frac{3i\pi}{n} + \ldots\right\} \\ A_i &= \frac{2}{n}\left\{x_1 \sin\frac{i\pi}{n} + x_2 \sin\frac{3i\pi}{n} + \ldots\right\} \end{aligned}\right\} \quad . \quad . \quad . \quad (17.)$$

Then if the particular values of $f\left(\frac{(2i'+1)\pi}{n}\right)$ be given, or tabulated, we may with very little trouble form tables of u_1, u_2, &c.; v_1, v_2, &c.; and from these last tables of w_1, w_2, &c.; x_1, x_2, &c. Thus the labour of computation required by (15.) would be reduced to one half of the same in (16.), and to one fourth of it in (17.).

We may reduce (14.) once in a similar manner, omitting the last terms in the values of B_i and A_i, which will be $\frac{2}{n}f(2\pi)$ and 0 respectively; but this can be done only once. It appears, therefore, that (15.) is preferable to (14.) when we have tables of the particular values.

If we employ a smaller value of n, we shall have for certain values of i

$$\sin\frac{ik}{2} = 0, \quad \sin(p-i)\frac{k}{2} = 0, \quad \sin(p+i)\frac{k}{2} = 0;$$

and we should find

$$B_0 + B_n + B_{2n} + \ldots\ldots$$

$$B_i + B_{n-i} + B_{n+i} + B_{2n-i} + \ldots\ldots$$

$$A_i - A_{n-i} + A_{n+i} - A_{2n-i} + \ldots\ldots$$

in the place of B_0, B_i, and A_i respectively. These might be employed to determine some of the quantities B_i and A_i, when a part of them has been found by the method before given.

Gunthwaite Hall, April 9, 1849.

LV. *On some Combinations of Boracic Acid with Oxide of Lead.* *By* THORNTON J. HERAPATH, *Esq.**

NEUTRAL Borate of Lead, $PbO + BO^3$, may be obtained by digesting the heavy white precipitate which is formed when biborate of soda is added to a solution of any neutral salt of lead, for twelve or fourteen hours, in a strong solution

* Read before the Bristol Philosophical and Literary Society, April 19, 1841, and now communicated by the Author.

of caustic ammonia (I., IV.). It would also appear to be produced when the basic acetate of lead is imperfectly precipitated by biborate of soda (II., V.), or when an acid solution of either of the subsequently described borates is supersaturated by strong *liquor ammoniæ* (III., VI.).

It is a heavy, white, amorphous powder, which is almost insoluble in water, both hot and cold. It is perfectly insoluble in alcohol. It dissolves with great facility in dilute nitric acid, even when cold, and likewise in boiling acetic acid; from these solutions it may be again precipitated unaltered, by adding a large excess of ammonia. It is easily decomposed by sulphuric and hydrochloric acids, and likewise by a boiling solution of caustic potash or soda. Before the blowpipe, it intumesces, gives off water, and becomes dark in colour; and at a low red heat fuses into a clear colourless glass, which possesses a specific gravity of 5·5984, at 56° F., and is softer than common flint-glass. Heated to redness on charcoal, it is partially reduced, and the fused mass contains numerous globules of metallic lead.

The following are the results of my analysis of the hydrated salt, after it had been exposed to a temperature of about 212° F., in a Liebig's drying-tube, for three or four hours.

I. 9·2 grains were heated to redness in a platina capsule; loss =0·598 gr.=6·5000 per cent.

II. 10 grs., treated as before, lost 0·683 gr. in weight =6·8300 per cent.

III. 11·01 grs. lost 0·684 gr. in weight=6·2125 per cent.

IV. 10 grs. were dissolved in dilute nitric acid, and the solution precipitated by an excess of diluted sulphuric acid; PbO, SO^3 9·77 grs.=PbO 7·198 grs.=71·9800 per cent.

V. 20 grs. gave of PbO, SO^3 19·80 grs.=PbO 14·578 grs. =72·8900 per cent.

VI. 20 grs. gave of PbO, SO^3 19·03 grs.=PbO 14·0221 grs.=70·1105 per cent.

	I.	II.	III.	IV.	V.	VI.	Mean.
HO	6·5000	6·8300	6·2125	...	...	...	6·5141
PbO	...	...	...	71·9800	72·8900	70·1105	71·6601
BO^3	...	...	...	...	...	...	21·8258

Now, if we consider the composition of this hydrated salt to be represented by the formula PbO, BO^3+HO, it ought to contain—

Water	1	9 or	5·7692 per cent.
Oxide of lead . .	1	112 ...	71·7939 ...
Boracic acid . .	1	35 ...	22·4369 ...

This salt begins to lose water between 250° and 300° F.; and by long-continued desiccation at a temperature of from 450°

to 500° F., it may be rendered perfectly anhydrous without experiencing any perceptible change of colour.

Sesquiborate of Lead, $2PbO+3BO^3$.—The salt which is produced when a boiling solution of the nitrate, or any other soluble salt of lead is precipitated by a great excess of biborate of soda, has been hitherto considered by chemists to be composed of $PbO+2BO^3$. According to my experiments, however, it would appear to consist of $2PbO+3BO^3$. It is a white powder, like the preceding, which it closely resembles in its properties. Before the blowpipe it fuses into a colourless glass, the specific gravity of which is rather lower than that of the neutral borate, being 5·2352; its hardness is very nearly equal to that of flint-glass. The following are the results of my analysis of this salt dried at 212° F.:—

I. 10 grs., heated to redness, lost 0·918 gr. in weight =9·1800 per cent.

II. 11·26 grs., treated as before, lost 1·106 gr. =9·8223 per cent.

III. 25 grs. lost 2·3 grs. in weight=9·2000 per cent.

IV. 10 grs. gave of PbO, SO^3 8·62 grs.=PbO 6·3515 =63·5150 per cent.

V. 10 grs. gave of PbO, SO^3 8·34 grs.=PbO 6·1452 =61·4520 per cent.

VI. 10 grs. gave of PbO, SO^3 8·411 grs.=PbO 6·1975 =61·9750 per cent.

	I.	II.	III.	IV.	V.	VI.	Mean.
HO .	9·1800	9·8223	9·2000	...	...	...	9·4007
PbO .	...	...	...	63·5150	61·4520	61·9750	62·3140
BO^3 .	...	...	...	...	...	...	28·2853

These numbers indicate a composition very closely approximating to the formula $2PbO, 3BO^3+4HO$, as will be seen upon comparing them with those given below:—

Water	4	36	or 9·8630	per cent.
Oxide of lead . .	2	224	... 61·3690	...
Boracic acid . . .	3	105	... 28·7680	...

Dried between 350° and 400° F., it loses two of its atoms of water, and its composition is now expressed by the formula $2PbO, 3BO^3+2HO$.

I. 10 grs. of the salt in this state of hydration lost 0·495 gr. upon being heated to redness, =4·9500 per cent.; calculation requires 5·1873 per cent.

Biborate of Lead, $PbO+2BO^3$, may be easily obtained by boiling either of the preceding recently-precipitated salts, whilst still moist, in a concentrated solution of boracic acid. It is a light amorphous powder, which at a red heat fuses with difficulty into a vitreous mass. From the almost impossibility, however, of obtaining this glass free from air-bubbles, I was unable to ascertain its true specific gravity. It was slightly superior to flint-glass in hardness.

The hydrated salt, dried for some time at 212° F., yielded upon analysis the following numbers:—

I. 10 grs., when heated to redness, lost 1·579 grs. in weight =15·790 per cent.

II. 25 grs., treated as before, lost 4·021 grs. in weight =16·084 per cent.

III. 10 grs. gave of PbO, SO^3 7·07 grs.=PbO 5·2095 =52·095 per cent.

IV. 25 grs. gave of PbO, SO^3 17·784 grs.=PbO 13·1040 =52·416 per cent.

	I.	II.	III.	IV.	Mean.
Water . . .	15·790	16·084	...	...	15·9370
Oxide of lead .	...	...	52·095	52·416	52·2555
Boracic acid .	...	...	...	...	31·8075

Now, supposing its composition to be expressed by the formula PbO, $2BO^3+4HO$, it ought to be composed of—

Water	4	36 or	16·513 per cent.
Oxide of lead . . .	1	112 ...	52·376 ...
Boracic acid . . .	2	70 ...	32·111 ...

Dried between 400° and 450° F., it contains 4·435 per cent. of water=one atom; calculation requires 4·712 per cent.

Nitro-Borate of Lead.—When either of the above-described borates of lead are dissolved in moderately strong nitric acid to saturation, the solution filtered and concentrated by evaporation until a pellicle appears upon the surface, and then allowed to cool, the sides of the vessel containing the solution in a short time become covered with numerous irregular, glistening crystals. These, when heated to somewhat above 250° F., become nearly opake, slightly decrepitate, and give off water and traces of nitric acid vapour. Heated to redness, they evolve large quantities of nitrous acid fumes, and the residue fuses into a transparent colourless glass. They are, therefore, obviously a nitro-borate of lead; but from the discordant results of my analyses, I have as yet been unable to satisfy myself with regard to their true composition. They are most probably composed of PbO, BO^3, +PbO, NO^5 +HO.

Chloro-Borate of Lead.—This curious and interesting double salt was formed accidentally whilst attempting to prepare a borate of lead by precipitating a hot solution of biborate of soda by a boiling concentrated solution of chloride of lead. By filtering the mixed solutions whilst still warm, and washing the white flocculent precipitate which remained upon the filter with lukewarm water, the new salt was obtained in a state of purity.

This, when examined under a microscope of high power, was found to consist of exceedingly minute irregularly-acicular crystals, which depolarized light and possessed a nacreous lustre.

The compound thus obtained does not appear to be acted upon by cold water; boiling water, however, slowly but gradually decomposes it into its constituent salts. It is perfectly insoluble in alcohol. It dissolves with facility in hot dilute nitric acid, being decomposed, and chloride of lead set free, which, upon cooling, separates from the solution in long needle-formed crystals. When heated to from 250° to 300° F., it loses about 3·59 per cent. of water, and then becomes anhydrous. At a low red heat it readily fuses into a clear amber-coloured globule; this, upon cooling, solidifies into a transparent and almost colourless glass, which is slightly opalescent. When heated to redness, however, on charcoal, or in an open platina capsule, it behaves differently; white fumes are now given off, and the fused mass becomes gradually darker in colour, and of a thicker consistence, until it very much resembles melted sulphur in appearance. If it be now allowed to cool, it will be found to have undergone a very considerable change; it rapidly concretes into an opake, straw-coloured brittle mass, which is made up of a multitude of long, radiating, acicular crystals, and bears a striking resemblance to molybdic acid.

The crystallized hydrated salt, dried by exposure to sulphuric acid at the ordinary temperature, yielded upon analysis the following numbers:—

I. 4·42 grs. were taken, and heated to redness in a tube of Bohemian glass; the aqueous vapour having been drawn off by suction, and the apparatus allowed to cool, it was found to have lost 0·180 gr. in weight=4·072 per cent.

II. 3·86 grs., treated as before, lost 0·12 gr. in weight =3·109 per cent.

III. 5·00 grs. were dissolved in boiling dilute nitric acid, and the solution was precipitated by nitrate of silver; Ag, Cl, (fused)=2·60 grs. in weight=0·6411 Cl=12·822 per cent.

IV. 3·32 grs., treated as above, gave of Ag Cl 1·82 grs. =Cl 0·4487 gr.=13·515 per cent.

V. 4·581 grs. gave of PbO, SO^3 4·824 grs.=Pb 3·3009 =72·0580 per cent.

VI. 3·6 grs. gave of PbO, SO^3 3·833 grs.=Pb 2·6226 grs. =72·8510 per cent.

	I.	II.	III.	IV.	V.	VI.	
Water	4·072	3·109	...	...	...	...	3·5905
Chlorine	...	...	12·822	13·515	...	...	13·1685
Lead	...	...	...	...	72·058	72·851	72·4545
Oxygen } Boracic acid }	...	...	...	...	...	...	10·7865

The formula that agrees best with these numbers is PbO, BO^3, + Pb, Cl + HO; supposing this to be its composition, it ought to contain—

Water	1	9	or 3·040	per cent.
Chlorine	1	36	... 12·162	...
Lead	2	208	... 70·030	...
Oxygen	1	8	... 2·703	...
Boracic acid	1	35	... 12·065	...

The excess of chlorine and lead shown by the analysis was doubtlessly owing to the difficulty of entirely removing the excess of chloride of lead, which was carried down by the salt, without producing a decomposition of the salt itself.

All subsequent attempts to reproduce this compound having failed, I have been unable to verify the above results by a repetition of my analysis.

Mansion House, Old Park,
Bristol, March 4, 1849.

LVI. *Notices respecting New Books.*

Statistics of Coal. The Geographical and Geological distribution of Fossil Fuel or Mineral Combustibles employed in the Arts and Manufactures: their production, consumption, commercial distribution, prices, duties, and international regulations, in all parts of the world; including four hundred statistical tables and eleven hundred analyses of mineral bituminous substances. With incidental statements of the statistics of Iron Manufactures, &c., derived from official reports and accredited authorities. Illustrated with Coloured Maps and Diagrams. By Richard Cowling Taylor, *F.G.S.* London: John Chapman, 142 Strand. Philadelphia: J. W. Moore. 1848.

COMPREHENSIVE as the title of this work appears, it does not, yet, convey a just idea of its scope, or of the extent of its subject-matter. Did its title stand, "Coal, the civilizer; its natural history, production and applications," it would perhaps convey to the casual reader a more just idea of the object and contents of the work. We confess that we ourselves closed the book with very different feelings from those with which we opened it. We have no hesitation in saying that we have long ceased to entertain that extraordinary respect for mere statistics which it has been very much the habit of late years to inculcate. We have seen too many instances, and too many instances are daily occurring, in which statistics are made the mere instrument of the partizan and the theorist.

He must have been very unobservant of public events of late years who is not aware that statistics may be made use of with equal confidence to support any side of any question. Hence the cautious inquirer who really desires to get at some actual and permanent result, will always look with extreme suspicion upon every thing that comes before him with an ostentatious parade of statistics, aware that there is nothing so easily abused, nothing which is more liable to abuse.

But a perusal of these pages has shown us that mere statistics form but a very subordinate part of the design of the author. A long and intimate practical acquaintance with mines and mining operations in different parts of the world had necessarily led him to amass a great quantity of materials; the value of which, as a constant object of reference for his own use, led him to feel the utility of a digested and methodized arrangement of those materials, in a permanent shape, for the use of others. But there is found throughout these pages a pervading spirit beyond that merely materialistic and dry one which the title would indicate, and which the professional engagements of the author might have led us to anticipate. We perceive impressed on every section the idea, *not* of coal the mere *wealth producer*, the mere material instrument of the human animal, but of coal as an important agent in promoting civilization. "We take it for granted," are the first words of the introduction, "that every one who may chance to peruse the summary of statistics of mineral fuel which we have embodied in the present section will be impressed with the immense importance of those substances, particularly as developed of late years; how vastly enlarged the area and bulk of their production in all countries; how essential they now are to the comfort of the human family; how much they have done towards the extension of the useful arts; how gloriously they have aided the progress of invention and improvement; how mighty are the results which have followed their increased application." (P. xiii.) And again in p. lxxxiv. the author justly says: "Respecting the wondrous influence which the employment of mineral combustibles has had, even in our own days, upon the whole world, by the acquisition of new forces; by the extension of mechanical powers, of manufacturing capabilities; by the impulse given to the industrial arts and the creation of new sources of wealth; by rapid and cheap modes of transportation and enlarged commercial facilities; *above all*, by the improved condition of the people, we will not here dilate. Abundant evidence of all these will be found in this volume." It is in the same spirit, and imbued with the same everywhere pervading high moral sentiment, that the author more than once (pp. xiv. and xxxviii) calls attention to the vastly greater importance of iron than of gold and silver,—a truth which it is not by any means beside the mark to touch upon in these days of California-mania. Adam Smith long ago remarked that the adventurer in a silver mine ran every probability of being ruined, but that the adventurer in a gold mine was certain of being ruined. It will not be amiss to put, beside such an authority, the following passage from the work before us:—

"It would be no difficult task to show in figures how vastly more profitable is the application of labour in the mining and working and transporting of coal than in that of the precious metals. The annual production of all the gold and silver mines of North and South America was estimated by Baron Humboldt at 9,243,000*l.*, and at present at less than 5,000,000*l.* Now the value of the coal produced annually, in Great Britain alone, is computed at near 10,000,000*l.* at the pit's mouth, and at from 15,000,000*l.* to 20,000,000*l.* sterling at the places of consumption. At the same time, the value of the iron brought into a manufactured state through the agency of this coal is 17,000,000*l.* more. We shall enter more particularly into this subject in a future page. We cannot but mark also the superior character and condition of the inhabitants of the coal-producing and consuming countries, such as those of the northern hemisphere, especially since the introduction of steam-power, to that of the people of the southern and tropical latitudes, to whom coal has either been wholly denied or is not applied to any use. The industry, activity, moral culture, and intelligence concentrated around any of the great depositories of coal and iron in the temperate regions, have no parallel in the countries from which such treasures have been withheld." (P. xiv).

And it is not only in these respects that the author departs from a mere dry statistical detail. He justly considers every matter connected with the history of the formation of coal, and with its most important applications, to be necessary parts of the information which will be desired by those who would thoroughly understand the subject. Thus we find him including the iron manufacture, and the extent and application of railroads and steam-vessels, as parts of his subject; "so closely," he justly observes, "do all these matters seem interwoven with each other." But, beyond this, a very large amount of valuable and very interesting information is communicated on the methods of working mines; on the casualties to which mines and miners are liable; and on the various means which have been adopted in various countries for the mitigation of these casualties, and for the promotion of the healthiness and security of this occupation. Benefit societies and provident institutions, as they exist in mining districts, claim a large share of the author's evidently cordial and sympathetic attention.

A very interesting and comprehensive sketch is further given of fossil botany and of the organic remains found among the coal measures. The author's observations on the interest of this branch of his subject seem to us so just and pertinent that we transcribe some of them.

"In intimate connection with the matter of the present volume, a knowledge of the forms, the botanical classification, the geological arrangement, of the vegetable remains of our ancient world seems to be almost indispensable. It embraces facts, at least, sufficiently valuable to ensure for it, as a collateral branch of natural science, a conspicuous section of this book. Independently of its usefulness, there is a never-failing interest attached to such an investigation which enables us to trace the history, as it were, of the past condition, the present adaptation of the primæval flora; that magnificent vegetation which amidst the mutations of our planet yet survives for our use; its character changed, it is true, but only to become more serviceable to man."

"A happy provision was it that secured for the ultimate advantage of the human race, ages before its appearance upon the globe, the trees of gigantic size, the densely growing shrubs, the most delicate even of the lesser plants—that flora which covered in such profusion the islands and plateaux, and filled the humid valleys, of the early world. A happy provision was it that, amidst the early catastrophes of the earth, those convulsions which modified its entire surface, overwhelmed its primæval forests, and buried them beneath enormous accumulations of earthy debris, of sediment, and of rocky debacle,—still perpetuated and matured, during the lapse of countless ages, that primitive vegetation which finally, in the form of mineral combustibles, we are now busy in exploring, mining, and appropriating in a thousand ways and for a thousand purposes. A happy provision was that,—a beneficent one surely,—by which, at the moment when man is compelled to level the existing forests to make room for the progress of agriculture and the cultivation of the present surface, he finds nigh at hand, yet buried beneath that surface within the shallow basins and woody islands of the antediluvian world, those inexhaustible stores of a combustible now rendered infinitely more precious and effective than that existing vegetable fuel whose destruction is the inevitable consequence of advancing civilization." (P. lxxxiv.)

It must be very obvious that the labour of collecting the materials for a volume so wide in its scope, so multifarious in the subject-matters of which it treats, must have been very great, and the task one of great difficulty. "The information required was not accessible in any single work, nor even in a number of works: it was nowhere to be found." In a work like this it is no slight matter to have gathered together, in an accessible shape, certain and definite information which may be used as a key by other inquirers in individual regions or departments; which may have something of the character of a complete skeleton by which the general relations of the various parts of the subject may be seen. That imperfections should be found to exist in the work when any single part is submitted to a rigid test, is a necessary result from the very nature of the subject, in which new facts are hourly arising and being recorded. But the value of this, as a general work of reference, will not thereby be lessened.

There is one cause of the difficulty with which our author has had to grapple on which it is not uninteresting to remark. This is the circumstance that, while there are "official" returns and other documents in all the continental countries of Europe, none such exist in the two by far most important coal-producing countries in the world, namely Great Britain and the United States of America. Our author several times calls attention to this circumstance, as having occasioned him much difficulty. Though, however, he sometimes does intimate something like a wish that we had, in Great Britain, a "corps of state engineers," he does not, like many theorists, allow this personal inconvenience to himself to blind him to the true circumstances out of which the want of such "official" returns arises. He is awake to the fact that such "official" completeness is only to be obtained at the price of the sacrifice of national liberty and individual independence. He candidly admits that such a "process must, at all times, be unpopular, and the results extremely uncertain. This species of investigation savours too much of scrutiny into the private concerns of men." (P. xl.) The volume before us supplies additional illustrations to the numberless ones which every honest inquirer will find, of the importance to the prosperity of any country, and of any branch of industry, that the latter should be unshackled by the meddling interference of government officials. It is a heavy price to pay for the merely superficial, but never really reliable, result of regularly published official returns, that enterprise should be checked, individual energy cramped, self-dependence prohibited; and that two or three revolutions in a century should be necessary to keep the state from the anarchy of despotism. There is too much tendency in England at this time to follow in that centralizing path which has brought so much suffering on the continent. The specious pretences of schemers and theorists have already succeeded too far in their attempts at this official interference. We are quite content to have it still said that "as there is no system of supervision adopted in the mining regions here, as in all the other countries of Europe, it is impossible to arrive at any exact account of the quantity of coal which is annually raised in the mines." (P. 259.) It is far more satisfactory to us than the most perfect returns could be to find it stated that "what the wise direction of public authority has established in Germany, the spirit of association, the sentiment of individual independence, the habit of calculation and of observation, have consecrated in Great Britain." (P. cxxvi.) And, while we have every reason to be grateful for "the bounteous supply of mineral wealth which nature has assigned to England," we are infinitely more grateful for the spirit of independence which has resisted that despotic and pernicious "system of supervision" which has elsewhere prevailed; for that "enterprising character of her people, who have *turned* that supply of mineral wealth *to such good account*." (P. 257.)

Mr. Taylor, very properly, does not confine himself to that description of mineral which is commonly called COAL. He includes full and valuable information on the *Lignites* of the geological formations above the carboniferous group, and also on the recent *Peat*. It is clear that, in strictness, all these may with exactly as much propriety be included under the term "Coal" as the substance itself which is commonly known by that name. The use of that substance is comparatively modern; but the word itself is an ancient Saxon one, and one common indeed to the dialects of the old northern languages. As our author dates from the other side of the Atlantic, it will not be out of place to illustrate this fact by a reference to an ancient Icelandic Saga in which we find this word used in a way which clearly shows that something very different was meant by it from that heavy mineral which we commonly now call "coal." In the Saga which relates the explorations of Thorfinn Karlsefni on the American coast, A.D. 1008, we find it related that, when his party were on that part of the coast now known as Rhode Island, they had some encounters with the natives. It is expressly told how these latter came, on one occasion, in their canoes in numbers "as many as if *coals* had been strewn upon the bay" (*svá mart sem* kolum *væri sat fyrir hópit*). What the sort of coals were which, as floating on the surface of the water, suggested this simile we will not undertake to say, but they were certainly much more likely to have been charcoal or peat than stone coal. The name remains however no less apt at this day than it was at that in its application to the substance, in each case of vegetable origin, which is used as fuel,—whether that substance be the peat, spongy and light, of yesterday's growth, or the prostrate giant trees, compressed and heavy, which grew and flourished and were embedded in ages of unknown remoteness. It is interesting to find that the following passage will properly include *coals* of every age, and of every growth, and of every shade of meaning of the word, ancient or modern :—

"Each stratum of coal is the product of a peculiar vegetation, frequently different from that which precedes and that which follows it,—vegetations which have given rise to the superior and inferior layers of coal. Each stratum resulting in this manner from a distinct vegetation, is frequently characterized by the predominance of certain impressions of plants; and the miners, in numerous cases, distinguish the different strata which they remove by the practical knowledge they possess of the accompanying fossils. Any seam of coal and its overlying rock or slate should consequently contain the various parts of the living plants at the period of its formation: and, by carefully studying the association of these various fossils, which form so many special floras, containing generally but few species, we may hope to be able to reconstruct these anomalous forms of the ancient world." (P. xc.)

The value of this volume is greatly enhanced by a series of maps, in which the position and extent of all the ascertained coal-basins throughout the world are laid down.

It is certainly a remarkable spectacle to see the extraordinary increase of the production and consumption of mineral coal, and the changes which have been wrought in the habits of millions of human beings thereby. Our author tells us that "in Great Britain coal, according to some authorities, was mentioned as occurring in England as early as the ninth century, A.D. 853 [query, *stone* coal, or such coal as above alluded to in the Saga of Thorfinn Karlsefni]. It was certainly known and applied to various economical purposes in the middle of the twelfth century. In 1239, King Henry III. granted the privilege of digging coals to the good men of Newcastle. But it is little more than 250 years since coal came to be in general use, as fuel, in London. Upon its first introduction there, one or two ships were sufficient for the whole trade. At the present day there are several thousand ships constantly engaged in the transportation of that combustible." In 1845 upwards of *thirty-four millions of tons* of coal were produced in Great Britain (p. 259); and in the same year 11,987 ships' cargoes of coal were entered for duty at the *port of London* alone. As many as 282 cargoes,—amounting to upwards of 80,000 tons,—have been sold in the City of London Coal-market in one day (p. 263). And the iron manufacture has followed with no halting steps upon this amazing increase in the production of coal. It is not seventy years since William Hutton wrote his History of Birmingham. He alludes to an iron furnace in the neighbourhood of that town, and calculates the age of those iron-works from the mound of calx or cinder which lay near the refuse of the furnace. Reckoning by the ordinary rate of the increase to that mound, even at the accelerated ratio of his own day, and while in constant active work, he concludes that that very furnace must have been in active working there for at least *three thousand years*,—twelve centuries prior to the invasion of the Romans. And his calculation was probably within the mark instead of exceeding it*. But, within the part of one century which has passed since he wrote, many and many a mountain has grown up, to the disfigurement, alas! of many a fair and fertile plain, in many parts of England, any one of which throws that ancient mound at Aston, the growth of more than thirty centuries, altogether into the shade; and this, although, from the superior manner of working the ore, a given quantity would not leave nearly so much *refuse* as formerly. This curious calculation of Hutton's would have formed a striking introduction to Mr. Taylor's account of the progress of the iron manufacture. It appears from his work that the production of iron in that same district (the Staffordshire district) had increased from 13,210 tons in 1796, fifteen years after Hutton wrote, to 500,760 tons in 1846; the total produce of Great Britain in the same year being upwards of 2,000,000 of tons.

Another striking picture of the progress in the production of coal, with all its innumerable physical and moral consequences, may be drawn entirely from our author's pages. He is speaking of the anthracite beds of Pennsylvania. A quarter of a century ago, he tells us, a few tons of an unknown combustible were brought to Philadelphia from a wild and desert tract, known by the not unapt title of "the Wilderness of Saint Anthony."

"But the miner," proceeds our author, "has entered into this Wilderness of Saint Anthony, and canals have penetrated it, and railroads have traversed it; basin after basin of this combustible has been discovered in it; tract after tract has supplied productive collieries in it; until in a single year, 1847, it had furnished the surprising amount of 3,000,000 of tons, and 11,439 vessels cleared from

* See, further, a very interesting paper in the last Number of this Journal, by Mr. John Phillips, calling attention to the antiquity of metal works in England.

the single port of Philadelphia in that season, loaded with a million and a quarter of tons, for the service of the neighbouring states." (p. 20.) Further on, the author, in the spirit which we have already remarked as pervading the entire work, concludes another division of the same subject by the observation: "It is beyond the scope of human vision to contemplate, in our day, the results associated with these millions,—the industrial facilities, the wealth, and power, and influence at home and abroad, which they must inevitably confer upon the future inhabitants of the country." (P. 33.)

Another illustration of a more special nature, of a means of the consumption of coal accompanied by a vast amount of benefit and comfort to the community, is touched upon in this work, which we cannot omit to notice. "It was in 1803," says the author, "that Mr. Winsor first exhibited the effect of gas-light at the Lyceum Theatre, London." It appears that there were in London in 1845 nineteen gas companies, who produced, on an average, 10,000,000 cubic feet of gas every twenty-four hours, which is at the rate of 3,650,000,000 cubic feet a year. The whole number of lights is calculated at 100,000. In 1838 these London gas companies alone consumed 340,000 tons of coals annually.

It would be obviously impossible to direct attention to all the matters, however great their interest, which are discussed in this volume. We must content ourselves with referring to two more points, both of them of much importance, and both of them among those numerous ones which we have already said that the title of the volume will hardly lead the reader to expect to find discussed within it.

The comparative merits of anthracite and bituminous coal are very fully discussed in all their bearings, as to the application of each, both to domestic use and to manufacturing purposes. We are especially glad thus to see done "*justice to anthracite* in pointing out the incalculable value of a species of fuel previously rejected and despised, as amongst the most inferior and most impracticable of all the combustibles." (P. 365.) It cannot be denied that an absurd prejudice still exists in this country against the use of anthracite coal. Our countrymen seem in love with smoke. Two centuries ago "the famous city of London petitioned the parliament of England against two nuisances or offensive commodities which were likely to come into great use and esteem," one of which was "Newcastle coals, in regard of their stench," &c. (P. xl.) But the citizens seem to have now become in love with that same stench; and, while no effectual means have been taken, as they might be, to prevent the waste of material which takes place in the escape of smoke,—which is the mere result of imperfect combustion—the use of the smokeless anthracite encounters hopeless prejudices. Our author well remarks, that "the difficulty suggested about ignition, even were it found so in practice [which it is not in reality], is deprived of all weight, from the consideration that, with ordinary attention, a fire, when once kindled in the fall of the year, may be kept up until the return of summer if needed. The supposed tendency of anthracite to emit a greater amount of noxious vapours during combustion than bituminous coal, is contradicted by the daily experience of those who employ the former in their apartments, and is much less objectionable on that head than bituminous coal. Our tables of analyses at the end of this volume will, if doubts remain, decide this matter. In fact, they show that the anthracites contain less sulphur than the blazing coal." (P. 92.)

We can speak on this matter from our own experience, having ourselves used the anthracite coal for many years, both in open grates and in various other ways, and having always found it productive of far less trouble and of far greater comfort than any kind of bituminous coal. We cannot conceive the inducement by which we should be persuaded to return to the use of the smoke-giving system. The matter is one of importance in other respects in this country, besides those of present health, cleanliness, and comfort; for we are told by our author that "Great Britain possesses a far larger area of anthracite than exists in America or any other part of the world." (P. 92.)

Another matter, not unconnected in several respects with the last, is the incredible amount of waste which takes place annually in the coal after it has been actually fetched to the pit's mouth. It appears that nearly one-third part of the best coals are thus wasted at Newcastle,—amounting to more than a million of chaldrons of coal annually (p. 368). The author directs special attention to this subject in a section (p. 367) on "*prepared* fuel," in which he notices the fact of clay balls—coal dust and clay mixed—having long been in use in Wales and elsewhere, and being actually preferred to pure lump coal, where both are at hand. He gives a sketch of several attempts made to introduce the use of different compounds, by which at the same time the present enormous and quite needless annual waste would be saved, and a fuel provided as convenient, œconomical and useful as the best coal.

We cannot conclude without cordially recommending this work to the attention of our readers. While it will be an invaluable book of reference to every future inquirer into the numerous œconomic questions connected with our most important industrial operations and manufactures, and into the great social questions arising out of them, it will form an indispensable part of the library of every geologist.

LVII. *Intelligence and Miscellaneous Articles.*

SNOWY MOUNTAIN IN EASTERN AFRICA.

THE Rev. Mr. Rebmann, of the Church Missionary Society's East Africa Mission, has recently sent home an account of a journey made by him into the interior. At about 100 miles due west from Mombas, in 4° S. lat., he came to the foot of an elevated table-land, and saw before him a lofty mountain named Kilimandjáro, the summit of which is covered with perpetual snow. The elevation of this mountain can scarcely be less than about 20,000 feet, and from other sources we learn that it is crossed by the road to the country of Mono-Moezi. In the native languages of this part of Africa "Moezi" means "moon;" so that it is not unreasonable to conclude, as Dr. Beke does, that Mount Kilimandjáro forms a portion of the "Mountains of *the Moon*," in which Ptolemy places the sources of the Nile, and the snows of which he describes as being received into the lakes of that river.

It is by proceeding into the interior westwards from Mombas that Dr. Bialloblotzky, whose exploratory journey into Eastern Africa has on several occasions been noticed in this Journal, expects to reach the sources of the Nile; and the discovery of this snowy mountain and high table-land so near the coast argues favourably for the success of his undertaking. According to the last letters received from him, he arrived on January 3rd at Muscat, whither he had gone on by steamer from Aden and Maculla; and he was there looking for a native vessel to take him across to Mombas or Zanzibar.

ON MR. STRUVE'S MINE VENTILATOR. BY J. RICHARDSON, C.E.

This machine has now been three weeks in full operation at the Eaglesbush Colliery, near Neath, and the unequivocal success which has attended it is a matter of sincere congratulation, not only to the talented inventor, but to all engaged in mining. Its beautiful simplicity of design, its easy adaptation to the peculiar circumstances of any mine, and its certainty of effect, are its chief characteristics; whilst the comparatively small amount of capital required in its construction, and the slight annual expense incurred by it, are strong recommendations for its general adoption. It is well known, that in the best-managed collieries recourse is had to the furnace as a means of ventilation—not because it is perfect, but as the best system known. Without entering into a description of this mode, with which most of your readers are familiar, it will perhaps be sufficient to mention some of the most serious objections to it, and see how far they are remedied or avoided by this invention.

After describing the imperfections and evils of the modes of ventilation by furnaces, Mr. Richardson continues:—By Mr. Struve's machine all the advantages resulting from the use of the furnace are retained and augmented, additional benefits are secured, the evils complained of are removed, and are not replaced by others; at least such is the opinion of the writer, who devoted a day to the careful examination of it and its effects, both above and underground, and who is uninfluenced by any partiality arising from pecuniary interests or connexion with either the inventor or the proprietors of the colliery.

By referring to the annexed plans, section, and description of the machine now in operation at Eaglesbush, the reader will be able readily to understand the construction of the ventilator and the mode of its operation. The machine could have scarcely been tried under circumstances more unfavourable to its success than in this instance; for independent of the additional friction caused by drawing a large quantity of air through ways of little more than 11 feet area, owing to the men having been watered out of that part of the mine where the principal works have recently been carried on, it was found needful to change the direction of the air-ways, and, in consequence, it is at present conducted through temporary passages, which are ill-calculated for such a purpose, and which permit an immense leakage of the air into the waste parts of the colliery. The enlargement of the areas of the upcast shaft, which is now only 3 feet diameter, and of the air-ways, is now in progress, which, when completed, will materially add to the effective performance of the machine. The engine, too, is an old one, and has been injured by long exposure, is less than half the power necessary to work the machine to its full effect, and is of a defective construction. Yet under all these disadvantages and impediments to the development of its powers, the machine worked steadily at $7\frac{1}{2}$ strokes per minute. The diameter of the aërometers is 12 feet, and the length of stroke 4 feet.

Therefore 12 feet diameter $=$ 113 feet area $\times 2 = 226$ feet area
and 4 feet stroke $\times 2 = 8 \times 7\frac{1}{2}$ strokes per minute 60 feet velocity

$=$ to $\overline{13{,}560}$ cubic feet of air drawn out of the mine per minute. The greatest quantity of air passing through this mine previous to the erection of this machine was 3000 cubic feet per minute; whilst this machine, if worked to its full extent, is capable of drawing 40,000 cubic feet per minute. By increasing the diameter of the aërometers to 15 feet, then 70,000; and if to 25 feet, then 125,000 cubic feet of air per minute would be drawn out of the mine, provided the engine-power was also increased. No sooner was the machine set to work, than its effects were immediately felt in every part of the mine. Stalls in which the fire-damp was so prevalent that it required the utmost caution to be used even with the Davy lamp, the cylinders of which were so heated as to require to be frequently taken into another part of the mine to be cooled, were cleared of this dangerous enemy as if by magic, and all indications of the presence of fire-damp vanished; indeed, so effectually has the machine removed all apprehensions of danger, that naked lamps and candles are now substituted for the safety-lamp. Even the waste parts of the mine, which are at a considerable distance from the direct course of the air-way, and which were so foul and fiery as to render the introduction of even a safety-lamp into it very hazardous, were unexpectedly, and to the astonishment of the men, completely cleared. The abandoned stalls, which have hitherto been magazines of explosive air, can now be entered with safety with a candle; and the whole atmosphere of the mine is so much improved and purified, that, according to the concurrent testimony of both masters and men, a collier now cuts three tons of coal with less fatigue than he could previously cut two tons. The effect of the machine in clearing the colliery of noisome vapours is plainly indicated by the offensive odour of the air discharged from it, and by the fact observed yesterday, of a dense volume of powder smoke issuing from the outlet valves of the machine, almost immediately after the discharge of a blasting shot in the mine. When the ventilator was first put in operation, the furnace, which is situated at a short distance from it, in one of the air-tunnels on the surface, was also in action, when, with almost the first motions of the pistons, the fire was swept off the bars, and the red-hot cinders carried along the inlet air-passages to the aërometers—a fact clearly illustrative of the superior draught of this machine, as compared with that caused by the furnace.

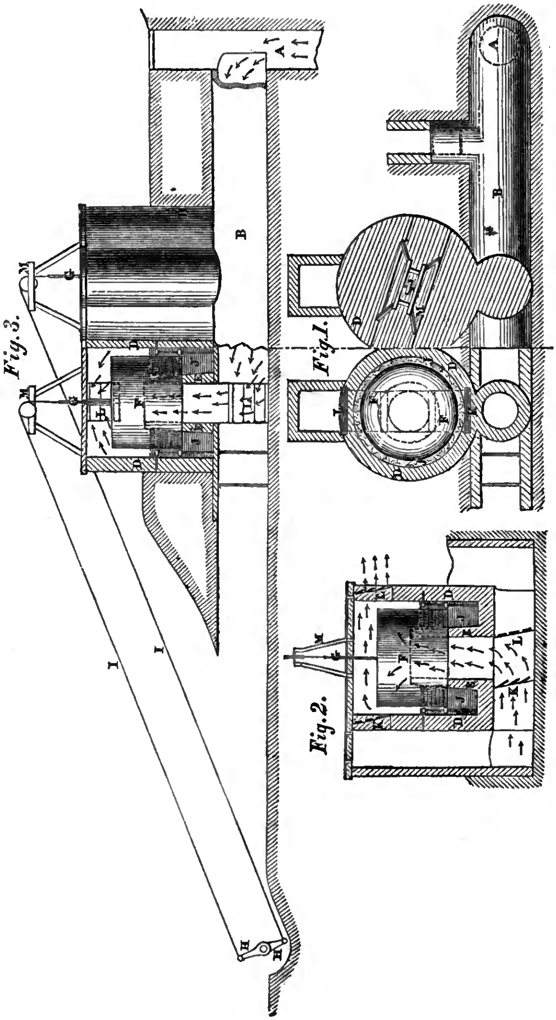
Other facts, confirmatory of what has been stated, might be added; but it is presumed sufficient has been said to prove that, although tried under great disadvantages, yet the success of this new mine ventilator has been unequivocally demonstrated; and that with the slight improvements which experience may point out as expedient, this mode of ventilating mines will constitute a means far superior to the furnace, high-pressure steam, or any other mode which has hitherto been attempted.

It will be seen, on a reference to the plans, that it is perfectly unaffected by the thermometrical and barometrical changes in the atmosphere; that it is capable of being so constructed, as to double or treble the quantity of air ordinarily required, on the occurrence of a diminished pressure of the atmosphere, or on other emergences; that its effective operation is uninfluenced by fogs or wind; that it constitutes an air-gauge, indicating the quantity of air at any time passing through the colliery, and gives immediate and unequivocal indications of the neglect of the man attending it; that it may be applied with facility to drawing as well as to other upcast shafts and levels, and thus effect a great saving of the expense now incurred by the rapid destruction of the chains, ropes, tubbing, &c., besides diminishing the existing dangers caused thereby to the men.

These, Sir, are a few of the benefits which will result from this valuable invention; to which may be added its easy adaptation to the peculiar circumstances of any mine, without requiring any alteration in its internal works. The cost of erecting a machine of the same dimensions as that at Eaglesbush, independent of the engine power, is about 300*l.*, and it requires the attendance of only one man, consuming somewhat less than two tons of small coal per week; so that, to its other numerous advantages, cheapness of cost and a small annual expense are to be added. In many collieries there is sufficient spare power, so as to render the erection of an engine for this purpose unnecessary; and where this is not the case, 200*l.* will amply provide the requisite power.

It is highly gratifying to Messrs. Penrose and Evans, the proprietors of Eaglesbush colliery, that their efforts to improve the ventilation of their valuable mine have resulted in such signal success, and converted one of the most dangerous into one of the safest collieries in the district; and it is to be hoped that their laudable example in thus providing for the comfort and safety of the numerous men in their employment will have a beneficial influence on other coal-owners, whose mines are in a dangerous state from imperfect ventilation.—J. Richardson, C.E.

STRUVE'S PATENT VENTILATING APPARATUS.



Description of the Engraving.

Figs. 1, 2, 3 are a plan, section and elevation of the mine ventilator. A represents the upcast pit, which may be either the coal or pumping-pit.

B culvert, 5 feet by 6 feet, connecting the upcast pit with the mine ventilator; thus an uninterrupted communication is established with the whole of the air-passages of the colliery.

DD are two cylinders of masonry, 14 feet interior diameter, and 16 feet long.

EE are interior cylinders, 9 feet 6 inches long, and 4 feet 6 inches diameter: the space between the cylinders is filled with water, 7 feet deep, and marked J.

FF are two aërometers, of 12 feet in diameter and 8 feet 6 inches long, made to balance each other, and to move vertically in the water by means of guides.

GG, connecting-rods, with the chains from the crank shaft, and which also serve as guides.

HH, two cranks, placed in opposite direction on a shaft, and to which an engine is attached to give them a rotatory motion.

II, two chains connecting the cranks with the aërometers, and giving a vertical motion to the aërometers.

KKKK, four sets of inlet-valves to admit the air from the mine.

LLLL, four sets of outlet-valves for the discharge of the air into the atmosphere.

M is the framing, which supports two shelves, of 2 feet in diameter, over which the chain moves, and which have to support the whole weight of the aërometers.

N, embankment formed from the cuttings of the foundations.

The operation of the machine is as follows:—A steam-engine or other power gives a rotatory motion to the shaft and cranks HH, and by means of the chains II, a reciprocating motion is given to the aërometers FF, equal to twice the length of the cranks; in this case the machine can work a 4-feet or a 6-feet stroke: the aërometers balance each other, and descend by their own weight—the lower inlet-valves opening at the same moment as the upper outlet-valves, a rapid passage of air takes place through the pumps.

The water forms the packing, or hermetical seal, which prevents air escaping, or being admitted, except through the inlet or outlet-valves. This machine is capable of discharging 40,000 cubic feet per minute when moving at the rate of 200 feet per minute; and there is no reason why it may not be worked much faster. The machine is moved by a five-horse power high-pressure engine.

The following testimonial has been received from Messrs. Penrose and Evans, relative to the patent mine ventilator, which has been lately erected at the Eaglesbush colliery.

"Dear Sir,—Your patent mine ventilator has now been at work at our colliery for a month, and gives us perfect satisfaction. In our case not only is the gas and foul air drawn from the stalls and general workings of the mine, but the old goaves and abandoned parts are likewise kept clear. Our men now all work in their stalls with naked lamps. We work the ventilator from about five in the morning till six in the evening, it being unnecessary to work it at night, as on entering the mine in the morning the overman takes a Davy lamp with him; and however much of gas there may be there, it is immediately drawn off on the working of the ventilator. Our men say that the mine is now cool, and wholesome to work in, and we observe that they finish their labour in a much shorter time. The current of air underground is uniform, and quite independent of barometrical or thermometrical changes. We shall at all times be ready to give facilities to any parties you may wish to have an opportunity of viewing the working of the machine.—PENROSE and EVANS, Eaglesbush Colliery, Neath, March 5."—*From the Mining Journal for* March 20.

ANALYSIS OF FAUJASITE. BY M. A. DAMOUR.

The author states, that in a notice inserted in the first volume of the fourth series of the *Annales des Mines*, he gave a description of a mineral belonging to the zeolite family, which, on account of its crystalline form and composition, appeared to him to constitute a distinct species; to this mineral he gave the name of Faujasite. The rarity of the mineral at the time it was discovered prevented M. Damour from employing more than a very small quantity for analysis. Having, however, lately procured several specimens, they were employed in repeating the analysis.

The fresh analysis gave as the composition of this mineral:—

Silica	46·12
Alumina	16·81
Lime	4·79
Soda	5·09
Water	27·02
	99·83

In his first notice the author stated that faujasite retained its transparency after heating to redness, and was acted upon by acids; he has since found that the mineral loses these properties when heated to near its melting-point; it then disengages the last traces of water, becomes milk-white, and hydrochloric acid cold or boiling does not act upon it.—*Ann. des Mines*, tom. xiv. p. 67.

ANALYSIS OF CALIFORNIA GOLD.

M. Rivot, mining engineer, has analysed a specimen of California gold sent by Mr. Peabody to the Ecole des Mines. The specimen contained—small flattened grains, of a fine yellow colour, and extremely small and smooth grains, attracted by the magnet, which appeared to be titaniferous iron. A rather large, yellow and irregularly rounded grain, weighing 0·628 grs., the density of which was only 14·60, was fused on a small cupel in a muffle, and gave a button of alloy, the density of which was 17·48.

The analysis of the grains of gold, performed on one gramme, gave the following results:—

Gold	90·70
Silver	8·80
Iron	0·38
	99·88—*Ibid.*

ON THE VANADIATE OF LEAD AND THE DOUBLE VANADIATE OF LEAD AND COPPER. BY M. IGNACE DOMEIKO.

The formation of secondary porphyry of Chili, which has been already remarkable for specimens of the native amalgam of Arqueros, and of iodide of silver of Algodones, also contains a very rich formation of vanadiate of lead and of copper.

The mine in which these specimens were discovered is twelve kilometres to the east of the silver mines of Arqueros, and is known by the name of the Mina Grande, or Mina de la Marqueza, and was considered as one of the richest silver mines in Chili. A miner who was about to recommence working the mine, found a heavy yellow mineral, which he brought to Coquimbo for analysis. It was found by M. Domeiko to be very poor in silver, but contained vanadium.

This mineral is of a dirty yellow colour, sometimes of a sulphur-yellow, or slightly orange or greenish; its powder is of a yellowish white colour; its texture is compact, sometimes slightly earthy, and sometimes of a weak resinous lustre. It contains numerous irregular cavities, the interior of which is always incrusted with a brownish matter, often consisting of globular concretions; the mass sometimes exhibits greenish earthy particles, coloured by carbonate of copper, and also white carbonate of lead.

Before the blowpipe, the mineral fuses with intumescence into a gray metallic scoria, slightly frothed, and giving a blue colour to the flame. On charcoal, with the addition of carbonate of soda, there are obtained a perfectly malleable button of lead and a yellowish gray scoria; when melted on a platina wire, with the salt of phosphorus, it yields a transparent bead, which assumes a fine green colour in the interior flame, and becomes yellowish brown in the exterior flame; when heated in the matrass, it yields a little water derived from the argillaceous gangue; nothing sublimes in the open tube.

Dilute nitric acid dissolves it readily, even when cold, without producing either effervescence or nitrous vapours, and leaves only a residue of brownish or reddish gelatinous matter. Acetic acid has no action upon it. The action of sulphuric acid determines the absence of fluorine.

The process which succeeded best in analysing this mineral was the following:—

The mineral reduced to an impalpable powder is treated with cold dilute nitric acid, and digested for 24 hours; it is then to be gently heated and filtered to separate the ferruginous clay, unacted upon. The chlorine is to be determined by nitrate of silver, the excess of which is to be precipitated by a little hydrochloric acid. The greater part of the lead is then to be precipitated by sulphuric acid; the filtered liquor is to be largely diluted, and sulphuretted hydrogen is to be passed through it cold, and the operation is to cease as soon as the lead and copper are precipitated; filter and precipitate the arsenic by saturating and repeatedly heating the solution. Evaporate the filtered liquor to dryness; treat the residue with hot dilute nitric acid, dilute the solution and precipitate with excess of ammonia the phosphates of lime, zirconia, iron and alumina (A). The filtered liquor is concentrated, and a fragment of sal-ammoniac is immersed in it, with the addition of a few drops of ammonia. The vanadium is immediately precipitated in the state of vanadiate of ammonia, which is to be collected on a filter and washed, at first with a saturated solution of sal-ammoniac, and then with alcohol. The solutions containing sal-ammoniac are evaporated and the residue slightly calcined. Water is then to be added, which separates a little silica, and the phosphoric acid is to be determined by iron according to Berthier's process. As to the precipitate (A), if it contain a notable quantity of vanadium, it is to be redissolved in nitric acid, and to be again precipitated by excess of ammonia, &c. In the opposite case, it is to be fused with one part of silica and three parts of carbonate of soda, and treated with water; phosphoric acid is to be sought for in the alkaline liquor, and the insoluble residue, composed of silica, alumina, lime, oxide of iron, and zirconia, is to be analysed.

The mean of several analyses yielded—

Chloride of lead	9·05
Oxide of lead	58·31
Oxide of copper	0·92
Arsenic acid	11·55
Phosphoric acid	5·13
Vanadic acid	1·86
Lime	7·96
Alumina, zirconia (?), traces of oxide of iron	1·10
Argil	2·00
Water	1·12
	99·00

The presence of copper in the above-described mineral induced the author to examine whether vanadiate of copper might not be found among the accompanying minerals, this mineral having been stated to exist in Siberia.

The green earthy portions were first examined, and these were associated with traces of vanadium; other portions examined did not contain more, and M. Domeiko was about to give up the examination, when he found that the blackish-brown portion which he had taken for ferruginous argillaceous gangue, was much richer in vanadium than the yellow mineral.

This substance is amorphous, porous, heavy, of a more or less deep blackish-brown colour, and of a texture which is either compact or earthy; by the heat of the taper, it melts into a black bead, which is somewhat frothed. By the blowpipe, it gives a green bead, with phosphorus salt, a cupreous globule of lead upon charcoal, and in the matrass it yields a little water. It is rather soft, and its powder is brownish yellow. It lines the cavities of the yellow arsenio-phosphate mineral, and is frequently associated with the amorphous carbonates of lead and copper. At first sight it is mistaken for hydrate of iron, from which it differs by its great fusibility, its ready solubility in dilute nitric acid, and its reaction with the blowpipe, &c.

The mean of two analyses gave as its composition—

Oxide of lead	53·43
Oxide of copper	15·78
Vanadic acid	13·41
Arsenic acid	4·64
Phosphoric acid	0·64
Chloride of lead	0·33
Silica?	1·16
Lime	0·54
Oxide of iron, alumina, &c.	3·46
Argillaceous residue	1·26
Water	2·70
	97·35

From these results the author is induced to suppose that the above mineral contains a double vanadiate of lead and copper, the composition of which approaches the formula $\dot{Pb}^6\dddot{V}+\dot{Cu}^6\dddot{V}$, or Pb^2V+Cu^2V.—*Ann. des Mines*, tom. xiv.

NEW MINERAL FROM BRAZIL.

M. Dufrenoy exhibited before the Academy a specimen of a mineral from Brazil, which appears to be to the diamond what emery is to corundum, as stated by M. Elie de Beaumont. Among some specimens recently sent to the Ecole des Mines by M. Hoffinan, a dealer in minerals, were two which were stated to be hard enough to polish the diamond; and in fact the hardness of these specimens was found to be superior to that of the topaz.

This substance was analysed by M. Rivot, mining engineer, who had at his disposal one large fragment weighing 65·760 grs., and several small pieces weighing rather less than 0·50 gr.; the latter only were analysed. The large fragment appeared to come from the same alluvial formation as that in which the Brazilian diamonds occur. Its edges are rounded by long friction; but it has not the appearance of a rolled flint. It is of a slightly brownish dull black colour. Viewed with a glass, it appears riddled with small cavities separating very small irregular laminæ, which are slightly translucent and iridescent. The brown colour is very unequally distributed throughout the mass. On one of the faces the cavities are linear, which gives it a fibrous aspect similar to obsidian. It cuts glass readily, and scratches quartz and topaz; its density is only 3·012. The small fragments subjected to analysis weighed 0·444 gr., 0·410 gr. and 0·332 gr.; their densities were respectively 3·141, 3·416 and 3·255.

These numbers indicate great difference in the porosity of the specimens; they lead, however, to the conclusion, that the density of the substance is very nearly the same as that of the diamond.

By means of long calcination at a bright red heat in a covered crucible, the specimens were not altered; they retained their aspect, hardness and weight; they do not therefore contain any substance volatilizable by calcination out of contact of the air. This result certainly does not prove the igneous origin of these diamonds, but renders improbable the idea expressed by M. Liebig, that diamonds are derived from the transformation of organic vegetable matter.

The three specimens were successively burnt in pure oxygen gas in the apparatus employed by M. Dumas for the combustion of the diamond. The oxygen obtained from chlorate of potash was contained in a gasometer; it was dried and purified before it reached the combustion-tube by passing through two tubes containing sulphuric acid and pumice, and one tube with potash; employing this method with the precautions indicated by M. Dumas, 100 of the

	Carbon.	Ash.	Loss.
1st specimen gave	96·84	2·03	1·13
2nd ..	99·73	0·24	0·03
3rd ..	99·37	0·27	0·36

In the combustion of the first specimen only one bulb-tube with potash was employed, so that a portion of the carbonic acid produced by the combustion was lost; but in the other two experiments, in which two bulb-tubes containing potash were used, the second increased in weight some centigrammes.

The two last analyses prove perfectly that the specimens are composed entirely of carbon and ash. The ash was yellowish, and in the first specimen it had retained the form of the diamond. When examined by the microscope, the ash appeared to be composed of ferruginous alumina and small transparent crystals, the form of which could not be ascertained.—*L'Institut,* Mars 2, 1849.

ANALYSIS OF THE WATER OF THE MEDITERRANEAN ON THE COAST OF FRANCE.

M. J. Usiglio analysed the water taken from the foot of Mount St. Clair, about 4000 metres from the port of Cette.

100 parts gave—

Chloride of sodium	2·9424
Bromide of sodium	0·0556
Chloride of potassium........	0·0505
Chloride of magnesium	0·3219
Sulphate of magnesia........	0·2477
Sulphate of lime	0·1357
Carbonate of lime	0·0114
Peroxide of iron	0·0003
Water	96·2345
	100·000

Comptes Rendus, October 1848.

IMPURITY OF COMMERCIAL BROMINE.

M. Poselger, in distilling some samples of commercial bromine, found that the boiling-point was not 122° F., but 248° F.; and that the colour of the liquid became gradually lighter, till it was eventually quite colourless. On continuing the distillation to dryness, he obtained a residue of charcoal. On separating the bromine from the last portions of the distilled liquid by means of a solution of potash, an aromatic, oily, colourless liquid was obtained, which analysis proved to be carburet of bromine; this existed in various specimens of bromine to the extent of 6 or 8 per cent., and there is every reason to conclude that it was derived from the æther employed in the preparation of this substance.—*Journ. de Ph. et de Ch.*, Février 1849.

METEOROLOGICAL OBSERVATIONS FOR MARCH 1849.

Chiswick.—March 1. Cloudy: clear and windy: cloudy. 2. Fine: cloudy: clear. 3. Overcast. 4. Clear: cloudy and fine: clear. 5. Fine: frosty. 6. Frosty: fine: overcast. 7. Cloudy: boisterous, with rain at night. 8. Fine: hail-shower: clear and frosty at night. 9. Clear and frosty: very fine: slight snow. 10. Clear: cloudy. 11, 12. Overcast throughout. 13. Fine. 14. Cold dusky haze. 15, 16. Overcast. 17. Foggy: fine: clear. 18. Foggy, with heavy dew: hazy: foggy and damp. 19. Foggy: overcast. 20. Dusky haze: overcast: clear. 21. Overcast: clear. 22. Foggy: cold haze: overcast. 23. Overcast: cold haze: densely overcast. 24. Fine, but cold: clear and frosty at night. 25. Snowing: cloudy and cold: overcast. 26. Densely clouded. 27. Dry haze. 28. Foggy: overcast throughout. 29. Hazy: rain: cloudy. 30. Heavy showers. 31 Clear: fine: cloudy.

Mean temperature of the month	41°·56
Mean temperature of March 1848	42 ·43
Mean temperature of March for the last twenty years	42 ·62
Average amount of rain in March	1·36 inch.

Boston.—March 1. Cloudy: rain A.M. and stormy. 2. Cloudy. 3. Fine. 4. Cloudy. 5, 6. Fine. 7. Fine: rain P.M. 8. Cloudy: snow P.M. 9, 10. Fine. 11. Cloudy. 12. Fine. 13. Fine: rain early A.M. 14—16. Cloudy. 17. Fine. 18, 19. Foggy. 20—22. Cloudy. 23. Cloudy: rain P.M. 24. Snow. 25. Cloudy. 26. Fine. 27. Cloudy. 28. Rain: rain A.M. 29. Cloudy: rain P.M. 30, 31. Fine: rain P.M.

Applegarth Manse, Dumfries-shire.—March 1. Fair and clear A.M.: getting cloudy: rain P.M. 2. Fair A.M.: showers. 3. Fair: wind rising. 4. Fair: cloudy: fine sunset. 5. Slight shower: cleared. 6. Fair: cloudy: high wind P.M. 7. Rain all day, but light. 8. Frost hard: clear all day. 9. Frost keen. 10. Frost increasing in keenness. 11. Frost slight: shower. 12. Fine spring morning: got colder. 13—17. Fine dry weather. 18. Very fine day. 19. Still finer, like May. 20. The same, beautiful weather. 21. The same: fog P.M. 22. Frost during the night: fine day. 23, 24. Fine, though dull. 25. The same: brighter. 26. Frost: dull P.M. 27. The same dullness: no frost. 28. Clear and cool. 29. Shower early. 30. Showery. 31. Slight drops of rain: slight frost.

Mean temperature of the month	41°·8
Mean temperature of March 1848	41 ·2
Mean temperature of March for twenty-five years	39 ·1
Rain in March 1848	4·1 inches.
Rain in March for twenty years	2·3 „

Sandwick Manse, Orkney.—March 1. Bright: hail: light showers. 2. Cloudy: showers. 3. Cloudy: clear. 4. Clear: showers. 5. Sleet: showers. 6. Showers. 7. Sleet: snow-showers. 8. Snow-showers. 9. Drift: snow-showers. 10. Thaw: showers. 11. Rain: drizzle. 12. Cloudy: snow. 13. Snow-showers: thaw. 14. Drizzle. 15. Fog: damp. 16. Fog: fine. 17. Fine: fog. 18, 19. Fog. 20. Fine: clear aurora. 21. Fine: cloudy. 22. Fog: cloudy. 23. Rain. 24. Damp: drizzle. 25. Drizzle: aurora. 26. Sleet-showers: clear aurora. 27. Cloudy: showers. 28. Cloudy: clear aurora. 29. Cloudy. 30. showers. 31. Damp: clear.

Boston; *by the* Rev. W. Dunbar, *at Applegarth Manse*, Dumfries-shire; *and by the* Rev. C. Clouston, *at Sandwick Manse*, Orkney.

Days of Month.	Barometer.							Thermometer.							Wind.				Rain.			
	Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Dumfries-shire.	Orkney, Sandwick.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.
1849. March.	Max.	Min.		9 a.m.	9 p.m.	9¼ a.m.	8½ p.m.	Max.	Min.		Max.	Min.	9¼ a.m.	8½ p.m.								
1.	30·026	29·426	29·02	29·43	29·58	29·46	29·31	49	39	38·5	45½	33	40½	42	nw.	nw.	w.	sw.	......	......	......	·13
☽ 2.	30·283	30·126	29·70	29·73	29·93	29·40	29·65	51	34	35	48	39	41	46	sw.	wsw.	w.	w.	......	·51	......	·24
3.	30·461	30·366	29·95	30·10	30·15	29·92	29·93	53	41	43	49½	39	47	47½	sw.	calm	w.	w.	......	......	......	·05
4.	30·459	30·447	30·00	30·18	30·11	29·92	29·81	58	29	47	48½	43	50	47	sw.	calm	w.	sw.	......	......	......	......
5.	30·605	30·457	29·98	30·15	30·32	29·96	30·01	54	26	48·5	48	42½	42	42	sw.	calm	w.	wsw.	......	......	......	·18
6.	30·618	30·280	30·14	30·31	29·80	29·76	29·20	55	41	41	47	37½	47	48	sw.	calm	w.	sw.	......	......	......	·12
7.	30·062	29·879	29·50	29·55	29·64	29·29	29·58	54	38	47	46	41½	35½	35	w.	w.	w.	n.	·06	......	......	·16
8.	29·918	29·810	29·46	29·69	29·76	29·63	29·74	49	23	41	48½	29	32	30	nw.	calm	w.	nw.	·02	·26	0·18	·04
○ 9.	29·949	29·917	29·50	29·82	30·03	29·93	30·15	43	29	32	42	25½	33½	33	nw.	n.	nnw.	n.	·01	·20	......	·15
10.	30·432	30·276	29·95	30·23	30·22	30·04	30·04	43	24	33	42	23½	41	41	n.	n.	nw.	wnw.	......	......	......	·09
11.	30·424	30·271	30·05	30·10	29·85	29·63	29·64	46	36	39·5	45½	34	44	46	w.	calm	wsw.	wnw.	......	......	......	·15
12.	30·164	30·128	29·70	29·95	30·00	29·97	29·92	56	43	47	52	43½	41½	33½	n.	w.	wsw.	calm	......	......	......	·04
13.	30·302	30·030	29·60	29·90	......	30·12	30·22	59	36	52	...	45	34½	37	sw.	wnw.	......	nnw.	......	·09	......	·41
14.	30·308	30·268	29·90	......	......	30·04	30·04	50	40	41	...	...	43½	46	sw.	w.	......	w.	......	......	......	·14
15.	30·312	30·292	29·93	......	......	30·12	30·14	53	45	48	...	...	46½	46½	nw.	calm	......	w.	......	......	......	·10
16.	30·337	30·309	29·88	......	......	30·18	30·21	57	28	50	...	...	46½	44½	ne.	calm	......	w.	......	......	0·00	·02
☾ 17.	30·306	30·201	29·93	......	......	30·15	30·09	59	30	42	...	...	46	41	n.	calm	......	ese.	......	......	......	......
18.	30·159	30·057	29·71	......	29·90	29·99	29·96	49	38	43	45	...	44	40½	w.	......	......	ese.	·01	......	......	......
19.	30·042	30·025	29·67	29·85	29·85	29·90	29·90	51	26	41·5	51	41½	43	42	e.	......	......	se.	......	......	......	......
20.	30·242	30·108	29·80	30·00	30·09	29·99	30·10	46	27	41	55½	37½	47	44	e.	......	se.	w.	......	......	......	......
21.	30·300	30·231	29·90	30·15	30·13	30·14	30·14	53	26	46	57	41	48	43½	sw.	......	se.	sse.	......	......	......	......
22.	30·197	30·086	29·82	30·13	30·12	30·12	30·15	42	39	41·5	56½	32½	43	41½	e.	......	se.	sse.	......	......	......	......
23.	30·037	29·979	29·70	30·13	30·00	30·15	30·09	44	30	43	50	39	45	42	ne.	......	se.	s.	......	......	0·00	·12
● 24.	29·986	29·955	29·65	29·94	30·04	29·96	29·98	44	27	38	44	38	44	42	n.	e.	se.	s.	......	·10	......	·06
25.	29·914	29·907	29·67	30·05	29·93	29·87	29·96	40	33	38	43	34	47½	43	ne.	e.	se.	nw.	·01	......	......	·10
26.	29·896	29·842	29·60	29·89	29·74	29·83	29·79	43	32	40	52	38	40½	35	ne.	nw.	ese.	se.	......	......	......	·12
27.	29·570	29·351	29·21	29·40	29·35	29·56	29·67	45	34	41	48	36	41½	39	se.	calm	ese.	e.	·14	......	......	......
28.	29·399	29·250	29·04	29·40	29·50	29·85	29·86	45	38	41	43½	37	41½	38	se.	e.	ne.	ne.	......	·08	......	·13
29.	29·385	29·343	29·10	29·40	29·24	29·66	29·48	51	29	45·5	41	36½	43	38	e.	calm	e.	e.	·14	......	......	......
30.	29·433	29·280	29·00	29·18	29·21	29·46	29·42	52	29	44	49	34	43	38	sw.	se.	e.	ese.	·16	·12	0·09	·12
☽ 31.	29·607	29·560	29·20	29·33	29·35	29·35	29·42	59	34	47	55	38	43½	43	s.	s.	e.	se.	·30	·14	......	·04

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LVIII. *The* Astronomer Royal *on a difficulty in the Problem of Sound.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

An apparent difficulty in the problem of sound has been discussed in successive numbers of your Journal by Professor Challis and Mr. Stokes. Upon this subject I beg leave to lay before you a few remarks. My communication will consist, not of strict mathematical investigation, but of analogies and conjectures; and will therefore be fairly liable to the charge of vagueness to which all papers of this character are exposed. At the same time I must assert that I should not think it proper to transmit such a paper for insertion in your respectable Journal, if I were not persuaded that the analogies are valid and the conjectures probable.

Attention has been called by Professor Challis to the nature of the accurate solution of the partial differential equation applying to plane waves of air. And Mr. Stokes has shown that a distinct meaning can be given to this solution up to a certain point in the progress of the wave.

This solution, although complete as far as it goes, is still but a functional differential equation of the first order with regard to the co-ordinates of a particle of air. For my present purpose, it will be more convenient to take the differential equation applying immediately to the co-ordinates of the particles; although in that case also the complete solution cannot (so far as I am aware) be obtained.

If the original ordinate of a particle of air be x, and its disturbed ordinate be $x+\mathrm{X}$, then the differential equation applying to its motion is

$$a^2.\frac{d^2\mathrm{X}}{dx^2}=\left(1+\frac{d\mathrm{X}}{dx}\right)^2.\frac{d^2\mathrm{X}}{dt^2}.$$

Now, as the first subject of my communication, I propose to compare this with the equation applying to the motion of long waves of water in shallow canals, when the proportion of the vertical movement of the particles to the depth of the canal is not neglected: a case which I have in some measure discussed in the *Encyclopædia Metropolitana*, article *Tides and Waves*. That equation has the form

$$a^2.\frac{d^2X}{dx^2}=\left(1+\frac{dX}{dx}\right)^3.\frac{d^2X}{dt^2}.$$

The equations are generally similar in form, with the difference that the exponent in one case is 2 and in the other is 3. Imperfect solutions of both (as my friend Professor De Morgan has pointed out to me) may be obtained in the following forms:—

For the first,

$$\frac{dX}{dt}=C+a.\log\left(1+\frac{dX}{dx}\right).$$

For the second,

$$\frac{dX}{dt}=C+\frac{2a}{\sqrt{\left\{1+\frac{dX}{dx}\right\}}}.$$

These solutions give us no assistance further than this, that they indicate a critical state of the movement of the particles in both as occurring when $\frac{dX}{dx}=-1$, which in both equations makes $\frac{dX}{dt}$ infinite.

But a very distinct idea of the nature of the progressing wave in both cases may be obtained by solving the equations by the method of successive substitution. Thus, of the equation

$$a^2.\frac{d^2X}{dx^2}-\frac{d^2X}{dt^2}=\left\{2\frac{dX}{dx}+\left(\frac{dX}{dx}\right)^2\right\}.\frac{d^2X}{dt^2},$$

a first solution may be obtained by neglecting the right-hand side (a quantity of the second order as regards the movement of the particles), which solution, if the wave be supposed to travel in one direction only, is

$$X=c.f(at-x).$$

If this value be substituted in the right-hand side of the equation, terms of the second order only being retained, and if the solution be then effected under the conditions proper for successive substitution, namely that no new terms of the first order be introduced, but that the terms of the second order may have the most general form; and if we avail ourselves of that generality to secure the conditions that the general value of X shall not contain $at+x$, and that when x is $=0$ the value of X shall have strictly the form $c.f(at)$; then the value of X to the second order will be

$$X=c.f(at-x)+\frac{c^2}{2}.x.\{A+\overline{f'(at-x)}|^2\},$$

where A is an arbitrary constant which may be used to remove the non-periodical part of $\overline{f'(at-x)}|^2$. Thus if

$$f(at-x)=\sin\overline{n(at-x)},$$

then

$$\overline{f'(at-x)}|^2=n^2.\cos^2\overline{n(at-x)}=\frac{n^2}{2}\left(1+\cos\overline{2n(at-x)}\right);$$

and here it will be proper to take $A=-\frac{n^2}{2}$; and then the value of X to the second order is

$$X=c.\sin\overline{n(at-x)}+\frac{c^2n^2}{4}.x.\cos\overline{2n(at-x)}.$$

If we had treated the equation for waves of water in the same manner, we should have found, to the second order,

$$X=c.\sin\overline{n(at-x)}+\frac{3c^2n^2}{8}.x.\cos\overline{2n(at-x)}.$$

And in both cases, if we carried on the solution to the 3rd, 4th, &c. orders, we should introduce the second power of x multiplying trigonometrical functions of the simple and triple argument, the third power of x multiplying functions of the double and quadruple argument, and so on. The forms of the solutions would be similar, but the coefficients would be different.

If now for any definite value of x we construct a curve graphically representing the motion of the particles, we find that the value of $\frac{dX}{dt}$ is represented by an unsymmetrical curve, the deviation from symmetry increasing as larger and larger values of x are taken. The nature of the asymmetry is this, that the interval of time from the extreme backward motion of a particle to the extreme forward motion is less than half the whole period of vibration; and that this inequality is greater as we consider the movement of particles whose original position is more and more advanced. And this happens in the same manner for the particles of air and for the particles of water.

I might have shown this at once by merely requesting your readers to compare the diagram drawn by Mr. Stokes, in the

November Number of your Journal, with the diagrams drawn by me in the *Encyclopædia Metropolitana*. But I have preferred to show, step by step, that there is strong analogy in every part of the mathematical process in the two cases.

The rapid change of velocity implies in both cases very violent forces of compression among the particles; and very sudden alterations of the surface for the water, and very rapid expansions for the air.

Now in the case of waves of water we cannot by mathematical process (so far as I know) pursue the investigation of the condition of the particles to its limit, but we can in some measure observe it experimentally. And the fact which we observe is this, that in a long river, where the magnitude of the tide is considerable, the tide in the upper parts of the river presents the phænomenon of a *bore*. The surf formed upon extensive flat sands is probably a phænomenon of the same class. Physically considered, the expression of this fact is, that the continuity of the particles is interrupted.

Now I imagine that in the critical state of the waves of air the same thing occurs, namely that the continuity of the particles is interrupted; and that with it the laws of motion of the air, depending essentially as they do upon the existence of that continuity, are interrupted. I imagine that the air is in a state exactly analogous to that of a bore or a surf. Adopting this analogy, I do not think that there is anything of the nature of reflexion of the wave (for I have never been able to observe the smallest trace of reflected wave from a surf), although at the same time I am utterly unable to account for the disposal of the *vis viva*.

Taking this view of the matter, it appears to me that the expression "a plane wave of air is impossible" stands on precisely the same footing as the expression "a tide in the Severn is impossible." In both cases, if the assumption is made "that the waves are to preserve the same character through infinite space and infinite time," the wave is impossible. In both cases, the ordinary understandings on the theory of waves will apply for a considerable distance.

I believe also that in both cases, if by friction or other causes the coefficient of vibration be materially diminished, the waves may travel to a very great distance without any aquatic or aërial *bore*.

The second subject of my communication is the probable sensational indication of the physical phænomenon, "interruption of continuity of particles of air." And it is my belief that it produces that sound which, according as it is in a high key, or is in a low key, or is articulately interrupted, is called a *hiss*, a *buzz*, or a *whisper*, and of which the phonetic symbol is S or Z. Perhaps also the sound which we call a *roar*, of which the symbol is R, is related to it; for philologists appear to take for granted that these letters represent cognate sounds (thus Niebuhr assumes as indubitable that Aurunci and Ausonii are the same word). My reasons for connecting the sound of S with the interruption of continuity, or with the *broken* character of the aërial wave, are the following:—

1. It has long ago been remarked that the sound of S is not returned by an ordinary echo. In like manner, a broken-headed sea is not reflected by a vertical pier. When a broken-headed sea strikes a pier perpendicularly, it is thrown upwards; when it strikes it obliquely, it is partly thrown upwards and partly it runs horizontally along the face of the pier. In neither case is there any reflexion of the broken head, or any creation of a broken wave travelling in the opposite direction, although the swell is reflected according to the usually understood laws.

2. It is well known that in whispering galleries the sound of a whisper is carried along the surface of the dome, and never quits that surface, and can be heard on the opposite side only by applying the ear very near to that surface; while an ordinary sound is not transmitted along the surface, and is not heard at the opposite side of the dome in any striking intensity. In like manner, a broken-headed sea will run along the face of a vertical pier for a considerable distance without showing any disposition to quit it; while an ordinary fluctuation is thrown off it at once, in the form of a swell rolling away at an angle given by the usual laws of reflexion.

Whether there is any well-established instance of the conversion of a clear musical sound into a hiss or buzz by mere distance of transmission, I am not able to say; but I should think that, long before the wave could have received its change of character to the degree necessary to produce discontinuity of particles, even supposing that its coefficient had remained undiminished, that coefficient would from friction or from expansion of the wave have become so small, that there would be no perceptible tendency in the wave to change its form. So far however as the tendency to change the form exists, it will be greater for loud sounds than for faint sounds, and greater for sounds in a high key than for sounds in a low key; or in other words, the sounds which might be expected soonest to degenerate into hiss or buzz would be loud sounds on a high key.

I am, Gentlemen,

Your obedient Servant,

G. B. Airy.

Royal Observatory, Greenwich,
May 17, 1849.

LIX. *On the Symbols of Algebra, and on the Theory of Tessarines. By* James Cockle, *Esq., M.A., of Trinity College, Cambridge, and Barrister-at-Law of the Middle Temple*.*

AT page 436 of the last (33rd) volume of this Journal, I proposed to include under the genus *imaginary* two independent species of quantity which I distinguished by the respective terms *unreal* and *impossible*. But, if we extend the meaning of the word "imaginary" so as to make it comprehend all quantity that is not *real*, a third species of quantity, for which I would suggest the name of *ideal*, must be added to the two already included in the common genus of imaginaries. And perhaps it will be conducive to distinctness of conception and to convenience if we admit a *fourth* species of imaginaries, which I propose to call *typal*, from their squares, &c. running into certain *types*.

The peculiar symbols of the quaternion theory of Sir W. R. Hamilton, of the octave theory of Mr. John T. Graves and Mr. Cayley, of the triple algebra of Professor De Morgan, and of the pluquaternion theory of the Rev. T. P. Kirkman, all belong to the species *ideal*. It may be stated as the characteristic of ideal quantities, that, in their combinations, they do not follow the laws of that ordinary algebra to which Mr. De Morgan applies the term Double Algebra. The imaginaries in some of the systems of Mr. Cayley's theory of couples (the systems marked C, D, C′, and D′, Phil. Mag., S. 3, vol. xxvii. pp. 39–40) are *ideal*. And so, in general, are those of the last two systems (E and E′, *Ib.* p. 40) in Mr. Cayley's paper. Mr. J. T. Graves would call the couples involved in the above systems *anomalous*.

But in other of the systems of Mr. Cayley (those marked A, B, A′, B′, *Ib.* p. 38 *et seq.*) the imaginaries are *typal*; and this is also the case in Mr. J. T. Graves's theory of couples (*Ib.* vol. xxvi.). Borrowing a term from the learned writer last mentioned, we may call all the couples mentioned in this paragraph *normal*. The characteristic of *typal* quantities is, that, although in their laws of combination they follow the rules of ordinary algebra, yet the types or conditions by which they are defined are not consistent with that algebra. A *typal* differs from an *impossible* quantity in this: that the ordinary algebra *forces* impossible quantity upon our notice, and defines it by means as purely algebraic as those by which unreal quantity is defined; while on the other hand, typal (as well as

* Communicated by Dr. John Cockle, F.R.C.S.

ideal) quantity is the offspring of arbitrary ultra-algebraic definition*.

I should propose to apply the term *hyper-algebraic* to typal and ideal quantities, and to confine it to those quantities. Ought we to apply the word *hyper-algebraic* to impossible quantity? I think not. If I may be permitted to use the term *possible* so as to include under it not only real quantities but also the *unreal* quantities of ordinary algebra, I would suggest that it is only in respect of certain anomalous results† (results, however, that do not defy explanation ‡) that impossible differs from possible quantity, and consequently that impossible quantity must be regarded as *algebraic*. It is unquestionably

* I must not be understood as desiring to underrate these symbolic children of definition. So far from it, I think it within the limits of possibility so to define symbols as that they may have their prototypes in nature, and serve to expound other of her phænomena than those to which symbols have been yet applied. For instance, might not arbitrary symbols be made the representatives of *chemical* phænomena? Mr. Boole's Mathematical Analysis of Logic is a step out of the beaten track which symbolic science has hitherto persevered in; and although to pass from mental to chemical phænomena may not authorise us to hope that such sciences as chemistry may be rendered symbolic, yet I cannot help thinking, that by a proper notation for *affinity*, &c. chemical decompositions might be represented: at any rate it may be worth a trial.

† Vide *suprà*, pp. 41, 42. It is not a little singular, that if we abandon the principle laid down, *suprà*, p. 39, note †, we have

$$(1+\sqrt{j})(1-\sqrt{j})=1-\sqrt{j}^2=1-1=0;$$

and also that if we preserve that principle, we derive from the equation (1.), *suprà*, p. 39, the following:

$$(-1)^4=(+\sqrt{j})^4=(\sqrt{j}\times\sqrt{j})\times(\sqrt{j}\times\sqrt{j})=j\times j,$$

or

$$1=j^2.$$

On the other hand, although from the relation

$$(1+k)^2=2k=2ij$$

we may deduce

$$\frac{1}{4}(1-i+j+k)^2=j,$$

still, seeing, from the relation (*suprà*, p. 40)

$$+\sqrt{j}=-1,$$

the anomalous nature of the evolution of impossibles, we must not attempt to express $\sqrt{j}$ as a linear function of i, j, k.

I may add, that in previous papers (*suprà*, pp. 45, and 135,) the radius of the (larger) sphere is supposed to be unity.

‡ See paragraph 8 (and 10) of the Rev. Prof. Charles Graves's paper on Triple Algebra (*suprà*, p. 119–126). I propose to call Mr. C. Graves's system a *trinar*, and Mr. De Morgan's a *ternary* algebra; the latter form of name being given to the quadratic system, in which the *square* of the imaginary is *negative*. And hence the respective terms *trine* and *ternion* suggest themselves as *distinctive*.

algebraic in its origin, nor is it to be considered as other than algebraic in its laws of combination with other symbols; and if in first approaching the subject of impossible quantity some mysteries present themselves and some difficulties arise, it may be worth considering whether our estimate of the range of ordinary algebra has not been too limited, and whether its own inherent powers, when sufficiently developed, may not explain the mystery and clear away the difficulty. With these views it will not excite surprise if I state here, that I regard paragraph 8 of the Rev. Charles Graves's paper on Triple Algebra (*suprà*, pp. 123, 124) as a valuable contribution to *ordinary* algebra.

I take the opportunity of adding one or two remarks on tessarines, premising that I shall use i' j' and k' to denote the imaginaries which enter into those expressions.

1. The product of two tessarines of the form

$$-\frac{xz}{y}+i'x+j'y+k'z$$

is of the same form, and the *moduli* and *amplitudes* of the factors and product are related in the same manner, and the latter may be constructed as readily, as if the factors and the product were quaternions.

2. The product of the two tessarines

$$i'x_1+j'y_1+k'z_1, \text{ and } i'x_2+j'y_2+k'z_2,$$

will be of the form

$$i'x_3+j'y_3+k'z_3,$$

provided that

$$y_1y_2-x_1x_2-z_1z_2=0. \quad \ldots\ldots \quad (\varpi.)$$

But, if the two systems of values x_1, y_1, z_1, and x_2, y_2, z_2, respectively satisfy the condition

$$y^2-x^2-z^2=0, \quad \ldots\ldots \quad (\text{w.})$$

and if, moreover,

$$\frac{x_1}{z_1}=\frac{x_2}{z_2},$$

then (ϖ.) may be satisfied*. But (w.) represents a right-

* If we multiply the two equations that result from substituting the two systems of values of x y and z respectively, we shall have after reduction, &c.

$$(y_1y_2-x_1x_2-z_1z_2)(y_1y_2+x_1x_2+z_1z_2)=0,$$

which suggests matter for future observation. With reference to the subject of Imaginary Geometry (*suprà*, p. 132–135), and indeed of analytical geometry in general, I may remark that I propose to call the real primary axis (that of x) the *axe*, the unreal secondary axis (that of y) the *perpe*, and the impossible tertiary axis (that of z) the *norme*. Thus, in the equation angled cone, whose axis is the axis of y, and whose vertex is the origin, the axes being rectangular. Hence, taking $i'x+j'y+k'z$ to denote a point whose rectangular co-ordinates are x, y, z, we see that if two points be taken in the same generatrix of the cone (w.), their tessarine product, considering the vertex as origin, will be the point* whose co-ordinates are

$$x_3=y_1z_2+z_1y_2, \quad y_3=-x_1z_2-z_1x_2, \quad z_3=x_1y_2+y_1x_2.$$

(1.) (*suprà*, p. 133) A is the axe, B the perpe, and C the norme. Let me add that the equations

$$a-x=0, \quad a^2+y^2=0, \quad \sqrt{a}+\sqrt{z}=0,$$

(of which the respective solutions are

$$x=a, \quad y=i'a, \quad z=j'a,)$$

denote, when considered separately, three points, each at a distance a from the origin, but in axes at right angles to each other.

* My friend Professor Davies has (*suprà*, p. 37, and vol. xxix. p. 171–175) intimated or expressed an opinion adverse to the interpretability of the symbol $\sqrt{-1}$ in geometry. If eminence in geometric science can confer a right, not only to express such an opinion, but to have that expression duly weighed, then I think that there are few, if any, English geometers who possess those rights more unquestionably. I must however confess that I do not see the force of the reasoning employed in Mr. Davies's proposition (*Ib.* p. 174). The inconsistency alluded to in paragraph 4 of the proposition could never arise—at least I am unable to perceive how it could. In saying that a rectangle is equal to the product of its sides, we mean that the *numbers of linear units* in the sides, when multiplied together, give a number equal to the *number of square units* in the rectangle. But the *signs* are not elements in the consideration when we multiply the sides. Unless I am mistaken, the inconsistency in question must arise in some such manner as the following:—Take A in BB′; and let it be required that the rectangle B′A × AB shall equal half the square on BB′. We should then (as to this *vide suprà*, p. 43) find $AC=\pm a\sqrt{-1}$, and that would be a solution of the problem. I consider, then, that my much-valued friend's argument rests solely on the inductive ground previously assigned (vol. xxix. p. 172). But there are strong inductive reasons on the other side of the question. The symbol $\sqrt{-1}$ appears to indicate that more *dimensions* of the subject-matter must be taken into consideration than are stated in the data of a question. If we are dealing with a *two-dimensioned* subject, and meet with the symbol $\sqrt{-1}$, that symbol indicates impossibility or not according to the fact of our having or not having two dimensions given in our data. Thus, in that spherical geometry with which Mr. Davies's name must ever be associated, and in which a point may be determined by its longitude (ϕ) and its latitude (ψ) (see Camb. Math. Journ., vol. i. p. 193; ii. p. 37, &c.), such an expression as

$$\cos\phi + \cos\psi\sqrt{-1}$$

is unmeaning; while in plane analytical geometry the expression $x+y\sqrt{-1}$ indicates possibility *in space*. I may here observe, that if we regard space under a purely graphic aspect, and consider all lines drawn through a point as determined by their inclination to two fixed lines, then we have a strictly *two-dimensioned* science. With reference to the subject of space, I would add that "Symbolical Geometry" may be made to take an almost infinite

This will readily be verified by multiplication, or by substitution in the formulæ of page 437 of the last volume of this Journal. So, we might have considered the conditions requisite in order that the product

$$(w_1+i'x_1+j'y_1)(w_2+i'x_2+j'y_2)$$

may have the same form as its factors; and the same of other forms. These concluding remarks are not offered as continuations of the *general* development of the tessarine theory, but as individual instances not perhaps altogether unworthy of notice.

2 Church Yard Court, Temple,
May 8, 1849.

LX. *Remarkable Solar Halos and Mock Suns seen at the Observatory of* Henry Lawson, *Esq., F.R.S. &c., Lansdown Crescent, Bath. By* Edward Joseph Lowe, *Esq., F.R.A.S.*

[With a Plate.]

1849. FEBRUARY 12^d and 13^d. From $12^d\ 22\frac{1}{2}^h$ a halo, A, Plate I. fig. 1, was formed encircling the sun, S, apparently in a very thin vapour, for the sky was quite blue. All the morning there had been a dense ground-fog of some extent.

$23^h\ 30^m$. The horizontal diameter of the halo A 45° and its vertical diameter 47°. The portion of the heavens enclosed within this circle was many shades darker than that without, especially so near the circle, for it gradually became lighter near the sun. The sun was also surrounded with a burr, and had his rays carried out considerably, at times 20°. The inner edge of the halo was well-defined, but the outer edge very confused; the breadth about 1°.

$23^h\ 40^m$. Two mock suns, B and C, fig. 2, faintly visible, partaking of the same tint as the halo, viz. *pale yellow*, were formed in the edge of the halo A on the sun's horizontal level,

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variety of forms. Thus, let A, B, C, D be four points in space, then the equation

$$D-C=B-A \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (a.)$$

may be used to indicate either that the lines DC and BA are equal and *parallel*, as is done in the system of Sir W. R. Hamilton; or we may use (*a.*) to denote that those lines are equal and directed to some one point in space; and, in this latter system, (when the last-mentioned point is at an *infinite* distance,) we may be reconducted to the former one. And other systems might be devised, each geometrical system having probably a corresponding one in analysis, and *vice versâ*.

the one on the right and the other on the left side of the true sun.

$23^h\,41^m$. Mock sun C had disappeared.

$23^h\,44^m$. Mock sun B disappeared.

$23^h\,45^m$. From the part of the halo A where the mock sun C had appeared, there proceeded outwards an arc of a circle, D, which was apparently a segment of a circle whose diameter was about 90°. There were 30° of this arc visible.

$23^h\,46^m$. Another segment of a circle, E, of seemingly the same diameter as D, cut the circle A at 12° on either side the base. D had disappeared.

$23^h\,55^m$. E had vanished, and the circle A was indistinct; much linear cirri now appearing.

$13^d\,0^h\,10^m$. Circle A again brilliant, and the upper half of another circle, G, fig. 3, appeared, of 46° radius, which had the sun for its centre; it was colourless and 1° in breadth. There was about 140° of this circle visible. Clouds much thinner, the sky being now clear, with the exception of a very thin uniform haze.

$0^h\,15^m$. Mock sun B again just visible, and another, H, at the apex of the circle A, colourless.

$0^h\,20^m$. The phænomenon had vanished excepting the circle A.

$0^h\,35^m$. A indistinct. From this time until 1 o'clock no change, the halo being feeble for a time, and then brightening up again.

1^h. A curious feature now showed itself; within the circle A an ellipse, I, fig. 4, was formed, and immediately became brilliant; its horizontal diameter (measured in the centre of the ellipse) was 20° and its vertical diameter 30°; within this ellipse the sky was much brighter than that without; its lower edge blended with the solar burr; at its apex was a mock sun, K, and also the mock sun H again formed.

$1^h\,5^m$. The appearance had changed; mock sun K and the ellipse I gone; but a segment of a circle, L, fig. 5, of about 140° in diameter, rose from the sun S, and cut the circle A at its summit, and extended towards the north-west; 90° in length of this segment was plainly visible. Where it cut the circle A at H was a mock sun; also two other mock suns, M and N, were formed in the circle A at the distance of 8° on either side the vertex of this circle. All colourless. Sky becoming loaded with colourless cirri, but were less abundant near the phænomenon. Prospect, foggy; wind, west; temperature in shade, 43°; Franklin's hygrometer, 95; barometer, 30·15 inches.

$1^h\,9^m$. Another portion of a circle, O, fig. 6, of like dimensions with L, rose from the true sun, passed through the vertex of the circle A, where it cut the segment L, and stretched out towards the north-east. This was inverted with respect to L. 10° of a circle of large dimensions also cut the segment L at P. On the west horizon a few long muddy cirro-strati were just visible above the fog.

$1^h\ 15^m$. The circle A alone remained, the other portion of the phænomenon having disappeared soon after $1^h\ 10^m$.

$1^h\ 30^m$. More clouds; the halo A fainter; but another feature was at this hour traceable—a ring, Q, fig. 7, whose centre was a few degrees above the true sun, cut the circle A at R and the circle G at T; the vertical diameter was 70° and the horizontal diameter 55°. The circle A had become more elliptical; its vertical diameter was 48° and its horizontal diameter $44\frac{1}{2}$°.

$1^h\ 37^m$. Another change took place; the circle Q vanished, but there were two other segments of circles, U and W, fig. 8, of apparently 140° in diameter; these crossed each other at the point X, which was distant 70° from the sun. These segments both went eastward; there were again three mock suns visible, viz. M, N and C; the latter was bright but colourless, and had a tail of 8° in length, diminishing to a point, and proceeding diametrically opposite to the true sun.

$1^h\ 40^m$. All had vanished but the circle A.

$1^h\ 50^m$. Circle A disappeared. Sky becoming clear. Temperature, 44°; hygrometer, 94; wind, west and calm.

$1^h\ 55^m$. Faint portion of the circle A again formed above the true sun, and at this time prismatic. It finally disappeared at $2\frac{1}{4}^h$, and the sky became thinly and partially scattered over with clouds of cirrocumuli.

The width of all the circles was 1°.

A fine night, succeeded by a foggy morning and slight solar halo. On the 16th all day a prismatic solar halo, and at $3^h\ 25^m$ there were two prismatic mock suns formed on the horizontal level of the true sun in the halo of 45° diameter; these lasted 7 minutes.

It is well to add, that all the measurements were made with an admirable and at the same time simple instrument invented by Mr. Lawson for this purpose. It is hung in the observatory, and is ready for use at a moment's notice.

Observatory, Lansdown Crescent, Bath,
February 17th, 1849.

LXI. *On an Improvement in the Analysis of Equations.* By J. R. Young, *Professor of Mathematics in Belfast College**.

IN the analysis of numerical equations our chief difficulties are with those of an even degree, into which equations of an odd degree may always be changed. The following brief sketch of an improved method of discussing such equations will, I think, be acceptable to the readers of the Philosophical Magazine, although I reserve minute details, as well as some important extensions, for a future communication.

Let

$$x^{2n}+ax^{2n-1}+bx^{2n-2}+cx^{2n-3}+\dots+k=0 \quad . \quad . \quad \text{(A)}$$

be any equation of an even degree with numerical coefficients; then the left-hand member may be decomposed into the following pair of conjugate factors, namely,

$$x^n+\frac{1}{2}ax^{n-1}+\sqrt{\left\{\left(\frac{1}{4}a^2-b\right)x^{2n-2}-cx^{2n-3}-\dots-k\right\}},$$

and

$$x^n+\frac{1}{2}ax^{n-1}-\sqrt{\left\{\left(\frac{1}{4}a^2-b\right)x^{2n-2}-cx^{2n-3}-\dots-k\right\}};$$

and, consequently, if these be separately equated to zero, and either of the roots of the proposed equation substituted for x, one of these two new equations will always be satisfied. It follows, therefore, that when a *real* root is substituted, the expression under the radical, namely,

$$\left(\frac{1}{4}a^2-b\right)x^{2n-2}-cx^{2n-3}-\dots-k, \quad . \quad . \quad . \quad . \quad \text{(B)}$$

must always be *positive*; otherwise a real expression, viz. that which precedes the radical, would neutralize an imaginary one.

Hence, if we perform the partial analysis of the equation (A) by the method of Budan, commonly called the method of Fourier, we may discuss the doubtful intervals, which that analysis leaves for further examination, by an appeal to the inferior polynomial (B). Applying the before-mentioned analysis to this, in reference to the doubtful intervals, we should infer that whenever, throughout any such interval, (B) was plus, the sought roots may be real; and that when, throughout, the sign was minus, they must be imaginary. When the sign of (B) passed from plus to minus, or from minus to plus, in the interval, the doubt would remain; and

* Communicated by the Author.

our business would then be to take a portion of only the *positive* region of this interval, and to employ its limits in the corresponding interval of the original analysis: if two roots were still indicated, within these limits, we may suspect them to be real: if no roots be indicated, we must widen the positive interval in the second analysis: if the roots are real, we shall thus at length inclose them; but if no roots become indicated till we trench upon the negative portion of the interval in (B), we may be sure that the roots are imaginary.

At present I shall give but a single example in illustration. Let the equation be

$$x^4 - 4x^3 - 3x + 23 = 0,$$

the complete analysis of which, by the method of Fourier, is attended with a good deal of trouble. (See *Analyse des Equations*, p. 137.)

As in this example b is zero, the expression (B) is

$$4x^2 + 3x - 23;$$

and as the only doubtful interval, as ascertained by the partial analysis of Budan (ibid. p. 138), is the interval [1, 10], it is in reference to this alone that we have to examine the signs of the quadratic expression just written. Of this interval, the portion [1, 2], being negative, we reject it: taking therefore the interval [3, 10], which is wholly positive, and employing it, instead of the wider interval [1, 10], in Budan's analysis, a root is detected; and consequently, since no root can lie in the rejected region, the other root must lie in the interval [2, 3]. And thus the character and places of the roots are ascertained.

It is proper to add, that if the second term be absent from the proposed equation (A), then the rational part of each conjugate factor will be

$$x^n + \frac{1}{2} b x^{n-2},$$

and the expression under the radical will be

$$-cx^{2n-3} + \left(\frac{1}{4} b^2 - d\right) x^{2n-4} - \ldots - k,$$

to which all the preceding remarks apply.

I need scarcely observe, after what has been shown in my paper in the April Number of this Journal, that the conjugate factors here employed form but one pair, out of several that might be used for a similar purpose; although the forms here given will, as far as they go, be those most eligible; and I venture to think that, by our availing ourselves of them in the analysis of equations, considerable facilities will often be introduced into the numerical process. I shall only observe, in conclusion, that, by using other conjugate forms, it is easy to see how equations of an odd degree may be brought, in a direct manner, under the operation of the above method; or, without using any change of form at all, the method becomes immediately applicable upon introducing, into an equation of an odd degree, the new root $x=0$; or by supposing this to be done after the partial analysis of the equation in its original state.

But the general principle here sketched, admits of important modification. I shall hereafter show that, for an equation of the degree $2n$, the polynomial under the radical may always be reduced to the degree $n-1$; and that for an equation of the degree $2n+1$, it may be reduced to the degree $n+1$: so that the analysis of a biquadratic equation will depend on the examination of an expression of the first degree; that of an equation of the fifth or sixth degree, upon the examination of an expression of the second degree; and so on.

In what is stated above, I have alluded to certain circumstances of doubt, of which no mention is made in the general announcement in this last paragraph. I have done so because, in this brief sketch of the initial steps of the investigation in which I am engaged, no explicit guidance in these circumstances is actually furnished. My present purpose is simply to make known the path I am pursuing in an important inquiry; and to point to the results which I confidently expect to arrive at.

Belfast, May 12, 1849.

LXII. *On the Multiple Sounds of Bodies.* *By* M. Duhamel, *Member of the Institute**.

THE question under consideration is far from being new, and still it may be said to be as yet unsolved; that is to say, scientific men have not yet arrived at a common and uncontested opinion on this point. The object of the present memoir is to establish such an opinion.

What follows will perhaps appear so evident, and so little different from what is already known, that persons who have for a long time been too easily satisfied on this subject, may, whilst they accept my explanation, remain persuaded that they have never viewed the matter otherwise. I expect this, and consent, if they will have it so, that all my ideas and ex-

* From the *Annales de Chimie et de Physique* for January 1849.

periments are to be found in previous memoirs of physicists and geometers. All I ask is, that they should acknowledge that there can no longer be two opinions on this matter: I shall but have learnt one thing,—that all are agreed; that is enough for me.

The coexistence of several sounds emanating from one and the same vibrating body is undoubtedly one of the most remarkable phænomena of acoustics. Experience shows that sounds which may be produced singly by the same body may often be produced in it simultaneously. This phænomenon has given rise to very various explanations, none of which has obtained the complete assent of geometers and physicists. I propounded, in 1840, some new views on this subject; and the experiments which I made to confirm them appeared to throw some light on the question. I was however not entirely satisfied, and announced that my researches on this point were not terminated. The problem now appears to me completely solved. I have for several years been in possession of its solution. It seemed to me so natural, that I thought it would present itself to others besides myself; and hence, no doubt, the little eagerness I felt to call the attention of men of science to it. Perhaps I might have remained silent still longer, had not recent publications proved to me that there was still something to learn on this point.

And we shall remark first, that there can be no occasion to explain how we perceive several sounds at a time, any more than to explain how we experience at once several sensations of any other kind. Our aim should be to include the phænomenon in question in a more general class of recognized phænomena; but this is precisely what has not been sought by those who have studied the subject; it is what I have attempted for several years, and in this I think I have now succeeded.

I shall begin by recalling in a few words what had been said before me on this subject.

Father Mersenne, in his *Harmonie Universelle*, after refuting certain explanations that had been given of this phænomenon, sought to prove that it is produced by different reflexions of the air on the surface of the body which emits several sounds at a time. His theory not having been adopted, we shall abstain from stating it in detail, and shall pass to that which has obtained the assent of the greatest number of physicists.

This theory, first suggested by Sauveur, has been so much developed and extended by Daniel Bernouilli, that he is in some measure considered as its author. This illustrious philosopher, in his solution of the problem of vibrating strings, considers the initial figure of the string as formed by the superposition of an indefinite number of curves, having for their bases, some the entire length of the string, and others the half, the third, the quarter, &c. If these different curves were taken separately for the figure of the string, the fundamental note would be heard for the first, the octave for the second, the twelfth for the third, &c. Now analysis shows that if the ordinates of the initial figure were the sums of those which correspond to any number of these curves, the ordinates variable with the time would always be the sums of those which should correspond to them, at the same instant, in the partial motions answering to each of the curves, taken as initial figure. Thence Daniel Bernouilli concludes that the movement of each point being capable of being decomposed into a succession of others, which, if they existed singly, would give the sounds 1, 2, 3, 4, &c., all these sounds must necessarily be heard at once. He expresses himself as follows, in the particular case in which the octave alone is heard along with its fundamental sound:—"This absolute movement of the point D comprises really two periodical movements, one with relation to the point C, and the other with relation to the point B. The number of the first periodical returns will always be double that of the second. *The mind perceives each species of these periodical returns, and thence remarks two sounds, one of which is the octave of the other.*"

This rather subtile explanation, however, did not satisfy all geometers; and in reality it is not an explanation, since the fact in question is not referred to other admitted facts.

Lagrange well observed, that this decomposition of the movement was a purely geometrical conception, but one which proved nothing relative to the sound produced; he added, that this sound could alone result from the absolute movement, which was single. Daniel Bernouilli was much surprised at these objections, which in no degree altered his views. The following passage occurs in his memoir entitled *Recherches Physiques, Mécaniques, et Analytiques, sur le son, et sur les tons des tuyaux d'orgues*:—"I have given the explanation of this phænomenon with relation to strings in the *Mémoires de Berlin* of 1753. This explanation is so luminous and so self-evident, that the celebrated M. de Lagrange cannot have examined it with sufficient attention. Not content with rejecting it, he blames the learned M. Euler for having approved it. Let l be the length of the string, π the semicircle in the radius $=1$; can it be doubted that the equation

$$y=\alpha \sin\frac{\pi x}{l}+\beta \sin\frac{3\pi x}{l}$$

defines the state of a string which makes at the same time vibrations of the first and the third order, and which consequently gives at once the fundamental note and its twelfth? Why should the string rather give the fundamental sound than its twelfth; since on making $\alpha=0$ only the said twelfth will absolutely be heard, and making $\beta=0$, only the fundamental note will be heard?"

It must be acknowledged that this reasoning is little conclusive; for might it not be that a single sound should be heard, and be determined by the relation of α to β? and if several were heard, why should they be only those obtained when α or β are null?

The obscurity of all this reasoning was inevitable; it partakes of the faulty direction which this great philosopher followed: he tried to demonstrate directly that two sensations must be experienced at once, instead of seeking simply to refer the phænomenon to another. This reduction is the object which ought always to be proposed; and even when one class of phænomena is referred to another class as yet little known, science has always made progress, new relations being discovered, and instead of two difficulties, but one remaining. In the present case, the object cannot be, as we have already said, to explain the double sensation; we must only attempt to bring the phænomenon investigated into another class, in which this double sensation is admitted.

Struck by the want of solidity in the reasons alleged in favour of these various theories upon a question in itself so interesting, I endeavoured, as Lagrange required, to ascertain the absolute movement of the different points of the vibrating body. Reasonings applicable to every kind of body, followed by precise calculations relative to the simple case of strings, led me to a proposition which may be stated thus:—

When a body is made to vibrate by several causes which would produce separately the simple sounds which it can give, its surface generally divides itself into a certain finite number of parts, in each of which the vibrations have unequal durations. These different durations have relation to sounds corresponding to the different causes; and we are in the same position as if we had several separate surfaces, each having a peculiar movement of vibration.

In order to ascertain the truth of this proposition in the simple case of two coexistent sounds, I must first observe that if each of them existed singly, it would correspond to a different system of nodal lines. Now if the two causes which would produce each of these movements are made to act at the same time, or successively, a movement would result composed of those two movements, according to the general principle of superposition of small movements. Whence it follows that, in all the points of the nodal line of any one of these two movements, the other movement alone will be produced. But it is easy to ascertain, by calculation, that the neighbouring points of one of these lines will make an equal number of vibrations; and in passing, for example, from that in which the number of vibrations is greatest towards the other, there will be a certain number of vibrations the amplitude of which will successively diminish, and will become null when we pass the line of separation of the two regions related to each of the sounds: the number of the vibrations will then remain the same until we reach the limit of the region entered.

This reasoning shows clearly that the surface will divide generally into two or more parts, in each of which the number of vibrations will correspond to one of the two sounds; and it was very natural to conclude from thence that these two sounds would be heard as proceeding from different surfaces.

This proposition appeared entirely new when I stated it in 1840. And, in fact, in the works published up to that time, none but vague suppositions are to be found, such as are met with in the work of Father Mersenne, and to which he did not himself adhere; or again, some accidental result of calculation offered in particular examples, and from which moreover no induction was drawn similar to mine. I may even say that I did not remark these remote relations until a long time after I had demonstrated my general proposition, and upon a minute investigation of all that might have any analogy with it. I shall add lastly, that it was so little expected as to be at first disputed, principally by M. Savart, when I announced it as a theoretical consequence of the laws of motion; he even predicted to me that the experiments which I proposed to make to confirm it would not yield the result I expected. For my part, I had no doubt on the point; and I shall in a few words describe these experiments, confining myself to those which relate to plates or to bells, as the most easily performed, and more conclusive than those which relate to strings. I first took a square plate about two decimetres wide by four millimetres thick, fixed at the centre and free at all its other points. On moving the bow perpendicularly to the plane of the plate and at any one of its corners, the deepest sound was obtained, and the nodal lines were the two parallel to the sides, drawn through the centre. On the contrary, when the bow was placed in the middle of one of the sides, the plate quitting its state of rest, a sound was obtained rather higher than a fifth, and the nodal lines were the two diagonals of the plate.

If now, after producing one of these sounds, which continues for a considerable time, the bow is employed in the same manner as when it is used to produce the other sound when the plate is at rest, the two sounds are heard at once: the nodal lines disappear, as M. Savart had remarked, and as Daniel Bernouilli had already observed in the case of strings. There only remains to determine the relation of the number of vibrations executed in the same time by two points situated near each of the nodal lines, which only exist when each sound takes place separately. This I have done by means of the apparatus already mentioned; and I constantly found the relation of 31 or 32 to 45, as I ought to find for the two sounds, whose distance was a little less than a fifth.

I should add that another verification, less susceptible of accuracy, might be made. On bringing the ear close to one of the corners, scarcely any but the low note was heard; and, on the contrary, on bringing it near to the middle of one side, only the high note was heard; whence it appeared to follow, that the two sounds were produced by different parts of the plate.

I sought on the outline the point of separation of the parts related to each number of vibrations; but this point varies with the relation of the intensities of the two causes of vibration, and this inquiry is only of secondary interest.

I communicated these results to M. Savart, and he wished immediately to repeat the experiment with me, employing resonant tubes, which he put in unison, first with one sound and then with the other. On bringing one of the tubes in succession close to the different parts of the plate, the sound was considerably strengthened in some, and was not perceptibly in others: the first appeared therefore only to produce the sound of this tube; the second produced a similar effect with the second tube. These results perfectly agree with mine.

We finally considered the simultaneous sounds produced by a large bell, and thought that we perceived in the case of two, and even of three sounds, that each of them existed singly in the same part of the surface. We did not perform this experiment with the same degree of precision as the first, because the result presented itself naturally as it was expected.

Both the reasonings, therefore, and the experiments in question, led to the admission of this general law of the coexistence of sounds, that—

When one and the same vibrating surface simultaneously emits *several sounds, each of them exists in one or more finite parts of the surface, and appears to be perceptible there only; so that the ear is affected as it would be by several separate surfaces, which should each give one only of the sounds in question.*

In this memoir, resting on new facts, which I first arrived at theoretically and then demonstrated by experiment, I attempted to bring the phænomenon in question under another class of phænomena admitted without dispute, and which consists in our perceiving simultaneously the sounds produced by the vibrations of different points. These inductions were not disputed by any physicist. M. Poisson himself, who was much occupied with acoustics, raised no objection to them. It is to this same class of phænomena that I shall proceed now to refer those under consideration; but it will be by means of a more simple and at the same time more general theory, which will supply the voids and uncertainties which still existed, and which induced me to engage anew in this subject.

General explanation of the simultaneous sounds produced by a single body.

We admit that when several points of a medium have different vibratory movements, we hear, in general, the different sounds which each of them would give if it alone were in motion; and we propose to refer to this phænomenon that of the perception of several simultaneous sounds produced by a single point in motion. In other words, it has to be proved that our organs are sensibly affected in the same manner by several movements existing in distinct points of the surrounding medium, as by a single movement resulting from the composition of the several movements in a single point of this medium.

In the first place, when a point of the medium is not at a very small distance from the ear, its movement produces in all parts of our organ movements which do not differ perceptibly from those which would take place, if for the first point of the medium any other were substituted not very distant from it, and which was affected by the same movement. This is easily demonstrated by calculation and experiment.

This being settled, we know, by the principle of the superposition of small movements, that in any system of material points, homogeneous or not, but whose mutual actions depend only on their distances; if one or more of these points have movements resulting from the composition of several others, the displacement and velocity of every point of the system may be considered at every instant as the resultant of the composition of those which would be observed in them at the end of the same time, in the movements of the system corresponding to the different movements composing the first points.

But, according to the preceding remark, our organs will be affected in the same manner by the movement of a point of the medium, or by an identical movement attributed to another point near the first. Hence results the following proposition:—

When any point of the medium which surrounds us is affected by a movement resulting from the composition of several others, all parts of our organs are sensibly affected in the same manner as they would be if these different component movements, instead of being united in the same point, existed separately at different points of the first.

And reciprocally:—

If several points of a medium affected by different vibratory movements cause us to hear several sounds at once, it will suffice, in order that a single point of the medium should cause us to hear all these same sounds at a time, to give to this point the movement resulting from the composition of the former ones.

It is seen, then, as we stated, that the phænomenon of the multiplicity of the sounds which the same body gives, enters into another class of phænomena, that of the coexistence of the sounds produced by distinct bodies which simultaneously agitate the medium. It suffices, in fact, that the initial state of a sonorous body, with respect to the displacement of its molecules and the impressed velocities, be considered as resulting from the composition of several initial states corresponding to different simple sounds which it can produce, for all these sounds to be produced in us, by each of the points of the surface of this body.

It is possible, moreover, that one of these sounds may be produced more strongly than another in certain parts of the body, and even that there may be points in which it entirely predominates. These different circumstances will depend on the velocity in the different vibrations which are compounded at each point. In fact, since we receive the same impressions as if distinct points of the medium were respectively excited by these elementary movements, the simultaneous sounds which will proceed from the same point will have very different intensities, if the magnitude of the velocities is itself very different in the component vibrations. Experiment confirms this proposition; for, as I have already had occasion to say, when a body emits several sounds at a time, there are portions of its surface which seem to give only one sound, although we may convince ourselves, by particular processes, that they emit several others.

Experiments which confirm the preceding theory.

The theoretical considerations on which I have founded the explanation of the multiple sounds of bodies, seem to me incapable of giving rise to any difficulty; and the assumption cannot be denied, that the movement of a single point may produce the sensation of several sounds, as soon as we admit that this effect may result from the movement of several. Nevertheless I have thought it not uninteresting to demonstrate this fact experimentally. The first thing required was to find an accurate method for determining the sound given by each point of the surface of the vibrating body. I was at first obliged to give up that which I had already employed, to depict upon a disturbed plane the movement of the vibrating point, since the object was to verify a sensation; and it could not even have been affirmed that a movement which might have appeared to depict the same vibrations as when only one sound was heard, would not have contained some difference imperceptible to the eye, but which would have produced an effect sensible to the ear: it was therefore to be referred solely to this last sense. I tried different processes, the results of which were always attended with uncertainty, and I at last stopped at that which I shall proceed to describe and which is free from all error.

I shall first call to mind, that when a rod or an elastic wire, indefinitely extended in one direction, has its extremity subjected to any small movement, each of its points is affected successively with this same movement, which is propagated with a constant velocity. If the wire is of a definite length, this first movement is complicated with a second, which depends on the length of the wire, but is insensible with relation to the other; and experiment shows, in fact, that the only sound which the wire or the rod transmits, is that which corresponds to the vibrations communicated to its extremity.

Hence it results that an effectual mode of studying the proper movement of any point of the surface in a vibrating body, will be to fix to it one of the extremities of an elastic wire, to put the other extremity in communication with one ear, closing the other carefully, and preventing the sound reaching the first except by the medium of the wire. This is very easily accomplished, and it is easy also to verify its success. In fact, it is to be remarked, that the wire must be stretched for the sound to be perceptible: the entire wire can at pleasure be stretched, or that part can be left unstretched which is near the vibrating surface; and we find that in the first case a very distinct sound is heard, but none in the second.

This proves two important things; namely, first, that the sound which is heard is transmitted by the wire alone; and in the second place, that it only proceeds from the point where it is joined to the surface, and that the other parts of this surface do not act perceptibly upon it by the medium of the air; for if that were the case, we should still hear a sound when this wire was stretched throughout its whole length, except near the point where it is attached.

Once in possession of so simple and sure a process for ascertaining the sound emitted by any point of the surface of a vibrating body, I applied it to the investigation for which I had devised it, and the following are the results at which I have arrived.

I caused a square plate to vibrate so as to produce two sounds; I fixed the end of a thread of caoutchouc successively at different points of the surface, and I always heard the two sounds, satisfying myself that they were transmitted only by the wire; this took place even at the points where the geometrical influence of one of the movements was imperceptible. Whence it follows that each point of the plate emits the double sound, as the theory which I have explained had rigorously established; and they are distinguished by this method, even when one of them has become so feeble that it would no longer be heard through the medium of the air.

When the plate emitted three sounds, the wire still gave the sensation. Instead of a plate, bells, strings, or bodies of any form may be chosen, and the same fact will generally be observed. Nevertheless we may imagine such forms as that this law would be liable to exceptions, and not to hold good over the whole extent of the surface. It might be that the movement relative to one of the sounds would be weak in certain parts of the surface, that even though it existed there singly, it would make no sound heard; in this case, still more, it would not be heard if this movement were combined with another; and it will always be easily ascertained that the particular cases which, at first sight, would seem to constitute exceptions, are explained naturally by means of our principles.

I shall sum up the whole of this memoir by saying that I have established theoretically and experimentally the following proposition:—

If the vibratory motion of a point be decomposed into several others, the ear is perceptibly affected in the same manner by the movement of this point, as it would be by so many distinct points, each under the influence of one of these component motions.

The phænomenon of the multiplicity of sounds emitted by a single point is thus referred to that of the simultaneous audition of the sounds emitted by separate points. Being referred to an admitted phænomenon, it is explained; and I think I may say that it had not been completely explained before.

The conclusion of these researches is, therefore, that the phænomena of simultaneous perception of several sounds proceeding from the movement, whether of several points or of a single one, are only modifications of one general phænomenon, which may be stated in the following manner:—

"When our organ of hearing is affected by a movement that may be geometrically decomposed into several others, which, if they existed separately, would yield different sounds, we generally perceive all these sounds at the same time."

LXIII. *On Quaternions; or on a New System of Imaginaries in Algebra.* *By* Sir WILLIAM ROWAN HAMILTON, *LL.D., M.R.I.A., F.R.A.S., Corresponding Member of the Institute of France, &c., Andrews' Professor of Astronomy in the University of Dublin, and Royal Astronomer of Ireland.*

[Continued from p. 343.]

71. BEFORE entering on any discussion of this new form of the equation of the ellipsoid, namely the form

$$\mathrm{TV}\frac{\eta\rho-\rho\theta}{\mathrm{U}(\eta-\theta)}=\theta^2-\eta^2, \text{ eq. (139.), art. 70,}$$

it may be useful to point out another manner of arriving at the same equation of the ellipsoid, by a different process of calculation, from that construction or generation of the surface, as the locus of the circle which is the mutual intersection of a pair of equal spheres, sliding within two fixed cylinders of revolution whose axes intersect each other; while the right line, connecting the centres of the two sliding spheres, moves parallel to itself, or remains constantly parallel to a fixed right line in the plane of the fixed axes of the cylinders: which mode of generating the ellipsoid was published in the Philosophical Magazine for July 1848 (having also been communicated to the Royal Irish Academy in the preceding May), as a deduction from the Calculus of Quaternions. And whereas the fixed right line, through the centre of the ellipsoid, to which the line connecting the centres of the two sliding spheres is parallel, may have either of two positions, since it may coincide with either of the two cyclic normals, we shall here suppose it to have the direction of the cyclic normal ι, or shall consider the second pair of sliding spheres mentioned in article 64, of which the quaternion equations are, by article 62 (Phil. Mag. for July 1848),

$$\mathrm{T}(\rho-\mu)=\mathrm{T}(\rho-\lambda')=b. \quad (114.)$$

72. Here (see Phil. Mag. for May 1848), we have for μ the value,

$$\mu=h'(\kappa-\iota), \text{ eq. (91.), art. 57;}$$

and

$$\lambda'(\kappa'-\iota')=\kappa'\rho+\rho\kappa', \text{ eq. (110.), art. 60;}$$

also

$$\iota\kappa'=\iota'\kappa=\mathrm{T}.\iota\kappa, \text{ eq. (107), same article;}$$

whence we derive for λ' the expression,

$$\lambda'=\frac{\iota^{-1}\rho+\rho\iota^{-1}}{\iota^{-1}-\kappa^{-1}}=\frac{\iota\rho+\rho\iota}{\iota-\iota^2\kappa^{-1}}. \quad . \quad . \quad . \quad (140.)$$

But

$$(\iota-\iota^2\kappa^{-1})^{-1}=\{\iota(\kappa-\iota)\kappa^{-1}\}^{-1}=\kappa(\kappa-\iota)^{-1}\iota^{-1}; \quad . \quad (141.)$$

and by (104.),

$$\iota\rho+\rho\iota=-h'(\kappa-\iota)^2; \quad . \quad . \quad . \quad . \quad (142.)$$

therefore

$$\lambda'=-h'\kappa(\kappa-\iota)\iota^{-1}=h'(\kappa-\kappa^2\iota^{-1}). \quad . \quad . \quad (143.)$$

If then we make, for abridgement,

$$g=-h'\mathrm{T}\frac{\iota-\kappa}{\iota}, \quad . \quad . \quad . \quad . \quad . \quad (144.)$$

and employ the two new fixed vectors η and θ, defined by the equations (see Phil. Mag. for May 1849),

$$\eta=\mathrm{T}\iota\mathrm{U}(\iota-\kappa), \ \theta=\mathrm{T}\kappa\mathrm{U}(\kappa^{-1}-\iota^{-1}), \quad (131.)$$

which have been found to give

$$\iota-\kappa=\eta\mathrm{T}\frac{\iota-\kappa}{\iota}, \quad \kappa-\kappa^2\iota^{-1}=-\theta\mathrm{T}\frac{\iota-\kappa}{\iota}, \ (132.)$$

we shall have the values,

$$\mu=g\eta; \quad \lambda'=g\theta; \quad . \quad . \quad . \quad . \quad . \quad (145.)$$

and the lately cited equations (114.) of the two sliding spheres will become,

$$\mathrm{T}(\rho-g\eta)=b; \ \mathrm{T}(\rho-g\theta)=b; \quad . \quad . \quad . \quad (146.)$$

between which it remains to eliminate the scalar coefficient g, in order to find the equation of the ellipsoid, regarded as the locus of the circle in which the two spheres intersect each other.

73. Squaring the equations (146.), we find (by the general

rules of this Calculus) for the two sliding spheres the two following more developed equations:

$$\left.\begin{array}{l}0=b^2+\rho^2-2g\mathrm{S}.\eta\rho+g^2\eta^2;\\ 0=b^2+\rho^2-2g\mathrm{S}.\theta\rho+g^2\theta^2.\end{array}\right\} \quad . \quad . \quad . \quad (147.)$$

Taking then the difference, and dividing by g, we find the equation

$$g(\theta^2-\eta^2)=2\mathrm{S}.(\theta-\eta)\rho; \quad . \quad . \quad . \quad . \quad (148.)$$

which, relatively to ρ, is linear, and may be considered as the equation of the plane of the varying circle of intersection of the two sliding spheres; any one position of that plane being distinguished from any other by the value of the coefficient g. Eliminating therefore that coefficient g, by substituting in (146.) its value as given by (148.), we find that the equation of the ellipsoid, regarded as the locus of the varying circle, may be presented under either of the two following new forms:

$$\mathrm{T}\left(\rho-\frac{2\eta\mathrm{S}.(\theta-\eta)\rho}{\theta^2-\eta^2}\right)=b; \quad . \quad . \quad . \quad (149.)$$

$$\mathrm{T}\left(\rho-\frac{2\theta\mathrm{S}.(\eta-\theta)\rho}{\eta^2-\theta^2}\right)=b; \quad . \quad . \quad . \quad (150.)$$

respecting which two forms it deserves to be noticed, that either may be obtained from the other, by interchanging η and θ. And we may verify that these two last equations of the ellipsoid are consistent with each other, by observing that the semisum of the two vectors under the sign T is perpendicular to their semidifference (as it ought to be, in order to allow of those two vectors themselves having any common length, such as b); or that the condition of rectangularity,

$$\rho-\frac{(\theta+\eta)\mathrm{S}.(\theta-\eta)\rho}{\theta^2-\eta^2}\perp\theta-\eta, \quad . \quad . \quad . \quad (151.)$$

is satisfied: which may be proved by showing (see Phil. Mag. for July 1846) that the scalar of the product of these two last vectors vanishes, as in fact it does, since the identity

$$(\theta-\eta)(\theta+\eta)=\theta^2+\theta\eta-\eta\theta-\eta^2,$$

resolves itself into the two following formulæ:

$$\left.\begin{array}{l}\mathrm{S}.(\theta-\eta)(\theta+\eta)=\theta^2-\eta^2;\\ \mathrm{V}.(\theta-\eta)(\theta+\eta)=\theta\eta-\eta\theta;\end{array}\right\} \quad . \quad . \quad . \quad (152.)$$

of which the first is sufficient for our purpose. We may also verify the recent equations (149.) (150.) of the ellipsoid, by observing that they concur in giving the mean semiaxis b as the length $\mathrm{T}\rho$ of the radius of that diametral and circular section, which is made by the cyclic plane having for equation

$$\mathrm{S}.(\theta-\eta)\rho=0; \quad . \quad . \quad . \quad . \quad . \quad . \quad (153.)$$

this plane being found by the consideration that $\eta-\theta$ has the direction of the cyclic normal ι, or by making the coefficient $g=0$, in the formula (148.).

74. The equation (149.) of the ellipsoid may be successively transformed as follows:

$$\begin{aligned} b(\theta^2-\eta^2) &= \mathrm{T}\{(\theta^2-\eta^2)\rho-2\eta\mathrm{S}.(\theta-\eta)\rho\} \\ &= \mathrm{T}\{(\theta^2-\eta^2)\rho-\eta(\theta-\eta)\rho-\eta\rho(\theta-\eta)\} \\ &= \mathrm{T}\{\theta^2\rho-\eta(\theta\rho+\rho\theta)+\eta\rho\eta\} \\ &= \mathrm{TV}\{(\theta-\eta)\theta\rho-\eta\rho(\theta-\eta)\} \\ &= \mathrm{TV}.(\rho\theta-\eta\rho)(\theta-\eta) \\ &= \mathrm{TV}.(\eta\rho-\rho\theta)(\eta-\theta); \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (154.) \end{aligned}$$

and by a similar series of transformations, performed on the equation (150.), we find also (remembering that $\theta^2-\eta^2$, being equal to $\varkappa^2-\iota^2$, is positive),

$$b(\theta^2-\eta^2)=\mathrm{TV}.(\rho\eta-\theta\rho)(\eta-\theta). \quad . \quad . \quad . \quad (155.)$$

The same result (155.) may also be obtained by interchanging η and θ in either of the two last transformed expressions (154.), for the positive product $b(\theta^2-\eta^2)$; and we may otherwise establish the agreement of these recent results, by observing that, in general, if Q and Q′ be any two *conjugate quaternions* (see Phil. Mag. for July 1846), such as are here $\eta\rho-\rho\theta$ and $\rho\eta-\theta\rho$, and if α be any vector, then

$$\mathrm{TV}.\mathrm{Q}\alpha=\mathrm{TV}.\mathrm{Q}'\alpha; \quad . \quad . \quad . \quad . \quad . \quad . \quad (156.)$$

for

$$\left.\begin{aligned} \mathrm{V}.\mathrm{Q}\alpha &= \alpha\mathrm{SQ}-\mathrm{V}.\alpha\mathrm{VQ}, \\ \mathrm{V}.\mathrm{Q}'\alpha &= \alpha\mathrm{SQ}+\mathrm{V}.\alpha\mathrm{VQ}; \end{aligned}\right\} \quad . \quad . \quad . \quad (157.)$$

and because

$$0=\mathrm{S}.\alpha\mathrm{V}.\alpha\mathrm{VQ}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (158.)$$

the common value of the two members of the formula (156.) is

$$\mathrm{TV}.\mathrm{Q}\alpha=\sqrt{\{(\mathrm{TV}.\alpha\mathrm{VQ})^2+(\mathrm{T}\alpha.\mathrm{SQ})^2\}}. \quad . \quad (159.)$$

If then we substitute for b its value,

$$b=\mathrm{T}(\eta-\theta), \text{ eq. (135.), art. 70,}$$

and divide on both sides by this value of b, we see, from (154.), (155.), that the equation of the ellipsoid may be put under either of these two other forms:

$$\mathrm{TV}.(\eta\rho-\rho\theta)\mathrm{U}(\eta-\theta)=\theta^2-\eta^2, \quad . \quad . \quad . \quad (160.)$$

$$\mathrm{TV}.(\rho\eta-\theta\rho)\mathrm{U}(\eta-\theta)=\theta^2-\eta^2. \quad . \quad . \quad . \quad (161.)$$

But the versor of *every* vector is, in this calculus, a square root of negative unity; we have therefore in particular,

$$(\mathrm{U}(\eta-\theta))^2=-1; \quad . \quad . \quad . \quad . \quad . \quad (162.)$$

and under the sign TV, as under the sign T, it is allowed to divide by -1, without affecting the value of the tensor: it is therefore permitted to write the equation (160.) under the form

$$\mathrm{TV}.\frac{\eta\rho-\rho\theta}{\mathrm{U}(\eta-\theta)}=\theta^2-\eta^2, \quad (139.)$$

which form is thus demonstrated anew.

75. A few connected transformations may conveniently be noticed here. Since, for any quaternion Q,

$$(\mathrm{TVQ})^2=-(\mathrm{VQ})^2=(\mathrm{TQ})^2-(\mathrm{SQ})^2, \quad . \quad (163.)$$

while the tensor of a product is the product of the tensors, and the tensor of a versor is unity; and since

$$\mathrm{S}.(\rho\eta-\theta\rho)(\eta-\theta)=\mathrm{S}(\rho\eta^2-\rho\eta\theta-\theta\rho\eta+\theta\rho\theta)=-2\mathrm{S}.\eta\theta\rho, \quad (164.)$$

because

$$0=\mathrm{S}.\rho\eta^2=\mathrm{S}.\theta\rho\theta, \text{ and } \mathrm{S}.\rho\eta\theta=\mathrm{S}.\theta\rho\eta=\mathrm{S}.\eta\theta\rho; \quad . \quad (165.)$$

we have therefore, *generally*,

$$\left.\begin{array}{l}\mathrm{T}.(\rho\eta-\theta\rho)\mathrm{U}(\eta-\theta)=\mathrm{T}(\rho\eta-\theta\rho);\\ \mathrm{S}.(\rho\eta-\theta\rho)\mathrm{U}(\eta-\theta)=-2\mathrm{T}(\eta-\theta)^{-1}\mathrm{S}.\eta\theta\rho;\end{array}\right\} \quad . \quad (166.)$$

and there results the equation,

$$\mathrm{TV}.(\rho\eta-\theta\rho)\mathrm{U}(\eta-\theta)=\surd\{\mathrm{T}(\rho\eta-\theta\rho)^2-4\mathrm{T}(\eta-\theta)^{-2}(\mathrm{S}.\eta\theta\rho)^2\}, (167.)$$

as a general formula of transformation, valid for *any three vectors*, η, θ, ρ. We may also, by the general rules of the present calculus, write the last result as follows,

$$\mathrm{TV}.(\rho\eta-\theta\rho)\mathrm{U}(\eta-\theta)=\surd\{(\rho\eta-\theta\rho)(\eta\rho-\rho\theta)$$
$$+(\eta-\theta)^{-2}(\eta\theta\rho-\rho\theta\eta)^2\}; \quad . \quad . \quad . \quad . \quad . \quad (168.)$$

the signs S and T thus disappearing from the expression of the radical. For the ellipsoid, this radical, being thus equal to the left-hand member of the formula (167.), or to that of (168.), must, by (161.), receive the constant value $\theta^2-\eta^2$; so that, by squaring on both sides, we find as a new form of the equation (161.) of the ellipsoid, the following:

$$(\theta^2-\eta^2)^2=(\rho\eta-\theta\rho)(\eta\rho-\rho\theta)+(\eta-\theta)^{-2}(\eta\theta\rho-\rho\theta\eta)^2. \quad . \quad (169.)$$

Or, by a partial reintroduction of the signs S and T, we find this somewhat shorter form:

$$\mathrm{T}(\rho\eta-\theta\rho)^2+4(\eta-\theta)^{-2}(\mathrm{S}.\eta\theta\rho)^2=(\theta^2-\eta^2)^2. \quad . \quad (170.)$$

And instead of the square of the tensor of the quaternion $\rho\eta-\theta\rho$, we may write any one of several general expressions for that square, which will easily suggest themselves to those who have studied the transformations (already printed in this Magazine), of the earlier and in some respects simpler equation of the ellipsoid, proposed by the present writer, namely the equation

$$T(\iota\rho+\rho\kappa)=\kappa^2-\iota^2. \text{ eq. (9.), art. 38.}$$

For instance, we may employ any of the following general equalities, which all flow with little difficulty from the principles of the present calculus:

$$\begin{aligned} T(\rho\eta-\theta\rho)^2 &= T(\eta\rho-\rho\theta)^2 \\ &= (\rho\eta-\theta\rho)(\eta\rho-\rho\theta)=(\eta\rho-\rho\theta)(\rho\eta-\theta\rho) \\ &= (\eta^2+\theta^2)\rho^2-\rho\eta\rho\theta-\theta\rho\eta\rho \\ &= (\eta^2+\theta^2)\rho^2-\eta\rho\theta\rho-\rho\theta\rho\eta \\ &= (\eta+\theta)^2\rho^2-(\eta\rho+\rho\eta)(\theta\rho+\rho\theta) \\ &= (\eta^2+\theta^2)\rho^2-2S.\eta\rho\theta\rho \\ &= (\eta+\theta)^2\rho^2-4S.\eta\rho.S.\theta\rho \\ &= (\eta-\theta)^2\rho^2+4S(V.\eta\rho.V.\rho\theta); \quad \ldots \quad (171.) \end{aligned}$$

and which all hold good, independently of any relation between the three vectors η, θ, ρ.

76. As bearing on the last of these transformations it seems not useless to remark, that a general formula published in the Philosophical Magazine of August 1846, for any three vectors α, α', α'', namely the formula

$$\alpha S.\alpha'\alpha''-\alpha' S.\alpha''\alpha=V(V.\alpha\alpha'.\alpha''), \text{ eq. (12.) of art 22,}$$

which is found to be extensively useful, and indeed of constant recurrence in the applications of the calculus of quaternions, may be proved symbolically in the following way, which is shorter than that employed in the 23rd article:

$$\begin{aligned} V(V.\alpha\alpha'.\alpha'') &= \tfrac{1}{2}(V.\alpha\alpha'.\alpha''-\alpha''V.\alpha\alpha')=\tfrac{1}{2}(\alpha\alpha'.\alpha''-\alpha''.\alpha\alpha') \\ &= \tfrac{1}{2}\alpha(\alpha'\alpha''+\alpha''\alpha')-\tfrac{1}{2}(\alpha\alpha''+\alpha''\alpha)\alpha'=\alpha S.\alpha'\alpha''-\alpha' S.\alpha''\alpha. \quad (172.) \end{aligned}$$

The formula may be also written thus:

$$V.\alpha''V.\alpha'\alpha=\alpha S.\alpha'\alpha''-\alpha' S.\alpha\alpha''; \quad \ldots \quad (173.)$$

whence easily flows this other general and useful transformation, for the vector part of the product of any three vectors, α, α', α'':

$$V.\alpha''\alpha'\alpha=\alpha S.\alpha'\alpha''-\alpha' S.\alpha''\alpha+\alpha'' S.\alpha\alpha'. \quad . \quad (174.)$$

Operating on this by $S.\alpha'''$, we find, for the scalar part of the product of any *four* vectors, the expression:

$$S.\alpha'''\alpha''\alpha'\alpha=S.\alpha'''\alpha.S.\alpha'\alpha''-S.\alpha'''\alpha'.S.\alpha''\alpha+S.\alpha'''\alpha''.S.\alpha\alpha'. (175.)$$

But a quaternion, such as is $\alpha'\alpha$ or $\alpha'''\alpha''$, is always equal to the sum of its own scalar and vector parts; and the product of a scalar and a vector is a vector, while the scalar of a vector is zero: therefore

$$\alpha'\alpha = \mathrm{S}.\alpha'\alpha + \mathrm{V}.\alpha'\alpha, \quad \alpha'''\alpha'' = \mathrm{S}.\alpha'''\alpha'' + \mathrm{V}.\alpha'''\alpha'', \quad . \quad (176.)$$

and

$$\mathrm{S}.\alpha'''\alpha''\alpha'\alpha = \mathrm{S}.\alpha'''\alpha''.\mathrm{S}.\alpha'\alpha + \mathrm{S}(\mathrm{V}.\alpha'''\alpha''.\mathrm{V}.\alpha'\alpha). \quad (177.)$$

Comparing then (175.) and (177.), and observing that

$$\mathrm{S}.\alpha\alpha' = +\mathrm{S}.\alpha'\alpha, \quad \mathrm{V}.\alpha\alpha' = -\mathrm{V}.\alpha'\alpha, \quad . \ . \quad (178.)$$

we obtain the following general expression for the scalar part of the product of the vectors of any two binary products of vectors:

$$\mathrm{S}(\mathrm{V}.\alpha'''\alpha''.\mathrm{V}.\alpha'\alpha) = \mathrm{S}.\alpha'''\alpha.\mathrm{S}.\alpha'\alpha'' - \mathrm{S}.\alpha'''\alpha'.\mathrm{S}.\alpha''\alpha; \quad (179.)$$

while the vector part of the same product of vectors is easily found, by similar processes, to admit of being expressed in either of the two following ways (compare equation (3.) of article 24):

$$\begin{aligned}\mathrm{V}(\mathrm{V}.\alpha'''\alpha''.\mathrm{V}.\alpha'\alpha) &= \alpha'''\mathrm{S}.\alpha''\alpha'\alpha - \alpha''\mathrm{S}.\alpha'''\alpha'\alpha \\ &= \alpha\mathrm{S}.\alpha'''\alpha''\alpha' - \alpha'\mathrm{S}.\alpha'''\alpha''\alpha; \quad . \ . \ . \quad (180.)\end{aligned}$$

of which the combination conducts to the following general expression for any fourth vector α''', or ρ, in terms of any three given vectors α, α', α'', which are not parallel to any one common plane (compare equation (4.) of article 26):

$$\rho\mathrm{S}.\alpha''\alpha'\alpha = \alpha\mathrm{S}.\alpha''\alpha'\rho + \alpha'\mathrm{S}.\alpha''\rho\alpha + \alpha''\mathrm{S}.\rho\alpha'\alpha. \quad . \quad (181.)$$

If we further suppose that

$$\alpha'' = \mathrm{V}.\alpha'\alpha, \quad . \ . \ . \ . \ . \ . \ . \quad (182.)$$

we shall have

$$\mathrm{S}.\alpha''\alpha'\alpha = (\mathrm{V}.\alpha'\alpha)^2 = \alpha''^2; \quad . \ . \ . \quad (183.)$$

and after dividing by α''^2, the recent equation (181.) will become

$$\rho = \alpha\mathrm{S}\frac{\alpha'\rho}{\alpha''} + \alpha'\mathrm{S}\frac{\rho\alpha}{\alpha''} + \frac{\mathrm{S}.\alpha''\rho}{\alpha''}; \quad . \ . \ . \quad (184.)$$

whereby an arbitrary vector ρ may be expressed, in terms of any two given vectors α, α', which are not parallel to any common line, and of a third vector α'', perpendicular to both of them. And if, on the other hand, we change α, α', α'', α''' to θ, ρ, ρ, η, in the general formula (179.), we find that generally, for any three vectors η, θ, ρ, the following identity holds good:

$$\mathrm{S}(\mathrm{V}.\eta\rho.\mathrm{V}.\rho\theta) = \rho^2\mathrm{S}.\eta\theta - \mathrm{S}.\eta\rho.\mathrm{S}.\rho\theta; \quad . \quad (185.)$$

which serves to connect the two last of the expressions (171.), and enables us to transform either into the other.

77. To show the geometrical meaning of the equation (185.), let us divide it on both sides by $\mathrm{T}.\rho^2\eta\theta$; it then becomes, after transposing,

$$-\mathrm{SU}.\eta\theta=\mathrm{SU}.\eta\rho.\mathrm{SU}.\rho\theta+\mathrm{S}(\mathrm{VU}.\eta\rho.\mathrm{VU}.\rho\theta). \quad (186.)$$

Here, by the general principles of the geometrical interpretation of the symbols employed in this calculus (see the remarks in the Philosophical Magazine for July 1846), the symbol $\mathrm{SU}.\eta\theta$ is an expression for the cosine of the supplement of the angle between the two arbitrary vectors η and θ; and therefore the symbol $-\mathrm{SU}.\eta\theta$ is an expression for the cosine of that angle itself. In like manner, $-\mathrm{SU}.\eta\rho$ and $-\mathrm{SU}.\rho\theta$ are expressions for the cosines of the respective inclinations of those two vectors η and θ to a third arbitrary vector ρ; and at the same time $\mathrm{VU}.\eta\rho$ and $\mathrm{VU}.\rho\theta$ are vectors, of which the lengths represent the sines of the same two inclinations last mentioned, while they are directed towards the poles of the two positive rotations corresponding; namely the rotations from η to ρ, and from ρ to θ, respectively. The vectors $\mathrm{VU}.\eta\rho$ and $\mathrm{VU}.\rho\theta$ are therefore inclined to each other at an angle which is the supplement of the dihedral or spherical angle, subtended at the unit-vector $\mathrm{U}\rho$, or at its extremity on the unit-sphere, by the two other unit-vectors $\mathrm{U}\eta$ and $\mathrm{U}\theta$, or by the arc between their extremities: so that the scalar part of their product, in the formula (186.), represents the cosine of this spherical angle itself (and not of its supplement), multiplied into the product of the sines of the two sides or arcs upon the sphere, between which that angle is included. If then we denote the three sides of the spherical triangle, formed by the extremities of the three unit-vectors $\mathrm{U}\eta$, $\mathrm{U}\theta$, $\mathrm{U}\rho$, by the symbols, $\widehat{\eta\theta}$, $\widehat{\eta\rho}$, $\widehat{\rho\theta}$, and the spherical angle opposite to the first of them by the symbol $\widehat{\eta\rho\theta}$, the equation (186.) will take the form

$$\cos\widehat{\eta\theta}=\cos\widehat{\eta\rho}\cos\widehat{\rho\theta}+\sin\widehat{\eta\rho}\sin\widehat{\rho\theta}\cos\widehat{\eta\rho\theta}; \quad . \quad (187.)$$

which obviously coincides with the well-known and fundamental formula of spherical trigonometry, and is brought forward here merely as a verification of the consistency of the results of this calculus, and as an example of their geometrical interpretability.

A more interesting example of the same kind is furnished by the general formula (179.) for *four* vectors, which, when divided by the tensor of their product, becomes

$$\mathrm{S}(\mathrm{VU}.\alpha'''\alpha''.\mathrm{VU}.\alpha'\alpha)=\mathrm{SU}.\alpha'''\alpha.\mathrm{SU}.\alpha'\alpha''$$
$$-\mathrm{SU}.\alpha'''\alpha'.\mathrm{SU}.\alpha''\alpha; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (188.)$$

and signifies, when interpreted on the same principles, that

$$\sin \widehat{\alpha\alpha'} . \sin \widehat{\alpha''\alpha'''} . \cos (\widehat{\alpha\alpha'} \hat{} \widehat{\alpha''\alpha'''}) = \cos \widehat{\alpha\alpha''} . \cos \widehat{\alpha'\alpha'''}$$

$$- \cos \widehat{\alpha\alpha'''} . \cos \widehat{\alpha'\alpha''} ; \quad \ldots \ldots \quad (189.)$$

where the spherical angle between the two arcs from α to α' and from α'' to α''' may be replaced by the interval between the poles of the two positive rotations corresponding. The same result may be otherwise stated as follows: If L, L', L'', L''', denote any four points upon the surface of an unit-sphere, and A the angle which the arcs LL', $L''L'''$ form where they meet each other, (the arcs which include this angle being measured in the directions of the progressions from L to L', and from L'' to L''' respectively,) then the following equation will hold good:

$$\cos LL'' . \cos L'L''' - \cos LL''' . \cos L'L''$$

$$= \sin LL' . \sin L''L''' . \cos A. \quad \ldots \quad (190.)$$

Accordingly this last equation has been incidentally given, as an auxiliary theorem or lemma, at the commencement of those profound and beautiful researches, entitled *Disquisitiones Generales circa Superficies Curvas*, which were published by Gauss at Göttingen in 1828. That great mathematician and philosopher was content to prove the last written equation by the usual formulæ of spherical and plane trigonometry; but, however simple and elegant may be the demonstration thereby afforded, it appears to the present writer that something is gained by our being able to present the result (190.) or (189.), under the form (188.) or (179.), as an identity in the quaternion calculus. In general, all the results of plane and spherical trigonometry take the form of *identities* in this calculus; and their expressions, when so obtained, are associated with a reference to *vectors*, which is usually suggestive of *graphic* as well as *metric* relations.

78. Since

$$\rho\eta - \theta\rho = \mathrm{S}.\rho(\eta - \theta) + \mathrm{V}.\rho(\eta + \theta), \quad \ldots \quad (191.)$$

the quaternion $\rho\eta - \theta\rho$ gives a pure vector as a product, or as a quotient, if it be multiplied or divided by the vector $\eta + \theta$ (compare article 68); we may therefore write

$$\rho\eta - \theta\rho = \lambda_1(\eta + \theta), \quad \ldots \ldots \quad (192.)$$

λ_1 being a new vector-symbol, of which the value may be thus expressed:

$$\lambda_1 = \rho - 2(\eta + \theta)^{-1}\mathrm{S}.\theta\rho. \quad \ldots \ldots \quad (193.)$$

The equation (192.) will then give,

$$\left.\begin{array}{l} T(\rho\eta-\theta\rho)=T\lambda_1.T(\eta+\theta); \\ T(\rho\eta-\theta\rho)^2=\lambda_1^2(\eta+\theta)^2. \end{array}\right\} \quad . \quad . \quad . \quad (194.)$$

We have also the identity,

$$(\theta^2-\eta^2)^2=(\eta-\theta)^2(\eta+\theta)^2+(\eta\theta-\theta\eta)^2; \quad . \quad . \quad (195.)$$

which may be shown to be such, by observing that

$$\begin{aligned}(\eta-\theta)^2(\eta+\theta)^2&=(\eta^2+\theta^2-2S.\eta\theta)(\eta^2+\theta^2+2S.\eta\theta)\\ &=(\eta^2+\theta^2)^2-4(S.\eta\theta)^2=(\eta^2-\theta^2)^2+4(T.\eta\theta)^2-4(S.\eta\theta)^2\\ &=(\eta^2-\theta^2)^2-4(V.\eta\theta)^2=(\theta^2-\eta^2)^2-(\eta\theta-\theta\eta)^2; \quad . \quad . \quad (196.)\end{aligned}$$

or by remarking that (see equations (152.)(163.)),

$$\left.\begin{array}{l} \eta^2-\theta^2=S.(\eta-\theta)(\eta+\theta), \quad \eta\theta-\theta\eta=V.(\eta-\theta)(\eta+\theta), \\ \text{and } (\eta-\theta)^2(\eta+\theta)^2=(T.(\eta-\theta)(\eta+\theta))^2; \end{array}\right\} \quad (197.)$$

or in several other ways. Introducing then a new vector ε, such that

$$\eta\theta-\theta\eta=\varepsilon T(\eta+\theta), \text{ or, } \varepsilon=2V.\eta\theta.T(\eta+\theta)^{-1}; \quad . \quad (198.)$$

and that therefore

$$(\eta\theta-\theta\eta)^2=-\varepsilon^2(\eta+\theta)^2, \quad . \quad . \quad . \quad . \quad (199.)$$

and

$$2S.\eta\theta\rho=S.\varepsilon\rho.T(\eta+\theta), \quad 4(S.\eta\theta\rho)^2=-(S.\varepsilon\rho)^2(\eta+\theta)^2; \quad (200.)$$

while, by (135.),

$$T(\eta-\theta)=b, \quad (\eta-\theta)^2=-b^2; \quad . \quad . \quad . \quad (201.)$$

we find that the equation (170.) of the ellipsoid, after being divided by $(\eta+\theta)^2$, assumes the following form:

$$\lambda_1^2+b^{-2}(S.\varepsilon\rho)^2+b^2+\varepsilon^2=0. \quad . \quad . \quad . \quad (202.)$$

But also, by (193.), (198.),

$$S.\varepsilon\lambda_1=S.\varepsilon\rho; \quad . \quad . \quad . \quad . \quad . \quad . \quad (203.)$$

the equation (202.) may therefore be also written thus:

$$0=(\lambda_1-\varepsilon)^2+(b+b^{-1}S.\varepsilon\rho)^2; \quad . \quad . \quad . \quad (204.)$$

and the scalar $b+b^{-1}S.\varepsilon\rho$ is positive, even at an extremity of the mean axis of the ellipsoid, because, by (195.) (199.) (201.), we have

$$(\theta^2-\eta^2)^2=-(b^2+\varepsilon^2)(\eta+\theta)^2=(b^2-T\varepsilon^2)T(\eta+\theta)^2, \quad . \quad (205.)$$

and therefore

$$T\varepsilon<b. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (206.)$$

We have then this new form of the equation of the ellipsoid, deduced by transposition and extraction of square roots (according to the rules of the present calculus), from the form (204.):

$$T(\lambda_1-\epsilon)=b+b^{-1}\,S.\epsilon\rho. \quad . \quad . \quad . \quad . \qquad (207.)$$

By a process exactly similar to the foregoing, we find also the form

$$T(\lambda_1+\epsilon)=b-b^{-1}\,S.\epsilon\rho; \quad . \quad . \quad . \qquad (208.)$$

which differs from the equation last found, only by a change of sign of the auxiliary and constant vector ϵ: and hence, by addition of the two last equations, we find still another form, namely,

$$T(\lambda_1-\epsilon)+T(\lambda_1+\epsilon)=2b; \quad . \quad . \quad . \qquad (209.)$$

or substituting for λ_1, ϵ, and b their values, in terms of η, θ, and ρ, and multiplying into $T(\eta+\theta)$,

$$T\left(\frac{\rho\eta-\theta\rho}{U(\eta+\theta)}-2V.\eta\theta\right)+T\left(\frac{\rho\eta-\theta\rho}{U(\eta+\theta)}+2V.\eta\theta\right)$$
$$=2T.(\eta-\theta)(\eta+\theta). \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (210.)$$

79. The locus of the termination of the auxiliary and variable vector λ_1, which is *derived* from the vector ρ of the original ellipsoid by the *linear* formula (193.), is expressed or represented by the equation (209.); it is therefore evidently a certain *new* ellipsoid, namely an *ellipsoid of revolution*, which has the mean axis $2b$ of the old or given ellipsoid for its major axis, or for its axis of revolution, while the vectors of its two *foci* are denoted by the symbols $+\epsilon$ and $-\epsilon$. If a denote the greatest, and c the least semiaxis, of the original ellipsoid, while b still denotes its mean semiaxis, then, by what has been shown in former articles, we have the values,

$$T\eta=T\iota=\frac{1}{2}(a+c); \quad T\theta=T\kappa=\frac{1}{2}(a-c); \quad . \qquad (211.)$$

and consequently (compare the note to art. 70),

$$a=T\eta+T\theta; \quad c=T\eta-T\theta; \quad . \quad . \quad . \qquad (212.)$$

therefore

$$ac=T\eta^2-T\theta^2=\theta^2-\eta^2; \quad . \quad . \quad . \quad . \qquad (213.)$$

also

$$T(\eta+\theta)^2+b^2=-(\eta+\theta)^2-(\eta-\theta)^2=-2\eta^2-2\theta^2$$
$$=2T\eta^2+2T\theta^2=(T\eta+T\theta)^2+(T\eta-T\theta)^2, \qquad (214.)$$

and

$$T(\eta+\theta)^2=a^2-b^2+c^2; \quad . \quad . \quad . \quad . \qquad (215.)$$

whence, by (205.),

$$T\epsilon^2=b^2-\frac{a^2c^2}{a^2-b^2+c^2}=\frac{(a^2-b^2)(b^2-c^2)}{a^2-b^2+c^2}. \quad . \qquad (216.)$$

Such, then, is the expression for the square of the distance of either focus of the new or derived ellipsoid of revolution, which has λ_1 for its varying vector, from the common centre of the new and old ellipsoids, which centre is also the common origin of the vectors λ_1 and ρ: while these two foci of the new ellipsoid are situated upon the mean axis of the old one. There exist also other remarkable relations, between the original ellipsoid with three unequal semiaxes a, b, c, and the new ellipsoid of revolution, of which some will be brought into view, by pursuing the quaternion analysis in a way which we shall proceed to point out.

80. The geometrical construction already mentioned (in articles 64, 71, &c.), of the original ellipsoid as the locus of the circle in which two sliding spheres intersect, shows easily (see art. 72) that the scalar coefficient g, in the equations (146.) of that pair of sliding spheres, becomes equal to the number 2, at one of those limiting positions of the pair, for which, after cutting, they *touch*, before they cease to meet each other. In fact, if we thus make

$$g=2, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (217.)$$

the values (145.) of the vectors of the centres will give, for the interval between those two centres of the two sliding spheres, the expression

$$T(\mu-\lambda')=gT(\eta-\theta)=2b; \quad . \quad . \quad . \quad . \quad (218.)$$

this interval will therefore be in this case equal to the diameter of either sliding sphere, because it will be equal to the mean axis of the ellipsoid: and the two spheres will touch one another. Had we assumed a value for g, less by a very little than the number 2, the two spheres would have cut each other in a very small circle, of which the circumference would have been (by the construction) entirely contained upon the surface of the ellipsoid; and the plane of this little circle would have been parallel and very near to that other plane, which was the common tangent plane of the two spheres, and also of the ellipsoid, when g received the value 2 itself. It is clear, then, that this value 2 of g corresponds to an *umbilicar point* on the ellipsoid; and that the equation

$$S.(\theta-\eta)\rho=\theta^2-\eta^2, \quad . \quad . \quad . \quad . \quad (219.)$$

which is obtained from the more general equation (148.) of the plane of a circle on the ellipsoid, by changing g to 2, represents an *umbilicar tangent plane*, at which the normal has the direction of the vector $\eta-\theta$. Accordingly it has been seen that this last vector has the direction of the cyclic normal ι; in fact, the expressions (131.), for η and θ in terms of ι and

κ, give conversely these other expressions for the latter vectors in terms of the former,

$$\iota = \mathrm{T}\eta\,\mathrm{U}(\eta-\theta); \quad \kappa = \mathrm{T}\theta\,\mathrm{U}(\theta^{-1}-\eta^{-1}): \quad . \quad (220.)$$

whence (it may here be noted) follow the two parallelisms,

$$\mathrm{U}\iota - \mathrm{U}\kappa = \mathrm{U}(\eta-\theta) + \mathrm{U}(\eta^{-1}-\theta^{-1}) \parallel \mathrm{U}\eta + \mathrm{U}\theta; \quad . \quad (221.)$$

$$\mathrm{U}\iota + \mathrm{U}\kappa = \mathrm{U}(\eta-\theta) - \mathrm{U}(\eta^{-1}-\theta^{-1}) \parallel \mathrm{U}\eta - \mathrm{U}\theta; \quad . \quad (222.)$$

the members of (221.) having each the direction of the greatest axis of the ellipsoid, and the members of (222.) having each the direction of the least axis; as may easily be proved, for the first members of these formulæ, by the construction with the *diacentric sphere*, which was communicated by the writer to the Royal Irish Academy in 1846, and was published in the present Magazine in the course of the following year. The equation (219.) may be verified by observing that it gives, for the length of the perpendicular let fall from the centre of the ellipsoid on an umbilicar tangent plane, the expression

$$p = (\theta^2-\eta^2)\mathrm{T}(\eta-\theta)^{-1} = acb^{-1}; \quad . \quad . \quad . \quad (223.)$$

agreeing with known results. And the vector ω of the umbilicar point itself must be the semisum of the vectors of the centres of the two equal and sliding spheres, in that limiting position of the pair in which (as above) they touch each other; this *umbilicar vector* ω is therefore expressed as follows:

$$\omega = \eta + \theta; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (224.)$$

because this is the semisum of μ and λ' in (145.), or of $g\eta$ and $g\theta$ when $g=2$. (Compare the note to article 70.) As a verification, we may observe that this expression (224.) gives, by (215.), the following known value for the length of an umbilicar semidiameter of the ellipsoid,

$$u = \mathrm{T}\omega = \mathrm{T}(\eta+\theta) = \sqrt{(a^2-b^2+c^2)}. \quad . \quad . \quad (225.)$$

By similar reasonings it may be shown that the expression

$$\omega' = \mathrm{T}\eta\,\mathrm{U}\theta + \mathrm{T}\theta\,\mathrm{U}\eta, \quad . \quad . \quad . \quad . \quad (226.)$$

which may also be thus written, (see same note to art. 70,)

$$\omega' = -\mathrm{T}.\eta\theta.(\eta^{-1}+\theta^{-1}), \quad . \quad . \quad . \quad . \quad (227.)$$

represents *another* umbilicar vector; in fact, we have, by (224.) and (226.),

$$\mathrm{T}\omega' = \mathrm{T}\omega, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (228.)$$

and

$$\left.\begin{array}{l} \omega+\omega' = (\mathrm{T}\eta+\mathrm{T}\theta)(\mathrm{U}\eta+\mathrm{U}\theta), \\ \omega-\omega' = (\mathrm{T}\eta-\mathrm{T}\theta)(\mathrm{U}\eta-\mathrm{U}\theta); \end{array}\right\} \quad . \quad . \quad . \quad (229.)$$

so that the vectors ω ω' are equally long, and the angle between them is bisected by $U\nu+U\theta$, or (see (221.)) by the axis major of the ellipsoid; while the supplementary angle between ω and $-\omega'$ is bisected by $U\eta-U\theta$, or (as is shown by (222.)) by the axis minor. It is evident that $-\omega$ and $-\omega'$ are also umbilicar vectors; and it is clear, from what has been shown in former articles, that the vectors η and θ have the directions of the axes of the two cylinders of revolution, which can be circumscribed about that given or original ellipsoid, to which all the remarks of the present article relate.

81. These remarks being premised, let us now resume the consideration of the variable vector λ_1, of art. 78, which has been seen to terminate on the surface of a certain derived ellipsoid of revolution. Writing, under a slightly altered form, the expression (193.) for that vector λ_1, and combining with it three other analogous expressions, for three other vectors, λ_2, λ_3, λ_4, as follows,

$$\left.\begin{array}{ll} \lambda_1=\dfrac{\rho\eta-\theta\rho}{\eta+\theta}; & \lambda_2=\dfrac{\rho\theta-\eta\rho}{\eta+\theta}; \\ \lambda_3=\dfrac{\rho\theta^{-1}-\eta^{-1}\rho}{\eta^{-1}+\theta^{-1}}; & \lambda_4=\dfrac{\rho\eta^{-1}-\theta^{-1}\rho}{\eta^{-1}+\theta^{-1}}; \end{array}\right\} \quad (230.)$$

it is easy to prove that

$$T\lambda_1=T\lambda_2=T\lambda_3=T\lambda_4; \quad . \quad . \quad . \quad . \quad (231.)$$

and that

$$S.\eta\theta\lambda_1=S.\eta\theta\lambda_2=S.\eta\theta\lambda_3=S.\eta\theta\lambda_4=S.\eta\theta\rho; \quad . \quad (232.)$$

whence it follows that the four vectors λ_1, λ_2, λ_3, λ_4, being supposed to be all drawn from the centre A of the original ellipsoid, terminate in four points, L_1, L_2, L_3, L_4, which are the corners of a quadrilateral inscribed in a circle of the derived ellipsoid of revolution; the plane of this circle being parallel to the plane of the greatest and least axes of the original ellipsoid, and passing through the point E of that ellipsoid, which is the termination of the vector ρ. We shall have also the equations,

$$\frac{\lambda_2-\rho}{\lambda_1-\rho}=\frac{S.\eta\rho}{S.\theta\rho}=V^{-1}0; \quad \frac{\lambda_3-\rho}{\lambda_4-\rho}=\frac{S.\eta^{-1}\rho}{S.\theta^{-1}\rho}=V^{-1}0; \quad (233.)$$

which show that the two opposite sides L_1L_2, L_3L_4, of thisinscribed quadrilateral, being prolonged if necessary, intersect in the lately-mentioned point E of the original ellipsoid. And because the expressions (230.) give also

$$V\frac{\lambda_2-\lambda_1}{\eta+\theta}=0, \quad V\frac{\lambda_4-\lambda_3}{\eta^{-1}+\theta^{-1}}=0, \quad . \quad . \quad (234.)$$

these opposite sides L_1L_2, L_3L_4, of the plane quadrilateral thus inscribed in a circle of the derived ellipsoid of revolution, are parallel respectively to the vectors $\eta+\theta$, $\eta^{-1}+\theta^{-1}$, or to the two umbilicar vectors ω, ω', of the original ellipsoid, with the semiaxes abc. At the same time, the equations

$$V\frac{\lambda_3-\lambda_2}{\eta}=0, \quad V\frac{\lambda_1-\lambda_4}{\theta}=0, \quad . \quad . \quad . \quad (235.)$$

hold good, and show that the two other mutually opposite sides of the same inscribed quadrilateral, namely the sides L_2L_3, L_4L_1, are respectively parallel to the two vectors η, θ, or to the axes of the two cylinders of revolution which can be circumscribed about the same original ellipsoid. Hence it is easy to infer the following theorem, which the author supposes to be new:—*If on the mean axis 2b of a given ellipsoid, abc, as the major axis, and with two foci* F_1, F_2, *of which the common distance from the centre* A *is*

$$\overline{AF_1}=\overline{AF_2}=e=\frac{\sqrt{(a^2-b^2)}\sqrt{(b^2-c^2)}}{\sqrt{(a^2-b^2+c^2)}}, \quad . \quad . \quad (236.)$$

we construct an ellipsoid of revolution; and if, in any circular section of this new ellipsoid, we inscribe a quadrilateral, $L_1L_2L_3L_4$, *of which the two opposite sides* L_1L_2, L_3L_4 *are respectively parallel to the two umbilicar diameters of the given ellipsoid; while the two other and mutually opposite sides,* L_2L_3, L_4L_1, *of the same inscribed quadrilateral, are respectively parallel to the axes of the two cylinders of revolution which can be circumscribed about the same given ellipsoid; then the point of intersection* E *of the first pair of opposite sides* (namely of those parallel to the umbilicar diameters), *will be a point upon that given ellipsoid.* It seems to the present writer that, in consequence of this remarkable relation between these two ellipsoids, the two foci F_1, F_2, of the above described ellipsoid of revolution, which have been seen to be situated upon the mean axis of the original ellipsoid, of which the three unequal semiaxes are denoted by a, b, c, may be not inconveniently called the TWO MEDIAL FOCI of that original ellipsoid: but he gladly submits the question of the propriety of such a designation, to the judgement of other and better geometers. Meanwhile it may be noticed that the two ellipsoids intersect each other in a system of two ellipses, of which the planes are perpendicular to the axes of the two cylinders of revolution above mentioned; and that those four common tangent planes of the two ellipsoids, which are parallel to their common axis, that is to the mean axis of the original ellipsoid abc, are parallel also to its two umbilicar diameters.

[To be continued.]

LXIV. *Further Researches on Electro-Physiology.* *By* M. Ch. Matteucci*.

I HOPE that the Academy, which has always been pleased to encourage me in my researches upon electro-physiology, will permit me to communicate some new investigations upon this subject. I cannot commence the exposition of these researches without very briefly recapitulating the four principal points from which I started, and which, to a certain extent, form a summary of the whole of my former labours.

1. In each cell of the electric organ of fishes, the two electricities become separated under the influence of the nervous action propagated from the brain towards the extremities of the nerves. A relation exists between the direction and the intensity of the nervous current, and the position and the quantity of the two electricities developed in the cell. In accordance with this relation which has been established experimentally, if, as was done by Ampère in the case of electro-magnetic action, we represent the nervous current by a man lying extended upon the nerve, and with his face turned towards the caudal extremity of the Torpedo or the dorsal surface of the Gymnotus, the positive electricity of the cell always exists on the left of the man: since each cell of the organ forms a temporary electric apparatus, this explains the position of the poles at the extremities of the prisms, and the intensity of the discharge being proportional to the length of the prisms, as established by experiment.

2. It has been shown by experiment, that the greatest analogy exists between the discharge of electric fishes and muscular contraction. There is no circumstance which modifies one of these phænomena which does not act equally upon the other.

3. The contraction of a muscle developes in a nerve which is in contact with this muscle, the cause by which the nerve excites contractions in the muscles through which it ramifies. Although experiment has not yet enabled us to decide whether this phænomenon is an instance of nervous induction, or a proof of an electric discharge developed by muscular contraction, we are led by all analogy to admit the second hypothesis.

4. The electric current modifies the excitability of the nerve according to its direction. The electric current, when propagated in the direction of the ramification of the nerve, destroys its excitability; when propagated in a contrary direction to the ramification, it augments the excitability of the nerve. The phænomena brought into play by the cessation

* From the *Comptes Rendus* for April 30, 1849.

of an electric current traversing the nerves of an animal, depend upon the modification which the excitability of the nerve has experienced by the passage of the current, according to its direction. The same cause explains the voltaic alternations, *i. e.* the muscular contractions excited by a current, which is made to traverse a nerve in a contrary direction to that in which it had ceased to produce any effect.

In this first extract I shall confine myself to communicating to the Academy a result which I regard as fundamental to the theory of electro-physiological phænomena. By a very simple experiment, and one which is easily repeated, I have shown that an electric current which traverses a muscular mass in the direction of its fibres, and consequently in a direction which is normal or oblique to that of the ultimate nervous ramifications which are distributed through it, developes in these filaments a nervous current, the direction of which varies according to that of the electric current, relatively to the ramification of the nerve. This law is the same as that which establishes the relation between the direction of the nervous current and the position of the contrary electric conditions in the organ of electric fishes; in other words, it is the reaction of electricity upon the nervous force. In discovering a new analogy, and that the most intimate possible, between the electric discharge of fishes and muscular contraction, I have shown that, just as in the electric apparatus of the torpedo, the nervous current developes the two electricities in a determinate direction, according to its own direction. In a muscular mass the two electric states, diffused through the elements of its fibres, produce a current, the direction of which, varying with that of the electric current, is established, like the direction of the discharge in the torpedo, by that of the nervous current which excites it. I have taken every pains to establish by experiment this result, which I shall henceforth consider as the foundation of the theory of electro-physiological phænomena. Whatever may be the nature of the nervous force, of which we are ignorant, as of that of the other great natural agents, it is a fact that this force is propagated in the nerves sometimes from the brain to the extremities, sometimes in a contrary direction. It is entirely independent of hypothesis, and, in fact, in accordance with experiment to admit, that in the act of muscular contraction excited by the action of the will or by the stimulation of the nerve, a nervous current is propagated in the direction of the ramification of the nerve: on the other hand, the nervous current follows an opposite direction, when sensation is experienced by the stimulation of the extremities of the nerve.

I have already shown in my former researches, and by direct experiments, the great difference between the nervous and the muscular substance as regards the conduction of the electric current. Regarding these experiments, which it would be impossible for me to describe here in detail, I shall confine myself to the account of one, the evidence afforded by which is perfect, and which may be applied to the case in point. This experiment consists in introducing the nerve of a very sensitive galvanoscopic frog into the interior of a muscular mass, cut with a knife in the direction of its fibres. On passing a tolerably strong electric current through the muscular mass, contractions are never excited in the prepared frog. In this case, besides the better conductibility of the muscular substances, we have for the production of the effect observed the great difference between the relative mass of the muscle and of the nerve. It is unnecessary to state, that the contraction of the prepared frog occurs if the poles of the battery are closely approximated to its nerve, or if the muscular mass, by its contractions, produces the phænomenon called *induced contraction.* The experiment succeeds perfectly on taking the muscles of one of the mammalia or a bird, after their irritability has ceased; so that the passage of an electric current through these muscles does not excite any sensible contraction.

It is then proved by experiment, that when a muscular mass is traversed by an electric current, the nervous filaments diffused through the mass do not produce any sensible part of this current, so that the effects obtained can only be due to the direct action of the electric current upon the muscular fibre, and to the indirect action or the *influence* of the electric current upon the nervous force.

The following are these effects:—If, in a living rabbit, dog or frog, we expose the muscles of the legs, by entirely removing the integuments, and pass an electric current from a pile of thirty or forty elements through these muscles, applying one of the poles to the upper and the other to the lower part of the leg—if the positive pole is placed above and the negative pole below, so that the electric current traverses the muscular substance in the direction of the ramification of the nerves, a very powerful contraction is produced, not only in the muscles of the leg, but also in those of the foot. If the current is passed in the contrary direction, the animal cries out from pain, the contraction is much less, and only occurs in that muscle which is traversed by the current.

On repeating these experiments many times and upon different animals, which I have taken care to do, we readily discriminate the principal results which I have described from those slight modifications which sometimes occur, especially at the commencement of the experiment.

These results can only be explained in one way. The very powerful contraction excited in the muscles of the leg and in the foot by the passage of the electric current, proves the existence of a nervous current propagated from the extremities towards the centre, and developed under the influence of an electric current which traverses the muscular mass in the contrary direction to that of the ramification of the nerve.

As an electric current, when propagated through a muscle, never leaves the muscular fibre to follow the nervous filaments, we have perfect evidence that the nervous currents of which we have spoken are due to the *influence* of the electric states which are propagated in the muscle.

To demonstrate the entire importance of these conclusions, we only require to be made acquainted with their connexion with the law of the electric discharge in fishes; this connexion is as intimate as is possible. In fishes, the electric discharge arises from the production of a nervous current by the stimulation of the nerve which is distributed in the organ. In the experiments which we have described, a nervous current is produced by the electric discharge which is passed through the muscle. When this discharge is passed through the muscle in such a manner that the positive and negative electric states are disposed with regard to the nerves in the same manner as in the discharge of the electric fishes, *a nervous current is produced by the influence of the electric current.* This nervous current has the same direction in both cases; *but in the discharge of the torpedo the electric states are produced by the animal, whilst in the experiment of the muscular contraction the nervous current is produced by the influence of the electric current.*

When the electric current traverses a muscular mass in a contrary direction to that of the ramification of the nerve, it follows, from the facts which we have established, that the electric current developes a nervous current, the direction of which is opposed to that which it developes on traversing a muscle in the opposite direction. This is shown experimentally by the phænomena of sensation or of pain which are produced by an electric current traversing a muscle in the contrary direction to the ramification of these nerves.

LXV. *An easy Rule for Formulizing all Epicyclical Curves with one moving circle by the Binomial Theorem.* *By* S. M. Drach, *Esq., F.R.A.S.**

I REFER to the monography "Trochoidal Curves" in the Penny Cyclopædia for the various forms, but which recent article does not mention the following generalization, extending the use of Newton's theorem to these curves as well as to the interpolation series.

Origin is at the deferent's center, x positive towards one apo-center.

$x=r\cos\theta=a\cos q\phi+b\cos p\phi$	$qn=p$
$y=r\sin\theta=a\sin q\phi+b\sin p\phi$	$\phi n=\phi$ of Pen. Cyc.
$r^2=x^2+y^2=a^2+b^2+2ab\cos(p\phi-q\phi=\psi)$	

The resulting function (r^2, x) shows the general symmetry as regards y; when $n=\frac{1}{n}$, interchange a and b.

Case 1. $a=b$, p positive.

$$\therefore x=2a\cos\frac{p+q}{2}\phi\times\cos\frac{p-q}{2}\phi,\quad r=2a\cos\frac{p-q}{2}\phi,$$

$$\theta=\frac{p+q}{2}\phi,\ \frac{r}{a}=2\cos(p-q)\cdot\frac{\theta}{p+q},\ \frac{x}{r}=\cos(p+q)\frac{\theta}{p+q}.$$

Hence $2\cos(p-q)\theta$ developed as

$$\Sigma 2^i A_i\left(\cos\theta=\frac{x}{r}\right)^i$$

is to be put equal to $2\cos(p+q)\theta'$ developed as

$$\Sigma B_j\left(2\cos\theta'=\frac{r}{a}\right)^j$$

for the general equation of the curve.

Ex. $$n=\frac{7}{3},\quad p-q:p+q::4:10::2:5,$$

and

$$2\cos 2\theta=4\frac{x^2}{r^2}-2=2\cos 5\theta'=\frac{r^5}{a^5}-\frac{5r^3}{a^3}+5\frac{r}{a}$$

is the equation.

Case 2. $a=b$, p negative; change sign of q in first case, and

$$2\cos(p+q)\theta=\Sigma 2^i A_i\left(\cos\theta=\frac{x}{r}\right)^i$$

* Communicated by the Author.

is to be put equal to

$$2\cos(p-q)\theta' = \Sigma B_j\left(2\cos\theta' = \frac{r}{a}\right)^j.$$

Ex. $n = -\frac{7}{3}$, $p-q:p+q::4:10:2:5$,

$$2\cos 5\theta = \frac{32x^5}{r^5} - \frac{40x^3}{r^3} + \frac{5x}{r} = 2\cos 2\theta' = \frac{r^2}{a^2} - 2.$$

Case 3. a and b unequal, p positive. For brevity, put

$$\cos\alpha + \sin\alpha.\sqrt{-1} = (c+s)_\alpha = (c\pm s)^\alpha,$$

$$\therefore r\cos\theta \pm r\sin\theta.\sqrt{-1} = a(c\pm s)_{q\phi} + b(c\pm s)_{p\phi} = a(c\pm s)^q_\phi$$
$$+ b(c\pm s)^p_\phi = r(c\pm s)^\theta,$$

$$\therefore 2r^{p-q}\cos(p-q)\theta = r^{p-q}(c+s)^{p-q}_\theta + r^{p-q}(c-s)^{p-q}_\theta$$
$$= \{a(c+s)^q_\phi + b(c+s)^p_\phi\}^{p-q} + \{a(c-s)^q + b(c-s)^p_\phi\}^{p-q}$$
$$= 2a^{p-q}\cos(pq\phi - q^2\phi = q\psi) + \Sigma\frac{b^i a^{p-q}}{a^i}.$$

$$\frac{(p-q)(p-q-1)\ldots(p-q-i+1)}{1.2..i}\left(\begin{array}{l}(c+s)^{pq-q^2-iq+ip=q\psi+i\psi}\\ +(c-s)^{q\psi+i\psi} = 2\cos\overline{q\psi+i\psi}\end{array}\right)$$
$$= 2b^{p-q}\cos p\psi + \Sigma 2a^i b^{p-q-i}\frac{(p-q)(p-q-1)..(p-q-i+1)}{1\ .\ 2\ ..\ i}$$
$$\cos(p-i)\psi,$$

agreeably to the binomial development. Hence

$$2\cos(p-q)\theta = \Sigma 2^i A_i\left(\cos\theta = \frac{x}{r}\right)^i$$

put equal to the sum of

$$2\cos(q+i)\psi = \Sigma Bj\left(2\cos\psi = \frac{r^2-a^2-b^2}{ab} = \frac{Q}{ab}\right)^j$$

is the equation of the curve, and easily expansible by the formula for expressing the cosine of a multiple angle in powers of the cosine of the simple angle. Thus, for

$p-q=1$; $2x = 2ar\cos p\psi + 2ar\cos(p-1)\psi$

$p-q=2$; $4x^2 - 2r^2 = 2b^2r^2\cos p\psi + 4abr^2\cos(p-1)\psi$
$+ 2a^2r^2\cos(p-2)\psi$

$p=5$, $q=3$; $a^5b^3(4x^2-2r^2) = Q^5 - 5a^2b^2Q^3 + 5a^4b^4Q + 2a^2Q^4$
$- 8a^4b^2Q^2 + 2a^6b^4 + a^4Q^3 - 3a^6b^2Q.$

Case 4. a and b still unequal, but p negative;

$$\psi' = p\phi + q\phi r(c \pm s)_\theta = a(c \pm s)^q_\phi + b(c \mp s)^p_\phi$$

$$r^{p+q}\{(c+s)^{p+q}_\theta + (c-s)^{p+q}_\theta\} = 2r^{p+q}\cos(p+q)^\theta = 2a^{p+q}\cos q\psi'$$

$$+ a^{p+q}\Sigma\frac{(p+q)(p+q-1)\,..\,(p+q-i+1)}{1\,.\,2\,..\,i}\, b^i a^{-i}$$

$$\left\{\begin{array}{l}(c+s)^{pq+q^2-iq}_\phi (c-s)^{ip}_\phi \\ +(c-s)^{pq+q^2-iq}_\phi (c+s)^{ip}_\phi = 2\cos\psi' q - i\psi'\end{array}\right\}.$$

As before, develope

$$2\cos(p+q)\theta = \Sigma 2^i A_i \left(\cos\theta = \frac{x}{r}\right)^i,$$

and put it equal to the sum of

$$2\cos(q-i)\psi' = \Sigma B_j\left(2\cos\psi' = \frac{r^2-a^2-b^2}{ab} = \frac{Q}{ab}\right)^j,$$

each having its binomial multiplier for the general equation of the curve, in both cases.

Ex. $n=-3$, $p+q=4$, $q=1$; $\therefore$ $16x^4 - 16x^2r^2 + 2r^4 = \frac{Q}{a^3b}$

$$+ 8ba^3 + 6baQ + \frac{4b}{a}(Q^2 - 2a^2b^2) + \frac{b}{a^3}(Q^3 - 3a^2b^2Q).$$

All these examples verify a previous individual and troublesome method of eliminating cos ϕ from the original equations.

Generally

$$x = r\cos\theta,\ r^2 - a^2 - b^2 = 2ab\cos\psi;$$

and for p positive,

$$r^{p-q}.\,2\cos(p-q)\theta = (bK^0 + aK)^{p-q},\ K^i = 2\cos(p-i)\psi;$$

p negative,

$$r^{p+q}.\,2\cos(p+q)\theta = (aK^0 + bK)^{p+q},\ K^i = 2\cos(q-i)\psi,$$

an easily memory-retained formula, to be developed by the binomial theorem, akin to the finite difference series

$$y_x = (1 + \Delta y_0)^x.$$

The final equation with p negative, $p+q=P$, being

$$r^P.\,2\cos^P\theta = (aK^0 + bK)^P,$$

the left-hand member is

$$\overline{2x}|^P - Pr^2(2x)^{P-2} + \frac{P(P-3)}{1\,.2}r^4(2x)^{P-4}\,..$$

$$\pm\frac{P(P-j-1)(P-j-2)..(P-2j+1)}{1.2.3..j}.r^{2j}(2x)^{P-2j};$$

and the right-hand member is

$$a^{\text{P}}\left(\text{K}^0+\frac{b^2}{a^2}\frac{\text{P}}{2}(\text{P}-1)\text{K}^2+\frac{b^4}{a^4}\frac{\text{P}(\text{P}-1)..(\text{P}-3)}{1.2.3.4}\text{K}^4\ \&\text{c.}\right)$$
$$+\text{P}a^{\text{P}-1}b\left(\text{K}^1+\frac{(\text{P}-1)(\text{P}-2)}{1.2.3}.\frac{b^2}{a^2}\text{K}^3\ \&\text{c.}\right),$$

divided into odd and even angles as cos $\text{P}\theta=$ funct. $(\cos\theta)$ decreases by *even* exponential differences. Hence the first { } is represented by

$$\Sigma(-1)^j a^{\text{P}}.\left(\frac{r^2-a^2-b^2}{ab}\right)^{q-2j}\times$$
$$\left\{\begin{array}{lcl} 1 & \times & \dfrac{q(q-j-1)(q-j-2)..(q-2j+1)}{1.2..3..j} \\ -\dfrac{b^2}{a^2}.\dfrac{\text{P}(\text{P}-1)}{2} & \times & \dfrac{(q-2)(q-j+1)(q-j)..(q-2j+3)}{1.2..(j-1)} \\ +\dfrac{b^4}{a^4}.\dfrac{\text{P}(\text{P}-1).(\text{P}-3)}{1.2.3.4} & \times & \dfrac{(q-4)(q-j+3)\ .\ (q-2j+5)}{1.2..(j-2)} \\ -\dfrac{b^6}{a^6}.\dfrac{\text{P}(\text{P}-1)..(\text{P}-5)}{1.2..6} & \times & \dfrac{(q-6)(q-j+5)\ ..\ (q-2j+7)}{1.2.(j-3)} \\ \&\text{c.} & & \&\text{c.} \end{array}\right\};$$

and the second { } is replaceable by

$$\Sigma(-1)^j\text{P}b.a^{\text{P}-1}.\left(\frac{r^2-a^2-b^2}{ab}\right)^{q-2j-1}\times$$
$$\left\{\begin{array}{lcl} 1 & \times & \dfrac{(q-1)(q-j)(q-j-1)\ ..\ (q-2j)}{1.2.3..j} \\ -\dfrac{b^2}{a^2}.\dfrac{(\text{P}-1)}{2} & \times & \dfrac{(q-3)(q-j+2)(q-j+1)..(q-2j+2)}{1.2..(j-1)} \\ +\dfrac{b^4}{a^4}.\dfrac{(\text{P}-1).(\text{P}-3)}{1.2.3.4} & \times & \dfrac{(q-5)(q-j+4)\ ..\ (q-2j+4)}{1.2..(j-3)} \\ -\dfrac{b^6}{a^6}.\dfrac{(\text{P}-1)..(\text{P}-5)}{1.2..6} & \times & \dfrac{(q-7)(q-j+6)\ ..\ (q-2j+6)}{1.2..(j-5)} \\ \&\text{c.} & & \&\text{c.} \end{array}\right\};$$

the sum of these two Σ is the right-hand member developed as ΣQ^j.

When $a=b$, $n=-3$, there results a three-looped curve,

$$r^6=4a^2r^4-16r^2a^2x^2+16x^4a^2;$$

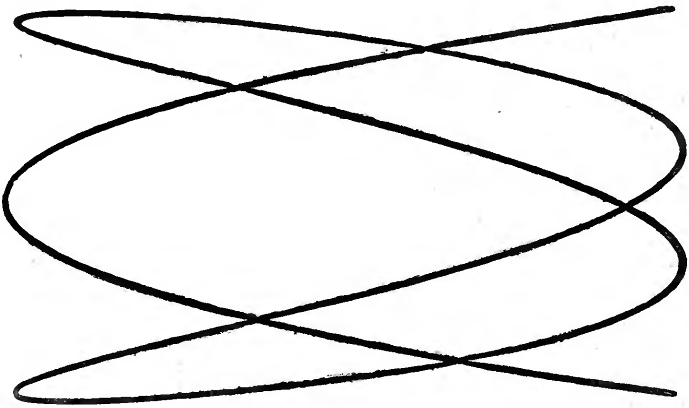
hence

$$r^6=4a^2(x^2-y^2)^2,\quad r^4=4a^2(x^2-y^2),\quad r^2=4a^2(x^2-y^2)^0,$$
$$r^0=4a^2(x^2-y^2)^{-1},\quad r^{-2}=4a^2(x^2-y^2)^{-2},$$

are this three-looped curve, two-looped lemniscate, one-looped circle, equilateral hyperbola, quadrilateral equal hyperbola,

&c. The *floral* appearance of many of these curves induces me to suggest the name of *petaloids*. They may possibly one day lead to geometrical disclosures on the structure of *flowers*, as Naumann and Moseley (Phil. Trans. 1838) have successfully shown in *shells*; each individual shell having its own numerical parameter, which a verbal nomenclature would vainly follow, as every additional digit increases the number of varieties tenfold, three already denoting 999 varieties.

Mr. Perigal's finite spiroeids are very curious, especially the retrogressive syphonoids ($x=a\cos q\phi$, $y=b \begin{smallmatrix}\cos\\ \sin\end{smallmatrix} p\phi$), the two-branched syphonoid being the common conical parabola. (*Vide* Mr. Sang's paper On the Vibration of Wires, Edin. Phil. Journ. 1832, p. 317.) All these which terminate in points are *finite portions of generally infinite curves*; for in the latter $y^2=ax$, if x be proportional to the *periodic* quantity $\cos^2 \lambda$, and y proportional to $\cos \lambda$, $y^2 \div x$ is still constant; the parabola is described, but only between the limits of $\pm 1 = \cos \lambda$, towards which points the motion gradually slackens to zero, as publicly shown by Mr. Perigal at Lord Northampton's scientific *soirée* in March 1846. The Royal Society, Astronomical Society and Royal Institution, possess three volumes of various singular epicyclical curves executed by Mr. Perigal's machinery, some of which are highly ornamental, and I think might be useful for the arts, *e. g.* the drawing of volutes to Ionic columns, &c.



Kinematic Parabola. A retrogressive Syphonoid produced by compound circular motion.

7 Bedford Place, Hampstead Road,
September 1848.

LXVI. *On Spherical Waves in an Elastic Fluid, in reply to* Mr. Stokes. *By the* Rev. J. Challis, *M.A., F.R.S., F.R.A.S., Plumian Professor of Astronomy and Experimental Philosophy in the University of Cambridge**.

THE question which has been discussed by Mr. Stokes and myself in several recent Numbers of this Magazine, is of the following nature. On supposing the waves in a compressible fluid to be spherical, I deduced from that supposition, by reasoning given at length in the Philosophical Magazine for last February, a result inconsistent with one of the fundamental principles of hydrodynamics, viz. that of constancy of mass. Hence I concluded that the supposition of spherical waves is inadmissible. Mr. Stokes undertook to dispute this inference. Now although by an acknowledged rule of logic, the inference could not be set aside except by showing some fallacy in the reasoning which conducted to the absurdity, Mr. Stokes, in three attempts to set it aside, has not once alluded to any step in the reasoning. In the first attempt he produced an argument which took for granted the very point in dispute; in the next he denied, without giving any reason, what was altogether undeniable; in the third attempt (Phil. Mag. for May) he admits what he before denied, and denies, again without assigning a reason, what in the second attempt he admitted. The denial in this instance refers to the possibility of the propagation of a solitary wave of arbitrary condensation and constant type. I infer the possibility from the principle of discontinuity. Mr. Stokes calls this inference a gratuitous assumption, without making the slightest allusion to the principle on which it rests; and yet he has drawn a like inference from the same principle in the same way. (Phil. Mag., vol. xxxiv. p. 54. l. 27–31.)

With respect to my having deduced only a *part* of the results to which the supposition of spherical waves leads, I am able to give a very good reason for not proceeding further. I obtained, as already stated, from that supposition, by reasoning of which Mr. Stokes has not shown, and I am unable to perceive, the fallacy, a result inconsistent with one of the fundamental principles of hydrodynamics. The supposition I consider to be thereby condemned. If I allowed myself to qualify this course of reasoning by another from the same supposition, I should proceed in direct opposition to an incontrovertible rule of logic, and neutralize a highly important and significant result.

Mr. Stokes appeals with great confidence to the results he

* Communicated by the Author.

has obtained by two courses of reasoning in the Phil. Mag., vol. xxxiv. pp. 54–57 and 57–59. I cannot concede that these results have any weight *against* my position, because the reasoning from which they are derived takes for granted the question in dispute; but I may adduce them in favour of it so far as they exhibit inconsistencies. The first argument, which professes to be a *general* consideration of a solitary wave of arbitrary condensation, conducts to the result that the sum of the condensations is exactly equal to the sum of the rarefactions. Now if the reasoning be restricted to the case in which the sum of the condensations is equal to the sum of the rarefactions by the original disturbance, it ceases to be general, and the result is a mere truism without meaning; and if it be not so restricted, it is impossible that the result can be true. Mr. Stokes's argument cannot escape from this dilemma. The second argument, which applies to a case in which condensation prevails over rarefaction, is included in the general argument above mentioned, if that be of any value, and its leading by a different process to a different result is only another phase of contradiction.

As I consider that this hydrodynamical question has now been so fully discussed that it is not likely to receive any additional elucidation, as far as I am concerned the discussion is closed.

Cambridge Observatory,
May 23, 1849.

LXVII. *On the Magnetic Relations of the Positive and Negative Optic Axes of Crystals.* *By* Professor Plücker *of Bonn, in a letter to, and communicated by,* Dr. Faraday.

ALLOW me, Sir, to communicate to you several new facts which, I hope, will spread some light over the action of the magnet upon the optic and magnecrystallic axes.

I. The first and general law I deduced from my last experiments is the following one:—"There will be *either repulsion or attraction* of the optic axes by the poles of a magnet, according to the crystalline structure of the crystal. If the crystal is a *negative* one, there will be *repulsion*; if it is a *positive* one, there will be *attraction*."

The crystals most fitted to give the evidence of this law are *diopside* (a positive crystal), *cyanite*, *topaz* (both negative), and other ones, crystallizing in a similar way. In these crystals the line (A) bisecting the acute angles made by the two optic axes, is neither perpendicular nor parallel to the axis (B) of the prism. Such a crystal, suspended horizontally like a prism of tourmaline, staurolite, or "red ferridcyanide of potassium," in my former experiments, will point neither axially nor equatorially, but will take always a fixed intermediate direction. This direction will continually change if the prism be turned round its own axis B. It may be proved by a simple geometrical construction, which shows that during one revolution of the prism round its axis (B), this axis, without passing out of two fixed limits C and D, will go through all intermediate positions. The directions C and D, where the crystal returns, make, *either* with the line joining the two poles, *or* with the line perpendicular to it, on both sides of these lines, angles equal to the angle included by A and B; the first being the case if the crystal is a *positive* one, the last if a *negative one*. Thence it follows, that if the crystal by any kind of horizontal suspension should point to the poles of a magnet, it is a *positive* one; if it should point equatorially, it is a *negative* one. This last reasoning conducted me at first to the law mentioned above.

The magnecrystallic axis, I think, is, optically speaking, the line bisecting the (acute) angles made by the two optic axes; or in the case of one single axis, this axis itself. The crystals of bismuth and arsenic are positive crystals; antimony, according to my experiments, is a negative one: all are uniaxal.

II. The cyanite is by far the most interesting crystal I have examined. If suspended horizontally, it points very well to the north, *by the magnetic power of the earth only.* It is a true compass-needle, and more than that, you may obtain its declination. If, for instance, you suspend it in such a way that the line A bisecting the two optic axes of the crystal be in the vertical plane passing through the axis B of the prism, the crystal will point exactly as a compass-needle does. By turning the crystal round the line B you may make it point exactly to the north of the earth, &c. The crystal does not point according to the magnetism of its substance, *but only in obedience to the magnetic action upon its optical axes.* This is in full accordance with the different law of diminution by distance of the pure magnetic and the opto-magnetic action. If you approach to the north end of the suspended crystal the south pole of a permanent magnetic bar, strong enough to overpower the magnetism of the earth, the axis B of the prism will make with the axis of the bar (this bar having any direction whatever in the horizontal plane) an angle exactly *the same* it made before with the meridian plane, the crystal being directed either more towards the east or more towards the west.

The crystal showed, resembling in that also a magnetic needle, strong polarity; the same end being always directed to the north. I think this may be a *polarity of the opto-magnetic power*. Two questions too may easily be answered:—1st. Is the north pole indicated by the forms of crystallization? 2nd. Did the crystal obtain, when formed, its polarity by the magnetism of the earth? Between the poles of the strong electro-magnet the permanent polarity disappeared as long as the magnetism was excited.

I am obliged, by the new facts mentioned above, to take up my former memoir; I must reproduce it under a quite new shape. I will examine again the rock-crystal, which, being acted upon weakly by a magnet, induced me to deny in that memoir, what I ascertain now and what I thought most probable, as soon as I received the first notice of your recent researches. [That you will find in the memoir given to M. Poggendorff two or three months ago.] Perhaps the exceptional molecular condition of rock-crystal, as indicated by the passage of light through it, will produce a particular magnetic action.

I should be very much obliged to you, if you would give notice of the contents of my present letter to M. De la Rive, when he calls on you, as he intended to do. I showed him several of my experiments when he passed through Bonn the 12th of May. The following day I obtained the different results mentioned above.

My best wishes for your health.

Very truly yours,
PLÜCKER.

Bonn, the 20th of May 1849.

LXVIII. *Notices respecting New Books.*

Outlines of Astronomy. By Sir JOHN F. W. HERSCHEL, *Bart., K.H., &c.* 8vo. London: Longman and Co., 1849.

THE treatise on Astronomy by Sir John Herschel, published in 1833 as a Part of the Cabinet Cyclopædia, is familiar to every student of that science, and justly prized as containing one of the most lucid and eloquent expositions of its facts and principles ever given to the world. A new edition of this popular and standard work, brought down to the present day, had become necessary by the progress of discovery. In the course of the sixteen years which have elapsed since the original publication, astronomy in all its branches has been assiduously and successfully cultivated. Six new planets have been added to the solar system (and while we write a seventh is announced); a satellite to Saturn, and one or two to the recently discovered large planet; a multitude of comets have been observed and their orbits computed; and our knowledge of the sidereal heavens has been greatly extended, not merely in consequence of continued observation, but by the more general use of large instruments. Hence the work of 1833, however complete at that time, necessarily leaves the student much behind the point to which this branch of knowledge has actually attained. To supply the defects, record the recent discoveries, state the modifications of former views to which they lead, and represent the science as it exists at the present moment, are the objects of the work now before us. It appears under an enlarged form, and with a different title; and though the substance and arrangement of the original treatise have been preserved, the additions and alterations are so extensive and important as to render it rather a new book than a new edition of the former one.

The present work is divided into four parts. The first embraces what is sometimes called Descriptive Astronomy, beginning with the general notions of the science, and including all the topics usually treated of under this branch of the subject;—the shape and size of the earth, the atmosphere, refraction, the theory and use of instruments, the apparent and true motions, as well as the appearances and physical nature of the bodies of the solar system. On many of these heads there was little to alter in the original work; much new matter has however been introduced, and we may instance in particular the chapter on comets, which will be found to be replete with interest. In this we have a condensed account of the remarkable phænomena exhibited by some comets which have recently appeared; by Halley's at its return in 1835, when it was observed under very favourable circumstances by the author himself at the Cape; by the comet of 1843, which approached so near to the surface of the sun, that the intensity of light and radiant heat must have been 47,000 times greater than at the surface of the earth, and whose tail at the perihelion passage was whirled round, unbroken, through an angle of 180° in little more than two hours; by Biela's comet, which at its last apparition in 1846 was seen to separate itself into two distinct bodies, which, "after thus parting company, continued to journey along, amicably, through an arc of upwards of 70° of their apparent orbit, keeping all the while within the same field of view of the telescope pointed towards them." Some other comets recently observed, which seem to describe elliptic orbits in short periods, are also taken notice of; and the description of the phænomena is followed by some most ingenious and highly interesting speculations on the physical nature of those enigmatical bodies.

The second Part treats of the planetary perturbations; a subject which from its nature can never be rendered very popular, but which, nevertheless, as is proved in the present instance, may be explained in such a manner that the principal effects on the motion of a system of bodies produced by their reciprocal attraction may be clearly and readily apprehended by a reader having no more than an elementary knowledge of geometry and mechanics. The accurate and minute computation of these effects is quite another thing, and must be left to the few who possess the requisite technical knowledge. Many attempts have been made to give elementary explanations of the inequalities of the celestial motions, but seldom with much success: indeed, if we except Maclaurin's Account of Newton's Discoveries, Mr. Airy's Treatise on Gravitation, and the work now under review, it would be difficult to point out one from which a student about to enter on the works of Newton and Laplace would derive any considerable aid; and even in respect of these master-pieces it may perhaps be said, that their merit will hardly be appreciated excepting by those who have proceeded to some extent in the technical examination. But Sir John Herschel has not confined himself merely to the illustration of methods already known. In discussing certain effects of perturbation he has struck out an entirely new path, and presented the subject in a light which certainly renders it much easier of comprehension; and on this account the work must be regarded as an important contribution to physical astronomy. To these new views he thus alludes in his preface:—

"In delivering a rational as distinguished from a technical exposition of this subject, the course pursued by Newton in the section of the Principia alluded to has by no means been servilely followed. As regards the perturbations of the nodes and inclinations, indeed, nothing equally luminous can be substituted for his explanation; but as respects the other disturbances, the point of view chosen by Newton has been abandoned for another which it is somewhat difficult to perceive why he did not, himself, select. By a different resolution of the disturbing forces from that adopted by him, and by the aid of a few obvious conclusions from the laws of elliptic motion, which would have found their place, naturally and consecutively, as corollaries of the seventeenth proposition of his first book (a proposition which seems almost to have been prepared with a special view to this application), the momentary change of place of the upper focus of the disturbed ellipse is brought distinctly under inspection; and a clearness of conception introduced into the perturbations of the eccentricities, perihelia and epochs which the author does not think it presumptuous to believe can be obtained by no other method, and which certainly is not obtained by that from which it is a departure. . . . . The reader will find one class of the lunar and planetary perturbations handled in a very different manner from that in which their explanation is usually presented. It comprehends those which are characterized as incident on the epoch, the principal among them being the annual and secular equations of the moon, and that very delicate and obscure part of the perturbational theory (so little satisfactory in the manner in which it emerges from the analytical treatment of the subject), the constant or permanent effect of the disturbing force in altering the disturbed orbit. I will venture to hope that what is here stated will tend to remove some rather generally diffused misapprehensions as to the true bearings of Newton's explanation of the annual equation."

The third Part relates to Sidereal Astronomy. If the former is that which is likely to have the fewest readers, this undoubtedly is the one which will have the greatest number. The extreme interest inherent in the subject would naturally secure this result; but in the present case the interest is greatly heightened by the circumstance, that the author himself stands in the foremost rank among those to whose labours we are indebted for the progress recently made in this department of astronomy. The subject, indeed, may be said to belong to him of hereditary right. He has devoted to it a large portion of the labours of his life; and after scrutinising the heavens from pole to pole, he has here become the expositor of the great discoveries in which he has taken so large a part, and in language alike remarkable for its eloquence and perspicuity, has presented us with an epitome of all that is known about the fixed stars, —their parallaxes, distances and distribution—their motions, relative and systematic—about variable and periodic stars—double stars and binary systems—clusters and groups of stars—the classification, distribution and resolvability of nebulæ, the zodiacal light, &c. It may be said without any exaggeration, that in the whole range of natural philosophy there is nothing more interesting in respect of subject-matter, or more admirable as regards the mode of treatment, than the three chapters forming this part of the work.

Part IV. contains a single chapter which treats of the account of time, or the calendar.

We had marked some passages for the purpose of extracting them; but on consideration this appeared to be unnecessary, as the book itself will be in the hands of every one who takes an interest in the science of astronomy.

LXIX. *Proceedings of Learned Societies.*

CAMBRIDGE PHILOSOPHICAL SOCIETY.

[Continued from p. 227.]

Nov. 13, 1848. ON the Elements of Plane Geometrical Trigonometry, applicable to Trigonometrical Formulæ. By the Rev. F. Calvert.

The object of this paper is to define as distinctly as possible the elementary terms of trigonometry, and to explain the conventional use of the negative sign in expressing such simple functions of angles as the sine, cosine, tangent, &c.

Nov. 27.—On Clock Escapements. By E. B. Denison, Esq., of Trinity College.

The object of this paper is, first to point out the real cause of the general excellence of the dead beat escapement; and secondly, to show that in a gravity, or remontoir escapement, in which the pendulum raises an arm carrying a small weight, from an angle γ up to its extreme semiarc α, which follows the pendulum down again to an angle β (either + and less than γ, or $= -\gamma$), there is a certain proportion between α, β, and γ, which will cause the errors of the clock for small variations of α to be much smaller than in the dead escapement, and in fact inappreciable.

The author adopts the equations obtained by Mr. Airy in his paper on this subject in vol. iii. of the Transactions of the Society, and shows that the increase of the time of an oscillation

$$= \Delta\left(\frac{d\phi}{\phi} - \frac{3d\alpha}{\alpha}\right),$$

where Δ is the difference between the time of oscillation of a free pendulum and one affected by this escapement (which in clocks of the best construction he shows will amount to about 1 second a day); ϕ is the angular accelerating force of the escapement on the pendulum; $d\phi$ the variation in this force due to the variation of the friction of the train and of the state of the oil on the acting part of the pallets; $d\alpha$ the variation of the arc from the same causes, and also from the state of the oil on the dead or circular part of the pallets. It appears therefore that the two causes of error have a tendency to correct each other; and in practice it is found that $\frac{3d\alpha}{\alpha}$ is generally not far short of $\frac{d\phi}{\phi}$, which is the reason of these clocks going so well.

In a gravity escapement there is no variation of the force; and the author shows from Mr. Airy's equations that $\frac{d\Delta}{d\alpha}=0$ if $\alpha=\gamma\sqrt{2}$ in that escapement where the remontoir weight is taken up at γ and follows the pendulum again to $-\gamma$; and in the other kind of gravity escapement $\frac{d\Delta}{d\phi}\frac{d\Delta}{d\alpha}=0$ when

$$\alpha^2 = 2\sqrt{\alpha^2-\gamma^2}\sqrt{\alpha^2-\beta^2}.$$

This last construction however is barely practicable, if this condition is to be satisfied, on account of the small difference between β and γ which is allowed by the deduction of the value necessary for $\alpha-\gamma$, the angle in which the unlocking of the escapement is effected; although this is the construction which has been used in nearly all the gravity escapements that have been tried; and of course the proper condition has been very far from satisfied, and the clocks have failed.

In a supplement to this paper the author proposes, chiefly for turret clocks, a new construction of a spring remontoir on the axis of the escape-wheel. The object of such remontoirs is to remove from the escapement (of any ordinary kind) the great inequalities of force caused by the varying friction of the heavy train and dial-work, and by the action of the wind on the hands; and also to cause the minute-hand to move only at visible intervals, such as $\frac{1}{2}$ a minute, and the striking to take place exactly at the right second. The Royal Exchange clock, made under the superintendence of the Astronomer Royal, has a gravity remontoir in the train introduced for these purposes; but it is too complicated and expensive for ordinary use, and has a good deal of friction, from which the proposed remontoir is free. Spring remontoirs winding up at similar intervals have been tried in France, but without success, from defects in their construction.

March 12, 1849.—On the Intrinsic Equation to a Curve, and its application. By the Master of Trinity.

The author remarked that the expressions for the lengths of curves, their involutes and evolutes, in the ordinary methods, are complex and untractable, which arises in a great measure from the properties of *extrinsic* lines being introduced, namely, coordinates. But a curve may be represented without any such additions, by an equation between the length and the angle of flexure, which is therefore called the *intrinsic* equation. This equation gives, with remarkable facility, the radii of curvature; involutes and evolutes of most curves. It also expresses very simply what may be called *running* curves; namely, curves which run like a pattern along a strip of ornamented work. A very simple equation expresses, for instance, the inclined scroll pattern so common in the antique, and by altering the constants, gives to this pattern an endless variety of forms. If s be the length of the curve and ϕ the angle, the *intrinsic* equation to the circle is $s=a\phi$; to the cycloid $s=a \sin\phi$. The equation to an epicycloid or hypocycloid is $s=a\sin m\phi$, according as m is less or greater than unity. The equation to an undulating pattern is $\phi=m\sin s$, which assumes very various shapes by varying m. The method was also used in proving that if we take the successive involutes of a curve an indefinite number of times, the resulting curve (with certain limitations) bends to become *the equiangular spiral* if the unwrapping be always in the same direction, and tends to become *the cycloid* if the unwrapping be alternately in opposite directions. The latter proposition had already been discovered by Bernouilli and proved by Euler.

Feb. 26.—On a New Method of finding the Rational Roots of Numerical Equations. By Robert Moon, Esq.

The author proposes to found a new experimental method of finding the integral roots of numerical equations upon the following theorem.

If the equation

$$x^n+p_1x^{n-1}+p_2x^{n-2}+\&c. +p_ix^{n-i}+\&c.+p_n=0$$

has a positive and integral root m, we shall have $-p_n$ equal to m terms of the following series :—

$$A_{n-1}+1.\,A_{n-2}+1.2.\,A_{n-3}+1.2.3\,A_{n-4}+\&c. +1.2\ldots(n-i+1)\,A_i + \&c. +1.2\ldots(n-1)\,A_0$$

$$+A_{n-1}+2.\,A_{n-2}+2.3\,A_{n-3}+2.3.4\,A_{n-4}+\&c. +2.3\ldots(n-i+2)\,A_i + \&c. +2.3\ldots n\,A_0$$

$$+A_{n-1}+3.A_{n-2}+3.4\,A_{n-3}+3.4.5\,A_{n-4}+\&c. +3.4\ldots(n-i+3)\,A_i + \&c. +3.4\ldots(n+1)\,A_0$$

$$+A_{n-1}+4.\,A_{n-2}+4.5\,A_{n-3}+4.5.6\,A_{n-4}+\&c. +4.5\ldots(n-i+4)\,A_i + \&c. +4.5\ldots(n+2)\,A_0$$

&c. &c.

where

$A_i=(n-i)(p_i-h_1p_{i-1}+h_2p_{i-2}+\&c.+(-1)^r h_r p_{i-r}+\&c.\pm h_{i-1}p_1\mp h_i)$

and

h_1=the sum of the natural numbers $1, 2, 3 \ldots\ldots (n-i)$.

h_2=the sum of the homogeneous products of the same quantities of two dimensions.

h_3=the sum of the homogeneous products of the same quantities of three dimensions, and so forth.

From the above formula for A_i may be determined all the coefficients A except the first, which is determined from the equation

$$A_{n-1}=p_{n-1}-p_{n-2}+p_{n-3}-\&c.\pm p_1\mp 1.$$

Having determined the quantities A in any particular case, let them be substituted in the first line of the series. If the sum of that line be equal to $-p_n$ unity is a root of the equation. Let the second line be then written down and added to the first. If the sum of the two equals $-p_n$, 2 is a root of the equation, and so by adding successive lines we shall ascertain whether the successive integers 3.4., &c. are or are not roots of the equation.

The quantities h in the expression for A_i depend upon the number of the coefficient and the number of the dimensions of the equation. The author proposes that these should be calculated and tabulated for equations of all dimensions up to a certain limit, by which means we should be in possession of so many skeletons of equations, ready for application in any particular case, and the calculation in particular instances would be thus greatly facilitated.

It will be observed that each successive line is derived from that preceding by a simple division and multiplication of the separate terms of the latter, and thus each succeeding trial facilitates those which follow; contrary to what obtains in the ordinary method by successive substitutions, in which each attempt proceeds *de novo*.

If the addition of a term makes the series from being greater than p_n less than it, or *vice versâ*, a fractional or surd root will lie between the number expressing the number of the term so added and the number next below it.

If all the roots are impossible, the series will be either always greater or always less than p_n, whatever be the number of terms taken.

For an example take the cubic

$$x^3+px^2+qx+r=0.$$

Here

$$\begin{aligned}-r=&3\times 1.2+2(p-3)1+q-p+1\\ &+3\times 2.3+2(p-3)2+q-p+1\\ &+3\times 3.4+2(p-3)3+q-p+1\\ &+\ \&c.\ \text{to } x \text{ terms,}\end{aligned}$$

if x is a positive integer.

The method in common with other experimental methods applies to the discovery of all roots, possible or impossible, which do not involve surds.

ROYAL ASTRONOMICAL SOCIETY.

[Continued from p. 225.]

March 9, 1849.—On Irradiation. By Professor Powell.

After adverting to the history of researches on this subject, the author dwells particularly on the method of exhibiting the phænomenon adopted by M. Plateau, which forms the basis of all his own experiments, and which consists of a card or lamina, cut so that one half of a long parallelogram is cut out whilst the other remains, having the portions at the sides cut away. Viewed against the light, the enlargement of the bright half, in breadth, is seen contrasted with the opake, and might be subjected to measurement.

The first question on the subject refers to the supposition of a peculiar *physiological cause* affecting the eye to produce the apparent enlargement of the bright image. After fully allowing for some portion of such phænomena being fairly attributable to ocular causes, such as dazzling, contrast, &c., experiments are adduced to show that *precisely similar phænomena are produced in an artificial eye, or camera obscura*; whence the hypothesis of any peculiar affection of the retina is rendered unnecessary. The same conclusion is further confirmed by *photographic* impressions of the image of the card cut as before, which exhibit the same enlargement. Specimens of these impressions, taken by Mr. N. S. Maskelyne, were exhibited.

These results, clearly pointing to an *optical* cause, agree with the conclusions of the undulatory theory, relative to the "diffraction of a lens," as investigated by Mr. Airy, which apply to the eye considered as an optical instrument, as well as to the object-glasses of telescopes; in either case *the image of a point* being an *extended disc*, which, if the light be bright enough, will be surrounded by rings. A luminous surface will exhibit a like enlargement.

Without reference to any theory, it is an ascertained law that the enlargement *increases with the intensity of the light*. The enlargement also is formed with a rapid decrease in brightness towards the edge. On these grounds it is easy to explain the fact of the great diminution or total destruction of irradiation by the *interposition of lenses*, which would follow immediately from the weakening of the intensity in proportion to the square of the linear magnification. The author has examined particularly into the extent to which this effect takes place, and announces that *low powers* (from 5 to 20) *are sufficient to obliterate all irradiation even in the most intense light which the eye can bear*.

Various results of M. Plateau and others as to the effects of *contrast* in making a narrow bar or wire continue visible, though the irradiations ought to overlap, have been examined, and found only to hold good with low intensities.

The author next considers the effect in *telescopes*. Here that portion of the effect which regards the *ocular* image being placed out of consideration from the influence of the magnifying power (already referred to), we have only to consider that part which affects the *focal image* of the object-glass. The *diminution of the aperture in-*

creases the irradiation, but at the same time it diminishes the light. At a certain point, then, these two causes counterbalance each other, and no further enlargement takes place. This limit will vary with each instrument, and we have no certain grounds on which to determine it. Various observations are referred to in which its influence is evinced.

The astronomical facts connected with these causes are then examined from the testimony of various observers. In particular the application of these principles to some of those singular phænomena occasionally noticed in eclipses, transits, occultations, &c. seems easy in theory abstractedly considered. The difficulty lies in explaining why they are observed only in some cases and not in others. The author dwells particularly on the desirableness of a closer attention to stating *all* the conditions of the *telescopes* employed, especially the *apertures*.

In particular the phænomenon "*the neck*," in the transits of Mercury and Venus, would be an obvious consequence of irradiation, which would diminish the planet's disc and enlarge that of the sun except at the small portion of the circumferences in contact, when the absence of *both* irradiations would produce a "neck."

Both theory and experiment show that a small dark disc would have for its image a diminished disc with a bright internal concentric ring, which, if the disc be very small, will be contracted to a central bright point. This seems to agree with the appearance noticed by several observers in the transit of a white spot on the centre of the planet. On a former occasion, however, Professor Moll and others saw such a spot *excentrical*. The projection of a star on the *bright* limb of the moon would also be an effect of irradiation, which would cause the disc of the moon simply to overlap the star.

Lastly, the author suggests a method for obtaining *measures of the amount of irradiation under any given light*, by placing a card, cut as before, at the focus of a lens, opposite to the object-glass of a telescope, and attached to it by a short tube; when the enlargement of the image of the card, illumined by the light from any source, can be subjected to the exact measurement of the micrometer of the telescope.

On a New Method of Observing Transits. By A. D. Bache, Esq., Director of the American Coast Survey.

"Permit me to invite your attention, and that of the members of the Royal Astronomical Society, to a brief abstract of an official report made to me on the 15th inst. by Mr. Sears C. Walker, one of the assistants of the United States Coast Survey. It relates to the printing, by the use of an electro-magnetic clock, in connexion with Morse's telegraph register, of the actual dates of any celestial phænomena, which are ordinarily made the subject of observation by astronomers.

"The electro-magnetic clock of Mr. Wheatstone is described in the Proceedings of the Royal Astronomical Society for Nov. 19, 1841. Mr. Steinheil has described his in Schumacher's Astronomical *Jahrbuch* for 1844.

"Recently Prof. Bond and Dr. Locke have invented different processes, which are described in Mr. Walker's Report.

"Prof. Bond proposes to make circuit by the metallic contact of insulated portions of the pallet and escapement-wheel. Dr. Locke, like Mr. Wheatstone, uses a metallic wheel on the arbour of the seconds' hand. This wheel has sixty teeth, each of which when horizontal strikes against a platinum lever or tilt-hammer, weighing two grains. The rising and falling of the hammer from a bed of platinum breaks and makes the galvanic circuit. The fulcrum of the tilt-hammer and the platinum bed rest severally on a small block of wood.

"The object of all these methods is to cause a delicate astronomical clock to make and break the galvanic circuit every second, without injury to the machinery or rate of the clock. The mode of action of such alternations on Morse's electro-magnetic telegraph register, as now in daily use in the United States, is the same for each of these methods.

"The *automatic clock register* thus formed consists of a graduated fillet of paper delivered pretty uniformly at the rate of an inch per second. The beginnings of minutes, and fives and tens of minutes, and of seconds, and fives and tens of seconds, are distinguished from each other by the lengths of the corresponding imprinted blank spaces. The printed second consists of an indented line of about nine-tenths of a second or less, and of a blank space for the remainder. The rate of the delivery of the paper is regulated by a centrifugal clock like those of the Munich equatoreals. An error of two seconds per minute in the rate of delivery causes only an average error of one-hundredth of a second in the register of a date.

"The printing of the date of any event not susceptible of automatic register, but dependent for our knowledge of its occurrence upon human sensations, is effected by tapping gently at this date on a *break circuit telegraph key*, so as to insert in the line of the *automatic clock register* a short blank space, whose beginning marks the instant of the tap. Should this blank space occur near that of the *automatic clock register*, the fact would identify its date. For isolated events the finger dwells long enough on the key to be sure of cutting off some portion of one of the indented lines. The dates susceptible of impression with advantage on the *automatic clock register* are such as the phases of an eclipse or occultation, or the bisections of a star or comet, or of a planet's centre or limb, by the wires of a transit instrument. The association of the nerves and sensations of sight and touch is known to be far more intimate than that of those of the eye and ear. The art of tapping at the proper dates requires far less practice and experience than that of counting beats and estimating fractions of a second. The labour of counting beats and of writing down the dates being here dispensed with, the equatoreal intervals of the transit wires may be reduced to two seconds of time, or even to less, and fifty bisections may now be registered in the same time as seven are in the ordinary way. The three advantages of Mr. Walker's method are respectively,—

"1st. The facility of acquirement of the practical skill for observing.

"2nd. The *twofold* precision nearly of a single observation.

"3rd. The *sevenfold* multiplication of observations in the same interval of time, or in the single transit of one, or the relative transits of two or more heavenly bodies.

"From all these sources it will be apparent that Mr. Walker's method of printing dates has nearly a *tenfold* advantage over the ordinary mode of using the transit instrument.

"A single transit of a star, or a night's or even a year's work by this method of printing, may take the place of some *ten* times those quantities by the method now in use.

"The experiment of printing the dates of bisections of transit wires by a star, on the ordinary registering fillet of Morse's telegraph, was made by Mr. Walker in 1846. It was repeated this last summer for some twenty or more stars, in connexion with Prof. Bond and Prof. Loomis, for a distance of some three hundred miles from Cambridge to New York. In October last it was repeated for a like number of stars between Philadelphia and Cincinnati, in connexion with Prof. Kendall and Prof. Mitchell, through a distance of seven hundred and fifty miles. The taps made on the telegraph key at the time of bisection at each place were registered at both. In these operations, however, the year was used to estimate fractions of a second by the *audible* beats of the telegraph and observing clocks, and no use was made of the *visible* register.

"Dr. Locke's electro-magnetic clock of his own invention and construction (Wheatstone's method not being known to him at the time) was used for some two hours or more, on the 17th of November last, to make the *automatic clock register* such as is described above. The distance tried was about four hundred miles from Cincinnati to Pittsburg.

"The experiment was completely successful. The interruption of the line from Pittsburg to Philadelphia that night prevented the actual continuation of the two operations on the same fillet of paper, namely, the graduation of the paper by the automatic clock, and the reciprocal imprinting of the dates of transits of stars at the two observatories. Each process, however, has been tried by itself to a sufficient extent by Mr. Walker and his associates, to warrant his conclusions with respect to their combination, for a more full trial of which he now waits for the construction of the most approved apparatus.

"In order to make the precision of the other appendages of a transit instrument commensurate with the *tenfold* increase of that of the art of imprinting the dates of bisections for a single culmination, Mr. Walker recommends the use of a cast-iron box for the frame.

"Each side should carry three or more levels.

"The number read on each occasion should depend upon the degree of precision aimed at. The instrument should admit of rapid reversal, even on equatoreal stars. For use at the station of the Coast Survey, Mr. Walker prefers to retain the micrometer adjustment of the azimuth, like that of the new Simms's transit instruments recently made for the survey.

"In the telegraph operations for longitude, two such transit instruments of moderate size are to be mounted, at any two stations, distant one or more thousand miles. All the levels are to be read with the instrument pointing to the zenith, then twenty bisections of a circum-zenith star are to be imprinted on the *automatic clock register* previous to reversal. The like number for the same star on the same wires are to be imprinted after reversal, and the levels are again to be read.

"A similar operation is performed for the transit of the same star at the western station.

"The primitive astronomical clock may be located and rated at the central station of the coast survey. The *automatic clock register* may be made and kept there, even if the distance be a thousand miles from either station.

"Clock registers in any number may be made at the separate stations. The transits of two fundamental stars at remote dates, at either of the three stations, may give the rate of the primitive or central clock.

"One such transit of the same star over each station with twenty printed registers of normal bisections, and six normal levelings, with independent levels, at or near the position of actual observation, with the increased precision of the instrumental adjustments, will give in the form of a permanent printed record (with multiplied copies) the relative longitude of the two stations.

"The uncertainty of such a result need be only a few hundredths of a second, and may be such only as attends our present knowledge of the relative longitudes of Greenwich and Paris, the two oldest observatories extant.

"I avail myself of the occasion to remark, that the Coast Survey operations were completely successful this autumn between Philadelphia and Cincinnati, while actually working on the line from Philadelphia to Louisville. The distance of the line in the air is nine hundred miles, that of the circuit is eighteen hundred. I learn from an authentic source that the same success attends the use of the line from Philadelphia to the Mississippi river opposite St. Louis. The length of this circuit is *one-tenth* of the circumference of the earth. The inference from this trial is clear, that a line round the earth, if such could be constructed, might be worked with facility at one stroke. The expense of acids to supply the thousand Grove's pint cups, required for the motive power, would be about one pound sterling (five dollars) per day."

ROYAL SOCIETY.

[Continued from p. 314.]

March 29, 1849.—"Examination of the Proximate principles of some of the Lichens."—Part II*. By John Stenhouse, Esq., F.R.S.

Gyrophora pustulata.

The author states that this lichen, which is the "Tripe de Roche" of the Canadian hunters, has been long employed by the manufac-

* [An abstract of Part I. will be found at p. 300 of the 32nd volume of this Journal.—Ed.]

turers of archil, though the quantity of colouring matter contained in it is by no means considerable, being little more than a twelfth of that in the *Roccella Montagnei*. The *Gyrophora pustulata*, on which the author operated, was brought from Norway, where it is annually collected in considerable quantity for the manufacture of archil. The colouring principle was extracted by maceration with milk of lime, and was precipitated in a gelatinous state by neutralizing the lime solution by muriatic acid precisely in the way so frequently described in the author's former paper (Phil. Trans. 1848). The precipitate was gently dried, and then dissolved in hot spirits of wine. On the cooling of the liquid, the colouring principle was deposited in small soft crystals, which by digestion with animal charcoal and repeated crystallizations were rendered quite colourless. This principle, to which the author has given the name of *Gyrophoric acid*, is almost insoluble in either hot or cold water, and is also much less soluble in hot spirits of wine than either orsellic, erythric, or any of the analogous colouring principles. It is neutral to test-paper, and possesses no saturating power, as the smallest quantity of an alkali gives its solutions an alkaline reaction. Gyrophoric acid strikes a bright red fugitive colour with hypochlorite of lime; and when macerated with a solution of ammonia, it is slowly converted into a purplish-red colouring matter, similar to that yielded by the analogous acids under the same circumstances. When subjected to analysis, the formula of gyrophoric acid was found to be $C_{36} H_{18} O_{15}$.

Gyrophoric acid when boiled for some hours in alcohol yields an ether similar in appearance and properties to the erythric and lecanoric ethers; its formula is $C_4 H_5 O + C_{36} H_{18} O_{15}$.

Gyrophoric acid unites with the alkalies and metallic oxides, but the compounds which it forms possess little stability and cannot be procured of an uniform composition.

Lecanora tartarea.

This lichen, like the *Gyrophora pustulata*, has been employed from an early period in the manufacture of archil. It is found in considerable abundance in the hilly districts of the northern parts both of Scotland and Ireland. The lichen on which the author operated came from Norway. He found it also to contain gyrophoric acid, in much about the same quantity as the *Gyrophora pustulata*. This fact was established by the analysis of the acid itself and of its ether compound.

Brom-orcine.

In the author's former paper on the proximate principles of the lichens, read before the Royal Society on the 3rd of February 1848, he described a crystalline body obtained by cautiously adding bromine to an aqueous solution of orcine. In this second part he states that, in the 'Comptes Rendus' for August of the same year, Messrs. Laurent and Gerhardt describe the very same compound obtained in precisely the same way, without even hinting that it had been previously discovered. These gentlemen however give a different formula for the compound, viz. $C_{14} H_5 Br_3 O_4$, or orcine in which three equivalents of hydrogen are replaced by three equivalents of bromine; and the author is disposed to adopt this formula, as on repeating the analysis of the compound he found that he had somewhat over-estimated the amount of bromine contained in it, while its other constituents were determined correctly enough.

Beta-orcine.

This substance, described by the author in the Philosophical Magazine for July 1848, may be obtained from usnic acid, either by destructively distilling it, or by acting on it with alkalies.

Beta-orcine crystallizes very beautifully in four-sided prisms surmounted at either end by four-sided pyramids. These crystals have a brilliant lustre, and are from three quarters of an inch to an inch long. Their solution strikes a fugitive bright-red colour with hypochlorite of lime, and with a solution of ammonia it yields a permanent blood-red colouring matter which becomes darker on standing. The formula of beta-orcine, which however is merely empirical, is $C_{16} H_{10} O_4$.

Quintonitrated-erythromannite.

In his former paper on the lichens, the author has described, under the name of *pseudo-orcine*, a remarkably beautiful crystalline body which is obtained by boiling either picro-erythrine, or erythric acid, with an excess of lime or baryta. This substance he then regarded as very analogous to mannite both in its composition and properties, and this view having been amply verified by an experiment which he has recently made, he has been induced to change the name of this compound to *erythro-mannite*, as at once indicating its origin and its most striking properties. After referring to the discovery by Messrs. Flores Domonte and Menard, of "Mannite quintonitrique" or mannite in which five equivalents of water are replaced by five equivalents of nitric acid, and which possesses the remarkable property of detonating so violently when struck by a hammer that M. Sobrero has proposed employing it, instead of fulminate of mercury, in the manufacture of percussion caps, the author states that when erythro-mannite is treated with fuming nitric acid, in exactly the same way as mannite, it yields a perfectly analogous compound, or erythro-mannite in which five equivalents of water are replaced by five equivalents of nitric acid. This compound, which he has called *quintonitrated erythromannite*, is also insoluble in water, but crystallizes out of hot spirits in large flat crystals resembling those of benzoic acid, only larger and exhibiting a much more pearly lustre. Quintonitrated-erythromannite also detonates with great violence when it is mixed with a little dry sand, and is strongly struck with a hammer.

In order to exhibit more distinctly the close analogy which subsists between the four compounds, their rational formulæ are given, viz.

Mannite .... $= C_{12} H_{14} O_{12}$;
Erythro-mannite .... $= C_{11} H_{14} O_{11}$;
Quintonitrated mannite .... $= C_{12} H_9 O_7 + 5NO^5$;
Quintonitrated erythromannite $= C_{11} H_9 O_6 + 5NO^5$.

May 10.—"Remarks on M. De la Rive's Theory for the Physical Explanation of the Causes which produce the Diurnal Variation of the Magnetic Declination," in a letter to S. Hunter Christie, Esq., Sec. R.S., from Lieut.-Col. Sabine, For. Sec.R.S. Communicated by S. Hunter Christie, Esq.

My dear Sir, Woolwich, April 16, 1849.

The *Annales de Chimie et de Physique* for March last contains a letter from M. De la Rive to M. Arago*, in which a theory is proposed, professing to explain, on physical principles, the general phenomena of the diurnal variation of the magnetic declination, and, in particular, the phenomena observed at St. Helena and at the Cape of Good Hope, described in a paper communicated by me to the Royal Society in 1847, and which has been honoured with a place in the Philosophical Transactions.

Although I doubt not that the inadequacy of the theory proposed by M. De la Rive for the solution of this interesting problem will be at once recognised by those who have carefully studied the facts which have become known to us by means of the exact methods of investigation, adopted in the magnetic observatories of recent establishment; yet there is danger that the names of De la Rive and Arago, held in high and deserved estimation as authorities on such subjects, attached to a theory,—which moreover claims reception on the ground of its accordance with "well-ascertained facts" and "with principles of physics positively established,"—may operate prejudicially in checking the inquiries which may be in progress in other quarters into the causes which really occasion the phenomena in question; I have thought it desirable therefore to point out, in a very brief communication, some of the important particulars in which M. De la Rive's theory fails to represent correctly the facts which it professes to explain, and others which are altogether at variance with, and opposed to it.

1. M. De la Rive's theory, in a few words, is as follows:—

In consequence of the inequalities of temperature in the higher and lower strata of the atmosphere, electric currents are generated, which in the higher regions proceed from the equator to the poles, and return at the surface of the earth from the poles to the equator; the return current causing in the northern hemisphere the north end of the magnet to deviate in the one direction, and in the southern hemisphere in the opposite direction; the deviation being at any given place greatest at the hour (about 1^h·30 p.m.) when the difference of temperature in the upper and lower strata of the atmosphere is greatest, and of course increasing until that hour, and subsequently diminishing.

That the north end of the magnet does thus deviate in the forenoon towards the west in the northern hemisphere, and towards the east in the southern hemisphere, and return in both cases in the opposite directions in the afternoon, were facts known before the establishment of the magnetic observatories; but M. De la Rive's

* [A translation of this letter appeared in the Phil. Mag. for April 1849, p. 286.—Ed.]

explanation of them appears to have been suggested, and its appropriateness, as he considers, shown, by its affirmed accordance with the remarkable peculiarity in the phenomena made known to us by the observations at the Magnetic Observatory at St. Helena, and communicated to the Royal Society in the paper referred to. This peculiarity is briefly as follows: the deviation which constitutes the principal part of the diurnal variation at St. Helena is *not uniform in its direction* throughout the year; in one part of the year it is to the west, and in the other part of the year to the east; and consequently during certain months of the year the movement of the magnet is in the contrary direction to that which prevails at the same hours during the other months of the year.

Now St. Helena is situated within the tropics, and M. De la Rive infers from his theory that in all places so situated, the diurnal variation should be in one direction when the sun's declination is north of the latitude of the place, and in the contrary direction when the sun's declination is south of the latitude of the place: and hence he too hastily concludes that his theory accords with the characteristics of the diurnal variation at St. Helena; when however the facts are more closely examined it is seen that they do by no means accord with M. De la Rive's supposition.

That it may be quite clear that I do not misapprehend either M. De la Rive's theory, or his supposition in regard to the facts at St. Helena, I subjoin his own expressions, which convey his meaning, as that gentleman's writings generally do, with most commendable precision.

The first extract defines the limit which, according to his theory, should separate the electric currents proceeding respectively from each of the poles to the equator; and should consequently separate the parts of the globe in which the diurnal variation is in the one direction, from the parts in which it is in the opposite direction; whilst the second extract describes what he believes to be the facts of the phenomenon at St. Helena.

Extract 1.

"La limite qui sépare les régions occupées par chacun de ces deux grands courants n'est pas l'équateur proprement dit, car elle doit être variable : elle est, d'après la théorie que je développe, celui des parallèles compris entre les tropiques, qui a le soleil à son zénith; elle change par conséquent chaque jour."

Extract 2.

"À St. Hélène, la variation diurne a lieu à l'ouest tant que le soleil est au sud de l'île, à l'est dès que le soleil est au nord. En effet, dans le premier cas, ainsi que j'ai remarqué plus haut, St. Hélène doit faire partie de la région dans laquelle les courants électriques vont sur la surface de la terre du pole boréal aux régions équatoriales; et, dans le second cas, de la région dans laquelle ces courants vont du pôle austral vers l'équateur."

Whoever will be at the pains to refer to the paper printed in the Philosophical Transactions, describing the phenomena of St. Helena, or to the volume containing the details of the observations on the diurnal variation in each month during the five years in which hourly observations were maintained day and night at that observatory, will perceive,—on evidence which admits of no uncertainty,—that the two portions of the year in which the diurnal variation is in contrary directions at that island, are not determined as M. De la Rive supposes by the declination of the sun relatively to the *latitude of the place*, but by the declination of the sun relatively to the *equinoctial line*. The sun is vertical at St. Helena, passing to the south in the first week of November; and again when passing to the north in the first week of February: consequently the two portions into which the year is thus divided, are respectively the one of *three*, and the other of *nine* months' duration; but the actual portions in which the contrary diurnal movements of the magnets take place at St. Helena are of *equal* duration, and consist of *six* months and *six* months; the dividing periods coinciding unequivocally, not with the sun's verticality at St. Helena, but with the equinoxes.

2. But if M. De la Rive's explanation be thus inconsistent in respect to the dates of the transition periods of the phenomena at St. Helena, it must be regarded as altogether at variance with, and opposed to, the phenomena described in the same paper at the Cape of Good Hope, where also they have been observed at the Magnetic Observatory at that station with an exactness which leaves no uncertainty whatsoever as to the facts themselves. The Cape is *not* situated within the tropics; its latitude is 33° 56′ south; the sun is consequently throughout the year well to the north of its zenith; and therefore according to M. De la Rive's theory, the deviations should be in one and the same direction throughout the year. But the fact is not so; for the same contrariety in the direction of the diurnal variation at different portions of the year takes place at the Cape as at St. Helena; the two portions of the year in which the opposite phenomena prevail, are also identical at the two stations; and at both the change in the direction of the deviation takes place when the sun crosses the equinoctial line; the deviation being to the west at both stations when the sun is in the northern signs, and to the east when he is in the southern signs.

3. But in considering a theory which comes before us, claiming the high distinction of affording a physical explanation of facts which are known to us by well-assured observation, we ought not to confine our view to those points only for which it professes to supply the explanation: these are certainly tests as far as they go;—and in the present instance the conclusion from them is not favourable to the theory proposed;—but we should also notice its deficiencies; or those points wherein it neither furnishes, nor attempts to furnish, explanations of circumstances which are certainly amongst the most remarkable facts of the case. They may be possibly amongst the most difficult to explain, but no physical theory can be regarded as meeting the facts which does not at least attempt an explanation of them. I may name as the most prominent in interest amongst these the striking fact, that the Cape of Good Hope should be one of the stations at which this remarkable peculiarity of a contrariety of movement at different periods of the year takes place.

It is known that it does not occur at places situated in corresponding latitudes north of the geographical equator; at Algiers, for example,—which is moreover nearly in the same geographical meridian as the Cape, and where the magnetic inclination is nearly the same towards the north as is the case at the Cape towards the south. It may be quite correct perhaps to view the corresponding phenomena at St. Helena and the Cape as those belonging to *magnetically*-equatorial stations; but they are certainly not those peculiar to or characteristic of *geographically*-equatorial stations, which would be the condition in M. De la Rive's theory. There are thus two parts in the problem demanding physical explanation; on the one hand, the cause is required of the contrariety of movement, as well as of the times at which the different movements occur, the latter having obviously a dependence on the sun's position either in the northern or the southern signs; and on the other hand, the cause must be shown why certain stations are thus affected and others not: a distinction which obviously does not depend on situation in regard to the geographical equator or to the tropical divisions of the globe.

I have myself been led to infer that the peculiarity in question results from and is indicative of proximity to the line of *least magnetic force*, regarded as physically the separating line on the surface of the globe between the northern and southern magnetic hemispheres; the peculiarity would thus be strictly a magnetically-equatorial one.

It results from the present position of the four points of maximum intensity at the surface of the earth, that the intermediate line of least intensity departs considerably in the Southern Atlantic from the middle or geographically-equatorial portion of the globe, passing between the Cape and St. Helena, and consequently not far from either of these stations.

As far as I have yet been able to examine, I have found that the same remarkable peculiarity does exist at all other stations which are near this line, and at none which are remote from it. But however this may be, the accordance of the phenomena at the Cape of Good Hope and St. Helena, and their dissimilarity from those at other stations is a well-ascertained fact, of far too much bearing and importance to be passed without notice; and we may safely anticipate that its cause must occupy a prominent place in the theory which shall be ultimately received, as affording an adequate solution of the problem of the diurnal variation.

Believe me, my dear Sir, sincerely yours,

EDWARD SABINE.

S. H. Christie, Esq., Secretary to the Royal Society.

LXX. *Intelligence and Miscellaneous Articles.*

ON SOME METEOROLOGICAL PHÆNOMENA. BY PROF. E. WARTMANN.

On a mirage with a strong bise.—Most of the mirages described by authors appear to be manifested in a tranquil state of the air. M. Kämtz even affirms that a calm atmosphere is indispensable to their production*. Although this may be the ordinary circumstance, it is not always the case. More than once vessels have been carried along by the wind, and their images shifted in a similar manner. This phænomenon is often remarked on the Black Sea, from Odessa. The accounts of the observations of Woltmann near Cuxhaven†, and of Vince at Ramsgate‡, leave no doubt with respect to this fact. The following is a similar instance, observed at Nyon in the summer of 1848 by M. Thury, formerly professor at Lausanne, and which is still better characterized than the previous ones.

It was twenty minutes to 8 o'clock, A.M. The bise had raised foaming waves upon the lake. In the south-east some vapours floated on the horizon: the sky in every other part was of a clear blue. By the aid of a good telescope of 0·068 millim. aperture, and which magnified thirty times, M. Thury descried on the heights of Coppet, in the direction of Geneva, the two lateen sails of a bark the hull of which was not at all visible. A little below the lower extremity of these sails, the commencement of their image was seen inverted. This incomplete image terminated abruptly on the agitated and clear surface of the water. The space which separated the sails from their image was of a uniform greenish-blue colour. The lowest strata of air undulated in a very perceptible manner.

This last circumstance, and the situation of the image below the object which it represented, are proofs that the phænomenon resulted from a greater heating of the atmosphere in the lower strata than in the more elevated regions. But for the hull of the vessel to be invisible, and for the contours of the objects, examined with a telescope at fourteen metres above the level of the water, to be perfectly well-defined, the warmest zone of air must have terminated under the wind toward the base of the sails, that is to say, at three or four metres above the surface of the lake. The existence of a zone thus limited is therefore possible with the bise blowing sufficiently strong during the few minutes necessary for the observation. This fact recalls the persistence of the undulation in the strata of air which are close to the roofs and the ground during the warm hours of a summer's day, or above the chimneys in winter, notwithstanding the agitation produced by intense winds.

II. *On blue rays.*—On the 30th of November last, a little before sunrise, M. Thury perceived at Nyon, above the mountains which border the lake on the east, horizontal strata of light clouds tinged with a beautiful yellow. The sky, seen in the spaces between them, was of a deep azure colour. Toward the point of the horizon where the sun was about to appear, a dark blue ray rose divergingly up to a great height, and occupied a space in which no cloud was perceptible. This appearance vanished after two or three minutes.

Dr. Gosse has found, among his father's papers, the account of an analogous observation made at Lyons toward the end of the last

* *Lehrbuch der Meteorologie,* vol. iii. p. 87.—*Cours Complet de Météorologie,* translated by Ch. Martins, p. 422.

† Gilbert's *Annalen der Physik,* vol. iii. p. 397.

‡ Philosophical Transactions, 1799, p. 13.

century by this venerable founder of the Helvetic Society of Natural Sciences. The manuscript unfortunately has not been preserved.

What has been called a *ray* appears to me, on the contrary, to be a phænomenon of shadow. The light of the sun, arrested by some obstacle out of sight, left the transparent vapours situated near the horizon invisible in a determinate region of the sky, whilst to the right and left it tinged them by playing upon them. The space not illumined should offer to the eye a blue colour, the more remarkable as it contrasted strongly with those of the adjacent strata of a golden yellow. The diverging form of the ray is a well-known illusion in perspective. What apparently increased the separation of its margins toward the upper part, was the less quantity of vapour existing in elevated regions.

It is to be regretted that M. Thury was not able to measure the height of the ray; this would have aided in determining the position and perhaps the nature of the screen (*écran*) which produced it. If this was one of the principal summits of the Savoy Alps, the appearance should recur periodically toward the 30th of November and the 13th of January, when the atmospheric conditions are favourable. Observers placed in proper localities would easily decide the truth of this supposition.

III. *On solitary crepuscular rays.*—In 1846* I described a meteor of a character quite different from that of the blue rays, and which consisted of a single, vertical, luminous band, 35° high, without any trace of divergence. I afterwards found that Professor Christie had twice seen a phænomenon nearly analogous to this in 1834†. The rays examined by Mr. Christie were less extended than the band seen at Lausanne and Geneva. Moreover they had a perceptible divergence, whilst the edges of the latter were absolutely parallel. Lastly, on the 31st of May 1846, there was such a bise blowing that the sky was perfectly clear. The meteorological registers of Saint-Bernard, Lausanne and Geneva, prove this. It therefore does not seem that these various appearances can be entirely assimilated. The theory proposed by the English savant must be subjected to new observations, as he himself admits. It will be useful to make, with the polariscope, some researches on the state of the light of the solitary rays, and of that of the atmosphere in the adjacent parts.—*Extracted from the Bibliothèque de Universelle Genève.*

ON THE REFLEXIONS OF DIFFERENT KINDS OF HEAT BY METALS. BY MM. F. DE LA PROVOSTAYE AND P. DESAINS.

Those philosophers who have been occupied with the study of heat, seem to admit that the rays of different natures are reflected in the same proportion on polished metals.

On the other hand, the very precise experiments of M. Jamin

* *Archives des Sciences Phys. et Natur.*, vol. ii. p. 166.

† Report of the Fourth Meeting of the British Association for the Advancement of Science: Transactions of the Sections, p. 566. London, 1835.

agree with the formulæ of M. Cauchy, to prove that the intensity of metallic luminous reflexion depends on the colour of the light employed. The numerous analogies which exist between heat and light, scarcely admit of an essential difference on this head. The authors, indeed, are of opinion, that they have in fact proved by experiment that this difference does not exist, and that the rays of heat of different natures are reflected in very unequal proportions on the same metallic mirror.

The plan adopted and followed by the authors is precisely the same as that previously followed in their Researches on Metallic Reflectors. The source of heat was always a Locatelli's lamp; only they operated successively with the direct rays, and with the same rays transmitted, sometimes through a plate of natural sal gem, and at others with it smoked, and lastly through a lamina of glass 5 millimetres in thickness. The incidence of the rays being about 60°, the following results were obtained:—

Experiments made with the Metal of the Reflectors of Telescopes.—The metal of the reflector employed reflected 0·80 or 0·84 of the direct heat of Locatelli's lamp. It reflected only 0·74 of the heat derived from the same source when modified by passing through a lamina of glass of $0^{m}\cdot005$ in thickness. Lastly, it reflected 0·82 to 0·83 of the same heat transmitted through sal gem.

Experiments with Silver.—The silver mirror reflected 0·95 to 0·96 of the natural heat, and 0·91 of the heat which had passed through 0·005 of glass.

Experiments with Platina.—The platina employed reflected 0·79 of the natural heat; 0·77 to 0·78 of the heat which had traversed sal gem; 65 to 66 of that which had traversed $0^{m}\cdot005$ of glass; and lastly, 83 of that which had passed through smoked sal gem.

Some experiments were also made with plates of gold and unpolished silver, which were employed by the authors in an investigation respecting the diffusion of heat. The proportion of the incident flow which these plates reflect back to the pole when it is placed in the direction of the regular reflexion, is extremely different, according as the heat has previously traversed glass or sal gem.

It results from these numbers, that the heat most transmissible through glass is reflected in smaller proportion on the various metals tried; and that the heat, which is transmitted in larger proportion through smoked sal gem, is reflected more abundantly upon the same substances. A marked consequence of these experiments is, that a bundle of heat rays reflected on a metallic mirror has generally a composition entirely different from that of the incident bundle, and that consequently it should not suffer the same loss in traversing diathermanous substances. This has been directly verified by the authors in the following manner:—

1. They determined the loss of intensity which the heat of a Locatelli's lamp suffered in traversing a lamina of glass of $0^{m}\cdot005$ in thickness.

2. The loss suffered by heat from the same source twice reflected on parallel mirrors.

In the first case the lamina of glass employed transmitted 0·44 of the incident heat; in the second only 0·33 to 0·34.

These two methods afford then concordant results; and the authors are of opinion that they have determined that, on a great number of metals, and probably on all, the different kinds of heat are reflected unequally, and that the reflexion on polished metals changes the proportions of the different kinds of heat which compose the incident bundle.—*Comptes Rendus*, Avril 16, 1849.

ON CHLORONICEIC ACID. BY M. E. SAINT-EVRE.

For the preparation of this acid, the first operation consists in passing a current of moist chlorine gas into a cold and strongly alkaline solution of benzoate of potash. After many trials, the proportions which succeeded best were found to be 60 grms. of benzoic acid, 200 grms. of hydrate of potash, and 300 to 350 grms. of water, according to the degree of hydratation of the potash of commerce, which is far from being constant.

The potash is to be dissolved at a gentle heat, and the benzoic acid afterwards added; and the chlorine is not to be passed till everything is dissolved. After some time the solution assumes successive shades of yellow, greenish-yellow and bright green; it then again becomes yellow, and eventually deposits an abundant pulpy compound, which is grayish and crystalline. An abundant evolution of carbonic acid takes place during the operation, which is readily detected by passing it into a vessel containing barytes water. An operation performed with the quantities above described continues about two days.

The precipitate is composed,—1st, of chlorate of potash, the crystallization of which is completely modified by the presence of organic matter, the salt being usually obtained in the form of four-sided prisms; they are long, hard and brittle; 2nd, of a small quantity of unaltered benzoate of potash; 3rd, of a salt of potash containing the new acid; and lastly, the supernatant liquor contains benzoate of potash and chloride of potassium.

About half its volume of water is to be added to the mass. The solution is to be saturated, at a moderate temperature, by means of a current of carbonic acid, the saturation being completed by the addition of a small quantity of dilute hydrochloric acid. The solution is then to be boiled. The magma which is precipitated gradually redissolves, and there soon appears an oleaginous substance which is fusible at about 270° F.; it is of an amber colour, and heavier than water. It sometimes precipitates to the bottom of the capsule, and sometimes, on the contrary, it floats, according to the degree of concentration of the solution. The solution being poured off, an oily matter remains, which soon concretes by cooling.

The crude acid thus prepared is hard, brittle, of a slight yellow colour, and contains a notable quantity of benzoic acid. It is freed from this by repeated solutions in boiling distilled water; and the purification is eventually completed by repeated crystallizations in alcohol, or in a mixture of alcohol and æther.

The benzoic acid separated in this manner is added to that which the decanted liquor contains, which is then strained through a cloth and washed with cold water. It is to be carefully pressed; and by treating it in a capsule with a small quantity of hot water, a fresh and often considerable quantity of the new acid is separated, which had been dissolved during the first separation.

The pure acid consists of granular crystals of a cauliflower form. When examined by the microscope, they present the form of four-sided acicular prisms. It melts at 302°, and when melted its density is 1·29; it boils at 270° F. It volatilizes without decomposing, and is deposited during distillation on the sides of the vessel in flat needles of a greasy lustre and grouped around a common centre.

Its odour, especially when it has been melted, is sharp and penetrating, like that of all chlorinated compounds in general, but entirely different from that of the acid from which it is derived.

Analysis showed that it was composed of—

C^{24}	72	50·00
H^{10}	5	3·47
Cl^{2}	35	24·30
O^{4}	32	22·23
	144	100·00

When chloroniceic acid is treated with fuming sulphuric acid, they combine so as to form with barytes a soluble salt, probably represented by $2SO^3, C^{24} H^8 Cl^2 O^3, BaO, H^2 O$, like the corresponding sulphobenzoate.

When distilled with lime or barytes, with proper precautions, two hydrocarburets are formed; the first is liquid and the second solid. It resists long-continued exposure to the action of dry chlorine, even under the influence of heat, and also the dechlorizing action of the amalgam of potassium.—*Ann. de Chim. et de Phys.*, Avril 1849.

ON THE NATURE AND COMPOSITION OF VARIOUS CHLORONICEATES. BY M. E. SAINT-EVRE.

Chloroniceate of Ammonia.—When freshly prepared by the direct saturation of [chloro ?] niceic acid dissolved in alcohol, [chloro ?] niceate of ammonia crystallizes in large micaceous laminæ, which undergo change by the action of light, becoming brown and acid to litmus-paper. When pure, this salt is fusible and volatile, without undergoing decomposition. By analysis it appeared to be composed of—

C^{24}	72	44·72
H^{16}	8	4·96
Cl^{2}	35	21·73
O^{4}	32	19·90
Az^{2}	14	8·69
	161	100·00

Chloroniceate of Barytes.—This salt is a white crystalline powder, which is slightly soluble in water, but readily so in hot alcohol. It is decomposed by heat, yielding a mixture of two hydrocarbons, one solid, the other liquid; and a coaly residue is formed. It appears to be composed of—

C^{24}	72	34·12
H^8	4	1·89
Cl^2	35	16·58
O^4	32	15·19
Ba	68	32·22
	211	100·00

Chloroniceate of Silver.—When prepared in the usual manner, in alcoholic liquors, this salt is precipitated in the form of white flocculi, which washing and drying convert into a crystalline powder. By analysis it yielded—

C^{24}	72	28·68
H^8	4	1·59
Cl^2	35	13·94
Ag	108	43·02
O^4	32	12·77
	251	100·00

Ann. de Chim. et de Phys., Avril 1849.

ON THE REACTION OF SULPHATE OF POTASH AND SULPHATE OF COPPER. BY M. J. PERSOZ.

When a saturated solution of sulphate of potash containing some sulphate of copper is made to boil, it becomes in a very short time extremely acid, and yields a very dense precipitate, which adheres to the bottom of the vessel, and causes so much bumping that it is requisite to use a porcelain capsule.

In the first experiment, 210 grms. of sulphate of potash were dissolved in 1·5 litre of water; and 150 grms. of sulphate of copper in 1·2 litre.

These solutions, previously filtered, were mixed, and the solution was kept boiling for an hour, and then suffered to cool; then having poured off the acid liquor, and washed the triple salt formed till the washings ceased to be acted upon by ferrocyanide of potassium, the salt was pressed between folds of blotting-paper, and dried in a stove at 212° F. This triple salt weighed 9·95 grms.

The second experiment, made with the same proportions of the salts, but with less water, yielded 27·25 grms. of triple salt.

Lastly, a third experiment was performed, in which 210 grms. of sulphate of potash were dissolved in 1·2 litre of hot water; and when made to boil, 155 grms. of crystallized sulphate of copper were added in small portions to it, so as not to reduce the temperature. In a quarter of an hour an abundant precipitate of the triple salt was obtained, which, washed with cold water, expressed and dried by the water-bath, weighed 56·7 grms. When the sulphate of potash is added to the sulphate of copper, the same quantity of precipitate is not obtained.

It results from the above-stated experiments, that the quantities of the triple salt vary according to the proportions of water, and whether the sulphate of copper is added to the sulphate of potash, or the reverse; when the proportion of sulphate of potash is increased, a larger quantity of the sulphate of copper is decomposed, but without in any case obtaining bisulphate of potash and the triple salt only.

As the proportions of the triple salt thus formed are variable, it is natural that the solutions which yield it should differ in composition; some are found to contain much of the copper salt, whilst in others the sulphate of potash is in excess.

On subjecting these solutions to careful evaporation, crystals of a double salt, $\dddot{S}\,\dot{Cu}\,\dddot{S}\dot{K}+6H^2O$, are formed, which by two or three successive concentrations separate completely. It is a curious phænomenon to observe such very soluble salts produced so perfect by simple crystallization. The mother-waters eventually resulting from these crystallizations are merely bisulphate of potash.—*Ann. de Chim. et de Phys.*, Mars 1849.

ON OCTOHEDRAL AND CUBIC ALUM. BY M. J. PERSOZ.

It is well known, that when a solution of octohedral alum is saturated with potash, or for a short time put in contact with trissulphate of alumina, it cannot be heated to 140° F. without becoming turbid; and there are formed octohedral alum, soluble at all temperatures, and trissulphate of alumina, which is precipitated. When the solution, however, instead of being subjected to so high a temperature, is subjected to evaporation at a gentle heat, cubic alum is obtained, which readily becomes octohedral alum by dissolving it in water slightly acidified with sulphuric acid. It may then be evaporated and redissolved at pleasure without undergoing any alteration. Lastly, if a certain quantity of cubic alum be dissolved in water and boiled, it yields basic sulphate of alumina insoluble in water; and the mother-water and that used in washing, when mixed and evaporated, give only octohedral alum. Hence it is concluded that these two alums are not identical, and that cubic alum contains most alumina.—*Ann. de Chim. et de Phys.*, Mars 1849.

ON ANISOL AND ITS DERIVATIVES. BY M. A. CAHOURS.

Anisol presenting with respect to toluol (benzoène of M. Deville) the same relations of composition that phénol does to benzene, the author resumed the examination of this product, in order to complete his researches respecting the compounds of the anisic series.

It has been shown that anisol treated with fuming nitric acid exchanged 2 or 3 equivalents of hydrogen for 2 or 3 equivalents of hypoazotic vapour. There was therefore wanting in this series of the derivatives of anisol, its first term, that is to say, that which would result from the replacement of 1 equivalent of hydrogen by

1 equivalent of hypoazotic vapour, and which the author calls in the nomenclature adopted for these compounds mononitric anisol. M. Cahours has succeeded in obtaining it by treating anisol with small portions of fuming nitric acid, taking care to keep the vessel containing the reacting substances extremely cold. Operating with these precautions, a thick liquid of a blackish blue is obtained, which is purified by submitting it, at first, to repeated washings with slightly alkaline water, and afterwards by distilling it, having first digested it over fused chloride of calcium. Thus prepared, mononitric anisol is a liquid of an amber colour and heavier than water. It boils between 503° and 507° F., and possesses an aromatic odour. Solution of potash, even when heated, does not alter it. Concentrated sulphuric acid dissolves it when gently heated, and water added to the solution separates the product from the liquor unaltered. Treated with an alcoholic solution of hydrosulphate of ammonia, it is readily acted upon; sulphur is deposited, and the alcohol holds in solution a new organic base, which differs from toluidine only in containing two molecules of oxygen.

Mononitric anisol submitted to analysis yielded nearly—

14 eqs.	Carbon........	84	54·90
7 ...	Hydrogen......	7	4·57
1 ...	Nitrogen	14	9·14
6 ...	Oxygen........	48	31·39
		153	100·00

The substance thus formed differs from anisol by the substitution of one equivalent of hypoazotic acid for one equivalent of hydrogen, which justifies the name of mononitric anisol bestowed upon it.

The analysis of the new base, formed by the action of mononitric anisol and hydrosulphate of ammonia, leads to the formula $C^{14}H^{9}NO^{2}$; it forms a crystalline salt with hydrochloric acid. As mononitric anisol is prepared with difficulty, M. Cahours has obtained only a small quantity of it, and he proposes to call it *anisidine*.

Benzene and binitric cumene being easily acted upon by hydrosulphate of ammonia, and transformed into nitric alkaloids, binitric anisol was submitted to the same reagent. By treating an alcoholic solution of binitric anisol with hydrosulphate of ammonia, an abundant deposit of sulphur is obtained, whilst the alcohol retains in solution a substance which perfectly saturates acids, and forms with them crystallizable acids [salts ?].

The new base thus formed crystallizes in long needles of a reddish brown colour, possessing much lustre; it is insoluble in water, but dissolves readily in boiling alcohol, the greater portion separating on cooling. This alkaloid yields well-formed crystalline salts with sulphuric, nitric and hydrochloric acids; some of them are perfectly colourless when pure.

The analysis of this substance indicated its composition to be—

14 eqs. of	Carbon	84	50·00
8 ...	Hydrogen	8	4·76
2 ...	Nitrogen......	28	16·67
6 ...	Oxygen	48	28·57
		168	100·00

It will be observed that this substance differs from the preceding only in one equivalent of hydrogen being replaced by one equivalent of hypoazotic vapour; for this reason the author gives it the name of anisidine nitrée. This base forms with hydrochloric acid, a colourless salt crystallized in long needles, represented by the formula ClH, $C^{14}H^{8}N^{2}O^{6}$.

The chloroplatinate crystallizes in needles of a golden yellow colour; its formula is ClH, $PtCl^{2}$, $C^{14}H^{8}N^{2}O^{6}$; the nitrate has the form of prisms of considerable size, which are slightly soluble in water; the formula is NO^{5}, HO, $C^{14}H^{8}N^{2}O^{6}$; the sulphate is very soluble in water, it crystallizes in very fine needles, grouped around a common centre; its formula is SO^{3}, $C^{14}H^{8}N^{2}O^{6}$.

When toluol is treated with fuming nitric acid, it forms two compounds, one of which is liquid, and is the mononitric toluol; the other is crystallized, and is the binitric toluol; when the latter was treated with an alcoholic solution of hydrosulphate of ammonia, it yielded a very fine alkaloid corresponding to anisidine nitrée, differing from it only by two equivalents of oxygen. This new alkali the author calls toluidine nitrée; its formula is $C^{14}H^{8}N^{2}O^{4}$.

The number of alkaloids increases daily; their study affords results of great interest, and the hope may be entertained that those presented by nature may eventually be formed by art. M. Würtz has described two very remarkable alkalies obtained by the action of potash on cyanic æther, alcohol, and pyroxylic spirit; petinine, recently discovered by M. Anderson in the products of the distillation of animal matters, is to be added to the group. The strong ammoniacal odour, the manifest analogy of the properties of its salts, with those of the salts formed by the alkalies of M. Würtz, induced M. Cahours to suppose that petinine belongs to this series. Adopting the formula $C^{8}H^{15}N$, proposed by M. Gerhardt, from the analysis of the chloroplatinate, it will be seen that petinine is merely butyrammonia $C^{8}H^{9}$, NH^{2}. M. Anderson has also noticed, in the oil derived from the distillation of animal substances, some very volatile alkaline products, among which will probably be found the curious alkalies of M. Würtz.

When fuming nitric acid is made to react upon anisic acid, or nitranisic acid, binitric or trinitric anisol is formed, according to the proportion of the matters reacting and the duration of the reaction; besides these two substances, there is formed, and often in great abundance, an acid which crystallizes from an alcoholic solution as it cools, in the form of rhomboidal plates of a magnificent golden yellow colour; this acid, which M. Cahours calls chrysanisic acid, has a very remarkable composition: it is isomeric with trinitric anisol; consequently it is an homologue of picric acid (phénol trinitrée).

This acid submitted to analysis gave—

14	eqs. of Carbon	84	34·57
5	... Hydrogen	5	2·05
3	... Nitrogen......	42	17·29
14	... Oxygen	112	46·09
		243	100·

This acid, differently from all others of the same kind, forms a very soluble salt with potash.—*Comptes Rendus*, March 19, 1849.

COMPOUNDS OF HYDROCHLORATE OF STRYCHNIA AND CYANIDE OF MERCURY.

M. Brandes states that when a mixture is made of hydrochlorate of strychnia with one of cyanide of mercury, a crystalline precipitate is obtained, the composition of which has been hitherto unknown. The author thinks that it is a combination of hydrochlorate of strychnia and cyanide of mercury, corresponding to the formula Str, HCl + 4 Hg Cl.—*Journ. de Ph. et de Ch.*, Janvier 1849.

METEOROLOGICAL OBSERVATIONS FOR APRIL 1849.

Chiswick.—April 1. Rain: fine: showery. 2. Densely clouded. 3. Fine: cloudy. 4. Foggy: rain. 5. Drizzly: fine. 6. Heavy dew: very fine. 7. Cloudy: drizzly. 8. Hazy. 9. Foggy: densely overcast. 10. Hazy: heavy clouds. 11. Cloudy and cold: clear and frosty at night. 12. Cloudy. 13. Rain. 14. Slight haze: fine: clear and frosty. 15. Rain. 16. Cloudy throughout. 17. Cold and dry: clear and frosty at night. 18. Clear: snow: cloudy. 19. Heavy fall of rain, sleet and hail throughout the day. 20. Snow and hail in forenoon, stormy showers: clear and frosty at night. 21. Clear: cloudy: clear and frosty. 22. Overcast: rain at night. 23. Rain. 24. Cloudy. 25. Drizzly: fine. 26. Rain: cloudy: clear: slight frost. 27. Foggy: cloudy: slight rain. 28. Fine: heavy showers: partly hail. 29. Slight haze: very fine: overcast. 30. Fine: clear.

Mean temperature of the month	44°·29
Mean temperature of April 1848	47 ·33
Mean temperature of April for the last twenty-three years	47 ·53
Average amount of rain in April	1·46 inch.

Boston.—April 1. Rain: rain A.M. 2. Cloudy: rain P.M. 3. Cloudy. 4. Fine. 5. Rain: rain A.M. and P.M. 6. Fine. 7. Fine: rain P.M. 8, 9. Rain: rain A.M. and P.M. 10. Rain: rain A.M. 11, 12. Cloudy. 13. Rain: rain A.M. and rain and snow P.M. 14. Cloudy: rain A.M. 15, 16. Cloudy. 17. Fine: snow P.M.: stormy. 18. Fine: rain and snow P.M. 19, 20. Cloudy. 21. Fine. 22, 23. Fine: rain P.M. 24, 25. Fine. 26. Cloudy: rain early A.M. 27. Fine. 28. Fine: rain P.M. 29, 30. Fine.

Applegarth Manse, Dumfries-shire.—April 1. Showers: thunder. 2. Spring showers. 3. Showers: thunder. 4. Frost: calm and fine. 5. Showers. 6. Fair: beautiful day. 7. Fair, but rigid and ungenial. 8. Slight shower A.M.: parching wind. 9, 10. Fair and chilly. 11. Fair and chilly: frost A.M. 12. Cloudy A.M.: rain P.M. 13. Frost: snow: hail: fine P.M. 14. Snow an inch deep: rain P.M. 15. Fair and fine. 16. Frost: slight shower of snow. 17. Frost very hard: shower of snow. 18. Frost: snow-showers: rain P.M. 19. Frost: shower of snow: fair and keen P.M. 20. Frost: clear and cold: slight shower of snow. 21. Frost very hard: clear and cold. 22. No frost: rain gentle: cloudy. 23. Rain: soft and warm: blessed change of weather. 24. Fine A.M.: grew cloudy: rain P.M. 25. Fine A.M.: shower of hail: rain. 26. Fine A.M.: shower. 27. Shower early: rain and wind P.M. 28. Growing day: one slight shower. 29. Fair and fine: cloudy P.M. 30. Most beautiful spring day.

Mean temperature of the month	42°·3
Mean temperature of April 1848	43 ·2
Mean temperature of April for the last twenty-five years	44 ·4
Rain in April 1848	2·52 inches.
Average amount of rain in April for the last twenty years	1·76 „

Sandwick Manse, Orkney.—April 1. Cloudy: rain. 2. Showers: drizzle. 3. Damp: showers. 4. Drizzle. 5. Damp: showers. 6. Damp. 7. Damp: cloudy. 8. Bright: clear. 9. Bright: cloudy. 10, 11. Cloudy. 12. Showers: cloudy. 13. Bright: cloudy. 14. Bright: clear: aurora. 15. Cloudy. 16. Snow-showers: snow. 17. Snow-showers: drift: snow-showers. 18. Snow-showers: frost: aurora. 19. Sleet: hail-showers: aurora. 20. Bright: frost: clear: aurora. 21. Bright: clear: aurora. 22. Cloudy. 23. Drops: cloudy. 24. Bright: hazy. 25. Clear. 26. Cloudy. 27. Clear. 28. Clear: very clear. 29, 30. Fine: cloudy.

Meteorological Observations made by Mr. Thompson *at the Garden of the Horticultural Society at* Chiswick, *near London; by* Mr. Veall, *at* Boston; *by the* Rev. W. Dunbar, *at Applegarth Manse,* Dumfries-shire; *and by the* Rev. C. Clouston, *at Sandwick Manse,* Orkney.

Days of Month.	Barometer.							Thermometer.								Wind.				Rain.			
	Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		Orkney, Sandwick.			Chiswick. 1 p.m.	Boston.	Dumfries-shire.	Orkney, Sandwick.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.
1849. April.	Max.	Min.		9 a.m.	9 p.m.	9½ a.m.	8½ p.m.	Max.	Min.		Max.	Min.	9¾ a.m.	8¼ p.m.								
1.	29·524	29·448	29·20	29·34	29·22	29·47	29·41	55	40	45	52	37½	47	43	sw.	se.	sse.	se.	·12	·30	......	......
2.	29·437	29·410	29·12	29·28	29·38	29·45	29·56	56	26	49	53	41	43	43	s.	sse.	sse.	ese.	·10	......	......	·42
3.	29·684	29·526	29·17	29·43	29·50	29·67	29·78	58	26	43	50½	37½	44½	43	sw.	sse.	sw.	se.	......	·15	......	·03
4.	29·656	29·420	29·27	29·51	29·42	29·65	29·73	56	42	45	51½	32½	44	42	s.	s.	ese.	se.	·08	......	......	·13
5.	29·556	29·381	29·11	29·40	29·39	29·78	29·59	57	25	46	45½	36½	44	42	sw.	se.	ese.	ese.	·01	·08	......	......
6.	29·609	29·440	29·16	29·38	29·32	29·48	29·54	63	38	51	56	42½	46	43	se.	s.	se.	se.	......	·01	......	·05
○ 7.	29·514	29·364	29·06	29·31	29·41	29·72	29·84	55	34	50	52	42½	41½	40	sw.	e.	e.	e.	·05	......	......	......
8.	29·429	29·346	29·00	29·40	29·48	29·85	29·92	61	31	47	46	38½	42	39½	sw.	ne.	ene.	e.	·02	·20	1·00	·04
9.	29·536	29·459	29·20	29·60	29·70	29·95	29·98	52	42	44	44½	39	43	41	ne.	e.	e.	nne.	......	·46	......	......
10.	29·664	29·538	29·28	29·72	29·80	29·99	29·96	50	34	42	47	36	45½	40	ne.	nne.	e.	nnw.	·04	·24	......	......
11.	29·800	29·691	29·40	29·74	29·76	29·91	29·84	47	25	41	49	32½	40	39½	ne.	n.	ne.	wnw.	......	·02	......	·06
12.	29·819	29·598	29·47	29·65	29·30	29·34	29·23	51	38	41	47½	34	40½	36	nw.	nw.	sw.	w.	·01	......	......	·09
13.	29·341	29·218	28·97	29·15	29·13	29·22	29·46	51	25	41	47½	28	40½	38	sw.	s.	se.	e.	·12	·14	......	......
14.	29·533	29·288	29·02	29·25	29·54	29·76	29·92	55	32	39·5	43½	32½	45	38	sw.	s.	se.	e.	·03	·27	......	·11
☾ 15.	29·719	29·584	29·38	29·75	29·78	29·93	29·77	50	36	44	46½	36	45	43	e.	s.	ne.	w.	·08	·25	......	......
16.	29·785	29·719	29·96	29·69	29·64	29·68	29·71	50	34	41·5	50	30½	34	29	n.	n.	ne.	n.	......	......	0·67	·08
17.	29·776	29·718	29·40	29·68	29·70	29·69	29·87	47	25	36	38	23	32	33	nw.	nw.	nw.	nw.	·02	......	......	·19
18.	29·921	29·861	29·55	29·77	29·26	29·68	29·53	50	32	38	41½	29	37	33	w.	nw.	nw.sw	ne.	·28	·04	......	·09
19.	29·484	29·204	29·15	29·50	29·60	29·65	29·74	39	32	39	46½	31½	33	33	ne.	e.	n.	nw.	·45	·04	......	·27
20.	29·806	29·564	29·32	29·70	29·72	29·76	29·76	47	26	38·5	46½	29	38	33	n.	n.	n.	e.	·01	......	......	·05
21.	29·926	29·864	29·02	29·70	29·77	29·80	29·86	48	27	41	51	25½	43	37	w.	nnw.	se.	ese.	......	......	......	·01
● 22.	29·930	29·762	29·60	29·76	29·56	29·77	29·65	48	39	45	44	35½	42½	40½	sw.	se.	e.	sse.	·22	......	......	......
23.	29·633	29·561	29·26	29·49	29·53	29·56	29·47	50	38	49	51½	39	45	43	sw.	nnw.	nne.	se.	·21	·22	0·65	·10
24.	29·894	29·704	29·40	29·57	29·52	29·35	29·24	56	43	50	53	38	48	43½	n.	e.	sw.	se.	......	·28	......	......
25.	29·745	29·728	29·30	29·51	29·55	29·15	29·25	62	44	52	55	39½	44	43	w.	w.	sw.	w.	·03	......	......	·15
26.	29·836	29·724	29·30	29·52	29·57	29·25	29·21	60	29	51	53	43	49	42½	nw.	sw.	sw.	s.	·01	·04	......	......
27.	29·838	29·650	29·37	29·53	29·29	29·23	29·19	61	40	53	55½	36	49	45	s.	w.	sw.	ssw.	·04	......	......	·04
28.	30·000	29·681	29·20	29·46	29·74	29·20	29·49	59	32	54	56	39½	49	44	w.	wsw.	sw.	se.	·28	·02	......	......
☽ 29.	30·260	30·176	29·73	29·98	30·10	29·66	29·74	68	45	56	59½	35	53	49	sw.	calm	s.	ese.	......	·07	......	......
30.	30·284	30·208	29·90	30·16	30·16	29·87	29·96	66	35	58	64½	46	57	47	e.	e.	s.	sse.	......	......	0·20	......
Mean.	29·731	29·594	29·31	29·564	29·561	29·612	29·640	54·76	33·83	45·7	49·6	35·5	43·50	40·21					2·21	2·83	2·52	1·91

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SUPPLEMENT TO VOL. XXXIV. THIRD SERIES.

LXXI. *On Circular Magnetic Polarization.* *By* M. A. Bertin*.

SINCE the discovery of Prof. Faraday, circular magnetic polarization has formed the subject of several important investigations; but nevertheless there still remain some conditions of the phænomenon to discuss, some conclusions to verify, some obscure points to clear up. I have attempted to do this, aided by the valuable assistance of MM. Pouillet and Edmund Becquerel, who have kindly placed at my disposal the apparatus they made use of in their researches on this subject.

The method of experimenting is now too well known to require description. I shall only say that all the numbers referred to in this paper represent the *total rotation* produced in the plane of polarization by a change in the direction of the current. It is this total rotation which I have always measured, as it is immediately presented in the experiment; because, corresponding to a more intense phænomenon, it is for that very reason measured with greater accuracy; and lastly, because it is independent of the determination, always very uncertain, of the zero, that is to say of the position of the analyser, for which the light was extinguished before the passage of the current.

As is seen, the measurement of the rotation results from the determination of two planes of polarization, or of two tints of passage; each of these observations being liable to an error of $\frac{1}{4}$ degree, it is necessary to admit the possibility of an error of $\frac{1}{2}$ degree in the rotation, which moreover also is subject to the influence of small irregularities in the transmission of the current by the commutator. The variations which affect the cur-

* From the *Annales de Chimie et de Physique*, vol. xxiii. p. 5.

rent itself during a series of experiments would produce differences far more considerable, which must be avoided by comparing only the rotations observed at short intervals, and, as it were, the one following the other.

It has been observed, that for the success of these experiments it is indispensable that the glasses experimented with should be annealed; but happily this is not the case, otherwise these researches would be impossible, since the greater part of them are unannealed, or soon become so. When such a glass is suitably placed between two crossed Nichol's prisms, one or several black lines appear in it, which serve as index points. On viewing one of these lines, which may nearly always be isolated in the field of vision, it is seen to disappear when the current passes; and it is again found, on turning the analyser, absolutely like the black image of annealed glass. In white light it experiences the same variations of tints as this, and it is always easy to determine the azimuth in which it presents the tint of passage. It is true that, in consequence of the movement of the plane of polarization, it is not the same black ray which reappears, but another slightly different. However, the rotation is not influenced by this circumstance, for I have ascertained that it is independent of the black ray to which the eye is directed.

The most happy modification which has been made in the original apparatus of Prof. Faraday, has been to cause the ray of light to pass, not merely near the line of the poles, but through this line itself, by piercing the electro-magnet in this direction. This condition is satisfied in M. E. Becquerel's electro-magnet by the perforated terminations which he places on the two poles*, and to them must be attributed the greater part of the force of this apparatus. The following table leaves no doubt on this point.

Rotations observed with the Electro-magnet of M. Becquerel.

Substances placed between the two poles.			With the terminations.	Without the terminations.
	mm.		° ′	° ′
Very thick flint-glass	55·1	thickness.	21 00	4 30
Faraday's flint-glass†	48·3	...	25 6	6 30
... ...	18·3	...	18 20	2 30
Distilled water	130·0	...	5 30	3 00
... ...	30·0	...	3 50	0 00

* *Ann. de Chim. et de Phys.*, third series, vol. xvii. p. 437.

† I am indebted to M. Dumas for this flint-glass.

The same condition is still better satisfied in Ruhmkorff's* apparatus, where the helices themselves are pierced in the direction of their axes. The Ecole Normale has such an apparatus, 54 kilogrammes in weight, which is as powerful as that belonging to M. Becquerel, although it is not more than one-third its weight.

The effect produced by an electro-magnet of the same shape depends on the mass. Thus, an apparatus one-fifth the size of the preceding, produced, under the same circumstances, rotations twice as feeble.

The dimensions of the wire have also a certain influence. In general it should be thick. That used in the Ruhmkorff apparatus is 2·50 millims. in diameter; and in M. Becquerel's electro-magnet the maximum effect is obtained by doubling the section of the conducting wire. With respect to the mass employed, the instrument-makers at present are in the habit of rolling around the iron nucleus a thickness of wire equal to its radius, so that the external diameter of the reel is twice that of the interior cylinder.

Lastly, the intensity of the phænomenon likewise depends on the current, or rather on the relation existing between the dimensions of the electro-magnet, and the force of the battery exciting it; so that a very powerful apparatus may appear very feeble when it is not set in action by a battery of sufficient strength. With the same apparatus the intensity of the effects produced increases with the intensity of the current; as to this, it increases with the number of the elements of the battery, but is far from being proportional to it. Again, when the current has a certain force, it is more advantageous to increase the quantity of electricity than the tension; that is to say, it is better to enlarge the surface than the number of the elements of the battery. It is therefore desirable to ascertain what is the arrangement for a given battery which will produce the maximum effect. Thus, having at my disposal 80 Bunsen's elements, I have found that the best arrangement to be given to them in order to excite the great Ruhmkorff apparatus, was to join, by poles of the same name, four batteries of 20 elements. The results just stated follow from the subjoined table, which contains the rotations observed in one of Faraday's flint-glasses of 39 millimetres in length placed between the two poles of the Ruhmkorff apparatus.

* *Ann. de Chim. et de Phys.*, third series, vol. xviii. p. 318.

Number of elements.	Tension of the battery.	Surface of the battery.	Rotation observed.
80	80	1	23° 30′
80	40	...	25 20
80	20	4	26 30
40	40	1	20 30
40	20	2	22 30
40	10	4	18 30
20	20	1	17 30
20	10	2	16 40
20	5	4	13 00
10	10	1	14 30
5	5	1	10 00
1 selected element	1	1	9 30

By means of the above-mentioned apparatus I have been able to make the experiment so as to render it visible at a public lecture. The arrangement presents no other difficulties than those resulting from the narrowness of the beam of light traversing the electro-magnet. I therefore removed the diaphragms placed at the extremities of the helices, and was then able to work with a ray of light of two centimetres in diameter. This ray proceeded from a lamp of M. Soleil's construction, placed before one of the reels. It traversed successively a polarizer formed of one large pile of glasses, a Faraday's flint-glass, of 48 millimetres, placed between the two reels and in contact with them, an analyser formed of a large doubly refracting prism, and then a convergent lens which projected it upon a screen. One of the two images of the prism having been extinguished while the current passed in a certain direction, it was seen to reappear as soon as the direction of the current was changed; and it was again extinguished, or rather it was brought to the tint of passage by a suitable rotation of the analysing prism. But the experiment is much more striking when a doubly rotating Soleil's plate of quartz is placed behind the polarizer. The lens then projects upon the screen two images of complementary tints, the two halves of which, brought at first to an equality of tints by means of the analyser, change in a contrary direction as soon as the current is altered. On turning the analyser a certain extent, we reobtain an uniformity of tint in each image. This experiment is precisely the same as that of M. Pouillet*.

The *direction of the rotation* impressed on the plane of polarization was correctly recognized by Prof. Faraday, and may be determined in a simple manner. The *rotation has the same direction as the current which produces magnetization*, or rather

* *Comptes Rendus des Séances de l'Académie des Sciences*, vol. xxii. p. 135.

it has the same direction as the current which, according to Ampère, would be established under the influence of an electro-magnet in a piece of soft iron inserted in the place of the substance experimented with.

It may not be without interest to connect this general law with the theory which M. Fresnel has given of circular polarization. After having shown that a ray of light polarized at right angles can be decomposed into two others polarized circularly in a contrary direction, and *vice versâ*, it sufficed for him to suppose that a plate of quartz cut perpendicular to the axis has the property of allowing to pass, with unequal velocities, rays polarized circularly in one or other direction, and all the rotatory properties of the quartz followed from this simple supposition. Let us admit, in like manner, that the presence of the electro-magnet, or of a circular current, which is the same thing, communicates to transparent bodies the property of allowing the circularly polarized rays to pass more easily, the luminous molecules of which rotate in the same direction as the current, and the general law which I have announced will result from this simple hypothesis.

To be convinced of the accuracy of this law, it suffices to observe, as I have done, the direction of the rotation for all positions of the glass, or of the transparent substance in general, in relation to the current.

First. *If the glass is placed between the two poles of the electro-magnet*, two cases may occur.

Either these two poles are directly opposed to the glass, as is the case in Ruhmkorff's apparatus: then there is no doubt as to the direction of the current. For instance, on viewing the glass from the surface which touches the south pole, we see that it is subject to a current proceeding from left to right; and the rotation observed is, in effect, in this direction;—it changes direction with the current. Or the two branches of the electro-magnet, instead of being in the axis of the glass, are perpendicular to it, which takes place in the iron horseshoe electro-magnet, and then the currents are oblique in relation to the glass, or parallel to its axis. But the effect ought always to be the same as above; for a piece of soft iron inserted in the place of the glass would assume the same poles in both cases: only the intensity would be much less; and to increase it, it would be necessary to bring the polar axes nearer to that of the glass. This effect is produced by M. Becquerel's terminations.

Secondly. *In a straight electro-magnetic helix traversed in the direction of its axis by a ray of light*, the current is in the same direction the whole length; consequently the rotation observed ought to be in the same direction, whether the glass is placed in front or behind; and if the helix is viewed from the south pole, a rotation to the left should occur: it would be to the right if the helix were viewed from the other pole, or, which comes to the same thing, the current is changed without altering the position of the eye.

Hence it follows, that in an apparatus formed of two similar helices, the direction of the rotation should be the same when the flint-glass is between the two helices or reels as when it is at the extremities; so that in the whole length of a file of reels thus arranged, the direction of the rotation should not change.

If, then, several glasses are placed in the intervals separating these reels, the rotations produced by all these glasses will add to each other, and thus give the means of multiplying indefinitely the action of a substance, and consequently of rendering that action visible, however feeble it may be.

All these suppositions are confirmed by experiment, as I have found by means of two systems of straight reels. In the one system, consisting of two reels, 28 centimetres in length, and inclosing an iron nucleus, 8 centimetres in diameter, both of them in contact with the flint-glass of Faraday, of 48 millimetres in length, produced a rotation of nine degrees. The other system, consisting of four reels 10 centimetres in length, and inclosing a cylinder of 3 centimetres in diameter, perforated likewise in the direction of the axis; they are centred one after the other in a wooden trough*. This file of reels, including the external ones, presents five intervals in which the substances submitted to magnetism may be placed. Some experiments made with this apparatus are here added.

1st. *Experiment made with five cells containing sulphuret of carbon* 1 *centimetre in thickness.*

	Rotations. °	′
With five cells placed in the five intervals . . .	8	5
With the two outer cells removed	6	25
With only the centre cell left	2	00
The five cells in contact, with two reels on either side	4	00

2nd. *Experiment on water.*

With one cell placed between the first and second reels	0	55
With a second placed between the second and third reels	1	40

* This apparatus was lent to me by M. Pouillet.

	Rotations.
With a third placed between the third and fourth reels	° ′ 2 30
With two reels placed on either side of the three cells	1 20

3rd. *Experiment on flint-glass.*

With a very thick flint-glass, of 55 millimetres, placed between two reels	5 00
With Faraday's flint-glass, of 48 millimetres . .	6 10
With two flint-glasses placed at two different intervals	11 10
The two flint-glasses in contact, with two reels on either side	9 30

The last experiment of each series clearly shows that the increase observed in the rotation depends, not on the increase in thickness of the magnetized substance, but on the distribution of its different layers in the intervals between the reels. I need not observe that, in employing successively all these intervals during the experiment, the reels remained strictly in the same position, and consequently retained their magnetism intact.

Thirdly. *In a helix perpendicular to a polarized ray*, as, for instance, in a vertical reel receiving on its upper base the glass traversed by the light: if this is rotated around the pole, placing it successively on all the radii of the reel, a rotation in the same direction is found on viewing it always from the same surface; for instance, from that which is turned towards the pole; and this rotation is to the right if the pole is southern, to the left if it is northern. The rotation again changes direction when the glass is viewed from the opposite surface.

It follows, therefore, that if the glass is viewed placed in two symmetrical positions in relation to the pole, the position of the analysing prism being fixed, we shall observe rotations of contrary directions. Consequently, if employing the horseshoe electro-magnet, we look through the flint-glass always placed on the line of the poles, but successively at the extremities and then in the middle, we should observe, as M. Pouillet did, rotations of the same direction outside the poles, but in the middle a rotation in the contrary direction.

These positions, where we observe change of direction in the rotation, are separated by others where there is no effect; the latter are precisely at the poles. But it was seen that in this case a rotation was perceived on looking in the axis of the current, or of the magnet supposed to be perforated in this direction.

Wishing to assure myself that in the electro-magnet of M. Becquerel, the axis of which is filled up, the rotation on the pole occurred in the same manner as in the hollow reels where I had observed it, I endeavoured to receive the ray in the axis of the reel by the aid of reflexion. For this purpose I fixed Nörremberg's apparatus on the pole. The tinned horizontal mirror being placed directly on the end face of the electro-magnet, I received on the oblique glass the light of the clouds. This light is, as is known, reflected a first time from above downwards upon this glass; then a second time on the horizontal glass, which sends it vertically from below upwards to the analyser; only, from its being very imperfectly polarized, it is difficult to determine the plane of polarization, and consequently the rotation which it might experience. But this becomes easy by placing upon the crystal bearer the double rotating plate of M. Soleil. The position of the plane of polarization is then determined by that of the analysing prism, which gives the equality of tints in the two halves of the plate. With this arrangement, let us place upon the horizontal mirror the flint-glass of Prof. Faraday. As long as the current does not pass, we observe no change, except that which arises from a slight unannealed condition of the glass; but as soon as the current passes, we see the double rotation plate vary its tints in an extremely brilliant manner; and to re-establish the identity, it will be necessary to turn the analyser ten degrees if the flint-glass is 18 millimetres in thickness, and twenty-one degrees if it is 48. As to the direction of the rotation, it takes place from right to left when the pole is southern, and from left to right when it is the contrary.

This method allows of our observing the action of an electro-magnetic reel parallel to its axis, in another direction than this axis, and the results thus obtained deserve attention.

Let us picture to ourselves the horizontal section of the electro-magnet of M. Becquerel. It is composed of two equal circles corresponding to the two vertical arms, not exactly touching, but only 1 centimetre apart and 23 centimetres in diameter. Each of these circles is formed by an interior circle of 11 centimetres, which is the section of the iron nucleus, surrounded by a copper ring of 6 centimetres in width, appertaining to the reel properly so called. If the flint-glass is moved along the line of the centres whilst the electro-magnet is in action, the following takes place. In the middle, equidistant from the centres, the rotation will be null; it will increase until in contact with the iron, where it will be 9 degrees; then quite close to it, on the iron nucleus, it will increase suddenly to 21 degrees. It will remain nearly fixed throughout the whole extent of this circle except in the centre, where it will be somewhat less; then on the outside of this circle it decreases, but less rapidly than it increased at first, being 13 degrees at the interior portion of the copper ring, 7 degrees at the circumference, and 3 degrees at 1 centimetre distance, which corresponds with the initial position of the centre; finally, it will still be perceptible at more than 1 decimetre.

During this progress the rotation will not have changed direction; it will always take place from right to left if the pole is southern, and from left to right if it is northern. These phænomena are interesting when compared with the directly opposite phænomena, which are observed in the direction of the line of the poles; so that with the same position of the flint-glass the rotation may be right or left, null or very powerful, according as it is viewed parallel or perpendicular to the current. Is it necessary to add, that in all cases the direction of the rotation is always determined by the general law which I have stated at the commencement?

As I have just observed, that which strikes us at first, when Nörremberg's apparatus is employed, is the great intensity of the action observed over the pole. It depends on two causes; one portion must be attributed to the circumstance that the current acts in the very direction of the luminous ray, instead of being oblique to it; but this especially depends on the reflexion of the ray, which is thus compelled to traverse the magnetized substance twice. This double passage through the quartz would have the effect of causing its natural rotatory power to disappear, by producing two equal rotations of contrary direction, because the rotation of the quartz is independent of the direction in which it is viewed. This is also an excellent method of proving the circular magnetic polarization in quartz, as it is requisite first of all to destroy the atomic polarization in this substance, as remarked by M. Becquerel. In magnetized flint-glass, on the contrary, during the double passage of the light through its thickness, the current acts so as to produce two rotations of the same direction, and consequently the effect is found to be double. I convinced myself of this by making two experiments; the first according to the usual method, by looking directly through the flint-glass, and the second by causing the ray of light to traverse it twice by means of Nörremberg's apparatus. The rotation was always twice as great in the second case as in the first. This influence of the reflexion on the intensity of the magneto-rotatory power had already been discovered in a different manner by Prof. Faraday*.

* Phil. Mag. S. 3. vol. xxix. p. 153.

The change of rotation with the direction in which it is observed establishes between the magnetized flint-glass and quartz a difference which is rendered still more perceptible by the experiments just cited. However, that is nearly the only difference. The dispersion of the planes of polarization for the different colours is the same in the two bodies. I have proved it in the following manner.

With the flint-glass placed between the two poles of the electro-magnet under the most favourable conditions to produce a great rotation (29 degrees), I counterbalanced this rotation by the contrary effect of a plate of quartz of the requisite thickness, which is easily obtained with M. Soleil's compensator. The system was then perfectly neutral, and would remain so in every position of the analyser, if the quartz and magnetized flint-glass acted in the same manner on the light, which is, in effect, what I have observed in all the different kinds of flint-glass I have experimented with.

Let us now examine the various circumstances which cause the *magnitude of the rotation* to differ.

The nature of the bodies should rank first. The differences are considerable in the various kinds of glasses; they are less perceptible in liquids, and indeed, according to some experimenters, all solutions have the same rotatory power. Thus Prof. Faraday (in 2185 of his memoir) considers as probable that, in aqueous solutions, the [ruling] rotative matter is the water, and not the other substance. But this opinion will no longer be entertained, when we observe in the first place that the most energetic liquids are precisely those which are anhydrous; and secondly, that among the dissolved bodies there are some which increase the rotatory power of the water, and others that diminish it. Moreover, on increasing the proportion of water in one and the same solution, the rotatory power is seen gradually to approach to that of pure water,—a conclusive proof of the influence of the substance in solution. Alcoholic solutions lead to the same result.

I will here enumerate a few of the numerous experiments made on this subject. The concentration represented by 1 is that of the most saturated solution; $\frac{1}{2}$ represents the degree of concentration of the same solution diluted with water, and so on.

1. *Rotations produced by some anhydrous liquids.*

Name of liquid.	Thickness. Centim.	Rotations. ° ′	Rotation of water. ° ′
Bichloride of tin	1	7 30	2 20
Sulphuret of carbon	1	7 00	2 20
Sulphuret of carbon	8	14 5	4 30
Protochloride of phosphorus	1	5 00	2 20

2. *Rotations produced by some aqueous solutions.*

Name of liquid.	Concentration.	Thickness. Centim.	Rotations. ° ′
Chloride of calcium . . .	1	13	6 20
Chloride of calcium . . .	½	...	4 55
Chloride of calcium . . .	¼	...	4 40
Chloride of calcium . . .	⅓	...	4 00
Water	...	...	3 40
Chloride of magnesium .	1	...	6 5
Chloride of magnesium .	½	...	5 30
Chloride of magnesium .	⅛	...	4 5
Water	...	...	3 30
Chloride of zinc	...	8	10 00
Water	...	...	4 30
Chloride of strontium . .	...	...	5 30
Water	...	...	4 15
Nitrate of ammonia	...	13	3 45
Water	...	...	4 55
Sulphate of iron	...	...	4 20
Water	...	...	6 00

3. *Rotations produced by alcoholic solutions.*

Chloride of magnesium .	...	13	3 20
Chloride of strontium . .	...	...	3 50
Alcohol of 36° Beaumé .	...	...	3 00
Distilled water	...	...	4 15

The rotatory power of sulphuret of carbon is remarkable; it is three times greater than that of water, and only twice less than that of the flint-glass of Faraday. This liquid is consequently valuable, because it may be substituted for the majority of the rare glasses required for this class of experiments. In the same substance the rotation varies in intensity with the thickness; but the law which regulates this variation has been differently expressed by the various experimenters who have studied this subject. Some have stated that the rotation was proportional to the thickness; others that it was independent of it; and others, again, that it increased with the thickness up to a certain limit, from which it diminished, and was finally reduced to zero. It is easily seen how much is true and how much is erroneous in these assertions.

In the first place, it is evident that if we consider the action of a single pole on a substance of indefinite length, this action, variable with the distance, will decrease from the first layer to the second, from the second to the third, to a certain distance, beyond which it will be null; so that the more distant layers will no longer be acted upon by the magnetism. The effects upon all the successive layers being added together, it will be seen that if we submit to the influence of a single pole increasing thicknesses of the same substance, the rotation will increase with the thickness up to a certain point, beyond which it will remain constant, an increase of thickness only producing layers which are no longer influenced.

It is likewise evident, that if the substance is submitted to the contact action of two poles of constant equal force, the effects will only be doubled, and that in consequence the law will still be the same.

But on interposing fresh strata between the poles, it becomes requisite to separate them more and more, which somewhat diminishes their intensity by decreasing the influence which they exert one on the other. Three cases may then occur; either this diminution of intensity will compensate the effect produced by the increase of thickness, or it will have a more feeble influence, or lastly, it will preponderate.

In the first case, the rotation will be independent of the thickness; in the second, it will increase with the thickness up to a certain point, beyond which it will remain constant; and lastly, on the third hypothesis, it will attain a maximum, beyond which it will decrease, *but without returning to zero*, the two poles always producing effects which conjoin; so that the extent of the rotations will be twice the effect produced by a single pole.

It will be seen from the following table that the two first cases may occur with M. Becquerel's electro-magnet, where the variation of the poles of the keepers must be considerable.

Substances experimented with.	Thickness. mm.	Rotations. ° ′
Flint-glass of Faraday	18·3	18 20
Flint-glass of Faraday	48·3	25 5
Very thick flint-glass	55·1	22 30
Very thick flint-glass	110·3	23 30
Distilled water	10·0	2 00
Distilled water	20·0	3 30
Distilled water	30·0	4 20
Distilled water	80·0	4 30
Distilled water	130·0	5 00
Distilled water	155·0	5 00

If instead of always bringing the poles in contact with the magnetized substance, they are left at the same distance, by placing between them a gradually increasing number of strata, the rotation will be seen to increase in a continuous manner until the thickness is equal to the distance of the poles. Again, if these poles are sufficiently removed from the various strata of the substance, so that the variations of their distance do not produce perceptible variations in their rotations, the action will be equal upon all, and the rotation observed will be proportional to the thickness of the substance. This, in fact, is the law discovered by Prof. Faraday, on employing horse-shoe electro-magnets not furnished with keepers.

The law of the variations with the thickness is evidently connected with that of the variations with the distance; but this is not better known than the first. It is therefore to the simultaneous examination of these two laws that my attention was necessarily directed, and I made it as soon as I had Ruhmkorff's great apparatus at my disposal.

Law of the thickness and of the distance.

The action of the two reels or coils of the apparatus being no more than the sum of the rotations produced by each of them, I had at first, in order to simplify the problem, to study the action of a single coil upon a substance of known thickness, placed upon the axis at a fixed distance.

Action of a single pole.—One of the coils being removed, I placed the flint-glass upon which I wished to experiment in contact with the remaining coil, then I removed it a certain distance, which I valued by the passage of its support over a divided scale. Now, *on increasing the distance of the flint-glass from the coil in arithmetical progression, the rotations of the plane of polarization decrease in geometrical progression.* In proof of this I will enumerate only three series of experiments in which I varied the distances; at first 1 millimetre, then 5 millimetres, and lastly 10 millimetres. The relations of the successive rotations are—

In the first case . . $0{\cdot}97587=r^1$,
In the second case . $0{\cdot}88504=r^5$,
In the third case . . $0{\cdot}78233=r^{10}$.

1. *Experiments with the flint-glass of* Mr. Faraday, *thickness* 38·9 *millims.*

Distance from the flint-glass to the coil. x.	Rotation observed. y.	Relation of the rotations. $\frac{y'}{y}$	Calculated rotation. $y'_1=y.0{\cdot}97587$.	Difference. $y_1'-y'$.
0	11° 12′	....	0° 00′	
1	11 00	0·9821	10 56	− 4′
2	10 26	0·9470	10 44	+19
3	10 7	0·9712	9 57	−10
4	9 50	0·9719	9 51	+ 1
5	9 30	0·9661	9 35	+ 5
6	9 20	0·9824	9 16	− 4
7	8 47	0·9417	9 4	−17
8	8 35	0·9772	8 34	− 1
9	8 20	0·9709	8 22	+ 2
10	7 55	0·9508	8 6	+11
0	9 50			

2. *Experiments with the flint-glass of* Mr. Faraday, *thickness* 38·9 *millims.*

Distance from the flint-glass to the coil. x.	Rotation observed. y.	Relation of the rotations. $\frac{y'}{y}$	Calculated rotation. $y_1'=y.0·88504$.	Difference. $y_1'-y'$.
0	12° 30'	....	0° 00'	
5	11 10	0·8934	11 4	− 6'
10	9 35	0·8582	9 54	+19
15	8 30	0·8870	8 30	
20	7 25	0·8726	8 31	+ 6
25	6 35	0·8876	6 33	− 2
30	5 45	0·8735	5 50	+ 5
35	5 5	0·8840	5 5	
40	4 35	0·9016	4 31	− 4
45	4 00	0·8728	4 4	+ 4
50	3 35	0·8957	3 32	− 3

3. *Experiments with the flint-glass of* Matthiessen, *thickness* 44 *millimetres.*

Distance from the flint-glass to the coil. x.	Rotation observed. y.	Relation of the rotations. $\frac{y'}{y}$.	Calculated rotation. $y_1'=y.0·78233$.	Difference. $y_1'-y'$.
0	7° 40'	....	0° 00'	
10	6 20	0·8261	6 1	−19'
20	5 00	0·7895	4 56	− 4
30	3 40	0·7333	3 53	+14
40	2 50	0·7727	2 53	+ 3

This law may be represented by a very simple formula. On expressing by A the rotation produced by the flint-glass in contact with the coil, if Ar is the rotation produced by the same flint-glass at the distance of 1 millimetre, the action of the coil at a distance x millimetres will be $y=Ar^x$.

This formula proving true for all thicknesses, it must be concluded that it represents the elementary action of a pole upon any stratum whatsoever; for instance, upon a stratum of 1 millimetre. It may consequently lead us to the law connecting the rotation with thickness, if in every instance each of the different sections of a substance receives an impression as if it were a single one. To convince myself of this, I placed two flint-glasses in contact between the two poles in certain positions, and observed the rotations produced by the two flint-glasses collectively, and by each of them singly, in the position which it first occupied.

The following experiments show that the first rotation is always the sum of the two others.

Flint-glass in juxtaposition.	Rotations produced.		Difference between the third and the sum of the two first.
	By the flint-glasses separately.	By the sum of the two flint-glasses.	
	° ′	° ′	′
Flint-glass of Faraday, of ...... 18·3	8 10	25 10	− 5
do. 38·9	17 5		
do. 38·9	12 12	26 10	−14
do. 48·3	14 12		
do. 38·9	12 32	24 10	+18
Flint-glass of M. Matthiessen of 44·0	11 20		
Flint-glass of Faraday .... 38·9	12 15	19 32	+12
Common flint-glass of .... 43·5	7 5		
Experiment with a single pole.			
Flint-glass of Faraday, of ...... 18·3	5 35	12 55	+10
do. 38·9	7 10		

Thus the action of a pole upon any section whatever of a substance depends solely on the distance of this section from the pole, and according to a known law. If, then, in a thickness of e millimetres, we consider e sections of 1 millimetre, and represent by c the rotation which each of these sections would produce if it were in contact with the pole, the rotation produced on contact by the thickness e will be the sum of the terms of a geometrical progression, the first term of which is c, the cause r, and the number of the terms e; that is to say, we shall have

$$A=c\frac{1-r^e}{1-r},$$

whence

$$y=c\left(\frac{1-r^e}{1-r}\right)r^x.$$

This formula represents the general action of a single pole. It may be proved by comparing the rotations observed at the same distance x by two thicknesses e and e' of the same flint-glass; for if y and y' represent the two rotations observed, it is evident that we ought to have

$$\frac{y'}{y}=\frac{1-r^{e'}}{1-r^e},$$

and we are able to compare this rotation with that given by experiment. This comparison confirms the accuracy of the formula, as will be seen by the following table:—

Name of the flint-glass.	Distance. x.	Thickness. e.	Rotation. y.	Relation of the rotations. $\frac{y'}{y}$.	Calculated relation. $\frac{1-r^{e'}}{1-r^{e}}$.	Calculated rotation. y_1.	Difference. y_1-y.
Flint-glass of Mr. Faraday...	0	48·3	9° 55′	1·951	1·916	9° 51′	− 4′
	...	18·3	5 5	1	1	5 9	+ 4
Flint-glass of Mr. Faraday...	0	18·3	4 47	1	1	4 54	− 7
	...	48·3	8 50	1·847	1·916	9 23	+33
	...	38·9	8 10	1·704	1·697	8 19	+ 9
	...	57·2	10 30	2·195	2·082	10 12	−18
	...	87·2	11 50	2·474	2·438	11 57	+ 7
Flint-glass of Mr. Faraday...	13·3	18·3	3 25	1	1	3 19	− 6
	...	48·3	6 10	1·823	1·916	6 16	+ 6
	...	38·9	6 00	1·756	1·697	5 36	−24
	...	57·2	7 20	2·146	2·082	6 52	−28
	...	87·2	8 10	2·390	2·438	8 24	+14
Flint-glass of M. Matthiessen	0	44·0	7 57	2·695	2·374	7 40	−17
	...	13·3	2 57	1	1	3 14	+17
Flint-glass of M. Matthiessen	0	44·0	7 0	2·540	2·374	6 51	− 9
	...	13·3	2 45	1	1	2 54	+ 9
Common flint-glass...........	0	43·3	4 25	2·210	2·190	4 24	− 1
	...	14·5	2 0	1	1	2 1	+ 1

Action of the two poles of the apparatus.—The formula $y=\mathrm{A}r^{x}$, which represents the action of a single coil, gives us also that of two electro-magnetic coils facing each other with the poles of opposite names, as in the case of our apparatus. If, in fact, these two coils are at a distance d, the flint-glass of e thickness, placed at a distance x from the first, will be distant $d-e-x$ from the second; and as the two actions add to each other, the total rotation will be

$$z=c\left(\frac{1-r^{e}}{1-r}\right)(r^{x}+r^{d-e-x}).$$

Even the form of this expression shows that if we only vary the distance x *in taking three consecutive rotations* z, z', z'', *observed in the same flint-glass placed successively at the distances* x, $x+\alpha$, $x+2\alpha$, *the sum of the two extreme rotations will be to the intermediary rotation in a fixed relation equal to* $r^{\alpha}+r^{-\alpha}$; that is to say, that

$$\frac{z+z''}{z'}=r_{\alpha}+r^{-\alpha}.$$

This conclusion is confirmed by experiment, as is seen in the following tables :—

1. *Experiment with sulphuret of carbon,* $e=41{\cdot}1$ $d=77$.

Distance. x.	Rotation. z'	Relation. $\frac{z+z''}{z'}$.	Calculated rotation. $z'_1=\frac{z+z''}{2{\cdot}06}$.	Difference. z'_1-z'.
5	6° 00′	......	6° 00′	0′
15	5 00	2·08	5 3	+3
25	4 25	2·08	4 27	+2
35	4 10	2·02	4 7	−3
45	4 5	2·07	4 7	+2
55	4 20	2·06	4 19	−1
65	4 50	2·03	4 46	−4
75	5 30	Mean 2·06		

2. *Experiment with the flint-glass of* Mr. Faraday, $e=48{\cdot}3$ $d=125$.

Distance. x.	Rotation. z'	Relation. $\frac{z+z''}{z'}$.	Calculated rotation. $z'_1=\frac{z+z''}{2{\cdot}06}$.	Difference. z'_1-z'.
0	9° 40′	......	9° 40′	0′
10	8 25	2·04	8 20	− 3
20	7 35	2·02	7 36	+ 1
30	6 45	2·06	6 47	+ 2
40	6 25	2·06	6 26	+ 1
50	6 30	2·14	6 45	+15
60	7 30		7 15	
50	......	Mean 2·06	6 39	
40	6 25	......	6 25	0
30	......	......	6 28	
20	6 55	......	6 55	0
10	......	......	7 38	
0	8 50	......	8 39	−11

The rotations compared being here more considerable, I have deduced the value of r from the equation

$$r^{10}+r^{-10}=2{\cdot}06,$$

whence

$$r=0{\cdot}97587.$$

The numbers compared with the experiments in the preceding tables have all been calculated with this value of r.

The general formula also furnishes us with another series of proofs. If we vary merely the thickness of the flint-glass by placing it in contact with one of the coils, we shall obtain as the relation of two rotations z and z', produced by two thicknesses e and e',

$$\frac{z'}{z}=\left(\frac{1-r^{e'}}{1-r^{e}}\right)\left(\frac{1+\frac{r^{d}}{r^{e'}}}{1+\frac{r^{d}}{r^{e}}}\right).$$

In the following table is shown the comparison of the results calculated in this manner with those furnished by experiment.

Name of flint-glass.	d.	Thickness. e.	Rotation. z.	Relation. $\frac{z'}{z}$.	Calculated relation.	Calculated rotation. z_1.	Difference z_1-z.
Faraday's flint-glass ...	48·3	48·3	22° 12′	2·537	2·587	22° 19′	+ 7
	......	18·3	8 45	1	1	8 38	− 7
do.	48·3	48·3	21 45	2·534	2·587	21 57	+12
	......	18·3	8 35	1	1	8 23	−12
do.	57·2	57·2	25 10	3·073	3·014	25 19	+ 9
	......	38·9	17 5	2·086	2·002	16 49	−16
	......	18·3	8 10	1	1	8 24	+14
do.	73·0	48·3	12 45	2·390	2·350	12 40	− 5
	......	18·3	5 20	1	1	5 25	+ 5
do.	77·0	48·3	12 45	2·250	2·314	12 52	+ 7
	......	18·3	5 40	1	1	5 33	− 7
do.	87·2	87·2	26 10	2·147	2·197	26 8	− 2
	......	48·3	14 12	1·164	1·204	14 20	+ 8
	......	38·9	12 12	1	1	11 56	−16
do.	110·3	48·3	11 20	2·261	2·113	11 10	−10
	......	18·3	5 5	1	1	5 15	+10
Matthiessen's flint-glass	44·0	44·0	17 30	3·365	3·226	17 18	−12
	......	13·3	5 12	1	1	5 24	+12
do.	48·3	44·0	16 20	3·322	3·165	16 8	−12
	......	13·3	4 55	1	1	5 7	+12
do.	77·0	44·0	10 10	2·652	2·836	10 21	+11
	......	13·3	3 50	1	1	3 39	−11
Common flint-glass......	48·3	43·3	10 25	2·841	2·869	10 28	+ 3
	......	14·5	3 40	1	1	3 37	− 3
do.	73·0	43·3	6 10	2·400	2·620	6 20	+10
	......	14·5	2 35	1	1	2 25	−10
do.	110·3	43·3	5 20	2·667	2·385	5 13	− 7
	......	14·5	2 0	1	1	2 7	+ 7

This comparison is the last verification which can be made of our formula. We might, it is true, vary d, and then, as $x=0$, we have

$$z=A(1+r^{d-e}),$$

or

$$z-A=\frac{A}{r^e}r^d.$$

We conclude that the quantity $z-A$ ought to decrease in geometrical progression as the distance of the poles increases in arithmetical progression. But experiment no longer confirms this conclusion, which is owing to the coefficient A no longer being constant, but varying with the distance of the poles; for these, by reacting one upon the other, change the intensity of their magnetism. If this reaction did not take place, the action of the two poles in contact with the flint-glass would be twice the action of a single one, whilst it is much stronger. In one experiment, for example, it was 28° 10′ in the first case, and only 12° 30′ when one of the coils was removed.

To sum up, we may state that the rotation produced by the two coils of our apparatus is represented by the formula

$$z=c\left(\frac{1-r^e}{1-r}\right)(r^e+r^{d-e-x}),$$

which gives the action of a single coil in making $d=\infty$.

In this formula r appears neither to depend on the intensity of the magnetism nor on the nature of the substance. As to c, it depends on both; but it remains constant in all the experiments compared, because they were always made at very short intervals, and moreover upon the same substance, and with the same distance between the poles.

It would undoubtedly be curious to ascertain why c varies with the intensity of the magnetism; but it may already be said that the law is the same for all substances; so that the relations of the rotations produced by these substances do not depend on the force of the magnetism, as may be seen by the following experiment made with M. Becquerel's electro-magnet.

Flint-glass of Mr. Faraday, of 18·3 millims.	Sulphuret of carbon, of 10 millims.	Relation between the rotations.
° ′	° ′	
7 42	3 18	0·43
13 48	6 00	0·43
19 00	8 18	0·43

Three other experiments made with M. Ruhmkorff's apparatus gave—

	° ′	° ′	° ′
For Faraday's flint-glass .	27 30	16 25	13 40
For Matthiessen's flint-glass	21 40	13 40	10 30
For common flint-glass . .	13 45	8 50	6 45

And in these three series the rotation of the flint-glass of M. Matthiessen remained nearly equal to 0·8, and that of the common flint-glass equal to 0·5 of the rotation produced by the flint-glass of Prof. Faraday.

For this reason I propose to call c the *coefficient of magnetic polarization.* The value is calculated by comparing two rotations observed at short intervals upon two substances placed under certain circumstances, but always between two poles at the same distance, that is to say, by deducing the value of c

from the equations which give y or z. We shall have, for example, for $x=0$,

$$\frac{c'}{c}=\frac{y'}{y}\cdot\frac{1-r^{e}}{1-r^{e'}},$$

or

$$\frac{c'}{c}=\frac{z'}{z}\cdot\frac{1-r^{e}}{1-r^{e'}}\cdot\frac{1+r^{d-e}}{1+r^{d-e'}}.$$

The following table has been formed in this manner: it contains the coefficients of magnetic polarization of the different substances experimented with, compared with the flint-glass of Prof. Faraday.

Faraday's flint-glass	1·00
Guinant's flint-glass	0·87
Matthiessen's flint-glass	0·83
Very thick flint-glass (from the Conservatoire)	0·55
Common flint-glass	0·53
Bichloride of tin	0·77
Sulphuret of carbon	0·74
Protochloride of phosphorus	0·51
Chloride of zinc dissolved	0·55
Chloride of calcium dissolved	0·45
Water	0·25
Ordinary alcohol of 36° Beaumé	0·18
Æther	0·15

I ought not to conclude without remarking, that all the preceding experiments merely relate to the action of electro-magnets upon exterior substances. When the flint-glass experimented with, instead of being outside the electro-magnetic coil was placed in the interior, I did not observe the rotation. I have noticed a very faint one when the second coil was brought near to the first; but this action was very considerably less than that which this second coil would have produced upon the same flint-glass placed at the same distance externally. These negative experiments are not opposed to those of Prof. Faraday, for they were not made under the same circumstances.

If Prof. Faraday has observed a rotation in flint-glasses placed in helices, still it was very faint; again, the interposition of iron nuclei only increased it when they were longer than the helix; and lastly, this interposition diminished, on the contrary, the rotation when the interior iron cylinder, having the same length as the helix, had at the same time a convenient thickness (§ 2209 of his memoir). Thus in a helix of 673 millimetres in length, of 121 millimetres in external diameter, and of 63 millimetres internal diameter, the insertion of an iron nucleus of 9 millimetres in thickness diminished the rotation of the substances placed in the interior. In my experiments I have never made use of coils enclosing such thin iron cylinders; the thickness of these cylinders was not less than 25 millimetres; their length was moreover always equal to that of the external helix.

LXXII. *On the Theory of Sound.* *By* G. G. Stokes, *M.A., Fellow of Pembroke College, Cambridge.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

As I see no advantage likely to result from further discussion of the question of the possibility of the existence of spherical waves of sound, it is not my intention to continue the controversy. I have, as I conceive, shown that the "contradiction" arrived at by Professor Challis has no real existence; and I am quite content to leave the question as it now stands to the judgement of mathematicians.

I feel it, however, to be but justice to myself to notice one sentence in Professor Challis's last paper; for if your readers take their views of the controversy from this sentence, they must think I have strange notions of reasoning.

Professor Challis, in speaking of my last three papers, observes, "In the first attempt he produced an argument which took for granted the very point in dispute; in the next he denied, without giving any reason, what was altogether undeniable; in the third attempt he admits what he before denied, and denies, again without assigning a reason, what in the second attempt he admitted." In the first of these papers it was not until I had, as I conceived, overthrown Professor Challis's *à priori* demonstration of the impossibility of spherical waves by pointing out that it rested on a tacit assumption (Phil. Mag., vol. xxxiv. p. 54), that I proceeded to inquire whether there was any foundation for this assumption in the received equations of motion. Properly speaking, the mere pointing out of the omission in Professor Challis's train of reasoning was my answer to his argument. As to my second paper, what Professor Challis regards as *undeniable* I regard as *untrue.* In this paper I admitted the possibility (as a particular case of possible motion) of a solitary wave of condensation, and I continue to admit it; in my next I denied that in this instance of motion the velocity of the fluid can be confined to the wave of condensation, except when a special condition is fulfilled, and I continue to deny it. Hence Professor Challis is mistaken in supposing that I admitted what before I denied, and denied what before I admitted.

It is not my intention to attack Professor Challis's new views respecting the theoretical velocity of sound; because if Professor Challis and I cannot agree on what to my own mind seems so plain a matter as the theory of spherical waves, I see little chance of our agreeing on the subject I have mentioned.

I am, Gentlemen,

Your obedient Servant,

G. G. Stokes.

Pembroke College, Cambridge,
June 5, 1849.

LXXIII. *On the Magnetism of Steam.* *By* Reuben Phillips, *Esq.**

1. THE following investigation has resulted from an attempt made with a view to the better understanding of the relation between electric and magnetic forces, by ascertaining whether the only form of the electric current, the nature of which, principally from the researches of Dr. Faraday, is very completely comprehended, possesses the usual magnetic properties. In this I was baffled (but the way is now open) by an unexpected phænomenon, the nature of which it became of primary consequence to develope, and which forms the subject of the present paper.

2. A little wooden stick was laid across the mouth of a small Bohemian beaker; the stick was placed parallel with the bottom of the glass, and held in its position with sealing-wax: the beaker was 3·5 inches high. A common sewing-needle No. 7, and another No. 8, were magnetized and stuck through a slip of thin card, the north end of one magnet being opposed to the south of the other, the needles being two inches apart. This partially astatic arrangement was suspended from the stick by a single fibre of silk, and the length of the fibre between the points of suspension was one inch. The needles made one vibration in about two seconds. I found if the needles were much more astatic than this, they were very subject to slow irregular variations of position, which appeared to proceed from a twisting of the silk arising from changes in its hygrometric or calorific condition. The mouth of the glass was closed with a card cover furnished with a rim.

3. The beaker was now placed on the stage of a microscope

* Communicated by the Author.

and fastened to the triangular bar by means of cork and thread, and a hole about an inch across was cut in the card cover, the centre of the hole being over the point of the uppermost needle; the arm for carrying the optical tube was now brought over the hole, and then the optical tube, fitted with an object-glass, was screwed into its place; the arm came down nearly close upon the card cover, and the tube of the object-glass passed through the cover and stood over the point of the upper needle. The fine adjustment was used to focus the instrument; and to bring the point of the needle to its right position under the object-glass, I employed the motion of the arm which carried the optical tube, and also that imparted to the whole instrument by laying hold on the extremity of one of the prongs of the tripod stand and making it describe a small portion of a circle on the table. The point of the needle was viewed by means of a pencil of light thrown from the mirror through the bottom of the beaker. The magnifying power used was about 450 diameters.

4. A micrometer eye-piece was employed, the scale of which formed an angle of about 25° with the edge of the needle to be observed. The optical power of this micrometer corresponded to the third, or deepest eye-piece, with which microscopes are generally furnished; because that for such experiments a shallow object-glass is to be preferred.

5. This galvanoscope was sheltered by a rectangular zinc plate, ·1 inch thick, bent somewhat into the shape of the smaller segment of the convex surface of a right cylinder, made by a plane parallel to, and at a considerable distance from, its axis. The length of a line drawn perpendicularly from one straight side to the other was 10 inches, and the maximum length of a perpendicular drawn to this line and extending to the nearest point of the zinc was 2·8 inches; in consequence of this curve, the zinc plate could easily be made to stand on end, in which position it was 18·5 inches high. This shield was employed throughout all the following experiments on steam.

6. In order to obtain accurate results, I found it necessary to avoid moving any mass of iron near the galvanoscope, and also to keep moderately still, although the hands and arms could be freely used without affecting the galvanoscope. During these experiments I sometimes observed a very singular effect produced on the magnetic needles: when the steam was turned on, the needle would begin to move across the field of view with a peculiar slow motion; and when the steam was shut off, it as slowly or more slowly returned to its former position, one of such vibrations occupying half a minute or more. This I at length found was produced by the steam

heating the zinc shield; and thus this motion could be produced or avoided at pleasure, and was, I think, sufficiently accounted for.

7. The steam was obtained from a small hydro-electric machine; and the various apparatus for effecting the discharge were screwed into the condenser at the place made to receive the Armstrong's jet. The condenser was always dry, except where I have noted the contrary, and the steam was discharged horizontally towards the north.

8. A galvanic current was sent through a wire in the neighbourhood of the galvanoscope; the wire lay parallel with the needles in the path taken by the steam (9.), and acted about equally on both; the wire of course lay about north and south, and the electricity passed along it from north to south, supposing the current to pass through the conducting wire of a voltaic circle from the platinum to the zinc. When the circuit was completed, the needle moved to one side, A, of the field of view from the opposite side C. Throughout these experiments the motion of the same end of the same needle was always recorded, and the galvanoscope always stood to the east of the current of steam.

9. To a brass jet, 1·8 inch long, was fastened a piece of glass tube, 11·5 inches long and $\frac{3}{20}$ inch diameter inside, and the juncture was made tight, or nearly so, with caoutchouc; the aperture at the end of the brass jet which projected from the condenser (7.) was circular, and $\frac{1}{12}$ inch in diameter. The nearest point of the convex surface of the zinc shield was about $\frac{1}{10}$ inch from the glass tube; the stage of the microscope in this and the following experiments came nearly close to the shield. The fibre which suspended the needles was about $1\frac{3}{4}$ inch from the nearest part of a plane drawn through the end of the brass jet, and making a right angle with its bore; the steam was used in this experiment at about 35 lbs. on the inch. Things being so arranged, I found when the steam was turned on that the needle immediately began to move towards C; and by alternately checking the steam and letting it off, a considerable swing of the needles was produced; and by reversing the times of letting off the steam, the swing of the needles could be again reduced. I had ascertained by previous trials, that turning the cock of the boiler without letting off the steam produced no effect on the needles. The experiment was made by screwing a stop-cock in the place of the above-mentioned brass jet; and then the cock of the boiler could be worked without letting off the steam, and without affecting the galvanoscope.

10. The galvanoscope was raised a few inches, so that the

steam might act principally on the lower side of the undermost needle, instead of acting equally on both as before, everything else being as in the former experiment. When the steam was turned on, the needle began to move towards A; and by alternately shutting off the steam and letting it issue at the corresponding positions of the needle, a vibration through full half of the micrometer was obtained; and then by making the blasts of steam synchronical with the opposite vibrations of the needle, the motion was checked.

11. The glass tube was now taken away, the galvanoscope lowered as in (9.), and the shield and galvanoscope moved horizontally about $\frac{3}{4}$ inch in a perpendicular direction to the path of the steam, which was used at about 35 pounds on the inch. Operating as before, I could with this jet of steam more easily obtain the swing, the motion being towards C when the steam was turned on. Water being placed in the Armstrong's condenser, produced no alteration in the magnetic effects of the jet of steam.

12. The effect of this jet (11.) was much greater than that of the current of electricity of an Armstrong's jet under the most favourable circumstances. The comparison was made in the following manner:—I found that an Armstrong's jet could discharge more steam in a given time than the brass jet (11.), also that the electricity produced by the Armstrong's jet could deflect the needle of a galvanometer of the ordinary construction 3 or 4 degrees. I then found that a small voltaic arrangement capable of deflecting the needles of the galvanometer 4°, and having a conducting wire lying in the path of the steam, acted far less on the needles of the galvanoscope than the blast of steam from the brass jet (11.). Also when an Armstrong's jet was substituted for the brass jet, and water placed in the condenser, I could perceive no difference in the swing, whether the steam passed by the galvanoscope in a highly electrified condition, or whether the electricity was, in a great measure, collected by means of a number of fine points almost as soon as it left the wooden channel. The points were supplied by two small concentric loops of wire-gauze, placed edgeways in the steam at a distance in different experiments of from $\frac{1}{2}$ to $\frac{3}{4}$ inch from the end of the Armstrong's jet. The galvanoscope was as before (11.), except that it was placed about $\frac{3}{4}$ inch further from the jet, but at about the same distance from the path of the steam.

13. There is a singular variation which I have sometimes observed in the magnetic effect produced by the steam issuing from an Armstrong's jet; namely, that the action of the steam on the galvanoscope is much stronger when one of the needles

is in the same horizontal plane as the central line of the path of the steam, than when the galvanoscope is placed so that the steam may pass equally near to both needles. I have occasionally observed the swing to be five or six times greater in one position than in the other; but sometimes the swing is the same in either position. I have not perceived a similar effect with any other jet.

14. A pewter tube 6 feet long, having a bore $\frac{3}{20}$ inch diameter, was coiled up after the fashion of the wire of a galvanometer: it made six convolutions. One end of the tube was jambed on the brass jet (9.), and the coil stood horizontally like the coil of a galvanometer; the zinc shield was brought nearly close to the coil, and the height of the galvanoscope was adjusted so that the coil might act on both the upper and lower sides of the lower needle; the steam entered on the upper side of the coil in the same direction as before. By this arrangement I succeeded in producing a marked swing with steam of a very low pressure, I think below 5 lbs. on the inch; but I did not exactly ascertain the amount of this low pressure, because the safety-valve of the boiler is not graduated below 40 lbs. As the pressure rose, the power exerted on the needles became greater; and at 40 lbs. on the inch, a few puffs of steam caused the needle to move through the whole length of the micrometer; the motion, when the steam was turned on, being towards C.

15. Instead of allowing the steam to pass continuously through the tube during the whole of each alternate vibration, it was shut off before the vibration was completed. The swing was now much less, showing that it was not the first gust of steam which alone moved the needles; and this agreed with the visible motion of the needle, which manifestly increased in velocity after it began to move.

16. Water being placed in the condenser of the hydro-electric machine did not sensibly alter the force exerted on the needles by the coil.

17. The coil was now attached to the boiler by means of a piece of brass, the steam-way of which was so large that the coil only might be looked upon as opposing the exit of the steam. Various pressures were tried, as the boiler became heated, up to 40 lbs. per inch. A lead pipe was, in this experiment and frequently afterwards, placed at a short distance from the coil to catch the steam and convey it up a chimney, whereas in former experiments it had freely escaped into the apartment; everything else remained as before (14.). The needles began to be affected at a very low pressure; and as the pressure increased, the swing became certain and steady, the needle

moving towards C when the steam was turned on. The intensity of the swing-producing force was, I think, at its maximum at 10–15 lbs. per inch; as the pressure rose, the swing was obtained with more difficulty, and at 40 lbs. on the inch no certain swing could be produced. On repeating this experiment at a subsequent period with steam at 40 lbs. on the inch, a feeble swing was obtained; but in this latter case the coil, instead of being bright, had become covered with a deposit of carbonate of lime.

18. The galvanoscope was lowered until the upper needle was in the same horizontal plane as the undermost side of the upper portion of the coil, and the lower needle was in a horizontal plane which came to about $\frac{1}{4}$ inch below the lower surface of the lower half of the coil. When the steam was turned on, the needle made a start towards A; and by shutting off the steam during the next vibration, and repeating these operations two or three times, a considerable swing was obtained; but on attempting to increase this swing by continuing these intermittent operations, the swing rapidly diminished. The order in which the jet of steam and its cessation took place was now inverted, which soon produced a very powerful swing, the needle moving towards C when the steam was turned on. The steam was used at from 30 to 40 lbs. on the inch. These rather irregular motions (17, 18.) are probably connected with those of the Armstrong's jet (13.).

19. The coil was moved through an angle of 180°, the angular motion being performed parallel to a plane forming a right angle to the path of the steam, and the galvanoscope was adjusted so that the interior surface of the coil might act on the lower needle; by this arrangement the direction of the steam as regards the needle was reversed. Many different pressures were observed from a pound or two on the inch to 40 lbs. As soon as any distinct swing was produced, it was occasioned by the needle moving towards A when the steam was turned on; as the pressure rose, this swing increased until the steam was, I think, about 25 lbs. on the inch, after which the increased pressure only occasioned a somewhat diminished swing.

20. The apparatus (19.) was now rather differently disposed, the shield and galvanoscope were moved horizontally about $\frac{3}{4}$ inch, the direction of the motion beirg perpendicular to the path of the steam; also a piece of an i ron gun-barrel 7 inches long and open at both ends, was fixed to a support, so that the gun-barrel might easily be thrust into, or removed from, the coil without bearing upon it; the axis of the gun-barrel when placed in the coil formed a right angle to the path of the steam, and was horizontal, and its direction lay not far

from east and west; also one end came to $\frac{1}{3}$ inch of the shield opposite to the lower needle of the galvanoscope. This piece of a gun-barrel had been made red-hot and slowly cooled, and its magnetism when in the above position was nearly $=0$; the diameter of its external surface at that end which was placed in the coil, in this and the following experiments, was about one inch, and this end was made of iron about $\frac{1}{10}$ inch thick, the other end being thinner. There was a distance of about $\frac{1}{3}$ inch between the nearest upper or lower part of the iron and the respective inside surfaces of the coil.

21. The steam being at 40 lbs. per inch, and the gun-barrel in the coil, five puffs of steam, each puff acting during every alternate vibration of the needles, produced a swing of about 25° of the micrometer; the gun-barrel was now removed, when five puffs of steam, acting as before, produced a vibration of only 10°. This was done many times, and always with the same result. I think better results would have been obtained at a lower pressure than 40 lbs.; for on examining the vibration at various pressures, the iron being in the coil, I found that a considerable swing was produced almost as soon as the water began to boil at the atmospheric pressure; and shortly afterwards, as the pressure rose, the vibrations became very strong, much stronger than at 40 lbs. The point of the needle moved towards A when the steam was turned on.

22. During these last experiments, I ascertained that the first puff of steam which passed through the coil when it was cold produced a much greater effect on the galvanoscope than any immediately succeeding puff. This was guarded against by letting off the first puff, then checking the motion of the needles by some inverse puffs, and then proceeding to make the vibration which was to be compared (21.).

23. I next endeavoured to find the cause of the strong action of the first puff.

24. The jet and pewter coil (14.), instead of being affixed to the boiler, were attached to a copper box, which inclosed about ninety cubic inches, and the shield and galvanoscope adjusted as before (14.); air was now pumped into the box until the pressure rose to about 40 lbs. on the inch, and then discharged through the jet as the steam had been. The needles of the galvanoscope were quite unaffected; consequently, air of about the same temperature as the surrounding atmosphere cannot act on a magnet like steam.

25. It now appeared very possible that the increased action produced by the iron arose from its cooling powers; and also that by further cooling the coil, a more intense action would be obtained.

26. The apparatus (17.) had the coil partly immersed in

water, by bringing a copper pan filled with water under it, so that the water might cover the lower portion of the coil; the upper portion of the coil was kept moist by having had some loosely spun cotton twisted about it, the ends of which dipped into the water. The gun-barrel was placed in the coil much as before (20.), and was supported without touching the coil; consequently any little alteration, produced by the heat of the steam, in the position of the coil, could not move the iron; the gun-barrel lay entirely under water. The steam was used at 35 to 40 lbs. on the inch, as at that pressure a difference of a few pounds did not much affect the galvanoscope. A few puffs of steam made the water about the coil to boil; after which, when the circumstances of the experiment appeared to be very steady, I observed that one puff of steam could move the edge of the needle across the field of view. The gun-barrel was removed, and now two or three puffs of steam, acting through each alternate vibration, could only produce a swing half across the micrometer. A solid brass rod, ·7 inch diameter and about 6 inches long, being now laid in the place of the gun-barrel, produced no alteration in the swing of the magnetic needles; the brass rod being removed, and the gun-barrel replaced, the motion of the magnets at once became as strong as before. It now follows that the action of the iron on the galvanoscope is independent of any little change of temperature it may produce. The swing was towards C when the steam was turned on.

27. In these experiments I did not perceive any difference between the first and following puffs of steam; and the swing was, I think, at least as great at 40 lbs. as at any lower pressure.

28. The brass jet (9.) was screwed into the end of the coil from which the steam escaped (26.), which apparatus in other respects remained as before; this alteration caused an increased pressure in the coil, and the condensation was consequently very rapid; so much so, that what escaped from the brass jet looked more like water than steam. The swing, when the gun-barrel was either in or out of the coil, remained just as before (26.), the pressure in the boiler being about 40 lbs. on the inch.

29. A pewter tube, 9 feet long and $\frac{1}{4}$ inch internal diameter, was made into a dense cylindrical coil 4 inches long, the diameter of the external surface of which was 2·5 inches, and the diameter of the interior 1·25 inch. This coil was attached to the boiler by means of a short brass connecting piece, the steam-way of which was cylindrical, and $\frac{3}{20}$ inch diameter; the coil was supported horizontally, and pointed about east

and west. The shield stood $\frac{1}{5}$ inch at its nearest part from the end of the coil, and the galvanoscope was adjusted so that the lower needle was opposite to the coil; the steam circulated in the same direction as before (14, 26.). At 40 lbs. four puffs of steam produced a swing about one-fourth across the micrometer, and the swing was not very much stronger at any lower pressure. The copper pan, which had previously been placed in position under the coil, was now filled with water, by which the coil was about half-covered. The first motions of the needle were not very powerful; but after three or four puffs, which heated the water about the coil, one puff of steam would move the needle from A quite out at C; the swing was in the same direction when there was no water in the pan.

30. The cylindrical pewter coil had a turn or two opened out, in order to give sufficient length between the brass connecting piece and the coil to allow the latter to be immersed in the water of the copper pan. The gun-barrel was placed in the coil, and was supported by wires attached to the copper pan, and so prevented from touching the coil; the gun-barrel projected about $\frac{1}{2}$ inch beyond the end of the coil on the side nearest to the galvanoscope. The optical tube of the galvanoscope was removed, and the hole in the cover was closed with a piece of glass. The steam was used at about 40 lbs. on the inch. After the water in the copper pan had been heated, I could easily by successive puffs of steam produce a vibration through an arc of 20 or 30 degrees.

31. Putting together these gun-barrel experiments, and that with condensed air (24.), I come to the conclusion that a difference of temperature is necessary to produce these peculiar magnetic effects; which accounts for the greater force of the first blast of steam (22.), and for the superior force of a jet (11.). Also the similarity which exists between the magnetic effects of the steam current, and the magnetic effects of the voltaic current, both as regards magnets and soft iron, renders it nearly or quite certain that this force of the steam is magnetism.

32. Now a jet of steam, even when mixed with much water, is an excellent non-conductor of electricity; for when discharged from an Armstrong's jet, it is seen that electricity even of a very high intensity cannot pass through it, and a jet of dry steam must be at least as good a non-conductor; hence the jet of steam (11.) cannot be travelled by any feeble current of electricity, a thermo-electric current for instance. Again, this magnetic effect of the steam must be independent of any electricity carried forward by the steam, as when discharged by an Armstrong's jet; for the greatest amount of electricity which

I have been able to obtain from an Armstong's jet was found sufficient to charge a Leyden jar of about 340 square inches, both sides taken together, twenty-eight times in a minute; the spark being $\frac{1}{3}$ inch long, and the steam at 40 lbs. on the inch: now whether the steam passed by the galvanoscope in this highly excited condition, or nearly divested of its electricity, the effect on the galvanoscope was the same, and always many times greater than what could be produced by this largest quantity of frictional electricity that could be obtained (12.). Besides, the frictional electricity of steam increases much as the pressure rises from 10 to 40 lbs. (Armstrong, Matteucci). But I have not generally perceived that the swing of the galvanoscope thus increases with the pressure, but rather the reverse. Also Dr. Faraday has shown, that dry pure steam (11.) cannot develope frictional electricity.

33. From these considerations, I conclude that no continuous electric current passes through or by means of the steam jet; however, many very small currents may circulate in it For instance, if we may suppose that a particle of steam when brought into contact with a particle of colder water developes a momentary current of electricity in a direction bearing some fixed relation to those particles, and then if a continual succession of such particles ensues, the majority of which are similarly placed, we should have something answering to an ordinary electric current, and not very unlike those currents imagined in Ampère's theory of magnetism. This notion accounts for the change in the direction of the magnetism produced by changing the direction of the steam, the effect of the difference of temperature, and the manifest want of equivalency between the steam power expended and the magnetic force obtained. But it may be well to bear in mind, that perhaps magnetism may ultimately come to be regarded as some function of ordinary matter and the æther. I can only look upon the experiments (12.) as going to show that magnetism is not always bound up with current electricity; I should probably have made a decisive experiment on this point, but that the steam apparatus at my disposal was not sufficiently powerful.

34. It is possible instances may be found on board steamers in which the compasses are much disturbed by the steam. Clouds, too, in the act of formation and passing rapidly over a magnet may somewhat affect it.

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LXXIV. *On some Points relating to the Theory of Fluid Motion. By the* Rev. J. Challis, *M.A., F.R.S., F.R.A.S., Plumian Professor of Astronomy and Experimental Philosophy in the University of Cambridge**.

IN a memoir on certain questions in the theory of the motion of fluids, published (1847) by Professor P. Tardy of Florence, for a copy of which I am indebted to the author, reference is made to a communication contained in tom. xxiii. of the *Comptes Rendus* of the Academy of Sciences of Paris, in which several of my investigations on fluid motion are brought under review by M. J. Bertrand. Professor Tardy states at the same time, that he had himself previously made remarks on one of the points on which I am supposed to be in error. The citation of Professor Tardy first made me aware that M. Bertrand had taken notice of my labours. On turning to the article in the *Comptes Rendus*, I perceived that the important errors (*erreurs graves*) attributed to me were partly due to misconception of my reasoning, which, I am ready to admit, may not have been developed with sufficient clearness; and partly to the circumstance, not unusual in the history of science, that new truths appear to be errors so long as the errors they replace are supposed to be truths. It will suffice for the present to advert to one point of primary importance. I have repeatedly contended that, to complete the analytical theory of hydrodynamics, a new general equation is absolutely required. M. Bertrand first calls in question the principles on which this equation is established, and then contends that an equation which I derived from a combination of the new equation with that which is usually called the equation of continuity, is identical with a particular case of this latter equation. The following investigations will supply answers to these objections.

1. I propose first to exhibit the principles on which the new equation rests, and to deduce it accordingly. If the equation $\psi(x, y, z, t)=0$ express any given relation between the coordinates x, y, z and the time t, any other relation between the same quantities may be expressed by the equation $\psi(x+\delta x, y+\delta y, z+\delta z, t+\delta t)=0$, the increments δx, δy, δz being in general functions of the coordinates and the time. Supposing the increments to be indefinitely small, we obtain

$$\frac{d\psi}{dt}\delta t+\frac{d\psi}{dx}\delta x+\frac{d\psi}{dy}\delta y+\frac{d\psi}{dz}\delta z=0.$$

* Communicated by the Author.

If u, v, w be the resolved parts of the velocity of a given particle of fluid in motion, and we suppose that

$$\delta x = u\delta t \quad \delta y = v\delta t \quad \delta z = w\delta t,$$

the above equation becomes

$$\frac{d\psi}{dt} + \frac{d\psi}{dx}u + \frac{d\psi}{dy}v + \frac{d\psi}{dz}w = 0.$$

The signification of this equation depends entirely on the nature of the curve surface defined by the equation $\psi(x,y,z,t)=0$. If, for instance, this be the surface of a fixed or moveable boundary with which the fluid is in contact, the equation affirms that the same particle remains in contact with the boundary in successive instants. If the function ψ be the general expression for the pressure p, then since $p=0$ is the equation of the free boundary, the above equation would express in this instance the condition that a given particle is situated on the free boundary in successive instants. Let now

$$(d\psi) = \frac{u}{\lambda}dx + \frac{v}{\lambda}dy + \frac{w}{\lambda}dz.$$

Then, as is known, $\psi=0$ is the equation of a surface cutting at right angles the directions of the motions of the particles through which it passes. The factor $\frac{1}{\lambda}$ is applied for the sake of generality, because it may be assumed that such a surface always exists, and consequently that the right-hand side of the above equality is an exact differential, although it cannot be affirmed that $udx+vdy+wdz$ is always an exact differential. Assuming, therefore, the integrability of

$$\frac{u}{\lambda}dx + \frac{v}{\lambda}dy + \frac{w}{\lambda}dz,$$

it follows that

$$u = \lambda\frac{d\psi}{dx} \ldots(1.) \qquad v = \lambda\frac{d\psi}{dy} \ldots(2.) \qquad w = \lambda\frac{d\psi}{dz} \ldots(3.).$$

Hence, substituting in the foregoing equation,

$$\frac{d\psi}{dt} + \lambda\left(\frac{d\psi^2}{dx^2} + \frac{d\psi^2}{dy^2} + \frac{d\psi^2}{dz^2}\right) = 0. \quad . \quad . \quad . \quad . \quad (4.)$$

This is the new equation which it was proposed to obtain. The course of the investigation shows that this equation expresses the condition that the directions of the motion in a given element are in successive instants normals to surfaces of continued curvature. The fulfilment of this condition ensures the continuity of the motion; and the above equation may consequently be called the *equation of continuity*, while the equation usually so named may with more propriety be called the *equation of constancy of mass*, with reference to the principle on which it is based.

To the equations (1.), (2.), (3.), and (4.) are to be added the two following:

$$\frac{d\rho}{dt}+\frac{d.\rho u}{dx}+\frac{d.\rho v}{dy}+\frac{d.\rho w}{dz}=0 \quad \ldots\ldots\ldots\ldots\ldots (5.)$$

$$\frac{(dp)}{\rho}+\left(\frac{du}{dt}\right)dx+\left(\frac{dv}{dt}\right)dy+\left(\frac{dw}{dt}\right)dz=0 \quad \ldots\ldots (6.)$$

the fluid being supposed to be acted upon by no impressed forces. When the relation between the pressure p and density ρ is given, these six equations serve to determine the six unknown quantities, ψ, λ, u, v, w and ρ.

The equation (5.) is equivalent to the following:

$$\frac{d\rho}{dt}+\frac{d\rho}{dx}\cdot\frac{dx}{dt}+\frac{d\rho}{dy}\cdot\frac{dy}{dt}+\frac{d\rho}{dz}\cdot\frac{dz}{dt}+\rho\left(\frac{du}{dx}+\frac{dv}{dy}+\frac{dw}{dz}\right)=0.$$

Hence, if $u^2+v^2+w^2=V^2$ and $ds=Vdt$,

$$\frac{d\rho}{dt}+V.\frac{d\rho}{ds}+\rho\left(\frac{du}{dx}+\frac{dv}{dy}+\frac{dw}{dz}\right)=0.$$

But by what is proved in the Cambridge Philosophical Transactions (vol. vii. part 3. p. 385, 386), where, however, it is proper to remark, the use of equation (4.) is not absolutely necessary, we have

$$\frac{du}{dx}+\frac{dv}{dy}+\frac{dw}{dz}=\frac{dV}{ds}+V\left(\frac{1}{r}+\frac{1}{r^1}\right),$$

r and r^1 being the principal radii of curvature at the point xyz, of the surface which cuts the directions of motion at right angles. Hence, by substitution,

$$\frac{d\rho}{dt}+\frac{d.V\rho}{ds}+V\rho\left(\frac{1}{r}+\frac{1}{r^1}\right)=0 \quad \ldots\ldots\ldots (7.)$$

This equation has also been derived from elementary considerations in the memoir above cited (p. 387). By whatever process it be obtained, it involves the principle expressed analytically by the equation (4.), viz. that the directions of motion are normals to surfaces of continued curvature. It cannot, therefore, be identical with any equation which does not involve the same principle, or does not contain *explicitly* the radii of curvature r and r^1. An application of the equation will illustrate this remark. Suppose the fluid to be incompressible: then

$$\frac{dV}{ds} + V\left(\frac{1}{r} + \frac{1}{r^1}\right) = 0.$$

Hence, since $ds = dr = dr^1$, we have by integrating,

$$V = \frac{\varphi(t)}{rr^1}.$$

This expression for the velocity is general, having been obtained prior to the consideration of any particular case of motion. It establishes the general law, that the quantity of fluid which passes in a given small time a given small element of a surface of displacement, being proportional to Vrr^1, is given for a given value of $\varphi(t)$, and consequently that the momentary trajectory of the surfaces of displacement to which a single disturbance gives rise is a straight line.

If the motion take place in space of two dimensions, we have

$$V = \frac{\varphi(t)}{r}.$$

I formerly obtained this result for the case in which $udx + vdy$ is an exact differential, by a process which Professor Tardy and M. Bertrand object to, and which I do not now insist upon, because the above reasoning is inclusive of the particular case, and the result is obtained in a more direct manner.

To apply the above general value of V to a given instance: suppose a perfectly smooth sphere to move in any manner in an incompressible fluid of unlimited extent, its centre remaining on a given straight line. The general value of V applies to the velocity impressed on the fluid by the surface of the sphere. If V^1 be the velocity of its centre, then the motion impressed at any point of the surface the radius to which makes an angle θ with the direction of motion, is $V^1 \cos\theta$. Hence, R being the radius of the sphere,

$$V^1 \cos\theta = \frac{\varphi(t)}{R^2}, \text{ or } \varphi(t) = R^2 V^1 \cos\theta.$$

Consequently, as there is no other arbitrary quantity to satisfy,

$$V = \frac{R^2}{r^2} V^1 \cos\theta,$$

where r^2 is substituted for rr^1, because as one surface of displacement is spherical, all are spherical, their momentary trajectory being rectilinear. This result does not agree with that given by the received hydrodynamical equations employed in the usual manner,—first, because those equations do not include the principle of continuity expressed by equation (4.); and secondly, because in treating this problem, $udx+vdy+wdz$ has without reason been assumed to be an exact differential.

2. I proceed next to trace the consequences of introducing into the general equations the condition that $udx+vdy+wdz$ is an exact differential. Let $(d\varphi)=udx+vdy+wdz$. Then $(d\varphi)=0$ is the differential of the equation of a surface cutting at right angles the directions of motion. Hence the value of $\frac{1}{r}+\frac{1}{r'}$ in equation (7.) may be expressed by means of the partial differential coefficients of φ. This expression being substituted in (7.), and ρ being eliminated by means of (5.) and (6.), the result is identical with the known equation (n) in the *Mécanique Analytique* (Part 2. Section XII. p. 344). I have indicated this process for the purpose of remarking, that it does not thence follow that equations (7.) and (n) are identical, or that the former is a particular case of the latter. Both equations are equally general. The essential difference between them is, that (7.) involves an expression of the condition that the directions of motion are normals to surfaces of continued curvature; whereas (n) involves no expression of this condition, being usually obtained by simply supposing φ to be a certain function, the partial differential coefficients of which with respect to x, y and z are respectively u, v and w. It is not possible to pass from (n) to (7.) without introducing this condition of continuity, and equation (7.) consequently signifies something *more* than equation (n).

We have now to consider the change which equation (4.) undergoes by supposing $udx+vdy+wdz$ to be an exact differential. It might at first sight be supposed that for this purpose it is sufficient to put unity for λ. That this would be a false step is clear from the consideration, that there would then be six equations and but five unknown quantities, without any reasons for concluding that the equations would be consistent with each other. In fact, it would be found on trial that on this supposition they are inconsistent. The only legitimate process is to trace the consequence of supposing $udx+vdy+wdz$ to be an exact differential, by reasoning generally according to the rules of analysis.

Between the functions φ and ψ we have the relation, that $(d\psi)=0$ and $(d\varphi)=0$ are both differential equations of the same curve surface. But $(d\varphi)=0$ being the differential equation of a curve surface, it is clear that $\big(d.\mathrm{F}(\varphi)\big)=0$, or $\mathrm{F}'(\varphi)(d\varphi)=0$, is a differential equation of the same surface. Consequently

$$(d\psi) = F'(\varphi)(d\varphi).$$

But

$$(d\psi) = \frac{1}{\lambda}(d\varphi);$$

therefore

$$\lambda = \frac{1}{F'(\varphi)}.$$

Also by integration,

$$\psi = F(\varphi) + \chi(t).$$

Hence

$$\frac{d\psi}{dt} = F'(\varphi)\frac{d\varphi}{dt} + \chi'(t) \qquad \frac{d\psi}{dx} = F'(\varphi)\frac{d\varphi}{dx}$$

$$\frac{d\psi}{dy} = F'(\varphi)\frac{d\varphi}{dy} \qquad \frac{d\psi}{dz} = F'(\varphi)\frac{d\varphi}{dz}.$$

Consequently by substituting in (4.), and having regard to the value of λ, we obtain

$$\frac{d\varphi}{dt} + \frac{d\varphi^2}{dx^2} + \frac{d\varphi^2}{dy^2} + \frac{d\varphi^2}{dz^2} + \frac{\chi'(t)}{F'(\varphi)} = 0. \quad . \quad . \quad (8.)$$

Thus we have an equation involving the same variables as the general equation (n) already referred to, and yet not identical with it. The interpretation of this analytical circumstance is, that the function φ has not an arbitrary, but a *particular* form; and it is a matter of importance to ascertain what that form is. The following investigation may perhaps suffice for this purpose, but is not the most general that might be adopted. For the sake of avoiding long processes I shall confine the reasoning to the first order of approximation.

The equation (8.) may be put under the form,

$$\frac{d\varphi}{dt} + \frac{d\varphi^2}{ds^2} + \frac{\chi'(t)}{F'(\varphi)} = 0, \quad . \quad . \quad . \quad . \quad . \quad (9.)$$

where ds is the increment of a line drawn in the direction of the motion, so that $\frac{d\varphi}{ds} = V$. Now neglecting the term involving V^2, we have

$$F'(\varphi)\frac{d\varphi}{dt} + \chi'(t) = 0;$$

and by integration,

$$F(\varphi) + \chi(t) = \theta(s).$$

Hence

$$\varphi = f\big(\theta(s) - \chi(t)\big).$$

But we have already found, without using equation (9.), that

$$F(\varphi)+\chi(t)=\psi.$$

Hence

$$\varphi=f(\psi-\chi(t)).$$

A comparison between these two values of φ gives

$$\psi=\theta(s).$$

The above value of φ must satisfy the following linear equation:

$$0=a^2\left(\frac{d^2\varphi}{dx^2}+\frac{d^2\varphi}{dy^2}+\frac{d^2\varphi}{dz^2}\right)-\frac{d^2\varphi}{dt^2}. \quad . \quad . \quad (10.)$$

It may therefore be assumed that $\theta(s)$ and $\chi(t)$ are linear quantities, and accordingly that

$$\theta(s)=s, \text{ and } \chi(t)=ct.$$

Also since $\psi=0$ is the equation of a curve surface, we have in general,

$$\psi=z+c'+q(x, y, t).$$

Hence

$$s=z+c'+q(x, y, t).$$

It does not appear possible to satisfy this last condition unless the line of motion be rectilinear; that is, unless the motion be along an *axis*, which may be supposed to coincide with the axis of z. The function q, being of given form depending on the equation of the surface, cannot express the value of s. This function must therefore disappear on making $x=0$ and $y=0$; and we thus obtain

$$s=z+c',$$

and

$$\varphi=f(z+c'-ct).$$

This value applies strictly to motion along the axis of z. Before proceeding to substitute in equation (10.), it is necessary to express the value of φ for points immediately contiguous to the axis. For this purpose suppose

$$\varphi=f(z+c'+q(x, y, t)-ct).$$

Then substituting the letters f and q for the functions themselves, we shall have

$$\frac{d^2\varphi}{dx^2}=f''.\frac{dq^2}{dx^2}+f'\frac{d^2q}{dx^2}=f'.\frac{d^2q}{dx^2}, \text{ because } \frac{dq}{dx}=0,$$

$$\frac{d^2\varphi}{dy^2}=f'.\frac{d^2q}{dy^2}, \quad \frac{d^2\varphi}{dz^2}=f'', \quad \frac{d^2\varphi}{dt^2}=f''\left(\frac{dq}{dt}-c\right)^2+f'\frac{d^2q}{dt^2}.$$

Also supposing that

$$q = \alpha x + \beta y + gx^2 + hxy + ky^2 + \&c.,$$

it follows that

$$\frac{dq}{dx} = \alpha = 0, \quad \frac{dq}{dy} = \beta = 0, \quad \frac{d^2q}{dx^2} = 2g,$$

$$\frac{d^2q}{dy^2} = 2k, \quad \frac{dq}{dt} = 0, \text{ and } \frac{d^2q}{dt^2} = 0.$$

The coefficient h may be made to disappear by changing the direction of co-ordinates, and g and k must be supposed to be independent of the time. Hence by substituting in (10.) we obtain for determining the form of f the following equation:

$$f'' - \frac{2(g+k)a^2}{c^2 - a^2} f' = 0.$$

Putting m for the coefficient of f', and ν for the quantity of which f is a function, the integral of this equation becomes

$$f = \mathrm{A}e^{m\nu} + \mathrm{B}.$$

The form of f is thus ascertained. Since q is of arbitrary value, we may multiply it by $\sqrt{-1}$, and the value of f will then become

$$f = \mathrm{A}e^{-(\mathrm{G}x^2 + \mathrm{H}y^2)} . e^{m(z + c' - ct)\sqrt{-1}} + \mathrm{B}.$$

The equation (10.) being linear, may be satisfied by the sum of this value and that which results by changing the sign of $\sqrt{-1}$. So that putting $x = 0$ and $y = 0$, and suppressing the constant B, the final result is,

$$\varphi = \mu \cos m(z + c' - ct).$$

By putting $\frac{2\pi}{\lambda}$ for m, the resulting value of c is the following,

$$c = a\left(1 + \frac{(g+k)\lambda}{\pi}\right)^{\frac{1}{2}}.$$

This value agrees with what I have previously obtained in a different manner. The reasoning in the present method is more direct, in consequence of the use that has been made of the new hydrodynamical equation. I shall conclude this investigation with the remark, that the results arrived at are wholly incompatible with those deduced from the supposition of plane-waves, although the reasoning proceeded on the general hypothesis that $udx + vdy + wdz$ was an exact differential, and ought not, if that supposition were allowable, to have led to any contradiction. I infer that the supposition of plane-waves is not allowable.

I have been induced to make the last remark from having seen it asserted by the Astronomer Royal in the Number of the Philosophical Magazine for June, that I have pointed out a difficulty in the interpretation of an equation applying to the case of plane-waves. Mr. Stokes asserted the same thing before; and I then disclaimed, as I now disclaim, having pointed out any difficulty. The equation is a very simple one, and easily interpreted. A few steps of plain deduction conducts to a result incompatible with fluid motion. It follows in due course that the supposition of plane-waves cannot be made. This inference is in perfect accordance with the argument contained in this communication, which I think Mr. Airy may find to be worthy of some consideration. Any other inference would have presented a real difficulty.

Cambridge Observatory,
June 22, 1849.

LXXV. *Appendix to* Mr. Drach's *Paper on Epicyclic Curves in the last June Number.*

ANOTHER example, with series expanded.

$$n=-\frac{5}{2}\therefore p+q=7, \quad q=2.$$

Let $Q=Q'.ab$. The equation is

$$\overline{2x}|^7-7r^2.(2x)^5+14r^4(2x)^3-7r^6(2x)=a^7(Q'^2-2)+7a^6bQ'$$
$$+42a^5b^2+35a^4b^3Q'+35a^3b^4(Q'^2-2)+21a^2b^5(Q'^3-3Q')$$
$$+7ab^6(Q'^4-4Q'^2+2)+b^7(Q'^5-5Q'^3+5Q').$$

When $a=b$, the second member

$$=a^7\{(Q'+2)^5-3(Q'+2)^4=r^{10}a^{-10}-3r^8a^{-8}\}$$
$$=r^7\{r^3a^{-3}-3ra^{-1}\},$$

agreeably to Case 2.

The following errors have to be corrected in the last Number.

Ex. $$n=-3 \text{ for } \frac{Q}{a^3b} \text{ read } \frac{Qa^3}{b};$$

the general or sth term of $\left(\frac{Q}{ab}\right)^{q-2j}$'s bracketed factor is

$$\pm\left(\frac{b}{a}\right)^{2s}\cdot\frac{P(P-1)\ldots(P-2s+1)}{1.2.3\ldots2s}$$

$$\cdot\frac{(q-2s)(q-j-s-1)(q-j-s-2)..(q-2j+1)}{1.2.3..(j-s)},$$

and of $(Q\div ab)^{q-2j-1}$ is

$$\pm\left(\frac{b}{a}\right)^{2s}\cdot\frac{(P-1)(P-2)..(P-2s)}{1.2.3..(2s+1)}$$

$$\cdot\frac{(q-2s-1).(q-j-s-2).(q-j-s-3)..(q-2j)}{1.2.3.j-s}$$

$\pm$ as stated, so that both series end always with $q-2j+1$ or $q-2j$; two consecutive terms being in the ratio of

$$1 \text{ to } \frac{b^2}{a^2}\times\frac{P-2s}{2s+1}\cdot\frac{P-2s-1}{2s+2}\cdot\frac{q-2s-2}{q-2s}\cdot\frac{j-s}{q-j-s-1},$$

and

$$1 \text{ to } \frac{b^2}{a^2}\times\frac{P-2s-1}{2s+2}\cdot\frac{P-2s-2}{2s+3}\cdot\frac{q-2s-3}{q-2s-1}\cdot\frac{j-s}{q-j-s-2}$$

respectively.

Mr. Perigal's finite syphonoids, strongly resembling a distiller's 'worm,' are expressed by

$$x=a\cos q\phi,\quad y=b\cos p\phi.$$

Hence

$$2\cos p.q\phi=\Sigma A_i\left(\cos q\phi=\frac{x}{a}\right)^i=2\cos q.p\phi$$

$$=\Sigma B_j\left(\cos p\phi=\frac{y}{b}\right)^j.$$

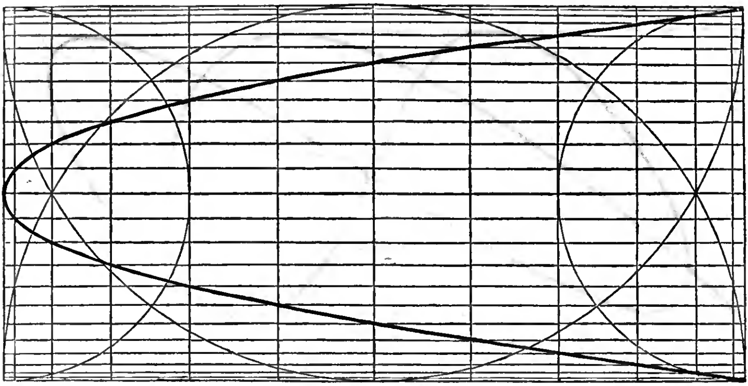
The nodal or lemnoid curves of a finite number of knots are comprised in

$$x=a\cos q\phi,\quad y=b\sin p\phi.$$

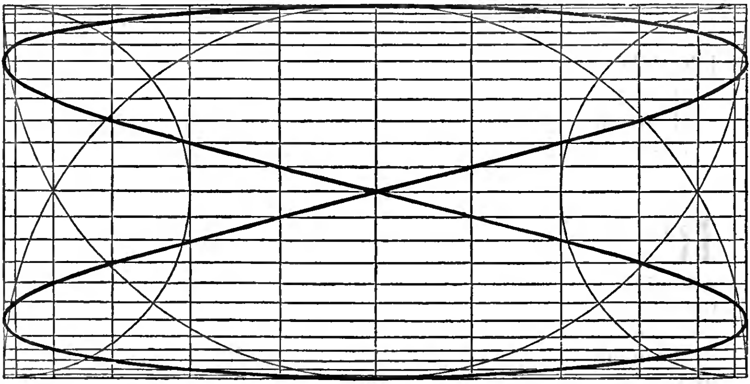
Hence

$$2\cos p.(2q\phi)=\Sigma A_i\left(\cos 2q\phi=\frac{2x^2}{a^2}-1\right)^i=2\cos q(2p\phi)$$

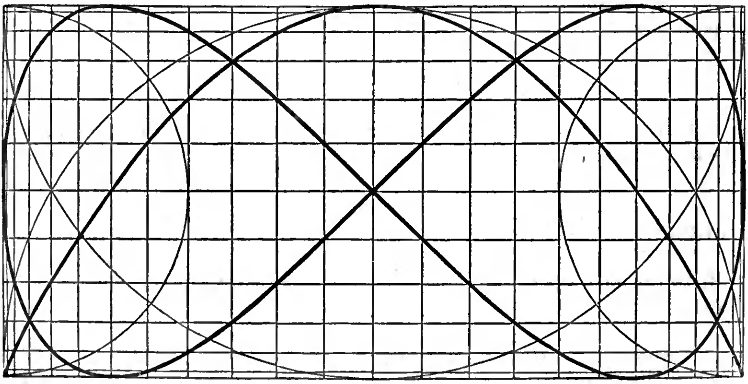
$$=\Sigma B_j\left(\cos 2p\phi=1-\frac{2y^2}{b^2}\right)^j.$$



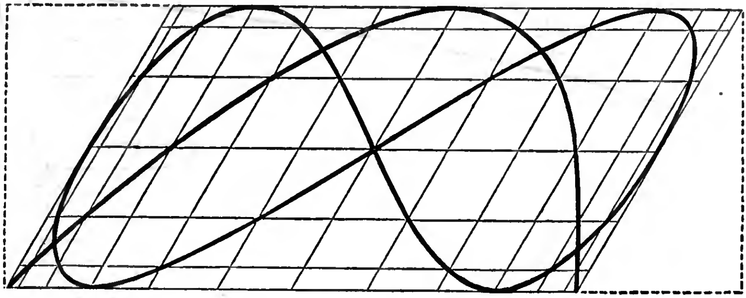
Retrogressive Syphonoid. $x = a \cos \phi$, $y = b \cos 2\phi$. $\therefore a^2(b+y) = 2bx^2$.



Oblate Lemnoid. $x = a \cos \phi$, $y = b \sin 2\phi$. $\therefore a^4y^2 = 4b^2x^2(a^2 - x^2)$.



Prolate Lemnoid and retrogressive Syphonoid.
Particular cases (extremes) of the same law of compound circular motion.



Oblique Lemnoid and retrogressive Syphonoid.

S. M. DRACH.

June 20, 1849.

LXXVI. *On the Distribution of the Superficial Detritus of the Alps, as compared with that of Northern Europe. By Sir* RODERICK IMPEY MURCHISON, *F.R.S., V.P.G.S. &c.; Mem. Imp. Ac. Sciences of St. Petersburgh, Corresp. Member of the Academies of Paris, Berlin, Turin, &c.**

REFERRING to his previous memoir upon the whole structure of the Alps and the changes which those mountains underwent, the author calls attention to the fact, that whilst during the formation of the molasse and nagelflue a warm climate prevailed, so after the upheaval of these rocks an entire change took place, as proved by the uplifted edges of these tertiary accumulations being surmounted by vast masses of horizontally-stratified alluvia, the forms of whose materials testify that they were deposited under water. The warm period, in short, had passed away and the pine had replaced the palm upon the adjacent lands, before a glacier was formed in the Alps or a single erratic block was translated.

Though awarding great praise to the labours of Venetz, Charpentier and Agassiz, which have shed much light on glaciers, and particularly to the work of Forbes for so clearly expounding the laws which regulate the movement of these bodies; Sir Roderick conceives, that the physical phænomena of the Alps and Jura compel the geologist to restrict the former extension of the Alpine glaciers within infinitely less bounds than have been assigned to them by those authors. True old glacier moraines may, he thinks, be always distinguished, on the one hand, from the ancient alluvia, and on the other from tumultuous accumulations of gravel, boulders and far

* Abstract of a Memoir read before the Geological Society May 30, 1849, when His Royal Highness Prince Albert honoured the meeting with his presence for the first time as a Fellow of the Society.

transported erratic blocks, as well as from all other subsequent detritus resulting from various causes which have affected the surface. He first shows, from the remnants of the old water-worn alluvia which rise to considerable heights on the sides of the valleys, that in the earliest period of the formation of the Alpine glaciers, water, whether salt, brackish or fresh, entered far into the recesses of these mountains, which were then at a considerably lower level, *i. e.* not less than 2500 or 3000 feet below their present altitude.

He next appeals to the existing evidences in the range of Mont Blanc to show, that as each glacier is formed in a *transverse* upper depression, and is separated from its neighbour by an intervening ridge, so by their movement such glaciers have always protruded their moraines across the adjacent longitudinal valleys into which they descended—and were never united to form one grand stream of ice. It is stated that there are no traces of lateral moraines on the sides of the main valleys at considerable heights above their present bottoms, whether on the flank of the great ridge from whence the glaciers issued or on the opposite side of each longitudinal valley, which must have been the case if a large mass of glacier ice had ever descended the general valley. On the contrary, examples of the transport of moraines and blocks *across* such *longitudinal* depressions are cited from the valley of Chamonix on the one flank and from the Allée Blanche and Val Ferret on the other flank of the chain of Mont Blanc. Another proof is seen in the ancient moraine of the Glacier Neuva, the uppermost of the valley of the Drance; and a still stronger case is the great chaotic pile of protogine blocks accumulated on the Plan y Bœuf, 5800 French feet above the sea, which have evidently been translated right across the present deep valley of the Drance, from the opposite lofty glacier of Salenon.

Having thus shown that none of the upper longitudinal and flanking valleys around Mont Blanc were ever filled with general ice-streams, the author has still less difficulty in demonstrating that all the great trunk or lower valleys of the Arve, the Doire, and the Rhone, offer no vestiges of what he calls a true moraine; all the detritus from great heights above their present bottoms exhibiting either water-worn pebbles or occasional large erratic blocks, more or less angular,—the latter being for the most part irregularly and sporadically dispersed. As Venetz and Charpentier have attached great importance to the original suggestion of an old peasant of the Upper Vallais, that a great former glacier alone could have carried the erratic blocks to the sides of the lower valley of the Rhone, so on the other hand the author relies on the practised eye of his intelligent Chamonix guide Auguste Balmat, who declares that he has never recognized the remains of "moraines" in that detritus of the larger valleys which has been theoretically referred to glacier action. In descending from the higher Alps into such trunk valleys, Sir Roderick found many examples of rocks rounded on the side which had been exposed to the passage of boulders and pebbles, with abrupt faces on the side removed from the agent of denudation, all of them reminding him forcibly of the storm and lee sides of the Swedish rocks over which similar water-worn materials have passed.

Seeing, then, that this coarse drift or water-worn detritus is distributed sometimes on the hard rocks and often on the summits of the remnants of the old valley alluvia, he believes that the whole of the phænomena can be explained by supposing that the Alps, Jura, and all the surrounding tracts have undergone great and unequal elevations since the period of the formation of the earliest glaciers—elevations which, dislodging vast portions of those bodies, floated away many huge blocks down straits then occupied by water, and hurled on vast turbid accumulations of boulders, sand and gravel. To these operations he attributes the purging of the Alpine valleys of the great mass of their ancient alluvia, and also the conversion of glacier moraines into shingle and boulders. He denies that the famous blocks of Monthey opposite Bex, can ever have been a portion of the left lateral moraine of a glacier which occupied the whole of the deep valley of the Rhine,—as Charpentier has endeavoured to show; and he contends that if such had been the case they would have been associated with numberless smaller and larger fragments of all the rocks which form the sides of the valley through which such glaciers must have passed. They are, however, exclusively composed of the granite of Mont Blanc; and must therefore, he thinks, have been transported by ice rafts,—which, having been forced with great violence through the gorge of St. Maurice, served to produce many of the striæ which are there so visible on the surface of the limestone*. Fully admitting that the stones and sand of the moraines of modern glaciers scratch, groove, and polish rocks, Sir Roderick Murchison still adheres to the idea he has long entertained from surveys in Northern Europe, that other agents more or less subaqueous, including icebergs and great masses of drift, have produced precisely similar results. He cites examples in the Alps, where perfectly water-worn or rounded gravel being removed, the subjacent rocks are found to be striated in the directions in which such gravel has been moved; and he quotes a case in the gorge of the Tamina, above the Baths of Pfeffers, where this ancient striation, undistinguishable from that caused by existing glaciers, has, by a very recent slide of a heavy mass of gravel from the upper slope of the same rock, been crossed by fresh scorings and striæ, transverse to those of former date, from which the markings made in the preceding year only differ in being less deeply engraved. He also adverts to the choking up of some valleys, particularly of the Vorder Rhein below Dissentis, by the fracture, *in situ*, of mountains of limestone, which constitute masses of enormous thickness, made up of innumerable small fragments, all of which have been heaped together since the dispersion of the erratic blocks; and he further indicates the effects of certain great slides or subsidences within the historic æra.

* Mr. Charles Darwin, in a recent letter to the author, adheres to his old opinions derived from observations in America, and says, " I feel most entirely convinced that *floating ice* and *glaciers* produce effects so similar, that at present there is, in many cases, no means of distinguishing which formerly was the agent in scoring and polishing rocks. This difficulty of distinguishing the two actions struck me much in the *lower parts* of the Welsh valleys."

In considering the distribution of the erratic detritus of the Rhone, Sir Roderick having denied that it can ever have been carried down the chief valley to the Lake of Geneva in a solid glacier, still more insists on the incredibility of such a vast body of ice having issued from that valley, as to have occupied all the low country of the cantons Vaud, Friburg, Berne and Soleure, and to have extended its erratics to the slopes of the Jura, over a region 100 miles in breadth from north-east to south-west as laid down in the map of Charpentier. He maintains that in the low and undulating region between the Alps and the Jura, the small debris derived from the former has everywhere been water-worn, and that there is in no place anything resembling a true moraine; and he therefore believes, that the great granitic blocks of Mont Blanc were translated to the Jura by ice-floats, when the intermediate country was under water. He further appeals to the water-worn condition of all the detritus of the high plateaux around Munich, 1600 and 1700 feet above the sea, to show that a subaqueous condition of things must be assumed when the great erratic blocks were carried to their present positions.

Prof. Guyot of Neufchatel has endeavoured to show, that the detritus of the rocks of the right and left sides of the upper valley of the Rhone have also maintained their original relative positions in the great extra Alpine depression, and that these relations are proofs, that nothing but a solid glacier could have arranged the blocks in such linear directions. But the author meets this objection by suggesting that there are notable examples to the contrary. He also refers to the great *trainées* of similar blocks which preserve linear directions in Sweden and the low countries south of the Baltic, to show that as this phænomenon was certainly there produced by powerful streams of water, so may the Alpine detritus have been arranged by similar agency. In alluding to the drainage of the Isère he further points to the admission of Prof. Guyot, that nearly all its erratic detritus, both large and small, is rounded and has undergone great attrition; and he quotes a number of cases in which such boulders and gravel, derived from the central ridges of Mont Blanc, have been transported *across* tracts now consisting of lofty ridges of limestone with very deep intervening valleys; and therefore he infers that the whole configuration of these lands has been since much changed, including the final excavations of the valleys and the translation of enormous masses of broken materials into the low countries of France.

In conclusion it is suggested, that the dispersion of the far-travelled Alpine blocks is a very ancient phænomenon in reference to the historic æra, and must have been coeval with the spread of the northern or Scandinavian erratics, which it has been demonstrated was accomplished chiefly by floating ice, at a time when large portions of the Continent and of the British Isles were under the sea. Viewing it therefore as a subaqueous phænomenon, Sir Roderick is of opinion that the transport of the Alpine blocks to the Jura falls strictly within the dominion of the geologist, who treats of bygone events, and cannot be exclusively reasoned upon by the meteorologist, who invokes a long series of years of sunless and moist summers to account for the production of gigantic glaciers upon land. This last hypothesis is at variance even with the physical phænomena in and around the Alps, whilst it is in entire antagonism to the much grander and clearly established distribution of erratics of the North during the glacial period. The effect in each case is commensurate with the cause. The Scandinavian chain, from whence the blocks of central Europe radiated, is of many times larger area than the Alps, and hence its blocks have spread over a much greater space. All the chief difficulties of the problem vanish when it is admitted, that enormous changes of the level of the land in relation to the waters have taken place since the distribution of large erratics; the great northern glacial continent having subsided, and the bottom of the sea further south having been elevated into dry land, whilst the Alps and Jura, formerly at lower levels, have been considerably and irregularly raised.

LXXVII. *Note on the Theory of Permutations.* *By* A. Cayley*.

It seems worth inquiring whether the distinction made use of in the theory of determinants, of the permutations of a series of things all of them different, into positive and negative permutations, can be made in the case of a series of things not all of them different. The ordinary rule is well known, viz. permutations are considered as positive or negative according as they are derived from the primitive arrangement by an even or an odd number of inversions (*i. e.* interchanges of two things); and it is obvious that this rule fails when two or more of the series of things become identical, since in this case any given permutation can be derived indifferently by means of an even or an odd number of inversions. To state the rule in a different form, it will be convenient to enter into some preliminary explanations. Consider a series of n things, all of them different, and let abc... be the primitive arrangement; imagine a symbol such as (xyz) (u) (vw)... where x, y, &c. are the entire series of n things, and which symbol is to be considered as furnishing a rule by which a permutation is to be derived from the primitive arrangement abc... as follows, *viz.* the (xyz) of the symbol denotes that the letters x, y, z in the primitive arrangement abc... are to be interchanged x into y, y into z, z into x. The (u) of the symbol denotes that the letter u in the primitive arrangement abc... is to remain unaltered. The (vw) of the symbol denotes that the letters x, y in the primitive arrangement are to be interchanged x into y and y into x, and so on. It is easily seen that any permutation

* Communicated by the Author.

whatever can be derived (and derived in one manner only) from the primitive arrangement by means of a rule such as is furnished by the symbol in question*; and moreover that the number of inversions requisite in order to obtain the permutation by means of the rule in question, is always the smallest number of inversions by which the permutation can be derived. Let α, β... be the number of letters in the components (xyz), (u) (vw) &c., λ the number of these components. The number of inversions in question is evidently $\overline{\alpha-1}+\overline{\beta-1}+$&c., or what comes to the same thing, this number is $(n-\lambda)$. It will be convenient to term this number λ the exponent of irregularity of the permutation, and then $(n-\lambda)$ may be termed the supplement of the exponent of irregularity. The rule in the case of a series of things, all of them different, may consequently be stated as follows: "a permutation is positive or negative according as the supplement of the exponent of irregularity is even or odd." Consider now a series of things, not all of them different, and suppose that this is derived from the system of the same number of things *abc*... all of them originally different, by supposing for instance $a=b=$&c., $f=g=$&c. A given permutation of the system of things not all of them different, is of course derivable under the supposition in question from several different permutations of the series *abc*... Considering the supplements of the exponents of irregularity of these last-mentioned several permutations, we may consider the given permutation as positive or negative according as the *least of these numbers* is even or odd. Hence we obtain the rule, "a permutation of a series of things not all of them different, is positive or negative according as the minimum supplement of irregularity of the permutation is even or odd, the system being considered as a particular case of a system of the same number of things all of them different, and the given permutation being successively considered as derived from the different permutations which upon this supposition reduce themselves to the given permutation." This only differs from the rule, "a permutation of a series of things, not all of them different, is positive or negative according as the minimum number of inversions by which it can be obtained is even or odd, the system being considered, &c.," inasmuch as the former enunciation is based upon and indicates a direct method of determining the minimum number of inversions requisite in order to obtain a given permutation; but the latter is, in simple cases, of the easiest application. As a very simple

* See on this subject Cauchy's *Mémoire sur les Arrangemens, &c., Exercices d'Analyse et de Physique Mathématique,* t. iii. p. 151.

example, treated by the former rule, we may consider the permutation 1212 derived from the primitive arrangement 1122. Considering this primitive arrangement as a particular case of *abcd*, there are four permutations which, on the suppositions $a=b=1$, $c=d=2$, reduce themselves to 1212, viz. *acbd*, *bcad*, *adbc*, *bdac*, which are obtained by means of the respective symbols $(a)(bc)(d)$; $(abc)(d)$; $(a)(bdc)$; $(abdc)$, the supplements of the exponents of irregularity being therefore 1, 2, 2, 3, or the permutation being negative; in fact it is obviously derivable by means of an inversion of the two mean terms.

58 Chancery Lane,
June 1849.

LXXVIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 469.]

Feb. 22, 1849. DESCRIPTION of an Infusory Animalcule allied to the genus Notommata of Ehrenberg, hitherto undescribed. By John Dalrymple, Esq., F.R.C.S.

The examination of various specimens of the animalcule described by the author, disclosed the diœcious character of one of the more highly organized of the rotiferous class of Infusoria, hitherto supposed to be androgenous. This discovery was first made by observing the difference in the form and development of the embryo while still enclosed in the ovisac of the parent animal. From the extreme transparency of this form of rotifer, it is possible to trace the progressive development of the young from the Græffian vesicle in the ovary to the period of mature gestation, when the embryo is expelled, the whole machinery of whose organs has been perfected while still within the body of the female.

Thus, although the young one observed in the ovisac, when nearly ready to be expelled, was in the great majority of instances a miniature portrait of the parent, yet occasionally an embryo was seen of a different aspect, within whose body a vesicle was discovered filled with actively moving spermatozoa.

A further investigation of the subject brought clear evidence of the functions performed by this male,—its copulation with the young females; but it also displayed the singular fact, that although the organs of reproduction and locomotion were highly developed, there was a total absence of those of assimilation; in fact, that neither mouth, nor stomach, nor other digestive cavity or glands, were present in its curious organization.

In the early part of the paper the author describes the anatomy of the female, which differs from the family of Notommata of Ehrenberg, in the absence of intestine and anal orifice, and forcipated or caudal foot. In every other respect the organization is so similar to that class, that the author believes the proper place for this animalcule to be in a *sub-genus* of Notommata.

In relation to physiology, the author submits a new theory of the mechanism of circulation and respiration in the general group of Rotifers, a subject which is but obscurely treated of by the great German observer, who appears to have believed in the existence of tubular vessels or true vascular system. The author thinks, however, that these functions are performed in a manner more resembling that of insects, viz. that the blood is contained in the general cavity of the animal and circulates round the lung, which is here represented by a contractile vesicle that receives and expels the water in which the animalcule lives, and so comes to be in intermediate relation with the air mixed with the water. The difference therefore between the aëration of the blood of insects and that of this rotifer is rather due to the difference of the media they respectively inhabit, than of design. In both, the blood is contained in a general cavity and brought in contact with the air, without the intervention of any true vascular system.

The beautiful transparency of the animal, and the facility with which the development of the ovum may be traced through all its stages, induces the author to believe it to be well-suited to the inquiries of the embryologist and of those who devote themselves to the study of the metamorphosis of cell into tissue.

This animalcule has hitherto been discovered only in a few situations (in Norfolk near Norwich, and in Warwickshire near Coventry), but it is believed, from the very general dispersion of Infusoria, that it may be more extensively met with, especially in the months of June, July, August and beginning of September.

The author concludes by expressing his belief that re-examination of the whole order of Rotifera is necessary to determine the disposition of the sexes, and to assign them their proper situation in the scale of animated beings.

"On the Integration of Linear Differential Equations." By the Rev. Brice Bronwin. Communicated by C. J. Hargreave, Esq., F.R.S.

The method chiefly employed in this paper, is analogous to one which the author had previously applied (Camb. Math. Journal, No. 4) to the integration of such equations in cases where the coefficients are integer functions of the independent variable. Here they are any functions of that variable, it being however understood that in all integrable cases there must be some relation among these coefficients. The integration is effected by a general theorem of the form

$$D^n f(\varpi_{m+n})u = f(\varpi_m)D^n u,$$

where D denotes any function of x, and ϖ a function of symbols both of operation and quantity. By means of this theorem, and the substitution $u = \varpi_1 \varpi_2 \ldots \varpi_n v$, or some other similar one, the equation is either reduced to an integrable form, or to an equation of a lower order; or, when neither of these objects can be accomplished, the method may be employed to effect a transformation.

The method applies most readily to equations of the second order; but may be applied to those of a higher order, the coefficients becoming more restricted as the order rises. The integrable cases are very numerous and vary considerably in form; and, as each distinct form requires a variation in the process, they are distributed into classes. In each class, a few particular examples, derived from the general cases, are given.

By means of the general theorem, the equation

$$\varpi_m \varpi_n u + p\rho u = X$$

may be integrated in the most general case, or when the coefficients are any functions of x, having, however, certain relations between them.

Several theorems of the form $\pi_n \rho u = \rho \pi_{n-1} u$, where $\rho = D + \theta$, $\pi_n = D^2 + A_n D + B_n$, or similar to it, are given. They are not found without difficulty; are much more restricted in their application than the general theorem; and lead to but few results; but they are deserving of notice on the ground that they may possibly succeed in a particular case when all other methods fail.

A few general examples of a class of equations, the solution of which is attended with considerable difficulty, are next given. These are of the forms,

$$D\varpi^2 u + b\varpi^2_n u = X, \quad D^2\varpi^2_n u + b\varpi^2_{n+m} u = X,$$

and others varying a little from them.

The concluding part of the paper is occupied with the transformation and application of one or two of the general theorems which have been given by the author in the Cambridge Mathematical Journal, New Series, vol. iii., from which a few examples, more or less particular, have been derived.

March 1, 1849.—"Minute Examination of the Organ of Taste in Man." By Augustus Waller, M.D. Communicated by Richard Owen, Esq., F.R.S.

The author commences by describing his mode of observation, which differs from that followed by previous observers. It consists in removing from the living tongue one of the papillæ, and immediately subjecting it to examination. He then proceeds to describe,—1st, the epithelium; 2nd, the fungiform papillæ; 3rd, the conical papillæ; and 4th, the inferior surface with its mucous glands, &c.

1. The epithelium is of two kinds; the flat plates with a central nucleus, which are mostly found clothing the stem and other regions of the fungiform papillæ; and the globular cells which compose most of the external parts of the processes of the conical papillæ.

2. The fungiform papillæ are found to consist of numerous small cones seated on a common stem. These secondary cones, already described by Albinus, are completely hidden by a common investment of epithelium which fills up the irregular spaces between them. Each of these cones contains capillary vessels, which, at the apex of the cone, either form a simple loop or a complex coil which is covered only by epithelian scales of the most attenuated nature. The author states that in these capillary vessels the motion of the blood may be observed for several seconds after the removal from the living body,

and may be excited for a long time by the application of a slight degree of pressure. By these means he has been enabled to watch the passage of the red and white globules contained in the blood, and to detect in the human papillæ all the various phenomena in the transparent membranes of the lower animals. By allowing the blood to coagulate in the vessels, beautiful examples of injected papillæ may be obtained. The congestion of the vessels is much increased by compressing the point of the tongue before the removal of the papillæ. The capillaries are connected together at the bases of the secondary papillæ, and arise from a common trunk immersed in the body of the papilla. The nerves are found to subdivide in the separate cones, in which they ascend to the apex and terminate in abrupt extremities, as in the frog, toad, &c. In the fœtus the fungiform papillæ are stated to consist of a simple cone without any secondary papillæ.

3. The conical or filiform papillæ of man are described to be of a compound nature, consisting of numerous secondary cones springing from a common stem. Each of these secondary cones is clothed with an elongated process which is fitted on the cone like a sheath. This process consists of elongated epithelial scales ascending towards the summit, and resembling in general appearance the feather of an arrow. At their summit these processes are clothed with an external zone of granular matter, which considerably adds to their thickness. This granular matter is often detached after the papilla has been removed a short time from the tongue. The blood-vessels form a simple loop at the summit of the papilla, and the nerves are arranged in a similar manner.

4. The inferior surface is described as very smooth, presenting numerous follicles abundantly supplied with blood-vessels and nerves. These follicles are generally of a conical shape and surrounded with an arch composed of epithelial cells. The nerves may frequently be detected and followed over the surface of the follicle, but their extremities are hidden amidst the blood-vessels.

The author has illustrated the paper by several drawings.

ROYAL ASTRONOMICAL SOCIETY.

[Continued from p. 463.]

March 9, 1849.—Substance of the Lecture delivered by the Astronomer Royal on the large Reflecting Telescopes of the Earl of Rosse and Mr. Lassell, at the last November Meeting.

The Astronomer Royal gave that evening an account of the large reflecting telescopes of the Earl of Rosse and Mr. Lassell, which he had personally examined in the course of the last summer.

Premising that the subject might be considered interesting to the Society on these two grounds, first, that the reflecting telescope is exclusively a British instrument in its invention and improvement, and almost exclusively so in its use; and secondly, that it had been almost exclusively the instrument of amateurs—a circumstance which seemed to prove both the difficulty of constructing it and its great excellence when properly constructed—the Astronomer Royal remarked that his account would consist in some measure of a statement of the differences between the processes of these two amateur constructors. These differences, he thought, would be found well-worthy the attention of all who were engaged in or contemplated the construction of reflecting telescopes. It is certain that both systems of methods are successful; and it may be doubtful how far the differences are connected with the difference of dimensions of the telescopes: for in all that follows it must be borne in mind, that Lord Rosse's largest telescopes are 6 feet in clear aperture and more than 50 feet in focal length, while Mr. Lassell's are 2 feet in clear aperture and about 20 feet in focal length; that the thicknesses of the specula are nearly in the same proportion as their diameters; and hence that the weights of the specula are nearly as 27 to 1 (that of Lord Rosse's being about four tons, and that of Mr. Lassell's being about three hundred-weight), a difference which in itself might be expected to require some difference of construction.

I. The first difference is in the constitution of the metal-mixture used for the speculum.

In Lord Rosse's specula the metal is purely a mixture of tin and copper, in a proportion understood to be an atomic proportion, the weight of the copper being something more than double that of the tin. In the process itself there appears to be strong evidence that this proportion is truly atomic. When the metals are mixed in the intended proportions, it is found that the addition of one or two ounces of either metal to a mass of forty pounds of the mixture produces a difference so striking, that it is at once recognised by every person who is employed on the works. The mixed metal, when in fusion, possesses a remarkable union of penetrating power and of viscidity. It is so penetrating that Lord Rosse found it impossible to retain it in the ordinary cast-iron crucibles, which are cast with the mouth downwards; and he was compelled to have crucibles made expressly for this purpose, cast with the mouth upwards. It is at the same time so viscous, that when, in the casting of the speculum, the liquid metal is poured upon what Lord Rosse calls a "bed of hoops," that is, a broad base of the mould, formed by pressing together the flat surfaces of a great number of iron hoops, whose edges (trimmed into shape by turning and other mechanical operations) form the base, and through the interstices of which the air can escape, the metal itself does not in the smallest degree enter into these interstices. As soon as the cooling metal has acquired some consistency, it is dragged into the annealing furnace, every part of which has been brought to a low red heat. Great attention is given to the form of the floor of this furnace, as the metal is still in such a state that it receives figure from the furnace-floor. The furnace is then closed (care being taken to prevent one part from cooling, by radiation or convection, more rapidly than another), and after some weeks it is opened and the metal is taken out.

In Mr. Lassell's specula, the metal is made by first mixing copper and tin in the same proportions as those employed by Lord Rosse, and then adding a small quantity of white arsenic. It is well known that very great importance was attached to the admixture of arsenic by the principal constructors of reflecting telescopes in the last century. Lord Rosse has uniformly stated it as the result of his experience, that when the copper and tin are properly proportioned, nothing is gained by adding arsenic; Mr. Lassell is equally confident that the brightness of the metal is much improved by it. It would be idle to express an opinion on this point without comparing specimens of the two kinds of speculum-metal side by side; and the Astronomer Royal only undertook to say that both bear a very high polish. The Astronomer Royal believed that the operation of annealing Mr. Lassell's mirrors was nearly similar to that for Lord Rosse's, although the trouble and risk of the process are of course materially diminished by the much smaller dimensions of the speculum.

The next step in Lord Rosse's operations is to turn (by a grinding process, with emery as the abrading powder) the edge of the mirror, the mirror being placed with its broad surface horizontal, with its lower surface and about one-third of its depth immersed in water, and being turned horizontally by a vertical spindle passing through a stuffing-box in the bottom of the water vessel. The purpose of this turning is, to make a nearly air-tight fitting for a covering which is to be applied to the mirror when it is not in use, and with which is connected a box of quicklime, for the desiccation of the air in contact with the mirror; and the object of turning it in water is, to keep all parts of the speculum in the same temperature, a caution which is necessary in every part of the operations. This caution is probably requisite only for specula as large as those of Lord Rosse. For these it was found by Lord Rosse, that if the iron grinder (to be mentioned presently) be washed with warm water, and be then applied to the speculum, the metal almost infallibly cracks. A surface of warm pitch may, however, be applied without producing the same bad effect.

II. The subject next worthy of attention, rather for the similarity of the methods of the two constructors than for their dissimilarity, is the method of mounting the mirrors. And this seems to be a proper place for mentioning the mounting, because it is indispensable (at least with Lord Rosse's specula) that the mirror be ground and polished on the very same supports, applied in the very same manner, as when the mirror is in use in the telescope.

The special object of Lord Rosse's support is this: supposing the surface of the speculum to be divided into any number of equal portions, each of a form not particularly elongated in any direction, then the supports are to be so arranged that every one of these portions shall necessarily sustain the same upward pressure, acting at its centre of gravity. At the same time it is necessary, for definiteness of support, that the ultimate support upon the fixed frame be upon three points. These objects are thus attained: the surface of the mirror is divided into twenty-seven equal portions, and these are grouped into nine groups, of three in each; to each portion is attached, by felt and pitch, a small plate of cast iron; and in a small hole sunk in this cast iron, opposite to the centre of gravity, a projecting pin of a triangular plane lever takes its bearing. This triangular plane lever is merely a triangle, having three points to sustain pressure, and a small hole for the fulcrum at the centre of gravity of the three points considered as equal weights; it is, then, evident that if that point is made really the fulcrum, the pressures at those three points are necessarily equal. The same construction being applied to each of the nine groups, then it is only necessary to support the nine fulcra in such a way that the pressures upon them are necessarily equal; and this is done by grouping three of their fulcra as points of pressure upon the three points of another and stronger triangle, whose fulcrum is at the centre of gravity of these three points considered as equal weights; and this fulcrum is one of the points of the fixed frame. It will thus be evident that every one of the first-mentioned pins sustains exactly one twenty-seventh part of the whole weight of the mirror, or rather of the whole pressure of the mirror perpendicular to its surface, neither less nor more. [When the mirror is in the telescope, it exerts also an edgewise pressure, which in Lord Rosse's construction was at first sustained by fixed pillars of the fixed frame; of these more will be said hereafter.] The fixed frame has small wheels; while it is in the grinding-trough, it is so lifted off the wheels that it takes a firm bearing upon the rotating frame; when it is to be carried to the telescope it is lowered to take bearing upon its wheels, the side of the grinding-trough is taken off, the fixed frame carrying the speculum is wheeled upon a proper carriage, the carriage conveys it to a place very near the telescope, where is a railway at proper height for receiving the small wheels; the telescope is placed vertical, its lower end is opened, and continuation-rails are laid to it, and the fixed frame is thus wheeled into the telescope, carrying the mirror; then by powerful screws the bearing of the fixed frame is received upon three points, the wheels being entirely lifted off their bearings. [Since giving this account, the Astronomer Royal has learnt from Lord Rosse that a speculum which was raised for a few minutes only from its lever-bearings received in that time a permanent change of figure.]

In Mr. Lassell's operations the speculum is supported on eighteen points, the grouping being first made by two and two, with straight levers, and then the fulcra of the straight levers being by means of triangular levers supported upon three ultimate points. The Astronomer Royal was not able to say whether the same cautions as to retaining the speculum at all times upon the same bearings, which Lord Rosse found necessary, were required for Mr. Lassell's mirrors; but it is evident that the difficulties of support of every kind are here very much less. The edgewise pressure, when in the telescope, is here supported by a semicircular iron hoop, of which more will be said hereafter.

III. The next point deserving special comparison is the apparatus for grinding and polishing.

The apparatus used by Lord Rosse imitates very closely, but with that superior degree of regularity which is given by machinery, the operation of polishing by hand. Proceeding from the quick motions to the slow ones, the order of movements is as follows:—(1.) By a steam-engine, a rotatory motion round a vertical spindle is given to a crank, which by a connecting rod acts upon a sliding rod that moves the grinder or polisher backwards and forwards. This sliding rod passes through a fixed guide at the end next the connecting rod, and through a slowly moving guide [described under (2)] at the other end; and thus every stroke of the grinder is very nearly a straight stroke. The sliding rod is interrupted in its middle, and there its place is supplied by a hoop which loosely surrounds the grinder: this construction is necessary, because, on account of the great size and weight of the grinder, it is necessary that it be supported by counterpoises in various parts, at the same time that it is necessary that (in order to prevent the formation of striæ in a definite direction on the grinder) the grinder be left free to revolve: that motion of revolution is given merely by the friction upon the mirror, whose rotation will be mentioned under (3), and depends upon the weight with which the grinder presses the mirror. (2.) A band from the crank-spindle (1) passes round a wheel which carries a crank, in which is the forked guide (turning by a spindle in the crank end) that guides the distant end of the sliding rod; and thus the strokes of the grinder do not pass uniformly over the centre of the mirror, but pass in their direction a certain distance to the right and left. But as, in the ordinary crank motion, the duration of the strokes at the extreme right and left would be too great, the wheel on the spindle of this grinding-crank is elliptical, the proportion of its axes being about three to one: its angular motion is therefore unequal; and the strokes are thus made to dwell a shorter time near the extreme right and left, and a longer time near the centre. (3.) Another band from the crank spindle (1) passes round a large wheel on the vertical axis, that passes through a stuffing-box in the bottom of the grinding-trough, and there carries a broad frame, on which is placed the fixed frame of the mirror, supporting the mirror itself. Thus the mirror turns slowly round to receive the strokes of the grinder in every direction. It is only necessary to add to this, that a large part of the grinder's weight is equally supported at twelve points by a lever counterpoise above it, which supports the centre of a triangle, each point of which (by a roller over which a cord passes) supports two points, each of which points is the middle of a straight lever whose ends are attached to the grinder.

In Mr. Lassell's apparatus (omitting some details, which are not now necessary, as a detailed account of this apparatus has lately been given by Mr. Lassell himself to the Society), the steam-engine puts in motion (1) a vertical spindle below, which turns the mirror slowly, and (2) a vertical spindle above, which has a horizontal arm, in which is a planet-wheel turned by its tooth-connexion with a fixed sun-wheel. The axis of this planet-wheel carries another wheel that works in a third wheel, whose spindle is carried by the horizontal arm, but whose place can be fixed at pleasure near to or far from the centre of the vertical spindle (2). An arm from the spindle of the third wheel carries a spike pointing downwards (the distance of this spike from the centre of the spindle of the third wheel being adjustable), and this spike lodges loosely in the centre of the grinder and moves it upon the face of the mirror. Thus it will be seen that the centre of the grinder is always moved in an epitrochoid (including the circle as a possible case, if the adjustment is made for that curve), in which the proportion of velocities of the two circles is fixed, but the radii and their proportions are adjustable. The grinder, as in Lord Rosse's construction, is allowed to take freely the rotatory motion which it may receive from the friction on the mirror; but no counterpoise is used, the weight of the grinder being comparatively small.

The essential difference of these constructions, as regards the movements of the grinder, is therefore this: that in Lord Rosse's apparatus every stroke is very nearly straight, while in Mr. Lassell's apparatus there is no resemblance to a straight movement at any part of the stroke.

IV. The process of grinding is nearly the same (with differences corresponding to the difference of dimensions) in the operations of both constructors.

In Lord Rosse's grinding, the speculum having received an approximate figure from the form of the annealing furnace, the cast-iron grinder is brought very exactly to form by turning upon the lathe, with proper reference to a gauge, and (if not done before) its surface is scored with cross-furrows about two inches apart and nearly an inch deep, leaving the acting part of the surface in squares. The grinder is then mounted in the apparatus; and this is the most dangerous part of the whole operation. The slightest jar of the iron grinder upon the mirror would break the mirror; and to avoid this risk, a great number of thin wooden wedges is placed upon the edge of the mirror, the grinder is slowly lowered upon them, and then by degrees they are gently withdrawn. The grinder is then used to grind the surface, with the intermediate powder of emery and water; coarser and finer emery in succession. A heavy weight is allowed to press the grinder upon the mirror; and as the grinder itself suffers much in form, it is repeatedly re-turned upon the lathe. This operation sometimes lasts many days.

Of the grinder used by Mr. Lassell the Astronomer Royal could give no account, but believes that it is of wood, the same which is used for polishing. The abrading powder used is the same as Lord Rosse's (emery, coarser and finer in succession).

V. The next point deserving attention is the important process of polishing.

When the figure of the speculum given by grinding is supposed by Lord Rosse to be sufficiently accurate, the projecting squares of the cast-iron grinder are covered with a coating of resin and turpentine, of such a consistence that, at a temperature of about 50° Fahrenheit, the nail can easily make an indentation in it. This is then covered with another coating, of a substance formed by combining the mixture last-mentioned with a certain quantity of wheatflour, and of such a consistence that, at a temperature of 80° Fahrenheit, the nail can make an indentation in it. It is not to be understood that there is any particular virtue in the temperatures 50° and 80°, but (for reasons to be hereafter given) it is necessary to have two strata of different degrees of hardness (the harder being the exterior); and the hardnesses defined by those two temperatures having been used in many experiments, the other adjustments have been determined, using these as bases; and if these bases were now changed, every other adjustment must be changed.

The necessity for the different strata of resin is thus explained. It is necessary that the polisher yield a little, else the polishing surface could not be in contact with the mirror at all parts of its stroke (the mirror being supposed parabolical), and this yielding is given by the first or soft stratum. But it is also necessary that the stratum next to the metal be hard; for if, in passing across a scratch or furrow, it were able to accommodate its form to that of the furrow, it would round off and polish the edges of the furrow, and this would very much injure the image of a bright object.

The coating is heated to softness by the flame of a torch, and the grinder is then lowered upon the mirror, and the coating takes the proper form. The mirror is exposed to no danger of cracking from this application.

The powder used is the red oxide of iron, prepared by precipitating (by means of ammonia) the black oxide of iron from a solution of sulphate of iron, and then heating the black oxide in a furnace with access of air. Lord Rosse finds that no other method of preparing the red oxide is successful. No polishing-putty or other powder of any kind is employed.

The powder is moistened with water to a degree known by experience, and then, the counterpoise of the grinder being so much increased that the remaining friction is enough to turn the grinder only once for about sixteen revolutions of the mirror, the operation goes on for about eight hours. It is essential, for reasons similar to those lately mentioned, that the temperature of the air and the temperature of the dew-point have nearly certain definite values (the latter, that the water mixed with the powder may dry in the proper degree): if the external air is too dry, the air of the room is moistened by a jet of steam; if it is too damp, the polishing is not attempted.

After about eight hours the grinder is lifted and a fresh application is made of powder mixed with "ammonia soap," a substance formed by treating common soap with ammonia. The metal then dries more rapidly, and the labour to the engine becomes much greater: the work is continued till the surface is dry, or very nearly dry; the grinder is taken off, and the mirror is found finished, having a parabolic figure, and a very high polish.

It is to be remarked here that the smaller mirrors made by Lord Rosse (3 feet diameter, about 25 feet focal length,) were tried before they were used, by means of an object fixed something more than 50 feet above them, whose image accordingly is found at something less than 50 feet above them. But for the larger mirror such a trial is impracticable (the height of the object must exceed 100 feet), and the mirrors have therefore been placed in the telescope without any trial, and their definition has been found perfect.

Mr. Lassell uses for polisher a wooden plate formed of two thicknesses of pine-wood with the grain crossed; and this apparently yields to accommodate itself to the form of the mirror. In the arrangement of its square protuberances, it is similar to Lord Rosse's; but it is covered with only one coating of pitch. The polishing powder used is the same as Lord Rosse's. The Astronomer Royal believed that the attentions to temperature, moisture, &c., which Lord Rosse found indispensable for his large mirrors, are not found necessary by Mr. Lassell. Mr. Lassell finishes the operation with the speculum wet.

VI. The next point to which allusion was made is the form and mounting of the telescopes.

Lord Rosse's telescope is a wooden tube, its interior diameter exceeding 6 feet in every part, being at the middle about 7 feet, and nearly 50 feet in length. This is fixed to a cube of 10 feet, which has folding-doors on that side which, when the telescope is horizontal, is the upper side (at which side the fixed frame supporting the mirror is introduced, as has already been said), and which carries the fixed frame by three large screws in that side of the cube which is opposite the mouth of the telescope. To this side of the cube is attached the universal joint by which the lower end of the telescope is connected with a fixed support, the joint being a few feet below the general surface of the ground. On each side (east and west) of the telescope is an enormous pier of solid masonry, about 70 feet long, in the north and south direction, between 40 and 50 feet high, and in its thickest part nearly 20 feet thick. [None of these dimensions are taken from actual measure.] The fixed support is nearer to the north than to the south ends of these piers. Near the top of the piers, on the interior faces, in the east and west plane passing through the universal joint, are two cranes with pulleys (the turning crane being no bigger than suffices to carry a large pulley, whose edge is in the vertical axis of the crane); over these cranes the chains pass which are attached to the telescope; and to the lower ends of the chains, after they have passed fixed pulleys on the walls, are attached the counterpoises, weighing about four tons each. These counterpoises are not allowed to depend freely, but are connected by bridle-chains with wooden horns that project from the north ends of the piers; the effect of this arrangement is, that when the telescope tube is nearly horizontal, and the force required to support it is very great, the weight of the counterpoises acts very nearly vertically on the chains, and is entirely effective for the support of the telescope; but when the telescope is considerably elevated, and less supporting force is required, the weight of the counterpoises is supported in a great measure by the bridle-chains, and very little tension is given to the supporting chains. For the sake of supplying some slight defects in the laws of tension thus produced, and also for the sake of constantly producing a small tendency in the telescope towards the south horizon, other counterpoises, in a pit south of the fixed support, are brought successively into action as the telescope is raised. There is then only a comparatively small and very manageable tendency of the telescope towards the south, and this is supported by a light chain which passes over a pulley on a bar connecting the horns before mentioned (the pulley being in the direction of a polar axis passing through the lower universal joint, and the motion of the telescope, therefore, for a given length of the chain, being equatoreal), and this chain is shortened or lengthened, and the telescope is thereby raised or depressed, by a windlass a little way north of the fixed support. Upon the inner face of the eastern pier is an iron arc of a circle, upon which slides a runner connected with a rod that passes through a frame on the telescope tube and near to its mouth, and is there racked for working with a pinion: by the movement of this pinion the distance of the telescope from the pier is altered, and thus a motion in hour-angle is given. At the south ends of the piers there are strong ladders, upon which (assisted by counterpoises) there slides a stage; upon which stage a small observing-box travels east and west: this is used for observing, so long as the mouth of the telescope is below the end of the pier. For greater elevations, the top of the western pier being shaped by slopes so as to approximate to a circular arc, there are mounted upon it curved galleries, which are carried by beams that run above and below pulleys fixed to the top of the pier; and the galleries are carried out by rack-and-pinion work, to approach the side of the telescope. It is intended to give the power of observing as far north as the pole; but at present the galleries extend only to the zenith. The telescope is Newtonian, the minor axis of the small mirror being about six inches, and the observer looks into the side of the tube.

Mr. Lassell's tube is of sheet-iron; and this tube is not carried immediately by the mounting, but is inserted in a long box of cast iron, in which it can be turned round its own axis. This movement is necessary to place the eyepiece exactly in the same side-position in all directions of the telescope, and also to cause the edgewise support of the mirror to act always in the same way. The long box is mounted equatoreally, the polar axis turning in two bearings below the declination axis, and carrying an hour-circle, upon which are fixed two supports, in which turn the two pivots of the declination axis of the long box. The telescope is Newtonian, the eyetube being in one side; but the smaller dimensions of the small mirror (a diameter of two inches only being required) enable Mr. Lassell to use the reflexion at the internal surface of a glass prism, by which much more light is reflected than by a metallic reflector. At first much annoyance was caused by the deposition of dew on the glass, but this was remedied by attaching to it a case carrying a small piece of heated lead; and when proper attention is given to the inclosure of the lead, no inconvenience is sustained from the effect of the hot metal in disturbing the air in the tube of the telescope. The whole is covered by a revolving dome thirty feet in diameter, and the observer is mounted for observation on a stage which is carried by the dome.

The Astronomer Royal then proceeded to describe some of the difficulties to which instruments of this class are yet liable, founded partly upon his own observations with Lord Rosse's telescope.

Upon directing the telescope to an object very near the zenith, it was seen very well defined, or at least with no discoverable fault. It must be remarked that the image of a star never assumes the neat spherical form to which the eye of an observer with a fine refractor is so much accustomed. This arises evidently from the circumstance that (from the great aperture of the mirror) the diffraction image and diffraction rings are invisibly small, and the form of the blurred image is probably determined by the irregular sensibility of the nervous membranes of the eye. The same effect exactly is produced by a large refractor when a power is employed too low to exhibit the rings.

But when the telescope was directed to a star as low as the equator its image was very defective. The defect, however, followed that simple law which the present Master of Trinity College has described by the word *astigmatism*. When the eyepiece was thrust in, the image of the star was a well-defined straight line, 20 seconds long, in a certain direction; when the eyepiece was drawn out a certain distance (about half an inch from the former position) the image of the star was a well-defined straight line, 20 seconds long, in a direction at right angles to the former. Between these two positions the image was elliptical, or, at the middle position, a circle of 10 seconds diameter. The image of Saturn (then without a ring) was, in the two positions above-mentioned, an oval (not an ellipse) whose length was about double its breadth; or, in the middle position, it was a confused circle, whose diameter was about 30 seconds instead of 20. The position of the astigmatic lines had no distinct relation to the vertical plane; and this circumstance, as well as the magnitude of the astigmatism, proved that it was not produced by a tilt of the mirror.

Lord Rosse immediately suggested a probable cause of this defect. The triangular levers which support the mirror are all, to a certain degree, elastic. When the telescope is dropped from the vertical position, the edge of the mirror begins to press the fixed pillars in the fixed frame mentioned under No. II.; and as the edgewise pressure increases faster than the excess of the elastic force of the levers over the pressure on the levers, the edge is firmly locked to the pillars by the friction against them. When the telescope is much depressed, this friction is perhaps not much less than two tons, or is equal to the greatest strain of six horses, all exerting a force perpendicular to the face of the mirror at a part intended to sustain no such force, and therefore tending to bend the mirror out of shape. The obvious remedy was to suspend the edge of the mirror in such a manner as to leave it free to play in a direction perpendicular to the face of the mirror; and for this purpose, first an iron hoop, and secondly a chain, were used, the bearing against the pillars being entirely destroyed. The effect was at first very satisfactory; definition was made very good; Saturn's ring was well seen on September 2 as a narrow line. But in subsequent observations the effect was not so satisfactory. Here, again, Lord Rosse discovered the cause. The triangular levers which immediately support the mirror are necessarily not plane, inasmuch as the points which take hold of the plates that adhere to the mirror must project higher (the telescope being vertical) than the fulcra of these levers. If, then, when the telescope is lowered, the mirror is allowed to slip edgewise, it throws the lower points of each lever partly out of bearing, and then the mirror is not supported in the way to which its figure is adapted. It was found that some of the points were not in bearing; and it was also found that, when the telescope was directed to a star, the image was rendered extremely good by screwing or unscrewing to the proper degree the supports of the hoop or chain. It was found, moreover, that all this adjustment was affected by the bending of the iron base of the fixed frame. On the whole, this part of the mounting is not in a satisfactory state. The Astronomer Royal was not able to say what is the nature of the edge-bearing adopted by Lord Rosse at the present time; still it is evident that, with due attention, the mounting above-mentioned may be made perfectly available. [The Astronomer Royal has lately been informed, that Lord Rosse has entirely overcome these difficulties, by placing between the back of the mirror and the plates in which the points of the triangular levers act, sheets of tin, which allow the mirror to slip upon the plates, instead of the felt and pitch which formerly united the mirror and the plates.] It may be proper here to mention, that Lord Rosse has informed the Astronomer Royal that the mirror which he saw under the polisher has been mounted, and that it shows very well the third star of γ Andromedæ,—no small proof both of the perfection of the figure and of the efficiency of the support of the mirror in that position.

The Astronomer Royal then explained his own ideas upon the nature of the mounting, to which (whatever might be the practical difficulties in the mere mechanical operation) he thought it would be necessary to approach. He thought that it was absolutely necessary to give the edge-support by counterpoises; and this might be done, retaining the present levers, by making the point of each of the small triangles a socket for a ball-and-socket joint, in which turns a lever whose point lodges in the mirror. The extreme hardness of the speculum metal makes it, however, difficult to drill the holes.

In terminating now the account of the mirror, the Astronomer Royal alluded to the impression made by the enormous light of the telescope; partly by the modifications produced in the appearances of nebulæ figured by Sir John Herschel, partly by the great number of stars seen even at a distance from the Milky Way, and partly from the prodigious brilliancy of Saturn (the only planet which he had an opportunity of seeing). The account given by another astronomer of the appearance of Jupiter was, that it resembled a coach-lamp in the telescope; and this well expresses the blaze of light which is seen in this instrument.

The Astronomer Royal then stated that he had had no opportunity of trying Mr. Lassell's telescope; but he had understood from

Mr. Lassell that some difficulties had been found in the arrangement of the edge-bearing of the mirror, which had been overcome by suspending it in an iron hoop. These, however, from the great difference of dimensions, would probably be trifling compared with those of Lord Rosse's mirrors.

Adverting again to the mounting of these telescopes, the Astronomer Royal suggested for consideration whether it might not be advantageous to mount the telescope with an altitude and azimuth movement, by an overhanging fork inserted in a vertical pillar. If a rod were joined to the stalk of this fork, and, by means of an ordinary parallel motion, were compelled to move parallel to the axis of the telescope, and if any part of this rod were connected by another rod (adjustable in length according to the polar distance of the object) to a universal joint on the ground, in such a position that the line drawn from that universal joint to the stalk of the fork is a polar axis, then the motion of the telescope would be equatoreal. For the observer, it was proposed that an observing-box should be fixed to the telescope, and that the access should be by a spiral staircase round the pillar, by a narrow platform near its top, and by a staircase along the side of the tube.

In conclusion, the Astronomer Royal observed that it was impossible to overrate the advance that had been made in the construction of telescopes by the two amateur constructors of whom he had spoken. Lord Rosse had shown that it was possible, without any important manual labour, to produce with certainty, by means of machinery, mirrors of a size never before attained, and perhaps with a perfectness of definition which had not been reached before; and he had, by publication and by private communication, made these methods accessible to the world. This success was the more remarkable, because the whole of the work was done by workmen found on the spot; even the steam-engine, by which (to a late time) the whole of the machinery was driven, was made by native workmen under Lord Rosse's personal instructions. To Mr. Lassell also much was due, for the example which he had set of what may be done by a man possessing less ample means, and whose time is fully occupied in business; and much also for the elegant and compendious and manageable apparatus arranged by him, which promises to be of the greatest use in the construction of large object-glasses as well as of mirrors.

LXXIX. *Intelligence and Miscellaneous Articles.*

DEFLECTION OF THE MAGNETIC NEEDLE BY THE ACT OF VOLITION*.

THIS curious and interesting experiment is due to the investigations of M. Du Bois Reymond of Berlin, and his method of performing it is as follows:—He takes a very sensitive galvanometer, and attaches to the terminal wires thereof two perfectly homogeneous strips of

* We are indebted to the kindness of W. G. Lettsom, Esq., for the communication of this notice, by whom we are also informed that the experiment has been repeated with success.

platina. These strips are dipped down into two vessels filled with salt and water, into which fluid, as contained in each vessel, two corresponding fingers of the two hands are to be plunged. On the first immersion of the fingers there is almost always observable a more or less decided deflection of the needle, this deflection not being amenable to any known law, and being in the opinion of the experimenter due to the difference existing to some extent and in some way or another between the cutaneous covering of the two fingers. Whenever there is a wound on one of the fingers the deflection is greater than usual; and its direction is uniformly such that the injured finger behaves like the zinc-side of an arc of zinc and copper, which we may conceive to be inserted between the two vessels instead of the human body. It need hardly be remarked, that it is not this sort of action to which in the experiment in question it is purposed to direct the attention. On the contrary, in order to observe the effects alluded to, it is requisite to wait either till the needle has gone back to the zero-point of its scale, or at least until it has assumed a constant deflection attributable to the residue of a current which it is beyond us to eliminate. As soon as this state is attained, the whole of the muscles of one of the arms must be so braced that an equilibrium may be established between the flexors and the extensors of all the articulations of the limb, pretty much as in a gymnastic school is usually done when one wants to let a person feel the development of one's muscles.

As soon as this is done the needle is thrown into movement, its deflection being uniformly in such a sense as to indicate in the braced arm "an inverse current," according to Nobili's nomenclature; that is to say, a current passing from the hand to the shoulder. The braced arm then acts the part of the copper in the compound arc of zinc and copper mentioned above.

With his own galvanometer, and when M. Du Bois Reymond himself performs the experiment, the deflection amounts to 30°. He obtains however movements in the needle of far greater extent by contracting alternately the muscles, first of one arm and then of the other, in time with the oscillations of the needle. On bracing simultaneously the muscles of both arms, inconsiderable deviations are observable, sometimes in one direction, sometimes in another; and these minute deflections are evidently attributable to the difference between the contractile force of the two limbs. Hence it arises that when the experiment is repeated many times in succession, the results diminish gradually in amount, not only in consequence of the energy of the contractions becoming less and less, but also because it becomes more and more difficult to restrain the act of slackening or letting down the muscles to only one of the two arms.

The amount of deviation, *cæteris paribus*, depends upon the amount of the development and the exercise of the muscles. The author is said to have an arm of considerable power, and among the number of savans that have tried the experiment at his residence, there has not as yet been found one who excelled or even came up to him in this respect. There are indeed individuals who do not possess the power of producing a sensible deflection in the needle of his galvanometer, but it is readily ascertained that in these instances there is a want of sufficient muscular tension.

There is one remark, to conclude, which the author has been frequently led to make, namely, that the habitual superiority of the right hand over the left in this experiment is to be interpreted by the preponderance of the amount of deflection produced by the tension of the right arm. This peculiarity was likewise observed when the experiment was performed by M. von Humboldt. The impulsion impressed on the needle by the contraction of the muscles of his right arm was appreciably more considerable than that produced by his left arm.

For his own part, M. von Humboldt has addressed to M. Arago a letter of the following tenor:—He says, the fact of the experiment of affecting a magnetic needle by the alternate tension of the muscles of the two arms, an effect due to volition, is established beyond all question or doubt. Notwithstanding my advanced years and the little strength that I have in my arms, the deflections of the needle were very considerable; but they were naturally more so when the experiment was performed by M. J. Müller or by M. Helmkoltz, who are younger men. To facilitate the experiment it is advisable to plunge the fore-fingers into the water, and to support the palms of the hands, to enable one to brace up well the muscles of the arm which it is purposed to bring into play.

ON THE ARTIFICIAL FORMATION OF MINERALS IN THE HUMID WAY. BY M. DE SENARMONT.

Many mineral species very nearly approach some compounds obtained by the usual processes of chemistry, and even supply the deficiencies, which are still left in certain natural series. Such for example are the carbonates of magnesia, of protoxide of iron, manganese, nickel, cobalt and zinc, which are placed near rhombic carbonate of lime, and are met with in nature in a state of purity, or of isomorphic union, yielding hybrid species, forming connecting links between pure species.

These natural compounds, which have not been hitherto formed artificially, could not evidently have arisen under the usual conditions of laboratory experiments; for we have no reason for supposing that the same causes could produce different effects at different epochs. It is therefore a subject of great interest to determine with precision the circumstances necessary to the production of all minerals; and the solution of this question would undoubtedly be the best method of raising the veil behind which are hidden the phænomena which attended the formation of a great number of rocks and of a part of the terrestrial globe.

Some happy synthetical attempts have already afforded valuable data on this head. MM. Mitscherlich and Berthier have obtained several fusible minerals in the dry way, and M. Ebelmen has made an additional step in his researches on the formation of infusible silicates and aluminates. M. G. Rose has ably examined the conditions under which carbonate of lime is precipitated in the state of aragonite; lastly, the beautiful experiment of M. Haidinger has thrown great light on the controverted question of the formation of dolomites and the general problem of metamorphism.

The author conceived the idea of forming certain mineral species in the humid way, and his first essays were made with the carbonates. As it is clearly demonstrated that heat generally favours dehydration, it occurred to the author that the formation of neutral carbonates might be a simple question of pressure and temperature; but before attempting to precipitate them by the disengagement of the excess of carbonic acid serving them as a solvent at a high temperature under strong pressure, the author attempted double decompositions in the humid way. These first attempts set out therefore from the beautiful experiments of M. Haidinger.

The substances were exposed to each other in glass tubes, hermetically sealed, after having exhausted them. If they were of a kind to react upon each other immediately, they were at first separated, and were then by turning moved at the proper time. For high temperatures the tubes were inclosed in a gun-barrel, hermetically sealed and half-filled with water, so as to equalize as much as possible the internal and external pressures on the glass tube. The tubes were heated in small closed chambers in the furnaces of a gas-work.

The following mineral species were by these means produced:—

Carbonate of Magnesia.—By the double decomposition of sulphate of magnesia and carbonate of soda, at about 322° Fahr. It was in the state of white crystalline sand, which was scarcely acted upon by weak acids. Having understood, during the course of his experiments, that M. Maurignac had performed analogous experiments on the reaction of chloride of magnesium and carbonate of lime, hereafter to be described, the author did not proceed further.

Carbonate of Protoxide of Iron.—Obtained by double decomposition: first, of sulphate of protoxide of iron and carbonate of soda, at about 302° F. and above; second, from protochloride of iron and carbonate of lime, at temperatures between 266° F. and 392° F., kept up during twelve, twenty-four, and thirty-six hours. The carbonate of iron was in the state of crystalline sand, of greater or less fineness, of a grayish-black colour, nearly unalterable in dry air, very slowly acquiring a flaxen colour in moist air, and scarcely acted upon by diluted acids. This crystalline sand has then all the properties of spathose iron ore. Its gray colour is more or less dark, and its spontaneous alterability is the smaller according as it is formed at high and long-continued temperatures. Perhaps to circumstances of this kind may be attributed the differences which exist in this respect among natural spathose iron ores; differences which are not sufficiently explained by variation in their composition.

Carbonate of Manganese.—Obtained by double decomposition: first, of chloride of manganese and carbonate of soda, at about 320° F.; second, chloride of manganese and carbonate of lime, at temperatures between 284° F. and 338° F., kept up twelve and forty-eight hours. It is in the state of a fine white powder, slightly rose-tinted, no crystalline appearance, and is unalterable at a moderate heat.

Carbonate of Zinc.—Obtained under the same conditions as that of iron; it is a fine white powder, not crystalline, unalterable at a moderate heat.—*Comptes Rendus*, Juin 4, 1849.

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END OF THE THIRTY-FOURTH VOLUME.

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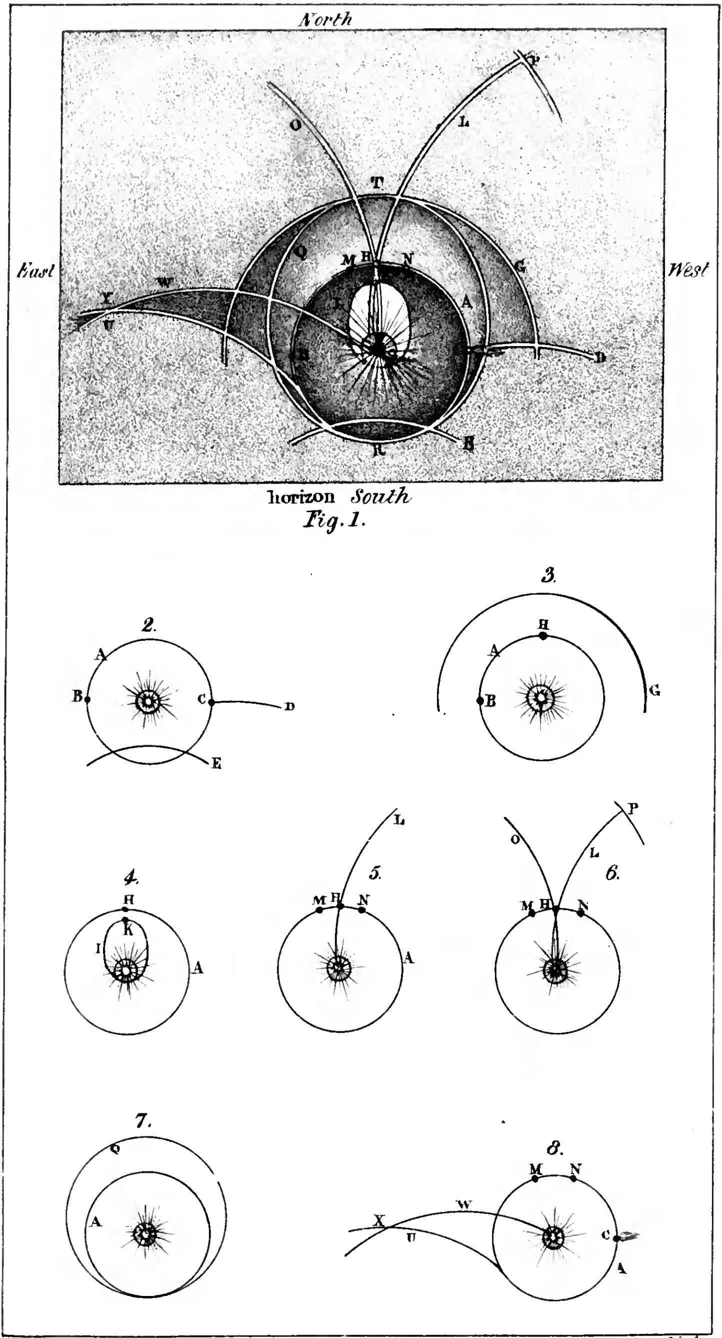

Fig. 1.

J. Basire lith.