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

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

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

J. B. Pinelli ad Mazonium.

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[FOURTH SERIES.]

JANUARY 1852.

I. *Observations in the Alps on the Optical Phænomena of the Atmosphere. By* Dr. Hermann Schlagintweit*.

[With a Plate.]

Deportment of the atmosphere towards heat and light. Transmission of heat. *Pyrheliometer. Difference of the thermometer in the sun and in the shade. Thermometer with blackened bulb.* Saussure's *heliothermometer. Nightly radiation.* Transparency. *Absorption of light in general. Diaphanometer. Transparency of the atmosphere in large masses. Optical illusions through altered transparency.*

Among the optical phænomena of the atmosphere, those which relate to its colour and its deportment towards the rays of light and heat are peculiarly subject to alteration with the height. The investigation of this subject at great elevations, as, for instance, in the higher alpine regions, obtains an interest from the fact, that here, in regard to its weight, a considerable portion of the atmosphere is absent†.

The experiments on the intensity of heat and light are, however, subject to so many accidents and disturbances, that in the following pages we must sometimes limit ourselves to the results of single experiments.

Transmission of Heat.

To convince ourselves of the high permeability of the atmosphere with regard to the rays of heat, we made use of one of

* Extracted from the Researches on the Physical Geography of the Alps, by Hermann Schlagintweit and Adolph Schlagintweit. Leipzig. T. A. Barth, 1850.

† At a height of 12,000 feet the loss, in this respect, amounts to 0·37; at 14,500 feet, to 0·44 of the entire weight.

those ingenious instruments which have been devised by Pouillet*. The construction of the direct Pyrheliometer (Plate I. fig. 1) is as follows:—The vessel (*aa*) is a shallow metallic cylinder, with a blackened surface (*b*). The diameter of the cylinder is 1 decimetre, and its height 1·5 centimetre; it can therefore contain 150 grammes of water. To the bottom of the vessel a cylinder is appended, within which is a thermometer held fast by a cork; on the outside of this cylinder is a screw (*c*), by means of which the instrument may be fastened to a vertical stick. [In this way the stand recommended by Pouillet was superseded.] The double motion of the screw in horizontal and vertical direction enabled us to place the instrument so that the sun's rays always fell perpendicularly upon the blackened upper surface. Of this we convinced ourselves by fixing a pasteboard disc (*e*) of the same diameter as the cylindrical vessel (*aa*), near the end of the thermometer. When the entire disc was shaded by the vessel at the other end, we might be sure that the rays fell perpendicularly upon the latter; and by this arrangement the thermometer-tube is protected from the direct action of the sun. The bulb of the thermometer is contained within the cylinder (*aa*), that is to say, it is directed upwards. Were the air completely removed from the thermometer, on placing it in this position the mercury would flow downwards, and thus render the reading of the instrument impossible; but a small quantity of air, left intentionally in the tube during its preparation, hindered this descent of the mercury, without however invading the exactitude of the results. To impart a uniform temperature to the entire mass of water, the pyrheliometer, during the experiment, is turned round its longer axis; to permit of this, the screw (*c*) must not be made too fast. In the filling of the instrument great care must be taken that no air remains in the cylinder, as this would spread itself between the water and the upper metallic plate, and thus modify the quantity of heat received by the water.

During its exposure to the action of the sun, the instrument loses a quantity of heat by contemporaneous radiation from its surface. This source of disturbance cannot indeed be avoided, but its magnitude may be determined, by observation, for each experiment. The procedure is as follows:—The vessel is first filled with water, to which time is given to assume the temperature of the surrounding air. The instrument is then brought into the vicinity of the spot where it is intended to be exposed to the action of the sun, and so placed in the shade that its

* Mémoire sur la chaleur solaire, sur les pouvoirs rayonnants et absorbants de l'air atmosphérique, et sur la température de l'espace.—*Comptes Rendus*, 1838, vol. vii. p. 24–65. Compare also Herschel's actinometer, in Kæmtz's Treatise on Mineralogy, vol. iii. p. 14.

blackened surface may radiate against an unclouded portion of the sky. After somewhat more than four minutes, the surface is covered with a screen and directed perpendicularly against the sun's rays. At the end of the fifth minute the screen is removed, and the apparatus is permitted to remain five minutes longer in the sun. At the end of the tenth minute it is again, as at first, brought into the shade and permitted to radiate five minutes longer. This operation can be often repeated if necessary. The two quantities which are here to be made use of are, the increase of temperature during the five minutes' exposure to the sun, and the decrease of temperature during the periods of radiation immediately before and after; the arithmetical mean of the two latter may be regarded as the quantity of heat lost by radiation during the five minutes in the sun. The action of the sun (t) alone will therefore be

$$t = \mathrm{T} + \frac{r + r'}{2},$$

where T represents the observed temperature, r and r' the amounts of the radiation before and after.

After the second radiation we generally placed the instrument once more in the sun, and afterwards permitted it again to radiate; in this way two series of observations were obtained.

To secure perfect accuracy in the experiments, it would be desirable that the temperature of the air during the time occupied by each should remain constant; for any inconstancy in this respect would be accompanied by an increased or diminished cooling. But in experiments which occupy fifteen minutes and upwards, trifling alterations are unavoidable; by taking the arithmetic mean, however, they are sufficiently compensated.

Of the experiments with the pyrheliometer, the maxima alone are chosen for nearer consideration; for in these cases only is it probable that no turbidity of the atmosphere by vapours, fine veils of clouds, &c. takes place. In making such experiments, deviations in the transparency are often recognised which are totally inappreciable with the telescope or with the naked eyes, but which afterwards announce themselves by the presence of thin clouds, &c.

In the observations on the Johannishütte (7581 P. F.), the maxima of the increase* in August and September 1848 were 4·9 to 5°·2 C.; the mean height of the barometer at the time of the experiment being =571 millims. All the observations were

* Under 'increase' is here to be understood the increase of temperature with the radiated heat $\left(\frac{r+r'}{2}\right)$ added to it.

made at midday, between 12 and 1 o'clock. The 4th of September 1848, on which the observations on the Rachern (10362 P. F.) were made, seems to have been a peculiarly favourable day. It was not only entirely cloudless, but its transparency, when the distant alps were observed, was very striking; on this day the difference between the sunned and shaded thermometer was unusually great.

The observations on the Rachern gave as follows:—

Time of observation, Sept. 4, 1848, 1^h 10^m P.M.*	
Height in Parisian feet	10362
Reduced height of barometer in millimetres . .	512
Temperature of the air	5°·8 C.
Temperature of the water at the commencement .	8°·3 C.
Temperature after five minutes' radiation . . .	6°·4 C.
Temperature after five minutes' exposure to the sun	10°·1 C.
Temperature after five minutes' further radiation	7°·9 C.
Increase for five minutes without radiation . .	5°·75

The maximum of the increase obtained by Pouillet†, after the elimination of the radiation, was 5°·1 C. (Paris, May 11, 1838, 12^h).

The insolation was also determined by observations made with two thermometers; one of which was set in the sun, and the other, in the usual manner, in the shade. Lambert‡, Alexander von Humboldt§, De Gasparin‖, and Quetelet¶, have pointed out the import of such determinations, and the two latter have applied the method in a long series of experiments.

We here communicate a number of insolations which we have had opportunity to observe. They are the results of single experiments; and perhaps, even as separate phænomena, on account of the considerable elevation to which they refer, are deserving of some attention.

* The insolation at 12 o'clock might be taken at something over 5·8.

† For the limit of the atmosphere, the observations of Pouillet made at different hours of the day gave 6°·27 C. If we omit to compare our observations with those of Pouillet, and the similar ones made by Kæmtz and Forbes with Herschel's actinometer, our apology is, that for these investigations numerous observations are necessary, as the single observations are so liable to disturbances from the altered serenity of the sky.

‡ Lambert, *Pyrometrie*, § 283; *Photometrie*, § 886.

§ Alexander von Humboldt, *De Distributione Plantarum*, 1813, p. 167.

‖ De Gasparin, *Cours d'Agriculture*.

¶ Quetelet, *Instruction sur l'observation des phénomènes périodiques*, and *Sur le Climat de la Belgique*, chap. 4. 1846. From the *Annales de l'Observatoire de Bruxelles*.

A. *Insolations on the summits of the Alps* (1848).

No.	Location.	Height in Parisian feet.	Day and hour.	Thermometer. In the shade.	Thermometer. In the sun.	Difference.
1	Grossglockner	12,158	Aug. 29. 1^h p.m.	$3\overset{\circ}{\cdot}8$	$4\overset{\circ}{\cdot}9$	1·7
2	Adlersruhe	10,432	Aug. 29. 2^h p.m.	10·2	15·9	5·9
3	Adlersruhe	10,432	Aug. 29. 3^h p.m.	9·1	11·5	2·4
4	Rachern	10,362	Sept 4. 1^h p.m.	5·8	6·6	0·8
5	Rachern	10,362	Sept. 4. 2^h p.m.	4·2	5·9	1·7
6	Rachern	10,362	Sept. 4. 3^h p.m.	3·9	4·5	0·6
7	Todtenlöcher	10,340	Sept. 1. 1^h p.m.	4·4	6·9	2·5
8	Névé region of the Pasterze above the Burgstall rocks...	8,781	Sept. 1. 9^h a.m.	3·8	5·6	1·8
9	Pasterze, on the foot of the great Burgstall	8,236	Sept. 3. 2^h p.m.	5·4	9·5	4·1
10	Wallnerhütte	6,510	Aug. 28. 2^h p.m.	14·5	18·6	4·1
11	Gössnitz	5,796	Aug. 22. 11^h a.m.	17·4	23·5	6·1
12	Georgenstein	4,697	Aug. 20. 9^h a.m.	18·1	19·4	1·3

B. *Insolations on Johannishütte* (7581 P. F.).

Hour.	Shaded.	In the sun.	Increase in the sun.	Hour.	Shaded.	In the sun.	Increase in the sun.
h 7 a.m.	−1·0	+1·5	2·5	h 1 p.m.	10·4	12·1	1·7
	+6·4	8·5	2·1		9·1	15·2	6·1
8	9·5	12·2	2·7	2	5·4	9·5	4·1
	4·6	9·4	4·8		12·0	12·4	0·4
	8·7	11·5	2·8				
9	5·8	7·9	2·1	3	11·0	14·0	3·0
	6·1	9·1	3·0		8·9	14·1	5·2
10	9·3	13·0	3·7	4	11·4	12·2	0·8
	0·7	7·1	6·4				
	11·1	12·8	1·7				
11	0·5	4·5	4·0	5	7·4	8·9	1·5
	8·1	10·7	2·6				
	11·3	13·3	2·0				
Noon.	11·8	15·1	3·3				
	12·1	16·2	4·1				

C. *Insolations observed contemporaneously on Johannishütte and on the Glacier* (*horizontal distance from the edge* 900 P. F.).

		Thermometer.		Difference.	Diff. G.—Diff. J.
		Shaded.	In the sun.		
	h	°	°		
a. 7	J.	−1·0	+1·5	2·5	0·9
	G.	−2·0	+1·4	3·4	
b. 8	J.	9·5	12·2	2·7	2·3
	G.	4·6	9·6	5·0	
c. 10	J.	9·3	13·0	3·7	0
	G.	4·1	7·8	3·7	
d. 11	J.	2·7	9·1	6·4	0·3
	G.	0·7	7·4	6·7	
e. 12	J.	11·8	15·1	3·3	3·8
	G.	4·3	11·4	7·1	
				Mean	1·46

The differences can in general be very considerable; even on the higher alpine summits (Table A.), notwithstanding the low temperature of the air, they are always very appreciable. A difference of 2°·5 is frequently observed even there, and on very favourable days it amounts even to 6° C. The experiments upon the Johannishütte show that the insolations increase towards midday, but many disturbances are at the same time observed during the process of experiment. We therefore limit ourselves to the results of the hours, with the remark, that the observations were made on days widely separated from each other. The Table C. includes observations, every two of which contained between the braces are correspondent; the upper one was made on the Johannishütte, the other at the same time upon the glacier. The temperatures of the shadowed atmosphere are very different at both points; the height of the thermometer in the sun is, however, in both cases very nearly the same. A disturbance due to the reflexion of light from the surface of the glacier might in the present case be expected; but this was prevented by the introduction of a pasteboard screen beneath. The difference of the sunned and shaded thermometers is here always greater than that observed over the rocks which surround the glacier; at noon the difference in the former case exceeds that in the latter by 1°·46 C. This deviation is due to the circumstance of our having in one case an atmosphere artificially cooled by the peculiar surface upon which it rests. Both thermometers indeed are hereby depressed; the difference, however, must be greater on the glacier, as the intensity of the solar rays, in the observations made contemporaneously at both places, is the same, and is therefore more appreciable in the relatively cold atmosphere.

True, the radiation in the latter case is likewise more energetic, but not sufficiently so to annul the comparatively great increase of temperature observed upon the glacier.

The reverse of this must take place when the observations are made immediately over the surface of the bare rocks, where the temperature of the air is known to be considerably heightened. In this case the thermometer placed in the sun could not differ so considerably from that in the shade. This suggests to us the great caution necessary to be observed in experiments of this nature.

We have also made several observations with a thermometer with a blackened bulb, similar to the photometer of Leslie*. By the pyrheliometer we obtained the increase of the temperature of a quantity of water during an arbitrary unit of time; while instruments of the present description exhibit how far the respective thermometers, in the sun and in the shade, can diverge from each other, and thus furnish results which, in comparison with the former, may be named absolute or maxima differences. In our experiments we used two thermometers, one of which was placed in the shade, and the other, the bulb of which was blackened, exposed to the direct sunlight. Instruments of this description can only be compared with each other†. For the sake of brevity we will name the thermometer with the blackened bulb, and which stood in the sun, the *photometer*.

Observations with the blackened Thermometer.

No.	Place of observation.	Height.	Day.	Hour.	Temperature of the air in the shade.	Photometer.
		feet.		h	°	
1	Grossglockner, second peak	12,158	Aug. 29.	1 p.m.	3·2	16·1
2‡	Similaun (summit)	11,135	Sept. 13.*	$12\frac{3}{4}$ p.m.	0·8	13·7
3	Adlersruhe	10,432	Aug. 29.	2 p.m.	10	34·0
4	Rachern	10,362	Sept. 4.	1 p.m.	5·8	17·3
5	Todtenlöcher	10,340	Sept. 1.	12	4·4	19·2
6	Névé of the great Petzthal-glacier §	8,500	Sept. 11.*	1 p.m.	2·4	25·0
7		......	Sept. 11.*	2 p.m.	2·4	23·8
8		......	Sept. 11.*	3 p.m.	2·2	13·6
9	Névé of the Niederjoch	......	Sept. 13.*	3 p.m.	2·4	15·7
10	Gössnitzthal	......	Aug. 21.	12	17·4	33·1

* John Leslie, Short notice of Experiments and Instruments which relate to the Deportment of the Air towards Heat and Moisture. Leipzic, 1828. 8vo.

† The instrument of Leslie is a differential thermometer with a blackened bulb. It is placed under a glass shade and set in the sun; notwithstanding this, however, the results given by different instruments cannot be well compared with each other. Compare Ritchie, Edinburgh Journal, Sc. iii. p. 106.

‡ The observations marked thus * are from 1847.

§ Nos. 6 to 9 inclusive, immediately over the surface of the granular snow (névé).

The days chosen were all very clear. The least shading of the heavens by clouds is, however, capable of causing such considerable changes in the maxima of insolation, that a relation between the latter and the height is not to be given with certainty. The temperatures given by the photometer are, however, in the first place, deserving of some attention, because the dark rocks and the most elevated accumulations of earthy matters exhibit similar temperatures very often. They attain sometimes, even at considerable elevations, a temperature from 20 to 30 degrees, and again sink under zero by the radiation at night.

Experiments on temperature in the direct sunshine which are made with the heliothermometer of Saussure*, are comparable with each other only so long as the same instrument is used. The thickness and transparency of the glass, the space of the cylinder, the conductive power of the material, the more or less air-tight closing, &c., are generally very different in different instruments. It will therefore be sufficient, in the present case, to exhibit briefly those experiments which we made on the Johannishütte (7581 P. F.). We placed the instrument generally from 10 o'clock in the morning to 4 o'clock in the afternoon in the sunshine; after this hour there was no further increase of temperature observable.

We obtained—

I.	40° C.	with	6°·7 C.	temperature of air in the shade†.
II.	35	...	5°·1 ...	
III.	48	...	7°·9 ...	
IV.	31	...	5°·2 ...	
V.	49	...	5°·8 ...	

The radiation also varies with the height; it becomes more energetic as we ascend. The experiments on radiation were made with a Rumford's minimum, which was placed upon a layer of down and left uncovered. The down served to prevent any lateral conduction of heat to the instrument. This is Pouillet's arrangement‡. The observations were made upon Johannishütte in Heiligenblut. The minimum temperature of the night air was observed at the same time as the radiating instrument. (Column 4.)

* The instrument consists of a wooden box, which is blackened inside, and in which the bulb of the thermometer is placed. It is closed above by three glass plates. Fourier has given the experiments made by Saussure with this instrument in his investigations on obscure heat. *Mém. de l'Acad. des Sciences*, Paris, vol. vii. p. 585.

† Saussure saw his instrument upon Mount Cramont rise to 87° C., the temperature of the air at the time being 6°·2 C.—*Voyages*, § 932.

‡ *Comptes Rendus*, vol. vii. 1838, p. 56.

Collection of Night Temperatures with and without Radiation.
7581 P. F. 1848.

No.	Month.	Night.	Minimum.	Radiation.	M−R.	Remarks.
1	August.	15—16	+2·0	− 5·1	7·1	Very clear,
2	......	26—27	−4·1	− 4·3	0·2	Fog in the morning.
3	......	30—31	+5·0	− 2·9	7·9	Very clear.
4	Sept.	2—3	2·5	− 3·5	6·0	Light cirri in the morning.
5	......	3—4	−3·1	−10·1	7·0	Very clear.
6	......	4—5	+1·2	− 3·0	4·2	Fog in the morning.

The last column but one contains M−R, that is, the temperature of the air on Johannishütte minus the results of the radiation thermometer. The contemporaneous maximum of radiation in Heiligenblut, at a height of 4004′, amounted to 5°·2 C. In the peculiarly clear nights, Nos. 3, 1 and 5, the radiation was nearly constant, varying only from 7·9 to 7·0. As the minimum of the air varied from +5·0 to −3·1, we may conclude that the differences in the temperature of the air have no influence upon the radiation. Similar results have long served to support the assumption of the intense cold of the planetary spaces, in comparison with which the differences of temperature observed on the earth's surface almost vanish. It is very difficult to fix upon proper nights for such observations, the latter are so often disturbed by light clouds or by the morning fog. At still greater altitudes, the temperature of the air and the results given by the radiation thermometer differ from each other still more. Martins and Bravais, during their ascent of Mont Blanc, found the following differences on the Grand Plateau :—

	Difference on the Grand Plateau.	Difference in Chamouni.
Aug. 28, 29, 1844.	13·4*	5·7
Aug. 31—Sept. 1, 1844.	13·5	6·1

These observations also were made with a thermometer placed upon down.

Different substances exposed at night exhibit different powers of radiation. The temperature which they assume depends, in a great degree, as well upon their constitution as upon their form. This difference exhibits itself very evidently in nature, particularly in the case of plants. In connexion with this subject we give the following extract from the exceedingly careful investigation of Glaisher†, which refers to various and very cha-

* *Monit. Univers.*, 1844, p. 2796.

† "On the Amount of Radiation of Heat at night from the Earth, and

racteristic localities. The difference between the temperature of the air and a thermometer placed in long grass is assumed to be 1000; the difference between the temperature of the air and the radiation thermometer in the remaining positions is expressed in parts of the above number. The thermometer used to determine the temperature of the air was placed four feet above the ground.

Relative Powers of Radiation of different Bodies.*

Long grass	1000	2 feet above the points of the grass	86
Short grass	870	4 feet above the points of the grass	69
An inch above the ground. Covered with grass	209	6 feet above the points of the grass	52
On the ground under long grass	66	8 feet above the points of the grass	17
On the ground under short grass	200	12 feet above the points of the grass	14
1 inch † above the points of the grass	671	Garden earth	472
2 inches above the points of the grass	570	Gravel	288
3 inches above the points of the grass	477	River sand	454
6 inches above the points of the grass	282	On stone	390
1 foot above the points of the grass	129	White sheep's wool	821
		In the focus of a parabolic metallic mirror	858

Transparency.

The absorption of the rays of light by the atmosphere varies also with the rarification of the latter. For a luminous point in the zenith seen from the earth's surface, the absorption amounts to 0·80 of the brightness which would reach us did no absorption by the atmosphere take place‡. Many investigations on the brightness of the stars can be regarded as excellent determinations for the transparency of the atmosphere§; in general,

from various bodies placed on or near the surface of the Earth." By James Glaisher, Esq., of the Royal Observatory, Greenwich. Phil. Trans. Lond. 1847, part 2, pp. 119, 217.

* Glaisher in the place cited, tab. 45, p. 147.

† Inches and feet are here English measure.

‡ This number is the mean of the results of Bouguer and Seidel: "Erste Resultate Photometrischer Messungen am Sternhimmel."—*Müncher Gel. Anzeig*, July 2, 1846, No. 131, p. 18. The two measurements were made by quite different methods; their close approximation therefore renders them the more deserving of confidence. Seidel found 0·78, Bouguer 0·81.

§ Compare Humboldt's astrometer and his experiments therewith.—*Voy.* ed. 8vo, vol. iv. p. 32 and 287. Steinheil's measurements of brightness.—*Denkschriften der Müncher Acad.*, 1837, p. 1–142. Herschel in Schumacher's *Astr. Nachr.*, vol. xvi. No. 372, p. 190. Seidel, as cited above. Compare also the earlier experiments of Bouguer, *Optici de diversis luminis gradibus dimetiendis* 4. Viennæ, 1762; and Lambert's Photometria. An elaborate collection of the proper instruments and methods of observation in Herschel on Light; II. Photometry, § 17–87.

however, such investigations are made more with a view to determine the relative brightness of the stars, compared with each other, than to ascertain the alterations in the intensity of their light at various zenith distances. For the more direct investigation of the transparency of the atmosphere, Saussure's diaphanometer seemed to us to be peculiarly suitable*. This consists of two white circles, in the middle of which are placed black discs of various diameters (fig. 2). The greater circle (*aa*) has a diameter of 2 P. F.; the black disc within it (*bb*), a diameter of 1 foot; the entire circle is surrounded by a green rim (*c*). In one corner of the latter a smaller circle is similarly placed; its diameter is 2 inches, and the diameter of the black disc within it 1 inch. Saussure's instrument was double the size of ours. We chose these smaller dimensions, because we feared that on the higher summits it would be difficult to obtain the distances necessary to cause the disappearance of the larger discs; more difficult still would have been the accurate measurement of such distances at these elevations. We sometimes encountered, even with our smaller instruments, difficulties of this nature.

In the application, the experimenter recedes from the discs thus placed together, until the black centre of the smaller one can no longer be distinguished from the surrounding surface. The same process is repeated with the large disc. If the air were perfectly transparent, the angles under which both discs disappear would be equal; and as the angles in this case are very small, it may be assumed that the distances from the centre and from the borders of the discs are the same. The respective distances are then in the ratio of the diameters of the two discs. The diaphanometer of Saussure is indeed no absolute standard for the transparency of the atmosphere; the eye of the observer, the intensity of the colour of the instrument, and the manner in which it is set up, all have an influence upon the results. By using proper caution, however, the error may be greatly lessened; and the differences at various elevations are so considerable, that its influence on this account is still further diminished. As the observer's eye seems to be the most variable among these subjective sources of error, a few physiological remarks on this subject may assist in the forming of a judgement as to the following experiments†. When we recede from the discs until the black circle vanishes, the angle under which the instrument is then seen is so small, that the contiguous portions of the retina are impressed at the same time by the black and white portions of the surface; the impressions thus unite to form a dull gray

* *Mém. de Turin*, vol. iv. 1788 and 1789, p. 425–440.

† Compare Volkmann's beautiful memoir *Ueber das Sehen* in Rudolph Wagner's Physiological Dictionary, Lit. S. p. 263–351.

image, similar to that formed by the intimate mixture of a white and a black powder. The distance at which this occurs is, as might be expected, different for different eyes; the possible error in judging of the transparency of the atmosphere is, however, greatly lessened by the circumstance, that only the ratio of the distances from both discs is to be taken into account, and the short-sighted eye will observe the disappearance of the small black circle also at a much smaller distance. While considering the angle under which the circles disappear, it may not be uninteresting to introduce the results obtained by the labours of Hueck* upon this subject. They may be stated as follows:—

1. A normal eye, which can accommodate itself to all distances, observes the disappearance of small objects, whether they be near or distant, under the same angle of vision.

2. With larger objects the angle necessary to their recognition increases a little. [The absence of perfect transparency seems to have made itself appreciable here.]

3. A stroke is seen further than a spot whose diameter is equal to the breadth of the stroke.

4. White objects on a black ground are better seen than when the arrangement is reversed.

5. The smallest angle of vision under which black spots on a white ground were visible amounted to $2' \, 6''$; whereas for white strokes on dark ground it was $1' \, 2''$. A spider's thread was seen by Hueck under an angle of $0' \, 6''$; a shining white wire under an angle of $0' \, 2''$. In our case No. 2 and No. 3 are especially worthy of attention. White objects on a black ground appear more striking, because the impression made by a luminous surface upon the retina spreads itself laterally by irradiation†. [For this reason, slopes which are partly covered with snow appear at a distance much more uniformly covered than they really are.] Hence the necessity, if we would obtain comparable results, to use the same illumination in all our experiments. It was a matter of indifference whether we used direct sunlight or chose the shade. We preferred the latter, as we knew we should always have it in our power to shade the instrument. This enabled us also to make our own choice as to the direction in which we receded from the diaphanometer. Not unimportant for the obtaining of comparable results is the precaution, that the eye should not be wearied by continual fixation upon the disc; it is better to rest the eye from time to time upon suitably dark-coloured objects; and thus be certain that the black circle does not disappear too soon. We will remark, lastly, that the direc-

* The Motion of the Crystalline Lens, by Dr. A. Hueck. Leipzig, 1844.

† This subject is treated in a profound and elaborate manner by Plateau in the *Mémoires de l'Académie de Bruxelles*, vol. xvi.

tion in which the observer recedes from the diaphanometer must be perpendicular to the surface of the latter, as otherwise the quantity of light reflected would be very different in different experiments*.

Table of observations with Saussure's Diaphanometer, Circle *a* 1 inch, Circle A 1 foot in diameter.

No.	Place of observation.	Height in Par. F.	Distance for Circle *a*.	Distance for Circle A.	Angle for Circle *a*.	Angle for Circle A.	Q.
					′ ″	′ ″	
1	From the peak of the Gross-glockner downwards towards the Adlersruhe......	12,000	230	2750	1 15	1 15	11·957†
2	From the Adlersruhe towards the Hohenwarte	11,000	222	2640	1 17	1 18	11·892
3	Rachern towards Wasserrad-kopf, somewhat inclined...	10,300	229	2735	1 15	1 16	11·943
4	Rachern towards the Todten-löche; almost horizontal. Breadth of the smallest rocks, which were scarcely visible 10 P. F.	10,500	......	26100‡	......	1 19	
5	Johannishütte over the glacier	7,600	203	2390	1 24	1 26	11·773
6	Johannishütte, using a tele-scope which magnified four times	7,600	......	......	......	1 32	
7	Lienz. On the plane between the Drau and Isel	2,300	215	2210	1 20	1 33	10·279

The column for Circle *a* contains the distances at which the small circle disappeared; the column Circle A, the distances at which the large circle vanished. In the two following columns stand the calculated angles under which the respective circles disappeared. The last column (Q) contains the ratio of the respective distances,—a number which, as already remarked, for a perfectly transparent medium ought to be =12, but in the present case is always less. In Nos. 4 and 6, though only one distance could be measured, the angle under which the objects disappeared was of interest.

In general the quotient increases with the height, *i. e.* the

* Compare Biot, *Traité de Physique,* vol. iv. p. 776. Black marble reflects, according to Bouguer, 600 rays of every 1000 under an angle of 3° 35′; under an angle of 30°, 50 of every 1000. Under an angle of 0°·3, white marble reflects 721; under an angle of 2°·3, 614; and under an angle of 15°, 211 rays.

† Notwithstanding this great transparency, no stars were visible.

‡ This number is in reference to the rocks 10 feet broad, mentioned at No. 4.

higher we ascend, the more nearly does the atmosphere approach the state of perfect transparency. When, however, the barometer stood at 479 millims., a loss of light was still appreciable. The degree of transparency during serene, and to all appearance, perfectly clear days, is subject to variation; which perhaps depends upon general psychrometric circumstances, but more immediately upon the condensation of atmospheric moisture occasioned by the peculiarities of locality and temperature. To this may be attributed the difference between the Wasseradkopf and Adlersruhe. Water distributed throughout the atmosphere in a gaseous form increases the transparency. It is known, for instance, that the outlines of neighbouring mountains are peculiarly visible immediately before the descent of rain.

The greatest number of luminous rays are absorbed by the atmosphere in the immediate vicinity of the source from which they, either directly or by reflexion, proceed,—a law quite analogous to that which, as before observed, Melloni discovered for the rays of heat. The most evident case of this kind is obtained from a comparison of Nos. 3 and 4. An object at a distance

a of 229′	disappeared under	1′ 15″
b ... 2740′	...	1′ 16″
c ... 26100′	...	1′ 19″.

The differences of the distances increase here far more quickly than those of the angles.

For a perfectly transparent atmosphere the quotients (column 8 of the table in page 13) would be =12. Calling this 1000, we obtain for the quotients due to the respective altitudes the following numbers:—

On the Grossglockner		996
On the	Adlersruhe	991
	Rachern	995
On the Johannishütte		981
In Lienz		856

Differences which are great enough to be the cause of considerable errors in judging of distances at great heights, show themselves here. If objects, the size of which is approximately known to us, as men, animals, houses, &c., be observed at great elevations, we are generally induced to consider them nearer than they really are. Objects which enable us to draw no conclusion as to distance, such as masses of projecting rock, &c., appear to us too small. The reverse of this property of transparency to diminish the apparent size of bodies is exhibited when the atmosphere is obscured by fog, &c. In this case mountain summits are considerably elevated, and appear to us rougher and steeper than usual. In one single instance the transparency of the atmosphere seems to increase the size of objects, and that is when the chain of the Alps are observed from the plain to the south or north. In moist weather, generally before the descent of rain, the mountains appear darker and at the same time somewhat larger. This illusion appears to be due to the fact, that in the latter case they are much more clearly and sharply defined against the horizon*.

The transparency of the atmosphere has the power to modify in a great measure the magnitude of the prospect commanded by a great altitude. This is never so great as to permit of being calculated from the curvature of the earth and the refraction, for in the lower portions the prospect is always considerably limited by vapours†. This explains why we see more clearly looking from below upwards, than from a height downwards. In the latter case, however, another influence operates. The objects seen from above exhibit a uniform obscure colouring, and do not present the same striking contrasts among themselves as the rocks and the snow-covered mountains against the sky. The greater transparency of the upper regions of the atmosphere is strikingly exhibited when we direct our glance to higher summits. It is surprising how plainly the latter stand out before us, and with what distinctness we can recognise the objects which rest upon them. The reason of this is, that we look through a higher and more rarified atmospheric region.

The intensity of the rays of light can also be approximately determined by their chemical action upon colours ‡; although the results depend upon the material, &c., the increase of intensity with the height is plainly manifested. We made use of strips of paper on which a uniform wash of carmine was laid. In each experiment one-half of each strip was exposed to the sun from 11 to 2 o'clock, while the other half was shaded by an opake screen. The altered colours were imitated by carefully mixing together carmine and white in different proportions (the exact process may be learned where the cyanometer is described). In this way we obtained the following corresponding quantities; the

* A very simple practical rule to calculate the circle of view from the height is that used by seamen. The square root of the height in Hamburg feet gives the radius in sea miles, 60 to the degree. The refraction is here taken into account. A Hamburg foot is $=0{\cdot}286$ met. $=127{\cdot}0$ Parisian lines. For heights of 12,000 feet, we obtain 118 sea miles $=$ 29 geographical miles; for Mont Blanc, Saussure gives 136 sea miles (*Voyages*, vol. iv. 4to, p. 194).

† The same was observed by Humboldt and Bonpland (*Tableau Physique des Régions Equinoxiales*. Paris, 1807, p. 135); and also by Gay-Lussac during his aërial journeys.

‡ Saussure, *Mém. de Turin*, vol. iv. p. 441–453; *Voyages*, vol. iv. p. 297.

percentage quantity of carmine present being estimated from a mixture of white and red of the same brightness.

2000′.			4000′.			7000′.			10,000′.		
In the shade.	In the sun.	Difference.	In the shade.	In the sun.	Difference.	In the shade.	In the sun.	Difference.	In the shade.	In the sun.	Difference.
21	19	2	15	12	3	19	16	3	23	18	5
18	15	3	24	21	3	18	14	4			
			17	15	2	21	18	3			
						23	18	5			
Mean diff. 2·5			Mean diff. 2·7			Mean diff. 3·7			Mean diff. 5		

The chemical action of the light attains its maximum a little before noon, which was demonstrated by experiments with Daguerreotype plates. Alexander von Humboldt* has drawn attention to the fact, that at hours equally distant from noon, for example at 10 o'clock A.M. and 2 o'clock P.M., at 8 o'clock A.M. and 4 o'clock P.M., &c., the most decided divergences are exhibited. This is chiefly due to the alteration in the transparency of the atmosphere through the condensation of vapours, and hence in different localities may exhibit small variations: these depend upon the distribution of the relative moisture† at different elevations,—a subject which has been already treated of.

[To be continued.]

II. *Description of several New and Simple Stereoscopes for exhibiting, as solids, one or more representations of them on a Plane. By* Sir David Brewster, *K.H., D.C.L., F.R.S., and V.P.R.S. Edin.*‡

[With a Plate.]

THE ingenious stereoscope, invented by Prof. Wheatstone, for representing solid figures by the union of dissimilar plane pictures, is described in his very interesting paper "On some remarkable and hitherto unobserved Phænomena of Binocular Vision§"; and in a paper published in a recent volume of the Edinburgh Transactions||, I have investigated the cause of the perception of objects in relief, by the coalescence of dissimilar pictures.

Having had occasion to make numerous experiments on this

* *Asie Central*, Mahlmann's edition, vol. ii. p. 76.

† See Physical Geography of the Alps, p. 398–425.

‡ From the Transactions of the Royal Scottish Society of Arts, 1849. See also the Report of the British Association at Birmingham, 1849, Trans. of Sect. p. 47.

§ Phil. Trans. 1838, p. 371.

|| Ibid. vol. xv. part 3, p. 360.

subject, I was led to construct the stereoscope in several new forms, which, while they possess new and important properties, have the additional advantages of cheapness and portability. The first and the most generally useful of these forms is—

1. *The Lenticular Stereoscope.*

This instrument consists of two semilenses, placed at such a distance that each eye views the picture or drawing opposite to it through the margin of the semilens, or through parts of it equidistant from the margin. The distance of the portions of the lens through which we look must be equal to the distance of the centres of the pupils, which is, at an average, $2\frac{1}{2}$ inches. The semilenses should be placed in a frame, so that their distance may be adjusted to different eyes, as shown in Plate II. fig. 1.

When we thus view two dissimilar drawings of a solid object, as it is seen by each eye separately, we are actually looking through two prisms, which produce a second image of each drawing; and when these second images unite, or coalesce, we see the solid object which they represent. But in order that the two images may coalesce, without any effort or strain on the part of the eye, it is necessary that the distance of similar parts of the two drawings be equal to *twice* the separation produced by the prism. For this purpose, measure the distance at which the semilenses give the most distinct view of the drawings; and having ascertained, by using one eye, the amount of the refraction produced at that distance, or the quantity by which the image of one of the drawings is displaced, place the drawings at a distance equal to twice that quantity, that is, place the drawings so that the average distance of similar parts in each is equal to twice that quantity. If this is not correctly done, the eye of the observer will correct the error by making the images coalesce, without being sensible that it is making any such effort. When the dissimilar drawings are thus united, the solid will appear standing, as it were, in relief, between the two plane representations of it.

In looking through this stereoscope, the observer may probably be perplexed by the vision of *only the two dissimilar drawings.* This effect is produced by the strong tendency of the eyes to unite two similar, or even dissimilar drawings. No sooner do the refracted images emerge from their respective drawings, than the eyes, in virtue of this tendency, force them back into union; and though this is done by the convergency of the optic axes to a point nearer the eye than the drawings, yet the observer is scarcely conscious of the muscular exertion by which this is effected. This effect, when it does occur, may be counteracted by drawing back the eyes from the lenses, and shutting them before they again view the drawings. It exists chiefly with short-sighted persons, for whom the stereoscope may be constructed with concave semilenses or quarters of lenses, placed as in fig. 16; and when there are only *two* drawings, it may be prevented by a partition, which hides the right-hand drawing from the left eye, and the left-hand drawing from the right eye.

The instrument, as fitted up for use, is shown in fig. 2, where ABCD is a frame of tin or wood, consisting of an upper and a lower plate, and two ends, AB and CD. The semilenses are placed in CD, with an opening for the nose at NN, a part of the lower plate being cut away for this purpose. The *three* dissimilar drawings, as shown at C, fig. 4, are placed in the end AB, and are illuminated by the light which enters by the two open sides, AC, BD*. If the drawings are upon thin or transparent paper, or are executed as transparencies like the diagrams used in the magic lantern, the box ABCD may be closed, and the light admitted only through the end AB. In the form shown in fig. 2, where the drawings slide into an open frame, either opake or transparent figures may be used. It is often convenient to have the drawings separate, so that, like the semilenses, they may be made to approach to or recede from one another; and when the drawings are thus separate, we can obtain the arrangement at B, fig. 4, from the drawings at A, or all of them from the three drawings at C.

While the semilenses thus double the drawings and enable us to unite two of the images, they at the same time magnify them, —an advantage of a very peculiar kind, when we wish to give a great apparent magnitude to drawings on a small scale, taken photographically with the camera. But while the magnifying power of any lens is the same through whatever portion of it we look, its prismatic angle varies with the distance of that portion from the margin. In the semilens LL, for example, fig. 3, the prismatic angle is a maximum at the margin A, less at A′, and still less at A″, so that when the drawing is very small, we can double it, and refract it sufficiently by looking through A″, when larger through A′, and when larger still through A. By using a thicker lens, without changing the curvature of its surface, or its focal length, we can increase the prismatic angle at its margin, so as to produce any degree of refraction that may be required for the purposes of experiment, or for the duplication of large drawings.

* It is sometimes more convenient to close the sides, and leave the upper and under sides open, or we may cut off a circular segment from its upper and lower plate, as shown in fig. 2. The use of this opening in the lower plate is to illuminate the drawings when we turn the stereoscope and figures upside down, which increases the relief in a surprising degree.

It is obvious, from the very nature of the lenticular stereoscope, that it may be made of any size. The one from which fig. 2 is copied is 8 inches long, and 5 inches at its widest end; but I have made them only *three* inches long, and have now before me a *microscopic stereoscope,* which can be carried in the pocket, and which exhibits all the properties of the instrument to the greatest advantage*.

If we suppose the two figures at A, fig. 4, to represent a cone, as seen by the right and left eye, the stereoscope will unite them into a *raised* cone, with the circular apex *nearest* the eye. If they are placed as at B, they will appear as a *hollow* cone, the apex being furthest from the eye. In Mr. Wheatstone's stereoscope, the drawings must be turned upside down, in order that the *raised* and *hollow* cone may be seen in succession; but with the lenticular stereoscope, we have only to place *three figures,* as at C, fig. 4, and between A, B, fig. 2, in order to see at the same time the *raised* and the *hollow* cone; the *former* being produced by the union of the *first* with the *second,* and the latter by the union of the *second* with the *third* figures.

This method of exhibiting at the same time the raised and the hollow solid, enables us to give an ocular and experimental proof of the usual explanation of the cause of the large size of the horizontal moon, of her small size when in the meridian at a considerable altitude, and her intermediate apparent magnitude at an intermediate altitude. As the summit of the raised cone *appears* to be nearest the eye of the observer, the summit of the hollow cone furthest off, and that of the flat drawing on each side at an intermediate distance, these distances will represent the apparent distance of the moon in the zenith of the elliptical celestial vault, in the horizon, and at an altitude of 45°. The circular summits thus seen are in reality exactly of the same size, and at the same distance from the eye, and are therefore precisely in the same circumstances as the moon in the three positions already mentioned. If we now contemplate them in the stereoscope, we shall see the circular summit of the hollow cone the *largest,* like the *horizontal* moon, because it seems at the *greatest* distance from the eye; the circular summit of the *raised* cone the *smallest,* because it appears at the *least* distance, like the *zenith* moon; and the circular summit of the cones on each of an *intermediate* size, like the moon at an altitude of 45°,

* In place of using semilenses, as I at first did, I now use quarters of lenses, which answer the purpose equally well. With a single lens, therefore, we can construct two stereoscopes of exactly the same power. This is the first time that a quadrant of a lens has been used in optics. The eye-end of the stereoscope should consist of two short tubes, with the lenses at their extremities.

because their distance from the eye is intermediate. In the accompanying model this effect will be distinctly seen, by placing three small wafers of the same size and colour on the square summits of the drawings of the cones or four-sided pyramids. No change is produced in the apparent magnitude of these circles by making one or more of them less bright than the rest, and hence we see the incorrectness of the explanation of the size of the horizontal moon, as given by Dr. Berkeley*.

When the observer fails to see the object in relief from the cause already mentioned, but sees only the *two* drawings, if there are *two*, or the *three* drawings, if there are *three*, the plane of the drawings appears *deeply hollow*; and, what is very remarkable, if we look with the eccentric lenses at a flat table from above, it also appears deeply hollow; and if we touch it with the palm of our hand, *it is felt as hollow*, while we are looking at it, but the sensation of hollowness disappears upon shutting our eyes. The sense of sight, therefore, instead of being the pupil of the sense of touch, as Berkeley and others have believed, is in this, as in other cases, its teacher and its guide†.

2. *The Total-Reflexion Telescope.*

This form of the stereoscope is a very interesting one, and possesses valuable properties. It requires only a small prism and *one* diagram, or picture of the solid, as seen by one eye; the other diagram, or picture which is to be combined with it, being created by total reflexion from the base of the prism. This instrument is shown in fig. 5, where D is the picture of a cone as seen by the left eye L, and ABC a prism, whose base BC is so large, that when the eye is placed close to it, it may see, by reflexion, the whole of the diagram D. The angles ABC, ACB must be equal, but may be of any magnitude. Great accuracy in the equality of the angles is not necessary; and a prism constructed by a lapidary out of a fragment of thick plate-glass, the face BC being one of the surfaces of the plate, will answer the purpose‡. When the prism is placed at *abc*, fig. 6, at one end of a conical tube LD, and the diagram D, at the other end, in a cap which can be turned round so as to have the line *mn*, which passes through the centre of the base and summit of the cone parallel to the line joining the two eyes, the instrument is ready for use. The observer places his left eye at L, and views with

* Berkeley's Works, p. 98; Essay on the Theory of Vision, § 67–78. Lond. 1837.

† See Edinburgh Transactions, vol. xv. p. 672.

‡ In this case the prism may have the form B*cd*C, fig, 5, the parallel sides BC, *cd* being the original faces of the piece of the plate-glass, and the inclined faces B*c*, C*d* only, the work of the lapidary.

it the picture D, as seen by total reflexion from the base BC or *bc* of the prism, figs. 5 and 6, while with his right eye R, fig. 5, he views the same picture directly. The first of these pictures being the reverse of the second D, like all pictures formed by one reflexion, we thus combine two dissimilar pictures into a *raised* cone, as in the figure, or into a *hollow* one, if the picture at D is turned round 180°. If we place two diagrams, one like one of those at A, fig. 4, and the other like the other at A, fig. 4, vertically above one another, we shall then see, at the same time, the *raised* and the *hollow* cone as produced in the lenticular stereoscope by the three diagrams in fig. 4 at C. When the prism is good, the dissimilar image produced by the two refractions at B and C, and the one reflexion at E, is of course more accurate than if it had been drawn by the most skilful artist; and therefore this form of the stereoscope has in this respect an advantage over every other in which two dissimilar figures, executed by art, are necessary. In consequence of the length of the reflected pencil DB + BE + EC + CL being a little greater than the direct pencil of rays DR, the two images combined have not exactly the same apparent magnitude; but the difference is not perceptible to the eye, and a remedy could easily be provided were it required.

If the conical tube LD is held in the left hand, the left eye must be used; and if in the right hand, the right eye must be used; so that the hand may not obstruct the direct vision of the drawing by the eye which does not look through the prism. The cone LD must be turned round slightly in the hand till the line *mn* joining the centre and apex of the figure is parallel to the line joining the two eyes. The same line must be parallel to the plane of reflexion from the prism; but this parallelism is secured by fixing the prism and the drawing.

It is scarcely necessary to state, that this stereoscope is applicable only to those diagrams and forms where the one image is the reflected picture of the other.

If we wish to make a microscopic stereoscope of this form, or to magnify the drawings, we have only to cement plano-convex lenses, of the requisite focal length, upon the faces AB, AC of the prism, or, what is simpler still, to use a section of a deeply convex lens ABC, fig. 7, and apply the other half of the lens to the right eye, the face BC having been previously ground flat and polished for the prismatic lens. By using a lens of larger focus for the right eye, we may correct, if required, the imperfection arising from the difference of paths in the reflected and direct pencils. This difference is so trivial, that it might be corrected by applying to the right eye the central portion of the same lens whose margin is used for the prism.

3. *The Single Prismatic Stereoscope.*

The prismatic stereoscope, represented in fig. 8, consists of a single prism P, with a small refracting angle, capable of refracting the image of the figure A, so as just to combine it with the dissimilar figure B, seen directly by the right eye. The second picture should be placed close to A, in order that they may be united by a prism with the smallest refracting angle. There is a slight degree of colour in the refracted image, but it does not injure the general effect. The prism, therefore, should not be made of flint-glass, or any glass with a high dispersive power. A single face ground by a lapidary upon one of the faces of a morsel of plate-glass, the size of the pupil of the eye, will give a prism sufficient for every ordinary purpose. Any person may make one for himself by placing a little bit of window-glass upon another piece inclined to it, and inserting in the angle between them a drop of water. When the figures are small and near one another, a water prism with the requisite angle will scarcely produce any perceptible colour*.

If we make a double prism, as shown at PP′, fig. 9, and apply it to the two dissimilar figures A, B, so that with the left eye L looking through the prism P, we may place the refracted image of B upon A, as seen by the right eye R, we shall see a *hollow* cone; and if with the left eye L′, looking through the other prism P′, we place the refracted image of A upon B, as seen with the right eye R′, we shall see a *raised* cone.

4. *The Singly-Reflecting Stereoscope.*

A very simple stereoscope may be constructed, as in fig. 10, by using a small piece of black glass, or plate-glass with one side covered with black wax. This piece of glass MN reflects to the left eye L a reverted image of the figure B, which, when seen in the direction LCA, and combined with the figure A, seen directly by the right eye R, gives a raised cone. The cone will be seen hollow by reversing the figures A, B. As BC + CL is greater than AR, the reflected image of B will be slightly less than A; but the difference is so little, that it does not affect the appearance of the *hollow* or the *raised* cone. By bringing B a little nearer the reflector MN, the two pictures may be made exactly the same. The small reflector and the dissimilar figures may be fitted up in a conical tube, like that shown in fig. 6, the tube having an elliptical section to accommodate *two* figures at its further end, the major axis of the ellipse being parallel to the line joining the two eyes.

* Professor Wheatstone has, we believe, used *two* achromatic prisms, but they are not necessary.

5. *The Double-Reflecting Stereoscope.*

In this form of the instrument a second reflector is added for the right eye, as shown at M′N′, fig. 11, and the effect of this is to exhibit at the same instant the *raised* and the *hollow* cone. The image of B seen by reflexion from MN at the point C is combined with the direct picture of A, seen by the right eye, and forms a hollow cone; while the image of A seen by reflexion from M′N′ at the point C′, is combined with the direct picture of B, seen by the left eye. These reflectors may be placed in an elliptical tube, with an opening near the end AB to illuminate the figures A, B, or we may dispense with an opening by having the figures drawn upon thin or transparent paper. When the figures are drawn in transparent lines on a ground of opake varnish, like the diagrams in the magic lantern, the effect is very fine.

Another form of the double-reflecting stereoscope is shown in fig. 12, which differs from that shown in fig. 11 in the position of the two reflectors, and of the figures to be united. The reflecting faces of the mirrors are turned outwards, their distance being less than the distance between the eyes; and the effect of this is to unite into a *hollow* cone the same figures which the other form in fig. 11 unite into a *raised* one. The superiority of this position of the reflectors is, that they are more easily enclosed in a tube, and that the instrument is more portable.

In describing these various forms of the stereoscope, by which the instrument may not only be rendered portable, but may be constructed out of materials which every person possesses, and without the aid of an optician, we have supposed the two dissimilar figures to be those of the frustum of a cone as seen by each eye separately; the large circle being the representation of the base of the cone, and the small circle the representation of its truncated summit. If we join similar points of these two circles by lines, as is done in the figures, the conical figure will be more distinct.

If we take the drawing of a six-sided pyramid as seen by the right eye, as shown in fig. 13, and place it in the total-reflexion stereoscope at D, fig. 5, so that the line MN coincides with *mn*, and is parallel to the line joining the eyes of the observer, we shall perceive a perfect raised pyramid of a given height, the reflected image of CD, fig. 13, being combined with AF seen directly. If we now turn the figure round 30°, CD will come into the position AB, and unite with AB, and we shall still perceive a raised pyramid with less height and less symmetry. If we turn it round 30° more, CD will be combined with BC, and we shall still perceive a raised pyramid with still less height, and still less symmetry. When the figure is turned round other 30°, or 90° from its first position, CD will coincide with CD seen directly, and the combined figures will be perfectly flat. If we continue the rotation through other 30°, CD will coincide with DE, and a slightly hollow, but not very symmetrical pyramid, will be seen. A rotation of other 30° will bring CD into coalescence with EF, and we shall see a still more hollow and more symmetrical pyramid. A further rotation of other 30°, making 180° from the commencement, will bring CD into union with AF; and we shall have a perfectly symmetrical hollow pyramid of still greater depth, and the exact counterpart of the raised pyramid which was seen before the rotation of the figure commenced. If the pyramid had been square, the *raised* would have passed into the *hollow* pyramid by rotations of 45° each. If it had been rectangular, the change would have been effected by rotations of 90°. If the space between the two circular sections of the cone in fig. 12 had been uniformly shaded, or if lines had been drawn from every degree of the one circle to every corresponding degree in the other, in place of from every 90th degree, as in the figure, the raised cone would have gradually diminished in height by the rotation of the figure till it became flat, after a rotation of 90°; and by continuing the rotation, it would have become hollow, and gradually reached its maximum depth after a revolution of 180°.

There are two classes of phænomena of a very interesting kind, to which the stereoscope is not properly applicable, namely, those where it is required to unite a great number of similar and equidistant patterns, such as those which compose paper-hangings, carpets, and the openings in the cane bottoms of chairs; and those in which we binocularly unite, and give a new position to, lines meeting at or converging to a point, the eye being placed at different heights above the plane of the paper, and at different distances from the angular point*. In studying these phænomena, we produce the required union by straining the eyes, or by contemplating the objects while the eyes are directed to a point either nearer to or further from them. The power of doing this with facility is possessed by very few persons, and it is therefore necessary to have a simple and infallible method of effecting the union of such objects without instrumental assistance. The following method, when practised for a short time, will answer this purpose.

* These two classes of phænomena are described in my paper "On the Knowledge of Distance given by Binocular Vision," published in the Edinburgh Transactions, vol. xv. p. 663.

6. *Method of uniting Similar or Dissimilar Figures.*

Upon a piece of glass MN, fig. 14, place a very small circle of white paper D, and let A, B, C be similar patterns which we wish to unite, A with C, or A with B. Hold the piece of glass MN in both hands, and at such a distance from the eyes that, when with the left eye L, and shutting the right eye, we see the circle D covering C, we also, upon opening the right eye B, see with it the circle D covering A. By continuing for a short time to look at the circle D with both eyes open, we shall see the patterns all united, and the wall or plane which contains them situated at the same distance from the eye as the circle D. If there are one or more intermediate patterns, such as B, the piece of glass MN must be held further from the eyes in order to unite A with B instead of A with C. Those who acquire in this way the art of uniting dissimilar and similar figures, will not require in any case the aid of the stereoscope, unless when there is only one figure or object; in which case they must have recourse to the total-reflexion stereoscope, in order to convert the single figure into a solid, by creating and uniting with it its opposite or reflected image.

7. *Method of Drawing on a Plane the Dissimilar Representations of Solids for the Stereoscope.*

Let L, R, fig. 15, be the left and right eye, and A the middle point between them. Let MN be the plane on which an object or solid, whose height is CB, is to be drawn. Through B draw LB, meeting MN in c; then if the object is a solid, with its apex at B, Cc will be the distance of its apex from the centre C of its base, as seen by the left eye. As seen by the right eye R, Cc' will have the same value, but c' will lie on the left side of C. Calling E the distance between the two eyes, and h the height BC of the solid, we shall have $\mathrm{AB} : h = \frac{\mathrm{E}}{2} : \mathrm{C}c$ and $\mathrm{C}c = \frac{h\mathrm{E}}{2\mathrm{AB}}$, which will give us the results in the following table, AC being $=8$ and $\mathrm{E}=2\frac{1}{2}$ inches:—

Height. BC=h.	AB.	Cc.
1	7	0·279 inch.
2	6	0·4166 ...
3	5	0·75 ...
4	4	1·25 ...
5	3	2·088 ...
6	2	3·75 ...
7	1	8·75 ...
8	0	Infinite.

If we now wish, by directing the axes of the eyes beyond MN to b, to ascertain the value of Cc', which will give different depths d of the *hollow* solids corresponding to different values of Cb, we shall have $Ab : \frac{E}{2} = d : Cc'$ and $Cc' = \frac{dE}{2AB}$; which, making AC8 inches as before, will give the following results:—

Depth. $Cb=d$.	Ab.	Cc'.
1	9	0·139 inch.
2	10	0·25 ...
3	11	0·34 ...
4	12	0·4166 ...
5	13	0·48 ...
6	14	0·535 ...
7	15	0·58 ...
8	16	0·625 ...
9	17	0·663 ...
10	18	0·696 ...
11	19	0·723 ...
12	20	0·75 ...

The values of h and d, when the excentricities Cc, Cc', as we may call them, are known, will be found by the formulæ $h = \frac{CcE}{2AB}$ and $d = \frac{Cc'E}{2Ab}$. As Cc is always equal to Cc' in each pair of figures or dissimilar pictures, the depth of the *hollow* solid will always appear much greater than the height of the *raised* solid one. When Cc and Cc' are both 0·75 $h : d = 3 : 12$, and when they are both 0·4166, $h : d = 2 : 4$, and when they are both 0·139 $h : d = 0·8 : 1·0$.

III. *Account of a Binocular Camera, and of a Method of obtaining Drawings of Full Length and Colossal Statues, and of Living Bodies, which can be exhibited as Solids by the Stereoscope. By* Sir David Brewster, *K.H., D.C.L., F.R.S., and V.P.R.S. Edin.**

In explaining the construction and use of the lenticular and other stereoscopes, I have referred only to the duplication and union of the dissimilar drawings on a plane of geometrical and symmetrical solids. The most interesting application, however, of these instruments is to the dissimilar representations of statues and living bodies of all sizes and forms, and also to natural scenery, and the objects which enter into its composition. Professor

* From Trans. of Royal Scottish Society of Arts, 1849. See also Report of British Association at Birmingham, 1849, Trans. of Sect., p. 5.

Wheatstone had previously applied his stereoscope to the union of dissimilar drawings of small statues, taken by the Daguerreotype and Talbotype processes; and in an essay on Photography, lately published*, I have mentioned its application to statues of all sizes, and even to living figures, by means of a binocular camera. The object of the present paper is to describe the binocular camera, and to explain the principles and methods by which this application of the stereoscope is to be carried into effect.

The vision of bodies of three dimensions, or of groups of such bodies combined, has never been sufficiently studied either by artists or philosophers. Leonardo da Vinci, who united in a remarkable degree a knowledge of art and science, has, in a passage of his *Trattato della Pittura,* quoted by Dr. Smith of Cambridge†, made a brief reference to it insofar as binocular vision is concerned; but till the publication of Professor Wheatstone's interesting memoir "On some remarkable and hitherto unobserved Phænomena of Binocular Vision‡," the subject had excited no attention.

In order to understand the subject, we shall first consider the vision with *one eye* of objects of three dimensions, when of different magnitudes and placed at different distances. When we thus view a building or a full-length or colossal statue at a short distance, a picture of all its visible parts is formed on the retina. If we view it at a greater distance, certain parts cease to be seen, and other parts come into view; and this change on the picture will go on, but will become less and less perceptible as we retire from the original. If we now look at the building or statue from a distance through a telescope, so as to present it to us with the same distinctness, and of the same apparent magnitude as we saw it at our first position, the two pictures will be essentially different; all the parts which ceased to be visible as we retired will still be invisible, and all the parts which were not seen at our first position, but became visible by retiring, will be seen in the telescopic picture. Hence the parts seen by the near eye, and not by the distant telescope, will be those towards the middle of the building or statue, whose surfaces converge, as it were, towards the eye; while those seen by the telescope, and not by the eye, will be the external parts of the object whose surfaces converge less, or approach to parallelism. It will depend on the nature of the building or the statue which of these pictures gives us the most favourable representation of it.

* North British Review, vol. vii. p. 502, August 1847.

† Complete System of Optics, vol. ii. Remarks, p. 41. § 244.

‡ Phil. Trans., 1838, p. 371; see also Edinburgh Transactions, vol. xv. pp. 349 and 663.

If we now suppose the building or statue to be reduced in the most perfect manner,—to half its size, for example,—then it is obvious that these two perfectly similar solids will afford a different picture, whether viewed by the eye or by the telescope. In the reduced copy, the inner surfaces visible in the original will disappear, and the outer surfaces become visible; and, as formerly, it will depend on the nature of the building or the statue whether the reduced or the original copy gives the best picture.

If we repeat the preceding experiments with *two eyes* in place of *one,* the building or statue will have a different appearance. Surfaces and parts, formerly invisible, will become visible, and the body will be better seen because we see more of it; but then the parts thus brought into view being seen, generally speaking, with one eye, will have only one-half the illumination of the rest of the picture. But, though we see more of the body in binocular vision, it is only parts of vertical surfaces perpendicular to the line joining the eyes that are thus brought into view, the parts of similar horizontal surfaces remaining invisible as with one eye. It would require a pair of eyes placed vertically, that is, with the line joining them in a vertical direction, to enable us to see the horizontal as well as the vertical surfaces; and it would require a pair of eyes inclined at all possible angles, that is, a ring of eyes $2\frac{1}{2}$ inches in diameter, to enable us to have a perfectly symmetrical view of the statue.

These observations will enable us to answer the question, whether or not a reduced copy of a statue, of precisely the same form in all its parts, will give us, either by monocular or binocular vision, a better view of it as a work of art. As it is the outer parts or surfaces of a large statue that are invisible, its great outline and largest parts must be best seen in the reduced copy; and consequently its relief, or third dimension in space, must be much greater in the reduced copy. This will be better understood if we suppose a *sphere* to be substituted for the statue. If the sphere exceeds in diameter the distance between the pupils of the right and left eye, or $2\frac{1}{2}$ inches, we shall not see a complete hemisphere unless from an infinite distance. If the sphere is larger, we shall see only a segment, whose relief, in place of being equal to the radius of the sphere, is equal only to the versed sine of half the visible segment. Hence it is obvious that a reduced copy of a statue is not only better seen from more of its parts being visible, but is also seen in stronger relief.

With these observations, we shall be able to determine the best method of obtaining dissimilar plane drawings of full-length and colossal statues, &c., in order to reproduce them in three dimensions by means of the stereoscope. Were a painter called upon to take drawings of a statue, as seen by each eye, he would fix, at the height of his eyes, a metallic plate with two small holes in it, whose distance is equal to that of his eyes, and he would then draw the statue as seen through the holes by each eye. These pictures, however, whatever be his skill, would not be such as to reproduce the statue by their union. An accuracy, almost mathematical, is necessary for this purpose; and this can only be obtained from pictures executed by the processes of the Daguerreotype and Talbotype. In order to do this with the requisite nicety, we must construct a binocular camera, which will take the pictures simultaneously and of the same size; that is, a camera with two lenses of the same aperture and focal length, placed at the same distance as the two eyes. As it is impossible to grind and polish two lenses, whether single or achromatic, of exactly the same focal lengths, even if we had the very same glass for each, I propose to bisect the lenses, and construct the instrument with semilenses, which will give us pictures of precisely the same size and definition. These lenses should be placed with their diameters of bisection parallel to one another, and at the distance of $2\frac{1}{2}$ inches, which is the average distance of the eyes in man; and, when fixed in a box of sufficient size, will form a binocular camera, which will give us, at the same instant, with the same lights and shadows, and of the same size, such dissimilar pictures of statues, buildings, landscapes, and living objects, as will reproduce them in relief in the stereoscope.

It is obvious, however, from observations previously made, that even this camera will only be applicable to statues of small dimensions, which have a high enough relief, from the eyes seeing, as it were, well around them, to give sufficiently dissimilar pictures for the stereoscope. As we cannot increase the distance between our eyes, and thus obtain a higher degree of relief for bodies of large dimensions, how are we to proceed in order to obtain drawings of such bodies of the requisite relief?

Let us suppose the statue to be colossal, and *ten* feet wide, and that dissimilar drawings of it about *three* inches high are required for the stereoscope. These drawings are *forty* times narrower than the statue, and must be taken at such a distance that, with a binocular camera having its semilenses $2\frac{1}{2}$ inches distant, the relief would be almost evanescent. We must, therefore, suppose the statue to be reduced n times, and place the semilenses of the binocular camera at the distance $n \times 2\frac{1}{2}$ inches. If $n=10$, the statue will be reduced to $\frac{10}{10}$, or to 1 foot, and $n \times 2\frac{1}{2}$, or the distance of the semilenses will be 25 inches. If the semilenses are placed at this distance, and dissimilar pictures of the colossal statue taken, they will reproduce by their union a statue *one* foot high, which will have exactly the same appearance and relief as if we had viewed the colossal statue with eyes 25 inches distant. But the reproduced statue will have also the same appearance and relief as a statue a foot high, reduced from the colossal one with mathematical precision; and therefore it will be a better and a more relieved representation of the work of art than if we had viewed the colossal original with our own eyes, either under a greater, an equal, or a less angle of apparent magnitude.

We have supposed that a statue *a foot broad* will be seen in proper relief by binocular vision; but it remains to be decided whether or not it would be more advantageously seen, if reduced with mathematical precision to a breadth of $2\frac{1}{2}$ inches, the width of the eyes, which gives the vision of a hemisphere $2\frac{1}{2}$ inches in diameter, with the most perfect relief. If we adopt this principle, and call B the breadth of the statue of which we require dissimilar pictures, we must make $n=\frac{B}{2\frac{1}{2}}$, and $n \times 2\frac{1}{2}=B$, that is, the distance of the semilenses in the binocular camera, or of the semilenses in two cameras, if two are necessary, must be made equal to the breadth of the statue.

In the same manner we may obtain dissimilar pictures of living bodies, buildings, natural scenery, machines, and objects of all kinds, of three dimensions, and reproduce them by the stereoscope, so as to give the most accurate idea of them to those who could not understand them in drawings of the greatest accuracy.

The art which we have now described cannot fail to be regarded as of inestimable value to the sculptor, the painter, and the mechanist, whatever be the nature of his production in three dimensions. Lay figures will no longer mock the eye of the painter. He may delineate at leisure on his canvas, the forms of life and beauty, stereotyped by the solar ray and reconverted into the substantial objects from which they were obtained, brilliant with the same lights and chastened with the same shadows as the originals. The sculptor will work with similar advantages. Superficial forms will stand before him in three dimensions, and while he summons into view the living realities from which they were taken, he may avail himself of the labours of all his predecessors, of Pericles as well as of Canova; and he may virtually carry in his portfolio the mighty lions and bulls of Nineveh,—the gigantic sphinxes of Egypt,—the Apollos and Venuses of Grecian art,—and all the statuary and sculpture which adorn the galleries and museums of civilized nations.

IV. *Notice of a Chromatic Stereoscope.* *By* Sir David Brewster, *K.H., F.R.S., V.P.R.S. Edin.**

In the year 1848, I communicated to the British Association, at Swansea, a brief notice of the principle of this instrument†.

If we look with both eyes through a lens, about $2\frac{1}{2}$ inches in diameter or upwards, at an object having colours of different refrangibilities, such as the coloured lines on a map, a red rose among green leaves, or any scarlet object upon a blue ground, or in general any two simple colours not of the same degree of refrangibility, the *two* colours will appear at different distances from the eye of the observer.

In this experiment we are looking through the margin of two semilenses or virtual prisms, by which the more refrangible rays are more refracted than the less refrangible rays. The doubly-coloured object is thus divided into two as it were, and the distance between the two blue portions is as much greater than the distance between the two red portions (red and blue being supposed to be the colours) as *twice* the deviation produced by the virtual prism, if we use a large lens or two semilenses, or by the real prisms, if we use prisms.

The images of different colours being thus separated, the eyes unite them as in the stereoscope, and the *red* image takes its place nearer the observer than the *blue* one, in the very same manner as the two nearest portions of the dissimilar stereoscopic figures stand up in relief at a distance from their more remote portions. The reverse of this will take place if we use a concave lens, or if we turn the refracting angles of the two prisms inwards.

Hence it follows, and experiment confirms the inference, that we give solidity and relief to plane figures by a suitable application of colour to parts that are placed at different distances from the eye.

These effects are greatly increased by using lenses of highly-dispersing flint glass, oil of cassia, and other fluids, and avoiding the use of compound colours in the objects placed in the stereoscope.

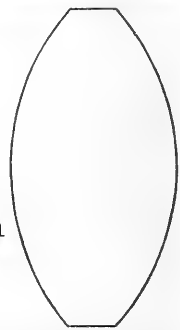
* Read before the Royal Scottish Society of Arts, Dec. 10, 1849.

† See Report of the British Association at Swansea, 1848, Trans. of Sect., p. 48.

V. *Account of Experiments with a powerful Electro-magnet.* *By* J. P. Joule, *F.R.S. &c.**

SOME years ago I announced that if a particle of wire conducting a voltaic current be made to act upon a very large surface of iron, the intensity of the induced magnetism will not be much diminished by an increase in the distance of that particle from the surface of the iron. Guided by this principle, I constructed a very powerful electro-magnet in 1843†, and soon after prepared the iron of the electro-magnet employed in the experiments related in the present paper. This was a plate of the best wrought iron, 1 inch thick, 22 inches long, 12 inches broad at the centre, but tapered thence to the breadth of 3 inches, as represented in the adjoining sketch (fig. 1). The plate was then bent into a semicircular shape, so as to bring its ends within 12 inches of one another. Previously to fitting up this bar as an electro-magnet, I made a few experiments with a view to test the principle above named more completely than I had hitherto done.

Fig.1



A length of about eight yards of insulated copper wire, $\frac{1}{20}$th of an inch in diameter, was divided into two exactly equal portions, one of which was wound four times round the broadest part of the iron, and close to its surface; the other was also wound four times round the broadest part of the iron, but was kept at the distance of one inch from its surface by means of interposed pieces of wood. A constant current of electricity was alternately passed through the wires; and the deflections of a magnetic needle half an inch long, placed at the distance of two feet from the iron bar, were observed to be as follows:—

6° 23′ with the wire close to the surface of the iron.
6° 9′ with the wire at the distance of one inch from the surface of the iron;

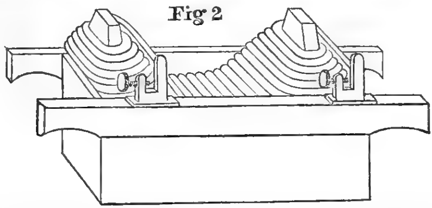
showing only a trifling diminution of effect in consequence of the removal of the wire to the distance of one inch from the surface.

Having been thus fortified in my previous conclusion as to the propriety of enveloping broad electro-magnets with a very large quantity of coils, even though the outer ones should be

* Communicated by the Author.
† Philosophical Magazine, S. 3, vol. xxiii. p. 268.

removed to a considerable distance from the surface of the iron, I proceeded to fit up the large bar already described with a coil consisting of a bundle of copper wires 68 yards long, and weighing 100 lbs. The electro-magnet thus formed was placed in a wooden box, on the side of which two large brass clamps were screwed, the latter being soldered to the terminals of the coil. The accompanying sketch represents the apparatus in its com-

Fig 2



pleted state; excepting, however, two brass straps, by means of which the coil is kept securely in its place, which are omitted for the sake of clearness.

In experimenting with the electro-magnet, I employed a battery consisting of sixteen Daniell's cells, the copper of each exposing an active surface of nearly two square feet. They were arranged so that I could with facility use either one cell alone, four cells in a series of two, or sixteen in a series of four elements. The cells and the liquids in them being similar in every respect, it was evident that these arrangements must produce through the electro-magnetic coils currents represented by 1, 2 and 4. I therefore was enabled to dispense with the use of a galvanometer, which would have been acted upon by the powerful electro-magnet, even if it had been placed at the distance of many yards from it.

Experiment I.—A magnetic needle, $1\frac{1}{2}$ inch long, was suspended at the distance of three feet from the electro-magnet measured on a line at right angles to that joining the poles. The northward tendency of the needle having been counteracted by means of a permanent magnet, I observed the following vibrations per minute resulting from the action of the electro-magnet:—

With 1 cell	in a series of	1 . .	48 vibrations.
... 4 cells	...	2 . .	63 ...
... 16	...	4 . .	96 ...

The vibrations are evidently in the ratio of the square root of the quantity of current circulating around the electro-magnet, and consequently we may infer that the magnetism induced in the latter was simply in proportion to the current.

Experiment II.—Having provided a pair of tapered poles terminating in vertical edges, 1 inch long and $\frac{1}{8}$th of an inch in breadth, I caused them to be slid on the poles of the electro-magnet until within $1\frac{1}{4}$ inch from each other. A cylindrical bar of bismuth $1\frac{3}{8}$ inch long, $\frac{1}{4}$ of an inch in diameter, and weighing 174 grains, was suspended by a filament of silk from a proper support, so as to vibrate between the tapered poles. The average numbers of vibrations in each minute of time through the quadrant of a circle were then found to be—

with 1 cell in a series of	1 . . .	$4\frac{1}{4}$	vibrations.
... 4 cells ...	2 . . .	$9\frac{1}{2}$	...
... 16 ...	4 . . .	17	...

The currents being as 1, 2 and 4, and the vibrations $4\frac{1}{2}$, $9\frac{1}{2}$ and 17, or nearly in the same ratio, it follows that the repulsive action of the magnetic poles was as the square of the current, and consequently that the diamagnetism of the bismuth is a quality not self-inherent, but induced by the magnetic action to which it is exposed. I am happy to have been thus enabled to confirm the important fact, discovered by M. Ed. Becquerel and Dr. Tyndall, by experiments made without any knowledge* of the researches they were conducting almost simultaneously on the same subject.

Experiment III.—The tapered poles remaining at $1\frac{1}{4}$ inch asunder, I suspended a piece of soft iron, 3 inches long, 1 inch deep, and $\frac{1}{5}$th of an inch thick, at the distance of a quarter of an inch above the poles. Using one cell of the battery, this small piece of iron was attracted with a force of $6\frac{3}{4}$ oz.; but with 16 cells in a series of 4, with a force of no less than $71\frac{1}{2}$ oz. In this instance we notice a slight falling away from the theoretical attraction, owing no doubt to the gradual approach of the limit to magnetizability in the small bar of iron.

Experiment IV.—The tapered poles having been removed, a flat bar of soft iron, 14 inches long, 3 inches in breadth, and 1 inch thick, was placed at various distances from the poles of the electro-magnet, and the attractions measured as follows:—

	$\frac{1}{4}$ in. dist.	$\frac{1}{2}$ in. dist.	1 in. dist.	2 in. dist.
	oz.	oz.	oz.	oz.
with 1 cell	102	38	$13\frac{3}{4}$	$3\frac{3}{4}$
with 16 cells in a series of 4	976	320	140	47

Here, again, we have evidences of an approach towards the limit

* The electro-magnet with which the above experiments had been made was sent to the Exhibition of Industry in the middle of February. M. Becquerel's paper was published in the *Annales de Chimie* for May, Dr. Tyndall's in this Magazine for September, after having been previously communicated to the Ipswich Meeting of the British Association.

of magnetizability, for the attractions with a current of 4 are only ten times, instead of sixteen times as great as those observed with a current of 1.

The electro-magnet I described some years ago* consisted of a core of iron, half an inch thick, enveloped by a coil of wires weighing 60 lbs. With a battery of ten cells, similar to those employed in the present experiments, a bar of iron 3 inches broad and $\frac{1}{2}$ an inch thick, was attracted at the distance of $\frac{1}{4}$ of an inch with a force of 480 oz., at $\frac{1}{2}$ an inch with a force of 168 oz., and at 1 inch with a force of 77 oz. Both electro-magnets having been constructed on the same principle, their attractive powers ought to be proportional to the weight of coil and number of cells, and therefore to be represented by $60 \times 10 = 600$, and $100 \times 16 = 1600$. As this is tolerably well borne out by comparing the actual results of the above experiments, we may infer that little or no advantage was obtained by increasing the thickness of the core of iron from half an inch to one inch.

Experiment V.—A flat bar of iron, $1\frac{3}{4}$ inch deep and $\frac{1}{5}$th of an inch thick, being placed with its thin edge in contact with the poles of the electro-magnet, the following weights had to be applied in order to overcome the attraction in contact:—

With 1 cell in a series of	1	. . .	64 lbs.
... 4 cells ...	2	. . .	72 ...
... 16 ...	4	. . .	96 ...

But when the bar of iron used in Experiment IV. was placed in contact with the poles, so as just to leave a quarter of an inch in breadth for the place of contact of the flat bar, the attraction of the latter with 16 cells was found to be only 82 lbs. Thus it would appear that 14 lbs. out of the 96 lbs. in the previous experiment were owing to the distant attraction of that part of the poles not in contact with the bar. We may therefore conclude, that while the attraction in contact, using one cell, was 64 lbs., minus say 1 lb. for distant attraction, that produced by a current four times as great was only increased to 82 lbs. And it must be remarked, that the greater part of this small increase was doubtless owing to the action of the broader part of the iron core which still remained unneutralized. It would therefore appear, that the greatest observed attraction in contact was, in this electro-magnet, about $70 \times 5 = 350$ lbs. per square inch of the surface of each pole, or otherwise that the greatest magnetic attraction of one square inch of surface for another square inch was 175 lbs. Several years ago I gave 140 lbs. as the apparent limit of attraction in contact†. The force of current employed in obtaining

* Philosophical Magazine, S. 3, vol. xxiii. p. 268.

† Ibid. S. 4. vol. ii. p. 453.

this result was only one-tenth of that which, in the present instance, did not produce a greater attraction than 175 lbs. It is therefore improbable that any force of current could give an attraction equal to 200 lbs. per square inch.

Experiment VI.—The magnetic needle was suspended as in Experiment I., and its vibrations, with 4 cells in a series of 2, were found to be 63 per minute. I then placed the large bar used in Experiment IV. across the poles, so as to neutralize their action. The number of vibrations of the needle per minute was then found to be 62, or only 1 less than before; showing that the neutralization of the magnetic tension of the poles (which were only 3 inches in breadth, that of the core at its greatest being 12 inches) permitted the tension of the remaining unneutralized breadth of 9 inches to be increased so as to prevent almost any diminution in the action on the needle.

Acton Square, Salford,
Dec. 15, 1851.

VI. *On Frictional Electricity.* *By* Reuben Phillips, *Esq.**

THE following electro-chemical theory is so far identical with that propounded by Sir H. Davy, that it regards the most simple forms of matter as electrified, and chemical action to consist only in the redistribution of the electric force. Sir H. Davy's theory was found at the time of its publication to be rather unmanageable, and accordingly Berzelius gave it a generally received version, which says that electricity is generated by the proximity of diverse particles. But this, as I understand it, requires the admission of an unknown force, which the proximity of the molecules developes into electricity.

Taking, for example, a volume of hydrogen, it by no means follows, that because it does not affect the electrometer, that therefore the gas contains no electricity. For, suppose each particle to be electrified by having positive electricity developed on one end, and its equivalent of negative electricity on the other end, then, however intense this polarization may be, the neutrality of the mass is perfect, because of the equality of the two opposite electric forces, and the minuteness and independence of the particles†.

* Communicated by the Author.

† It has been represented to me, that the particles cannot be neutral and independent if thus polarized, and I therefore state the experiments which appear to me to justify the assumption. If we take two plates of metal and insulate them in the air, with their surfaces parallel and some distance apart, and charge one positively, and the other, to an equal amount, negatively, then on approximating these two parallel surfaces, the external electrical excitement, as indicated by an electrometer, can be made to dis-

If a volume of any other gas, as chlorine, be made to combine with the hydrogen, the action, it would seem, can be consistently imagined in the following way. Each particle of either gas previously to combination has on it both positive and negative electricity; this may be sufficiently symbolized by writing $+\cdot-$ for either particle, where the dot signifies the atomic centre to which the two signs belong. On bringing a particle of hydrogen near to a particle of chlorine, the electricity may stand thus, $\begin{array}{c}+\cdot-\\-\cdot+\end{array}$ before combination, and $\begin{array}{c}+\cdot+\\-\cdot-\end{array}$ in hydrochloric acid. There is in this process a transfer, but no generation of electricity.

appear; and consequently we have here a system of polarization which produces no external effect. This experiment can be conveniently made with the ordinary condenser, the only alteration required being to insulate the moveable plate. The common Leyden jar affords another example of this species of electric distribution, which I suppose to exist in the molecules.

My intention in this paper was not to state any new opinions as to the nature of electricity, but to take the old and generally serviceable electro-chemical theory, and, after making a few necessary repairs, to apply it to frictional electricity; so that, when this electro-chemical theory receives its true physical interpretation, frictional electricity may be simultaneously explained. Perhaps, however, I had better briefly state my present views, so far as I think it worth while, and so prevent it from being supposed that I regard the symbols as fully representing the condition of the particles. In the first place, then, I regard it as very probable that the particles have no electric poles, as I think poles are usually understood, but that the two electricities are distributed in the form of concentric layers. Also, as I can neither regard electricity as consisting of two fluids, nor acquiesce in Franklin's hypothesis, I can only look upon positive electricity as consisting of motion, similar in other respects, but opposite in direction, to negative electricity. From this I conclude, that Davy's *electric* atmospheres really consist of two or more spherical orbs revolving in *some manner* in opposite directions—the neutrality of the particle being a consequence of the *vis viva* of one direction being equal to the *vis viva* of the opposite direction. When two particles run together and form one, as in chemical action, which I have represented by the symbol $\begin{array}{c}+\cdot+\\-\cdot-\end{array}$, I suppose that the plus spherical orbs combine to form one spherical orb or set of orbs, and that the minus orbs also combine; the electro-motive force generated at the instant of combination being due to the *vis viva* of the combined electric atmospheres of the particles, being less than before combination. A good illustration of this change of *vis viva* seems to be afforded in the mingling of streams of air, having similar directions but unequal velocities. With respect to cohesion, where the particles must be side by side, the orbits of the electric forces cannot be spherically disposed; and, fixing attention on any interior particle, it would seem that the plus electricity of this particle must combine with the plus electricity of a particle on one side, and the minus electricity with the minus electricity of another particle on the other side.—Dec. 10, 1851.

[The electrified plates appear to us to illustrate the present electro-chemical theory rather than Mr. Phillips's modification of it. The plates are unipolar, whereas his electrified particles are bipolar.—Ed.]

Let now hydrochloric acid be submitted to electrolysis, as can easily be done by electrolysing its aqueous solution. A line of particles of hydrochloric acid extended between platinum electrodes may be represented by $\genfrac{}{}{0pt}{}{+\cdot+\ +\cdot+\ +\cdot+}{-\cdot-\ -\cdot-\ -\cdot-}$; and in order that the current may pass, the particle situated at the anode must receive plus electricity, and that of the cathode, minus electricity. On throwing one quantity of plus electricity into a molecule of hydrochloric acid, it becomes $\genfrac{}{}{0pt}{}{+\cdot+}{0\cdot-}$; and on adding another of these quantities, it becomes $\genfrac{}{}{0pt}{}{+\cdot+}{+\cdot-}$; and on adding two quantities of negative electricity to a molecule of hydrochloric acid, it becomes $\genfrac{}{}{0pt}{}{+\cdot-}{-\cdot-}$. If now the particle of chlorine which has become $+\cdot-$, and consequently neutral and uncombined, escapes, together with the hydrogen developed on the other pole, and if we then consider the intermediate particles of hydrogen and chlorine as shifting in the way usually supposed, the series $\genfrac{}{}{0pt}{}{+\cdot+\ +\cdot+\ +\cdot+}{-\cdot-\ -\cdot-\ -\cdot-}$ becomes $\genfrac{}{}{0pt}{}{\quad +\cdot+\ +\cdot+\quad +\cdot-}{+\cdot-\cdot-\cdot-\ -\cdot-}$.
Thus this theory readily applies to electrolysis, and therefore generally to all electro-chemical phænomena. Further, at one period of the electrolysis, a particle of either gas really has, or in some way approaches to, $0\cdot-$ or $0\cdot+$; and this may be the condition of the nascent state, and is intermediate between the combined and free condition of a particle.

The electro-motive force which combining particles possess, must depend on the persistence with which the electric form $\genfrac{}{}{0pt}{}{+\cdot+}{-\cdot-}$ is retained as compared with $\genfrac{}{}{0pt}{}{+\cdot-}{-\cdot+}$. This electro-motive force, and consequently that of chemical affinity, varies excessively, and is sometimes so feeble as to be controlled by simple pneumatic pressure, of which the decomposition of the carbonate of lime by heat, and of steam by red-hot iron, are sufficient examples. And since pneumatic pressure can be measured by cohesion, it follows that cohesion is quite comparable, and in many instances, probably at least equal in intensity with that of chemical force. Cohesion and chemical force are each also molecular forces capable of producing the aggregation of particles—and many other resemblances could no doubt be pointed out. It may therefore perhaps be fairly inferred, that the cohesion of liquids or solids produces some modification of the molecular electricity differing from the arrangement of the electricity in gases. I must, however, go a step further, and assume that this peculiar distribution of the electric force generates cohesion.

The particles of a substance, on approximating each other, may possibly effect a mutual discharge, as in chemical action, and cohere in consequence. As an example, suppose steam to change to ice. A line of particles of steam, $+\cdot-+\cdot-+\cdot-$, on approaching another similar set of particles, may discharge and together become $\dfrac{+\cdot+\quad+\cdot+\quad+\cdot+}{-\cdot-\quad-\cdot-\quad-\cdot-}$. On adding two chains, one on each side of the former two, the molecules may take the form

$$\begin{array}{l} \quad -\cdot-\quad-\cdot-\quad-\cdot- \\ +\cdot+\quad+\cdot+\quad+\cdot+ \\ \quad -\cdot-\quad-\cdot-\quad-\cdot-, \\ +\cdot+\quad+\cdot+\quad+\cdot+ \end{array}$$

the electric force of the two added lines passing through the intervening particles. Other modes of superposition can be imaged; and if the hypothesis is true, it would seem that some others must indeed exist to account for the varieties of crystalline form.

Now, since combination generates heat, and decomposition consumes heat, the same, according to this theory, must take place with cohesion; and thus may be so far explained the loss of heat produced by evaporation, and its reproduction by condensation*.

This hypothesis will also at once apply to the production of heat by the friction of solids, at least in some cases. For all the force expended in tearing the two rubbing surfaces is, at the first separation of the particles, represented by the electricity developed: now if the particles thus separated, or the surfaces to which they are attached, did not during the process of rubbing discharge to each other, clearly all the force expended in friction should appear as electricity. But from the nature of the rubbing process, it is impossible not to suppose that much of the electricities produced do neutralize each other; which thereby, as in the case of chemical combination, produces for the electricity its equivalent of heat.

An attempt was made by Wollaston to apply Davy's electro-chemical theory to the electricity of friction. This view, as expounded by Faraday, consists in supposing that "during the act of rubbing, the particles of opposite kinds must be brought more or less closely together, the few which are most favourably circumstanced being in such close contact as to be short only of that which is consequent upon chemical combination. At such moments they may acquire, by their mutual induction and partial discharge to each other, very exalted opposite states; and when, the moment after, they are by the progress of the rub

* Phil. Mag., S. 4. vol. ii. p. 6.

removed from each other's vicinity, they will retain this state if both bodies be insulators, and exhibit them upon their complete separation*."

It appears to me that this theory cannot be generally true; for by means of it I am unable to see how it comes to pass, that gases being driven on solids produce no electricity. This exception is all the more worthy of attention, inasmuch as gases are known to cohere with great force to some solids, as glass, charcoal and platinum; and probably all solids more or less condense gaseous matter. Now the friction of gases on solids is characterized by this remarkable circumstance, that no permanent abrasion of the solid is produced; and if a particle of the adherent gas becomes torn from the solid, it can be immediately replaced by a similar particle. I conclude from this, that to develope frictional electricity, there must be some permanent mechanical division of one at least of the rubbing surfaces. I will now take water as an example, and endeavour to account for the electricity produced by its friction, by using the foregoing electro-chemical theory.

We know from Prof. Henry's experiments, that the cohesion exerted between particles of water is very great; perhaps at some temperatures the cohesion of water may be even greater than that of ice, for ice is lighter than water. It seems, therefore, reasonable to regard the abrasion of water as quite comparable with the abrasion of a solid. And since the abrasion of water always causes the rubber to become negative, it is necessary to suppose, when a mass of water is in its neutral condition, that the external layer is negative with regard to the next layer. Thus when a drop of water is in the air, the order of the alternations of the molecular layers, reckoned from the surface, is —·— +·+ —·— &c.; but as each of these layers contains more particles than the next below, some at least of the inner particles must be more highly charged than the more external. The possibility of a particle receiving more than one equivalent of electricity, seems proved by the existence of such compounds as peroxides or perchlorides.

Let then

—·— —·— —·—
    +·+ +·+ +·+
—·— —·— —·—
    +·+ +·+ +·+
—·— —·— —·—

stand for a mass of water, the inner molecules containing an excess of plus electricity corresponding to the negative condition of the outer layers. Suppose that by means of a current of steam or air the upper layer of particles becomes removed; at

* Faraday's Researches in Electricity, vol. i. p. 555.

the first instant of their removal they are −·− −·− −·−; but to pass to the uncombined state, they must absorb positive electricity, and become +·− +·− +·−, and consequently the vapour which has removed them becomes negatively electrified. The remaining mass of water at the instant of the removal of the particles becomes

    +·+ +·+ +·+
−·− −·− −·−
    +·+ +·+ +·+ ;
−·− −·− −·−

but if any way is open for the escape of the positive electricity, the molecules may become at the instant of the transmission

    −·− −·− −·−
+·+ +·+ +·+
    −·− −·− −·−,
+·+ +·+ +·+

and finally

    −·− −·− −·−
+·+ +·+ +·+
    −·− −·− −·−,
−·− −·− −·−

by the lower layer giving up its positive charge, since by hypothesis it must be negative; while the interior particles still contain the unrepresented quantity of positive electricity, corresponding to the excess of represented negative electricity. The lower stratum of the molecules is thus considered as resting on a conducting body, as when steam rubs along a wetted tube; and the passage of the plus electricity from the upper to the lower particles of the mass shows how electricity may pass through a conductor, by means of the alternate change of the signs of the particles, being an action very analogous to electrolysis.

The above regards the particles of water as polarized as wholes—and not as containing positive hydrogen and negative oxygen—the cohesion of water being thus considered as similar to the cohesion of a simple substance. It may be as well for me to remark, too, that the before-mentioned lower line of particles, +·+ +·+ +·+, in becoming −·− −·− −·−, yields half of its positive electricity to the mass of water, and the other half to the conductor.

This theory would lead one to expect, that anything which favours the divisibility of water will increase the power with which it developes electricity; and accordingly it is found, that electricity is more abundantly produced by the friction of hot water than by the friction of cold water against air*. But as it

* Phil. Mag., S. 3, vol. xxxvi. p. 507.

may be supposed that the increased effect, which is uniformly observed with hot water, may be only the result of an improved insulation, I take the following from the notes of the experiments, which extract I omitted at the time of publication as not then appearing of much importance.

The stream of water, by flowing along the arm of the tin pipe (108.), produces a breeze through the pipe; and the current of air thus carried forward must convey away the negative electricity, as from the circumstances of the experiment there is no other outlet for the negative electricity. Accordingly, on nearly closing the orifice of the shorter arm of the tin pipe with a bung, the production of the positive electricity is diminished; and so is also the current of air, which, however, continues to escape in some measure from the other end of the pipe. This is precisely similar to what happens with an ordinary electric machine, the prime conductor of which soon ceases to afford much electricity if the negative electricity is retained on the rubber. Now I observed, when cold water was discharged from the fountain, that on closing the shorter arm of the tin pipe, the quantity of positive electricity transmitted to the electrometer was scarcely diminished, but that with hot water the effect was very striking. The only explanation of this is, I think, the following: the quantity of air which passed through the tin pipe being the same under similar circumstances with either hot or cold water, that with the cold water, the air which continued to escape after the bung was inserted, was nearly sufficient to convey away all the negative electricity; while with the hot water so much negative electricity was produced, that the same quantity of air was quite inadequate for its removal.

Perhaps the reason why water is always positive when rubbed on a solid is, that its particles are so much more easily abraded than that of solids, that the abrasion of the solid takes no part in producing the electricity, and consequently the order of the + and − alternations of the solid rubber does not enter into the final result.

Although a stream of air in falling on a solid produces nothing answering to abrasion, yet it is conceivable that a stream of air, in flowing along such a thing as a channel of ice, might strike upon some projecting portions, and thereby produce such a condensation of air about them as to cause their liquefaction; but such cases, if any, obviously belong to the abrasion of a fluid, and may produce electricity. The friction of air in an orifice may be explained in a similar way. The air suffers a condensation in striking on the sides of the orifice; and the heat which is produced by this condensation must be more or less absorbed by the walls of the orifice, thus occasioning a corresponding diminution in the elasticity of the condensed volume of gas; therefore the gaseous particles, after having undergone reflexion from the walls of the tube, possess a lower velocity than that which they previously had.

Dr. Faraday's theory may perhaps be regarded as a particular instance of this more general theory of abrasion. On bringing the two surfaces together, certain particles which are most favourably circumstanced may discharge to each other. No frictional electricity is produced by this operation; but the particles are united by cohesion, and could remain thus combined for an indefinite period without interfering with the subsequent process, which consists in the rending of these two sets of particles from each other, and which, as in other instances of permanent abrasion, developes the electricity. The following appears to be an example of this process, and there are plenty of the same sort. Place melted sealing-wax on a glass plate—this brings the two surfaces so near together that they discharge and cohere; on rending the sealing-wax from the glass, the electricity is developed.

The production of hail cannot generally be unaccompanied with the development of electricity. We know from Dr. Waller's observations, that hail at one period of its formation consists of conglomerations of snow mingled with water. As long, therefore, as the surface remains moist, the particles of water can be torn away by the wind; and it is even probable, from the solidity of the mass, that the abrasion from the surface of a partially frozen hail-stone may be even greater than if the whole mass was water at the same temperature.

7 Prospect Place, Ball's Pond Road,
November 19, 1851.

VII. *On the Heat of Chemical Combination.* *By* Thomas Woods, *M.D.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen, Parsonstown, Dec. 1851.

(13.) HAVING proved *experimentally* in my paper in this Magazine of last October, "*that the decomposition of a compound body occasions as much cold as the combination of its elements originally produced heat,*" I will now premise my theory of the cause of the heat of chemical combination by a few remarks on the Molecular Constitution of Matter.

If we take two equal cubes of any substance, iron for example, one heated to 1000° C., the other at 0°, and place them together, we find that the former contracts and the latter expands until they come exactly to the same condition with respect to the distance between their particles; but we shall also see that the cube which contracted lost more space than was gained by the other. The coefficient of expansion increasing with the temperature, there must be more space lost from 1000° to 500° than gained from 0° to 500°.

If, instead of the colder cube of iron we substitute a cube of *ice,* we find that the ice expands until it reaches its melting-point; it then becomes fluid, and ultimately is converted into vapour; its expansion in the last case being enormously greater than the contraction of the iron which occasions it.

(14.) Now what do we learn from this experiment?

1. That if two bodies unequally heated be placed together, contraction occurs in one, expansion in the other, until their particles are at a common distance, proving that HEAT cannot consist IN MOTION *abstractedly considered*; for the particles of the cooling body move as well as those of the body becoming heated, but in an opposite direction; therefore it would be as correct to say that *heat* is absorbed, as to say it is produced by *motion.*

2. That the theory of *heat* being a subtle fluid gives no explanation of *expansion*; for we see that this fluid should itself expand, as when converting the water into steam it occupies a much greater space than it did in the iron; therefore, although it might be said that heat expands bodies, we should still inquire, what expands the heat?

3. That the nearer the particles of bodies are to each other, the less they require to move to produce a given expansion or contraction in those of another body.

(15.) As far as either of these two bodies, *taken by itself,* is concerned, all we see is, that for every temperature it has a certain volume, a constant volume for a constant temperature; and *as far as our experiments enable us to judge,* any body taken by itself, that is, uninfluenced by others, would always remain at the same temperature, with the same degree of expansion among its particles; for instance, a body will not expand or become hotter without some other body more heated than itself being present; and as far as our *experimental knowledge goes,* a body cannot cool without some colder one taking up the heat. I say, as far as our experimental knowledge goes, for theoretically a body is said to give off its heat independent of the presence of other bodies, and it is taken for granted that heat can be radiated into space and exist uncombined with matter; but we have no *proof* of it. As far then as our *experience,* our *positive* knowledge allows us to go, we must admit that the particles of bodies are placed at certain distances from each other; that they have no power to move themselves, inasmuch as the presence of a second body is always necessary; and hence that *we might altogether dispense with the idea of attractions and repulsions between the particles of matter.*

(16.) But if these "forces" do not exist, if the particles of matter are passive with respect to each other, what is their bond of union? what makes them cohere? A little consideration shows us that no change occurs in one direction only; no change in the conditions of matter or motion takes place without an opposite one being produced at the same time; hence what is called cause and effect, action and reaction, &c., one being always accompanied by the other. However disguised its opposite may be, still for every change an opposite must exist. Grove's "correlation of forces," although not intended to demonstrate this principle, offers many instances of it. We cannot set a body in motion without producing rest or opposite motion in some other body; we cannot elevate a weight without depressing the earth; we cannot warm one body without cooling some other. No matter what is done, reflection will bring us to admit that some opposite change takes place; that, in fact, to produce any result, a certain power must be employed, and this power when analysed is equal and *opposite* to the work done. Now if we apply this first principle to the expansion and contraction of bodies, what need have we of any such powers as attractions and repulsions between the particles? If one body expand, some other should contract; motion in one direction requiring motion in the reverse. Joining this truth therefore with what is palpable, that for all temperatures bodies have a certain fixed volume, or that a relation exists between matter and the space it occupies, and consequently a relation between the volumes of different substances, we see that it is of no consequence how devoid of action the particles of the same bodies may be on each other. Not only they must cohere, but at first sight it would appear impossible to produce expansion or contraction at all; for to compress or expand a piece of iron for instance, not only must an opposite effect be produced in some other body, but all other matter reacts to keep up the relation which should exist between the volumes. But we also see that if by any means we *could* produce contraction, a corresponding expansion *must* result among other bodies.

(17.) As I said above, I believe the only argument in favour of radiation of heat into space, or contraction without corresponding expansion, is that the earth *is said* to radiate. We have unfortunately no means of testing its truth experimentally. We have however an indirect method of proving that bodies do not part with volume without an accompanying increase in some other; for let a gas, suppose, or any other substance, be made to contract by pressure, however small this contraction may be, we find heat or expansion in other bodies is produced. All our powers cannot compress a body beyond the limit where other bodies take up the volume it loses; and if we fail to cause a body to contract without this corresponding expansion, is it probable a body can of itself part with its heat or become smaller, independent of the expansion that is generated when pressure is used?

(18.) The liquefaction of the gases by Faraday will illustrate my meaning practically. When pressure was first applied to some of the gases in order to liquefy them, no other effect than a diminution of volume to a comparatively small extent was produced, the solid bodies surrounding the gas being capable of taking up the volume lost by it only to a limited extent. Solid carbonic acid and æther being now placed in the vicinity of the gas, and their volume at the temperature employed, or when gaseous, being so enormous compared with the state of solidity, expansion to an immense extent went on, the corresponding contraction being supplied by the gas under pressure, and consequently, *both changes having been provided for*, success was obtained. This experiment proves,—1st, that the contraction of the gas could not go on without the corresponding expansion; 2nd, that the carbonic acid in expanding took away a like amount of contraction; and 3rd, that as both expansion and contraction require an opposite change to be going on at the same time, they cannot be due either to attraction or repulsion.

(19.) Just to fix my ideas more firmly in the mind, I will say, I do not believe the nebular hypothesis to be correct. It assumes the nebulæ to be in a state of vapour or mist, gradually contracting to form stars. Now if no other body sufficiently large be expanding so as to produce the opposite effect, it is contrary to all our other *experience* if such contraction take place.

I believe our atmosphere is limited in extent, as *it*, as well as all other bodies, has a *certain volume*, just as I believe steam can expand to a *limited* distance if relieved from all pressure, this distance corresponding with the amount of contraction suffered by the body which heated it.

And I think, if we admit that the particles of matter only move as they are influenced by, or rather as they are accompanied with, an opposite movement, and not by any force exerted between themselves, we have a beautiful proof, not only of the stability of our system, but that the arrangements of matter never could have exceeded their present limits; that no meeting by chance of atoms in space could occur; that all *power*, as well as all *matter*, has been supplied by the existing state of things, inasmuch as nothing can be accomplished without its opposite. Nothing therefore, neither force nor matter, can be added or taken away; everything seems to have come literally "finished" from the hands of the Creator. I do not speak theologically; but, philosophically considered, the molecular constitution of matter proves that the present arrangement of things must have existed since their formation.

(20.) I do not think then that it is necessary to suppose an attraction exists between the particles of bodies to account for their coherence; and as to repulsion, to account for the dilatation of a gas when pressure is removed, I think it not only unnecessary, but inconsistent when applied to different cases. For instance, it is generally imagined repulsion is the only force operating in gaseous bodies, whether this repulsion is attributed to that force existing in the particles of matter themselves in the state of gas, or resulting from the repulsive power of the supposed subtle fluid, heat. When, however, a cold body is placed in contact with steam, the steam is condensed into water. How could such a change take place according to the usually received theory? Suppose even that the cold body abstracted the heat, what is to bring the particles of steam together? There is no attraction; in fact there is nothing to account for it. If, however, we attend to the circumstance, that as much expansion as is lost by the steam is taken up by the iron, that is, relatively to the space they occupy, we can at once see that at the temperature attained by the two bodies when in contact, each possesses a definite bulk, and that the one supplies the opposite movement to enable the other to gain this certain volume. But the subject of attractions and repulsions is admittedly full of inconsistencies; the theory, or rather the observed facts which I bring forward, show they are not necessary. I offer no theory to account for the cause of contraction and expansion requiring to be accompanied by each other, or why they are present in *different* bodies. It is a final cause, and as such, I believe, can never be explained; and the less we add hypothetical fluids and forces to the phænomena we witness, the better I believe we shall understand what takes place. I will just mention, that in the Supplementary Number of this Journal for June last, a paper from Mr. Rankine shows that "the compressibility of water varies according to the same law with that of a gas;" and this affords, when properly considered, a reason that we should not attribute repulsion to the particles of a gas, if not to water.

(20*.) If pressure be removed from a gas, it expands for the same reason that a cold body expands in the vicinity of a hot one, viz. to attain the relation that must exist between its volume and that of surrounding bodies; and as the hot body contracts in proportion to the expansion of the cold one, so do the surrounding bodies contract in proportion to the expansion of the gas. When the two bodies are solid, the *contraction* of the hot one being the more apparent effect, the changes are ascribed to ATTRACTION; but when one body is gaseous, *expansion* being the more perceptible action, the changes are said to be owing to REPULSION. These changes, however, manifestly result from the same cause in both cases—the law that relative volumes must be attained, and opposite movements simultaneously exist.

(21.) I showed in (14.) that the nearer the particles of a body are to each other, the greater expansion or contraction do they compensate in other bodies by a reverse movement; so that if we could compress iron, suppose at 32° F., ever so little, it would occasion a great increase in the bulk of a gas. We also saw, that the further out the particles were removed, the greater the distance they afterwards moved for the same amount of contraction in another body; or, in other words, that the volume gained by any body in compensating a contraction depended on the volume it already possesses. Let us see:—does this rule hold good in all the expansions of bodies?

(22.) *All liquids in becoming gases expand inversely as their atomic volume, or some multiple of it.*—This law I thought I discovered, but found on consideration it resulted as a matter of course from the equality of combining volumes of gases; for if the volumes of the combining proportion of two gases are equal, their expansion from their liquid state must be inversely as their *liquid* volume. The smaller the one was in a liquid state, the greater its expansion to make it equal to the other in the gaseous. But the smaller the atom is, the greater the space for equal sizes; therefore the greater the space between the particles, the greater the expansion when a liquid becomes a gas. We saw the same law in the expansion of solids (14.).

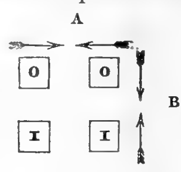
(23.) When a solid becomes a liquid, it is not so easy to tell the amount of expansion on account of crystallization, &c. influencing the result. But what is called the latent heat, that is, the volume of expansion necessary for the change of state, reveals the distance the particles separate; and Person has shown (*Comptes Rendus,* vol. xxiii. p. 162) that this depends on the distance of the fusing-point from a certain number of degrees below zero; or in other words, the higher the fusing-point, the greater the expansion: the amount of expansion when a solid becomes a liquid depends therefore on the degree of expansion existing already in it. Person has also shown (*Comptes Rendus,* vol. xxiii. pp. 327, 524), that the researches of Favre and Silberman prove that the latent heat of vapours is as their expansion. These two paragraphs, (22.) and (23.), show that the expansion into liquids or gases follows the same law as the expansion of a solid.

(24.) Another law connected with the expansion of liquids and solids into gases, and one which has hitherto been unnoticed, is that *the amount of expansion of compound bodies, when changing from one state to another, as from a liquid to a gas, is determined by* ONE *of the elements only.* For instance, the atomic volume of the vapour of water is $\frac{9}{\cdot 622}$ = say 14: here there is only one proportion of oxygen. The vapour of æther has for its atomic volume $\frac{37}{\cdot 258}$ = say 14, and in it there is only one proportion of oxygen also. Now alcohol has *two* proportions of oxygen, and the volume of the combining atom is $\frac{46}{\cdot 160}$ = say 28, or twice 14, showing twice the expansion that occurred in the other cases. In methylic æther and acetone there is only *one* proportion of oxygen, and their atomic volume is expressed by 14; but in pyroxylic spirit, and all compounds having more than one proportion, notwithstanding the diversity of their composition, the atomic volume is 28. In the same way, the mercury in calomel is twice the mercury in the bichloride, and the atomic volume of the vapour of the former is twice that of the latter, the chlorine not influencing the result: the atomic volume of vapour of calomel is $\frac{2360}{820}$ = say 28, that of bichloride of mercury $\frac{1360}{944}$ = say 14. In nitrous oxide and nitric oxide the same circumstance is observed: indeed in all cases of expansion of solids or liquids into gases this law holds good. It is an interesting subject of inquiry, whether the same law influences the expansion of solids and liquids when not changing their state. It would anticipate the result of some investigations I am making on the change of solids and liquids into gas to say more at present than what is contained in this paragraph (24.). However, enough has been said to show, by combining it with (22.), (23), &c., that *bodies when expanding or contracting gain and lose definite volumes depending on their previous state; that the expansions and contractions of different bodies which always accompany each other have a fixed relation,* or in other words, *the same amount of expansion or contraction in one body from the same state is always accompanied by a fixed amount of opposite motion in another; and that in compound bodies, in cases where the expansions and contractions are best marked, as in changing their states, these expansions and contractions depend for their amount on only* ONE *of the elements.*

(25.) Now before applying these principles to the explanation of the heat of chemical combination, I will notice that, even in what is called attraction of gravitation, equal and opposite motion exists—in this case between the masses, as in what is called the attraction of cohesion between the particles. I allude to it here to speak of a circumstance to which I will refer by and by.

It is that the limit of the approach of the masses is determined by the state of expansion among the particles; or, if two bodies are heated, their particles expand, and, when in approximation, they recede from each other. Many experiments show this fact. The most elegant is that of Dr. Powell's with the two pressed-together pieces of convex glass, in which the alteration of Newton's rings by heat shows the recession of the glasses from each other.

(26.) The following, then, is the theory I offer to account for the heat or expansion produced when bodies chemically combine. Seeing that the approximation of any particles is always attended by an expansion in others, and *vice versâ,* it might almost be said the two opposite movements being one and the same,—seeing, too, that chemical combinations produce expansions in bodies, and chemical decompositions the reverse (12.), am I not justified in saying *that the heat of chemical combination, or the expansions it occasions in other bodies, results as a matter of course from the approximating of the particles which form the compound body?* just as the approximation of the particles of one piece of iron causes those of another to separate. The accompanying diagram will explain my meaning. Let O and O be two particles of oxygen, and I and I two particles of iron. We know that when O and O, or I and I, move in the direction of the arrows at A, that is, when iron or oxygen contracts, expansion or heat in other bodies is the consequence. Now why not extend the same principle to the case where O and I approximate, or the particles move in the direction of the arrows at B? I must remark, that as two pieces of iron at sensible distances coming together do not cause heat or expansion in other bodies, so the oxygen and iron require to form one body before the coming togther of their particles produces a like effect. Bodies may be mechanically mixed, and not give rise to heat or expansion; but the instant they form one body, or are at insensible distances, then they act, as far as the movement of their particles is concerned, exactly as if the one body they form were composed of *like* particles.



(27.) The heat of chemical combination is thus looked on as produced exactly in the same way that heat is produced by a simple body whose temperature is higher than surrounding ones; in the latter case the particles come together just as they do when different particles combine in chemical union, but not to the same extent, consequently do not give rise to the same amount of heat or expansion in other bodies (14.). The reason why the particles come together when a chemical compound is formed has nothing to do with our present inquiry, as we are only looking for the cause of the *heat* produced in chemical combination, not for the cause of the combination itself. I have no doubt, however, but that the clue to the proper understanding of chemical combination lies, not in any attractions and repulsions of particles, but in the preservation of the balance between the distances of these particles of which I have spoken above. And if we can extend the laws that regulate the phænomena observed among simple bodies to their combinations, we have, I think, gained much. We divest chemically combining particles of all mysterious influences, such as caloric, investing atmospheres of electro-negative and electro-positive fluids, &c., and look on them only as particles of matter influenced by external circumstances, and seeing the cause of their adherence, not in attraction, but in the closeness with which they are placed to each other, and the absence of an equal and opposite movement among other particles which should accompany their separation.

(28.) The expansion among the particles of some bodies when combination takes place, such as in the formation of carbonic acid, the explosion of gunpowder, &c., would seem to show that *expansion* is sometimes accompanied with *heat*; but this expansion is effected, not between the bodies combining, as between the oxygen and carbon, but between the compound particles, as between those of the carbonic acid. And this expansion is not without its effect—*it produces cold*; for the heat produced by combination is not so great when the resulting compound is a gas or liquid, as when it is a solid. For instance, when oxygen combines with hydrogen and forms water, the heat produced amounts to 43 units; but when it unites with zinc, the oxide of zinc being a solid, the heat amounts to 53 units. When oxygen and phosphorus combine and form a solid compound, the heat evolved is nearly twice as much as when they give rise to a gaseous one. The difference, however, does not, I think, entirely result from the distances between the compound particles of the two oxides of phosphorus being unequal, but, as I have shown (25.), that the amount of expansion between the masses and particles of which they are composed react on each, or determine each other's limits; so if the distance between the particles of the compound, water, be greater than that between the particles of oxide of zinc, may it not cause the particles of the oxygen and hydrogen to be further apart than the particles of the oxygen and zinc, and consequently be productive of less expansion or less heat in other bodies?

(29.) My theory of the molecular constitution of matter, and assimilation of the expansions and contractions of bodies, with chemical combination, from which I make them only differ in degree, and that in the latter case the contractions take place between *different* bodies, leads to Berthollet's view of chemical action, viz. that affinity depends on external circumstances. It would be needless to quote the many instances by which this idea is verified—instances in which the power to combine between substances is altogether destroyed or heightened, according to the circumstances in which they are placed. It will be found that in proportion as the two opposite effects are provided for, so will bodies combine, just as condensation or expansion can be accomplished, as the reverse is made easy. The objection that, because chemical action changes the properties of bodies, it cannot be a mere mechanical one, does not, I think, hold good. An acid and alkali combining neutralize each other's previous effect, and an innocuous compound results; but their properties are not changed. If we take a certain quantity of steam and of ice, one will scald, the other freeze—one is solid, the other gaseous. When mixed, the compound is innocuous, and neither solid nor gaseous; yet it must be admitted that it is a mechanical mixture.

(30.) I would remark, before summing up, that not only does the approximation of *diverse* particles, as in chemical combination, assimilate in the effect it produces, the contraction of *like* particles in the same body, but that, as we noticed in (24.), the amount of expansion or contraction in certain cases depended on ONE of the elements only, so in chemical combinations *one* element determines the amount of heat or expansion also; for instance, oxygen uniting with several combustibles gives the same amount of heat; the same base always produces the same quantity of heat, no matter how different the body may be with which it unites; these analogies between the conduct of the particles of the same body, and that of particles of a diverse nature, adding to the proof that the theory I advance to account for the heat of chemical combination is the true one.

(31.) I have endeavoured to condense as much as possible into a reasonable limit, but I am afraid I have taken up too much space, I will therefore sum up briefly what I think I have proved.

That there is a balance between the distances of the particles of all matter, these distances being different for different bodies.

That to preserve this balance, motion among the particles of one body cannot be effected without a relatively equal and opposite one in some other.

That in contractions and expansions, as the volume gained or lost has a relation to the volume already possessed by the body, an extensive movement among particles far apart compensates a small one among those which are close together.

That, therefore, when particles, though of an opposite nature, come together in chemical combination, the particles of other bodies must expand to supply the opposite effect; and when these chemically combined particles separate in decomposition, a contrary movement, or contraction, or cold is produced.

That these propositions having been proved, show that the heat produced by chemical combination, or the expansion occasioned by it among the particles of bodies, differs in nothing from that occasioned by the contraction of *like* particles; that in both cases the resulting heat or expansion in other bodies is merely the necessary effect, or rather accompaniment of the contraction going on in the combining or contracting ones.

And that the analogy between the approximating of particles in chemical combination, and that of those of a cooling body, extends also to other particulars.

VIII. *On the Cause of the Aberration of Light.* *By* Professor Challis.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

In page 568 of the Supplementary Number of the Philosophical Magazine for December, the following passage occurs in an extract from the *Comptes Rendus* of the French Academy for September 29, 1851:—"Many hypotheses have been proposed to account for the phænomena of aberration in accordance with the doctrine of undulations. Fresnel, in the first instance, and more recently Doppler, Stokes, Challis, and many others, have published memoirs on this important subject; but it does not seem that any of the theories proposed have received the entire assent of physicists. In fact, the want of any definite ideas as to the properties of the luminous æther and its relations to ponderable matter, has rendered it necessary to form hypotheses," &c. As it might be supposed from this statement, that, in common with others, I had attempted to account for the aberration of light on certain hypotheses respecting the æther, I beg permission to say a few words for the purpose of correcting such a misapprehension.

In what I have written on aberration, I have expressly maintained that the phænomenon may be explained by known facts, without making any hypothesis whatever, and independently of any theory of light. As all that is essential in the explanation I have given on these principles admits of being condensed within a very small compass, and as by exhibiting it I shall best attain the object of this Note, I propose to reproduce it here, after making one preliminary remark. The first attempts to explain aberration took account of the course of the ray, and only *one* point on its course which partakes of the earth's motion, namely, the eye of the spectator. The explanation I have proposed takes account of *two* such points. This addition, however simple it may appear, removes all the obscurity attaching to the original explanations. The two points selected were the eye of the spectator, or rather the point of the eye through which the axes of all rays pass, and the point where the course of the ray intersects the wire of the telescope. It would answer the purpose equally well, and would perhaps be more distinct, to select with the latter point the optical centre of the object-glass, through which the axes of all rays incident upon the object-glass from a star necessarily pass.

Let O be the position *in space* of the optical centre of the object-glass at the instant when light from a star passes through it, and let W be the position *in space* of the point of the wire on which the same portion of light impinges. As it is a known fact that light near the earth's surface travels through small spaces sensibly in straight lines, the straight line joining O and W is the course of the ray in space. As it is also a known fact that light occupies time in passing from one point of space to another, the optical centre of the object-glass is carried by the earth's motion to some position ϖ, during the transit of the light from O to W. Now the instrument to which the telescope is attached *necessarily* determines the direction of the ray to be the straight line joining the two points ϖ and W, through which it is known that the ray has passed. For these are points of the instrument; and that which the instrument performs is, to determine the direction of the line joining these points with reference to certain fixed directions. But the course of the ray in space is from O to W. The angle $OW\varpi$ is aberration. The constant of aberration is the ratio of $O\varpi$ to OW, that is, the ratio of the earth's velocity to the velocity of light. This ratio is known by the theory of the earth's motion about the sun, and by observations of eclipses of Jupiter's satellites. Thus the angle $OW\varpi$ for a given star at a given time may be obtained by calculation and expressed numerically. The same angle may be measured instrumentally. The two values when compared are found to agree as nearly as possible, and thus aberration is accounted for in as complete and satisfactory a manner as can be desired. The explanation is a strict deduction from admitted facts; and the cause assigned for aberration, being a *vera causa*, admits of no dispute.

At the same time that I maintain the above to be the explanation of aberration, I am fully sensible of the value of an experiment, such as that of M. Fizeau, which appears to demonstrate that the motion of bodies alters the velocity with which light propagates itself in their interior. This fact must necessarily be of great importance with regard to the undulatory theory of light, but appears to me to be in no way inconsistent with the preceding account of aberration.

I am, Gentlemen,

Your obedient Servant,

J. Challis.

Cambridge Observatory,
Dec 22, 1851.

IX. *Explanation of an Optical Illusion.* *By* Sir David Brewster, *K.H., F.R.S., & V.P.R.S. Edin.**

ABOUT eleven years ago I received from a correspondent well-instructed in optics, a letter informing me that he had composed an essay "On the seeming anomalies which take place in the vision of persons whose eyes are either long-sighted or short-sighted, and which, though they are most truly surprising, have hitherto, as far as I can find, escaped observation."

"The leading fact," he adds, "which gave rise to the composition of this essay is as follows:—

"If a silhouette or black profile of the human face (the features of which should be very little prominent) looking from RIGHT TO LEFT be placed against a window, and be viewed by a short-sighted eye through a narrow slit (about the thirtieth of an inch wide) in a sheet of black cardboard placed at some distance (about a foot or so) from the window, while the spectator himself is at about the same distance from the slit, *that silhouette will be seen by that short-sighted eye looking from* LEFT TO RIGHT."

After an elaborate investigation of the progress of the rays before reaching the retina, founded upon the known structure of the eye, he deduces a fundamental proposition, "from which, by means of a train of demonstrations (which he has not given) he shows—

"1. If a silhouette is fixed in a window looking from *left to right,* and a short-sighted spectator stands with one eye shut at some distance (two feet or so) from it, and moves a piece of black cardboard with a smooth edge, at about two-thirds of the way between him and the window, from RIGHT TO LEFT, then as the left edge of that piece of cardboard approaches the silhouette, a *second or phantomic silhouette* will appear in that left edge looking from RIGHT TO LEFT, so that the two silhouettes will look each other in the face; and this astonishing appearance takes place accordingly."

My correspondent deduces from his train of demonstrations other two results, in one of which the cardboard and the silhouette are made to change places, and are held at greater distances than before. In this case "the phantom silhouette appears to come out behind the real silhouette, and to look in the same direction." In the third case analogous phænomena are seen by long-sighted persons, the distances of the card and the silhouette being varied.

Not having seen the demonstrations above referred to, I cannot of course state that they contain an explanation of these apparently very extraordinary phænomena; but upon carefully repeating the experiments, I soon saw that the *phantom silhouettes*

* Communicated by the Author.

had a much simpler origin than my correspondent supposed; that they were not images derived optically from the real silhouettes, but were only phænomena arising from the union of penumbral shadows of bodies held at different distances from the eye, and seen indistinctly by persons with every variety of sight.

When a finger of each hand, A and B, is held up near the eye against a luminous ground, so that the nearest A does not eclipse the remotest B, and that both are seen very indistinctly with a penumbral shadow, then if we bring the two fingers together, the most prominent part of B seems to *swell outwards* till it meets A. Now if we suppose the edge of the finger A to be a silhouette, or a notched line like a saw, then it is obvious that when the straight edge of B comes near it, a part of this edge opposite the nose of the profile will swell out and meet it, whereas the notch below the nose will not. In like manner the parts opposite the two projecting lips of the chin will swell out till the straight edge of the finger has prominences exactly like those of the silhouette or profile, and we shall have the appearance of two silhouettes, a phantom one and a real one, looking at each other.

If we next suppose the edge of the finger B to be a silhouette, the projecting parts of the profile will swell out when they approach the edge of the finger A, and a phantom silhouette will thus appear to rise out of the other, and looking in the same direction as described by my correspondent.

In some long-sighted eyes, where the crystalline lens has begun to decay, and to give slightly double images of objects, the phantom silhouette emerging from the real one will seem to be slightly separated from it; but in good eyes the prominence of the projecting features is merely increased, that is, the nose, the two lips, the chin of the former swell out, and give the appearance of a phantom silhouette coming out of the real one.

All these phænomena may be produced merely by receiving upon a white ground the shadows of a silhouette and a straight edge, so that they have penumbras analogous to those which they have when seen directly by the eye.

As there can be no doubt that the phantom silhouettes are produced by the *swelling* of the penumbral shadows, in the manner I have described, we have only now to refer the reader to an explanation of the cause of this *swelling.*

The swelling of shadows seems to have been long ago observed by optical writers, and was erroneously ascribed by some to the inflexion of light. Our countryman Mr. Melville, however, nearly 100 years ago, gave the true explanation of it*, which is

* Essays and Observations, Physical and Literary. 3 vols. Edinburgh, 1754.

so exceedingly simple that no optical knowledge is required to understand it. Dr. Priestley has reprinted Mr. Melville's explanation, with the necessary diagram, in his History of Vision, Light and Colours*, and we therefore refer the reader to either of these works.

When I received from my correspondent, then living on the Continent, and whose name I do not feel myself at liberty to mention, his account of the phantom silhouettes, I failed entirely in producing them. I resumed the subject more than once with the same want of success, and from this cause I believe I did not take any notice of his communication. If he has published his essay on the subject, I regret that I have not been able to find it; if he has not, and if this notice should meet his eye, I trust he will enable me to mention his name, and give him the credit of having first discovered the phænomena to which I have called the attention of the reader.

St. Leonard's College, St. Andrews,
December 2, 1851.

X. *Notices respecting New Books.*

Elementary Physics; *an Introduction to the Study of Natural Philosophy. By* Robert Hunt, *Professor of Mechanical Science in the Government School of Mines, &c.* Reeve and Benham. 1851.

THIS book has been undertaken with the laudable intention of placing clearly before the reader all the great deductions of physical science without the introduction of mathematics. Those who read the preface and read the book, will be able to say whether the hope held out by the former has been realized. Our own experiment in this way is far from satisfactory. During the perusal of the work, the total absence of scientific precision is constantly suggested; very little attention is paid to the definition of terms, and just as little to the natural order of thought. A certain unscientific forgetfulness is often evinced; where, for example, the author speaks of "*this* law," and when you inquire, "what law?" you find him vanishing in the haze of his own speculations. Thus at page 58 we read, "by squaring the number of seconds, and multiplying the product by $16\frac{1}{2}$ feet—a close approximation to the *truth*—we have the height or the depth required." We can fancy the youthful reader, longing for intellectual sustenance, demanding here, "*what truth?*" Mr. Hunt does not inform him; nay, he is worse than silent, for in a foregoing page he has thrown him off the scent by assuming that the distance through which a body is drawn by the force of gravity in one second is 15 feet. A man may understand a subject, and still be blessed with no faculty of expression; and the book before us demonstrates the converse of this proposition—that a clear under-

* Vol. ii. p. 725, fig. 163.

standing is by no means the invariable accompaniment of the pen of a ready writer.

Mr. Hunt informs us that "if we cut a cone perpendicularly to the base, the section is a triangle." It is so in one case only, and there are a million other cases where the section is not a triangle. The definitions are obscure; take, for instance, the following:—"The centre of gravity is nothing more than the central point of parallel or equal forces." In page 72 reference is made to the leaning towers of Pisa and Bologna; what the lines *cd* and *ab*, referred to by Mr. Hunt, have to do with the matter we are at a loss to conceive; they are not the lines of direction, for neither of them passes through the centre of gravity. In page 85 Mr. Hunt speaks "of the *centre of gravity* being no longer *at right angles* to the plane;" and in page 86 he says, "The gravity of the body is decomposed into two forces, one drawing it to the earth, *acting at right angles to the plane*, and causing the pressure, the other acting parallel to the inclined plane and forcing the weight down it!" It would be useless to comment on the infraction of the first principles of mechanics and of common sense involved in these quotations. Mr. Hunt's definition of centrifugal force is incorrect, being the definition of quite another force; and when, in connexion with this subject, he speaks of "an impulsive force exactly balanced against a statical power, a system of harmony being the result," we confess our inability to understand him.

In page 121 it is stated, "that fluids issuing from orifices have a velocity proportional to the height of the surface of the fluid above the orifice." This is incorrect; the velocities are proportional to the square roots of the heights. The example in page 69 is wrong. Did Mr. Hunt practically test Bunsen's cells before he condemned them? An experience of many years enables us to state that our author's animadversions on this admirable invention are wholly groundless. If the zinc collars are attacked as stated, it is the fault of the experimenter, not of the battery. For the sake of those who possess cells of Bunsen's construction we may remark, that the chief point to be secured is a good metallic contact between the coal cylinder and its encompassing ring; the interior of the latter must be rendered clean and bright by the application of a little sand and dilute hydrochloric acid.

In page 293 we are informed, that "if we hang at the end of a magnet a weight which is nearly as much as it will support, and then bring another magnet near to the end to which the weight is attached, it will fall off." This is true if the poles are of opposite qualities, but false if the poles are similar; in the latter case the weight will cling with increased intensity. In page 336 our author writes:—"We speak of free caloric, and mean thereby the circumstance of heat becoming *sensible*, as when diffusing itself in its tendency towards an equilibrium through all surrounding bodies. When an equilibrium is restored, and all neighbouring bodies are at an equal temperature, the agency is said to be *latent*, or in a state of repose." The evident security with which this extraordinary annotation on the celebrated discovery of Black is advanced is most amusing. Speaking of the polarization of heat, our author remarks, "It may be described, in general terms, as a power of turning the ray of heat half round; and it is regarded as proving that the influence of lateral vibrations *are* different from the onward waves in calorific propulsion." If Mr. Hunt were compelled to write this nonsense, we should pity him; but it is his own free act, and we are therefore disposed to be angry with him. What follows, however, is still worse. In page 382 we are told, "In the centre of the cornea is a circular opening, the pupil, and within it is the crystalline lens containing the vitreous humour." We entreat Mr. Hunt to think once more. *Is* there an opening in the cornea? *Does* the crystalline lens contain the vitreous humour? With a very slight expenditure of trouble, Mr. Hunt might have informed himself on this important subject, and thus spared us the pain of exposing his reckless inaccuracy. He has only to look into the eyes of his neighbour, or his cat, to convince him of at least a portion of his error.

In page 391 we find the following:—"A very simple contrivance, by which the relative illuminating powers may be ascertained, is to allow the shadows from a single object to fall upon a screen, and then remove the sources of light from or towards it until all the shadows are of the same depth; the distances at which the illuminating bodies are from the screen express the relative intensity of the light of each." Count Rumford would never agree to this; he would have said the *squares* of the distances. In the next page we find the following piece of information:—"By *aberration* is meant the difference between the real and the apparent place of the stars. As the light is proceeding from them, the earth is moving onward; consequently they appear to be rather more *backward* than they really are in the direction of the earth's annual motion." This passage is suggestive of the scientific standing of its writer; it is the production of an amateur, who abides by first impressions, and gives himself no further trouble. The effect of aberration is precisely the reverse of what our author states it to be. The star from which the ray comes appears more *forward* than it really is in the direction of the earth's annual motion.

These are not superficial errors which might be attributed to imprudent haste; they are cases in which general vagueness, confusion, and ignorance of the subject handled, blossom out into palpable absurdity. Did the book contain excellences, we should be glad to bring them forward; but it is obscure and unphilosophic throughout. Even a quotation, in Mr. Hunt's hands, is not safe; his statement of the law of Newton, for example, in page 46, puts that law in a dubious light. Had we not known something of the subject beforehand, we should have derived but little enlightenment from the following:—"The distance from the middle point of a magnet being the same, the force opposite the poles, or in the direction of the axis, is double the force in the magnetic equator." .... "In all cases the south pole of a magnet will be found weaker than the north pole!" .... "All bodies, such as glass, which allow magnetism to permeate them freely, are called diamagnetic, from *dia* a way; and hence all bodies in nature are now grouped under the two classes of magnetic and diamagnetic." Not "hence," Mr. Hunt; you are confusing terms. The discoverer of diamagnetism happens to entertain the precisely opposite view, as to the permeability of diamagnetic bodies. Hammering does not render brass magnetic, as stated by Mr. Hunt; the magnetism is borrowed from the iron tools used in the operation. "If a source of light," says Mr. Hunt, "be placed in the focus of a concave mirror, there will be no image, but a brilliant reflexion in parallel lines from *every point* on its surface." In one case only is this correct, and that is when the mirror is parabolic; but Mr. Hunt is here speaking of "curved mirrors" without limitation. Again, we are informed, "The image seen in a concave mirror is always magnified, whereas in a convex one it is *very considerably* reduced." Why "very considerably?" The reduction may be infinitesimal if the radius of the mirror be only long enough; where the radius is infinite, there is no reduction at all. The rainbow is thus explained by Mr. Hunt: "A ray of light falling upon a drop of rain becomes refracted on entering the first surface; it is reflected from the other surface of this sphere, and thus emerging from a medium point, suffers prismatic refraction; the least refrangible rays, the red, forming the inner portion of the bow; the most refrangible, the violet, its outer edge." Part of this is unintelligible, and part false. What is meant by emerging from a medium point? Mr. Hunt is here speaking of the primary bow; and, if he takes advantage of the next sunny shower, he may inform himself that the disposition of the colours is precisely the reverse of what he states it to be. Indeed, had Mr. Hunt, as a general rule, trusted more to his eyes and less to his fancy, we should have had a better book.

We now close it—not for want of material for further remark, for every page of the book is a comment on the incompetency of its author. We regret to be obliged to state this, but the imperfections of the work would justify still stronger condemnation. A recent review in our contemporary, the Literary Gazette, opens with the following words:—"There is an amazing quantity of bad science floating in society, the result chiefly of pretence and imperfect education." What hope can we have of a better state of things, when we find a man, whose knowledge of the subject is evidently inferior to that of the veriest tyro, exalted, under government sanction, into the position of a teacher of physical science?

The Calculus of Operations. By John Paterson, *A.M.* Albany, 1850. 8vo.

How the author got the two letters which follow his name, or why he lowered his pretensions so far as to adopt them, we do not know. He has been his own teacher, and he is a working printer; the materials of the book are the fruit of his evening leisure, and the setting of the types was done by himself. It does not surprise us that in a country where ordinary education is widely diffused, and the higher manual arts are well paid, a compositor should work at his own book; but we do hold it to be rather a curious phænomenon, that one of the earliest of such books should be an attempt to associate metaphysics with mathematics, or to introduce the former into the latter, to a greater extent than is usually done. The following is a brief description of the character and contents of the work.

It divides mainly into two parts: the first, on the complete explanation of the symbols of algebra; the second, on the relation of successive differential coefficients, considered as standing to each other in the relation of product and power, or effect and cause. The first part may be looked on as an attempt to evolve the contents of Dr. Peacock's algebra (first edition) in a more *à priori* form, and to make them the necessary consequences of a metaphysical view of the fundamental operations. Those who know and profit by the clearness of the *principle* of fluxions (unfortunately, as we think, discarded with its *language*), will look with satisfaction at Mr. Paterson's attempt to reinstate the differential coefficients in their old position of indicating something more than result of algebraic operation. Every reader who can truly profess to understand the subject, must, if he have thought about the progress of his own mind, remember how much he was indebted to the mechanical connexion of the function and its first and second differential coefficients with distance, velocity, and force. Mr. Paterson has endeavoured to put this connexion on a more abstract footing, and to make the laws of algebraic development a consequence of his treatment of it. We doubt if his success is complete; that is, we doubt whether a mathematical proof of Taylor's theorem fairly results: unless, indeed, the author had more in his mind than he has fully made manifest; a reserve which should always be made in matters metaphysical. Be this as it may, we can but express a hope that, both in Europe and America, investigators will attempt to sound the channels which connect the mathematics with the fundamental laws of thought.

Mr. Paterson is evidently well-read in the writings of mental philosophers. He is far too metaphysical for the general run of mathematicians, and *vice versâ*. Nevertheless, as there are always a few to whom such speculations are welcome, and, even where they do not convince, suggestive, and as upon these few mainly depends the advance of mathematics as a *discipline*, we hope that he will find encouragement to proceed, and that we have not seen the last of him.

Four Introductory Lectures delivered at the Government School of Mines and of Science applied to the Arts; Museum of Practical Geology.

The struggle carried on for some years between the *Gymnasialisten* and *Realisten* of Germany has at length found distinct expression in England. Science has found her advocates in the Jermyn Street Institution, who earnestly uphold her claims, and forcibly protest against our present exclusively classical system of education. In 1835, a proposal was made by Sir Henry De la

Beche to take advantage of the opportunity afforded by the Geological Survey for the collection and classification of specimens, the result being that space was granted by Government for the reception of such specimens. As years rolled on the collections increased, additional space was needed, and "finally the necessity of proper accommodation became so pressing, especially after 1845, when the Geological Survey and the Museum of Practical Geology were placed under the same department," that the building in Jermyn Street was erected.

In his Inaugural Discourse, Sir Henry conducts us through the building and describes its stores—its specimens of architectural stones, ceramic products, collections of minerals and fossils, its chemical laboratory, metallurgic and mining departments. Those who have visited Durham Cathedral and other similar edifices in this country, and seen the havoc made of the stone by atmospheric action, will appreciate the importance of a collection for architectural purposes. Had such a collection been open to those who raised the Four Courts in Dublin, the irretrievable ruin of that splendid edifice by atmospheric influence might have been avoided. Our advance in ceramic manufactures is illustrated by the progress made in the transfer of prints to porcelain: a century ago one colour only could be transferred; we can now paint a picture. Referring to the ignorance generally prevailing in mining districts as to the value of minerals, the following striking fact is cited. "Ores regarded only as important for the copper they contained, were raised upon the property of the Duke of Argyll in Scotland. After a time the works were abandoned, as the copper found in the ores was not sufficiently abundant to pay for the cost of obtaining them, and much was thrown aside as not worth dressing. The Duke, impressed with a certain character in the ores, brought specimens to this Institution for analysis; and it was found that they contained 11 per cent. of nickel, a valuable metal, and, as you are aware, extensively employed at this time in different alloys, such as those known as German silver." "Even within these few days," adds the lecturer, "a case has occurred in Devonshire where a field-wall was constructed of grey copper ore, and the breaking of a gate-post led to a knowledge of the fact." Referring to the reclaiming of mud-banks which surround estuaries, the lecturer observes, "The body of water entering and passing out is important; and yet what do we often find done, and done, too, by Act of Parliament? The body of water entering, and consequently passing out, is diminished for the purpose of *reclaiming*, as it is termed, certain mud-banks, often extensive; thousands of tons of water are thus sometimes cut off from performing the work by which they aided in keeping the channel to the sea clear; the bottom of the channel rises, and the port is damaged."

Calling the mineral produce of Great Britain 1, that of Russia is about $\frac{2}{7}$, of Prussia $\frac{1}{9}$, of France $\frac{1}{4}$, of Sweden $\frac{2}{19}$, of Norway $\frac{1}{55}$. A country possessing such vast resources in this respect as Great Britain, might be expected to devote particular attention to this point; but this is not the case. "Although the raw mineral produce of

Great Britain and Ireland is valued at £24,000,000 per annum, or about four-ninths of that of all Europe, there existed until now no means in this country for affording needful instruction to those who thus raise so large an amount of mineral matter; all was left to chance, and the result is well known. Many who can afford it go to other lands to study in the mining schools provided by their governments; some fight through their difficulties at home, becoming valuable and useful men; while the mass of our miners remain uninstructed, except so far as they can pick up practical information from each other in the mines." "Are our miners," demands the lecturer, "less deserving of attention than those of other lands, or are they supposed to be so dull and disinclined to knowledge as not to be capable of profiting as well as the miners of other nations by instruction? Let those who thus believe visit our mining districts, especially such as are metalliferous, where the miner has so often to gain his daily bread by the exercise of his judgement, and they will speedily be undeceived. They will find men as able and willing to profit by instruction as elsewhere in our land. They will see many with powerful minds, who have risen from amid all their difficulties, adding continually and greatly to our stock of practical knowledge, but who would evidently have accomplished far more, if in their early day they had possessed the advantage of starting with the knowledge of the time applicable to their pursuits."

The wants here indicated it is the design of the School of Mines in some measure to supply. It is proposed to instruct by means of the collections, the laboratories, the Mining Record Office, the lectures, and the Geological Survey. It is also purposed to explain by evening lectures to the working men of London,—those really engaged in business, and whose good characters can be vouched for by their employers,—such parts of the collections as may be thought to be usefully interesting to them. There are also indications of movements in this direction in the mineral districts; and it is trusted that those who locally distinguish themselves by the application of their abilities may find in the Government School of Mines, free of cost, the means of still further advancing their own knowledge.

We next take up the Introductory Lecture of Professor Playfair, On the National Importance of studying Abstract Science. This lecture opens the Chemical Course for the present session. Ably and convincingly the writer demonstrates the dependence of practical results upon abstract investigations; proves that discoveries, apparently the most remote and unpromising, have resulted in the most important practical issues; takes us to the gardens of the Luxembourg, and shows us Malus looking at the open window through a crystal of calcareous spar—apparently a most unpractical act, yet one, by the following up of which we are now enabled to pierce the ocean and investigate its rocks and shoals, and which in the hands of Biot has led to the most refined method of ascertaining the quantity of sugar in saccharine solutions; introduces us to Galvani operating upon a dead frog on the iron palisades of Bologna, and shows how the discovery there made has resulted in the electric telegraph, and the manifold applications of the electrotype; exhibits the safety-lamp growing out of the thought of Davy, and chloroform distilling from the brain of Dumas; dwells upon that domestic wonder—a lucifer-match, and shows its progressive development up to its present stage. Schœnbein discovers that cotton, without changing its appearance, becomes more destructive than gunpowder; another chemist finds that it is soluble in æther, and in this state becomes, in the hands of the surgeon, an artificial skin to cover the wounds which it made in its old form. Looms are not now required to make coarse calico fine, for immersion in soda makes it take the form of fine cambric. In the sixteenth century Paris lighted up her streets by fires of pitch and rosin, but to the chemist was reserved the triumph of superseding the clumsy invention by our brilliant coal-gas. These were results unsought for; they are the necessary 'off-shoots,' as the lecturer aptly terms them, of abstract investigation. The necessity of cultivating abstract science is enforced by the fact, that local position no longer gives to nations that superiority which it formerly did. The tendency of things is to make the competition of nations a competition of intellect, and the unfitness and insufficiency of English training for this great race are strongly deprecated. One of the lecturer's remarks in connexion with this portion of his subject has given offence to the *Times* newspaper: "The philosophy of our times does not expend itself in furious discussions on mere scholastic trivialities or unmeaning questions of theology." It is not the irrepressible yearnings of the human heart of which the *Times* speaks that are here aimed at, but it is the over-refining of the human intellect—those 'mumps and measles of the soul' which find material for quarrelling and discussion in objects intrinsically worthless. How many months have passed away since the entire theology of England was in spasms over a crotchet of this character?

A few weeks ago, we happened to converse with a thoroughly practical gentleman on scientific subjects. He spoke of a machine recently applied to the electric telegraph, and in which the electricity was generated by magnetism. The result delighted him, and he praised the genius of its Birmingham discoverer. Before us hung a picture of Faraday—one of those capital prints got up by George Ransome of Ipswich; we looked upon the pale features and massy brow, stamped with a certain energy of conflict, as if nature sometimes required to be forced as well as wooed; but no association existed in the mind of our companion between the picture and the machine. The genius which gave this wonderful expansion to the simple experiment of Arago, and to which all practical applications of magneto-electricity must be traced, as streamlets to their spring, was never once thought of!

We now pass on to the Introductory Lecture of Professor Forbes, on the Relations of Natural History to Geology and the Arts. We felt a strong desire to shake the author by the hand as we read his lecture. There is a force and fire within this man which cannot fail to gather disciples round him. In these pages we have clearness of conception, vigour of utterance, and an intellect which can pierce details, and hold communion with "the great universal thought which pervades the one great creative action." This man is master of his materials, and handles them with the audacity of one who knows his own power; the rocks are plastic in his hand, and the fossils live again. Two grand qualifications are necessary in an educator—the ability to provoke and the ability to instruct; the former, though often forgotten, is the more important of the two. There is merit in the smoothing away of a difficulty, but far greater merit in arousing an amount of force able to cope with and to overcome it. These two qualifications are, if we mistake not, possessed by the man before us. There is life in his sentences which propagates itself to those who read them. Who could resist the following?—"In conducting the business of this class, I look forward to the holding of field excursions, regarding them to be quite as essential as lectures for the instruction of the student, who, to benefit by his studies, must become a practical fossilist, and learn to observe carefully fossils *in situ,* and appreciate on the spot the evidence afforded by their associations. During the progress of our Winter Courses this can be done effectually in the neighbourhood of London, or by means of the facilities of transport afforded by lines of railroad. I trust that before the end of this session a compact band of undaunted investigators, belted, strapped, and bag-bearing, armed with stout hammers and sharp chisels, under the veteran generalship of our Director-in-chief, and officered by my mineral and geological colleagues and myself, will make the rocks shake and yield up their treasures for many a mile around the great metropolis." Such appeals thrill the heart of the student like electric fire, and awaken an ardour which renders his task heroic.

The lecturer assigns a high vocation to the naturalist. "It is not an uncommon fancy to suppose that naturalists are occupied entirely with the naming and describing of the kinds of animals and plants; that provided they can enumerate, in clear though technical language, the characteristics or features of a being submitted to their examinations, usually in the state of a preserved specimen, and, on discovery of the species being one hitherto unnoticed, give it a name by which it may be remembered by their brother naturalists to the end of time, or thereabouts, they have attained all their aim and fulfilled all their ambition. This notion of their offices and duties is a libel. It takes notice of only a fragment of their labours. To name and describe are but to enrol an object with a true spelling and correct definition, in the great dictionary of science. Words in dictionaries are exhibitions of the raw materials out of which literature is made; and species arranged in zoological and botanical systems are orderly and beautiful displays of the raw materials of natural history science. Words may be wasted and species misused. But the study of species, which is the basis of all natural history science, does not take note merely of their external, or even their internal organization. It deals also with their relation to conditions in time and space. It seeks out the epoch of their first appearance, and traces them through their diffusion under favouring, or limitation and extinction under unfavourable influences. It searches for the causes inherent in their organization, by which, of two similar, yet not identical creatures, the one has the power to battle with varied and very different forces, and to maintain a vitality which braves alike the freezing cold of the poles and the feverish warmth of the equator, to spread its individuals over more than half the globe. Whilst the other, distinguished it may be from its congener by some apparently slight and useless difference,—though the mark be an indelible brand by which nature has stamped that member of her flock, and that one only,—is incapable of assuming protean variations, or of enduring even a slight change in the physical conditions under which it first appeared. It enjoys a fleeting existence during a short segment of time, dies out ere it has spread beyond a mere speck on the earth's surface, disappearing never to reappear;—perchance, if it belonged to some primæval fauna, never to become known to man with all his research, unless some bony or shelly frame-work gave consistence to its otherwise perishable substance."

The lecture is full of passages of exceeding force and beauty, and evinces a power of illustration which, without once forsaking the precision of science, is almost poetic. Speaking of the service to be rendered by the naturalist to the designer, the lecturer concludes as follows:—"What is ornamental art but the isolation and embodiment in works of human skill of the beauty that is diffused through all the works of God? And that beauty lies not merely in the bulk of objects, nor on their surface, but is as manifest in every part and atom composing them as in the combined whole. It is in itself composite; the combination, not of lesser, but of minuter beauties. To imitate—to approach—we must attempt a like arrangement, in order to obtain the same exquisite result. And how, except through earnest and scientific study, can we attain the knowledge that shall enable us to discover the pathway leading towards perfection?"

The fourth lecture, on the Importance of cultivating Habits of Observation, was delivered by Professor Hunt, Keeper of Mining Records.

In illustration of his subject, he refers to the observation of Thales, that rubbed amber attracted light bodies; to the discoveries of Galvani and Œrsted; to the attempts made to turn magnetism to account as a motive power; to Faraday's discovery of induced currents; to the discovery of the planet Neptune; to the steam-engine; to the researches of Boutigny on the spheroidal condition of bodies; and to the glass used to protect the plants in the palm-house in Kew Gardens. In his remarks on lightning-conductors, we cannot help thinking that the lecturer, in recoiling from one error, has fallen into another equal and opposite. The author concludes his discourse with the following quotation:—

"Divine Philosophy!
Not harsh and crabbed as dull fools suppose,
But musical as is Apollo's lute,
And a perpetual feast of nectar'd sweets
Where no crude surfeit reigns."

XI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from vol. ii. p. 568.]

Dec. 11, 1851. — THE reading of Dr. Faraday's paper, entitled "Experimental Researches in Electricity. Twenty-eighth Series. On Lines of Magnetic Force; their definite character; and their distribution within a Magnet and through Space," which was commenced on the 27th November, was resumed and concluded.

The author defines a line of magnetic force to be that described by a very small magnetic needle, when it is so moved, in either direction correspondent to its length, as to remain constantly a tangent to the line of motion; or as that along which if a transverse wire be moved in either direction, there is no tendency to the formation of an electric current in the wire, whilst if moved in any other direction there is such a tendency. Such lines are indicated by iron filings sprinkled about a magnet. These lines have a determinate direction; they have opposite qualities in and about this direction, and the forces in any part of them are determinate for a given magnet. They may, as the author thinks, be employed with great advantage to represent the magnetic force as to its nature, condition, direction, and comparative amount; and that in many cases when other representations of the force, as centres of action, will not apply.

The term *line of force*, as defined above, is restricted to mean no more than the condition of the force in a given place, as to *strength* and *direction*; and not to include any idea of the nature of the physical cause of the phenomena: at the same time if reason should arise to think that the physical condition of the force partakes generally of the nature of a current or of a ray, a view which the author inclines to, he sees no objection in the term, any more than to the terms *current* and *ray*, as they are used in considerations regarding electricity and light, because it may accord with such a view.

The *lines of magnetic force*, as defined above, may be recognized either by a magnetic needle or by a moving wire; but the two methods are founded on very different conditions and actions of the magnetic force, and the moving wire appears to have the largest application. Its principle can be applied in places which are inaccessible to the needle, and it can sum up the forces in a given plane or surface at any distance from the central magnet. It has no reference to results of attraction or repulsion, and in some cases is opposed to them; but the author thinks it gives a true view of the disposition of the magnetic powers, and leads, and will lead to a more correct understanding of the nature of the force. For these reasons he advocates its adoption, not to the exclusion of the needle, but in conjunction with it; and proceeds to develope the experimental methods and their results, and first in the case of a bar magnet.

Two bar magnets, each 12 inches long, 1 inch in width, and 0·4 of an inch in thickness, were fixed, side by side, a little apart, with like ends in the same direction, on and parallel to an axis, so that they might act as one bar magnet and be revolved at pleasure about the common axial line. A wire, which entering at one pole was carried along the axis of the magnetic arrangement, was at the centre turned outwards at the equatorial part, and then made to return at a distance outside the magnet to the place from whence it commenced. At times this wire was in three parts; the axial part being one, a radial part extending from the centre to the surface at the equator and there connected with a copper ring surrounding the magnet, being another, and the part from this ring on the outside of the magnet, back to the place of commencement, being the third; and each of these could revolve either separately or in conjunction with the other parts, the electric contact being complete in all the cases, whilst the wire was insulated from the magnet by the covering of silk. The ends of this loop, as it may be called, were connected with a galvanometer, and thus the presence or absence of electric currents ascertained, and their amount measured. Two galvanometers were used; one by Rheinkorf, containing fine wire, and very delicate in its action; the other, constructed by the author, of copper wire 0·2 of an inch thick, passing only once round each needle; this, for abundant currents of low intensity, such as those generated in the moving wire, was found many-fold more delicate in its indications than the former.

The general relations of a moving wire to the magnetic lines of force are then specified, and a reference is made to their discovery and description by the author in the First Series of these Experimental Researches; and the law of the evolution of the induced electrical current is given. Referring to an easy natural standard, it may be said, that if a person in these latitudes, where the lines of force dip 69 degrees, as shown by the dipping-needle, move forward with arms extended, then the direction of an electric current which would tend to be produced in a wire represented by the arms, would be from the right hand through the arms and body to the left.

It will be seen, upon a little consideration, that a wire which touches a regular bar magnet at one end, and is then continued through the air until it touches it again at the equator, if moved once round the magnet, slipping at the equator contact so as to resume its first position at the end of the revolution, will have intersected, *once, all* the lines of force external to the magnet, and neither more nor less, whatever its course through the air, or distance in parts from the magnet, may be. Now when the external part of the loop above described is moved in this manner a certain number of degrees round the axis of the magnet, the latter being still, a current of electricity in a given direction is shown by the galvanometer; and the proper precautions (which are described) being taken, the current is of the same amount for the same number of degrees of revolution, whether the motion be quicker or slower, or whether the wire be at a greater or a less distance in its course from the magnet.

If the external part of the loop be retained fixed, as also the axial part, and the magnet with the short radial part of the wire be revolved, an electric current is again produced, of a strength exactly equal to the former for the same number of degrees of revolution; but its *direction* is the reverse of the first current, when the direction of revolution is the same. In either case, reversing the direction of the revolution reverses the current produced by it. The moving radial part of the wire is in this case insulated from the magnet, and many other experiments, as with discs at the ends of the magnet, show, that the motion of the magnet itself is indifferent; and that whether it revolve or is still, provided the wire move, the result is the same. When the radial wire or part of the loop, and the external part move together, then their effects exactly neutralize each other, as they ought to do, being in contrary directions, for the same revolution; and not the slightest trace of a current under the extremest conditions of motion, or of the experiment, can be perceived. Such is the case, whatever the course or distance of the external part of the loop may be, or even when the loop is altogether external to the magnet, but moving at the same angular velocity either with or around it.

When the axial part of the loop is revolved it produces no effect; neither if this part revolve or be still does it produce the least influence on any of the results already described; it acts simply as a conductor, and is in other respects perfectly indifferent. This axial wire may be replaced by the magnet itself; for when it exists only from the magnetic pole outwards, and when the radial wire has contact with the magnets at the centre, so as to complete the electric circuit, the results are exactly the same as before: or the axial wire may proceed to the centre and then make contact with the magnet, and the radial wire be removed; when precisely the same results occur: or both axial and radial parts may be removed, the magnet serving both for conductor and moving radius, and still the results are unchanged.

From such results as these, the author draws the following conclusions, in relation to the *lines of magnetic force* as defined at the commencement. The amount of magnetic force (as shown by the electric current evolved) is determinate; and *the same* for the same lines of force, whatever the distance of the point or plane on which their power is exerted is from the magnet: or it is the same in any two or more sections of the same lines of force. There is no loss or destructibility, or evanescence or latent state of the magnetic power. Convergence or divergence of the lines of force causes no difference in the amount of their power. Obliquity of intersection causes no difference. In an equal field of magnetic force the electricity evolved is proportionate to the time of motion, or to the velocity of motion, or to the amount of lines of force intersected.

The *internal state* of the magnet is then examined by means of the results obtained with the radial wire, or the moving magnet when the latter makes part of the circuit; and the conclusion is arrived at, that there are within the magnet lines of magnetic force as defined as, and exactly equal in amount to, those outside of it; that these are continuations of the former; and that every line of magnetic force, whatever distance it may extend to from a magnet, (and in principle that is infinite,) is a closed curve, which in some part of its course passes through the magnet in conformity with what is called its polarity.

A current being thus induced in a closed wire, when it travels across magnetic lines of force, an inquiry is next made into the effect of altering the *mass* or *diameter* of the wire, and another form of apparatus is employed, in which loops of wire are made to intersect a given amount of lines; each loop consisting of a given length of wire, but either of wires of different diameter, or of one or more wires of the same diameter. The conclusion arrived at is, that the current or amount of electricity evolved is not simply as the space occupied by the *breadth* of the wire correspondent to the direction of the line of force, which has relation to the polarity of the power; nor by that *width* or dimension of it which includes the number or amount of lines of force intersected, and, which corresponding to the direction of the motion has relation to the equatorial condition of the lines; but is jointly as the two, or as the mass of the wire.

The moving wire was next surrounded by different media, as air, alcohol, water, oil of turpentine, &c., but the result was the same in all.

Wires of different metals were used, and results in accordance with those obtained and described in the Second Series of these Researches were obtained: the conclusion is, that the current excited appears to be directly as the conducting power of the substance employed. It has no particular reference to the magnetic character of the body; for iron comes between tin and platinum, presenting no other distinction than that due to conducting power, and differing far less from these metals than they do from metals not magnetic.

Magnetic *polarity* then comes under consideration. The author understands by this phrase, the opposite and antithetical actions which are manifest at the opposite ends, or the opposite sides, of a limited portion of a line of force. He is of opinion that these qualities, or conditions, are not shown with certainty in every case, by attractions and repulsions; thus a solution of sulphate of iron will be attracted by a magnetic pole if surrounded by a solution weaker than itself, as shown in former researches on diamagnetic and paramagnetic action; but if surrounded by a solution stronger than itself it will be repelled. Yet the direction of the lines of force passing through it and the surrounding media cannot be reversed in these two cases, and therefore the polarity remains the same. The moving wire however shows, in similar cases, the true polarity or direction of the forces; and for an application of its principles, in this respect, to the metals, an apparatus is described by which discs of different metals can be revolved between the poles of a horse-shoe magnet and the electric currents evolved in them carried off to the galvanometer. Now, whether the discs be of paramagnetic or diamagnetic metals, whether of iron, or bismuth, or copper, or tin, or lead, the direction of the current produced shows, that the lines of magnetic force passing through the metals is the *same* in all the cases, and hence the polarity within them the same.

The author then gives a more explicit meaning, in accordance with the definition of *line of magnetic force* contained in this paper, to some of the expressions used in the three last series of his Researches on Magnetic Condition, Atmospheric Magnetism, &c.: and by referring to former results obtained since the year 1830, illustrates how much the idea of lines of force has influenced the course of his investigations, and the results obtained at different times, and the extent to which he has been indebted to it; and then, recommending for many special reasons the mode of examining magnetic forces by the aid of a moving conductor, he brings for the present his subject to a conclusion.

ROYAL ASTRONOMICAL SOCIETY.

[Continued from vol. ii. p. 326.]

June 13, 1851.—On some Improvements in Reflecting Instruments. By Prof. Piazzi Smyth.

In the course of his lectures on Practical Astronomy to the students of the Edinburgh University last winter, Prof. P. Smyth had unusual opportunity of ascertaining those points in the making of the generality of the observations of navigators by sea and of travellers on land, which presented the greatest difficulty to beginners. And as these points generally consisted of needless peculiarities, sometimes absolute imperfections in the instruments, the Professor proceeded to remove them as well as he could, and the result may, perhaps, be more extensively useful, especially as the difficulties were generally felt on the sextant being applied to observations of *stars by night*, a more exact means than the sun by day, and therefore to be encouraged and assisted in every way.

In naval observations the impediments were, both by the experience of the class, and by the testimony of naval officers,—

1st. Difficulty of seeing and of bringing down the star.

2nd. Difficulty of seeing the horizon line at night.

3rd. Difficulty of reading off the angle on the limb.

The first of these, in so far as it depended on the dark field of the telescope, he proposed to remedy by employing a telescope of large aperture, say 2 inches, in place of the usual size, $\frac{1}{2}$ or $\frac{3}{4}$ inch; in so far as the loss of light was occasioned by reflexion and absorption at the glasses, he intended to remove this by employing *metal* reflectors, by which, too, the occasional nuisance of *second* images would be avoided and greater accuracy obtained. He had tried speculum metal for the purpose with great advantage; but, under some circumstances, he was in hopes of being able to employ silver, which has lately been found to be capable of reflecting near double the amount of light that speculum metal does, though that retains more than quicksilvered glass; and then, in so far as the loss of the star in "bringing down" is caused by the diminished surface exposed by the index-glass at large angles, he proposed to make that larger than usual; besides which, the reflexion taking place in the metal at the first surface, there would be no loss, as now, from the thickness of the edges of the glass or the sides of the brass box containing it.

The second difficulty would be alleviated by the same adoption of the large object-glass: besides the loss of light by transmission through the so-called transparent part of the present horizon-glass would be done away with by the employment of the metal reflectors.

The third difficulty was also shown to be gratuitous, for the reflector of the reading-glass was in general so placed that the light of the lamp could not get to it, and if it did, would be thrown away from the arc instead of on it: and were even that managed, the surface of the vernier and arc being in different planes, the same ray of light would not illumine them *both* at the *same* time. By placing them, however, both in the same plane, and by putting the reflector at an angle of 45° to the limb, instead of parallel to it, so as to receive parallel light and throw it straight down to the divisions, it was found that they could easily be read by a very faint light.

For accuracy, opposite readings were deemed essential, and a circle insisted on in place of a sextant or quadrant: and the author, considering that the failure of the reflecting circle in securing a permanent footing in the navy arose from its being made in general too large and heavy, and complicated, he had devised a very small, but strong and simple form; the telescope was more firmly connected by moving in grooves on the large surface of the face of the circle, instead of rising by the usual single screw; and in place of the inconvenient plan of having to reverse the hands so as to put the instrument into its box face uppermost (which makes the getting of it out again without pulling at the reflector or some such delicate parts, difficult), by placing the legs not on the back but on the face, the instrument may be either put into its box, or down on the floor, or anywhere, face first, with the same hand which was moving it in the observation, with the divisions and the reflectors protected from all accidents, and the whole instrument ready at any time on a moment's notice (for the telescope never need be taken off, with its improved fixing), to take advantage of an instantaneous opportunity of observation.

So much for the use of the reflecting instrument at sea: as used on land, the following difficulties were found, and are generally recognised:—

1st. The impossibility of measuring in the mercury either sun or star when within, say 20° of the zenith, from the reflecting instrument not taking in so large an angle; and again, when within, say 10° of the horizon, from the foreshortening of the reflecting surface.

2nd. The difficulty of seeing the referring point all night, viz. the reflected image of the star, when black glass is employed; and the trouble with wind when employing mercury, as well as with other causes producing vibration; and the great weight and liability to loss in long journeys through difficult and uncivilized countries.

All these difficulties seemed to be met by making the reflecting surface of speculum metal, leveled by a spirit-level; and when the reflected object could not be seen, attaching to the metal a collimating telescope, whose optical axis was parallel with the previously leveled surface, and was defined at the focus end by a horizontal slit, illuminated by a lamp at night, so as completely to remove all difficulty of seeing the referring object, and allowing of almost the whole object-glass being brought to bear on the star.

Difficulty having been found by the students in keeping sight of an object reflected from the artificial horizon, the latter was generally placed on a stand so as to bring it near the eye, and make it thereby offer a large angular space, which was pretty sure not to be exceeded by the shaking of the hand or involuntary movement of the head of an unpractised observer; but it was found requisite, not only to make the stand firm, but to improve the steadiness of the leveling screws, which was done by making them parts of a fixed frame, with the reflector moveable on them, and capable of being fastened in any position between opposite nuts.

A sextant with all the improvements (except the opposite readings), a full-sized model of a circle, and one of the reflecting horizon, were shown; but Prof. Smyth did not mean to claim any part of them as his own invention; for without making any special inquiries as to how far he might have been preceded by any one else, he believed that he had only brought to bear on this subject individual improvements long and well known in other departments of the science; but as they had never, he thought, been so completely united before, and as such a reunion might enable observations often to be obtained when now they are given up, he hoped that the communication might not be uninteresting to some of the numerous working members of the Society,

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XII. *Intelligence and Miscellaneous Articles.*

ON THE PRODUCTION OF INSTANTANEOUS PHOTOGRAPHIC IMAGES. BY H. F. TALBOT, ESQ.

IT will probably be in the recollection of some of your readers that in the month of June last a successful experiment was tried at the Royal Institution, in which the photographic image was obtained of a printed paper fastened upon a wheel, the wheel being made to revolve as rapidly as possible during the operation.

From this experiment the conclusion is inevitable, that it is in our power to obtain the pictures of all moving objects, no matter in how rapid motion they may be, provided we have the means of *sufficiently* illuminating them with a sudden electric flash. But here we stand in need of the kind assistance of scientific men who may be acquainted with methods of producing electric discharges more powerful than those in ordinary use. What is required, is, vividly to light up a whole apartment with the discharge of a battery:—the photographic art will then do the rest, and depict whatever may be moving across the field of view.

I had intended to communicate much earlier the details of this experiment at the Royal Institution, but was prevented from doing so at the time; and soon afterwards I went on the Continent in order to observe the total solar eclipse of the 28th of July. This most interesting phænomenon I had the pleasure of witnessing at the little town of Marienburg, in the north-eastern corner of Prussia. The observations will appear, I believe, in a forthcoming volume of the Transactions of the Royal Astronomical Society. Among other things, I was enabled to make a satisfactory estimate of the degree of darkness during the total obscuration; which proved to be equal to that which existed one hour after sunset the same evening, the weather being during that evening peculiarly serene, so as to allow of a just comparison.

This Continental journey having effectually interrupted my photographic labours, I have only recently been able to resume them. I shall therefore now proceed to describe to you exactly the mode in which the plates were prepared which we used at the Royal Institution; at the same time not doubting that much greater sensibility will be attained by the efforts of the many ingenious persons who are now cultivating the art of photography. And it is evident that an increased sensibility would be as useful as an augmentation in the intensity of the electric discharge.

The mode of preparing the plates was as follows:—

1. Take the most liquid portion of the white of an egg, rejecting the rest. Mix it with an equal quantity of water. Spread it very evenly upon a plate of glass, and dry it at the fire. A strong heat may be used without injuring the plate. The film of dried albumen ought to be uniform and nearly invisible.

2. To an aqueous solution of nitrate of silver add a considerable quantity of alcohol, so that an ounce of the mixture may contain three grains of the nitrate. I have tried various proportions, from one to six grains, but perhaps three grains answer best. More experiments are here required, since the results are much influenced by this part of the process.

3. Dip the plate into this solution, and then let it dry spontaneously. Faint prismatic colours will then be seen upon the plate. It is important to remark, that the nitrate of silver appears to form a true chemical combination with the albumen, rendering it much harder, and insoluble in liquids which dissolved it previously.

4. Wash with distilled water to remove any superfluous portions of the nitrate of silver. Then give the plate a second coating of albumen similar to the first; but in drying it avoid heating it too much, which would cause a commencement of decomposition of the silver. I have endeavoured to dispense with this operation No. 4, as it is not so easy to give a perfectly uniform coating of albumen as in No. 1. But the inferiority of the results obtained without it induces me for the present to consider it as necessary.

5. To an aqueous solution of protiodide of iron add *first* an equal volume of acetic acid, and then ten volumes of alcohol. Allow the mixture to repose two or three days. At the end of that time it

will have changed colour, and the odour of acetic acid as well as that of alcohol will have disappeared, and the liquid will have acquired a peculiar but agreeable vinous odour. It is in this state that I prefer to employ it.

6. Into the iodide thus prepared and modified the plate is dipped for a few seconds. All these operations may be performed by moderate daylight, avoiding however the direct solar rays.

7. A solution is made of nitrate of silver, containing about 70 grains to one ounce of water. To three parts of this add two of acetic acid. Then if the prepared plate is rapidly dipped once or twice into this solution it acquires a very great degree of sensibility, and it ought then to be placed in the camera without much delay.

8. The plate is withdrawn from the camera, and in order to bring out the image it is dipped into a solution of protosulphate of iron, containing one part of the saturated solution diluted with two or three parts of water. The image appears very rapidly.

9. Having washed the plate with water, it is now placed in a solution of hyposulphite of soda, which in about a minute causes the image to brighten up exceedingly by removing a kind of veil which previously covered it.

10. The plate is then washed with distilled water, and the process is terminated. In order, however, to guard against future accidents, it is well to give the picture another coating of albumen or of varnish.

These operations may appear long in the description, but they are rapidly enough executed after a little practice.

In the process which I have now described, I trust that I have effected a harmonious combination of several previously ascertained and valuable facts—especially of the photographic property of iodide of iron, which was discovered by Dr. Woods of Parsonstown, in Ireland, and that of sulphate of iron, for which science is indebted to the researches of Mr. Robert Hunt. In the true adjustment of the proportions, and in the mode of operation, lies the difficulty of these investigations; since it is possible by adopting other proportions and manipulations not very greatly differing from the above, and which a careless reader might consider to be the same, not only to fail in obtaining the highly exalted sensibility which is desirable in this process, but actually to obtain scarcely any photographic result at all.

To return, however, from this digression.—The pictures obtained by the above-described process are negative by transmitted light and positive by reflected light. When I first remarked this, I thought it would be desirable to give these pictures a distinctive name, and I proposed that of *Amphitype*, as expressive of their double nature—at once positive and negative. Since the time when I first observed them, the Collodion process has become known, which produces pictures having almost the same peculiarity. In a scientific classification of photographic methods, these ought therefore to be ranked together as species of the same genus. These Amphitype pictures differ from the nearly related Collodion ones in an important circumstance, viz. the great hardness of the film and the firm fixation of the image, which is such that in the last washing, No. 10, the image may be rubbed strongly with cotton and water without any injury to it; but, on the contrary, with much improvement, as this removes any particles of dust or other impurity, and gives the whole picture a fresh degree of vivacity and lustre. A Daguerreotype picture would be destroyed by such rough usage before it was completely fixed and finished.

In examining one of the Amphitype pictures, the first thing that strikes the observer is, the much greater visibility of the positive image than of the negative one; which is at least in the proportion of *ten* to *one*, since it is not rare to obtain plates which are almost invisible by transmitted light, and which yet present a brilliant picture full of details when seen by reflected light.

The object of giving to the plates a second coating of albumen, as prescribed in No. 4, is chiefly in order to obtain this well-developed positive image; for it is a most extraordinary fact, that a small change in the relative proportions of the chemical substances employed enables us at pleasure to cause the final image to be either entirely negative or almost entirely positive. In performing the experiment of the rotating wheel the latter process must be adopted, since the transmitted or negative image is not strong enough to be visible unless the electric flash producing it be an exceedingly bright one.

I now proceed to mention a peculiarity of these images which appears to me to justify still further the name of Amphitype, or, as it may be rendered in other words, "ambiguous image." Until lately I had imagined that the division of photographic images into *positive* and *negative* was a complete and rigorous one, and that all images must be of either the one or the other kind. But a third kind of image of a new and unexpected nature is observed upon the Amphitype plates. In order to render this intelligible, I will first recall the general fact that the image seen by transmitted light is negative and that by reflected light positive. Yet, nevertheless, if we vary the inclination of the plate, holding it in various lights, we shall not fail speedily to discover a position in which the image is positive although seen by transmitted light. This is already a fact greatly requiring explanation. But the most singular part of the matter is, that in this new image (which I call the *transmitted positive*), the brightest objects (viz. those that really are brightest, and which appear so in the *reflected positive*) are entirely wanting. In the places where these ought to have been seen, the picture appears pierced with holes, through which are seen the objects which are behind. Now, if this singularity occurred in all the positions in which the plate gives a positive image, I should be satisfied with the explanation that the too great brightness of the objects had destroyed the photographic effect which they had themselves at first produced. But since this effect takes place in the *transmitted positive* but not in the *reflected positive*, I am at a loss to suggest the reason of it, and can only say that this part of optical science, dependent upon the molecular constitution of bodies, is in great need of a most careful experimental investigation.

The delicate experiment of the revolving wheel requires for its success that the iodide of iron employed should be in a peculiar or definite chemical state. This substance presents variations and anomalies in its action which greatly influence the result. Those photographers, therefore, who may repeat the experiment will do well to fix their principal attention upon this point. It is also requisite in winter to warm the plates a little before placing them in the camera. In pursuing this investigation, I have been much struck with the wide field of research in experimental optics which it throws open. By treating plates of albumened glass with different chemical solutions, the most beautiful Newtonian colours, or "colours of thin plates," may be produced. And it often happens that the landscapes and pictures obtained by the camera present lively though irregular colours. These not being in conformity with nature are at present useless; with this exception, nevertheless, that in many pictures I have found the colour of the sky to come out of a very natural azure blue. I hope soon to have the leisure requisite for pursuing this very interesting branch of inquiry, and in the mean time I venture to recommend it to the notice of your scientific readers.—*Athenæum*, Dec. 6, 1851.

ON COPPER CRYSTALLIZED BY MEANS OF PHOSPHORUS. BY F. WÖHLER.

The experiments of Böck and Vogel, sen., have taught us that the whole of the copper in a solution of the sulphate contained in closed vessels is reduced by phosphorus; crystalline laminæ of greater or less thickness, according to the duration of the reaction, having the form of the piece of phosphorus and of a beautiful bright copper colour, being formed. If the pieces of phosphorus are placed in contact with bright copper wires, reduction of the copper also takes place upon them, and this in distinct, mostly well-formed octohedral crystals, the form of which, when the process is allowed to continue for weeks and months, with a quantity of undissolved crystals of the sulphate in the solution, is distinguishable to the naked eye; at the same time the whole of the phosphorus disappears, and the masses of copper reduced by it are found filled inside with black pulverulent phosphuret of copper.—*Ann. der Chem. und Pharm.* vol. lxxix. p. 126.

ON THE ACCIDENTAL COLOURS WHICH RESULT FROM LOOKING AT WHITE OBJECTS. BY M. D. M. SEGUIN.

1. If after having looked for some time at a white object the eyes are closed, a coloured image of the object is seen. This image presents a number of colours, which change little by little: as an example, I will narrate the following instance. After looking at a very brilliant object, such as a white screen seen by the transmitted light of the sun, on closing the eyes the image appears at the first moment green, olive-green or yellow; but there is a red border all round, followed by much darker tints. After a few moments the image becomes decidedly yellow, but the coloured border approaches towards the centre of the image: the latter acquires a deeper yellow, a zone of orange and a zone of red gain gradually upon the yellow, and at the same time the dark tint which was

beyond the red separates into a number of coloured zones of great intensity, presenting violet, indigo, blue, green. All the colours advance one after the other towards the centre of the image, which they successively occupy.

By varying the brightness of the object, and the length of time of looking at it, I have been able to detect one or two constant series of colours, apparently very different, which these accidental images present.

2. When the accidental image is formed in the eyes, if they are opened towards a white surface, the image remains; but it generally passes from the tint which it has to one of those which it would assume at a later period if the eyes were kept closed, and at the same time the tints which still remain at the border advance more towards the centre, which they occupy successively. The white light which enters the eye has therefore the effect of accelerating the progression of the colours from the circumference to the centre of the image. I have traced this influence of the exterior white light, whether by opening the eyes before a surface more or less lighted, or by gradually opening them, and I have found that the tint to which the image passes is more advanced in the series when the exterior light is more intense.

3. My experiments have enabled me to observe those instances in which the accidental image of a white object passes through alternations of brightness and darkness; I have always observed that the images are coloured.

In the hope of being able to account for these effects, I have entered upon the study of the accidental images produced by coloured objects. This part of the question has been much disputed. I have repeated almost all the experiments described by various authors, and have frequently been astonished at the results which I have obtained. I shall describe these in a second memoir.—*Comptes Rendus*, Dec. 8, 1851.

EXTRAORDINARY SPOTS ON THE SUN.

On Saturday last, the 29th of November, the solar maculæ, which have of late been very numerous, assumed a remarkable shape and occurred in very considerable number. Dr. Forster, who has been occupied of late in taking drawings of these spots, observes that he has never seen any spot on the sun's disc so large or unusual in form as that which occurred on Saturday: it was of a long and irregular form, densely black, and surrounded with a widely-spreading greyish margin, as well as by several other smaller *maculæ*. Many other more round and compact spots appeared on other parts of the disc. But the most remarkable circumstance was the rapid changes observed in these phænomena. While Dr. Forster was observing them, several new spots broke out into view.

The connexion of these phænomena with the abundance of wet and cold were formerly noticed by the late Dr. Herschel. Now that the weather has been dry in England, a more than ordinary quantity of snow and rain has fallen on the Continent.

Bruges, Dec. 5, 1851.

OBITUARY.—MR. SAMUEL VEALL.

Died at Boston, Lincolnshire, on the 17th of August 1851, aged 71 years.

It may be said of him, that in youth, and until his mental powers had become enfeebled by age, he was diligent in the attainment of knowledge. From his early days he was fond of books and experimental science. At a time when philosophy was by no means fashionable, especially about 1808 and 1809, he was amongst the earliest projectors and friends of a Literary and Philosophical Society in Boston, his native town. In connection with this Society, he became Secretary, and delivered lectures on Electricity, Optics, Galvanism, &c., and it is believed continued his efforts so long as he could find coadjutors to act with him. He engaged in those pursuits simply for the improvement of himself and his neighbours.

It may well be presumed, that his Meteorological Journal, which he kept methodically and perseveringly for many years, and communicated to this Magazine from the year 1816, has aided in throwing some light upon the laws which govern the changes of the atmosphere, and may have induced others to contribute in like manner to meteorological science.

He was considerate to a fault of those whom he employed in business; and though often injured himself, he was not known to act injuriously towards others. Punctiliously honest, he even made scruples where many individuals esteemed upright would see nothing to blame. He has left a widow and family to revere his memory and imitate his virtues.

METEOROLOGICAL OBSERVATIONS FOR NOV. 1851.

Chiswick.—November 1. Overcast: very fine: clear: frosty. 2. Fine: hail-shower. 3. Hoar-frost: very fine: cloudy: rain. 4. Rain: fine, but cold. 5. Clear and frosty: slight rain at night. 6. Clear and fine: cloudy. 7. Cloudy and cold. 8. Fine: rain. 9. Foggy: fine: rain. 10. Very fine: drizzly at night. 11, 12. Very fine. 13. Foggy. 14. Clear and fine. 15. Frosty: very fine: clear. 16. Frosty: clear and fine: cloudy. 17. Clear and cold: frosty at night. 18. Clear and cold: severe frost at night. 19. Sharp frost: fine: cloudy. 20. Clear and frosty: very clear throughout. 21. Overcast. 22. Cloudy: fine. 23. Frosty: clear and fine: rain at night. 24. Densely clouded: foggy at night. 25. Frosty: very fine. 26. Foggy. 27. Hazy. 28. Frosty: very fine: frosty. 29. Frosty, with fog: fine: foggy. 30. Dense fog.

Mean temperature of the month	35°·86
Mean temperature of Nov. 1850	45 ·29
Mean temperature of Nov. for the last twenty-five years ...	43 ·43
Average amount of rain in Nov.	2·35 inches.

Boston.—Nov. 1. Fine. 2. Rain: rain early A.M. 3, 4. Fine. 5, 6. Fine: rain P.M. 7. Cloudy. 8. Cloudy: rain P.M. 9, 10. Fine: rain P.M. 11. Foggy. 12—16. Fine. 17. Fine: snow P.M. 18. Fine. 19. Cloudy. 20. Fine. 21. Cloudy: rain P.M. 22. Cloudy: rain A.M. and P.M. 23. Cloudy: rain P.M. 24, 25. Fine. 26. Cloudy. 27. Fine. 28. Cloudy. 29. Fine. 30. Foggy.

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

Meteorological Observations made by Mr. Thompson *at the Garden of the Horticultural Society at* CHISWICK, *near London; by* Mr. Veall, *at* BOSTON; *and by the* Rev. C. Clouston, *at Sandwick Manse,* ORKNEY.

Days of Month	Barometer.					Thermometer.					Wind.			Rain.		
	Chiswick.		Boston. 8¾ a.m.	Orkney, Sandwick.		Chiswick.		Boston. 8¾ a.m.	Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Orkney, Sandwick.	Chiswick.	Boston.	Orkney, Sandwick.
1851. Nov.	Max.	Min.		9¼ a.m.	8½ p.m.	Max.	Min.		9¼ a.m.	8½ p.m.						
1.	29·613	29·559	29·22	29·22	29·08	52	28	41·5	42	39	nw.	w.	sw.	·02	......	·19
2.	29·550	29·336	29·00	29·07	29·40	50	31	44	40	39	n.	w.	nnw.	·01	·07	·47
3.	29·920	29·759	29·47	29·60	29·68	45	26	34	35	35½	w.	nw.	nnw.	·06	......	·15
4.	30·002	29·762	29·59	29·82	29·95	40	22	30	37	40	n.	n.	w.	·02	......	·19
5.	30·084	29·884	29·70	29·80	29·97	45	37	31·5	47	42	w.	nw.	n.	·02	......	·19
6.	29·870	29·846	29·56	29·98	30·07	47	38	40	42	41	nw.	nw.	calm	......	·03	·05
7.	29·835	29·770	29·55	29·91	29·88	47	35	41	45	49	ne.	n.	n.	......	·26	·26
○ 8.	29·874	29·835	29·59	29·82	29·73	48	38	41	49	50	ne.	nw.	nw.	·07	......	·08
9.	29·869	29·803	29·58	29·70	29·51	43	39	41	46	45½	se.	s.	w.	·14	·18	·12
10.	29·757	29·626	29·34	29·61	30·06	49	28	39	44	46	ne.	n.	n.	·01	·20	·53
11.	30·120	30·005	29·75	30·18	30·18	49	30	34	46	46	n.	nnw.	w.	......	·02	·09
12.	30·347	30·230	29·94	30·24	30·25	46	29	36·5	45	46	n.	n.	w.	·02	......	·05
13.	30·434	30·345	30·09	30·17	30·26	47	36	34	46	37	sw.	w.	n.	......	......	·10
14.	30·289	30·100	30·00	30·32	30·16	44	25	36	39	39	n.	n.	s.	......	......	·13
15.	30·022	29·946	29·72	29·96	29·96	41	19	33	40½	41	w.	nw.	nw.	......	......	·29
☾ 16.	29·845	29·815	29·56	29·92	29·98	42	26	34	35	31	n.	nw.	nnw.	......	......	·06
17.	29·821	29·701	29·55	29·86	29·94	35	27	28·5	32	37	n.	nnw.	n.	......	......	......
18.	29·871	29·839	29·60	29·91	29·88	41	16	31	34	32	n.	nw.	calm	......	·22	·09
19.	29·849	29·644	29·57	29·66	29·76	37	25	30	37	37	sw.	wnw.	se.	......	......	......
20.	30·062	29·861	29·63	29·95	29·72	43	24	31	39	48	n.	nw.	w.	......	......	·03
21.	29·930	29·836	29·56	29·83	30·06	46	34	37	40½	40	w.	w.	n.	......	......	·21
22.	30·149	29·998	29·72	30·17	30·11	45	25	37·5	41	44	n.	nnw.	w.	......	·03	·08
● 23.	30·133	29·855	29·80	29·66	29·40	45	32	32·5	40½	41⅓	w.	nw.	w.	·18	·04	......
24.	29·493	29·452	29·16	29·31	29·36	46	23	39	35	35	nw.	w.	se.	......	·45	·35
25.	29·492	29·469	29·17	29·42	29·66	46	23	29	40	38½	sw.	wnw.	e.	......	......	·07
26.	29·738	29·599	29·30	29·74	29·83	36	28	30	38½	40	nw.	calm	calm	......	......	·11
27.	29·899	29·788	29·47	29·92	29·98	42	26	33	38	37	nw.	nnw.	calm	......	......	·08
28.	30·073	29·990	29·67	30·01	30·07	44	21	34	39½	34	nw.	nw.	n.	......	......	·06
29.	30·170	30·158	29·82	30·11	30·16	39	22	28	37	37	sw.	wnw.	calm	......	......	......
☽ 30.	30·247	30·162	29·88	30·28	30·40	39	20	27·5	40	39	ne.	......	e.	......	......	·08
Mean.	29·945	29·832	29·58	29·838	29·881	43·96	27·76	34·6	40·35	40·23				0·55	1·50	4·11

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[FOURTH SERIES.]

FEBRUARY 1852.

XIII. *Reports on the Progress of the Physical Sciences.* *By* JOHN TYNDALL, *Ph.D.*

[With a Plate.]

On Thermo-electric Currents, by Prof. MAGNUS *of Berlin*. Experiments of* MM. SVANBERG† *and* FRANZ‡ *on Monothermic Electricity* §. *Application of the results of* M. MAGNUS *to the solution of certain difficulties encountered by* M. REGNAULT.

EXACTLY thirty years have flown by since the discovery of thermo-electricity by Seebeck in Berlin. Since that time our knowledge of facts in connexion with this subject has been enriched by the labours of Becquerel, Sturgeon, Matteucci, Henrici and others; but our advance towards principles has been slow. Indeed some of the facts at present generally accepted are of so incomprehensible a nature; the results of various experimenters—and even of the same experimenter at different times—are so perplexing and contradictory, as pressingly to indicate the necessity of further and stricter examination. In the production of thermo-currents and the determination of their directions, so many hidden influences come into play, that if one subject more than another require the exercise of patience and experimental tact it is this. Until very lately every attempt at progression in this department of inquiry was accompanied by the unpleasant conviction that there was no sure starting-point; and hence he that would advance had to begin afresh, and

* Pogg. *Ann.*, vol. lxxxiii. p. 469. † *Comptes Rendus*, vol. xxxi. p. 250.
‡ Pogg. *Ann.*, vol. lxxxiii. p. 374.
§ I have taken the liberty of applying this term to the electricity developed by the heating of *a single metal*.—J. T.

jealously test every result of his predecessors. This is the state of things which the investigation of M. Magnus is intended to remedy, and his memoir on the subject furnishes internal evidence of the precision with which the inquiry has been conducted. The investigation is far from exhausting the subject, but it lets us know precisely where we are; new and striking facts have been added, errors have been corrected, anomalies accounted for, and the first great step made towards the reduction to law of those inexplicable phænomena which have hitherto perplexed philosophers.

The wire usually applied in the construction of galvanometers often presents a difficulty in inquiries like the present. That purchased at the merchants is so magnetic as greatly to interfere with the purity of the experiments. To obviate this defect, some precipitated copper was obtained from a galvano-plastic manufactory; but the metal, after having been cast into cylindrical moulds, was found so magnetic as to necessitate its rejection. The pure metal was finally obtained in the following manner: an excess of ammonia was added to a solution of sulphate of copper, the precipitated oxide being thus redissolved, and the iron mixed with the salt separated; the solution was filtered, evaporated to dryness, and the ammonia expelled; the sulphate thus procured was redissolved in water and precipitated by the voltaic current. This metal, however, was exceedingly brittle, and required to be melted eight times in succession before it could be drawn into wire; when drawn, however, it was found to answer its purpose perfectly.

In the following pages we shall often have occasion to speak of the direction of the current, and it is therefore prudent to define clearly in the first instance what is meant by this expression. If a strip of copper and a strip of zinc be immersed in a conducting fluid, and the exposed ends be united by a copper wire, the current is said to proceed from the copper through the uniting wire to zinc, and hence from the zinc through the fluid to copper. Suppose a bit of antimony to supersede the strip of copper, and a bit of bismuth in the place of the zinc, and doing away with the fluid, let the free ends of both be brought into contact and the place of contact heated; the consequent thermo-current will act upon a magnetic needle exactly as that developed by the zinc and copper pair. The current therefore passes from antimony *through the wire* to bismuth (from A to B), but from bismuth to antimony (against the alphabet) *across the place of junction.* Whenever it is stated in this Report that the current passes from one metal to another, the words "across the place of junction" are always implied.

It was soon ascertained that a difference in point of hardness was sufficient to give rise to a current. When a portion of a wire which had been rendered hard by drawing was heated to redness and thus softened, on warming the point of junction between hard and soft, a current was always obtained. In like manner, when a portion of the wire was rendered harder by hammering, a current was produced on heating the junction of hard and soft.

For these experiments a particular arrangement of apparatus was devised; and to prevent any new change in the structure of the wire, it was rarely heated beyond the temperature of boiling water. Particular care was also taken to preserve the points where the two ends of the wire experimented with joined the wire of the galvanometer at the same temperature, a condition absolutely necessary to prevent the formation of a current at these points of junction.

M. Becquerel was the first to demonstrate, that when a wire is knotted and heated in the vicinity of the knot, a current is exhibited. As, however, M. Becquerel employed a red heat in his experiments, it is possible that the current obtained was due to a softening of a portion of the wire, while the knotted portion retained its hardness. Such a result is still more probable in the case where the point of junction of a thick and thin wire is strongly heated. Up to the present time it has been an accepted fact, that a difference in point of thickness merely is sufficient to originate a current.

For the stricter examination of this question two semi-cylinders of brass were procured, and along the axis of each a semi-cylindrical hollow was worked out from end to end. Into this hollow a brass wire was accurately fitted; so that when one piece was placed upon the other, the whole had the appearance represented in Plate III. fig. 1. The slightest heating of the wire in the neighbourhood of its thick case was sufficient to develope a current, and the currents developed when the wire was heated at both ends of the case in succession were in opposite directions.

What is the proximate source of the electricity in this case? is it the result of a mere difference in point of thickness; is it to be referred to a difference in chemical composition; or is it due to a difference of hardness between the wire and its encompassing cylinder? If a piece of metal be laid upon another piece of a different metal, in the manner represented in fig. 2, when the point *c* is warmed a thermo-current is evoked, which circulates for the most part within the boundaries of the two pieces. If, however, the extreme ends of the bar be united with a galvanometer, a branch current will exhibit itself; hence if the thin wire spoken of above and its encompassing sheath be not perfectly homogeneous, something similar may be expected to take place.

To decide this question, the following experiments were carried out:—A brass wire 6 feet long was encircled by a number of pieces, each 1 foot in length, cut from the same piece as the 6-feet wire; the short pieces were tied round the latter by a non-conducting thread. When the portion of the wire adjacent to the surrounding bundle was heated, *no current was observed*; the experiment was repeated with a second wire, but with the same result.

A brass wire 3 feet long and 3 lines in thickness was so reduced, that a length of 6 inches of its central portion had a diameter of only half a line (see fig. 3). Both of the points *g* were heated in succession, but in neither case was a current exhibited. Eighteen inches of another brass wire of 3 lines diameter were reduced to 0·7 of a line, and each end of the portion thus reduced was screwed into a piece of the thick wire from which it was taken; on heating the place of junction of thick and thin there was no current. Again, part of a piece of brass wire 3 lines in thickness was drawn out until a diameter of half a line was obtained; both thick and thin portions were then heated to redness, and the oxide carefully removed from the surface; their ends were laid one upon the other and thus heated, but no current was observed. In all these cases care has been taken to have the thick and thin portions homogeneous; and we see that when this point is secured, a current *never exhibits itself.* The mere difference in point of thickness is therefore not sufficient to originate a current, as heretofore believed. M. Magnus explains the knot experiments of M. Becquerel by reference to the fact, that the structure of the wire was first altered by heating it to redness. If the temperature applied do not exceed 100° C., no current is ever observed.

It has also been affirmed, that the production of a thermo-current is in some measure dependent on the radiative power of the metals employed. A German silver wire was covered by galvanic precipitation with a coating of copper throughout a portion of its length; the wire was heated at the place where the coating ceased, and a tolerably strong current was the consequence. Was this result due to a contact of chemically different metals, or to a difference in the radiative power of both? The wire was next covered with various non-conducting substances, such as soot, gutta percha, wood, &c.; but when the point where the coating ceased was heated, no current was observed. In like manner, when one portion of a wire was finely polished, and an adjacent portion rendered rough by sand-paper or by the file, on heating the junction between rough and smooth there was no current, although the radiative powers of both portions must have been very different. It is thus proved that a difference in respect to radiative power is not sufficient to originate a thermo-current.

It has been already stated, that where a difference in point of hardness exists, a current is produced. To examine this point further, a number of wires each 6 feet long and 0·45 of a line in thickness were chosen; and of those which could bear a high temperature, two feet in the middle were heated to redness and thus rendered soft. Of the more fusible metals, such as tin, lead, zinc, &c., two feet were heated in an oil-bath at 200° C. for an hour. When cooled, the two ends of each wire were united with the galvanometer; one of the junctions between hard and soft was heated, and the consequent current observed. The following table exhibits the results of these experiments: it will be observed that the direction of the current does not preserve a constant relation to hard and soft. In some cases it flows from soft to hard, in other cases in the opposite direction.

Name of metal.	Direction of current.	Deflection.
Brass	From soft to hard	55°
Silver (pure)	do.	46
Steel	do.	45
Silver with 25 per cent. copper	do.	40
Cadmium	do.	25
Copper	do.	18
Gold No. 1, with 9·7 per cent. copper	do.	10
Platinum	do.	5
Gold No. 2, with 2·1 per cent. silver	do.	2
German silver	From hard to soft	34
Zinc	do.	30
Tin	do.	5
Iron	do.	4
Lead	Uncertain	

By means of the pretty little instrument represented in fig. 4*, and which its inventor has named the monothermic pile, the action may be considerably increased. A length of hard brass wire is taken, every alternate six inches of which are rendered soft by heating to redness. Thus six inches of soft wire succeed six inches of hard throughout the entire length. The wire is then wound round a frame of suitable size, and presents when wound the appearance of a rectangle, two of the opposite sides of which are composed of hard and soft wire respectively; the centres of the other two sides are the junctions of hard and soft.

* Another form of this instrument is represented in fig. 5.

The two ends of the wire being connected with the galvanometer, if either the hard side or the soft side be heated we have no action; but if one of the junctions of hard and soft be taken between the finger and thumb, the heat of the hand is sufficient to cause a deflection of 90 degrees. The writer has to thank Prof. Magnus for an instrument of this kind. The wire presents the same uniform appearance throughout; and to an observer ignorant of the process to which the wire has been subjected, the deportment is exceedingly striking and enigmatical.

If two wires of the same material be taken, and if one be heated and the other permitted to remain cool, on causing the hot and cold wires to touch each other a current is observed. This is modified if the one wire be hard and the other soft: sometimes the difference of temperature and difference of hardness work together and increase the current by their united action; sometimes they oppose each other, and a decrease of the current is the consequence. This matter has been investigated very fully. It will perhaps be well to describe beforehand the manner in which the experiments were made.

In a tin cylindrical vessel, AB, fig. 6, two tubes, *ab* and *cd*, crossing each other at right angles, were introduced; each tube had a diameter of half an inch; from *f*, where the tubes crossed, another vertical tube abutted upwards and passed through the cover of the vessel; the three tubes communicated with each other inside; through one of the horizontal tubes the wire to be heated was introduced and fastened by corks at *a* and *b*; to prevent contact with the metallic vessel, all three tubes were lined by smaller ones of glass; at *f* the wire was exposed, and rested upon a flat piece of wood introduced beneath it; in the vertical tube was a wooden rod which nearly filled it, but could be moved through the tube with freedom; the rod carried at its end a pound weight of lead, P; the cylindrical vessel was filled with water and kept constantly boiling, and as soon as it was certain that the wire within had assumed the temperature of boiling water, the wooden rod was raised, and the cold wire was introduced crossing the warm one; this being effected, the rod was permitted to descend, and the wires were pressed together by the weight P. The following table shows the results of this inquiry. First *both* wires were *hard*, next *both* were *soft*; and finally, the one was hard and the other soft.

One wire 100° C., *the other* 8° C.

Name of metal.	Both wires.		One hard, the other soft.	
	Hard.	Soft.	The hard warm.	The soft warm.
German silver..	fr. c to w 40°	fr. c to w 72°	fr. c to w = fr. s to h 5° immediately afterwards fr. w to c = fr. h to s 24	fr. c to w = fr. h to s 80°
Silver (pure) ...	do. 7	do. 3	fr. c to w = fr. s to h 73	fr. w to c = fr. s to h 68
Copper	do. 3	do. 8	do. do. 24	do. do. 15
Tin	do. 7	do. 10	fr. w to c = fr. h to s 7	fr. c to w = fr. h to s 20
Zinc	fr. w to c 28	fr. w to c 28	do. do. 62	do. do. 34
Platinum	do. 24	do. 22	do. do. 13	fr. w to c = fr. s to h 36
Gold No. 2, with 2·08 p. c. silver	do. 5	do. 6	do. do. 3	do. do. 5
Gold No. 1, with 9·7 p. c. copper	do. 6	do. 5	do. do. 2 immediately afterwards fr. c to w = fr. s to h 11	do. do. 19
Cadmium	do. 26	do. 15	do. do. 53	do. do. 55
Brass	do. 3	do. 12	do. do. 90	do. do. 90
Silver, with 25 per c. copper..	do. 6	do. 12	do. do. 82	do. do. 78
Mercury.........	do. 0	0	0	0
Lead	......	......	uncertain	

w signifies *warm*; c, *cold*; h, *hard*; s, *soft*.

The discussion of this table would give rise to some interesting speculations, which, however, we forbear dwelling upon, as M. Magnus himself has not thought proper to introduce them—doubtless because he considers the subject not yet ripe for such. We shall therefore content ourselves with the expression of a hope, that results so suggestive will receive at the hands of their discoverer the development of which they seem capable.

Experiments such as these are always valuable as points of reference; we therefore introduce a second table, in which a temperature of 250° C. was applied.

One wire 250° C, *the other* 8° C.

Name of metal.	Both wires.		One hard, the other soft.	
	Hard.	Soft.	The hard warm.	The soft warm.
Silver (pure) ...	fr. c to w 20°	fr. c to w 17°	fr. c to w = fr. s to h 90°	fr. c to w = fr. h to s 3 immediately afterwards fr. w to c = fr. s to h 90
Platinum	fr. w to c 84	fr. w to c 80	fr. w to c = fr. h to s 90	do. do. 90
Gold No. 2, with 2·08 p. c. silver	do. 17	do. 28	do. do. 12	do. do. 27
Gold No. 1, with 9·7 p. c. copper	do. 54	do. 31	do. do. 10 immediately afterwards fr. c to w = fr. s to h 30	do. do. 69
Silver with 25 per c. copper...	do. 90	do. 90	fr. w to c = fr. h to s 6 immediately afterwards fr. c to w = fr. s to h 90	do. do. 90
Mercury.........	0	0	0	

We have already mentioned a variety of notions entertained by physicists as to the origin of thermo-currents. To these M. Magnus adds the discussion of the hypothesis, that the cause is to be sought in the unequal decrease of temperature on both sides of the place heated, and of the notion that they are to be referred to a difference of conductibility for heat on the part of the metals employed. He dissents from both these views; and proves, in the following manner, that the conductibility of the hard wire was in no way different from that of the soft one.

From a stout brass wire 2·25 lines in diameter, and which was rendered quite hard by the act of drawing, two pieces each 4 feet long were separated. One of these was heated to redness, and thus rendered soft; both wires were then brought into the tin vessel already described and there subjected to the same temperature; the ends of the wires without the vessel were at such a distance from it, that they retained the same temperature; to one end of the galvanometer wire before described a bar of antimony was attached, and to the end of the other wire a bar of bismuth, both being bevelled off to an edge; the edge of one of these bars was laid upon the soft wire, and the edge of the other upon the hard wire; when a difference of temperature existed between the points of contact, a current was exhibited on the galvanometer; when the temperatures were alike, no current was visible. By finding points of equal temperature in this manner, and by measuring the distance between these points on each wire, their respective conductive powers were ascertained. It was found that the conductibility of both was the same. The conclusion finally arrived at by M. Magnus is, that the currents are produced by the contact of unhomogeneous metals.

In connexion with this subject, M. Svanberg has laid an interesting communication before the Academy of Sciences at Paris, from which we extract the following:—In large masses of bismuth and antimony the crystalline texture is never in all parts the same, but it is not difficult to find some homogeneous portions. From these little bars may be formed, the length of which may be at various inclinations to the planes of crystallization.

Among the planes of cleavage of these two metals in a crystallized state, there is one, which, as Mr. Faraday was the first to observe, is distinguished from all others by its superior brilliancy. This plane is perpendicular to the crystallographic axis. Among the other planes there is one which does not fall far short of the above in point of brightness. Let the bars whose length coincides with the intersection of those two planes be named (A), and those bars whose length is perpendicular to the plane of most eminent cleavage be named (B).

In the case both of bismuth and antimony the bars (A) are more positive, and the bars (B) more negative, in the thermo-electric series, than any other bar which can be formed of the same metal. The thermo-electric force between the antimony (A) and the antimony (B), and between the bismuth (A) and the bismuth (B), is pretty considerable. If a bar intermediate between (A) and (B) be taken, that is to say, such that the direction of its length is otherwise inclined to the plane of most eminent cleavage, or if it do not possess a regular crystalline texture, such a bar is negative with (A) and positive with (B).

This variability in the thermo-electric power of bismuth and antimony seems to furnish a key to the explanation of the currents observed by Seebeck, Sturgeon and Matteucci, in circuits formed of a single one of these metals. They have not been explained hitherto.

With regard to the direction of the currents between the warm bismuth and the cold bismuth, the warm antimony and the cold antimony, different experimenters have arrived at different results. Vorsselmann de Heer, the last who has occupied himself with this subject, has observed the current to pass sometimes from the cold to the warm metal, and at other times from the warm metal to the cold. He concluded from his observations, that the direction of the current depends on the greater or less difference of temperature between the two bars. These cases of reversion exhibited themselves in an especial manner with antimony.

That such experiments should have any value, it is absolutely necessary that the bars made use of should occupy the same place in the thermo-electric series. Thus, for example, we must compare (A) with (A), and (B) with (B), but not (A) with (B). In the first place, it ought to be ascertained whether the two bars be absolutely homogeneous. It is a remarkable fact, that the deportment of (A) towards (A) is not the same as that of (B) towards (B).

M. Svanberg's mode of experimenting was as follows:—The two bars were fixed in copper handles, and these were connected with a very sensitive galvanometer. Up to the point of contact with the copper, the bars were enveloped in snow almost to the free extremities. In this case, when the extremities are brought into contact and then heated to any temperature whatever, there ought to be no current; and this furnishes a test as to whether the bars are thermo-electrically homogeneous. But if *before* bringing the bars into contact, the end of one of them be either heated or cooled, a current is observed, the direction of which is indicated by the galvanometer. If the two bars be of the bismuth (A) or of the antimony (A), the current proceeds from the cold to the warm metal; with the bars (B), however, the direction of the current is the opposite, it passes from the warm metal to the cold. This result is exceedingly remarkable, but it has been proved by multiplied experiments.

Another memoir on this subject by M. Franz of Berlin has recently appeared in Poggendorff's *Annalen.* He uses cubes of bismuth. The cubes are placed between two small copper pillars connected with a galvanometer; the pillars are moveable, and thus permit of the cubes being pressed together. We will call the direction from pillar to pillar the axial direction, and that perpendicular thereto, the equatorial. In some cubes the plane of most eminent cleavage formed two of the opposite sides, and in some the said plane was inclined at an angle of 30° or 60° to two opposite sides. When two of the former were so placed that the cleavage *throughout both* stood either axial or equatorial, no current was observed on heating. When the cleavage of one cube was axial and that of the other equatorial, there was a deflection of 45°. When a pair of the other cubes were placed so that the cleavage of each made an angle of 30° with the plane of the horizon, a current of 30° was observed; when the angle with the horizon was 60°, the deflection was 19°·7. Bismuth was also found to change its thermo-electric power in contact with other metals, when the position of the plane of most eminent cleavage in relation to the plane of contact of both metals was altered. These results appear to stand in intimate connexion with those of M. Magnus.

Application of the results of M. Magnus *to the solution of certain difficulties encountered by* M. Regnault.

An exceedingly interesting memoir, "On the Measurement of Temperatures by Thermo-electric Currents," by M. Regnault, appears in the Philosophical Magazine for June 1850. In the course of experiment some very perplexing and indeed unexplainable phænomena presented themselves, the solution of which appears to be furnished by the experiments of M. Magnus. This does not appear to have been noticed by the latter philosopher, as he is silent on the subject. I have carefully plotted the seven series of results given by M. Regnault; taking the difference of temperature of iron and platinum as abscissæ, and the difference between bismuth and antimony as ordinates, and using a horizontal scale of twenty, and a vertical scale of ten divisions to an inch. In the curves formed by the plotting of the last three series, where every pains was taken to remove all possible causes of disturbance, the anomalies are most striking. Laying the datum line of one upon that of another, and commencing at a common point, the curves ought to superpose; but they do not;

that derived by plotting the 5th series falls considerably below those obtained by plotting the 6th or 7th. A mere inspection of the table exhibits the same in particular cases. For example, a difference of temperature of 268°·64 between iron and platinum, corresponds in the third series to a difference of 13°·71 between bismuth and antimony; whereas in the 6th series, a difference of 268°·66 between the former corresponds to a difference of 17°·77 between the latter; and in the 7th series, a difference of 268°·56 is equivalent to one of 18°·60. It hence appears that the thermo-electric force of iron and platinum is relatively greater in the 6th and 7th series than in the 5th. We shall now endeavour to account for this hitherto inexplicable result. Turning to the table at page 85 of this Report, we observe that the current formed at the junction of hard and soft in an iron wire passes from hard to soft, which proves that the iron is rendered *more negative* when it is softened by heat. Let us now devote a moment's attention to the result with platinum wire at page 87. In the case of two homogeneous wires, the current passes from warm to cold, causing a deflection of 24° when both wires are hard. When a hard and soft wire are taken, and the former is heated, the current passes as before from warm to cold, causing, however, a deflection of only 13°. It thus appears that the soft wire is less negative, or what is the same, more positive than the hard wire. Consistently with this, if the heated wire be the soft one, the fact of its being hot and soft at the same time ought to make the current developed a maximum—this is the case. The deflection observed under these circumstances is 36°.

The general facts being thus established, that iron, when softened by heat, becomes more negative, and that platinum, when softened by heat, becomes more positive, let us apply them to the case before us. M. Regnault commenced his 5th series with a fresh couple of iron and platinum, increasing the difference of temperatures between the hot and cold junctions gradually until it reached 273°·46. The absolute temperature of the hot junction at this point was in all probability 300°. After the couple had been thus heated, it was allowed to cool, and the 6th series was commenced: here the anomaly before alluded to at once presented itself; a certain difference of temperature produced a stronger current than in the 5th series, a result which might be inferred *à priori* from the foregoing considerations. For the iron by being once heated to 300° has become more negative, as before proved, while the platinum has become more positive; the thermo-electric force of the couple has, in short, been increased, and a more powerful current is the necessary consequence. This is still more strikingly exhibited in the 7th series, where M. Regnault commences with a difference of 103°·80, and goes on increasing to 282°·18; then, without interrupting the series, allows the difference to sink again to 148°·97. The bismuth and antimony equivalent for this is 12°·30; whereas for a difference of 152°·29 between the iron and platinum, *before the difference of temperature had reached the above amount* (282°·18), the antimony and bismuth equivalent is only 11°·69. This fluctuation in the 7th series causes the curve derived from plotting to present somewhat of the appearance of a railway section over undulating ground, whereas in all the other cases it presents a gradual and almost uniform ascent. The 'sudden leaps' noticed by M. Regnault, whose cause he considered it impossible to ascertain, appear to be thus capable of satisfactory explanation.

XIV. *Observations in the Alps on the Optical Phænomena of the Atmosphere.* *By* Dr. Hermann Schlagintweit.

[Concluded from p. 16.]

Colour of the atmosphere. *Different kinds of Cyanometers. Alteration of the intensity of the blue with the height. Determinations with the tricoloured Cyanometer. Cloud colours.*

Colour of the Atmosphere.

THE blue colour of the sky, as well as the transparency of the atmosphere, deepens as we ascend. De Luc* has already noticed this. Saussure and Humboldt have published a long series of experiments on the subject. The instrument used by both was the cyanometer of Saussure. It consists of a number of strips of paper, washed over with different shades of prussian blue. The differences of shade are so regulated, that two strips, which at a certain distance could not be distinguished from each other, constituted divisions upon the scale. As normal distance, Saussure assumed that at which the black circle of a diaphanometer $1\frac{3}{4}''$ in diameter disappeared. Black was added by little and little until perfect black was obtained. At zero the scale was perfectly white, at the extreme end perfectly black. Within these two limits the scale was divided into 53 degrees. With this the colour of the sky was compared, and the nearest degree was set down as the expression thereof†.

* *Modifications de l'Atmosphère,* vol. iv. § 117, p. 930.

† As an example of Saussure's degrees, we may mention that the mean position of his cyanometer amounts—

For Germany, to	15°—17°
For the torrid zone	20—24
On Mont Blanc	39

Humboldt found (*Tableau Physique,* p. 103)—

In the tropics	23°
On the peak of Teneriffe	41
On the Andes at 3000 toises	43

which has also been observed by Gay-Lussac.

To attain a more varied change of tint, Parrot* made use of a rotating disc on which were laid sectors of prussian blue; he thus obtained a mixed colour capable of far greater modification. His instrument was also divided into degrees. It seems, however, very difficult to obtain instruments of both descriptions which are quite capable of being compared with each other. After some experiments, we found it advantageous to apply the colours in a different manner; and instead of expressing the tint in degrees, to express it according to the proportions of the mixture. We constructed two cyanometers, the first was of the same form as that used by Parrot. A disc 20 centimetres in diameter was covered with a layer of white lead, a substance which, when properly manufactured, possesses everywhere the same degree of whiteness, whereas different descriptions of white bleached paper vary greatly from each other in this respect. The rim was divided into 100 degrees (1° being =3·6 of the usual divisions), and by means of these the whole surface was divided into distinct sectors. This disc was fixed upon another of pasteboard by means of little supports, which sustained the centre and the rim merely. The rest of the space between both discs was hollow. From three points situated 33·3 of the rim divisions apart (120° in the common sense), a knife was drawn along the corresponding radii. Through the slits thus formed, blue and black segments could be pushed in until the required portion of them was visible upon the surface; the remaining portion slid into the hollow space between the discs. The blue segments were coloured by a layer of cobalt (oil colour) carefully laid on; on the others was placed a layer of raven-black (oil colour). These colours can be found everywhere, and exhibit such slight deviations of shade that they may be regarded as constant. Pl. I. fig. 3 exhibits the mechanical arrangement of this apparatus; the section of it is given at B. *a* is the plate of paper on which the layer of white lead is laid; *b* is a disc of pasteboard parallel to the latter; at *c* are the sections of the supports which connect both discs at the centre and rim; *d* is the projecting periphery which carries the graduation; *e* is a small cylinder of wood, 2 centimetres long, which is fast glued behind. Around this passes a strap, which being pulled downwards, imparts a rotary motion to the disc sufficiently quick, and of sufficiently long continuance, to permit of comparing the disc with the portion of the firmament to be investigated. The screw *f* holds the instrument fast to the upright which supports it during the rotation; at *g* are plates used to strengthen the apparatus.

In fig. A the surface of the cyanometer, as fitted for experiment, is represented. The blue sectors partially cover the white

* *Physik der Erde*, § 278, p. 102.

surface. As the radii of the sectors are the same as those of the disc, the exposed surfaces of both are proportional to the number of degrees embraced by the circular contours. We have in the present case—

$Blue_1$	15	parts
$Blue_2$	13	...
$Blue_3$	5	...
The remainder of White	67	...
Sum	100	...

If the disc be now set in rotation, we shall obtain a mixed colour the same as if we had blended—

33 per cent. of blue,
and 67 per cent. of white

most intimately together. It will be afterwards seen, that in this way a colour may be obtained, which, although it approaches very near to that of the portion of the firmament under examination, still does not necessarily possess that tone which we denominate the colour of the air. It would thus be possible to attain the brightness corresponding to the position of the instrument shown in the figure in another manner, that is, by omitting $blue_3$ and setting in its place a sector of black (of course much lighter). For the simplicity of the process and the comparability of the results, we have found it more advantageous never to use black as long as pure cobalt, which itself is a very dark colour, was not lighter than the firmament*.

As the setting up of the apparatus and the rotation of the disc demanded considerable time, we found it convenient to have an instrument similar to that of Saussure, that is to say, coloured strips of paper, with which, however, neither the prussian blue on the white paper, nor a division into degrees, was made use of, but which was so arranged that the per-centage of cobalt could be immediately ascertained. In the construction we proceeded in the following manner:—

A uniform cylindrical glass syringe was divided into 300 equal volumes, and then filled alternately with Kremser white and carefully prepared pure cobalt (both oil colours and of the same consistency); a series of equal volumes of white and cobalt were now placed beside each other on a palette. We had thus constant colours, capable of being easily imitated by subsequent experimenters. Oil colours, further, permit of being very inti-

* Compare Arago's ingenious cyanometer, in which a plate of quartz, cut perpendicular to the axis of the crystal, is used for the production of the blue with which the colour of the sky is to be compared.—*Annales de Chimie*, vol. iv. p. 98.

mately mixed, and of being uniformly laid on the surface, which in our case was that of weakly-sized Bristol-board. In this way we obtained fifteen divisions of a scale; the first of which was white, and the last pure cobalt. The difference from one division to the next was a matter of indifference in the application of the instrument, as the per-centage content of cobalt and not the number on the scale was noted. The increase of cobalt from one division to that next to it was not uniform. We endeavoured to have the differences of shade from leaf to leaf tolerably alike; and here we remarked, that a uniform addition of cobalt becomes less appreciable when a considerable quantity of the colour is already present. In the last leaves, therefore, we used a greater proportion of cobalt than in the first; the immediate object of the latter was to render the instrument more uniform.

Cyanometrical experiments are, in general, determinations of the brightness rather than of the colour; it is, however, of some interest to investigate the shades of the latter a little more closely. A mixture of white and cobalt cannot fully accomplish this. The most direct way of proving this, is by looking at a landscape painted in oil, where only white and blue are used in the treatment of the sky. An addition of red or yellow is always necessary. As the shades of colour exhibit considerable changes, it seemed to us not unimportant to determine their relations, at least approximately, for different elevations. The colour which is generally added to complete the sky tone is light ochre (hydrate of iron); this unfortunately is a colour which, strictly speaking, cannot be regarded as constant in all manufactories. But the smallness of the quantity used, which never exceeded 11 per cent., served to render the disturbance arising from this less appreciable.

In the construction of this second scale, and of a third for the colour of the clouds, we have been assisted by the advice of that distinguished landscape-painter, M. A. Zwengauer of München, to whose kind and friendly support we take this opportunity of expressing our deep obligation.

The basis of the tricoloured cyanometer, consisting of a union of cobalt, white and ochre, was formed by three different mixtures of the last two colours. The first consisted of 20 parts of white and 1 part of ochre; the second of 20 parts of white and 2 of ochre; and the third of 20 parts of white and 3 of ochre. To each of these separately were added 4, 8, 12, 20, and 50 parts of cobalt, so that for every tone we had five divisions of the scale; we had, therefore, fifteen divisions in all. In the formation of a scale for judging of the colours of the clouds, such a simple process could not be followed. The most suitable procedure appeared to us to be that of imitating the most marked colours which actually appear, and to give the per-centage content of these. The whole of the colours thus obtained were laid upon strips of strong paper, 2 centims. wide and 6 centims. long. These were fastened together in three small but rather wide books, so that they took up in each case three of the edges, while a small uncoloured strip within contained the description of the mixture. The coloured papers were separated, each from its neighbour, by an interposed sheet, as otherwise the sudden opening of the book would be accompanied by a separation of the colour. The same series might also be attained with the rotating disc, if, instead of the third blue sector, one of ochre was substituted. The latter process, however, was more rarely resorted to in the determination of the colours of the clouds than in finding the depth of the atmospheric blue. In the case of clouds, the greater number of compound colours would have delayed the experiments too much; and the result, on account of the subjective peculiarities of the eye itself, would still be only approximative.

In stating the observations, we will begin with the simplest, that is to say, with those in which the mere per-centage of cobalt is given. Besides the zenith, which was in every case examined, we sometimes made determinations of the side portions of the firmament. The latter were always so chosen, that they lay directly opposite the position of the sun at the time. Their zenith distance is contained in the sixth column. The experiments were made in 1847 and 1848.

Observations with the Cyanometer of two Colours (No. I.).

Number.	Month.	Place of observation.	Height in P. F.	Per-centage of cobalt in the zenith.	Zenith distance.	Per-centage of cobalt.	Remarks.
1—4	Aug.	Summit of the Grossglockner.	12,158	92 Co.	50°	70 Co.	Ultramarine alone was too bright in the zenith.
					60	51 Co.	The same from here downwards to the horizon.
					80	14 Co.	
5	Sept.	Summit of the Wildspitze.	11,489	(64 Co.)	......	......	The horizon was for the most part clouded. This point also was not entirely free from cirri.
6	Sept.	Summit of the Similaun.	11,135	81 Co.	......	......	The weather on the following day was dull. During the experiment no disturbance was remarked.
7, 8	Sept.	Rachern	10,362	84 Co.	54	30 Co.	Beautiful clear weather.
9	Aug.	Todtenlöcher Pass	10,340	81 Co.	......	......	Single blue patches between clouds.

Table (*continued*).

Number.	Month.	Place of observation.	Height in P. F.	Per-centage of cobalt in the zenith.	Zenith distance.	Per-centage of cobalt.	Remarks.
10—20	Aug.	Johannishütte......	7581	65 Co.	50°	48 Co.	Fine weather.
	Sept.	do.	......	......	60	30 Co.	Some cumuli.
		do.	do.	64 Co.	...	......	Quite clear.
		do.	do.	(51 Co.)			
		do.	do.	74 Co.	...	......	Very cold and dry.
	8 A.M.	do.	do.	66 Co.	...	......	Clear.
	7 A.M.	do.	do.	33 Co.	...	......	Slight vapour; no clouds.
	9 A.M.						
	8 A.M.	do.	do.	......	40	(69 Co.)	Blue patch between clouds.
	10 A.M.	do.	do.	67 Co.			
	5 P.M.	do.	do.	(39 Co.)	...	......	Commencement of the evening red in the west; somewhat vapoury.
21—23	Aug.	do.	do.	65 Co.	45	48 Co.	Very weak, evening red.
	Sept. 6 P.M.	do.	do.	......	60	33 Co.	
24, 25	Aug. 6 A.M.	Near the Wallnerhütte.........	7219	40 Co.	70	23 Co.	Very clear day.
26	Sept.	Great Oetzthaler glacier, left bank	6920	56 Co.	...	......	Clear sky after a fall of snow.
27—29	Sept.	Hintereishütte......	6792	57 Co.	60	45 Co.	Unsettled weather.
		do.	......	54 Co.	...	......	
30	Sept.	Kiepeler mountain.	6498	43 Co.			
31—33	Sept.	Vent	5791	47 Co.	50	30 Co.	Very clear after three days' snow.
				41 Co.			
34—39	Aug.	Heiligenblut	4004	48 Co.	60	29 Co.	Clear.
	Aug.	do.	do.	(52 Co.)	...	......	Blue patch between clouds.
	Aug.	do.	do.	41 Co.			
	Aug.	do.	do.	39 Co.	45	20 Co.	Vapours, but no clouds.
40—42	Aug.	Lienz	2310	39 Co.	...	......	Clear.
	Sept.	do.	do.	41 Co.	...	......	
		do.	do.	43 Co.	...	......	After rain and thunder.
43—46	Oct.	Bludenz	1670	(46 Co.)	45	(39 Co.)	Darkest moment during the eclipse of the sun on the 9th of Oct. 1847.
		do.	do.	(32 Co.)	45	(23 Co.)	After the eclipse.
47, 48	Oct.	Lake of Como......	700	(47 Co.)	...	......	Very clear; rain the day before.
		Middle of the lake..	......	(46 Co.)	*		

In comparing the results given by the cyanometer for higher and lower situations, it will be most advantageous to choose those which may be regarded as maxima, as these only are free from disturbances, and hence most capable of being compared with

* The zenith distances were determined by a suitable instrument, as the apparently compressed shape of the firmament always causes considerable error in mere estimation. At great elevations the firmament appears much lower than from lower positions.

each other. The remaining results appear between parentheses in the table. To these belong, for example, No. 5, the Wildspitze, evidently too little blue. For the graphic representation, and for the calculations which follow, the arithmetic mean of the observations from No. 11 to 20 is taken, the following excepted:—Nos. 14, 17, 20, all of which are too low. No. 18 deserves a little notice here. Considering its lateral position, the result given is too high; but this scarcely justifies the conclusion, that on this day a point in the zenith would have been much darker than on the other days, had the clouds permitted us to observe it. We are inclined to believe that the depth of this "blue patch" between clouds was an optical illusion, created by the contrast with the bright surrounding clouds. This explanation suggested itself to us immediately after the experiment, and induced us in the determinations of colour to choose the freest situations possible. With the exception of No. 36, the arithmetic mean of the observations made at Heiligenblut is taken. Nos. 43 and 46 (Bludenz) are not taken into account, because after the eclipse the sky was somewhat obscured by clouds. In like manner the Lake of Como is omitted, on account of its position being more southern than those of the other stations. The remaining observations are united to a curve in fig. 4.

Somewhat more regularly formed, after the manner of a broken line, for every 1000 feet of ascent the following values are given:—

2,000	Par. feet.	40	per cent. cobalt.	Diff.
3,000	...	41	...	1
4,000	...	43	...	2
5,000	...	45	...	2
6,000	...	47	...	2
7,000	...	55	...	8
8,000	...	64	...	9
9,000	...	72	...	8
10,000	...	80	...	8
11,000	...	87	...	7
12,000	...	92	...	5

These numbers exhibit—

1. A very slow ascent at the lower end of the curve.
2. A quick ascent between 6000 and 10,000 feet.
3. A new but inconsiderable diminution of the increase, from 10,000 feet upwards.

The first is due to the same cause as that of the light colour in the vicinity of the horizon, that is, to a mixture of watery vapour which collects in the valleys on account of the evaporation from the bottom and sides*. The sudden acceleration

* A similar brightening of the atmosphere by watery vapour is also exhibited on the sea-shore; the sky towards the land is always darker than towards the sea.—Humboldt's *Voyages*, vol. ii. p. 123.

of the increase at heights above 6000 feet coincides, in the Alps, with the disappearance of the larger valleys. From this forwards the alterations, as we ascend, are very uniform*. A corresponding point of greater acceleration is not therefore to be expected at the same height on mountains whose mean altitudes are unlike. This point would move upwards as the heights of the mountains increase. The latter assertion is corroborated by a comparison of the observations made by Alexander von Humboldt on the Andes and on the Alps†.

We have heretofore confined ourselves to a comparison of the maxima depths of colour at different elevations; although a regular increase is here exhibited, the higher and lower situations approximate very near with regard to the degree of shade to which the blue of the sky can sink. We observed in some instances, at a height of more than 7000 feet, 35 per cent. and less of cobalt when no trace of cloud or fog was to be perceived either with the naked eye or with the telescope. A singular clearness of the heavens is observed in the zenith itself, even at the greatest elevations, at the beginning of the morning twilight. The minimum occurs on ordinary days between 2 and 3 o'clock in the morning, the maximum a little before noon; the firmament is afterwards in general lighter on account of the ascent of vapours.

The difference between the maximum and the minimum brightness within twenty-four hours increases with the height, because the minima in high and in low situations are very similar; for the same reason the differences between max. and min. are greater in the tropic regions than in higher latitudes‡.

* How considerably the presence of valleys can alter the increase of blue as we ascend is very evident from the observation of Saussure, that Chamouni has often a less deeply coloured sky than Geneva.

† With regard to the different attempts made to explain the colour of the atmosphere, see the elaborate memoir of Forbes, "The Colours of the Atmosphere with reference to a previous paper, 'On the colour of Steam under certain circumstances.'"—Philosophical Magazine, vol. xiv. pp. 121, 419. And Pogg. *Ann.* 1842; Supplementary volume, vol. i. pp. 49–78. Compare also the interesting memoirs of Clausius, "Ueber die Natur derjenigen Bestandtheile der Erdatmosphäre durch welche die Lichtreflexion in derselben bewirkt wird."—Pogg. *Ann.*, vol. lxxvi. pp. 161–188; and "Ueber die blaue Farbe des Himmels und die Morgen und Abendröthe."—Pogg. *Ann.*, vol. lxxvi. pp. 188–195. It is shown in the last memoir, that in the reflexion of light from thin plates the blue has the advantage; in the transmission of the light, the orange has the advantage. That the blue of the heavens is due to reflected light is also corroborated by experiments on polarization. In this respect there is scarcely any difference observed between high and low situations. Forbes, who examined the polarization on the summit of the Jungfrau, found it "normal, but somewhat less strong than in the depths below."—Désor. *Excursions*, p. 405.

‡ Alex. von. Humboldt's observations, *Voyages*, vol. ii. p. 123, and vol. xi. p. 13.

If the side portions of the heavens be examined, a decrease in the depth of blue is observable which does not seem to be irregular. Alexander von Humboldt's observations on the Atlantic Ocean (18° 53′ N. L.)—at a place, therefore, where lateral disturbances through local changes of temperature, so common in mountains, was not to be feared—proved that the blue colour varied nearly as the cosine of the zenith distance *. In communicating these observations, we retain the original division of the cyanometer of Saussure into degrees.

Height.	Cyanometer.		Difference.
	Observed.	Calculated.	
90°	22°·4	23°·4	+1·0
70	22·4	22·0	−0·4
60	21·0	20·3	−0·7
50	18·3	18·0	−0·3
45	15·5	16·6	+1·1
30	12·0	11·7	−0·3
20	8·5	8·0	−0·5
10	4·0	4·1	+0·1

The same law may be recognised through many of our observations, but rarely with this exactitude. The zenith distance must be always measured by a proper instrument, and not estimated by the eye alone. In the latter case the peculiar shape of the dome of the firmament might be the source of considerable error. Thus by contemporaneous observations made on the Johannishütte, we found—

Series A.

Zenith distance.	Observed.	Calculated.
0°	65 per cent. cob.	65 per cent. cob.
30	48 ...	56 ...
60	30 ...	32·5 ...
Series B.		
0	65 per cent. cob.	65 per cent. cob.
45	48 ...	46 ...
60	33 ...	32·5 ...

The similarity between these results and those in the foregoing table will appear more evident when it is remembered, that one degree of Saussure corresponds to 3 or 4 per cent. of cobalt in our case. The unequal increase between every two of Saussure's degrees, when the latter are expressed in per-centage of cobalt (compare p. 95 above), can have no disturbing influence here.

* Humboldt's *Voyage*, vol. ii. p. 122. Observed on June 30, 1799.

On the Grossglockner also, by observing the zenith and the lowest visible point of the horizon, we found a striking coincidence with the law above mentioned; we there found—

Zenith distance.	Observed.	Calculated.
0°	92 per cent. cob.	92 per cent. cob.
80	14 ...	16 ...

But between these limits the increase was not quite so regular. Thus we found at a zenith distance of 50°, 70 per cent. of cobalt, whereas the calculation gives only 59; an error, however, which at this altitude scarcely exceeds that due to a single degree of Saussure's cyanometer. When the firmament is observed from deep valleys, the lateral intensity of the blue colour is very irregular. In this case local vapours amass themselves, and cause the observed depth of hue to be much less than that obtained from calculation.

Pursuing the method already described, we have also attempted to ascertain the quantity of yellow and reddish colour (ochre) which enters into the composition of the sky. The quantity of the latter naturally depends on the colour of the cobalt and white, as these themselves are not absolutely pure colours. This combination of more than simple blue and white has also the advantage, that by it we are enabled to determine the brightness itself with greater certainty. The coincidence of shade between the actual and the artificial colours greatly facilitates the comparison of both.

Table of Observations with the Tricoloured Cyanometer (No. II.).

No.	Month and hour.	Place of observation.	Zenith distance.	Per cent. of colours.			Remarks.
49—51	Aug.	Peak of the Grossglockner (12,158P. F.).	0°	92 Co.	1 Och.	7 W.	In the neighbourhood of the horizon the colour was the same as at 80° zenith distance.
			50	69 Co.	3 Och.	28 W.	
			80	12 Co.	10 Och.	78 W.	
52, 53	Sept.	Rachern (10,362 P. F.)	0	78 Co.	5 Och.	17 W.	Beautiful clear weather.
			54	29 Co.	7 Och.	64 W.	
51—61	Aug. Sept.	Johannishütte (7581 P. F.)...	0	64 Co.	3 Och.	33 W.	Fine weather; some cumuli.
	8 A.M.	....	4	74 Co.	2 Och.	24 W.	Blue patch between clouds.
			40	69 Co.	3 Och.	28 W.	
	9 A.M.	....	0	32 Co.	3 Och.	65 W.	Light vapour. [red.
	5 P.M.	....	0	36 Co.	3 Och.	61 W.	Commencement of evening
			0	45 Co.	7 Och.	48 W.	Weak evening red did not quite attain a height of 30° above the horizon.
	6 P.M.	....	45	20 Co.	8 Och.	72 W.	
			60	16 Co.	8 Och.	76 W.	
62, 63	Aug. 6 A.M.	Near the Wallnerhütte (7219 P. F.)	0	39 Co.	3 Och.	58 W.	Day very clear.
			70	27 Co.	4 Och.	69 W.	
64, 65	Aug.	Heiligenblut (4004 P. F.)...	0	47 Co.	3 Och.	50 W.	Fine. [sphere.
			0	42 Co.	2 Och.	56 W.	Some cumuli in the atmo-

The foregoing table shows, that for different points of the same vertical circle the decrease of the blue is accompanied by a decided increase of the ochre. The ochre appeared strongest at the horizon of the Grossglockner. The atmosphere in this portion had therefore a slight tint of green*, which resulted from the blending of the three colours, blue, yellow and white.

White objects seen from a distance have always a yellowish or reddish tint imparted to them by the atmosphere. This is plainly observable on clouds, houses, snow-covered slopes, &c. It is a general rule, that, in the painting of such objects, a little ochre must be added to the white. The colour of the brightest cloud-masses, even when the sun is in a high position, contains generally from 1 to 2 per cent. of ochre†. Distant mountains often appear blue when the sun is opposite; their own colour seems to have some influence in this case, as the same mountains in winter when covered with snow show a reddish-white colour‡. Those summits of the Alps which are covered with perpetual snow, when seen from a great distance in direct sunlight, exhibit this reddish tinge blended with the whiteness.

The colouring of the light by its transmission through the atmosphere is peculiarly remarkable in the hues exhibited by the sky at daydawn and at sunset—the morning and the evening red. Forbes§ was the first to connect these beautiful colours with the existence of watery vapour in a certain state of condensation. The phænomenon of the morning and evening red is of too intense and changeable a nature to be investigated by means of our cyanometer. The evening glow of the Alps is peculiarly well known as a splendid exhibition of the evening red. It begins soon after sunset; the precipices and snow-crowned summits assume a dazzling glow, which disappears almost instantly when the shadow of the earth has attained the heights. A second glow is often observed, particularly in the more southern alpine groups; on Mont Blanc, Monte Rosa, &c. it is exhibited

* A strong green colour (grass- or bottle-green) may sometimes be observed, at considerable elevations above the horizon, on clouds and mountain peaks when glowing with intense red. This has been often observed by Brandes and others. The phænomenon is merely a subjective colouring, occasioned by the wearying of the eye in gazing on the shining red. The complementary green is observed more frequently and plainly when the eye is directed, not on the firmament, but upon white objects.

† The clouds sometimes exhibit a very dark hue—thunder-clouds, for example. Sometimes the very finest of them cause important alterations in the colour of the heavens, without being recognizable as distinct groups. Humboldt has also observed such masses.—*Voyages*, vol. iii. p. 318. 4to.

‡ Compare also Saussure's *Voyages*, vol. iv. § 2088, note 1.

§ "On the colour of Steam under certain circumstances."—Philosophical Magazine, vol. xiv. pp. 121, 419; and Pogg. *Ann.*, vol. xlvii. p. 593; and supplementary volume, vol. i. 1842, p. 49. The last memoir contains an extensive collection of the earlier notions entertained upon this subject.

in great splendour. On the masses of dolomite in the Fassathal we observed the same twice.

A related phænomenon, which we had the opportunity of observing, deserves to be mentioned here. From the crest of the Wildspitze, on the 18th of September 1847, we had a fine prospect towards the north. The entire series of the northern limestone alps, from Salzburg to the Bodensee, was unfolded before us with extraordinary clearness. In the mean time a storm blowing towards the north increased in violence, and before we attained the summit (11,489 P. F.) the northern mountains exhibited an extraordinary colour. They had obtained a decidedly red tone, although the sun stood high, it being but 3 o'clock in the afternoon. We had left the summit scarcely half an hour, when immense cloud-masses were driven upon us from the side at which the red colouring had been observed. During this time the barometer fell considerably. It seemed as if the watery vapour of the atmosphere, during its gradual condensation to mist, had occasioned the redness in the same manner as the morning and the evening red is produced. For the observation of this phænomenon, it is first of all necessary that large masses of air should lie between the observer and the object; in the present case the distance amounted to eleven or twelve miles (German). It is only from a high position that objects distant enough, and with surfaces large enough to exhibit the modification of colour, can be observed. Opportunities to see the phænomenon occur but rarely, as it is but seldom that the observer finds himself at such elevations during similar states of the weather. The colour was not the shining red of the evening, but more of a purplish-blue tinge, undimmed by fog of any kind, and in the production of which the gray colouring of the limestone masses had a share.

Direct sunlight, when it passes through mist, has also a red tone imparted to it; but the colours of rocks, &c. being the products of reflected light, disappear long before the red tone can be assumed. We have in some cases endeavoured to determine the intensity of the red which occurs on the passage of the light through fog.

Colour of Fogs with transmitted Light.

No.	Height.	White.	Ochre.	Burnt ochre.	Remarks.
1	6510	91	4	5	Fog during a fall of snow.
2	7540	94	1	5	Covered in both cases the surface of the glacier.
3	7610	95	...	5	
4	8350	92	6	2	Separate round masses.
5	8250	97	2	1 (cob.)	Not very dense, disappeared a few hours afterwards. It reached from 8000 to 11,500 P. F.
6	7360	91	8	1	Tolerably dense.
7	7540	90	2	8	
8	7480	87	9	4	

The red colour in the present instance appeared sometimes more intense than is generally observed on plains. When the light falls upon the fog, it exhibits the usual uniform gray, similar to a mixture of 91 white with 9 per cent. black.

A few words now remain to be said upon the duration of the twilight. It is everywhere known in the Alps, that on high mountains the duration is longer, although this is sometimes over-estimated. It may be almost regarded as a tradition repeated for every mountain, even when it is but a few thousand feet high, that the evening and the morning twilight touch each other at midnight. Though this is an exaggeration, a difference in the duration of the twilight is very appreciable in the higher regions. As the horizon expands from an Alpine summit, it is evident that the higher we ascend the greater will be the arch which separates sunrise from sunset, and hence the longer the day. In valleys, on the contrary, it often occurs that the direct sunlight is held back by interposed mountains, and hence is present only a few hours of the day. The feeble twilight is also considerably diminished by the same cause; thus the position of valleys with regard to the horizon may be such, that night sets in very soon after the setting of the sun*. The twilight, in our latitude, continues on an average upon the plains until the sun has descended 18° under the horizon. Upon mountains the sun attains a much greater depth before the twilight departs*. It is difficult to express this with exactness, as the alterations in the transparency of the atmosphere on different days exercise so considerable an influence.

XV. *On a Method of obtaining a perfect Vacuum in the Receiver of an Air-pump.* *By* Thomas Andrews, *M.D., F.R.S., M.R.I.A.*†

THE space left vacant in the upper part of a long glass tube, which after being filled with mercury is inverted in a basin of the same metal, affords the nearest approach to a perfect vacuum which has hitherto been obtained. It is true that it contains a little mercurial vapour at the ordinary temperature of our summers, and probably also at lower temperatures; but the quantity is exceedingly small, and its influence in depressing the barometric column must be altogether inappreciable. Besides the mercurial vapour, a trace of air may generally be detected even in tubes which have been carefully filled, and in which the air interposed between the glass and mercury has been expelled

* Compare also Martin's *Monit. Univers.* 1844, p. 2796; and Kæmtz, *Lehrbuch de Meteorol.*, vol. iii. p. 50 and following.

† Communicated by the Author.

by ebullition. This is best observed by inclining the tube till the mercury comes into contact with the upper end, when any air that may have been diffused through the vacuum will be seen collected in a small bubble, but greatly rarefied. It is easy to calculate approximately the depression of the mercurial column produced by this residual air. For this purpose the tube must be inclined till the bubble is exposed to a pressure of a few inches of mercury, measured in a vertical direction. In this position its apparent diameter is measured, as also the pressure to which it is exposed. For the object in view, the volume of the bubble may be calculated on the assumption that it is a sphere. The space occupied by the vacuum must also be estimated; and with these data, the depression of the mercurial column may easily be calculated.

Let V be the volume of the space above the mercury when the tube is vertical;
p, the pressure under which the diameter of the bubble of air has been measured;
r, the semidiameter of the bubble;
x, the depression of the mercurial column.

Then
$$x = \frac{4}{3} r^3 . \pi p \frac{1}{V}.$$

If the diameter of the bubble $2r$ be 0·02 inch, the pressure p 2 inches, and the space V 1·2 cubic inch, the value of x is nearly 0·00001 inch; or the depression of the mercury, in consequence of the vacuum not being absolutely perfect, amounts only to $\frac{1}{100,000}$dth of an inch. It is easy in actual practice to realize this close approximation to a perfect vacuum. The quantities now stated apply, in fact, to a barometric tube employed in an experiment which will be subsequently described.

The Torricellian vacuum leaves therefore scarcely anything to be desired in point of completeness; but it is unfortunately applicable to very few physical investigations. No instrument of any kind can be introduced into it, nor even any substance which is acted on by mercury. The vacuum obtained by the exhausting pump is not liable to these objections; but even with machines of the most perfect construction, and in the best order, a very imperfect approach can be attained to a complete exhaustion. A good ordinary pump with silk valves seldom produces an exhaustion of 0·2 inch.; and it is very rare indeed, if the manometer is properly constructed, to have it carried to 0·1 inch. In his "Etudes Hygrométriques" (*Ann. de Chim.* 3rd Series, vol. xv. p. 190), M. Regnault has given the following method for pushing the exhaustion further after the valves have ceased to act. In a large glass globe of from 20 to 25 litres capacity

($4\frac{1}{2}$ to $5\frac{1}{2}$ English gallons), he places an hermetically sealed capsule of glass containing from 40 to 50 grms. of sulphuric acid. He also introduces into the globe 2 or 3 grms. of water, and exhausts till the water has entirely disappeared and the machine ceases to act. By agitating the globe, the capsule is ruptured; when the sulphuric acid coming into contact with the vapour of water, which has displaced nearly all the residual air in the receiver, condenses it and leaves a vacuum nearly perfect. This globe thus exhausted is next placed in communication with the apparatus in which a very perfect vacuum is desired, taking care to remove the air from the interior of the connecting tubes. On opening the stop-cocks, the air becomes uniformly diffused through the two spaces; and if the capacity of the globe is considerable compared with that of the other vessel, the elastic force of the air may be reduced to a small fraction of a millimetre. If, on the contrary, the capacity of the latter is considerable, this operation must be repeated several times.

This ingenious process is not adapted to give a very perfect vacuum in the second vessel, unless the operation be repeated several times, which would be exceedingly laborious. It is also liable to other difficulties in the execution, which will at once occur to any one accustomed to experiments of this kind. Besides, it does not afford the means of obtaining a vacuum, which, as far as the indications of a mercurial manometer can be observed, is perfect; as in M. Regnault's observations, the elastic force of the air was still capable of measurement, although only amounting to a small fraction of a millimetre.

By using the necessary precautions, a vacuum may be obtained by the following process, with very little trouble, in the ordinary receiver of an air-pump, so perfect that the residual air exerts no appreciable elastic force. Even after this limit has been reached, the exhaustion may be pushed still further, till it must become at last not less complete than the Torricellian vacuum; while at the same time, by suppressing the manometer, the existence of mercurial vapour may be altogether prevented. The manipulations required to arrive at this result will not interfere with the presence of the most delicate instruments in the receiver.

Into the receiver of an ordinary air-pump, which is not required to exhaust further than to 0·3 inch, or even 0·5 inch, but which must retain the exhaustion perfectly for any length of time, two open vessels are introduced, one of which may be conveniently placed above the other; the lower vessel containing concentrated sulphuric acid, the upper a thin layer of a solution of caustic potash, which has been recently concentrated by ebullition. The precise quantities of these liquids is not a matter of importance, provided they are so adjusted that the acid is capable of desiccating completely the potash solution without becoming itself notably diminished in strength, but at the same time does not expose so large a surface as to convert the potash into a dry mass in less than five or six hours at the least. The pump is in the first place worked till the air in the receiver has an elastic force of 0·3 or 0·4 inch, and the stop-cock below the plate is then closed. A communication is now established between the tube for admitting air below the valves and a gas-holder containing carbonic acid, which has been carefully prepared so as to exclude the presence of atmospheric air. After all the air has been completely removed from the connecting tubes by alternately exhausting and admitting carbonic acid, the stop-cock below the plate is opened and the carbonic acid allowed to pass into the receiver. The exhaustion is again quickly performed to about the extent of half an inch or less. If a very perfect vacuum is desired, this operation may be again repeated; and if extreme accuracy is required, it may be performed a third time. It is not likely that anything could be gained by carrying the process further. On leaving the apparatus to itself, the carbonic acid which has displaced the residual air is absorbed by the alkaline solution, and the aqueous vapour is afterwards removed by the sulphuric acid. The vacuum thus obtained is so perfect, that even after two operations it exercises no appreciable tension.

To give a clear conception of the progress of the absorption, I will describe in detail one observation in which the tension was measured simultaneously by a good syphon-gauge and by a manometer, formed of a barometric tube 0·5 inch in diameter, inverted in the same reservoir of mercury as a similar tube communicating with the interior of the receiver. The barometer had been carefully filled, and the depression of the mercury estimated by the method already described at less than $\frac{1}{100,000}$dth of an inch.

Previous to the admission of the carbonic acid, the exhaustion was carried only to 0·4 inch; it was again carried to 1 inch; and a third time to 0·5 inch, after which the apparatus was left to itself. The manometer indicated a pressure in—

15′	of	0·25	inch.
30′	...	0·17	...
80′	...	0·10	...
200′	...	0·02	...

In twelve hours the difference of level was just perceptible, when a perfectly level surface was brought down behind the tubes till the light was just excluded. In thirty-six hours not the slightest difference of level could be detected. The vacuum has remained without the slightest change for fourteen days.

It is evident that the only limit to the completeness of the vacuum obtained by this process, arises from the difficulty of preparing carbonic acid gas perfectly free from air. This may be very nearly overcome by adopting precautions which are well known to practical chemists. When an extreme exhaustion is required, the gas-holder should be filled with recently boiled water, and the first portions of carbonic acid that are collected in it should be allowed to escape.

The substitution of phosphoric for sulphuric acid would remove the possibility of either aqueous or acid vapours being present even in the smallest amount, but such a refinement will rarely be found necessary.

In the experiment just described, the theoretical residue of air would be $\frac{1}{135,000}$dth part of the entire quantity in the receiver, which would cause a depression of $\frac{1}{4500}$dth of an inch. This result must have been nearly realized. If the exhaustion had been carried at each time to 0·2 inch, the residue by theory would have been only $\frac{1}{3,375,000}$th part. But the experimental results will not continue to keep pace with such small magnitudes.

Queen's College, Belfast,
January 7, 1851.

XVI. *On the Polarization of Atmospheric Heat. By* Elie Wartmann, *Professor of Natural Philosophy in the Academy of Geneva*.*

The observations of M. Arago and Sir David Brewster have long since established, that the light by which our atmosphere is illuminated is polarized in certain directions. It might be supposed from analogy, that the heat proceeding from the same source is endowed with similar properties; the following experiments place this supposition beyond the pale of doubt.

The means of polarizing a ray of heat, without greatly diminishing its intensity, are less perfectly known than those of polarizing a ray of light, and the result is a corresponding inferiority in the exactitude with which the calorific ray can be analysed. In the use of the thermo-electric pile, the experimenter must be on his guard against numerous sources of error. The blackened face of the instrument radiates into space, and is cooled to a degree which depends partly upon the transparency of the air, partly upon its temperature. The other face, although protected by a closed tube, is not entirely free from the influence of conduction in prolonged experiments. The thermometric state of the atmosphere changes capriciously every moment, owing to the unequal mixture of the ascending and descending

* From the *Bibliothèque Universelle,* October 1851.

columns of air. The variations in the transparency of the air, the calorific reflexions which proceed from the surface of the earth and from the clouds, render in general the intensity of the heat radiating in any given direction extremely inconstant.

These obstacles being known, I endeavoured to combat them by the following arrangements:—The thermo-electric pile of Melloni was placed in a capacious chest, so that its uncovered face was turned towards an opening in the centre of one of the sides. This face is provided with its cone of polished brass fixed in a cylinder of wood, which is lined with a tube of pasteboard. The extremity of this tube enters the circular opening in the side, and moves in it with strong friction; screens and diaphragms of various substances can be attached to it. At its extremity, the piece destined to contain the analyser is fixed level. It carries a collar, to which the hand imparts a rotative motion by means of a strong handle, and which carries an index pointing to a dial, three decimetres in diameter, fixed against the chest. This piece is surrounded by a cylindrical case of white pasteboard blackened in the interior, six decimetres long, open in front, and destined to circumscribe the portion of space to be examined, the oblique rays being arrested.

The analyser which I made use of in my first experiments was a pile of thin plates of mica. It would have been easy to render it moveable round a line perpendicular to the axis of the thick pasteboard cylinder which enclosed it; but I preferred arresting it at an angle of 35° with this axis, and placed a similar pile parallel to it and six centimetres in advance. This assemblage polarizes and analyses the heat completely; it prevents the currents of air from acting upon the solders of bismuth and antimony, and destroys the radiation of those metals so effectually as to render all other preservative unnecessary. I afterwards replaced this portion of the instrument by a very large Nicol's prism constructed by M. Ruhmkorff. It is 0·086 of a metre in length; the greater diagonal of the base is 0·036 of a metre, and the lesser 0·028 of a metre*.

* With this apparatus I have repeated the experiments which I published in 1846, relative to the rotation of the plane of polarization of radiant heat under the influence of magnetism. A solar ray traverses a Nicol's prism 0·07 of a metre long, the diagonals of which measure 0·03 and 0·023 of a metre respectively. It then passes through a parallelopiped of heavy glass 0·029 of a metre long, and the square bases of which measure 0·0175 of a metre the side. This glass is made by Prof. Faraday, to whose kindness I am indebted for it. It was placed between the polar pieces of an electro-magnet, the soft iron cylinders of which were 0·09 of a metre in diameter, and carried nine layers of copper wire 0·003 of a metre thick, each layer consisting of sixty windings. This wire is brought into the circuit of a battery of ten of Grove's large cells. The ray arrives at the thermo-electric pile

The body of the pile is sheltered against variations of temperature by filling the entire chest with carded cotton. In the side opposed to that which contains the analyser is a rectangular glazed window, through which by means of a good thermometer the temperature of the envelope can be read off. Finally, a little hole pierced in the bottom permits of the passage of two wires from the poles of the pile to the rheometer. The whole is preserved in a place less warm than the surrounding atmosphere, so that during the experiments the pile must necessarily be affected by any accession of heat. The sense of the deviation of the rheometer serves to prove that this condition is fulfilled.

The chest furnished with horizontal axes of hard wood turns in a rectangular frame, which permits of the pile being retained at any angle whatever with the horizon, in the vertical plane which it describes. This angle of declination is estimated on an appropriate dial by means of a plummet and an index which follows the chest in its motion. The frame, in its turn, moves round a vertical foot, in which it is steadied by friction. The azimuths are read on a fixed horizontal dial, which permits of the adjustment of the apparatus. No magnetic metal ought to be used in the construction of the latter.

I have said that the temperature of the air is subjected to almost perpetual fluctuations, which cause corresponding variations in the thermo-electric current. To lessen this grave inconvenience, the pile was caused to act near the window of a closed room. In the reading of the rheometric deviations, it is better to determine the arcs described by the index at each change of the plane of analysis, than the positions at which it tends to come to rest after a number of excursions, which become less rapid the more nearly astatic is the system of needles. The results agree exactly with those deduced from fixed deflections, in those rare cases when the atmosphere is calm and permits of the operation being carried on in the open air, as also within doors.

The success of these researches depends also upon the goodness of the rheometer. I have obtained an excellent multiplier from M. Rhumkorff. It is composed of two short and thick wires rolled on a frame of bone. The dial is of pure copper, with its graduated circumference silvered. The needles, suspended from a fibre of silk extremely fine, and 0·15 of a metre

placed at a convenient distance, traversing the second Nicol's prism which serves as analyser. Although it has to pierce a total thickness of 0·185 of a metre of diathermic bodies, it retains the power to produce a current very appreciable by the rheometer. The difference of the value of deviation, according as the magnet is or is not in a state of activity, entirely confirms the results which I obtained three years ago.

in length, make only a single oscillation in twenty-four seconds. When the calorific radiations are weak, I found the compensator of M. Melloni to be of service, more especially as the object was not to obtain absolute measures, but the ratios of the deviations.

Operating in the manner just described, it is found there are two positions of the analyser 180 degrees apart, at which the deviations are equal and maximum; and two other positions at 90 degrees from the former and at 180 degrees from each other, at which the deviations are equal and minimum. The positions of the analyser, which for a given point of the heavens procure the maximum and minimum transmissions, correspond to those of greatest intensity of the direct bands and inverse bands of the polariscope of Savart*. They are thus determined without difficulty.

The atmospheric heat can be depolarized by means of a plate of mica placed near the extremity of the exterior tube, and perpendicular to the incident rays. The analyser being in the position of the minimum of transmission, the deviation of the index experiences no sensible diminution when the principal section of the interposed mica coincides with the plane of polarization, while the deviation is augmented when the rotation of the mica in its own plane brings its principal section to an angle of 45° with the primitive plane of polarization.

The phænomena of the polarization of atmospheric heat are much less apparent in winter than in summer. The difference is doubtless due to the want of sufficient sensibility in the apparatus, to the greater difficulty of experimenting at low temperatures, and to the small proportion of polarized rays which on the most favourable days accompany the natural heat. The serenity of the air exercises a very marked influence on this proportion, which becomes probably null when the heavens are obscured. Finally, it is easy to satisfy oneself, particularly if the atmosphere be calm and without clouds, that the polarization augments from the environs of the sun up to a certain limit, from which forward it decreases. I have found it inappreciable in the regions occupied by the neutral points.

It may be concluded from these researches, that *the heat and the light of the atmosphere proceeding from the sun are similarly polarized in the same circumstances.*

* I name *direct* bands those which are in the plane of polarization of the light which has traversed a plate of tourmaline; they present in the middle a black band between two white ones. The *inverse* bands, perpendicular to the primitive plane of polarization, present a central white band between two black ones.

XVII. *Notes on the Resolution of Equations of the Fifth Degree.* *By* G. B. Jerrard, *Esq.**

1. It is clear that an expression for a root of the general equation of the fifth degree must involve radicals characterized by each of the symbols $\sqrt[2]{\ }$, $\sqrt[3]{\ }$ and $\sqrt[5]{\ }$. If, however, we examine all the solutions which have hitherto been discovered of particular equations of that degree, we shall find that into none of them do cubic radicals enter. A great if not an unpassable barrier seems at first view to oppose their introduction. For how can cubic radicals arise unless there be a cubic equation? And how can there be a cubic equation, unless, in opposition to the well-known theorem of M. Cauchy, the number of different values of a non-symmetric function of five quantities can be depressed to three? I propose now to inquire whether the method which I have given in my "Reflections on the Resolution of Algebraic Equations of the Fifth Degree," will enable us to solve these questions.

2. Turning to No. 44, which contains the first application of that method, we find (see this Journal for June 1845, vol. xxvi. p. 572)—

"The equation of which

$$W_{f^{\backprime}}+{}^{1}R(W_{f^{\backprime}})$$

is a root will evidently be of the third degree. For omitting the parentheses connected with ${}^{1}R$, we see that

$${}^{1}RW_{f^{\backprime}}={}^{1}R^{2}W_{f^{\backprime}(\underset{\cdot\cdot}{\beta\epsilon})}=W_{f^{\backprime}(\underset{\cdot\cdot}{\beta\epsilon})},$$

the exponent, as is usual, indicating a repetition of an operation; and that consequently the root in question will not be affected by writing $f^{\backprime}(\underset{\cdot\cdot}{\beta\epsilon})$ instead of $f^{\backprime}$.

"We must also have

$$(W_{f^{\backprime}}+{}^{1}RW_{f^{\backprime}})(\underset{\cdot\cdot}{ab})(\underset{\cdot\cdot}{cd})\ldots=(V_{F})(\underset{\cdot\cdot}{ab})(\underset{\cdot\cdot}{cd})\ldots,$$

when $(\underset{\cdot\cdot}{ab})(\underset{\cdot\cdot}{cd})\ldots$ takes the form $(\underset{\cdot\cdot}{ab})(\underset{\cdot\cdot}{ab})$; but not for all values of a, b, c, d, . . : since the method of continuous substitutions will not generally be applicable to processes based upon the theorem (v, w.), which is, as we must remember, hypothetical in itself.

"Hence I conclude that there will be an equation of the third degree with given coefficients simultaneous with the equation $V^{15}+C_1V^{14}+\ldots=0$, which cannot be depressed [by any further equalization of its roots] below the 15th degree without inducing certain relations among $A_1, A_2, \ldots A_5$."

I proceed to verify a result apparently so inexplicable.

* Communicated by the Author.

3. From the form which the equation for V first assumes, we see that we must have (p. 564)

$$\left.\begin{array}{l}(V-V_F)\quad(V-V_G)\quad(V-V_H)\times\\(V-V_{F(\alpha\beta)})(V-V_{G(\alpha\beta)})(V-V_{H(\alpha\beta)})\times\\\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\\(V-V_{F(\alpha\epsilon)})(V-V_{G(\alpha\epsilon)})(V-V_{H(\alpha\epsilon)})\\\qquad\qquad=0;\end{array}\right\}\quad\ldots\quad(o')$$

V_G and V_H denoting what V_F becomes when f is changed successively into g and h.

Let us examine the five sets of factors which compose the function here presented to us.

4. Resuming the equation

$$V_F=P_f+P_{f(\beta\epsilon)_n}+P_{f'}+P_{f'(\beta\epsilon)_n},$$

and observing that

$$f_\alpha=y_\epsilon+y_\beta+a_\iota y_\alpha,$$

we may instantly perceive that the eight functions

$$\begin{array}{ll}V_F, & V_{F'},\\V_{F(\beta\epsilon)}, & V_{F'(\beta\epsilon)},\\V_{F(\gamma\delta)}, & V_{F'(\gamma\delta)},\\V_{F(\beta\epsilon)(\gamma\delta)}, & V_{F'(\beta\epsilon)(\gamma\delta)},\end{array}$$

will be equal to each other; $(\beta\epsilon)$ and $(\gamma\delta)$ being the complementary interchanges relatively to f_α.

Again, it is clear that the equations

$$V_G=P_g+P_{g(\gamma\epsilon)_n}+P_{g'}+P_{g'(\gamma\epsilon)_n},$$

$$g_\alpha=y_\epsilon+y_\gamma+a_\iota y_\alpha$$

will furnish a corresponding set of eight equal functions; the complementary interchanges, which in this case must be taken relatively to g_α, being $(\gamma\epsilon)$ and $(\beta\delta)$.

And a similar result is obtainable from the equations

$$V_H=P_h+P_{h(\delta\epsilon)_n}+P_{h'}+P_{h'(\delta\epsilon)_n},$$

$$h_\alpha=y_\epsilon+y_\delta+a_\iota y_\alpha.$$

We shall thus have twenty-four functions distributed in three groups, consisting each of eight functions.

Analogous groups must also exist for $V_{F(\alpha\beta)}$, $V_{G(\alpha\beta)}$, $V_{H(\alpha\beta)}$, for $V_{F(\alpha\gamma)}$, $V_{G(\alpha\gamma)}$, $V_{H(\alpha\gamma)}$, . . and for $V_{F(\alpha\epsilon)}$, $V_{G(\alpha\epsilon)}$, $V_{H(\alpha\epsilon)}$;

any root, x_τ, continuing fixed during the formation of the same set of groups.

5. If now we represent by

$$\text{ס}(\mathrm{V}_{\mathrm{F}(\underset{\cdot\cdot}{\alpha v})},\ \mathrm{V}_{\mathrm{G}(\underset{\cdot\cdot}{\alpha v})},\ \mathrm{V}_{\mathrm{H}(\underset{\cdot\cdot}{\alpha v})})$$

any symmetric and rational function of $\mathrm{V}_{\mathrm{F}(\underset{\cdot\cdot}{\alpha v})}$, $\mathrm{V}_{\mathrm{G}(\underset{\cdot\cdot}{\alpha v})}$, $\mathrm{V}_{\mathrm{H}(\underset{\cdot\cdot}{\alpha v})}$*; on supposing, as is permitted, x_α, x_β, x_γ, x_δ, x_ϵ successively to become fixed, we may very readily obtain

$$\left.\begin{array}{l}
\text{ס}(\mathrm{V}_{\mathrm{F}},\quad \mathrm{V}_{\mathrm{G}},\quad \mathrm{V}_{\mathrm{H}})={}^1r(x_\alpha),\\
\text{ס}(\mathrm{V}_{\mathrm{F}(\underset{\cdot\cdot}{\alpha\beta})},\ \mathrm{V}_{\mathrm{G}(\underset{\cdot\cdot}{\alpha\beta})},\ \mathrm{V}_{\mathrm{H}(\underset{\cdot\cdot}{\alpha\beta})})={}^2r(x_\beta),\\
\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\\
\text{ס}(\mathrm{V}_{\mathrm{F}(\underset{\cdot\cdot}{\alpha\epsilon})},\ \mathrm{V}_{\mathrm{G}(\underset{\cdot\cdot}{\alpha\epsilon})},\ \mathrm{V}_{\mathrm{H}(\underset{\cdot\cdot}{\alpha\epsilon})})={}^5r(x_\epsilon);
\end{array}\right\}\quad .\ .\quad (\Sigma)$$

${}^1r$, ${}^2r$, .. ${}^5r$ being expressive of rational functions.

6. Hence it appears that the equation (o$'$) may take the form

$$\left.\begin{array}{l}
\left(\mathrm{V}^3+{}^1r_1(x_\alpha)\mathrm{V}^2+{}^1r_2(x_\alpha)\mathrm{V}+{}^1r_3(x_\alpha)\right)\times\\
\left(\mathrm{V}^3+{}^2r_1(x_\beta)\mathrm{V}^2+{}^2r_2(x_\beta)\mathrm{V}+{}^2r_3(x_\beta)\right)\times\\
\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\\
\left(\mathrm{V}^3+{}^5r_1(x_\epsilon)\mathrm{V}^2+{}^5r_2(x_\epsilon)\mathrm{V}+{}^5r_3(x_\epsilon)\right)\\
\qquad\qquad=0.
\end{array}\right\}\quad .\ .\quad (\mathrm{o}'')$$

It only remains therefore to investigate the nature of the rational functions designated by ${}^1r_1$, ${}^1r_2$, .. ${}^5r_3$.

7. Now the first member of the equation just arrived at must, when expanded, be capable of coinciding throughout its whole extent with the function $\mathrm{V}^{15}+\mathrm{C}_1\mathrm{V}^{14}+\mathrm{C}_2\mathrm{V}^{13}+\,..\,+\mathrm{C}_{15}$. Each therefore of the fifteen coefficients arising from such an expansion must be a symmetric function of the roots x_α, x_β, .. x_ϵ.

Whence it is manifest that either

$${}^1r_n={}^2r_n={}^3r_n={}^4r_n={}^5r_n,\qquad .\ .\ .\ .\quad (\sigma_1)$$

or†

$$\left.\begin{array}{l}
{}^1r_n(x_\alpha)={}^1r_n(0),\\
{}^2r_n(x_\beta)={}^2r_n(0),\\
\cdot\quad\cdot\quad\cdot\\
{}^5r_n(x_\epsilon)={}^5r_n(0),
\end{array}\right\}\quad .\ .\ .\ .\ .\ .\ .\quad (\sigma_2)$$

n being equal to any number in the series 1, 2, 3.

* ס is the Hebrew letter sâ′-mĕkh.

† By $r(0)$ I mean the absolute term of the series for $r(x)$ when reduced to its most simple form $a+bx+cx^2+dx^3+ex^4$. Thus $r(0)$ is here equal to a.

In which, then, of these two ways will the coincidence take place?

8. We shall see that it is the second system (σ_2) which must generally obtain*.

In effect, if, in the formation of the groups, we suppose the roots to become fixed in the order x_ϵ, x_α, x_β, x_γ, x_δ, there will arise a new system of equations,

$$\left.\begin{array}{l} \mathfrak{D}(V_F, \quad V_G, \quad V_H) = {}^1\dot{r}(x_\epsilon), \\ \mathfrak{D}(V_{F(\ddot{\alpha\beta})}, V_{G(\ddot{\alpha\beta})}, V_{H(\ddot{\alpha\beta})}) = {}^2\dot{r}(x_\alpha), \\ \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \\ \mathfrak{D}(V_{F(\ddot{\alpha\epsilon})}, V_{G(\ddot{\alpha\epsilon})}, V_{H(\ddot{\alpha\epsilon})}) = {}^5\dot{r}(x_\delta)\,; \end{array}\right\} \quad . \quad . \quad (\dot{\Sigma})$$

wherein ${}^1\dot{r}$, ${}^2\dot{r}$, .. ${}^5\dot{r}$ are also expressive of rational functions.

Accordingly, on comparing (Σ) and $(\dot{\Sigma})$, we shall find

$$\begin{array}{l} {}^1r(x_\alpha) = {}^1\dot{r}(x_\epsilon), \\ {}^2r(x_\beta) = {}^2\dot{r}(x_\alpha), \\ \cdot \quad \cdot \quad \cdot \\ {}^5r(x_\epsilon) = {}^5\dot{r}(x_\delta). \end{array}$$

But the roots of the general equation of the fifth degree do not admit of being inseparably linked together in any such system of rational expressions. It is certain therefore that the two sets of functions, ${}^1r(x_\alpha)$, ${}^2r(x_\beta)$, .. ${}^5r(x_\epsilon)$, and ${}^1\dot{r}(x_\epsilon)$, ${}^2\dot{r}(x_\alpha)$, .. ${}^5\dot{r}(x_\delta)$, must merge into ${}^1r(0)$, ${}^2r(0)$, .. ${}^5r(0)$.

9. Thus the equation (o'') will become

$$\left.\begin{array}{l} \left(V^3 + {}^1r_1(0)V^2 + {}^1r_2(0)V + {}^1r_3(0)\right) \times \\ \left(V^3 + {}^2r_1(0)V^2 + {}^2r_2(0)V + {}^2r_3(0)\right) \times \\ \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \\ \left(V^3 + {}^5r_1(0)V^2 + {}^5r_2(0)V + {}^5r_3(0)\right) \\ \qquad = 0\,; \end{array}\right\} \quad . \quad . \quad (o''')$$

and will consequently be resolvable into five cubic equations, the coefficients of which will be known rational functions of A_1, A_2, .. A_5. A result in perfect accordance with the one which we proposed to verify—"that there will be an equation of the third

* I thought, indeed, at one time, while viewing the subject at a greater distance, that there was no way to escape from the first system (see my remarks in this Journal for January 1846); but I implicitly assumed, what I believe has never been contested, the universality of M. Cauchy's theorem. It will, however, appear from what follows, that the theorem in question cannot be safely rested on, but must yield its place to another theorem consisting of two parts or branches, which, exclusively of particular cases, are distinct, and incapable of coincidence when n is equal to 5 or to any higher number.

degree with given coefficients simultaneous with the equation

$$V^{15}+C_1V^{14}+\ldots=0."$$

The verification would indeed have been even more striking had we taken into account the equations analogous to (aa).

10. It is obvious that generally

$$V_{F(\alpha v)}+V_{G(\alpha v)}+V_{H(\alpha v)}=-B_1;$$

and that therefore

$${}^1r_1={}^2r_1={}^3r_1={}^4r_1={}^5r_1:$$

if, then, the roots of a particular equation of the fifth degree be so related to each other as to cause the equation

$$V^{15}+C_1V^{14}+\ldots=0$$

to take the form

$$\left(\left(V+\frac{1}{3}r(0)\right)^3\right)^5=0,$$

the system (σ_1) will in that case coexist with (σ_2).

11. With respect to the theorem of M. Cauchy, it fails to apply to a system of *congeneric* expressions such as

$$\left.\begin{array}{l} \mathfrak{D}(V_F, \quad V_G, \quad V_H)={}^1r(0), \\ \mathfrak{D}(V_{F(\alpha\beta)}, V_{G(\alpha\beta)}, V_{H(\alpha\beta)})={}^2r(0), \\ \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \\ \mathfrak{D}(V_{F(\alpha\epsilon)}, V_{G(\alpha\epsilon)}, V_{H(\alpha\epsilon)})={}^5r(0). \end{array}\right\} \quad . \quad . \quad . \quad (\Sigma')$$

12. Every difficulty is therefore at an end. We are thus a second time brought to the conclusion, which (guarded as it now is and fenced round on every side) must soon approve itself to the minds of mathematicians: *that the roots of the general equation of the fifth degree admit of being expressed by finite combinations of radicals and rational functions.*

Long Stratton, Norfolk,
January 13, 1852.

I subjoin a list of *Errata* in my "Reflections" for June 1845.

Page.	Line.	Error.	Correction.
549	12	ι^{β}	ν_{β}
549	15	$\iota+\iota^3$	$1-\iota^3$
549	16	ν	λ
551	14	$1+\iota+\iota^2+\iota^3+\iota^4$	$\iota+\iota^2+\iota^3+\iota^4$
553	25	expression	expressions
562	6	[*omitted*]	$V_{F(\beta\epsilon)(\gamma\delta)}$, $V_{F'(\beta\epsilon)(\gamma\delta)}$
562	29	h'_{τ}	$-h'_{\tau}$
563	14	g'_{v}	$-g'_{v}$
565–6		Θ *passim*	Θ
566	19	$r'_{4-n}(\rho t)^n$, $r''_{4-n}(\rho^4u)^n$.	$(r'_{4-n}(\rho t)^n)^5$, $(r''_{4-n}(\rho^4u)^n)^5$
573	34	(ab)	(ab)

XVIII. *On the supposed Identity of the Agent concerned in the Phænomena of ordinary Electricity, Voltaic Electricity, Electro-magnetism, Magneto-electricity, and Thermo-electricity.* By M. Donovan, *Esq., M.R.I.A.**

Section I.—*On the Constitution of the Electric Fluid.*

TO refer the greatest number of effects to the least number of causes has been a favourite effort with those who imagine that, by so doing, they vindicate and hold up to admiration what they call the simplicity of Nature's operations. It may be questioned, however, whether the faculties of man enable him to perceive and appreciate what is thus alleged to be a perfection. It being as easy for the Almighty to bring into operation a million of causes as one, the grounds are not obvious on which simplicity has been imagined an attribute of Divine agency. Nor does it appear what is meant by the term when thus employed; for if we diminish the number of agents, we must increase the number of their properties, in order to explain, with any degree of probability, the diversity of natural phænomena; hence nothing is thereby simplified, and no advantage gained.

Simplicity does not seem to be the order of nature: scarcely any object is simple; almost everything is compound. The thousands of mineral substances that constitute the mass of the globe are numerously compounded; they rarely consist of single elements; and the waters which surround it are composed of a variety of ingredients. Animals and plants, if we consider their component parts in conjunction with their properties, as constituting living organizations, are complex beyond all comprehension. Even the atmosphere, and the solar rays which penetrate it, are of the same heterogeneous structure. If the sun's light be thus of compound constitution, why should we doubt that the allied element, electricity, partakes of the universal character which the Almighty has impressed on his works?

This question brings us to the immediate object of the first part of the present essay—Is electricity a simple element?

In the existing state of knowledge, it is impossible to come to any positive determination. It appears to me, however, that the agent called the electric fluid falls within the general analogy of nature; that far from being a homogeneous elastic medium, as it is generally conceived to be, it consists of several elementary constituents, each possessing different properties. The nature of this agent may be considered without reference to the question of its being matter, or motion of matter, or of an æthereal fluid: motion may be compound as well as matter.

* Communicated by the Author.

The compound nature of the electricity which appears in the phænomena called galvanic has been often suspected, and the difficulty of explaining them without some such admission has been felt. Dr. Davy asks—"May we suppose, according to the analogy of the solar ray, that the electrical power, whether excited by the common machine, or by the voltaic battery, or by the torpedo, is not a simple power, but a *combination* of powers, which may occur *variously associated*, and produce all the varieties of electricity with which we are acquainted*?" Sir H. Davy believed that the current of a voltaic series is a mixture of magnetism and electricity†. Professor Hare conceives that the fluid extricated in the voltaic series is a combination of electricity, caloric and light‡. There are other conjectures which need not be here particularly noticed.

That the electric fluid contains heat is clearly proved by its power of fusing metals, and setting fire to various combustibles. That these phænomena are due to the presence of the principle called caloric can scarcely be doubted; the same that is evolved in ordinary cases of combustion, attrition, combination, and other manifestations. If it be not so, we must no longer speak of caloric as *the* principle which, by addition and subtraction, causes the sensations and phænomena of heat and cold: we must admit two calorics; and if so, why not as many other kinds as there are different sources? There is no advantage in supposing that the heat is extricated from the matter acted on by the electric fluid.

That light is contained in the electric fluid is rendered obvious by many beautiful contrivances; and that it is common light is evident from experiments made on its refraction, reflexion, polarization, absorption and decomposition. Were it denied that electric light is the same as ordinary light, it should be admitted as a consequence that all luminous phænomena are not attributable to the old well-known element, but that there are other principles in nature created to exercise one and the same function, which is improbable.

The existence of magnetism in the electric fire is rendered probable by those experiments in which steel has been magnetized by explosion of a Leyden jar, and the poles of magnetic needles have been reversed. But there is direct evidence of its presence in the arch of flame which Sir H. Davy obtained from the terminal charcoal points of a voltaic battery; there was even undoubted proof of what may be called a current of magnetism; for by means of a powerful magnet, the arch of flame could be attracted or repelled according to the pole of the magnet applied. This fact, suspected by Arago, was proved by De la Rive and

* Philosophical Transactions, 1832, p. 275.

† Phil. Mag. S. 1. vol. lviii. p. 415.

‡ Ibid. 1821, pp. 286, 293.

Davy. Besides, the copper connecting wire of a voltaic battery will attract iron filings when the series is excited; the filings with respect to each other assuming a polar arrangement, as if they were collected round a magnet. The arch of flame is accredited as the electric current.

But as the electric fluid exerts powers of attraction and repulsion very different from those of magnetism, some separate element may be present in it which exercises these influences on masses of matter, and produces other dynamic effects: it may be different from the heat, light and magnetism, which are associated with electricity. Perhaps this may be the basis of the fluid; and as a name to distinguish it will be convenient in the sequel, it may be here called "electricity proper."

But what is it that causes the attractions of the atoms which compose heterogeneous matter, or in other words, occasions chemical combination? is it the same as that element just spoken of which causes attraction and repulsion of masses of matter? Some of our eminent authorities have maintained that it is. In my Essay on the Origin, Progress, and Present State of Galvanism, I have endeavoured to defend the contrary opinion; and in the sequel of the present essay some additional considerations will be adduced having a similar tendency.

The physiological phænomena, comprising the shock and muscular contractions, constitute an important series of effects; they will scarcely be supposed to be produced by the light, heat, or magnetism of the electric fluid. They may perhaps be caused by the element which I have designated by the name of "electricity proper," in the case of frictional electricity; but it will be a part of the object of this essay to render it probable that, in what is called voltaic electricity, a power exists of causing a shock of a very different nature.

Concerning the agent in the electric fluid which possesses the remarkable property of coercing the magnetic needle into a position transverse to the magnetic meridian, there is an important problem to be solved: Is it the same as that element of the electric fluid which causes attraction and repulsion of masses of matter—the *electricity proper*? This is the grand question; and for the present I shall say no more, than that in the sequel many arguments and facts will be adduced with a view of rendering it probable that electricity proper is not the force which causes deflection of the magnetic needle. It may be either some different force, or all the forces conjointly.

The conclusion at which I arrive is, as has been already stated, that what is called the electric fluid does not consist of one homogeneous element, but of several; that the difference between the various exhibitions of it which produce frictional, voltaic, electro-magnetic, magneto-electric, and thermo-electric phænomena, depends on the ratio, or variable energy, or mode of association of the constituent elements, or on the influence of other modifications which, under different circumstances, they are capable of exerting on each other; in one word, that these various classes of phænomena are caused by different agents;—these agents as much differing from each other as hundreds of chemical compounds, which, consisting of the same elements, are so combined and grouped as to constitute and be recognised as independent forms of existence, requiring different names. This modifying influence is probably of the same character as that which the forces of nature exercise on each other on the great scale of creation, controlling, antagonising, and regulating each others' effects; thus producing the diversified phænomena of the universe, but rarely acting independently. It is by opposing forces that the earth is maintained in its orbit; by the interposition of a third force its unity of motion and integrity of substance are preserved. It is by the antagonism of an attractive and repulsive power that hardness, softness, liquidity, and fluidity exist; but for this antagonism the materials of creation would be bound in perpetual rigidity, or attenuated throughout space; neither animal nor vegetable could exist; there could not be either air or water. Were it not for the control which the various energies of nature exercise over each other, new forms of things would be produced and destroyed in rapid succession; change would be perpetual; nothing would be permanent. It would be easy but useless to multiply examples, since a cursory view of creation will prove that the Almighty rarely regulates the course of natural events by the operation of insulated forces; there is scarcely a physical phænomenon in which it is not possible to detect the operation of several; and thus, by combination of elementary causes, a vast variety of effects is produced.

If it be true that complication of matter and of forces is the method of nature, can we without risk of error single out that most remarkable of her agents, electricity, and affirm that it presents itself to our senses in a state different from that in which all other objects occur,—in an undisguised, uncompounded form; and that it produces its wonderful, diversified, and important functions in virtue of one single or homogeneous element?

The opinions here promulgated differ widely from those which are now almost universally maintained by philosophers. By a kind of tacit consent, it has been commonly assumed that the calorific, luminous, dynamic, magnetic, chemical, and physiological properties of the electric fluid are exercised by it as an uncompounded element; of a constitution invariable and identical under every aspect, unless in the two great varieties of the positive and negative states, the difference of which is at present incomprehensible although remarkable. This opinion has been the source of all the difficulties experienced in the efforts of philosophers to reconcile the differences of effect in the various forms of electricity. By admitting the compound nature of the electric fluid, the occasional variation in its constitution already alluded to, and the consequent existence of combinations of these constituents so different from each other as to constitute distinct agents, we reduce the conflicting phænomena under one comprehensive explanation, namely, that they are the effects of causes specifically different.

If it be hypothetical to assume, as I do, the compound nature of the electric fluid, surely it is as much so to suppose with others that it is a simple element. The former is supported by analogy and several undoubted facts; the latter, as will be hereafter shown, leads to contradictions and embarrassments. Besides, the compound nature of electricity is in some sort maintained by all those who conceive the existence of two fluids which by combination neutralize each other, and thus constitute the natural state of equilibrium. Perhaps the notion here entertained may suggest the idea that in the excitation of electricity, whether by friction, chemical action, heat or magnetism, the compound fluid is decomposed into the positive and negative states belonging to each variety of electricity, an unequal division of the constituent elements giving rise to the difference of properties which then becomes so remarkable. It is only in this state of decomposition that electricity of any kind is sensible or active.

The different elementary forces, which are here supposed to constitute the electric fluid, are in some manner embodied into a singular state of existence, neither solid, liquid, nor aëriform; a something of extreme tenuity; imponderable, yet possessing some of the characters of materiality. Mr. Sturgeon appears to me to have given the preponderance to the probability of the opinion that electricity is matter, difficult as it may be to comprehend such a strange constitution. He seems to have been successful also in proving that the idea of vibrations, as applied to electricity, is incomprehensible. An experiment which I made and published (Phil. Mag. S. 1. vol. xliv.) many years since, induces me to consider the doctrine of vibrations less probable than that of the materiality of the electric fluid: it was as follows:—A wire depending from the prime conductor of an electrical machine was made to convey a charge down through the long slender neck of a very thin glass flask, the lower hemisphere of which was externally coated with tin-foil, and filled to the same height with mercury. The flask, now a Leyden phial, being charged, was withdrawn from the charging wire; its neck was sealed at a lamp by melting the glass; and the whole was immersed in a vessel of water, and kept there for fourteen days. The flask being withdrawn and dried, its neck was cut off. Holding the coating in one hand, and introducing a wire into the mercury with the other, I received a shock, reduced, it is true, from what it had been. It can scarcely be believed that this electricity consisted in vibrations: it were singular if vibrations could be thus confined in a bottle for fourteen days and still continue. The same experiment was made long afterwards by Faraday; but the glasses being kept for two or three years, the whole charge escaped.

Some persons may object to my experiment, that during this period the electricity was quiescent, and that on its liberation the vibrations recommenced. But this implies that there was a something confined which was capable of vibrating:—What was it? If it was an æthereal fluid, as some say, it may as well be named the electric fluid.

It is indeed of little consequence to the opinions here advocated, whether electricity be considered as matter, or vibrations of some peculiar æther, or of any conceivable state of existence; it will answer the present purpose to view the electric fluid as a combination of elementary forces or agents of whatever kind. These constituent forces or agents must be maintained in a state of association, coerced by some peculiar attraction or affinity. Whatever the bond of union, there must be some such bond, be its nature or name what it may; as without the intervention of such a power, the integrity of the compound fluid would be without an assignable cause, and no explanation could be given of the passage of *all* the constituent elements through certain kinds of matter, which under other circumstances would have been impervious to some of them. We know that in the constitution of other imponderables such a power exists. The phosphorescence of certain bodies was at one time attributed to chemical attraction mutually subsisting between light and the phosphorescent body, to the consequent absorption of the former, and its extrication in the dark. It is true that the present term used to express this effect is *adhesion*, which still implies attraction of some kind.

This attraction or adhesion is generally admitted to maintain the state of combination between other imponderable elements; such seems to subsist in the sun-beam, the heterogeneous rays of which travel from the sun to the earth 96 millions of miles in a state of integrity: they may be separated, it is true, by various processes; but without the employment of such they cohere with obstinacy. Mr. Mackintosh has several times observed the sun's rays to be so much concentrated by the convex glasses which answer the purpose of windows in a diving-bell, that the clothes of the labourers were set on fire when exposed to the focus, although these rays had to pass through twenty-five feet depth of water. Captain Scoresby succeeded in burning bodies by the sun's rays passed through a lens of ice, the ice itself remaining unmelted. Heat is held combined in voltaic electricity, as appears from the fact, that a thin platinum wire may be melted by a current which has passed through tinfoil immersed in a freezing mixture. Those who maintain the identity of light and heat will deny that these facts prove anything. Their effect would certainly fail if it can be *satisfactorily* explained why a sun-beam, by falling on the moon, is so far altered, that when reflected to the earth and concentrated 3000 times by our specula, the light has no effect on the most sensible thermometers, or even on a thermo-multiplier*.

That electricity is combined with light in the sun's beam by some such force as is here presumed to hold the elementary constituents of the electric fluid together, is rendered strikingly probable by an experiment of Matteucci. When the sun's rays are made to fall on a perfectly dry glass plate, the latter becomes electrical; a second plate on which the light falls after passing through the first does not become so, although there is little diminution of the light. That the effect is due to the electricity of the beam, and not to excitement of the glass by heat, is shown by the failure of any effect on the second plate, and also by the fact that heating by fire has no such power. The electric fluid is carried forward in the beam by the coercive power alluded to, yet is so far obedient to the law of non-conductors that it is intercepted by the first glass plate, and does not pass to the second. This, at least, seems the most probable explanation; and, in such recondite subjects, probability is our chief guide.

It is to be remembered that chemical attraction or affinity is attributed to electricity in direct terms by the philosophers of the present day. They speak of combinations of electricity with hydrogen, oxygen, and other bodies, and found systems on the existence of such combinations, in which cases the affinity must be mutual. If they are warranted in attributing affinity to integral electricity, I am not less so in attributing the same power to its constituents.

All these considerations show, that in my assumption of a coercive power which preserves the integrity of the electric fluid by holding its constituent elements in union, there is nothing irreconcileable or repugnant, inasmuch as a similar power appears to act on the heterogeneous elements of other imponderable matter.

If it be admitted that the electric fluid consists of heteroge-

* Gmelin, vol. i. p. 166.

neous elements held together by an attractive force, varying in the ratio and perhaps in the mode of combination according to the circumstances of the excitement which produced it, we shall be at no loss to understand why electricity appears under such different forms as those called frictional, voltaic, thermal and magnetic, and why these different forms pervade and are conducted by the same bodies. It has always been one of the chief arguments of those who contend for the identity of the voltaic and electric agents, that they are transmitted by the same kind of matter. But this is what ought to happen, according to the view here given; and hence the conduction of the different kinds of electricity by the same bodies is of no force as an argument for identity and against dissimilarity. The passage over the same conductors may very well take place if the elementary constituents be the same, although the other circumstances are so different as to impress on the agent the character of total dissimilarity. It is however to be expected that this difference of circumstances would produce some difference in the effects. That it does is abundantly evident; so much so, indeed, that there are very few points of real resemblance.

One very striking difference of properties, which may be fairly attributed to the variation of constitution in the electric agent, is the facility with which statical electricity is conducted by water, and the insuperable difficulty experienced when attempts are made to pass thermo-electricity or thermo-triboelectricity through the smallest portion of even salt water, as if the constituent element were absent that acts as the *vector* of all those which in their own nature do not move through conductors. The most powerful frictional electric machine can scarcely be excited if the atmosphere be damp; and a small jar or large battery will not retain the highest or lowest intensity if the glass be not perfectly dry. But so different is the constitution of electricity furnished by heating two very slender wires of different metals in contact, that although they will cause deflection of the galvanometer wire to 60° (which the most powerful frictional machine and battery will scarcely effect), yet such electricity will not pass through $\frac{1}{60}$th of an inch of salt water interposed between the conducting wires. In an essay not long since read to the Royal Irish Academy, I showed that by causing rapid revolution of a bismuth wheel against a rubber of antimony, each metal being connected with the galvanometer, the needle stood permanently at 60° or 70°; but the interposition of $\frac{1}{40}$th of an inch of salt water between the conducting wires stopped the current, although 40 inches of the same water were traversed by the electricity arising from a surface of zinc and a surface of platinum, each half an inch square, the galvanometer needle standing permanently at

60°. Can the constitution of the electricity arising from all these sources be the same?

Whatever may be the nature of the power which, when associated with matter, constitutes chemical affinity, it seems to be found in the electric fluid *combined* with the other constituent elements, and to be transmissible through conductors; for it is known that in this state it energetically effects combinations and decompositions of bodies. In assuming that the principle which causes chemical effects may, by being a constituent element of the electric fluid, be thus transferred through solid matter, or air, or even a vacuum, I do not conceive that I take an unwarrantable liberty with the facts, for they seem actually to invite the assumption. I advanced the opinion that affinity might be transferred from one body to another many years since, in an essay which, in a different form, was honoured with the prize by the Royal Irish Academy; but even then the idea was not a novelty; for Sir H. Davy virtually maintained the same notion more extensively when he promulgated his doctrine of the identity of electricity and affinity; Professor Faraday entertains it now; and Professor Schönbein employs the very same idea in the following sentence, which seems to convey his assent to the notion of transferred affinity, although it is apparently expressed conditionally: he says, "if there be any instance of chemical affinity being transmitted in the form of a current by means of conducting bodies, I think the fact just stated may be considered as such*." Faraday speaks explicitly and decidedly: he says, "all the facts show us that the power commonly called chemical affinity can be communicated to a distance†;" and "the force of chemical affinity is then transferred through the two metals‡."

That Davy, Berzelius, Ampère, Faraday, and some others, all admitted the principle of the transference of chemical affinity to a distance and through space need scarcely be adverted to, when it is well known, that, in the school of these philosophers, electricity and affinity are the same forces; hence if the former can be transferred, so can the latter; and my views, formerly designated by a few persons "a startling novelty," are protected, in their present more decided form, from the imputation of unwarrantable innovation.

That a distinct constituent element, possessing chemical powers should exist in the compound called the electric fluid, associated with heat, prismatic rays, and magnetism, is neither less intelligible nor more improbable than that the very same elements should be found associated in the sun's beam. I am aware that the existence in the solar ray of a power which produces the phæ-

* Phil. Mag. S. 3. July 1836. † Researches, &c. p. 272.
‡ Ibid. p. 284.

nomena of magnetism has been denied; but when I find it affirmed by such a number of witnesses, comprising Morichini, Carpi, Ridolfi, Gmelin, Davy, Playfair, Barlocci, Zantedeschi, Christie, Somerville, Baumgartner, and the Messrs. Knox, I cannot believe that they were all deceived. The statement of Playfair, as given by Sir David Brewster, is too strong, striking and circumstantial, to admit the suspicion of mistake.

The advantage to be derived from admitting that the electric fluid is a compound of elementary forces, which, according to the circumstances of its excitation, may vary in its constitution, is that our explanations of phænomena are thereby disentangled from a multiplicity of embarrassments occasioned by the supposed identity of the various forms of electricity; and the endeavour to reduce irreconcileable effects under the operation of a single cause is no longer necessary. These laboured efforts to sustain the doctrine of identity, have had their influence in causing the contradictory speculations promulgated concerning electricity in connexion with matter. Were there as much truth as boldness in them, we should by this time have attained a thorough knowledge of the internal structure of matter; the very shape of atoms, and their individual constitution and properties, must have been ascertained. By one philosopher we are informed that atoms are spherical; others find that electricity is an integrant part of their composition, and even declare that without it the atoms could not exist. Again, we are informed that each atom has two electrical poles. This is utterly denied by another authority; the argument adduced is, that polarity is incompatible with sphericity, all the points in the surface of a sphere being symmetrically placed with relation to the centre. The idea of polarity is also denied by others, who have discovered that some atoms are positively and some negatively electrical throughout their mass. To this is also added, that the electricity proper to each atom is disguised by an electrical atmosphere which surrounds it. Some philosophers declare that the polar electricities of atoms are not of equal intensity, one always predominating: this is most emphatically denied by others, who conceive that the admission of equality is indispensable. It is not to be forgotten also, that some will admit but one electric fluid; others must have two, or they explain nothing; but others, again, explain all the phænomena without any electric fluid at all. The list of contradictory speculations should be considered in connexion with this very extraordinary fact, that the fundamental principle of one theory of galvanism is, that water is a conductor of electricity; but in the rival one it is assumed to be an insulator; and without the admission of one or other of these conflicting positions neither theory can stand its ground.

In the observations thus made, it is as far from being my wish, as it is beyond my capability, to depreciate the labours of the illustrious persons who have erected splendid specimens of art on the foundations just described. They used these foundations as they found them; and if they be not sound and permanent, we have only to lament that so much skill, ingenuity and industry, were not bestowed on a more solid basis.

11 Clare Street, Dublin.

[To be continued.]

XIX. *Remarks on the Researches of* Dr. Goodman "*On the Identity of the Existences or Forces, Light, Heat, Electricity and Magnetism.*" *By* Dr. Tyndall*.

THE December Number of the Philosophical Magazine contains an abstract of a paper bearing the above title, and recently read before the Royal Society by its Secretary Mr. Bell. Dr. Goodman finds, that on suspending a magnetized sewing-needle within the helix of a galvanometer, and covering the instrument with a glass shade, when the sun is permitted to shine strongly upon the instrument an electric current is developed in the helix, which is indicated by its action upon the magnetized needle. The direction of the current varies with the portion of the instrument shone upon, and in some cases a permanent deflection of 10 or 12 degrees was obtained *even when the two ends of the galvanometer wire were disunited.* "During the course of the experiments the circuit was established by means of a connecting wire between the mercury cups, and the circuit was again and again completed, and as frequently broken, without any deviation occurring in any of the results." The remarks made further on will, perhaps, excuse me to Dr. Goodman if I propose the following modifications of his experiment: first, to remove the magnetized sewing-needle and put an unmagnetized one in its place; secondly, to remove the steel needles altogether and substitute in their stead one of copper or of wood; thirdly, to remove the helix also, and leave the wooden or copper needle and dial-plate alone within the shade. If, on submitting the apparatus to the conditions described, in none of these proposed cases an action quite the same as that exhibited by the magnetized needle can be observed, then is the discovery a most surprising one, and the momentous conclusion drawn from it in some measure justified. I believe, however, that it has been the lot of many experimenters to deal with phænomena similar to those

* Communicated by the Author.

described by Dr. Goodman, in cases where there was no possibility of an electric current being formed.

During the inquiry on diamagnetism and magnecrystallic action, an account of which appears in the September Number of this Magazine for 1851, the torsion balance there described was placed before a window through which the sun shone during the forenoon. In experimenting with the spheres of bismuth, I was often perplexed and baffled by the contradictory results obtained at different hours of the same day. With the spheres of calcareous spar, where the diamagnetic action was weak, the discrepancies were still more striking. Once while gazing puzzled at the clear ball of spar resting on the torsion balance, my attention was attracted to the bright spot of sunlight formed by the convergence of the beams which traversed the spar, and the thought immediately occurred to me that this little "fire-place" might be sufficient to create currents of air strong enough to cause the anomalies observed. The light was shut out, and thus the source of my perplexity was effectually cut away. The air-currents, however, were far more owing to the warming of the glass cover of the instrument than to the convergence of the sun's rays; during the whole inquiry I was obliged to experiment every sunshiny forenoon with closed shutters. On mentioning this fact to Prof. Magnus, he informed me that during his investigation on thermo-electric currents, a report of which appears in the present Number of the Philosophical Magazine, he was obliged to protect his galvanometer from the action of the sun, as the unequal heating of its glass shade rendered his astatic needles quite unsteady. It is almost incredible how slight a difference of temperature is sufficient to create these currents, and thus disturb the action of a finely suspended needle. M. Kohlrausch, whose refined experiments I had the pleasure of witnessing for several successive days last spring, has been obliged to construct a table for the express purpose of making allowance for the little whirlwinds which sometimes establish themselves in his electrometer. In his instrument, a needle of silver wire is suspended from a glass fibre of extreme tenuity, the needle being protected by a vessel of brass with a glass cover. The days on which we experimented were cold ones; and as long as a good fire was kept in the stove which heated the room, the experiments were satisfactory; but as soon as the fire became low, and radiation set in strongly against the cold window-panes before which the electrometer was placed, the action of the air within the brass vessel upon the needle was at once exhibited, and increased to such a degree, with the decreasing temperature, that further experiment had to be abandoned. M. Kohlrausch has mapped these little currents with great care. They resemble, to compare small things with great, the cyclones of Colonel Reid. To preserve equability of temperature, a screen of pasteboard was often found serviceable. Now that a needle suspended from a silken fibre sixteen inches long, covered with a glass shade and placed in strong sunlight, which are the conditions of Dr. Goodman's experiments, should also be influenced by air-currents, is exceedingly probable, and that a permanent current of electricity should circulate in a helix of covered copper wire with its two ends disconnected being exceedingly improbable, it appears worth the trouble to subject the matter to one or more of the three tests which have been above proposed.

Queenwood College,
Jan. 1, 1852.

XX. *Homogeneous Functions, and their Index Symbol**. *By* Robert Carmichael, *A.B., Trinity College, Dublin*†.

IN a valuable memoir published in the Philosophical Transactions for the year 1844, Professor Boole, by the aid of two fundamental principles, has given general methods of solution for certain extensive classes of linear differential equations. It is the chief object of the present paper to show that, by a generalization of those principles and a suitable development of the consequences of the higher principles, we can obtain similar general methods of solution of corresponding classes of *partial* differential equations. The solutions of such partial differential equations will be found to be unaffected by the number of independent variables which the equations may contain; but more especial reference is made to those in most common occurrence, containing but two independent variables, x and y.

In the course of the investigation, extensions of many familiar and elementary theorems are furnished, which seem to possess much practical utility. From the spontaneity with which they evolve themselves, and the facility with which they admit of employment, they appear to open a large and fruitful field for future speculation.

By an application of the general principles to the subject of

* The principal portion of the first seven articles in this paper has been already published in the Cambridge and Dublin Mathematical Journal, November 1851. In the Number of the same Journal for February 1851, will be found an interesting and masterly paper by Mr. Sylvester, "On certain general Properties of Homogeneous Functions." The same symbol is there applied to equations in finite terms, with a view to the subjects of surfaces and the linear solution of systems of indeterminate equations. In the present paper the symbol is applied to the subjects of partial differential equations and multiple definite integrals.

† Communicated by the Author.

Multiple Definite Integrals, it will be seen that valuable results can be obtained, and some examples are furnished. It will be observed that these general principles present the means of extending all multiple definite integrals, in which the variables enter, as complicated functions, in the indices of known quantities unconnected with the limits. This seems to be an important step, but an adequate development of its consequences would much exceed the limits of the present paper.

The instrument employed is the symbol which occurs in the well-known theorem of homogeneous functions. The relation which the result of the operation of this symbol upon any homogeneous function bears to the degree of the function, seems to give ground for the appellation, Index Symbol.

In conclusion, the writer begs to express in the most ample manner his acknowledgements to the distinguished mathematician above named.

1. In general, if

$$u_m = f(x, y, z, \&c.)$$

be a homogeneous function of the mth degree between the n independent variables x, y, z, &c.,

$$x\frac{du_m}{dx} + y\frac{du_m}{dy} + z\frac{du_m}{dz} + \&c. = mu_m;$$

or, putting the operating symbol

$$x\frac{d}{dx} + y\frac{d}{dy} + z\frac{d}{dz} + \&c. = \nabla,$$

we have

$$\nabla . u_m = m . u_m,$$

and by successive operation,

$$\nabla^p . u_m = m^p . u_m .$$

Hence the theorem

$$F(\nabla) . u_m = F(m) . u_m, \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

which is an extension of the theorem

$$f\left(x\frac{d}{dx}\right) . x^m = f(m) . x^m, \text{ or } f(D) . e^{m\theta} = f(m) . e^{m\theta},$$

the first fundamental principle employed by Professor Boole. In fact, x^m is a particular homogeneous function of the mth degree, and $x\frac{d}{dx}$ is the first term of ∇.

2. Now if U be any mixed rational function of x, y, z, &c., it can, in general, be put under the form

$$U = u_0 + u_1 + u_2 + \&c. + u_m;$$

and we have a theorem for mixed rational functions corresponding to (1), namely,

$$F(\nabla) . U = F(0)u_0 + F(1)u_1 + F(2)u_2 + \&c. + F(m)u_m . \quad . \quad . \quad . \quad (2)$$

As an example, let the result of the operation of a^∇ on U be investigated. Then

$$a^\nabla . U = u_0 + au_1 + a^2u_2 + \&c. + a^m u_m ;$$

and the interpretation of this result is readily seen to be, that the operation of a^∇ upon the mixed rational function U converts the several variables x, y, z, &c. *throughout it* into ax, ay, az, &c.

If U be supposed to contain the *two* distinct sets of variables

$$x, y, z, \&c.,$$
$$a, b, c, \&c.,$$

it may be exhibited in either of the two forms

$$U = u_0 + u_1 + u_2 + \&c. + u_m ,$$
$$U = v_0 + v_1 + v_2 + \&c. + v_\mu .$$

As
$$\nabla = x\frac{d}{dx} + y\frac{d}{dy} + z\frac{d}{dz} + \&c.,$$

so let
$$\Box = a\frac{d}{da} + b\frac{d}{db} + c\frac{d}{dc} + \&c.$$

Then, since these two symbols are commutative with each other, we have

$$\Phi(\nabla) . \Psi(\Box) . U = \Psi(\Box) . \Phi(\nabla) . U,$$

whence the theorem

$$\Phi(\nabla) . \{\Psi(0)v_0 + \Psi(1)v_1 + \&c.\} = \Psi(\Box) . \{\Phi(0)u_0 + \Phi(1)u_1 + \&c.\}.$$

3. Since y, z, &c. are constant relative to x, and therefore $\frac{d}{dx}, \frac{d}{dy}$, &c. commutative, writing

$$\nabla_2 = x^2\frac{d^2}{dx^2} + y^2\frac{d^2}{dy^2} + \&c. + 2xy\frac{d^2}{dxdy} + \&c.$$

$$\nabla_3 = x^3\frac{d^3}{dx^3} + y^3\frac{d^3}{dy^3} + \&c. + 3x^2y\frac{d^3}{dx^2\,dy} + 3xy^2\frac{d^3}{dx\,dy^2} + \&c.,$$
$$\&c.,$$

we have

$$\nabla(\nabla - 1) = \nabla_2,$$
$$\nabla(\nabla - 1)(\nabla - 2) = \nabla_3,$$
$$\&c.,$$

and generally

$$\nabla(\nabla - 1) \ldots (\nabla - n + 1) = \nabla_n, \quad . \quad . \quad . \quad . \quad (3)$$

a theorem analogous to Professor Boole's

$$xD(xD-1)\ldots(xD-n+1)=x^nD^n.$$

As an example of the operation of this latter symbol, let the subject be x^n, and

$$xD(xD-1)\ldots(xD-n+1)\,.\,x^n=1\,.\,2\,.\,3\ldots n\,.\,x^n.$$

To which we have the corresponding theorem for homogeneous functions, by (1),

$$\nabla(\nabla-1)\ldots(\nabla-m+1)\,.\,u_m=1\,.\,2\,.\,3\ldots m\,.\,u_m.$$

As a second example, we shall seek a general proof of a theorem first given by Euler, namely,

$$\frac{n(n-1)\ldots(n-m+1)}{\overline{m}}\,.\,u_n=\Sigma\frac{x^\alpha\left(\frac{d}{dx}\right)^\alpha}{\overline{\alpha}}\cdot\frac{y^\beta\left(\frac{d}{dy}\right)^\beta}{\overline{\beta}}\cdot\frac{z^\gamma\left(\frac{d}{dz}\right)^\gamma}{\overline{\gamma}}\ldots u_n,$$

where $\alpha+\beta+\gamma+\&c.=m$. Now as

$$(1+a)^\nabla=(1+a)^{x\frac{d}{dx}}\,.\,(1+a)^{y\frac{d}{dy}}\,.\,(1+a)^{z\frac{d}{dz}}\ldots,$$

expanding and equating the coefficients of a^m on both sides, and then condensing by the formula above,

$$\frac{\nabla(\nabla-1)\ldots(\nabla-m+1)}{\overline{m}}\,.\,U=\Sigma\frac{x^\alpha\left(\frac{d}{dx}\right)^\alpha}{\overline{\alpha}}\cdot\frac{y^\beta\left(\frac{d}{dy}\right)^\beta}{\overline{\beta}}\cdot\frac{z^\gamma\left(\frac{d}{dz}\right)^\gamma}{\overline{\gamma}}\ldots U,$$

and when $U=u_n$, we get Euler's theorem.

4. Again, as the symbol xD furnishes solutions of the class of ordinary differential equations represented by

$$Ax^\alpha\frac{d^\alpha y}{dx^\alpha}+Bx^\beta\frac{d^\beta y}{dx^\beta}+\ldots=X,$$

in the form

$$y=F(xD)\,.\,X+F(xD)\,.\,0,$$

where

$$F(xD)=\left\{\begin{array}{l}\quad A\,.\,xD(xD-1)\ldots(xD-\alpha+1)\\+B\,.\,xD(xD-1)\ldots(xD-\beta+1)\\+\ \&c.\end{array}\right\}^{-1}:$$

in like manner we obtain the solutions of the particular class of partial differential equations represented by

$$\left.\begin{array}{l}A\left(x^\alpha\frac{d^\alpha z}{dx^\alpha}+\alpha x^{\alpha-1}y\frac{d^\alpha z}{dx^{\alpha-1}dy}+\frac{\alpha(\alpha-1)}{1\,.\,2}x^{\alpha-2}y^2\frac{d^\alpha z}{dx^{\alpha-2}dy^2}+\ldots\right)\\+B\left(x^\beta\frac{d^\beta z}{dx^\beta}+\beta x^{\beta-1}y\frac{d^\beta z}{dx^{\beta-1}dy}+\frac{\beta(\beta-1)}{1\,.\,2}x^{\beta-2}y^2\frac{d^\beta z}{dx^{\beta-2}dy^2}+\ldots\right)\\\qquad+\ \&c.\end{array}\right\}=\Theta,$$

where Θ is a given function of x and y, in the form

$$z = F(\nabla) \,.\, \Theta + F(\nabla) \,.\, 0, \quad . \quad . \quad . \quad . \quad . \quad . \qquad (4)$$

in which

$$F(\nabla) = \{A\nabla(\nabla-1)..(\nabla-\alpha+1) + B\nabla(\nabla-1)..(\nabla-\beta+1) + \&c.\}^{-1},$$

and in which the value of *the first* term is perfectly definite, and can be had at once by formula (2). It appears, then, that as far as equations of this class are concerned, the *number* and *character* of *the arbitrary functions* in a solution, which are due solely to the second term, are unaffected by the number of independent variables which the equation may contain, and are solely dependent on its order.

When the roots of the equation

$$A.\nabla(\nabla-1)\ldots(\nabla-\alpha+1) + B.\nabla(\nabla-1)\ldots(\nabla-\beta+1) + \&c. = 0$$

are all real and unequal, the arbitrary portion of the solution is of the form

$$u_m + u_n + u_p + \&c.,$$

m, n, p, &c. being the values of the roots.

When it contains α equal roots, whose common value is m, its form is

$$u_m\{\log x + \log y\}^{\alpha-1} + v_m.\{\log x + \log y\}^{\alpha-2} + \&c. + u_n + u_p + \&c.,$$

where u_m, v_m, &c. are different arbitrary homogeneous functions of the same degree. Finally, when this equation contains pairs of imaginary roots, the form of the arbitrary portion of the solution is

$$u_{m+n\sqrt{-1}} + u_{m-n\sqrt{-1}} + \&c. \;\; + u_{\tilde{p}} + \&c.$$

5. By a single very obvious reduction, similar to the first which Legendre has employed (*Mémoires de l'Académie,* 1787) for the solution of the corresponding class of ordinary differential equations, we obtain at once, by the method of the last article, the solution of the class of partial differential equations,

$$\left.\begin{array}{l}
A\left\{(m+\lambda x)^{\alpha}\dfrac{d^{\alpha}z}{dx^{\alpha}} + \alpha(m+\lambda x)^{\alpha-1}.(n+\lambda y)\dfrac{d^{\alpha}z}{dx^{\alpha-1}dy}\right. \\
\qquad \left. + \dfrac{\alpha(\alpha-1)}{1\,.\,2}(m+\lambda x)^{\alpha-2}(n+\lambda y)^2\dfrac{d^{\alpha}z}{dx^{\alpha-2}dy^2} + \&c.\right\} \\
\qquad\qquad + \\
B\left\{(m+\lambda x)^{\beta}\dfrac{d^{\beta}z}{dx^{\beta}} + \beta(m+\lambda x)^{\beta-1}.(n+\lambda y)\dfrac{d^{\beta}z}{dx^{\beta-1}dy}\right. \\
\qquad \left. + \dfrac{\beta(\beta-1)}{1\,.\,2}.(m+\lambda x)^{\beta-2}.(n+\lambda y)^2\dfrac{d^{\beta}z}{dx^{\beta-2}dy^2} + \&c.\right\} \\
\qquad\qquad + \&c.
\end{array}\right\} = \Omega,$$

without the necessity of any further transformation.

6. As an example of this method of solution of partial differential equations, let it be required to find the integral of

$$x^2r+2xys+y^2t-n(xp+yq-z)=0.$$

When thrown into the symbolic shape, this equation becomes

$$\nabla(\nabla-1)z-n(\nabla-1)z=0,$$

and the solution is given by

$$z=\frac{1}{(\nabla-n)(\nabla-1)}.0=\frac{\mathrm{N}}{\nabla-n}.0+\frac{\mathrm{N}'}{\nabla-1}.0,$$

or is at once, including N and N′, in the homogeneous functions, *which are given in degree but arbitrary in form*,

$$z=u_n+u_1\text{*}.$$

As *a second* example, required the integral of

$$x^2r+2xys+y^2t=\Theta_m+\Theta_n,$$

where Θ_m, Θ_n are given homogeneous functions in x and y of the mth and nth degrees, respectively. Then

$$z=\frac{1}{\nabla(\nabla-1)}\{\Theta_m+\Theta_n\}+\frac{\mathrm{I}}{\nabla(\nabla-1)}.0,$$

or, by (1),

$$z=\frac{\Theta_m}{m(m-1)}+\frac{\Theta_n}{n(n-1)}+u_0+u_1,$$

which is the required solution.

As *a third* example, let the integral of the partial differential equation of the third order in three independent variables x, y, z,

$$\left.\begin{array}{l} x^3\dfrac{d^3u}{dx^3}+y^3\dfrac{d^3u}{dy^3}+z^3\dfrac{d^3u}{dz^3} \\ +3\left(x^2y\dfrac{d^3u}{dx^2dy}+x^2z\dfrac{d^3u}{dx^2dz}+xy^2\dfrac{d^2u}{dxdy^2}+\&c.\right) \end{array}\right\}=\Phi_m+\Phi_n$$

---

* If $n=-\frac{3}{m-1}$, this value of z renders the integral

$$\int\int(px+qy-z)^m dxdy,$$

a maximum or a minimum within certain assigned limits (Jellett's Calculus of Variations, p. 253).

In general, by the method stated above, it can be readily seen that the form of the function w, which, for certain assigned limits, renders the symmetrical multiple integral containing p independent variables

$$\int dx\int dy\int dz\,..\left(x\frac{dw}{dx}+y\frac{dw}{dy}+z\frac{dw}{dz}+\&c.-w\right)^m,$$

a maximum or a minimum is, as before,

$$w=u_n+u_1,$$

where

$$n=-\frac{p+1}{m-1}.$$

be investigated, Φ_m, Φ_n being given homogeneous functions in x, y, z of the mth and nth degrees, respectively.

The required solution is

$$u=\frac{\Phi}{m(m-1)(m-2)}+\frac{\Phi_n}{n(n-1)(n-2)}+u_0+u_1+u_2.$$

7. Supposing two of the independent variables to vanish in the last example and one in each of the preceding, we are at once furnished with the solutions of the following ordinary linear differential equations:—

$$\left.\begin{aligned} x^3\frac{d^3y}{dx^3}&=ax^m+by^n,\\ x^2\frac{d^2y}{dx^2}&=ax^m+by^n,\\ x^2\frac{d^2y}{dx^2}-nx\frac{dy}{dx}&+ny=0, \end{aligned}\right\}$$

which are, respectively,

$$\left.\begin{aligned} y&=\frac{ax^m}{m(m-1)(m-2)}+\frac{bx^n}{n(n-1)(n-2)}+C_0+C_1x+C_2x^2,\\ y&=\frac{ax^m}{m(m-1)}+\frac{bx^n}{n(n-1)}+C_0+C_1x,\\ y&=C_nx^n+C_1x. \end{aligned}\right\}$$

Now it must be remembered that the solutions given by the symbol ∇ are the same in form, no matter how large the number of independent variables may be. For instance, the solution of

$$\left.\begin{aligned} &x_1^2\frac{d^2w}{dx_1^2}+x_2^2\frac{d^2w}{dx_2^2}+x_3^2\frac{d^2w}{dx_3^2}\ \&\text{c.}\\ &+2\left(x_1x_2\frac{d^2w}{dx_1dx_2}+x_2x_3\frac{d^2w}{dx_2dx_3}+\&\text{c.}\right) \end{aligned}\right\}=\Psi_m+\Psi_n \quad . \quad (a)$$

is exactly the same in form as that of

$$x^2\frac{d^2z}{dx^2}+2xy\frac{d^2z}{dxdy}+y^2\frac{d^2z}{dy^2}=\Theta_m+\Theta_n\,; \quad . \quad . \quad . \quad (b)$$

namely,

$$w=\frac{\Psi_m}{m(m-1)}+\frac{\Psi_n}{n(n-1)}+u_0+u_1,$$

the only difference being in the number of independent variables contained in u_0, u_1.

Hence, in order to find *the form* of the integral of an equation of the class (a) containing any number n of independent variables, it is sufficient to have found the form of the integral of a corresponding equation (*b*) containing any lower number of independent variables. Hence is derived the following conclusion, which seems to be of some importance.

The solution of an ordinary linear differential equation of the class represented by (No. 4)

$$\mathrm{A}x^{\alpha}\frac{d^{\alpha}y}{dx^{\alpha}}+\mathrm{B}x^{\beta}\frac{d^{\beta}y}{dx^{\beta}}+\&\mathrm{c.}=ax^{m}+bx^{n}+\&\mathrm{c.} \quad . \quad . \quad (c)$$

being given, we can at once write down the solution of a partial differential equation of the class represented by

$$\left.\begin{array}{l}\mathrm{A}\left(x^{\alpha}\dfrac{d^{\alpha}z}{dx^{\alpha}}+\alpha x^{\alpha-1}y\dfrac{d^{\alpha}z}{dx^{\alpha-1}dy}+\&\mathrm{c.}\right)\\ +\mathrm{B}\left(x^{\beta}\dfrac{d^{\beta}z}{dx^{\beta}}+\beta x^{\beta-1}y\dfrac{d^{\beta}z}{dx^{\beta-1}dy}+\&\mathrm{c.}\right)\\ \qquad\qquad +\ \&\mathrm{c.}\end{array}\right\}=\Theta_{m}+\Theta_{n}+\&\mathrm{c.}\ (d)$$

by substituting for ax^m, bx^n, &c. the corresponding known homogeneous functions Θ_m, Θ_n, &c., leaving the numerical coefficients introduced by the process of integration untouched, and by introducing for each term in the solution of the ordinary linear differential equation in which *an arbitrary constant* is introduced, such as $\mathrm{C}_m x^m$, *a homogeneous function of the same degree, but of arbitrary form in x and y.*

Thus the solution of partial differential equations of the class (*d*) is reduced to the solution of the corresponding class (*c*) of ordinary linear differential equations.

8. So far we have only investigated and applied the analogue of the *first* fundamental principle employed by Professor Boole,

$$f(\mathrm{D}).e^{m\theta}=f(m).e^{m\theta},$$

namely,

$$\mathrm{F}(\nabla).u_m=\mathrm{F}(m).u_m.$$

By its aid we have been enabled to obtain the solutions of a very extensive class of partial differential equations, and the examples seem to show that the method possesses both generality and flexibility.

Let us proceed to investigate the analogue of the *second* fundamental principle,

$$f(\mathrm{D}).e^{m\theta}\omega=e^{m\theta}.f(\mathrm{D}+m).\omega,$$

by the aid of which many results, both important and elegant, are obtained in the memoir with considerable ease.

Putting $x=e^{\theta}$, it becomes

$$f\left(x\frac{d}{dx}\right).x^{m}\omega=x^{m}.f\left(x\frac{d}{dx}+m\right).\omega,$$

and at a glance we catch the proposed analogue,

$$F(\nabla).\Theta_m W = \Theta_m . F(\nabla + m).W, \quad . \quad . \quad . \quad . \qquad (5)$$

where Θ_m is a known homogeneous function of the independent variables of the mth degree.

This result, which seems to afford the same facilities of application to the subjects of *partial* differential equations, and *multiple* definite integrals, as the elementary theorem to those of *ordinary* differential equations and *single* definite integrals, is readily proved by the substitutions

$$x = e^{\theta}, \quad y = e^{\phi}, \ \&c.;$$

since then, generally,

$$\nabla.UW = \left(\frac{d}{d\theta} + \frac{d}{d\phi} + \&c.\right).UW = U.\nabla W + W.\nabla U,$$

and in this particular case,

$$\nabla.\Theta_m W = \Theta_m.(\nabla + m).W.$$

9. By the same substitutions we obtain extensions of the more general theorems, of which important use has been made by Mr. Hargreave in connexion with the subject of the integration of linear differential equations*, namely,

$$\phi(D).u\omega = u.\phi(D)\omega + \frac{u'}{1}.\phi'(D)\omega + \frac{u''}{1.2}.\phi''(D)\omega + \&c.$$

and

$$u\phi(D)\omega = \phi(D)u\omega - \phi'(D).u'\omega + \frac{\phi''(D)}{1.2}.u''\omega - \&c.,$$

where u and ω contain θ and D is $\frac{d}{d\theta}$. These extensions are, respectively,

$$\left.\begin{array}{l} \Phi(\nabla).UW = U.\Phi(\nabla)W + \dfrac{\nabla U}{1}.\Phi'(\nabla)W + \dfrac{\nabla^2 U}{1.2}.\Phi''(\nabla)W + \&c. \\ \text{and} \\ U.\Phi(\nabla)W = \Phi(\nabla).UW - \dfrac{\Phi'(\nabla)}{1}.\nabla U.W + \dfrac{\Phi''(\nabla)}{1.2}.\nabla^2 U.W - \&c., \end{array}\right\} (6)$$

where U and W are now functions of the n variables x, y, z, &c., and ∇ is the symbol before employed.

In the particular case in which U is homogeneous and of the mth degree, or in which

$$U = \Theta_m,$$

we fall back upon the case discussed in the last number. But the two equivalent expansions,

* Philosophical Transactions, 1848.

$$\mathrm{F}(\nabla).\Theta_m \mathrm{W} = \Theta_m \left\{ \mathrm{F}(\nabla)\mathrm{W} + \frac{m}{1}.\mathrm{F}'(\nabla)\mathrm{W} + \frac{m^2}{1.2}.\mathrm{F}''(\nabla)\mathrm{W} + \&\mathrm{c.} \right\}$$

$$\mathrm{F}(\nabla).\mathrm{W}\Theta_m = \Theta_m \left\{ \mathrm{F}(m).\mathrm{W} + \frac{\mathrm{F}'(m)}{1}.\nabla\mathrm{W} + \frac{\mathrm{F}''(m)}{1.2}.\nabla^2\mathrm{W} + \&\mathrm{c.} \right\},$$

which are in reality but the evolutions of

$$\Theta_m.\mathrm{F}(\nabla + m)\mathrm{W},$$

according to ascending powers of m and ∇ respectively, may possibly not have occurred to the reader, and are brought prominently forward by the general theorem.

An adequate application to the subject of partial differential equations, of a principle corresponding to that which Mr. Hargreave has employed with so much success in connexion with the subject of ordinary differential equations, would extend the present paper much beyond due limits, and offers too many difficulties to be treated of within a confined space. It will be sufficient, then, to endeavour to evolve the various consequences of the more simple theorem given in the preceding number.

10. By the aid of the two fundamental principles mentioned, Professor Boole has shown that ordinary differential equations of the form

$$(a + bx + cx^2 + \&\mathrm{c.})\frac{d^\alpha u}{dx^\alpha} + (a' + b'x + c'x^2 + \&\mathrm{c.})\frac{d^\beta u}{dx^\beta} + \&\mathrm{c} = \mathrm{X}$$

may always be reduced to the form

$$\phi_0(\mathrm{D}).u + \phi_1(\mathrm{D}).e^{\theta}u + \phi_2(\mathrm{D}).e^{2\theta}u + \&\mathrm{c.} = \Theta.$$

It is obvious that the solutions of such ordinary differential equations may, then, be exhibited in the shape

$$u = \begin{cases} \{\phi_0(\mathrm{D}) + e^{\theta}.\phi_1(\mathrm{D}+1) + e^{2\theta}.\phi_2(\mathrm{D}+2) + \&\mathrm{c.}\}^{-1}.\Theta \\ + \\ \{\phi_0(\mathrm{D}) + e^{\theta}.\phi_1(\mathrm{D}+1) + e^{2\theta}.\phi_2(\mathrm{D}+2) + \&\mathrm{c.}\}^{-1}.0. \end{cases}$$

Similarly, the solutions of partial differential equations of the form

$$(\Theta_0 + \Theta_1 + \&\mathrm{c.}).\Phi(\nabla)z + (\mathrm{H}_0 + \mathrm{H}_1 + \&\mathrm{c.}).\Psi(\nabla)z + \&\mathrm{c.} = \Omega$$

can be exhibited in the shape

$$z = \begin{cases} \{(\Theta_0 + \Theta_1 + \&\mathrm{c.}).\Phi(\nabla) + (\mathrm{H}_0 + \mathrm{H}_1 + \&\mathrm{c.}).\Psi(\nabla) + \&\mathrm{c.}\}^{-1}.\Omega \\ + \\ \{(\Theta_0 + \Theta_1 + \&\mathrm{c.}).\Phi(\nabla) + (\mathrm{H}_0 + \mathrm{H}_1 + \&\mathrm{c.}).\Psi(\nabla) + \&\mathrm{c.}\} \quad .0. \end{cases}$$

The following examples will illustrate this result.

(I.) $$xp + yq - (\Theta_m + \Theta_n)z = 0,$$

$$z = u_0.e^{\frac{\Theta_m}{m} + \frac{\Theta_n}{n}}.$$

$$\text{(II.)} \qquad xp+yq-\frac{m\Theta_n}{1+\Theta_n}.z=0,$$

$$z=u_0(1+\Theta_n)^{\frac{m}{n}}.$$

11. Again, it is shown in the same memoir that the solution of ordinary differential equations of the class

$$u+af(\mathrm{D}).e^{m\theta}u+bf(\mathrm{D}).f(\mathrm{D}-m).e^{2m\theta}u+\&c.=0,$$

is reducible to the solution of the system of equations

$$\left.\begin{array}{c} u-q_1f(\mathrm{D})e^{m\theta}u=0 \\ u-q_2f(\mathrm{D})e^{m\theta}u=0 \\ \&c. \end{array}\right\},$$

where q_1, q_2, &c. are the roots of the equation

$$q^n+aq^{n-1}+bq^{n-2}+\&c.=0.$$

Similarly, the solution of the class of partial differential equations represented by

$$z+\mathrm{A}.\mathrm{F}(\nabla).\Theta_m z+\mathrm{B}.\mathrm{F}(\nabla).\mathrm{F}(\nabla-m).\Theta_m^2 z+\&c.=0,$$

is reduced to the solution of the system

$$\left.\begin{array}{c} z-\mathrm{Q}_1.\mathrm{F}(\nabla).\Theta_m z=0 \\ z-\mathrm{Q}_2.\mathrm{F}(\nabla).\Theta_m z=0 \\ \&c. \end{array}\right\},$$

where Q_1, Q_2, &c. are the roots of the equation

$$\mathrm{Q}^n+\mathrm{AQ}^{n-1}+\mathrm{BQ}^{n-2}+\&c.=0.$$

It is obvious that partial differential equations of the class

$$z+\mathrm{A}\Theta_m.\mathrm{F}(\nabla)z+\mathrm{B}\Theta_m^2.\mathrm{F}(\nabla+m).\mathrm{F}(\nabla)z+\&c.=0$$

admit of a similar reduction.

12. It remains to examine the application of the general principles to the subject of multiple definite integrals, an application of which they are obviously susceptible, and which seems to open an interesting field for investigation. It would be impossible, however, here adequately to follow up such an inquiry in its details, and a general theorem, with its application to two particular cases, must for the present suffice.

If

$$\int dx\int dy\int dz...\Omega(xyz\,\&c.).a^{\phi(xyz\,\&c.)}.b^{\chi(xyz\,\&c.)}.c^{\psi(xyz\,\&c.)}...=\mathrm{K},$$

the quantities a, b, c, &c. being unconnected with the limits, then will

$$\int dx\int dy\int dz...\Omega(xyz\,\&c.).\mathrm{F}(\phi+\chi+\psi+\&c.).a^{\phi}.b^{\chi}.c^{\psi}...=\mathrm{F}(\square).\mathrm{K}$$

where, as before,

$$\square = a\frac{d}{da} + b\frac{d}{db} + c\frac{d}{dc} + \&c.$$

This is obvious, since, from the supposition made relative to a, b, c, &c., we can operate with the symbol $\square$ under the integral signs. It will be observed that the result bears a strong resemblance to Liouville's well-known extension of Dirichlet's integral.

We seem to have here made a step towards the solution of that which has been long a difficulty in the treatment of multiple definite integrals, namely, the generalization of those in which the variables enter *as complicated functions in the indices* of known quantities. The most valuable extensions yet obtained are those in which the element of the primary multiple definite integral exhibits the variables under finite forms solely.

We shall conclude by giving the two particular instances of the general theorem above alluded to. It can be easily proved that

$$\int_0^\infty dx\int_0^\infty dy\int_0^\infty dz\,.\,a^{-x^2}.b^{-y^2}.c^{-z^2} = \frac{1}{8}\pi^{\frac{3}{2}}\frac{1}{\{\log a.\log b.\log c\}^{\frac{1}{2}}};$$

hence

$$\int_0^\infty dx\int_0^\infty dy\int_0^\infty dz\,.\,\mathrm{F}(x^2+y^2+z^2)a^{-x^2}.b^{-y^2}.c^{-z^2}$$

$$= \frac{1}{8}\pi^{\frac{3}{2}}.\mathrm{F}(-\square)\frac{1}{\{\log a.\log b.\log c\}^{\frac{1}{2}}}.$$

Again, we readily see that

$$\int_0^\infty dx\int_0^\infty dy\int_0^\infty dz\,.\,a^{-px}.b^{-qy}.c^{-rz}.x^{l-1}.y^{m-1}.z^{n-1}$$

$$= \frac{\Gamma(l)\Gamma(m)\Gamma(n)}{p^l.q^m.r^n}.\frac{1}{(\log a)^l.(\log b)^m.(\log c)^n};$$

and hence

$$\int_0^\infty dx\int_0^\infty dy\int_0^\infty dz\,.\,\Phi(px+qy+rz)a^{-px}.b^{-qy}.c^{-rz}.x^{l-1}y^{m-1}z^{n-1}$$

$$= \frac{\Gamma(l).\Gamma(m).\Gamma(n)}{p^l.q^m.r^n}\Phi(-\square)\frac{1}{(\log a)^l.(\log b)^m.(\log c)^n}.$$

38 Trinity College, Dublin,
December 1851.

XXI. *Mineralogical Notes.* *By* Edward J. Chapman, *Professor of Mineralogy in University College, London**.

UNDER the above title, it is the intention of the writer to offer from time to time a series of remarks and investigations on subjects relating to mineralogy.

(1.) *Crednerite.*—If we allow the isomorphism, atom for atom, of silica and alumina, and through alumina, of silica and the sesquioxides generally†, Rammelsberg's crednerite—the mangankupfererz of Credner—may be admitted into the augite group. The cleavage form of crednerite is certainly monoclinic; and the angles, so far at least as they can be estimated in the specimens hitherto obtained, assimilate to those of the augite prism. The minerals of the augite type have the general formula $3RO, 2Si\,O^3$, which, with the substitution of sesquioxide of manganese for silica, is exactly that of crednerite as deduced by Rammelsberg, viz. $3(CuO, BaO), 2Mn^2\,O^3$. A proof of the isomorphism of $Si\,O^3$ and $Mn^2\,O^3$ is afforded by the manganese garnets and spinels, and more particularly as belonging to a system of variable angles, by Vesuvian and Hausmannite.

(2.) *Helvine.*—The helvine in the classification of Mohs bears the name of tetrahedral garnet; and with the spinel and garnet group it must in fact be placed, unless it stand alone, for to no other type amongst the silicates and their isomorphs can it be referred. On the supposition that $Si\,O^3$ and $Mn^2\,O^3$ are isomorphous, and that sulphur and oxygen are equally so, the atomic constitution of the helvine falls into the common garnet formula, $r+R$. The composition of helvine, for instance, as usually represented, $=(Be^2O^3, Fe^2O^3), SiO^3+2MnO, SiO^3+MnS$. This may be reduced into three atoms of (MnO, MnS), and three atoms of $(R^2\,O^3, Si\,O^3)$; or into equal atoms of base and acid, $r+R$.

(3.) *False cleavage in Garnet.*—A small garnet in the author's possession, from the Zillerthal, exhibits a peculiar and interesting example of false cleavage in relation to the crystalline structure of metamorphic rocks. This garnet is a combination of a trapezohedron, $\frac{1}{2}O$, with the rhombic dodecahedron; and entirely through its mass run lines of false cleavage, parallel to the cleavage planes of the mica-slate in which it is imbedded.

* Communicated by the Author.

† See a paper by the Author on the isomorphous relations of silica and alumina, in the report of the British Association for 1850.

(4.) *Phenacite and Beryl.*—Phenacite was at first mistaken for quartz, hence the derivation of its name; it is, however, very closely related to that substance. Both quartz and phenacite are hemihexagonal: in the former, the relative length of the vertical axis $=1{\cdot}095$; in the latter, $0{\cdot}5471$; so that the common phenacite rhombohedron $=\frac{1}{2}$R compared to the quartz form as unity. In beryl, again, from the two triaxial pyramids occasionally present in that mineral, we obtain for the relative length of the vertical axis in the protaxial form, the values 0·9968 and 0·4980. The first is in close accordance with that of the protaxial form of quartz.

Phenacite and beryl may therefore be considered members of the quartz group, in which, if glucina be looked upon as a sesquioxide, there can be no difficulty in placing them. On the other hand, if the formula of glucina be written BeO, the isomorphism of 3BeO with $Si\,O^3$, and the sesquioxide isomorphs of the latter, must be allowed. Of the other glucina compounds, the chrysoberyl may represent a trimetric, and the euclase a monoclinic quartz. With the former are associated staurolite, andalusite, topaz, &c. Helvine, as shown in note 2, belongs to the garnet type.

(5.) *Sphene and Epidote.*—If sphene constitute not a type of its own, the only group to which it can be referred is that of the epidote series. In epidote, the general formula—uniting the $Si\,O^3$ and $R^2\,O^3$—becomes $r^3\,R^5$. In sphene, the number of atoms $=3CaO$, $3TiO^2$, $2Si\,O^3$; and by uniting the acids, we obtain equally with epidote the formula $r^3\,R^5$.

That silica and the sesquioxides are at times isomorphous with titanic and stannic acid, we have evidence in the isomorphism, on the one hand, of idocrase, hausmannite and anatase; and, on the other hand, of zircon, rutile and cassiterite. All of these forms belong to the dimetric system, but their axial relations separate them into two distinct groups.

(6.) *Chlorite spar and Chloritoid.*—These minerals, which closely resemble each other, may be looked upon as allied to epidote. Chlorite spar is, in fact, an iron epidote. Erdmann's analysis gives in atoms $3FeO$, $3Al^2\,O^3$, $2Si\,O^3, =r^3R^5$.

The chloritoid, according to Bonsdorff's analysis, contains $3RO$, $2Al^2\,O^3$, $2Si\,O^3$, $3H^2O$. If we admit that three atoms of water may replace one atom of silica, or of alumina, these numbers produce, as before, the formula r^3R^5. It may be remarked in support of the above suggestion, that Rammelsberg considers one of the iolite metamorphs—the praseolite—to be an iolite in which one atom of silica has given place to three atoms of water.

(7.) *Wichtyne. Minerals of the Epidote Type.*—Laurent's wichtyne, the wichtisite of Hausmann, appears also to belong to the epidote type—if it be not actually an altered variety of epidote. It yields, however, by Laurent's analysis, $3RO$, R^2O^3, $4SiO^3$, whilst the epidote contains $3RO$, $2R^2O^3$, $3SiO^3$.

The epidote type may thus consist of the following minerals:—allanite or orthite (including bagrationite, &c.), gadolinite, epidote, sphene, wichtyne, chloritoid and chlorite spar. The allanite and epidote are strictly isomorphous, but their chemical formulæ are by no means alike. The former contains, in atoms, $3RO$, Al^2O^3, $2SiO^3$; the latter, $3RO$, $2R^2O^3$, $3SiO^3$. Isomorphism, therefore, is no proof of kindred composition, or some extended hypothesis must be adopted to meet the above case. If we assume that $3r = 1R$—*id est,* that one atom of silica, or of a sesquioxide, = three atoms of RO—the difficulty vanishes, and the two formulæ enter of course under one common term. This hypothesis is necessarily at present a purely gratuitous one, admitting, in fact, of the widest licence, and consequently of the widest abuse; but unless some hypothesis of the kind be, at least provisionally, adopted, we cannot retain our existing formulæ and effect at the same time a satisfactory distribution of minerals. Every fresh observation shows the insufficiency, for instance—even if convenience plead for its retention—of the division of the silicates according to the oxygen relations of their so-called bases. Few mineralogists will now disallow the propriety of placing truly vicarious or isomorphous compounds under the same common type; but the difficulty lies in the legitimate employment of heteromerous isomorphism as a classification-element. That heteromero-isomorphous compounds should in some cases be placed together and in others be kept distinct, is, however, sufficiently evident; the grounds of union or separation constitute, therefore, the question at issue. Besides crystallization characters, three other elements should here be looked to;—first, the general chemical nature of the substance; secondly, its other physical characters; and thirdly, its circumstances of occurrence. On these data I would place phenacite and beryl with quartz, but not quartz with chabasite; acmite and augite, again, together, but not augite with borax. Numerous other examples will readily occur to those conversant with the subject.

(8.) *Chrome Tourmalines.*—Many of the Siberian tourmalines contain a small amount of chromium, probably as Cr^2O^3. These specimens are generally in acicular groups, and of an extremely fine green colour. At first sight they might be mistaken for actynolite; and, indeed, a specimen which I examined had a label attached to it bearing that name. $H = 7{\cdot}0$; sp. gr. $= 3{\cdot}181$. Fusible.

(9.) *Detection of Manganese in Limestone Rocks.*—A considerable number of limestone rocks contain a minute proportion of carbonate of manganese. I have noticed this more particularly in the darker magnesian limestones of the Permian epoch, but also in various other limestones of different ages and from different localities. The common blowpipe-test—fusion with carbonate of soda—generally of so delicate an appreciation, here fails, even on the addition of nitre, to point out the presence of manganese. This is owing to the insolubility of the limestone in the carbonate of soda. If, however, a very small quantity of borax be added, so as to attack and dissolve a portion of the mass, the well-known greenish-blue enamel is quickly produced.

(10.) *Barytine.*—The crystals of barytine (BaO, SO^3) from the fuller's-earth pits in the greensand formation of Nutfield, near Bletchingly in Surrey, possess the general configuration of the Cumberland, Schemnitz, and other crystals in which the basal form P (OP of Naumann) predominates. The forms usually present in the Nutfield crystals are the prismatic forms P and D, and the diaxial pyramidal forms $\frac{1}{2}A$, $\frac{1}{4}A$, and E. L, A, and $\frac{1}{3}A$ are occasionally seen, as also the triaxial forms O, $\frac{1}{4}O$; but the latter are in general very minute.

In Naumann's notation, $P = OP$, $L = \infty \breve{P} \infty$, $D = \infty P$, $A = \bar{P}\infty$, $E = \breve{P}\infty$, $O = P$.

$D:D = 101° \; 42'$; $P:A = 121° \; 46'$; $P:\frac{1}{2}A = 141° \; 4' \; 30''$;
$P:\frac{1}{4}A = 158° \; 0' \; 30''$; $P:\frac{1}{3}A = 162° \; 6'$; $P:E \; 127° \; 15'$;
$P:O = 115° \; 39'$. Axes: $V = 1{\cdot}315$; $T = 1$; $F = 0{\cdot}8141$.

Viewed in regard to their general configuration, the crystals of sulphate of baryta fall into six groups. In the following tabular arrangement, the vertical axis is denoted by V, the frontal by F, and the transverse (right and left axis) by T.

Group 1, with P predominating.—The crystals of this group have a more or less flattened or tabular appearance. Localities: Felsobanya, Schemnitz, Freiberg, Cumberland, Nutfield, &c.

Group 2, with $\frac{1}{2}A$ predominating.—Crystals elongated parallel to axis T. Puy-de-dome, Marienberg, Przibram, &c. A combination frequently met with in trap and volcanic districts.

Group 3, with E predominating.—Crystals elongated parallel to axis F. Puy-de-dome, &c. This configuration is very rare.

Group 4, with P and E predominating conjointly.—These crystals are also elongated in general along the frontal axis, and are much more common than the above. Mies in Bohemia, Freiberg, Baden, Lancashire, Nutfield, Cheshire in Connecticut, &c.

Group 5, with $\frac{1}{2}A$ and E predominating.—These forms produce an irregular octahedron, generally elongated along the frontal axis F. Puy-de-dome, &c.

Group 6, with D predominating.—Crystals elongated vertically. A very rare configuration principally exhibited by a few crystals from Siberia, and from the Siebenbirgen district.

The most common forms of sulphate of baryta are P, $\frac{1}{2}$A, and E. D, O, and L are also of frequent occurrence, but they are comparatively of small size. L is more common than the back and front monaxial form M: the two are not often found in the same combination.

XXII. *On the Electrical Properties of Flame.* By H. Buff, *Professor of Natural Philosophy in the University of Giessen**.

PROFESSOR BUFF commences his memoir with a review of the divergent notions at present existing as to the electricity of flame; Becquerel finds electric opposition in all directions in flame, which depends upon the difference of the temperature of the metals immersed in it; Pouillet recognises a motion of electricity only from the interior to the exterior, and hence also from the base to the summit of the flame; according to Hankel†, however, the motion of the electric fluid, at least in flames obtained by the ignition of spirit, is exactly opposite, and independent of the temperature of the immersed conductor. To solve these contradictions was the object of the present investigation.

Two small strips of platinum were introduced into a glass tube closed at one end; they were separated by an interval of 1·5 line of air. The air within the tube could *not* be heated to a degree sufficient to permit the electricity of two of Daniell's cells to pass through it. When the glass became soft by heating, and both pieces of platinum were permitted to touch it, a strong deflection of the needle of a galvanometer was the consequence.

A porcelain tube two feet long and six lines wide was encompassed with glowing coals, and air was drawn slowly through it; this air could not be heated so as to allow the passage of the electricity from the source above mentioned, although the two platinum wires sunk in the air were less than a line apart, and were glowing red.

A metal web was placed over the flame of a spirit-lamp; the flame did not pass through; over the web the platinum strips were held a line apart—there was no passage of electricity.

The galvanometer used in these experiments was extremely sensitive. When two persons who were connected simply by the

* *Annalen der Chemie und Pharmacie*, vol. lxxx. S. 1.

† Phil. Mag., S. 4. vol. ii. p. 542.

wooden floor touched the ends of the wire which formed the helix of the instrument with different metals, a deflection of several degrees was obtained. The two cells before mentioned, when connected by the floor, caused a deflection of 25°. The wooden floor was thus proved to be an incomparably better conductor than air heated to 400°.

When the strips of platinum were exposed to the direct action of the flame of a spirit-lamp, the first notice of the passage of electricity was obtained when they were placed at about three inches above its extreme point, and began to show signs of redness. The deflection increased as the strips were lowered into the flame, and attained its maximum at a small distance beneath the point of the cone into which the flame shaped itself. When the flame was strongest, there was a permanent deflection of 75°.

In these experiments care was taken to preserve the strips of platinum as nearly as possible at the same temperature. The two cells were removed, and the electricity of the flame itself was exhibited when the two strips were placed, the one above the other, within the flame, with their flat surfaces horizontal, so that they assumed different temperatures. The flame-current passed always from the hottest platinum strip through the separating interval of gas to the other strip.

Another attempt was made to ascertain the point at which heated gas permitted the passage of electricity. In the centre of the flame from a Berzelius's lamp is a cone-shaped obscure mass of air as yet unburned, but strongly heated by its vicinity to the flame; into this two platinum wires connected with the two cells were introduced from beneath; they were not heated to redness, but the gas nevertheless possessed a weak capacity of conduction. An approximation to the blue rim of the flame showed an increase of conductive power, and a deflection of several degrees was obtained.

When in this case one of the wires was caused to approach the blue edge of the flame, while the other remained at a distance, a deflection of 1° to 2° was obtained after the removal of the two cells; the deflection indicated the passage of a current from the hotter to the cooler wire.

The aperture through which the air passed upwards into the flame was stopped, and thus the dark interior of the flame became formed of the vapour of alcohol and the products of its decomposition; two isolated platinum wires were introduced through the stopping-cork into the central space, but as long as they were kept at some lines distant from the inflamed portion no trace of electricity passed from one to the other. When they were caused to approach the burning portion, the described phænomena exhibited themselves. In this case also a current was observed to pass from the warmer to the less warm wire through the intervening space of gas.

The author concludes from these experiments, that air and other gases, when heated, and thus rendered conductible, excite electrically bodies plunged in them. Gases thus range themselves in the same list as other conductors of electricity. When two metallic wires, or other conductors which are connected at one end, are brought into contact with a sufficiently heated gas, we have, properly speaking, a closed circuit. If one of the places of contact with the gas be more strongly heated than the other, a thermo-electric current is the necessary consequence.

There is, however, another source of electrical excitation in the flame, as is proved by the following experiment:—One platinum wire was introduced into the obscure centre of the flame, the other was brought near its outer surface; a current immediately exhibited itself, which passed through the flame from the interior to the exterior wire. It continued to pass in the direction even after the outer wire had attained a bright red heat, while the inner one glowed but feebly. It is evident that the thermo-current which would have passed from the hotter to the cooler wire, was in this case overcome by a current, the source of which was the place of contact of the flame and the air. The electricity here developed is so feeble, that the condensing electrometer is better suited to its examination than the multiplying galvanometer. It is easy to see, observes the author, how experimenters who have neglected to separate these two sources of excitation may have arrived at contradictory results.

By properly connecting a platinum wire, which was dipped into the *centre* of the flame, with a condensing plate, the latter became charged with negative electricity, and hence the author concludes that positive electricity is given off by the *outer* surface of the flame. The charging here is exceedingly slow, and can be greatly accelerated when a second wire, which is connected with the other plate of the condenser, is held over the flame.

One end of the galvanometer wire was connected with the platinum wire which dipped into the centre of the flame, the other end of the same was connected with the earth. The current thus obtained was too feeble to cause the slightest motion of the galvanometer needle. But when a spacious platinum dish containing water was brought over the flame and connected with the other end of the galvanometer wire, it required no very sensitive instrument to demonstrate the existence of a current.

"Hence," observes the author, "as the strength of the flame-current by an equal chemical activity and equal conduction of the inner portion of the flame is essentially dependent on the nature of the conduction from its upper portion, it must be conjectured that the formation and carrying away of carbonic acid exercises only a subordinate influence in the matter."

Two pieces of charcoal, one of which is less heated than the other by the flame, deport themselves exactly as a pair of platinum wires under the same circumstances. Silver, copper, brass and zinc, have been also examined, all of which exhibited the same electrical deportment as platinum when brought into contact with heated air.

The following conclusions are drawn from the experiments above described:—

1. Gaseous bodies which have been rendered conductible by strong heating are capable of exciting other conductors, solid as well as gaseous, electrically.

2. When a thermo-electric circuit is formed of air, hydrogen or carburetted hydrogen, alcohol vapour, charcoal, or finally a metal, whether combustible or incombustible, an electric current is developed, which proceeds from the hottest place of contact through the air to the less warm place.

3. The development of electricity which has been observed in processes of combustion, and particularly in flame, is due to thermo-electric excitation, and stands in no immediate connexion with the chemical process.

4. The products of combustion do not therefore by any means occupy the relation to the burning body which has been assumed by Pouillet; if positive electricity rises with the ascending gases, it is only in the degree in which the burning body and the air exterior to the place of combustion, or rather exterior to the place of hottest contact, are connected by a proper conductor.

XXIII. *Notices respecting New Books.*

A Treatise on Problems of Maxima and Minima, solved by Algebra. By Ramchundra, *Teacher of Science, Delhi College.* Calcutta. 8vo. 1850.

THE time will come when Hindu antiquaries will search out the history of the revival of algebra in their country, by the agency of its introduction from the West. It will then perhaps appear worthy of note, that one of the earliest native attempts to write algebra in the European form is also an attempt to show that the domain of pure algebra can be extended, without prejudice to the superior facility of the differential calculus and of its equivalents.

The author's method, in general terms, is as follows:—If ψx be a function of the nth degree, which is to be made a maximum or minimum, it is assumed that $x^{n-2}+ax^{n-3}+\dots$ is a divisor of $\psi x-r$. The division being made, the identification of the remainder with zero leads to $n-2$ equations between the $n-1$ quantities r, a, &c.;

and the quotient, being of the second degree, shows the value of r in terms of a, b, &c., which separates the real from the imaginary roots. This value is the maximum or minimum required, and the equations are then numerous enough to determine r in terms of the coefficients of ψx. The author applies this method to cases as high as the sixth degree, with quantities of two terms, and then takes various problems in which more variables than one are found.

If it were given to one of our mathematicians to make $mx^5 - x^6$ a maximum without any use of hypothetical increments added to hypothetical values, that is, without any use of the principle of the differential calculus, he would soon do justice to the ingenuity of the Delhi teacher; and this though he might smile at two pages of algebra substituted for two lines of the higher analysis. But the student of history has seen the use of compelling investigative power to work under restrictions. And the denial of tools of one kind has always been the stimulus to the improvement of others.

XXIV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 71.]

Jan. 15, 1852. CHARLES WHEATSTONE, Esq., F.R.S., delivered the Bakerian Lecture, "Contributions to the Physiology of Vision."—Part II. On some remarkable, and hitherto unobserved, phenomena of Binocular Vision.

The first part of these researches was communicated to the Royal Society in 1838, and published in the Philosophical Transactions for that year.

The second part, now presented, commences with an account of some remarkable illusions which occur when the usual relations which subsist between the magnitude of the pictures on the retinæ and the degree of inclination of the optic axes are disturbed. Under the ordinary circumstances of vision, when an object changes its distance from the observer, the magnitude of the pictures on the retinæ increases at the same time that the inclination of the optic axes becomes greater, and *vice versâ*, and the perceived magnitude of the object remains the same. The author wished to ascertain what would take place by causing the optic axes to assume every degree of convergence while the magnitude of the pictures on the retinæ remains the same; and, on the other hand, the phenomena which would be exhibited by maintaining the inclination of the optic axes constant while the magnitude of the pictures on the retinæ continually changes. To effect these purposes, he constructed a modification of his reflecting stereoscope; in this instrument two similar pictures are placed, on moveable arms, each opposite its respective mirror; these arms move round a common centre in such manner that, however they are placed, the reflected images of each picture in the mirrors remains constantly at the same distance from the eye by which it is viewed; the pictures are also capable of sliding along these arms, so that they may be simultaneously brought nearer to, or removed further from, the mirrors. When the pictures remain at the same distance and the arms are removed round their centre, the reflected images, while their distances from the eyes remain unchanged, are displaced, so that a different inclination of the optic axes is required to cause them to coincide. When the arms remain in the same positions and the pictures are brought simultaneously nearer the mirrors, the reflected images are not displaced, and they always coincide with the same convergence of the optic axes; but the magnitude of the pictures on the retinæ becomes greater as the pictures approach. The experimental results afforded by this apparatus, so far as regards the perception of magnitude, are the following: the pictures being placed at such distances, and the arms moved to such positions, that the binocular image appears of its natural magnitude and its proper distance, on the arms being moved so as to occasion the optic axes to converge less, the image appears larger, and on their being moved so as to cause the optic axes to converge more, the image appears less; thus, while the magnitude of the pictures on the retinæ remains constantly the same, the perceived magnitude of the object varies, through a very considerable range, with every degree of the convergence of the optic axes. The pictures and arms being again placed so that the magnitude and distance of the object appear the same as usual, and the arms being fixed so that the convergence of the optic axes does not change; while the pictures are brought nearer the mirrors the perceived magnitude of the object increases, and it decreases when they are removed further off; thus, while the inclination of the optic axes remains constant, the perceived magnitude of the object varies with every change in the magnitude of the pictures on the retinæ. After this the author takes into consideration the disturbances produced in our perception of distance under the same circumstances, and concludes that the facts thus experimentally ascertained regarding the perceptions of magnitude and distance, render necessary some modification in the prevalent theory regarding them.

The author next reverts to the stereoscope and its effects. He recommends the original reflecting stereoscope as the most efficient instrument, not only for investigating the phenomena of binocular vision, but also for exhibiting the greatest variety of stereoscopic effects, as it admits of every required adjustment, and pictures of any size may be placed in it. A very portable form of this instrument is then described, and also a refracting stereoscope suited for Daguerreotypes and small pictures not much exceeding the width between the eyes. In the latter instrument the pictures are placed side by side and viewed through two refracting prisms of small angle which displace the pictures laterally, that on the right side towards the left, and that on the left side towards the right, so that they appear to occupy the same place. When the first part of these investigations was published the photographic art was unknown, and the illustrations of the stereoscope were confined to outline and shaded perspective drawings; when, however, in the succeeding year, Talbot and Daguerre made their processes known, Mr. Wheatstone was enabled to obtain binocular Talbotypes and Daguerreotypes of statues, buildings, and even portraits of living persons, which, when presented in the stereoscope, no longer appeared as pictures, but as solid models of the objects from which they were taken. This application was first announced in 1841.

The two projections of an object, seen by the two eyes, are different according to the distance at which it is viewed; they become less dissimilar as that distance is greater, and, consequently, as the convergence of the optic axes becomes less. To a particular distance belongs a specific dissimilarity between the two pictures, and it is a point of interest to determine what would take place on viewing a pair of stereoscopic pictures with a different inclination of the optic axes than that for which they were intended. The result of this inquiry is, that if a pair of very dissimilar pictures is seen when the optic axes are nearly parallel, the distances between the near and remote points of the object appear exaggerated; and if, on the other hand, a pair of pictures slightly dissimilar is seen when the optic axes converge very much, the appearance is that of a bas-relief. As no disagreeable or obviously incongruous effect is produced when two pictures, intended for a nearer convergence of the optic axes, are seen when the eyes are parallel or nearly so, we are able to avail ourselves of the means of augmenting the perceived magnitude of the binocular image mentioned at the commencement of this abstract. For this purpose the pictures, placed near the eyes, are caused to coincide when the optic axes are nearly parallel; and the diverging rays proceeding from the near pictures are rendered parallel by lenses of short focal distance placed before the mirrors or prisms of the stereoscope.

Some additional observations were next brought forward respecting those stereoscopic phenomena which the author, in his first memoir, called "conversions of relief." They may be produced in three different ways:—1st, by transposing the pictures from one eye to the other; 2ndly, by reflecting each picture separately, without transposition; and 3rdly, by inverting the pictures to each eye separately. The converse figure differs from the normal figure in this circumstance, that those points which appear most distant in the latter, are the nearest in the former, and *vice versâ*.

An account is then given of the construction and effects of an instrument for producing the conversion of the relief of any solid object to which it is directed. As this instrument conveys to the mind false perceptions of all external objects, the author calls it a Pseudoscope. It consists of two reflecting prisms, placed in a frame, with adjustments, so that, when applied to the eyes, each eye may separately see the reflected image of the projection which usually falls on that eye. This is not the case when the reflexion of an object is seen in a mirror; for then, not only are the projections separately reflected, but they are also transposed from one eye to the other, and therefore the conversion of relief does not take place.

The pseudoscope being directed to an object, and adjusted so that the object shall appear of its proper size and at its usual distance, the distances of all other objects are inverted; all nearer objects appear more distant, and all more distant objects nearer. The conversion of relief of an object consists in the transposition of the distances of the points which compose it. With the pseudoscope we have a glance, as it were, into another visible world, in which external objects and our internal perceptions have no longer their habitual relations with each other. Among the remarkable illusions it occasions, the following were mentioned. The inside of a teacup appears a solid convex body; the effect is more striking if there are painted figures within the cup. A china vase, ornamented with coloured flowers in relief, appears to be a vertical section of the interior of the vase, with painted hollow impressions of the flowers. A small terrestrial globe appears a concave hemisphere; when the globe is turned on its axis, the appearance and disappearance of different portions of the map on its concave surface has a very singular effect. A bust regarded in front becomes a deep hollow mask; when regarded *en profile*, the appearance is equally striking. A framed picture, hung against a wall, appears as if imbedded in a cavity made in the wall. An object placed before the wall of a room appears behind the wall, and as if an aperture of the proper dimensions had been made to allow it to be seen; if the object be illuminated by a candle, its shadow appears as far before the object as it actually is behind it.

The communication concludes with a variety of details relating to the conditions on which these phenomena depend, and with a description of some other methods of producing the pseudoscopic appearances.

XXV. *Intelligence and Miscellaneous Articles.*

ON M. GILLARD'S LIGHT FOR ILLUMINATION OBTAINED FROM THE BURNING OF HYDROGEN. BY B. SILLIMAN, JUN.

WE have had an opportunity of seeing the successful application of M. Gillard's patent in the extensive silver plate works of Messrs. Christolef in Paris. It is well known that M. Gillard claims the production of a useful light and great heat from the combustion of hydrogen in contact with a coil of platinum wire, the hydrogen being produced by the decomposition of water. The apparatus employed is very simple, and consists essentially of one or more cylinders of iron arranged horizontally in a furnace similar in all respects to the usual arrangement for the production of coal-gas. The retorts are charged with wood-charcoal reduced to small fragments of uniform size and heated to an intense degree. Through each of the retorts steam is conducted in a tube pierced with numerous very minute holes so disposed as to distribute the steam in a uniform and very gradual manner over the heated coal. The boiler for the production of the steam is conveniently situated in the same furnace employed for heating the retorts. Decomposition of water ensues of course, accompanied with the production of carbonic acid (CO^2), carbonic oxide (CO) in small quantity, of free hydrogen and a limited quantity of light carburetted hydrogen gas (C^2H). The mixture of these gases is conducted through a lime purifier to remove carbonic acid, and without further washing or purification the product is ready for use. Consisting almost wholly of hydrogen gas, the flame of its combustion is of course very feebly luminous; to obviate this difficulty, it is burned in contact with a cage or network of platinum wire-gauze surrounding an ordinary Argand burner, protected by a glass chimney. This simple contrivance (so well known in the lecture-room) is perfectly successful, and the light given out from gas lamps of this construction is extremely vivid and constant.

This invention claims the following advantages in practice:—1. The gas so produced is cheaper than any other mode of artificial light, costing, as is asserted by M. Gillard, and sustained by the ample experience of M. Christolef, only about $\frac{1}{16}$th the average cost of coal-gas. 2. The gas has no unpleasant odour, being entirely free from the volatile hydrocarbons which are so peculiarly offensive in oil and coal-gas. 3. The products of its combustion are almost solely water, so little carbonic acid resulting in the combustion, that practically it may be disregarded. 4. This mode of producing gas may be applied to any existing gas-works by a slight modification of the retorts, and without any essential change in other portions of the apparatus, the platinum cages being applied to the Argand burners. 5. The cheapness of this mode enables us to apply it with great advantage as a fuel for cooking and for numerous purposes in the arts. For example, we saw in the establishment of M. Christolef, the soldering of silver plate accomplished in a rapid and remarkably neat manner by a powerful jet of this gas, driven by a pneumatic apparatus. Its perfect manageableness, the ease with which an intense heat is applied locally and immediately when it is wanted, coupled with advantages of employing for such a purpose so powerful a deoxidizing agent as hydrogen, render this mode of soldering preferable to every other, and peculiarly suited for the process of autogenous soldering. 6. The nuisances resulting from the presence of large coal-gas works in populous districts are entirely avoided by this mode, which is as free from objection as a steam-engine. 7. The arrangements are so simple and inexpensive, that every establishment, where it is desired to employ light and heat, may erect its own apparatus even in the most isolated situation, all the materials employed being everywhere accessible.

It is understood that M. Gillard has secured his patent in the United States, and it is presumed that his method will soon be practically tested there.

We merely add that the result of M. Gillard's invention in one particular differs from the anticipation of chemists; that is, we should expect from the decomposition of water in this mode the production of carbonic oxide CO, carbonic acid CO^2, and light carburetted hydrogen C^2H, with a limited amount of free hydrogen. The result of his experience, however, seems to establish the statements already made, as may be seen in a report of the Commissioner of the Society for the Encouragement of Industry, &c., to whom the subject was referred.—Silliman's *Journal*, September 1851.

ON THE CRYSTALLIZATION OF SULPHUR. BY CH. BRAME.

Since the time that Mitscherlich demonstrated that melted sulphur crystallized in oblique rhombic prisms, and confirmed Hauy's statement that sulphur, dissolved in bisulphuret of carbon, crystallized out in rhombic octohedrons, the opinion has been entertained that sulphur crystallizes in oblique prisms after melting, in octohedrons with a rhombic base from a solution, and that the prismatic sulphur becomes opake on account of the gradual assumption of the octohedral structure.

The author now shows in his paper that rhombic octohedrons are produced by the influence of mechanical subdivision, the removal by means of steam of several bodies which also act mechanically upon melted sulphur. A temperature of 122° F. produces them in the utricles of sulphur (utricules de soufre). At 212° F. small soft utricles (dendrites) are converted into rhombic octohedrons, as well as a part of the vesicles.

On the contrary, prismatic plates are always formed, however thin the stratum of melted sulphur may be; as, for example, that which is deposited upon a glass plate when sulphur vapour at 392° F. condenses slowly. These crystals are generally right rhombic prisms; but as soon as the stratum becomes only a little thicker, the crystals obtained are oblique rhombic.

According to the author, sulphur crystallizes in oblique prisms from the melted state only when an excess of fluid sulphur is present, however thin the stratum may be. In the opposite case, the true or modified octohedron presents itself.

By subdivision the melted sulphur may be separated into a multitude of minute drops, which from solidifying upon the surface become covered with a more or less thick crust. If this is very thin, the drops of sulphur are converted into utricles. If it is thicker, the sulphur drops are more or less regularly flattened by pressure, and there results instead of an utricle a flattened drop with a quadratic basis, which is or is not modified at its corners, and appears altogether like a considerably modified rhombohedron. The extremely thin coats of melted sulphur which are obtained by vaporization appear to explain, by their behaviour described above, how it is that any given pressure acts: it always produces a right rhombic prism. The rhombic octohedron appears, on the contrary, to be formed when that crust has become so thick that it is capable of resisting the pressure; and then there is a pressure upon the interior mass, and a contrary pressure upon the interior surface of the drop; the crystalline form which is assumed under such conditions is the rhombic octohedron.

The author does not consider that it may be admitted as proved, that the sulphur becomes opake on account of the transition of the oblique prisms into rhombic octohedrons. Pasteur did not find any octohedrons among the fragments of prisms which had been formed from sulphur in solution; and the author found that, in consequence of the crystallization of the utricles, octohedrons might be contained in the oblique rhombic prisms which are obtained from sulphur by melting. Therefore when octohedrons are found in prisms of sulphur, they have originated from previously formed utricles, and do not indicate a transition of the oblique rhombic prism into the rhombic octohedron.—*Comptes Rendus*, vol. xxxiii. pp. 338–540.

ON THE ELECTRO-MAGNETIC MOTOR OF FESSEL. BY M. PLÜCKER.

It is known that Mr. Page, a physicist in North America, has recently endeavoured to produce a motive power by an extended application of the force which attracts a mass of iron within an electro-magnetic helix. Th. Hankel of Leipzic has made the same attempt, and has established an important practical law, namely, that this force is as the square of the power of the current. M. Fessel has on his part constructed a model of a machine at my request, the value of which I am not for the moment able to appreciate in case it were made on a large scale, but which as a piece of physical apparatus explains and clears up the application of the force in question.

The model of Fessel is formed of two helices placed end to end in a horizontal position. They serve to conduct the current always in the same direction, but in such a way that it traverses alternately each of the two helices, and consequently only one at a time. In the interior of the helices is a bar of iron, which is alternately attracted from the one into the other by constantly maintaining the same polarity, and which thus executes a motion backwards and forwards. To the two extremities of the bar are fixed two slender horizontal shanks of brass, which rest upon two pulleys attached to the two extremities of the apparatus, and which thus support the whole weight of the iron. One of these shanks sets a wheel in motion. A commutator is moved by an excentric by means of a directing-rod, which is placed so as to be able to make the machine move backwards and forwards as in steam-vessels. In one of the machines the commutator has been fixed immediately to the axis.

Two couples of Grove's cells are sufficient to communicate to this apparatus a great rapidity. With six couples, the rapidity became such that it threatened to break the apparatus; and fearing this, I stopped the passage of the current.

I have just received from him the news that he has nearly completed the construction of a new apparatus, in which he has replaced the pulleys by oscillating shanks of metal rod, similar to the oscillating cylinders of the steam-engines.—*Bibliothèque Universelle de Genève*, December 1851.

ON THE PRESENT CONDITION OF VESUVIUS.

BY B. SILLIMAN, JUN.

The eruption of Vesuvius in February 1850, and that of the year previous, entirely changed the summit features of this ancient mountain of fire. The former crater disappeared, being filled with scoria and ashes, while *two* craters now occupy the summit of the cone. The deepest and most active of these is that of February 1850, which is situated on the side of the cone nearest to Pompeii. It is somewhat lower and has a much greater depth than its immediate neighbour, which is on the side of the bay of Naples. We had no means of measuring its depth accurately, but judging from the time required for the returning sound of a stone cast into its mouth, as well as from inspection and comparison, we assumed the depth of the new crater to be from 800 to 1000 feet. It is acutely funnel-shaped at an angle of not less than 60°. It is impossible, because of the steam and vapours of sulphurous acid, to see its bottom, even if not prevented by the danger of the descent to a position where one might hope to catch a glimpse of its bottom. Its activity at present is confined to the emission of vapour, and even this seems at times, when viewed from the sea, to be wanting. On the summit, however, these vapours appear dense enough and are sufficient to prevent the possibility of making the entire circuit of the crater. From this cause we were unable to examine the lip dividing the crater of 1850 from its neighbour. The observer is much struck not only with the change of form in the summit, as shown by the drawings of Prof. Scacchi, but also with the sharpness of the lip of both craters, which is such that it is hardly possible for more than two persons to stand abreast upon it. During the late eruption, the lava found vent from the base of the cone on a level with the sand plain which fills the ancient crater of Somma. It here poured out a torrent of scoriaceous red lava through a well-defined canal. This is now entirely cold, and we collected from its sides abundant specimens of aphthitalite, which frosted over the rugged cavern like snow. Near this spot also are two fumeroles, formed during the last eruption; the largest about 25 feet high, with an aperture of near ten feet, its outer walls black, rugged and forbidding. The flow of lava from the eruption of 1849 was in the direction of the ancient Pompeii, and it was copious enough to destroy a small village with its vineyards at the distance of several miles. The king of Naples has since erected a new village for the unfortunate inhabitants near the site of the former one.

During the past six years the king of Naples has also constructed a carriage road up the side of Vesuvius as far as the Hermitage, where he has a Royal Meteorological Observatory, under the direction of the celebrated Melloni. This road follows in a very serpentine path over and around the hill of ashes, which all who have seen Vesuvius will remember as forming a remarkable feature in its topography. In this manner, sections have been opened in the hill for a distance of three or four miles, and were these viewed without reference to the immediate proximity of the volcano which has produced the deposit, it would be easy to refer the whole to an alluvial origin, so characteristic are the undulating lines of deposition, the alternation of coarse and fine materials interstratified, including now large angular masses of rock, and again graduating into the finest silt and mud. In some places the lines of deposition are curved in regular undulations, and in others they meet at a sharp unconformable angle. Close observation alone detects that the whole material is volcanic—pumice, scoria, sand and fine dust, including large blocks of inflated lava and tufa.

It is impossible to see any difference in the general character of these deposits and of those which cover Pompeii, only that the latter being mostly the result of one eruption are less varied than the former, and more regularly stratified. In both, the evidence of aqueous action is very obvious; and we have historical as well as geological evidence of the eruption of vast volumes of aqueous vapour with the lapilli, scoria and fine ashes from Vesuvius, which, condensing into rain, produced a deluge of hot mud, filling the most intricate recesses of the Pompeian houses, and producing the appearance of an aqueous deposit in the ash hills of the flanks of Vesuvius. In Herculaneum we see the same phænomena in a more remarkable manner. Here, owing to a much larger accumulation of material—to subsequent overflows of lava and the superincumbent weight thus produced, with the aid of water, the ashes were consolidated into so compact a mass, that some writers have even doubted whether Herculaneum had not been destroyed by an overflow of lava in the first instance. That such was not the fact is well known, and the condition of the antiquities imbedded there quite forbid the idea were no other evidence attainable.—Silliman's *Journal*, September 1851.

ON THE SULPHUR DEPOSITS AT SWOSZOWICE AND RADOBOJ.

Professor L. Zeuschner has given a description of the sulphur stratum of Swoszowice near Cracow. It is situated in the tertiary formation. Sulphur and gypsum lie in parallel beds in a deposit of marl of considerable thickness. The entire deposit is 243 feet thick, and contains five layers of sulphur at almost equal distances of twelve feet. The uppermost layer of sulphur consists of grains of sulphur about the size of hemp-seed, which are disseminated through the marl. Sometimes the grains are attached like bunches of grapes. The second layer of sulphur is separated from the first by a gray marl of from 12 to 30 feet in thickness. The layer itself consists of small nodules of compact sulphur, is thicker than the former, being from 2 to 9 feet, and presents parallel layers separated by marl. The sulphur contains scarcely any admixture of foreign substances. In some places groups of sulphur crystals occur mixed with small crystals of calcareous spar. Only these two upper layers are worked, while the three lower ones are only known by boring experiments.

The sulphur layer at Radoboj in Croatia has been described by

A. v. Morlot. It was discovered accidentally in 1811 by a peasant, and has been worked from that time. The sulphur lies in a slaty marl, which is situated between the miocene formation and the *calcaire grossier*, and itself belongs to the eocene formation. The latter adjoins the dolomite of the magnesian limestone, and has an inclination rather less than 45°. The sulphur bed consists of four layers. The uppermost layer, for the most part 8 to 10 inches thick, contains nodules of sulphur from the size of a nut to that of a man's head lying separately in marl slate, and is only now and then accompanied by gypsum. Then follows an argillaceous sandstone 10 to 12 inches thick, which contains a remarkable quantity of fossil remains, not only of plants, but especially of insects and fishes. Under it lies a second deposit of sulphur, 10 to 12 inches thick, in a dark bituminous marly slate from which the sulphur has to be separated by distillation. A clayey bituminous marly slate, 12 inches thick, forms the bottom stratum. The sulphur beds are covered by, and rest upon hard marly slates.—*Arch. de Pharm.* 2 *R.*, vol. lxvi. pp. 315, 316.

METEOROLOGICAL OBSERVATORY OF MOUNT VESUVIUS.

The Meteorological Observatory recently erected at Mount Vesuvius was projected by Prof. Melloni, so well known to all the world by his memorable researches on heat, and the most distinguished of all the Italian physicists. The king of Naples gave the enterprise his sanction, and furnished the means to construct the building. The house is of ample dimensions, standing on an artificial terrace at the summit of the hill of ashes which forms the limit of the arable region of Vesuvius, and at an elevation of about 2000 feet. The centre has three floors above the basement, and the two wings each one floor above the basement; in the rear and joining the main building is a round tower, and the roofs are conveniently arranged for meteorological purposes. All the plans were furnished by Prof. Melloni, who also superintended its erection, which by an inscription on the exterior appears to have been begun in 1841.

Unfortunately for science, the revolution of 1848 entirely arrested the further progress of the undertaking; the house stands vacant, no instruments are provided, and worst of all, Prof Melloni has been removed, not only from his direction in the Observatory, but also from his Professorship in the University, under the caprice of a despot who knows no law but his own will, and who has shown in this act that he was unworthy of so noble a subject.—Silliman's *Journal*, September 1851.

EXPERIMENTS ON THE APPLICATION OF ELECTRO-MAGNETISM AS A MOTIVE FORCE. BY M. ARISTIDE DUMONT.

The author announces in the following terms the consequences to be deduced from the experiments reported in his memoir:—

1. The electro-magnetic force, although it cannot yet be compared to the force of steam in the production of great power, either as it

regards the absolute amount of power produced, or the expense, may nevertheless in certain circumstances be usefully and practically applied.

2. While in the development of great power the electro-magnetic force is very far inferior to that of steam, it becomes equal and even superior to it in the production of small forces, which may be thus subdivided, varied, and introduced into trades and occupations using but small capitals, where the absolute amount of mechanical power is less exerted than the facility of producing it instantaneously and at will.

3. In this point of view the electro-magnetic force assists, as it were, the usefulness of steam, in place of uselessly competing with it.

4. Other things being proportional, electro-magnetic machines with direct alternating movement present a great superiority of the power developed over rotating machines; since in the first there are no components lost, and with the same expense a much more considerable power is obtained than with rotating machines.

5. In machines of direct movement, the influence of the currents of induction appears less considerable than in rotating machines.

6. Finally, in the calculation of the expense, it is proper to include deduction of the value of the sulphate of zinc produced, and to take into consideration, that, in apparatus of any considerable size, the same battery may be used at the same time for the production both of the power and light.—*Comptes Rendus*, August 25, 1851.

METEOROLOGICAL OBSERVATIONS FOR DEC. 1851.

Chiswick.—December 1. Frosty: fine: uniformly overcast at night. 2. Overcast: clear. 3. Hazy: cloudy: frosty at night. 4. Frosty: fine. 5. Hazy: cloudy: overcast. 6. Densely overcast. 7. Fine: cloudy. 8. Cloudy: clear and very fine. 9. Foggy. 10. Cloudy. 11. Clear and fine. 12. Very dense fog. 13. Foggy: hazy throughout. 14. Foggy. 15. Hazy. 16. Foggy: overcast. 17, 18. Foggy. 19. Very fine. 20. Hazy and drizzly: densely overcast at night. 21. Rain: boisterous at night. 22. Rain: clear at night. 23. Clear and fine. 24. Hazy: fine. 25. Clear and fine: cloudy at night. 26. Fine: sharp frost. 27. Frosty: overcast: slight rain. 28. Fine: densely clouded: clear. 29. Slight haze. 30. Foggy. 31. Frosty and foggy: hazy.

Mean temperature of the month	38°·88
Mean temperature of Dec. 1850	38 ·47
Mean temperature of Dec. for the last twenty-six years ...	39 ·69
Average amount of rain in Dec.	1·52 inch.

Boston.—Dec. 1. Fine. 2—4. Cloudy. 5. Cloudy: rain A.M. 6. Cloudy. 7—9. Fine. 10. Cloudy: rain P.M. 11, 12. Fine. 13. Foggy. 14—19. Cloudy. 20. Fine. 21. Rainy: rain A.M. and P.M. 22. Cloudy. 23. Fine. 24. Cloudy. 25, 26. Fine. 27. Cloudy. 28. Cloudy: rain P.M. 29, 30. Cloudy. 31. Fine.

Sandwick Manse, Orkney.—Dec. 1. Cloudy: damp. 2. Damp. 3. Showers: damp. 4. Rain: showers. 5. Showers: drizzle. 6. Bright: drizzle. 7. Cloudy. 8. Damp: showers: clear. 9. Damp: drizzle. 10. Cloudy: rain. 11. Damp: drizzle. 12. Bright: cloudy. 13. Drizzle: clear. 14. Fine. 15. Fine: damp. 16. Bright: fine: damp. 17. Damp: fine: damp. 18. Bright: fine: aurora. 19. Cloudy: fine: aurora. 20. Cloudy: drizzle. 21. Rain: clear: aurora. 22. Frost: clear: aurora. 23. Bright: clear: aurora. 24. Frost: aurora. 25. Frost: cloudy. 26. Fine: clear: aurora. 27. Fine: cloudy. 28. Cloudy. 29. Cloudy: damp. 30. Drizzle: rain. 31. Drizzle: cloudy.

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

Days of Month.	Barometer.					Thermometer.					Wind.			Rain.		
	Chiswick.		Boston. 8¾ a.m.	Orkney, Sandwick.		Chiswick.		Boston. 8¾ a.m.	Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Orkney, Sandwick.	Chiswick.	Boston.	Orkney, Sandwick.
1851. Dec.	Max.	Min.		9¼ a.m.	8½ p.m.	Max.	Min.		9¼ a.m.	8½ p.m.						
1.	30·257	30·245	29·97	30·32	30·31	39	32	32	42	42	w.	nw.	nnw.	....	....	....
2.	30·267	30·233	29·87	30·24	30·23	41	27	38	41½	41	nw.	wnw.	nnw.	....	....	....
3.	30·285	30·187	29·94	30·16	30·16	39	24	35·5	40	39½	sw.	wnw.	sw.	....	....	·07
4.	30·245	30·234	29·90	30·02	29·93	43	35	30	43½	50	sw.	w.	wsw.	....	....	·07
5.	30·241	30·229	29·85	29·81	29·74	47	39	43	48	50	sw.	w.	w.	·02	....	·20
6.	30·221	30·204	29·77	29·64	29·60	49	44	49	50	50	sw.	w.	ssw.	....	·05	....
7.	30·202	30·069	29·78	29·55	29·42	51	40	42	50	49	s.	sw.	s.	....	....	·03
○ 8.	30·214	29·919	29·46	29·02	29·75	54	25	48	47	47	w.	sw.	wnw.	....	....	....
9.	30·189	30·091	29·72	29·51	29·46	54	52	40	45	50	sw.	ssw.	s.	....	....	·42
10.	30·038	29·981	29·50	29·38	29·54	53	33	51·5	49	45	s.	sw.	w.	....	....	·27
11.	30·521	30·366	29·94	29·94	30·16	51	27	43·5	49	51½	w.	wsw.	wsw.	....	·04	·41
12.	30·516	30·442	30·12	30·24	30·26	38	30	32	50	47	nw.	w.	wsw.	....	....	....
13.	30·404	30·389	30·08	30·41	30·48	44	37	35	50	46	e.	calm	ssw.	....	....	·02
14.	30·472	30·414	30·13	30·45	30·37	41	36	42	43	42	e.	e.	s.	....	....	·03
☾ 15.	30·437	30·387	30·07	30·22	30·08	40	36	38	41	48	s.	w.	ssw.	....	....	....
16.	30·365	30·336	29·96	30·04	30·11	43	39	38	46	43½	sw.	s.	ese.	·01	....	·23
17.	30·301	30·232	29·94	30·08	30·00	44	32	43	45	43	s.	nw.	calm	....	....	·05
18.	30·185	30·136	29·86	29·73	29·76	45	34	36	43	45	s.	nw.	ssw.	....	....	....
19.	30·118	29·995	29·70	29·77	29·75	52	43	42	44	41	s.	n.	se.	·01	....	....
20.	30·087	29·968	29·67	29·55	29·41	53	46	48	45	48½	s.	n.	se.	·02	....	....
21.	29·760	29·564	29·35	29·36	29·53	49	38	49	43	39	sw.	nw.	se.	·15	....	·29
● 22.	29·832	29·543	29·31	29·62	29·87	46	35	41	36	43	e.	n.	nw.	·31	·45	·03
23.	30·226	30·063	29·75	30·07	30·16	46	30	37	44	41	ne.	n.	sw.	....	....	·03
24.	30·288	30·261	29·95	30·15	30·13	41	24	32	36	37	se.	nw.	ese.	....	....	....
25.	30·357	30·318	29·98	30·18	30·32	41	25	34·5	34	38	sw.	w.	e.	....	....	....
26.	30·532	30·439	30·20	30·45	30·52	43	17	32	40	32½	e.	se.	calm	....	....	....
27.	30·488	30·349	30·20	30·49	30·51	40	29	32	39	45½	sw.	nnw.	wnw.	·02	....	....
28.	30·427	30·300	30·12	30·52	30·47	42	31	42·5	46	46½	e.	ne.	w.	·08	....	....
29.	30·493	30·485	30·20	30·27	30·17	40	36	38	49	49	ne.	ne.	w.	....	·04	....
☽ 30.	30·488	30·398	30·12	30·06	29·92	40	21	36	46½	49	sw.	w.	w.	....	....	·14
31.	30·262	30·044	29·91	30·85	29·88	36	28	34	46½	42½	sw.	sw.	calm	....	....	·30
Mean.	30·281	30·188	29·88	30·003	30·000	44·67	33·09	39·1	44·25	44·58				0·62	0·58	2·59

THE

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XXVI. *On the Optical Properties of a newly-discovered Salt of Quinine, which crystalline substance possesses the power of polarizing a ray of Light, like Tourmaline, and at certain angles of Rotation of depolarizing it, like Selenite. By* William Bird Herapath, *M.D. London University, M.R.C.S. Engl., Member of the Bristol Microscopical Society, &c.**

[With a Plate.]

SOME short time since my pupil called my attention to some peculiarly brilliant emerald-green crystals, which had formed by accident in a solution of the disulphate of quinine. He could give me no account of their formation. Some experiments made upon them convinced me of their importance, both chemically and optically, and led me to suspect that iodine was in some way necessary to their composition.

Upon dropping tincture of iodine into the solution of disulphate of quinine in diluted sulphuric acid, an abundant deposition of similar crystals immediately occurred. However, it was found exceedingly difficult to experiment in a satisfactory manner upon the crystals thus formed, as it was almost impossible to isolate them from their mother-liquid. It was subsequently found, that by dissolving the disulphates of quinine and cinchonine of commerce in concentrated acetic acid, upon warming the solution, and dropping into it a spirituous solution of iodine carefully by small quantities at a time, and placing the mixture aside for some hours, large brilliant plates of this substance were produced. These could be readily separated from their mother-liquid, and by frequent recrystallization, purified.

The crystals of this salt, when examined by reflected light,

* Communicated by the Author; to whose liberality we are likewise indebted for the beautiful plate which accompanies this paper.

have a brilliant emerald-green colour, with almost a metallic lustre; they appear like portions of the elytra of cantharides, and are also very similar to murexide in appearance. When examined by transmitted light, they scarcely possess any colour, there is only a slightly olive-green tinge; but if two crystals crossing at right angles be examined, the spot where they intersect appears as black as midnight, even if the crystals are not $\frac{1}{500}$dth of an inch in thickness. (See Plate IV. fig. 1.) If the light used in this experiment be in the slightest degree polarized, as by reflexion from a cloud, or by the blue sky, or from the glass surface of the mirror of the microscope placed at the polarizing angle, 56° 45′, these little prisms immediately assume complementary colours. One appears green and the other pink; and the part at which they cross is a chocolate or deep chestnut-brown instead of black.

Their optical properties will be more minutely examined hereafter.

Their *chemical characters* are the following:—

They are immediately redissolved upon heating the acid liquid to 180°, and recrystallize on cooling; those formed in the sulphuric acid solution, if exposed to the air on a narrow slip of glass, upon the concentration of the mother-liquid by evaporation, will slowly disintegrate by dissection, and at length dissolve completely. They are also altered by diluting the solution with distilled water, appearing to become disintegrated. The only mode of preparing these crystals as microscopic objects, is cautiously to neutralize the excess of acid of the mother-liquid by the addition of liquid ammonia, but to take care that it be added in successive small quantities, short of precipitation of the excess of disulphates of quinine and cinchonine; then depositing upon a glass slide with a dropping tube a portion of the fluid charged with these crystals, allowing the crystals to subside, gradually removing the fluid by the capillary attraction of bibulous paper, and immediately drying the crystals by a current of cold air. They may then be mounted in Canada balsam in the usual way; taking care, however, to use no heat in liquefying the balsam; otherwise the crystals would be immediately destroyed.

Boiling alcohol readily dissolves these crystals; a clear orange-yellow solution results; this on cooling deposits crystals in abundance, having the same optical and chemical characters; but they have lost the prismatic form, and now appear as rosettes of minute hexagonal plates, or forms derived from the hexagon by truncation of the angles. Cold alcohol does not dissolve them.

Sulphuric æther and chloroform appear to have no solvent power over them.

Ammonia rapidly decomposes them; this power is greatly increased by heat. A colourless solution results, and an opake Naples-yellow precipitate remains, which is fusible at the boiling temperature of the ammoniacal liquid. A deep brownish-yellow resinous mass results; this is a compound of iodine and the alkaloid.

Liquor potassæ has the same action on these crystals; but the resulting resin is deeper in colour, being now a chocolate-brown.

The alkaline solutions in both instances contain sulphuric and hydriodic acids.

Analysis.—About ten grains of the mixed disulphates of quinine and cinchonine were dissolved in half an ounce of pyroligneous or acetic acid; into the hot solution was dropped a spirituous solution of iodine (without iodide of potassium); as the mixture cooled, these little gems gradually formed; they were carefully separated on a filter, and again dissolved in acetic acid by the aid of heat; a few drops of the tincture of iodine, as before, added, and on cooling they were again deposited; a second recrystallization removed all traces of impurity; they were collected on a filter and dried.

(A.) About three grains of these purified acetic crystals were redissolved in acetic acid, and whilst the solution was hot, acetate of baryta was added; a white precipitate was immediately produced; this was insoluble in concentrated nitric acid, and proved to be sulphate of baryta. [The acetic acid used in this experiment gave no trace of sulphuric acid when tested in the same way.]

(B.) Into the filtered portion from A (the excess of baryta having been removed by a solution of sulphate of ammonia and again filtered) were dropped nitric acid and then granules of starch; an abundant indication of the presence of iodine was instantly made evident.

(C.) Into the fluid filtered from B was dropped solution of ammonia; a flocculent, white, gelatinous precipitate was produced; this separated and fell to the bottom after some delay. Upon agitating the mixture with sulphuric æther, the precipitate was dissolved, and separated by decanting the æthereal solution from the watery fluid; upon slowly evaporating the æther by exposure to the air, a gummy resinous mass remained, nearly destitute of colour and crystalline appearance; it was probably quinine, as it did not crystallize.

Other experiments were instituted to decide the question whether the alkaloid was quinine or cinchonine.

1st. By taking advantage of the different solubility of the two disulphates, they were separated, and at length rendered perfectly pure.

2nd. It was found that by treating solutions of each alkaloid in a precisely similar manner with iodine, crystals possessing the peculiar properties were only produced in the solution of the pure disulphate of quinine.

The disulphate of cinchonine solution was merely slightly reddened upon the addition of iodine; becoming turbid, a cinnamon-brown precipitate falling, which upon heating in contact with the mother-liquid became indigo-coloured, and did not redissolve. No crystals were produced.

Therefore it became probable that iodine, sulphuric acid, and quinine, were absolutely necessary for the production of these crystals.

1st. To decide the question whether the sulphuric acid was absolutely essential, a portion of the crystals was dissolved in acetic acid, and whilst hot, acetate of baryta was dropped in until no further precipitation occurred; the solution was filtered whilst hot, and upon cooling there was no appearance of any crystallization after remaining several days.

2nd. Another portion of these crystals was dissolved by boiling n rectified spirit; a sherry wine-coloured fluid resulted; it was divided into two portions.

(A.) The first was allowed to cool spontaneously; the crystals were deposited again, but in rosettes, as before described.

(B.) The second portion of the alcoholic solution was treated whilst hot with acetate of baryta; the sulphate precipitated directly; the supernatant fluid on cooling remained perfectly transparent, and no crystals formed after some days.

3rd. An alcoholic solution of the pure alkaloid quinine was carefully prepared, and an alcoholic solution of iodine added; a sherry wine-coloured fluid resulted; no crystals were deposited; and upon spontaneous evaporation an ochry-yellow precipitate remained, without crystalline form, and having a very resinous appearance.

Therefore it now became evident that iodine, sulphuric acid, and quinine, were the constituent elements of this peculiar body. How associated, it is difficult to say; but it is probable that they are arranged as a binary compound, the disulphate of quinine acting as a feebly electro-positive base to the iodine as an electro-negative. It is conjectured therefore to be an iodide of the disulphate of quinine.

It now became an interesting question to decide whether any other of the vegetable alkaloids would act in a similar manner with iodine. The same experiments were tried with the salts of morphine, brucine, strychnine, salicine and cinchonine, but without success; we may therefore almost confidently depend on the production of these crystals being indicative of the presence of quinine in a given solution. It is to be regretted that the atomic weight has not yet been determined, but hitherto time has not permitted the necessary experiments to be undertaken.

This substance presents itself under a variety of crystalline forms; a slight change in the manner of producing them will occasion an alteration in their shape. (See fig. 2.)

When formed from a solution of the disulphates of quinine and cinchonine in diluted sulphuric acid, they present the form of parallelopipeds, exceedingly slender and elongated; the terminal planes are rectangular, the thickness being scarcely appreciable, even less than $\frac{1}{1000}$dth part of an inch, the breadth and length being variable.

The transition from this form to the square plate is very easy, and frequently observed.

By truncating the angles of the square plate we derive the octagonal plate; very common.

Under other circumstances, the aciculæ change the form of their terminal planes and become acutely pointed.

By shortening the length and increasing the breadth we obtain the half hexagon.

By joining two of these, base to base, we obtain the hexagonal plate. Very frequently found in crystals deposited from the acetic acid solution.

The rhomboidal plate is a very common form.

When a quantity of the disulphates is dissolved in acetic acid, a very few drops of a spirituous solution of iodine employed (say four or five), and the mixture left some hours in perfect repose to cool and crystallize, very large broad plates are produced, apparently formed of many aciculæ cohering by their elongated edges. These plates, by very careful manipulation indeed, may be transferred to a thin plate of microscopic glass and dried; when set up in Canada balsam and properly mounted, this becomes available as a polarizer; and in this way a crystal has been mounted by the author, and adapted to his microscope in place of a tourmaline. Frequently these crystals assume a form derived from the cuboid plate, several of which joined edge to edge produce a compound plate, the angles being at the same time more or less truncated.

Occasionally the constituent rhombic or square plates cohere by their flat surfaces instead of by their edges. They are all arranged in the same optical plane; and are not merely superimposed by accident in this peculiar position, all the crystals formed in the solution having the same extraordinary shape.

Under other circumstances we obtain this substance in the form of most beautiful compound rosettes, the component crystal being either the minute hexagon or a form derived from it; or the lozenge, like lithic acid. The crystals deposited from alcohol are of this character.

At other times it forms small stellæ, composed of acicular crystals radiating from a centre like the spokes of a wheel.

A more cautious crystallization will produce short pyramids like the ammoniaco-magnesian phosphate from alkaline urine. This is the case when a solid plate of iodine is suspended in a solution of the disulphates in acetic acid. Some days elapse ere they form, in consequence of the very slow solution of the iodine. (See fig. 2.)

The primary form of these crystals appears to be derived from the rhombic prism, but it is very possible that the substance may be dimorphous.

One remarkable fact is evident throughout the whole of this crystalline metamorphosis,—the optical properties remain the same; and the merest film of this remarkable substance possesses decided power over the rays of light.

In the following examination of their optical properties I made use of Oberhauser's achromatic microscope, with half an inch object-glass and No. 2 eye-piece; a low power, certainly under 100 diameters.

A. Their brilliant emerald-green colour reflected to the eye has been already noticed. This beam of green light produced by reflexion is decidedly a polarized ray when the plane of the crystal is inclined 41° to the plane of the incident ray.

B. Their perfectly transparent and almost colourless appearance when examined by transmitted light has also been noticed.

C. The production of complementary colours, when examined by means of a slightly polarized light, has also been spoken of.

D. The action of a single tourmaline upon them is very decided.

E. The action of two tourmalines must also be investigated.

F. The action of one tourmaline and a selenite stage is also very peculiar, and will be minutely examined.

G. The action of two tourmalines and a selenite stage must also be explained.

H. The phænomena exhibited by these crystals, when used as polarizers and analysers, is also worthy of remark, and of course permit of various crystalline substances being used as tests of their remarkable polarizing properties.

I. The phænomena of depolarization by these crystals will be touched upon under sections C. and E. &c.

(B.) The perfect polarizing powers which these crystals exhibit must now be proved and illustrated.

When two crystals of the prismatic form are examined in a superimposed condition, the following effects will be apparent:—

1st. If the two prisms are perfectly parallel in their long diameters, the ray of light will pass through unaltered. (Figs. 3 and 4.)

2nd. When they cross each other at a right angle, the small square spot where they cross will be as black as midnight. The two rays are both obstructed; that is, about half the incident ray of ordinary light is stopped or absorbed by the first or lowest crystal, the other half transmitted by it in a polarized state; this impinging upon the superior crystal is stopped by it effectually.

3rd. When the two crystals intersect each other at an angle of 45°, polarization also occurs, but not to the same extent; the spot where they are superposed is decidedly darkened.

4th. There is a perceptible polarizing effect produced at an angle of 30°; below this there does not appear to be any effect on the transmitted light.

5th. Similar effects are equally well observed in the superposition of the hexagonal plates and other forms. (See figs. 4 and 5.)

(C.) When three crystals are examined in a superimposed condition, two being crossed at right angles, and therefore dark, and a third introduced between them, the phænomena of depolarization are produced: the interposed crystal permits the light to pass through, and at the same time communicates to it the order of colour, equivalent to its thickness, in the same manner as a plate of selenite would do if interposed between two tourmalines at right angles to each other.

The angle of depolarization appears to be 45° to the plane of either polarizing or analysing crystal; but the phænomenon will take place in a minor degree at other angles. (Vide fig. 5.)

Similar phænomena are produced by the hexagonal plates and other crystalline forms. (Vide fig. 5.)

(D.) The action of a single tourmaline or Nicol's prism is very marked, and proves beyond a doubt that these crystals possess both the polarizing and the depolarizing powers.

In the first place, upon examining two of these crystals placed at right angles the one to the other, with a single tourmaline or Nicol's prism, one crystal is perfectly black and obstructs all the light, the other is as transparent as ever. Upon more closely analysing this experiment, it will be found that the crystal whose length crosses the plane of the tourmaline at right angles is the dark one, whilst that one whose long diameter is parallel to the plane of the tourmaline is transparent. (Fig. 6.)

Upon rotating the tourmaline 90°, or one quarter of a circle, the crystal which was before transparent becomes dark, and that which was dark now becomes transparent. (Figs. 6 and 7.)

Their polarizing power may now be considered to be established.

It remains to prove their depolarizing power with equal certainty.

If three crystals, *a*, *b* and *c*, arranged as in fig. 8 be examined with one tourmaline, the latter (*c*) is inferior to the two former, which of course cross it at an angle of 45°, and at 90° to each other respectively. Upon placing a tourmaline over the eye-piece of the microscope at right angles to the plane of *c*, the phænomenon of polarization will be exhibited by *c*; it will appear black.

The crystals *a* and *b* are of course at 45° respectively to both the tourmaline and to *c*; they are therefore at the angle of depolarization as in section B, and will consequently exhibit coloured images where they cross the polarizing crystal *c*, one being complementary in colour to the other; and as they intersect each other at right angles, they there exhibit the appearance due to polarization, and darkness is the result.

Similar phænomena are exhibited by the hexagonal plates, &c.

(E.) The next phænomena to be described will be the result of examining these new polarizing and depolarizing crystals by means of two tourmalines, a polarizing and an analysing plate as they are commonly called; in fact, they would be submitted to the ordinary arrangement of the polarizing microscope.

Select two crystals superimposed and crossing at right angles, and the whole object capable of being revolved horizontally on its own axis.

Let the crystals as a cross coincide with the planes of the tourmalines. (Fig. 9, *a* and *b*.)

The field of the microscope will be dark, as the tourmalines are at right angles, and consequently nearly the whole of the incident light will be obstructed or polarized.

The crystal (*a*) being at right angles to tourmaline (*d*), of course produces an increase to the polarizing effect.

The crystal (*b*) being at right angles to the tourmaline (*c*), also polarizes and increases the depth of darkness.

And at the centre (*e*) we have the combined influence of the two tourmalines and the two crystals also; we consequently have the maximum polarizing effect which it is possible to produce with this combination.

In the second place, we will rotate the object through an arc of 45° whilst the tourmalines remain stationary. The crystals are now in the position most favourable for exhibiting depolarization: they compel the light to pass through, and at the same time communicate colour to the beam, unless their thickness be too great, when of course white light will be transmitted. (Fig. 9, *c*, *f*.) Similar results follow in the examination of hexagonal plates.

(F.) We will now proceed to examine these crystals by means of a single tourmaline and the selenite stage.

This arrangement consists in placing a tourmaline in the centre of the field of the microscope on the stage, and superimposing upon it a plate of selenite, of such a thickness that it will give a brilliant wine colour, or the complementary green, when examined by the second or analysing tourmaline placed over the eye-piece. But in the experiment now to be described the *superior* plate of tourmaline is *not employed.*

In fig. 10 four prismatic crystals of the iodide of disulphate of quinine are supposed to be arranged at various angles of rotation. (*a*) is placed in the position from 0° to 180°, and at right angles to the plane of the tourmaline. This crystal, acting as a tourmaline in the field of the microscope, developes the colour of the selenite stage, and of course appears wine-coloured.

(*b*) is across the field at 90° to the former one; it shows the complementary green.

The crystals (*c*) and (*d*) are across the field at 45° to the plane of the tourmaline; they are therefore in a position to exert but a minimum of polarizing power: an olive-green tint is produced.

The force of the argument may not be apparent at first sight, but upon experimenting with the hexagonal plates we are soon convinced of the fact. Here *a* is at right angles to the tourmaline below the stage, and therefore appears wine-coloured; whilst *b* is parallel to the polarizing plate, and of course is complementary.

In the centre of the field the crystals cross at 90°, and therefore polarize. But any lingering doubt we may yet have of the truth of this position is most certainly removed upon proceeding to the following experiment, in which we simply substitute for the pink selenite stage a plate of the same substance of a different thickness, one which developes the sky-blue tint in polarized light.

We now perceive that the crystal which is at right angles to the tourmaline is a beautiful blue, whilst that crystal which is parallel to it is the complementary yellow. The two intermediate crystals are of a slight neutral tint, as they produce but a minor degree of polarizing power at these angles.

The hexagonal crystals show the same phænomenon, but in a more marked degree.

The phænomena exhibited by this substance in these experiments were so remarkable, and so different from those of any crystals I had previously examined, that I was induced to make a comparative series of experiments upon some other crystalline compounds, as I felt convinced that the single tourmaline and the selenite stage would become a very delicate test of the power which any substance may possess of polarizing a ray of light.

Upon submitting disulphate of cinchonine to this experiment, I found it to possess a decided power of polarizing light.

This substance crystallizes as a tuft of minute radiating aciculæ, sometimes arranged in a perfect circle. (Fig. 11.) Upon placing such a tuft above the *red* selenite stage, having a tourmaline beneath it, one-half the circle appears red, the other half green. But there are four segments to the circle; one quarter red, one green, one red, and the fourth green.

Now all those prisms which are arranged at right angles to the plane of the tourmaline are red; all those parallel to it are the complementary green; but as the power exists in a minor degree on each side of this line through an arc of 45°, of course we get the whole half-circle so coloured, but in two quarter-segments placed apex to apex.

The same phænomenon is also to be found with the blue selenite stage, but it requires a better light of illumination to discover it: the segments are respectively blue and yellow alternately.

Upon examining pure cinchonine in a crystalline state as deposited from its hot alcoholic solution, the evidence of the same power exhibits itself.

The oxalurate of magnesia (discovered in the urine of a patient afflicted with the oxalic acid diathesis, after having administered the bicarbonate of magnesia for some time) possesses the same power to a considerable extent. The colours are shown in these dumb-bell crystals with tolerable splendour.

Taurine possesses this power in a very slight degree only.

Some radiating crystals of carbonate of lime or magnesia found between the tegumentary layers of the shrimp are also capable of polarizing, or rather analysing the ray to a slight degree.

The nitrate of urea, the oxalate of urea, nitrate of potassa, and nitrate of soda (rhombic), possess this power in great splendour.

There is very little doubt that more time spent in the investigation of this phænomenon would considerably enlarge the catalogue of those substances which possess the faculty of polarizing light; but none of those enumerated possess it to the extent of the iodide of the disulphate of quinine.

The disulphate of quinine does not exert the slightest influence upon the ray of light under these circumstances.

(G.) To return from our digression to the point from which we started, our next mode of examining the properties of the new crystals will be by the *two* tourmalines and by the selenite stage.

Fig. 12 shows four crystals arranged at various angles as before.

It will be understood that in this experiment the two tourmalines are arranged at right angles; one being on the stage, the other upon the eye-piece of the microscope; the plate of selenite superimposed on the inferior tourmaline. In fact, it is the same as the last arrangement, but with the addition of the superior tourmaline. We will first employ the pink selenite stage.

The prism (*a*) is at right angles to the inferior tourmaline and parallel to the superior; it developes the red colour of the stage.

The prism (*b*), being parallel with the inferior tourmaline, is at *right angles* to the superior tourmaline; it consequently obstructs the whole of the light.

But the crystals *c* and *d* are at 45° to either tourmaline, and therefore at that angle which is most favourable for showing the phænomena of depolarization. They are coloured green and yellow respectively, as they now add the influence of their own thickness to that of the selenite stage.

The experiment being varied by employing the blue selenite plate, all the other arrangements being as before, of course the field will be blue.

The prism (*a*) becomes blue from the same cause, the superior tourmaline having no influence upon it.

The crystal (*b*) is dark, as before, the superior tourmaline obstructing the beam polarized by it.

The crystals *c* and *d* are now violet and orange respectively, being complementary in colour the one to the other. They act as depolarizing crystals to the light polarized by the inferior tourmaline, and analysed by the superior tourmaline, and add their thickness to the selenite stage; in this position they exert the same influence upon polarized light that any other crystalline substance belonging to the rhombic prismatic series would do under similar circumstances.

Upon revolving the superior tourmaline, the whole appearance changes; the field passes to green with one stage and yellow with the other. The crystals pass through various changes in colour and appearance, each in its turn becoming a polarizer in action with the superior tourmaline.

(H.) It has already been stated that the author has succeeded in adapting one of these artificial tourmalines to the stage of his microscope; it is sufficiently large to give an uniform tint to the whole field, and covers a surface of an eighth of an inch in diameter. This crystal will bear magnifying to any extent, and he has been enabled to illuminate a field of eleven inches in diameter with light polarized by its means.

It is at once evident that such a crystalline plate would be far too small to be serviceable as the analysing plate above the eye-piece; a crystal of at least half an inch in diameter would be necessary for this purpose. There is frequently found in the mother-liquid a crystal sufficiently large to be so used, were it possible to transfer it safely from the fluid to a plate of glass in order to mount it: the difficulty consists in the extreme fragility of these microscopically thin compound plates; the slightest touch is sufficient to disrupt them. The slightest movement in the liquid will at times destroy the connexion existing between the edges of the component prisms; they thus lose their uniform and parallel arrangement. Wherever they cross, polarization and obstruction of the rays of light necessarily occur, and the plate is of course useless for the purpose designed.

But although it is not possible to obtain one large enough to surmount the eye-piece, yet it is perfectly easy to procure plates of sufficient size to act as analysing crystals upon the field or stage of the instrument, of course used superimposed on the tourmaline, or artificial tourmaline attached to the stage; and when these are placed at right angles, the phænomena of polarization may be exhibited with great splendour. (Vide fig. 13.)

When these crystals have been thus arranged, and the selenite stage interposed, the field becomes coloured according to the thickness of the plate of selenite; and the extent of the field so coloured will depend on the magnitude and breadth of the superior artificial tourmaline employed; frequently the whole field of seven or eight inches, or even eleven inches in diameter, has been coloured with an uniform tint.

Fig. 13 exhibits a polyhedral compound crystal of the new substance employed as an analysing plate, the artificial tourmaline being placed beneath it. The radiating crystals are those of disulphate of quinine, which crystallized upon the plate of glass used to mount the analysing crystal, in consequence of the evaporation of the mother-liquid from which the plate was formed, and, depositing the excess of disulphate beneath the plate, thus produced the splendid specimen now attempted to be depicted. The crystals of course depolarize the light, and it is transmitted by the superior or analysing plate; and if the crystals are thin enough, the prismatic colours are shown. But when the selenite stage is placed upon the polarizing plate on the stage of the microscope, and therefore inferior to the analysing plate with the disulphate of quinine beneath it, the appearances exhibited are of the most gorgeous character—it is in vain to attempt to depict them. The analysing plate of course developes the colour of the stage employed; its whole breadth therefore assumes the colour of the stage if at right angles to the plane of the plate below, or the complementary tint if parallel to it. The radiating crystals of disulphate of quinine assume every hue and tint of the spectrum: the experiment must be witnessed to be fully understood and properly appreciated.

It will be recollected that in this experiment it is not at all necessary to employ a tourmaline; the whole phænomenon may be exhibited with equal brilliancy by using the two plates of iodide of the disulphate of quinine; one as a polarizer, the other as an analyser, the selenite and disulphate of quinine being interposed. This will fully establish the fact of this substance possessing optical properties precisely equivalent to those of the tourmaline, or of a Nicol's prism, and will be sufficient to show that all the phænomena capable of being produced by the one may be exhibited by the other.

Upon submitting these artificial polarizing plates to micrometrical admeasurement, it was found that those which possessed sufficient thickness to adhere together in clusters, and to raise themselves on their edges so as to show their thickness, were none of them more than $\frac{1}{300}$dth of an inch; many were about one-half or one-third of this thickness—$\frac{1}{600}$ or $\frac{1}{900}$dth of an inch. But even these were much larger than any of those thin broad plates so readily broken; and some of which, after great trouble, were mounted and experimented with. The tourmalines commonly sold and employed for optical purposes are from $\frac{1}{100}$dth to $\frac{1}{200}$dth of an inch thick; such a one as the latter size was employed in the comparative experiment above related, whence it follows that this newly-discovered substance possesses the power of polarizing a ray of light with at least *five times* the intensity that the best tourmaline is capable of. It must consequently be the most powerful polarizing substance known, and it has been proved to be a new salt of a vegetable alkaloid.

32 Old Market Street, Bristol,
Nov. 30, 1851.

XXVII. *Reports on the Progress of the Physical Sciences.* *By* John Tyndall, *Ph.D.*

On Electric Currents of the First and Higher Orders. By P. Riess, Poggendorff's *Annalen*, vol. lxxxi. p. 428, and vol. lxxxiii. p. 309.

When an electric battery is discharged, the current which passes through the connecting wire is known to be capable of inducing a secondary current in another wire brought near it; and if the secondary current be permitted to operate upon a third wire, a tertiary current will be induced, which in its turn will induce a current of the fourth order in a fourth wire, and so on. The current which passes through the wire directly connected with the battery will in the following be called the *principal* or *primary current.* The object of M. Riess appears to have been to make a strict investigation of these various currents, the circumstances under which they appear, their influence upon each other and upon themselves; and finally, to clear up the doubt which at present exists as to their directions.

Rejecting the method of inferring the strength of a current from its effect in the magnetization of a steel needle, the author makes the heating of a fine platinum wire, introduced into the circuit, and passing through an air-thermometer of his own construction, the measure of the strength. The following short paper, which has been translated for some time, and held back with the view of presenting at once an abstract of the whole of this important investigation to the readers of the Philosophical Magazine, establishes the fact, that not only does the primary current affect a second wire placed near it, but that the various portions of the said current affect each other; the strength of the current being thus proved to depend in some measure upon the shape of the wire through which it passes.

It is known, writes the author, that the current of the electric battery acts inductively upon the mass of the connecting wire. If a second conducting wire be connected with two points of the original circuit, the action of the induced current may be exhibited, partly in a direct manner, and partly, as I have already shown, by the disturbances which the laws of the branch current experience. From this, however, it does not follow that in the simple connecting wire itself the induced current will exhibit any sensible action; for in this case, as it has no circle to move in, its effect must be very small in comparison with that of the original current. The question, whether by an approximation of two portions of the connecting wire an alteration of the current of discharge takes place, must be referred to experimental decision. This experiment was made by me long ago with 26 feet of wire wound into two plane spirals which were brought near each other, the heating of the wire in the remaining portion of the circuit being at the same time observed. There was no difference exhibited from which any inference could be drawn. Similar experiments were made afterwards by Hankel with 317 feet of wire, which was wound into two cylindrical spirals. To test the current, however, Hankel chose, not the heating of the wire, but the magnetization of steel needles which lay near it, and which, by successive charges of the battery, were magnetized; the magnetization was found different according to the manner in which the spirals were united. We have here gained a particular fact of considerable interest, but the general question raised by me remains still unanswered. The question was, when two portions of the current are in a certain manner brought close together, is this act accompanied by an increase of the temperature of the wire, or by a decrease thereof, or does the temperature remain unchanged? The experiments on magnetizing decide none of these questions; and I found myself, therefore, compelled to return to my old method of experiment, applying at the same time a more powerful apparatus. Two flat spirals, each of which consisted of $53\frac{1}{2}$ feet of copper wire wound into 31 coils, were set one after the other in a circuit which contained a sensitive air-thermometer. The combination of the spirals with the battery and with each other was so arranged, that the current could enter both at the same place (centre or rim), or at different places; the directions of the current in the spirals being in the former case alike, and in the latter case opposite. The spirals were first placed at a distance of about 9 inches apart, and stood oblique to each other (the straight line which joined their centres made an oblique angle with their surfaces), and the following temperatures were observed. The balls of the unit-jar were $\frac{1}{2}$ a line apart. The temperature for the unit of charge is the mean value of the constant a in the formula $\phi = a\frac{q^2}{s}$*.

Number of jars s.	Quantity of electricity q.	Principal current in the spirals. In the same direction. Temperature ϕ.	Principal current in the spirals. In opposite directions. Temperature ϕ.
3	6	13·5	13·2
	8	22·2	22·7
	10	34·9	34·0
4	6	10·5	10·4
	8	17·5	17·7
	10	27·0	27·0
5	6	8·5	8·8
	8	14·7	14·8
	10	21·8	21·5
Unit of charge	...	(1·11)	(1·11)

The equality of the temperatures exhibited when the directions

* Let q be the quantity of electricity, and y its mean density; then the time of discharge being directly as the quantity, and inversely as the density, will be $\frac{q}{y}$. But the strength of the current, or what is the same, its heating power, is directly as the square of the quantity, and inversely as the time of discharge; hence it is $= \frac{aq^2}{\frac{q}{y}} = aqy$, where a is a constant. Further, the density y is equal to the quantity, divided by the surface over which it is spread; hence $= \frac{q}{s}$, when $s =$ the surface of the battery. Substituting this value of y in the above expression, we have the strength of the current, or $\phi = a\frac{q^2}{s}$.

of the current through both spirals were the same and when they were opposite, compels the conclusion, that, in the position above described, the wires exercise no influence upon each other, which result was indeed anticipated. The spirals were now set normally opposite, and brought within a line of each other. In the following experiments the combinations of the spirals were essentially the same as in the former; the observations differing merely therein, that for the first vertical column, under the head 'Temperature,' the thermometer stood nearest to the inner coating of the battery, while for the second column the spirals were nearest to the same coating.

Principal current in the same direction through both spirals.			
Number of jars.	Quantity of electricity.	Temperature.	
3	6	11·9	11·8
	8	20·0	20·6
	10	31·5	30·8
4	6	9·2	9·3
	8	15·6	16·1
	10	24·0	24·8
5	6	7·8	7·8
	8	13	13·2
	10	19	19·2
Unit of charge	...	(0·98)	(0·99)

Principal current in opposite directions through the spirals.			
3	6	14·1	14·7
	8	23·3	24·4
	10	36·5	37·0
4	6	11·2	10·6
	8	18·9	18·6
	10	28	27·7
5	6	9·2	9·4
	8	15·8	14·7
	10	23·7	23·0
Unit of charge	...	(1·18)	(1·17)

A comparison of these results with those first obtained shows that an alteration of the current takes place when one portion of the connecting wire is brought near to another and parallel to it. In order to express the result in a brief manner, let the connecting wire be conceived to be of three different shapes; stretched out straight, bent into the form of a U, and bent into the form of an N (the two parallel sides of the latter being brought very near each other). The discharge of a battery through the N form gives the feeblest result, that through the U form the strongest, the current through the straight wire being intermediate between both. In general the modification of the current is so inconsiderable, that in common experiments with the battery it may be entirely neglected. To place the fact beyond doubt, we see that it is necessary to place 107 feet of a current 119 feet in length within a line of each other, by winding them into two spirals, the most distant parts of each of these being not more than a foot apart. In the wire screws, used so frequently in the circuit for the sake of sparing room, the distance between two windings is much less than the diameter of the screw; hence a wire wound into this form must, as in the case of the N wire, principally weaken the current.

In the series of experiments given above, the spirals were united by a copper wire 29 inches long and $\frac{3}{8}$ths of a line in thickness. The measured currents bore nearly the following proportions to each other:—

Without the action of the spirals.	Current in same direction. N combination of the spirals.	Current in opposite directions. U combination of the spirals.
100	89	106

The connecting wire was exchanged for one of $39\frac{1}{2}$ inches in length and $\frac{1}{6}$th of a line thick. Out of twenty-seven observations, the following numbers were found for the respective currents:—

100	91	106

Finally, a steel wire, $34\frac{1}{3}$ inches in length and $\frac{7}{24}$ths of a line in thickness, was introduced, and with this I obtained the following numbers:—

100	88	109

The phænomenon is therefore independent of the retarding value of the wire which unites the spirals; for here we have very different values of retardation, but currents of almost the same proportions. In all these cases the increase of the current is less than the diminution, which therefore must not be regarded as an accidental circumstance.

The result established may be thus expressed:—

Two portions of the connecting wire of a battery, which run closely parallel, act upon each other. The current will be weakened by this action when its two parallel portions move in the same direction, and strengthened when they move in opposite directions.

The secondary current.—To excite a secondary current, two wires must be placed near each other; the most convenient way of effecting this being to wind them into spirals either as discs or cylinders (we shall call them in future induction-discs, or induction-cylinders). From former experiments the author was led to conclude, that the heating in the thermometer was proportional to the number of coils; but this is only approximately correct, as the following experiments prove. It will add much to the reader's comfort if a clear conception of the arrangement of the spirals and thermometer be obtained. In the case now to be described there were two circuits, a primary and a secondary; in the primary circuit were the battery and two induction-discs placed one after the other, and in the secondary circuit two others of the same size, and the air-thermometer; the two primary spirals were connected by a wire which proceeded from the centre of one to the rim of the other, and the two secondaries were united in the same manner; the other ends of the secondary spirals were connected with the thermometer. The experiments were made in the following manner:—First, one secondary was placed parallel to its primary and two lines distant from it; the battery was discharged, and the power of the secondary current was observed on the thermometer; secondly, the other secondary spiral was brought within two lines of its primary, the two former being widely separated, and the heating was again observed; thirdly, both secondaries were brought within two lines of both primaries, and the strength of the current induced in the whole secondary circuit was ascertained: the following are the results of these experiments:—

Number of jars.	Quantity of electricity.	Heating with		
		1st spiral.	2nd spiral.	Both spirals.
3	8	10·8	10·1	18·2
	10	16	15	27·3
	12	21·1	21·5	38·3
4	8	7·7	7·7	13·8
	10	11·4	11·8	21·0
	12	16·2	16·5	30·5
Unit of charge	...	0·47	0·46	0·84

These experiments prove that the strength of the secondary current increases in a somewhat smaller ratio than the length of the wire, a result which might be predicted when the reaction of the secondary upon the primary current is taken into account. Thus, if we imagine the secondary circuit divided into two portions, the sum of the actions of both portions, taken separately, is greater than the action of both together.

The reaction of the secondary current upon the primary was demonstrated by former experiments in the following manner:—The primary circuit contained two induction-discs, as in the case just described. Opposite to each of these was placed a secondary disc, which could be closed so as to form a continuous circuit in itself; the secondary discs were first left open, and the strength of the primary current was measured; the secondary discs were next closed by short copper wires, and the primary current was again measured; the strength of the latter was found to be quite unchanged by this closing of the spirals. If, however, instead of being closed by a short copper wire, the ends of one of the secondary spirals were united by increasing lengths of fine platinum wire, the principal current was observed to *decrease* to a certain limit, from which forward it *increased.* A secondary current induced by a primary thus circumstanced may be expected to partake of the fluctuations of the latter, which conclusion has been established experimentally by the author in his present investigation. The primary circuit contained two induction-discs, and the secondary two others; the ends of one of the secondaries were united to the thermometer by short copper wire; the two ends of the other secondary were united, first by a short copper wire, and afterwards by increasing lengths of platinum wire of 0·028 of a line radius. The following table gives the result of the experiments:—

Closing of the second spiral.					
Copper 16 inches.	Platinum 1·98 feet.	5·95	17·9	37·6	97·2
Secondary current in the first spiral.					
100	75	53	35	37	51

Here we observe that the effect of the platinum wire in weakening the principal current was a maximum when its length was 17·9 feet; and that the effect of a wire 5·95 feet in length was very nearly equal to that of a wire 97·2 feet in length.

The *mediate* action of one secondary current upon another is established by the foregoing experiments; for the secondary current in a spiral is shown to be modified by the action of another distant secondary spiral upon the principal current. But the direct action of one secondary upon another can also be shown by permitting both to be induced by the same portion of the primary circuit. The experiment is frequently so arranged, that, besides the spiral whose secondary current is observed, another spiral is introduced, either between the former and the primary circuit, or at the opposite side of the latter. Instead of spirals, metallic bodies have been sometimes introduced; and it has been found that the better conductor the body is, the greater will be the weakening of the secondary current; to this action Henry has applied the term "screening." The precise action which takes place will perhaps be best understood from the discussion of the next table, which contains the results of a number of experiments conducted in the following manner:—Round a wooden cylinder, 13 inches high and $6\frac{1}{2}$ inches diameter, three copper wires were wound spirally. Each wire was $\frac{7}{12}$ths of a line in thickness, 53 feet long, and made 31 revolutions, possessing a 'pitch' of $4\frac{1}{3}$ lines. The first spiral was introduced into the primary circuit, the second was connected with the thermometer, and the third was closed by wires of successively increasing retarding values. The results obtained are as follows:—

Closing the 3rd spiral by	Length.	Retarding value.	Value of the secondary current in the 2nd spiral.
			100.
Copper 0‴·31 rad.	23 inches.	3·1	61
do.	44 ...	5·9	63
do.	67 ...	9·0	69
	53 feet.	85·6	93
Platinum 0‴·04098 rad.	59·2 lines.	254	66
do. 0 02857 ...	0·49 feet.	609	59
do.	1·98 ...	2435	56
do.	5·95 ...	7298	59
do.	7·94 ...	9737	62
do.	9·92 ...	12162	66
do.	11·9 ...	14587	69
do.	13·9 ...	17093	70
do.	19·8 ...	24269	77
do.	29·7 ...	36399	81
do.	43·6 ...	53429	87
do.	63·5 ...	77809	90
do.	79·3 ...	97169	92
do.	103·2 ...	126459	97

When the third spiral was open, the value of the secondary current induced in the second was 100; the uniting of the ends of the third spiral by a copper wire 23 inches in length brought the secondary current down to 61. Now it is proved by experiment, that the action of a secondary spiral upon the primary current when its ends are connected by a wire of the above length (23 inches), is just the same as if the spiral were left open; hence the diminution of the secondary current is due, not to any modification which the primary has undergone, but to the direct action of the other secondary. The first four of these experiments show, moreover, that the stronger the current in the third spiral, the greater is the amount of weakening in the second*. By increasing the resistance in the third spiral, the primary current at length becomes modified, and a decrease of the current in the second spiral up to a certain point is the consequence; from this point forward the current again increases, until with a retarding value of 126459 it attains almost the same strength which it possessed when the third spiral was altogether inactive. The result of this *immediate* and *mediate* action of the one secondary upon the other, is the occurrence of two maxima and two minima in the series of observations.

* Hence the formation of a secondary current is checked by permitting the primary wire to excite a second secondary at the same time; the reader will do well to remember this, as the circumstance is turned to account further on.

The dependence of the *principal* current on the shape of the wire through which it passes, or in other words, the action of the current upon itself, has been already demonstrated. This action is exhibited in a more striking degree by the *secondary* current. A small induction-disc was introduced into the primary circuit, and a corresponding one into the secondary circuit; the remaining portion of the secondary circuit consisted of 44·7 feet of copper wire $\frac{7}{24}$ths of a line in thickness, and the wire of the thermometer. The 44·7 feet of copper wire were first so stretched out, that when a secondary current passed through it the action of one portion of it upon another was null, and the strength of the secondary current under these circumstances was measured. The same wire was then wound into a spiral shape, the *form* alone of the circuit being thus altered, its length remaining as before, and the strength of the secondary current in this case was also measured. The following are the results obtained:—

In the secondary circuit 44 feet copper wire.					
Stretched out.			In form of a plane spiral.		
Number of jars.	Quantity of electricity.	Heating.	Number of jars.	Quantity of electricity.	Heating.
3	10	5·9			
	12	7·4	3	20	2·2
	14	9·1	......	25	3·8
Unit of charge	...	0·16	......	......	0·017

Thus we see that by a mere alteration of the *form* of the circuit, without any diminution whatever of its *length*, the secondary current is weakened in the proportion of 100 : 11. In this case the direction of the secondary current was *the same* in both the spirals through which it passed, the shape of the circuit being therefore that which has already been illustrated by the letter N; we observe that the effect of this shape is exactly similar to that which occurs in the primary circuit under the same circumstances.

The large proportion of the entire circuit which, in the above experiment, received the spiral form, accounts for the magnitude of the diminution; in the following experiments another arrangement was adopted. From a copper wire $\frac{5}{8}$ths of a line in thickness, three portions, each 53 feet long, were cut; the first was stretched out so that no action could occur between its parts; the second was wound into a plane spiral, similar to the induction-discs so often alluded to; and the third was carried back and forward, in a zigzag manner, from side to side of an oblong frame about a foot in width; twenty-five Us were thus formed, the legs of which were 1·2 line apart. In order to limit the action to that of the one leg of the same U upon the other, and prevent its extension from U to U, each alternate U was bent upward, so as to form an angle of 50 or 60 degrees with the horizon. A large induction-disc was placed in the primary circuit, and a corresponding one in the secondary circuit, which, together with this, contained the thermometer and one of the lengths of copper wire just described. The current through the stretched-out wire was first proved, then through the spiral, and finally through the system of Us. The following are the results obtained:—

Number of jars.	Quantity of electricity.	In the secondary circuit 53 feet copper wire.		
		Stretched out.	As plane spiral.	As U-arrangement.
		Heating.		
3	6	9·6	......	11·5
	8	16·2	10·8	18·3
	10	24·5	16	27
	12	......	21·2	
4	6	7·5	......	8·2
	8	12·8	7·7	15·2
	10	18·8	11·4	21·2
	12	......	16·2	
Unit of charge	...	0·78	0·47	0·89
Proportion ...	...	100	60	114

The strength of the secondary current, when no action takes place between its various parts, is thus shown to be intermediate between those of the N-form and U-form. In passing through the former, the current is weakened in the ratio of 100 : 60, while in passing through the latter it is strengthened in the ratio of 100 : 114. The law of the primary current is therefore applicable to the secondary:—*Two portions of a secondary current which run closely parallel, act upon each other: when the directions of the current through both portions are identical, a weakening is the consequence; and when the directions in both portions are opposed to each other, a strengthening of the current is the result.*

What is the cause of this? During his investigation of the primary current, M. Riess conjectured that the action of the primary upon itself was due to the formation of a secondary current in the primary wire. Consistent with this view, we should infer that the action of the secondary upon itself is due to the formation of a tertiary current in the mass of the secondary wire, an inference which the author has established experimentally in the following manner:—It has already been shown that a secondary current is greatly weakened if the portion of the primary wire which excites it excite at the same time a second secondary in a well-closed circuit, a case to which the reader's attention has been directed in a note; hence the possibility of lessening the supposed tertiary current by the intentional formation of a second tertiary. This was effected as follows:—The primary circuit contained a large induction-disc; the secondary circuit a similar one, the wire of the thermometer and 53 feet of copper wire, which was first stretched out, and afterwards exchanged for a plane or a cylindrical spiral formed from the same length of wire. Parallel to this plane or cylindrical spiral, and at about a line distant from it, ran another spiral, which we shall call the tertiary spiral; the secondary current was first measured while the tertiary spiral remained open, and afterwards when it was closed by 23 feet of copper wire. The following are the results:—

In the secondary circuit 53 feet of copper wire.				
Stretched out.	As cylindrical spiral.		As plane spiral.	
		Tertiary spiral closed.		Tertiary spiral closed.
100	74	98	65	95

The secondary current, which, with a stretched-out wire, had the value of 100, was weakened to 74 when the same length of wire was wound to the shape of a cylindrical spiral, and to 65 when its shape was that of a plane spiral. But on permitting of the formation of a tertiary current by closing the ends of the tertiary spiral, the current rose to 98 and 95 in these respective cases. Thus the production of a second tertiary checked, as conjectured, the formation of the one to which the weakening of the secondary was due, a striking increase of the latter being the consequence. It was proved by other experiments that the strengthening of the secondary current by the U-form of its wire is also due to the formation of a tertiary current in the mass of the latter.

Following up the system of procedure indicated in the foregoing pages, M. Riess has subjected currents of the third, fourth, and fifth orders to experimental examination. The laws of action in each respective case are precisely the same as those which apply to the secondary current, and which we have just described. The tertiary reacts upon the secondary in a manner similar to the reaction of the secondary upon the primary; in fact, the relation of any given current to that of the next higher order is in all respects that of primary to secondary; the tertiary current is modified by changing the shape of its wire, being weakened by the N-form and strengthened by the U-form thereof; and the same is true of currents of all other orders.

With regard to the *directions* of these currents much uncertainty exists; Henry, Matteucci, Verdet and Knochenhauer have given utterance to various and contradictory opinions on this subject. With admirable ingenuity M. Riess has brought the foregoing experiments to bear upon this point. Let us suppose an induction-disc to be placed in the primary circuit, and parallel to it another with its ends united, thus forming an isolated circuit in itself. The passage of a current through the former will arouse an induced current possessing a certain direction, either opposed to the primary or coincident with it, in the latter. Without altering the relative position of the discs, let the second one be conceived to be brought into the primary circuit; the current, in passing through the first, will, as in the former case, induce a current in the second; but now primary and secondary are in the same wire, and it evidently depends upon the manner in which the two discs are connected with each other whether both currents meet in opposition* or flow on in the same direction. If by the N-combination secondary and primary flow on together, then by the U-combination they will oppose each other, and *vice versâ*. Now the constant weakening effect of the N-form, and strengthening effect of the U-form in currents of all orders, demonstrate a constant relation between the directions of the induced and inducing currents. If the relation of any one secondary to its primary†, with respect to direction, be determined, the same relation holds good in all other cases; if the currents have the same direction in one case, they will have it in all cases; if opposed once, they are opposed throughout the entire series. Commencing at the current which passes direct from the battery, it is easy to see that whatever be the direction of the secondary which it arouses, the tertiary evoked by the secondary must necessarily have the same direction as the current passing from the battery; for if the secondary be opposed to the primary, the tertiary will be opposed to the secondary, and hence have the same direction as the primary; and if the secondary have the same direction as the primary, the whole series will have this direction. Thus we arrive at the following necessary conclusion:—*Currents of the third, fifth, and other odd orders, have the same direction as the original*

* We must guard ourselves here against the notion that the opposition of primary and secondary is in any degree similar to the mechanical opposition of two forces, or even to the opposition of two currents *of the same order*.

† The terms secondary and primary are used here in a relative, not in an absolute sense.

current; and those of the second, fourth, and other even orders, have among themselves one and the same direction.

With regard to the direction of the currents of even orders as compared with that of the primary current, the author arrives at the probable conclusion, that they also have the same direction as the primary; but as this portion of the subject remains hypothetical, we will content ourselves with the mere indication of the author's opinion.

Queenwood College,
November 1851.

XXVIII. *On some Thermo-electrical Experiments. By* Richard Adie, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen, Liverpool, Feb. 10, 1852.

I SHOULD be glad to be allowed to avail myself of the medium of your Journal to give a brief notice of some thermo-electrical experiments which I made in the years 1842 and 1843*, and which I believe may be of service in elucidating some of the results noted in your February Number in Dr. Tyndall's interesting review of Professor Magnus's researches on this subject.

Where thermo-electrical couples are formed of feebly thermo-electric agents, such as bars of the same metal in different states of density, particularly of the softer malleable metals, the electrical force developed is so weak, that slight changes in the mode of manipulation, or small differences in the elements employed, which are either unknown to the operator or are generated during his experiment, will produce contradictory results. On which account it appears to me to be better to leave that class of agents and first study the production of thermo-electricity by metals, where the force is so decided, that, in the hands of different experimenters, uniform actions can be obtained.

Steel is a substance possessed of the advantage of being readily changed in density in opposite directions by two modes of hardening; and as these different methods are accompanied by a thermo-electrical current governed in its direction by the kind of hardening employed, the experiments with steel appear to me to show that the molecular arrangement of the particles of a body exercises a constant influence over the thermo-electrical currents generated by the unequal heating of it. When a couple is made by joining to a bar of soft steel a similar bar hardened by hammering, on heating the junction of the steel in the two different states an electrical current passes from the soft to the hard.

* See Edinburgh Philosophical Journal, Nos. 70 and 71.

Again, with a bar of soft steel, and a similar bar made hard and light by the well-known process of heating and immersing in cold water, the thermo-electric action in this arrangement is from the hard and light part to the soft and more dense portion of the metal; in this respect differing from the first couple, where the current passed to the denser side of the two pieces of metal. A couple formed of hard and soft steel elements affords another good proof of the influence of the molecular state of a bar over the thermo-electrical currents generated by the unequal heating of it, the hardening of the steel being effected by heating and quenching in water. The energy of the action of such a mono-thermo-electric couple is very decided while the hardened steel is undergoing the process of softening; but after this has been effected by the heat applied, the arrangement loses all its energy, thus clearly showing a connexion between the molecular state of the bar and its power to generate thermo-electrical currents. By casting bars of antimony in hot and cold moulds, thermo-electric couples can be made of this metal which exactly resemble steel in their action; but they are not so uniform in their indications, the hardening and softening of antimony being much less easily controlled.

The metals bismuth and antimony occupy in thermo-electric batteries the same relative position that zinc and platina do in hydro-electric arrangements,—the bismuth corresponding to the zinc, the antimony to the platina. When bismuth and antimony are long employed in generating thermo-electricity, the antimony, like the platina of the hydro-electric battery, is found unaltered, while a minute change is always observed on the bismuth; this, then, points to bismuth as the active agent in developing the thermo-electrical current, in the same manner as zinc is in the hydro-electric couple. Bismuth is a metal possessed of some remarkable properties, and it is to these that we should naturally turn to examine the origin of the thermo-electric current. A bar of bismuth and antimony soldered with bismuth only for a solder, when put by the aid of a gas flame into as energetic action as the fusing temperature of bismuth will admit of, develope electricity, which if used to precipitate copper or silver after the well-known manner of the electrotype, will after the lapse of a few months give a precipitation equivalent to the weight of the antimony and bismuth employed; for this action a reaction must be found somewhere; all that the couple shows, is a minute allotropic change of the bismuth where it has been in contact with the antimony. When I first made this experiment, I was inclined to believe that the small action in the joint was the equivalent of the chemical effect produced at the electrodes; but subsequent reflection inclines me to doubt the accuracy of this view; for however indispensable these minute molecular changes may be for the production of thermo-electrical currents, they do not appear sufficient to account for the chemical effects obtained; but if it could be shown that the contact of dissimilar metals, or of dissimilar particles of the same metal, while they undergo a slow change by the action of heat, can so react on the heat as to derive electricity from it, then the apparently wide difference between the chemical effect derived from a thermo-electrical couple and the minute action on the joint of it may be more easily comprehended.

Bismuth, in addition to its being by far the best metal for supplying the place of a positive thermo-electric bar, has been shown in the valuable magnetic researches of Dr. Faraday, to be the most energetic diamagnetic substance at present known. It is likewise remarkable in its alloys. In the gold coinage a minute addition of bismuth greatly impairs the durability of the coin; and with lead or tin, mixtures are made which melt at lower temperatures than the melting heat of the metals combined together. These properties are due to conditions of the forces of aggregation of which science is at present able to give no account, but which appear to point to an ample field for inquiry for the future, in which the properties of thermo-electricity, when further unmasked, may render much assistance.

I remain, Gentlemen,

Yours very respectfully,

RICHARD ADIE.

XXIX. *On the Integration of Linear Differential Equations. By the* Rev. BRICE BRONWIN*.

THE present paper is an addition to one published in the Number of this Journal for December last, and is intended to illustrate the use of the arbitrary function $\lambda(\mathrm{D})$.

In the Cambridge Mathematical Journal, first series, vol. ii. page 193, is given a solution of

$$x\mathrm{D}^m u - pm\mathrm{D}^{m-1}u + kxu = 0,\ p \text{ integer.}$$

By the conversion of symbols this may be solved as an equation of the first order, but it serves to show the use which may be made of the symbolical function $\lambda(\mathrm{D})$. Make

$$\varpi = \mathrm{D}x + \lambda(\mathrm{D}),$$

then

$$\varpi u = \mathrm{D}xu + \lambda(\mathrm{D})u, \text{ and } xu = \mathrm{D}^{-1}\varpi u - \mathrm{D}^{-1}\lambda(\mathrm{D})u.$$

In the last, change u into $\mathrm{D}^m u$, and we have

$$x\mathrm{D}^m u = \mathrm{D}^{-1}\varpi\mathrm{D}^m u - \mathrm{D}^{-1}\lambda(\mathrm{D})\mathrm{D}^m u.$$

* Communicated by the Author.

Substituting these values in the proposed equation, and operating on the result with D, we find

$$\varpi \mathrm{D}^m u - \lambda(\mathrm{D})\mathrm{D}^m u - pm\mathrm{D}^m u + k\varpi u - k\lambda(\mathrm{D})u = \mathrm{D}0.$$

Assume

$$\lambda(\mathrm{D})\mathrm{D}^m + pm\mathrm{D}^m + k\lambda(\mathrm{D}) = 0,$$

which gives

$$\lambda(\mathrm{D}) = -pm(\mathrm{D}^m + k)^{-1}\mathrm{D}^m,$$

and our equation is reduced to

$$\varpi \mathrm{D}^m u + k\varpi u = \mathrm{D}0, \text{ or } u = (\mathrm{D}^m + k)^{-1}\varpi^{-1}\mathrm{D}0.$$

Now

$$\varpi = \mathrm{D}x - \frac{pm\mathrm{D}^m}{\mathrm{D}^m + k} = \mathrm{D}\left(x - \frac{pm\mathrm{D}^{m-1}}{\mathrm{D}^m + k}\right);$$

therefore by the known formula

$$f\{x + \Phi'(\mathrm{D})\}u = \epsilon^{\Phi(\mathrm{D})} f(x) \epsilon^{-\Phi(\mathrm{D})} u,$$

we easily find

$$\varpi^{-1}\mathrm{D}0 = \left(x - \frac{pm\mathrm{D}^{m-1}}{\mathrm{D}^m + k}\right)\mathrm{D}^{-1}\mathrm{D}0 = \left(x - \frac{pm\mathrm{D}^{m-1}}{\mathrm{D}^m + k}\right)^{-1} 0$$

$$= (\mathrm{D}^m + k)^{-p} x^{-1} (\mathrm{D}^m + k)^p 0,$$

and therefore

$$u = (\mathrm{D}^m + k)^{-p-1} x^{-1} (\mathrm{D}^m + k)^p 0.$$

If p be positive,

$$x^{-1}(\mathrm{D}^m + k)^p 0 = 0, \text{ and } u = (\mathrm{D}^m + k)^{-(p+1)} 0.$$

If p be changed into $-p$, we have

$$x\mathrm{D}^m u + pm\mathrm{D}^{m-1}u + kxu = 0, \quad u = (\mathrm{D}^m + k)^{p-1} x^{-1} (\mathrm{D}^m + k)^{-p} 0.$$

We must make

$$\mathrm{D}^m + k = (\mathrm{D} + k_1)(\mathrm{D} + k_2) \ldots . (\mathrm{D} + k_m),$$

but I shall not stop to reduce these solutions. It may be well to remark, that we cannot immediately operate with the form

$$\varpi = x\mathrm{D} + \lambda(\mathrm{D});$$

but

$$\varpi = \mathrm{D}x + \lambda(\mathrm{D}) = x\mathrm{D} + 1 + \lambda(\mathrm{D}).$$

Therefore by suitably changing the form of $\lambda(\mathrm{D})$ we can perform the operations.

As another example I shall take

$$x^2\mathrm{D}^m u - p(p-1)\mathrm{D}^{m-2}u + kx^2 u = 0,$$

where p or $p-1$ is divisible by m. This is taken from page 197 of the volume before referred to. Both this and the preceding example will serve to illustrate the manner in which an equation may frequently be put under the required form more easily than by the rules given in the former paper.

Making $\varpi = \mathrm{D}x + \lambda(\mathrm{D})$, we have

$$x^2u = \mathrm{D}^{-2}\varpi^2 u + \mathrm{D}^{-2}\big(1-\lambda(\mathrm{D})\big)\varpi u + \mathrm{D}^{-2}\big(\lambda(\mathrm{D})^2 - \lambda(\mathrm{D}) + \mathrm{D}\lambda'(\mathrm{D})\big)u$$

and changing u into $\mathrm{D}^m u$,

$$x^2\mathrm{D}^m u = \mathrm{D}^{-2}\varpi^2\mathrm{D}^m u + \mathrm{D}^{-2}\big(1-\lambda(\mathrm{D})\big)\varpi\mathrm{D}^m u + \mathrm{D}^{-2}$$
$$\big(\lambda(\mathrm{D})^2 - \lambda(\mathrm{D}) + \mathrm{D}\lambda'(\mathrm{D})\big)\mathrm{D}^m u.$$

Substituting these values, and operating with D^2 on every term of the result, we find

$$\varpi^2(\mathrm{D}^m + k)u + \big(1-\lambda(\mathrm{D})\big)\varpi(\mathrm{D}^m + k)u + \big(\lambda(\mathrm{D})^2 - \lambda(\mathrm{D}) + \mathrm{D}\lambda'(\mathrm{D})\big)$$
$$(\mathrm{D}^m + k)u - p(p-1)\mathrm{D}^m u = \mathrm{D}^2 0.$$

Here make $\lambda(\mathrm{D}) = 0$, and the last is reduced to

$$\varpi^2(\mathrm{D}^m + k)u + \varpi(\mathrm{D}^m + k)u - p(p-1)\mathrm{D}^m u = \mathrm{D}^2 0,$$

or

$$\big(\varpi^2 + \varpi - p(p-1)\big)\mathrm{D}^m u + k(\varpi^2 + \varpi)u = \mathrm{D}^2 0,$$

or

$$(\varpi - p + 1)(\varpi + p)\mathrm{D}^m u + k\varpi(\varpi + 1)u = \mathrm{D}^2 0.$$

To solve this, assume

$$u = \frac{(\varpi - p + 1)(\varpi - p + m + 1)\ \ldots\ \big(\varpi - p + (r-1)m + 1\big)}{\varpi(\varpi + m)\ \ldots\ \big(\varpi + (r-1)m\big)}u_1.$$

Then by (C) of the former paper we have

$$\mathrm{D}^m u = \frac{(\varpi - p + m + 1)\ \ldots\ (\varpi - p + rm + 1)}{(\varpi + m)\ \ldots\ (\varpi + rm)}\mathrm{D}^m u_1.$$

If $rm = p$, substituting these values, and operating with the inverse of all the factors common to the first member of the resulting equation, we have

$$\mathrm{D}^m u_1 + ku_1 = \mathrm{X}_1 = \frac{(\varpi + m)\ \ldots\ (\varpi + p - m)}{(\varpi - p + 1)\ \ldots\ (\varpi + 1)}\mathrm{D}^2 0.$$

Therefore

$$u_1 = (\mathrm{D}^m + k)^{-1}\mathrm{X}_1,$$

whence u may be found.

In this and the former example m is supposed to be an integer.

Again, make

$$u = \frac{(\varpi - m)(\varpi - 2m)\ \ldots\ (\varpi - rm)}{(\varpi + p - m)(\varpi + p - 2m)\ \ldots\ (\varpi + p - rm)}u_1.$$

Then

$$\mathrm{D}^m u = \frac{\varpi\ \ \ldots\ \ (\varpi - rm + m)}{(\varpi + p)\ \ldots\ (\varpi + p - rm + m)}\mathrm{D}^m u_1.$$

If $rm=p-1$, we shall find by substitution and reduction,

$$\mathrm{D}^m u_1+ku_1=\mathrm{X}_1=\frac{(\varpi+p-m)\ \ldots\ (\varpi+m+1)}{\varpi\ \ \ldots\ \ (\varpi-p+1)}\mathrm{D}^2 0,$$

and

$$u_1=(\mathrm{D}^m+k)^{-1}\mathrm{X}_1.$$

These are the two cases of the proposed equation considered by its author. To take a case not noticed by him, change $p(p-1)$ into n^2; then the equation becomes

$$x^2\mathrm{D}^m u-n^2\mathrm{D}^{m-2}u+kx^2u=0.$$

In this case make $\lambda(\mathrm{D})=1$, and the transformed becomes

$$\varpi^2(\mathrm{D}^m+k)u-n^2\mathrm{D}^m u=\mathrm{D}^2 0,$$

or

$$(\varpi-n)(\varpi+n)\mathrm{D}^m u+k\varpi^2 u=\mathrm{D}^2 0.$$

Here assume

$$u=\frac{(\varpi-n)(\varpi-n+m)\ \ldots\ \big(\varpi-n+(r-1)m\big)}{\varpi(\varpi+m)\ \ \ldots\ \ \big(\varpi+(r-1)m\big)}u_1,$$

and we shall have by (C)

$$\mathrm{D}^m u=\frac{(\varpi-n+m)\ \ldots\ (\varpi-n+rm)}{(\varpi+m)\ \ \ldots\ \ (\varpi+rm)}\mathrm{D}^m u_1.$$

Make $rm=n$, and we find by substitution and reduction,

$$\mathrm{D}^m u_1+ku_1=\mathrm{X}_1=\frac{(\varpi+m)\ \ldots\ (\varpi+n-m)}{(\varpi-n)\ \ \ldots\ \ \varpi}\mathrm{D}^2 0,$$

and

$$u_1=(\mathrm{D}^m+k)^{-1}\mathrm{X}_1.$$

Perhaps by assigning other forms to $\lambda(\mathrm{D})$ we might obtain the solutions of other cases of this equation.

An example of the use of the arbitrary function $\lambda(x)$ shall now be given.

Let

$$x^2\mathrm{D}^2u+2a\mathrm{D}u+\left(b+\frac{3a^2}{x^2}\right)u=0.$$

Here, according to the former paper,

$\pi=x\mathrm{D}+\lambda(x),\ \ \pi u=x\mathrm{D}u+\lambda(x)u,$

$\mathrm{D}u=x^{-1}\pi u-x^{-1}\lambda(x)u,$

$\mathrm{D}^2u=x^{-2}\pi^2u-x^{-2}\big(1+2\lambda(x)\big)\pi u-x^{-2}\big(\lambda(x)^2+\lambda(x)+x\lambda'(x)\big)u.$

If we substitute these values in the proposed equation, we shall find that by making $\lambda(x)=\dfrac{a}{x}$ the result will be reduced to the very simple form

$$\pi^2u-\pi u+bu=0,$$

or

$$(\pi-m_1)(\pi-m_2)u=0, \text{ and } u=(\pi-m_1)^{-1}(\pi-m_2)^{-1}0;$$

$$\pi=x\mathrm{D}+\frac{a}{x}=x\left(\mathrm{D}+\frac{a}{x^2}\right).$$

It will often happen that the conversion of symbols will very greatly facilitate the solution of an equation. Thus, if

$$x^2\mathrm{D}^2u+kx^2\mathrm{D}u-2u=\mathrm{X},$$

change D into x and x into $-\mathrm{D}$, and we have

$$\mathrm{D}^2x^2u+k\mathrm{D}^2xu-2u=\mathrm{X};$$

or by reduction,

$$(kx+x^2)\mathrm{D}^2u+(2k+4x)\mathrm{D}u=\mathrm{X}.$$

This may easily be put under the form

$$\mathrm{D}\{(kx+x^2)^2\mathrm{D}u\}=(kx+x^2)\mathrm{X},$$

whence

$$u=\mathrm{D}^{-1}(kx+x^2)^{-2}\mathrm{D}^{-1}(kx+x^2)\mathrm{X}.$$

Now changing the symbols back again, we have

$$u=x^{-1}(\mathrm{D}^2+k\mathrm{D})^{-2}x^{-1}(\mathrm{D}^2+k\mathrm{D})\mathrm{X}$$

for the solution required, the correctness of which is very easily verified. If the last term of the given equation had been $-n(n+1)u$ instead of $-2u$, it would have been integrable after the conversion of symbols and $n-1$ differentiations.

Again, let

$$x^2\mathrm{D}^2u+(k^2x^2-2)u=\mathrm{X}.$$

By the same conversion of symbols as in the last example we have

$$\mathrm{D}^2x^2u+(k^2\mathrm{D}^2-2)u=\mathrm{X},$$

or

$$(k^2+x^2)\mathrm{D}^2u+4x\mathrm{D}u=\mathrm{X},$$

and

$$\mathrm{D}\{(k^2+x^2)^2\mathrm{D}u\}=(k^2+x^2)\mathrm{X};$$

whence

$$u=\mathrm{D}^{-1}(k^2+x^2)^{-2}\mathrm{D}^{-1}(k^2+x^2)\mathrm{X}.$$

Change the symbols back again, and we have

$$u=x^{-1}(\mathrm{D}^2+k^2)^{-2}x^{-1}(\mathrm{D}^2+k^2)\mathrm{X},$$

which may be easily shown to satisfy the proposed.

Sometimes the commutation of symbols may be employed with effect in the middle of a solution, or in some of the steps of it; but to give examples would too much enlarge this paper. If the last example had been

$$x^2\mathrm{D}^2u+(kx^2-m)u=\mathrm{X},$$

after commuting the symbols and differentiating the result i times, we should find

$$(k^2+x^2)D^{i+2}u+(2i+4)xD^{i+1}u-\big(m-(i+1)(i+2)\big)u=D^iX.$$

Making $(i+1)(i+2)=m$, this becomes

$$(k^2+x^2)D^{i+2}u+(2i+4)xD^{i+1}u=D^iX,$$

which is integrable when multiplied by $(k^2+x^2)^{i+1}$.

Each of the values of i gives a solution, and the two together the complete one. It may be remarked, that we might integrate i times instead of differentiating, integrating each term of the equation by parts. The solutions will not be practicable when i is not integer, but they may often be transformed by the introduction of a definite integral.

January 28, 1852.

XXX. *On the Development and Extinction of regular doubly refracting Structures in the Crystalline Lenses of Animals after Death. By* Sir DAVID BREWSTER, *K.H., D.C.L., F.R.S., and V.P.R.S. Edin.**

[With a Plate.]

SINCE the year 1816, when I communicated to the Royal Society an account of the doubly refracting structures which exist in the crystalline lenses of fishes and other animals, I have examined a great variety of recent lenses, with the view of ascertaining the origin of these structures, the order of their succession in different lenses, and the purpose which they answered in the animal œconomy. Although I had found that in the lenses of the cod, the salmon, the haddock, the frog-fish, the skate, and several other fishes there were three structures, the innermost of which had negative double refraction, the next *positive*, and the outermost negative double refraction, yet in the lenses of animals the greatest discrepancies presented themselves. In every case, however, excepting one, I have found the central structure in all quadrupeds† to be positive, while it is always negative in fishes when there are three structures; but this positive structure sometimes existed alone, with faint traces of a negative structure; sometimes it was followed by another *positive* structure, separated from the first by a black neutral circle, in which the double refraction disappeared. Sometimes these two positive structures were succeeded by an external negative structure. Sometimes the central and external positive structures were separated by a negative structure, and at other times

* From Phil. Trans. 1837, p. 253–258.

† Excepting the hare. See Phil. Trans. 1836, p. 37.

the lens exhibited *four* structures, a negative and a positive one alternating. As these discrepancies appeared in the lenses of animals of the same species, I conceived that they were owing to differences of age or sex, or to some change in the health of the animal. I was therefore led to make new observations in reference to these probabilities, and to observe the phænomena with additional attention when the structure differed from that which was most common. In these observations I sometimes noticed in the dark or neutral line, which separated two positive structures, something like a trace of an intervening structure, which was either about to disappear, or about to be developed. This conjecture was confirmed by observations on the lenses of a cow eleven years old.

The lenses, after being carefully taken out, were freed from the adhering portions of the vitreous humour by the gentle application of blotting-paper, so as not to disturb their internal structure. The lenses were elliptical. Their longest diameter was 0·774 inch, their shortest diameter 0·747 inch, and their thickness 0·513 of an inch. The first lens which I exposed to polarized light was in the highest perfection, and the symmetry of the optical figure unusually beautiful. I have represented it in Plate V. fig. 1, in which only two structures, or two series of positive sectors, are visible*. The lens was now a day old, and there seemed to be a faint light within the two black rings, especially in the outer one, which was either the remains of an old, or the germ of a new structure. If this were the case, then the anomalous combination of two positive structures would be converted into a combination of four structures, in which a negative and a positive one alternated.

On the following day I prepared the other lens with the same care, and found my conjecture completely verified. In the middle black ring, which was distinctly brownish in the first lens, the negative structure had evidently commenced at one part, and the colour of the whole ring was a brighter brown than in the first lens. In the outer black ring another negative structure had also appeared, and had advanced considerably upon the positive structure. These phænomena I have represented in fig. 2, where the four structures are distinctly seen, the second

* Upon referring to my earlier observations, I find that in both the lenses of an ox there was only one structure which was a positive one, and which had not yet divided itself into two structures, as in that of the cow under consideration. There was the appearance of a black space near the margin of the lens, but the polarized light both within and without that black ring was positive.

In the lens of another ox, and of a bull, I found the *positive* structure separated into two positive structures by a distinct black ring, while an external negative structure was clearly developed.

being a faint blue of the first order. On the third day the two new structures had become more prominent. The structure No. 2, now a pale white of the first order, was completely developed, having encroached upon and almost obliterated the third structure. The structure No. 4, which was not in existence on the first day, had now the maximum tint, namely a bright white of the first order. On the fourth day the structures No. 2 and 4, which at first were not in existence, are now the structures with the maximum tints, and No. 3, which had the maximum tint, is now almost obliterated, a little faint brown light remaining in one of the quadrants.

On the fifth day the four sectors of the inner structure No. 1 have almost disappeared. No. 3 has disappeared entirely, and No. 2, which is almost the only polarizing structure, exhibits a more intense white of the first order than appeared in any part of the lens. The ring No. 2 divides the radius of the lens equally.

On the sixth day the structure No. 2 was still bright and uniform, but the polarized light had disappeared from every other part of the lens.

On the seventh day the lens, which was always placed in water, burst its capsule, and there was no longer any trace of distinct polarizing structures.

My next observations were made on the lenses of a cow nearly twenty years old. The following were the dimensions of the eye and the lenses:—

	inch.
Diameter of eyeball	1·66
Chord of the cornea (largest) . .	1·30
Chord of the cornea (shortest) .	1·02
Longest diameter of lens . . .	0·827
Shortest diameter of lens . . .	0·793
Thickness of lens	0·50

Both the lenses of the cow exhibited when taken out of the eye four beautiful structures, in which the positive and negative structures alternate. The first and fourth were very faint, being the palest white of the first order. The third was also faint, but the second was both bright and large, and its tint was a *brilliant yellow* of the first order. After lying four days in water the lenses swelled so much that their dimensions were as follows:—

	inch.
Diameter of one lens	0·807
Diameter of the other	0·793
Thickness of the first	0·647
Thickness of the second . . .	0·620

The lenses were still transparent, and the tint of the structure No. 2 had risen to an *orange-red* of the first order.

Having experienced great difficulty in the course of the preceding experiments in preserving the capsule of the lens transparent for several days, I made trial of various fluids, but found distilled water more suited to my purpose than any other. I therefore began a regular course of observations on the crystalline lens of the sheep when placed in distilled water, which have afforded me very satisfactory results.

The lens of a sheep a year and a quarter old, when newly taken out of the eye, exhibited in the distinctest manner only one structure, with slight traces of an external one. This structure was *positive,* and occupied almost the whole of the lens, as shown in fig. 3. The traces of an external structure, when carefully examined, showed it to be negative. On the following day this lens burst in the direction of the three septa.

In the lenses of another sheep I found two structures like the preceding, but with this difference, that the external negative structure was more developed, as in fig. 4. On the following day this negative structure had extended itself inwards, but in consequence of an accident the lenses burst their capsule.

In the lenses of another Cheviot sheep, where the external negative structure had just begun to appear, the wide positive structure shown in fig. 3 had just begun to separate itself by a dark neutral line, which was seen only in one of its four sectors, and which divided that sector into two.

In another Cheviot sheep the principal positive structure had distinctly divided itself into two positive structures, separated by a dark neutral ring, as shown in fig. 5. The same appearance was shown in the other lens; and I have found it a very common structure in the lenses of sheep at that age when they are killed for the table.

When this division of the principal structure takes place the central one is at first faint, and the other a bright white of the first order, as in fig. 4. It becomes, however, brighter and brighter till it nearly rivals the other in the intensity of its polarized tint, as in fig. 5, when another change begins to show itself.

This change, similar to that which I have described in the lens of the cow, arises from the absorption of distilled water by the capsule of the lens. It first shows itself by the appearance of a brown tint in the dark neutral ring which separated the two positive structures. In the middle of the brownish-black ring a trace of faint bluish light appears, generally in one of the sectors only, but gradually extends itself into a blue ring, which has negative double refraction, and which is separated by distinctly formed black rings from the two positive structures, between which it lies. This state of the polarizing structure is shown in fig. 6, which is nearly the same as in the lens of the cow.

The structure No. 1, beginning at the centre, was pretty bright, but No. 3 was much more so, and No. 4 very faint, though perfectly distinct.

On the second day the blue ring No. 2 was much enlarged, and had encroached greatly on the brightest structure No. 3, having reduced it both in breadth and intensity. No. 4 has also extended itself at the expense of No. 3.

On the third day the new structure No. 2 had become the brightest of all. No. 4 had increased also, whilst No. 1 had become smaller and fainter, and No. 3 was wholly obliterated.

In another pair of lenses one of them burst at this stage of the development of the polarizing structures, while in the other the effect was singularly fine. No. 3 was wholly, and No. 1 nearly obliterated; while the two new structures, which had no existence at first, were the only ones that remained. The new negative structure No. 2 consisted of four beautiful blue sectors of polarized light; but in consequence of the great absorption of distilled water, and the consequent distension of the lens, it soon burst.

I have already remarked that only one case has occurred in the course of my experiments in which the central structure of the lenses of quadrupeds was negative, as in fishes. In this case, however, the centre of the lens had its structure affected by some change in the condition of the fibres at their union in the three septa, which were not only distinctly seen, but had the polarizing structure clearly related to them. The polarized light filled up each of the three angles of 120° which lay between the three septa, and the intensity of the light was a maximum close to the three septa. Hence it is evident that the central *negative* structure was the result of an induration of the lens related to the septa, and had obliterated the *positive* structure which would otherwise have existed there.

In examining the lenses of the *horse,* I have observed the progressive development of its three structures as the animal advanced in age, and the extinction of all of them but one when the age of the animal was great.

In both the lenses of a young horse three years old I found only one positive structure.

In both the lenses of a horse whose age was unknown, I observed three structures beautifully developed. The central ones, which were extremely distinct and more beautiful in form and more intensely luminous than in any other quadruped which I had examined, were *positive,* the next structure *negative,* and the external one *positive.*

In the lens of another horse, whose age was also unknown to me, the remains of three structures were visible; but the two positive ones, namely, the central and external structures, had just disappeared, but were not encroached upon by the intermediate negative one. They were therefore black when seen by polarized light, as shown in fig. 7, while the remaining *negative* one was of the most brilliant yellow colour.

In the lenses of a third horse, probably of an intermediate age, I found a structure intermediate between that of the two preceding ones. The following were the dimensions of its lenses:—

	First lens.	Second lens.
	inch.	inch.
Longest diameter . . .	0·827	0·820
Shortest diameter . . .	0·793	0·793
Thickness . . .	Not measured	0·500

The first lens having been carefully prepared and immersed in distilled water, exhibited the beautiful optical figure which is but imperfectly represented in fig. 8. The central sectors were *positive,* but faintly illuminated. The wide and brilliant yellow and white structure was *negative,* and the external structure, which had just begun to appear, was *positive.*

On the second day the black mass round the central sectors had enlarged itself, and become very black, having the form of a square lozenge. The *yellow* ring has risen in its tint to a *brilliant pink* yellow at the edges, the white ring within it having increased in width, and the white ring without it having diminished.

On the third day the diameter of the lens had increased to 0·86 in all directions, and its thickness from 0·50 to 0·717 of an inch. The coloured ring has not changed greatly.

On the fourth day the bright pink of the negative structure has risen to a bright blue, the pink and yellow being seen at its margin; and the external positive structure seems to be now conjoined with the blue negative structure, in consequence, no doubt, of the extension of the latter to the margin of the lens. The thickness of the lens was now upwards of 0·86, and the capsule came off, in consequence of which two of the blue sectors have become of a pale pink colour. The instant the capsule came off, the lens shrunk in all its dimensions nearly the *tenth* of an inch.

The *second* lens on the *third* day gave exactly the optical figure shown in fig. 8, having been newly placed in distilled water; but the external ring seems to be slightly *negative,* like the yellow one. Its appearance was grayish and indistinct.

On the *fourth* day the yellow ring had risen to a *pale pink* of the first order, and the outer ring was *negative*, as on the preceding day.

On the *fifth* day the *pink ring* had increased in intensity, and the other structures remained the same as before.

On the *sixth* day the *pink* had risen to a very *bright blue*. The diameter of the lens was now 0·867 of an inch, and its thickness 0·733, being an increase of 0·233 of an inch in thickness.

On the *seventh* day the capsule burst, and upon removing it and the soft pulp which formed about *one-tenth* of an inch of the outer margin of the lens, the pink ring, with the white band both within and without it, and the black mass at the centre of the rectangular cross, were as distinct as ever. Hence it is manifest that the rise of the tint from *yellow* was not the effect of any expansive pressure produced by the swelling of the lens and the reaction of the capsule.

The descent of the tint from *bright blue* to pink was no doubt owing to the polarizing action of the extended capsule being withdrawn. In order to prove this, I took the capsule, which is a tough and elastic membrane, and having stretched it, I found that it polarized, just before it tore, a white of the first order. Now the value of this tint is nearly equal to the difference between the values of the *pink* and *blue* of the second order of colours.

The preceding results throw much light on the physiology of the crystalline lens; and I shall have occasion, in a separate paper, to point out the conclusions to which they lead respecting the cause and cure of cataract.

Allerly by Melrose,
May 6, 1837.

XXXI. *On the supposed Identity of the Agent concerned in the Phænomena of ordinary Electricity, Voltaic Electricity, Electro-magnetism, Magneto-electricity, and Thermo-electricity.* By M. Donovan, *Esq., M.R.I.A.*

[Continued from p. 127.]

Section II.

Fully aware that a conviction of the identity of the agent in all the phænomena called electric is firmly established in the minds of the scientific, and that experiments of apparently so convincing a nature have been brought to bear upon the subject that doubts seem to be no longer entertained, I scarcely know how to declare, in terms that shall protect me from the imputation of presumption, that I have *never been able to view the matter in the same light*. It is my duty to assign reasons for thus venturing to dissent from universally accredited opinions: I shall therefore enter into a critical examination of the chief arguments which have been made use of to establish the alleged identity. In doing so, I shall take occasion to refer to the explanations of these phænomena suggested by the views developed in the preceding pages of this essay, relative to the presumed compound nature of the electric fluid which with this object I stated in the commencement.

Professor Faraday informs us, that at the period when he undertook the investigation of the agent concerned in these phænomena, the question of identity was as yet undecided. He says, "Notwithstanding, therefore, the general impression of the identity of electricities, it is evident that the proofs have not been sufficiently clear and distinct to obtain assent of all those who were competent to consider the subject:" hence his investigations rendered it necessary for him to "ascertain the identity or difference of common and voltaic electricity."—(266.)

Being assured by so high an authority that, up to the period indicated, the question was doubtful, and having long felt a strong impression that the identity, far from being established, was rendered more problematical by every newly-discovered fact when viewed without prepossession or prejudice, I was anxious to put myself in a condition to appreciate Professor Faraday's reasonings. To these I have therefore addressed myself, aware that if they do not establish their object, the question which so long occupied the attention of inquirers would return to that original state of doubt in which this eminent philosopher found it, when, as he declares, he deemed a resumption of the investigation necessary.

The vast difference of properties observable in electric and voltaic phænomena has been conceived to be explicable on the supposition, that in the former the quantity of electricity is small and the intensity great; while in the latter, the quantity is great and the intensity low. Professor Faraday thus expresses this universally accredited proposition: "Hence arises still further confirmation, if any were required, of the identity of common and voltaic electricity, and that the differences of intensity and quantity are quite sufficient to account for what were supposed to be their distinctive qualities*." Again, he says, "The great distinction of the electricities (common and voltaic) is the very high tension to which the small quantity, obtained by the aid of the machine, may be raised; and the enormous quantity in which that of comparatively low tension, supplied by the voltaic battery, may be procured†." In another place he says, "The general conclusion which must, I think, be drawn from this collection of

* Researches, par. 378. See also 360. † Ibid. 451.

facts is, that electricity, whatever may be its source, *is identical in its nature*. The phænomena in the five kinds or species quoted differ, not in their character, but only in degree; and in that respect vary in proportion to the variable circumstances of quantity and intensity*."

In this expression of opinion I believe most, if not all, philosophers of the present day agree, whether they view the electric fluid as homogeneous or not. The same view is given by Davy, Becquerel, Roget, and other eminent authorities, all following in the footsteps of Cavendish; I shall therefore devote my chief attention to the consideration of it.

It is, in the first place, necessary to understand precisely what is meant by these terms quantity and intensity; they have been always used in explaining the difference between frictional and voltaic electricity; but, as it appears to me, without giving any satisfactory account of how these conditions of the electric fluid act. Dr. Faraday thus expresses himself:—"The term *quantity* in electricity is perhaps sufficiently definite as to sense; the term *intensity* is more difficult to define strictly. I am using the terms in their ordinary and accepted meaning." To understand his views, it is therefore necessary to inquire what the "ordinary and accepted meaning" of the word intensity is.

Professor Hare gives a definition which I believe is a concise enunciation of the opinions of most philosophers on this subject. He asks, "What does intensity mean as applied to a fluid? Is it not expressed by the *ratio of quantity to space*? If there be twice as much electricity within one cubic inch as within another, is there not twice the intensity†?" Elsewhere he says, he is unable to form any other idea of intensity than "that of the ratio of quantity to space." Sir H. Davy gives his opinion to the same effect‡. Dr. Roget, in his excellent essay on galvanism in the *Encyclopædia Metropolitana*, p. 208, says, "that when all the circumstances relative to the conductors and the surrounding bodies are the same, the intensity will increase in a certain ratio with the absolute quantity of electricity that has been given to the conductor by the machine." Mr. Snow Harris's opinion may be resolved into the same meaning. Pouillet says that intensity is "precisely in the inverse ratio to the length of the circuit§." The same opinion is conveyed by Gmelin in the following sentence:—"The difference between the effects of the voltaic pile and those of the electric machine consists of the two following points: 1, by means of the latter a large quantity of

* Researches, par. 360.
† Philosophical Magazine, 1821, p. 292.
‡ Elements of Chemical Philosophy, p. 137.
§ *Elem. de Physique*, i. 633 *et seq.*

electricity may easily be accumulated in a body of small dimensions, in consequence of which the electricity acquires a high tension or a strong tendency to combine with electricity of the opposite kind, and makes its way through non-conductors, such as the air, in the form of a spark," &c.* In Liebig and Gregory's edition of Turner's Chemistry it is said, "Of any number of electrified substances, that will have the highest intensity which has the most free electric fluid on unity of surface;" and (page 96) the same is said of galvanic electricity†. In fine, we may shorten the definition of intensity, as Dr. Bostock and Mr. Goodman have done, by the synonym "concentration."

From these definitions it is plain that quantity and intensity are easily convertible into each other. But it is to be inquired what is the meaning of "a great quantity at a low intensity." Can we conceive a great quantity of electricity unless we give it an adequate place of residence? Can it have dimensions not limited by a boundary—by a containing body? We know nothing of quantity beyond that portion which we call into action, and to which we give a locality. When the electricity generated by a voltaic series is passed through a slender wire, the wire is its momentary place of abode, the quantity contained in it at any point of time, and at that moment active, is all that we can take cognizance of, because no more is at that time generated; and be the passage ever so rapid, the effect is at this instant the same as it was the instant before, or as it will be the instant after. Where, then, is the great quantity reposited? and if there be no place for it, how can it act or exist? There can be no accumulation of effect unless there be an accumulation of the agent which is the cause of that effect; but that would constitute intensity.

It is plain, then, that "a great quantity at a low intensity" must have reference to a great extent of surface on which the electric fluid subsists. Reduce the surface to one-half or quarter, the whole electricity being retained, and you double or quadruple the intensity. Hence this vague expression aims at conveying to the mind a condition which is incomprehensible, unless reference be at the same time made to an equivalent extent of surface occupied. But in the dimensions of a small wire conveying electricity away as fast as it is generated, how is it possible to conceive a great quantity unless at a very high intensity?

We know but little of the nature of the force which causes electrical phænomena. It has never been, and perhaps never will be determined, whether electricity is matter; or whether it is analogous to light, heat or magnetism; or whether it consists of vibrations of some æthereal fluid, or of matter in vibration;

* Handbook, i. 418. † Eighth edition, page 83.

in short, we are totally unacquainted with its form of existence. We can only judge of what, for want of a better phrase, we call its quantity, by the degree of its effects upon matter of determinate dimensions. The degree of effect is therefore the exponent of the quantity. But this degree is another name for intensity of effect: hence the quantity of electricity is only recognised by its intensity; the ideas are inseparable; and intensity is the only significant condition of which the external senses take cognizance. In fine, intensity is the measure of quantity; and of this a beautiful experimental proof is given by Pouillet, which it is well worth while here to describe. He proposes the following question, and gives its resolution:—

"When the intensity of a current increases, does the quantity of electricity which is in circulation, and which constitutes that current, increase in the same ratio? To resolve this question, it must be admitted as evident that the quantity of electricity which passes in a circuit of constant intensity is proportional to the time, that is to say, that in 2″ there passes twice as much as in 1″, &c. It suffices, then, to examine if, in reducing the time during which a current passes to one-half, we equally reduce to one-half its action on the needle; for if it be so, we may truly affirm that the quantity of electricity is proportional to the electro-magnetic effect of the current, or to its intensity."

He then describes some beautifully contrived experiments calculated to ascertain this point. A brass toothed wheel, having the spaces between the teeth filled up with wood, was constructed; the brass teeth and those of wood were in this instance of equal size, although other wheels were also prepared in which the brass and wooden teeth bore various ratios to each other. The edge of the wheel was united like the periphery of a disc, and thus alternately presented conducting and non-conducting surfaces. This wheel with its metallic axis was contrived to revolve with any required degree of rapidity; one pole of a voltaic pile communicated with the axis, and the other with a wire, which, after passing over the needle of a compass, terminated in a tongue, and this pressed against the edge of the wheel. By giving the wheel a very slow motion, the needle oscillated backward or forward as the tongue came in contact with the wood or the brass, the current being interrupted or conducted accordingly. The interruptions were obliterated and a permanent deflection produced when a greater velocity was given to the wheel; and this deflection was not increased by a much more rapid revolution. In an experiment made with the wheel, in which the peripheral surfaces of the brass and wooden teeth were equal, the deflection of the needle while the wheel was at rest was 60°; a slow motion made it oscillate; five turns per second caused a deflection of

25° 45′; and the needle remained stationary at this degree when the wheel was made to revolve at any greater velocity. Now the sine of 25° 45′ is half that of 60°. When the ratio of surface of the brass and wooden teeth was different from that above mentioned, the results corresponded. M. Pouillet says in conclusion, "hence it finally results that the quantity of electricity which constitutes the current is proportionate to the intensity of this current: also the intensity may be taken for the measure of the quantities of electricity*."

If it be admitted that intensity is the measure of quantity, and that one must be in the direct ratio of the other, it would appear that a "great quantity at a low intensity" cannot exist in a current, always including in the word "current" the dimensions of the wire in which the electricity flows. The expression seems to imply a contradiction in terms.

Suppose that the coil of a galvanometer is the surface over which a current is to pass. Each succeeding portion of electricity can only act by its own quantity, irrespectively of all that is afterwards to follow. It is obvious that the total amount, if it equalled a discharge from the clouds and passed in a long-continued current, has no more influence in deflecting the needle than any small portion of it which may occupy the coil at any particular moment. The *efficient* quantity can only be the electricity which is then in the coil, without reference to what is to follow, and which, in common cases of its production by metals, may be said not yet to exist, as the zinc intended to evolve it is only in progress of solution. How, then, does the agency of quantity assist in explaining the much more powerful influence of voltaic over frictional electricity in producing deflections? Quantity can only aid in *continuing* an effect; but how was that effect originally produced?

If it be proved that the quantity at any one time present in the coil is small, it is also proved that a succession of such small portions, however rapid, can neither increase the effect of those which passed away, nor anticipate the effect of those which are yet to arrive. But it may be said that velocity of successive transmissions may produce accumulation of effect. I reply, that the whole catalogue of electro-magnetic phænomena are evidences of the instant cessation of all effect when the cause ceases; hence there can be no accumulation of effects. We find the galvanometer needle steady during the passage of a current, provided the chemical action on the voltaic combination continues equable.

Should it be conceived that the conducting power of the wire constituting the coil is adequate to the transmission of voltaic electricity with such rapidity that enormous quantities can pass

* Pouillet, *Elem. de Physique*, vol. i. p. 633 *et seq.*

off without increasing the intensity, it would be proper to consider the following facts. It is a part of the voltaic hypothesis that frictional electricity is the electric fluid existing in small quantity at a high intensity. If a very thin wire, such as is used for forming the coil of a galvanometer, be made the medium of communication between the positive and negative conductors of a large and powerful electric machine, it will be found on turning the cylinder that a pair of pith balls, hung by wet threads from the conductor, will diverge with energy. This proves, that small as the quantity of electricity present is supposed to be, the thin wire is incapable of conducting the whole of it, although much does pass, and much is dissipated, as it would be from any sharp-edged or pointed conductor. If, then, the so-called small quantity of electricity is not freely conducted by so thin a wire, how is the much (alleged) greater quantity of electricity conducted which a voltaic combination is affirmed to generate? It does not appear from these considerations that facility of conduction can obviate the necessity of admitting in the argument (what is contrary to fact), that a high intensity should exist in the coil, and that enormous quantities can pass through it without creating high intensity.

The absence of all dynamic effects from a voltaic combination of a single pair of large plates militates strongly with the notion of the vast quantity of electricity said to be present in the current which circulates between them. But it may be said, that although in the case of a single pair of plates no discoverable effects are manifested by the electrometer, yet when there is an extensive series of plates in operation the effects are evident. The truth is, that the most extensive voltaic combinations scarcely exhibit greater attractions and repulsions than would be produced by rubbing a stick of sealing-wax. If the power of one or two thousand pairs of plates be so trivial in this respect, what must be the effect of one pair! how inadequate to account for the ignitions, fusions, combustions, and dazzling illuminations which we obtain from a single large pair! Can such phænomena be caused by quantity, which being avowedly without accumulation, must flow in consecutive small portions, not one of which is separately effective? yet the effect of each succeeding portion is the same as that of its predecessor; and there can be no increase of effect, as there is no increase of cause. If a portion of a current could pass off and leave its effects still in operation, the next portion might do the same, and an accumulation of effects might be imagined to subsist without an accumulation of electricity. But no property of the electric fluid can remain after itself has disappeared. A thick wire might remain red-hot for a moment after voltaic current had been withdrawn; but this would be a property of the wire, not of the current; the ignition remains momentarily because it was once excited; but the original ignition is the phænomenon to be explained. So also with attraction and repulsion.

If a temperature of 100° could be transmitted through a bar of iron in an equable flow, the bar would no more rise beyond 100° than the temperature of a metallic tube would exceed 212°, because boiling water had been long passed through it. So electricity passing in a feeble, although abundant current, any one portion of which is incapable of even affecting an electrometer, cannot by its total quantity passing in successive portions, however rapidly, effect anything unless by accumulation; and then intensity must proportionally increase. But high intensity is excluded by the hypothesis, and denied to exist in the case of one pair of plates; yet one large pair will exercise powers of ignition which no other means in nature are capable of.

If the effective current from a single pair of large plates be electricity, we might naturally expect it, whether in large or small quantity, to evince its usual attributes of attraction and repulsion. The circumstance of its being in large quantity, or in rapid flow, should not deprive it of properties which are characteristic, as we have reason to believe inseparable, and the absence of which has been always admitted as a proof of the absence of the fluid itself. Why, then, is not that absence of properties in certain voltaic phænomena admitted equally as a proof that electricity is not in operation? We should expect that great quantity in rapid flow would rather promote than destroy these peculiar effects.

I think all the preceding considerations tend to show that the agency of a large quantity of common electricity, at a low intensity, fails to explain voltaic phænomena; or to account for the absence of the properties of common electricity, while these alleged large quantities of it are in the act of producing voltaic phænomena; or to assign a reason for the presence, in a high degree, of properties developed during voltaic phænomena, which belong to common electricity only in a small degree. In fine, I can find no evidence to prove that the alleged large quantity of common electricity is at all present, since the most feeble demonstrations of it only are discoverable. But more of this hereafter.

Quantity, considered as a cause, seems therefore to be of no avail; intensity appears to be the only intelligible source of activity. I do not think there is one known phænomenon the explanation of which receives any real assistance from the assumed agency of quantity. M. Biot, probably observing this defect in its supposed operation, has substituted the influence of *velocity*.

There may be some advantage in the change which I do not perceive; but if my view of the inefficiency of quantity be correct, it does not appear what may be the use of its quick passage.

To all the foregoing reasonings it might be replied, that it is in vain to search too minutely into the *modus operandi* of a natural cause. The argument would be a good one if it were proved that quantity *is* the natural cause of the phænomenon; but this is the point at issue. Taking leave of the question whether quantity *can* produce the effects attributed to it, let us turn to the more profitable one of whether it *does* produce them.

Those who sought to establish identity had long been embarrassed by the failure of all efforts to produce deviation of the galvanometer needle by means of common electricity, although it is so easily effected through the agency of voltaic. M. Colladon of Geneva, imagining that this want of success was occasioned by an insufficient supply of the electric fluid, or by imperfect insulation of the coil of the galvanometer, employed a charged Leyden battery of 4000 square inches of coated surface, and a galvanometer coil of 100 turns covered with double silk, and oiled silk interposed between the layers. He armed each extremity of his coil with a sharp point, and applied one of them to the external coating of the battery. The other point being approached, by means of a glass handle, to the ball connected with the inside coating of the jars, the needle deviated 23°; the experiment was often repeated. What I conceive to be an important result of these trials is, that the *deviation of the needle increased with the intensity of the charge and the proximity of the point to the ball of the battery.* Sometimes the deviation amounted to 40°, but the average was 20° to 30°. The direction of the needle was determined by that of the current, according to a well-known law.

Laying aside the battery, he employed a Nairne's electrical machine, furnished with a positive and negative conductor, one extremity of the galvanometer being attached to each. When the cylinder was made to revolve, he obtained a deflection of 3° or 4° only; but recollecting that the charge of a Leyden phial passes, with little reduction of power, through a wire many thousand metres in length, he made a galvanometer with a coil of 500 turns, doubly covered with silk, the layers of the coil being separated from each other by varnished silk. He knew that, in the case of common electricity, the greater the number of turns the greater would be the deflection, which however is not the case with voltaic electricity. On connecting one end of this coil with the rubber of a plate electric machine of 5 feet diameter, and approaching the other end to the positive conductor, a deflection ensued, which varied with the distance of the wire from the conductor; when very close, the deflection was 20°, and that was the maximum. When a Nairne's cylinder machine was substituted, the deflection amounted to 35°; but to effect this, it was necessary to give the cylinder three revolutions in a second*.

On these experiments, and some others of the same kind that will hereafter be noticed, an argument in favour of the identity of the agent in all electrical phænomena has been founded, and apparently a very strong one. There are some points of view, however, under which the evidence afforded by them is very much weakened; the following will show in what manner.

I insulated a galvanometer of exceeding sensibility, and connected its binding screws, by means of stout copper rods, with the two conductors of a cylinder machine, which was capable of giving twelve-inch sparks, and which, while in action, caused a divergence in a gold-leaf electrometer (not connected with the machine, and placed four yards distance) to such a degree that the leaves often struck the pallets. On causing the cylinder to revolve, sometimes very slowly, sometimes very rapidly, the galvanometer remained undisturbed; not the least symptom of deflection could be obtained.

The astatic needle of this galvanometer weighed but 4 grs.

The cause of the want of correspondence between this result and that of M. Colladon is obvious. He employed a peculiar galvanometer, so insulated as to prevent lateral communication of electricity from layer to layer; for unless his coil were, by better insulation, in a condition to confine and sustain a charge or current of common electricity at a higher intensity than such coils ordinarily are, there would be no deflection. The high intensity of electricity, caused by the interposition of a Leyden battery, greatly promoted the effect; and when the conductors alone of a Nairne's machine were used, he found it necessary to give intensity by a most rapid rate of revolution to the cylinder, no less than three turns per second.

In my experiment, the galvanometer coil, being only covered with single silk, and having no oiled silk interposed between the layers, was incapable of sustaining a high intensity, and therefore it failed to produce deflection even in the slightest degree. But from this failure (an intentional one) important consequences result, as will be seen from the following comparative experiments.

A voltaic pair of elements was prepared, consisting of a copper wire $\frac{1}{310}$th of an inch in diameter and a thicker platinum wire. These wires, being properly connected with a galvanometer, were plunged to the depth of one-twelfth of an inch into concentrated

* *Annales de Chimie et de Physique*, xxxiii. 62.

nitric acid. The needle whirled round the whole circle three times. The immersed portion of copper wire weighed $\frac{1}{1152}$nd of a grain, and no sensible portion of it was dissolved during the momentary action.

I coated a piece of the same wire with sealing-wax; and having cut off a piece so that a fair section of the wire was presented, the wire was connected with the galvanometer, as also a small platinum wire. The two wires were now immersed in a few drops of nitric acid; the needle started off 60°. This took place in an instant of time, before $\frac{1}{10000}$dth of a grain of metal could have been dissolved. The surface of copper exposed in this case was a circular disc $\frac{1}{310}$th of an inch in diameter.

A still more remarkable case is the following:—A voltaic combination was made of two platinum wires, to the end of one of which was affixed a bit of gold-leaf weighing by calculation about $\frac{1}{6000}$dth of a grain. This combination, when brought in contact with a single drop of nitromuriatic acid, caused the needle to start off 160°; not more than half of the gold appeared to be acted on by the acid.

Here then the most feeble voltaic electricity that could be produced was found highly active in causing deflection, although, of common electricity, neither the highest nor the lowest intensity had the slightest effect. The defence that the supporters of identity would offer against the evidence of these comparative experiments would be, that the quantity of voltaic electricity, although feeble in intensity, is immeasurably greater, in the case of the minute particles of metals above described, than any current which the frictional electricity can supply. Such an explanation may, in the present instance, be pronounced unsatisfactory. On the contrary, the quantity of common electricity in my experiment must have been far greater, because the galvanometer coil was charged with its highest *endurable* intensity; and so much so, according to Colladon's explanation, as to cause a lateral overflow, although the excess must have left the wire charged as fully as its single insulation could sustain. So high an intensity could not have subsisted in the coil unless the quantity of electricity were very much greater than the coil could transmit, for intensity is the ratio of quantity to space. Yet it is remarkable that the same coil readily transmitted, and endured without lateral escape, all the voltaic electricity which the minute particles of zinc and platinum had evolved.

Some persons draw a distinction between statical and current electricity, which they deem all-sufficient in explaining differences of effects such as the foregoing. Now a current has never been proved to *run* in voltaic phænomena in any other manner than it does when a stream of electricity passes from the positive to the negative conductor of a common electrical machine. The agent in both cases runs, or rather flies, with a rapidity which surpasses all comprehension, and, as far as we know, with equal rapidity in both. In my experiment with common electricity, the current was as abundant and rapid as it could be imagined to be in the case of the voltaic combination of the wires with gold-leaf; for the coil of the galvanometer was maintained permanently as full in all parts as it could contain. The current passed as rapidly through the coil as the wire could transmit it from the positive to the negative conductor of the electrical machine. In what way can a more rapid current be conceived? In fine, it is difficult to comprehend how the quantity could be less in the experiment with statical electricity, when the indications of its presence were infinitely greater; and as to the greater rapidity of motion of the current, sometimes claimed in the case of the voltaic combinations, it is but an assumption unsupported by any known fact, and contradicted by everything we know of the electric fluid. I shall hereafter have to consider more fully the peculiarities attributed to current electricity.

The failure of a common galvanometer to produce deflections with frictional electricity, is explained by Colladon by the lateral overflow of electricity from one layer to another, in consequence of the surcharge of the coil. The inference therefore is, that deflections are produced by a minute pair of voltaic elements, such as I employed, because there is no such surcharge or overflow. Can this explanation be admitted, when it is considered, that although the coil may overflow with common electricity, it must retain at least its full quantum of charge; and that is all it can derive from the minute pair of voltaic elements, if it can derive so much? The coil is therefore under at least equal circumstances in both cases; why then is there in one case deflection, and in the other none? I know of no answer, unless it be received as one, according to the suggestion given in the commencement of this essay, that the agent in the former was electricity, which contained the maximum of the deflecting elementary constituent; and that in the latter the agent was electricity with its natural minimum of the deflecting constituent, because it was developed by mere friction. Hence the necessity of the presence of such electricity in considerable quantity and intensity to produce the required effect.

I made an experiment which appears to be explicable in the same manner, although it is a difficulty in the way of the popular hypothesis. I selected a galvanometer the astatic needle of which weighed 60 grs. With the ends of its coil I connected a platinum and a zinc plate, by platinum wires; the platinum plate was half a superficial inch square; the zinc plate, made of the thinnest foil, exposed a surface of only one-hundredth of a square inch. The platinum plate was immersed in a mixture of equal parts of nitric acid and water; the zinc was then introduced, when instantly the needle whirled round the circle completely. In order to estimate what was the equivalent of the voltaic power that produced this effect, I arranged a *couronne des tasses* of twenty pairs of copper and zinc plates, each three-quarters of a square inch in surface, the exciting liquid being dilute sulphuric acid. The polar wires were connected with an electrometer, consisting of two gold leaves detached and insulated from each other, and so contrived that they could be made to recede or approach. By causing the gold leaves connected with the polar wires to approach very gradually, it was found that when they were within about one-tenth of an inch of each other, an attraction was observable. Each gold-leaf proved by calculation to weigh one-fiftieth of a grain; consequently each was 3000 times lighter than the galvanometer needle. The *couronne des tasses* produced its electrical effects on the gold leaves by a surface of metal more than 1000 times greater than the pair of minute plates which whirled the needle round. If then the electrical influence on the gold leaves was barely observable, the electrical effect of the minute pair of plates of platinum and zinc must have been the one-thousandth of a barely observable influence, that is, practically speaking, no electrical effect at all; yet a violent impulse was communicated to a needle 3000 times heavier than each of the gold leaves. How is it possible that the agent which influenced the gold leaves and the galvanometer needle could be precisely the same?

In coming to the conclusion that the agent was different in each case, we are relieved from all embarrassment relative to any supposed influence of intensity or quantity, as it was voltaic electricity that acted in both instances. I have represented the effects here according to the condition most favourable to the common hypothesis. I have taken the power of the *couronne des tasses* in its state of highest intensity, that is, when the circuit was open, in order that the electrical power of the minute pair of zinc and platinum plates might be fairly estimated. It may be supposed that the *quantity* of electricity of the *couronne des tasses* would have been greater had the circuit been closed, but then there could have been no appreciable intensity.

There is a familiar fact, which seems to give no small support to the notion of the compound nature of the electric fluid, and the different ratio in which its constituent elements exist under various circumstances. Connect a zinc and silver plate by means of a straight copper wire placed horizontally. Let a sharp point of a sewing-needle be made to stand erect on the copper wire, and on this place a magnetic needle within an eighth of an inch of the wire. The apparatus, set standing in a proper vessel, is to be so turned that the copper horizontal wire shall coincide with the direction of the magnetic needle. Pour a sufficiency of very dilute sulphuric acid into the vessel, and instantly the needle will be deflected between 30° and 40°. This is the same degree of deflection which the galvanometer needle suffered in Colladon's experiment with common electricity; but in order to obtain that amount, he was obliged to use a galvanometer coil consisting of 500 turns; that is, he was under the necessity of employing the combined effect of 500 wires carrying an intense power of common electricity; whereas in the experiment just described, where the effect was truly voltaic, the same amount of deflection was produced by a single wire carrying the most feeble voltaic electricity.

The experiments of M. Colladon were repeated and varied by Professor Faraday. He employed a Leyden battery having a surface of 3510 square inches of coated glass. By successive discharges of this battery through the galvanometer, conducted by a wet thread 4 feet long, the needle at length suffered deflection to the amount of 40° on each side of the line of rest. He obtained the same deflection also by electricity direct from the prime conductor, without the battery*.

The plate of the machine used by Faraday, as described by him, is 50 inches in diameter; it is furnished with two sets of rubbers; one revolution of the plate will give from ten to twelve sparks from the conductor, each 1 inch long. The battery which produced deflection on the needle had been charged with forty revolutions, consisting of 440 one-inch sparks; and as it was found necessary to repeat the discharge several times in order to produce a deflection of 40°, at least 2000 one-inch sparks were required for that purpose.

These deflections, however, both in the case of the battery and of the conductor, derived assistance from other sources beside electricity, which greatly magnified their amount. The swings of the needle were promoted, from very small arcs, to one of 40° by alternate circulation and interruption of the electric current. Hence the amount of angular deviation was rather a semblance than the reality of voltaic deflection. A heavy pendulum may be made to oscillate in considerable arcs by causing the weakest force to act on it at intervals corresponding with the time of its oscillations. Since the deflection, assisted as it was by the method of production, was but 40°, how feeble must have been the force that produced it!

Mr. Armstrong, with an hydro-electric machine which dis-

* Researches, p. 85.

charged torrents of electricity and gave sparks 22 inches long, could only produce a deflection of 20° or 30°.

In my experiments already described, a bit of copper wire weighing $\frac{1}{1132}$nd of a grain, in connexion with a platinum wire, by a momentary contact with nitric acid, caused the needle to whirl round three times, that is, with an effect incomparably greater than the maximum in the experiments of Colladon and Faraday. An atom of gold-leaf weighing $\frac{1}{6000}$dth of a grain caused deflection to 180°. In neither case was perhaps the ten-thousandth part of a grain of metal dissolved before deflection commenced. Is it possible that such a chemical action, which almost exceeds comprehension for minuteness of effect and of duration, should, in its results on the galvanometer, rival the enormous powers of the plate machine and the hydro-electric machine, if the agent in both cases had been the same? Some scientific questions are best decided by common sense; and if common sense decides that a particle of copper scarcely visible dipped in nitric acid for an instant, producing no obvious effect of electricity, does notwithstanding evolve more of that fluid than a hydro-electric machine, which pours out an incessant stream of long sparks, I must then admit that my arguments are worthless.

But there is one experiment of Faraday which deserves particular notice. He found that, without any Leyden battery, he could produce deflection merely by conducting electricity from the prime conductor to one end of the galvanometer coil, while the other end was in communication with a discharging train, that is, a metallic connexion with the gas-pipes and water-pipes in the street. Thus the electric fluid passed from the conductor directly through the galvanometer, and hence to the common reservoir. The principal feature in this experiment is, that no negative conductor was employed, nor were means used for bringing the negative state of electricity into operation; the deflection was therefore obtained by a current of positive electricity only. The condition for producing deflection by voltaic electricity, is invariably by means of two polar conductors, one of which is said to carry positive electricity, the other negative, no matter whether these be different states or different fluids; both of the poles must be in operation, and the moment either is withdrawn, by interrupting the circuit, the power that causes deflections, and all the other phænomena, ceases to act. But in Professor Faraday's experiment this condition was not fulfilled. There was no connexion of two polar conductors with the coil; positive and negative electricity were not in operation; there was no circuit; in fine, the circumstances were totally different from those under which voltaic deflection is produced. In the voltaic series, the negative wire is not passive, like the discharging train; it is as active as the positive wire; it collects round it a whole class of bodies with as much energy as the positive wire does an opposite class; how then are we to understand its absence in the deflections obtained by Faraday with common electricity in the experiment described? If we look upon the influence acting in two indispensable poles as the cause of voltaic phænomena, can we consider phænomena produced by one pole as emanating from the same cause?

For my own part, I cannot dismiss from my mind a strong impression that the agent in Faraday's experiment was not the same as that which causes voltaic phænomena. Nay, more than this, if it be proved by his experiment that common electricity does not require a twofold polar arrangement in order to produce deflections, I cannot see what the use is of the two poles used in his and Colladon's experiments with the Leyden battery; one of them must have been superfluous. If this be so, we arrive at this general proposition, that voltaic electricity is composed of elements existing in such ratio, and so combined and modified, that it must be brought to bear upon the subject of its action by means of two poles simultaneously and equally energetic; while the proportions and mode of combination in the common electric fluid are such that it produces the same effect with one pole only. Thus, by Faraday's experiments, if my reasonings be correct, an important difference is established, instead of an identity; other facts and arguments of the same tendency will be hereafter brought forward.

I now take leave of this part of the subject, and proceed to consider some other evidences which have been brought forward in support of the affirmed efficiency of quantity to explain the differences observable between the effects of common and voltaic electricity. It is a subject deserving full consideration, as on this foundation is raised the whole superstructure.

[To be continued.]

XXXII. *On Rubian and its Products of Decomposition.* *By* Edward Schunck, *F.R.S.**

Part I.

AMONG the many discussions to which the subject of madder has given rise among chemists, there is none which is calculated to excite so much interest as that concerning the state in which the colouring matter originally exists in this root, and there is no part of this extensive subject which is at the same

* From the Philosophical Transactions for 1851, part ii.; having been received by the Royal Society January 9, and read February 13, 1851.

time involved in such obscurity. It is a well-known fact that the madder root is not well adapted for the purposes of dyeing until it has attained a growth of from eighteen months to three years, and that after being gathered and dried it gradually improves for several years, after which it again deteriorates. During the time when left to itself, especially if in a state of powder, it increases in weight and bulk, in consequence probably of absorption of moisture from the air, and some chemical change is effected, which, though not attended by any striking phænomena, is sufficiently well indicated by its results. There are few chemical investigations that have thrown any light on the nature of the process which takes place during this lapse of time, and in fact most of the attempts to do so have merely consisted of arguments based on analogy. It has been surmised that the process is one of oxidation, and that the excess of atmospheric air is consequently necessary. We are indeed acquainted with cases, in which substances of well-defined character and perfectly colourless, as for instance orcine and hematoxyline, are converted by the action of oxygen, or oxygen and alkalies combined, into true colouring matters. A more general supposition is, that the process is one of fermentation, attended perhaps by oxidation, and in confirmation of this view the formation of indigo-blue from a colourless plant, by a process which has all the characters of one of fermentation, may be adduced. What the substance is however on which this process of oxidation or fermentation takes effect, what the products are which are formed by it, whether indeed the change is completed as soon as the madder has reached the point when it is best adapted for dyeing, or whether further changes take place when it is mixed with water and the temperature raised during the process of dyeing, are questions which have never been satisfactorily answered, if answered at all. It has indeed been suspected by several chemists, that there exists originally some substance in madder, which by the action of fermentation or oxidation is decomposed and gives rise by its decomposition to the various substances endowed either with a red or yellow colour, which have been discovered during the chemical investigations of this root. That several of these substances are merely mixtures, and some of them in the main identical, has been satisfactorily proved by late investigators. But there still remain a number, which, though extremely similar, have properties sufficiently marked to entitle them to be considered as distinct.

In my papers on the colouring matters of madder*, I have described four substances derived from madder, only one of which is a true colouring matter, but all of them capable, under

* Phil. Mag. August 1848, and September 1849.

certain circumstances, as for instance in combination with alkalies, of developing red or purple colours of various intensity. To seek for a common origin for these various bodies so similar to one another and yet distinct, is very natural, and the discovery of it no improbable achievement.

Persoz* asserts the probability of this view in the following words:—"We may hence venture to conclude that the colouring matters which we extract from fabrics dyed with madder, as well as the alizarine which is obtained by submitting the products derived from madder to sublimation, do not exist ready-formed in this root, and are only products derived from another substance which has not yet been isolated..... From numerous experiments which I have made on this subject, it follows that the colouring matter of madder may be compared, in respect to the derivatives to which it gives rise, to tannin, so that I do not despair of being able, as far as regards their metamorphoses, to establish a parallel between the products derived from madder and those obtained from tannin. If it should be possible to confer on the former that tendency to assume regular forms with which the latter are endowed, the separation of the proximate colouring or colour-giving (*colorable*) matters of madder will be easy, and it will thus be possible to establish their elementary composition and thence their relation to one another."

To Mr. J. Higgin is due the merit of having first called attention to the fact, that important changes take place during the process of dyeing with madder, which can only be explained by supposing that an actual formation of colouring matter takes place during the process. In his paper On the Colouring Matters of Madder†, Mr. Higgin has detailed his experiments on that peculiar substance discovered in madder by Kuhlmann and called by him *Xanthine.* I have shown, on a former occasion, that the xanthine of Kuhlmann and other investigators is not a pure substance, but a mixture of two distinct substances. This fact however does not affect the correctness of Mr. Higgin's conclusions, the general accuracy of which I shall have great pleasure in confirming in the course of this paper. The presence of xanthine is easily ascertained by the deep yellow colour and intensely bitter taste which it communicates to cold water. Guided by these two tests, Mr. Higgin arrived at the conclusion, that in an infusion of madder, made with cold or tepid water, when left to itself, or more rapidly when heated to 120° or 130° Fahr., the xanthine gradually disappears and there is formed a gelatinous or flocculent substance, which possesses all the tinctorial power originally belonging to the infusion, while the liquid has lost all trace of any such power, and that as alizarine is the only

* *Traité de l'Impression des Tissus*, t. i. p. 501.

† Philosophical Magazine for Oct. 1848.

substance contained in madder capable of dyeing, the xanthine must, during this process, have been changed into alizarine. Mr. Higgin found that this process is completely arrested by heating the infusion to the boiling-point, or by adding alcohol, acids or acid salts to it, and hence he concludes that the decomposition of the xanthine is caused by the action of a peculiar ferment contained in madder, and which is extracted together with xanthine by cold water. Mr. Higgin did not however succeed in converting his xanthine into alizarine or effecting any change in it by means of fermentation, in consequence, as he supposed, of not being able to obtain the exciting substance in a soluble and consequently active condition. His inferences were all derived from experiments made by either removing from an extract of madder the xanthine contained in it, or by adding to it an additional quantity of that substance, and then ascertaining the effects produced by dyeing in the usual way with liquids thus artificially prepared. By the action of sulphuric acid on xanthine, Mr. Higgin obtained a dark brown powder, which he seems to consider as devoid of any tinctorial power.

A very simple experiment suffices to prove that madder, in its dry state, contains very little, if any alizarine ready-formed. If an extract of madder be made with cold water, it will be found that the brownish-yellow liquid thus obtained when gradually heated will dye as well and as strongly as the madder from which it has been prepared. Now if the colouring matter were originally present in the form of alizarine, this could not take place, since alizarine is almost insoluble in cold water; and in employing it for the purpose of dyeing, it is necessary to dissolve it in warm or boiling water before it begins to exert any effect, as is plainly seen in the case of garancine, which contains alizarine ready-formed. Nor is there much colouring matter left behind in the madder after extraction with cold water, for after converting the residue in the usual manner into garancine by means of sulphuric acid, it is found to be capable of dyeing only very slightly. Nay more, if madder be extracted with hot water instead of cold, I have found the residue to give a garancine which dyed darker colours than that obtained from the residue of an equal weight of madder extracted with cold water, so that it appears that the colour-producing substance is more completely removed by cold than by hot water. If an extract of madder with cold water be left to stand, there is formed in it, as Mr. Higgin has shown, a flocculent substance, while the liquid loses its bitter taste, part of its yellow colour and the whole of its power of dyeing, which is now found to reside in its whole extent in the flocculent substance. This change takes place equally well with or without the access of atmospheric air.

By adding a variety of substances to an extract of madder with cold water, I was enabled to ascertain under what circumstances and by what means the tinctorial power of the liquid is destroyed, and consequently what is the general character of the substance or substances to which it is due. I found that by adding sulphuric or muriatic acid to the extract and heating, the liquid, after neutralization of the acid, was no longer capable of dyeing. The tinctorial power was also destroyed by the addition of hydrate of alumina, magnesia, protoxide of tin and various metallic oxides, but not by carbonate of lime or carbonate of lead. In all cases in which the property of dyeing in the extract was destroyed, I invariably found that its bitter taste and bright yellow colour were lost. Now in my former papers on this subject, I have shown that the intensely bitter taste of madder and its extracts is due to a peculiar substance, to which I have given the name of *Rubian*; and as it appeared from these preliminary experiments that this substance, though itself no colouring matter, is in some way concerned in the changes whereby a formation of colouring matter is induced in aqueous extracts of madder, I proposed to myself to examine its properties and products of decomposition more in detail than I had hitherto done.

The first step necessary to be taken for the attainment of this object, was of course to find a method of procuring this substance in quantities sufficiently large for the purposes of examination. I was at the commencement however far from appreciating the difficulties with which its preparation in a state of purity is attended. The process which I had formerly described, by precipitation with sulphuric acid, is not well adapted to the purpose, since rubian in a state of perfect purity is not precipitated by sulphuric acid, besides which it is easily decomposed by an excess of that acid. Neither is it precipitated by any metallic salt, with the exception of basic acetate of lead, which, from the circumstance of its precipitating also other substances from the extract, is not applicable to the purpose. It is decomposed by alkalies and alkaline earths. Even bicarbonate of lime exerts a decomposing effect on it in conjunction with the oxygen of the atmosphere. These substances must therefore be discarded in its preparation. Besides its great tendency to decomposition, there is another circumstance which presents obstacles to almost all attempts to prepare rubian in a state of purity. There is no investigation of madder which does not make mention of a substance, which when its solution in water is mixed with sulphuric or muriatic acid and boiled, gives rise to the formation of a dark green powder. To this substance, which possesses no bitter taste, and is in fact devoid of any characteristic property except the one mentioned, I have restricted the name of xanthine. The xanthine of most other chemists is however a mixture of rubian with this substance, and possesses therefore the bitter taste of the former, while showing the characteristic behaviour of the latter towards acids. To avoid confusion, I shall no longer employ the name of xanthine, and I shall call the substance which gives the green powder with acids *Chlorogenine.* Now these two substances, though of very different nature, behave similarly towards many reagents. If, for instance, basic acetate of lead be added to a watery extract of madder, according to the method proposed by Berzelius for the preparation of xanthine, and adopted with a slight modification by Mr. Higgin, there is produced a red precipitate, which after being washed and decomposed with sulphuretted hydrogen or sulphuric acid, gives a solution containing rubian; but the presence of chlorogenine is also indicated by its turning dark green when boiled with the addition of sulphuric or muriatic acid. Hence it follows that chlorogenine, though it is not thrown down by basic acetate of lead, when present alone in a solution, is still in part precipitated thereby when rubian is present at the same time. The same circumstance takes place with other precipitants.

After numerous experiments I discovered a property of rubian, which is perhaps more characteristic of it than any other, and that is the remarkable attraction which is manifested by it towards all substances of a porous or finely-divided nature, and it was this property by means of which I was at length enabled to obtain it in a state of purity. If to a watery extract of madder a quantity of protochloride of tin be added, a light purple lake is precipitated. Most of the rubian remains in the solution, which still retains its yellow colour and bitter taste. If, however, after filtering, sulphuretted hydrogen be passed through it, then, provided the quantity of tin still in solution be sufficiently large, the sulphuret of tin, at the moment of precipitation, carries down the whole of the rubian, and the solution loses its bitter taste and the greater part of its yellow colour. The whole of the chlorogenine remains in solution, and may easily be detected in the filtered liquid by means of acids. If the sulphuret of tin, after being collected on a filter and well washed with cold water until the percolating liquid no longer gives a green colour on being mixed with acid and boiled, be treated with boiling alcohol, a yellow solution is obtained, which on evaporation gives pure rubian, without any admixture of chlorogenine, in the shape of a dark yellow, brittle substance. The same effect is produced by sulphuret of lead. If sugar of lead be added to an extract of madder, a dark reddish-brown precipitate falls, the liquid still containing the rubian of the extract, as seen by its deep yellow colour and bitter taste. If sulphuretted hydrogen be now passed through the filtered liquid, a great part of the rubian goes down with the sulphuret of lead, and may again be separated from it by means of boiling alcohol. That this action of the sulphurets on rubian depends very much on their state of division, and is therefore mainly of a mechanical, and not chemical nature, is proved by the fact, that the sulphurets of tin and lead, if prepared by precipitation from solutions of salts in water, and then allowed to settle and repose for some time before being added to a watery extract of madder, remove far less rubian from it than they do, if they are formed in the extract itself, whence it follows that it is only in the minute state of division, in which they exist at the moment of precipitation, before the particles have time to cohere, that these sulphurets exert any great attraction for rubian. That they do however combine with some portion of the rubian, is proved by the fact, that the power of dyeing in an extract of madder is very much diminished by adding to it sulphuret of tin or lead, previously precipitated. Of the two sulphurets, the sulphuret of tin, which is always precipitated in much finer particles than the other, is by far the most powerful absorbent of rubian. If equivalent quantities of protochloride of tin and acetate of lead be added to equal measures of watery extract of madder, the sulphuret of tin from the former absorbs at least twice as much rubian as the sulphuret of lead from the latter. Sulphuret of copper acts differently. If sulphate of copper be added to the extract of madder, a precipitate is produced, as in the case of almost all metallic salts. On passing sulphuretted hydrogen through the filtered liquid, the latter becomes dark brown, but no sulphuret of copper is precipitated.

This attraction of surface exerted towards rubian by bodies in a state of minute division is not confined to metallic sulphurets. There are few bodies which exceed animal charcoal in porosity, or which, in other words, possess for the same bulk a greater extent of surface. I found accordingly that animal charcoal exhibits a still greater attraction for rubian than even sulphuret of tin. A very small quantity of animal charcoal is sufficient to deprive an aqueous extract of madder of its bitter taste and of its tinctorial power. Lamp-black acts in the same manner, though much less powerfully. Wood charcoal however has no absorbent effect whatever on rubian. All these substances attract rubian alone, leaving the other substances contained in the extract, such as chlorogenine, sugar and pectine, untouched. By means of boiling alcohol part of the rubian in combination with them is again removed, and thus an easy and efficient means is given of obtaining rubian in a state of purity. Of these substances none is so well adapted in all respects as animal charcoal.

In employing sulphuret of tin, which is the only one that at all approaches it in efficiency, much time is consumed in the process of filtration and washing. Besides this, I found that on operating with it on a large scale, the rubian obtained was in great part decomposed on evaporating the alcoholic solution, just as if it contained a quantity of acid; and even on treating a portion of the solution with carbonate of lime, for the purpose of neutralizing any free acid that might be present, and evaporating over sulphuric acid at the ordinary temperature, there was obtained a deliquescent mass, which as further experiments showed, could not be considered as pure rubian. After many trials I at length adopted the following method of preparation, which surpasses all others in facility and certainty of execution.

A weighed quantity of madder* being placed on a piece of calico or fine canvas stretched on a wooden frame, boiling water, which is preferable to cold water, as all decomposition of the rubian by means of fermentation is thereby arrested, is poured on it, four quarts of the latter being sufficient for every pound of madder. A dark yellowish-brown liquor is obtained, to which there is added, while hot, for every pound of madder taken 1 ounce of animal charcoal, prepared in the usual way from bones. This proportion of charcoal should not be exceeded, for if an excess of it be taken, as for instance $1\frac{1}{2}$ ounce for every pound of madder, the whole of the rubian is certainly removed from the solution; but on afterwards treating the charcoal with alcohol very little rubian is dissolved, from which it appears that the solvent power of the alcohol only overcomes the attraction of the charcoal for rubian in part. In using the first proportion, part of the bitter taste of the extract remains, showing that the rubian is in excess. The liquid being well stirred with the charcoal, the latter is allowed to settle, which it does in a very short time, and the liquid, which still retains a brown colour, is decanted. The charcoal is then placed on a piece of calico or on a paper filter and washed with cold water, until the percolating liquid, when mixed with muriatic acid and boiled, no longer acquires a green colour, which is a sign that the chlorogenine is removed. These operations occupy a very short time, in consequence of the rapidity with which the animal charcoal may be washed. The animal charcoal is now treated with boiling alcohol, which is filtered boiling hot, and the treatment is repeated until it no longer communicates to the alcohol any yellow colour. The rubian obtained by evaporating the alcoholic liquid is however impure; it contains a considerable quantity of chlorogenine, however carefully the charcoal may have been washed with water, and consequently gives a green powder when treated with boiling sulphuric or muriatic acid.

* Avignon madder, of the variety called *Rosa*, was the kind used.

This proceeds from the circumstance, that fresh animal charcoal, when used in the preparation of rubian, invariably takes up, besides rubian, a quantity of chlorogenine, which is not removable by cold water, but which afterwards dissolves together with the rubian in boiling alcohol. Nevertheless, on using the charcoal which has been once employed, after treatment with alcohol, a second time for the same purpose, it seems to take up rubian alone and no chlorogenine, notwithstanding its being, as might be supposed, in the same condition for again absorbing the latter as it was in the first instance. At all events, the alcohol dissolves only rubian out of the charcoal, when it is used a second time; and if the alcohol should still contain chlorogenine, there will certainly not be a trace of the latter in the alcoholic solution, when the charcoal is used for the third time. That the attraction of the charcoal for rubian is not diminished after it has been once used and then exhausted with alcohol, however indifferent it then becomes towards chlorogenine, is proved by the fact that far more rubian is obtained when the charcoal is employed for the second time than in the first instance. If the animal charcoal, after being once used and exhausted, be heated red-hot so as to destroy all organic matter contained in it, it again behaves towards the two substances in the same manner as in the first instance, that is, it absorbs a mixture of rubian and chlorogenine. It is therefore advisable to reject the rubian which is obtained from the charcoal that has been used for the first time*. If a small portion of the alcohol with which the charcoal has been treated no longer gives a green colour when mixed with acid and boiled, but remains of a pure yellow, it is distilled or evaporated. During evaporation a small quantity of a dark brown flocculent substance is deposited, which is separated by filtration. The solution now contains, besides rubian, another substance in small quantity, which is a product of decomposition of rubian itself, and is probably formed by the application of too great a heat in the process of drying the madder. There are two ways in which this substance may be removed. The first consists in adding to the solution sugar of lead, which precipitates it in dark reddish-brown flocks. These being separated by filtration, the rubian is precipitated by means of basic acetate of lead, and the light red compound or lake, after being washed with alcohol to remove all excess of lead salt, is decomposed either with sulphuretted hydrogen, or better still with sulphuric acid, the excess of the latter being removed by carbonate of lead.

* This impure rubian cannot be purified by means of basic acetate of lead, since when rubian is present in a solution together with chlorogenine, the latter is, though not entirely, still in great part precipitated together with the rubian by that salt.

The other method, which is more expeditious, consists in adding sulphuric acid to the cold solution, after the greatest part of the alcohol has been evaporated. The sulphuric acid completely decomposes the foreign substance, provided a sufficient quantity is employed, and converts it into a substance which renders the solution milky, and then falls in the shape of brown resin-like drops. The sulphuric acid being neutralized with carbonate of lead, the filtered solution, which is yellow and now contains pure rubian, is evaporated to dryness. It is necessary to employ carbonate of lead, and not carbonate of baryta, for the neutralization of the sulphuric acid in both cases; for if carbonate of baryta be used, the bicarbonate of baryta which is usually formed, even if present only in small quantity, causes part of the rubian to undergo decomposition. In evaporating the solution of rubian, care must be taken not to employ too great a heat when the evaporation approaches to a conclusion. The ordinary heat of a sand-bath is sufficient to decompose rubian in great part, especially if a large quantity of the substance be present. It is therefore advisable, when the solution is nearly evaporated, to complete the evaporation either in a water-bath or in a moderately warm place. The free access of atmospheric air need not be feared, as rubian is not thereby decomposed, unless some other substance be present at the same time. The quantity of rubian which I have obtained, according to this method of preparation, amounts to about 1000 grs. from 1 cwt. of madder. It may be mentioned that the method of preparing rubian, as above described, by means of animal charcoal and alcohol, is not new in principle. Lebourdais* has proposed the same method for the preparation of several vegetable substances, such as colocynthine, strychnine, quinine, &c.

Properties of Rubian.—When prepared according to the method just described, rubian is obtained as a hard, dry, brittle, shining, perfectly uncrystalline substance, similar in appearance to gum or dried varnish. It is not in the least deliquescent, as xanthine is described to be. In thin layers it is perfectly transparent and of a beautiful dark yellow colour. In large masses it appears dark brown. It is very soluble in water and alcohol, more so in the former than the latter, but insoluble in æther, which precipitates it from its alcoholic solution in brown drops. Its solutions have an intensely bitter taste. When it is pure, its solution in water gives no precipitates with the mineral or organic acids, nor with salts of the alkalies or alkaline earths. Acetate of alumina, alum, protacetate and peracetate of iron, acetate of zinc, neutral and basic acetate of copper, acetate of lead, nitrate of

* On the Nature and Preparation of the Active Principles of Plants, *Ann. de Chem. et de Phys.* 3me ser. t. xxiv. p. 58. [Chem. Gaz. vol. vi. p. 442.]

silver, perchloride of tin, protonitrate of mercury, perchloride of mercury and chloride of gold produce no precipitate whatever in a watery solution of pure rubian, nor does any reaction take place, except a darkening of the solution in the case of some of these salts. If the rubian be impure, which is always the case when the solution has been incautiously evaporated and the rubian has been exposed to too great a heat after evaporation, then its solution, though it does not differ in appearance from one of pure rubian, when mixed with any mineral or organic acid, even acetic acid, or the salts of the alkalies or alkaline earths, is rendered milky, and a quantity of dark brown transparent resinous drops, mixed with yellow flocks, are deposited. These drops, in the case of the salts, consist merely of a substance insoluble in saline liquids, which dissolves again in pure water; but in the case of acids, they are, though similar in appearance, a product of decomposition of the latter substance, and do not redissolve in pure water. Sugar of lead gives, in a solution of impure rubian, a dark reddish-brown precipitate. Most metallic salts also give precipitates, consisting either of the substance itself which accompanies the rubian, or of compounds of this substance, with the respective metallic oxides. I shall return to these reactions when I come to treat of the action of heat on rubian. Basic acetate of lead gives a copious light red precipitate in a solution of pure rubian, the solution becoming colourless. This is the only definite compound of rubian with a base that I am acquainted with. Concentrated sulphuric acid dissolves rubian with a blood-red colour; on boiling the solution it becomes black and disengages sulphurous acid gas in abundance, after which water precipitates a black carbonaceous mass. If sulphuric acid be added to a watery solution of rubian, and the mixture be boiled, the solution, if dilute, becomes opalescent, and on cooling a quantity of light yellow flocks are deposited; and if the solution was concentrated, these are formed in such abundance on cooling as to render the liquid thick. If these flocks exhibit the least tinge of green, the presence of chlorogenine is indicated. Muriatic acid acts in precisely the same manner. Nitric acid produces in the cold no effect in a solution of rubian; but on boiling, a disengagement of nitrous acid takes place, the liquid becomes light yellow, and now contains the acid which I called in my former papers alizaric acid, and which Laurent and Gerhardt consider as identical with naphthalic acid. Phosphoric, oxalic, tartaric and acetic acids produce no effect on the solution, even on boiling for some time. When a stream of chlorine gas is passed through a watery solution of rubian, the solution immediately becomes milky and begins to deposit a lemon-yellow powder, into which, on continuing the action, the whole of the rubian is converted, the liquid becoming colourless. Caustic soda turns the colour of the solution from yellow to blood-red, and on neutralizing the alkali with acid, a clear yellow solution is again obtained. By boiling the solution to which the soda has been added, the colour changes from blood-red to purple; and on now supersaturating the alkali with acid, a reddish yellow precipitate falls, while the supernatant liquid becomes almost colourless. Ammonia changes the colour of a solution of rubian to blood-red; the colour is not changed by boiling; and by supersaturating the ammonia with acid either before or after boiling, no precipitate is formed. Lime and baryta water give dark red precipitates in a solution of rubian, which are soluble in pure water, forming dark red solutions. Magnesia turns the solution dark red; the solution contains magnesia. The carbonates of lime and baryta produce no perceptible effect on a solution of rubian; they do not change its colour, nor do they take up any rubian. Hydrate of alumina, when placed in a solution of rubian, acquires a brownish-yellow colour. If sufficient alumina be taken, the liquid is rendered almost colourless. Hydrated peroxide of iron acts in a similar manner. Oxide of copper also removes most of the rubian from its solution. Alkaline solutions of rubian do not reduce the oxides of silver and copper on the addition of salts of these oxides, but they reduce salts of gold to the metallic state. When heated on platinum foil, rubian melts, swells up very much, burns with a flame and gives a carbonaceous residue, which does not entirely disappear on being further heated, but leaves a quantity of ash. When heated gradually in a tube, it begins to undergo decomposition, accompanied by loss of water at a temperature of about 130° C., and is converted into another substance, which I shall describe further on. When heated to a still higher degree in a tube or retort, it gives fumes of an orange colour, which condense on the colder parts of the vessel to a crystalline mass, consisting chiefly of alizarine.

Rubian cannot be considered as a colouring matter in the ordinary sense of the word. It imparts hardly any colour to mordanted cloth, when an attempt is made to dye with it in the usual way, the alumina mordant only acquiring a slight orange, the iron mordant a light brown colour.

Composition of Rubian.—In determining the composition of rubian, I found it necessary to take into consideration the fact of its leaving when burnt a considerable quantity of ash. This ash consists almost entirely of carbonate of lime. The amount of ash is not uniform in different specimens; it is greatest when the rubian has been purified by means of sulphuric acid, but I have never been able to obtain it in a state in which it burns without any residue. Even after being precipitated with basic acetate of lead and again separated from the oxide of lead, rubian leaves some ash on being burnt, so that it appears as if the lime which it contains were an essential constituent, or at all events, that it follows it into the lead compound, from which it cannot be removed by means of water or alcohol.

The following results were obtained on analysis:—

I. 0·3880 grm. rubian, which had been purified by means of sulphuric acid, dried at 100° C., gave, when burnt with oxide of copper, 0·7210 carbonic acid and 0·1745 water.

II. 0·4780 grm. of the same preparation, burnt with oxide of copper, gave 0·8865 carbonic acid and 0·2180 water.

III. 0·4755 grm. of the same preparation, burnt with oxide of copper, gave 0·8835 carbonic acid and 0·2180 water.

0·1690 grm., on being incinerated, left 0·0130 grm. of ash=7·69 per cent.

IV. 0·3910 grm. rubian, purified by means of acetate and basic acetate of lead, burnt with chromate of lead, gave 0·7455 carbonic acid and 0·1880 water.

0·4050 grm. of this preparation left 0·0215 grm. of ash=5·30 per cent.

V. 0·4235 grm. rubian, purified in the same way as I. and burnt with chromate of lead, gave 0·7890 carbonic acid and 0·2020 water.

0·6400 grm. of this preparation left 0·0465 ash=7·26 per cent.

VI. 0·4390 grm. rubian, purified in the same way as IV., burnt with chromate of lead, gave 0·8370 carbonic acid and 0·2120 water.

0·8400 grm. of this preparation left 0·0440 ash=5·23 per cent.

After making the necessary corrections for the ash, these numbers correspond in 100 parts to—

	I.	II.	III.	IV.	V.	VI.
Carbon . . .	54·89	54·79	54·89	54·90	54·78	54·84
Hydrogen . .	5·41	5·48	5·51	5·64	5·71	5·66
Oxygen . . .	39·70	39·73	39·60	39·46	39·51	39·50

Rubian contains no nitrogen. On burning it with oxide of copper and collecting the gas over mercury, I found the latter to be entirely absorbed by caustic alkali. When burnt with lime and soda, only a minute trace of chloride of platinum and ammonium was obtained. The statement contained in my former paper, which was made at a time when I had not obtained rubian in a state of absolute purity, that nitrogen is one of its constituents, must therefore be corrected.

From the above analyses the following composition may be deduced:—

	Eqs.		Calculated.
Carbon . . .	56	336	55·08
Hydrogen . .	34	34	5·57
Oxygen . . .	30	240	39·35
		610	100·00

The compound with oxide of lead, which was the only one that could be employed for the determination of the atomic weight, was prepared by dissolving rubian in alcohol, adding acetate of lead, precipitating with a little ammonia, taking care to leave an access of rubian, and washing with alcohol. If it be prepared by precipitation from a watery solution by means of basic acetate of lead, great difficulties are experienced in the course of filtration; the liquid begins to run through slowly, the precipitate becomes somewhat mucilaginous and adheres to the paper, and sometimes even it seems to be decomposed and no longer gives unchanged rubian, but a dark brown viscid substance. Its analysis gave the following results:—

I. 0·3670 grm., dried at 100° C. and burnt with chromate of lead, gave 0·3520 carbonic acid and 0·0875 water.

0·3360 grm. gave 0·2390 sulphate of lead.

II. 0·4440 grm. of another preparation, burnt with chromate of lead, gave 0·4190 carbonic acid and 0·1115 water.

0·4320 grm. gave 0·3100 sulphate of lead.

III. 0·4635 grm. of the same preparation as the last gave 0·4450 carbonic acid and 0·1050 water.

0·5405 grm. gave 0·3880 sulphate of lead.

These numbers lead to the following composition:—

	Eqs.		Calculated.	I.	II.	III.
Carbon . . .	56	336	26·25	26·15	25·73	26·18
Hydrogen . .	34	34	2·65	2·64	2·87	2·51
Oxygen . . .	30	240	18·76	18·89	18·62	18·51
Oxide of lead .	6	670	52·34	52·32	52·78	52·80
		1280	100·00	100·00	100·00	100·00

Hence it appears that oxide of lead in combining with rubian does not replace any basic water, as is usually the case.

It may easily be conceived that a body so readily decomposed as rubian gives a number of different products of decomposition. It is decomposed by acids, alkalies, chlorine, heat and ferments; and I shall now proceed to describe the products of decomposition to which these various reagents give rise.

[To be continued.]

XXXIII. *Notices respecting New Books.*

Three Introductory Lectures delivered at the Government School of Mines and of Science applied to the Arts; Museum of Practical Geology.

A SHORT time ago it became our pleasing duty to direct attention to four introductory discourses delivered at the Government School of Mines, one by the Director of the Institution, and the others by three of its Professors. A new session brings three other lecturers before us: Mr. Warington Smyth on the Value of an Extended Knowledge of Mineralogy and the Process of Mining; Mr. Andrew C. Ramsay on the Science of Geology and its Applications; and Dr. Percy on the Importance of special Scientific Knowledge to the Practical Metallurgist. The distinct position assumed by each lecturer is maintained with ability, and instances of existing ignorance are plentifully adduced to show the necessity of scientific culture in each respective department. Mr. Smyth commences by clearly defining what mineralogy is, and shows the necessity of cultivating the conterminous sciences. No man can be said to grasp a science unless he knows something of those which lie around it; a map is incomplete without what surveyors call its abutting detail; and to a successful prosecution of mineralogy, some knowledge of geometry, chemistry and natural philosophy, is undoubtedly necessary. By examples drawn from the soft sandstones and massive architecture of ancient Egypt, from the marble of Attica and the sculpture of Phidias, and others of a similar nature, the influence of mineral products upon the arts and character of nations is shown; and the iron ores of Britain are pointed at as one great source of her present manufacturing eminence.

"The mining districts of England, however, are so utterly destitute of the means of mineralogical education, whether in schools or suitable collections, that it need be no source of wonder to find the most intelligent miner acquainted only with some two or three of the substances which in the routine of his employment have been brought prominently before him, and often neglecting others from ignorance of their nature, or dangerously confounding things which are totally distinct from each other. It is matter of history, that the copper ores of Cornwall were recognised as useful only at a comparatively late date, the miners having concentrated all their attention upon the tin with which that county was so plentifully supplied. More wonderful does it appear, that even at the commencement of the last century, when the yellow ore or pyrites had been long appreciated, the far more valuable redruthite, or sulphide of copper, was thrown as worthless rubbish over the cliffs of St. Just into the Atlantic; and Pryce informs us, that many thousand pounds worth of the rich black ore or oxide of copper was washed into the rivers and discharged into the North Sea from the old Pool mine." These things occurred in England when the value of these substances was well known in other countries.

The lecturer proceeds to recount instances of loss and ruin, the result of ignorance and duplicity, which have come under his own notice. To one of these we will confine ourselves :—"I have known," says Mr. Smyth, "zinc blende taken for lead ore, and honoured with the erection of a smelting furnace, when to the chagrin of the manager the volatile metal flew away up the chimney, leaving only disappointment and loss behind. Again, from a faint resemblance which some of the varieties bear to certain iron ores, a resemblance which would at once disappear before accurate observation, a considerable quantity was bought not long since by one of the greatest iron-masters in this country. It was carried to the furnaces, duly mingled with fuel and flux, and after a strenuous effort had been made to get it to yield iron, it all, as the proprietor naïvely remarked, 'went off in smoke.'"

But a matter of far more importance than the correction of isolated mistakes is the investigation of the principles which regulate the accumulation of ore in metalliferous veins,—a subject so enveloped in mystery, that our present mining enterprises are almost as much a matter of chance as such enterprises were three centuries ago. "Copious stores of knowledge have, it is true, been acquired by many of the captains and tributers in Cornwall and elsewhere; but besides the difficulty, according to the various views of individuals, in collating them, they have generally, for want of early educational opportunity, been accumulated upon an unsafe basis; and finally, the experiences perish with the men, leaving society no richer for their acquisition."

Referring to the vast mineral resources of Great Britain, and the multitude which derive employment and support from this source, the lecturer earnestly proceeds: "Let us then consider the great population supported directly by the extraction of these minerals, and indirectly by their application to the arts—the maintenance of hundreds of thousands of men by these not inexhaustible stores, and the entire dependence of our whole manufacturing and commercial system on the supply of fossil fuel; and we cannot fail to arrive at the conviction, that in exercising the stewardship of such gifts of Heaven the nation has a high and responsible duty to perform, that waste and improvidence are a national sin, and that it behoves all who are in any way connected with the working of our mines to lend their best endeavours to the perfecting of the most œconomical and efficacious means of rendering all the products of our mines available to the uses of mankind."

Among the methods employed for ascertaining the existence of useful deposits, the lecturer refers at some length to the art of boring, and recommends steel instead of iron borers;—refers to the pneumatic dam of M. Triger for keeping back water while sinking shafts; to the ventilation of mines; to the necessity of accurate surveys, and the vast dangers incurred in this respect; to the dressing of the ores, and the improvidence at present exhibited in California and Australia; to the ignorant assertion that England can afford to squander her mineral riches, holding up the boast of Xenophon of the inexhaustibility of the silver mines of Laurion as a warning to ourselves. "When the day comes that our preponderance in natural resources is reduced to something nearer equality, when deeper and thinner coal-seams must be wrought, when poorer ores of the metals must be more highly prized, and when the products of our manufactures can only be brought into commerce at higher prices, then must the star of England's prosperity decline, unless we keep our vantage-ground by the superior skill and knowledge to which technical education must greatly contribute."

Following the lectures in the order in which we accidentally perused them, the lecture of Mr. Ramsay next presents itself. He refers in his introduction to the resistance offered to the earlier advances of geological inquiry. "Geology was not so fortunate as chemistry, when princes vied with each other in the encouragement of alchemical discovery. There was no heresy in the transmutation of the baser metals into gold. Geology, on the contrary, was for a long time generally esteemed a pestilent heresy; and though its cultivators escaped the prison, yet even in our day a few angry men are not wanting, who, steeped in ignorance or a mistaken zeal, still re-echo the time-worn cry."

The lecturer arranges his subject under two principal heads—Physical Geography and Palæontology; the former dealing with the nature and modes of formation of rocks, and the latter with the organic forms which they contain. The beautiful investigations of Bunsen in Iceland are referred to as a fine example of the bearing of chemistry upon the metamorphism of rocks and the theory of volcanoes. Werner and Hutton were the first to generalize in a grand and comprehensive manner the facts and speculations of previous observers. "Of Werner it might be said that his merit consisted in this, 'that he infused into the body of the science a new spirit.' The breadth of his views respecting the universal superposition of strata, his application of their structure to mining, and the eloquent sincerity with which he advocated his doctrines, raised an enthusiasm that spread over Europe and gained numerous disciples to the cause."

The lecturer pays a noble tribute to the memory of Hutton. "Of all men who have hitherto illustrated the science of geology none is greater than Hutton, whose name was so long used as their watch-word by the opponents of the Wernerians. He at once threw aside the minor proofless speculations with which older writers bewildered their readers; and by the strict union of observation and generalization, his comprehensive mind grasped the main outlines of the physical section of the subject, and brought geology within the pale of inductive reasoning." To William Smith, however, the lecturer considers "that we owe the first clear enunciation of the law of the stratigraphical succession of species—a law alike great in theoretical results and in the strictly practical applications arising therefrom."

It is interesting to observe the earnest devotion displayed by the lecturer in rescuing the memory of Smith from undeserved obscurity; indeed we have felt as keen a pleasure in looking through these lectures as through spectacles, into the men who delivered them, as in the contemplation of the results and arguments which they bring forward. The Government School of Mines may, we think, be congratulated on the amount of highmindedness, vigour, and ability which has been enlisted in its cause. Here and there the private hope and aspiration of the lecturer crops out, and it is always a noble hope and aspiration. We have in the majority of cases the express qualities necessary to the founders of a new institution—earnestness and enthusiasm, united to intellectual power sufficient to control and regulate both.

The author proceeds to consider the results and bearings of the law of superposition, and the absurd and ruinous speculations which have flowed from ignorance of that law. "At the very moment I now write I have received a letter from Mr. Aveline, one of the geologists of the Survey, in which he says, 'I have a narrow slip of coal-measures running between the Permian and the new red beds, and the old red sandstone that you saw at Bewdley. A person found out the only place where the coal is well shown, and sunk a pit; but finding the coal worthless, *he has gone a little way off on the old red sandstone*, where he is sinking after the most approved manner, bricking his shaft round.' Near Trefgarn, Caermarthen," &c., proceeds the lecturer, "the black slates are dotted with shafts, borings, and levels, sunk or driven in delusive searches for coal. While in progress, the cry still is 'the indications are good, go a little deeper;' and the pit, the disappointment, and the ruin often deepen together, till, abandoned in despair, the speculator is left to console himself with the parting assurance, 'We are not to blame,—had you only gone a little deeper.' Long after, when the wandering geologist visits such spots, he is informed that the miners actually found coal, but were bribed to hush it up by the coal-owners, jealous of their markets."

To Professor Ramsay's condemnation of exaggerated vertical sections we see no reason to subscribe. No man of any experience could, we imagine, be misled in this way. In the exaggerated section we are not required to trust our eyes, but can obtain, by direct measurement with the scale, the precise thickness of the seam of coal. In the natural section we doubt whether this precision is possible; the thickness of the finest line would, we imagine, amount to some feet; and thus, though the eye may be furnished with a correct general impression, accurate measurement appears to be out of the question.

We shall here transcribe Professor Ramsay's interesting account of the artesian well at Grenelle near Paris. "The nature of the artesian wells is simple. If I take a bent tube and pour therein any quantity of water, it will maintain a corresponding level on either side; and if I insert another tube shorter than the curved arms (we shall suppose at the lowest point of the curve), then by virtue of a law of hydrostatic pressure the water will rise in the inserted tube, an equal amount being displaced in the curved arms on either side. There it will rest. But if a constant supply be yielded to one or both of the openings of the curved reservoir, then the water will overflow at the mouth of the central inserted tube, which thus represents the boring of an artesian well.

"The strata around Paris are in a general way very similar to those forming and surrounding the London Basin (as it is often termed), with which many of you are familiar. Its highest members are composed of tertiary strata, of sand and calcareous sandstone, beneath which are beds of mottled clay. The chalk on which this lies is 1477 feet thick, resting on 150 feet of greensand, which in its turn lies on the gault. This last is for the most part composed of clay, and nearly impermeable to water. The whole, over a width of many miles, is arranged in the form of what geologists call a basin; that is to say, the strata from their outcrops have a tendency to slope towards a general centre, where for a space they lie more or less horizontally.

"On the margin of the basin, strata of greensand and gault rise to the surface at heights in many places approaching to 330 feet above the sea, Grenelle being only about 100 feet above that level. Geologists knew that the water which fell on these strata at their outcrop would of necessity percolate in the direction of the inclination of the beds; so that at the lower points of the curvature a great body of water must exist, confined, as it were, in a sponge, and unable to escape below, because of the impermeable quality of the beds on which the porous strata rest. This deep-seated reservoir being tapped by boring, the water would rise to the surface in the manner I have explained.

"In 1832 the municipal corporation of Paris, impressed with the sanitary necessity of further supplies of water, voted 18,000 francs for the construction of three artesian wells, a sum so ridiculously small that the project was immediately abandoned. M. Mulot, however, one of their engineers, having previously sunk in the chalk at Suresne, at Chartres, and at Laon, to the depth of 1082 feet, proved that it would be necessary to bore completely through that formation to obtain a sufficient supply. This conclusion, based on strict geological reasoning, was confirmed by MM. Arago and Walferdin, and in November 1833 the work was begun. With infinite energy, skill and perseverance, M. Mulot carried it on, overcoming every opposition, physical and moral; for he had not only to conquer those natural difficulties which beset so unexampled an undertaking, but he had also to contend with municipal parsimony, that shrunk from the continuance of supplying funds for a project based on purely theoretical grounds. When he reached the depth of 1640 feet, at an expense of 263,000 francs, they stopped those supplies; but so great was the faith of M. Mulot in the correctness of the principle involved, that he determined to continue the work at his own charges. On the 26th of February 1841, the borer fell suddenly several yards; and immediately, from a depth of 1800 feet, there sprang from the orifice a huge column of water, cold at first but warm afterwards. It now steadily yields more than 740,000 gallons a-day. At the first burst the supply was greater."

Thirdly and lastly, we take up the lecture of Dr. Percy, the production, if we mistake not, of a mind differently constituted from either of the former. Pounds, shillings, and pence constitute the lever by which the Doctor moves his audience. His vocation is the formation of a good metallurgist, and he leaves fine-spun theories of human culture and advancement to those who can enjoy them. His lecture is a solid substantial production; there is something infinitely more pleasing in this sturdy adherence to what he considers to be the facts of the case, than in any affectation of philosophical sentimentality; and as long as Dr. Percy thus manfully stands by his convictions, and endeavours, as far as in him lies, to enact them practically, he has a claim to the respect of every lover of straightforwardness. Men, however, who take this view of things, are rarely slow to affirm that it is the only view; which affirmation carries them all unconsciously from the region of fact into that of fallacy. The greatest mistakes of individuals, the bigotry of sects, and the animosity of rival theorists, are to be traced to one-sidedness, to the putting of a part for the whole, to looking at an object from one point of view, and denying that it possesses any other phase or character than that which they discern from this point. Now that the body must be clothed and nurtured is a physiological axiom which nobody will feel inclined to dispute; and as this is done through the instrumentality of pounds, shillings and pence, such considerations appear to be perfectly justifiable as incentives to exertion and improvement. But we sincerely believe that the man whose theory of human culture rises no higher than this, will prove defective even as a practical man. It is not the love of gain, but the love of truth, as incidentally remarked by Mr. Ramsay, which has produced our greatest practical results. Most heartily do we sympathize with those outbursts of a higher faith which shine like sunbeams here and there through the discourses of most of the professors of the School of Mines. They are not the outbursts of a vain enthusiasm, but the aspirations of men who have encountered the difficulties of culture and tasted of its sweets; and even should circumstances render it necessary on their part to observe an extreme frugality in the enunciation of these higher principles, our hope is that they will not suffer them to decay; and that even among their most practical hearers, to borrow the concluding words of Mr. Smyth, there may be some few who will not stop short at that point whence they may obtain their worldly ends, but will persevere towards that goal of higher knowledge which has been, and always will be, the object of the noblest of mankind.

Optical Investigations occasioned by the Total Eclipse of the Sun on the 28*th of July* 1851. *By* Dr. v. FEILITZSCH. Greifswald, 1852: Th. Kunike.

The author was one of the numerous band of observers who planted themselves within the moon's shadow upon the day above mentioned. His place of observation was Karlskrona in Sweden. He traces the doubt and mystery which have hitherto enveloped the phænomena attendant upon solar eclipses to the fact, that they were observed solely by astronomers, and not by physicists. This remark appears to be scarcely applicable where such men as Professor Airy are concerned. Surely the man whose investigations on optics have won him such high renown is not likely to fall into the error of regarding the phænomena in question as lying beyond the limits of physical explanation, or of forgetting the possible influence of diffraction and interference in their production.

Grounding his views on the theory of undulation, the aim of the author is to show that the corona, the coloured light, and the red projections from the moon's rim during a solar eclipse, are all the production of diffraction and interference: the results of his inquiry, which certainly evinces considerable ingenuity and a patient study of the phænomena, are as follows:—

The corona observed from the absolute shadow of the moon owes its existence to the diffraction of the sun's rays at the moon's edge.

The coloured fringes, caused by interference, exterior to the shadow, are the origin of the various colours observed on clouds during a total eclipse, as also of the colours which precede and follow the total occultation.

The coloured light observed during the total eclipse is the light reflected from the coloured atmospheric envelope which immediately surrounds the absolute shadow.

The dark, bright, and oblique-directed radiations of the corona are phænomena of interference, due to diffraction by the mountains on the moon's edge, when these mountains lie in or near the line which connects the observer with the sun.

If, however, these mountains are peculiarly shaped, or if they lie outside the above line of connexion, the light diffracted by them creates the appearance of the red projections.

The red colour of the projections, and of the surfaces which appear detached from the moon's rim, and the increase and decrease of the projections according to the relative position of sun, moon, and observer, are due to the deportment of the light sent to the observer from the æther particles in free space, when these particles, through the interference of the light diffracted on the mountains at the edge of the moon, are more strongly excited than the neighbouring ones.

XXXIV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 152.]

Jan. 22, 1852. — A PAPER was read, entitled, "Researches on the Geometrical Properties of Elliptic Integrals." By the Rev. James Booth, LL.D., F.R.S. &c. Received November 17, 1851.

In this paper the author proposes to investigate the true geometrical basis of that entire class of algebraical expressions, known to mathematicians as elliptic functions or integrals. He sets out by showing what had already been done in this department of the subject by preceding geometers. That the elliptic integral of the second order represented an arc of a plane ellipse, was evident from the beginning. Hence indeed the name "elliptic functions," derived from a part, was given to the whole. Here then the question naturally arose: What geometrical types did the first and third orders represent? This question long remained without complete solution; and investigators in this department of analysis were compelled to take the fundamental expressions as arbitrary data, and to forgo the inquiry what the geometrical theorems were which these algebraical expressions represented. Various but unsuccessful attempts were made by geometers to represent them by quadratures, or by plane curves, either algebraical or transcendental. About ten years ago, however, Messrs. Guderman and Catalan showed that the circular form of the third order represented the curve of intersection of a cone of the second degree and a concentric sphere; but they did not extend their researches to the first order, nor to the logarithmic form of the third.

The main object of the paper is to prove that elliptic integrals of every kind, the parameter taking any value whatever between positive and negative infinity, represent the intersections of surfaces of the second order.

These surfaces divide themselves into two classes, of which the sphere and the paraboloid are the respective types; from the former arise the circular functions of the third order, from the other the logarithmic and exponential. In the course of these investigations it is shown that the formulæ for the comparison of elliptic integrals, which are given by Legendre, follow simply as geometrical inferences from the fundamental properties of those curves. The ordinary conic sections are merely particular cases of those more general curves, to which the author has given the name Hyperconic Sections.

The author remarks, that it will doubtless appear not a little singular, that the principal properties of these functions, their classification, their transformations, the comparison of elliptic integrals of the third order, with conjugate or reciprocal parameters, were all investigated and developed before geometers had any idea of the true geometrical origin of those functions. It is as if the formulæ of common trigonometry had been derived from an algebraical definition, before the geometrical conception of the circle had been admitted. As trigonometry may be defined, the development of the properties of circular arcs, whether described on a plane, or on the surface of a sphere, so this higher trigonometry, or the theory of elliptic integrals, may be defined as the development of the relations which exist between the arcs of hyperconic sections.

It may be said, we cannot by this method derive any properties of elliptic integrals which may not algebraically be deduced from the fundamental expressions appropriately assumed. It cannot, however, be truly asserted that the properties of curve lines should be developed without any reference to their geometrical types. We might, starting from certain algebraical expressions, derive every known property of curve lines, without having in any instance a conception of the geometrical types which they represent. The theory of elliptic integrals was developed by a method the inverse of that pursued in establishing the formulæ of common trigonometry.

In the latter case, the geometrical type was given—the circle—to determine the algebraical relations of its arcs. In the theory of elliptic integrals, the relations of the arcs of unknown curves are given, to determine the curves themselves; this is the principal object of the present communication.

The problem resolves itself into twelve distinct cases, depending on the magnitude of the parameter, and the sign with which it is affected; out of the discussion of these cases arise many new and important relations of elliptic integrals. It would excite little interest to give the bare enunciations of those theorems, and a mere outline of the methods by which they are established would be unintelligible. Not the least interesting of those theorems is the proposition, that it is always possible to express an elliptic integral of the first order as the sum of two elliptic integrals of the third order, with parameters which are conjugate, reciprocal and imaginary.

The author hopes, in a future communication to the Royal Society,—the present having grown under his hands beyond the limits he anticipated—among other points, to extend his researches to the case of elliptic integrals with imaginary parameters, and to show the true geometrical meaning of such expressions. It will also be shown, that imaginary expressions may be found for a logarithmic elliptic arc analogous to the well-known imaginary exponential expressions for the sines snd cosines of circular arcs.

XXXV. *Intelligence and Miscellaneous Articles*

ON THE ARTIFICIAL FORMATION OF SEVERAL MINERALS. BY M. BECQUEREL.

IN ordinary chemical operations, when one body is made to act upon another,it is customary to powder them, to dissolve them,or to bring them to a state of igneous fusion. It is then almost impossible to observe the results of slow action, such as nature presents so often, and the electrical effects resulting from immediate contact, which may in certain cases aid in bringing about the former, or giving them a greater energy. Electro-chemistry, therefore, differs from chemistry in employing electricity as a subsidiary means of exciting affinity or rendering it more efficacious, and in its requiring the mutual presence of three bodies, of which one at least must be in the solid state and another liquid. Such is the point of view under which I have constantly regarded electro-chemistry, which furnishes means of analysis and synthesis of which advantage might be taken. These researches have moreover the advantage of making known the necessary conditions under which solutions containing one or more combinations can react upon insoluble compounds with which they are in contact.

The weak actions which have particularly attracted my attention are those which commence as soon as the rocks, the metallic and other substances which occupy veins and beds, come in contact with the mineral waters which rise from all parts of the earth's interior. Time then becomes an element in the growth of the crystalline substances formed, an element which enters indefinitely into all natural phænomena, but which we can employ only within certain limits, sufficient however to obtain marked effects, as is shown by the results obtained during the period which has elapsed since 1845.

Among the methods adopted in these experiments were the following:—

First process.—This consisted in making a solution of silica or alumina in caustic potash or soda react weakly upon a couple formed of a plate of oxidizable metal, and a copper or platinum wire round which the plate is bent, the whole being contained in a vessel closed by a cork, and left to spontaneous action.

In 1845, an apparatus was arranged with a plate of amalgamated zinc surrounding a copper wire, and a solution of silica in potash marking 22° on the areometer; water was decomposed, with evolution of hydrogen and formation of oxide of zinc, which dissolved. A fortnight afterwards, very small regular octohedral crystals began to be perceptible upon the zinc plate, the composition of which was represented by the formula ZnO, HO. The bulk of these crystals increased gradually, without passing a certain limit, about 1 millim. on each side.

In operating with alkaline solutions more or less concentrated, it was observed that the crystals were larger and better defined when the strength was not beyond 20° or 25°. Other arrangements were made in 1845, by substituting for the zinc-copper couple a lead-copper one, and employing an alkaline solution of 25°; the lead was slowly attacked, the protoxide formed dissolving, and after saturation was deposited upon the surface of the plate of lead in anhydrous crystals (PbO).

These crystals, some of which measured several millimetres, were transparent, of a darkish green colour, and gave on trituration a yellowish powder. They were so grown together, that only parts of their extremities were visible. Other reasons make it probable that the crystals are derivatives of a right rhombic prism.

Second process.—Sulphuret of lead or galena (PbS) was made to act upon a saturated solution of sulphate of copper and of chloride of sodium diluted with an equal volume of distilled water, with a view of obtaining compounds of lead, having analogues in nature.

In May 1845, I made several arrangements of galena and the mixture of chloride of sodium and sulphate of copper, which were left to themselves until the present time. The following are the products which have been formed, either upon the pieces of galena, the bottom or partitions of the vessels:—

1. Chloride of sodium in cubes, cubic octohedrons, and even octohedrons having great transparency, very definite form, and from several millimetres to 1 centimetre in length.

2. Chloride of lead, in needles and cubes, slightly yellowish, of very perfect form.

3. Sulphate of lead, in cuneiform octohedrons, much modified, precisely resembling in form the crystallized sulphate of lead of Anglesea.

4. Chlorosulphate, in needles.

5. Basic chloride, in microscopic crystals, disseminated here and there throughout the whole product.

6. Sulphuret of copper, black, without any appearance of crystallization.

The whole of these substances covering the piece of galena, gave it the appearance of a specimen from a mineral vein.

In some of the vessels there were formed only chloride and chloro-sulphate of lead, in others chloride and sulphate, which depended no doubt upon the proportions of the sulphate of copper and of chloride of sodium, and the density of the solutions. A voltaic couple, formed of a piece of galena surrounded by a platinum wire, placed in a saturated solution of common salt and sulphate of copper diluted with 3 vols. of water, gave rise to the formation of a considerable quantity of crystallized chloride of lead in cubes, without any other product; they were similarly deposited, though a little larger, upon a fragment of malachite which was placed in the solution.

There is no evidence in opposition to the opinion that these reactions take place in nature. In fact, the pluvial waters which reach the mineral masses and veins, formed of metallic combinations, become charged with chloride of sodium and sulphate of copper, arising from the decomposition of the cupreous pyrites; the resulting solutions, once in contact with the galena, react upon it weakly, and give rise to the various compounds described above.

Two other compounds have been obtained, PbO,CO^2 and CaO,CO^2, by the following processes :—Into a saturated solution of carbonate of soda and carbonate of copper was introduced a plate of lead, 4 centims. by 2, surrounded by a platinum wire, the whole placed in a glass vessel imperfectly closed, and left to spontaneous action for seven years. The lead gradually oxidized at the expense of the atmosphere; the oxide formed, slightly soluble in water, reacted upon the carbonate of copper, whence resulted hydrated oxide of copper and carbonate of lead (PbO, CO^2). This was in very small crystals, covering the plate of lead, and their form appeared the same as the natural carbonate. The carbonate of lime was obtained by effecting the decomposition of the sulphate of that base, a salt slightly soluble in water, and naturally abundant, by a solution of bicarbonate of soda, a compound found in several mineral waters. A plate of Montmartre gypsum was introduced into the solution (saturated or not) of the latter salt; it soon lost its vitreous brilliancy, and was covered with small rhombohedrons of carbonate of lime. At the moment of contact, the gypsum dissolved, and reacted immediately upon the bicarbonate of soda. There was a separation of carbonic acid, which partly remained in the solution on account of imperfect closeness of the vessel. The formation of sulphate of soda and carbonate of lime in such a way that the plates which successively separated from the gypsum were formed of small attached rhombohedrons, cannot be supposed as solely owing to a double decomposition. It is probable that the dissolving action of the carbonic acid plays a part in the phænomenon. These effects always present themselves with weak solutions of bicarbonate.

These facts prove two principles, by the aid of which a certain number of insoluble crystalline compounds may be produced similar to the natural ones. The first consists in slowly oxidizing a body in a solution of substances, upon which the oxide formed reacts, and whence result oxides and various crystallized insoluble compounds. The second relates to the feeble reactions which take place when a slightly soluble body is placed in contact with a solution containing several compounds, giving rise to double decomposition, in which case insoluble compounds are formed, which crystallize.—*Comptes Rendus*, Feb. 1852.

ELOIN'S IMPROVED MINER'S SAFETY LAMP.

Important as was the discovery by Sir Humphry Davy, of the property possessed by thin wire-gauze to prevent the passage of flame, yet it could hardly be expected that the details of any arrangement embodying this principle could be at once made perfect. Attempts to improve the structure of the original Davy lamp have, therefore, been numerous; but few of them have been generally adopted; and in most of our collieries the original form of lamp is still used.

The principal defects of the common Davy lamp are,—first, deficient light, rendering the collier always unwilling to use it, unless compelled by the presence of a highly explosive atmosphere; second, liability of injury to the gauze of the cylinder, either by a blow from a pike, a fall to the ground, or otherwise; third, the possibility of a current of explosive atmosphere being carried through the gauze cylinder, either by the swinging of the lamp in the hand of a person when walking, or by its being exposed to the powerful blowers of gas, which are sometimes given off with great force; fourth, the heating to redness of the gauze, by which explosions actually take place, from the contact of the explosive atmosphere with the heated wire. This danger is often increased by the presence of small particles of coal-dust, which, floating in the air of the mine, attach themselves to the gauze; and also from the deposit of soot on the gauze, arising from the imperfect combustion of the oil, which in the common Davy lamp always gives off a dense column of smoke.

In the improved lamp of M. Eloin these defects are obviated. In reference to light, the cylinder above the flame is closed, and air is admitted only below the flame, through a narrow breadth of gauze; but the air which is admitted is brought into actual contact with the flame, by the application of a cap, on the principle of the solar lamp; and thus perfect combustion is produced and light given off equal to at least five or six ordinary Davy lamps. As to the liability of injury to the gauze, this is obviated by using, first, a strong short cylinder of glass, through which the light passes, capped over the flame with a brass or iron cylinder, which cannot be injured except by actual violence. It might be supposed that the glass portion of the cylinder would be liable to accident; but in practice this is not found to be the case: bound, at top and bottom, by a strong brass ring, if it were even to crack, either from a blow, or from unequal expansion by heat, no danger would result, as the pieces into which it would be separated would still be held together by the brass beadings. The closed nature of the cylinder entirely prevents the passage of an explosive atmosphere into the lamp by any current of air; no swinging of the lamp causes any action on the flame; and no blower of gas can blow into the flame, in consequence of the protection of the cylinder: all danger also, by reason of the heating to redness of the wire-gauze, is entirely removed.

Besides the removal of the defects common to the Davy lamp, M. Eloin's lamp possesses, from its structure, some peculiarities that render it much safer. The air which enters through the narrow breadth of wire gauze, below the flame, being only such as is necessary to support the flame of the wick, and the combustion being of so perfect a character, that portion of the cylinder which is above the flame must always be filled with the products of combustion, and never with an explosive atmosphere. This is clearly seen by the flame being extinguished whenever the general upward current is by any means reversed.

The weight of the lamp (always an important consideration where it has to be carried for any length of time in the hand) is not at all objectionable; and its cost in Belgium does not exceed 7 francs.

A conical brass shade is attached to, and made to slide upon, the rods surrounding the glass portion of the cylinder, by which the light can be directed downwards if wished, so as to throw the light over the floor of the mine.—Newton's *London Journal*, February 1852.

METEOROLOGICAL OBSERVATIONS FOR JAN. 1852.

Chiswick.—January 1. Hazy: overcast. 2. Overcast: fine: slight rain. 3. Foggy: very fine: cloudy: boisterous at night. 4. Clear and very fine: frosty. 5. Frosty: clear and fine. 6. Clear: very fine. 7. Rain. 8. Cloudy: boisterous. 9. Quite clear: overcast. 10. Frosty: clear and fine: rain. 11. Rain: overcast. 12. Constant rain. 13. Foggy, with rain. 14. Foggy: rain. 15. Cloudy. 16. Densely overcast: fine. 17. Very fine. 18. Hoar frost: very fine. 19. Frosty: very fine. 20. Densely clouded. 21. Fine: rain at night. 22, 23. Clear and very fine. 24. Rain. 25, 26. Very fine. 27. Fine: rain. 28. Foggy: fine: clear: frosty at night. 29. Foggy and frosty: very fine. 30. Rain: heavy clouds: clear. 31. Densely overcast: rain.

Mean temperature of the month	39°·66
Mean temperature of Jan. 1851	40 ·07
Mean temperature of Jan. for the last twenty-six years ...	36 ·79
Average amount of rain in Jan.	1·68 inch.

Boston.—Jan. 1, 2. Cloudy. 3. Fine. 4. Fine: hail-storm early A.M. 5, 6. Fine. 7. Cloudy: rain P.M. 8. Cloudy. 9. Cloudy: rain A.M. 10. Fine. 11. Fine: rain early A.M. 12. Cloudy: rain early A.M. 13. Cloudy: rain P.M. 14. Cloudy. 15. Cloudy: rain A.M. and P.M. 16—19. Fine. 20. Cloudy: rain P.M. 21. Fine: rain P.M. 22. Fine: rain early A.M. 23. Fine. 24. Cloudy: rain A.M. and P.M. 25, 26. Fine. 27. Cloudy: rain P.M. 28. Fine. 29. Foggy. 30. Rainy: rain A.M. and P.M. 31. Cloudy: rain A.M. and P.M.

Sandwick Manse, Orkney.—Jan. 1. Rain. 2. Showers: sleet-showers. 3. Cloudy: sleet-showers. 4. Snow-showers. 5. Rain: cloudy. 6. Rain: showers. 7. Showers: snow-showers. 8. Frost: cloudy. 9. Showers: snow-showers. 10. Snow-showers: cloudy. 11. Snow-showers: clear: aurora. 12. Cloudy: showers. 13. Showers: clear: aurora. 14, 15. Bright: cloudy. 16. Showers: cloudy. 17. Showers. 18. Showers: damp. 19. Bright: clear: aurora. 20, 21. Cloudy: rain. 22. Cloudy: showers: thunder and lightning. 23, 24. Showers: clear: aurora. 25. Sleet-showers: aurora. 26. Drops: cloudy. 27. Hazy: cloudy. 28. Fine: clear: large halo. 29. Drizzle: showers. 30. Showers: sleet-showers. 31. Showers: thunder and lightning: showers.

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

Days of Month.	Barometer.					Thermometer.					Wind.			Rain.		
	Chiswick.		Boston. 8¾ a.m.	Orkney, Sandwick.		Chiswick.		Boston. 8¾ a.m.	Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Orkney, Sandwick.	Chiswick.	Boston.	Orkney, Sandwick.
1852. Jan.	Max.	Min.		9¼ a.m.	8½ p.m.	Max.	Min.		9¼ a.m.	8½ p.m.						
1.	29·956	29·784	29·61	29·66	29·30	35	21	37	44	43	n.	sw.	w.	......	......	·15
2.	29·694	29·637	29·36	29·03	29·20	41	26	36	42½	40	sw.	wsw.	w.	·02	......	·24
3.	29·746	29·512	29·43	29·24	28·84	49	38	30	35½	37½	sw.	s.	w.	·10	......	·15
4.	30·122	29·539	29·30	29·28	29·72	44	20	42·5	32½	35	nw.	wnw.	w.	......	·18	·36
5.	30·198	30·064	29·86	29·53	29·26	45	29	32	37	48½	sw.	s.	w.	......	......	·65
6.	29·919	29·787	29·60	29·17	28·98	49	35	33·5	49	49	s.	s.	s.	......	......	·59
○ 7.	29·805	29·508	29·22	28·80	29·40	49	27	42	39	31	sw.	w.	nw.	·06	......	·36
8.	29·863	29·181	29·50	29·45	29·02	49	32	35	33	31	sw.	s.	n.	......	·05	......
9.	29·460	29·148	28·72	28·90	29·14	39	26	35·5	33	30	sw.	w.	n.	......	......	......
10.	29·714	29·663	29·40	29·50	29·52	50	25	28	30	30	w.	w.	e.	·08	·25	......
11.	29·210	29·022	28·70	28·89	28·80	51	43	45	34	34	w.	sw.	ese.	·07	·16	·28
12.	29·326	29·151	28·92	28·58	28·90	54	36	43	38	39	sw.	sw.	nw.	·62	·07	·22
13.	29·538	29·443	29·28	29·39	29·57	50	39	38	39	38	e.	s.	w.	·24	......	·02
☾ 14.	29·739	29·701	29·42	29·64	29·43	51	39	42	34	41	sw.	se.	s.	·16	·26	·02
15.	29·602	29·455	29·15	29·14	29·18	56	44	45	44	43	sw.	s.	se.	·26	·12	·08
16.	29·840	29·703	29·08	29·18	29·37	52	37	44	42	43½	sw.	w.	w.	·07	·35	·05
17.	30·241	29·951	29·56	29·57	29·84	47	24	39	43	43	w.	w.	w.	......	......	·26
18.	30·326	30·276	29·90	29·86	29·82	44	22	35	47	45	sw.	sw.	sw.	......	......	·09
19.	30·141	29·980	29·79	29·66	29·58	47	28	35	45	44	s.	s.	s.	......	......	·11
20.	29·769	29·695	29·36	29·41	29·42	49	28	43·5	45	45	s.	s.	w.	·03	......	......
● 21.	29·967	29·377	29·52	29·32	28·58	53	38	34	40	43	sw.	ssw.	sse.	·26	·10	·54
22.	29·396	29·263	28·80	28·38	28·35	47	31	39	40	44	sw.	sw.	ssw.	·01	·18	·09
23.	29·829	29·423	29·10	28·78	29·22	46	29	35	46	43½	sw.	wsw.	w.	·01	......	·08
24.	29·831	29·559	29·38	28·67	28·86	48	34	37	43	41½	sw.	s.	sw.	·10	......	·11
25.	29·945	29·775	29·38	29·19	29·33	50	27	34·5	40	39½	sw.	sw.	sw.	·06	·18	·07
26.	30·073	29·903	29·64	29·48	29·38	51	41	33·5	42	42½	sw.	ssw.	sse.	......	......	·10
27.	29·588	29·467	29·20	29·20	29·32	46	29	43	45	43	s.	ssw.	s.	·28	......	......
28.	29·883	29·730	29·40	29·70	29·74	45	21	33	41	39	nw.	wnw.	s.	......	·07	·08
☽ 29.	30·090	29·924	29·70	29·32	29·44	46	34	31	47	42	sw.	s.	sw.	·04	......	·14
30.	29·979	29·624	29·16	28·88	29·24	53	29	46	41½	39	w.	sw.	w.	·11	·20	·19
31.	29·919	29·510	29·53	29·41	29·30	52	39	35	39	40	s.	w.	s.	·14	·02	·20
Mean.	29·829	29·605	29·35	29·232	29·259	48·00	31·32	37·5	40·35	40·24				2·72	2·19	5·23

THE

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[FOURTH SERIES.]

APRIL 1852.

XXXVI. *Contributions to the Physiology of Vision.—Part the First. On some remarkable, and hitherto unobserved, Phænomena of Binocular Vision. By* CHARLES WHEATSTONE, *F.R.S., Professor of Experimental Philosophy in King's College, London*.*

[With Two Plates.]

§ 1.

WHEN an object is viewed at so great a distance that the optic axes of both eyes are sensibly parallel when directed towards it, the perspective projections of it, seen by each eye separately, are similar, and the appearance to the two eyes is precisely the same as when the object is seen by one eye only. There is in such case no difference between the visual appearance of an object in relief, and its perspective projection on a plane surface; and hence pictorial representations of distant objects, when those circumstances which would prevent or disturb the illusion are carefully excluded, may be rendered such perfect resemblances of the objects they are intended to represent as to be mistaken for them; the Diorama is an instance of this. But this similarity no longer exists when the object is placed so near the eyes that to view it the optic axes must converge; under these conditions a different perspective projection of it is seen by each eye, and these perspectives are more dissimilar as the convergence of the optic axes becomes greater. This fact may be easily verified by placing any figure of three dimensions, an outline cube, for instance, at a moderate distance before the eyes, and while the head is kept perfectly steady, viewing it with

* From the Philosophical Transactions for 1838, part ii.; having been received and read by the Royal Society June 21, 1838.

each eye successively while the other is closed. Plate VIII. fig. 13 represents the two perspective projections of a cube; *b* is that seen by the right eye, and *a* that presented to the left eye; the figure being supposed to be placed about seven inches immediately before the spectator.

The appearances, which are by this simple experiment rendered so obvious, may be easily inferred from the established laws of perspective; for the same object in relief is, when viewed by a different eye, seen from two points of sight at a distance from each other equal to the line joining the two eyes. Yet they seem to have escaped the attention of every philosopher and artist who has treated of the subjects of vision and perspective. I can ascribe this inattention to a phænomenon leading to the important and curious consequences, which will form the subject of the present communication, only to this circumstance; that the results being contrary to a principle which was very generally maintained by optical writers, viz. that objects can be seen single only when their images fall on corresponding points of the two retinæ, an hypothesis which will be hereafter discussed, if the consideration ever arose in their minds, it was hastily discarded under the conviction, that if the pictures presented to the two eyes are under certain circumstances dissimilar, their differences must be so small that they need not be taken into account.

It will now be obvious why it is impossible for the artist to give a faithful representation of any near solid object, that is, to produce a painting which shall not be distinguished in the mind from the object itself. When the painting and the object are seen with both eyes, in the case of the painting two *similar* pictures are projected on the retinæ, in the case of the solid object the pictures are *dissimilar*; there is therefore an essential difference between the impressions on the organs of sensation in the two cases, and consequently between the perceptions formed in the mind; the painting therefore cannot be confounded with the solid object.

After looking over the works of many authors who might be expected to have made some remarks relating to this subject, I have been able to find but one, which is in the *Trattato della Pittura* of Leonardo da Vinci*. This great artist and ingenious philosopher observes, "that a painting, though conducted with the greatest art and finished to the last perfection, both with regard to its contours, its lights, its shadows and its colours, can never show a relievo equal to that of the natural objects, unless these be viewed at a distance and with a single eye." "For," says he, "if an object C [Plate VII. fig. 1] be viewed by a single eye at

* See also a Treatise of Painting, p. 178. London, 1721; and Dr. Smith's Complete System of Optics, vol. ii. r. 244, where the passage is quoted.

A, all objects in the space behind it, included as it were in a shadow ECF cast by a candle at A, are invisible to the eye at A; but when the other eye at B is opened, part of these objects become visible to it; those only being hid from both eyes that are included, as it were, in the double shadow CD, cast by two lights at A and B, and terminated in D, the angular space EDG beyond D being always visible to both eyes. And the hidden space CD is so much the shorter, as the object C is smaller and nearer to the eyes. Thus the object C seen with both eyes becomes, as it were, transparent, according to the usual definition of a transparent thing; namely, that which hides nothing beyond it. But this cannot happen when an object, whose breadth is bigger than that of the pupil, is viewed by a single eye. The truth of this observation is therefore evident, because a painted figure intercepts all the space behind its apparent place, so as to preclude the eyes from the sight of every part of the imaginary ground behind it."

Had Leonardo da Vinci taken, instead of a sphere, a less simple figure for the purpose of his illustration, a cube, for instance, he would not only have have observed that the object obscured from each eye a different part of the more distant field of view, but the fact would also perhaps have forced itself upon his attention, that the object itself presented a different appearance to each eye. He failed to do this, and no subsequent writer within my knowledge has supplied the omission; that two obviously dissimilar pictures are projected on the two retinæ when a single object is viewed, while the optic axes converge, must therefore be regarded as a new fact in the theory of vision.

§ 2.

It being thus established that the mind perceives an object of three dimensions by means of the two dissimilar pictures projected by it on the two retinæ, the following question occurs: What would be the visual effect of simultaneously presenting to each eye, instead of the object itself, its projection on a plane surface as it appears to that eye? To pursue this inquiry, it is necessary that means should be contrived to make the two pictures, which must necessarily occupy different places, fall on similar parts of both retinæ. Under the ordinary circumstances of vision, the object is seen at the concourse of the optic axes, and its images consequently are projected on similar parts of the two retinæ; but it is also evident that two exactly similar objects may be made to fall on similar parts of the two retinæ, if they are placed one in the direction of each optic axis, at equal distances before or beyond their intersection.

Fig. 2 represents the usual situation of an object at the intersection of the optic axes. In fig. 3 the similar objects are placed in the direction of the optic axes before their intersection, and in fig. 4 beyond it. In all these three cases the mind perceives but a single object, and refers it to the place where the optic axes meet. It will be observed, that when the eyes converge beyond the objects, as in fig. 3, the right-hand object is seen by the right eye, and the left-hand object by the left eye; but when the axes converge nearer than the objects, the right-hand object is seen by the left eye, and conversely. As both of these modes of vision are forced and unnatural, eyes unaccustomed to such experiments require some artificial assistance. If the eyes are to converge beyond the objects, this may be afforded by a pair of tubes (fig. 5) capable of being inclined towards each other at various angles, so as to correspond with the different convergences of the optic axes. If the eyes are to converge at a nearer distance than that at which the objects are placed, a box (fig. 6) may be conveniently employed; the objects $a\ a'$ are placed distant from each other, on a stand capable of being moved nearer the eyes if required, and the optic axes being directed towards them will cross at c, the aperture bb' allowing the visual rays from the right-hand object to reach the left eye, and those from the left-hand object to fall on the right eye; the coincidence of the images may be facilitated by placing the point of a needle at the point of intersection of the optic axes c, and fixing the eyes upon it. In both these instruments (figs. 5 and 6) the lateral images are hidden from view, and much less difficulty occurs in making the images unite than when the naked eyes are employed.

Now if, instead of placing two exactly similar objects to be viewed by the eyes in either of the modes above described, the two perspective projections of the same solid object be so disposed, the mind will still perceive the object to be single; but instead of a representation on a plane surface, as each drawing appears to be when separately viewed by that eye which is directed towards it, the observer will perceive a figure of three dimensions, the exact counterpart of the object from which the drawings were made. To make this matter clear, I will mention one or two of the most simple cases.

If two vertical lines near each other, but at different distances from the spectator, be regarded first with one eye and then with the other, the lateral separation between them when referred to the same plane will appear different; if the left-hand line be nearer to the eyes, the separation seen by the left eye will be less than that seen by the right eye; fig. 7 will render this evident; $a\ a'$ are vertical sections of the two original lines, and $b\ b'$ the plane to which their projections are referred. If now the two lines be drawn on two pieces of card, at the respective lateral distances at which they appear to each eye, and these cards be afterwards viewed by either of the means above directed, the observer will no longer see lines on a plane surface, as each card separately shows; but two lines will appear, one nearer to him than the other, precisely as the original vertical lines themselves. Again, if a straight wire be held before the eyes in such a position that one of its ends shall be nearer to the observer than the other is, each eye separately referring it to a plane perpendicular to the common axis, will see a line differently inclined; and then if lines having the same apparent inclinations be drawn on two pieces of card, and be presented to the eyes as before directed, the real position of the original line will be correctly perceived by the mind.

In the same manner the most complex figures of three dimensions may be accurately represented to the mind, by presenting their two perspective projections to the two retinæ. But I shall defer these more perfect experiments until I describe an instrument which will enable any person to observe all the phænomena in question with the greatest ease and certainty.

In the instruments above described, the optic axes converge to some point in a plane before or beyond that in which the objects to be seen are situated. The adaptation of the eye, which enables us to see distinctly at different distances, and which habitually accompanies every different degree of convergence of the optic axes, does not immediately adjust itself to the new and unusual condition; and to persons not accustomed to experiments of this kind, the pictures will either not readily unite, or will appear dim and confused. Besides this, no object can be viewed according to either mode when the drawings exceed in breadth the distance of the two points of the optic axes in which their centres are placed.

These inconveniences are removed by the instrument I am about to describe; the two pictures (or rather their reflected images) are placed in it at the true concourse of the optic axes, the focal adaptation of the eye preserves its usual adjustment, the appearance of lateral images is entirely avoided, and a large field of view for each eye is obtained. The frequent reference I shall have occasion to make to this instrument will render it convenient to give it a specific name; I therefore propose that it be called a stereoscope, to indicate its property of representing solid figures.

§ 3.

The stereoscope is represented by figs. 8 and 9; the former being a front view, and the latter a plan of the instrument.

A A′ are two plane mirrors, about four inches square, inserted in frames, and so adjusted that their backs form an angle of 90° with each other; these mirrors are fixed by their common edge against an upright B, or which was less easy to represent in the drawing, against the middle line of a vertical board, cut away in such manner as to allow the eyes to be placed before the two mirrors. C C′ are two sliding boards, to which are attached the upright boards D D′, which may thus be removed to different distances from the mirrors. In most of the experiments hereafter to be detailed, it is necessary that each upright board shall be at the same distance from the mirror which is opposite to it. To facilitate this double adjustment, I employ a right and a left-handed wooden screw, *r l*; the two ends of this compound screw pass through the nuts *e e′*, which are fixed to the lower parts of the upright boards D D′, so that by turning the screw pin *p* one way the two boards will approach, and by turning it the other they will recede from each other, one always preserving the same distance as the other from the middle line *f*. E E′ are pannels, to which the pictures are fixed in such manner that their corresponding horizontal lines shall be on the same level: these pannels are capable of sliding backwards and forwards in grooves on the upright boards D D′. The apparatus having been described, it now remains to explain the manner of using it. The observer must place his eyes as near as possible to the mirrors, the right eye before the right-hand mirror, and the left eye before the left-hand mirror, and he must move the sliding pannels E E′ to or from him until the two reflected images coincide at the intersection of the optic axes, and form an image of the same apparent magnitude as each of the component pictures. The pictures will indeed coincide when the sliding pannels are in a variety of different positions, and consequently when viewed under different inclinations of the optic axes; but there is only one position in which the binocular image will be immediately seen single, of its proper magnitude, and without fatigue to the eyes, because in this position only the ordinary relations between the magnitude of the pictures on the retina, the inclination of the optic axes, and the adaptation of the eye to distinct vision at different distances are preserved. The alteration in the apparent magnitude of the binocular images, when these usual relations are disturbed, will be discussed in another paper of this series, with a variety of remarkable phænomena depending thereon. In all the experiments detailed in the present memoir I shall suppose these relations to remain undisturbed, and the optic axes to converge about six or eight inches before the eyes.

If the pictures are all drawn to be seen with the same inclination of the optic axes, the apparatus may be simplified by omitting the screw $r\,l$ and fixing the upright boards D D′ at the proper distances. The sliding pannels may also be dispensed with, and the drawings themselves be made to slide in the grooves.

§ 4.

A few pairs of outline figures, calculated to give rise to the perception of objects of three dimensions when placed in the stereoscope in the manner described, are represented in Pl. VIII. figs. 10 to 20. They are one half the linear size of the figures actually employed. As the drawings are reversed by reflexion in the mirrors, I will suppose these figures to be the reflected images to which the eyes are directed in the apparatus; those marked *b* being seen by the right eye, and those marked *a* by the left eye. The drawings, it has been already explained, are two different projections of the same object seen from two points of sight, the distance between which is equal to the interval between the eyes of the observer; this interval is generally about $2\frac{1}{2}$ inches.

a and *b*, fig. 10, will, when viewed in the stereoscope, present to the mind a line in the vertical plane, with its lower end inclined towards the observer. If the two component lines be caused to turn round their centres equally in opposite directions, the resultant line will, while it appears to assume every degree of inclination to the referent plane, still seem to remain in the same vertical plane.

Fig. 11. A series of points all in the same horizontal plane, but each towards the right hand successively nearer the observer.

Fig. 12. A curved line intersecting the referent plane, and having its convexity towards the observer.

Fig. 13. A cube.

Fig. 14. A cone, having its axis perpendicular to the referent plane, and its vertex towards the observer.

Fig. 15. The frustum of a square pyramid; its axis perpendicular to the referent plane, and its base furthest from the eye.

Fig. 16. Two circles at different distances from the eyes, their centres in the same perpendicular, forming the outline of the frustum of a cone.

The other figures require no observation.

For the purposes of illustration I have employed only outline figures, for had either shading or colouring been introduced it might be supposed that the effect was wholly or in part due to these circumstances, whereas by leaving them out of consideration no room is left to doubt that the entire effect of relief is owing to the simultaneous perception of the two monocular projections, one on each retina. But if it be required to obtain the most faithful resemblances of real objects, shadowing and colouring may properly be employed to heighten the effects. Careful attention would enable an artist to draw and paint the two component pictures, so as to present to the mind of the observer, in the resultant perception, perfect identity with the object represented. Flowers, crystals, busts, vases, instruments of various kinds, &c., might thus be represented so as not to be distinguished by sight from the real objects themselves.

It is worthy of remark, that the process by which we thus become acquainted with the real forms of solid objects, is precisely that which is employed in descriptive geometry, an important science we owe to the genius of Monge, but which is little studied or known in this country. In this science, the position of a point, a right line or a curve, and consequently of any figure whatever, is completely determined by assigning its projections on two fixed planes, the situations of which are known, and which are not parallel to each other. In the problems of descriptive geometry the two referent planes are generally assumed to be at right angles to each other, but in binocular vision the inclination of these planes is less according as the angle made at the concourse of the optic axes is less; thus the same solid object is represented to the mind by different pairs of monocular pictures, according as they are placed at a different distance before the eyes, and the perception of these differences (though we seem to be unconscious of them) may assist in suggesting to the mind the distance of the object. The more inclined to each other the referent planes are, with the greater accuracy are the various points of the projections referred to their proper places; and it appears to be a useful provision that the real forms of those objects which are nearest to us are thus more determinately apprehended than those which are more distant.

§ 5.

A very singular effect is produced when the drawing originally intended to be seen by the right eye is placed at the left hand side of the stereoscope, and that designed to be seen by the left eye is placed on its right hand side. A figure of three dimensions, as bold in relief as before, is perceived, but it has a different form from that which is seen when the drawings are in their proper places. There is a certain relation between the proper figure and this, which I shall call its *converse* figure. Those points which appear nearest the observer in the proper figure seem the most remote from him in the converse figure, and *vice versâ,* so that the figure is, as it were, inverted; but it is not an exact inversion, for the near parts of the converse figure appear smaller, and the remote parts larger than the same parts before the inversion. Hence the drawings which, properly placed, occasion a cube to be perceived, when changed in the manner described, represent the frustum of a square pyramid with its base remote from the eye: the cause of this is easy to understand.

This conversion of relief may be shown by all the pairs of drawings from fig. 10 to 19. In the case of simple figures like these the converse figure is as readily apprehended as the original one, because it is generally a figure of as frequent occurrence; but in the case of a more complicated figure, an architectural design, for instance, the mind, unaccustomed to perceive its converse, because it never occurs in nature, can find no meaning in it.

§ 6.

The same image is depicted on the retina by an object of three dimensions as by its projection on a plane surface, provided the point of sight remain in both cases the same. There should be, therefore, no difference in the binocular appearance of two drawings, one presented to each eye, and of two real objects so presented to the two eyes that their projections on the retina shall be the same as those arising from the drawings. The following experiments will prove the justness of this inference.

I procured several pairs of skeleton figures, *i. e.* outline figures of three dimensions, formed either of iron wire or of ebony beading about one tenth of an inch in thickness. The pair I most frequently employed consisted of two cubes, whose sides were three inches in length. When I placed these skeleton figures on stands before the two mirrors of the stereoscope, the following effects were produced, according as their relative positions were changed. 1st. When they were so placed that the pictures which their reflected images projected on the two retinæ were precisely the same as those which would have been projected by a cube placed at the concourse of the optic axes, a cube in relief appeared before the eyes. 2ndly. When they were so placed that their reflected images projected exactly similar pictures on the two retinæ, all effect of relief was destroyed, and the compound appearance was that of an outline representation on a plane surface. 3rdly. When the cubes were so placed that the reflected image of one projected on the left retina the same picture as in the first case was projected on the right retina, and conversely, the converse figure in relief appeared.

§ 7.

If a symmetrical object, that is one whose right and left sides are exactly similar to each other but inverted, be placed so that any point in the plane which divides it into these two halves is equally distant from the two eyes, its two monocular projections are, it is easy to see, inverted fac-similes of each other. Thus fig. 15, *a* and *b* are symmetrical monocular projections of the frustum of a four-sided pyramid, and figs. 13, 14, 16, are corresponding projections of other symmetrical objects. This being kept in view, I will describe an experiment which, had it been casually observed previous to the knowledge of the principles developed in this paper, would have appeared an inexplicable optical illusion.

M and M′ (fig. 21) are two mirrors, inclined so that their *faces* form an angle of 90° with each other. Between them in the bisecting plane is placed a plane outline figure, such as fig. 15 *a*, made of card all parts but the lines being cut away, or of wire. A reflected image of this outline, placed at A, will appear behind each mirror at B and B′, and one of these images will be the inversion of the other. If the eyes be made to converge at C, it is obvious that these two reflected images will fall on corresponding parts of the two retinæ, and a figure of three dimensions will be perceived; if the outline placed in the bisecting plane be reversed, the converse skeleton form will appear; in both these experiments we have the singular phænomenon of the conversion of a single plane outline into a figure of three dimensions. To render the binocular object more distinct, concave lenses may be applied to the eyes; and to prevent the two lateral images from being seen, screens may be placed at D and D′.

§ 8.

An effect of binocular perspective may be remarked in a plate of metal, the surface of which has been made smooth by turning it in a lathe. When a single candle is brought near such a plate, a line of light appears standing out from it, one half being above, and the other half below the surface; the position and inclination of this line changes with the situation of the light and of the observer, but it always passes through the centre of the plate. On closing the left eye the relief disappears, and the luminous line coincides with one of the diameters of the plate; on closing the right eye the line appears equally in the plane of the surface, but coincides with another diameter; on opening both eyes it instantly starts into relief*. The case here is exactly analogous to the vision of two inclined lines (fig. 10) when each is presented to a different eye in the stereoscope. It is curious, that an effect like this, which must have been seen thousands of times, should never have

* The luminous line seen by a single eye arises from the reflexion of the light from each of the concentric circles produced in the operation of turning; when the plate is not large the arrangement of these successive reflexions does not differ from a straight line.

attracted sufficient attention to have been made the subject of philosophic observation. It was one of the earliest facts which drew my attention to the subject I am now treating.

Dr. Smith * was very much puzzled by an effect of binocular perspective which he observed, but was unable to explain. He opened a pair of compasses, and while he held the joint in his hand, and the points outwards and equidistant from his eyes, and somewhat higher than the joint, he looked at a more distant point; the compasses appeared double. He then compressed the legs until the two inner points coincided; having done this the two inner legs also entirely coincided, and bisected the angle formed by the outward ones, appearing longer and thicker than they did, and reaching from the hand to the remotest object in view. The explanation offered by Dr. Smith accounts only for the coincidence of the points of the compasses, not for that of the entire leg. The effect in question is best seen by employing a pair of straight wires, about a foot in length. A similar observation, made with two flat rulers, and afterwards with silk threads, induced Dr. Wells to propose a new theory of visible direction in order to explain it, so inexplicable did it seem to him by any of the received theories.

§ 9.

The preceding experiments render it evident that there is an essential difference in the appearance of objects when seen with two eyes, and when only one eye is employed, and that the most vivid belief of the solidity of an object of three dimensions arises from two different perspective projections of it being simultaneously presented to the mind. How happens it then, it may be asked, that persons who see with only one eye form correct notions of solid objects, and never mistake them for pictures? and how happens it also, that a person having the perfect use of both eyes, perceives no difference in objects around him when he shuts one of them? To explain these apparent difficulties, it must be kept in mind, that although the simultaneous vision of two dissimilar pictures suggests the relief of objects in the most vivid manner, yet there are other signs which suggest the same ideas to the mind, which, though more ambiguous than the former, become less liable to lead the judgement astray in proportion to the extent of our previous experience. The vividness of relief arising from the projection of two dissimilar pictures, one on each retina, becomes less and less as the object is seen at a greater distance before the eyes, and entirely ceases when it is so distant that the optic axes are parallel while regarding it. We see with both eyes all objects beyond this distance precisely

* System of Optics, vol. ii. p. 388. and r. 526.

as we see near objects with a single eye; for the pictures on the two retinæ are then exactly similar, and the mind appreciates no difference whether two identical pictures fall on corresponding parts of the two retinæ, or whether one eye is impressed with only one of these pictures. A person deprived of the sight of one eye sees therefore all external objects near and remote, as a person with both eyes sees remote objects only, but that vivid effect arising from the binocular vision of near objects is not perceived by the former; to supply this deficiency he has recourse unconsciously to other means of acquiring more accurate information. The motion of the head is the principal means he employs. That the required knowledge may be thus obtained will be evident from the following considerations. The mind associates with the idea of a solid object every different projection of it which experience has hitherto afforded; a single projection may be ambiguous, from its being also one of the projections of a picture, or of a different solid object; but when different projections of the same object are successively presented, they cannot all belong to another object, and the form to which they belong is completely characterized. While the object remains fixed, at every movement of the head it is viewed from a different point of sight, and the picture on the retina consequently continually changes.

Every one must be aware how greatly the perspective effect of a picture is enhanced by looking at it with only one eye, especially when a tube is employed to exclude the vision of adjacent objects, whose presence might disturb the illusion. Seen under such circumstances from the proper point of sight, the picture projects the same lines, shades and colours on the retina, as the more distant scene which it represents would do were it substituted for it. The appearance which would make us certain that it is a picture is excluded from the sight, and the imagination has room to be active. Several of the older writers erroneously attributed this apparent superiority of monocular vision to the concentration of the visual power in a single eye*.

There is a well-known and very striking illusion of perspective which deserves a passing remark, because the reason of the effect does not appear to be generally understood. When a perspective of a building is projected on a horizontal plane, so that the point of sight is in a line greatly inclined towards the plane, the building appears to a single eye placed at the point of sight, to be in bold relief, and the illusion is almost as perfect as in the binocular experiments described in §§ 2, 3, 4. This effect wholly

* "We see more exquisitely with one eye shut than with both, because the vital spirits thus unite themselves the more, and become the stronger: for we may find by looking in a glass whilst we shut one eye, that the pupil of the other dilates."—Lord Bacon's Works, *Sylva Sylvarum*, art. Vision.

arises from the unusual projection, which suggests to the mind more readily the object itself than the drawing of it; for we are accustomed to see real objects in almost every point of view, but perspective representations being generally made in a vertical plane with the point of sight in a line perpendicular to the plane of projection, we are less familiar with the appearance of other projections. Any other unusual projection will produce the same effect.

§ 10.

If we look with a single eye at the drawing of a solid geometrical figure, it may be imagined to be the representation of either of two dissimilar solid figures, the figure intended to be represented, or its converse figure (§ 5). If the former is a very usual, and the latter a very unusual figure, the imagination will fix itself on the original without wandering to the converse figure; but if both are of ordinary occurrence, which is generally the case with regard to simple forms, a singular phænomenon takes place; it is perceived at one time distinctly as one of these figures, at another time as the other, and while one figure continues it is not in the power of the will to change it immediately.

The same phænomenon takes place, though less decidedly, when the drawing is seen with both eyes. Many of my readers will call to mind the puzzling effect of some of the diagrams annexed to the problems of the eleventh book of Euclid; which, when they were attentively looked at, changed in an arbitrary manner from one solid figure to another, and would obstinately continue to present the converse figures when the real figures alone were wanted. This perplexing illusion must be of common occurrence, but I have only found one recorded observation relating to the subject. It is by Professor Necker of Geneva, and I shall quote it in his own words from the Philosophical Magazine, Third Series, vol. i. p. 337.

"The object I have now to call your attention to is an observation which has often occurred to me while examining figures and engraved plates of crystalline forms; I mean a sudden and involuntary change in the apparent position of a crystal or solid represented in an engraved figure. What I mean will be more easily understood from the figure annexed (fig. 22). The rhomboid AX is drawn so that the solid angle A should be seen the nearest to the spectator, and the solid angle X the farthest from him, and that the face ACDB should be the foremost, while the face XDC is behind. But in looking repeatedly at the same figure, you will perceive that at times the apparent position of the rhomboid is so changed that the solid angle X will appear the nearest, and the solid angle A the farthest; and that the face ACDB will recede behind the face XDC, which will come forward, which effect gives to the whole solid a quite contrary apparent inclination."

Professor Necker attributes this alteration of appearance, not to a mental operation, but to an involuntary change in the adjustment of the eye for obtaining distinct vision. He supposed that whenever the point of distinct vision on the retina is directed on the angle A, for instance, this angle seen more distinctly than the others, is naturally supposed to be nearer and foremost, while the other angles seen indistinctly are supposed to be further and behind, and that the reverse takes place when the point of distinct vision is brought to bear on the angle X.

That this is not the true explanation, is evident from three circumstances : in the first place, the two points A and X being both at the same distance from the eyes, the same alteration of adjustment which would make one of them indistinct would make the other so ; secondly, the figure will undergo the same changes whether the focal distance of the eye be adjusted to a point before or beyond the plane in which the figure is drawn ; and thirdly, the change of figure frequently occurs while the eye continues to look at the same angle. The effect seems entirely to depend on our mental contemplation of the figure intended to be represented, or of its converse. By following the lines with the eye with a clear idea of the solid figure we are describing, it may be fixed for any length of time ; but it requires practice to do this or to change the figure at will. As I have before observed, these effects are far more obvious when the figures are regarded with one eye only.

No illusion of this kind can take place when an object of three dimensions is seen with both eyes while the optic axes make a sensible angle with each other, because the appearance of the two dissimilar images, one to each eye, prevents the possibility of mistake. But if we regard an object at such a distance that its two projections are sensibly identical, and if this projection be capable of a double interpretation, the illusion may occur. Thus a placard on a pole carried in the streets, with one of its sides inclined towards the observer, will, when he is distant from it, frequently appear inclined in a contrary direction. Many analogous instances might be adduced, but this will suffice to call others to mind ; it must however be observed, that when shadows, or other means capable of determining the judgement are present, these fallacies do not arise.

§ 11.

The same indetermination of judgement which causes a drawing to be perceived by the mind at different times as two different figures, frequently gives rise to a false perception when objects in relief are regarded with a single eye. The apparent conversion of a cameo into an intaglio, and of an intaglio into a cameo, is a well-known instance of this fallacy in vision; but the fact does not appear to me to have been correctly explained, nor the conditions under which it occurs to have been properly stated.

This curious illusion, which has been the subject of much attention, was first observed at one of the early meetings of the Royal Society*. Several of the members looking through a compound microscope of a new construction at a guinea, some of them imagined the image to be depressed, while others thought it to be embossed, as it really was. Professor Gmelin, of Wurtemburg, published a paper on the same subject in the Philosophical Transactions for 1745; his experiments were made with telescopes and compound microscopes which inverted the images; and he observed that the conversion of relief appeared in some cases and not in others, at some times and not at others, and to some eyes also and not to others. He endeavoured to ascertain some of the conditions of the two appearances; "but why these things should so happen," says he, "I do not pretend to determine."

Sir David Brewster accounts for the fallacy in the following manner †:—"A hollow seal being illuminated by a window or a candle, its shaded side is of course on the same side with the light. If we now invert the seal with one or more lenses, so that it may look in the opposite direction, it will appear to the eye with the shaded side furthest from the window. But as we know that the window is still on our left hand, and as every body with its shaded side furthest from the light must necessarily be convex or protuberant, we immediately believe that the hollow seal is now a cameo or bas-relief. The proof which the eye thus receives of the seal being raised, overcomes the evidence of its being hollow, derived from our actual knowledge and from the sense of touch. In this experiment the deception takes place from our knowing the real direction of the light which falls on the seal; for if the place of the window, with respect to the seal, had been inverted as well as the seal itself, the illusion could not have taken place. The illusion, therefore, under our consideration is the result of an operation of our own minds, whereby we judge of the forms of bodies by the knowledge we have acquired of light and shadow. Hence the illusion depends on the accuracy and extent of our knowledge on this subject; and while some persons are under its influence, others are entirely insensible to it."

* Birch's History, vol. ii. p. 348. † Natural Magic, p. 100.

These considerations do not fully explain the phænomenon, for they suppose that the image must be inverted, and that the light must fall in a particular direction; but the conversion of relief will still take place when the object is viewed through an open tube without any lenses to invert it, and also when it is equally illuminated in all parts. The true explanation I believe to be the following. If we suppose a cameo and an intaglio of the same object, the elevations of the one corresponding exactly to the depressions of the other, it is easy to show that the projection of either on the retina is sensibly the same. When the cameo or the intaglio is seen with both eyes, it is impossible to mistake an elevation for a depression, for reasons which have been already amply explained; but when either is seen with one eye only, the most certain guide of our judgement, viz. the presentation of a different picture to each eye, is wanting; the imagination therefore supplies the deficiency, and we conceive the object to be raised or depressed according to the dictates of this faculty. No doubt in such cases our judgement is in a great degree influenced by accessory circumstances, and the intaglio or the relief may sometimes present itself according to our previous knowledge of the direction in which the shadows ought to appear; but the principal cause of the phænomenon is to be found in the indetermination of the judgement arising from our more perfect means of judging being absent.

Observers with the microscope must be particularly on their guard against illusions of this kind. Raspail observes * that the hollow pyramidal arrangement of the crystals of muriate of soda appears, when seen through a microscope, like a striated pyramid in relief. He recommends two modes of correcting the illusion. The first is to bring successively to the focus of the instrument the different parts of the crystal; if the pyramid be in relief, the point will arrive at the focus sooner than the base will; if the pyramid be hollow, the contrary will take place. The second mode is to project a strong light on the pyramid in the field of view of the microscope, and to observe which sides of the crystal are illuminated, taking however the inversion of the image into consideration if a compound microscope be employed.

The inversion of relief is very striking when a skeleton cube is looked at with one eye, and the following singular results may in this case be observed. So long as the mind perceives the cube, however the figure be turned about, its various appearances will be but different representations of the same object, and the same primitive form will be suggested to the mind by all of them: but it is not so if the converse figure fixes the at-

* *Nouveau Système de Chimie Organique,* 2[me] edit. t. i. p. 333.

tention; the series of successive projections cannot then be referred to any figure to which they are all common, and the skeleton figure will appear to be continually undergoing a change of shape.

§ 12.

I have given ample proof that objects whose pictures do not fall on corresponding points of the two retinæ may still appear single. I will now adduce an experiment which proves that similar pictures falling on corresponding points of the two retinæ may appear double and in different places.

Present, in the stereoscope, to the right eye a vertical line, and to the left eye a line inclined some degrees from the perpendicular (fig. 23); the observer will then perceive, as formerly explained, a line, the extremities of which appear at different distances before the eyes. Draw on the left-hand figure a faint vertical line exactly corresponding in position and length to that presented to the right eye, and let the two lines of this left-hand figure intersect each other at their centres. Looking now at these two drawings in the stereoscope, the two strong lines, each seen by a different eye, will coincide, and the resultant perspective line will appear to occupy the same place as before; but the faint line which now falls on a line of the left retina, which corresponds with the line of the right retina on which one of the coinciding strong lines, viz. the vertical one, falls, appears in a different place. The place this faint line apparently occupies is the intersection of that plane of visual directions of the left eye in which it is situated, with the plane of visual directions of the right eye, which contains the strong vertical line.

This experiment affords another proof that there is no necessary physiological connection between the corresponding points of the two retinæ,—a doctrine which has been maintained by so many authors.

§ 13. *Binocular Vision of Images of different Magnitudes.*

We will now inquire what effect results from presenting similar images, differing only in magnitude, to analogous parts of the two retinæ. For this purpose two squares or circles, differing obviously but not extravagantly in size, may be drawn on two separate pieces of paper, and placed in the stereoscope so that the reflected image of each shall be equally distant from the eye by which it is regarded. It will then be seen that, notwithstanding this difference, they coalesce and occasion a single resultant perception. The limit of the difference of size within which the single appearance subsists may be ascertained by employing two images of equal magnitude, and causing one of them to recede from the eye while the other remains at a constant distance; this is effected merely by pulling out the sliding board C (fig. 8) while the other C′ remains fixed, the screw having previously been removed.

Though the single appearance of two images of different size is by this experiment demonstrated, the observer is unable to perceive what difference exists between the apparent magnitude of the binocular image and that of the two monocular images; to determine this point the stereoscope must be dispensed with, and the experiment so arranged that all three shall be simultaneously seen; which may be done in the following manner:—The two drawings being placed side by side on a plane before the eyes, the optic axes must be made to converge to a nearer point as at fig. 4, or to a more distant one as at fig. 3, until the three images are seen at the same time, the binocular image in the middle, and the monocular images at each side. It will thus be seen that the binocular image is apparently intermediate in size between the two monocular ones.

If the pictures be too unequal in magnitude, the binocular coincidence does not take place. It appears that if the inequality of the pictures be greater than the difference which exists between the two projections of the same object when seen in the most oblique position of the eyes (*i.e.* both turned to the extreme right or to the extreme left), ordinarily employed, they do not coalesce. Were it not for the binocular coincidence of two images of different magnitude, objects would appear single only when the optic axes converge immediately forwards; for it is only when the converging visual lines form equal angles with the visual base (the line joining the centres of the two eyes) as at fig. 2, that the two pictures can be of equal magnitude; but when they form different angles with it, as at fig. 24, the distance from the object to each eye is different, and consequently the picture projected on each retina has a different magnitude. If a piece of money be held in the position *a* (Pl. VII. fig. 24), while the optic axes converge to a nearer point *c*, it will appear double, and that seen by the left eye will be evidently smaller than the other.

§ 14. *Phænomena which are observed when pictures, which are neither similar nor the binocular complements of each other, are simultaneously presented to corresponding parts of the two retinæ.*

If we regard one picture with the right eye alone for a considerable length of time it will be constantly perceived; if we look at the other with the left eye alone its effect will be equally permanent; it might therefore be expected that if each of these pictures were presented to its corresponding eye at the same time the two would appear permanently superposed on each other. This, however, contrary to expectation, is not the case.

If *a* and *b* (fig. 25) are each presented at the same time to a different eye, the common border will remain constant, while the letter within it will change alternately from that which would be perceived by the right eye alone to that which would be perceived by the left eye alone. At the moment of change the letter which has just been seen breaks into fragments, while fragments of the letter which is about to appear mingle with them, and are immediately after replaced by the entire letter. It does not appear to be in the power of the will to determine the appearance of either of the letters, but the duration of the appearance seems to depend on causes which are under our control: thus if the two pictures be equally illuminated, the alternations appear in general of equal duration; but if one picture be more illuminated than the other, that which is less so will be perceived during a shorter time. I have generally made this experiment with the apparatus, fig. 6. When complex pictures are employed in the stereoscope, various parts of them alternate differently.

There are some facts intimately connected with the subject of the present article which have already been frequently observed. I allude to the experiments, first made by Du Tour, in which two different colours are presented to corresponding parts of the two retinæ. If a blue disc be presented to the right eye and a yellow disc to the corresponding part of the left eye, instead of a green disc which would appear if these two colours had mingled before their arrival at a single eye, the mind will perceive the two colours distinctly, one or the other alternately predominating either partially or wholly over the disc. In the same manner the mind perceives no trace of violet when red is presented to one eye and blue to the other, nor any vestige of orange when red and yellow are separately presented in a similar manner. These experiments may be conveniently repeated by placing the coloured discs in the stereoscope, but they have been most usually made by looking at a white object through differently coloured glasses, one applied to each eye.

In some authors we find it stated, contrary to fact, that if similar objects of different colour be presented one to each eye, the appearance will be that compounded of the two colours. Dr. Reid* and Janin are among the writers who have fallen into this error.

* Enquiry, Sect. xiii.

§ 15.

No question relating to vision has been so much debated as the cause of the single appearance of objects seen by both eyes. I shall in the present section give a slight review of the various theories which have been advanced by philosophers to account for this phænomenon, in order that the remarks I have to make in the succeeding section may be properly understood.

The law of visible direction for monocular vision has been variously stated by different optical writers. Some have maintained with Drs. Reid and Porterfield, that every external point is seen in the direction of a line passing from its picture on the retina through the centre of the eye; while others have supposed with Dr. Smith that the visible direction of an object coincides with the visual ray, or the principal ray of the pencil which flows from it to the eye. D'Alembert, furnished with imperfect data respecting the refractive densities of the humours of the eye, calculated that the apparent magnitudes of objects would differ widely on the two suppositions, and concluded that the visible point of an object was not seen in either of these directions, but sensibly in the direction of a line joining the point itself and its image on the retina; but he acknowledged that he could assign no reason for this law. Sir David Brewster, provided with more accurate data, has shown that these three lines so nearly coincide with each other, that "at an inclination of 30°, a line perpendicular to the point of impression on the retina passes through the common centre, and does not deviate from the real line of visible direction more than half a degree, a quantity too small to interfere with the purposes of vision." We may, therefore, assume in all our future reasonings the truth of the following definition given by this eminent philosopher:—"As the interior eye-ball is as nearly as possible a perfect sphere, lines perpendicular to the surface of the retina must all pass through one single point, namely the centre of its spherical surface. This one point may be called the centre of visible direction, because every point of a visible object will be seen in the direction of a line drawn from this centre to the visible point."

It is obvious, that the result of any attempt to explain the single appearance of objects to both eyes, or, in other words, the law of visible position for binocular vision, ought to contain nothing inconsistent with the law of visible direction for monocular vision.

It was the opinion of Aguilonius, that all objects seen at the same glance with both eyes appear to be in the plane of the horopter. The horopter he defines to be a line drawn through the point of intersection of the optic axes, and parallel to the line joining the centres of the two eyes; the plane of the horopter to be a plane passing through this line at right angles to that of the optic axes. All objects which are in this plane, must, according to him, appear single because the lines of direction in which any point of an object is seen coincide only in this plane and nowhere else; and as these lines can meet each other only in one point, it follows from the hypothesis, that all objects not in the plane of the horopter must appear double, because their lines of direction intersect each other, either before or after they pass through it. This opinion was also maintained by Dechales and Porterfield. That it is erroneous, I have given, I think, sufficient proof, in showing that, when the optic axes converge to any point, objects before or beyond the plane of the horopter are under certain circumstances equally seen single as those in that plane.

Dr. Wells's "new theory of visible direction" was a modification of the preceding hypothesis. This acute writer held with Aguilonius, that objects are seen single only when they are in the plane of the horopter, and consequently that they appear double when they are either before or beyond it; but he attempted to make this single appearance of objects only in the plane of the horopter to depend on other principles, from which he deduced, contrary to Aguilonius, that the objects which are doubled do not appear in the plane of the horopter, but in other places which are determined by these principles. Dr. Wells was led to his new theory by a fact which he accidentally observed, and which he could not reconcile with any existing theory of visible direction; this fact had, though he was unaware of it, been previously noticed by Dr. Smith; it is already mentioned in § 8, and is the only other instance of binocular vision of relief which I have found recorded previous to my own investigations. So little does Dr. Wells's theory appear to have been understood, that no subsequent writer has attempted either to confirm or disprove his opinions. It would be useless here to discuss the principles of this theory, which was framed to account for an anomalous individual fact, since it is inconsistent with the general rules on which that fact has been now shown to depend. Notwithstanding these erroneous views, the "essay upon single vision with two eyes" contains many valuable experiments and remarks, the truth of which are independent of the theory they were intended to illustrate.

The theory which has obtained greatest currency is that which assumes that an object is seen single because its pictures fall on corresponding points of the two retinæ, that is on points which are similarly situated with respect to the two centres both in distance and position. This theory supposes that the pictures projected on the retinæ are exactly similar to each other, corresponding points of the two pictures falling on corresponding points of the two retinæ. Authors who agree with regard to this property, differ widely in explaining why objects are seen in the same place, or single, according to this law. Dr. Smith makes it to depend entirely on custom, and explains why the eyes are habitually directed towards an object so that its pictures fall on corresponding parts in the following manner:—"When we view an object steadily, we have acquired a habit of directing the optic axes to the point in view; because its pictures falling upon the middle points of the retinas, are then distincter than if they fell upon any other places; and since the pictures of the whole object are equal to one another, and are both inverted with respect to the optic axes, it follows that the pictures of any collateral point are painted upon corresponding points of the retinas."

Dr. Reid, after a long dissertation on the subject, concludes, "that by an original property of human eyes, objects painted upon the centres of the two retinæ, or upon points similarly situated with regard to the centres, appear in the same visible place; that the most plausible attempts to account for this property of the eyes have been unsuccessful; and therefore, that it must be either a primary law of our constitution, or the consequence of some more general law which is not yet discovered."

Other writers who have admitted this principle have regarded it as arising from anatomical structure and dependent on connexion of nervous fibres; among these stand the names of Galen, Dr. Briggs, Sir Isaac Newton, Rohault, Dr. Hartley, Dr. Wollaston and Professor Müller.

Many of the supporters of the theory of corresponding points have thought, or rather have admitted, *without thinking*, that it was not inconsistent with the law of Aguilonius; but very little reflection will show that both cannot be maintained together; for corresponding lines of visible direction, that is, lines terminating in corresponding points of the two retinæ, cannot all at the same time meet in the plane of the horopter unless the optic axes be parallel, and the plane be at an infinite distance before the eyes. Some of the modern German writers* have inquired what is the curve in which objects appear single while the optic axes are directed to a given point, on the hypothesis that objects are seen single only when they fall on corresponding points of the two retinæ. An elegant proposition has resulted from their investigations, which I shall need no apology for introducing in this place, since it has not yet been mentioned in any English work.

* Tortual, *die Sinne des Menschen.* Münster, 1827. Bartels, *Beitrage zur Physiologie der Gesichtssines.* Berlin, 1834.

R and L (fig. 26) are the two eyes; CA, C′A the optic axes converging to the point A; and CABC′ is a circle drawn through the point of convergence A and the centres of visible direction CC′. If any point be taken in the circumference of this circle, and lines be drawn from it through the centres of the two eyes CC′, these lines will fall on corresponding points of the two retinæ DD′; for the angles ACB, AC′B being equal, the angles DCE, DC′E are also equal; therefore any point placed in the circumference of the circle CABC′ will, according to the hypothesis, appear single while the optic axes are directed to A, or to any other point in it.

I will mention two other properties of this binocular circle: 1st. The arc subtended by two points on its circumference contains double the number of degrees of the arc subtended by the pictures of these points on either retina, so that objects which occupy 180° of the supposed circle of single vision are painted on a portion of the retina extended over 90° only; for the angle DCE or DC′E being at the centre, and the angle BCA or BC′A at the circumference of a circle, this consequence follows. 2ndly. To whatever point of the circumference of the circle the optic axes be made to converge, they will form the same angle with each other; for the angles CAC′ CBC are equal.

In the eye itself, the centre of visible direction, or the point at which the principal rays cross each other, is, according to Dr. Young and other eminent optical writers, at the same time the centre of the spherical surface of the retina, and that of the lesser spherical surface of the cornea; in the diagram (fig. 26), to simplify the consideration of the problem, R and L represent only the circle of curvature of the bottom of the retina, but the reasoning is equally true in both cases.

The same reasons, founded on the experiments in this memoir, which disprove the theory of Aguilonius, induce me to reject the law of corresponding points as an accurate expression of the phænomena of single vision. According to the former, objects can appear single only in the plane of the horopter; according to the latter, only when they are in the circle of single vision; both positions are inconsistent with the binocular vision of objects in relief, the points of which they consist appearing single though they are at different distances before the eyes. I have already proved that the assumption made by all the maintainers of the theory of corresponding points, namely that the two pictures projected by any object on the retinæ are exactly similar, is quite contrary to fact in every case except that in which the optic axes are parallel.

Gassendus, Porta, Tacquet and Gall maintained, that we see with only one eye at a time though both remain open, one according to them being relaxed and inattentive to objects while the other is upon the stretch. It is a sufficient refutation of this hypothesis, that we see an object double when one of the optic axes is displaced either by squinting or by pressure on the eyeball with the finger; if we saw with only one eye, one object only should under such circumstances be seen. Again, in many cases which I have already explained, the simultaneous affection of the two retinæ excites a different idea in the mind to that consequent on either of the single impressions, the latter giving rise to the idea of a representation on a plane surface, the former to that of an object in relief; these things could not occur did we see with only one eye at a time.

Du Tour * held that though we might occasionally see at the same time with both eyes, yet the mind cannot be affected simultaneously by two corresponding points of the two images. He was led to this opinion by the curious facts alluded to in § 14. It would be difficult to disprove this conjecture by experiment; but all that the experiments adduced in its favour, and others relating to the disappearance of objects to one eye really proves, is, that the mind is inattentive to impressions made on one retina when it cannot combine the impressions on the two retinæ together so as to occasion a perception resembling that of some external object; but they afford no ground whatever for supposing that the mind cannot under any circumstances attend to impressions made simultaneously on points of the two retinæ, when they harmonize with each other in suggesting to the mind the same idea.

A perfectly original theory has been recently advanced by M. Lehot †, who has endeavoured to prove, that instead of pictures on the retinæ, images of three dimensions are formed in the vitreous humour which we perceive by means of nervous filaments extended thence from the retina. This theory would account for the single appearance to both eyes of objects in relief, but it would be quite insufficient to explain why we perceive an object of three dimensions when two pictures of it are presented to the eyes; according to it, also, no difference should be perceived in the relief of objects when seen by one or both eyes, which is contrary to what really happens. The proofs, besides, that we perceive external objects by means of pictures on the retinæ are so numerous and convincing, that a contrary conjecture cannot be entertained for a moment. On this account it will suffice merely to mention two other theories.

* *Act. Par.* 1743. M. p. 334.

† *Nouvelle Théorie de la Vision*, Par. 1823.

Vallée *, without denying the existence of pictures on the retina, has advocated that we perceive the relief of objects by means of anterior foci on the hyaloid membrane; and Raspail † has developed at considerable length the strange hypothesis, that images are not painted on the retina, but are immediately perceived at the focus of the lenticular system of which the eye is formed.

§ 16.

It now remains to examine *why* two dissimilar pictures projected on the two retinæ give rise to the perception of an object in relief. I will not attempt at present to give the complete solution of this question, which is far from being so easy as at a first glance it may appear to be, and is indeed one of great complexity. I shall in this place merely consider the most obvious explanation which might be offered, and show its insufficiency to explain the whole of the phænomena.

It may be supposed, that we see but one point of an object distinctly at the same instant, the one namely to which the optic axes are directed, while all other points are seen so indistinctly, that the mind does not recognise them to be either single or double, and that the figure is appreciated by successively directing the point of convergence of the optic axes successively to a sufficient number of its points to enable us to judge accurately of its form.

That there is a degree of indistinctness in those parts of the field of view to which the eyes are not immediately directed, and which increases with the distance from that point, cannot be doubted, and it is also true that the objects thus obscurely seen are frequently doubled. It may be said, this indistinctness and duplicity is not attended to, because the eyes shifting continually from point to point, every part of the object is successively rendered distinct; and the perception of the object is not the consequence of a single glance, during which only a small part of it is seen distinctly, but is formed from a comparison of all the pictures successively seen while the eyes are changing from one point of the object to another.

All this is in some degree true; but were it entirely so, no appearance of relief should present itself when the eyes remain intently fixed on one point of a binocular image in the stereoscope. But on performing the experiment carefully, it will be found, provided the pictures do not extend too far beyond the

* *Traité de la Science du Dessin*, Par. 1821, p. 270.

† *Nouveau Système de Chimie Organique*, t. 2. p. 329.

centres of distinct vision, that the image is still seen single and in relief when this condition is fulfilled. Were the theory of corresponding points true, the appearance should be that of the superposition of the two drawings, to which however it has not the slightest similitude. The following experiment is equally decisive against this theory.

Draw two lines inclined towards each other, as in Plate VIII. fig. 10, on a sheet of paper, and having caused them to coincide by converging the optic axes to a point nearer than the paper, look intently on the upper end of the resultant line, without allowing the eyes to wander from it for a moment. The entire line will appear single and in its proper relief, and a pin or a piece of straight wire may without the least difficulty be made to coincide exactly in position with it; or, if while the optic axes continue to be directed to the upper and nearer end, the point of a pin be made to coincide with the lower and further end or with any intermediate point of the resultant line, the coincidence will remain exactly the same when the optic axes are moved and meet there. The eyes sometimes become fatigued, which causes the line to appear double at those parts to which the optic axes are not fixed, but in such case all appearance of relief vanishes. The same experiment may be tried with more complex figures, but the pictures should not extend too far beyond the centres of the retinæ.

Another and a beautiful proof that the appearance of relief in binocular vision is an effect independent of the motions of the eyes, may be obtained by impressing on the retinæ ocular spectra of the component figures. For this purpose the drawings should be formed of broad coloured lines on a ground of the complementary colour, for instance red lines on a green ground, and be viewed either in the stereoscope or in the apparatus, fig. 6, as the ordinary figures are, taking care however to fix the eyes only to a single point of the compound figure; the drawings must be strongly illuminated, and after a sufficient time has elapsed to impress the spectra on the retinæ, the eyes must be carefully covered to exclude all external light. A spectrum of the object in relief will then appear before the closed eyes. It is well known, that a spectrum impressed on a single eye and seen in the dark, frequently alternately appears and disappears: these alternations do not correspond in the spectra impressed on the two retinæ, and hence a curious effect arises; sometimes the right eye spectrum will be seen alone, sometimes that of the left eye, and at those moments when the two appear together, the binocular spectrum will present itself in bold relief. As in this case the pictures cannot shift their places on the retinæ in whatever manner the eyes be moved about, the optic axes can during the experiment only correspond with a single point of each.

When an object, or a part of an object, thus appears in relief while the optic axes are directed to a single binocular point, it is easy to see that each point of the figure that appears single is seen at the intersection of the two lines of visible direction in which it is seen by each eye separately, whether these lines of visible direction terminate at corresponding points of the two retinæ or not.

But if we were to infer the converse of this, viz. that every point of an object in relief is seen by a single glance at the intersection of the lines of visible direction in which it is seen by each eye singly, we should be in error. On this supposition, objects before or beyond the intersection of the optic axes should never appear double, and we have abundant evidence that they do. The determination of the points which shall appear single seems to depend in no small degree on previous knowledge of the form we are regarding. No doubt, some law or rule of vision may be discovered which shall include all the circumstances under which single vision by means of non-corresponding points occurs and is limited. I have made numerous experiments for the purpose of attaining this end, and have ascertained some of the conditions on which single and double vision depend, the consideration of which however must at present be deferred.

Sufficient, however, has been shown to prove that the laws of binocular visible position hitherto laid down are too restricted to be true. The law of Aguilonius assumes that objects in the plane of the horopter are alone seen single; and the law of corresponding points carried to its necessary consequences, though these consequences were unforeseen by its first advocates, many of whom thought that it was consistent with the law of Aguilonius, leads to the conclusion, that no object appears single unless it is seen in a circle passing through the centres of visible direction in each eye and the point of convergence of the optic axes. Both of these are inconsistent with the single vision of objects whose points lie out of the plane in one case and the circle in the other; and that objects do appear single under circumstances that cannot be explained by these laws, has, I think, been placed beyond doubt by the experiments I have brought forward. Should it be hereafter proved, that all points in the plane or in the circle above mentioned are seen single, and from the great indistinctness of lateral images it will be difficult to give this proof, the law must be qualified by the admission, that points out of them do not always appear double.

XXXVII. *On the Expansion of some Solid Bodies by Heat.* *By* Hermann Kopp*.

THE method of experiment adopted by Professor Kopp in his laborious and valuable investigation is to ascertain the specific gravities of a body when immersed in fluids of various temperatures, and thence, by means of the known expansion of the fluid, to determine the cubic expansion of the body. A flask was taken furnished with a carefully ground glass stopper; and the first point to be ascertained was, "What weight of water, freed from air, and at different temperatures, was the flask able to contain?" For low temperatures, the flask and its contained water were placed in a large vessel filled with the same fluid, the temperature of which was shown by two thermometers immersed in it. When it was certain that the flask had assumed the temperature of the surrounding water, the stopper (which was preserved at the same temperature) was set on, the flask dried, and then carefully weighed. For temperatures of 40° or 50° C., the flask was immersed in a large beaker filled with water, which again was immersed in a second larger beaker, also full of water; the latter was heated, and after some time the water surrounding the flask acquired a uniform temperature of the required height; the glass stopper, which up to this time had been preserved in water of the same temperature, was now set on, the flask removed, dried, and weighed as before. When the quantity of boiling water held by the flask was to be ascertained, the latter was properly fixed in the neck of a large bolt-head, in which a quantity of water was kept violently boiling. The flask was here surrounded by steam, and precautions were taken to prevent any inconvenient loss of heat by radiation or by contact with the surrounding air.

Having ascertained the amount of water embraced by the flask at numerous temperatures, a proceeding exactly similar was followed to ascertain the specific gravity of the substance. The flask with the substance alone was first weighed; the flask was then filled with water, the air completely expelled by boiling, and then the weight of the known quantity of solid substance, plus the weight of the water necessary to fill the flask at various temperatures, was ascertained.

Suppose the weight of the flask of water at the temperature t^0 to be W, the weight of the solid substance to be examined to be P, and the weight of the water and substance which together filled the flask at t^0 to be S, then we have

$$\frac{P}{W-(S-P)}=D_t,$$

* *Ann. der Chem. und Pharm.*, vol. lxxxi. No. 1. p. 1–67.

where D_t expresses the specific gravity of the substance referred to water of the temperature t^0 as unit. Further, is $\frac{D_t}{V_t} = D_0 =$ the specific gravity of the solid substance at t^0 referred to water at 0^0 as unit, where V_t expresses the volume which 1 volume of water at 0^0 assumes on being heated to t^0.

Supposing that for two temperatures t and t', the former of which is lower than the latter, the specific gravities D_0 and D_0' respectively be found, then is the cubic expansion of the body

$$= \frac{1}{t' - t} \cdot \left(\frac{D_0}{D_0'} - 1 \right).$$

The expansion of water by heat was made the subject of special inquiry, and numerous substances were examined whose linear expansion had been determined by other methods and other men; the agreement between M. Kopp's results and those already determined furnishes a proof that the method pursued and the precautions taken may be relied on.

We here transcribe a tabular statement of M. Kopp's results, premising that each is the mean of several experiments:—

Substance.	Formula.	Cubic expansion for 1°.	Determined by means of
Copper	Cu	0·000051	Water
Lead	Pb	0·000089	Water
Tin	Sn	0·000069	Water
Iron	Fe	0·000037	Mercury
Zinc	Zn	0·000089	Water
Cadmium	Cd	0·000094	Water
Bismuth	Bi	0·000040	Water
Antimony	Sb	0·000033	Water
Sulphur	S	0·000183	Water
Galena	PbS	0·000068	Water
Zinc blende	ZnS	0·000036	Water
Iron pyrites	FeS^2	0·000034	Water
Rutile	TiO^2	0·000032	Water
Oxide of tin	SnO^2	0·000016	Water
Oxide of iron	Fe^2O^3	0·000040	Water
Magnetic ore	Fe^3O^4	0·000029	Water
Fluor spar	$CaFl$	0·000062	Water
Arragonite	CaO, CO^2	0·000065	Water
Calcareous spar	CaO, CO^2	0·000018	Water
Bitter spar	$CaO, CO^2 + MgO, CO^2$	0·000035	Water
Carbonate of iron	$Fe\,(Mn, Mg)\,O, CO^2$	0·000035	Water
Heavy spar	BaO, SO^3	0·000058	Water
Cœlestine	SrO, SO^3	0·000061	Water
Quartz	SiO^3	0·000042	Water
		0·000039	Mercury

Substance.	Formula.	Cubic expansion for 1°.	Determined by means of
Ortholdas . . .	$KO, SiO^3 + Al^2O^3, 3SiO^3$	0·000026	Water
		0·000017	Mercury
Glass, soft soda glass		0·000026	Water
Glass, soft soda glass, another kind . .		0·000024	Mercury
Glass, hard potash glass		0·000021	Mercury

Taking every possibility of error into account, M. Kopp considers that we may infer with certainty from the preceding numbers, that the expansion of solid substances is by no means determined by their chemical nature. The difference between the coefficients of expansion for arragonite and calcareous spar is so great as to destroy all hope of establishing any relation of the kind. Neither does the expansion appear to depend altogether on the arrangement of the atoms; for although bitter spar and carbonate of iron agree, and heavy spar differs but little from cœlestine, in the cases of carbonate of iron and carbonate of lime, and of rutile and oxide of tin, no such agreement exists. The table further shows that there are many non-metallic substances which expand as much under the action of heat as the metals themselves.

XXXVIII. *On the Classification of the Silicates and their allied Compounds. By* Edward J. Chapman, *Professor of Mineralogy in University College, London*.*

IN the present state of chemical and mineralogical knowledge, two distinct classifications of minerals seem to be necessary; the one, strictly chemical, having regard solely to the actual composition of the substance, and being independent, consequently, of all deductions based upon isomorphism or other modifying causes; and the other, a chemico-physical distribution, founded upon a careful study and interpretation of the entire nature of the mineral in all its bearings. The first shows us what a mineral really is in its relations to existing chemistry, but only in these relations; being unable, on the one hand, amongst other apparent contradictions, to define and separate isomero-heteromorphous bodies; and forced, on the other hand, to forego natural analogies, in the separation of substances evidently akin to one another. A classification of this kind is, nevertheless, necessary: first, as a classification of convenience in a chemical and œconomical point of view; and secondly, as a check upon the too hasty generalizations to which the chemico-physical system naturally gives rise.

In the following classification I have attempted to develope a chemico-physical distribution of the silicates and their allied

* Communicated by the Author.

compounds; these latter being the sesquioxides generally, and the so-called metallic acids. This union has not been adopted from any preconceived views, but has been forced upon me in the course of these investigations. The minerals of the above series are, in fact, so linked together, that it is frequently impossible to separate them without breaking through self-evident analogies. I am willing to allow that, in places, I may have gone too far; but in a system like that employed, much is necessarily left to the judgement; and if the entire subject be carefully reviewed, I think it will be admitted that the present essay has upon the whole an opposite tendency. Several of the proposed types, for instance, might upon certain grounds have been united; but they have been kept separate, partly in deference perhaps to almost irradicable prejudices, which bias one's opinion even against the will, and partly as a matter of convenience. It is to be remarked, however, that the present arrangement is merely offered as a provisional one; and that, from the difficulties with which the subject is beset, it is easier to point out imperfections than to remedy them.

Minerals of the Silica, Alumina, and Metallic Acid Series.

Braunite Type:—Dimetric.

Braunite, Mn^2O^3.

Corundum Type:—Hexagonal.

1. *Corundum sub-type.*—Erysiderite, Fe^2O^3 (Appendix, hydrosiderite, $Fe^2O^3+xH^2O$); ilmenite (Fe^2O^3, Ti^2O^3); corundum, Al^2O^3 (App. hydrargillite).

2. *Quartz sub-type.*—Quartz, SiO^3 (App. opal); phenakite, Be^2O^3, SiO^3; beryl (Be^2O^3, Al^2O^3), $2SiO^3$.

Staurolite Type:—Trimetric.

Staurolite (Al^2O^3, SiO^3); andalusite; chrysoberyl (Be^2O^3, Al^2O^3); topaz $\{(Al^2O^3, SiO^3)+(Al^2Fl^3, SiFl^3)\}$.

Cyanite Type:—Triclinic, Monoclinic.

Pycnite (?); cyanite; wœrthite; sillimanite; monrolite (?); bamlite; euclase, $4(Be^2O^3, Al^2O^3), 3SiO^3$.

Spinel Type:—Monometric.

General formula $=RO, R^2O^3$. Spinel; pleonaste; gahnite; franklinite; chromolite; zophosine (pitchblende); magnetite, FeO, Fe^2O^3; iserine; garnet, including pyrope, &c.; helvine*; eulytine (?).

In order to œconomise space, I have not cited the different

* See the present volume of the Philosophical Magazine, Art. XXI.

kinds of garnets (melanite, uwarowite, &c.), although I consider these quite as much entitled to the rank of distinct species, as the various spinels and their allied aluminates, ferrites, chromites, &c.; the theoretical value of the term "species" will, however, become obsolete as the science advances. In placing isomorphous substances under one common type or sub-type, we do not necessarily consider them to be one mineral, as some opponents of the chemico-physical system have attempted to establish.

Idocrase Type:—Dimetric.

Idocrase, $3RO, SiO^3 + R^2O^3, SiO^3 = rR$; hausmannite, MnO, Mn^2O^3; anatase, TiO^2; fergusonite, $6(YO, ClO), Ta^2O^3$ (?).

Cassiterite Type:—Dimetric.

Rutile, $Ti\,O^2$; cassiterite, SnO^2; zircon, Zr^2O^3, SiO^3; malacon, $2(Zr^2O^3, SiO^3) + H^2O$; œrstedite; azorite, CaO, Ta^2O^3 (?).

Brookite Type:—Trimetric.

Brookite, TiO^2; polianite, MnO^2; pyrolusite, MnO^2; gæthite, $Fe^2O^3 + H^2O$; manganite, $Mn^2O^3 + H^2O$; diaspore, $Al^2O^3 + H^2O$; columbite, $3(FeO, MnO), 2(Ta^2O^3, Pp^2O^3, Ni\,O^3)$; yttro-tantalite (?), $3YO, Ta^2O^3$; tantalite (kimitolite); samarskite; mengite; polymignite; polycrase; æschynite; ostranite; euxenite; wœhlerite (?); eucolite (?).

Pyrochlore Type:—Monometric.

Perowskite, CaO, TiO^2; pyrochlore, $2RO, Ta^2O^3$; microlite (?); pyrrhite, Zr^2O^3, Ta^2O^3.

Cerite Type:—Hexagonal.

Cerite, $3(CeO, RO), SiO^3 + 3H^2O$; thorite, $3ThO, SiO^3 + 3H^2O$; eudialite, $2(3RO, 2SiO^3) + Zr^2O^3, 2SiO^3$; schorlamite, $FeO, SiO^3 + 2CaO, TiO^2$?; willemite, $3ZnO, SiO^3$; dioptase, $3CuO, 2SiO^3 + 3H^2O$ (Appendix, chrysocolla).

Chrysocolla bears the same relation to dioptase as brown iron ore to specular iron, and opal to quartz, amongst other examples. Eudialite and dioptase are closely allied in a crystallographic point of view. The two known rhombohedrons of eudialite give for the relative lengths of the vertical axis, 2·110 and 0·5279; whilst the dioptase forms yield for the same, 1·059 and 0·530. If we consider the more obtuse forms as protaxial, we have, for the notation of the above rhombohedrons, R and 4R in eudialite, and R, 2R in dioptase.

Electric Calamine Type:—Trimetric.

Electric calamine, $2(3ZnO, SiO^3) + 3H^2O$.

Chrysolite Type:—Trimetric, Monoclinic ?.

Chrysolite, $3RO, SiO^3$; tephroite, $3MnO, SiO^3$; knebellite, $3(MnO, FeO), SiO^3$; forsterite; humite (?); chondrodite, $MgFl + 7MgO, 2SiO^3 = r^4R$; lievrite, $3(3RO, SiO^3) + 2Fe^2O^3, SiO^3$.

Epidote Type:—Monoclinic.

Orthite or allanite, $3RO, SiO^3 + R^2O^3, SiO^3$; gadolinite, $3(YO, RO), SiO^3$; warwickite; enceladite; epidote, $3RO, SiO^3 + 2(R^2O^3, SiO^3)$; sphene, $3CaO, 3TiO^2, 2SiO^3$; wichtyne, chloritoid, chlorite spar. (See the present volume of the Philosophical Magazine, Art. XXI.)

Axinite Type:—Triclinic.

Axinite, $3RO, 2(SiO^3 BO^3) + 2R^2O^3(SiO^3 BO^3)$; danburite, $RO, BO^3 + 4ROSiO^3$.

Tourmaline Type:—Hexagonal.

Tourmaline.

Augite Type:—Monoclinic.

Augite, $3RO, 2SiO^3$; wollastonite, $3CaO, 2SiO^3$; rhodonite, $3MnO, 2SiO^3$; crednerite, $3(CuO, CaO), 2Mn^2O^3$; spodumene; acmite, $NaO, SiO^3 + Fe^2O^3, 2SiO^3$; hornblende, $RO, SiO^3 + 3RO, 2SiO^3$; babingtonite, $3ROSiO^3 + 3RO, 2SiO^3$; glaucophane (?), $3(3RO, 2SiO^3) + 2(Al^2O^3, 2SiO^3)$.

Spodumene is placed under this type on the authority of Hartwell and Dana*. In respect to crednerite, see the present volume of the Philosophical Magazine, Art. XXI. Glaucophane should perhaps be placed near iolite.

Muscovite Type:—Monoclinic.

Muscovite (potash mica), $3RO, 12Al^2O^3, 16SiO^3$; margarodite, $RO, H^2O, 2R^2O^3, 3SiO^3$; margarite, $3RO, 12Al^2O^3, 8SiO^3$; lepidolite; emerylite; euphyllite.

This type is by no means a satisfactory one; but so little is known respecting the true nature of the micas, that it seems advisable at present to keep them distinct from other minerals. Muscovite was so named by Dana.

Phlogopite Type:—Trimetric.

Phlogopite (magnesia mica), $9RO, 2Al^2O^3, 5SiO^3$; rubellane (a variety of phlogopite ?).

* See the third edition of Dana's very able Treatise on Mineralogy, p. 693. I should confess, however, that the cleavage form of spodumene seems to me to be triclinic.

Biotite Type:—Hexagonal.

Biotite (hexagonal or uniaxial mica), $3RO, R^2O^3, 2SiO^3$; lepidomelane, $3RO, 3R^2O^3, 4SiO^3$.

Talc Type:—Monoclinic.

Talc, $xMgO, xSiO^3$; agalmatolite; meerschaum, MgO, SiO^3, H^2O; schillerspar, $15RO, 8SiO^3, 12H^2O$; pyrallolite (triclinic?).

The minerals of this type are evidently allied, as aberrant members, to the augite and hornblende group; but the physical and geological characters of the talcs are quite sufficient to demand for these compounds a distinct place in the system. In a linear series, their present position seems to be the most appropriate.

Serpentine Type:—Trimetric (?).

Serpentine, $9MgO, 4SiO^3, 6H^2O$; picrosmine, $6MgO, 4SiO^3, 3H^2O$; villarsite, $12(MgO, FeO, MnO), 4SiO^3, 3H^2O$; monradite, $12(MgO, FeO), 8SiO^3, 3H^2O$; pyrosclerite, $12RO, 2Al^2O^3, 6SiO^3, 9H^2O$,—vermiculite, chonikrite, kammererite (varieties of pyrosclerite?); spadaite, $5RO, 4SiO^3, 4H^2O$; kerolite, $6MgO, 4SiO^3, 9H^2O$; antigorite, $4RO, 2SiO^3, H^2O$; aphrodite, $3RO, 2SiO^3$, 2 (or 3) H^2O; picrophyll, $3RO, 2SiO^3, 2H^2O$; dermatine, $3RO, 2SiO^3, 6H^2O$; gymnite, $2RO, SiO^3, 3H^2O$; hydrophite, $2RO, SiO^3, 3H^2O$; pimelite, RO, SiO^3, H^2O.

To the above may be added the following, consisting chiefly of MgO Al^2O^3 or Fe^2O^2, SiO^3, and H^2O in various proportions: saponite, mountain cork, xylite (rockwood), pyrargillite, groppite, damourite, rosellane (polyargite, &c.), onkosin, metaxite, and others. Very few of the members, however, of this group, and of the two next in succession, can fairly claim a place in a mineralogical system. Many, without doubt, are altered or metamorphic products referable to some of the preceding types, or to some of those which follow. If admitted, therefore, into a classification of minerals, they should occupy an intermediate position, as in the present arrangement.

Chlorite Type:—Hexagonal (?).

Chlorite (leuchtenbergite), $9RO, 2R^2O^3, 4SiO^3, 9H^2O$ (?); thuringite, $9RO, 2R^2O^3, 4SiO^3, 9H^2O$; epichlorite, $9RO, 2R^2O^3, 6SiO^3, 9H^2O$; ripidolite, $9RO, 3R^2O^3, 4SiO^3, 9H^2O$; kammererite, $6RO, R^2O^3, 3SiO^3, 6H^2O$; stilpnomelane, $6RO, R^2O^3, 6SiO^3, 6H^2O$; ottrelite, $3RO, 2R^2O^3, 4SiO^3, 3H^2O$ (?); iberite, $3RO, 3Al^2O^3, 4SiO^3, 3H^2O$; diphanite, $4RO, 6Al^2O^3, 5SiO^3, 4H^2O$; pennine, $3RO, 2R^2O^3, 3SiO^3, 2H^2O$; cronstedite, $3RO, R^2O^3, SiO^3, 3H^2O$; xanthophyllite (clintonite), $4RO, R^2O^3, SiO^3, H^2O$; disterrite (brandisite), $7RO, 4R^2O^3$,

$2SiO^3$, $2H^2O$; sideroschisolite, $6FeO$, SiO^3, 3 (or 2) H^2O; chamoisite; hisingerite, $3RO$, $2Fe^2O^3$, $3SiO^3$, $6H^2O$; thraulite; gillingite; crocidolite; chlorophæite; carpholite, $3RO$, $3Al^2O^3$, $4SiO^3$, $6H^2O$; pyrosmalite, $12RO$, Fe^2O^3, $8SiO^3$, $6H^2O + Fe\,3Cl$, $= 12RO + (Fe^2O^3, FeCl^3, 8SiO^3, 6H^2O) = rR$, if $3H^2O = 1R^2O^3$ or $1SiO^3$. *Appendix*:—Pyrophyllite; anthosiderite.

Kollyrite Type:—consisting principally of earthy semi-decomposed minerals, for the most part hydrated silicates of alumina or sesquioxide of iron. The water is chiefly hygroscopic.

Kollyrite, scarbroite, halloysite, lenzinite, allophane, schrotterite (opal-allophane), nontronite, chloropal, pinguite, cimolite, lithomarge (including myeline, carnatite, &c.), tuesite, bole, malthasite, catlinite, samoite, razoumoffskin, myloschin or serbian, wolchonskoite, chrome-ochre, smelite, fuller's earth, &c.

Iolite Type:—*Trimetric.*

Iolite, $3RO$, $2SiO^3 + 3(R^2O^3, SiO^3)$. *Metamorphic products*: Pinite = iolite + $2H^2O$ or $3H^2O$; chlorophyllite = iolite + $2H^2O$; esmarkite = iolite + $3H^2O$; gigantolite, same formula as esmarkite; bonsdorffite, fahlunite, = iolite + $6H^2O$; praseolite = iolite − $SiO^3 + 3H^2O$; aspasiolite, &c.

Barsowite, $3RO$, $2SiO^3 + 3(Al^2O^3, SiO^3)$; bytownite, $3RO$, $2SiO^3 + 3(Al^2O^3, SiO^3)$. The crystallization of these two minerals is not yet known. Hermann refers the latter to lepolite.

Feldspar Type:—*Monoclinic.*

Ryacolite, $(NaO\ KO)$, $SiO^3 + Al^2O^3$, SiO^3; loxoclase, RO, $SiO^3 + Al^2O^3$, $2SiO^3$; orthoclase (feldspar), KO, $SiO^3 + Al^2O^3$, $3SiO^3$; petalite, $3(LiO, NaO)$, $4SiO^3 + 4(Al^2O^3, 4SiO^3)$; kastor, LiO, $3SiO^3 + 2(Al^2O^3, 3SiO^3)$; couseranite, $3RO$, $SiO^3 + Al^2O^3$, SiO^3; saussurite, $3RO$, $SiO^3 + 2(Al^2O^3, SiO^3)$; baulite, RO, $2SiO^3 + Al^2O^3$, $6SiO^3$. *Appendix*:—Obsidian, pitchstone, &c.

Albite Type:—*Triclinic.*

Anorthite, $3RO$, $SiO^3 + 3(Al^2O^3, SiO^3)$; lepolite, same formula as anorthite; spodumene (?); vosgite, $3(RO, SiO^3) + 3Al^2O^3$, $2SiO^3$; hyposclerite, $3(RO, SiO^8) + 2Al^2O^3$, $3SiO^3$; labradorite, RO, $SiO^3 + Al^2O^3$, SiO^3; chladnite (?), MgO, SiO^3 from Shepherd's analysis; latrobite, $3RO$, $SiO^3 + 4(Al^2O^3, SiO^3)$; andesine, $3RO$, $2SiO^3 + 3(Al^2O^3, 2SiO^3)$; oligoclase, RO, $SiO^3 + Al^2O^3$, $2SiO^3$; albite, NaO, $SiO^3 + Al^2O^3$, $3SiO^3$. *Appendix*:—Chesterlite, $3RO$, $2SiO^3 + 2(Al^2O^3, 3SiO^3)$; Kerndt's green feldspar from Bodenmais (Liebig's First Report, vol. ii. p. 409 of Dr. Hofmann's translation), same formula.

Wernerite Type:—Dimetric.

Gehlenite, $3(3RO, Si O^3) + 3R^2 O^3, Si O^3$; humboldtilite, $2(3RO, Si O^3) + R^2 O^3, Si O^3$; dipyre, $4(RO, SiO^3) + 3(Al^2O^3, Si O^3)$; meionite, $3CaO, Si O^3 + 2(Al^2 O^3, Si O^3)$; wernerite, $3Ca O, SiO^3 + 3(Al^2O^3, SiO^3)$; scapolite, $3RO, 2SiO^3 + 2(Al^2O^3, SiO^3)$. The three latter formulæ afford another proof of the insufficiency of our present chemical system in its applications to mineralogy; unless, indeed, it be admitted that chemical and physical phænomena have no relations to each other—an idea the mind refuses to receive.

Palagonite may perhaps be attached to this type as a kind of scapolite-obsidian.

Leucite Type:—Monometric.

Leucite, $3KO, 2Si O^3 + 3(Al^2 O^3, 2Si O^3)$; analcime, $3NaO, 2Si O^3 + 3(Al^2 O^3, 2Si O^3) + 6H^2 O$ = leucite + $6H^2 O$; glottalite, $3CaO, 2SiO^3 + Al^2O^3, SiO^3 + 9H^2O$.

Lapis-Lazuli Type:—Monometric.

Lapis-lazuli; haüyne, $3(NaO, KO), SiO^3 + 3(Al^2O^3, SiO^3) + 2(CaO, SO^3)$; nosean, $3NaO, SiO^3 + 3(Al^2O^3, SiO^3) + NaO, SO^3$; skolopsite, $3(3RO, 2SiO^3) + 3(Al^2O^3, SiO^3) + NaO, SO^3$; ittnerite $[3RO, SiO^3 + 3(Al^2O^3, SiO^3) + 6H^2O] + CaO, SO^3$ and NaCl; sodalite, $3NaO, SiO^3 + 3(Al^2O^3, SiO^3) + NaCl$.

Nepheline Type:—Hexagonal.

Davyne, cancrinite, $2RO, SiO^3 + 2(Al^2 O^3, Si O^3) + RO, CO^2 + H^2O$; stroganowite, formula like the preceding but without the water; nepheline, $2RO, Si O^3 + 2(Al^2 O^3, Si O^3)$; chabasite, $3RO, 2SiO^3 + 3(Al^2O^3, 2SiO^3) + 18H^2O$, also (herschellite, etc.) the same $- 3H^2O$, and (other varieties) $RO, SiO^3 + Al^2O^3, 2SiO^3 + 6H^2O$. Chabasite and its allied forms might perhaps with greater propriety be arranged as a separate type.

Apophyllite Type:—Dimetric.

Apophyllite $[8(CaO, SiO^3) + KO, 2SiO^3 + 16H^2O] + x CaF$ (?); faujasite, $3RO, 4SiO^3 + 3(Al^2O^3, 2SiO^3) + 24H^2O$; zeagonite, $2RO, SiO^3 + 2(Al^2O^3, SiO^3) + 9H^2O$; edingtonite (?).

Desmine (Stilbite) Type:—Trimetric.

Prehnite, $2CaO, Si O^3 + Al^2 O^3, Si O^3 + H^2 O$; thomsonite, $3RO, Si O^3 + 3(Al^2 O^3, Si O^3) + 7H^2 O$; phillipsite, $3RO, 2SiO^3 + 4(Al^2 O^3, 2Si O^3) + 18H^2 O$; harmatome, $3BaO, 2Si O^3 + 4(Al^2 O^3, 2SiO^3) + 18H^2 O$; natrolite (mesotype), $Na O, SiO^3 + Al^2 O^3, Si O^3 + 2H^2 O$; scolezite (? monoclinic), $RO, Si O^3 + Al^2O^3, SiO^3 + 3H^2O$; desmine (stilbite), $CaO, SiO^3 + Al^2O^3$,

$3SiO^3 + 6H^2O$; epistilbite, $RO, SiO^3 + Al^2O^3, 3SiO^3 + 5H^2O$. *Appendix*:—Okenite, $3CaO, 4SiO^3 + 6H^2O$.

Heulandite Type:—*Monoclinic.*

Datolite, $6CaO, 4SiO^3, 3BO^3, 3H^2O$, or (botryolite) $6H^2O$; laumonite, $3CaO, 2SiO^3 + 4(Al^2O^3, 2SiO^3) + 18H^2O$ = a monoclinic phillipsite; leonhardite, $3(CaO, SiO^3) + 4(Al^2O^3, 2SiO^3) + 15H^2O$; brewsterite, $(SrO, BaO), SiO^3 + Al^2O^3, 3SiO^3 + 5H^2O$; heulandite (stilbite of German authors), $3(CaO, SiO^3) + 4(Al^2O^3, 3SiO^3) + 18H^2O$.

XXXIX. *On the Amylum Grains of the Potatoe.* By A. G. C. Martin, *Librarian of the Imperial Polytechnic Institute of Vienna*.*

[With a Plate.]

FOR some time past I have been engaged in investigating the phænomena which occur when the amylum grains of the potatoe are subjected to the action of boiling water, and I have discovered a new fact which is not in accordance with the present theory and prevailing notions concerning their structure.

Although there is in microscopical observations great liability to erroneous conclusions, and consequently extreme caution necessary at every step, still too much timidity would speedily put a stop to the progress of science, and hence I trust that the scientific world will consider my numerous experiments worthy of being submitted to a trial.

Prior to commencing my experiments, I made myself thoroughly acquainted with all that had been written on the subject of amylum†, and found that in no instance was there any detailed account of the phænomena presented in the formation of amylum or the process of boiling. This is easily accounted for by the fact, that precisely at the above point our knowledge ceases. The object of my researches was to supply this defect.

1. The microscope I employed is one by Plössl. Most of my observations were made with the object-glasses Nos. 3, 4 and 5, and the eye-glass No. 2, a combination which gives a magnifying power of 198 diameters. Plössl, in his recent microscopes, has made the reflector moveable outside the axis, by which an

* Communicated by the Author.

† Fritsche on Amylum, Poggendorff's *Annalen*, vol. xxxii. p.129. Mohl, Anatomy and Physiology of Vegetable Cells, p. 48. Payen, Practical Chemistry, p. 347. Regnault, Elements of Chemistry, vol. iii. p. 161. Schleiden, Elements of Scientific Botany, first edition, p. 171. Schleiden and Schmidt, Encyclopædia of Natural Science, vol. iii. p. 33. Unger, Anatomy and Physiology of Plants, p. 39.

oblique or side light is obtained—an arrangement which cannot be too strongly recommended to microscopists. Before Plössl made this improvement, I had to adopt a substitute, with which every expert observer is probably familiar, but to which it may be as well to call attention, because the success of experiments requiring such nicety greatly depends on the proper adjustment of the light.

For observations by day-light, the reflector ought to be so adjusted that the greatest quantity of light possible will be thrown on the stage of the microscope, and the light is to be broken in the following manner. A strip of black paper, 3 inches long and 1 inch wide (*i. e.* according to the diameter of the reflector), must be placed, the black side outwards, in such a way over the reflector that a segment on both sides of the latter will remain uncovered, and the light be thrown obliquely on the stage. It will often be expedient to cover the entire half of the reflector, leaving one-half only exposed. I prefer these two methods of adjusting the light to the ordinary diaphragm, because in addition to modifying the light, they possess the advantage of producing an obliquely-directed light.

When the observations are made by candle-light, the following method is to be adopted. A stearine candle, in a candlestick 12 inches high, is to be placed at a distance of 12 inches from the microscope, and the object placed in the full light of the reflector. Then placing the fore-finger on the vertically moveable hoop of the reflector, the latter is to be turned a little to the right or left, which will instantaneously alter the light; elevations will cast shadows, cavities will be shaded, and the amylum grains will appear as complete bodies to the eye of the observer. I would advise that all observations be conducted both by candle-light (not by lamp-light, which may be too glaring) and day-light, because a comparison of the different results serves as a check to erroneous conclusions.

2. Passing over the facts, more or less known, concerning the external appearance of the amylum grains of the potatoe, and also the experiments upon the kernel, the layers, the action of acids, alkalies, roasting, &c., I at once proceed to the peculiarities which appear on boiling the amylum. These phænomena, which are of a highly interesting character, are difficult to observe, and not yet thoroughly understood. The probable cause of their not having been more diligently studied and more thoroughly investigated is, that most microscopists, Fritsche among the number, commence the process by means of a current of heated air from a candle or lamp, and only begin to observe the phænomena when they have nearly terminated.

I have succeeded in inventing a method by which the boiling can be conducted under the microscope without endangering it, and the effect on one and the same grain be observed from the first to the last stage. I have thus been enabled to establish the following facts, which will, I trust, correct the present views on the structure of the amylum grains of the potatoe.

The method I employed in prosecuting these experiments is as follows:—

Between two very thin glasses of the same size as the stage of the microscope, a little amylum with a sufficient quantity of water is to be put, and the former well spread out with the finger to prevent as much as possible the formation of bubbles. The number of amylum grains in the field of view should not exceed ten or fifteen. The glasses should lie freely on the spring-piece, which must be raised by means of two pieces of cork or thick coins introduced below it; so that while the two glasses are lying right upon the object-bearer, a current of cold air will ascend from below, to permit the little flame to continue burning in the hole of or below the stage. As the glasses are wide, they protect the microscope from too great a heat or other danger. The small flame is to be obtained from a common thread, doubled and slightly waxed. This, when ignited, gives a flame quite sufficient to boil the amylum. This method of applying heat is adapted to other experiments, and appears far preferable to the usual one of heating the extreme end of the glass on which the object is placed, until the heat is conducted to the object itself.

An assistant can hold the flame to the glasses by inserting it from below into the hole of the stage, and withdraw it if any alteration be observed until the paste is formed, or more correctly speaking, until the amylum grains swell up and are completely unfolded. But it will be much better if the observer apply the flame himself, in which case it may perhaps be sometimes extinguished, yet a little practice will soon give dexterity in the operation. The best way is to hold the thread, which being stiff will remain upright, between the thumb and fore-finger, and to rest the little finger on the table supporting the microscope. This will steady the hand, and the flame can be constantly kept under the centre of the stage. If the heat be unintermitting, the operation, especially in its last stage, proceeds very rapidly, and it will be necessary to repeat the experiment twenty or thirty times before the mind can clearly comprehend the entire process. It is best to employ middle-sized grains at first, and afterwards large ones only.

3. According to my observations, the phænomena which take place during the process of boiling are as follows:—First, the amylum grain sinks in, in that place where, according to Fritsche,

the kernel is situated. On the surface minute fissures appear, two of which almost regularly diverge towards the thicker end of the grain. The grain continues to be depressed inwards until a cavity is formed, which is surrounded by an elevated ridge. In proportion as the grain swells up, this ridge increases in circumference and decreases in breadth, that is, continues to get flatter, until fissures, mostly of a stellated form, appear in the hitherto little altered thicker part of the grain. The process is now very rapidly developed, and it is very difficult for the eye to follow it. Suddenly something is torn off, the grain is extended lengthways, and in the next moment a wrinkled skin of a round, generally oval shape lies on the glass. Middle-sized and small grains exhibit this shape most distinctly; and they have usually only one longitudinal wrinkle, the upper and lower ends of which are pointed. The constant appearance of this wrinkle is important for the development of my theory.

4. When, as mentioned above, very few grains are employed, so that after boiling they remain properly separated by intervening spaces, if the temperature has been the proper one, the smaller grains appear as round disc-like skins, almost without wrinkles. The middle and large-sized ones are also apparently transformed into flat discs. If the glasses are removed from the stage, pressed against each other, and at the same time slightly moved from one side to the other, again placed on the stage and viewed by an obliquely-directed light, it will be seen, in proportion to the success of the experiment, that the wrinkles of the skin are either entirely smoothed or merely pressed down. In the former case the discs so produced have a perfectly round or oval form. In the latter case the contour of the discs remains slightly contracted or twisted, which is chiefly the case with very large grains. In the course of my investigations I discovered a method of delaying the entire process of boiling, by which the above-mentioned discs are obtained in a perfectly secure manner. This may, perhaps, make the experiment less striking, but it answers admirably for the verification of my theory.

By the action of tincture of iodine the amylum grains are contracted, *i. e.* are condensed, being transformed into iodized amylum. If a small drop of tincture of iodine be added to a quantity of water, and the whole well mixed, amylum grains placed in this iodized water turn light blue, dark blue, or almost black, according to the quantity of iodine employed. The precise quantity, which a few experiments will determine, is that which renders the grains of a delicate sky-blue colour without depriving them of their transparency, or obliterating the layers, or at least the traces of them. If too little iodine be used, the process goes on too rapidly; if too much, too slowly; the grain in the latter case being so much condensed, that in boiling it is unfolded very little or hardly at all. In the proper proportion the grain is seen to increase continually during the boiling, then to split at the kernel; and at this point the colour becomes lighter in the centre, and the elevated band, which is of a darker colour, continually recedes towards the edge until a completely flat disc lies on the glass. When this disc is pressed between the two glasses, it becomes smooth; and if the pressure be strong, somewhat larger. The experiment seems to succeed still better in a concentrated solution of alum, with as much tincture of iodine as will colour the grains of a steel-blue. The characteristic longitudinal wrinkle also appears in some of the grains, but it is easily rendered smooth.

Although I shall subsequently return to this subject, I cannot avoid asking in this place, Where does the split or tear, at which the grain is considered to burst, actually occur?

5. Before developing my views, I have yet to refer to the often-mentioned disc. Its appearance demonstrates that it is *perfectly flat*, and has a slightly elevated edge, which also becomes flat on pressure. The contour is round, but perfectly sharp. If the two glasses be violently moved from one side to the other whilst pressing the amylum, the disc is torn, and it is distinctly seen, especially in the blue-coloured ones, to consist of two layers, an upper and lower one. Further examination shows that they are collapsed vesicular bodies, consisting of an extremely fine but strong and elastic membrane. Should a disc have a small wrinkle not easily smoothed, then if the lower glass, furnished with a good quantity of water, be moved over the upper one, such a vesicle will be seen, particularly in the alum solution, to turn round on its axis, and the wrinkle to slide over the upper and lower layers of the skin; over the upper layer in the direction in which the glass is moved, and over the lower layer in an opposite direction. That this vesicle cannot be the grain enlarged in dimensions will be apparent hereafter; and I now proceed to develope my theory.

6. The primary form of the amylum grain is, according to my view, a spherical or ovate vesicle. If this be considered as empty, and so contracted that one-half lies in the other half, a watch-glass-shaped basin is formed, which, I may here observe, after boiling and pressure between the two glasses, appears, in consequence of the delicacy and elasticity of the membrane, as a flat, round-edged disc.

Plate VI. fig. 1 represents the edge of the basin-shaped vesicle. At the formation of the grain this edge moves a little inwards, and rolls itself up inwardly, by which a band *b* (fig. 2) with spiral internal windings, which on the outside appear elliptical, is produced. During the progress of the rolling up, the space *a* is of course constantly decreasing, while the parts on the inner edge of the band offer a resistance. This resistance must be overcome by contraction, in a direction tangental to the internal circumference; consequently a part of the band which has been by accident, or from the unequal thickness of the vesicle, rolled up before, or more than the rest, prevents the remaining part from rolling itself up. Hence the cause of the elliptical form of the band. The space at *a* now becomes continually smaller, until the interior edges approach so close that they are joined together, and the small hole hitherto remaining is closed. The amylum grain with its elliptical layers so far complete is represented by fig. 3.

We have obtained, as it were, a sketch of the geometrical construction of the amylum grains, according to which the kernel is not a primary substance, but a secondary form only. Whether this space is occupied by a liquid or by air has nothing to do with the theory of formation here developed, and is a subject for special investigation. How far this theory agrees with microscopical observation must be shown by experiment. As for the physiological questions arising from this theory, they must be reserved for more expert pens than mine.

7. Let us now proceed to compare the theory hitherto admitted with the new one as explained above. When the vesicles, coloured with tincture of iodine and pressed flat, are once seen to revolve round their axes, it seems decisive that no such thing as splitting through and through takes place; for this skin is so homogeneous and so transparent, that even the slightest fold is easily perceived. Schleiden, although he calls the product of the last stage of boiling a thick skin, and not a vesicle, seems already to call in question the complete splitting open, for he expressly says, that during the boiling the split is transformed into a large cavity. Now, I ask, what has become of the layers, which must assuredly possess much more substance than the external skin of the unboiled grain? Are they dissolved, or merely separated, and one fixed in the other? Has the smallest one surrounding the kernel, and the larger one close to it, and the third still larger one, and, in fact, have all the layers become increased, or diminished to the exact size of the external skin in order to form with it a large bag? or are the interior soft layers (so called) boiled away to a jelly-like mass, which remains invisible within the bag and is pressed flat with it? Raspail's assertion, that part of the amylum grains is dissolved, belongs, as is well known, to the things long since refuted and forgotten. That the skins should lie fixed one within the other and yet not differ in their dimensions, nor any fissures appear, nor the disc be thicker in the

centre, appears to be physically impossible. That the internal layers boil away to a kind of jelly and become liquid, or at least mucilaginous, is also in opposition to the so often proved homogeneousness of the amylum substance. In short, the fact that a grain completely pressed to pieces, when boiled under the microscope, is seen to swell up in all its fragments, each of which forms a rag of transparent skin, is alone sufficient to answer all these questions in the negative.

Fritsche's opinion, that the internal layers escape when the external skin bursts, appears, considering the above, to be quite inadmissible. What, then, becomes of the layers? The new theory here propounded easily and satisfactorily answers the question—*they unfold themselves.* Of this the microscope will furnish the proof. As soon as the observer has learnt to follow the rapid progress of boiling, he will be able distinctly to see the separation of the seam where the edges of the bands are united; he will see immediately after this, that the folds thus laid free are pressed forward, spread out, and in large grains are laid in a wreath of folds around the flatter central part. While the bands are shrinking back, the interior edge thus being loosened is not visible, which is very natural, because a *double* vesicle has been rolled up, the external skin of which on unfolding is drawn over the internal one, and the real edge is thus concealed.

The longitudinal wrinkle generally seen in the boiled grains cannot be easily accounted for on the old system. But according to the new one, it appears that the vesicle which has been rolled up and kept in tension is extended all round; and when laid up flatly in its watch-glass-like shape produces a fold, wide in the middle and pointed at both ends, which, with ovate vesicles, exactly coincides with their long axes, precisely as observation demonstrates.

I have before remarked, that during the growing together of the bands, the parts around the so-called kernel must be condensed, a supposition also verified by observation; for no sooner does the smaller band separate, than the grain, especially a large one, extends with a jerk, as though forced by a spring just liberated. The external appearance, even of the layers, with their angular and frequently broken forms, is more in accordance with their rolling up than with their free formation.

As to the number of layers, four full windings would, as the vesicle is double, produce seventeen apparent layers, without counting the bends, which most probably are produced parallel to the windings in the *inside,* and which to the eye appear like layers.

Finally, let us consider the phænomena when the grains are viewed by polarized light. Why is it that they exhibit a black cross? The answer according to the old theory will be, that it is in consequence of the layers varying in density. But what answer will be given to the question, Why do the layers vary in density? Without calling in aid a new hypothesis, that the layers are formed from within (?), this question cannot be answered by the mere formation of layers; whilst the rolling up and ultimate growing together of the band produces, as already observed, a condensation or pressure of the parts around the so-called kernel. Hence the amylum grain resembles an unannealed glass disc, and consequently in polarized light it must exhibit the coloured cross.

8. Many additional proofs of my theory might be advanced, but I have no wish to trespass on the patience of the reader, and therefore will leave the discovery of contradictions to my adversaries; for had I myself found the slightest inconsistency, in a physical point of view, I would at once have rejected the whole theory or admitted its weakness. In conclusion, I will merely observe that, according to my experience, this theory will apply to other kinds of amylum, all of which, as far as I have hitherto seen, produce after boiling the vesicle as described, and in their unfolding perfectly agree with their different kinds of *curl*. My sincere wish is, that my observations may soon be either fully confirmed or completely refuted; a proceeding, which cannot, however, be performed by mere arguments, but must be achieved by observation on the stage of the microscope itself.

XL. *On a Remarkable Property of the Diamond. By* Sir David Brewster, *K.H., D.C.L., F.R.S., and V.P.R.S. Ed.**

[With a Plate.]

HAVING had occasion, some years ago†, to examine the structure of a diamond plano-convex lens which gave triple images of minute microscopic objects, I discovered, by a particular method of observation, that the whole of its plane surface was covered with hundreds of minute bands, some reflecting more and some less light; and I naturally drew the inference that this diamond consisted of a great number of layers of different reflective, and consequently refractive powers, from which arose all its imperfections as a single microscope. In this case the veins or layers lay parallel, or nearly so, to the axis of the lens, so as to produce the worst effect upon the refracted pencil; for if the axis of the lens had been perpendicular to the surfaces

* From the Phil. Trans. 1841, pp. 41, 42.
† This Journal, vol. vii. p. 245.

of these veins, its performance as a microscope would scarcely have been injured by them.

In repeating Mr. Airy's experiments on the action of the diamond in modifying Newton's rings near the polarizing angle, I was led to re-examine the flat surface of the diamond above mentioned; but though I found my former observations perfectly correct, yet I was induced to suspect the accuracy of the inference which I drew from them, and which I could not but draw in the circumstances under which the phænomenon was presented to me.

In order that the Society may be able to judge of the new results at which I have arrived, I have given in Plate VI. fig. 4 as accurate a drawing as I am able to make of the appearance of the flat surface of the diamond under consideration, as seen by light incident upon it nearly perpendicularly. The flat surface of the diamond is 0·058, or $\frac{1}{17}$th of an inch in diameter, and owing to the great convexity of its other surface, the light reflected by it does not interfere with the examination of the structure above mentioned.

The appearance shown in the figure is that which I observed some years ago; but upon shifting the line of illumination, I was surprised to perceive that *all the dark bands became light ones, and all the light bands became dark ones,* a phænomenon which placed it beyond a doubt that all *the bands were the edges of veins or laminæ whose visible terminations were inclined at different angles, not exceeding two or three seconds to the general surface.* Had this surface been an original face of the crystal there would have been nothing surprising in its structure, excepting the exceeding minuteness of the strata and the slight inclination of their terminal planes to each other; but being a surface ground and polished by art, the phænomenon which it presents is one extremely interesting.

The mineralogist will have no hesitation in admitting that this diamond is part of a composite crystal consisting of a great number of individual crystals, like certain specimens of *felspar, carbonate of lime,* and other minerals; but it is more difficult to conceive that the terminal planes of these individual crystals should retain their relative inclination after undergoing the operations of grinding and polishing upon a lapidary's wheel.

To many persons such a result may appear inadmissible; but there are several physical facts, which, when well considered, cannot fail to diminish its improbability. If we grind and polish a surface of *mother-of-pearl* obliquely to the strata of which it is composed, we shall find it impossible to produce a perfectly flat surface: even if we grind it on the finest and softest hone, and polish it with the smoothest powder, the termination of each stratum will remain; and while the general surface reflects a white image, the grooves or striæ will give rise to the beautiful prismatic images produced by interference*.

Another analogous fact presented itself to me many years ago in examining *calcareous spar*. Having had occasion to form an artificial face upon one of the edges of the rhomb containing the obtuse angle, I used a coarse file without water, and found that it exposed faces of cleavage which had never been previously seen, and which were inclined to the general surface produced by the file†.

In examining the optical figures produced by the disintegration of crystallized surfaces, I have found that by coarse sandstone, or the action of a rasp, or large-toothed file, we can expose surfaces of crystallization with their natural polish differently inclined to the general surface‡.

In all these cases the faces, exposed by the mechanical action of grinding or filing, preserve their natural surfaces and polish, and will preserve them more perfectly and readily if they are faces of easy cleavage. The facility of exposing such faces by the action of grinding must increase as the veins or strata become thinner, and it is probable that their exceeding minuteness in the diamond may have aided in the production of the structure which has been described.

I have found it quite impossible to measure the inclination of any of the faces by the goniometer; but I have succeeded, though with some difficulty, in taking an impression of the grooved surface upon wax.

This structure sufficiently explains the existence of three images when the lens was used as a microscope, without supposing that the veins had different refractive powers. Faces of different inclinations would, of course, converge the rays to different foci on the retina, as effectually as if there had been only a variation in their refractive indices.

St. Leonard's College, St. Andrews,
February 11, 1841.

XLI. *Geometry and Geometers. Collected by the late* Thomas Stephens Davies, *F.R.S.L. & E. &c.*§

No. IX.

[Continued from vol. ii. p. 446.]

THIS appears to be an appropriate occasion for offering a few suggestions for the consideration of geometers respecting the ancient geometry and its modern cultivators.

* See Philosophical Transactions, 1814.

† Edinburgh Journal of Science, Oct. 1828, vol. ix. p. 312.

‡ Trans. Royal Soc. Edinb. vol. xiv.

§ Communicated by James Cockle, Esq., M.A., Barrister-at-Law, who adds the following note:—

["Unlike the two papers of this series, which I have already forwarded

The charge most commonly made against the science is, the total absence of general methods of research, both as respects construction and demonstration. It is alleged, that of two properties of a figure intimately related as to their subject matter, the demonstration of the one furnishes no clue to the demonstration of the other; and that the most elegant construction of a problem fails to facilitate the construction of one nearly kindred to it—often, indeed, of a converse problem. "All is isolated," it is said; "and it rather requires a certain kind of haphazard dexterity of mind than the application of general methods to make an able geometer. In the coordinate geometry, on the contrary, we can always depend upon obtaining a solution, since we can always reduce the conditions into the form of equations, which only require the ordinary resources of algebraic transformation to complete the inquiry."

No doubt there is a certain degree of truth in this, but there is yet a greater degree of misapprehension. Still, the inference being made from the general writings of geometers, and that too from the survey which an unpractised mind is obliged to take, even the misapprehension is pardonable. The brevity with which geometers put down their steps (consisting only of what constructions they make in the individual case before them, and the relations which successively result amongst the parts of the figure), without the slightest reference as to why they adopted their special method, tends very much to justify the opinion to the mind of the uninitiated, that the ancient geometry is a system of special expedients, each adapted to the individual case, like the solution of an enigma, and the whole incapable of reduction to any general principles.

There is also another very plausible ground for the inference. It cannot be denied that nearly all geometers, however much they may add to the details of the science in the way of theorems or problems, do yet pursue it merely as a *technical system.* Their only ambition is "to discover new truths," to make *mere*

to the Philosophical Magazine, there is nothing on the face of the above autograph of Professor Davies to indicate with certainty, or to afford anything like a conclusive inference, that he intended it to occupy its present position. I am responsible for the title given to it. But, even if its original destination be doubtful, it may with great propriety form part of this set of articles. The manuscript now forwarded is a portion of a longer autograph of Mr. Davies, which I have divided into two portions, thinking that such a form would be more convenient for publication. When this has appeared in print, I shall forward the other part for insertion in this admirable Journal.

"James Cockle.

"2 Pump Court, Temple,
December 20, 1851."]

deductions from previously established properties. What relations these new truths have to any previously known, in respect of systematic classification, they know not, and they care not; the mission of these geometers is fulfilled in making the deduction, and upon this they rest their hopes of distinction as geometers. Yet in reality they are but "the hewers of wood and drawers of water" for geometry. They are analogous to the ingenious, but unreasoning, experimenters who abound in physical science; they are, to use the language of Hartsoker, the *manouvrières* of the philosopher, whether geometrical or physical. At the same time they are as necessary in all sciences as the "hod-man" is to the builder, or the "bellows-boy" to the organist; and happily they are found to exist in abundance, or unhappily in such superabundance as to create the desire for a large promotion of them into the order of actual philosophers. The consequence is, that there already exists such an immense mass of theorems and problems relating to the ancient geometry, scattered in the most sibylline confusion, and without the slightest indication of connection, that they may be deemed as useless as the unreduced accumulations of an observatory; or, indeed, worse than these, for observations are so kept together that they *can be* reduced, whilst the labour of the whole life of a geometer would not suffice to reduce into order (both as to subject and method) the accumulations of English geometry within the last hundred years.

The mathematician who looks at these accumulations in their present unreduced state, and considers them to be the end at which geometry proposes to terminate, may well be excused for the opinion he forms unfavourable to this form of the science. Yet this is not an inherent vice of geometry; though it may and does result from the inherent mental indolence of the geometer himself. He satisfies himself with the deduction, and affects to consider everything which relates to classification or method as "too speculative" for so able a geometer as he is! He "leaves it to the talkers who call themselves philosophers, but who cannot solve problems, to amuse themselves about such trivialities." The truth however is, in the language of an eminent living philosopher, the "contest of mind" which this requires is such as to transcend the powers of the great majority of men, even though their problem-solving powers may be altogether unquestioned.

Nor, if the inquirer apply directly for information on the subject from those geometers who are the most adroit in this class of deductions and constructions, does he find much to enlighten him. Such a geometer will at once sit down and analyse a problem or a theorem proposed to him; and in most cases obtain a solution, sometimes of considerable elegance, however complex the proposition may be; but if he be asked *why* he employed some particular method, or *how* he knew it would answer his purpose, he can give no other reason than that he "knew" (or he "thought," or he "perceived") that it would be effective. It thus gives to his processes the appearance of being the result of *mere tact* or *quickness of perception*; and tends to support the view, that geometry, even in the hands of its best cultivators, is only a system of expedients, that rather requires rapidity of apprehension than profundity of intellect. The vanity too of men who set a high value on present reputation, rather than on great efforts, is gratified by this character for "quickness" and "cleverness;" and instead of admitting that they are governed by certain principles (though perhaps seldom or never enunciated, even to themselves), they allow an erroneous opinion to exist uncontradicted and unquestioned. In many cases, however, these men have acquired the *use of principles* from long habit in the imitative processes, without ever having attempted to enunciate them in words, or to reflect further upon the processes to which they apply than to the special case immediately before them. I have known men eminently skilful in the use of the geometrical analysis, who were yet unable to give the least explanation of the principle on which it is based, or of the rules that governed them in the employment of the method. All they could tell me was, that it "answered the purpose admirably;" and more than this they knew not, nor cared to know. When I asked how I must proceed to acquire this knowledge and its concomitant power, the answer was, "study good examples." Upon this resource I was thrown, and it certainly was effective. The little tract of Lawson and the second volume of Leslie's Geometry were of some use; but in looking back upon that period when I did first study it, I have often regretted that some work more adapted to the wants of the student, and in *direct* illustration of the principles, has not been supplied.

The student has within the last three years, however, been placed in a considerably better position for the study of the geometrical analysis, by Mr. Potts's Appendix to his 8vo Euclid, 1847. The attempt to comprise under one enunciation a description of the analysis of theorems and problems has been abandoned, and the nature of the process is, in both cases, rendered intelligible. They are indeed so different in their character and details, that when we see the effect of their separation, we can only wonder that they should have ever been united*.

* In confirmation of the vagueness with which ordinary writers express their views on this subject, I copy the following from a work of considerable mathematical pretensions, published only about seven years ago:—"*Analysis*, or the *Analytic Method*, is that by which a remote truth is discovered

It can only have arisen from a passion for verbal generalization, whether the philosophy of the subject would admit of it or not. Some parts of Mr. Potts's discourses on the subject might be amplified with advantage, and especially with respect both to more numerous and more elaborate examples, and to the character of analysis when applied to local and indeterminate propositions. I am not altogether without the hope that these changes will hereafter be made.

XLII. *On the supposed Identity of the Agent concerned in the Phænomena of ordinary Electricity, Voltaic Electricity, Electro-magnetism, Magneto-electricity, and Thermo-electricity. By* M. Donovan, *Esq., M.R.I.A.*

[Continued from p. 213.]

Section III.

In furtherance of the objects described in the preceding section, Professor Faraday has made experiments to determine the quantity of electricity associated with the particles or atoms of matter. He says it is wonderful to observe how small a quantity of a compound body is decomposed by a certain portion of electricity. One grain of water will require for decomposition an electric current "equal to a very powerful flash of lightning*." Elsewhere he says, "the chemical action of a grain of water upon four grains of zinc can evolve electricity equal in quantity to that of a powerful thunder-storm†." And he further declares, that from his experiments "it would appear that 800,000 such charges of the Leyden battery would be necessary to supply electricity sufficient to decompose a single grain of water‡." The Leyden battery to which he here alludes consists of fifteen jars containing 3150 square inches, that is, about $24\frac{1}{2}$ square feet of coated glass, charged by thirty turns of a plate electrical machine, the plate being 50 inches in diameter, and of immense power, giving ten to twelve sparks an inch long for each revolution.

The estimate that 800,000 discharges of the battery of fifteen

by assuming that what is required to be done is done, and then by reasoning from the more complex to the more simple, finally arriving at a known truth. Analysis is the method usually employed in algebra."

Comment on this would be superfluous. Even the second sentence, repeated as it has been parrot-like from D'Alembert's calling algebra "analysis" down to our own time, is not more than partially true. It is usually true as regards the solution of problems, but very rarely so in the investigation of theorems. D'Alembert greatly confused our conceptions of science by that unfortunate substitution.

* Researches, par. 853. † Ibid. 873. ‡ Ibid. 861.

jars, equal to a powerful flash of lightning, would be necessary to resolve a single grain of water into its elements is certainly astounding, when it is recollected that, according to Faraday*, the quantity of electricity that decomposes a body is the equivalent quantity of electricity that had previously held the elements of that body in combination; for he, with Davy and others, conceives that electricity and chemical affinity are identical powers. Hence in one grain, that is, one drop of water, there must be, naturally existing and constituting the affinity between its oxygen and hydrogen, no less a quantity of electricity than 800,000 charges of a battery containing 3510 square inches of coated glass, or the equivalent of "a very powerful flash of lightning." If this quantity of electricity were converted into one spark, it would be 4166 miles in length, taking Professor Faraday's mean estimate of one charge of his battery as the basis of calculation. Can this exist in a drop of water?

Faraday's expressed opinion on this subject is not an hyperbole, intended to exalt the conception of the quantity of electricity in the drop: he means it literally, and the admission of it is necessary to the alleged identity of common and voltaic electricity. Who that has heard a near clap of thunder, which makes the very ground on which he stands tremble, or that has seen the awful flash which prostrates buildings, melts masses of iron, strikes deep cavities in the earth, and kills the largest animals, can reconcile to himself that the cause of all this destruction is contained in a drop of water?

With regard to the quantity of ordinary electricity necessary for the decomposition of a certain quantity of water, there are other estimates on record which it may be proper to compare with those of Faraday, as they are founded on experiments conducted with great attention to accuracy, and with immense labour. In 1789, a set of experiments was published by the associated Dutch chemists, MM. Paets Van Troostwyk, and Deiman†. By passing 600 discharges of ordinary electricity, from a jar containing one square foot of coated surface, through a slender tube containing distilled water previously freed from air, they obtained, in the only experiment exactly stated by them, a quantity of mixed oxygen and hydrogen, which stood three-eighths of an inch high in a tube one-eighth of an inch in diameter English measure. Whoever will take the trouble of the calculation will find, that at this rate, in order to decompose a grain of water, no less than 1,033,792 such discharges would be required. But as these were discharges from a jar containing one foot only of coated surface, whereas Professor Faraday's discharges were from 24·4 square feet, when the former are converted into the latter,

* Researches, par. 862. † *Journal de Physique*, vol. xxxv. p. 369.

in order to form a comparison, the number of discharges of the Dutch chemists was in effect but 42·677. But it was afterwards shown by Dr. Pearson, that in these experiments not more than half the quantity of electricity employed by the Dutch chemists was active in decomposing the water, owing to the too great distance of the conducting wires from each other within the tube; thus would their estimate be at once reduced to 21,389 discharges. Faraday's estimate for the same duty is 800,000 such discharges, or nearly thirty-eight times greater than that of the Dutch chemists.

In 1797, Dr. Pearson, assisted by Mr. Cuthbertson, made a set of experiments on this subject with unexampled labour. In the only experiment completely reported by Dr. Pearson, it appears that from 16,836 discharges of a jar, containing 150 square inches of coated surface, he obtained almost half a cubic inch of the mixed gas*. Hence 33,672 discharges would furnish almost a cubic inch of the mixed gas; and we may call 35,000 such discharges equal to an exact cubic inch. Therefore to obtain 7863 cubic inches of the two gases, which together constitute one grain of water, 275,205 discharges should be employed if the resulting mixed gas were pure. But five-eighths of it only were pure; hence 440,328 discharges would be required to produce 7863 cubic inches of pure gas from Dr. Pearson's Leyden jar of 150 square inches of coated surface. Now as Pearson's discharges were made from a Leyden jar of 150 square inches of coated surface, while Faraday's were from a battery containing 24·4 square feet, when the former are converted into the latter, the number of Pearson's discharges required for the decomposition of one grain of water would be reduced to 18,817, which is forty-two times less than Faraday's estimate.

The estimate of Van Troostwyk and Deiman, corrected according to Pearson, is 21,184 discharges for the decomposition of one grain of water; that of Pearson is 18,817; the difference is 2367. This is as close an agreement as could well be expected in a comparison of such experiments. But the vast difference of Faraday's 800,000 discharges, forty-two times greater than Pearson's estimate, is very striking, and leads to some suspicion of the universality of the law as laid down by that philosopher, namely, that water when subjected to the influence of the electric current, no matter what the intensity or the acting surface so that the quantity be the same, the quantity decomposed will be exactly proportionate to the quantity of electricity which has passed†. All this may be very true when applied to the voltaic influence; but if so, the law seems to individualize common

* Philosophical Transactions, vol. lxxxvii. p. 152.

† Researches, pars. 732, 726.

electricity, and to dissever it from its alleged identity with voltaic electricity. When we find two estimates of an effect to agree pretty well, while a third is forty-two times greater than one, and thirty-eight times greater than the other, it is plain there is a monstrous error somewhere; and hence, before we can venture to draw any conclusion, it will be proper to investigate the grounds on which the discordant opinion has been formed. This becomes the more necessary, when it is recollected that the stronghold of those who maintain the identity of the voltaic and electric agents is the almost unlimited supply of the latter at a low intensity, which they affirm can be brought into action during the exhibition of any phænomenon caused by the former.

Faraday has estimated, as has been already observed, that one grain of water decomposed by four grains of zinc can evolve electricity equal in quantity to that of a powerful thunder-storm, to a flash of lightning, and to 800,000 charges of a Leyden battery, consisting of $24\frac{1}{2}$ square feet of coated surface, charged each time with thirty turns of a powerful plate electrical machine, each turn of the plate giving ten or twelve sparks of one inch in length.

This is no trivial quantity; and it ought to be easy to obtain powerful if not fearful manifestations of its presence. In order to set this matter in a clear point of view, I made a few experiments, the result of which it was easy to foresee; and they were made as illustrations of the nature of my objections to the doctrine impugned, as topics to reason on, as facts to found calculations upon, rather than as instruments of research.

Having prepared a piece of very thin zinc-foil weighing four grains, and in surface measuring five-eighths of an inch by one inch, and also a plate of platinum five times the surface of the zinc, I connected each with one of the gold leaves of an electrometer, the detached gold leaves of which were separately insulated, and were moveable towards or from each other by means of glass handles. The gold leaves were then moved towards each other, until they and the brass arms from which they hung were in good contact. An insulated vessel containing sulphuric acid, at that moment diluted with double its bulk of water, was prepared; and while the mixture was still very hot, the zinc and platinum plates were immersed. The platinum gave off hydrogen, and the zinc was dissolved in $1\frac{1}{2}$ minute. While the solution of the zinc was in progress, the gold leaves were gently separated by their glass handles, and approached again until they touched: there was not the slightest appearance of attraction or repulsion when they were separated or approached. According to Faraday's estimate, electricity equal to no less than 240 millions of one-inch sparks passed through the gold leaves in $1\frac{1}{2}$ minute without the smallest evidence of its presence. How different would have been the result if even the smallest spark of common electricity had acted on the leaves! the attraction or repulsion would be sufficient to destroy them by the mere mechanical violence of the effort. But, in point of fact, a voltaic arrangement always renders one of the gold leaves positive and the other negative; if the series be adequate, the gold leaves manifest a decided attraction; and if in contact, will not separate without the application of a countervailing force. Such an attraction and adhesion I found to be evidenced by this differential electrometer, when the gold leaves were connected with so small a voltaic series as twenty pairs of plates, each three-quarters of a square inch in surface, and arranged as a *couronne des tasses*.

When we consider that, during the solution of the zinc in the foregoing experiment, no less than 240 millions of one-inch sparks are supposed to have passed, that is, nearly 2,700,000 in each second of time, the mind becomes bewildered by the inconceivable velocity of such a succession; and we cannot fail to be struck with the quiet transit of a flash of lightning through the gold leaves without melting them, or even producing the attraction or repulsion which a bit of excited sealing-wax would have done. The passage of this quantity of electricity in this almost instantaneous space of time through gold leaves, each weighing the one-fiftieth of a grain, must produce such an enormous intensity as would cause the dissipation of both, and the destruction of the whole apparatus with an awful flash.

Lest any should suppose that this quantity had been really evolved, but had been dissipated or lost in some unaccountable way, I made the following experiment, the result of which could have been easily anticipated; but simple as the experiment was, I did not choose to use it as an argument without making it. Two glass matrasses were procured, the necks of which were of the same diameter, and could easily be joined into one continuous straight neck by being melted at the lamp. Into one of these was introduced a bit of zinc-foil, of one inch by five-eighths surface, weighing four grains, soldered to the edge of a piece of copper double the surface of the zinc. The bottom of this matrass was coated outside with tin-foil. Into the other matrass was introduced dilute sulphuric acid, one-third of which by measure was concentrated acid. The two matrasses, held with their necks horizontally, were now joined by melting at the lamp, so that they were perfectly air-tight. This being done, and the glass cold, the double matrass with its tin-foil coating was laid on a condensing gold-leaf electrometer, and confined there in an insulated state. The double matrass being now in a vertical position, the acid ran down into the lower part which contained the compound metallic plate. The copper gave off hydrogen abundantly, and the zinc dissolved in two or three minutes entirely: there was not sufficient pressure of condensed hydrogen to burst the vessel. Not the slightest divergence of the gold leaves resulted; yet such was the dry state of the atmosphere (Feb. 13), that a bit of letter-paper merely touched with the hand, scarcely rubbed, caused the leaves to strike the sides of the electrometer.

In this experiment, the lower matrass being coated outside with tin-foil, and the inside covered with a liquid conductor, the whole is to be considered a Leyden phial hermetically sealed, in which 240 millions of one-inch sparks were called into action. As a Leyden phial can receive no charge in the inside without manifesting on its outside a quantity of electricity equal, although opposite to what it has received, it follows that there could have been no evolution or dissipation of free electricity within the matrass, as there was no effect on the gold leaves of the electrometer. What, then, became of the enormous quantity of electricity, which, according to Faraday's estimate, was here rendered active?

To this question it may be answered, that the electricity being positive and negative, the two states neutralized each other as fast as generated, and hence there was none in the free state.

There was a mode, however, of discovering whether such a reunion took place, and a very obvious one. It is a fact, that in the case of a single voltaic circle properly excited, like that above described, if the pair of plates be made to communicate by a fine platinum wire, the current of electricity, in the positive and negative states, will pass from the plates through the wires in contrary directions; and the reunion taking place in the wire, it will, if very short and thin, be ignited in consequence. Dr. Wollaston's thimble battery is an instance on a minute scale: his zinc plate was only three-quarters of a square inch; the other plate acted also as the containing vessel for the acid, and consisted of a silver thimble so far flattened that it held the zinc and the exciting liquid. Small as this voltaic arrangement was, it ignited one-thirtieth of an inch of an exceedingly fine platinum wire.

I therefore endeavoured to test the truth of the supposition that the above-mentioned enormous quantity of electricity might have been developed in the positive and negative states, and by reunion in the connecting wire had been neutralized and lost. A zinc plate, twice the length of the former one, and therefore weighing eight grains, was connected to a plate of platinum, of about the same surface, by means of a platinum wire half an inch long and $\frac{1}{100}$dth of an inch in diameter. This combination was introduced into a matrass, and so placed that the two plates stood vertically, and the wire horizontally, when the matrass lay on its side; and in this position the plates were secured by a little sealing-wax previously adhering to their edges, and now melted by heating the glass. A quantity of dilute sulphuric acid, of which one-third was concentrated acid, being introduced into another matrass with a neck of equal diameter with the former, both necks were joined by melting in a glass-blower's lamp. This done, and the glass cold, the end of the double matrass which contained the acid was elevated until the acid trickled down into that part where the pair of plates was cemented, and covered about half the height of the plates, the compound vessel lying horizontally on a gold-leaf electrometer, and that part of the matrass being externally coated with tin-foil. The portion of zinc exposed to the acid, *i. e.* four grains, was dissolved in about two minutes and a half; but the platinum wire was not in the most obscure degree reddened, although examined in the dark, its length and thickness being too grêat for the heating power of the voltaic combination; neither was there the slightest effect on the electrometer.

If, then, according to the supposition which I am endeavouring to disprove, the electricity arising from the solution of four grains of zinc, that is, 240 millions positive and negative of one-inch sparks, had passed through the platinum wire, at the rate of 1,600,000 per second, need it be inquired what would have become of the wire, nay, the whole apparatus and the operator? Van Marum, with one discharge of the great Leyden battery, consisting of 225 square feet of coated glass, melted forty feet of iron wire $\frac{1}{240}$th inch diameter*.

From both of these experiments with hermetically sealed matrasses, I infer that no electricity was evolved in a free or dispersed state during the voltaic solution of four grains of zinc; that no such quantity of electricity as has been supposed, nor the millionth part of it, passed through the platinum wire. Where, then, are we to look for the enormous quantity of this subtile fluid which is supposed to be the result of the voltaic solution of the zinc? The supporters of the doctrine here objected to may maintain that the alleged quantity was really in operation during the separation of the elements of the grain of water by four grains of zinc, but that it was retained and concealed in the constitution of the resulting gases. This seems to be the opinion of Faraday: he thus expresses himself: "in the combination of oxygen and hydrogen to produce water, electric powers to a most enormous amount are for the time active†."

* *Déscript. d'une très grande Mach. Elect. à Haarlem.* Prem. Cont. p.10.
† Researches, par. 960.

Again, he says, "I cannot refrain from recalling here the beautiful idea put forth, I believe, by Berzelius in his development of his views of the electro-chemical theory of affinity, that the heat and light evolved during cases of powerful combination are the consequence of the electric discharge (of positive and negative electricity) which is at the moment taking place. This idea is in *perfect accordance with the view I have taken of the quantity* of electricity associated with the particles of matter." It may be added that this was also the opinion of Sir H. Davy. But if the electric discharge thus constitute heat and light, how comes it to pass, that in the case of the small galvanic conductor just alluded to, the 240 millions of one-inch sparks condensed into heat and light (that is fire) did not boil and evaporate the water, melt the connecting wires, and destroy the whole apparatus?

All this should happen unless it be supposed that the evolved electricities enter into the composition of the resulting gases; and that they do not will presently appear. If it be conceded, that when a drop of water is resolved by electricity, and of course by any other means, into its constituent gases, the gaseous mixture, amounting to 7863 cubic inches, holds associated with it electricity to the amount of 240 millions of one-inch sparks; and if it be admitted that when the two gases recombine to form a grain of water, the two electricities or electric states, by their neutralization, produce the flash and explosion, these ought to amount to a flash of lightning and a clap of thunder, instead of the bright little flame and trivial crack which the detonation of seven or eight cubic inches of mixed oxygen and hydrogen produce when they are burnt. Really this is not an exaggerated conclusion. Professor Faraday himself everywhere compares the electricity of a drop of water to a flash of lightning, and surely I have a right to add the clap of thunder as a natural consequence.

But there is another difficulty in the way of the theory beside that of accounting for the disappearance of the electricity, alleged to be equal to 240 millions of one-inch sparks, which is said to be concerned in the decomposition of one grain of water by four grains of zinc, and which I have endeavoured to prove in my two experiments to be neither dissipated, nor reunited in the wire, nor combined in the resulting mixture of oxygen and hydrogen. Professor Faraday lays down as an essential principle, that "the electricity which decomposes, and that which is evolved by the decomposition of a certain quantity of matter, are alike*." He conceives that during the action of a voltaic combination, consisting of two metals, on acidulated water, such as I employed in my two experiments, the electricity evolved during the oxida-

* Researches, par. 868, *et alibi.*

tion of the zinc is that which decomposes the water*, and "is simply employed in overcoming electrical powers in the body (water) subject to its action:" "the quantity of electricity is dependent upon the quantity of zinc oxidized†;" and he conceives "that the quantity which passes is the equivalent of, and therefore equal to that of the particles separated; *i. e.* that if the electrical power which holds the elements of a grain of water in combination, or which makes a grain of oxygen and hydrogen in the right proportions unite into water when they are made to combine, could be thrown into a current, it would exactly equal the current required for the separation of that grain of water into its elements again." Elsewhere he says, "considering the definite relations of electricity as developed in the preceding parts of the present paper, the results prove that the quantity of electricity, which, being naturally associated with the particles of matter, gives them their combining power, is able when thrown into a current to separate those particles from their state of combination; or in other words, that the electricity which decomposes, and that which is evolved by the decomposition of a certain quantity of matter, are alike." I must, however, observe, that he has elsewhere made statements which have caused me much embarrassment in my unsuccessful endeavours to reconcile them: no doubt a fuller exposition on his part would have removed all difficulties of this kind. As it is, I have no other course left than to reason upon the different results which flow from the passages above quoted, and are reiterated in other parts of his Researches.

Thus, in the decomposition of a grain of water, the electricity, which being identical with affinity, had held the oxygen and hydrogen in combination, is evolved; but it must have been evolved by the power of an equal quantity of electricity produced by the oxidation of the four grains of zinc, and acting as a current through the water. If the decomposition had been effected, as stated, by the equivalent of 240 millions of one-inch sparks, we have 480 millions of such sparks to account for. Where is this enormous quantity of electricity to be detected? It cannot in my experiment have escaped out of the glass vessel; nor can it have remained in the resulting gases as a part of their constitution: it cannot have disappeared by neutralization of the two states, positive and negative, of which it consists; for in that case heat to an incredible amount must have been generated, the result of such an union being, as Faraday admits, fire, *i. e.* light and heat (868.). The amount of this heat may be judged from the quantity of iron which two flashes of lightning would be capable of melting or igniting, for such would be the equivalent

* Researches, par. 868, *et alibi.* † Ibid. 919.

according to his own calculation. If the ten-thousandth part of any such heat were generated in my experiment, what would have become of the whole apparatus?

Or even if we admit that the current consists of electricity which had previously existed in the water, associated with the particles of oxygen and hydrogen as their natural chemical affinity, for such Faraday views them, then the two electricities must have passed through the metals, as he admits, and should have destroyed them. Again, if the two electricities did not pass, why was the minute wire in Wollaston's thimble battery ignited? and this wire being ignited, why was not the water in my experiment heated to ebullition or total evaporation? In short, it were endless to express the objections which seem to beset this doctrine of the enormous quantity of electricity associated with the particles of matter, and which is indispensable to the hypothesis that quantity of electricity at a low intensity is capable of explaining all voltaic phænomena.

I hope that I have not misconceived Professor Faraday's views. Free to confess that I labour under the disadvantage of not wholly comprehending some of his opinions, I account for it by the fact, which he himself avows, that his doctrine is as yet incomplete. To guard as much as possible against misrepresentation, I have quoted his words, and have used them, as far as I could judge, in the sense therein implied, although I am aware that other passages might be selected from the "Researches," to reconcile which some additional helps from the author would be required.

[To be continued.]

XLIII. *On the Heat of Chemical Combination.* *By* Thomas Woods, *M.D.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen, Parsonstown, Feb. 1852.

I SHALL be obliged by your allowing the following to appear as an appendix to, or rather as a correction of an error in, my paper on the cause of the Heat of Chemical Combination published in this Magazine last January.

In some preliminary remarks on the changes which take place in a heating or cooling body, I endeavoured to show that *attraction* and *repulsion* between particles are not necessary to explain either expansion or contraction, if the law of *virtual velocities* be extended to those opposite movements which simultaneously occur; and that the expansion in one body is the compensation for the contraction in another; or that the quantity, so to speak, of proportionate volume in matter, or *heat* is always the same, and can neither be increased or diminished. Having referred to an experiment which shows that the nearer the particles of a body are to each other, the greater the effect they have in compensating an expansion or contraction in another, I endeavoured to show that this law holds good, not in some, but in all states in which a body may be placed. And I did so for this reason; that as it is generally imagined, the expansion of a body when its particles are more widely separated is greater than when they are more nearly together, because attraction has to a certain extent been overcome, if I could show that in those conditions of matter where attraction is not said to exist the same law was followed, it would argue that *attraction* is not necessary for the explanation. To prove this point, I showed that liquids change into vapours with an amount of expansion which is inversely as their atomic volume, or a multiple of it; and argued that, as in a certain bulk of atoms and space, the smaller the atom the larger the space, so the larger the space, or the greater the expansion already existing in the liquid state, the greater the subsequent expansion into vapour; but when liquids are expanding into vapour, attraction is not said to exist.

In proving that the same occurs when solids change into liquids, I said the amount of expansion could not be measured; I mean the amount of expansion between the atoms, because crystallization, &c. influenced the result; but as Person has shown that the latent heat of vapours is proportionate to their amount of expansion from the liquid state, I proposed to use the quantity of heat rendered latent when a solid becomes a liquid to express the atomic expansion that at the same time occurs, and thus find whether in this case also the expansion depended on the atomic volume. I here stated, that Person says the latent heat depended on the height of the fusing-point; and this is the error I wish to correct: not so much that it would materially affect my theory, but that when I discovered my mistake, and reasoned on the formula that Person really gives, I saw it proved clearly, that the expansion occurring when a solid changes to a liquid is influenced as to extent precisely as that of a liquid when it becomes a vapour; and that the amount of this expansion in both cases *is inversely as the atomic volume* taken in connexion with the boiling- and fusing-point.

Person shows (*Comptes Rendus,* vol. xxiii. pp. 327, 524), that if the temperature of the fusing-point of any body be increased by 160° C., and the result be multiplied by the difference of the specific heat in the solid and liquid state, the product will be equal to the latent heat. His formula is $(160+t)\times\delta=L$; where t is the temperature of the fusing-point, δ the difference of specific heat in the solid and liquid state, and L the latent heat. Now as Dulong and Petit show that the atomic weight is as the specific heat, we may substitute one for the other, and in the formula call the specific heat a; and as the specific heat increases with the expansion, in different states of a body it will be inversely as the specific gravity, or nearly so. Therefore we may say that the specific heat of a body in the solid and liquid state may be called $\frac{a}{s'}$ and $\frac{a}{s}$, where s and s' represent the specific gravity of the solid and liquid respectively. Then the δ of Person's formula may be replaced by $\overline{\frac{a}{s'} - \frac{a}{s}}$. But s' is s divided by the expansion of the body from the solid to the liquid state; and as we are taking L (or the latent heat) to represent that quantity, therefore $\frac{a}{s'} = \frac{a}{\frac{s}{L}}$, or $\frac{La}{s}$. Using this in the formula, we get

$$(160+t) \times \overline{\frac{La}{s} - \frac{a}{s}} = L, \text{ or } (160+t) \times \frac{a}{s} = \frac{L}{L-1}.$$

But as the greater $\frac{L}{L-1}$ is,—and consequently $\frac{a}{s}$ which represents the atomic volume—the less L is; *therefore for a given temperature connected with the fusing-point, the greater the expansion from the solid to the liquid state the less is the atomic volume.*

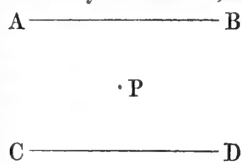
If in the change from a solid to a liquid state the amount of expansion were only due to atomic movement, that is, if crystallization or other structural arrangements did not cause *masses* to change relative places, L or the latent heat, and the expansion would be equal, as in the case of liquids becoming vapours; but on account of these arrangements, L and the expansion can be only approximations. Therefore in the subjoined table, where L is calculated by means of the formula $(160+t) \times \frac{a}{s} = \frac{L}{L-1}$, it is not meant to give the latent heat accurately, but merely to show that the *principle* of the substitution used above is correct. The numbers representing the latent heat are taken from Person's paper, as are also the fusing-points; the atomic volumes from Playfair and Joule's memoir published in the Transactions of the Chemical Society.

Name of substance.	Fusing-point plus 160° C. multiplied by atomic volume.	Latent heat divided by latent heat minus 1.	$\dfrac{(160+t)\frac{a}{s}}{\frac{L}{L-1}}$.
Tin	$235+160\times8{\cdot}1=3199$	$\frac{14}{13}$	2970
Bismuth	$270+160\times7{\cdot}2=3096$	$\frac{12\frac{1}{2}}{11\frac{1}{2}}$	2969
Zinc	$423+160\times4{\cdot}7=2720$	$\frac{27\frac{1}{2}}{26\frac{1}{2}}$	2700
Phosphorus	$44+160\times17{\cdot}=3468$	$\frac{4{\cdot}7}{3{\cdot}7}$	2730
Lead	$320+100\times9{\cdot}=4320$	$\frac{5}{4}$	3456
Ice	$160\times9{\cdot}8=1568$	$\frac{65}{64}$	1544

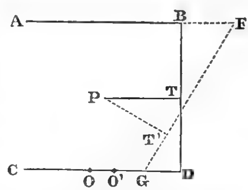
If L, the latent heat, were always equal to the expansion, the numbers in the last column should be equal, or multiples of each other: but I do not say it is equal to the expansion, it only approximates. In the case of *ice*, the table shows the expansion from the solid to the liquid state is only half what is represented by the latent heat. And this is evidently what is to be expected; for the contraction between *masses*, so to speak, whereby water is rendered heavier than ice, is opposed to the atomic expansion. In the case of lead, on the contrary, the expansion is somewhat greater. But generally the numbers show that the substitution of the *atomic* expansion for the latent heat would be correct if we knew exactly the amount. And therefore it is proved, *that as in the expansion of liquids into vapours, so in the expansion of solids into liquids, the amount of that expansion is, when taken in connexion with the fusing-point, inversely as the atomic volume or a multiple of it*; from which I deduce, that as the less the atom is the greater is the space, so the greater the expansion existing between the particles of matter, the greater the subsequent expansion *in all cases* for a given contraction in another body.

But in the formula $(160+t)\frac{a}{s}=\frac{L}{L-1}$, the less either factor in the left-hand side of the equation, the greater is L; and as the greater the expansion of a body is the less number of degrees of heat we must suppose it would require to be raised to its fusing-point, so the smaller t is, the greater we may take the expansion among its particles in the solid state to be; therefore from whatever other cause, as well as the smallness of the atomic volume, a greater expansion in the solid state exists, the greater will be the expansion of the body from that condition to the liquid.

The following explanation of expansion taking place in an increasing ratio does not require the supposition of a force gradually decreasing between the atoms. Instead of an action being exerted from particle to particle, let the operating cause of expansion and contraction be from one body to the other, and let the bodies be represented by the lines AB and CD. Now if a point P be taken in the centre of the figure, and that in the opposite movements of expansion and contraction the operating cause be represented by a line passing from one body to the other, and that this line be always at right angles to a line drawn from P, which point P, like the centre of gravity in a system of bodies moving equally in opposite directions does not change its place, the amount of expansion and contraction will be shown to be in an increasing ratio. Let the figure be completed. Join



B and D. Then BD represents the operating cause which preserves the bodies relatively equal. It is at right angles to PT. Now let AB expand and CD contract. If the operating cause pass still from one to the other at right angles to an equal line drawn from P, suppose PT′, then BF the increase is larger than GD the decrease; and by drawing in the same way other lines of force from one body to another, if they always pass at right angles to a line drawn from P, it can be shown that the amount of expansion will be a constantly increasing quantity.



It is evident that if a number of lines are thus drawn, the *locus* of their intersections (PT with FG, &c.) will be the circumference of a circle. Hence, as in the action of *masses* on each other—attraction—the operating cause has reference to a centre; in the case of expansion and contraction, or the phænomena of heat, it may be said to have reference to the circumference.

If C and D represent two dissimilar particles which can chemically unite, brought together at an insensible distance, so as to form one body, and that combination takes place between them so as to bring them to the points O and O′, by drawing lines from these points at right angles to a line equal to PT, we should see how greatly A.B, the body which compensates by its expansion for the coming together or contraction of C.D, must be expanded or heated; and this is the theory I advance to account for the cause of the heat of chemical combination.

XLIV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 235.]

Dec. 18, 1851. — THE following papers were read:—

"A Proof (by means of a series) that every Number is composed of 4 Square Numbers, or less, without reference to the properties of Prime Numbers." By Sir Frederick Pollock, Lord Chief Baron, F.R.S. &c.

The paper contains a proof, that if every number of the form $8n+4$ is composed of 4 odd squares, then every number whatever must be composed of 4 square numbers or less; also a proof of the converse of this, viz. that if every number is composed of 4 square numbers or less, then every number of the form $8n+4$ must be composed of 4 odd squares.

It is then proposed to show that every number of the form $8n+4$ is composed of 4 odd squares, by taking a number of the form $8n+4$, viz. an odd square $+3$, and showing that $8n+4$ in that case is divisible into 4 odd squares (other than the odd square and 1, 1, 1); thus $16n^2 \pm 8n+1$ is a form that includes every odd square, and $16n^2 \pm 8n+4$ is divisible into

$$\begin{aligned} &4n^2 \pm 4n+1, \\ &4n^2 \pm 4n+1, \\ &4n^2 \pm 4n+1, \\ &4n^2 \pm 4n+1. \end{aligned}$$

8 is then added, and the sum is shown to be still divisible into 4 odd squares; and again 8, and so on, until by successive additions of $8+8+8$, &c., the quantity added to $16n^2 \pm 8n$ becomes equal to the original term with which the operation commenced. The odd squares $+3$ form the series 4, 12, 28, 52, 84, &c.; and if the successive additions reach the third or some higher term, and also if the sum added to $16n^2 \pm 8n$ be equal to the term with which the operation commenced, it is contended the following term may be attained, and so on, and every number of the form $8n+4$ will be composed of 4 odd squares. The paper concludes by a suggestion that the method is applicable to several other similar problems.

2. "Observations on the Structure and Connexion of the Valves of the Human Heart." By W. Savory, Esq. Communicated by Edward Stanley, Esq., F.R.S.

The paper contains observations upon the structure and connections of the auriculo-ventricular and arterial valves of the human heart, which the author thinks will assist in explaining their nature and functions.

The relation of the "four orifices" in the base of the heart is examined, and it is shown that the aortic and left auriculo-ventricular apertures are not separated as the others are; that no muscular tissue of the ventricle intervenes between them, but that when the auricles and great vessels are separated from the ventricles (which may be accomplished with facility after prolonged boiling), the aortic aperture is separated from the left auriculo-ventricular only by the anterior mitral valve; and when this is removed (or even while it remains), it is plainly seen that only one aperture exists whose borders are formed by the muscular tissue of the ventricle, and in shape somewhat resembling the figure 8. This is divided into two portions, an anterior (aortic) and posterior (auriculo-ventricular) by the anterior mitral valve, and above it, by the posterior wall of the aorta, into which is inserted a large portion of the anterior wall of the left auricle, but no muscular tissue of the ventricle intervenes.

When the auricles and vessels are removed, it is seen that the three orifices are bounded by thick and convex borders formed by the bases of the ventricles. Those on the left side are broadest; the difference between the two sides corresponding with the difference in thickness between the walls of the ventricles. The formation of these muscular borders, and the general arrangement and direction of the muscular fibres at these parts, are examined. The fibres forming the walls of the ventricles converge around these apertures. The most superficial fibres may be traced up from the walls of the ventricles, curving obliquely over the convex border, and having their extremities, for the most part, fixed around the orifices. We may remove these fibres layer after layer, and still find the same arrangement to obtain, the deeper fibres lying more transversely and obliquely intersecting those above and below.

If now the auricles and great vessels which have been detached are replaced in their natural situation, it is observed that the auricles are connected with the *inner* surface of these convex borders, while the walls of the vessels pass down on to the outer surface. This fact is an important one when viewed in connection with the valves, and will be presently considered. In the mean time it may be remarked, that the formation of the auriculo-ventricular grooves in which the coronary vessels lie, is explained. These vessels are found in the angle between the border of the ventricles and the wall of the auricles.

The nature of the fibrous zones or tendinous circles surrounding the orifices is examined. These rings are in especial relation with the valves, being closely connected with their attached bases, and are not such distinct and independent structures as they have been hitherto considered. After referring to some previous descriptions of the arterial tendinous rings, the author attempts to show that what has been described as the upper and thickened festooned border, is the result of the attachment of the bases of the valves to the arterial coat, and is formed by an intimate union of the fibrous tissue composing the valves with the elastic coat of the artery.

(1) These festooned borders correspond exactly with the attached bases of the valves, and hence their shape. (2) They are thickest and most strongly marked at the angle formed by the junction of two valves, to which point the bands of fibrous tissue in the valves converge. (3) The microscope shows these festooned rings to be composed of a mixture of the white fibrous with the yellow elastic tissue,

an arrangement naturally to be expected from an intimate union of the tendinous tissue of the valve with the arterial coat.

The structure, connexions and relations of the valves is examined chiefly by means of vertical sections carried through their centres and adjacent parts. Such sections of the arterial valves disclose an important relation which they have with the upper border of the ventricles. The aorta and pulmonary artery, expanding towards their termination, are situated upon the outer edge of the ventricular border before described; the consequence of which arrangement is, that the portion of valve adjacent to the vessel passes over and rests upon the muscular substance, and is supported upon the inner border of the free edge of the ventricles surrounding the arterial orifices. This arrangement, in consequence of the small size of the parts, is not so obvious at the first glance in the human heart, but is more strikingly shown in an examination of the heart of any one of the larger animals. This appears of importance when viewed in connexion with the functions of the valves. The reflux of the blood is said to be sustained by the festooned rings at the base of the valves, but in fact they are thinnest at this very part, corresponding to the central portion of the convexity of the valves; and if the description previously given of the formation of the tendinous festooned rings be a correct one, it is obvious why it is so, the thicker portions being the projecting angle at the junction of two valves, to which points the tendinous fibres of the valves converge. Now, inasmuch as the posterior portion of the aortic orifice is continuous with the left auriculo-ventricular aperture, no muscular tissue of the ventricle existing at this part, the posterior aortic valve, and a portion of the adjacent one, have no support of this kind; but the muscular floor of the anterior aortic valve is especially broad, and it is the corresponding portion of the aorta which is particularly dilated, the posterior wall descending nearly vertically. The arrangement above described obtains in all three pulmonary valves; but as the border as well as the walls of the right ventricle are considerably thinner than those of the left, the muscular floor of these valves is much narrower than in the anterior aortic valve. All this is of course seen on a much larger scale in the hearts of the larger animals, as the Horse and Ox; and here, where the muscular floor of the valves (more especially the anterior aortic) is of very considerable breadth, the tendinous tissue of the valve may be traced over the muscular surface to form the wall of the vessel.

In the larger Ruminants there are found two considerable portions of bone, partly surrounding the orifice of the aorta; and smaller irregular fragments are occasionally observed between the principal pieces. The larger portions vary much in size and shape in different hearts even of the same species. They are usually elongated and curved. The chief bone, which exceeds the other considerably in size, embraces the whole of the right side, and the right half of the back part of the orifice of the aorta; while the little bone, not generally found in the smaller Ruminants, as the Sheep, its place being occupied by a portion of dense fibrous tissue, extends from the middle of the left side round to the posterior part, where it more or less nearly joins the extremity of the larger bone. Thus the lateral and posterior portions of the aortic orifice are surrounded by firm bony arches meeting posteriorly in the centre. From the large bone, a small process usually passes backwards for some distance into the muscular substance of the septum between the ventricles, and is gradually lost in the dense fibrous tissue found in this part, surrounding the right border of the left auriculo-ventricular aperture; and from the convex surface of the smaller portion, a thin process of dense fibrous tissue is continued round the left margins of the auriculo-ventricular orifice. These heart-bones are intimately connected above with the middle coat of the aorta, on the inner surface with the base of the adjacent arterial valves, and posteriorly with the anterior mitral valve; while at the sides, to their external and inferior surfaces, the muscular fibres of the ventricle are attached. They may be seen and felt in the base of the pouches formed by the two posterior aortic valves, and no doubt greatly assist in sustaining the "force of the reflux." They occupy the position of the two posterior festoons of the aortic valves. In the human heart, in the situation corresponding to the position of these heart-bones, the tissue composing the festooned rings is thicker and denser than elsewhere, offering to the knife, in some cases, almost the resistance of bone. The processes of dense fibrous tissue found in the anterior portion of the border of the ventricular septum, &c., and extending round the right and left margins of the auriculo-ventricular orifice, are intimately connected with the thickened portions of the adjacent festoons.

Among the tissues entering into the structure of the arterial valves, elastic fibres are described. They exist not only in the corpus arantii, but delicate fibres of elastic tissue are found throughout the valve; most abundantly in the thicker portions, but even in the thinner portions (lunulæ) a few delicate but well-marked elastic fibres may be seen, particularly after the addition of acetic acid, which of course assists greatly in bringing them into view.

Muscular fibres have not been found in the arterial valves.

The structure and connexions of the auriculo-ventricular valves are next examined by means of vertical sections. In tracing down the muscular wall of the auricle, it is observed to pass on to the inner surface of the ventricular border, and if minutely examined is seen to terminate by two attachments. The external portion, which is considerably the larger, is closely connected with the fibrous structure forming the "auriculo-ventricular ring," while the thinner internal portion is continued forwards for a very short distance between the surfaces of the valve, and terminates more or less abruptly by an attachment to its tendinous tissue. This is generally best seen in one of the tricuspid valves, where, in a vertical section, the muscular fibres may be observed terminating beneath its upper surface immediately beyond its attachment to the ring. In the posterior mitral valve the muscular fibres seldom penetrate so far forwards, and this appears to result, when a section of the parts is examined, from the much greater thickness and density of the lining membrane of the left auricle.

The connexions of the anterior mitral valve, being peculiar, require a separate consideration. In dissecting down between the anterior wall of the left auricle and the posterior surface of the aorta, it is seen that the central fibres of the auricular wall are closely attached to the adjacent wall of the vessel. A little further dissection on either side will show that the muscular substance of the left ventricle is deficient between these parts. At the sides indeed it is found, but is gradually lost at some distance from the mesial line. Hence these two orifices (the aortic and left auriculo-ventricular) are not separated as the others are by the intervention of the muscular fibres of the ventricle. The structure and connexions of the anterior mitral valve are examined by means of a vertical section, including the posterior wall of the aorta and the anterior wall of the left auricle. If the lining membrane of the auricle be traced downwards, it is found to be directly continued on to the posterior surface of the valve, and the membrane on the anterior surface of the valve is continued upwards over the tendinous festooned ring of the aorta, on to the under surface of its semilunar valves. The anterior mitral valve lies beneath a portion of the two posterior arterial valves. The muscular wall of the auricle is observed to terminate by two distinct insertions. The anterior (the larger) division of fibres is attached to the posterior surface of the aorta, opposite to, and below the festooned ring, while the posterior portion is continued directly downwards for a short distance into the valve, and terminates by an attachment to its fibrous tissue. The posterior wall of the aorta descends nearly vertically. Suddenly becoming much thinner opposite the upper border of the semilunar valve, it is continued down to the festooned ring, or in other words, it here becomes blended with the base of the semilunar valves. Below this a dense layer of fibrous tissue (which exists below, and fills up the spaces between the attached bases of the semilunar valves) descends for some distance into the anterior mitral valve, immediately behind its anterior surface. It is by a close attachment to the posterior surface of this layer that the muscular fibres of the auricular wall which descend into the valve, terminate. This layer of fibrous tissue, however, may be generally traced downwards into the valve farther than the muscular fibres.

The boundary, then, between the aortic and auricular apertures is formed above the mitral valve by the posterior wall of the aorta, terminating at its junction with the bases of the semilunar valves, and immediately below the posterior surface of which is attached the greater portion of the muscular fibres forming the anterior wall of the left auricle. The extremities of the two bones which in ruminants replace a portion of the lateral and posterior divisions of the "festooned ring," nearly meeting in the centre, behind, give additional support to the structures entering into the formation of the mitral valve.

In examining the structure and connexions of the auriculo-ventricular valves, it is noticed that a considerable portion of tendinous fibres pass from the insertions of the cords, through the valves, to the zones, and many of the smaller cords pass up directly into the angle formed between the under surface of the valve and the inner surface of the ventricle, and at once enter into the formations of the fibrous zones. These cords are short, and many of them spring from the wall of the ventricle, behind the valve. Therefore it results, that these zones are densest and most strongly marked in those portions corresponding to the attached borders of the valves, and gradually become less distinct towards the intervals between them. Hence the greater portion of the auriculo-ventricular zones is more properly to be considered in connexion with the valves.

The fibres of elastic tissue exist in the auriculo-ventricular valves, but more sparingly than in the arterial valves.

The many contradictory statements which have been advanced concerning the existence of muscular fibres in the auriculo-ventricular valves, may perhaps be explained by a consideration of the mode in which the muscular fibres of the auricles terminate, which has been already described. The internal fibres which have been mentioned, descending from the auricular walls into the valves just beyond their attached margins, may be traced to a greater distance in some cases than in others. They generally terminate by a tolerably well-defined margin, but this varies. They usually descend for a greater distance between the layers of the anterior mitral valve, immediately beneath its auricular surface; but even here they are seldom found stretching far into the valve, not terminating, however, so abruptly.

Therefore, if a portion of the attached border of a valve immediately below its upper surface be examined, muscular fibres in abundance will generally be detected; whereas if sought for in any other portion of the valve far from its attached border, according to the foregoing observations, they will not be found.

March 25, 1852.—The following paper was read:—Experimental Researches in Electricity. Twenty-ninth Series. By Michael Faraday, Esq., D.C.L., F.R.S. &c.

In the present series of researches the author endeavours in the first place to establish the principles he announced in the last, with regard to the definite character of the lines of magnetic force, by results obtained experimentally with the magnetic force of the earth. For this purpose he reverts to the thick wire galvanometer before described, and points out the precautions respecting the cleanliness of the coils, the thickness and shortness of the conductors, the perfect contacts, effected either by soldering or cups of mercury; and marks the value of double observations, i. e. observations afforded on both sides of zero. The nature of the impulse on the needles is pointed out; being not that of a constant current for a limited or unlimited time, but of a given amount of electricity exerted, either regularly or irregularly, within a short period; and it is shown experimentally that such impulses produce equal results of deflection, and also that when two or more such impulses are given within a limited time, the whole arc of swing is nearly proportional to their number; so that the amount of deflection, *within certain limits,* indicates directly, nearly the proportion of electricity which has passed as a current through the instrument.

If a wire be formed into a square of 12 inches in the side, and then fixed on an axis passing across the middle parallel to two of its sides, and if, when that axis is perpendicular to the line of dip, the whole is rotated, then two of the sides of the rectangle will, in one revolution, twice intersect the lines of force of the earth passing across or through one square foot of area. The currents then tending to move in the upper and lower parts of the rectangle, will conjoin to urge one current through the wire; and if this wire be cut at one place close to the axis, and be there connected with a commutator of simple construction, which is described in the paper, the currents round the rectangle may be conveyed away to the galvanometer, and there measured. Such a rectangle, constructed of copper wire one-twentieth of an inch in thickness, gave a certain arc of swing for one revolution. If five or ten revolutions were made, within the time of vibrating of the needle, nearly five or ten times this amount of deflection was produced: the mean result, in the present case, was $2^{\circ}\cdot 624$ per revolution. When the same length of the same wire was arranged in oblong or oblate rectangles, so as to diminish the inclosed area in different directions as regarded the axis of revolution, still the deflection was in every case proportional to the areas included; showing that the effect produced was proportional to the number of lines of force intersected by the moving wire. The same result was obtained when two squares having areas in the proportion of 1 to 9, were employed.

When squares of the same area were formed of copper wire of different thicknesses, then the effects of obstruction in the conducting part of the system were brought out and measured. Thus, with wires which were 0·05, 0·1 and 0·2 of an inch in diameter, and therefore in mass as 1, 4, and 16, the deflections were 1, 2·78, and 3·45; a result almost identical with that obtained for the same wires by the use of loops and a local magnet in the former researches. When two equal rectangles were compared, one containing a single circuit of 4 feet of wire 0·1 in thickness, and the other four circuits of 16 feet of wire 0·05 in thickness, then the first was found to evolve the largest quantity of electricity; but the second, electricity of the highest tension, by the same amount of motion: the accordance of these results with the principles advanced is pointed out. The author then refers to the use of wire rings of one or many convolutions, and indicates cases in which they may supply valuable means of experimental inquiry.

The relative amount and disposition of the forces of a magnet when it is alone, or associated with other magnets, forms the next point in the present paper; and a distinction is first taken between ordinary magnets, which are influenced much by other magnets, so that the amount of their external force varies greatly, and those which are very hard, where this influence is reduced to little or nothing. The power of a given magnet was measured according to the method described in the last series, by a loop once passed over its pole. A given hard magnet placed in an invariable position, being thus estimated, was found to have a force equivalent to $16°{\cdot}3$ of deflection. Another magnet, having a power of $25°{\cdot}74$, was then placed close to the first in different positions, with like or unlike poles near together, so as to tend sometimes to exalt its power and at other times to depress it; and the results observed. In the extremest favourable case, namely, when the two were conjoined as a horseshoe magnet, the force of the first magnet was only raised $2°{\cdot}45$, which fell directly the dominant magnet was removed; in the corresponding adverse case the depression was only 1°. A very hard magnet, made by Dr. Scoresby, of $6°{\cdot}88$ power, when under the influence of another of double its power, was not sensibly affected either way. When under the influence of one of six times its force, it could be affected to the extent of nearly 1°. Ordinary magnets can be affected to the extent of one half of their power or more; and indeed in extreme cases can be altogether overruled and inverted.

From these results the author concludes, that, with perfect unchangeable magnets, the lines of force (as before defined) of different magnets in favourable positions, coalesce: that there is no increase of the total force by this coalescence; the sections between the associated poles giving the same sum of power as the sections of the lines of either magnet alone: that as the external amount of force of the magnet is not varied, neither is the internal amount at all changed: that the increase of power upon a magnetic needle, or a piece of soft iron, placed between two opposite favourable poles, is caused by concentration of the lines which before were diffused, and not by the addition of the power represented by the lines of force of one pole to that of the lines of force of the other. There is no more power represented by *all* the lines of force than before, and a line of force is not in itself more powerful because it coalesces with a line of force of another magnet. In this and in other respects, the analogy of the magnet with the voltaic pile is perfect.

The paper concludes with some practical remarks upon the delineation of the forms of the lines of force by iron filings, and by a description of the inflection of the lines by hemispheres of hot and cold nickel; which the author considers as the corresponding case to the action of warm and cold oxygen in the atmosphere, as applied by him in the explication of some of the phenomena of atmospheric magnetism, and especially of the annual and daily variation.

ROYAL INSTITUTION OF GREAT BRITAIN.

Friday, February 13.—On the Heating Effects of Electricity and Magnetism. By W. R. Grove, Esq., M.A., F.R.S.

In the early periods of philosophy when any unusual phenomenon attracted the attention of thinking men it was frequently referred to a preternatural or spiritual cause; thus with regard to the subject about to be discussed, when the attraction of light substances by rubbed amber was first observed, Thales referred it to a soul or spiritual power possessed by the amber.

Passing to the period antecedent to the time of more strict inductive philosophy, viz. the period of the alchemists, we find many natural phænomena referred to spiritual causes. Paracelsus taught that the Archæus or stomach demon presided over, caused and regulated the functions of digestion, assimilation, &c.

Van Helmont, who may be considered in many respects the turning-point between alchemy and true chemistry, adopted with some modification the Archæus of Paracelsus and many of the opinions of the Spiritualists, but showed tendencies of a more correctly inductive character; the term 'Gas,' which he introduced, gives evidence of the thought involved in it by its derivation from 'Geist' a ghost or spirit. By regarding it as intermediate between spirit and matter, by separating it from common air, and by distinguishing or classifying different sorts of gas he paved the way for a more accurate chemical system.

Shortly after the time of Van Helmot lived Torricelli, who by his discovery of the weight of air was mainly instrumental in changing the character of thought and inducing philosophers to introduce, or at all events to develope the notion of fluids, as agents which affected the more mysterious phænomena of nature, such as light, heat, electricity, and magnetism.

Air being proved analogous in many of its characters to fluids as previously known, the idea of fluids or of an ether was carried on to other unknown agencies appearing to present effects remotely analogous to air or gases.

Sound was included by some in the same category with the other affections of matter, and as late as the close of the last century a paper was written by Lamarck to prove that sound was propagated by the undulations of an æther. Sound is now admitted to be an undulation or motion of ordinary matter, and Mr. Grove considered that what have been called the imponderables, or imponderable fluids, might be actions of a similar character, and might be viewed as motions of ordinary matter.

Heat was at an early period so viewed, and we find traces of this in the writings of Lord Bacon. Rumford and Davy gave the doctrine a greater development, and Mr. Grove, in a communication made by him at an Evening Meeting of this Institution in 1847, showed that what had hitherto been deemed stumbling-blocks in the way of this theory of heat, viz. the phænomena presented by what have been called latent and specific heat, might be more simply explained by the dynamic theory.

In this evening's communication he brought forward some experiments and considerations in favour of the extension of this view to electricity and magnetism; an extension, which he had for many years advocated, and which was, in his opinion, supported by many analogies.

The ordinary attractions and repulsions of electrified bodies present no more difficulties, when regarded as being produced by a change in the state or relations of the matter affected, than did the attraction of the earth by the sun, or of a leaden ball by the earth; the hypothesis of a fluid is not considered necessary for the latter; and need not be so for the former class of phænomena.

In the cases of heating, or ignition of a conjunctive wire or conducting body through which what is called electricity is transmitted, we have many evidences that the matter itself is affected, and in some cases temporarily, in others permanently changed; thus if a wire of lead is ignited to fusion by the voltaic battery, the fused lead being kept in a channel to prevent its dispersion, it gradually shortens, and the molecules seem impressed with a force acting transversely to the line of direction of the electricity; at length the lead gathers up in nodules which press on each other as do, to use a familiar illustration, a string of figs.

With magnetism we have many instances of the molecular change which a ferreous or magnetic substance undergoes when magnetized. If the particles are free to move, as for instance iron filings, they arrange themselves symmetrically. An objection may be made arising from the peculiar form of the iron filings, but Mr. Grove, in the year 1845, showed that the supernatant liquid, in which magnetic oxide had been formed, and which contains magnetic particles not mechanically but chemically divided, exhibits when magnetized a change in the arrangement of the molecules, as may be seen by its effect on transmitted light;—a molecular change is also evidenced by the note or sound produced by magnetism, and by other effects.

Assuming that the molecules of iron change their position *inter se* upon magnetization, then by repeated magnetization in opposite directions, something analogous to friction might be produced; and just as a piece of caoutchouc when elongated produces heat (as it was on this occasion experimentally shown to do), so a bar of soft iron might be expected, when subjected to rapid changes in its magnetic state, to exhibit thermic effects.

With the aid of the large magnet of the Institution and of a commutator for changing the direction of the electricity, a bar of soft iron was alternately magnetized in opposite directions; and in a few minutes a thermometer placed in an aperture in the iron showed a rise of temperature of 1°·5 Fahrenheit: the bar being separated from the magnet by flannel, and the magnet being at a notably lower temperature than the bar, this heat could nowise be attributed to conduction.

The effect of electricity in the disruptive discharge, as in the voltaic arc and the electric spark, would seem at first sight to offer greater difficulties of explanation on the dynamic theory. The brilliant phænomenal effects of the electric discharge, and the apparent absence of change in the matter affected by it, would at first lead the observer to believe that electricity was a specific entity.

With ordinary flame or the apparent effects of combustion, however, the idea has to a great extent been abandoned that such visual effects are due to specific matter, and it is regarded by many as an intense motion of the particles of the burning body. So with electricity, if in regard to the disruptive discharge it can be shown that the matter of the terminals or of the intervening medium is changed, the necessity for the assumption of a fluid or æther ceases, and, to say the least, a possibility of viewing electricity as a motion or affection of ordinary matter is opened.

To make evident to the audience the relation of the electrical discharge to combustion and the fact that the terminals were themselves affected, the voltaic arc was taken, first between silver and then between iron terminals: in the first case a brilliant green-coloured flame was produced; and in the second a reddish scintillation or spur fire effect, just as in the ordinary combustion of the metals.

So with the discharge of Franklinic electricity between the same two metals, a strip of silvered leather gave the bright green discharge, while a chain of iron gave the spur fire effect.

The known transport of particles of the terminals from one pole to the other,—the different effects of different intervening media on induction as shown in Faraday's experiments,—the polar tension of such media, &c. were instances of the train of molecular changes consequent upon electrical action.

Hitherto the polarity of the gaseous medium existing between the metallic or conducting terminals of the electrical circuit was only known as a physical polarity and not shown to have an analogous chemical character with that existing in electrolytes anterior to electrolysis; but Mr. Grove stated that in a recent communication to the Royal Society he had shown that mixtures of gases having opposite electrical or chemical relations, such as oxygen and hydrogen, or compound gases such as carbonic oxide, were electro-chemically polarized or had their electro-negative and electro-positive elements thrown in opposite directions: thus if a silvered plate be made positive, in such cases it is oxidized; if negative, the dark spot of oxide is reduced; and an experiment was shown in which such a plate was thus oxidized and the spot reduced in gaseous media.

Here, as in the other experiments, was an effect on the terminals and an effect of polarization of the intermedium. In the experiments hitherto shown, solid terminals were used; it became important to examine what would be the effect of liquid terminals, for instance water; the spark or disruptive discharge of Franklinic electricity was readily obtained from its surface, but hitherto no voltaic battery had been found to show a discharge at any sensible distance from the surface of water.

Mr. Gassiot had procured to be constructed 500 cells of the nitric acid battery, the combination discovered in 1839 by Mr. Grove and first shown at this Institution in the year 1840. The cells of this battery were all well insulated by glass stems, and as regards intensity of action it was probably far the most powerful ever seen. Mr. Gassiot had kindly lent this apparatus for the illustration of this evening's discourse, and by its aid Mr. Grove was able to show an experiment which he had first made when experimenting with Mr. Gassiot some time ago, and which produced the effect he had long sought for, viz. a quantitative or voltaic discharge at a sensible distance from the surface of water. The experiment was made as follows:—a platinum plate forming the anode of the battery was immersed in a capsule of distilled water, the temperature of which was raised. A cathode or negative terminal of platinum wire was now made to touch for a moment the surface of the water, and immediately withdrawn to a distance of about a quarter of an inch; the discharge took place, the extremity of the platinum wire was fused, and the molten platinum attached to the wire, but kept up by the peculiar repulsive effect of the discharge was exhibited, as it were, suspended in mid-air, giving an intense light, throwing off scintillations in directions away from the water, and only detaching itself from the wire when agitated.

Here water in the vaporous state must be transferred, for the immersed electrode gave off gas, without doubt oxygen, and the molecular action on the negative fused platinum resembled, if it were not identical in character with, the currents observed on the surface of mercury when made negative in an electrolyte.

It may be objected to the theory proposed, that electrical effects are obtained in what is called a vacuum, where there is no intermedium to be polarized ; but this objection, though not applicable to the projection of the terminals, could hardly be discussed until experimentalists had gone much further than at present in the production of a vacuum ; the experiments of Davy and others had shown that we are far off from obtaining anything like a vacuum where delicate investigations are concerned.

The view of the ancient philosophers that Nature abhors a vacuum which had been much cavilled at, and was supposed to be exploded by the discovery of Torricelli, Mr. Grove thought had been unjustly censured : giving the expression some degree of metaphorical license, it afforded a fine evidence of the extent and accuracy of observation of those who were unacquainted with inductive philosophy as a system, but who necessarily pursued it in practice. Whether a vacuum was possible might be an open question, experimentally it was unknown.

Lastly, in answer to those who might ask, to what practical results do researches such as these lead ? what accession of physical comfort or luxury do they bring ? Mr. Grove took occasion to offer his humble protest against opinions now perhaps too generally prevalent, that science was to be viewed only or mainly in its utilitarian or practical bearings. Even regarding it in this aspect, were it not for the devotion which the love of knowledge, which the yearning anxiety to penetrate into the mysteries of our being and of surrounding existences induced ; the practical results of science would not have been attained ; the band of martyrs to science from Socrates to Galileo would not have thought and suffered without a higher incentive than the acquisition of utilitarian results : without disparaging these results, indeed regarding them as necessary consequences of any advance in scientific knowledge, he considered that the love of truth and knowledge for themselves was the great animating principle of those who rightly pursued science ; that, based upon an enduring quality of our common nature, this feeling was rooted in far firmer foundations, that it led to greater and more self-sacrificing exertions than any capable of being induced by the hopes of augmenting social acquisitions, and was an attribute and an evidence of the non-transient part of our being.

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[Continued from vol. ii. p. 500.]

Dec. 8, 1851.—On the Oscillations of Suspension Bridges. By J. H. Röhrs, Esq., M.A.

In this paper the oscillations of a chain suspended at two points were discussed, with a view to explain the causes of fracture in suspension-bridges, by vibration arising from the tramping of troops, gusts of wind, &c., as well as to suggest means for obviating the mischief under those circumstances. The following were some of the most remarkable results arrived at:—

1st. That if the tension at the ends of the chain where it is suspended be kept constant by allowing play at those points, the variation of tension due to vibration at any other point of the chain will be but small.

2ndly. That if the chain be tied at the points of suspension so that it can have no motion there, a slight extent of vibration will produce comparatively a great increase of tension.

3rdly. That periodic forces, such as may be taken, for instance, to represent the effect of tramping in time of troops moving across the bridge, are dangerous in the extreme, as if they happen to coincide in period with any of the possible types of vibration, the extent of vibration will increase continuously, till it ceases to be represented approximately by a linear or even an equation of the second order; in this case, the chain will be divided by nodal points where there is no vertical motion.

4thly. That the mere transit, without tramping, of ordinary loads at an ordinary pace would not cause sensible vibration in a bridge of wide span; but that terms not periodic might be introduced by the variable pressure of wind sweeping in rapid gusts along the platform.

Feb. 16, 1852.—On the Composition and Resolution of Streams of Polarized Light from different Sources. By Professor Stokes.

In this paper the author investigates the nature of the light resulting from the union of several independent streams of polarized light. The refrangibility of the several streams is supposed to be the same, and the polarization to be of the most general nature, that is, to be elliptic. The following proposition is established.

When any number of independent polarized streams, of given refrangibility, are mixed together, the nature of the mixture is completely determined by the values of four constants, A, B, C, D, defined in the following manner:—Let J be the intensity of one of the elliptically-polarized streams, α the azimuth of its plane of maximum polarization, $\tan\beta$ the ratio of the axes of the ellipse described by the æthereal particles; then

$$A=\Sigma(J)\,;\ B=\Sigma(J\sin 2\beta)\,;\ C=\Sigma(J\cos 2\beta\cos 2\alpha)\,;$$
$$D=\Sigma(J\cos 2\beta\sin 2\alpha).$$

Two groups of polarized streams, of the same refrangibility, which are such as to give the same values to each of the four constants A, B, C, D, are defined to be *equivalent*; and the author has shown, that if two equivalent groups be transmitted through any optical train, and be afterwards analysed, they will present exactly the same appearance; so that equivalent groups may be regarded as optically identical.

It readily follows from the above theorem, that any group of polarized streams is equivalent to a stream of common light combined with a stream of elliptically-polarized light from a different source. If J, J′ be the intensities of these streams, α' the azimuth of the plane of maximum polarization of the latter, $\tan\beta'$ the ratio of the axes of the characteristic ellipse,

$$J = A - \sqrt{(A^2+B^2+C^2)};\ J' = \sqrt{(A^2+B^2+C^2)};$$

$$\sin 2\beta' = \frac{B}{\sqrt{(A^2+B^2+C^2)}};\ \tan 2\alpha' = \frac{D}{C}.$$

The author has applied these formulæ to a few examples, and has likewise shown, from the general principles established in the paper, that the changes which are continually taking place in the epoch and intensity of the vibrations of polarized light may be of any nature. In the case of common light, the author contends that there is no occasion to suppose the transition from a series of vibrations of one kind to a series of another kind to be abrupt, but that it may be of any nature.

XLV. *Intelligence and Miscellaneous Articles.*

ON GAS-BATTERIES, AND ON THE PREPARATION OF HYDRIODIC AND HYDROBROMIC ACIDS BY THE GALVANIC METHOD. BY M. OSANN.

THE phænomena exhibited by a circuit composed of gaseous elements appear at first sight to be explicable in the following manner. One of the tubes contains hydrogen, the other oxygen, both of which are over dilute sulphuric acid, but in such a manner that the ends of the two strips of platinum existing in the tubes dip into the liquid. Now as oxygen is somewhat soluble in dilute sulphuric acid, the strip of platinum in the hydrogen element comes into contact with hydrogen and the oxygen dissolved in the acid, and as platinum possesses the property of causing the two gases to combine, the simplest view seems to be that this combination occurs in the present case, and that the electric current is produced by this chemical action. But the following well-founded objection may be urged against this view. When oxygen and hydrogen combine, whether this arise from the inflammation of burning bodies, from the electric spark, or from finely divided platinum, considerable heat is produced; but in the present case this is absent, for elevation of temperature is never perceived. The action of the platinum must therefore be of a different kind here. It might be said, that in this case the platinum transformed the hydrogen into the same state as that in which it exists in the hydrogen-acids, which are decomposed in the well-known way by oxides, without simultaneous elevation of temperature from the union of the hydrogen with the oxygen. Grove and Schönbein adopt another explanation, assuming that in the hydrogen element the platinum causes the hydrogen to combine with the oxygen of the adjacent water-element, and in the oxygen element the platinum causes the oxygen to combine with the hydrogen of the next water-element. In this way a motion of the hydrogen and oxygen elements would occur from one side to the other, which would be simultaneous with an electric current.

As far as this point, the two above-mentioned philosophers agree. But in the following experiment, which may be made with the gas-battery, their opinions differ. If hydrogen be placed in one element over dilute sulphuric acid, and the other be completely filled with this liquid, at the moment of closure of the circuit a feeble current is detected by a multiplier, which emanates from the hydrogen element but soon disappears. Grove explains this phænomenon by stating, that the hydrogen which appears in the tube filled with dilute sulphuric acid combines with the small quantity of the oxygen absorbed from the air existing in the liquid, and that the current exists only so long as this is present. Grove also assumes, that the presence of oxygen in one of the tubes is essential to the formation of a current; Schönbein, on the other hand, puts forward the opinion, that the formation of the current arises solely from the hydrogen element, and that the oxygen in the other element only plays a passive part. The cessation of the current would in this case be caused by the hydrogen which appears in the element filled with dilute sulphuric acid, electrically polarizing the platinum in the same manner as occurs in the hydrogen element, whereby a counter-current is set up which must destroy the original one.

It is evident that this experiment does not decide the question as to which of these views is correct. The author therefore instituted a new one, the result of which is in favour of Schönbein's view. The author filled two of his gas-elements with muriatic instead of dilute sulphuric acid, and filled one of these elements over this acid with oxygen, but in such a manner that the strip of platinum dipped into the muriatic acid. When the two elements were then closed by a multiplier, the needle was quickly deflected, and this to a considerably greater extent than when it is deflected under the same circumstances with the use of dilute sulphuric acid and oxygen. The position of the needle was not, however, retained; it soon returned, and in a short time stood at zero.

The reason why, on thus substituting muriatic for dilute sulphuric acid, a stronger current was set up, is, according to the author, that the muriatic acid is a more easily decomposable liquid than dilute sulphuric acid. If this be the case, then less resistance to the conduction of the current is present; it circulates more quickly, and produces a more powerful effect upon the multiplier. But the second circumstance—that the needle returned to zero—is of far more importance. The muriatic acid contained no atmospheric air. Muriatic acid gas has so extraordinary an affinity for water, that on taking up this gas the atmospheric air is expelled. Hence in this case the action of absorbed oxygen in the liquid is out of the question. Neither could it be taken into consideration in regard to this circuit, because the body liberated in the tube filled with muriatic acid is chlorine, which resembles oxygen, and hence does not annihilate its action, but is associated with it in its effect. The cessation of the current can therefore only be explained by the chlorine which is liberated in the tube containing the liquid polarizing the platinum in it in the same manner as the oxygen does in the tube containing the gas. This corresponds perfectly with the fact found by Grove, according to which chlorine renders platinum even more strongly electrically polar than oxygen does.

Starting from this point of view, the author found that when a strip of platinum is brought into contact with amalgamated zinc in dilute sulphuric acid, hydrogen is evolved from the platinum. Now as platinum also possesses the property of causing hydrogen to combine with electro-negative bodies, the author took advantage of this circumstance to obtain hydriodic and hydrobromic acids. If iodine or bromine be added to the above-mentioned liquid, in a short time its colour disappears, and the hydrogen acids of these bodies are formed. In the course of three days a considerable quantity of hydriodic and hydrobromic acids may be obtained. By distillation they may be separated from the liquid, which contains sulphate of zinc in solution.—*Verhandl. der Physikal. Medicin. Gesellsch. zu Würzburg*, 1851, vol. ii. p. 329–331.

METEOROLOGICAL OBSERVATIONS FOR FEB. 1852.

Chiswick.—February 1. Rain: clear and fine. 2. Rain: cloudy and mild: densely overcast. 3. Clear: exceedingly fine: clear at night. 4. Uniformly overcast: rain. 5. Densely clouded: rain. 6. Clear: slight shower: clear: frosty. 7. Clear: very fine: overcast. 8. Boisterous, with rain: overcast: rain. 9. Cloudy and fine: showery: clear. 10. A few snow-flakes: cloudy and cold: clear at night. 11. Clear and frosty: fine: sharp frost at night. 12. Frosty and foggy: very fine: clear: slight frost. 13. Densely overcast: fine: overcast. 14. Hazy: uniformly overcast: clear. 15. Overcast. 16. Fine: densely overcast. 17. Cloudy: fine. 18. Low white clouds: clear. 19. Clear and cold. 20. Clear and frosty: very sharp frost at night. 21. Severe frost: fine. 22. Cloudy and cold. 23. Fine, but cold. 24, 25. Clear and cold. 26. Slight rain: uniformly overcast. 27. Cloudy: rain. 28. Slight rain: clear at night. 29. Clear: overcast and cold.

Mean temperature of the month	38°·72
Mean temperature of Feb. 1851	38 ·44
Mean temperature of Feb. for the last twenty-six years	40 ·06
Average amount of rain in Feb.	1·62 inch.

Boston.—Feb. 1. Fine. 2. Cloudy: rain early A.M. 3. Fine. 4, 5. Rain: rain A.M. and P.M. 6. Fine: stormy. 7. Fine. 8. Rain: rain early A.M. and P.M. 9. Cloudy: rain with lightning A.M. and P.M. 10. Cloudy: rain A.M. and P.M. 11, 12. Fine. 13, 14. Cloudy. 15, 16. Cloudy: rain P.M. 17. Cloudy. 18—21. Fine. 22, 23. Cloudy. 24. Fine. 25. Cloudy. 26. Cloudy: rain early A.M. and P.M. 27. Fine. 28. Cloudy. 29. Fine.

Sandwick Manse, Orkney.—Feb. 1. Clear: fine: aurora. 2. Cloudy. 3. Cloudy: sleet-showers. 4. Rain: showers. 5. Showery: clear. 6, 7. Sleet-showers. 8. Drizzle: rain. 9. Sleet-showers. 10. Bright: clear. 11. Cloudy. 12. Cloudy: clear: aurora. 13. Bright: showers: aurora. 14. Drops: drizzle. 15. Bright: hail-showers: S. aurora. 16. Hail-showers: S. aurora. 17. Rain: snow-showers: S. aurora. 18. Snow-showers: S. aurora. 19. Snow-drift: S. aurora. 20. Bright: cloudy. 21. Thaw: cloudy: aurora. 22. Cloudy: aurora. 23. Fine: cloudy: fine. 24, 25. Cloudy: fine. 26. Cloudy: showers. 27. Showers: drizzle. 28. Snow-showers: showers. 29. Snow-showers.

Meteorological Observations made by Mr. Thompson *at the Garden of the Horticultural Society at* CHISWICK, *near London*; *by* Mr. Veall, *at* BOSTON; *and by the* Rev. C. Clouston, *at Sandwick Manse*, ORKNEY.

Days of Month.	Barometer.					Thermometer.					Wind.			Rain.		
	Chiswick.		Boston. 8¾ a.m.	Orkney, Sandwick.		Chiswick.		Boston. 8¾ a.m.	Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Orkney, Sandwick.	Chiswick.	Boston.	Orkney, Sandwick.
1852. Feb.	Max.	Min.		9¼ a.m.	8½ p.m.	Max.	Min.		9¼ a.m.	8½ p.m.						
1.	29·996	29·717	29·22	29·46	29·65	57	39	51·5	41	40	w.	w.	sse.	·04	·24	·09
2.	30·003	29·850	29·53	29·22	29·29	53	35	47	46	43	sw.	sw.	sw.	·04	·02	·04
3.	30·142	30·016	29·55	29·26	29·26	51	34	38·5	42	43	sw.	sw.	w.	....	....	·09
4.	30·191	29·810	29·56	28·93	28·78	55	44	41	41	43½	sw.	sw.	wnw.	·13	....	·33
○ 5.	29·739	29·641	29·23	29·29	29·24	55	43	48	39	38½	sw.	sw.	w.	·23	·09	·43
6.	30·098	29·648	29·15	29·51	29·69	49	27	44	38	41	nw.	nnw.	nw.	·01	·15	·09
7.	30·155	29·994	29·72	29·61	29·49	50	41	36	43	43	nw.	w.	w.	·02	....	·22
8.	30·178	29·463	29·10	29·00	29·75	53	36	49	43	41½	sw.	w.	nw.	·38	·10	·30
9.	29·461	29·111	28·73	29·12	29·49	45	25	43	35	42	sw.	nnw.	nw.	·05	·13	·54
10.	30·036	29·631	29·35	29·93	30·03	45	25	39	38	36	n.	n.	sw.	·02	·10	·06
11.	30·099	29·998	29·80	29·72	29·75	44	17	30	39½	43	n.	n.	sw.	....	·05	....
☾ 12.	29·914	29·664	29·65	29·53	29·46	43	25	30	42½	41½	s.	s.	s.	....	....	·03
13.	29·824	29·584	29·33	29·45	29·62	42	30	33	40	39	s.	s.	w.	....	....	....
14.	30·206	30·008	29·74	29·65	29·65	43	22	34	40	47	se.	w.	w.	....	....	·28
15.	30·191	30·100	29·72	29·56	29·47	49	33	41	45½	37	sw.	w.	w.	....	....	·04
16.	30·176	29·777	29·18	29·42	29·45	53	43	40	37	40	sw.	w.	w.	·04	·03	·25
17.	29·858	29·558	29·40	29·45	29·22	56	37	49	38	37	w.	nnw.	w.	·03	·05	·05
18.	29·737	29·596	29·15	29·44	29·72	46	27	39	30½	31	nw.	nw.	n.	....	....	·31
19.	30·018	29·783	29·50	29·94	30·08	39	23	31	29	27	nw.	nnw.	n.	....	....	·11
● 20.	30·186	30·117	29·82	30·14	30·05	38	15	27	35	33½	nw.	nnw.	sw.	....	....	·02
21.	30·362	30·233	29·96	29·98	30·22	42	32	24	42½	39	w.	wnw.	nw.	·01	....	·22
22.	30·574	30·423	30·17	30·33	30·46	46	22	34	43½	46	ne.	nne.	w.	....	....	·03
23.	30·637	30·572	30·37	30·60	30·63	46	23	34	45	47	ne.	e.	s.	....	....	·03
24.	30·500	30·362	30·24	30·54	30·48	42	31	34·5	45½	45	ne.	e.	sw.	....	....	....
25.	30·333	30·205	30·07	30·60	30·71	45	29	37·5	41½	37	e.	e.	ne.	....	....	....
26.	30·334	30·225	30·06	30·51	30·30	43	33	38	41½	42	ne.	ne.	nw.	·01	·02	....
27.	30·136	29·963	29·80	30·09	29·72	46	32	36·5	45	45½	n.	nnw.	nw.	·03	·02	·11
☽ 28.	29·715	29·628	29·33	29·62	29·72	50	30	42·5	35½	36½	nw.	nnw.	nw.	·01	....	·07
29.	29·872	29·739	29·47	29·74	29·69	43	24	37	34	34½	nw.	nw.	nw.	·01	....	·07
Mean.	30·002	29·842	29·58	29·711	29·761	47·20	30·24	38·24	39·89	40·00				1·04	1·00	3·81

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[FOURTH SERIES.]

MAY 1852.

XLVI. *Reports on the Progress of the Physical Sciences.* *By* JOHN TYNDALL, *Ph.D.*

[With a Plate.]

On the Electroscopic Properties of the Voltaic Circuit; being an experimental verification of the Theory of Ohm. *By* Dr. KOHLRAUSCH, Poggendorff's *Annalen*, vol. lxxv. p. 220; vol. lxxviii. p. 1; and vol. lxxxii. p. 1.

THE following quotation bears so pertinently upon the subject of the present report, that an apology for its introduction here is scarcely necessary. It is extracted from a discourse by Professor Dove, before the Berlin Society for Scientific Lectures.

"As the (then considered) essential portions of a galvanic circuit were two metals and a fluid, innumerable combinations were possible, from which the most suitable must be chosen. This gigantic task was undertaken by Ritter, an inhabitant of a village near Liegnitz, who almost sacrificed his senses to the investigation. He discovered the peculiar pile which bears his name, and opened that wonderful circle of actions and reactions which, through the subsequent discoveries of Œrsted, Faraday, Seebeck and Peltier, drew with ever-narrowing band the isolated forces of nature into an organic whole. But he died early, as Günther did before him, exhausted by restless labour, sorrow, and disordered living. It was soon found that many experiments succeeded better with a single pair of large plates than with several small ones; and, in short, that every apparatus exhibited certain actions better than all others. Here men of science long groped in darkness, when in the year 1827, the theory of galvanism by Ohm, then of Berlin, now of Nürnberg, rose like a pole-star to illumine the obscurity. He showed that, as the apparatus itself was composed solely of conductors, the electric current must proceed not only along the connecting wire from pole to pole, but also through the apparatus itself; that the resistance offered to the passage of the current consisted therefore of two portions, one exterior to the apparatus and one within it. At a stroke, the difficulties which up to this time had beset the subject, and which were thought insuperable by those who had confined their attention to the exterior resistance only, crumbled away.

"Ohm brought forward his discovery in the simple earnest language which distinguishes the true investigator of nature. A theory, he says, which lays claim to immortality must not depend upon the idle garniture of words for the proof of its noble origin, but must show in all its parts, by its simple and complete correspondence with facts, and without the aid of eloquence, its affinity to that spirit which animates nature. The manner in which this theory was received was different in different lands. Henry of Princeton, North America, who at once saw its infinitely practical importance, observes, 'When I first read Ohm's theory, a light arose within me like the sudden illumination of a dark room by lightning.' The Royal Society of London awarded him the Copley Medal, the highest prize given by the Society for physical investigation. In France also the discovery met with the greatest recognition which a foreign investigator could expect there. A physicist of that country thought it convenient to rediscover the same thing years afterwards. He thought, cette découverte n'est pas Française, mais elle est digne d'être Française. But what reward did Ohm reap in Germany? While the most laborious empirical inquiries were instituted, among which those of Fechner in Leipzic deserve especial mention, to bring the theory in all possible ways to the touchstone of experience, that science whose function it is to think the great thoughts of the Creator over again, glanced down with divine satisfaction from her Olympic throne upon these sublunary occupations; in the Berlin *Jahrbücher für wissenschaftliche Kritik*, Ohm's theory was named a web of naked fancies, which can never find the semblance of support from even the most superficial observation of facts; 'he who looks on nature,' proceeds the writer, 'with an eye of reverence, must turn aside from this book as the result of an incurable delusion, whose sole effort is to detract from the dignity of nature.'"

The investigations, of which we now purpose giving a review, occupy themselves with the experimental verification of the entire theory of Ohm. A portion of that theory has been already tested by physicists of all lands and found true: this portion, which on account of its superior importance is called the law of Ohm, forms, however, but one link in the chain of causation which the philosopher's speculations place before us. The comparative want of recognition which the other portions of the theory have experienced, is to be chiefly referred to the difficulty of procuring instruments sufficiently delicate to test them experimentally. By the invention and skilful application of suitable instruments, M. Kohlrausch has been able to travel side by side with the speculations of Ohm, and to convert them one after another into experimental facts.

The fundamental portions of Ohm's theory may be briefly sketched as follows:—Let the ring, Plate IX. fig. 1, represent a homogeneous conductor, and let a source of electricity be supposed to exist at A. To fix the ideas, let us suppose an electric machine placed there. The electricity from the machine will diffuse itself over both sides of the ring; the positive passing towards *a*, and the negative towards *b*, both fluids uniting at *c*. Now if the electricity be so distributed over the ring that a heaping up of the fluid nowhere occurs, then it follows that equal quantities of electricity pass through all cross sections of the ring in the same space of time. If it be assumed that the passage of the fluid from one cross section to another is solely due to the difference of the electric tension at both these points; and further, that the quantity which passes is proportional to this difference of tension, the consequence is, that the positive fluid proceeding from A right to *c*, and the negative fluid proceeding from A left to *c*, must decrease in tension the further they recede from A.

The tension of the electricity at every point in the circuit may be represented by a diagram. Let the above ring be supposed to be stretched out into a straight line AA′, fig. 2; let the ordinate AB represent the tension of the positive electricity, and A′B′ the tension of the negative electricity at the point of excitation, then the ring being homogeneous and of the same diameter throughout, the straight line BB′ will express the tension for all points of the circuit.

From these considerations, the law of Ohm expressed by the celebrated formula

$$S=\frac{E}{R},$$

where S represents the strength of the current, E the electromotive force of the battery, and R the resistance, naturally follows. If the electromotive force AB+A′B′ remain constant, then the greater the length of AA′ the less steep will be the inclination of the line BB′; that is to say, the less will be the difference of tension in two contiguous cross sections. But by the hypothesis, this difference is proportional to the quantity of fluid which passes from one cross section to the other; and hence it follows, that the greater the length of the circuit, the less will be the amount of electricity which passes through any cross section in a given space of time.

If the conductor AA′ be composed of material which offers a greater resistance to the passage of the electricity than that above supposed, as long as its length remains unaltered the distribution of the electricity will be the same. But inasmuch as the moving force, that is, the difference of tension between two neighbouring cross sections, is also the same as before, a less quantity of electricity must pass from section to section in a given time than in the case of the good conductor; that is to say, the current must be weaker. A greater length of the better conductor would produce precisely the same effect. These results find definite expression in the law, that *the strength of the current is inversely proportional to the resistance of the circuit.* Preserving the length and material of AA′ unchanged, and regarding the force AA′+BB′ as variable, we deduce the law, that *the strength of the current is directly proportional to the electromotive force.*

One additional reference to the manner in which Ohm pictured to himself the electroscopic state of the circuit will suffice. Let the conductor AA′, fig. 3, consist of the same material throughout, but of two portions, possessing different cross sections. Let the cross section of A*d*, for example, be *m* times that of *d*A′; then if equal quantities pass through all sections in equal times, if through a unit of length of wire of *m* times the cross section no more fluid passes than through the thinner wire, the difference of tension at both ends of this unit of length in the former must be only $\frac{1}{m}$th of what it is in the latter. Thus the electric "fall," as Ohm terms it, that is, the decrease in the length of the ordinate for the unit of length of the abscissa, will be less in the case of the thick wire than of the thin, as shown by the line B*c* in the figure. The distribution of the electricity in such a circuit will be no longer represented by a continuous gradient, but can nevertheless be easily ascertained by calculation when the electromotive force of the circuit and the cross sections of its different portions are known. If, instead of one wire being thinner than the other, its specific resistance were greater, it would follow from the hypothesis of Ohm, that the greater the resistance of the metal the greater would be the electric fall. The result is summed up in the law, that *the "electric fall" is directly proportional to the specific resistances of the metals and inversely as their cross sections.*

Thus far we have travelled through a region of pure speculation. To test whether the actual distribution of electricity throughout a galvanic circuit bears any resemblance to that here supposed, an electrometer of surpassing delicacy was necessary. We shall give a brief description of the refined instrument made use of for this purpose by M. Kohlrausch.

A thin needle of silver wire, two inches in length, is suspended horizontally from a glass fibre of exceeding fineness; the fibre which passes in the usual manner through a glass tube is fastened to a torsion-head, the index of which being turned causes the little needle of silver wire at the other end to follow it. The needle lies across a thin strip of silver of its own length, through a slit in the centre of which the needle can descend; at the slit the strip is so bent right and left, that the needle, in following the index, can lay its entire length against the strip. This is the only portion of the instrument which requires a drawing to make it clear; it is represented in fig. 4. AB is the strip of silver, *cd* one-half of the needle crossing the strip in its centre, the other half is hidden by the strip. AB can be raised or lowered, so as to be in contact with the needle or detached from it. When the needle crosses the strip at right angles, the latter is raised so that the needle rests upon it, the apparatus thus forming a continuous cross of conducting material. Electricity, being communicated to the strip, distributes itself over the entire cross; when this is effected, the strip is lowered so that the needle again hangs free. The index above being turned, the needle will be solicited by the torsion of the fibre to approach the strip, but being charged with a like electricity, it will be repelled; by this play of torsion, on the one hand, and repulsion on the other, we arrive at a knowledge of the tension of the electricity communicated. The author has constructed tables from which the electric tension due to any observed amount of torsion can be instantly ascertained.

In connexion with the electrometer a condenser was made use of, the accuracy of which was carefully tested beforehand. For experiments with the galvanic circuit, both plates are of brass, suspended in a suitable frame by strings of silk, and separated from each other by three little patches of shell-lac placed at three different points near the periphery. When the poles of the battery are connected with these plates, the one becomes charged with positive, the other with negative electricity; and the strength of the charge is estimated by removing one of the plates to a certain fixed distance, and bringing the other, by means of an isolated copper wire, into connexion with the electrometer.

The electromotive force of a voltaic element, which Ohm expresses in his formula by the letter E, can be variously ascertained: the question suggested itself to M. Kohlrausch, whether any relation existed between this force and the tension of the electricity at the two poles of the element. The electromotive forces of various combinations were determined by Wheatstone's method. To ascertain the tension at the poles, the circuit, which had been permitted to remain in action for some time was suddenly broken, and the ends of the wires were brought into connexion with the plates of the condenser. The plates were then separated; one of them was immediately brought into connexion with the electrometer, and the strength of the charge was measured. The results derived from this process are contained in the following table:—

Description of element.	Electromotive force.	Tension at the ends of the broken circuit.
1. Zinc and platinum:—zinc in solution of sulphate of zinc, platinum in nitric acid of 1·357 specific gravity	28·22	28·22
2. Do. with nitric acid of 1·213 sp. gr...	28·43	27·71
3. Zinc and coal:—zinc in sulphate of zinc, coal in nitric acid of 1·213 sp. gr.	26·29	26·15
4. Zinc and copper:—zinc in sulphate of zinc, copper in solution of sulphate of copper	18·83	18·88
5. *a.* Silver and copper:—silver in cyanide of potassium or common salt, copper in solution of sulphate of copper	14·08	14·27
b. The same afterwards	13·67	13·94
c. The same some time afterwards...	12·35	12·36

This table establishes the important result, *that the electromotive force is proportional to the electric tension at the ends of the newly-broken circuit.*

The following experiments were instituted to ascertain the electroscopic properties of the active simple circuit. The author considers it practically impossible at present to construct an electrometer which shall directly declare the almost infinitesimal tension which obtains at the various points of the simple circuit, and hence the necessity of calling in the aid of the condenser: the manner in which the instrument was charged is as follows:—

From the lower condensing plate a wire of the same metal as the plate itself proceeded, and was buried in the earth. A branch was carried from this wire to a point *a* of the closed circuit. When another point, *b*, of the circuit was brought into metallic connexion with the upper plate of the condenser, it became charged to an amount which depends upon the tension existing at *b*, and on the condensing power of the plates. If several such points, *b*, be examined, the charges imparted to the condenser will be proportional to the electroscopic tension at the different points. Instead of connecting the lower plate with the earth, we might connect it and the point *a* directly, and bring the upper plate, as before, into connexion with *b*; experiment proves that the result obtained from this procedure is exactly the same as that obtained by the former method. The mode of observation first indicated is that pursued in the following experiments, the point *a* being deprived of all electric tension by its direct union with the earth.

Experiment 1.—The poles of the element were connected by a long fine wire, which was carried in a zigzag manner from side to side of a light wooden frame, and fastened to the latter by pins; the legs of the Vs thus formed were all of the same length.

a. Any point (*a*) being properly connected with the earth, when another point on that side of *a* from which the positive current proceeded was connected with the upper plate, the latter exhibited positive electricity; when, however, the point lay at the other side of *a*, a negative charge was obtained.

b. As long as the same length of wire existed between the point *a* and the point examined, exactly the same tension was shown by the electrometer, it mattered not in what portion of the circuit the examination took place.

c. When a series of points in the circuit at increasing distances from *a* were examined, the tension was observed to increase, the increase being exactly proportional to the length of wire intervening between *a* and the respective points. Calling to mind what has been said regarding the electric "fall," the case before us shows that, in a wire of uniform thickness, the "fall" is in all places the same.

Experiment 2.—The poles were united by two silver wires of equal lengths but of different diameters; the wires being smelted together in the flame of a spirit-lamp, so as to form one unbroken length: it was found,—

a. That in each of the wires the same electric fall existed throughout.

b. When one end of the thin wire was properly connected with the earth and the other end proved, the electrometer showed a charge of the strength E; when one end of the thick wire was connected with the earth and the other end examined, a charge *e* was obtained; the ratio of $E : e$ was the same as that of the cross section of the thick wire to that of the thin.

Experiment 3.—The wire connecting the poles was formed of two wires, one of copper, the other of German silver; the former presenting very little resistance to the current, while the resistance of the latter was considerable. The total resistance of each wire was previously ascertained by means of a rheochord. It was found that the entire increase of tension from one end to the other of the copper wire was to the entire increase along the German-silver wire in the direct proportion of the resistances.

The above results may be summed up as follows:—*In wires of different materials and of unequal thicknesses, the electric fall is directly proportional to the specific resistances of the metals, and* *inversely as their cross sections*; which is a complete verification of the hypothesis of Ohm.

Experiment 4.—A rectangular wooden trough was constructed, and its interior was coated with wax. At one end was placed a porous cell containing a solution of sulphate of zinc, in which a plate of zinc was immersed; the rest of the trough was filled with a solution of sulphate of copper, and at the opposite end a plate of copper was immersed. The zinc and copper plates were connected by a wire. The edge of the trough was graduated; two copper wires dipped into the solution of sulphate of copper, and by means of the graduation their exact distance asunder could be readily ascertained. One of these wires was well connected with the earth, the other was connected with the upper plate of the condenser. The mode of experiment was, in fact, the same as that pursued with the metallic portion of the circuit. Here also it was found that the tension at the point connected with the discharging wire was zero; right and left from this point a regular increase of tension was observed; on that side from which the current proceeded the electricity was positive, on the other side negative. Further, according to the view of Ohm, who imagined the electricity to make its way through the *interior* of both metallic and fluid conductors, the tension at every point in any given cross section is the same. In the case of a metallic conductor it is, of course, impossible to test this experimentally; but in the fluid portion of the circuit, M. Kohlrausch found exactly the same tension throughout each transverse section, whether he raised or sunk the wire (which in these experiments was everywhere coated with shell-lac except at its extreme end) in the fluid, or pushed it more or less aside laterally*.

I trust the reader bears in mind what has been said regarding the electric "fall." The greater the resistance offered to the passage of the current, the greater the fall. In a thin wire, the line expressing the tension at every point will be a steeper gradient than in a thick wire; and in the fluid portion of the circuit the gradient may be expected to be steeper than in either of the former cases, for here the resistance is greatest. The simplest possible circuit must therefore exhibit a series of gradients expressive of the tension of its various parts. There is the fall along the connecting wire, the fall along the zinc and copper plates (which, however, is practically zero, as they offer almost no resistance), and the fall along the fluid. But let us suppose

* Weber and Kirchhoff differ from Ohm here. They do not admit a motion of the fluid through the interior of the conductor, but solely along its surface. Their hypotheses, however, lead them to results which entirely agree with Ohm's.

the resistance in every portion of the circuit to be referred to a certain unit, and that the distances along the datum line from which the tensions are plotted are measured off with reference to this unit; that, for example, if an inch of the fluid portion exhibit a fall three times as great as an inch of the solid portion, the said fluid portion shall, on the datum line, be expressed by a distance three times as great as that which expresses an equal length of the solid portion; it is evident that when the resistances are thus referred to a common standard, the line which expresses the tension must be one uniform gradient from beginning to end. Ohm calls the length of a circuit referred to such a standard its *reduced length.*

It has already been stated, that when any point of the circuit is perfectly discharged, the tension at this point is null, and increases in tension right and left, showing positive electricity on that side of the point from which the current proceeds and negative electricity at the other side; the length of the circuit which shows the one fluid or the other will depend upon the position of the point; if exactly central, as at a'', fig. 5, the lengths will be alike. If the point be nearer to the zinc pole than to the copper pole of the arrangement, as at a', the length of wire exhibiting positive electricity will be greater than the length exhibiting negative electricity; and if the point be chosen contiguous to the zinc plate, as at a, the whole circuit will exhibit positive electricity.

Having the electromotive force bc, and the reduced length of the circuit, we are taught by the theory of Ohm to deduce by simple calculation the electroscopic state of every single point. Let the scheme in fig. 6 represent the state of things in a circuit where the discharged point a is contiguous to the zinc pole. The reduced length, ab, and the electromotive force, bc, being given, let d be any point whose tension, de, we wish to ascertain. Let $bc=a$, $de=u$, $ab=l$, $ad=\lambda$; then by similar triangles,

$$u:a=\lambda:l, \text{ or } u=\frac{\lambda}{l}\cdot a;$$

or, expressed in words, if the reduced length of the circuit between the discharged point and the point whose tension is sought be divided by the reduced length of the entire circuit, the quotient, multiplied by the electromotive force, gives the tension at the required point.

In submitting this formula to an experimental test, M. Kohlrausch made use of the wooden trough before alluded to. The copper and zinc plates were united, as in one of the experiments already described, by a long fine wire, bent from side to side of a wooden frame in a zigzag manner. The tensions of the points described below were determined by direct experiment. The electromotive force was also determined, the reduced length of the circuit was found by measuring the resistances of its various parts, and from these two, the electromotive force and the reduced length, the tensions due to the same points were calculated by the foregoing formula.

Points examined.

a. The second lower angle of the zigzag.
b. The fourth lower angle of the zigzag.
c. The sixth lower angle of the zigzag.
d. The point where the zigzag joined the copper.
e. The solution of sulphate of copper 2·02 inches from the plate of copper.
f. The solution of sulphate of copper 4·02 inches from the plate of copper.
g. The solution of the sulphate of copper 6 inches from the plate of copper.
h. The solution of sulphate of copper 8 inches from the plate of copper.

In the following table the results obtained by calculation are compared with those obtained by direct experiment; the quantity λ is the same as that contained in the formula.

	λ.	u calculated.	u observed.
a	118·5	0·93	0·85
b	237	1·86	1·85
c	355·5	2·80	2·69
d	474	3·73	3·70
e	610·3	4·80	5·03
f	745·3	5·86	5·99
g	879	6·91	6·93
h	1014	7·98	7·96

The truth of Ohm's formula, which he derived from considerations purely theoretical, appears to be placed beyond the pale of doubt by these results. Hitherto the celebrated law which usually bears his name has rested upon a basis of conjecture merely; and to the extraordinary patience and refined experimental skill of M. Kohlrausch is due the credit of giving to this conjectural foundation the stability of fact.

It may be stated, in addition, that the same physicist has also examined the thermo-circuit, and has not only demonstrated the existence of electric tension at its poles, but also proved that the electricity obeys the same law of distribution as that true for the voltaic circuit.

Queenwood College, March 1852.

XLVII. *A Mathematical Theory of* M. Foucault's *Pendulum Experiment. 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 remarkable experiment which recently attracted the attention of mathematicians, as being a practical demonstration of the earth's rotation, has already received various illustrations and theoretical explanations; but I am not aware that any explanation has yet been derived, according to rule, from the differential equations of motion, which on the principles of dynamics especially belong to the problem. I propose in this communication to form those equations, and by means of them to show that the facts observed may be explained on the hypothesis of the earth's rotation.

The problem may be generally stated thus:—To determine the motion of a ball suspended from a given point of the earth by a slender cord, and acted upon by gravity, the earth being supposed to revolve about an axis with a given angular velocity.

Conceive a line to be drawn through the point of suspension of the ball parallel to the axis of rotation of the earth, and a motion equal and opposite to that which this line has in space at any instant, to be impressed on all particles of the earth inclusive of the cord and ball. The line will thus be brought to rest, and all other points will begin to move as if they were revolving about it with the earth's angular motion. By supposing this axis to remain at rest, and the angular motion to continue, the motions we are about to consider will not be relatively altered. On this supposition, the direction of the force of gravity, being always perpendicular to the earth's surface, will revolve about the same axis. Consequently, the problem above enunciated is identical in its dynamical conditions with the following:—

To determine the motion of a ball suspended by a slender cord from a point in a fixed axis, and acted upon by a constant force in the direction of a line making a given angle with the axis and revolving about it with a given angular velocity.

Conceive O to be the point of suspension, and OX, OY, OZ to be fixed rectangular axes, of which OZ (drawn downwards) coincides with the axis of rotation. OA is the direction of gravity, making a constant angle AOZ(λ) with OZ, viz. the co-latitude of the place where the experiment is made. P is the position of the centre of the ball, OP$=a$ the length of the cord, and x, y, z are the coordinates of P at the time t. Let $\omega =$ the earth's velocity of rotation, and consequently the angular velocity

* Communicated by the Author.

of OA about OZ, and let $\omega t=$ the angle which the plane AOZ makes with the plane YOZ at the time t.

The force of gravity being g, the resolved parts in the directions of OX, OY, OZ are

$$g\,.\cos \mathrm{AOX},\qquad g\,.\cos \mathrm{AOY},\qquad g\,.\cos \mathrm{AOZ};$$

or

$$g\,.\sin\lambda\sin\omega t,\quad g\,.\sin\lambda\cos\omega t,\quad g\,.\cos\lambda.$$

The accelerative force of the tension of the cord being T, the resolved parts in the same directions are

$$-\frac{\mathrm{T}x}{a},\quad -\frac{\mathrm{T}y}{a},\quad -\frac{\mathrm{T}z}{a}.$$

Consequently,

$$\frac{d^2x}{dt^2}=g\sin\lambda\sin\omega t-\frac{\mathrm{T}x}{a}$$

$$\frac{d^2y}{dt^2}=g\sin\lambda\cos\omega t-\frac{\mathrm{T}y}{a}$$

$$\frac{d^2z}{dt^2}=g\cos\lambda-\frac{\mathrm{T}z}{a}.$$

These are the differential equations it was proposed to find.

It will be convenient to transform these equations into others containing new rectangular coordinates x', y', z' of P, referred to the same origin O, the axis of x' being in the plane AOZ at right angles to OA, the axis of y' perpendicular to this plane, and the axis of z' coincident with OA. It will be assumed that the positive direction of x' is towards the plane YOX, the positive direction of y' towards the plane YOZ, and the positive direction of z' that of the action of gravity. This being premised, the following relations between the two systems of coordinates may be readily found:

$$x=(z'\sin\lambda+x'\cos\lambda)\sin\omega t-y'\cos\omega t$$

$$y=(z'\sin\lambda+x'\cos\lambda)\cos\omega t+y'\sin\omega t$$

$$z=z'\cos\lambda-x'\sin\lambda.$$

By substituting these values of x, y, z in the foregoing equations, the following results may be obtained:

$$\frac{d^2x'}{dt^2}=-\frac{\mathrm{T}x'}{a}-2\omega\cos\lambda\frac{dy'}{dt}+\omega^2\cos\lambda(z'\sin\lambda+x'\cos\lambda)$$

$$\frac{d^2y'}{dt^2}=-\frac{\mathrm{T}y'}{a}+2\omega\left(\frac{dz'}{dt}\sin\lambda+\frac{dx'}{dt}\cos\lambda\right)+\omega^2y'$$

$$\frac{d^2z'}{dt^2}=g-\frac{\mathrm{T}z'}{a}-2\omega\sin\lambda\frac{dy'}{dt}+\omega^2\sin\lambda(z'\sin\lambda+x'\cos\lambda).$$

Multiplying these equations respectively by $2dx'$, $2dy'$, $2dz'$, adding them together, and integrating, we get (since $x'dx'+y'dy'+z'dz'=0$),

$$\frac{dx'^2}{dt^2}+\frac{dy'^2}{dt^2}+\frac{dz'^2}{dt^2}=C+2gz'+\omega^2.\{(z'\sin\lambda-x'\cos\lambda)^2+y'^2\}.$$

Let $\theta=$ the $\angle$ POZ which the cord makes with the fixed axis. Then

$$a^2\sin^2\theta=(z'\sin\lambda-x'\cos\lambda)^2+y'^2.$$

Hence if V = the velocity of the ball relatively to the plane AOZ, that is, the plane of the meridian, and if V_1, h, and θ_1 be the initial values of V, z', and θ, we have

$$V^2=V_1{}^2+2g(z'-h)+a^2\omega^2(\sin^2\theta-\sin^2\theta_1). \quad . \quad . \quad (\alpha)$$

Excepting for the last term, which is always very small on account of the factor ω^2, the expression for the velocity is the same as if the earth had no rotation.

Again, multiplying the first of the above equations by y', and the second by x', and subtracting, we have

$$y'\frac{d^2x'}{dt^2}-x'\frac{d^2y'}{dt^2}=-2\omega\cos\lambda\left(x'\frac{dx'}{dt}+y'\frac{dy'}{dt}\right)-2\omega\sin\lambda x'\frac{dz'}{dt}$$
$$+\omega^2y'\sin\lambda(z'\cos\lambda-x'\sin\lambda).$$

Hence by integration,

$$\left.\begin{array}{l} y'\dfrac{dx'}{dt}-x'\dfrac{dy'}{dt}=H-\omega\cos\lambda(x'^2+y'^2)-2\omega\sin\lambda\displaystyle\int x'\frac{dz'}{dt}\,dt \\ \quad +\omega^2\sin\lambda\displaystyle\int(y'z'\cos\lambda-x'y'\sin\lambda)dt \end{array}\right\}. \quad (\beta)$$

Putting now r'^2 for $x'^2+y'^2$, the term $-r'^2\omega\cos\lambda$ is twice the area described in the unit of time, by the perpendicular (r') from the centre of the ball on the vertical OA, in consequence of a horizontal angular motion of r' equal to $\omega\cos\lambda$. By considering in what directions x' and y' were reckoned positive, it will appear that the constant H is positive when the motion of the ball about OA is in the same direction as the earth's rotation, and consequently that the negative sign above indicates that the angular motion $\omega\cos\lambda$ is in the *contrary* direction. We have thus arrived at the following general result:—

Whatever other motion the ball may have, it has an apparent motion of rotation from left to right about the vertical, equal to the earth's rotation multiplied by the cosine of the co-latitude.

It will be seen that this is a complete explanation of the fact observed by M. Foucault. To compare the theoretical results more closely with the circumstances of the experiment, let us suppose the ball to have initially no velocity relatively to the plane AOZ, and also to be started in a direction perpendicular to the meridian at a small distance from the vertical. Thus we shall have in equation (α), $V_1=0$, $\theta_1=\lambda$ nearly, and the motion will at all times be small. The last term of that equation will be insignificant, both because ω^2 is very small, and because $\sin^2\theta-\sin^2\lambda$ will have in each excursion small positive, and small negative values of nearly equal magnitude. Thus very approximately,

$$V^2=2g(z'-h). \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (\gamma)$$

Again, in the equation (β), the term involving $\frac{dz'}{dt}$ will be insignificant, the motion being by supposition nearly perpendicular to the axis of z'. Also the factor ω^2 makes the last term very small. It is, however, to be observed, that while the product $y'z'$ is alternately positive and negative in each excursion of the ball by reason of the change of sign of y', the product $x'y'$ changes sign slowly, the excursions being nearly in a plane. The effect of the term involving $x'y'$ might possibly be sensible in a delicate experiment. Neglecting the small terms, we have

$$y'\frac{dx'}{dt}-x'\frac{dy'}{dt}=H-\omega\cos\lambda(x'^2+y'^2). \quad . \quad . \quad (\delta)$$

The equations (γ) and (δ) apply to any small vibrations affected by the earth's rotation. If the term in (δ) involving ω be omitted, the equations are the same that would be obtained on the supposition of no rotation, in which case, as is known, the ball moves in a small ellipse, the axes of which pass through the vertical and have given inclinations to the plane of the meridian. The effect of the rotation of the earth, from what has been shown, is to cause the ellipse to revolve relatively to the plane of the meridian about the vertical from left to right with the angular velocity $\omega\cos\lambda$.

The case of larger excursions might be treated in the manner employed by Mr. Airy in the Memoirs of the Royal Astronomical Society, vol. xx. p. 121, the foregoing result being taken as the first approximation, instead of supposing the axes of the ellipse to have fixed directions with respect to the plane of the meridian.

Cambridge Observatory,
April 5, 1852.

XLVIII. *On the supposed Identity of the Agent concerned in the Phænomena of ordinary Electricity, Voltaic Electricity, Electro-magnetism, Magneto-electricity, and Thermo-electricity.* By M. Donovan, *Esq., M.R.I.A.*

[Continued from p. 299.]

Section IV.

THE foregoing experiments and reasonings are adduced by Professor Faraday in support of his opinions relative to the enormous quantity of electricity which he conceives is naturally associated with matter. It was necessary to prove the existence of a source so abundant as the vast quantity supposed to constitute the current required. Without this peculiar character of the current, it could not be applied to explain the difference between the effects of ordinary and voltaic electricity. In further support of that opinion, his next object was to prove that supplies of ordinary electricity equally abundant are required to produce effects commensurate with those of voltaic electricity; and as the cause must equal the effect, he thus derives a new argument in favour of the intensely electrical condition of matter.

In furtherance of these views, he made many experiments intended "to obtain a common measure, or a known relation as to quantity, of the electricity excited by a machine, and that from a voltaic pile, for the purpose not only of confirming their identity" by proving "that the differences of intensity and quantity are quite sufficient to account for what were supposed to be their distinctive quantities," "but also of demonstrating certain general principles*."

To support the opinion of identity, and to account for the dissimilarity of the voltaic and ordinary electrical current, he has made it a chief object to adduce facts calculated to determine the ratio in which the two kinds of electricity are required to act in producing equal effects. A second object was to give additional support to his view of the absolute quantity of electricity with which matter is associated. To facilitate this and many other inquiries, he makes use of the following law:—"If the same absolute quantity of electricity pass through the galvanometer, whatever may be its intensity, the deflecting force upon the magnetic needle is the same†."

The general method adopted in his experiments, was to charge a Leyden battery with a certain number of turns of a powerful plate-electrical machine, occasionally varying the number of jars employed, to transmit the charge through a galvanometer, and to note the deflection. In the first experiment, eight jars were

* Researches, pars. 361, 378. † Ibid. par. 366.

"charged by thirty turns of the machine, and discharged through the galvanometer, a thick wet string about ten inches long being included in the circuit. The needle was immediately deflected 22°*."

Seven more jars were then added to the eight, and the whole fifteen were charged by thirty turns of the machine. When the discharge was passed through the galvanometer, the needle deviated exactly to the 22nd degree as before. The whole battery of fifteen jars was now charged with fifty turns of the plate, and the discharge was made through the galvanometer by means of various media. Through a thick wet string, the charge passed at once; with a thin string, it occupied a sensible time; and with a thread, it required two or three seconds. "The current, therefore, must have varied extremely in intensity in these different cases, and yet the deflection of the needle was the same in all of them†." Hence he infers the law already mentioned, that the same quantity of electricity affects the magnetic needle equally, no matter what the intensity.

In the first place, it is to be observed that this law has not received universal assent. Pouillet lays down the very reverse of it: he says, "We may call currents of the same *intensity* those which produce the same deflection:" "a current will have double or triple the intensity of another current when it produces deflections of which the sines are double or triple," as indicated by the compass of sines‡.

The experiments of Faraday, and the inference drawn from them, are very important, and require some scrutiny. He found, it is true, that thirty turns of the plate produced a quantity of electricity, which, whether received into eight or fifteen jars, and passed through the galvanometer, occasioned the same degree of deflection. But in this there appears nothing but what might have been expected and ought to happen. The quantity of electricity was the same in both cases: the intensity certainly differed when eight or fifteen jars were used, that is, while the electricity was contained in the jars; but in the act of discharging them through the galvanometer, the whole quantity, whether from eight or fifteen jars, passed through the coil, and was raised at that instant to the intensity which the small surface of the wire of the coil condensed it into and determined. It is quite indifferent whether the electricity of thirty turns of the plate-machine were diffused over the coated surface of eight jars, or fifteen, or one hundred; for although the intensity of that quantity while in the jars would, according to their number, be very different, yet the whole charge must pass from all the jars at the

* Researches, par. 363. † Ibid. 365.

‡ *Eléments de Physique*, vol. i. p. 326.

same instant; and then, by the reunion, the former intensity would be changed into a new one, which would be determined by the diameter of the wire constituting the coil, and its more or less perfect insulation. Hence the number of jars that contains the charge is of little consequence, when the whole electricity contained in them must be reunited in the galvanometer wire; be the number of jars what it may, the intensity in the coil will be the same so long as the total quantity is the same, and therefore the deflection will be the same.

The case is similar to that of the common experiment of melting an iron wire by the discharge of a Leyden battery. If the battery contain the quantity of electricity adequate to melt a certain length of the wire, it matters not whether the charge be contained in one jar, or ten, or one hundred. When the discharge takes place, the contents of all the jars will pass through the wire at the same instant, in the same degree of concentration as if the whole charge had been confined in one jar only; and the fusion of the same length of wire will as certainly follow in all cases. I take no account of a little waste which the connecting rods of the jars would cause.

Professor Faraday now charged the fifteen jars of the battery, not as before with thirty turns of the plate-machine, but with fifty; and made the discharge, sometimes with a mere wet thread, sometimes through 38 inches of thin string wet with distilled water, and sometimes through a string of twelve times the thickness, 12 inches in length, and soaked in dilute acid. The intensity of the current constituting the discharge must have varied, as he conceives, extremely in these several cases; and yet the deflection was "sensibly the same in all of them*."

In this experiment, so different in aspect yet so similar in results, there appears to be no real additional evidence in favour of the law deduced. In the first place, it is to be remembered that the greatest deflection of the needle in Colladon's experiments was 40°, and in Faraday's 41°. These deflections seem to have been produced by the maximum quantity of electricity which these particular galvanometer coils were capable of insulating; for the wire of the coil will not conduct, through its whole length, any quantity of common electricity which we choose to present to it. M. Colladon observes, that "electricity of great tension easily passes from one turn to another across the silk which separates them†." In proof of this, he found that by transmitting a current of electricity from a Nairne's machine, or a plate-machine of 5 feet diameter, through a Nobili's galvanometer, the deviation did not exceed 3° or 4°. But on employing a

* Researches, par. 365.

† *Annales de Chimie et de Physique*, vol. xxxiii. p. 67.

galvanometer of 500 turns of wire doubly covered with silk, each series of turns being separated by varnished taffetas, the deviation produced by the same machines was almost tenfold. Hence in Faraday's galvanometer, the silk was capable of carrying as much electricity as produced 40° or 44° of deflection*; and anything more than this quantity was probably transmitted laterally from wire to wire without passing through. He does not inform us what the amount of deflection was in these experiments; we therefore have no evidence of its agreement with the deflection produced by other charges of the Leyden battery.

But it is stated that the transmission of the charge through the different wet strings produced equal deflection, whatever its amount might have been. "With the thick string, the charge passed at once:" this cannot be intended to be literally understood, for then the discharge would constitute an explosion; the discharge must therefore be understood as having taken place in an exceedingly rapid current. "With the thin string, it occupied a sensible time;" that is, the current was not quite so rapid, yet still the period was so short that it was barely sensible. "And with the thread, it required two or three seconds before the electrometer fell entirely down."

We must carefully consider what happens when a Leyden battery is discharged, by a wet string, through a galvanometer. The charge of the battery in all Faraday's trials was the same, and would therefore in all the three cases make the same effort to pass at once, but would be retarded more or less according to circumstances. The circumstances which modify the passage of the electric discharge are the thickness, humidity, and length of the strings: as to the conducting power of the liquid with which the strings were moistened, it need not be considered in the case of common electricity. Faraday himself says, "the tension of machine electricity causes it, however small in quantity, to pass through any quantity of water, solutions, or other substances classing with these as conductors, as fast as it can be produced†." The only modifying influence which these three circumstances can exert, is to retard the velocity of the discharge; and yet the discharge must take place with all the velocity that the modifying circumstances will permit. This velocity, although somewhat retarded, is still very great. The intensity of the electricity confined in a Leyden battery is also very great; and that intensity will be communicated to a string wet with so good a conductor of common electricity as water is known to be, although it is not a perfect one. The electrical intensity of the string must be the same as that of the battery; and the quantity of electricity in the string at any one moment is as great as its

* Researches, pars. 297, 302, and 367. † Ibid. par. 453.

dimensions can endure: the conducting power of the water can only affect the velocity of the discharge, not the intensity of the charge. The difference of the dimensions of the strings will cause scarcely any difference of intensity in their charge, so exceedingly small is their surface compared with that of the coating of the Leyden battery. The surface of Faraday's battery being 3150 square inches, a thread 38 inches long and $\frac{1}{24}$th of an inch diameter will expose a surface 630 times less than the battery. The battery, therefore, being in all the three cases charged with fifty turns of the plate-machine, will impart the same intensity to the strings; and these will communicate the same intensity of charge to the galvanometer coil. As intensity is quantity compared to space, it follows that as the intensity in all the three cases is the same, so is the quantity in the coil at any particular instant of time; and so must be the deflection. But the state of the coil at the first instant would determine the swing of the needle; for the needle reaches its extreme deviation, not entirely in consequence of the true deflecting power of the electricity transmitted, but partly by the momentum which it has acquired from the first impulse; and before this ceases to act, or while the needle is in the condition of being constrained to resume its place by the power of terrestrial magnetism, the whole discharge will have taken place, no matter whether it occupied "a sensible time" only, or "two or three seconds."

Thus the velocity of the discharge from the Leyden battery, although modified by the thickness, length, and humidity of the string, has nothing to do with the intensity of the electricity which passes. One charge may pass more slowly than another, by meeting more resistance from the nature of the conducting substance, and yet be of the same intensity. The difference of time must, however, always be exceedingly small.

In another experiment, the battery of fifteen jars was charged by sixty revolutions of the machine, and discharged as before through the galvanometer: "the deflection of the needle was now as nearly as possible 44°," but the graduation was not accurate enough to determine that the arc was exactly double the former arc; to "the eye it appeared to be so." This result was expected: the charge in the battery was double, and the intensity of electricity in the galvanometer coil, through which the whole charge passed, was consequently doubled: no wonder, then, that the force which was to produce deflection of the needle was also doubled; but I shall have to make further observations on this experiment hereafter. We may therefore deny that the intensity was different in any of the experiments; and we are not bound to admit, that "if the same absolute quantity of electricity pass through the galvanometer, whatever may be its intensity, the deflecting force upon the magnetic needle is the same."

It is very important to recall here, that in Colladon's experiments it was clearly proved, although the inference was not drawn by him, that the highest deflections were produced by the highest intensities. With a Nairne's electrical machine, between the conductors of which a galvanometer was placed, he could only obtain a deflection of 3° or 4°; but when the charge of a large Leyden battery was silently passed through the galvanometer by means of a point, the deflection amounted to 40°. When he used a galvanometer capable of sustaining a high intensity, and consisting of 500 turns, a Nairne's machine required to be worked at the prodigious rate of three revolutions in a second in order to produce a deflection of 35°; and without this velocity, the necessary intensity not being produced, that amount of deflection could not be obtained. I do not see how these facts are reconcileable with the law in question.

The case appears to stand thus. The absolute quantity is the *total* charge contained in the Leyden battery, all of which the hypothesis assumes to pass in a current of successive portions. I think it will scarcely be denied, that no portion of electricity can act except that which is present in the galvanometer coil at any particular instant of time; the portions still in the Leyden battery can have no effect; or in other words, the total quantity is not the efficient cause of deflection. The portion in the coil at any particular instant can therefore only act according to its quantity, and the space occupied by it at that moment; and in point of fact, the first portion of electricity which enters it, as has already been shown, is that which determines the swing of the needle and its amount.

Even if it were admitted that, in the experiments with the three wet cords or threads, there were slight differences of intensity in them, the needle could scarcely be unequally affected, at least in any discoverable degree. We have only to admit, with Colladon, that the coil can carry a certain intensity of electricity, and no more; that Faraday's coil was charged to saturation with the current, which was conveyed into it by the thinnest of the wet strings; and that if any excess of electricity had been transmitted by the thicker strings, it overflowed laterally from wire to wire. The result of all these admissions would be, that the deflection must be equal for all. It is much to be regretted that we are not informed of the amount of these equal deflections.

There are other objections to the manner in which this law has been derived. It does not appear to correspond with common experience of the character of the electric fluid, to suppose that the discharge of a battery would pass through a wet thread without considerable loss from dispersion; or that dispersion by a wet thread would take place at the same rate as by 38 inches of a thicker wet string; or that the 38 inches of wet string would disperse equally with a string twelve times its thickness and one-third of its length: water, as everyone knows, has a singular power of dispersion. Nor can it be admitted without proof, that the galvanometer coil in all cases, especially that wherein the discharge of the battery occupied "two or three seconds," transmitted it without overflowing laterally from wire to wire, or from layer to layer, in the manner affirmed by Colladon to have taken place in his experiments, and which compelled him to use a coil covered with double silk, and with other silk interposed between the layers. Faraday used a galvanometer in which no such precautions were taken: if none such were necessary, why did Colladon fail when a common galvanometer was employed, and why did he succeed when he guarded against lateral communication?

One more observation may be made in connexion with these experiments. Faraday endeavoured to maintain the electric machine as much as possible at the same degree of excitation or power during the whole of his experiments with wet strings. On the equality of the power of the machine throughout depended the truth and value of the information conveyed by the galvanometer; and a very small change in the excitation of the machine would make a great difference in the amount of the charge communicated to the Leyden battery. Those who are in the habit of using electrical machines, know how much they are influenced by a number of causes: changes of weather, which sometimes take place within half an hour, alteration of the amalgam by friction, temporary cessation of working the cylinder, difference of rapidity in its revolutions, and other causes, will produce alterations of power which will be very manifest in the charge communicated to the Leyden battery. Every effort was no doubt made to maintain an equal excitation of the plate-machine during the continuance of the numerous experiments; but it is a question, is it possible to effect this object?

These arguments suggest a doubt that Professor Faraday's experiments warrant his inferences. I shall now assign reasons for believing that they lead to inferences of an opposite kind. He conceived that the deflections of the galvanometer are in the direct ratio of the quantity of electricity which pass through it. He found that a certain quantity of electricity thrown into eight jars, and passed through a galvanometer, produced a deflection of 22°; and that double the quantity of electricity thrown into fifteen jars caused a deflection, which, as near as the eye could judge, was 44°, or double the former deflection. The want of the sixteenth jar made no difference in the results; the double quantity of electricity was present. The inference drawn was, that the angular deflection was doubled because the quantity of electricity was doubled. The deflection and deflecting force would be in the same ratio, if every degree on the quadrant of the galvanometer were of equal value with regard to the resistance which terrestrial magnetism offers to the deflection of the needle*. But every experiment proves that this is not the case. It is proved by the researches of Lambert and Coulomb, that the effect of terrestrial magnetism on a freely suspended magnetic needle is as the sine of the angle formed by the magnetic meridian with the magnetic axis of the needle. The sine of an angle of 22° being taken as $=1$, the sine of an angle of 44° would be 1·854; to have doubled the sine, *i. e.* the force which produced a deflection of 22°, would require that the needle in the second instance should have stood at $48\frac{1}{2}$°. The law of Lambert would apply to every degree in the quadrant of a galvanometer if it had but one wire, and if that one coincided with the magnetic meridian; but in galvanometers with a coil, the law does not apply. Melloni has shown, that when the needles are nearly astatic, the first 20° are in the direct ratio of the deflecting forces†; but in the galvanometer of his construction, all degrees above 20° bear a different value. According to Melloni's table, Faraday's results would stand thus: the deflection 22° would indicate a force $=22·3$, but the deflection 44° would indicate a force $=78$. Hence when the needle pointed to 44°, instead of indicating the passage of double the quantity of electricity that traversed the wire when the deflection was 22°, it represented it just three and a half times greater. Thus it would appear that Faraday's experiments do not support the law that "the deflecting force of an electric current is directly proportional to the absolute quantity of electricity passed," but are at variance with it.

If the law itself fail, the comparison which he has drawn between the quantity of electricity produced during the chemical action of acidulated water on certain wires (to be immediately noticed), and that discharged from an electric machine, cannot be considered as proved. For it may be true that quantity has no more to do in the phænomenon than in the indications of Henley's electrometer; that intensity is the real condition for causing

* Pouillet, in describing a galvanometer with a coil, says "the deflection increases with the *intensity* of the current; but we know that it cannot in any manner be proportional to this intensity."—*Eléments de Physique*, vol. i. p. 501.

Faraday has not given the ratio of deflection to the deflecting force in his galvanometer.

† This holds true also according to the law of Lambert, for the sine of 20° is double the sine of 10°.

deflection; and that a little electricity, or one hundred times more, may produce equal deflection, provided that both quantities are constrained to pass through the galvanometer at the same degree of intensity. The intensity of the small quantity may endure for a much shorter time than that of the large quantity; but the effect on the needle once produced, it will not, during its swing, give any indication of the length of time (in cases of such momentary passage as that from a Leyden battery) during which the effort was sustained in the coil; the momentum of the needle will determine the rest.

The law already laid down was inferred by Faraday from his experiment of passing common electricity through a galvanometer under various circumstances. He states, that "the next point was to obtain a voltaic arrangement producing an effect equal to that just described" with common electricity. A wire of platinum and a wire of zinc, each being one-eighteenth of an inch in diameter, were properly connected with the galvanometer. Their other ends were plunged five-eighths of an inch deep in a mixture of one drop of strong sulphuric acid in four ounces of water, and were retained there for $\frac{8}{150}$ths of a minute, after which they were quickly withdrawn. The galvanometer needle was deflected to 22°, exactly as in the case of the previous experiment with common electricity. From this experiment with the wires, assisted by the law already referred to, he drew as a conclusion, that the electricity evolved during $\frac{8}{150}$ths of a minute, by a zinc wire and a platinum wire, each one-eighteenth of an inch diameter, immersed to the depth of five-eighths of an inch in four ounces of water containing one drop of sulphuric acid, is equal to the electricity produced by a Leyden battery charged by thirty turns of a powerful plate-machine which gave ten or twelve sparks an inch long each turn, from a brass conductor exposing 1422 square inches of surface. That is, a wire which presented one-ninth of a square inch in surface, afforded in $\frac{8}{150}$ths of a minute, electricity equal to 300 or 360 dense sparks taken from 1422 square inches (almost ten square feet) of a prime conductor. Hence, according to the above-mentioned law, he inferred the equality of the two "absolute quantities" of electricity from the fact, that the galvanometer needle suffered equal deflection from both quantities (371.).

The purpose of this experiment was still further to support the inferred identity of voltaic and frictional electricity, by reducing them under obedience to one common law; and next, to establish the estimate, already alluded to, of the enormous quantity of electricity with which matter is naturally associated. It is to be inquired how far the experiment supports either of these positions, although the considerations already adduced appear to offer sufficient objections without further arguments.

With this view I made the following experiments. A number of strips of thin milled zinc were prepared: each was half an inch in breadth and 10 inches long; one end of each terminated in a copper wire of about 6 inches in length; towards the other end, at an inch from the extremity, the strip was bent at a right angle; that inch of zinc was amalgamated at both sides, and well covered with a strong alcoholic solution of sealing-wax, except a quarter of an inch at the extreme end. By this method I intended, when these plates were dipped in dilute acid, to expose that portion of the metal only which was uncovered (viz. $\frac{1}{2}$ inch $\times$ $\frac{1}{4}$ inch) to its action. I had already found that mere immersion of the zinc strip to a regulated depth would not confine the action of the acid to the immersed surface; the effervescence produced always created an elevated ridge of bubbles round the zinc, and so the chemical action was extended above the level of the liquid more or less. I also prepared a number of zinc strips the same in all respects as the foregoing, except that they were not amalgamated.

A number of copper strips of the same size, shape and construction, were also prepared; they were well cleaned with sand-paper, and waxed as the rest.

A shallow porcelain tray was fitted into a stand, and from its sides diametrically opposite rose two standards, with a horizontal sliding cross-bar which moved up and down the standards; there were two notches in the cross-bar of such size as would confine the metallic slips when immersed in the tray.

The terminal wires of a strip of zinc and of a strip of copper being tightened by the binding-screws of an excellent galvanometer, the other ends of the plates which had been bent to a right angle now stood vertically over the porcelain tray, and were confined in the notches of the cross-bar; the chief length of each plate, therefore, lay horizontally from the galvanometer to the cross-bar. Two ounces measure of the exciting acid was poured into the porcelain tray previously levelled. Everything being ready, the time was noted, and the cross-bar carrying with it both the plates was moved downwards until their ends touched the bottom of the tray, and the unwaxed terminal portion of bright metal was immersed in the acid. The galvanometer needle started off; in a minute or less, steadily pointed to a certain degree, and after varying a little, suddenly fell, when the exposed portion of the zinc was dissolved away. The time was again noted, as also every period when the needle moved a degree or two.

I was thus precise, because the results seemed to be important: forty experiments were successively made with pairs of zinc and copper, one pair for each acid or strength of acid. It would be useless to give the details of all; the results of four experiments will suffice. In all cases the mixtures of acid and water were allowed to stand for twenty-four hours before using them, in order to guard against unequal temperature.

It may be premised that, independently of any objections now to be made, the law as laid down by Faraday is obnoxious to this objection, that if of several separate quantities of electricity the smallest be adequate to produce a deflection of 90°, all the greater quantities will be erroneously indicated by the same degree.

Experiment 1.—With two ounces by measure of acid, consisting of equal weights of concentrated sulphuric acid and water; the zinc plate newly amalgamated.

The needle settled at . .	69°
In 1 minute it fell to . .	68°
In 8′ rose to	$68\frac{1}{2}$°
In 20′ rose to	71°
In 32′ rose to	72°
In 48′ fell to	71°
In 51′ fell to	70°
In 53′ fell to	68°
In 55′ fell to	$66\frac{1}{2}$°
In 60′ fell to	66°
In 63′ fell to	65°
In 65′ fell to	64°
In 68′ fell to	63°

At this moment the solution of the zinc was completed, and the needle immediately sunk. Thus the exposed zinc was dissolved away in sixty-eight minutes.

Experiment 2.—With two ounces measure of acid, consisting of one part of concentrated sulphuric acid and five of water, both by weight; zinc newly amalgamated.

The needle settled at . .	67°
In one hour it rose to . .	68°
In 2° 45′ rose to	69°
In 3° 30′ fell to	68°
In 5° 10′ fell to	67°
In 5° 19′ fell to	60°
In 5° 30′ fell to	50°
In 5° 40′ fell to	45°

The solution of the zinc was now completed; the portion which had been exposed to the action of the acid was dissolved away in $5\frac{1}{2}$ hours nearly.

It appears that the same quantity of zinc was dissolved in the first experiment in 68 minutes, and in the second in $5\frac{1}{2}$ hours;

hence, averaging both, the quantity of electricity* that passed in every instant of time in the first case was five times greater than in the second; yet the deflections were scarcely different, far from being in the direct ratio of the quantity of electricity that was passing at any moment.

The next two experiments were made with unamalgamated zinc, but were in all other respects the same as the former.

Exp. 1.—With two ounces measure of acid, consisting of equal parts of concentrated sulphuric acid and water, both by weight.

The needle settled at . .	67°
In 7 minutes it fell to . .	65°
In 17′ fell to	62°
In 24′ fell to	61°
In 32′ fell to	59°

Thus the zinc was dissolved in 33 minutes.

Exp. 2.—With two ounces measure of acid, consisting of one part of acid and five of water, both by weight.

The needle settled at . .	66°
In 6 minutes it fell to . .	64°
In 7′ fell to	62°
In 9′ fell to	60°
In 10′ fell to	59°
In 11′ fell to	57°
In 13′ fell to	55°

The zinc was dissolved in 13 minutes; that is, in the first case the quantity of electricity that passed in every moment of time was two and a half times greater than that which passed in the second.

I repeated these experiments to the number of forty, with different acids, and different strengths of the same acid. All of them pretty nearly coincided in proving the general proposition, that notwithstanding the great difference in the quantities of electricity, which, according to the law in question, must have passed at any instant of time during the solution of the zinc, the deflections were as nearly the same as could be expected in cases of such delicacy, especially when the variations of electro-motive power incidental to milled zinc are taken into account†.

When the experiments were made with very weak acids, the galvanometer needle fell soon and rapidly, the cause of which I

* "The quantity of electricity is dependent upon the quantity of zinc oxidized."—Faraday's *Researches*, par. 919.

"The electricity of the voltaic pile is proportionate in its quantity to the quantity of matter which has been chemically active during its evolution."—Ibid. par. 916.

† Binks, Philosophical Magazine, N. S. vol. xi. p. 75.

found to be, that a blackish powder* was deposited on the zinc which partially defended it from further action. When this powder was removed and the plate replaced, the needle stood at the original degree, but soon fell again owing to the same cause.

It is to be observed, that in the foregoing experiments the same results may not be obtained if the same strip of copper be used more than once, the cause of which will be found in my "Essay on the Origin, Progress, and Present State of Galvanism," p. 288.

The term made use of in the enunciation of Faraday's law is "absolute quantity of electricity." The word *absolute*, perhaps, implies that the quantity is not restricted by the condition of intensity. If the idea of totality be involved, the expression can only apply to the discharge of the Leyden battery. When voltaic electricity is in question, no part of it can be considered active but that which is at any particular moment in the coil, without reference to the portion already passed through or yet to arrive, which latter may be said not yet to exist as the zinc is not yet dissolved. Hence the galvanometer can never be influenced by the whole quantity that is to pass; it indicates things present, not future.

If these experiments and reasonings be correct, and I do not perceive any source of fallacy, they appear unconformable to Faraday's law of equal quantities of electricity producing equal deflections irrespectively of other circumstances. Support is consequently withdrawn from his estimate of the enormous quantity of electricity naturally associated with matter, an estimate founded on his experiment in which the voltaic action of a pair of wires, acted on by acid, is said to have evolved electricity equal to 300 or 360 dense sparks from a powerful electric machine in the short space of $\frac{8}{150}$ths of a minute.

[To be continued.]

XLIX. *On the Longitudinal Lines of the Solar Spectrum. From a Letter to* Professor Dove *by* Professor Ragona-Scinà†.

[With a Plate.]

HERETOFORE the longitudinal lines of the solar spectrum have attracted little attention. It was believed by physicists that they were due to the minute imperfections of the glass of the prism, the little irregularities along the edge of the

* "Most zincs, when put into dilute sulphuric acid, leave more or less of an insoluble matter upon the surface in the form of a crust, which contains various metals, as copper, lead, zinc, iron, cadmium, &c., in the metallic state. Such particles, by discharging part of the transferable power, render it, as to the whole battery, local, and so diminish the effect." (Faraday, 1144.)

† From Poggendorff's *Annalen*, vol. lxxxiv. p. 590.

slit through which the light is admitted into the dark room, or to other similar causes; and that they were in no way related to the constitution of the light itself.

The numerous experiments which I have made in connexion with this subject, have led me to the conviction that the longitudinal lines are not due to the irregularities alluded to, but are produced by interference. Whoever is accustomed to experiments on light, will find the mere inspection of these lines sufficient to convince him that they are due to no mechanical cause. The clearness and beauty with which they exhibit themselves, and their sharp and definite character throughout their entire length, distinguish them at a glance from those which might be produced by unevenness of the slit's edge, particles of dust, imperfections of the apparatus, and so forth.

In the first place, I have observed that the longitudinal lines are entirely absent when a large lens is not applied, and when it is placed close to the prism and at right angles to the rays issuing from the same. I have further seen, that the lens changes the breadth of the spectrum only, and not its length.

Thus in one of my experiments, which was conducted with an equilateral vertical prism and a biconvex lens of 90 centimetres focal distance, after ascertaining by trial the position in which the spectrum was most clearly shown, I found its dimensions to be—

Length . . .	13·4 centims.
Breadth . . .	3·2 ...

The lens was then removed, and the position of the screen and prism remaining unchanged, the dimensions were found to be—

Length	13·4 centims.
Breadth . . .	15·8 ...

Hence the introduction of the lens caused the disappearance of 12·6 out of 15·8 parts of the spectrum; the light must have been compressed from a space of 15·8 to a space of 3·2.

In the latter space, the rays which had passed through the lens overlaid each other, as may be rendered evident by a very simple experiment. It is only necessary to move a bit of cardboard close to the lens from top to bottom, or the reverse, and thus to receive a portion of the rays passing through it. It is then seen, that no matter how great the portion may be which is thus intercepted, the dimensions of the spectrum remain unaltered, its brightness alone being more and more diminished as the intercepted portion becomes greater. This experiment establishes the fact of superposition, and the production of the longitudinal lines by interference is a simple consequence of this.

Let ST, Plate IX. fig. 7, be the breadth of the spectrum on the surface of the lens, and xy the plane of projection at the point where the longitudinal lines are visible. In the space *bc* a superposition of two bundles, *a*S*b*, *c*L*d*, takes place. The ray S*i*, which belongs to the first bundle, and the other L*i*, of the same colour, which belongs to the second, interfere with each other in *i* under the very small angle S*i*L. When the ray proceeding from S is intercepted by the card-board, the ray *i*S is absent at *i*, and consequently no interference occurs at this point of the spectrum.

It is really interesting to observe how every line may be caused to vanish by moving the card in a proper manner before the lens. From these experiments it follows, that the phænomenon of the longitudinal lines is not peculiar to the spectrum, but that in every case lines of interference must exist in light which has passed through a convex lens.

I therefore removed the prism, and made the slit in the window-shutter wider. White light now passed through the lens. By moving the plane of projection backwards and forwards, a position was at length found where the whole breadth of the white image was intersected by splendid black lines which crossed it horizontally.

It is scarcely necessary to remark, that I made many experiments to convince myself, that in the production of these lines no foreign influences come into play, which, however, is sufficiently proved by the mere inspection of them.

L. *On a Problem in Combinatorial Analysis.* *By* William Spottiswoode, *M.A. of Balliol College, Oxford**.

THE following problem,—

To arrange 7 systems, each consisting of 5 ternary combinations of the numbers 1, 2, 3, 4, 5, 6, 7, 8, 9, 10, 11, 12, 13, 14, 15, so that all the 15 numbers enter into each system, and no combinations whatever recur;

Or, as it is usually stated,—

To arrange 15 young ladies in such a manner that they may walk out 3 and 3 every day in the week, no lady ever walking twice with the same person,

is well known; but the following solution may, from its connexion with known laws of combination, be not without interest. I propose afterwards to notice some points respecting the general case to which the present problem belongs, or more strictly speaking, respecting those instances of the general case, to which the method here proposed is directly applicable.

First, to form the 35 ternary combinations, arrange the numbers as follows:—

* Communicated by the Author.

$$\left.\begin{array}{rrrrrrr}
\overset{15}{1.2} & \overset{14}{1.3} & \overset{13}{1.4} & \overset{12}{1.5} & \overset{11}{1.6} & \overset{10}{1.7} & \overset{9}{1.8} \\
 & \overset{13}{2.3} & \overset{14}{2.4} & \overset{11}{2.5} & \overset{12}{2.6} & \overset{9}{2.7} & \overset{10}{2.8} \\
 & & \overset{15}{3.4} & \overset{10}{3.5} & \overset{9}{3.6} & \overset{12}{3.7} & \overset{11}{3.8} \\
 & & & \overset{9}{4.5} & \overset{10}{4.6} & \overset{11}{4.7} & \overset{12}{4.8} \\
 & & & & \overset{15}{5.6} & \overset{14}{5.7} & \overset{13}{5.8} \\
 & & & & & \overset{13}{6.7} & \overset{14}{6.8} \\
 & & & & & & \overset{15}{7.8}
\end{array}\right\} \quad . \; . \; . \quad (1)$$

$$\left.\begin{array}{rrr}
\overset{15}{9.10} & \overset{14}{9.11} & \overset{13}{9.12} \\
 & \overset{13}{10.11} & \overset{14}{10.12} \\
 & & \overset{15}{11.12}
\end{array}\right\} \quad . \; . \; . \quad (2)$$

$$\left.\overset{15}{13.14}\right\} \quad . \; . \; . \quad (3)$$

Here it will be observed that the first triangular arrangement gives all the binary combinations of the numbers 1, 2, 3, 4, 5, 6, 7, 8, and the combinations of them with the numbers 9, 10, 11, 12, 13, 14, 15, so arranged that each of this latter group of numbers is combined once with all of the former group; the second triangular arrangement gives all the binary combinations of the numbers 9, 10, 11, 12, and the combinations of them with the numbers 13, 14, 15, so arranged that each of this latter group of numbers is combined once with all of the former group; and finally, the last arrangement, consisting, however, of only one term, is similar with respect to the numbers 13, 14 and 15. The third number in each ternary combination has been written *over* the numbers forming the binary combinations, in order to exhibit the laws of formation of the latter, and of the position of the former as distinctly from one another as possible.

To arrange the groups of 5 for each day, select a ternary combination from one of the two lower triangles, and omitting from the upper those combinations which the selected combination excludes, arrange the remainder (16 in number) as a determinant, omitting from the development of it those terms in which any of the upper numbers recur; thus, selecting the combination 13, 14, 15, the first determinant will be

$$\begin{array}{cccc}
\overset{12}{1.5} & \overset{11}{1.6} & \overset{10}{1.7} & \overset{9}{1.8} \\[1ex]
\overset{11}{2.5} & \overset{12}{2.6} & \overset{9}{2.7} & \overset{10}{2.8} \\[1ex]
\overset{10}{3.5} & \overset{9}{3.6} & \overset{12}{3.7} & \overset{11}{3.8} \\[1ex]
\overset{9}{4.5} & \overset{10}{4.6} & \overset{11}{4.7} & \overset{12}{4.8};
\end{array}$$

and omitting the superior numbers, the admissible terms will be

$$\begin{array}{ll}
(1.5)\,(2.7)\,(3.8)\,(4.6), & (1.5)\,(2.8)\,(3.6)\,(4.7), \\
(1.6)\,(2.7)\,(3.5)\,(4.8), & (1.6)\,(2.8)\,(3.7)\,(4.5), \\
(1.7)\,(2.6)\,(3.8)\,(4.5), & (1.7)\,(2.5)\,(3.6)\,(4.8), \\
(1.8)\,(2.5)\,(3.7)\,(4.6), & (1.8)\,(2.6)\,(3.5)\,(4.7), ;
\end{array}$$

repeating the process for the remaining 6 combinations in the triangle (2), *i. e.* for the remaining 6 days, there will result 8 terms for each day, from each of which groups one must be selected in such a manner that no combinations recur. The result is

15.1.2	14.1.3	13.1.4	12.1.5	11.1.6	10.1.7	9.1.8
14.6.8	13.6.7	15.5.6	15.7.8	12.3.7	14.2.4	15.3.4
11.4.7	12.4.8	11.3.8	13.2.3	10.2.8	13.5.8	14.5.7
10.3.5	11.2.5	9.2.7	10.4.6	9.4.5	9.3.6	12.2.6
9.12.13	15.10.9	14.12.10	14.11.9	15.14.13	15.12.11	13.11.10

which, on writing

$$1=a,\ 2=b,\ 3=c,\ 4=f,\ 5=d,\ 6=g,\ 7=e,\ 8=h,$$

$$15=i,\ 14=j,\ 13=k,\ 12=l,\ 11=m,\ 10=n,\ 9=o,$$

will agree with the solution given by Mr. Cayley in this Magazine, vol. xxxvii. p. 50.

This may be exhibited under a rather more general point of view, whereby all the tentative process is avoided, as follows. The 7 determinants of the fourth degree above mentioned are connected with the determinant

$$\begin{array}{cccccccc}
1.1 & 1.2 & 1.3 & 1.4 & 1.5 & 1.6 & 1.7 & 1.8 \\
2.1 & 2.2 & 2.3 & 2.4 & 2.5 & 2.6 & 2.7 & 2.8 \\
3.1 & 3.2 & 3.3 & 3.4 & 3.5 & 3.6 & 3.7 & 3.8 \\
4.1 & 4.2 & 4.3 & 4.4 & 4.5 & 4.6 & 4.7 & 4.8 \\
5.1 & 5.2 & 5.3 & 5.4 & 5.5 & 5.6 & 5.7 & 5.8 \\
6.1 & 6.2 & 6.3 & 6.4 & 6.5 & 6.6 & 6.7 & 6.8 \\
7.1 & 7.2 & 7.3 & 7.4 & 7.5 & 7.6 & 7.7 & 7.8 \\
8.1 & 8.2 & 8.3 & 8.4 & 8.5 & 8.6 & 8.7 & 8.8
\end{array}$$

(or, adopting Mr. Sylvester's notation,

$$\left\{\begin{matrix} 1 & 2 & 3 & 4 & 5 & 6 & 7 & 8 \\ 1 & 2 & 3 & 4 & 5 & 6 & 7 & 8 \end{matrix}\right\}$$

with the conditions

$$i\,.\,j=j\,.\,i,$$

i and j receiving successively all values from 1 to 8 inclusively), being those fourth minors which do not involve the principal constituents; and the solution of the problem will be given by selecting those terms in the development of the determinant which are perfect squares, and do not involve any of the principal constituents or repetitions of the superior numbers (supposed to be inserted as in the triangle (1)).

With respect to the general problem of ternary combinations of a given number of persons, it is clear that the number must be divisible by 3, and also uneven, because each person (*e. g.* No. 1) is to be combined with all the binary combinations of the rest in which no number recurs; a condition which would be obviously impossible if the number of terms with which No. 1 is to be combined (*i. e.* the given number less one) be odd, *i. e.* if the given number be even. From these considerations it is further observable, the number of binary combinations with which each number is to be combined will be $\frac{1}{2}(n-1)$, n being the given number. The first step, then, in the solution of the problem, according to the present method, will be to form a triangular arrangement of all the binary combinations of the first numbers $1, 2 \,..\, \frac{1}{2}(n+1)$ (in the above problem $n=15$, $\frac{1}{2}(n-1)=7$, $\frac{1}{2}(n+1)=8$). The next step will be to form a triangular arrangement of all the binary combinations of the numbers

$$\frac{1}{2}(n+1)+1, \quad \frac{1}{2}(n+1)+2, \,..\, \frac{1}{2}\left\{\frac{1}{2}(n+1)+1\right\};$$

and so on, until the numbers be exhausted. But here it must be again observed, that since the first triangular arrangement has exhausted all the binary combinations of the numbers $1, 2, \,..\, \frac{1}{2}(n+1)$ *inter se,* and all the combinations of the remaining numbers $\frac{1}{2}(n+1)+1$, $\frac{1}{2}(n+1)+2, \,..\, n$, with them, there now remains only the combinations of this last group *inter se;* and reasoning similar to that by which it was proved that the number n itself must be odd, would prove that $\frac{1}{2}(n-1)$ must be odd; and similarly, it would be seen that the number $\frac{1}{2}\left\{\frac{1}{2}(n-1)-1\right\}$

must be also odd, and so on. In other words, in order that the present method may be applicable, the operation of subtracting unity from the given number and dividing by two carried on successively, must never lead to an even number. From this it will be seen that n must belong to the series

$$\begin{gathered} 1 \\ 2.1+1 \\ 2(2.1+1)+1 \\ 2\big(2(2.1+1)+1\big)+1. \\ \cdot \quad \cdot \quad \cdot \end{gathered}$$

And, since the number n must also be divisible by 3, it can belong only to the even places (*i. e.* 2nd, 4th, . .) in this series. The numbers will be found to be

$$3,\ 15,\ 63,\ 255,\ 1023,\ 4095, \ldots;$$

the $2p$th term being

$$2^{2p-1}+2^{2p-2}+\ldots+2+1=2^{2p}-1,$$

which, as is well known, is always divisible by $2+1=3$, thus satisfying that condition of the problem.

In order to show that the number of ternary combinations formed in the manner above indicated is exactly sufficient for a solution of the problem, it may be remarked that the number of combinations in each triangular arrangement is $\frac{m(m+1)}{1.2}$, if m be the number of terms in the top row; and consequently, the numbers of ternary combinations will be

$$\begin{array}{lll} (2^{2p-1}-1)2^{2p-2}=2^{4p-3}-2^{2p-2} & & \text{in the 1st triangle,} \\ (2^{2p-2}-1)2^{2p-3}=2^{4p-5}-2^{2p-3} & & \text{in the 2nd triangle,} \\ \cdot \quad \cdot \quad \cdot & \cdot \quad \cdot & \cdot \quad \cdot \\ (2^2-1)2 & =2^3-2 & \text{in the } (2p-1)\text{th triangle,} \\ (2-1)1 & =2-1 & \text{in the } (2p-2)\text{th triangle,} \end{array}$$

the sum of which is

$$2.\frac{2^{4p-2}-1}{3}-2^{2p-1}+1=\frac{1}{3}(2^{4p-1}-2^{2p}-2^{2p-1}+1)$$

$$=\frac{1}{3}(2^{2p}-1)(2^{2p-1}-1),$$

i. e. the number of combinations is equal to the product of $\frac{1}{3}$ of the given number of ladies by the number of days on which the walks are to be taken, as it should be.

The solution of the general case here contemplated would depend upon the determinants

$$\begin{Bmatrix} 1, \ 2, \ldots 2^{2p-1} \\ 1, \ 2, \ldots 2^{2p-1} \end{Bmatrix}$$

$$\begin{Bmatrix} 2^{2p-1}+1, \ 2^{2p-1}+2, \ldots 2^{2p-1}+2^{2p-2} \\ 2^{2p-1}+1, \ 2^{2p-1}+2, \ldots 2^{2p-1}+2^{2p-2} \end{Bmatrix}$$

$$\cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot$$

$$\begin{Bmatrix} 2^{2p}-2, \ 2^{2p}-1 \\ 2^{2p}-2, \ 2^{2p}-1 \end{Bmatrix};$$

and taking the sum of the half of the orders of the alternate determinants of this series, as in the case of 15 persons, the sum is

$$\frac{2^{2p}-1}{3}$$

for the number of combinations for each day, as found above.

In conclusion, the following are the general results of the above considerations:—

Number of persons	3,	15,	63,	225,	1023,	4095,	$\ldots 2^{2p}-1$,
Number of days .	1,	7,	31,	127,	511,	2047,	$\ldots 2^{2p-1}-1$,
No. of combinations for each day	1,	5,	21,	85,	341,	1365,	$\ldots \dfrac{2^{2p}-1}{2+1}$.

LI. *On Rubian and its Products of Decomposition.* By Edward Schunck, F.R.S.

[Concluded from p. 226.]

Action of Sulphuric and Muriatic Acid on Rubian.—The action of these two acids is precisely the same; but for the purpose of studying it, it is better to employ sulphuric acid, as it is more easily removed again afterwards. On adding sulphuric acid in considerable quantity to a watery solution of rubian and boiling the liquid, no perceptible change takes place at first, except that the solution loses a little of its transparency and becomes slightly opalescent. If the solution was not very dilute, there begin to appear very soon a number of orange-coloured flocks. After boiling for some time and allowing to cool, these flocks are deposited in large quantities, and the liquid is now found to be much lighter in colour than before. After allowing to cool and filtering, the liquid, on being mixed with fresh acid and boiled again, often deposits on cooling a fresh quantity of these flocks. When after repeated boilings no more flocks separate on cooling, the process is completed. The last portions of rubian are usually more difficult to decompose than the first, and an additional quantity of acid is therefore necessary to effect their decomposition. The liquid retains to the last a light yellow colour. I shall return to it presently. The orange-coloured flocks are washed on the filter with cold water until all the acid is removed. They now consist of four different substances, three of which are bodies previously known, the fourth one which has not hitherto been observed. The three former are,—1st, *Alizarine*; 2ndly, the substance which in my former papers I have called *alpha-resin*, but to which I prefer giving the name of *Rubiretine*; 3rdly, the substance which I formerly termed *beta-resin*, but I shall now call *Verantine* from *Verantia*, the name applied to madder in the middle ages. The fourth substance I shall denominate *Rubianine*.

The presence of alizarine in this mixture is indicated by the dark and beautiful colours which are produced when it is employed for dyeing a piece of mordanted cloth, and which contrast forcibly with the faint and dull tints produced by rubian. It may also easily be separated from the other substances by dissolving the mixture in alcohol, adding hydrate of alumina to the solution, filtering, treating the alumina compound repeatedly with a solution of carbonate of potash or soda, until nothing more is dissolved by the latter, decomposing the alumina compound with acid, and dissolving the residue in alcohol, when on evaporating the latter crystals of alizarine with its usual characters are obtained. In order however to obviate all objections which might arise from the use of alkalies in regard to the effect which the latter might be supposed to have in causing the formation of the alizarine, I determined if possible to use acids and salts only in the separation of the substances mentioned above. Of the four substances contained in the orange-coloured flocks, two, viz. alizarine and rubianine, are soluble in boiling water, and may thereby be separated from the two others which are insoluble in water. This method of separation is however tedious, on account of the sparing solubility of alizarine and rubianine in boiling water. I therefore prefer using the following method. The orange-coloured flocks containing the four substances are treated with boiling alcohol, in which they dissolve with a dark reddish-yellow colour. The alcohol is filtered boiling hot, and deposits on cooling a small quantity of yellow crystalline particles, consisting chiefly of rubianine. The treatment with alcohol is repeated as long as the latter acquires a dark yellow colour. The greatest part of the rubianine remains behind as a yellow or brownish-yellow crystalline mass, which is treated repeatedly with boiling alcohol, in which the whole at last dissolves, the greatest part again separating on the solution cooling, either in yellow needles or as a brownish-yellow crystalline mass. If its colour is not a pure yellow, or if it is imperfectly crystallized, it contains verantine and must be purified. For this purpose the whole of the mass which has been deposited on the alcohol cooling, after being collected on a filter, is again dissolved in boiling alcohol, and sugar of lead is added to the solution, by which means the verantine is precipitated in combination with oxide of lead, while the rubianine remains in solution and is again deposited, when the solution, after being filtered boiling hot, is allowed to cool, in long, lemon-yellow silky needles, which may be rendered perfectly pure by recrystallization. The compound of verantine and oxide of lead may be decomposed with sulphuric acid, and the verantine separated from the sulphate of lead by boiling alcohol. The alcoholic liquid from which the rubianine has been deposited contains the three other substances besides a portion of the rubianine. By adding acetate of alumina to it, the whole of the alizarine as well as a part of the verantine are precipitated, in combination with alumina, in the shape of a dark red powder, while the liquid retains a dark brownish-red colour. This precipitate, after being collected on a filter and washed with alcohol until the latter runs through colourless, is decomposed with muriatic acid, which dissolves the alumina, leaving behind red flocks consisting of alizarine and verantine. These flocks, after being filtered off and washed with water, are again dissolved in alcohol, to which is then added a solution of neutral acetate of copper. This instantly changes the colour of the liquid to a beautiful dark purple. The copper compound of alizarine remains dissolved, while the verantine is entirely precipitated, in combination with oxide of copper, as a dark reddish-brown powder. The dark purple liquid, after filtration and evaporation, leaves a purple mass of alizarine-oxide-of-copper, which is decomposed with muriatic acid. Yellow flocks, consisting of alizarine, remain behind, which after being washed with water are dissolved in alcohol. The alcoholic solution on evaporation gives crystals of alizarine, which may be purified by recrystallization. The compound of verantine with oxide of copper is decomposed with muriatic acid. The liquid filtered from the alumina compound of alizarine and verantine is evaporated to dryness, muriatic acid is added to the residue, which is placed on a filter and washed with cold water until all the acid and salts of alumina are removed. On being now treated with boiling water, a quantity of dark brown resinous drops sink to the bottom of the vessel and cohere into a semi-fused mass, while brownish-yellow flocks float in the water. The water is decanted from the mass at the bottom, carrying with it the flocks. This process is repeated with fresh quantities of water until no more flocks are carried away by it. The resinous mass at the bottom now consists principally of rubiretine. It may be purified by dissolving in cold alcohol, which leaves behind a quantity of verantine. The brownish-yellow flocks consist chiefly of verantine and rubianine; they are treated with boiling water, in which the rubianine dissolves, and from which it is again deposited, on filtering the water boiling hot and allowing to cool, in orange-coloured flocks. The process is repeated until the water dissolves nothing more. The orange-coloured flocks of rubianine are collected on a filter and dissolved in boiling alcohol, out of which the rubianine crystallizes on cooling in yellow needles. The mother-liquor is somewhat darker than a mere solution of rubianine would be. It contains a little alizarine and rubiretine, which may be separated by means of acetate of alumina, as before described. The verantine which is left behind by the boiling water is mixed with the other portions obtained from the lead and copper compounds, and the whole is dissolved in a small quantity of boiling alcohol, out of which the verantine is deposited on cooling as a dark reddish-brown or yellowish-brown powder, which may be purified by a second solution in alcohol.

These substances can, as may be supposed, be obtained without any difference in properties by adding sulphuric or muriatic acid to an extract of madder made with boiling water, boiling the liquid, and treating the dark green precipitate obtained in the same way as the orange-coloured flocks, from the decomposition of rubian. The dark green colour of the precipitate in this case proceeds from the decomposition of chlorogenine by the acid; the product of decomposition does not however in any way interfere, as it is insoluble in alcohol. It may be remarked, however, that very little rubianine is obtained in this manner, its place being supplied, from a cause which I shall mention hereafter, by rubiacine.

There still remains in the acid liquid filtered from the orange-coloured flocks, a substance which is an essential product of the action of acids on rubian. This liquid has, as I mentioned before, a light yellow colour. After neutralizing the acid with carbonate of lead it becomes almost colourless, while the carbonate of lead acquires a pink tinge. After filtration it is found to contain neither sulphuric acid nor lead; nor does it give any precipitate with neutral or basic acetate of lead, nor with alkalies, either before or after neutralization, unless it be boiled with an excess of the latter. This absence of reaction proves that no substance of a basic nature has been formed during the process. The liquid however contains a considerable quantity of an organic substance, which is obtained by carefully evaporating at the ordinary temperature over sulphuric acid. It is not advisable to evaporate with the assistance of heat, as the solution then becomes dark brown from the action of the air. After evaporation over sulphuric acid there is left at last a brownish-yellow, transparent syrup, having a sweetish taste, which I shall prove by its properties and composition to be a species of sugar.

I shall now describe more in detail the properties of the substances just mentioned.

Alizarine.—The alizarine obtained from the decomposition of rubian exhibits all the usual properties of this well-known substance. Its colour is dark yellow without any tinge of brown or red. The crystals possess a lustre which I have never seen equalled in this substance. Its analysis gave the following results:—

0·3200 grm. of the crystals, on being heated in the water-bath, lost 0·0580 grm. of water=18·12 per cent. According to the formula $C^{14} H^5 O^4 + 3HO$, they should lose 18·24 per cent.

0·2575 grm. of the dry substance, burnt with chromate of lead, gave 0·6550 carbonic acid and 0·0945 water.

These numbers lead to the following composition:—

	Eqs.		Calculated.	Found.
Carbon . . .	14	84	69·42	69·37
Hydrogen . .	5	5	4·13	4·07
Oxygen . . .	4	32	26·45	26·56
		121	100·00	100·00

0·1050 grm. of the lead compound, prepared by precipitating the alcoholic solution with sugar of lead, gave 0·0720 sulphate of lead, equivalent to 0·0529 oxide of lead=50·44 per cent. The formula $C^{14} H^4 O^3 + PbO$ requires 49·90 per cent. oxide of lead.

The formula here given is the same to which I was led by my former experiments, and it now receives a new confirmation from the relation in which it stands to that of rubian.

The formation of alizarine from rubian admits of a very easy explanation. By simply losing 14 equivs. of water, 1 equiv. of rubian is converted into 4 equivs. of alizarine, as the following equation shows:—

$$C^{56} H^{34} O^{30} = 4(C^{14} H^5 O^4) + 14HO.$$

The action of sulphuric acid in the preparation of garancine from madder now becomes more intelligible. It consists simply, as far as the practical effect is concerned, in the conversion of rubian into alizarine.

MM. Wolff and Strecker, in a late paper 'On the Red

Colouring Matters of Madder*,' have given another formula for alizarine, which they prefer on account of the relation in which they suppose this substance to stand to naphthaline. This formula is $C^{20} H^6 O^6$, which requires in 100 parts—

Carbon	68·96
Hydrogen	3·45
Oxygen	27·59

In confirmation of this formula they adduce one analysis, in which they obtained from 0·0650 grm. alizarine 0·163 carbonic acid, equivalent to 68·4 per cent. If it be permitted to deduce any safe inference from the analysis of so small a quantity of substance, I should be inclined to say that the substance analysed was impure. Even when perfectly well crystallized, alizarine may contain an amount of impurity sufficient to affect its composition. This impurity generally consists of verantine. A large admixture of the latter substance entirely prevents alizarine from crystallizing, but a small quantity merely gives the crystals a brownish or reddish tinge. Alizarine can never be considered as perfectly pure unless it exhibits a pure dark yellow colour without admixture of red. In proof of this statement I may adduce the following experiments. In the course of my investigation I obtained from madder a specimen of alizarine in perfectly well-defined crystals, containing apparently no foreign substance, but having a brownish-red colour instead of the dark yellow characteristic of pure alizarine.

0·3530 grm. of these crystals, dried at 100° C., gave 0·8855 carbonic acid, equivalent to 68·41 per cent. of carbon.

The remainder of the substance was recrystallized from alcohol, and

0·2940 grm. now gave 0·7360 carbonic acid, equivalent to 68·27 per cent. of carbon.

On dissolving the rest in alcohol and adding to the solution acetate of copper, a dark reddish-brown precipitate of verantine-oxide-of-copper fell. From the dark purple solution, after being treated in the manner before described, there were obtained some beautiful dark yellow crystals of alizarine, which on analysis yielded the following result:—

0·1270 grm., dried at 100° C., gave 0·3245 carbonic acid, equivalent to 69·68 per cent. of carbon.

It appears therefore that alizarine cannot be separated from the last portions of impurity by recrystallization merely.

Were the above formula the correct one, it would be difficult to account for the circumstance that the purer the substance the greater is the excess, not only of hydrogen, but also of carbon.

* *Ann. de Chem. u. Pharm.*, vol. lxxv. p. 1.

An excess of 0·6 per cent. of hydrogen, as this formula would pre-suppose, is unusually large; an excess of 0·4 per cent. of carbon is seldom or never obtained in the analysis of a pure substance. The most common impurity of alizarine is verantine; and since the latter contains, as I shall presently show, more oxygen for the same amount of carbon and hydrogen, it follows that if a portion of it be mixed with the alizarine, the amount of carbon and hydrogen in the latter will be reduced, and the composition will approximate to that given by Wolff and Strecker.

Again, if the correct formula for alizarine be $C^{20} H^6 O^6$, the formula of rubian must necessarily be $C^{20} H^{11} O^{11}$, which requires in 100 parts—

Carbon	54·79
Hydrogen	5·02
Oxygen	40·19

These numbers, as will be perceived, do not agree so well with those of the analyses as those corresponding to the formula which I have given above. The lead compound of rubian can, under this supposition, only be represented by the formula $6(C^{20} H^{11} O^{11}) + 13PbO$, which requires in 100 parts—

Carbon	26·03
Hydrogen	2·38
Oxygen	19·10
Oxide of lead	52·49

Here also it will be seen there is less accordance with the numbers found by experiment than in the case of the other formula. But besides this, the latter formula is of too complicated a nature to be received; I therefore consider the formula $C^{14} H^5 O^4$, or perhaps $C^{28} H^{10} O^8$, for alizarine to be as firmly established as it can be with the means at present at our command.

Verantine.—This substance coincides in most of its properties with the substance to which I formerly gave the name of the beta-resin of madder. When prepared according to the method above described, it is obtained in the shape of a reddish-brown powder, similar in colour to snuff or roasted coffee. It has the following properties. When heated on platinum-foil it melts and then burns away without leaving any residue. When heated in a glass tube it gives a small quantity of an oily sublimate without a trace of anything crystalline. When however it contains alizarine, as it very often does, it gives on being heated a crystalline sublimate, consisting of the latter substance. It dissolves in concentrated sulphuric acid with a brown colour, and is reprecipitated by water in brown flocks. On heating the solution in concentrated sulphuric acid it becomes black, sulphurous acid is disengaged, and the substance is decomposed. Concentrated nitric acid dissolves it on boiling with a disengagement of nitrous acid, forming a yellow liquid, from which nothing separates on cooling. Dilute nitric acid does not affect it sensibly on boiling. It is almost insoluble in boiling water, but readily soluble in boiling alcohol with a dark brownish-yellow colour, and is again deposited, on the alcohol cooling, as a brown powder, which is its most characteristic property. It is soluble in alkaline liquids with a dirty brownish-red colour, and is reprecipitated by acids in brown flocks. If it be mixed with alizarine, then its solutions in alkalies have a reddish-purple colour. The ammoniacal solution loses its ammonia on evaporation, and leaves the substance behind as a brown transparent pellicle. The ammoniacal solution gives precipitates with the chlorides of barium and calcium. The alcoholic solution gives dark brown precipitates with the acetates of lead and copper, as I mentioned before. When it is free from alizarine, it does not communicate any colour to mordanted cloth, and is therefore no colouring matter in the usual sense.

In the opinion of most chemists who have examined madder, this root contains two distinct colouring matters, viz. alizarine and another, to which the names of *purpurine, oxylizaric acid* and *madder-purple* have been applied by different chemists. This opinion has been advocated with considerable ability by MM. Wolff and Strecker. I have however reason to suppose that purpurine is in fact no distinct substance, but a mixture of alizarine and verantine. The latter substance accompanies almost all the products which are obtained from madder, and it is this body which renders them so difficult to purify. It adheres so pertinaciously to alizarine, as to induce the belief that the two actually form a chemical compound. The mixtures of the two vary in appearance from that of dark red crystals to that of a red crystalline powder. In these mixtures the verantine may easily be detected by dissolving in alcohol and adding acetate of copper, which precipitates the verantine, as before described. It also accompanies rubianine and renders it difficult to crystallize, as I mentioned above, and I have never been able to obtain rubiretine without some trace of it. As a characteristic of purpurine is mentioned its property of giving a cherry-red solution with alkalies, having none of the violet appearance belonging to alkaline solutions of alizarine; and also its forming, when treated with boiling alum-liquor, a red opalescent solution, from which it separates again in orange-coloured flocks on the solution cooling. Now by adding to a solution of alizarine in caustic alkali a little verantine, the beautiful violet colour of the solution may be instantly changed to reddish-purple; and by dissolving in it still more of that substance the colour may be rendered cherry-red, these colours being evidently mixtures of the violet due to alizarine and the brownish-red produced by verantine. Pure alizarine is not more soluble in boiling alum-liquor than in water, as has been repeatedly shown; it only communicates to the liquor a yellow colour, and crystallizes out again on the liquid cooling. Verantine is still less soluble in alum-liquor. If this substance be dissolved in caustic alkali and be then precipitated with a solution of alum, the precipitate does not dissolve in the least degree, however much alum be added; it only communicates a slight yellow tinge to the liquid. If, however, a mixture of alizarine and verantine be dissolved in caustic alkali and they be then precipitated together by means of a solution of alum added in excess, then on boiling the precipitate with the liquid, a bright red solution is obtained, and on filtering and allowing to cool, orange-coloured flocks are deposited, while the liquid still remains red, but gives a yellow precipitate on the addition of acid. By treating the residue with additional quantities of alum-liquor more is dissolved with the same colour, and this continues until either the alizarine or the verantine, whichever of the two was present in the smallest quantity, is removed. From this experiment I am inclined to conclude that alizarine and verantine are capable of forming a double compound with alumina soluble in boiling water, and that a mixture of the two in the proportion in which they exist in this compound, constitutes what has been called purpurine. At all events, it follows that alum is not adapted as a means of separating the substances derived from madder. The fact of rubianine also dissolving in boiling alum-liquor and crystallizing out again on cooling, is an additional objection to its use.

The difficulty of obtaining pure verantine in sufficient quantity for the purposes of analysis, has prevented me from determining its composition with the requisite accuracy. I have however obtained approximations sufficiently near to remove almost all doubts on the question.

I. 0·3280 grm. verantine, dried at 100° C. and burnt with chromate of lead, gave 0·7865 carbonic acid and 0·1215 water.

II. 0·3220 grm. gave 0·7740 carbonic acid and 0·1205 water.

III. 0·2890 grm. gave 0·6995 carbonic acid and 0·1040 water.

IV. 0·1255 grm. gave 0·3010 carbonic acid.

These numbers agree best with the following composition:—

	Eqs.		Calculated.	I.	II.	III.	IV.
Carbon .	14	84	65·11	65·39	65·55	66·01	65·41
Hydrogen .	5	5	3·87	4·11	4·15	3·99	
Oxygen .	5	40	31·02	30·50	30·30	30·00	
		129	100·00	100·00	100·00	100·00	

The composition here given approaches that of the oxylizaric acid of Debus, who obtained in analysing that substance as a mean of his experiments in 100 parts,—

Carbon	66·40
Hydrogen	3·82
Oxygen	29·78

The baryta compound, prepared by precipitating the ammoniacal solution with chloride of barium, gave on analysis the following results:—

0·2605 grm., dried at 100° C. and burnt with chromate of lead, gave 0·4640 carbonic acid and 0·0740 water.

0·4055 grm. gave 0·1835 sulphate of baryta.

In 100 parts:—

Carbon	48·57
Hydrogen	3·15
Oxygen	18·60
Baryta	29·69

The formula $C^{42} H^{13} O^{13} + 2BaO = 2(C^{14} H^4 O^4 + BaO) + C^{14} H^5 O^5$ requires in 100 parts—

Carbon	48·27
Hydrogen	2·49
Oxygen	19·93
Baryta	29·31

The compound with oxide of copper gave the following results:—

0·3680 grm., dried at 100° C. and burnt with chromate of lead, gave 0·7050 carbonic acid and 0·1030 water.

0·4710 grm. gave 0·1200 oxide of copper.

These numbers correspond very nearly to the following composition:—

	Eqs.		Calculated.	Found.
Carbon	14	84	52·50	52·24
Hydrogen	4	4	2·50	3·10
Oxygen	4	32	20·00	19·19
Oxide of copper .	1	40	25·00	25·47
		160	100·00	100·00

Another specimen, prepared in exactly the same manner and having the same appearance, gave a different composition.

0·4375 grm. gave 0·8910 carbonic acid and 0·1345 water.

0·5530 grm. gave 0·1080 oxide of copper.

This gives the following composition:—

	Eqs.		Calculated.	Found.
Carbon	56	336	55·17	55·54
Hydrogen	17	17	2·79	3·41
Oxygen	17	136	22·34	21·53
Oxide of copper .	3	120	19·70	19·52
		609	100·00	100·00

The formula of this compound must be expressed in the following manner:—

$$3(C^{14}H^4O^4+CuO)+C^{14}H^5O^5.$$

These analyses show that verantine, like many substances, the acid character of which is not well-marked, combines with bases in various and complicated proportions.

It appears therefore that verantine differs from alizarine by containing 1 equiv. more of oxygen. According to Debus, the same relation exists between alizarine and his oxylizaric acid. He gives for alizarine the formula $C^{30}H^{10}O^9$, and for oxylizaric acid $C^{15}H^5O^5$, so that 2 equivs. of the latter contain 1 equiv. more oxygen than 1 equiv. of the former. If my view of the composition of these substances be the correct one, the relation subsisting between the two is still more simple. Nevertheless I have some hesitation in asserting that verantine is to be considered as a higher oxide of the same radical as alizarine, or in supposing that it may be formed by oxidation from the latter. Its formation is due, as I shall presently show, not to any process of oxidation, but rather to the splitting up of an atom of rubian into two bodies.

Rubiretine.—This substance is identical with that which I formerly called the alpha-resin of madder, from which it does not differ in properties. It is obtained as a dark brown, opake resinous mass, brittle when cold, but becoming soft and almost melting in boiling water. On being heated to a higher degree, it melts completely without being decomposed. It is generally found to be mixed with a small quantity of verantine, from which it may be separated by solution in cold alcohol, which leaves the greatest part of the verantine behind; I have however found it impossible to remove the last traces of that substance. It is almost insoluble in boiling water. Its solution in alcohol is dark yellow. It dissolves in concentrated sulphuric acid with a yellowish-brown colour, and is decomposed on boiling the solution with blackening and disengagement of sulphurous acid. Boiling nitric acid changes it into a yellow substance, which no longer softens at the temperature of boiling water, and is very little soluble in alcohol. It dissolves in alkaline liquids with a brownish-red colour, and is reprecipitated by acids in brown flocks, which on boiling the liquid cohere into dark-brown semifluid masses. When heated in a glass tube, it usually gives a small quantity of sublimed alizarine mixed with a brown oil. It is not capable of dyeing when quite free from alizarine. Its analysis yielded the following results:—

I. 0·5300 grm. from the decomposition of rubian, dried at 100° C. and burnt with chromate of lead, gave 1·3190 carbonic acid and 0·2405 water.

II. 0·3785 grm. of the same preparation as the last, heated to the melting-point, gave 0·9465 carbonic acid and 0·1730 water.

III. 0·4815 grm. obtained directly from madder, dried at 100° C., gave 1·2130 carbonic acid and 0·2335 water.

IV. 0·4290 grm. of the same preparation as the last, heated to the melting-point, gave 1·0735 carbonic acid and 0·2050 water.

V. 0·3120 grm. of another preparation obtained directly from madder, gave 0·7855 carbonic acid and 0·1460 water.

VI. 0·2350 grm. of the same preparation as the last, gave 0·5865 carbonic acid and 0·1065 water.

In 100 parts it therefore contains—

	I.	II.	III.	IV.	V.	VI.
Carbon . .	67·87	68·19	68·70	68·24	68·66	68·06
Hydrogen .	5·04	5·07	5·38	5·30	5·20	5·03
Oxygen . .	27·09	26·74	25·92	26·46	26·14	26·91

I endeavoured in vain to determine the atomic weight of this substance. Neither the lead nor the baryta compound gave results which harmonized either with one another or with the analyses of the substance itself. There is however only one formula which is in accordance with the analyses, and at the same time satisfactorily explains its formation. This formula is $C^{14}H^{6}O^{4}$, which requires in 100 parts—

Carbon	68·85
Hydrogen . . .	4·91
Oxygen	26·24

It may be remarked that this is also the composition of benzoic acid; and even if the formula of rubiretine should not be exactly that given above, but perhaps the double or triple of it, it still remains remarkable that two such very different substances should have the same percentage composition.

The formation of rubiretine from rubian can only be explained in connection with that of verantine. If 2 equivs. of verantine, 2 equivs. of rubiretine and 12 equivs. of water be added together, the sum will be equal to 1 equiv. of rubian, as follows :—

$$
\begin{array}{lll}
\text{2 equivs. of Verantine} & = & C^{28}\,H^{10}\,O^{10} \\
\text{2 equivs. of Rubiretine} & = & C^{28}\,H^{12}\,O^{8} \\
\text{12 equivs. of Water} & = & \phantom{C^{28}}\,\underline{H^{12}\,O^{12}} \\
\text{1 equiv. of Rubian} & = & C^{56}\,H^{34}\,O^{30}
\end{array}
$$

If this be the correct representation, it follows that verantine and rubiretine stand in an intimate relation to one another, that the formation of one always indicates that of the other. In confirmation of this view, I may state that I have never seen the formation of one of these substances taking place without it being possible to detect the presence of the other.

Rubianine.—This substance, as I mentioned before, has not hitherto been observed among the bodies derived from madder. It greatly resembles rubiacine in its appearance and many of its properties; it may however easily be distinguished by several characteristics, and above all by its composition. It is obtained from a solution in boiling alcohol in the form of bright lemon-yellow, silky needles, which, when dry, form an interwoven mass. It is soluble in boiling water, more so in fact than any of the products of decomposition hitherto mentioned. It crystallizes out again on the solution cooling in yellow silky needles. It is less soluble in alcohol than the preceding substances. Its colour is lighter than that of rubiacine. When heated on platinum-foil it melts to a brown liquid, then burns, leaving a carbonaceous residue, which on further heating disappears entirely. When heated in a glass tube it gives a small quantity of a yellow crystalline sublimate, but not by far so large a quantity as is obtained under the same circumstances from rubiacine, which, when carefully heated, may be almost entirely volatilized. It is soluble in concentrated sulphuric acid with a yellow colour; the solution on boiling becomes black and gives off sulphurous acid. A solution of rubiacine in concentrated sulphuric acid, remains quite unchanged on boiling. It is not affected either by dilute or concentrated nitric acid, even on boiling; it merely dissolves in them, and crystallizes out again on the acid cooling, just as from boiling water. When treated in the cold with a solution of carbonate of potash or soda, or liquid ammonia, it does not dissolve, nor is its colour at all changed. When the liquid is boiled, it dissolves however with a blood-red colour. Nevertheless it cannot be said to combine with the alkali, but merely to be dissolved by it; for on allowing these solutions to stand for some time, a yellow crystalline mass again separates, which is nothing but the substance itself. The ammoniacal solution gives red precipitates with the chlorides of barium and calcium. The alcoholic solution gives no precipitate with sugar of lead, whereas a solution of rubiacine gives a dark red precipitate with sugar of lead. It dissolves in a concentrated solution of perchloride of iron with a dark brown colour, but is not thereby converted into rubiacic acid. It communicates to mordanted cloth only a slight tinge of colour, similar to that produced by rubiacine.

Its analysis gave the following results:—

I. 0·3520 grm. substance, dried at 100° C. and burnt with chromate of lead, gave 0·7400 carbonic acid and 0·1750 water.

II. 0·3805 grm. of the same preparation gave 0·7990 carbonic acid and 0·1890 water.

III. 0·3965 grm. of another preparation gave 0·8330 carbonic acid and 0·1890 water.

IV. 0·2480 grm. of the same preparation as the last, recrystallized from alcohol, gave 0·5290 carbonic acid and 0·1280 water.

V. 0·3735 grm. of a third preparation gave 0·7925 carbonic acid and 0·1785 water.

VI. 0·3995 grm. of the same preparation gave 0·8450 carbonic acid and 0·1855 water.

These numbers correspond in 100 parts to—

	I.	II.	III.	IV.	V.	VI.
Carbon . .	57·33	57·26	57·29	58·17	57·86	57·68
Hydrogen .	5·52	5·51	5·29	5·73	5·31	5·15
Oxygen . .	37·15	37·23	37·42	36·10	36·83	37·17

I have as yet been unsuccessful in my attempts to determine the atomic weight of rubianine. The little affinity which it has for bases is proved by the fact above mentioned, of its crystallizing unchanged out of its alkaline solutions. The baryta compound, which is obtained by adding chloride of barium to its ammoniacal solution, is easily decomposed when it comes to be washed with pure water, the baryta being dissolved by the water, a yellow residue of rubianine being left at last. It is not capable of separating oxide of lead from acetic acid. In fact it nearly approaches the character of a perfectly neutral body, a circumstance which might be *à priori* foreseen from its containing more carbon and less oxygen than rubian itself, the properties of which are not far removed from those of an indifferent substance.

There are three formulæ which all of them give for 100 parts, numbers not widely differing from those found by experiment, viz. $C^{28} H^{17} O^{13}$, $C^{32} H^{19} O^{15}$ and $C^{44} H^{24} O^{20}$. These formulæ require for 100 parts of substance the following amount of constituents:—

	$C^{28}H^{17}O^{13}$.	$C^{32}H^{19}O^{15}$.	$C^{44}H^{24}O^{20}$.
Carbon	58·13	58·00	57·99
Hydrogen . .	5·88	5·74	5·47
Oxygen	35·99	36·26	36·54

It will be seen that the last formula is that with which the analyses agree best.

If the first formula be the true one, then the formation of this substance from rubian is easily explained. It would then differ by 5 equivs. of water from 2 equivs. of rubiretine; and 1 equiv. of rubianine, 2 equivs. of verantine and 7 equivs. of water added together would be equal to 1 equiv. of rubian, as seen by the following equation:

$$C^{28} H^{17} O^{13} + 2(C^{14} H^5 O^5) + 7HO = C^{56} H^{34} O^{30}.$$

I shall presently show, however, that there is more probability in favour of one of the two latter formulæ.

Sugar.—That the substance obtained from the acid liquid after the complete decomposition of rubian is a species of sugar, will, I think, be apparent from an enumeration of its properties. It is always obtained in the form of a transparent yellow syrup, which neither crystallizes, however long its solution may be left to stand, nor becomes dry, unless heated to 100° C. Its taste is sweetish, accompanied by a bitter after-taste, like that of burnt sugar. When heated for some time at 100° C. it loses a portion of its water, but remains soft and viscid. On allowing it however to cool, it becomes brittle and capable of pulverization. After a few moments' exposure to the air it again begins to attract moisture, which it does as rapidly as chloride of calcium, and is soon reconverted into syrup. This is a character which it has, in common with ordinary syrup, obtained by boiling a solution of cane-sugar in water. It is soluble in alcohol. It is not affected by dilute sulphuric acid, even on boiling; but on evaporating a solution to which sulphuric acid has been added, it is decomposed in proportion as the acid becomes concentrated, and is changed into a black powder like humus. Concentrated sulphuric acid destroys it immediately with disengagement of sulphurous acid. It is destroyed by nitric acid. By operating on a moderately large quantity of it, I was enabled to ascertain that the sole product of the action of nitric acid is oxalic acid. It is not precipitated from its watery solution by any metallic or other salt, not even by basic acetate of lead. On the addition of caustic potash or soda to its solution and boiling, the solution immediately becomes brown, and a brown powder falls, just as in the case of grape-sugar. It is capable of fermentation. The watery solution when mixed with yeast soon begins to ferment, though the process is not so lively as in the case of an equal quantity of common sugar; and by distilling the liquid and boiling the distillate with dry carbonate of soda, alcohol may be obtained.

The analysis was attended with some difficulty on account of the great affinity which it has for water. By heating it however for some time at 100° C., then allowing to cool and pulverizing while in its brittle state as quickly as possible, it was obtained in a condition fit for analysis. Even then however the state of hydration was not uniform, so that the analyses differed considerably from one another. The following results were obtained:—

I. 0·4765 grm., burnt with chromate of lead, gave 0·6860 carbonic acid and 0·2905 water.

II. 0·3050 grm. gave 0·4450 carbonic acid and 0·1815 water.

III. 0·3820 grm. gave 0·5650 carbonic acid and 0·2205 water.

These numbers give in 100 parts—

	I.	II.	III.
Carbon . .	39·26	39·79	40·33
Hydrogen .	6·77	6·61	6·41
Oxygen . .	53·97	53·60	53·26

As this substance does not combine with bases, its atomic weight could not be determined by direct experiment. There are, however, two formulæ, both of which agree with the analyses and explain its formation, viz. $C^{14}\ H^{14}\ O^{14}$ and $C^{12}\ H^{12}\ O^{12}$. Both of these formulæ require in 100 parts—

Carbon . . .	40·00
Hydrogen . .	6·66
Oxygen . . .	53·34

If the formula $C^{14}\ H^{14}\ O^{14}$ be the true one, then its formation from rubian admits of an easy explanation. It would then differ from verantine by 9 equivs. of water; and by adding together 2 equivs. of it and 2 equivs. of rubiretine, the sum would be equal to 1 equiv. of ruqian plus 6 equivs. of water, as the following equation shows:—

$$\left.\begin{array}{l}\text{2 eqs. Sugar . } = C^{28}H^{28}O^{28}\\ \text{2 eqs. Rubiretine} = \underline{C^{28}H^{12}O^{8}}\\ \qquad\qquad C^{56}H^{40}O^{36}\end{array}\right\} = \left\{\begin{array}{l}C^{56}H^{34}O^{30} = \text{1 eq. Rubian.}\\ \underline{\quad H^{6}O^{6}\ } = \text{6 eqs. Water.}\\ C^{56}H^{40}O^{36}\end{array}\right.$$

If the formula of rubianine be $C^{28}\ H^{17}\ O^{13}$, it may replace rubiretine in the above equation; 11 equivs. of water instead of 6 being added to the rubian, as follows,—

$$\left.\begin{array}{l}\text{2 eqs. Sugar . } = C^{28}H^{28}O^{28}\\ \text{1 eq. Rubianine} = \underline{C^{28}H^{17}O^{13}}\\ \qquad\qquad C^{56}H^{45}O^{41}\end{array}\right\} = \left\{\begin{array}{l}C^{56}H^{34}O^{30} = \text{1 eq. Rubian.}\\ \underline{\quad H^{11}O^{11}} = \text{11 eqs. Water.}\\ C^{56}H^{45}O^{41}\end{array}\right.$$

To this view however it may be objected that all the species of sugar capable of fermentation with which we are acquainted contain 12 equivs. of carbon. Indeed it is difficult to conceive how a body of the formula $C^{14}\ H^{14}\ O^{14}$ can be decomposed into alcohol and carbonic acid. It is therefore far more probable that the formula of this substance is $C^{12}\ H^{12}\ O^{12}$, which is also that of grape-sugar when dried at 100° C. In fact, were it capable of being crystallized, it would no doubt be considered as identical with grape-sugar. If this be granted, then it follows that the formula of rubianine must be either $C^{32}\ H^{19}\ O^{15}$ or $C^{44}\ H^{24}\ O^{20}$, as will be seen by the following equations:—

$$\left.\begin{array}{l}\text{2 eqs. Sugar . } = C^{24}H^{24}O^{24}\\ \text{1 eq. Rubianine} = \underline{C^{32}H^{19}O^{15}}\\ \qquad\qquad C^{56}H^{43}O^{39}\end{array}\right\} = \left\{\begin{array}{l}C^{56}H^{34}O^{30} = \text{1 eq. Rubian.}\\ \underline{\quad H^{9}\ O^{9}\ } = \text{9 eqs. Water.}\\ C^{56}H^{43}O^{39}\end{array}\right.$$

or

$$\left.\begin{array}{l}\text{1 eq. Sugar} \quad . = C^{12}H^{12}O^{12} \\ \text{1 eq. Rubianine} = C^{44}H^{24}O^{20}\end{array}\right\} = \left\{\begin{array}{l}C^{56}H^{34}O^{30} = \text{1 eq. Rubian.} \\ \quad H^{2}\ O^{2} \ = \text{2 eqs. Water.}\end{array}\right.$$

$$\overline{C^{56}H^{36}O^{32}} \qquad \overline{C^{56}H^{36}O^{32}}$$

The formula $C^{44}\ H^{24}\ O^{20}$ seems to me the more probable of the two. It agrees best with the results of analysis, and the high atomic weight of rubianine which follows from it explains the very neutral character of that substance. Hence it appears that rubianine stands in the same relation to the sugar as rubiretine does to verantine. When added together they contain the elements of rubian plus the elements of water, while rubiretine and verantine added together contain the elements of rubian minus the elements of water.

On the whole, it appears that the action of acids on rubian is not of so complicated a nature as might at first sight be supposed. The number of substances produced by this action is five. Nevertheless it does not follow that these five substances are all formed together, or in other words, that one atom of rubian by its decomposition gives rise to all five at the same time. From the composition of these substances, as compared with that of rubian, it follows that the latter by the action of acids undergoes decomposition in three different directions, or more correctly speaking, that the decomposition affects three separate atoms of rubian. One of these atoms loses 14 atoms of water, and is converted into alizarine. The second loses 12 atoms of water, and then splits up into verantine and rubiretine. The third takes up the elements of water, and then splits up into rubianine and sugar. What the circumstances are under which either one or the other of these three processes takes place I am unable to say. That the loss of a greater or smaller proportion of water or the addition, on the contrary, of the elements of water to those of rubian, are the immediate efficient causes of one or the other of the three processes taking place is very probable; but what again determines the elimination of more or less water from rubian, or, on the other hand, its combination with more water, remains uncertain. It is not unlikely however that the degree of temperature at which the decomposition is effected may have something to do with it. It is probable that the lower the temperature at which the acid acts on the rubian, the more rubianine and sugar are formed, and that at a higher temperature more alizarine, verantine and rubiretine are produced. In all experiments hitherto mentioned I have always obtained all five products of decomposition, though by no means in equal proportions, the alizarine being formed in the smallest quantity, the amount of rubiretine and verantine being somewhat greater, and the rubianine and sugar being produced in the largest quantity. In the course of this paper I shall have occasion to mention circumstances in which a still greater preponderance takes place in the amount of several of these substances formed over that of the others. Whether it would be possible to confine the decomposition of rubian entirely to one of these processes, or whether all three are essential, is a question of the highest importance, not so much in a theoretical, as a practical point of view. That beautiful substance, alizarine, is the only one of these products which is capable of yielding dyes. It is this body which in my opinion gives rise to all the beautiful colours for the production of which madder is employed. The others are not only useless, they are positively injurious, as I have shown on a former occasion. Though experimentally alizarine is formed in the smallest proportion, it is nevertheless theoretically possible to convert rubian entirely into alizarine, without the least quantity of the other substances being produced. From this point of view the other substances may be considered as formed at the expense of alizarine. In fact, by adding together 1 equiv. of verantine and 1 equiv. of rubiretine, and subtracting 1 equiv. of water, we obtain the elements of 2 equivs. of alizarine, for

$$C^{14} H^5 O^5 + C^{14} H^6 O^4 = 2(C^{14} H^5 O^4) + HO.$$

Also by adding together 1 equiv. of rubianine and 1 equiv. of sugar, and subtracting 16 equivs. of water, we obtain the elements of 4 equivs of alizarine, for

$$C^{44} H^{24} O^{20} + C^{12} H^{12} O^{12} = 4(C^{14} H^5 O^4) + 16HO.$$

If any chemist should succeed in changing rubian entirely into alizarine, an undertaking in which there is no occasion to despair of success, he would be the means of giving a great stimulus to many branches of manufacture and adding a large sum to the national wealth.

LII. *On Continued Fractions in Quaternions.* By Sir William Rowan Hamilton, *LL.D., M.R.I.A., F.R.A.S. &c., Andrews' Professor of Astronomy in the University of Dublin, and Royal Astronomer of Ireland**.

1. It is required to integrate the equation in differences,

$$u_{x+1}(u_x + a) = b,$$

where x is a variable whole number, but a, b, u are quaternions†.

* Communicated by the Author.

† The advertisement, that the present writer's Lectures on Quaternions were to be ready in January last, was inserted, contrary to his wishes, through the over-zeal of an agent: but the work in question is now nearly all in type.

Let q_1 and q_2 be any two assumed quaternions; then

$$u_{x+1}+q_1=b(a+u_x)^{-1}+q_1=(b+q_1a+q_1u_x)(a+u_x)^{-1},$$
$$u_{x+1}+q_2=b(a+u_x)^{-1}+q_2=(b+q_2a+q_2u_x)(a+u_x)^{-1},$$
$$\frac{u_{x+1}+q_2}{u_{x+1}+q_1}=\frac{b+q_2a+q_2u_x}{b+q_1a+q_1u_x}=q_2\frac{q_2^{-1}b+a+u_x}{q_1^{-1}b+a+u_x}q_1^{-1}.$$

If therefore we suppose that q_1, q_2 are roots of the quadratic equation

$$q^2=qa+b,$$

which gives

$$q^{-1}b+a=q,$$

we shall have

$$\frac{u_{x+1}+q_2}{u_{x+1}+q_1}=q_2\frac{u_x+q_2}{u_x+q_1}q_1^{-1},$$

and finally,

$$\frac{u_x+q_2}{u_x+q_1}=q^x{}_2\frac{u_0+q_2}{u_0+q_1}q_1^{-x}.$$

2. It was in a less simple way that I was led to the last written result. I assumed

$$u_x=\left(\frac{b}{a+}\right)^x c,$$

and treated this continued fraction as a particular case of the following,

$$u_x=\frac{b_1}{a_1+}\,\frac{b_2}{a_2+}\cdots\frac{b_x}{a_x+c}=\frac{\mathrm{N}_x}{\mathrm{D}_x}=\frac{\mathrm{N}'_x(a_x+c)+\mathrm{N}''_x b_x}{\mathrm{D}'_x(a_x+c)+\mathrm{D}''_x b_x}.$$

By changing c to $\dfrac{b_{x+1}}{a_{x+1}+c}$, I obtained the equations,

$$\mathrm{N}'_{x+1}=\mathrm{N}'_x a_x+\mathrm{N}''_x b_x,\quad \mathrm{N}''_{x+1}=\mathrm{N}'_x,$$
$$\mathrm{D}'_{x+1}=\mathrm{D}'_x a_x+\mathrm{D}''_x b_x,\quad \mathrm{D}''_{x+1}=\mathrm{D}'_x,$$

with the initial conditions

$$\mathrm{N}'_1=0,\quad \mathrm{N}''_1=1,\quad \mathrm{D}'_1=1,\quad \mathrm{D}''_1=0,$$

which allowed me to assume

$$\mathrm{N}'_0=1,\quad \mathrm{D}'_0=0.$$

Making next

$$a_x=a,\quad b_x=b,$$

there resulted

$$\mathrm{N}_x=\mathrm{N}'_x(a+c)+\mathrm{N}'_{x-1}b,\quad \mathrm{D}_x=\mathrm{D}'_x(a+c)+\mathrm{D}'_{x-1}b,$$
$$\mathrm{N}'_{x+1}=\mathrm{N}'_x a+\mathrm{N}'_{x-1}b,\quad \mathrm{D}'_{x+1}=\mathrm{D}'_x a+\mathrm{D}'_{x-1}b.$$

This led me to assume

$$\mathrm{N}'_x=lq^x{}_1+mq^x{}_2,\quad \mathrm{D}'_x=l'q^x{}_1+m'q^x{}_2,$$
$$q_1=a+q_1^{-1}b,\quad q_2=a+q_2^{-1}b,$$
$$l+m=1,\quad lq_1+mq_2=0,\quad l'+m'=0,\quad l'q_1+m'q_2=1;$$

whence there followed,

$$l=(q_1^{-1}-q_2^{-1})^{-1}q_1^{-1}=-q_2(q_1-q_2)^{-1},$$
$$m=-(q_1^{-1}-q_2^{-1})^{-1}q_2^{-1}=+q_1(q_1-q_2)^{-1},$$
$$l'=-m'=(q_1-q_2)^{-1}.$$

Hence

$$N_x=lq^x{}_1(q_1+c)+mq^x{}_2(q_2+c), \quad D_x=l'q^x{}_1(q_1+c)+m'q^x{}_2(q_2+c);$$

and making, for conciseness,

$$v_x=\frac{q^x{}_2(q_2+c)}{q^x{}_1(q_1+c)}=q^x{}_2\frac{q_2+c}{q_1+c}q_1^{-x},$$

it was found that

$$u_x=\left(\frac{b}{a+}\right)^x c=\frac{N_x}{D_x}=\frac{l+mv_x}{l'+m'v_x}=\frac{-q_2(q_1-q_2)^{-1}+q_1(q_1-q_2)^{-1}v_x}{(q_1-q_2)^{-1}(1-v_x)}.$$

Thus

$$u_x+q_1=\frac{1}{(q_1-q_2)^{-1}(1-v_x)}=(1-v_x)^{-1}(q_1-q_2);$$
$$u_x+q_2=v_x(1-v_x)^{-1}(q_1-q_2);$$

and finally,

$$\frac{u_x+q_2}{u_x+q_1}=v_x,$$

as before.

And because in no one stage of the foregoing process has the commutative principle of multiplication been employed, the results hold good for quaternions, and admit of interesting interpretations.

Observatory, March 20, 1852.

[To be continued.]

LIII. *On the Triple or Ammonio-magnesian Phosphates occurring in the Urine and other Animal Fluids.* By J. W. Griffith, M.D., F.L.S., *Member of the Royal College of Physicians**.

Two forms of the so-called triple or ammonio-magnesian phosphates have long been considered to occur in animal fluids. One of these is composed of microscopic stellæ with mostly six foliaceous rays, and is readily obtained by adding excess of solution of ammonia to urine; this is commonly known as the bibasic phosphate. The other exists in the form of microscopic prisms, most of which are trilateral, frequently with one of the edges truncated, the terminal facets being single and oblique; these are in reality hemihedric crystals. This salt is well known as being almost constantly found in putrid urine;

* Communicated by the Author.

and not unfrequently, in a somewhat modified form, in acid urine, and is called the neutral phosphate of ammonia and magnesia. These forms are both figured in my "Practical Manual," &c., 1843, and in most other works on animal chemistry; but their respective compositions have not, so far as I am aware, been determined, nor the connexion of the forms with the composition been ascertained. A knowledge of this deficiency induced me in 1845 to prepare artificially and analyse the bibasic or stellate form, which can be readily procured, and in a tolerable state of purity, from healthy urine. This analysis was published in the second part of my Manual, p. 58, and shows that this phosphate agrees in composition with that prepared artificially and analysed by Professor Graham*. The difficulty in determining the constitution of the neutral salt, as it is called, has depended upon the constancy with which it occurs mixed with either the basic salt, or mucus and other foreign substances. I lately, however, found a method of preparing it, in a tolerably pure state, from healthy urine, as from an artificial saline mixture. If healthy urine be diluted with water, and very dilute solution of ammonia be stirred into it in small quantities at a time, taking care that its acidity be not completely neutralized, the so-called neutral triple phosphate will be thrown down. It is also formed when dilute solution of ammonia is added to a dilute aqueous solution of phosphate of ammonia and sulphate of magnesia. The precipitates in both cases consist of prisms exhibiting all the forms of the neutral phosphate met with in animal fluids, sometimes grouped, at others isolated, and the forms are identical in both cases.

After washing the crystals with cold water, they were allowed to dry in the air, and analysed in the usual way. The composition of this "neutral" salt was then found to be identical with that of the "bibasic;" thus—

	Theory.			I.	II.	III.
NH^3 .	17	6·93		...	...	7·32
2MgO .	40	16·30	45·39	45·76	45·73	16·47
PO^5 . .	71·4	29·09				29·26
13HO .	117·0	47·68				
	245·4	100·00				

The conditions under which these two forms acquire their different crystalline figures appear to be these. The ammonio-phosphate of magnesia is less soluble in solution of ammonia than in water or urine. Hence when only just sufficient ammonia is present to form the ammonio-phosphate with the phosphate of magnesia in solution, the crystals are slowly pro-

* Trans. Royal Soc. of Lond. 1837, p. 68.

duced and assume the more perfect or prismatic forms; but when excess of ammonia is present, the formation of the crystals is hurried, and the stellate feathery crystals result. That the feathery crystals are truly mere aggregations of the prisms is shown by the fact, that where they are more slowly formed from a very dilute solution, we occasionally meet with the prisms projecting from the sides of the feathery portions. This explanation of the respective conditions under which these two forms are met with, is applicable to their occurrence naturally in animal fluids. When urine is kept, it gradually becomes ammoniacal; and as the ammonia is formed, it combines at once with the phosphate of magnesia; hence the prisms are produced; and we never find the feathery forms in either the urine or other animal fluids, unless some portion of the liquid has been prevented from admixture with the remainder by mucus, or some other such substance, until after the fluid has become ammoniacal. The "penniform" crystals of the ammonio-magnesian phosphate are merely modified forms of the stellate; they may be readily obtained by adding excess of ammonia to a very dilute solution of the phosphate of ammonia and sulphate of magnesia, and are remarkable for the curvatures of their rays.

9 St. John's Square,
March 1, 1852.

LIV. *On a remarkable Theorem in the Theory of Equal Roots and Multiple Points.* By J. J. SYLVESTER, *Barrister-at-Law**.

IN order that the theorem which I propose to state may be the more easily understood, and with the least ambiguity expressed, I shall commence with the case of a homogeneous function of two variables only, x and y.

Let

$$\phi = ax^n + nbx^{n-1}.y + n.\frac{n-1}{2}cx^{n-2}.y^2 + \&c.$$

$$+ \ldots + nb'xy^{n-1} + a'y^n,$$

and let the result of operating with the symbol

$$x^n.\frac{d}{da} + x^{n-1}.y\frac{d}{db} + \&c. \qquad + y^{n-1}.x\frac{d}{db'} + y^n\frac{d}{da'},$$

on any function of $a, b, c, \ldots b', a'$ be called the Evectant of such function, and the result of repeating this process r times the rth Evectant.

Understand by the multiplicity of the equation the number of equalities between the roots that exist; so that a pair of equal

* Communicated by the Author.

roots will signify a multiplicity 1, two pairs of equal roots, or three equal roots a multiplicity 2; a pair of equal roots and a set of three equal roots, a multiplicity $1+2$ or 3, and so on. Now suppose the total multiplicity of ϕ to be m: the first part of the proposition consists in the assertion that the 1st, 2nd, 3rd...$(m-1)$th Evectants of the discriminant of ϕ, *i. e.* of the result of eliminating x and y between $\frac{d\phi}{dx}$, $\frac{d\phi}{dy}$ (as well as the discriminant itself), will all vanish in whatever way the multiplicity is distributed; the second part of the proposition about to be stated requires that the mode should be taken into account of the manner in which the multiplicity (m) is made up. Suppose, then, that there are (r) groups of roots, for one of which the multiplicity is m_1, for the second m_2, &c., and for the rth m_2, so that $m_1+m_2+\ldots+m_2=m$. Then, I say, that the mth evectant of the determinant of ϕ is of the form

$$(a_1x+b_1y)^{m_1\cdot n}.(a_2x+b_2y)^{m_2\cdot n}\ldots(a_2x+b_2.y)^{m_r\cdot n},$$

where $a_1:b_1$ $a_2:b_2\ldots a_r:b_r$ are the ratios of $x:y$ corresponding to the several sets of equal roots.

This latter part of the theorem for the case of $m=1$ was discovered inductively by Mr. Cayley, by considering the cases when ϕ is a function and cubic, or a biquadratic function. I extended the theory to functions of any number of variables, and supplied a demonstration, *i. e.* for the case of one pair of equal roots. Mr. Salmon showed that my demonstration could be applied to the case of two pairs of equal roots, or two double points, &c., and very nearly at the same time I made the like extension to the case of three equal roots, cusps, &c., and almost immediately after I obtained a demonstration for the theorem in its most general form. This demonstration reposes upon a very refined principle, which I had previously discovered but have not yet published, in the Theory of Elimination.

I have here anticipated a little in speaking of the theorem as applicable to curves and other loci.

Suppose $\phi(x,y,z)=0$ to be the equation to a curve expressed homogeneously.

Let

$$\phi(x,y,z)=ax^n+(na'x^{n-1}.y+nb'x^{n-1}.z)$$
$$+n.\frac{n-1}{2}.a''x^{n-2}.y^2+n(n-1)b''x^{n-2}.yz+n.\frac{n-1}{2}c''x^{n-2}.y^2,$$
$$+\ \&\text{c}.\qquad \&\text{c}.,$$

and understand by the evectant of any quantity the result of operating upon it with the symbol

$$x^n.\frac{d}{da}+x^{n-1}.y\frac{d}{da'}+x^{n-1}.z.\frac{d}{db'}+x^{n-2}.y^2.\frac{d}{da'}+\&\text{c}.$$

Suppose, now, the curve to have double points, the $(r-1)$th evectant (and of course all the inferior evectants) of the discriminant of ϕ (meaning thereby the result of eliminating x, y, z between $\frac{d\phi}{dx}, \frac{d\phi}{dy}, \frac{d\phi}{dz}$) will all vanish, and the rth evectant will be of the form

$$(a_1x+b_1.y+c_1z)^n \times (a_2x+b_2.y+c_2.z)^n \ldots \times (a_rx+b_r.y+c_r.z)^n,$$

where $a_1:b_1:c_1$, $a_2:b_2:c_2, \ldots a_r:b_r:c_r$ are in the ratios of the coordinates at the respective double points. If there be cusps the multiplicity of each such will be 2; and calling the total multiplicity m, to every cusp will correspond a factor of the $2n$th power in the mth evectant; and so on in general for various degrees of multiplicity at the singular points respectively. The like theorem extends to conical and other singular points of surfaces; so that there exists a method, when a locus is given having any degree of multiplicity, of at once detecting the amount and distribution of this multiplicity, and the positions of the one or more singular points. In conclusion I may state, that precisely analogous results (*mutatis mutandis*) obtain, when, in place of a single function having multiplicity, we take the more general supposition of any number of homogeneous functions being subject to the condition of pluri-simultaneity, *i. e.* being capable of being made to vanish by each of several different systems of values for the ratios between the variables. Multiplicity in a single function is, in fact, nothing more nor less than pluri-simultaneity existing between the functions derived from it by differentiating with respect to each of the given variables successively. But as I purpose to give these theorems and their demonstration, which I have already imparted to my mathematical correspondents in a paper destined for reading before the Royal Society, I need not further enlarge upon them on the present occasion.

26 Lincoln's-Inn-Fields,
March 23, 1852.

P.S. In the above statement I have spoken only of cusps of curves which are the precise and unambiguous analogues of three coincident points in point-systems, in order to avoid the necessity of entering into any disquisition as to the species of singularity in curves or other loci corresponding to higher degrees of multiplicity in point-systems, a subject which has not hitherto been completely made out. I may here also add a remark, which gives a still higher interest to the theory, which is (to confine ourselves, for the sake of brevity, to functions of two variables), that if any root of $x:y$, say $a:b$, occur $1+\mu$ times, the total multiplicity of the equation being supposed m, and its degree n, then taking ι any integer number not exceeding μ, the $(m+\iota)$th evectant of the discriminant will contain the factor $(ax+by)^{(\mu-\iota)n}$. So that, for instance, if there be but a single group of equal roots, and they be $1+\mu$ in number, every evectant up to the $(\mu-1)$th inclusive will vanish, and from the μth to the $(2\mu-\iota)$th will contain a power of $(ax+by)^n$.

LV. *On a new Locality of Phenakite.* *By* Professor Miller *of Cambridge**.

In the Supplement to the sixth volume of the *Bibliothèque Universelle de Genève,* page 299, M. Marignac describes a supposed crystal of tourmaline exhibiting new forms in the following words:—

"Les cristaux de tourmaline dont je vais décrire les formes ont été observés sur un échantillon provenant probablement du Dauphiné, et portant des cristaux de quartz et d'anatase. Ils offrent de petits prismes à douze pans striés longitudinalement, parfaitement hyalins et incolores, rayant facilement le verre, mais non le quartz, et inaltérables au chalumeau. Un seul sommet est visible, l'autre étant engagé dans la gangue; ce sommet présente au moins trois systèmes de facettes dont aucun ne correspond aux modifications qui ont été décrites, à ma connaissance du moins, dans la tourmaline, bien qu'ils dérivent par des lois simples du rhomboèdre primitif de cette substance."

The angles of the forms described by M. Marignac approach closely to those of phenakite. For e, e', e'' being three faces of a rhombohedron of phenakite truncating the edges formed by the intersections of the faces r, r', r'' of another rhombohedron of the same mineral, the angle between normals to ee', according to Beirich's measurements of good crystals from Framont, is $35°\ 56'$, and between normals to rr', $63°\ 20'$, the corresponding angles in Marignac's crystal being $36°\ 0'$ and $63°\ 30'$. The third form described by Marignac is the regular six-sided pyramid, the faces of which truncate the edges in which the faces of the rhombohedrons e, e', e''; r, r', r'' intersect each other. The limpidity and absence of colour are favourable to the supposition that the crystal is phenakite, though in hardness it appears to be a very little inferior. Should it on further examination prove to be phenakite, it may possibly lead to the discovery of a new locality of that mineral, and will increase the probability of finding it in existing collections formed prior to the discovery of the mineral in Siberia and Alsace. It will also show the name phenakite, from its deceptive appearance, to have been very happily chosen by Von Nordenskiöld.

* Communicated by the Author.

LVI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 311.]

Jan. 15, 1852. A PAPER was read, entitled, "On the Development of the Ductless Glands of the Chick." By Henry Gray, Demonstrator of Anatomy at St. George's Hospital. Communicated by W. Bowman, Esq., F.R.S.

In this paper the author has demonstrated the evolution of the spleen, supra-renal and thyroid glands, and the tissues of which each is composed, in order to show the place that may be assigned to each in a classification of the glands.

The *spleen* is shown to arise between the 4th and 5th days, in a fold of membrane which connects the intestinal canal to the spine (the "intestinal lamina"), as a small whitish mass of blastema, perfectly distinct from both the stomach and pancreas. This fold serves to retain it and the pancreas in connection with the intestine. This separation of the spleen from the pancreas is more distinct at an early period of its evolution than later, as the increased growth of both organs causes them to approximate more closely, but not more intimately with one another; hence probably the statement of Arnold, that the spleen *arises* from the pancreas. With the increase in the growth of the organ and the surrounding parts, it gradually attains the position that it occupies in the full-grown bird, in more immediate proximity with the stomach; hence probably the statement of Bischoff, that it *arises* from the stomach. Later, when its vessels are formed, the membrane in which it was developed is almost completely absorbed.

The author then considers the development of the tissues of the spleen, which clearly establishes, not only the glandular nature of the organ itself, but the great similarity it bears with the supra-renal and thyroid glands.

The external capsule and the trabecular tissue of the spleen are both developed between the 8th and 9th days, the former in the form of a thin membrane composed of nucleated fibres, the latter consisting of similar fibres, which intersect the organ at first sparingly, and afterwards in greater quantity. The development of the blood-vessels and the blood are next examined. The former are shown to arise in the organ independent of those which are exterior to it. The development of the blood-globules is shown to arise from the blastema of the organ at the earliest period of its evolution, and continue their formation until its connection with the general vascular system is effected, at which period their development ceases. No destruction of the blood-globules could ever be observed. These observations disprove the two existing opinions of the use of the spleen, as the blood-discs are not formed there (excepting during its early development), as stated by Gerlach and Schäffner; nor are they destroyed there, as stated by Kölliker and Ecker.

The development of the pulp tissue is next examined. At an early period this closely corresponds with the structure of the supra-renal and thyroid glands at the earliest stages of their evolution, consisting of nuclei, nucleated vesicles, and a fine granular plasma, the former forming a very considerable portion of its structure. When the splenic vessels are formed, many of these nuclei are surrounded by a quantity of fine dark granules arranged in a circular form, and these increase up to the time when the splenic vein is formed, when nearly the whole mass is composed of nucleated vesicles, the nuclei of which gradually break up into a mass of granules which fill the cavities of the vesicles. The Malpighian vesicles are developed in the pulp by the aggregation of nuclei into circular masses, around which a fine membrane soon appears, in a manner precisely similar to those of the suprarenal and thyroid glands, with which they bear the closest analogy.

The author then traces out the development of the supra-renal glands, and shows the close analogy that exists between them, the spleen, and thyroid, from the similarity which their structure presents at the earliest period of their evolution with those glands, and from the development of the several tissues following the same stages in all.

They are shown to arise on the 7th day as two separate masses of blastema, situated between the upper end of the Woolfian bodies and the sides of the aorta, being totally independent (as concerns their development) of those bodies, or of each other. At this period their minute structure bears a close resemblance to that of the spleen, consisting of the same elements as that gland, excepting in the existence of more numerous dark granules, which give to the organ at a later period an opake and darkly granular texture. The gland tissue of the organ, in the form of large vesicles, makes its appearance on the 8th day, whereas in the spleen it did not exist until near to the close of incubation, an interesting fact in connection with the function of the former gland, which is mainly exercised during fœtal life, whilst the spleen exerts its function mainly in adult life; hence the difference in the development of the tissues at different periods. The manner in which this tissue is developed is similar to that by which the gland tissue of the spleen was formed, viz. by an aggregation of nuclei into circular masses, around which a limitary membrane ultimately forms: these are first grouped together in a mass, without any subdivision into cortical and medullary portions. On the 14th day the first trace of this subdivision becomes manifest, by the vesicles being aggregated into masses which radiate from the circumference towards the centre of the gland, in some cases complete tubes being formed by the junction of the vesicles, as indicated by hemispherical bulgings along their walls. At a later period the organs increase in size, they attain their usual position, and a more complete subdivision into cortical and medullary portions is now observed.

The author lastly traces out the development of the thyroid glands, and shows the great similarity that exists between them, the spleen and supra-renal glands, from the similar structure they present, and from the development of those structures occurring in a similar manner in each.

These glands are developed between the 6th and 7th days as two separate masses of blastema, one at each side of the root of the neck, close to the separation of the carotid and subclavian vessels, and between the trachea and the branchial clefts, but quite independent, as far as regards their development, of either of those parts. Their minute structure at an early period closely corresponds with that of the spleen and supra-renal glands. Later, when the gland tissue of which the thyroid gland ultimately consists is formed, it is developed in a manner precisely similar to the same tissues of the spleen and supra-renal glands, a fact which shows the analogy they bear to one another.

From these observations the author concludes that a close analogy exists between the glands already described, so that the propriety of their classification under one group, as the "Ductless Glands," may be considered clearly proved. And although the spleen by many has been excluded from them, the author considers that its classification with them is correct, for the following reasons:—1st. From its evolution being similar with that of the supra-renal and thyroid gland; 2nd. from its structure, which at an early period closely corresponds with them; and 3rdly, from the development of its tissues following the same law as that upon which the tissues of the allied glands are formed.

Jan. 29.—The reading of Dr. Handfield Jones' paper, "On the Structure of the Liver," was resumed and concluded.

Dr. Leidy and Professor Retzius, with Muller, Weber and Khronenberg, maintain the existence of plexuses of ducts in the parenchyma of the liver containing the cells in their tubes. Some other anatomists, especially Gerlach, believe the ducts to be prolonged into the lobules of the parenchyma, under the form of mere intercellular passages without walls.

Injections of acetate of lead in saturated solution, thrown into the ductus communis choledochus, produce appearances which seem to confirm the latter view. The author, however, believes them to be fallacious, and that the ducts really terminate, as he has described them in his former paper, by closed extremities, either rounded and even, or somewhat irregular. Further details are given of the condition of the ultimate and penultimate ducts in the several vertebrate classes.

In the class of Fishes, the minute ducts most commonly appear as solid cylinders of soft granulous substance, in which scarce anything but some oily molecules are to be discerned; but not very unfrequently two other conditions are observed, which seem to illustrate very well the active character of the function of the duct. In the first the granulous matter exists in much smaller quantity, and the nuclei imbedded in it are consequently seen much more distinctly; their presence is thus unequivocally determined; it is shown that there is no real difference between the ducts of the fish's and those of the mammalian liver, only that the granulous matter is usually accumulated in the former more abundantly than in the latter. The presence of free nuclei in granulous matter indicates an active change to be proceeding in the part. In the second condition sometimes observed the granulous matter lies imbedded in it, a varying number of pellucid vesicles of great delicacy, but quite distinct; these testify that a process of active growth takes place in the minute ducts, and show, the author thinks conclusively, that these minute ducts are not mere efferent canals.

Sugar was detected on two or three occasions in the livers of fishes; it seems to be absent when the organ is extremely fatty.

In the minute hepatic ducts of reptiles, the condition of the epithelium is very similar to that in fishes; the nuclei sometimes appearing with great distinctness, sometimes being obscured by much granulous matter, sometimes developing themselves into pellucid vesicles. The livers of frogs and toads almost constantly contain dark yellow masses which were formerly regarded by the author as biliary concretions, but are now considered to be only pigmentary deposits; they coexist sometimes with much diffused black matter.

The ultimate ducts have been traced recently very satisfactorily in Birds, Mammalia and Man, and the description given of them in the paper accords with the author's former account.

The development of the liver and its apparatus of ducts has been traced out in fishes and reptiles, and the following results obtained in both classes.

(1.) The liver (*i. e.* the parenchyma of the organ) is formed as an independent mass, and does not proceed as an effect from the intestine.

(2.) The gall-bladder is developed separately as a transparent vesicle, containing a clear fluid.

(3.) The gall-bladder elongates itself at one end, tends towards the intestine, and at last opens into it, while from one part of its extent hepatic ducts are developed; in the Frog the hepatic ducts seem, however, to be formed at the same time as the gall-bladder, and to be developed *pari passu* along with it. The cystic duct is lined by ciliary epithelium which plays very actively.

The examination of the process of development in the chick has confirmed, so far as it was carried, the account given in the former paper.

In Mammalia the subject of inquiry has been chiefly the following, viz. to ascertain how far there was evidence that the secretion of bile actually is effected in and by the hepatic cell, or whether its presence in them is accidental, and the bile is really and necessarily secreted by the ultimate ducts.

It is remarked that the existence of a portal vein conveying blood from the intestinal surface is coeval, not with the formation of a bile-secreting structure (for many animals have organs which secrete abundance of biliary matter without any portal vein), but with the addition of a parenchymatous mass to the biliary organ, to which mass exclusively the portal vein is distributed. It is known that the parenchyma of the liver during, and for many hours after, digestion of food, forms, from the blood supplied to it, abundance of sugar, which thus appears to be its proper secretion; and it is not proved that the hepatic cells in a healthy state contain biliary matter, though they often do in various morbid conditions. Extracts of the hepatic parenchyma tested for bile by Pettenkoffer's method, give only very imperfect and doubtful traces of the presence of biliary matter, and on the other hand the sugar formed by the parenchyma, which is found so abundantly in the blood of the hepatic vein, is absent from the bile. The case of fatty liver, as occurring either pathologically or normally, seems also to require an explanation consonant with the view to which the above facts point, for otherwise it seems impossible to understand how perfectly formed dark-green bile could be contained in the efferent channels of a gland whose tissue is a mass of oil.

The structural condition of the ultimate biliary ducts is compared to that of the epithelium of the thyroidal cavities, and the nucleated granular tissue surrounding the lacteal in a villus; and it is shown to be probable that the terminal portions of the ducts,—so far as they possess the peculiar characteristic structure, exert an active elaborating energy, by means of which bile is formed or generated out of oily, albuminous or saccharine material which surrounds,—may be said to bathe them.

Feb. 5.—The following papers were read:—

1. "Discovery that the veins of the Bat's wing, which are furnished with valves, are endowed with rythmical contractility, and that the onward flow of blood is accelerated at each contraction." By T. Wharton Jones, F.R.S., Fullerian Professor of Physiology in the Royal Institution of Great Britain, &c.

The author finds that the veins of the bat's wing contract and dilate rythmically, and that they are provided with valves; some of which completely oppose regurgitation of blood, others only partially. The act of contraction of the vein is manifested by progressive constriction of its calibre and increasing thickness of its wall; the relaxation of the vessel, by a return to the former width of calibre and thickness of wall. The rythmical contractions and dilatations of the veins are continually going on, and that, on an average, at the rate of ten contractions in the minute. The contractions *centrad* and *distad* of a valve appear to be simultaneous, as also the dilatations.

During contraction, the flow of blood in the vein is accelerated, and on the cessation of the contraction, the flow is checked, with a tendency to regurgitation, which brings the valves into play. But this check to the onward flow of the blood is usually only momentary; already, even while the vein is in the act of again becoming dilated, the onward flow recommences and goes on, though with comparative slowness, until the vein contracts again. It is the heart's action which maintains the onward flow of blood during the dilatation of the vein, whilst it is the contraction of the vein, coming in aid of the heart's action, which causes the acceleration.

The valves are composed sometimes of but a single flap, sometimes of two. In the situation of a valve and *centrad* of the insertion of its flaps, the veins present the usual dilatations or sinuses. The valves are a reduplication of the clear innermost coat of the vein, with sometimes an intervening layer of cellular tissue.

The veins closely accompany the arteries, the nerve only intervening.

The contractility of the arteries the author finds altogether different from that of the veins, being *tonic*, not *rythmical*. He has not been able to observe unequivocal evidences of *tonic* contractility of the veins, which they have been alleged to possess.

In figure 3, of drawing No. 1, illustrating his paper, the author represents, in reference to this point, an artery and a vein, as observed immediately after pressure had been applied over them. The artery is seen constricted at intervals both above and below the place of pressure. The vein is not so constricted, but at the place where the pressure was applied there is seen a greyish granular deposit of lymph within the vessel, giving rise to an appearance of constriction by narrowing the stream of blood. On watching a vein in this state, the author has observed portions of the lymphy deposit carried away by the stream of blood, with corresponding enlargement of the channel.

The author further finds that nowhere do the arteries and veins of the web of the bat's wing directly communicate, as has also been alleged; the only communication being the usual one through the medium of capillaries.

In an appendix to this paper, the author describes the result of his microscopical examination of the structure of the veins and arteries. Both artery and vein have a middle coat of circularly disposed muscular fibres; but the appearance of the fibres is different in the two vessels. The fibres of the vein are $\frac{1}{3600}$dth in. broad, pale, greyish, semitransparent and granular looking. In general aspect, they very much resemble the muscular fibres of the lymphatic hearts of the frog; but in none did the author detect an unequivocal appearance of transverse marking. The fibres of the middle coat of the artery are not so pale looking, are clearer, and exhibit a more strongly marked contour.

2. "Some Observations on the Ova of the Salmonidæ." By John Davy, M.D., F.R.S. Lond. and Ed., Inspector-General of Army Hospitals, &c.

The author prefaces his observations by a quotation from the work of M. Vogt on the Embryology of the Salmonidæ, in which a remarkable property of the vitellus is described, viz. its coagulation by admixture with water.

This inquirer's experiments were made chiefly on the ova of the *Palée* (*Coregonus Palæa*, Cuv.); the author's mostly on the ova of the Charr (*Salmo Umbla*). After giving a description of the mature eggs of this fish, he details the trials instituted by him:—1st, on the action of water, showing its coagulating effect, except when added in very minute quantity. 2ndly, on the action of heat; how that a dry heat, even so high as that of 212° Fahr., occasions the contraction of the vitellus from evaporation, but not its coagulation, an effect even not produced by steam of the same temperature, but which is occasioned by boiling in water, owing, it is inferred, to an admixture of water. 3rdly, on the action of alkalies and salts; how these, such as potassa, ammonia and their sesquicarbonates in solution, nitre, acetate of lead, common salt and others, when of moderate strength, not only do not coagulate the vitellus, but have the property of dissolving a certain portion of coagulum, and coagulate it only when very much diluted. 4thly, on the action of acids and some other agents; how the vegetable acids tried, as the tartaric, oxalic, acetic, whether strong or dilute, do not coagulate the vitelline fluid, but dissolve its coagulum; how the strong sulphuric and muriatic acids inspissate it, the weak coagulating it; and further, how it is coagulated by the nitric acid, by corrosive sublimate and by alcohol, but not by iodine.

The inference from the experiments drawn by the author is, that the vitellus of the Charr and of the eggs of the other Salmonidæ is distinct in its properties, both from the albumen and yolk of the eggs of birds. He conjectures from analogy that the ova of other species of osseous fishes will be found to be similar; but not so those of the cartilaginous fishes. According to the observations he has made, the yolk of the eggs of fishes of this order, whether they possess a white, as in the instance of the oviparous; or are destitute of a white, as in that of the viviparous, resembles in its general character that of the egg of birds: but he doubts that the white of the former will be found analogous to that of the albumen ovi of birds, at least in its chemical qualities; having in one instance, that of the egg of the *Squalus Catulus*, found it to be, whilst transparent and viscid, neither coagulated by heat nor by nitric acid.

In conclusion, he suggests that the coagulation of the ova of the Salmonidæ may have its use, inasmuch as the opaque white ova are more conspicuous than the transparent,—the dead than the living,—and in consequence, the one may serve as lures and divert from the others the many enemies to whom they are attractive food.

Feb. 12.—The following communications were read:—

1. The subjoined Letter from Professor Haidinger to Captain Smyth, R.N., For. Sec. R.S., dated Vienna, January 15, 1852.

Sir,—The great success with which optical researches are treated of in the publications of the Royal Society, must make me anxious to lay before the Society, in a few words, a concise and convincing demonstration of the theorem that in a ray of polarized light the vibrations are perpendicular to the plane of polarization, conformably to the views of MM. Fresnel and Cauchy, and not in the plane of polarization, as some other mathematicians have maintained.

My demonstration is founded on the nature of dichroitic crystals, as tourmaline, sapphire, idocrase, &c. Any perfectly homogeneous crystal of this description presents two different tints of colours. One of them appears in the direction of the axis, as well as in all directions perpendicular to it, and it is always polarized in a plane passing through the axis; the other tint appears in every azimuth in the directions perpendicular to the axis, and it is polarized in a plane perpendicular to the axis. The latter of these colours does not appear at all, if the crystal is examined in the direction of the axis; if it depend at all on transverse vibrations, all vibrations of this kind, transverse or perpendicular to the axis, are at once excluded, and the only vibrations that can possibly belong to the colour of the extraordinary ray produced in the crystal, are those parallel to the direction of the axis. But agreeably to observation the plane of polarization is itself perpendicular to the axis, the *vibrations* therefore *take place* in directions *perpendicular to the plane of polarization.*

Trichroitic crystals of course will yield a similar demonstration, as cordierite, andalusite, diaspore, axinite, and others.

I shall not fail to send a copy of the communication I am to present today to the Vienna Academy, as soon as it shall have been printed.

The importance of the subject will, I am confident, plead as an apology for my trespassing on your kindness in thus making the request, that you will lay the present communication before the Royal Society.

I have the honour to be,
My dear Sir,
Your obedient Servant,
W. HAIDINGER.

2. A Letter to Sir John W. Lubbock, Bart., F.R.S. &c., "On the Stability of the Earth's Axis of Rotation." By Henry Hennessy, Esq., M.R.I.A. &c. Communicated by Sir John Lubbock.

The author refers to a communication to the Geological Society by Sir John Lubbock, in which he appeals, in support of the possibility of a change in the earth's axis, to the influence of two disturbing causes, which appear to have almost entirely escaped the notice of Laplace and Poisson in their investigations on the stability of the earth's axis of rotation:—1. The necessary displacement of the earth's interior strata arising from chemical and physical actions during the process of solidification. 2. The friction of the resisting medium in which the earth is supposed to move.

With reference to the first of these disturbing causes, the author states, that in his Researches in Terrestrial Physics (Philosophical Transactions, 1851, Part 2.), he has been led to conclusions which may assist in clearing up the question. From an inquiry into the process of the earth's solidification which appears to him most in accordance with mechanical and physical laws, he has deduced results respecting the earth's structure which throw some light on the changes which may take place in the relation between its principal moments of inertia, which relation is capable of being expressed by means of a function which depends on the arrangement of the earth's interior strata.

He then states that he has found strong confirmation of his peculiar views respecting the theory of the earth's figure, in the experiments of Professor Bischof of Bonn, on the contraction of granite and other rocks in passing from the fluid to the solid crystalline state. From the results of these experiments, he has been led to assign a new form to the function expressing the relation of the earth's principal moments of inertia. Referring to his paper for the mathematical processes by which he arrived at this result, he states that, from the theory he has ventured to adopt, it follows that, as solidification advances, the strata of equal pressure in the fluid spheroidal nucleus of the earth acquire increased ellipticity, and each stratum of equal density successively added to the inner surface of the solid crust is more oblate than the solid strata previously formed.

From these considerations alone, he remarks, it is evident that the difference between the greatest and least moment of inertia of the earth would progressively increase during the process of solidification. It follows, therefore, that if the earth's axis of rotation were at any time stable, it would continue so for ever. But from the laws of fluid equilibrium the axis must have been stable at the epoch of the first formation of the earth's crust; consequently it continued undisturbed as the thickness of the crust increased during the several geological formations. Thus it appears that the displacement of the earth's interior strata, instead of having a tendency to change its axis of rotation, tends to increase the stability of that axis.

With reference to inequalities arising from the friction of a resisting medium at the earth's surface, the author observes that they could not exist, if, as in the manner here shown, the axis of rotation coincided from the origin with the axis of figure.

In conclusion, he remarks, that if we could assume for the planets a similarity of physical constitution to that of the earth, the theorem as to the difference of the greatest and least moments of inertia of the earth would be applicable to all the planets; and thus we should be as well assured of the stability of our system, with respect to the motion of rotation of its several members, as we are already respecting their motion of translation.

In a postscript, referring to a third cause of disturbance in the place of the earth's axis of rotation, suggested in a letter from Sir John Lubbock, namely, the effects of local elevation and depressions at the earth's surface, the author states; if, with Humboldt, we regard the numbers expressing the mean heights of the several continents as indicators of the plutonic forces by which they have been upheaved, we shall readily see that these forces are of an inferior order to those affecting the general forms and structure of the earth. If the second class of forces acted so as not to influence in any way the stability of the earth's axis of rotation, the former class might, under certain conditions, produce a sensible change in the position of the axis. But when the tendency of the second class of forces is to increase the stability of the earth's axis, it would not be easy to show the possibility of such conditions as to render the operation of the other forces, not only effective in counteracting that tendency, but also capable of producing a sensible change in the place of the axis of rotation.

Feb. 19.—The reading of Mr. Sharpe's paper, "On the Arrangement of the Foliation and Cleavage of the Rocks of the North of Scotland," commenced at the last meeting, was resumed and concluded.

The author applies the term, *cleavage* or *lamination*, to the divisional planes by which *stratified* rocks are split into parallel sheets, independently of the stratification; *foliation*, to the division of *crystalline* rocks into layers of different mineral substances; *slate*, to stratified rocks intersected by cleavage; and *schist*, to foliated rocks only which exhibit no bedding independent of the foliation.

He considers that no distinct line can be drawn between gneiss and mica schist, chlorite schist, &c., which pass from one into the other by insensible gradations; have the same geological relations, and foliation subject to the same laws. He states that their boundaries have been laid down arbitrarily on the published maps of Scotland. The quartz rock of Macculloch includes two formations; the one, a quartzose variety of gneiss, included in this paper under that head; the other, a stratified sandstone altered by plutonic action.

The author treats the foliation of gneiss and schist as a series of simple curves, obtained by observing the general direction, and disregarding the minor and more complicated folds. The convolutions are usually greatest where the dip is slightest, but where the foliation is vertical or nearly so, it usually follows true planes without contortion; thus the most correct observations are those taken where the foliation is vertical.

When the foliation of gneiss and schist is traced over extensive areas, and the minor convolutions disregarded, it is usually found to form arches of great length and many miles in diameter, bounded by vertical planes, between which the inclination increases with the distance from the axis. Each arch is succeeded by a narrow space in which the dip is irregular, and beyond which another arch commences of a form similar to the first. Portions of two adjoining arches seen without the rest form the fan-like structure observed by several geologists. The arrangement of the foliation in arches corresponds with that of the cleavage of the true slates previously described by the author, except in the greater convolution of the gneiss and schist.

Along the southern border of the Highlands a band of stratified clay slate rests on mica schist: at the junction, the foliation of the schist conforms to the cleavage of the slate, and the two together form an arch, but there is no connection between the stratification of the slate and the foliation; moreover, the divisional planes cross from one rock to the other, without change of direction, being planes of foliation in the mica schist, and of cleavage in the slate: these facts confirm Mr. Darwin's opinion, that cleavage and foliation are due to the same cause.

The author describes the parallel arches of foliation which cross the Highlands, illustrating his description by sections and a map on which they are laid down, and tracing in detail the vertical planes which bound the arches. Commencing on the south, the first vertical plane runs about four miles within the Highland border, with a mean direction of about N. 55° E.: it crosses more than once the junction of the clay slate and mica schist. South of this plane the cleavage of the slate forms the beginning of an arch, which ends abruptly at the junction of the slate with the Old Red Sandstone.

To the north of this vertical plane four arches run across the Highlands: the most southern of these, with a diameter of ten or twelve miles, is formed partly of the cleavage of the slate, and partly of the foliation of the mica schist. The hills on the south side of Loch Tay coincide with its central axis. The vertical plane which forms its northern boundary crosses Ben Lawers, and has a mean direction of N. 50° E. The next arch northward, consisting principally of gneiss, has a diameter varying from twenty-five to thirty miles; its axis runs for some distance along the central ridge of the Grampians. The granite of Cruachan and Ben Muich Dhui interfere with the regularity of the foliation of this district, and the lines are thrown to the north by the granite of Aberdeenshire: the line which bounds this arch on the north crosses the Spey near Laggan, and runs N. 40° E. through Corbine into the Monagh Leagh mountains. To the north of that line, the foliation of the gneiss forms an arch only ten miles wide, bounded on the north by a vertical plane running N. 35° E. which crosses Coryaraick. This plane forms the southern boundary of an arch, varying from fifteen to twenty-five miles wide, entirely of gneiss, bounded on the north by a band of vertical foliation which runs about N. 30° E. from Glen Finnan through the middle of Rosshire and across Ben Nevis. To the north-west of this band there is half an arch in the foliation, varying from twenty to thirty miles wide, which ends abruptly at a line to be drawn from Loch Eribol and Loch Maree, on the west of which the gneiss is unconformable to that hitherto described, but agrees with that of the Island of the Lewis, forming a series of arches which run about N.W.

From the want of parallelism in the lines of foliation of the Highlands, they would all nearly converge between Lough Foyle and Lough Swilly among the mica schists of the North of Ireland.

The most rugged and elevated hills are usually on or near the lines of vertical foliation; the axes of the arches are generally found in high land, and the principal valleys occur between the central axes of the arches and their vertical boundaries. Thus the main physical features of the Highlands are connected with the foliation of the gneiss and schists; but the granites and porphyries which have broken through those rocks, and disturbed the regularity of the foliation, have also greatly modified the surface of the country.

The contortions of gneiss and schists being unaccompanied by fracture, must, the author considers, have been produced when the matter of those rocks was semi-fluid: in this state the mineral ingredients appear to have separated and re-arranged themselves in layers according to their affinities, while the whole was subjected to pressure acting along certain axes of elevation, which raised those layers into arches.

Feb. 26.—The following paper was read:—"On the Motions of the Iris." By B. E. Brodhurst, Esq., M.R.C.S. Communicated by Thomas Bell, Esq., Sec. R.S.

The observations made in this paper are distributed under three heads. First, the author examines the iris in conjunction with the organic system of nerves. Secondly, he exposes the relation of the several nerves of the orbit in reference to the iris. And, thirdly, by tracing the membrane through the lower orders of animals, he shows the influence of the ophthalmic ganglion upon the iris, and the necessity of its presence for the accomplishment of the motions of the membrane, *i. e.* contraction and dilatation of the pupil.

It is shown that the pupil is most contracted during healthy sleep, and especially during that of childhood; that in death it assumes a median state, neither contracted nor dilated; and that, when disease is present, the pupil is always dilated, and dilated in accordance with the effect produced upon the trisplanchnic system of nerves. Also, it is stated that the pupil is dilated, when through disease the action of the voluntary muscles is abnormally increased, but that it is contracted when the functions of nutrition are well and actively performed; and that, with concussion and compression of the brain, the pupil is usually dilated when the power of the voluntary muscles yet remains; that it is fixed and immoveable when total insensibility exists; contracted when pressure or counter pressure is made upon the corpora quadrigemina of the opposite side, and dilated when the injury is more general, but less severe.

The author refers the first class of motions, or the *primary* motions of the iris, directly to the sympathetic system of nerves; whilst the *direct* movements, or those produced by the sensation of light, are effected through the cerebral nervous arc, as shown by Flourens, Marshall Hall, and others: and he thinks that contraction of the pupil, when a near object is presented to the eye, may be explained by the greater stimulus thus afforded to the retina and the sensorium; for he finds that when a near object is presented to the eye with a faint light, but a more distant one with a strong light, the pupil is most contracted for the more distant object. That the influence of the retina and the cerebral nervous arc is secondary in producing the motion of the iris, and that this membrane is not a mere diaphragm for the admission or exclusion of light, but that it yields to mental impressions, as well as to those which operate on the vegetative system of nerves, in preference to the effect upon the retina, is shown by the result which is produced upon the iris by any sudden passion in causing dilatation of the pupil, notwithstanding that a strong light be at the same time thrown upon the retina. Hippus, and the motions of the iris which are observed, especially in amaurotic children, are alluded to as motions independent of the light, and consequently, of the retina and sensorium.

The author then proceeds to state the effect of irritation and division of the several nerves of the orbit. He finds, that, on irritating the third nerve within the cranial cavity, slight contraction of the pupil ensues, to be followed by dilatation. On dividing the third nerve, the pupil becomes dilated beyond its median extent.

Irritation of the optic nerve within the cranium produces contraction of the pupil. Section of the same nerve gives rise to an insensible retina and a dilated pupil.

Irritation of the fifth nerve excites slight motion in the iris. Division of the fifth produces temporary contraction of the pupil. In the space of half an hour this effect will have ceased, and the pupil will have resumed its former diameter.

If a slight galvanic current be passed along the sympathetic, contraction of the pupil will be produced; but let the sympathetic be divided, at the superior cervical ganglion for instance, and instantly the pupil shall forcibly contract, and again widely dilate.

If a weak galvanic current be used, and the poles brought into contact with the sclerotica at its junction with the cornea, contraction of the pupil to two-thirds of its actual diameter takes place, and this effect continues so long as the current continues to be formed; but on breaking connection, the pupil immediately resumes its former diameter. So soon as life is extinct, galvanism ceases to affect the iris, whether applied to the membrane itself, through the external coats, or though the poles be in contact with the retina; but if applied to the sympathetic, movement may be excited in the iris.

The optic nerve being divided, the pupil is dilated: irritation of the third nerve then produces merely a slight and momentary effect upon the iris; but if the sympathetic be divided, the pupil will contract violently, and again dilate beyond its previous state of dilatation.

The sympathetic being divided, irritation of the cranial nerves does not affect the iris; but though the cerebrum and corpora quadrigemina be removed, division of the sympathetic will still excite the iris to motion. And, consequently, the author infers that the basic or primary motion of the iris is derived from the *vis motoria* of the excito-motor ganglionic system: he shows also, that where the ophthalmic ganglion is wanting, as in fishes and reptiles, the iris is motionless. Allusion is, lastly, made to some medicinal agents, to show their influence upon the nervous centres, and their consequent effect upon the iris: they are classed as follows:—

I. True depressors and pupil dilators.
II. True excitants and pupil dilators.
III. Stimulants which become depressors, which dilate the pupil.
IV. Excitors of voluntary nerves and pupil dilators.
V. Sedatives which terminate as depressors, which first contract and then dilate the pupil.
VI. Excitants which become sedatives, which first dilate and then contract the pupil.

And from what has gone before, it is concluded, that contraction of the pupil is the active state of the iris, and that dilatation is its enervated condition; that a healthy retina and cerebral nervous arc are necessary to the motions of the iris, and the ophthalmic ganglion to motion; and that the primary motion of the iris is due to organic nervous influence, but its forced or animal motion to the reflected stimulus of light upon the retina.

LVII. *Intelligence and Miscellaneous Articles.*

ON THE COMPOUND AMMONIAS, AND THE BODIES OF THE CACODYLE SERIES. BY T. S. HUNT.

THE beautiful researches of Hofmann and Wurtz have shown the existence of a large class of organic alkaloids closely related to ammonia. As regards their composition, we will only recall that in the alkaloids of Wurtz, the elements of an equivalent of ammonia are united with those of a carbo-hydrogen, CH^2, C^2H^4, C^5H^{10}, or what is the same thing, that CH^3, C^2H^5 and C^5H^{12}, the so-called radicals methyle, æthyle and amyle, may be regarded as replacing an atom of hydrogen in ammonia. Hence, as we have before remarked in speaking of them, they sustain to their corresponding alcohols the same relation that ammonia does to water. Water, as we have on more than one occasion shown, is not only the analogue, but the strict homologue of the alcohols, so that the molecule H^2 is the equivalent (homologue) of C^2H^6 and its homologues, and H of æthyle, methyle and amyle. The class of bodies under consideration presents some interesting illustrations of this relationship.

Dr. Hofmann has been able by the action of ammonia upon hydrobromic and hydriodic æthers, to form directly the corresponding salts of the new alkaloids; and these alkaloids, with other equivalents of the æthers, have yielded him compounds in which two and three equivalents of hydrogen are replaced by the same or by different carbo-hydrogens; so that representing C^2H^5 by Et, the final result of the action of ammonia is $N\,Et^3$, which is still an alkaloid. Other carbo-hydrogens not homologous with æthyle may be introduced, and Hofmann has obtained alkaloids containing one and two equivalents of phenyle C^6H^5, with one or more of æthyle.

Although ammonia and its derived alkaloids form with acids salts analogous to those of the inorganic bases, they must be distinguished from oxides like Zn^2O, inasmuch as they unite directly with HCl and NHO^3, while the oxides yield salts only by the elimination of water; in chloride of ammonium it is the hypothetical NH^4, which represents Zn in the chloride of zinc. The analogy between Zn^2O and H^2O leads us to suppose the possibility of such a compound as the oxide of ammonium which would be formed by a direct union of ammonia with the elements of water. But such compounds, if they exist, are very unstable; and as the alkaloids are either readily disengaged from their aqueous solutions by heat, or else are insoluble in water, it is very difficult to prove the existence of these oxides. If, however, an alkaloid could be made to unite with a homologue of water, the elements corresponding to H^2O might form a more stable combination, and the reality of the action be established. Such a result has actually been attained by Dr. Hofmann, who has indirectly formed a combination of triæthammine with alcohol. An ammonia uniting with water which has two atoms of replaceable hydrogen, might form either $NH^5O = NH^4, HO$ or $N^2H^8O = (NH^4)^2O$. Did triæthammine unite directly with hydric æther, we might obtain the alcohol compound corresponding to the latter oxide, but alcohol is EtHO containing but one atom of C^2H^5, and consequently we have $N\,Et^4, HO$. It is obtained by the action of triæthammine upon iodide of æthyle, which is the homologue of hydriodic acid; and as the acid produces with ammonia the iodide of ammonium, the æther yields the iodide of the new quasi-metal *tetræthylammonium*, which, when decomposed by oxide of silver, yields the hydrated oxide of the new base $(N\,Et^4H)O$, corresponding to $(KH)O$, hydrate of potash, which it closely resembles in its acridness, causticity, and powerfully alkaline characters, particularly as shown in its reactions with metallic salts, and in its power of saponifying oils. Although termed an organic alkaloid, it will be seen that this and its analogous compounds cannot be assimilated to the organic bases containing oxygen like quinine, with which they have been compared, as the latter combine directly with acids and carry their oxygen into their saline combinations, while Hofmann's bases eliminate an equivalent of water which contains their atom of oxygen.

The action of an alloy of potassium and antimony upon the iodide of æthyle has furnished to MM. Löwig and Schweizer a volatile liquid, spontaneously inflammable, and having the formula $C^6H^{15}Sb$, which corresponds to triæthammine, in which N is replaced by Sb*. It does not appear whether it forms direct compounds with acids. When slowly oxidized, it takes up an equivalent of oxygen and yields a viscid liquid which combines with acids, and forms salts with the elimination of an equivalent of water. M. Gerhardt has shown that this compound, which the authors designate as *oxide of stibæthyle*, is to be regarded as the hydrate of a new base, $C^6H^{13}Sb$, for which he proposes the name *stibæthine*†; it is formed from stibæthyle by the loss of H^2, as harmine is derived from harmaline. The constitution of the hydrate is analogous to Hofmann's new ammonium bases; $Sb\,C^6H^{15}, H^2O = (Sb\,C^6H^{14}, H)O$, and $Sb\,C^6H^{14}$ is equivalent to NH^4 ammonium. Nitric acid oxidizes H^2 in stibæthyle and forms an acid nitrate of stibæthine. Sulphur, chlorine, and bromine combine directly with stibæthyle, yielding compounds which have all the characters of salts of stibæthine, and may be formed by double decomposition from the salts of the oxide. Stibæthyle even decomposes strong hydrochloric acid, evolving hydrogen to form a chloride, which is also produced by the action of a metallic chloride upon the nitrate of stibæthine; its composition is that of an acid salt, $Sb\,C^6H^{13}$,

* Chem. Gaz. vol. viii. pp. 201, 372, 395, 420.

† *Comptes Rendus des Trauvaux de Chimie,* 1850, p. 400.

$2HCl = Sb\,C^6H^{14}$, Cl, HCl. It reacts like chloride of potassium or sodium with metallic solutions, and forms with sulphuric acid a sulphate with disengagement of hydrochloric gas.

More recently, M. Landoldt has obtained the methyle compound analogous to stibæthyle by a similar process *. It corresponds to trimethammine, being $Sb\,C^3H^9 = SbMe^3$, and yields a series of compounds like those of stibæthyle. When placed in contact with iodide of methyle, an energetic combination ensues, and a crystalline product is obtained which is $SbMe^4I$, the equivalent of Hofmann's iodide above described. Decomposed by oxide of silver, the hydrated oxide of the new base, which the author calls *stibmethylium*, is obtained; it closely resembles the oxide of tetræthylammonium, and its salts are said to be isomorphous with those of potash. The author writes the formula of the iodide as above, and the oxide $SbMe^4O$; this is evidently an error; the oxide will be $(SbMe^4H)O$, like the corresponding nitrogen compound. He has observed that stibæthyle yields similar compounds with iodide of æthyle, and with iodide of methyle, the analogous body $(SbEt^3Me)I$.

With these results before us, we are ready to inquire into the constitution of the bodies of the cacodyle series. It must be observed that the elimination of H^2 is characteristic of the alcohols, as is seen in the formation of aldehydes and acids, and they seem to preserve this same character in their combinations; thus the fourth æthyle atom which is combined with triæthammine is decomposed at the temperature of boiling water into C^2H^4 and H^2O.

Let arsenic replace nitrogen in Wurtz's *æthammine* $NC^2H^7 = NC^2H^5$, HH, and we have $As\,C^2H^7$; from which, if H^2 be abstracted, there remains $As\,C^2H^5 = As\,C^2H^3$, HH, a new base corresponding to stibæthine; such a base is contained in the chloride of cacodyle, and has been recognised by M. Gerhardt under the name of *arsine*†, of which the hydrochlorate and hydrobromate are Bunsen's chloride and bromide of cacodyle. The compound analogous to the oxide of stibmethylium will be $(As\,C^2H^5)^2$, H^2O or $C^4H^{12}As^2O$ (equivalent to K^2O), which is *alcarsine*. The relation between the oxide and the chloride is evident; it is difficult to believe that the bodies described by Bunsen as oxychloride and oxybromide of cacodyle are anything else than mixtures of alcarsine with hydrochlorate and hydrobromate of arsine; and the more so, as their composition after his analyses does not seem to be well-defined. M. Bunsen did not analyse the compounds of alcarsine with oxygen acids; indeed only the sulphate seems stable; it is acid and deliquescent, and is probably a bisalt—the bisulphate of arsine.

The sulphuret of cacodyle is analogous to the hydro-sulphuret of ammonia, and arsine sustains to alcarsine the same relation as ammonia to oxide of ammonium. It is worthy of notice, that there are two conditions of alcarsine; the one a fuming and spontaneously inflammable liquid, and the other, formed during the slow oxidation of the first, a viscid, syrupy substance, comparatively indifferent to

* Chem. Gaz. vol. ix. p. 181.

† *Précis de Chimie Organique,* vol. i. p. 389.

chemical agents, and but difficultly oxidized; the viscid, inactive form corresponds to stibæthine. Researches upon this variety of alcarsine and its salts would be very desirable. The compounds resulting from the action of chloride of mercury and nitrate of silver with alcarsine are probably compounds of arsine, analogous to the ammoniacal combinations of these salts. It is to be remarked, that while the salts of stibæthine are, from the very mode of their formation, acid, those of arsine, if we except perhaps the sulphate, are neutral.

Cacodyle is formed by the reduction of the hydrochlorate of arsine, *chloride of arsenium* by zinc; precisely as $2ZnCl + K^2$ give $2KCl + Zn^2$, we obtain chloride of zinc with the elimination of arsenium, that is, of $As\,C^2H^6 + As\,C^2H^6 = C^4H^{12}As^2$. Cacodyle is thus precisely analogous to a metal, and with chlorine or sulphur yields compounds of the arsine series; the above formula, however, represents two volumes of vapour, while the equivalent of the chloride is represented by $As\,C^2H^6$, Cl. I have, however, endeavoured on a previous occasion to show that the atom of the metals in their free state is represented by M^2, and hence cacodyle corresponds perfectly to Zn^2, which, in combining with chlorine, breaks up to form two equivalents of ZnCl; *alcargen*, cacodylic acid, is not an oxide of cacodyle, for its formula is $C^2H^5AsO^2$; and being anhydrous, it is equivalent to a compound of ammonia with oxygen, and not of ammonium, as M. Bunsen's theory demands.

MM. Löwig and Schweizer assert, that by oxidation, stibæthyle yields $C^4H^5Sb = SbEt$, which combines with O^5, S^5. As we have no other evidence that the type of the ammonia is ever thus destroyed, it is more probable that the action removes H^2 from one atom of Et, as in the formation of stibæthine, and oxidizes the two remaining atoms of æthyle, leaving H in their place; Sb, $C^2H^5 =$ Sb, C^2H^3, HH, corresponding to arsine, and like it combining with O^2, S^2 (O^5 in their notation not being divisible by two, is inadmissible, unless the formula is to be doubled). The properties of the new compound, *stibæthylic acid* of the author, and $C^4H^5SbO^5$ in his notation, but more probably $C^2H^5SbO^2$, lead us to conclude that it is the antimonial species corresponding to alcargen, $C^2H^5AsO^2$. It is a white solid, soluble in water and alcohol, but insoluble in æther, and is converted by H^2S into an odorous compound, in which its oxygen is replaced by sulphur: the history of the body is not, however, complete.

I remarked four years since, that glycocoll is the nitrogen species corresponding to alcargen, and published some experiments upon the action of sulphuretted hydrogen upon nitrous æther, undertaken with the hope of obtaining the nitrogen compound corresponding to alcarsine*. M. Laurent was, however, disposed to regard glycocoll as the amide of a bibasic acid $C^2H^4O^3$, the homologue of carbonic acid, and hence explained its monobasic acid character; but to this view it is to be objected, that the ordinary amides of bibasic acids are either neutral, like oxamide, or acid with-

* Chem. Gaz. vol. v. p. 386.

out any basic characters, like oxamic acid. Glycocoll is to be regarded as the isomer of glycollamic acid, precisely as the alkaloids, furfurine and benzoline, are known to be isomers—allotropic forms of the normal acids; and corresponds to æthammine less H^2+O^2, or to the product which should be obtained by the oxidation of SbEt. Its capacity to exchange H for K is unlike that of acetic acid or alcohol, for the saline power of these belongs not to the carbo-hydrogen elements, but to the unreplaced H of the H^2O. Nor in this view is the saline hydrogen of glycocoll similar to that of oxamic acid; it is an atom of hydrogen in the ammonia itself, which is replaceable, as in asparagine, itself the binamide of a bibasic acid, and in paramide.

I conclude these observations by calling attention to the results obtained by Dr. Hofmann in decomposing the compound ammonias by a nitrite *. I was the first to show that the elegant process by which Piria had succeeded in decomposing asparagine and some other amides, was applicable to the organic alkaloids, and that the action of nitric oxide upon a dilute acid solution of nitrate of aniline yields nitrogen gas and phenole †. Dr. Hofmann refers to my statement, but adds, that in repeating my experiment the aniline was transformed into a brown mass containing a crystalline matter which was nitric phenole. He probably obtained the binitric species which is the first product of the action of nitric acid upon phenole, and which, as described in my paper, I actually prepared from the phenole thus obtained, by treating it with strong nitric acid, in the process for preparing the nitropicric acid. If, keeping in mind the great readiness with which phenole is attacked by nitric acid, he will take the trouble to repeat the experiment with a dilute solution of the salt, avoiding a large excess of nitric acid, he will not find it difficult to obtain the characteristic oily product which I have described, and which is not easily confounded with nitrophenesic acid.

By the use of nitrite of silver, in accordance with my suggestion, for which he has found even nitrite of potash may be substituted, Dr. Hofmann was more successful. In distilling hydrochlorate of æthammine with a solution of nitrite of potash, nitrogen and nitrous æther were evolved, with a liquid containing apparently traces of alcohol and some drops of an oily matter. Similar results were obtained with butylamine, propylamine and amylamine. These nitrous æthers, as shown by M. Kopp and myself, are decomposed by sulphuretted hydrogen, the alcohols being regenerated. In this way Dr. Hofmann succeeded in forming from the alkaloid, amylic alcohol. As the transformations of organic substances furnish us with the means of obtaining the corresponding bases, the problem of obtaining the alcohols of the propionic and butyric series is solved.

The reaction which gives rise to nitrous æther is not clearly explained; the nitrite obtained by double decomposition will be C^2H^7N, NHO^2, and may be resolved into $N^2+H^2O+C^2H^6O$. The simultaneous evolution of nitrogen gas and nitrous æther, or indeed the formation of the latter, can only be explained by some secondary

* *Comptes Rendus des Travaux de Chimie*, Feb. 1851, p. 42.

† Chem. Gaz. vol. viii. p. 21.

action which it is not easy to foresee. We hope for more definite information upon the subject.

A curious subject of inquiry presents itself in regard to those bases which contain two and three equivalents of the alcoholic elements. If the decomposition were to take place in accordance with the formula above given, nitrate of biæthylamine would yield $C^4H^{10}O$, which is the æther of alcohol, or its isomer butyric alcohol, and triæthylamine, $C^6H^{14}O$, which is the formula of caproic alcohol. The decomposition of all the complex alkaloids by this reaction will be of great interest.—Silliman's *American Journal*, March 1852.

THE STEREOSCOPE.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

Having this day seen, in your publication of this month, Professor Wheatstone's communication regarding the "Physiology of Vision" and his recently produced instrument, the *Stereoscope*, I may perhaps be permitted to say that neither his views, nor the practical application of them, are so new as he supposes, since I constructed a stereoscope, in everything but the name, more than thirteen years ago, which, though since neglected by me, is still in existence, and can be produced, with evidence of its date.

I do not state this with a view to detract at all from the merit or originality of Professor Wheatstone's invention, as mine was never made public. I mention the circumstance rather as a curious fact than anything else.

I am, Gentlemen, your obedient Servant,

James Elliot.

1 St. Vincent Street, Edinburgh,
April 5th, 1852.

ON THE ARTIFICIAL PRODUCTION OF CRYSTALLIZED TUNGSTATE OF LIME. BY N. S. MANROSS.

Tungstate of lime, which occurs so beautifully crystallized as a mineral, and is known in this form under the name Scheelite, when prepared artificially by the mutual decomposition of a solution of an alkaline tungstate with a salt of lime, is only obtained in the form of a white powder. I have found that it may be procured in crystals allowing of measurement, by the mutual decomposition of the salts, not in an aqueous solution, however, but in a melted state at a high temperature—a method of crystallizing compounds which I have also begun to apply with favourable results to other bodies, especially those which occur crystallized as minerals*.

To procure tungstate of lime in a crystalline state, anhydrous

* I must remark, that M. Manross had arrived at this method before he had any knowledge of the very interesting experiments of Ebelmen in the same direction.—*Wöhler.*

tungstate of soda is fused in a Hessian crucible with excess of pure chloride of calcium at a moderate red heat, and the mass when cold, washed with water. The salt is then left, forming a heavy, very crystalline, sparkling powder, in which the separate crystals are easily distinguishable by a simple lens. By using some pounds of the materials, we might certainly succeed in obtaining crystals so large that their form might be distinguished by the naked eye. Even with a magnifying power of 45 diameters, this crystalline powder may be seen to consist of clear, very shining and sharp crystals, many of which appear to be true square octohedra, but most of them are combinations with many facets. Some of the former were sufficiently large to be measured by the goniometer. I found the angle of the basal edges of the octohedra $=130° 20' 30''$, thus agreeing with the corresponding angle of the compound occurring as a mineral.

The specific gravity of this artificially prepared Scheelite I found to be $=6{\cdot}0759$. That of the natural is given as 5·9 to 6·2.

With this agreement in form and density, an analysis was hardly requisite. However, I have also made this, by heating the finely powdered salt with carbonate of soda, as it has been shown, that, although acids separate yellow tungstic acid from it, it is only imperfectly decomposed by them. The lime was 19·58 per cent. Hence the loss considered as tungstic acid amounted to 80·42 per cent. The theoretical composition is

CaO..........	19·45
WO^3	80·55

Annal. der Chem. u. Pharm. Bd. 81, Heft 2.

ON THE GREEN COLOURING MATTER OF PLANTS, AND ON THE RED MATTER OF THE BLOOD. BY F. VERDEIL.

The green matter which can be extracted from the majority of plants by means of alcohol or æther was considered as a pure homogeneous organic substance, and received the name of *chlorophylle*, or green resin of plants.

I have discovered that this green resin is a mixture of a perfectly colourless fat capable of crystallizing, and of a colouring principle presenting the greatest analogies with the red colouring principle of the blood, which however had never yet been obtained in a completely pure state.

To isolate it, I precipitate a boiling solution of chlorophylle in alcohol by a small quantity of milk of lime. The solution becomes colourless; the alcohol retains the fat, whilst the lime precipitates all the colouring matter. This is separated from the lime by hydrochloric acid and æther, which dissolves the green matter, forming a coloured stratum at the top of the liquid. By evaporating the æther, the colouring matter is obtained in a state of perfect purity.—*Comptes Rendus*, Dec. 22, 1851.

EQUIVALENT OF PHOSPHORUS.

Prof. Schrötter has determined the equivalent of phosphorus by burning amorphous phosphorus in oxygen gas. A mean of ten determinations, which scarcely differ, gave as the true equivalent 387·5 on the oxygen, or 31 upon the hydrogen scale. One gramme of phosphorus according to this yields, on burning, 2·289186 grms. of phosphoric acid. Pelouze's determination, 32, is consequently too high.—*Journal für Praktische Chemie*, vol. liii. p. 435.

PRODUCTION OF CYANIDE OF POTASSIUM.

M. Rieken has confirmed by careful experiments the results of Bunsen and Playfair, that cyanide of potassium is formed when carbonate of potash intimately mixed with carbon is heated to whiteness in a current of previously heated nitrogen gas. The temperature must be that at which potassium is formed, and the nitrogen must be strongly ignited before passing over the mixture. The necessity of fulfilling these conditions will render the process very difficult of execution upon a large scale.—*Ann. der Chemie und Pharmacie*, vol. lxxix. p. 77.

METEOROLOGICAL OBSERVATIONS FOR MARCH 1852.

Chiswick.—March 1. Fine. 2. Overcast: fine: clear, with sharp frost. 3. Clear and frosty: fine: sharp frost at night. 4. Very fine: clear: severe frost at night. 5. Frosty: bright sun: frosty. 6. Slight haze: clear. 7. Frosty, with haze: fine: slight haze. 8. Uniform haze: overcast. 9. Cold dry haze: fine: clear. 10. Hazy: foggy at night. 11. Hazy: densely overcast. 12. Cloudy: clear. 13. Flying haze: cold and dry. 14. Uniformly overcast. 15. Foggy: dusky haze. 16. Slight drizzle: cloudy. 17, 18. Cloudy and cold. 19. Cold haze: white clouds: clear and frosty. 20. Clear and fine: frosty. 21, 22. Fine. 23. Slight haze: fine: clear: frosty. 24. Overcast: densely clouded. 25. Clear: overcast. 26. Clear: cloudy: frosty. 27. Frosty: cloudy: clear. 28. Overcast. 29. Hazy: fine: rain. 30. Rain: cloudy and mild: overcast. 31. Uniform haze: overcast and cold: cloudy.

Mean temperature of the month	36°·92
Mean temperature of March 1851	41 ·72
Mean temperature of March for the last twenty-six years ...	42 ·52
Average amount of rain in March	1·40 inch.

Boston.—March 1. Fine. 2. Cloudy. 3. Fine: snow A.M. and P.M. 4—7. Fine. 8, 9. Cloudy. 10. Foggy. 11. Cloudy. 12. Fine. 13. Cloudy. 14. Cloudy: rain A.M. 15. Cloudy: rain P.M. 16, 17. Cloudy. 18. Fine. 19. Cloudy. 20—26. Fine. 27, 28. Cloudy. 29. Fine. 30. Cloudy: rain early A.M. and P.M. 31. Cloudy.

Sandwick Manse, Orkney.—March 1. Snow-showers. 2. Snow-showers: snow. 3. Snow: fine. 4. Snow: fine: light halo. 5. Thaw: clear: fine. 6. Fine: clear: fine. 7. Fine: hazy. 8. Fog: fine: fog. 9. Fog. 10—15. Hazy: fine. 16. Drops: hazy: fine. 17. Hazy: fine: cloudy: fine. 18. Bright: fine: cloudy: fine. 19—21. Bright: fine: clear: cloudy: aurora. 22. Cloudy: fine. 23. Fog: cloudy. 24. Bright: cloudy. 25. Hail-showers. 26. Hail-showers: snow-showers. 27. Snow-showers. 28. Snow: bright: snow: clear. 29. Cloudy: snow-showers. 30. Bright: clear. 31. Cloudy.—This month has been remarkably fine and dry, with a high barometer and thermometer. The average quantity of rain in March for six previous years was 2·55, and in one month only was the quantity smaller, viz. Sept. 1846, when it was only ·60. The average temperature of March for twenty-six previous years 40°·38. The average state of the barometer has not been higher since May 1844, when it was 30·213.

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

Days of Month.	Barometer.					Thermometer.						Wind.			Rain.		
	Chiswick.		Boston. 8¾ a.m.	Orkney, Sandwick.		Chiswick.		Boston. 8¾ a.m.	Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Orkney, Sandwick.	Chiswick.	Boston.	Orkney, Sandwick.
1852. March.	Max.	Min.		9¼ a.m.	8½ p.m.	Max.	Min.		9¼ a.m.	8½ p.m.						
1.	29·803	29·710	29·39	29·57	29·70	49	34	37	37	31	w.	wnw.	ne.	·05	....	·15
2.	29·850	29·749	29·47	29·87	29·98	49	19	32	35	34	ne.	n.	n.	....	....	·14
3.	30·203	29·957	29·64	30·08	30·08	45	19	29·5	32	31	ne.	ne.	s.	....	....	....
4.	30·538	30·312	30·02	30·11	30·14	46	15	28	37	37	ne.	s.	sse.	....	·02	....
○ 5.	30·739	30·648	30·30	30·15	30·52	45	19	27	45	44	se.	s.	s.	....	....	·03
6.	30·739	30·699	30·42	30·53	30·60	46	25	31	49	47	ne.	e.	w.	....	....	....
7.	30·665	30·579	30·32	30·67	30·69	50	28	30	48	47	ne.	e.	sw.	....	....	....
8.	30·543	30·459	30·25	30·64	30·55	49	31	39	45	42	ne.	ene.	se.	....	....	....
9.	30·446	30·386	30·13	30·48	30·47	52	30	37·5	42½	42	ne.	e.	e.	....	....	....
10.	30·382	30·300	30·05	30·44	30·42	46	26	34·5	44	42	e.	ne.	calm	·01	....	....
11.	30·240	30·217	29·92	30·37	30·38	45	34	40	45	42½	ne.	nw.	calm	....	....	....
☾ 12.	30·382	30·316	30·00	30·40	30·43	49	25	39	43	43	ne.	nne.	calm	....	....	....
13.	30·428	30·371	30·05	30·47	30·51	48	26	40	45	44	e.	nnw.	wsw.	....	....	....
14.	30·422	30·417	30·07	30·48	30·48	48	25	38	47½	46	ne.	ne.	w.	....	....	....
15.	30·427	30·415	30·10	30·42	30·44	46	32	41	46½	46	ne.	e.	w.	....	·03	....
16.	30·402	30·348	30·04	30·40	30·36	48	35	44	45	44	ne.	n.	w.	·01	·01	....
17.	30·333	30·299	29·98	30·28	30·26	48	31	43	45	45	ne.	n.	w.	....	....	....
18.	30·262	30·182	29·90	30·26	30·24	44	31	35	47	43	ne.	n.	calm	....	....	....
19.	30·156	30·106	29·85	30·18	30·06	47	27	43	45	41½	e.	e.	sse.	....	....	....
● 20.	30·142	30·103	29·78	29·95	29·90	56	25	38	41½	42	se.	ese.	sse.	....	....	....
21.	30·191	30·107	29·80	29·86	29·87	63	27	38	48	44	se.	se.	e.	....	....	....
22.	30·120	30·108	29·79	29·91	29·92	66	28	43	49½	46	se.	s.	calm	....	....	....
23.	30·073	29·959	29·67	29·97	30·07	64	25	46	51½	43	ne.	sw.	ne.	....	....	....
24.	29·887	29·861	29·53	30·13	30·18	66	26	46	40	37	e.	ne.	n.	....	....	....
25.	30·094	29·941	29·68	30·13	30·07	48	35	38	38	37	ne.	n.	n.	....	....	....
26.	29·867	29·766	29·56	29·90	29·78	48	21	36·5	43	34½	ne.	n.	ne.	....	....	·14
27.	29·663	29·616	29·33	29·65	29·60	46	22	37·5	34	35	w.	w.	calm	....	....	·15
☽ 28.	29·632	29·615	29·40	29·63	29·67	50	38	36	43	35	ne.	e.	calm	....	....	·11
29.	29·533	29·271	29·30	29·64	29·64	58	45	41	42	36½	e.	e.	e.	·16	....	·01
30.	29·378	29·200	28·85	29·60	29·66	56	45	51	42	38	sw.	s.	ne.	·02	·33	·05
31.	29·852	29·500	29·20	29·82	30·00	48	39	45·5	44½	41	ne.	n.	ne.	....	·11	....
	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]				0·25	0·50	0·78

THE

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JOURNAL OF SCIENCE.

[FOURTH SERIES.]

JUNE 1852.

LVIII. *On the Physical Character of the Lines of Magnetic Force.* By Michael Faraday, *Esq., D.C.L., F.R.S. &c.**

[With a Plate.]

NOTE.—The following paper contains so much of a speculative and hypothetical nature, that I have thought it more fitted for the pages of the Philosophical Magazine than those of the Philosophical Transactions. Still it is so connected with, and dependent upon former researches, that I have continued the system and series of paragraph numbers from them to it. I beg, therefore, to inform the reader, that those in the body of the text refer chiefly to papers already published, or ordered for publication, in the Philosophical Transactions; and that they are not quite essential to him in the reading of the present paper, unless he is led to a serious consideration of its contents. The paper, as is evident, follows Series xxviii. and xxix., now printing in the Philosophical Transactions, and depends much for its experimental support upon the more strict results and conclusions contained in them.

3243. I have recently been engaged in describing and defining the lines of magnetic force (3070.), *i. e.* those lines which are indicated in a general manner by the disposition of iron filings or small magnetic needles, around or between magnets; and I have shown, I hope satisfactorily, how these lines may be taken as exact representants of the magnetic power, both as to disposition and amount; also how they may be recognised by a moving wire in a manner altogether different in principle from

* Communicated by the Author.

the indications given by a magnetic needle, and in numerous cases with great and peculiar advantages. The definition then given had no reference to the physical nature of the force at the place of action, and will apply with equal accuracy whatever that may be; and this being very thoroughly understood, I am now about to leave the strict line of reasoning for a time, and enter upon a few speculations respecting the physical character of the lines of force, and the manner in which they may be supposed to be continued through space. We are obliged to enter into such speculations with regard to numerous natural powers, and, indeed, that of gravity is the only instance where they are apparently shut out.

3244. It is not to be supposed for a moment that speculations of this kind are useless, or necessarily hurtful, in natural philosophy. They should ever be held as doubtful, and liable to error and to change; but they are wonderful aids in the hands of the experimentalist and mathematician; for not only are they useful in rendering the vague idea more clear for the time, giving it something like a definite shape, that it may be submitted to experiment and calculation; but they lead on, by deduction and correction, to the discovery of new phænomena, and so cause an increase and advance of real physical truth, which, unlike the hypothesis that led to it, becomes fundamental knowledge not subject to change. Who is not aware of the remarkable progress in the development of the nature of light and radiation in modern times, and the extent to which that progress has been aided by the hypotheses both of emission and undulation? Such considerations form my excuse for entering now and then upon speculations; but though I value them highly when cautiously advanced, I consider it as an essential character of a sound mind to hold them in doubt; scarcely giving them the character of opinions, but esteeming them merely as probabilities and possibilities, and making a very broad distinction between them and the facts and laws of nature.

3245. In the numerous cases of force acting at a distance, the philosopher has gradually learned that it is by no means sufficient to rest satisfied with the mere fact, and has therefore directed his attention to the manner in which the force is transmitted across the intervening space; and even when he can learn nothing sure of the manner, he is still able to make clear distinctions in different cases, by what may be called the affections of the lines of power; and thus, by these and other means, to make distinctions in nature between the lines of force of different kinds, or exertions, of power as compared with each other, and therefore between the powers to which they belong. In the action of gravity, for instance, the line of force is a straight line as far as we can test it by the resultant phænomena. It cannot be deflected, or even affected, in its course. Neither is the action in one line at all influenced, either in direction or amount, by a like action in another line; *i. e.* one particle gravitating toward another particle has exactly the same amount of force in the same direction, whether it gravitates to that one alone or towards myriads of other like particles, exerting in the latter case upon each one of them a force equal to that which it can exert upon the single one when alone: the results of course can combine, but the direction and amount of force between any two given particles remain unchanged. So gravity presents us with the simplest case of attraction; and appearing to have no relation to any physical process by which the power of the particles is carried on between them, seems to be a pure case of attraction or action at a distance, and offers therefore the simplest type of other cases which may be like it in that respect. My object is to consider how far magnetism is such an action at a distance; or how far it may partake of the nature of other powers, the lines of which depend, for the communication of force, upon intermediate physical agencies (3075.).

3246. There is one question in relation to gravity, which, if we could ascertain or touch it, would greatly enlighten us. It is, whether gravitation requires *time*. If it did, it would show undeniably that a physical agency existed in the course of the line of force. It seems equally impossible to prove or disprove this point; since there is no capability of suspending, changing, or annihilating the power (gravity), or annihilating the matter in which the power resides.

3247. When we turn to radiation phænomena, then we obtain the highest proof, that though nothing ponderable passes, yet the lines of force have a physical existence independent, in a manner, of the body radiating, or of the body receiving the rays. They may be turned aside in their course, and then deviate from a straight into a bent or a curved line. They may be affected in their nature so as to be turned on their axis, or else to have different properties impressed on different sides. Their sum of power is limited; so that if the force, as it issues from its source, is directed on to or determined upon a given set of particles, or in a given direction, it cannot be in any degree directed upon other particles, or into another direction, without being proportionately removed from the first. The lines have no dependence upon a second or reacting body, as in gravitation; and they require time for their propagation. In all these things they are in marked contrast with the lines of gravitating force.

3248. When we turn to the electric force, we are presented with a very remarkable general condition intermediate between the conditions of the two former cases. The power (and its lines) here requires the *presence* of two or more acting particles or masses, as in the case of gravity; and cannot exist with one only, as in the case of light. But though two particles are requisite, they must be in an *antithetical* condition in respect of each other, and not, as in the case of gravity, alike in relation to the force. The power is now dual; there it was simple. Requiring two or more particles like gravity, it is unlike gravity in that the power is limited. One electro-particle cannot affect a second, third and fourth, as much as it does the first; to act upon the latter its power must be proportionately removed from the former, and this limitation appears to exist as a necessity in the dual character of the force; for the two states, or places, or direction of force must be equal to each other.

3249. With the electric force we have both the static and dynamic state. I use these words merely as names, without pretending to have a clear notion of the physical condition which they seem meaningly to imply. Whether there are two fluids or one, or any fluid of electricity, or such a thing as may be rightly called a current, I do not know; still there are well established electric conditions and effects which the words *static, dynamic,* and *current* are generally employed to express; and with this reservation they express them as well as any other. The lines of force of the *static* condition of electricity are present in all cases of induction. They terminate at the surfaces of the conductors under induction, or at the particles of non-conductors, which, being electrified, are in that condition. They are subject to inflection in their course (1215. 1230.), and may be compressed or rarefied by bodies of different inductive capacities (1252. 1277.); but they are in those cases affected by the intervening matter; and it is not certain how the line of electric force would exist in relation to a perfect vacuum, *i. e.* whether it would be a straight line, as that of gravity is assumed to be, or curved in such a manner as to show something like physical existence separate from the mere distant actions of the surfaces or particles bounding or terminating the induction. No condition of *quality* or *polarity* has as yet been discovered in the line of static electric force; nor has any relation of *time* been established in respect of it.

3250. The lines of force of dynamic electricity are either limited in their extent, as in the lowering by discharge, or otherwise of the inductive condition of static electricity, or endless and continuous, as closed curves in the case of a voltaic circuit. Being definite in their amount for a given source, they can still be expanded, contracted, and deflected almost to any extent, according to the nature and size of the media through which they pass, and to which they have a direct relation. It is probable that matter is always essentially present; but the hypothetical æther may perhaps be admitted here as well as elsewhere. No condition of quality or polarity has as yet been recognised in them. In respect of *time,* it has been found, in the case of a Leyden discharge, that it is necessary even with the best conductors; indeed there is reason to think it is as necessary there as in the cases dependent on bad conducting media, as, for instance, in the lightning flash.

3251. Three great distinctions at least may be taken among these cases of the exertion of force at a distance; that of gravitation, where propagation of the force by physical lines through the intermediate space is not supposed to exist; that of radiation, where the propagation does exist, and where the propagating line or ray, once produced, has existence independent either of its source or termination; and that of electricity, where the propagating process has intermediate existence, like a ray, but at the same time depends upon both extremities of the line of force, or upon conditions (as in the connected voltaic pile) equivalent to such extremities. Magnetic action at a distance has to be compared with these. It may be unlike any of them; for who shall say we are aware of all the physical methods or forms under which force is communicated? It has been assumed, however, by some, to be a pure case of force at a distance, and so like that of gravity; whilst others have considered it as better represented by the idea of streams of power. The question at present appears to be, whether the lines of magnetic force have or have not a physical existence; and if they have, whether such physical existence has a static or dynamic form (3075. 3156. 3172. 3173.).

3252. The lines of magnetic force have not as yet been affected in their *qualities, i. e.* nothing analogous to the polarization of a ray of light or heat has been impressed on them. A relation between them and the rays of light when polarized has been discovered (2146.)*; but it is not of such a nature as to give proof as yet, either that the lines of magnetic force have a separate existence, or that they have not; though I think the facts are in favour of the former supposition. The investigation is an open one, and very important.

3253. No relation of *time* to the lines of magnetic force has as yet been discovered. That iron requires *time* for its magnetization is well known. Plücker says the same is the case for bismuth, but I have not been able to obtain the effect showing this result. If that were the case, then mere space with its æther ought to have a similar relation, for it comes between bismuth and iron (2787.); and such a result would go far to show that the lines of magnetic force had a separate physical existence.

* Philosophical Transactions, 1846, p. 1.

At present such results as we have cannot be accepted as in any degree proving the point of *time*; though if that point were proved, they would most probably come under it. It may be as well to state, that in the case also of the moving wire or conductor (125. 3076.), time is required*. There seems no hope of touching the investigation by any method like those we are able to apply to a ray of light, or to the current of the Leyden discharge; but the mere statement of the problem may help towards its solution.

3254. If an action in *curved* lines or directions could be proved to exist in the case of the lines of magnetic force, it would also prove their physical existence external to the magnet on which they might depend; just as the same proof applies in the case of static electric induction†. But the simple disposition of the lines, as they are shown by iron particles, cannot as yet be brought in proof of such a curvature, because they may be dependent upon the presence of these particles and their mutual action on each other and the magnets; and it is possible that attractions and repulsions in right lines might produce the same arrangement. The results therefore obtained by the moving wire (3076. 3176)‡, are more likely to supply data fitted to elucidate this point, when they are extended, and the true magnetic relation of the moving wire to the space which it occupies is fully ascertained.

3255. The *amount* of the lines of magnetic force, or the force which they represent, is clearly limited, and therefore quite unlike the force of gravity in that respect (3245.); and this is true, even though the force of a magnet in free space must be conceived of as extending to incalculable distances. This limitation in amount of force appears to be intimately dependent upon the dual nature of the power, and is accompanied by a displacement or removability of it from one object to another, utterly unlike anything which occurs in gravitation. The lines of force abutting on one end or pole of a magnet may be changed in their direction almost at pleasure (3238.), though the original seats of their further parts may otherwise remain the same. For, by bringing fresh terminals of power into presence, a new disposition of the force upon them may be occasioned; but though these may be made, either in part or entirely, to receive the external power, and thus alter its direction, no change in the amount of the force is thus produced. And this is the case in strict experiments, whether the new bodies introduced are soft iron or magnets (3218. 3223.)§. In this respect, therefore, the

* Experimental Researches, 8vo edition, vol. ii. pp. 191, 195.
† Philosophical Transactions, 1838, p. 16. ‡ Ibid. 1852.
§ Ibid. 1852.

lines of magnetic force and of electric force agree. Results of this kind are well shown in some recent experiments on the effect of iron, when passing by a copper wire in the magnetic field of a horseshoe magnet (3129. 3130.), and also by the action of iron and magnets on each other (3218. 3223.).

3256. It is evident, I think, that the experimental data are as yet insufficient for a full comparison of the various lines of power. They do not enable us to conclude, with much assurance, whether the magnetic lines of force are analogous to those of gravitation, or direct actions at a distance; or whether, having a physical existence, they are more like in their nature to those of electric induction or the electric current. I incline at present to the latter view more than to the former, and will proceed to certain considerations bearing on the question, with a view to the further and future elucidation of the subject.

3257. I think I have understood that the mathematical expression of the laws of magnetic action at a distance is the same as that of the laws of static electric actions; and it has been assumed at times that the supposition of north and south magnetisms, spread over the poles or respective ends of a magnet, would account for all its external actions on other magnets or bodies. In either the static or dynamic view, or in any other like them, the exertion of the magnetic forces outwards, at the poles or ends of the magnet, must be an essential condition. Then, with a given bar-magnet, can these forces exist without a mutual relation of the two, or else a relation to contrary magnetic forces of equal amount originating in other sources? I do not believe they can; because, as I have shown in recent researches, the sum of the lines of force are equal for any section across them taken anywhere externally between the poles (3109.). Besides that, there are many other experimental facts which show the relation and connexion of the forces at one pole to those at the other*; and there is also the analogy with static electrical induction, where the one electricity cannot exist without relation to, equality with, and dependence on the other. Every dual power appears subject to this law as a law of necessity. If the opposite magnetic forces could be independent of each other, then it is evident that *a charge* with one magnetism only is possible; but such a possibility is negatived by every known experiment and fact.

* The manner in which a large powerful magnet deranges, overpowers, and even inverts the magnetism of a smaller magnet, when it is brought near it in different directions without touching it, presents a number of such cases.

3258. But supposing this necessary relation, which constitutes polarity, to exist, then how is it sustained or permitted in the case of an independent bar-magnet situated in free space? It appears to me, that the outer forces at the poles can only have relation to each other by *curved* lines of force through the surrounding space; and I cannot conceive curved lines of force without the conditions of a physical existence in that intermediate space. If they exist, it is not by a succession of particles, as in the case of static electric induction (1215. 1231.), but by the condition of space free from such material particles. A magnet placed in the middle of the best vacuum we can produce, and whether that vacuum be formed in a space previously occupied by paramagnetic or diamagnetic bodies, acts as well upon a needle as if it were surrounded by air, water or glass; and therefore these lines exist in such a vacuum as well as where there is matter.

3259. It may perhaps be said that there is no proof of any outer lines of force, in the case of a magnet, except when the objects employed experimentally to show these lines, as a magnetic needle, soft iron, a moving wire, or a crystal of bismuth, are present; that these bodies, in fact, cause and develope the lines; just as in the case of gravity no idea of a line of gravitating force, in respect of a particle of matter by itself, can be formed: the idea exists only when a second particle is concerned. We are dealing, however, with a dual power; and we know that we cannot call into action, by magnetic induction upon soft iron or by electric currents, or otherwise, one magnetism without the other. Supposing, therefore, a bar of soft iron, or another bar-magnet, when brought end on and near to the first magnet, did by that approach develope the external force, the power which then only would become external should produce a corresponding external force of the contrary kind at the opposite extremity, or should not. If the first case occurs, it should be accompanied by the development of lines of force equivalent to it *within* the magnet. But I think we know, now, that in a very hard and perfect magnet there is no change of this kind (3223.). The outer and the inner lines of force remain the same in amount, whether the secondary magnet or the soft iron is present or away. It is the *disposition* only of the outer lines that is changed; their sum, and therefore their existence, remains the same. If the second case occurs, then the magnet, if broken in half under induction, should present in its fragments cases of absolute magnetic charge, or charge with one magnetism only (3257. 3261.).

3260. Or if it be imagined for a moment, that the two polarities of the bar-magnet are in relation to each other, but that whilst there is no external object to be acted upon they are related to each other through the magnet itself (an idea very difficult to conceive after the experimental demonstration of the course of the lines as closed curves (3117.3230.)), still it would follow, that upon the forces being determined externally, a change in the sum of force both within and without the magnet should be caused. We can now, however, take cognizance of both these portions of force; and it appears that, with a good magnet, whether alone or under the influence of soft iron or other magnets of fourfold strength, the sum of forces without (3223.), and therefore also within (3117. 3121.) the magnet, remains the same.

3261. If the northness and southness be considered so far independent of each other as to be compared to two fluids diffused over the two ends of the magnet (like the two electricities over a polarized conductor), then breaking the magnet in half ought to leave the two parts, one absolutely or differentially north in character, and the other south. Each should not be both north and south in equality of proportion, considering only the external force. But this never happens. If it be said that the new fracture renders manifest, externally, two new poles, opposite in kind but equal in force (which is the fact), because of the necessity of the case, then the same necessity exists also for the dependence and relation of the original poles of the original magnet, no matter what or where the first source of the power may be. But in that case the *curved lines* of force between the poles of the original magnet follow as a consequence; and the curvature of these lines appears to me to indicate their physical existence.

3262. If the magnetic poles in a bar-magnet be supposed to exert some kind of power internally, backward, as if they were centres of force, both within and without the magnet, by which they are able, upon the breaking of the magnet, to develope the contrary poles and their force, then that power cannot be the identical portion which is at the same time exerted externally; and if not the same, then when the magnet is broken, the two halves ought to have a degree of north or south charge. They ought not to be determinate magnets having equipotential poles. But they are so; and we may *break a hard magnet in half* whilst opposed to another powerful magnet which ought most to disturb the forces, and yet the broken halves are perfect magnets, equivalent in their polarities, just as if, when they were made by breaking, the dominant magnet was away. The power at the old poles is neither increased nor diminished, but remains in amount and in polar direction unchanged.

3263. Falling back, therefore, upon the case of a hard, well-made and well-charged straight bar-magnet, subject only to its own powers, it appears to me that we must either deny the joint external relation of the poles, and consider them as having no mutual tendency towards or action upon each other, or else admit that there is such an action exerted in or transmitted on through *curved* lines. To deny such an action, would be to set up a distinction between the action of the north end of a bar upon its south end, and its action upon the south end of other magnets, which, in the face of all the old experiments, and the new ones made with the moving wire (3076.), it appears to me impossible to admit. To acknowledge the action in curved lines, seems to me to imply at once that the lines have a physical existence. It may be a vibration of the hypothetical æther, or a state or tension of that æther equivalent to either a dynamic or a static condition; or it may be some *other state*, which though difficult to conceive, may be equally distinct from the supposed non-existence of the line of gravitating force, and the independent and separate existence of the line of radiant force (3251.)*. Still the existence of the state does not appear to me to be mere assumption or hypothesis, but to follow in some degree as a consequence of the known condition of the force concerned, and the facts dependent on it.

3264. I have not referred in the foregoing considerations to the view I have recently supported by experimental evidence, that the lines of force, considered simply as representants of the magnetic power (3117.), are closed curves, passing in one part of their course through the magnet, and in the other part through the space around it. These lines are identical in their nature, qualities and amount, both within the magnet and without. If to these lines, as formerly defined (3071.), we add the idea of physical existence, and then reconsider such of the cases which have just been mentioned as come under the new idea, it will be seen at once that the probability of curved external lines of force, and therefore of the physical existence of the lines, is as great, and even far greater, than before. For now no back action in the magnet could be supposed; and the external relation and dependence of the polarities (3257. 3263.) would, if it were possible, be even more necessary than before. Such a view would tend to give, but not necessarily, a dynamic form to the idea of magnetic force; and its close relation to dynamic electricity is well known (3265.). This I will proceed to examine; but before doing so, will again look for a moment at static electric induction, as an instance of the dual powers in mutual dependence by curved lines of force, but with these lines terminated, and not existing as closed circuits. An electric conductor polarized by induction, or an insulated, unconnected, rectilineal, voltaic bat-

* See Euler's views of the disposition of the magnetic force; also of the magnetic fluid, or æther and its streams. Letters, vol. ii. letters 62, 63.

tery presents such a case, and resembles a magnet in the disposition of the external lines of force. But the sustaining action (as regards the induction) being dependent upon the necessary relation of the opposite dual conditions of the force, is external to the conductor, or the battery; and in such a case, if the conductor or battery be separated in the middle, no charge appears there, nor any origin of new lines of inductive force. This is, no doubt, a consequence of the fact, that the lines of static inductive force are not continued internally; and, at the same time, a cause why the two divided portions remain in opposite states or absolutely charged. In the magnet such a division *does* develope new external lines of force; which being equal in amount to those dependent on the original poles, shows that the lines of force are continuous through the body of the magnet, and with that continuity gives the necessary reason why no absolute charge of northness or southness is found in the two halves.

3265. The well-known relation of the electric and magnetic forces may be thus stated. Let two rings, in planes at right angles to each other, represent them, as in Plate X. fig. 1. If a current of electricity be sent round the ring E in the direction marked, then lines of magnetic force will be produced, correspondent to the polarity indicated by a supposed magnetic needle placed at NS, or in any other part of the ring M to which such a needle may be supposed to be shifted. As these rings represent the lines of electro-dynamic force and of magnetic force respectively, they will serve for a standard of comparison. I have elsewhere called the electric current, or the line of electro-dynamic force, "an axis of power having contrary forces exactly equal in amount in contrary directions" (517.). The line of magnetic force may be described in *precisely the same terms*; and these two axes of power, considered as right lines, are perpendicular to each other; with this additional condition, which determines their mutual direction, that they are separated by a right line perpendicular to both. The meaning of the words above, when applied to the electric current, is precise, and does not imply that the forces are contrary because they are in reverse directions, but are *contrary in nature*; the turning one round, end for end, would not at all make it resemble the other; a consideration which may have influence with those who admit electric fluids, and endeavour to decide whether there are one or two electricities.

3266. When these two axes of power are compared, they have some remarkable correspondences, especially in relation to their position at right angles to each other. As a physical fact, Ampère* and Davy† have shown, that an electric current tends to

* *Ann. de Chim.*, 1822, vol. xxi. p. 47. † Phil. Trans. 1823, p. 153.

elongate itself; and, so far, that may be considered as marking a character of the *electric* axis of power. When a free magnetic needle near the end of a bar-magnet first points and then tends to approach it, I see in the action a character of the contrary kind in the *magnetic* axis of power; for the lines of magnetic force, which, according to my recent researches, are common to the magnet and the needle (3230.), are shortened, first by the motion of the needle when it points, and again by the action which causes the needle to approach the magnet. I think I may say, that all the other actions of a magnet upon magnets, or soft iron, or other paramagnetic and diamagnetic bodies, are in harmony with the same effect and conclusions.

3267. Again:— like electric currents, or lines of force, or axes of power, when placed side by side, attract each other. This is well known and well seen, when wires carrying such currents are placed parallel to each other. But like magnetic axes of power or lines of force repel each other: the parallel case to that of the electric currents is given, by placing two magnetic needles side by side with like poles in the same direction; and by the use of iron filings, numerous pictorial representations (3234.) of the same general result may be obtained.

3268. Now these effects are not merely *contrasts* continued through two or more different relations, but they are contrasts which *coincide* when the position of the two axes of power at right angles to each other are considered (1659. 3265.). The tendency to *elongate* in the electric current, and the tendency to *lateral* separation of the magnetic lines of force which surround that current, are both tendencies in the same direction, though they seem like contrasts, when the two axes are considered out of their relation of mutual position; and this, with other considerations to be immediately referred to, probably points to the intimate physical relation, and it may be, to the oneness of condition of that which is apparently two powers or forms of power, electric and magnetic. In that case many other relations, of which the following are some forms, will merge in the same result. Thus, unlike magnetic lines, when end on, repel each other, as when similar poles are face to face; and unlike electric currents, if placed in the same relation, stop each other; or if raised in intensity, when thus made static, repel each other. Like electric currents or lines of force, when end on to each other, coalesce; like magnetic lines of force similarly placed do so too (3266. 3295.). Like electric currents, end to end, do not add their sums; but whilst there is no change in quantity, the intensity is increased. Like magnetic lines of force similarly placed do not increase each other, for the power then also remains the same (3218.): perhaps some effect correspondent to the gain of intensity in the former case may be produced, but there is none as yet distinctly recognised. Like electric currents, side by side, add their quantities together; a case supplied either by uniting several batteries by their like ends, or comparing a large plate battery with a small one. Like magnetic lines of force do the same (3232.).

3269. The mutual relation of the magnetic lines of force and the electric axis of power has been known ever since the time of Œrsted and Ampère. This, with such considerations as I have endeavoured to advance, enables us to form a guess or judgement, with a certain degree of probability, respecting the nature of the lines of magnetic force. I incline to the opinion that they have a physical existence correspondent to that of their analogue, the electric lines; and having that notion, am further carried on to consider whether they have a probable dynamic condition, analogous to that of the electric axis to which they are so closely and, perhaps, inevitably related, in which case the idea of magnetic currents would arise; or whether they consist in a state of tension (of the æther?) round the electric axis, and may therefore be considered as static in their nature. Again and again the idea of an *electro-tonic* state (60. 1114. 1661. 1729. 1733.) has been forced on my mind; such a state would coincide and become identified with that which would then constitute the physical lines of magnetic force. Another consideration tends in the same direction. I formerly remarked that the magnetic equivalent to *static* electricity was not known; for if the undeveloped state of electric force correspond to the like undeveloped condition of magnetic force, and if the electric current or axis of electric power correspond to the lines of magnetic force or axis of magnetic power, then there is no known magnetic condition which corresponds to the static state of the electric power (1734.). Now assuming that the physical lines of magnetic force are currents, it is very unlikely that such a link should be naturally absent; more unlikely, I think, than that the magnetic condition should depend upon a state of tension; the more especially as, under the latter supposition, the lines of magnetic power would have a physical existence as positively as in the former case, and the curved condition of the lines, which seems to me such a necessary admission, according to the natural facts, would become a possibility.

3270. The considerations which arise during the contemplation of the phænomena and laws that are made manifest in the mutual action of magnets, currents of electricity, and *moving conductors* (3084. &c.), are, I think, altogether in favour of the physical existence of the lines of magnetic force. When only a single magnet is employed in such cases, and the use of iron or paramagnetic bodies is dismissed, then there is no effect of attraction or repulsion, or any ordinary magnetic result produced. The phænomena may all very fairly be looked upon as purely electrical, for they are such in character; and if they coincide with magnetic actions (which is no doubt the case), it is probably because the two actions are one. But being considered as electrical actions, they convey a different idea of the condition of the field where they occur, to that involved in the thought of magnetic action at a distance. When a copper wire is placed in the neighbourhood of a bar-magnet, it does not, as far as we are aware (by the evidence of a magnetic needle or other means), disturb in the least degree the disposition of the magnetic forces, either in itself or in surrounding space. When it is moved across the lines of force, a current of electricity is developed in it, or tends to be developed; and there is every reason to believe, that if we could employ a perfect conductor, and obtain a perfect result, it would be the full equivalent to the force, electric or magnetic, which is exerted in the place occupied by the conductor. But, as I have elsewhere observed (3172.), this current, having its full and equivalent relation to the magnetic force, can hardly be conceived of as having its entire foundation in the mere fact of motion. The motion of an external body, otherwise physically indifferent, and having no relation to the magnet, could not beget a physical relation such as that which the moving wire presents. There must, I think, be a previous state, a state of tension or a static state, as regards the wire, which, when motion is superadded, produces the dynamic state or current of electricity. This state would constitute and give a physical existence to the lines of magnetic force, and permit the occurrence of curvature or its equivalent external relation of poles, and also the various other conditions, which I conceive are incompatible with mere action at a distance, and which yet do exist amongst magnetic phænomena.

3271. All the phænomena of the moving wire seem to me to show the physical existence of an atmosphere of power about a magnet, which, as the power is antithetical, and marked in its direction by the lines of magnetic force, may be considered as disposed in sphondyloids, determined by the lines, or rather shells of force*. As the wire intersects the lines within a given

* The lines of magnetic force have been already defined (3071.). They have also been traced, as I think, and shown to be closed curves passing in one part of their course through the magnet to which they belong, and in the other part through space (3117.). If, in the case of a straight bar-magnet, any one of these lines, E, be considered as revolving round the axis of the magnet, it will describe a surface; and as the line itself is a closed curve, the surface will form a tube round the axis and inclose a solid form. Another line of force, F, will produce a similar result. The sphondyloid

sphondyloid external to the magnet, a current of electricity is generated, and that current is definite and the same for any or every intersection of the given sphondyloid. At the same time, whether the wire be quiescent or in motion, it does not cause derangement, or expansion, or contraction of the lines of force; the state of the power in the neighbouring or other parts of the sphondyloid remaining sensibly the same (3176.).

3272. The old experiment of a wire when carrying an electric current* moving round a magnetic pole, or of a current being produced in the same wire when it is carried per force round the same pole (114.), shows the electrical dependence of the magnet and the wire, both when the current is employed from the first, and when it is generated by the motion. It coincides in principle with the results already quoted, and it includes, experimentally, all currents of electricity, whatever the medium in which they occur, even up to the discharge of the Leyden jar and that between the electrodes of the voltaic battery. I think it also indicates the state of magnetic or electric tension in the surrounding space, not only when that space is occupied by metal or a wire, but also by air and other bodies; for whatever be the state in one case, it is probably general and therefore common to all (3173.).

3273. I will now venture for a time to assume the physical existence of the external lines of magnetic force, for the purpose of considering how the idea will accord with the general phænomena of magnetism. The magnet is evidently the sustaining power, and in respect of its internal condition or that of its particles, there is no idea put forth to represent it which at all approaches in probability and beauty to that of Ampère (1659.). Its analogy with the helix is wonderful; nevertheless there is, as yet, a striking experimental distinction between them; for whereas an unchangeable magnet can never raise up a piece of soft iron to a state more than equal to its own, as measured by the moving wire (3219.), a helix carrying a current can develope in an iron core magnetic lines of force, of a hundred or more times as much power as that possessed by itself, when measured by the same means. In every point of view, therefore, the magnet deserves the utmost exertions of the philosopher for

body may be either that contained by the surface of revolution of E, or that contained between the two surfaces of E and F, and which, for the sake of brevity, I have (by the advice of a kind friend) called simply the *sphondyloid*. The parts of the solid described, which are within and without the magnet, are in power equivalent to each other. When it is needful to speak of them separately, they are easily distinguished as the inner and outer sphondyloids; the surface of the magnet being then part of the bounding surface.

* Experimental Researches, 8vo edition, vol. ii. p. 127.

the development of its nature, both as a magnet and also as a source of electricity, that we may become acquainted with the great law under which the apparent anomaly may disappear, and by which all these various phænomena presented to us shall become *one*.

3274. The physical lines of force, in passing out of the magnet into space, present a great variety of conditions as to form (3238.). At times their refraction is very sudden, leaving the magnet at right, or obtuse, or acute angles, as in the case of a hard well-charged bar-magnet, fig. 2; in other cases the change of form of the line in passing from the magnet into space is more gradual, as in the circular plate or globe-magnet, figs. 3, 4, 5. Here the form of the magnet as the source of the lines, has much to do with the result; but I think the condition and relation of the surrounding medium has an essential and evident influence, in a manner I will endeavour to point out presently. Again, this refraction of the lines is affected by the relative difference of the nature of the magnet and the medium or space around it; as the difference is greater, and therefore the transition is more sudden, so the line of force is more instantaneously bent. In the case of the earth, both the nature of its substance and also its form, tend to make the refractions of the line of force at its surface very gradual; and accordingly the line of dip does not sensibly vary under ordinary circumstances at the same place, whether it be observed upon the surface or above or below it.

3275. Though the physical lines of force of a magnet may, and must be considered as extending to infinite distance around it as long as the magnet is absolutely alone (3110.), yet they may be condensed and compressed into a very small local space, by the influence of other systems of magnetic power. This is indicated by fig. 6. I have no doubt, after the experimental results given in Series xxviii., respecting definite magnetic action (3109.), that the sphondyloid representing the total power, which in the experiment that supplied the figure had a sectional area of not two square inches in surface, would have equal power upon the moving wire, with that infinite sphondyloid which would exist if the small magnet were in free space.

3276. The magnet, with its surrounding sphondyloid of power, may be considered as analogous in its condition to a voltaic battery immersed in water or any other electrolyte; or to a gymnotus (1773, 1784.) or torpedo, at the moment when these creatures, at their own will, fill the surrounding fluid with lines of electric force. I think the analogy with the voltaic battery so placed, is closer than with any case of *static* electric induction, because in the former instance the physical lines of electric force may be traced both through the battery and surrounding medium, for they form continuous curves like those I have imagined within and without the magnet. The direction of these lines of electric force may be traced, experimentally, many ways. A magnetic needle freely suspended in the fluid will show them in and near to the battery, by standing at right angles to the course of the lines. Two wires from a galvanometer will show them; for if the line joining the two ends in the fluid be at right angles to the lines of electric force (or the currents), there will be no action at the galvanometer; but if oblique or parallel to these lines, there will be deflection. A plate, or wire, or ball of metal in the fluid will show the direction, provided any electrolytic action can go on against it, as when a little acetate of lead is present in the medium, for then the electrolysis will be a maximum in the direction of the current or line of force, and nothing at all in the direction at right angles to it. The same ball will disturb and inflect the lines of electric force in the surrounding fluid, just as I have considered the case to be with paramagnetic bodies amongst magnetic lines of force (2806. 2821. 2874.). No one I think will doubt that as long as the battery is in the fluid, and has its extremities in communication by the fluid, lines of electric force having a physical existence occur in every part of it, and the fluid surrounding it.

3277. I conceive that when a magnet is in free space, there is such a medium (magnetically speaking) around it. That a vacuum has its own magnetic relations of attraction and repulsion is manifest from former experimental results (2787.); and these place the vacuum in relation to material bodies, not at either extremity of the list, but in the *midst* of them, as, for instance, between gold and platina (2399.), having other bodies on either side of it. What that outer magnetic medium, deprived of all material substance, may be, I cannot tell, perhaps the æther. I incline to consider this surrounding or outer medium as *essential* to the magnet; that it is that which relates the external polarities to each other by curved lines of power; and that these must be so related as a matter of necessity. Just as in the case of the battery above, there is no line of force, either in or out of the battery, if this relation be cut off by removing or intercepting the conducting medium, or in that of static electric induction, which is impossible until this related state be allowed (1169.)*; so in like manner I conceive, that without this external mutually related condition of the poles, or a related condition of them to other poles sustained and rendered possible in like manner, a magnet could not exist; an absolute northness or southness, or an unrelated northness or southness, being as impossible as an absolute or an unrelated state of positive or negative electricity (1178.).

* Philosophical Magazine, March 1843; or Experimental Researches, 8vo, vol. ii. p. 279.

3278. In this view of a magnet, the medium or space around it is as essential as the magnet itself, being a part of the true and complete magnetic system. There are numerous experimental results which show us that the relation of the surrounding space can be varied by occupying it with different substances; just as the relation of a ray of light to the space through which it passes can be varied by the presence of different bodies made to occupy that space, or as the lines of electric force are affected by the media through which either induction or conduction takes place. This variation in regard to the magnetic power may be considered as depending upon the aptitude which the surrounding space has to effect the mutual relation of the two external polarities, or to carry onwards the physical line of force; and I have on a former occasion in some degree considered it and its consequences, using the phrase *magnetic conduction* to represent the physical effect (2797.) produced by the presence either of paramagnetic or diamagnetic bodies.

3279. When, for instance, a piece of cold iron (3129.) or nickel (3240.) is introduced into the magnetic field, previously occupied by air or being even mere space, there is a concentration of lines of force on to it, and more power is transmitted through the space thus occupied than if the paramagnetic body were not there. The lines of force, therefore, converge on to or diverge from it, giving what I have called conduction polarity (2818.); and this is the whole effect produced as regards the amount of the power; for not the slightest addition to, or diminution of, that external to the magnet is made (3218. 3223.). A new disposition of the force arises; for some passes now where it did not pass before, being removed from places where it was previously transmitted. Supposing that the magnet was inclosed in a surrounding solid mass of iron, then the effect of its superior conducting power would be to cause a great contraction inwards of the sphere of external action, and of the various sphondyloids, which we may suppose to be identified in different parts of it. A magnetic needle, if it could be introduced into the iron medium, would indicate extreme diminution, if not apparent annihilation, of the external power of this magnet; but the moving wire would show that it was there present to its full extent (3152. 3162.) in a very concentrated condition, just as it shows it in the very body of a magnet (3116.); and the power within the magnet, it being a hard and perfect one, would remain the same.

3280. The reason why a magnetic needle would fail as a correct indicator of the amount of power present in a given space is, that when perfect, it, because of the necessary condition of hardness, cannot carry on through its mass more lines of force than it can excite (3223.). But because of the coalescence of like lines of force end on (3226.), such a needle, when surrounded by a bad magnetic conductor, determines on to itself many of the lines which would otherwise pass elsewhere, has a high magnetic polarity, and is affected in proportion; every experiment, as far as I can perceive, tending to show that the attractions and repulsions are merely consequences of the tendency which the lines of physical magnetic force have to shorten themselves (3266.). So when the magnetic needle is surrounded by a medium gradually increasing in conducting power, it seems to show less and less force in its locality, though in reality the force is increasing there more and more. We can easily conceive a very hard and feebly charged magnetic needle surrounded by a medium, as soft iron, better than itself in conducting power, *i. e.* carrying on by conduction more lines of force than the needle could determine or carry on by its state of charge (3298.). In that case I conceive it would, if free to move, point feebly in the iron, because of the coalescence of the lines of force, but would be repelled bodily from the chief magnet, in analogy with the action on a diamagnetic body. As I have before stated, the principle of the moving wire can be applied successfully in those cases where that of the magnetic needle fails (3155.).

3281. If other paramagnetic bodies than iron be considered in their relation to the surrounding space, then their effects may be assumed as proportionate to the conducting power. If the surrounding medium were hard steel, the contraction of the sphondyloid of power would be much less than with iron; and the effects, in respect of the magnetic needle, would occur in a limited degree. If a solution of protosulphate of iron were used, the effect would occur in a very much less degree. If a solution were prepared and adjusted so as to have no paramagnetic or diamagnetic relation (2422.), it would be the same to the lines of force as free space. If a diamagnetic body were employed, as water, glass, bismuth or phosphorus, the extent of action of the sphondyloids would expand (3279.); and a magnetic needle would appear to increase in intensity of action, though placed in a region having a smaller amount of magnetic force passing across it than before (3155.). Whether in any of these cases, even in that of iron, the body acting as a conductor has a state induced upon its particles for the time like that of a magnet in the corresponding state, is a question which I put upon a former occasion (2833.); but I leave its full investigation and decision for a future time.

3282. The circumstances dependent upon the shape and size of magnets appear to accord singularly well with the view I am putting forth of the action of the surrounding medium. If there be a function in that medium equivalent to conduction, involving differences of conduction in different cases, that of necessity implies also reaction or resistance. The differences could not exist without. The analogous case is presented to us in every part by the electric force. When, therefore, a magnet, in place of being a bar, is made into a horseshoe form, we see at once that the lines of force and the sphondyloids are greatly distorted or removed from their former regularity; that a line of maximum force from pole to pole grows up as the horseshoe form is more completely given; that the power gathers in, or accumulates about this line, just because the badly conducting medium, *i. e.* the space or air between the poles, is shortened. A bent voltaic battery in its surrounding medium (3276.), or a gymnotus curved at the moment of its peculiar action (1785.), present exactly the like result.

3283. The manner in which the keeper or sub-magnet, when in place, reduces the power of the magnet in the space or air around, is evident. It is the substitution of an excellent conductor for a poor one; far more of the power of the magnet is transmitted through it than through the same space before, and less, therefore, in other places. If a horseshoe magnet be charged to saturation with its keeper on, and its power be then ascertained, removing the keeper will cause the power to fall. This will be (according to the hypothesis) because the iron keeper could, by its conduction, sustain higher external conditions of the magnetic force, and therefore the *magnet* could take up and sustain a higher condition of charge. The case passes into that of a steel ring magnet, which being magnetized, shows no external signs of power, because the lines of force of one part are continued on by every other part of the ring; and yet when broken exhibits strong polarity and external action, because then the lines, which, being determined at a given point, were before carried on through the continuous magnet, have now to be carried on and continued through the surrounding space.

3284. These results, again, pass into the fact, easily verified partially, that if soft iron surround a magnet, being in contact with its poles, that magnet may receive a much higher charge than it can take, being surrounded with a lower paramagnetic substance, as air: also another fact, that when masses of soft iron are at the ends of a magnet, the latter can receive and keep a higher charge than without them; for these masses carry on the physical lines of force, and deliver them to a body of surrounding space; which is either widened, and therefore increased in the direction across the lines of force, or shortened in that direction parallel to them, or both; and both are circumstances which facilitate the conduction from pole to pole, and the relation of the external lines to the lines of force *within* the magnet. In the same way the armature of a natural loadstone is useful. All these effects and expedients accord with the view, that the space or medium external to the magnet is as important to its existence as the body of the magnet itself.

3285. Magnets, whether large or small, may be supersaturated, and then they fall in power when left to themselves; quickly at first if strongly supersaturated, and more slowly afterwards. This, upon the hypothesis, would be accounted for by considering the surrounding medium as unable, by its feeble magneto-conducting power, to sustain the higher state of charge. If the conducting power were increased sufficiently, then the magnet would not be supersaturated, and its power would not fall. Thus, if a magnet were surrounded by iron, it might easily be made to assume and retain a state of charge, which, if the iron were suddenly replaced by air, would instantly fall. Indeed, magnets can only be supersaturated by placing them for the time under the dominion of other sources of magnetic power, or of other more favourable surrounding media than that in which they manifest themselves as supersaturated.

3286. The well-known result, that small bar-magnets are far stronger in proportion to their size than larger similar magnets, harmonizes and *sustains* that view of the action of the external medium which has now been taken. A sewing-needle can be magnetized far more strongly than a bar twelve inches long and an inch in diameter; and the reason under the view taken is, that the excited system in the magnet (correspondent to the voltaic battery in the analogy quoted (3276.)) is better sustained by the necessary conjoint action of the surrounding medium in the case of the small magnet. For as the imperfect magneto-conducting power of that medium (or the consequent state of tension into which it is thrown) acts back upon the magnet (3282.), so the smaller the sum of exciting force in the centre of the magnetic sphondyloids, the better able will the surrounding medium be to do its part in sustaining the resultant of force. It is very manifest, that if the twelve-inch bar be conceived of as subdivided into sewing-needles, and these be separated from each other, the whole amount of exciting force acts upon, and is carried onwards in closed magnetic curves, by a very much larger amount of external surrounding medium than when they are all accumulated in the single bar.

3287. The results which have been observed in the relation of *length* and *thickness* of a bar-magnet, harmonize with the view of the office of the external medium now urged. If we take a small, well-proportioned, saturated magnet, as a sewing-needle; alone, it has, as just stated, such relation to the surrounding space as to have its high condition sustained; if we place a second like magnetic needle by the side of the first, the surrounding space of the two is scarcely enlarged, it is not at all improved in conducting character, and yet it has to sustain double the internal exciting magnetic force exerted when there was one needle only (3232); this must react back upon the magnets, and cause a reduction of their power. The addition of a third needle repeats the effect; and if we conceive that successive needles are added until the bundle is an inch thick, we have a result which will illustrate the effect of a thickness too large, and disproportionate to the length.

3288. On the other hand, if we assume two such needles similarly placed in a right line at a distance from each other, each has its surrounding system of curves occupying a certain amount of space; if brought together by unlike poles, they form a magnet of double the length; the external lines of force coalesce (3226.), those at the faces of contact nearly disappear; those which proceed from the extreme poles coalesce externally, and form one large outer system of force, the lines of which have a greater length than the corresponding lines of either of the two original needles. Still, by the supposition that the magnets are perfectly hard and invariable, the exciting force within remains, or tends to remain the same (3227.) in quantity, there is nothing to increase it. The increase in length, therefore, of the external circuit, which acts as a resisting medium upon the internal action, will tend to diminish the force of the whole system. Such would be the case if a voltaic battery surrounded by distilled water, as the analogous illustration (3276.), could be elongated in the water, and so its poles be removed further apart; and though in the case of magnets previously charged, some effect equivalent to intensity of excitement may be produced by conjoining several together end on, yet the diminished sustentation of power externally appears to follow as a consequence of the increased distance of the extreme poles, or external, mutually dependent parts. Static electric induction also supplies a correspondent and illustrative case.

3289. The usual case in which the influence of length and thickness becomes evident, is not, however, always or often that of the juxtaposition of magnets already as highly charged as they can be, but rather that of a bar about to be charged. If two bars, alike in steel, hardness, &c., one an inch long and the tenth of an inch in diameter, and the other of the same length but five-tenths of an inch in diameter, be magnetized to supersaturation, the latter, though it contains twenty-five times the steel of the former, will not retain twenty-five times the power, for the reason already given (3287.); the surrounding medium not being able to sustain external lines of force to that amount. But if a third bar, two inches long and also five-tenths in diameter, be magnetized at the same time, it can receive much more power than the second one. A natural reason for this presents itself by the hypothesis; for the limitation of power in the two cases is not in the magnets themselves, but in the external medium. The shorter magnet has contact and connexion with that medium by a certain amount of surface; and just what power the medium outside that surface can support, the magnet will retain. Make the magnet as long again, and there is far more contact and relation with the surrounding medium than before; and therefore the power which the magnet can retain is greater. If there were such limited points of resulting action in the magnet as is often understood by the word *poles,* then such a result could hardly be the case, on my view of the physical actions. But such poles do not exist. Every part of the surface of the magnet, so to say, is pouring forth externally lines of magnetic force, as may be seen in figs. 2, 3, 4, 5 (3274.). The larger the magnet, to a certain extent, and the larger the amount of external conducting medium in contact with it, the more freely is this transmission made. If the second magnet, being an inch long, be conceived to be charged to its full amount, and then, whilst in free space, could have half an inch of iron added to its length at each end, we see and know that many of the lines of force originally issuing from that part of its surface still left in contact with the air at the equatorial part, would now move internally towards the ends, and issue at a part of the soft iron surface; indicating the manner in which the tension would be relieved by this better conducting medium at the ends, and by increased surface of contact with the surrounding bad conductor of air or space. The thick, short magnet could evidently excite and carry on physical lines of magnetic power far more numerous than those which the space about it can receive and convey from pole to pole; and the increase in the length of the magnet may go on advantageously, until the increasing sum of power, sustainable by the increasing medium in the circuit, is equal to that which the magnet can sustain or transmit internally; for all the lines of power, wherever they issue from the magnet, have to pass through its equator; and in this way the equator or thickness of the magnet becomes related to its length. So the advantageous increase in length of the bar is limited by the increasing resistance within, and especially at the equator of the bar; and the increase in breadth, by the increasing resistance (for increasing powers) of the external surrounding medium (3287.).

3290. It is very interesting to observe the results obtained when an attempt is made to magnetize, regularly, a thin steel wire, about 15 or 20 inches in length, and 0·05 of an inch in diameter. It can hardly be effected by bars; and when the wire is afterwards examined by filings (3234.), it is found to have irregular and consecutive poles, which vary as the magnetization is repeated with the same wire, as if they broke out suddenly by a rupture of something like unstable equilibrium; the effects apparently being chiefly referable to the cause now assigned. Again, when a magnet is made out of a thin, hard, steel plate, whose length is ten or twelve times its width, it is well known how the lines of force issue from it in greatest abundance at the extreme angles, and then at the edges; and how a spot on the face gives exit to a much smaller number of lines than a like spot on the edge, at the same distance from the magnetic equator. Iron filings show such results readily, and so also do the vibrations of a magnetic needle, and likewise the revolutions of a wire ring (3212.). Now this state of the plate-magnet is precisely that which would be expected from the hypotheses of the necessary and dependent state of the magnet on the medium surrounding it.

3291. The mutual dependence of a magnet and the external medium, assumed in the view now put forth, bears upon, and may probably explain, numerous observations of the apparently superficial character of the magnetism of iron and magnets in different cases. If a hard steel bar be magnetized by touch of other magnets, both the vicinity of the superficial parts of the bar to the exciting magnet in the first instance, and afterwards to the surrounding sustaining medium, will tend to cause the magnetism to be superficial in the bar. If a small magnet or a horseshoe bar be surrounded by a thick shell of iron as its external medium, the inner surface of the iron, or that nearest to the magnet, with its neighbouring parts, will convey on more power than the parts further away. If a thick iron core be placed in a helix carrying a feeble or moderate electric current, it is the part of the core nearest to the helix which becomes most highly charged. Probably many other like results may appear, or be hereafter devised, and may greatly help to assist the discussion of the question of physical lines of force now under consideration.

3292. When, in place of considering the medium external to a magnet as homogeneous or equal in magnetic power, we make it variable in different parts, then the effects in it appear to me still to be in perfect accordance with the notion of physical lines of magnetic force, which, being present externally, are definite in direction and amount. The series of substances at our command which affect the surrounding space in this respect, do not present a great choice of successive steps; but having iron, nickel and cobalt, very high as paramagnetic bodies, we then possess hard steel, as very far beneath them; next, perhaps, oxides of iron, and so on by solutions of the magnetic metals to oxygen, water, glass, bismuth and phosphorus, in the diamagnetic direction. Taking the magnetic force of the earth as supplying the source of power, and placing a globe of iron or nickel in the air, we see by the pointing of a small magnetic needle (or in another case, by the use of iron filings (3240.)), the deflected course of the lines of force as they enter into and pass out of the sphere, consequent upon the conducting power of the paramagnetic body. These have been described in their forms in another place (3238.). If we take a large bar-magnet, and place a piece of soft iron, about half the width of the magnet, and three or four times as long as it is wide, end on to, and about its own width from one pole, and covering that with paper, then observe the forms of the lines of force by iron-filings; it will be seen how beautifully those issuing from the magnet converge, by fine inflections, on to the iron, entering by a comparatively small surface, and how they pass out in far more diffuse streams by a much larger surface at the further part of the bar, fig. 7. If we take several pieces of iron, cubes for instance, then the lines of force, which are altogether outside of them, may be seen undergoing successive undulations in contrary directions, fig. 8. Yet in all these cases of the globe, bar and cubes, I at least am satisfied that a section across the same lines of force in any part of their course, however or whichever way deflected, would yield the same amount of effect (3109. 3218.); at the same time, this effect of deflection is not only consistent with, but absolutely suggests the idea of a physical line of force.

3293. Then the manner in which the power disappears in such cases to an ordinary magnetic needle is perfectly consistent. A little needle held by the side of the soft bar described above (3292.), indicates much less magnetic power than if the iron were away. If held in a hole made in the iron, it is almost indifferent to the magnet; yet what power remains shows that the lines through the air in the hole are in the same general direction as those through the neighbouring iron. These effects are perfectly well known, no doubt; and my object is only to show that they are consistent with, and support the idea of external media having magnetic conducting power. But these apparent destructions of power, and even far more anomalous cases (2868. 3155.), are fully accounted for by the hypothesis; and the force absolutely unaffected in amount is found, experimentally, by the moving wire. I have had occasion before to refer to the modification of the magnetic force (in relation to the magnetic needle), where, its absolute quantity being the same, it passes across better or worse conductors, and I have temporarily used the words *quantity* and *intensity* (2866. 2868. 2870.). I would, however, rather not attempt to limit or define these or such like terms now, however much they may be wanted, but wait until what is at present little more than suggestion, may have been canvassed, and if true in itself, may have received assurance from the opinions or testimony of others.

3294. The association of magnet with magnet, and all the effects then produced (3218.), are in harmony, as far as I can perceive, with the idea of a physical line of magnetic force. If the magnets are all free to move, they set to each other, and then tend to approach; the great result being, that the lines from all the sources tend to coalesce, to pass through the best conductors, and to contract in length. When there are several magnets in presence and in restrained conditions, the lines of force, which they present by filings, are most varied and beautiful (3238.); but all are easily read and understood by the principles I have set forth. As the power is definite in amount, its removability from place to place, according to the changing disposition of the magnets, or the introduction of better or worse conductors into the surrounding media, becomes a perfectly simple result.

3295. As magnets may be looked upon as the habitations of bundles of lines of force, they probably show us the tendencies of the physical lines of force as they also occur in the space around; just as electric currents, when conducted by solid wires, or when passing, as the Leyden or the voltaic spark, through air or a vacuum, are alike in these essential relations. In that case, the repulsion of magnets when placed side by side, indicates the lateral tendency of separation of lines of magnetic force (3267.). The effect, however, must be considered in relation to the simultaneous gathering up of the terrestrial lines of force in the surrounding space upon each magnet, and also the tendency of each magnet to secure its own independent external medium. The effect coincides with, and passes into that of the lateral repulsion of balls of iron in a previously equal magnetic field (2814.); which again, by a consideration of the action in two directions, *i. e.* parallel to and across the magnetic axis, links the phænomena of separation with those of attraction.

3296. When speaking of magnets, in illustration of the question under consideration, I mean magnets perfect in their kind, *i. e.* such as are very hard and hold their charge, so that there shall be neither internal reaction of discharge or development (3224.), nor any external change, except what may depend upon such absolute and permanent loss of exciting power as is consequent upon an over-ruling change of the external relations. Heterogeneous magnets, which might allow of irregular variations of power, are out of present consideration.

3297. With regard to the great point under consideration, it is simply, whether the lines of magnetic force have a *physical existence* or not? Such a point may be investigated, perhaps even satisfactorily, without our being able to go into the further questions of how they account for magnetic attraction and repulsion, or even by what condition of space, æther or matter, these lines consist. If the extremities of a straight bar-magnet, or if the polarities of a circular plate of steel (3274.), are in magnetic relation to each other externally (3257.), then I think the existence of *curved* lines of magnetic force must be conceded (3258. 3263.)*; and if that be granted, then I think that the physical nature of the lines must be granted also. If the external relation of the poles or polarity is denied, then, as it appears to me, the internal relation must be denied also; and with it a vast number of old and new facts (3070. &c.) will be left without either theory, hypothesis, or even a vague supposition to explain them.

3298. Perhaps both magnetic attraction and repulsion, in all forms and cases, resolve themselves into the differential action (2757.) of the magnets and substances which occupy space, and modify its magnetic power. A magnet first originates lines of magnetic force; and then, if present with another magnet, offers in one position a very free conduction of the new lines, like a paramagnetic body; or if restrained in the contrary position, resists their passage, and resembles a highly diamagnetic substance. So, then, a source of magnetic lines being present, and also magnets or other bodies affecting and varying the conducting power of space, those bodies which can convey onwards the most force, may tend, by differential actions, with the others present, to take up the position in which they can do so the most freely, whether it is by pointing or by approximation; the best conductor passing to the place of strongest action (2757.), whilst the worst retreats from it, and so the effects both of attraction and repulsion be produced. The tendency of the lines of magnetic force to shorten (3266. 3280.) would be consistent with such a notion. The result would occur whether the physical lines of force were supposed to consist in a dynamic or a static state (3269.).

3299. Having applied the term *line of magnetic force* to an abstract idea, which I believe represents accurately the nature, condition, direction, and comparative amount of the magnetic forces, without reference to any physical condition of the force, I have now applied the term *physical line of force* to include the further idea of their physical nature. The first set of lines I *affirm* upon the evidence of strict experiment (3071, &c.). The

* See for a case of curved lines the inclosed and compressed system of forces belonging to the central circular magnet, fig. 6 (3275.).

second set of lines I advocate, chiefly with a view of stating the question of their existence; and though I should not have raised the argument unless I had thought it both important, and likely to be answered ultimately in the affirmative, I still hold the opinion with some hesitation, with as much, indeed, as accompanies any conclusion I endeavour to draw respecting points in the very depths of science, as, for instance, regarding one, two, or no electric fluids; or the real nature of a ray of light, or the nature of attraction, even that of gravity itself, or the general nature of matter.

Royal Institution,
March 6, 1852.

LIX. *On the Ten-year Period which exhibits itself in the Diurnal Motion of the Magnetic Needle.* By Dr. Lamont *of Munich**.

THE remarkable experiments of Faraday relative to the diurnal motion of the magnetic needle, have induced me to submit the peculiarities of this motion to a closer examination, and thus to obtain a clearer view of the facts which accompany it.

Among these there exists at present one, which, so far as I am aware of, has hitherto remained unrecognised, but which seems of so important a nature that it cannot be omitted in any theory which undertakes to render an account of the diurnal variation; it is the following:—

The magnitude of the variations of declination have a period of ten years. For five years there is a uniform increase, and during the following five years a uniform decrease in the variations.

With us the magnetic declination is a minimum at about 8 o'clock in the morning, and is greatest at 1 o'clock in the afternoon. Subtracting the declination at 8 o'clock from that at 1 o'clock, we obtain *the magnitude of the diurnal motion.* From the hourly observations conducted in this observatory since the month of August 1840, we ascertain the following to be the magnitude of the diurnal motion for each month separately:—

1840.	Magnitude of the diurnal motion. ′	1841.	Magnitude of the diurnal motion. ′	1841.	Magnitude of the diurnal motion. ′
August	+10·63	January	+3·72	July	+10·07
September	10·97	February	5·13	August	9·86
October	7·72	March	8·43	September	8·78
November	4·40	April	11·49	October	6·82
December	3·51	May	11·47	November	3·71
		June	11·49	December	2·89

* From Poggendorff's *Annalen*, vol. lxxxiv. p. 572.

	Magnitude of the diurnal motion.		Magnitude of the diurnal motion.		Magnitude of the diurnal motion.
1842.	′	1845.	′	1848.	′
January	+3·65	January	+2·20	January	+6·52
February	4·74	February	4·69	February	9·01
March	8·34	March	8·26	March	11·96
April	10·33	April	11·93	April	14·56
May	9·31	May	10·88	May	14·22
June	9·78	June	10·73	June	13·80
July	8·38	July	9·44	July	14·67
August	9·03	August	10·42	August	15·40
September	7·72	September	8·82	September	14·00
October	7·05	October	7·34	October	10·30
November	3·86	November	4·49	November	5·78
December	2·81	December	8·34	December	3·53
1843.		1846.		1849.	
January	3·82	January	3·30	January	7·27
February	4·08	February	6·94	February	8·42
March	6·87	March	9·53	March	14·08
April	9·71	April	12·27	April	16·86
May	9·24	May	12·58	May	13·67
June	10·14	June	11·21	June	13·86
July	9·57	July	11·37	July	12·57
August	10·08	August	11·49	August	11·54
September	8·81	September	10·39	September	10·79
October	6·82	October	7·82	October	9·12
November	3·82	November	5·66	November	5·41
December	2·79	December	3·22	December	4·09
1844.		1847.		1850.	
January	2·81	January	3·30	January	5·98
February	3·43	February	6·35	February	8·84
March	6·95	March	9·85	March	12·15
April	9·53	April	12·43	April	14·32
May	8·42	May	11·81	May	14·05
June	8·88	June	11·76	June	13·39
July	8·38	July	10·94	July	12·53
August	9·28	August	12·87	August	12·68
September	8·23	September	12·06	September	12·64
October	6·54	October	11·53	October	9·04
November	3·94	November	7·06	November	6·20
December	2·98	December	4·70	December	3·45

For the sake of a better oversight, we will set the averages of the years together; the first three months of the year being united with the last three under the name of the *winter half-*

year, and the others under the name of the *summer half-year*. In this way we obtain the following table:—

Magnitude of the diurnal motion.			
Year.	Mean for the year.	Winter.	Summer.
1841.	7·82	5·12	10·53
1842.	7·08	5·07	9·09
1843.	7·15	4·70	9·59
1844.	6·61	4·44	8·79
1845.	8·13	5·89	10·37
1846.	8·81	6·08	11·55
1847.	9·55	7·63	11·98
1848.	11·15	7·85	14·44
1849.	10·64	8·06	13·21
1850.	10·44	7·61	13·27

A glance at this table is sufficient to show that *a periodical increase and decrease* of the magnitude of the daily motion takes place. Applying the same method as that made use of by Sir John Herschel in similar cases, I have drawn a curve, which by regular curvature passes as nearly as possible through the ends of the single ordinates, and find 1843·5 to be the epoch of the minimum, and 1848·5 the epoch of the maximum.

Shortly before the commencement of the series of observations, a maximum must have taken place. To ascertain the epoch of this maximum, we will call in the aid of the Göttingen observations. From the *Resultaten des Magnetischen Vereins* we deduce the following numbers as expressing the magnitude of the motion for the single years.

Magnitude of the diurnal motion.			
Year.	Mean for the year.	Summer.	Winter.
1834.	......	......	10·39
1835.	9·57	7·02	12·13
1836.	12·34	8·78	15·90
1837.	12·27	9·39	15·15
1838.	12·74	9·05	16·42
1839.	11·03	8·01	14·05
1840.	9·91	7·33	12·50
1841.	8·70	6·12	11·27

Making use of the method above alluded to, I derive from these numbers that the maximum took place in 1837·5.

If we go still further back, the observations at present existing do not furnish us with any data until we reach 1820. In this year the series of observations undertaken by Col. Beaufoy at Bushy Heath was finished, and from these we are enabled to make further determinations. The magnitude of the motion for the single months, as derived from Beaufoy's observations, is as follows :—

	Magnitude of the diurnal motion.
1813.	′
April	11·90
May	8·87
June	9·70
July	8·53
August	7·57
September	6·77
October	7·20
November	2·70
December	2·47
1814.	
January	3·97
February	6·13
March	8·65
April	11·00
May	9·02
June	9·63
July	10·25
August	9·58
September	8·73
October	7·62
November	4·28
December	2·57
1815.	
January	3·43
February	6·67
March	8·85

	Magnitude of the diurnal motion.
1815.	′
April	11·68
May	10·52
June	11·12
July	9·90
August	8·10
September	7·05
1817.	
April	12·85
May	10·25
June	11·08
July	10·87
August	11·58
September	8·57
October	9·67
November	6·10
December	3·98
1818.	
January	5·92
February	6·48
March	8·32
April	10·73
May	9·52
June	11·40
July	10·58
August	11·30
September	10·88
October	8·03

	Magnitude of the diurnal motion.
1818.	′
November	8·28
December	4·27
1819.	
January	4·20
February	5·63
March	8·40
April	10·55
May	8·67
June	10·22
July	9·68
August	10·27
September	9·10
October	6·68
November	6·02
December	3·85
1820.	
January	3·80
February	5·80
March	8·77
April	9·85
May	9·43
June	9·43
July	10·32
August	9·58
September	9·22
October	8·55
November	5·25
December	3·52

The yearly and half-yearly averages are as follows :—

Magnitude of the diurnal motion.			
Year.	Average for the year.	Winter.	Summer.
	′	′	′
1813.	......	......	8·89
1814.	7·62	5·54	9·70
1815.	......	......	9·73
1816.			
1817.	......	......	10·87
1818.	8·81	6·88	10·74
1819.	7·77	5·80	9·75
1820.	7·79	5·95	9·64

From this we ascertain that a maximum took place in the year 1817.

To obtain further data, we must go back to the observations of Gilpin and Cassini. The former observed from 1786 to 1805 in the meeting-room of the Royal Society in London. Two circumstances, however, oppose the use of his results: first, they are incomplete, a period of six or seven months in one year being sometimes omitted; and secondly, the compass which he made use of does not seem to have been trustworthy. Gilpin, indeed, always repeated his observations, in order to lessen as far as possible the insecurity of his readings. We must still, however, look with some distrust upon results derived from a needle, which, when caused to oscillate, often deviated eight or ten or even a greater number of minutes from its former position. From 1795 to 1805, the solstitial months, June and December, and the equinoctial months, March and September, are given. I have set down these, and also the mean of the four months, in the following table:—

Magnitude of the diurnal motion.			
Year and month.	June and Dec.	March and Sept.	Mean.
1795.	′6·5	′8·7	′7·6
1796.	7·4	8·6	8·0
1797.	8·3	7·5	7·9
1798.	6·9	8·3	7·6
1799.	7·1	7·6	7·3
1800.	7·0	7·3	7·1
1801.	6·6	9·4	8·0
1802.	7·2	9·2	8·2
1803.	7·8	10·6	9·2
1804.	7·5	9·6	8·5
1805.	8·5	8·7	8·6

These numbers do not by any means support the hypothesis above stated; a decided period is not at all exhibited.

It is otherwise, however, with the observations of Cassini, which commence two years earlier than those of Gilpin, but are only carried out to 1788. The records were made on the 4th, 12th, 20th, and 28th of each month, and give the following results:—

1784.	Magnitude of the diurnal motion.	1784.	Magnitude of the diurnal motion.	1784.	Magnitude of the diurnal motion.
January	′8·77	May	′12·22	September	′11·71
February	8·97	June	11·44	October	9·11
March	10·72	July	10·03	November	6·23
April	11·16	August	10·81	December	4·46

1785.	Magnitude of the diurnal motion.	1786.	Magnitude of the diurnal motion.	1787.	Magnitude of the diurnal motion.
January	6·50	May	14·62	September	15·92
February	7·19	June	14·89	October	13·40
March	9·82	July	15·82	November	12·09
April	10·99	August	15·72	December	10·58
May	14·49	September	15·65	1788.	
June	11·75	October	16·82	January	10·20
July	11·74	November	10·24	February	10·65
August	13·49	December	10·82	March	16·87
September	14·35	1787.		April	20·51
October	12·22	January	14·30	May	16·88
November	9·25	February	15·13	June	15·00
December	7·84	March	18·19	July	12·09
1786.		April	16·03	August	11·71
January	10·27	May	14·58	September	13·59
February	10·68	June	14·72	October	12·09
March	15·41	July	17·93	November	11·75
April	17·06	August	18·80	December	10·42

Calculating from this, in the manner already pursued, the average for the year, and then the mean for the summer and winter half-year, we obtain—

Magnitude of the diurnal motion.			
Year.	Average for the year.	Winter.	Summer.
1784.	9·65	8·04	11·22
1785.	10·80	8·80	12·80
1786.	14·00	12·37	15·62
1787.	15·14	13·95	16·33
1788.	13·48	12·00	14·96

We here again detect a regular period, with a maximum at about 1786·5.

Setting the above results together, we find that we are in possession of four series of observations, made at different places and at different times, all of which show a periodical increase and decrease of the diurnal motion of the needle. The maxima fell in the following years:—

1848·5
1837·5
1817
1786·5

Permitting the first and last numbers to remain, and assuming the period to be $10\frac{1}{3}$ years, the maxima would fall upon the following years :—

1848·5
1838·2
1817·5
1786·5

The agreement with the actual observations is quite satisfactory.

In order further to show how far the observed increase and decrease agrees with the above hypothesis, I will choose for the period the simplest formula, that is,

$$a+b\sin\left(c+n\frac{360^\circ}{10\frac{1}{3}}\right).$$

Assuming, according to the observations in Munich, that a maximum occurred in 1848·5, and calculating the remaining constants from the values obtained at the time of maximum and minimum, we obtain for the magnitude of the yearly motion the following expression,—

$$8'\!\cdot\!70+2'\!\cdot\!1\sin(72^\circ\!\cdot\!58+n34^\circ\!\cdot\!84),$$

where n expresses the number of years beginning at 1848. Comparing the magnitude of the diurnal motion calculated from this with the observations before given, we obtain the following :—

Year.	Calculated diurnal motion.	Divergence from observation.
1841.	9·01′	+1·19′
1842.	7·26	+0·18
1843.	6·64	−0·51
1844.	6·77	+0·16
1845.	7·59	−0·54
1846.	8·80	−0·01
1847.	9·98	+0·43
1848.	10·70	−0·45
1849.	10·70	+0·06
1850.	9·98	−0·56

In Göttingen the diurnal motion is $\frac{1}{10}$th greater than in Munich; deducting this from the results of the Göttingen observations given above, and comparing with our formula, we obtain—

Year.	Calculated diurnal motion.	Divergence from observation.
1835.	7·97′	−0·65′
1836.	9·22	−1·89
1837.	10·29	−0·75
1838.	10·79	−0·68
1839.	10·53	+0·60
1840.	9·62	+0·70

Had it been my object to give, by means of a periodic function, the most exact representation possible of the observed numbers, there would have been no difficulty in choosing such a function. The labour at present, however, would lead to no useful result. We first need a much more extensive series of observations. I have therefore contented myself with an approximation sufficiently close to demonstrate the existence of a period.

It may not be amiss to remark here, that the daily motion, as shown by me on a former occasion, consists of *two* portions altogether different—of a polar wave and an equatorial wave. In carrying out the subject, it will be in the first place necessary to separate these portions, by comparing observations made at the equator with those made in higher latitudes. That both portions are subject to a contemporaneous increase or decrease does not appear to me probable.

I will remark, in conclusion, that the diurnal motion of the horizontal intensity is also subject to a considerable alteration; whether of the same period, or that of the declination, I have not yet ascertained. Our observations must be submitted to various tedious reductions before they are in a state to decide the above question.

Munich, Sept. 1851.

LX. *On a Mode of reviving Dormant Impressions on the Retina.* *By* W. R. Grove, *M.A., F.R.S.*

To Richard Taylor, Esq.

My dear Sir, May 4, 1852.

An experiment which I made lately on the revival of the images formed on the retina by bright objects may be worth recording. I have never seen it in any work which I have read; and on showing it to some gentlemen much better acquainted with this subject than I am, they regard it as new; if not so, my note will do no more harm than taking up a very small portion of your valuable space.

1st. Look steadily at a luminous object sufficiently bright to be borne by the eyes without great inconvenience, then turn the eyes upon a dark body or dark space: an image of the object looked at will be seen, a fact familiar to every body. When the image has completely faded away and is no longer visible, pass backwards and forwards between the eye and the dark body a white substance, say a sheet of paper, the image will be immediately revived, and may be thus indefinitely reproduced.

If the light is in the first instance not sufficiently vivid to produce the continued impression on the retina, but is nearly so, the invisible image may be brought out or first rendered visible by moving the white object between the eye and the dark body or dark space looked at. The white substance should be in a situation exposed to light so that its whiteness affects the eye, and not held in shadow. After a little practice, it is astonishing to what an extent and for how long a time images may be thus reproduced.

2ndly. Reverse the experiment, looking from the bright object at white paper, and a dark image of the object will be seen; when this has faded away, move between the eye and the paper a dark substance held so as to reflect as little light as possible to the eye, and the image is reproduced on the white paper, or may be in the first instance produced as with the converse experiment.

The explanation which occurs to me is, that the effect is one of contrast between the portions of the retina which have not been strongly affected and those which have.

The white paper dulls or deadens the sensitive portion of the retina for an instant, more than the part which has been previously rendered non-sensitive to other impressions than that which it has received by the bright light, and the black supervenes as a contrast to the parts affected by the white, but not to those unaffected. In the converse experiment, the black relieves or renders more sensitive the comparatively unaffected portions of the retina, but has little or no operation on the non-sensitive parts; thus at the moment of removing the black body, the unimpressed portions of the eye are affected by the white substance, but the impressed portion is comparatively dead to it. Probably coloured bodies looked at, and coloured screens moved to and fro, would give a series of complementary effects; and if I find the point has not been examined, I may with your permission add a few experiments to the two given in this note.

I remain, dear Sir,

Yours very truly,

W. R. Grove.

LXI. *On Algebraic Transformation, on Quadruple Algebra, and on the Theory of Equations.* By James Cockle, *M.A., of Trinity College, Cambridge; Barrister-at-Law of the Middle Temple*.*

THE following are intended as supplementary to previous observations of mine in this Journal.

(*a.*) Since the relation (f) (at p. 291 of vol. ii. S. 4) gives

$$x^{m+1}y+ax^m=0, \quad xy^{m+1}+ay^m=0, \quad . \quad . \quad . \quad (t)$$

* Communicated by the Author.

we see that, when two unknowns are connected by a quadratic equation, then any higher function of those unknowns admits of material simplifications depending upon the transformation of the quadratic to the form (f). Thus, a cubic function may be made to take the form

$$x^3+a^3y^3+b^2x^2+c^2y^2+dx+ey+f, \quad . \quad . \quad . \quad (u)$$

or that indicated by the relations

$$dx=-da^{-1}x^2y, \quad ey=-ea^{-1}xy^2,$$

or the following form,—

$$(x+ay)^3+(bx\pm cy)^2+d'x+e'y+f', \quad . \quad . \quad . \quad (v)$$

besides others. It may perhaps be worth while to inquire whether, by sacrificing the symmetry of (f), the cubic function may not be rendered symmetric; and also to examine the cubic system, corresponding to (h) and (i) of p. 292, vol. ii. S. 4, which may be obtained when the latter function is supposed to vanish; and to ascertain whether, as the solution of two simultaneous quadratics may be made to depend on that of a cubic, so the solution of a simultaneous quadratic and cubic may not be made to depend on that of an equation lower than the sixth degree. The same observations apply when in place of a cubic we have a higher function. I think it right to add, that I arrived at the solution of (n) and (o) (Ibid. p. 293) by an assumption of the form

$$x=\lambda x'+\mu y'+r,$$

and then seeking, by this linear transformation, to render the resulting system pseudo-homogeneous—that is to say, free from the first dimension of x' and y'. The expression (p) for y was at once indicated, and, thence, my solution of the given system —under which the two members become identical: x and y each satisfying a functional equation of the form

$$f^2(x)-x=g.$$

Compare this with the functional relation satisfied by the roots of a certain trinomial quintic (S. 3. vol. xxxii. p. 52).

(*b*.) There is an abnormal system of quadruple algebra in which the imaginaries are subject to the conditions

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

This I have proposed to call the coquaternion system (see p. 434 of vol. xxxv. S. 3.). Let M be the modulus of the coquaternion A, then we may make

$$\mathrm{A}=\mathrm{M}(s+\alpha t+\beta u+\gamma v).$$

where s, t, u, v, which are all real, satisfy the relation

$$(s \pm u \pm v)^2 + t^2 = 1,$$

the signs being regulated by the form of modulus selected. And we are at liberty to assume that

$$s \pm u \pm v = \cos p, \text{ and } t = \sin p.$$

(The angle p is the *amplitude* of the coquaternion.) And if we make

$$s = q \cos p, \quad u = r \cos p, \text{ and } v = \pm(1 - q \mp r) \cos p,$$

any coquaternion may be put under the form

$$\mathrm{M}\{q \cos p + \alpha \sin p + \beta r \cos p \pm \gamma (1 - q \mp r) \cos p\}.$$

It is to be observed that (1) the modulus of the product of two coquaternions is the product of the moduli of the factors, and (2) the amplitude of the product is the sum of the amplitudes of the factors. I have already given the corresponding expression of a tessarine (S. 3. vol. xxxvi. p. 290). Each system of multiple algebra may—like the quaternion theory with reference to geometry—be the appropriate exponent of some branch of science, or find some peculiar application in geometry itself.

I am indebted to Professor W. F. Donkin for pointing out to me, in a letter dated Keswick, August 29, 1850, that "it is easy to give an interpretation to the [tessarine] symbols considered as representing *transposition*; thus, according to Sir W. Hamilton's notation, we should have

$$i = \mathrm{R}_{-1, 0, -3, 2}, \quad j = \mathrm{R}_{2, 3, 0, 1}, \quad k = \mathrm{R}_{-3, 2, -1, 0}."$$

Professor Donkin adds, that "perhaps this might suggest a geometrical interpretation." Should such an interpretation be satisfactorily arrived at and acceded to, the normal nature of the tessarine system would afford a great practical convenience.

Let O, the origin of coordinates, be the centre of a sphere whose pole is A and radius unity, and which is intersected in B by the radius vector of a tessarine. Through B draw a great circle meeting the first meridian in C, and such that the angle ABC is equal to $\pi - \theta$; then

$$\cos \mathrm{C} = \cos \theta \cos \psi + \sin \theta \sin \psi \cos \phi,$$

and the equation for the submodulus (S. 3. vol. xxxiv. p. 47) may be expressed as follows,—

$$\nu^2 = \mu^2 \sin \theta \sin \phi \cos \mathrm{C}.$$

The new modulus and amplitude (Ibid. vol. xxxvi. p. 291) will however probably supersede the former expressions.

(*c*.) If, in certain of my previous researches on equations (Ibid. vol. xxvii. p. 293), we denote by ρ the radical in (18) the remaining coefficients of (16), viz. γ_4, γ_5, . . and γ_{10} (misprinted

γ_0) are given by the formula

$$\gamma_r = \frac{1}{2} \cdot \frac{\tau_{2,r}}{\gamma_2} \pm \frac{1}{2} \rho^{-1} \left(\tau_{3,r} - \frac{1}{2} \tau_{2,3} \tau_{2,r} \gamma_2^{-2} \right). \quad . \quad . \quad . \qquad (20)$$

Consistency must be observed in the sign of the radical ρ: hence, considering γ_2 as positive, and giving to $J_2^{(2)}$ its original quadratic form, if we make

$$f^2(9) = J_2^{(2)} + R, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (21)$$

and represent by $R_{p,q}$ the coefficient of $z_p z_q$ in R, and by γ'_r and γ''_r the two values of γ_r corresponding respectively to the positive and negative values of ρ, the following relations obtain,—

$$\tau_{p,q} = \gamma'_p \gamma''_q + \gamma'_q \gamma''_p + R_{p,q}; \quad . \quad . \quad . \quad . \quad . \qquad (22)$$

where, when p and q are equal, the middle term is suppressed.

The subject of impossible equations has been pursued with great assiduity and zeal by Mr. Robert Harley, who has discussed it in the memoirs of the Manchester Literary and Philosophical Society (vol. ix. pp. 207–235), and also in the Mechanics' Magazine (vol. l.). He has shown that if

$$\sqrt{x} + \sqrt{x+1} = 0, \text{ then } x = (-1)^2\{1-(-1)^2\}^{-1} \cdot$$

This equation I had previously (Phil. Mag. S. 3. vol. xxxvii. p. 283) stated to be insolvable. So far, therefore, as that equation is concerned, the views which I took in the paper last cited must be modified; but, as at present advised, I do not conceive that they require any further modification. The existence of *impossible* equations cannot but be admitted, until some satisfactory and consistent mode of affecting

$$\sqrt{m(+1)^2 + n(-1)^2} \text{ or } \sqrt{\rho}$$

with an appropriate sign shall enable us to satisfy such equations as $Y=0$ and $Z=0$. Consider for a moment the equation $Y=0$; its root x is subject to the conditions

$$x-4 = (+1)^2, \text{ and } x-1 = 4(-1)^2,$$

which can only be simultaneously satisfied by supposing that the 4 in $Y=0$ is of the form $4(-1)^2$, and the 1 of the form $(+1)^2$. What justifies these suppositions? What prevents their violation by the *express* form which we are at liberty to give to our symbols? It would seem that we should thus obtain an absolutely impossible equation. Is there any law of affection by which, x being taken of the form ρ, such difficulties can be obviated?

2 Pump Court, Temple,
April 19, 1852.

LXII. *On the Authorship of the Account of the* Commercium Epistolicum, *published in the Philosophical Transactions. By* Professor De Morgan*.

IN Number 342 (Jan. to Feb. 1714–15) of the Philosophical Transactions, is the celebrated paper headed "An Account of the Book entituled *Commercium Epistolicum*...," which was translated into Latin and French, the former in the preliminaries of the second edition of the *Comm. Epist.* itself, the latter in vol. vii. of the *Journal Litteraire.* This paper has been attributed to Newton by some; but in England it seems to be generally supposed that Keill was the author. Many writers offer no opinion. Sir David Brewster says that it is *falsely* ascribed to Newton, but without stating any reason beyond the *groundless* character of the ascription. Biot says that it appears to have been written by Newton. Montucla, on information received from England *par des notes de bonne main,* states that the *notes to the Comm. Epist.* are by Newton: this I suspect to be a confusion between the avowedly new and separate matter of the second edition, and the running notes in the first. And so the matter now stands.

Halley's letter to Keill of Oct. 3, 1715 (Edleston's Correspondence, &c. p. 184) concerning the French translation, may be interpreted in favour of the authorship of either Keill or Newton; but seems to render it probable that it must have been one or the other. What we can get from Keill himself amounts to the following. In his epistle to John Bernoulli, separately printed† in 1720, we find "Est etiam ejusdem Commercii Recensio sive Epitome in *Actis Londinensibus* edita: Prodierunt et nostræ Responsiones ad tuas amicorumque tuorum calumnias" (p. 8): and again, "In *Commercio Epistolico* ejusque Epitome, et in aliis scriptis a me editis...." (p. 20). In the first extract the *Recensio* is distinguished from Keill's own reply inserted in vol. iv. of the *Journ. Litteraire.* The second extract is ambiguous: if it give some help to those who would make Keill the author of the Epitome, it gives just as much to those (Watt and others) who set him down as the editor of the *Comm. Epist.* itself.

I shall now proceed to the evidence, external and internal, on which my mind is satisfied that Newton is really the author of the paper in question.

James Wilson, M.D., the witness in this matter, was probably

* Communicated by the Author.

† The *Biogr. Britann.* 'Keill,' states that this tract has the thistle on its title-page, with *Nemo me,* &c. My copy has no such thing, but only *Cohibe linguam tuam à malo, et labia tua ne loquantur dolum.* It may be that this was the original title-page, and that remonstrance or second thought suggested the erasure of this rather offensive motto.

about the same age as his friend Dr. Pemberton, with whom he studied medicine at Paris: Pemberton was born in 1694. The two lived in closest friendship and neighbourhood to the end of their lives: Pemberton died early in 1771, leaving his books to Wilson, who lived to publish his friend's course of Chemistry, with a memoir, and died* himself on Sept. 29 of the same year. Wilson also describes himself as the friend of Brook Taylor, and to this I have Taylor's attestation. In 1722, Wilson published Pemberton's answer to his questions about Cotes, *Epistola ad Amicum de Cotesii inventis*, with an appendix in 1723 (London, 4to). In his copy of the first, now in my possession, Taylor wrote *E libris Br. Taylor Ex dono eximii paris amicorum, autoris D. H. Pemberton, atque editoris D. J. Wilson.* Consequently, Wilson lived in friendship with at least two, and probably more, of those who had a direct connexion with the controversy; and that he was a very good mathematician, a high fluxionist, is obvious from the letter which Pemberton addressed to him. Again, his qualifications as an historian of research and accuracy are well attested by his sketch of the history of navigation, written for and published in Robertson's work on that subject, and reprinted by Baron Maseres in the fourth volume of the *Scriptores Logarithmici.* In 1761, as executor of his friend Robins, he published some works of the latter, in two octavo volumes; and at the end of the second volume he added, by way of appendix to Robins's tracts on the Analyst controversy, a very elaborate and well-referenced discussion on several points of the fluxional controversy. That he should have written this without consultation with his most familiar friend Pemberton, or that a single point of it capable of difference of opinion should have escaped discussion between them, is incredible; and that any fact on which Pemberton's memory differed from his own should have been repeated over and over again by him without statement of the discordance, is still more so. And this most especially as to matters in which Newton was personally concerned. For Pemberton was Newton's latest editor, the last man with whom he advised on matters relating to the great controversy, and who must have been on terms of intimate acquaintance with him at the very time when the republication of the *Comm. Epist.*, and the junction with it of the paper now under discussion, must have made many inquire into the authorship of that paper.

Now Wilson affirms that Newton wrote, not merely the *Recensio* in question, but the *Ad Lectorem,* and the remarks on John Bernoulli's letter which terminate the second edition of

* See the *Notes and Queries*, vol. v. pp. 362, 399, Rigaud's *Historical Essay on the Principia*, p. 107, and Wilson's own discourse on fluxions presently mentioned.

the *Comm. Epist.* And this not once, but many times; and not as laying any stress on the assertion of authorship, but just in the same manner as when he affirms that Maclaurin wrote on fluxions, or that John Bernoulli's letter is at page 448 of the *Journal Litteraire*: taking all these matters to be equally beyond controversy. Nowhere does he hint at a doubt upon the subject. The following extracts will establish these points:—

(Pp. 323, 324.) "But the weakness of this judgment [the celebrated *judicium primarii mathematici*] has been fully exposed by Sir Isaac Newton himself, at the end of the Commercium Epistolicum." The same assertion again in page 359.

(P. 331.) "They talk indeed of their exponential calculus as a great discovery* (*[note irrelevant]) but this Sir Isaac Newton himself has hinted to be really of no use†. († Philos. Trans. N° 342. p. 212. Or Comm. Epist. p. 46.)"

(P. 332, 333.) N° 342 again cited to prove that *Newton* repeats most explicitly that *he* had no physical cause of gravity.

(P. 334.) "Now Sir Isaac Newton has himself informed us†, († Comm. Epistolic. in the preface, page penult.) "Quæ novæ Principiorum editioni præmissa sunt, Newtonus non vidit antequam Liber in lucem prodiit;" and it is well known that he was much dissatisfied with that preface for more reasons than one . . ." That is, Wilson affirms the *Ad Lectorem* to be also written by Newton.

(P. 335.) "But that no opinions may be attributed to this great man which he never held; see what he has said excellently well himself in the Philosophical Transactions, N° 342 p. 222*, (* Com. Epist. p. 55, &c.) about his method of philosophising." In the same page the same reference occurs again, as reference to objections cleared up "by Sir Isaac Newton himself." And again in the next page on a note to "Sir Isaac Newton has himself told us his real motives." Again also in pp. 342, 355, 361.

(Pp. 367, 368.) ". . . . the second edition of the Commercium Epistolicum . . . . . where Sir Isaac Newton has, in the Preface, Account and Annotation, which were added to that edition, particularly answered . . . ." In a note this statement is repeated, both as to No. 342, and the preface, or *Ad Lectorem*, which Newton inserted in "a second edition, he made, of the Commercium Epistolicum."

This statement, namely, that Newton wrote the paper now chiefly under discussion, as well as the *Ad Lectorem* and postscript to the second edition of the *Comm. Epist.*, is thus woven into the very fabric of Wilson's argument, and could not possibly have been passed over by Pemberton. I shall now proceed to the internal evidence.

The paper, No. 342, was presented to the Royal Society in the three months which are always marked 1714–15: in the same period of the next year (Feb. 26) Newton wrote the letter to Conti; and subsequently, in April or May, the *Observations* which were printed after Leibnitz's death in Raphson's history of fluxions. Between these later productions, published under Newton's hand, and the anonymous *Account*, we find, I think, a considerable likeness of style, of a kind which cannot be made evident except to those who examine the whole. But we also find many marked similarities of phrase; and it will hardly be thought likely that Newton, personally engaged with the chief of his opponents, would pick up the very recent words of a nameless follower. The paging on the left is that of Raphson's history, on the right that of the *Account*.

Newton.	*Author of the Account.*
(P. 100.) They were collected and published by a numerous Committee of gentlemen of several nations.	(P. 221.) The Committee was numerous and skilful, and composed of gentlemen of several nations.
(P. 111.) He pressed the Royal Society to condemn Dr. Keill without hearing both parties.	(P. 221.) Mr. Leibnitz indeed desired the Royal Society to condemn Mr. Keill without hearing both parties.
(P. 102.) And what he then acknowledged, he ought still to acknowledge. (This phrase is repeated and varied several times in both works.)	(P. 211.) And this ...... which he then acknowledged to Dr. Wallis, he ought still to acknowledge.
(P. 115.) And second inventors have no right.	(P. 215.) For second inventors have no right.

I shall produce more instances so soon as I find that more are wanted; but any one who has access to the papers cited can find them for himself. For myself, however, I am more moved by that general similarity of style which cannot be established by instances, than by special accordances of phraseology. Having been accustomed to use the Latin translation of the *Account*, I never remarked this similarity until now.

Throughout the paper in question there is not one compliment to Newton (except in quotations introduced in proof of assertions), not one word expressive of admiration, and not one reference to anything he had done which he might not, in perfect good taste, have been the author of. Who could have written thus about Newton, in 1714, except Newton himself? No one certainly, champion or assailant, who then put his name to what he wrote. Keill and Leibnitz, Taylor and Bernoulli, always let us see, both directly and indirectly, that when they write of Newton, they acknowledge an exalted intellect.

The Latin translator, in one place which has caught my eye, seems to have been a little inclined to mend the baldness of his original. Newton (as I believe) writes, "And by this sort of Railery they are perswading the *Germans* that Mr. *Newton* wants Judgment, and was not able to invent the Infinitesimal Method." The Latin has it, "Atque hujusmodi cavillationibus, homines hi conterraneis suis persuasum esse cupiunt judicio eum *et acumine* parum valere; neque eum esse qui Methodum Infinitesimalem *rem tam arduam* invenire potuisset."

Throughout the paper Newton is "Mr. Newton," never "Sir Isaac Newton," nor simply "Newton." Now though the first designation be chronologically correct, inasmuch as knighthood was not honoured with Newton till after the events under discussion, still it is unlikely that any one but Newton himself could or would have been so correct throughout a long paper.

Keill took his (Oxford) doctor's degree at the Act in 1713. The author of the *Account* calls him *Mr.* Keill, Newton calls him *Dr.* Keill in the later papers.

It will be noticed that the *Ad Lectorem* and the *Annotation* in the Appendix belong to Newton on the external testimony of Wilson, in which is implied the testimony of Pemberton; while the *Recensio* has both the external and internal evidence. How much of the latter kind of evidence may belong to the two former pieces I am not now prepared to say.

In papers on Robins's tracts, printed in the *Republic of Letters* for 1735 and 1736, and reprinted by Wilson (*op. cit.*), there is the same frequency of reference to No. 342, and the same uniform attribution of it to Newton, which we have seen in Wilson's dissertation. Whether these reviews were written by Robins, by Wilson, or by another, they show us the assertion of Newton's authorship, publicly made within a few years of his death; and I am not aware that any one of the time denied it.

I suppose the information furnished by the *bonne main* to Montucla, and which he probably misunderstood, to have been given by some other than Wilson, probably by some one who saw Pemberton's papers (as Montucla states) in the hands of his representatives. Rigaud states that the residuary legatee of Dr. Pemberton was the husband of his niece, Mr. Miles, a timber merchant at Rotherhithe, who was alive in 1788, and had sons. It will be desirable to repeat this statement from time to time, so long as there is the least chance of discovering Pemberton's papers.

April 21, 1852.

LXIII. *On the supposed Identity of the Agent concerned in the Phænomena of ordinary Electricity, Voltaic Electricity, Electro-magnetism, Magneto-electricity, and Thermo-electricity.* By M. Donovan, *Esq., M.R.I.A.*

[Continued from p. 347.]

Section V.

HAVING now discussed the subject of the last section as fully as seemed necessary, I proceed to a most important branch of the investigation, one that is immediately connected with the preceding. The instantaneous charge which a large Leyden battery receives, by a momentary contact with an extensive voltaic series, has been always adduced as an argument in support of the affirmed enormous quantity of electricity which constitutes the voltaic current. The phænomenon when first discovered made a powerful impression on the minds of philosophers, and a strong conviction has been the result. I never was able to participate in this conviction; and the first circumstance which raised my doubts was the feebleness of the shock which a Leyden battery so charged is capable of communicating, when compared with that given by the voltaic series with which the Leyden battery was charged. Many years since, Sir H. Davy induced me to take the shock of a Leyden battery charged by a voltaic battery of one thousand pairs of zinc and copper, each four inches square, then newly constructed for the Royal Dublin Society: the shock was insignificant, and the spark trivial. Accident afterwards gave me the shock of the thousand pairs; it was the most tremendous sensation that can be conceived, and nearly prostrated me.

There are several accounts extant of the charge communicated by voltaic series to Leyden batteries; but many of the trials were made with dry piles, or water batteries, or the contact of the Leyden battery with the voltaic series was still maintained at the moment of the discharge; such trials therefore do not afford the kind of information here required. There are however other experiments on record relative to this subject, which supply materials for strong arguments.

Van Marum charged a Leyden battery of twenty-five jars, containing a coated surface of $137\frac{1}{2}$ square feet, by a momentary connexion with a pile consisting of 200 pairs of silver coins and zinc discs one inch and a half in diameter. It was charged to the exact same tension as the pile itself, which was such as to occasion a divergence of the gold leaves of Bennet's electrometer to the extent of five-eighths of an inch. The result was the same in many experiments. The shock given by the Leyden battery so charged, "extended to the shoulders with much force;" but they "had not however the same force as those of the pile from which they had been charged." He estimated the force of the shock given by the Leyden battery, when charged by the 200 pairs, as equal to the shock of 100 pairs when taken directly from the pile itself; that is, in all his trials with different numbers of pairs, the charge communicated to the Leyden battery had but half the power of the pile which afforded it. He then endeavoured to discover the ratio of the charging power of the pile compared with that of a 31-inch plate-machine belonging to the Teylerian Museum (not the large one); and after many experiments, found that one momentary contact of the pile with the Leyden battery charged the latter to the intensity above-mentioned, while six equally momentary contacts with the conductor of the plate-electrical machine, made by an insulated rod, were required to bring the Leyden battery to the same intensity. But as no account is given of the rate at which the plate was revolving at the instant of contact, the ratio of 1 to 6 taken by itself conveys no information*.

It appears, however, that the shock from this immense Leyden battery must have been very small. Those who have had experience of piles, are aware of these feeble effects compared with other forms of the voltaic apparatus. The superincumbent weight of the column presses the greater part of the exciting liquid from the interposed cloths; streams of a good conducting liquid are therefore continually trickling down, while the cloths are left almost dry; the insulation of the pairs is thus destroyed, and the chemical action on the zinc is rendered feeble. Hence the power of so small a number as 200 pairs to give shocks, when reduced to one-half by being transferred to the Leyden battery, could not be great. This conclusion is supported by the fact that the power of the 200 pairs to produce divergence of the gold-leaf electrometer, through the intervention of the Leyden battery, was equalled by the same battery when charged with six contacts, during a time "as short as possible," with the conductor of a 31-inch plate-machine, the period of each contact being estimated by Van Marum at one-twentieth of a second. Some estimate of the electricity thus thrown in may be formed by comparing the effects of this 31-inch plate-machine with those produced by a powerful one belonging to the Royal Institution, the plate of which is nearly of three times greater area (viz. 50 inches diameter) than Van Marum's. This large plate, as Professor Faraday informs us, gives ten or twelve sparks of an inch long for each revolution, the revolution occupying four-fifths of a second of time. At this rate, supposing each revolu-

* *Annales de Chimie*, xl. p. 289.

tion of Van Marum's machine to occupy half the time, the quantity thrown in during the six contacts would amount to a single one-inch spark. Indeed the divergence of the gold-leaf electrometer, connected with the Leyden battery, seems to have indicated a very low intensity, since it amounted to no more than five-eighths of an inch, an effect which would be doubled by the approach of a bit of excited sealing-wax, without any battery. It is quite plain therefore that the shock given by the Leyden battery must have been a very small one.

It is well known that the shock of the pile is remarkable for extending its influence but a short way along the arms; it affects the fore-arm rather than the arm; to convey it to the shoulder a considerable number of pairs must be requisite; and it is to be recollected that the pile is the weakest in its effects of all the forms of the voltaic battery. In Van Marum's case the shock was only equal to that of 100 pairs; and if this was felt at the shoulders, the fact only proves an uncommon degree of sensitiveness in the person. Perhaps when Van Marum stated that the shocks "extended to the shoulders with much force," he only meant "with much force making all due allowance for the weakness of the pile itself;" and in giving it this interpretation we must recollect that the statement is made in a complimentary letter from Van Marum to the illustrious inventor of the pile, Volta himself. The meaning will plainly appear to be this when his results are compared with those of Sir H. Davy, who had at his disposal the most powerful apparatus that had ever been constructed. Sir H. Davy's statement is on this account the most valuable. He used the enormous power of 2000 pairs of zinc and copper, each plate exposing 32 superficial inches of metal to the exciting liquid; the total surface being 128·000 square inches. The shock from such a battery would be dreadful, if not fatal: I can speak feelingly of the shock from 1000 pairs of new plates; anything more tremendous I could not conceive; it raised a large blister on one of the two parts that received it, and almost prostrated me; what must double the number be? Yet, wonderful to say, a Leyden battery charged by 2000 pairs gave a shock the force of which may be inferred from Sir H. Davy's expression, that "on making the proper connexion (with the hands), either a shock or a spark could be *perceived*." Thus the shock was merely perceptible; and this corresponds with my recollection of the shock from a battery charged by 1000 pairs: Volta himself represents the shock as *sensible*. It appears also that the spark was such that it could be merely "perceived." It should be observed here that any longer connexion than a momentary one, between the Leyden battery and the voltaic series, does not increase the charge of the former.

The trifling nature of the spark is shown strikingly by Mr. Gassiot's experiments: with a nine-gallon Leyden battery, charged by 1024 pairs of plates, that gentleman could only project a spark to a distance of $\frac{1}{5000}$dth of an inch.

In support of the inference here drawn of the very trifling nature of the shock, and the inconsiderable quantity of electricity which a Leyden battery is capable of communicating, when charged by a voltaic series, I shall detail an experiment lately made in the laboratory of the Royal Dublin Society, by Professor E. Davy and me. We used twenty Wedgwood-ware troughs, each containing ten cells; the number of plates was therefore 200 of zinc and the same of copper, each plate presenting a surface of 20 square inches on each side. The exciting liquid in each trough consisted of 140 ounces of water, $3\frac{1}{2}$ ounces of concentrated sulphuric acid, $1\frac{3}{4}$ ounce of nitric acid, and the same of muriatic acid, all taken by measure. When the battery was connected and put in action, the polar wires, armed with charcoal terminations, were brought in contact. An instantaneous burst of light, of dazzling splendour, announced that the battery was in high action. Having removed the charcoal terminations, we connected the polar wires with the inside and outside coatings of a Leyden battery of six jars, the total coated surface of which amounted to $12\frac{1}{3}$ square feet, and after two or three moments of contact, applied a discharging rod; but there was not the slightest spark; nor were several repetitions of the experiment attended by any better success. Having established the connexions as before, we tried to obtain a shock from the Leyden battery when it was detached, but neither of us experienced the slightest sensation after several trials.

We then proved that there was no defect in the Leyden battery, and that it was adequate to receive and retain the smallest charge, had there been any, by connecting it with a Nairne's electrical machine. It resulted that three turns of its cylinder were sometimes sufficient to afford a spark visible in day-light, from the Leyden battery; and that six turns were adequate to the communication of a very slight shock. This cylinder is $6\frac{3}{4}$ inches in diameter, the rubber is 8 inches in length; the conductors are each 3 inches in diameter and 12 inches in length. Thus it is a very small machine; its sparks vary from an inch to an inch and a quarter in length, are very dilute in appearance, and their snap feeble.

The experiment proved that three turns of this cylinder afforded from the Leyden battery a perceptible spark; hence whatever electricity the 200 pairs of plates had communicated to it must have been less, as it was altogether undiscoverable. A subsequent trial of the cylinder, made immediately after the experiments with the Leyden battery, proved that each revolution of the cylinder projected a spark of an inch in length. Thus the capability of the Leyden battery to manifest the presence of three one-inch sparks was proved; it was also proved that 200 pairs of plates did not communicate so much; and as six revolutions of the cylinder imparted to the Leyden battery such a quantity of electricity as gave a sensible shock, that shock was proved to be virtually occasioned by six one-inch sparks.

Our failure to obtain the results of Van Marum, or even of Sir H. Davy, is perhaps to be explained by the small size of the Leyden battery employed by us; it contained but $12\frac{1}{3}$ square feet, Van Marum's contained $137\frac{1}{2}$.

So far as these experiments with the Leyden battery warrant, I can see no evidence of great quantity of electricity; but even if there were, it can be shown that it would by no means support the notion of the enormous quantity of electricity which is said to constitute the current of the voltaic series, and that it would be quite foreign to the real question. Every one knows that there are two conditions of a voltaic series which are capable of producing very different results; viz. when the circuit is open, and when it is closed. In the open circuit, electricity is manifested which displays no chemical powers; Mr. Gassiot, with a well-insulated water battery of 320 pairs of plates, proved this; he connected one polar wire with the ground, and the other with paper moistened with solution of iodide of potassium; but there was not the least trace of decomposition, although we know that under such circumstances the electricity of the pole thus inactive is doubled in intensity, and will produce double the divergence in a gold-leaf electrometer; but when the polar wires were connected, the decomposition of the iodide was energetic*. On the other hand, when the circuit is closed, the chemical powers of the series are rendered evident, but the electrical appearances cease.

In all ordinary voltaic series, the smallest separation of the polar wires is an interruption of the circuit sufficient to cause cessation of the true voltaic action, and to restore the same electrical condition of the poles that existed before the connexion was made. Faraday found that the thinnest possible film of ice so perfectly interrupted the circuit that an interposed galvanometer was not affected.

When the polar wires of a voltaic series are connected, one with the inside of a Leyden battery and the other with the outside coating, the polar wires are not in contact, no circuit is formed, no chemico-voltaic action of the exciting liquid on the zinc takes place; hence there can be no more real voltaic action in operation than there is in any other case of an interrupted

* Philosophical Magazine, Oct. 1844, p. 290.

circuit. The polar wires are in contact with the two coatings of the jars; the two coatings are therefore now the real polar conductors, notwithstanding their extensive surface; but they are not in contact, for they are surrounded on all sides by an insuperable barrier of glass; they are in fact perfectly insulated. How is it possible that the Leyden battery can, under such circumstances, receive from an unclosed circuit a charge of what has a title to be considered the true current of the voltaic agent, and which can only circulate in the closed current?

The state of the case appears to be this. The opinion here impugned is, that the current is the condition of voltaic electricity which produces the phænomena, and that it consists of an enormous quantity of electricity in rapid flow. One of the proofs offered is, that a Leyden battery receives an immediate charge from a connexion with the voltaic series. But it has just been shown, that, during this connexion with the voltaic series, the current does not exist; hence the charge of the Leyden battery cannot be derived from it; and were the charge ever so great, it would prove nothing relative to the current. The charge merely shows that, in the unclosed circuit, free electricity is present, but that its quantity, at any moment, is in a position which even the supporters of the hypothesis in question deny, and justly, since it appears that the experiments of Sir H. Davy, Van Marum, Professor E. Davy and myself with the Leyden battery, evince that the quantity in the unclosed circuit is inconsiderable.

Indeed, it appears difficult to account for the shock given by a voltaic series at all according to received opinions. The shock of one or two thousand pairs of plates is tremendous and overpowering. The intensity is admitted to be exceedingly feeble, but the quantity is affirmed to be very great, and to its efficacy the shock is attributed. Abstracting from all other objections, the following seems to be of no small force.

That intensity is the efficient condition of electricity for giving a shock is shown by all known facts. If a person place one hand on the negative conductor of the largest electric machine, and his other hand on the positive one, the utmost power of the machine will not cause any sensation. But let him remove one hand to a distance of ten or twelve inches, retaining the other in its place, and he will obtain crooked sparks, every one of which will amount to a severe shock. At the distance of three or four inches he will be struck by a torrent of sparks which will be absolutely intolerable, if the electric machine possess great power. Yet here, the quantity of electricity is the same as when the two hands touched the conductors, but the intensity is very different: intensity gave powerful shocks, quantity did nothing. The intensity of a Leyden battery will kill an animal; but reconvert that intensity into quantity by causing the animal to draw off the charge by a sharp point, and no sensation will be experienced. If then intensity be thus proved to be the effective condition for giving the shock and spark, how is the fact reconcileable with the violent one which may be taken from a voltaic series in which the electric intensity is of the most feeble character?

The views which I suggested at the commencement of this essay, relative to the compound nature of the electric fluid, and the variable ratio of its constituent elements, would, in my opinion, accord better with the circumstances of the charge of a Leyden battery, by a voltaic series, than the assumed agency of quantity of electricity. Were I to offer an explanation, it would be based on the following notions.

That ordinary chemical agency developes electricity has long been known; combustion, evaporation, effervescence, and some other processes evolve this fluid. The solution of a metal in an acid is a case in point, and some instances have been supplied by Lavoisier and La Place* In one experiment some iron-filings were introduced into a wide-mouthed bottle, and sulphuric acid diluted with three parts of water was poured on. A brisk disengagement of hydrogen ensued, and in a few minutes the condenser of Volta became so highly charged with electricity that it afforded a brilliant spark.

Electricity of this kind has been always recognised as identical with common frictional electricity. In a voltaic series, a number of pieces of metal are subjected to the chemical action of an acid or some other menstruum; it is therefore quite an analogical fact that electricity of the ordinary kind is developed; but when one state of electricity is produced in one situation, the opposite state must exist somewhere near it. It is so in the voltaic series; the opposite poles are brought into opposite states and so maintained by some agency which I do not profess to comprehend, and which, notwithstanding the explanations hazarded by philosophers, seems as little understood as ever. In this state of electrical tension the poles remain, while the balance between dispersion and generation is preserved; such is the state of the open circuit.

But when the circuit is closed by connecting the poles, the case is very much altered; the two opposite states of electricity neutralize and destroy each other; hence all symptoms of free electricity cease, and a new set of phænomena are produced. A new mode of generation of electricity also seems to come into operation, constituting voltaic excitement; an electric fluid, which possesses more intense properties, is developed by the altered cir-

* *Mémoires de l'Académie Royale des Sciences*, 1781, p. 293.

cumstances of the chemical action now taking place, the result of which I conceive to be an alteration in the constitution of that agent. The cause or manner of this alteration I am no more able to assign than the supporters of the common hypothesis are to explain the difference between tension and current by the agency of quantity. The difference of properties between common and voltaic electricity is nevertheless so obvious, to all appearance, that it was formerly attributed to the operation of a distinct galvanic fluid or influence.

According to these views, the phænomena of the charged Leyden battery become perhaps a little more intelligible. When the connexion of the poles of the voltaic series is made with the coatings of the Leyden battery, the circuit still remains open, just as it did before the connexion. Ordinary electricity only is generated, as in the experiment of Lavoisier and La Place, and it gives a weak charge to the Leyden battery. This battery will accordingly give a common electric shock or spark, both of a very feeble kind, even although the battery be very large and the voltaic series extensive; but when the Leyden battery is removed, and the connexion between the two poles of the voltaic series is effected by the application of a wet hand to each, the circuit is at that moment closed by a good conductor; the true chemico-voltaic action of the exciting liquid therefore instantly takes place; the electric fluid, now altered by a change in the ratio of its constituent elements, is evolved, and a violent shock is received which is continued as long as the contact subsists. The moment the hands are removed, the true chemico-voltaic action ceases; the chemical action that succeeds is of the ordinary kind, and common electricity is evolved as at first.

This immediate charge of a Leyden battery is the circumstance from which the enormous quantity of electricity, affirmed to be generated by a voltaic series, derives its chief support. It is an interesting phænomenon; but if the foregoing reasonings be well-founded, the inquiry into that part of the subject is foreign to the question relative to the alleged identity, the real one being, not whether much electricity rapidly enters the Leyden battery, but whether that electricity is the cause of the phænomena which a voltaic series, not the Leyden battery, presents. If the shock given by the Leyden battery, charged by the voltaic series, be merely the effect of ordinary electricity, and not the same as the shock given by the series itself, it were beside our purpose to make any inquiries about it.

How the defenders of the electrical hypothesis of galvanism can acknowledge that the Leyden battery is charged with electricity from an unclosed circuit, and assume the fact as corroborative of their views, seems unaccountable, when they at the same time affirm that the current of electricity is only called into action when the circuit is closed, which is not the case in the instance under consideration.

Beside the foregoing objections against adducing the Leyden battery in support of the alleged efficiency of quantity to explain the difference between voltaic and ordinary electric phænomena, it may be worth while to make some further animadversions.

Various opinions have prevailed with regard to the nature of electricity: some suppose that it is an imponderable transferable elastic fluid; others that it consists of two such fluids; others that it is not a transferable elastic fluid, but vibrations of a stationary fluid; while others maintain that there is no fluid, and refer the phænomena to molecular vibrations of the electrified substance.

Be this as it may, those who believe in the existence of an elastic fluid, or two such, have explained electrical phænomena by positive and negative electricity. The passage of one or two electric fluids from one body to or through another, is called a current, and the idea of a current naturally enough involves the idea of quantity. It is thus that we talk familiarly of the enormous quantity of electricity which must circulate in a current, and which in an instant communicates a charge to a Leyden battery. All this is very intelligible, although not satisfactory, provided that such a fluid does really exist and that it flows in a current, but no one has ever been able to prove these conditions. In fine, when a Leyden battery is charged, we have no knowledge of its being filled with anything; all is conjecture: but a fostered hypothesis has produced a kind of common consent that it is full of a fluid. Now this position may be denied; and it has been questioned by no less a judge than Sir H. Davy, who inclined to the opinion that there is no specific fluid; and Faraday is far from discarding the same doubt. All arguments concerning the identity of voltaic and common electricity, founded on quantity and the instantaneous charge of a Leyden battery, must, in that view, at once fall to the ground, and the contemplated proof turn out a failure.

Those who adopt the opinion that electric phænomena are produced by vibrations of some elastic medium, must admit that as impulses communicated to elastic media will be transmitted through them with uniform velocity, a momentary contact of a Leyden battery with a voltaic series ought to throw the natural electricity of the former into a state of vibration similar to that of the electricity of the latter, in such a manner as air is supposed to be thrown when it is made the medium of sound. If the voltaic charge of the Leyden battery be of this vibratory kind, the wonder of its instantaneous communication is at an end, and the evidence in favour of the alleged identity derived from quantity is of no force. The adherents of the doctrine of identity may profess that their explanations of phænomena are independent of all hypothetical notions of two fluids, of one, or of none, or of vibrations. They may declare that they only use the language of these suppositions for convenience of communication. Let these persons, however, in conceiving the phænomena which they describe as resulting from an enormous quantity of electricity generated in the voltaic series, abstract from all notion of an elastic fluid, or two such, circulating in rapid currents, and then try if their minds be impressed with any real ideas when the words representing these ideas are thus deprived of the meaning which gave them currency in their reasonings.

In the early discussions which took place, relative to the identity of the electric fluid and the galvanic influence, the circumstance of a shock being communicated by both agents was deemed a strong corroboration on the affirmative side of the question; yet there is little force in the argument, as will appear from the following considerations. The word *shock* being used to express both sensations, the identity of the two agents is the more easily accredited. Let no one contumeliously deny the influence of language on his mind, for it is all-powerful. To judge by the sensation, it appears to me that the two shocks are totally different, although words will scarcely express the difference: sensations depend more on the nature of the organ in which they are induced than on the inducing cause. A blow on the orbital process gives the sensation of a flash of light; so does the electric or voltaic agent applied in the same quarter. A blow over the ulnar nerve at the elbow gives the same vibratory painful sensation as continued and rapid shocks from a very weak voltaic battery. Puncturing the lacrymal twig of the fifth nerve will produce a flow of tears, as will also emotions of the mind or pain. A voltaic current passed through the ear will affect the auditory nerve with the impression of loud noises. The semiparalytic state of a limb, when it is said to be asleep, resembles the vibration caused by a feeble electro-magnetic apparatus applied to the part. Volta produced an acid taste in the mouth by two small plates of different metals. A person who swallows vinegar affirms that it is sweet, if he have previously chewed the fruit of the shrub called Assaban. The peristaltic motion of the intestinal canal may be urged to dejections of its contents either by a voltaic current or by cathartics. He who unwittingly takes hold of a lump of frozen mercury, drops it, and declares he is burnt, and a blister will shortly appear on his fingers. Cantharides at length affect the skin like boiling water. The iris is dilated by the pressure of excessive blood in the head; so also is it by belladonna, by ardent spirit, or by the vapour of æther if breathed; but it is contracted by light or by opium in excess. Finally, in conformity with all these instances of manifold causes of the same effect, painful shocks may be produced by the action on the nerves of either the electric or voltaic agent, supposing them different; for in both cases these peculiar influences pass absolutely through the body; and it is most probable that, could any other influence be found which is capable of passing through the body with equal rapidity, it would excite the sensation of an electric shock.

Whether the agent which causes the shock of an electro-magnetic coil is different from all others, is a question which I shall not attempt to discuss: whether it is or is not different, the explanation of this shock is, in my opinion, irreconcileable with the received doctrine of the identity of the electric and voltaic agents, and it will be proper to state my reasons for coming to that conclusion.

A single voltaic combination of one square inch in surface, composed of zinc and platinum, cannot be made to affect the most sensible electrometer, nor to give the slightest sensation of a shock, nor the least appearance of a spark. But make a circuit with two very long copper wires lying closely together, but prevented from touching by interposed silk, and the apparatus becomes capable of communicating shocks that are absolutely insupportable: hundreds of such may be given in a minute by the coil apparatus now in common use, and brilliant sparks may also be obtained*. Professor Jacobi thus states his first attempt to repeat this experiment:—"Two copper wires 400 feet long and three-quarters of a line in diameter, carefully covered with silk ribbon, were coiled together in a helix round a hollow cylinder of wood one inch and a half in diameter; the ends of these two wires were united in a single one. The effect of this combination was beyond all my expectations; for by employing a voltaic pair of silver and zinc plates which had only a surface of *half a square inch*, I obtained at the moment of disjunction a brilliant spark, and a violent shock which could scarcely be borne. The same effects took place when the pair of plates was reduced to a wire of platina and zinc. After having placed a cylinder of soft iron in the hollow of the wooden cylinder, the action was still more considerable. The effects were not much increased by the enlargement of the surface of the pair†."

* Some of the modern discoveries on this subject were anticipated nearly half a century since in an observation made by Vassali-Eandi, one of the earliest cultivators of galvanism: "with a pile of fifty pairs he found that the fluid passed along a copper wire plated with silver 1151 feet in length, in a time incommensurable; the shock in this case was *three times as strong as that experienced by immediately touching the two extremities of the pile.*"—Philosophical Magazine, vol. xv. 1803.

† Scientific Memoirs, July 1837, p. 530.

Thus a wire of zinc and a wire of platinum can be made to give "a violent shock which can scarcely be borne," although they will give absolutely no signs of electricity to the most delicate electrometer. Can that shock then depend on electricity? It is hard to conceive a more persuasive fact in support of the position already advanced, that there are other kinds of shocks, or at least one other kind, beside an electric shock. Can it be believed that two bits of wire can thus evolve such a powerful charge of electricity as to give so tremendous a shock; and that this most feeble of all intensities, absolutely inappreciable by our most delicate instruments, could be capable of such an effect, were the quantity of electricity ten times what it is presumed to be, or what the most exuberant imagination can conceive? It need not be reverted to that it is not quantity of electricity, unless it be at a high intensity, that gives a shock; and it is to be observed that if the shock were derived from quantity alone, the two wires employed by Jacobi should be adequate by themselves without the coil; and large plates in a voltaic series should be proportionately more powerful than small ones, which is not a fact. Common sense would also point out that two wires which are capable of no more than convulsing the limbs of a frog, must be inadequate to give a violent shock to a man without some adscititious agent.

Having procured 120 feet of copper bell-wire well covered with sewing silk, I connected it with the positive prime conductor of an electrical machine then giving sparks twelve inches long. The wire was spread out round the room, and supported everywhere on insulators. The cylinder being put in action, I placed one hand on the negative conductor, and repeatedly made and broke contact with the end of the wire; but the electricity was reduced to the most feeble manifestations, scarcely affording a spark; and nothing in the least degree resembling a shock could be obtained, although without the wire the twelve-inch spark was as much as could be well endured. I thought myself entitled to a result as striking as that of Professor Henry, who, with 120 feet of *uncoated* wire and a single pair of plates, obtained a spark of maximum brilliancy, although with fifteen feet of wire the spark was barely visible; but, on the contrary, my sparks, instead of being increased by a *coated* wire, were reduced from twelve inches to almost nothing. Could the agent be the same? Another of Professor Henry's results is still more instructive. With the same pair of plates and a ribbon of sheet-copper 96 feet long and an inch and a half wide, covered with silk and coiled into a spiral, vivid sparks were produced of such size and power that the snaps occasioned by them "could be distinctly heard in an adjoining room." The coiled ribbon was also found capable of giving a shock felt at the elbows*.

* Scientific Memoirs, July 1837, p. 543.

All these facts, and many others which could be adduced, seem to render it highly probable, if not to prove, that the peculiar sparks and shocks occasioned by voltaic series are caused by an agent of a different nature from that which produces ordinary electrical phænomena. And further, reasons in my opinion sufficient, have been assigned for doubting the force of the evidence derived from the so-called immediate charge of a Leyden battery by a voltaic series, as proving the vast quantity of electricity which constitutes the voltaic current, and the identity of the agent in all electrical and voltaic phænomena.

[To be continued.]

LXIV. *On the possibility of solving Equations of any degree however elevated. By* G. B. JERRARD, *Esq.**

§ 1.

THERE is little difficulty in the theory of the solution of equations beyond those of the fifth degree. By following a method analogous to the one which, in No. 45 of my "Reflections on the Resolution of Algebraic Equations of the Fifth Degree†," brought us to a class of equations solved by Abel, we should always in our progress find ourselves conducted to a corresponding class of solvable equations of degrees more and more elevated. Various other methods, all leading to the same conclusion, might here be readily pointed out. But I am constrained, in the first place, to turn my attention to the particular classes of equations just alluded to, in order to consider an objection which, by some eminent mathematicians of the present day, is supposed to affect the validity of the method of solution given by Abel.

§ 2.

Passing to the 3rd section of Abel's *Mémoire sur une classe particulière d'Equations résolubles algébriquement*‡ (for it is against the process contained in this part of his memoir that the objection is mainly directed), we there find that illustrious mathematician maintaining that every equation of the μth degree, $\phi x=0$, the roots of which may be expressed by

$$x_1,\ \theta x_1,\ \theta^2 x_1,\ \ldots\ \theta^{\mu-1}x_1,$$

wherein θx_1 designates a rational function of x_1, will admit of being solved algebraically.

* Communicated by the Author.
† See this Journal for June 1845, vol. xxvi. p. 573.
‡ Crell's Journal, vol. iv.

Supposing α to be any root of the binomial equation $\alpha^{\mu}-1=0$, and ψx to be defined by

$$\psi x = (x+\alpha\theta x+\alpha^2\theta^2 x+ \, .. \, +\alpha^{\mu-1}\theta^{\mu-1}x)^{\mu}, \quad . \quad . \quad (1)$$

he states, as the first proposition to be proved, that ψx, which is obviously a rational function of x, must further admit of being expressed rationally by the coefficients of ϕx and θx. He then substitutes $\theta^m x$ for x in the expression for ψx, and combining the equation

$$\theta^{\mu+\nu}x=\theta^{\nu}x$$

with

$$\alpha^{\mu+\nu}=\alpha^{\nu},$$

he shows very clearly that

$$\psi\theta^m x=\psi x;$$

and thence

$$\psi x=\frac{1}{\mu}\{\psi x+\psi\theta x+\psi\theta^2 x+ \, .. \, +\psi\theta^{\mu-1}x\} : \quad . \quad (2)$$

from which he infers *that ψx will be a rational and symmetric function of all the roots of the equation $\phi x=0$, and will therefore be expressible rationally in known quantities.* It is this inference the truth of which has been contested. But his meaning has here, as we shall see, been misapprehended. It may be briefly explained thus. The expression for ψx, which, when considered as a function of the coefficients of ϕx and θx, may take the form $M_0+M_1x+M_2x^2+ \, .. \, +M_{\mu-1}x^{\mu-1}$ (wherein M_0, M_1, M_2, .. $M_{\mu-1}$ are certain rational functions of the two sets of coefficients in question), will, in virtue of equation (2), be subject to the condition

$$M_0+M_1x+M_2x^2+ \, .. \, +M_{\mu-1}x^{\mu-1}=$$
$$\frac{1}{\mu}\{\mu M_0+M_1\mathfrak{S}(1)+M_2\mathfrak{S}(2)+ \, .. \, M_{\mu-1}\mathfrak{S}(\mu-1)\}$$

if

$$\mathfrak{S}(n)=x^n+(\theta x)^n+(\theta^2 x)^n+ \, .. \, +(\theta^{\mu-1}x)^n;$$

and will consequently become M_0. For, since the proposed equation $\phi x=0$ is irreducible, the quantities M_1, M_2, .. $M_{\mu-1}$ must separately vanish.

§ 3.

It is evident that, except in the case of $\mu=2$, M_1, M_2, .. $M_{\mu-1}$ will not vanish of themselves, independently of the particular form of the proposed equation $\phi x=0$.

If, for instance, we take $\mu=3$, remembering that the roots of the equation $\alpha^3-1=0$ are 1, $-\frac{1}{2}+\frac{1}{2}\sqrt{-3}$, $-\frac{1}{2}-\frac{1}{2}\sqrt{-3}$, we shall have

$$\begin{aligned}\psi x &= (x+\alpha\theta x+\alpha^2\theta^2 x)^3\\ &= \xi_0+\alpha\xi_1+\alpha^2\xi_2\\ &= \xi_0-\frac{1}{2}(\xi_1+\xi_2)+\frac{\sqrt{-3}}{2}(\xi_1-\xi_2);\end{aligned}$$

if

$$\alpha=-\frac{1}{2}+\frac{1}{2}\sqrt{-3},$$

and therefore

$$\alpha^2=-\frac{1}{2}-\frac{1}{2}\sqrt{-3}.$$

ξ_0, ξ_1, ξ_2 of course do not explicitly involve α. In effect,

$$\begin{aligned}\xi_0 &= x^3+(\theta x)^3+(\theta^2 x)^3+6x\theta x\theta^2 x,\\ \xi_1 &= 3\{x^2\theta x+(\theta x)^2\theta^2 x+(\theta^2 x)^2 x\},\\ \xi_2 &= 3\{x^2\theta^2 x+(\theta x)^2 x+(\theta^2 x)^2\theta x\}.\end{aligned}$$

Now as the expression just obtained for ψx must be capable of being transformed into $M_0+0x+0x^2+\,..\,+0x^{\mu-1}$, we may at once perceive that the proposed cubic equation will necessarily be such, that the non-symmetric function of its roots, which is represented by $\xi_1-\xi_2$, shall not involve x.

Accordingly we must have

$$\begin{aligned}\xi_1 &= a_1+b_1 x+b_2 x^2,\\ \xi_2 &= a_2+b_1 x+b_2 x^2;\end{aligned}$$

a_1, a_2, b_1, b_2 being independent of x*.

Hence I conclude that the equation $\phi x=0$ will, when $\mu=3$, be subject to the condition

$$\xi_1-\xi_2=a_1-a_2. \quad . \quad . \quad . \quad . \quad . \quad . \quad (\xi)$$

And a similar result might be obtained for any value of μ greater than 3.

§ 4.

Legendre has indeed been led, by some remarkable researches on the class of equations we have been considering, to infer that the roots of the general equation of the third degree, x, x', x'', may be deduced from the successive equations

$$x'=\frac{a+bx}{1+cx},\quad x''=\frac{a+bx'}{1+cx'},\quad x'''=\frac{a+bx''}{1+cx''};$$

x''' being equal to the primitive root x. (See his *Théorie des*

* It might be seen from other considerations whether b_1 and b_2 will both of them vanish. But for the purpose in the text no question arises respecting their evanescence.

Nombres, 3rd edition, vol. ii. p. 438.) But he has overlooked the existence of the equation of condition (ξ), without which, essentially linked as it is with the irreducibility of the proposed equation, such a system of successive equations could not exist. It appears that Legendre was not himself aware that there was any antagonism between the results at which he had arrived and those of Abel. If, however, the non-existence of the condition (ξ) could without error be assumed, the objection of the learned author of the treatise on the Calculus of Functions in the *Encyclopædia Metropolitana* (p. 382) would undoubtedly be applicable to Abel's method.

Long Stratton, Norfolk,
April 14, 1852.

[To be continued.]

LXV. *Observations on a New Theory of Multiplicity.* *By* J. J. Sylvester, *Barrister-at-Law*.*

IN the Postscript to my paper in the last Number of the Magazine, I mis-stated, or to speak more correctly, I understated the law of Evection applicable to functions having any given amount of distributive multiplicity. The law may be stated more perfectly, and at the same time more concisely, as follows. Every point represented by the coordinates $\alpha_1, \beta_1, \ldots \gamma_1$, for which the multiplicity is m_1, will give rise in *every evectant*† of the discriminant of the function to a factor $(\alpha_1 x + \beta_1 . y + \ldots \gamma_1 z)^{m_1 . n}$, ($n$) being supposed to be the degree of the function. Hence if there be r such points, for which the several multiplicities are $m_1, m_2, \ldots m_r$, every evectant must contain $(m_1 + m_2 + \ldots + m_r) . n$ linear factors; and as the ιth evectant is of the degree $\iota . n$, it follows that all the evectants below the $(m_1 + m_2 + \ldots + m_r)$th evectant must vanish completely, and this Evectant itself be con-

* Communicated by the Author.

† Frequent use being made in what follows of the word Evectant, I repeat that the evectant of any expression connected with the coefficients of a given function (supposed to be expressed in the more usual manner with letters for the coefficients affected with the proper binomial or polynomial numerical multipliers) means the result of operating upon such expressions with a symbol formed from the given function by suppressing all the binomial or polynomial numerical parts of the coefficients to be suppressed, and writing in place of the literal parts of the coefficients a, b, c, &c. the symbols of differentiation $\frac{d}{da}, \frac{d}{db}, \frac{d}{dc}$, &c.; in all that follows it is the successive evectants of the discriminant alone which come under consideration. I need hardly repeat, that the discriminant of a function is the result of the process of elimination (clear from extraneous factors) performed between the partial differential quotients of the function in respect to the several variables which it contains, or to speak more accurately, is the characteristic of their coevanescibility.

tained as a factor in all above it*. When a function of only two variables is in question, there is no difficulty in understanding what property of the function it is which is indicated by the allegation of the existence of multiplicities $m_1, m_2, \ldots m_r$; as already remarked, this simply means that there are r distinct groups of equal roots, such groups containing $1+m_1, 1+m_2, \ldots 1+m_r$ roots respectively. So for curves and higher loci, the total distributive multiplicity is the sum of the multiplicities at the several multiple points. But the true theory of the higher degrees of multiplicity separately considered at any point remains yet to be elaborated, and will be found to involve the consideration of the theory of elimination from a point of view under which it has never hitherto been contemplated.

Confining our attention for the present to curves, we have a clear notion of the multiplicity 1: this is what exists at an ordinary double point. As well known, its analytical character may be expressed by saying that the function of x, y, z, which characterizes the curve, is capable, when proper linear transformations are made, of being expanded under the form of a series descending according to the powers of z, such that the constant co-coefficient of the highest power of z, and the linear function of x, y, which is the coefficient of the next descending power of z, may both disappear. Again, when the multiplicity is 2, the 3rd coefficient, which is a quadratic function of x and y, will become a perfect square. This is the case of a cusp, which, as I have said, is the precise analogue to that of three equal roots for a function of two variables. Before proceeding to consider what it is which constitutes a multiplicity 3 for a curve, it will be well to pause for a moment to fix the geometrical characters of the ordinary double point and the cusp.

If we agree to understand by a first polar to a curve the curve of one degree lower which passes through all the points in which the curve is met by tangents drawn from an arbitrary point taken anywhere in its own plane, we readily perceive that at an ordinary double point all the infinite number of first polars which can be drawn to the curve will intersect one another at the double point. Again, at a cusp all these polars will not only all intersect, they will moreover all touch one another at the cusp. Now we may proceed to inquire as to the meaning of a multiplicity of the third degree, which, strange to say, I believe has never yet been distinctly assigned by geometricians.

This is not the case of a so-called triple point, *i. e.* a point

* The constitution of the quotients obtained by dividing all the other evectants of the discriminant by the first non-evanescent one, presents many remarkable features which remain yet to be fully studied out, and promise a wide extension of the existing theory.

where three branches of the curve intersect. Supposing $x=0$, $y=0$, to represent such a point, the characteristic of the curve must be reducible to the form $(gx^3+hx^2y+kxy^2+ly^3)z^{n-3}+\&c.$, which, as is well known, involves the existence of four conditions. This, however, would not in itself be at all conclusive against the multiplicity at a triple point being only of the third degree; for it can readily be shown that there may exist singular points of any degree of *singularity* (as measured by the number of conditions necessary to be satisfied in order that such singularity may come into existence), but for which the multiplicity may be as low as we please; as, for instance, if at a double point (which is not a cusp) there be a point of inflexion on one branch or on both, or a point of undulation, or any other singularity whatever, still provided there be no cusps, the multiplicity will stick at the first degree and never exceed it; for only the discriminant itself will vanish on these suppositions, but no evectant of the discriminant. The reason, on the contrary, why a so-called triple point must be said to have a multiplicity of the degree 4, and not merely of the degree 3, springs from the fact that the 1st, 2nd, and 3rd evectants of the discriminant all vanish at such a point.

It is clear, then, that there ought to exist a species of multiplicity for which the 1st and 2nd evectants vanish, but not the 3rd. In fact, as at a double point the first polars all merely intersect, but at a cusp have all a contact with one another of the first degree, so we ought to expect that there should exist a species of multiple point such that all the first polars should have with each other a contact of the second degree (or if we like so to say, the same curvature) at that point. When the curve has a triple point, all its first polars will have that point upon them as a double point; and it is not at the first glance, easy *à priori* to say what is the nature of the contact between two curves which intersect at a point which is a double point to each of them: we know upon settled analytical principles, that when one curve having a double point is crossed there by another curve not having a double point, that the two must be said to have with one another, a contact of the 1st degree; and we now learn from our theory of evection, that if each have a double point at the meeting-point, the degree of the contact must from principles of analogy be considered to be of the 3rd degree*. Now, then, we come to the question of deciding definitely what is a multiple point for which the degree of multiplicity is 3. It is, adopting either test, whether

* This may easily be verified by direct analytical means; as also the more general proposition, that two curves meeting at a point where there are (m) branches of the one and (n) branches of the other, must be considered to have mn coincident points in common, *i. e.* if we like so to express it, to have a contact of the degree $mn-1$.

of first polar contact or of evection, a cusp situated or having its *nidus*, so to say, at a point of inflexion. In other words, $x=0$, $y=0$ will be a point whose multiplicity is intermediate between that of the cusp and that of a so-called triple point, when the characteristic of the curve admits of being written under the form

$$z^{n-2}x^2+z^{n-3}(gx^3+hx^2y+ixy^2)+z^{n-4} \text{ \&c.};$$

or in other words, when over and above the vanishing of the constant and linear coefficients, and the quadratic coefficient being a perfect square, as in the case of an ordinary cusp, this square has a factor in common with the next (the cubic) coefficient; or again, in other words, a curve has a point for which the multiplicity is 3 when its characteristic function admits of being expanded according to the powers of one of the variables, in such a manner that the first coefficient and the second (the linear) coefficient vanish, and that the discriminant of the third and the resultant of the third and fourth are both at the same time zero. This being the case, it may be shown that the first polars will all have with each other a contact of the second degree; and moreover, that all the evectants of the discriminant will have as a common factor a linear function of the variables, raised to a power whose index is 3 times that of the characteristic function. As, then, there is but one kind of ordinary double point, and but one kind of point with multiplicity 2, so there is one, and only one, kind of point with a multiplicity 3. A cusp is a peculiar double point; a flex-cusp (as for the moment I call the point last above discussed) is a peculiar cusp. This law of unambiguity, however, appears to stop at the third degree. A so-called triple point (which ought in fact to be called a *quintuple* point) is a point for which the multiplicity, as shown above, is of the fourth degree; but it is not the only point of that degree of multiplicity. Without assuming to have exhausted every possible supposition upon which such a degree of multiplicity may be brought into existence, it will be sufficient to take as an example a curve whose characteristic is capable of assuming the form

$$z^{n-2}.x^2+z_{n-3}(gx^3+hx^2y)+z^{n-4}.(kx^4+lx^3y+mx^2y^2+nxy^3)+z^{n-5}\text{\&c.}$$

It may readily be demonstrated that the first polars of this curve have all with one another at the point x, y a contact of a degree exceeding the 2nd, *i. e.* of at least the 3rd degree (and, I believe, in general not higher). Now the point x, y is evidently not a triple-branched point, but a cusp with three additional degrees of singularity; so that we have evidence of the existence of a point whose degree of singularity is 5, and whose multiplicity is at least 4, but which is in no sense a modified triple point. It is probably true (but to demonstrate this requires a further advance to be made than has yet been realized in the theory of the constitution of discriminants) that a cusp may be so modified by the *nidus* at which it is posited, as, without ever passing into a triple point, to be capable of furnishing any amount of multiplicity whatever, curiously in this contrasting with an ordinary double point, no amount whatever of extraordinary singularity imparted to which, or so to speak, to its *nidus*, can ever heighten its multiplicity so as to make it surpass the first degree without first converting it into a cusp. I may illustrate the nature of a flex-cusp by what happens to a curve of the third degree. When it breaks up into a line and a right line, there are two ordinary double points; for the existence of these double points, as for the existence of a cusp, two conditions are required. When, however, the right line and conic touch one another (a *casus omissus* this in the works of the special geometers), the characters of the cusp and the point of inflexion are combined at the point of contact; the multiplicity is of the third degree, and the singularity also of a degree not exceeding this; three conditions only being necessary to be satisfied in order that a given cubic may degenerate into such a form; and it will be found that the discriminant and the first and second evectants thereof vanish for this case, and that the 3rd evectant of the discriminant will be a perfect 9th power; whereas in order that the cubic may have a so-called triple point, *i. e.* may degenerate into a trident of diverging rays, four conditions must be satisfied, and it will be found that when this is the case, the first, second, and third evectants of the discriminant will all vanish, and the fourth will be a perfect 12th power of a linear function of the variables. I may mention, by the way, at this place, that the law of a discriminant and the successive evectants up to the mth inclusive, all vanishing, may be expressed otherwise (not in *identical*, but in *equivalent* or *equipollent* terms), by saying that the discriminant and all its derivatives of a degree not exceeding the mth will all vanish—understanding by a derivative of the discriminant any function obtained from the discriminant by differentiating it any specified number of times with respect to the constants of the function to which it belongs, the same constants being repeated or not indifferently*. And very surprising it must be allowed to be, stated as a bare analytical fact, that $(m+1)$ conditions imposed upon the coefficients of a function of any number of variables and of any degree should suffice to make the inordinately greater number of functions which swarm among the derivatives of the mth and inferior degrees of the discriminant each and all simultaneously vanish.

* Or, to speak more simply, the discriminant and its successive *differentials* up to the mth exclusive must all vanish simultaneously.

Without pushing these observations too far for the patience of the general reader, it may be remarked by way of setting foot with our new theory upon the almost unvisited region of the singularities of surfaces, that by the light of analogy we may proceed with a safe and firm step as far as multiplicity of the third degree inclusive.

The function characteristic of the surface being supposed to be expressed in terms of the four variables x, y, z, t, and expanded according to descending powers of t, then when x, y, z is an ordinary double point of the first degree of multiplicity, the constant and the linear coefficient disappear; when the point has a multiplicity 2, the discriminant of the quadratic coefficient will be zero, *i. e.* this coefficient will be expressible by means of due linear transformations under the form of x^2+y^2; and when the multiplicity is to be of the degree 3, the cubic coefficient will, at the same time that the quadratic coefficient is put under the form x^2+y^2 itself (for the same system of x and y assume the form of a cubic function of x, y, z, in which the highest power of z, *i. e.* z^3, will not appear); or in other words (restoring to x, y, z their generality), not only will the first derivatives of the quadratic function be nullifiable simultaneously with each other, but likewise at the same time with the cubic function itself. These three cases will be for surfaces, the analogues so far, but only so far as regards the degree of the multiplicity, to the double point, cusp, and flex-cusp of curves*. The analogue to the so-called triple point of the curves will be a point whose degree of singularity (depending upon the vanishing of the six constants in the 3rd coefficient (which is a quadratic function of x, y, z) at the same time as the three constants in the linear factor) would seem to be but 6 more than for a double point, *i. e.* in all $1+6$ or 7, but whose multiplicity, as inferred from the nature of the contact of its first polars, which will be of the 7th order, would appear to be 8 (a seeming incongruity which I am not at present in a condition to explain)†; so that there will apparently be 4

* At an ordinary conical point of a surface for which the multiplicity is 1, every section of the surface is a curve with a double point. When the multiplicity is 2, the cone of contact becomes a pair of planes, through the intersection of which any other plane that can be drawn cuts the surface in a section having an ordinary cusp of multiplicity 2, but which themselves cut the surface in sections, having so-called triple points, so that for these two principal sections (which is rather surprising) the multiplicity suddenly jumps up from 2 to 4. All other things remaining unaltered when the multiplicity of the conical point is 3, the cusp belonging to any section of the surface drawn through any intersection of the two tangent planes passes from an ordinary cusp to a flex-cusp.

† So, too, at a so-called quadruple point in a curve, the degree of the contact of the 1st polars is 8, and therefore the multiplicity of the curve at such point is 9; but the number of constants which vanish for this case

steps of multiplicity to interpolate between this case and the case analogous (*sub modo*) to the flex-cusp, last considered. Whether these intervening degrees correspond to singularities of an unambiguous kind, no one is at present in a condition to offer an opinion. I will conclude with a remark, the result of my experience in this kind of inquiry as far as I have yet gone in it, viz. that it would be most erroneous to regard it as a branch of isolated and merely curious or fantastic speculation. Every singularity in a locus corresponds to the imposition of certain conditions upon the form of its characteristic; by aid of the theory of evection we are able to connect the existence of these conditions with certain consequences happening to the form of the discriminant, and thereby it becomes possible, upon known principles of analysis, to infer particulars relating to the constitution of the discriminant itself in its absolutely general form, very much upon the same principle as when the values of a function for particular values of its variable or variables are known, the general form of the function thereby itself, to some corresponding extent, becomes known. Thus, for instance, I have by the theory of evection in its most simple application, been led to a representation of the discriminant of a function of two variables under a form very different and very much more complete and fecund in consequences than has ever been supposed, or than I had myself previously imagined to be possible.

According to the opinion expressed by an analyst of the French school, of pre-eminent force and sagacity, it is through this theory of multiplicity, here for the first time indicated, that we may hope to be able to bridge over for the purposes of the highest transcendental analysis, the immense chasm which at present separates our knowledge of the intimate constitution of functions of two from that of three, or any greater number of variables.

It is, as I take pleasure in repeating, to a hint from Mr. Cayley*, who habitually discourses pearls and rubies, that I am indebted

(viz. all those of the cubic coefficient in x, y) over and above what vanish for the case of a so-called triple point is only 4, which is a unit less than the difference between the measures of the multiplicities at the respective points; and this difference continues to increase as we pass on to so-called quintuple and higher multiple points in the curves.

* Mr. Cayley's theorem stood thus:—If

$$ax^n+nbx^{n-1}.y+\&c.+nb.xy^{n-1}+a'y^n$$

have two equal roots, and π be its discriminant, then will

$$\left\{y^n.\frac{d}{da}-x^{n-1}.y\frac{d}{db}\&c.\pm x^n.\frac{d}{da}\right\}\pi$$

be a perfect nth power. It will easily be seen that this theorem is convertible into a theorem of evection by interchanging in the result x and y with y and $-x$.

for the precious and pregnant observation on the form assumed by the first discriminantal evectant of a binary function with a pair of equal roots, out of which, combined with some antecedent reflections of my own, this new theory of multiplicity has taken its rise. The idea of the process of evection, and the discovery of its fundamental property of generating what, in my calculus of forms (Camb. and Dub. Math. Journ.), I have called *contravariants,* is due to my friend M. Hermite. The polar reciprocals of curves and other loci are contravariants and, as I have recently succeeded in showing, for curves at least, evectants but of course not discriminantal evectants; and I am already able to give the actual explicit rule for the formation of the polar reciprocal of curves as high as the 5th degree, which with a little labour and consideration can be carried on to the 6th, and in fact to curves of any degree (n) when once we are acquainted with any mode of determining all such independent invariants of a function of two variables as are of dimensions not exceeding $2(n-1)$ in respect of the coefficients.

By the special geometers (by whom I mean those who, unvisited by a higher inspiration, continue to regard and to cultivate geometry as the science of mere sensible space) this problem has only been accomplished, and that but recently, for curves whose degrees do not exceed the 4th. Mr. Salmon has made the happy and brilliant (and by the calculus of forms instantaneously demonstrable) discovery, communicated to me in the course of a most instructive and suggestive correspondence, that *a certain readily ascertainable evectant of every discriminant of any function whatever is an exact power of its polar reciprocal**.

I believe that it may be shown, that, with the sole exception of odd-degreed functions of two variables, the *polar reciprocal itself* (as distinguished from a power thereof) of every function is an evectant, not (of course) of the discriminant, but of some determinable inferior invariant.

26 Lincoln's-Inn-Fields,
May 14, 1852.

P.S. The terms pluri-simultaneous and pluri-simultaneity, used or suggested by me in my last paper in the Magazine, may be advantageously replaced by the more euphonious and regularly formed words consimultaneous, consimultaneity. Multiplicity and all its attributes and consequences are included as particular cases in the general conception and theory of consimultaneity, *i. e.* of consimultaneous equations, or, which is the same thing, of consimulevanescent functions.

* Viz. for a function of degree n, and variability (*i. e.* having a number of variables) p, the $(n-1)^{p-1}$th evect of the discriminant is the $(n-1)$th power of the polar reciprocal.

LXVI. *Notices respecting New Books.*

History of Physical Astronomy, from the earliest Ages to the middle of the Nineteenth Century. By Robert Grant, *F.R.A.S.* London: Baldwin. 8vo. (pp. 635.)

ABOUT eighteen months ago, in the continuation of the Library of Useful Knowledge undertaken by Mr. Baldwin, appeared the first number of a History of Physical Astronomy, by one Robert Grant, who was then wholly unknown. To write history on this subject was an attempt of the most ambitious kind; first, because no connected and consecutive history had ever been written; secondly, because such a thing would require a large amount of mathematical reading of the highest order; and thirdly, because the historical materials exist in great part among the long series of memoirs of academies, which are not very easy to get at, and are very troublesome to master. We say nothing of the many questions which demand the highest judgement; because we are speaking only of the difficulties which no amount of self-confidence could ignore or even materially underrate. It was, we have no doubt, to these difficulties that the want of such a history was due: and we think it probable that many took up the first number of the work before us with the impression that the genius of book-making must have been very hard put to it for materials, before he could have suggested the theory of gravitation, its mathematical aspect inclusive, as a subject of history for a popular series. But it was found, on examination, that the work bore evident marks of original reading, high mathematical knowledge, sound judgement, and careful writing: and it made some sensation in the astronomical world, that there should be any person in the country who had so mastered the subject, without first becoming known by some minor effort, in the usual way. During the publication of the numbers, Mr. Grant's name, which had at first been "spelt by th' unletter'd muse," acquired the four suffixes which stand at the head of our article. And the work is now as well established among the greater efforts of scientific history, as it could have been if the author had been previously known, and it had been waited for with the usual amount of announcement and previous discussion of its probable character. And, though it includes some subjects which proceed by the highest mathematics, it is nevertheless very popular in its requirements from the reader. On this point Mr. Grant would have deserved high commendation, though he had been only a compiler. It seems that the plan was at first of a limited character, but that it expanded during the execution. To this it is due that the words of the title-page, 'from the earliest ages,' are supported only by an introductory chapter, which is faultless as an introduction, but insufficient as a component part.

It does not lower Mr. Grant's credit, but very much raises it, that such an achievement as his proves the history of science to be in no forward state; for those things which are left open for any one to do who will, are generally those which there are few who can do. There did not exist any connected history of the whole theory of gravitation. The extent to which the subject is treated by Montucla is rather what might have been expected in a general history of mathematics than what is due to the subject. Still less can we find anything consecutively historical, and reaching to our own time, on the matters which constitute what is properly called *physical* astronomy.

Astronomy ought to be divided into geometrical, mechanical, and physical. To the first belongs all that concerns the determination of the actual places and motions of the heavenly bodies, without reference to their action on each other; together with all that relates to the use of such knowledge in the determination of latitude, longitude, and time. To mechanical astronomy (*Mécanique Céleste*) belongs the consideration of force or attraction, as an immediate maintaining cause of the order observed; being, in the widest sense, the theory of gravitation. To physical astronomy belongs all that is not geometrical nor mechanical; including the consideration of all optical phænomena except change of place, &c. By a curious misnomer, which we believe is due to Woodhouse, the term *physical astronomy* has been exclusively applied, in this country, to the theory of gravitation and its consequences. Mr. Grant has included this theory, together with a great deal of what is more properly called physical, in his valuable work.

Delambre, as is well known, did not treat the question of the progress of Newton's system. His article on Newton, in the *Astr. au 18ième Siècle*, shows plainly that he did not feel himself at home. He, usually the independent, stern, and sententious judge, there rests on Clairaut as on a staff; and seems happy when he can escape among the spherical triangles. We do not suppose that Delambre had ever paid much attention to mechanical astronomy, except to receive its results for use. He was above all men who ever wrote in his knowledge of the history of geometrical astronomy, and in his familiarity with the processes of all time. But he left the mechanical field quite open. Bailli, who is to a greater extent the historian of this last subject, has not been so much read as he deserved, which arises from the disadvantageous impression created by his ancient fictions and his Indian exaggerations. The third volume of his modern history, and the continuation by Voiron, formed, previously to Mr. Grant's publication, the most extensive separate history of the theory of gravitation. The *précis* of Laplace, and the historical summaries in the fifth volume of the *Mécanique Céleste*, are not for the general reader, even if a mathematician. M. Narrien's historical account of the origin and progress of astronomy, an excellent work, hardly goes beyond the time of Newton, except in a very summary manner. The history of astronomy already in the Library of Useful Knowledge, written by Dr. Rothman, was a valuable accession to the means of the English reader; but it does not touch the main subjects of the present work in any detail. Thus it will appear that Mr. Grant has chosen a field in which he has had no immediate predecessor.

There are many works as to which it is the object of our notices to put before the reader such an account as will enable each one to decide for himself on the expediency of consulting them. But in the present case, no such object is in view. Mr. Grant's book takes its place among standard works from its first appearance, by common consent; partly on account of the vacancy of the field, but more because the author is an historian from original materials, of good knowledge, good judgement, and good style. He is no strong partisan of anything or anybody; and he gives such accounts as those of the dispute between Flamsteed and Newton, or the discussion upon the discovery of Neptune, in a manner which inclines us to feel safe in his hands upon matters in which we have not consulted his originals. The work is brought up to the present time throughout; and we should have given a more detailed account of it, if we had not felt quite confident that it must, and speedily, not only be in the hands of all who are already interested in the history of astronomy, but awake much attention to that subject in others.

LXVII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 392.]

March 4, 1852. — A PAPER was read, entitled, "On the Anatomy of *Doris*." By Albany Hancock, Esq., and Dennis Embleton, M.D., Lecturer on Anatomy and Physiology in the Newcastle-on-Tyne College of Medicine, in connection with the University of Durham. Communicated by Professor E. Forbes, F.R.S.

The authors have proposed to themselves to describe the anatomy of the three genera typical of the three groups of the Nudibranchiate Mollusca. An account of the structure of *Eolis* has already appeared in the 'Annals of Natural History.'

A detailed description is given of the anatomy of *Doris*, the following species of which have been examined, and are referred to in the paper: *D. tuberculata*, Auct., *D. tuberculata*, Verany, *D. Johnstoni*, *D. tomentosa*, *D. repanda*, *D. coccinea*, *D. verrucosa*, *D. pilosa*, *D. bilamellata*, *D. aspera*, and *D. depressa*; but *D. tuberculata* of English authors has been taken as the type of the genus, and the standard of comparison for the rest.

Digestive System.—The mouth in all the species is a powerful muscular organ, provided with a prehensile tongue beset with siliceous spines, which when the tongue is fully developed, are arranged in a median and two lateral series. Certain species possess, besides, a prehensile spinous collar on the buccal lip, occasionally associated with a rudimentary horny jaw. The mode of development of the lingual spines is shown to be the same as that of the teeth of the Vertebrata.

The œsophagus varies in length; in some it is dilated at the top, forming a crop; in others it is simply enlarged previously to entering the liver mass. *The stomach* is of two forms; one, as in *D. tuberculata*, is very large, receiving the œsophagus behind, and giving off the intestine in front, and lying in advance of the liver; the other is received within the mass of the liver, and is very small. *The liver* in all is bulky, mostly bilobed, and variously coloured, and pours its secretion by one or more very wide ducts into the cardiac end of the stomach. A small laminated pouch—a rudimentary *pancreas*, is attached in some species to the cardiac, in others to the pyloric end of the stomach. The *intestine* is short, of nearly the same calibre throughout, rather sinuous in its course, and terminates in a nipple-formed anus in the centre of the branchial circle.

The Reproductive Organs are male, female and hermaphrodite. *The male organs* consist of penis and testis; the latter is connected with the former and with the oviduct. *The female organs* are, ovarium, oviduct, and mucus-gland. The ovarium is spread over the surface of the liver in the form of a branched duct with terminal ampullæ. The oviduct terminates in the mucus-gland. *The androgynous apparatus* is a tube or vagina opening from the exterior into the oviduct, having one or two diverticular spermathecæ communicating with it in its course. On the right margin of the body near the front is a common opening, to which converge the three parts of the reproductive organs. The spermatozoa are developed within large and fusiform spermatophora, and are observed in the spermathecæ, oviduct and ovary.

Organs of Circulation and Respiration.—The circulatory organs are, a systemic heart, arteries, lacunæ and veins. The existence of true capillaries in the liver-mass seems probable. A second heart—a ventricle, having a portal character, is also described. The systemic heart lies immediately beneath the dorsal skin, in front of the respiratory crown, and comprises an auricle and ventricle enclosed within a pericardium. In the systemic circle the blood is returned to the heart without having passed through the special respiratory organ. It is that blood only which is returned from the liver-mass that circulates through the branchiæ.

The authors conclude from their observations, that in the Mollusks there is a triple circulation: first, the systemic, in which the blood propelled along the arteries to the viscera and foot is returned, with the exception of that from the liver-mass, to the heart through the skin; there it becomes partially aërated, the skin being provided with vibratile cilia, and otherwise adapted as an instrument of respiration; second, the portal, in which venous blood from the system is driven by a special heart to the renal and hepatic organs, and probably to the ovarium, where it escapes, doubly venous, with the rest of the blood which has been supplied to these organs from the aorta, and which is therefore only singly venous, to the branchiæ; third, the branchial circulation, in which flows only the more deteriorated blood brought by the hepatic vein, but in which also that blood undergoes the highest degree of purification capable of being effected in the economy, namely in the special organ of respiration. This triple circulation has not yet, as far as the authors are aware, been described as existing in the Molluscan Subkingdom. From the fact of the blood in *Doris* being returned to the heart in a state of partial aëration, it is clear, they say, that this animal is, in this respect, on a par with the higher crustaceans; and from the blood arriving at the heart in the same condition, according to the researches of Garner and Milne-Edwards, in *Ostrea* and *Pinna*, the great *Triton* of the Mediterranean, *Haliotis*, *Patella* and *Helix*, it can scarcely be doubted that this arrangement will be found throughout the Mollusca.

From a consideration of the facts cited in the paper, it may be deduced that the skin or mantle is in the Mollusca the fundamental organ of respiration, and that a portion of that envelope becomes evolved into a speciality as we trace upwards the development of the respiratory powers.

Upon the dorsal aspect of the liver-mass is a branched cavity, that of the *renal organ*, lined with a spongy tissue, and opening externally at the small orifice near the anus.

Organs of Innervation.—These are in two divisions, one corresponding to the cerebro-spinal division, the other to the sympathetic or ganglionic system of the Vertebrata. The existence of the latter, it is stated, is now for the first time fully established. The centres of the first system are seven pairs and a half of ganglia. Of the seven pairs, five are supra-œsophageal, two, infra-œsophageal: the single ganglion belongs to the right side and has been named *visceral*. There are three nervous collars around the œsophagus, one of which connects the infra- with the supra-œsophageal. The total number of pairs of nerves from the œsophageal centres is twenty-one, and there are also four single nerves.

The sympathetic system exists, and is more or less demonstrable, in the skin, the buccal mass, and on all the internal organs. It consists of a vast number of minute distinct ganglia, varying in size and form, the largest quite visible to the naked eye, of a bright orange colour, like the ganglia around the œsophagus, and interconnected by numerous delicate, white nervous filaments, arranged in more or less open plexuses. This beautiful system is connected with both sets of œsophageal ganglia.

The authors having found the sympathetic nervous system in several species of *Doris*, in *Eolis papillosa*, and in *Arion ater*, believe it to exist in all the more highly organized Mollusca.

The supra-œsophageal nervous centres in the Mollusca are in some instances so concentrated as to have led to the idea that they form only one mass; in others the ganglia are more or less distinct, and separated from each other. *Doris* has been taken as the representative of one class, *Aplysia* of the other, and on a comparison of both the supra- and infra-œsophageal ganglia of these with each other, there has been found a close correspondence between them, with the exception of the visceral ganglion. The single one in *Doris* is represented in *Aplysia* by a pair of ganglia, situated in the posterior part of the body near the root of the branchiæ. The supra-œsophageal ganglia in the Lamellibranchiata appear homologous with those of *Doris*.

Having determined the existence of a true sympathetic or organic nervous system in *Doris*, the authors feel themselves more in a position to trace a parallelism between the œsophageal nervous centres of these Mollusca and the cerebro-spinal system of the Vertebrata, and accordingly they find there is a strict analogy between them, even to the individual pairs of ganglia of which they respectively consist, the general result being that the whole of the ganglia, grouped around the œsophagus in these Mollusca, answers to the encephalon, and a small portion of the enrachidion, of the Vertebrata.

Organs of the Senses.—The auditory capsules are microscopic, composed of two concentric vesicles, the inner enclosing numerous, oval, nucleated otolithes. *The eyes* are minute black dots, beneath the skin, attached by a pedicle to a small ganglion. They are made up of a cup of pigment, receiving from behind the nerve, and lodging in front a lens, having in advance of it a cornea, the whole enclosed by a fine capsule. The authors believe they have shown the dorsal tentacles to be the *olfactory organs*.

The organs of touch are, the general surface of the skin, but more particularly the oral tentacles or veil. *Taste* is most probably located in the lips and channel of the mouth, the tongue being a prehensile organ, and ill-adapted as the seat of such a function.

In conclusion, the authors comment on the high organization of the *Doridæ*, and express their belief that the genus, as at present understood, will require to be broken up into several groups.

ROYAL INSTITUTION OF GREAT BRITAIN.

Friday, April 2, 1852.—On the Blackheath Pebble-bed, and on certain Phænomena in the Geology of the Neighbourhood of London. By Sir Charles Lyell.

There are two kinds of flint-gravel used for making roads in the neighbourhood of London, both of them in certain places superficial, but which are of extremely different ages. The yellow gravel of Hyde Park and Kensington so often found covering the "London Clay" may be taken as an example of one kind; that of Blackheath, of the other. The first of these is, comparatively speaking, of very modern date, and consists of slightly rolled, and, for the most part, angular fragments, in which portions of the white opake coating of the original chalk flint remain unremoved. The more ancient gravel consists of black and well-rounded pebbles, egg-shaped or spherical, of various sizes, exhibiting no vestige of the white coating of the original flints, yet showing by the fossil sponges and shells contained in them that they are derived from the Chalk. In the pits of Blackheath and the neighbourhood, where this old shingle attains at some points a thickness of 50 feet, small pieces of white chalk sometimes occur, though very rarely intermixed with the pebbles. If we meet with thoroughly rounded flints in the more modern, or angular gravel, it is because the latter has been in part derived from the denudation of the older bed.

The researches of the Rev. H. M. De la Condamine have shown that the sand and pebble-beds of Blackheath and Greenwich Park, inclose in some of their numerous layers, freshwater shells of extinct species, such as *Cyrena cuneiformis*, &c., agreeing with fossils which characterize the Lower Eocene beds at Woolwich. At Lewisham the pebble-bed passes under the London Clay, and at Shooter's Hill this clay overlies it in great thickness.

At New Charlton, in the suburbs of Woolwich, Mr. De la Condamine discovered a few years ago a layer of sand in the midst of the pebble-bed, where numerous individuals of the *Cyrena tellinella* were seen standing endwise, with both their valves united, the posterior extremity of each shell being uppermost, as would happen if the mollusks had died in their natural position. Sir Charles Lyell described a bank of sandy mud in the delta of the Alabama river at Mobile, on the borders of the Gulf of Mexico, where, in 1846, he had dug out, at low tide, specimens of a living species of *Cyrena*, and of a *Gnathodon*, which were similarly placed, with their shells erect, a position which enables the animal to protrude its siphons upwards, and draw in water to lubricate its gills, and reject it when it has served the purposes of respiration. The water at Mobile is usually fresh, but sometimes brackish. Sir Charles examined lately the Woolwich beds with Mr. Morris, and they verified Mr. De la Condamine's observations, observing there several dozen specimens of the *Cyrena tellinella* in an erect position. From this circumstance the Lecturer infers, that a body of fresh or river water had been maintained permanently on that spot during the Eocene period, and the presence of rolled oysters in the associated pebbly layers, with other marine shells, mixed with species of *Melanopsis, Melania, Cerithium* and *Neritina*, demonstrate that the sea occasionally invaded the same area. To an overflow of the pebbly sand in which the Cyrenæ lived by salt water, may probably be attributed the poisoning of the mollusks which left their shells uninjured on the spot where they had lived.

The stratum called "the shell-bed," which contains at Greenwich, Woolwich, Upnor near Rochester, and other places, a great mass of freshwater, brackish-water and marine shells, especially oysters, is observed everywhere to underlie the great pebble-bed. Its mode of occurrence implies the entrance of one or more rivers into the Eocene sea in this region. Other rivers draining adjoining lands are indicated by a similar assemblage of fluvio-marine fossils near Guildford and at Newhaven in Sussex. The vicinity of land to the south and west of Woolwich is shown by the occurrence at New Cross, Camberwell, and Chelsea of *Paludina* and *Unio* in strata evidently a prolongation of the Woolwich beds, and by fossil leaves of dicotyledonous trees and layers of lignite in some of those localities. On the other hand, at the junction of the "London Clay," and the subjacent "plastic clays and sands," when followed in an opposite or easterly direction towards Herne Bay and the Reculvers, all signs of the freshwater formation disappear, and the pebble-bed is reduced to a thin layer, often a foot or a few inches in thickness. The origin of this shingle may have been chiefly due to the action of waves on a sea-beach. Its accumulation in great force at certain points where freshwater shells abound, seems to imply the entrance of rivers into the sea, which brought down some flints, and arrested the progress of others travelling as beach pebbles along a coast line, in a certain direction determined by the prevailing currents and winds. The spreading of the pebble-bed over a wide area may be accounted for by supposing a gradual subsidence of land, and the continually shifting of the coast-lines upon which shingle accumulated. This same subsidence is required to explain the superposition of the London Clay, a deep-sea deposit to the Blackheath or Woolwich beds which are of shallow water or littoral origin. One of the rivers of the Lower Eocene period swept into the sea at Kyson near Woodbridge in Suffolk the bones of a monkey of the genus *Macacus*, of a marsupial quadruped allied to the opossum, of a *Hyracotherium*, and other mammalia, which have been determined by Professor Owen, and which throw light on the inhabitants of the land, at an æra antecedent to the deposition of the London Clay.

Sir C. Lyell then exhibited some sections, recently published by Mr. Prestwich*, illustrative of the geology of the environs of London, and gave a rapid sketch of the successive Eocene groups from the London Clay and overlying Bagshot series with its nummulites to the Barton and Hampshire freshwater formations with their fossil quadrupeds. He then alluded to the tertiary strata next in the ascending order which he had recently studied in Limburg, Belgium, which are not represented in England, and next to the Miocene faluns of Touraine and the Pliocene strata or crag of Suffolk, and lastly to the still more modern glacial period and the brick-earth of the valley of the Thames. The last-mentioned formation contains the bones of extinct quadrupeds mingled with shells of recent species, terrestrial and fluviatile.

The numerous and important changes in the fauna of the globe, attested by these successive assemblages of extinct species, belonging to different tertiary æras, attest the vast lapse of ages which separate the time when the freshwater beds of Woolwich and Blackheath were formed from the human period. But revolutions of another and no less striking kind have taken place contemporaneously in the physical geography of the northern hemisphere, revolutions on so great a scale that the greater part of the present continents of Europe, Asia, Northern Africa and North America with which the geologist is best acquainted, have come into existence in the interval of time here alluded to. It may also be confidently affirmed that the colossal chain of the Alps is more modern than the tertiary shingle of Blackheath. There was deep sea at the period when the London Clay was forming, precisely in the area where the loftiest mountains of Europe now rise into the regions of perpetual snow. In proof of this the Lecturer referred to the works of several modern geologists, especially to those of Sir Roderick Murchison, and to a Lecture delivered by Sir Roderick in the Royal Institution to show that the nummulitic formation which belongs to the Eocene period, and not to the very oldest part of that period, attains an elevation in some

* Prestwich, Geological Enquiry respecting the Water-bearing Strata around London, &c. Van Voorst, 1851.

portions of the Swiss Alps of 8000 or even 10,000 feet, and enters into the structure and composition even of the central axis of the Alps, having been subject to the same movements and partaking of the same foldings and contortions as the underlying cretaceous and oolitic strata.

Sir Charles Lyell next proceeded to show that a great series of volcanic eruptions had occurred in Europe since the older Eocene strata of the neighbourhood of London were deposited. Not only Vesuvius and Somma as well as Etna and the extinct volcanoes of Southern Sicily, but the trachytic and basaltic eruptions of the extinct volcanoes of central France are more modern than the London Clay. The evidence consists not only of the superposition of igneous rocks several thousand feet thick, to lacustrine strata of the middle and upper Eocene periods, but also to the absence in the pebble-beds constituting the base of the tertiary series of Auvergne, Cantal, and Velay of any pebbles of volcanic origin.

The Lecturer concluded by stating that the formation of every mountain chain and every elevation and depression of land bears witness to internal changes at various depths in the earth's crust. The alteration has consisted sometimes of the expansion, and sometimes of the contraction of rock, or of the semi-liquefaction or complete fusion of stony masses and their injection into rents of the fractured crust occasionally manifested by the escape of lava at the surface. Every permanent alteration therefore of level may be regarded as the outward sign of much greater internal revolutions taking place simultaneously far below. Even the precise nature of the changes in the texture of rocks produced by subterranean heat and other plutonic influences since the commencement of the Eocene period can be detected in a few spots, especially in the central axis of the Alps, where the disturbing agency had been intense. The table might be covered with specimens of gneiss, mica-schist and quartz rock, once called primitive, and once supposed to be of a date anterior to the creation of living beings, which nevertheless were sedimentary strata of the Eocene period which assumed their crystalline form after the flints of Blackheath were rolled into shingle, and even after the shells of the London Clay and the nummulites of the overlying Bagshot sands were in existence.

Yet however remote may be the antiquity of the Blackheath pebble-bed as demonstrated by the vast amount of subsequent change in physical geography, in the internal structure of the earth's crust and in the revolutions in organic life since experienced, its origin is probably as widely separated from the æra of the Chalk as from our own times. For the fossils of the Chalk differ as much from those of the oldest tertiary strata near London, as do the last from the organic beings of the present æra. Nevertheless the white Chalk itself with its flints is considered by every geologist as the production of a modern æra, when contrasted with the long series of antecedent rocks now known, each formed in succession when the globe was inhabited by peculiar assemblages of animals and plants long since extinct.

LXVIII. *Intelligence and Miscellaneous Articles.*

ON THE PASSIVE STATE OF METEORIC IRON. BY PROF. WÖHLER.

I HAVE observed the remarkable fact that the greater portion of the meteoric iron which I have had the opportunity of examining, is in the so-called passive state, that is to say, that it does not reduce the copper from a solution of neutral sulphate of copper, but remains bright and uncoppered on immersion therein. But if touched in the solution with a piece of common iron, the reduction of the copper commences immediately upon the meteoric iron. It also becomes active instantaneously on the addition of a drop of acid to the solution of copper; but if the reduced copper be filed away, the new surface is again passive; indeed I was unable by filing away to produce an active or reducing surface on any passive meteoric iron. I convinced myself by experiments on meteoric iron, which had never been in contact with nitric acid and nevertheless was passive, that this state could not have been produced by the corrosion of the surface by the acid for the production of the Widmanstättean figures.

I thought at first that this deportment might be employed as a means of distinguishing true meteoric iron; but it soon appeared that some undoubtedly genuine meteoric iron was not in this state. In this respect I have observed the following differences:—

The Pallas iron, the iron which fell at Braunau in 1847, that of Schwetz, Bohumilitz, Toluca, Green County (N. America), Red River, and that from the Cape of Good Hope, are *passive*.

The iron from Lenarto, Chester County, Rasgata, Mexico, Senegal and Bitburg (the latter forged), is *active* or *reducing*.

Between the two stands the iron from Agram, Arva, Atacama and Burlington (N. America), which do not become coated with copper immediately, but on which the reduction gradually commences after a longer or shorter contact with the cupreous solution, and usually from one point or from the margins of the fluid.

These peculiarities appear to have no connexion either with the presence of nickel or the property of forming regular figures on corrosion, as is shown by the iron from Lenarto, which is active, although it contains 8·45 per cent. of nickel and 0·66 of cobalt, and exhibits the most beautiful figures on corrosion, and also by the iron brought by Boussingault from Rasgata in Columbia, which, according to my analysis contains 6·74 per cent. of nickel and 0·23 of cobalt. On the other hand, the iron from Green County, which is completely passive, contains 19 per cent. of nickel and exhibits no figures.

I also found that an artificially prepared alloy of iron and nickel, which on corrosion acquired a damasked surface, reduced the copper from solution in the same manner as common iron.

Whether this state is proper to all meteoric iron on its reaching the earth, and, as may have happened in the case of the active kinds, have only been lost in the course of perhaps a very long period of time, and what probable opinion can be formed of these phænomena, must be settled by experiments and observations of a more extended nature.—Poggendorff's *Annalen*, vol. lxxxv. p. 449.

INVENTION OF THE STEREOSCOPE.

To R. Taylor, Esq.

Dear Sir,

Had your correspondent, Mr. Elliot, attended to the original date of my first memoir on Binocular Vision, reprinted in the Philosophical Magazine for April last, he would have seen that there was no ground for impugning the originality of my researches. That paper was read at the Royal Society in June 1830, *fourteen* years ago, and published in the Philosophical Transactions. The subject was brought forward in September of the same year at the Meeting of the British Association at Newcastle, where it excited considerable interest, and was fully noticed in consequence thereof in the Athenæum, Literary Gazette, and other public journals; my Stereoscope also at that time was made and sold by the principal opticians in London. Whatever instrument Mr. Elliot made *thirteen* years ago was therefore made after my experiments had received extensive publicity.

Yours very truly,
C. Wheatstone.

King's College,
May 2nd, 1852.

[We have, since the publication of our last Number, received a note from Mr. Elliot, stating, that had he been aware that Prof. Wheatstone had produced his Stereoscope so early as 1838, he would not have sent the statement inserted therein.—Editor.]

ON THE SUN COLUMN AS SEEN AT SANDWICK MANSE, ORKNEY, IN APRIL 1852. BY C. CLOUSTON.

The perpendicular column of light which appeared repeatedly at sunset and sunrise during April, deserves a more particular account than the usual monthly report contains, as this is the most northern locality in which I have yet heard of its appearance.

When seen in the evening, it was generally immediately after the sun had sunk either below the horizon, or behind a bank of clouds there.

It was rather wider than the apparent diameter of the sun, and extended upwards for about 15°, widening a little towards the top, and becoming fainter, so that there was no defined boundary; but it was sometimes much shorter, and could be distinctly seen, when it was less than the semidiameter of the sun above the horizon, either when vanishing by descending, as it generally did, or as it last appeared on the 3rd of May, without rising more than about 1°.

Though at first it seemed to be a law that it must descend as the sun descended below the horizon, yet on one occasion, at least (on the 26th), it vanished by ascending, or the base disappeared first.

It was generally remarkably perpendicular, but sometimes had a perceptible inclination to one side, and followed the course of the sun northwards.

It had periods of greater and less brightness, but for the most part was steady, something like a sunbeam among the clouds, and never had any approach to the rapid motion of the aurora.

Its colour was pale or whitish in its upper portion, or when it appeared contrasted with the dark sky; but in passing through the red, copper, or orange-coloured sky that prevailed lower down, it partook of its shade, and tinged the thin strata of cloud that lay across it with a brighter hue of their own colour. Fifty-five minutes was the longest period that it was visible any evening. I am told that it also appeared very bright some mornings before sunrise.

If the phænomenon was uncommon, so was the state of the atmosphere when it occurred. The drought was unprecedented; only about $\frac{1}{10}$th of an inch of rain falling in April, which is about $\frac{1}{20}$th of the average quantity in that month in previous years. The atmospheric pressure was great, the mercury never being lower than 30·07, nor higher than 30·32. The temperature was also high for the month, being 47°·64, or more than 4° above the average for April.

The atmosphere was very calm, and the sky near the horizon of that red or copper colour which generally indicates dry and warm weather, so that at last we could anticipate its appearance. I do not presume to explain the mode of its production, but these circumstances may assist others in doing so.

METEOROLOGICAL OBSERVATIONS FOR APRIL 1852.

Chiswick.—April 1. Overcast and cold: fine: clear and frosty. 2. Cold dry haze: clear and frosty. 3. Slight fog: fine: clear. 4. Slight haze: overcast: clear. 5, 6. Fine. 7. Cloudy. 8. Cold and dry: clear. 9. Very fine. 10. Clear: hazy. 11. Foggy: very fine. 12. Hazy: clear at night. 13. Hazy: very fine: clear. 14. Dry haze: fine, with very dry air: clear. 15. Foggy: slight haze. 16. Cloudy and cold. 17. Clear and fine. 18. Cloudy and cold. 19. Clear and cold: cloudy: clear, with sharp frost at night. 20. Clear: very fine: sharp frost at night. 21. Clear, with excessively dry air. 22. Foggy: fine: clear. 23. Fine, with hot sun. 24. Boisterous. 25. White clouds: fine: clear and frosty at night. 26. Clear: fine: clear and frosty. 27. Cloudy: frosty at night. 28. Cloudy and fine: rain at night. 29. Rain: densely clouded. 30. Cloudy and fine.

Mean temperature of the month	44°·81
Mean temperature of April 1851	44 ·56
Mean temperature of April for the last twenty-six years ...	47 ·30
Average amount of rain in April	1·65 inch.

Boston.—April 1—4. Fine. 5—7. Cloudy. 8. Fine. 9. Cloudy. 10, 11. Fine. 12. Cloudy. 13, 14. Fine. 15—17. Cloudy. 18. Cloudy: rain A.M. 19. Cloudy. 20, 21. Fine. 22. Cloudy. 23. Fine. 24. Fine: stormy. 25, 26. Fine. 27. Cloudy. 28. Fine: rain P.M. 29. Cloudy: rain A.M. and P.M. 30. Cloudy.

Sandwick Manse, Orkney.—April 1. Clear: fine: clear. 2. Cloudy: fine: clear: fine. 3. Bright: fine: clear: fine. 4—7. Clear: fine. 8. Bright: damp. 9. Clear: fine: cloudy: fine. 10. Clear: fine: aurora. 11. Hazy: fine: clear: fine: aurora. 12, 13. Bright: fine: warm: fine. 14. Bright: fine: warm: fine: aurora. 15—17. Bright: fine: warm: fine. 18. Cloudy: fine: clear: fine: aurora. 19. Bright: fine: clear: fine. 20. Drops: fine: clear: fine: S. aurora. 21. Clear: fine: clear: aurora. 22, 23. Bright: cloudy: aurora. 24. Clear: fine. 25. Clear: fine: aurora. 26. Cloudy: fine. 27. Bright: fine: clear: fine. 28. Cloudy: fine: showers: fine. 29. Fog: damp. 30. Cloudy: clear: fine.

This month has been unprecedentedly fine, dry and warm, with the barometer high.

Mean temperature of this month	47°·64
Mean temperature of April for preceding twenty-five years ...	43 ·28
Average amount of rain in April for six years	2 inches.

The most singular meteorological phænomenon this month was the perpendicular column of light which appeared above the sun at setting, extending about 15° in height, wider than the apparent diameter of the sun, following his course northwards, and continuing one evening for 55 minutes. It appeared at sunset on the 6th, 11th, 16th, 24th, 26th and 27th, and once or twice before I noted the date, either this month or March, also before sunrise.

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

Days of Month.	Barometer.					Thermometer.					Wind.			Rain.		
	Chiswick.		Boston. 8¾ a.m.	Orkney, Sandwick.		Chiswick.		Boston. 8¾ a.m.	Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Orkney, Sandwick.	Chiswick.	Boston.	Orkney, Sandwick.
1852. April.	Max.	Min.		9¼ a.m.	8½ p.m.	Max.	Min.		9¼ a.m.	8½ p.m.						
1.	30·203	30·015	29·68	30·10	30·16	51	24	40	45	38	ne.	e.	se.	......	......	......
2.	30·303	30·263	29·96	30·17	30·24	52	25	40	47	46	ne.	e.	ese.	......	......	......
3.	30·326	30·246	29·98	30·28	30·33	51	29	39	48	42	e.	e.	se.	......	......	......
○ 4.	30·156	30·147	29·80	30·38	30·36	50	30	43	45½	42	se.	e.	se.	......	......	......
5.	30·141	30·099	29·76	30·20	30·12	58	28	47	46	41½	ne.	e.	......	......	......	......
6.	30·146	30·111	29·73	30·13	30·24	57	39	44	46	45	ne.	n.	w.	......	......	......
7.	30·218	30·150	29·80	30·28	30·35	50	40	45	49	45	ne.	nne.	w.	......	......	......
8.	30·270	30·232	29·89	30·28	30·31	54	28	44·5	50	48	e.	ne.	wnw.	......	......	......
9.	30·312	30·307	29·96	30·29	30·27	53	24	47	49	47½	se.	ene.	wnw.	......	......	......
☾ 10.	30·293	30·189	29·84	30·22	30·18	60	26	45	50	43½	e.	wnw.	......	......	......	......
11.	30·177	30·144	29·84	30·13	30·16	55	38	40	50	46	e.	ese.	e.	......	......	......
12.	30·278	30·236	29·92	30·15	30·16	57	27	44	54	45	e.	s.	sse.	......	......	......
13.	30·313	30·274	29·92	30·19	30·19	68	26	43	52	50	se.	ese.	se.	......	......	......
14.	30·262	30·171	29·80	30·21	30·23	73	29	50	55	51½	e.	nnw.	se.	......	......	......
15.	30·174	30·076	29·76	30·20	30·18	60	41	46·5	54	47	e.	ne.	ese.	......	......	......
16.	30·037	29·987	29·67	30·13	30·10	52	25	44·5	52	48	e.	ese.	......	......	......	......
17.	29·957	29·837	29·58	30·01	29·96	56	31	43	55	47	n.	wnw.	n.	·01	......	......
18.	30·010	29·763	29·47	29·97	30·04	50	34	44	53½	45	e.	ne.	se.	·01	·03	......
● 19.	30·096	30·069	29·74	29·94	29·86	48	20	42·5	50½	47	ne.	n.	s.	......	......	......
20.	30·118	30·043	29·72	29·86	29·99	59	21	36·5	49½	47	sw.	s.	w.	......	......	·03
21.	30·074	29·957	29·72	30·02	29·99	61	37	44	52	44	se.	ssw.	ese.	......	......	......
22.	29·883	29·821	29·53	29·94	30·02	70	43	52·5	49	45	se.	e.	ese.	......	......	......
23.	29·961	29·842	29·50	30·13	30·28	68	42	52	49	45	e.	e.	ese.	......	......	......
24.	29·903	29·896	29·66	30·36	30·32	53	33	46	49	42½	e.	e.	e.	......	......	......
25.	29·922	29·900	29·59	30·27	30·20	54	30	47·5	51	42	e.	e.	......	......	......	......
26.	30·071	29·953	29·64	30·17	30·18	63	27	47·5	48	45	ne.	ne.	ne.	......	......	......
☽ 27.	30·168	30·141	29·74	30·14	30·07	55	25	48	49	45½	n.	n.	se.	......	......	......
28.	30·156	29·974	29·70	29·84	29·71	64	44	49	51½	46	sw.	s.	......	·32	......	......
29.	29·820	29·686	29·32	29·67	29·64	61	50	56	52	47	sw.	sw.	ese.	·18	·03	·08
30.	29·600	29·546	29·10	29·69	29·81	66	44	57	48	45	w.	wnw.	n.	......	·14	......
Mean	30·111	30·025	29·71	30·111	30·121	57·63	32·00	45·6	49·98	45·30				0·52	0·20	0·11

THE

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AND

JOURNAL OF SCIENCE.

SUPPLEMENT TO VOL. III. FOURTH SERIES.

LXIX. *On the Heat disengaged in Chemical Combinations.* *By* JAMES PRESCOTT JOULE, *F.R.S.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

THE following memoir was communicated to the French Academy several years ago*, in order to compete for the prize offered for the best essay on the heat of chemical combinations. Owing to my not having been aware of the regulations to be observed by the competitors, and to the delay in procuring a good translation, my paper was deemed ineligible, but was referred to the Commission appointed to examine the memoirs presented on the same subject. The time has now arrived when I feel that I ought not to delay communicating the results of a laborious investigation to the scientific world, and I therefore transmit to you an exact copy of the paper from which the French translation, now in the possession of the Academy of Sciences, was made. The necessity of making no alterations whatever in the paper has prevented me from citing additional authorities, and making sundry small corrections which have occurred to me since it was written. I will therefore avail myself of the opportunity of making a few prefatory observations.

I would remark, in the first place, that the laws of the disengagement of heat by voltaic electricity received, nearly at the same time that the experiments contained in my paper were made, a new confirmation and important developments by the researches of Professor J. D. Botto of Turin. This valuable contribution to physical science will be found in the Memoirs of the Academy of Sciences of Turin, 2nd series, vol. viii. In the *Bulletin des Sciences* of the Berlin Academy for the 25th of November 1848, will also be found an able memoir on the same subject by Professor Poggendorff.

* See the *Comptes Rendus* for February 9, 1846.

I have learned from Professor Thomson that the length of the needle of my galvanometer is so small in comparison with the diameter of the coil, that no sensible error could have arisen from taking the tangents of the deflections as the measure of the currents traversing the coil. The amount of the correction that I have applied is, however, too trifling to affect materially the numerical results arrived at. Professor Thomson has kindly allowed me to describe the arrangement by means of which he has recently effected a very valuable improvement in the tangent galvanometer. The coil he employs is represented by Pl. XI. fig. 9. It consists of two concentric circles of flat copper wire, the outer one being 12 inches, the inner 9 inches in diameter. It is furnished with three terminals, *a*, *b* and *c*, which can be readily connected with a battery, or other apparatus, by means of the usual clamps. It will be seen that if the current pass from *a* to *c*, it will traverse the outer circle; that if it pass from *b* to *c*, it will traverse the inner circle; and that if it pass from *a* to *b*, it will traverse both circles in opposite directions. The diameters of the circles being in the proportion of 4 to 3, it is obvious that the effect of a constant current on the needle will in the three arrangements be respectively proportional to 3, 4 and 1. So that by making two circles of unequal force oppose one another, Professor Thomson obtains the means of measuring powerful currents accurately, without needlessly increasing the size of the galvanometer.

I observe with pleasure that Dr. Woods has recently arrived at one of the results of the paper, viz. "that the decomposition of a compound body occasions as much cold as the combination of its elements originally produced heat," by the use of an elegant experimental process described in this Magazine for October 1851. I ought, however, to remark, that previous to the year 1843 I had demonstrated "that the heat rendered latent in the electrolysis of water is at the expense of the heat which would otherwise have been evolved in a free state by the circuit*," a proposition, which Professor Thomson has shown to be an inevitable consequence of the dynamical theory of heat†.

I have the honour to remain, Gentlemen,

Yours very respectfully,

JAMES P. JOULE.

Acton Square, Salford,
March 30, 1852.

Actioni contraria semper et æqualis est reactio.—*Newton.*

1. My object in the present memoir is to communicate to the Academy of Sciences an inverse method for ascertaining the

* Philosophical Magazine, S. 3. vol. xxiii. p. 263.
† Memoir on the Dynamical Theory of Heat, § 18.

quantity of heat evolved by combustion, together with the results at which I have arrived by its use. Being convinced of the accuracy of the experiments of Dulong, and knowing that distinguished philosophers were engaged in confirming and extending his results, I thought that by examining the electrical reactions I should best fulfill the wishes of the Academy.

2. Davy drew from his electro-chemical experiments the conclusion, that the heat and light evolved in chemical combinations are caused by the reunion of the two electricities. Subsequently Berzelius has taken up Davy's theory, and, giving it new developments, has made it the foundation of modern chemistry. Ampère has still further modified the theory, in order to explain the permanency of chemical combinations. The views of these and other philosophers have been ably discussed by Becquerel in his *Traité de l'Electricité**, and therefore I need not attempt any criticism of them, were it indeed necessary to my design to do so. It will be sufficient for my purpose to admit,—1st, that when two atoms combine by combustion, a current of electricity passes from the oxygen to the combustible; 2nd, that the quantity of this current of electricity is fixed and definite; and 3rd, that it is the means of the evolution of light and heat, precisely as is any other current of electricity whatever.

The first of these propositions I consider to have been proved by the experiments of Pouillet and Becquerel†, and the second is naturally derived from the discoveries of Faraday. Therefore it is only necessary to obtain one element more, viz. the intensity or electromotive force of the electric currents passing between the atoms, in order to be able to estimate the heat due to chemical combinations.

3. But it is important to decide first, by what laws the evolution of heat by electricity is governed. Brooke and Cuthbertson found that the length of wire melted by an electrical battery varied nearly with the square of its charge; and Children and Harris showed that more or less heat is evolved by frictional electricity in proportion to the goodness or badness of the conductors. P. Riess, however, appears to have made the most extensive and accurate experiments on the calorific effects of frictional electricity. This philosopher has shown that the heat evolved by an electrical discharge is proportional to the square of the quantity of fluid divided by the extent of coated glass surface upon which it was induced; in other words, proportional to the quantity and density of the fluid‡.

* *Traité de l'Electricité,* vol. iii. p. 366.
† Becquerel, *Traité de l'Electricité,* vol. ii. p. 85.
‡ *Annales de Chimie et de Physique,* vol. lxix. p. 113.

In the year 1840* I commenced experiments on the calorific effects of voltaic electricity, having at that time no knowledge of what Riess had previously done in frictional electricity. By these experiments it was proved, that when a current of voltaic electricity is propagated along a metallic conductor, the heat evolved thereby in a given time is proportional to the resistance of the wire and the square of the quantity of electricity transmitted.

Pursuing the inquiry, I found that the law applied very well to liquid conductors; and hence I inferred that *the heat evolved by any voltaic pile is proportional to its intensity or electromotive force, and the number of chemical equivalents electrolysed in each cell of the circuit*; or in other words, *proportional to the intensity of the pile and the quantity of transmitted electricity*†.

The above law must be understood to hold good only when the pile is free from local or secondary action; for it is obvious that the heat evolved by any action not directly engaged in propelling the current ought to be eliminated. Those parts of the pile where these secondary and local actions are carried on, may be regarded as minute voltaic circles respectively evolving heat in quantities determined by the law; but this heat is not to be confounded with that due to the direct and useful action of the pile.

In applying the law, the intensity or electromotive force of the pile must be taken at its maximum, and not when under the influence of the polarization of Ritter, a phænomenon, which, as is well known, is occasioned by the deposit of electro-positive substances on the negative plates of the pile. When a pile is under the influence of this polarization, its intensity is diminished; but I have shown that the diminution of heat due to this diminution of the intensity of the pile is exactly counterbalanced by the evolution of an additional quantity of heat at the polarized plates; and hence it appears that the heat evolved is at all times proportional to the intensity of the pile when its plates are in the proper condition and free from the polarization of Ritter, multiplied by the quantity of transmitted electricity.

In a memoir‡ "On the Heat evolved during the Electrolysis of Water," I proved the three following propositions:—

1st. That the *resistance to conduction*, whether it exists in solid or in liquid conductors, occasions the evolution of a quantity of heat, which, for a given time, is proportional to the mag-

* Proceedings of the Royal Society.

† Philosophical Magazine, S. 3. vol. xix. p. 275.

‡ Memoirs of the Literary and Philosophical Society of Manchester, 2nd series, vol. vii. part 2.

nitude of the resistance to conduction and the square of the quantity of transmitted electricity.

2nd. That the *resistance to electrolysis* presented by water does not occasion the evolution of heat in the decomposing cell. At the same time, the heat evolved by the whole circuit, for a given quantity of transmitted electricity, is diminished on account of the decreased electromotive force of the current, owing to the resistance to electrolysis. It is reasonable to infer that this diminution of the heat evolved by the circuit is occasioned by the absorption of heat in the decomposing cell.

And 3rd. That the *resistance occasioned by the polarization of Ritter* occasions the evolution of heat at the surfaces on which this phænomenon takes place; and thus it happens that the diminution of the heat evolved by the circuit, in consequence of the diminished intensity of the pile, is exactly compensated for; so that the heat evolved by the whole circuit may be estimated by the chemical changes occurring in the pile, just as if no such polarization existed.

I have already given theoretical results* for the heat of combustion agreeing so well with the experiments of Dulong, as to convince me of the accuracy of the principles above advanced. My method was to ascertain how much of the intensity of a pile is spent in overcoming the affinity of the combustible for oxygen, and then to calculate the heat due to such an intensity, which heat ought, on our principles, to be equal to the heat occasioned by the recombination of the elements in combustion. I hope in the present paper to be able to show that still more accurate results may be attained by the use of a method, founded upon the same principles, but of greater simplicity. Previously, however, to the description of the new experiments, I intend to bring forward some new proofs of the correctness of the law upon which their accuracy entirely depends.

I am aware that M. Ed. Becquerel† and M. Lenz ‡ have separately, and by numerous and skilfully performed experiments, made it abundantly clear that the heat evolved by voltaic electricity is proportional to the resistance to conduction and the square of the current. Nevertheless I have made new experiments upon the subject, thinking it impossible to demonstrate too completely the accuracy of a law upon which all thermo-chemical phænomena depend. I have endeavoured to make the results of these experiments worthy of confidence by the employment of a galvanometer and thermometers of great delicacy and accuracy.

* Phil. Mag., S. 3. vol. xix. p. 276; and vol. xxii. p. 205, &c.

† *Annales de Chimie et de Physique,* 1843, vol. ix. p. 21.

‡ *Annalen der Physik und Chemie,* vol. lxi. p. 18.

4. The galvanometer was, in all its essential parts, constructed similarly to Pouillet's compass of tangents. A stout circular mahogany board (*aa*, figs. 1 and 2, Pl. XI.) supported upon three leveling screws, forms the base of the instrument. To the diameter of this a stout vertical board is fixed, having a semicircular hole in its centre for the reception of the graduated circle *bb*. This circle, constructed of brass entirely free from magnetic influence, is 6 English inches in diameter and divided to half-degrees; it is also furnished with leveling-screws, &c. At *ccc* are three screws furnished with nuts, for the purpose of affixing to the vertical board any coil that may be required.

The magnetic needle, of which I have given a full-size representation in fig. 3, consists of two pieces of hard steel, each half an inch long, kept about a millimetre asunder by the intervention of a small piece of brass to which they are cemented. An exceedingly delicate glass pointer*, weighing only 7 or 8 milligrammes, is affixed to the top of the needle. The ends of the pointer are painted black, in order to be the more distinctly seen as they traverse the silvered edge of the circle. A piece of fine copper wire is affixed to the needle in such a manner as to afford the means of suspension by a single filament of silk, and also of a rest when the instrument is not in use. The filament of silk is suspended from a graduated circle, *dd*, fig. 2, by means of which torsion can be given to the filament. There is also a piece of apparatus, *ee*, fig. 2, by means of which the needle can be elevated, depressed, or adjusted to the centre of the divided circle *bb*.

One can, as I have already hinted, affix to the vertical board of the galvanometer a coil consisting of a fewer or larger number of turns of copper wire, according to the nature of the experiment intended to be made. In the present research I employed one consisting simply of a thick copper wire bent into a circle of a foot (English) diameter. The connexion between the coil of the galvanometer and the voltaic battery (which were placed at a distance from each other of about six metres) was established by means of clamps and screws at *ff*.

I need hardly remark, that it was found necessary to protect the galvanometer against any vibrations of the floor of the laboratory in which the experiments were made. This was effected by boring three holes in the floor, and driving strong wooden stakes through them into the ground. The feet of the stool upon which the galvanometer is placed rest upon the tops of

* The use of a glass pointer was suggested to me by Mr. Dancer, the maker of my galvanometer. I have since found that it has been employed by Professor Bunsen of Marburg in his galvanometer of tangents.

these stakes, so that the floor of the room forms a platform entirely independent of the instrument.

Although the glass pointer weighs, as I have already observed, only 7 or 8 milligrammes, the resistance to its motion presented by the air was so great as to bring the magnetic needle to which it was affixed to a state of perfect tranquillity in ten seconds after the circuit was shut. Therefore I found it quite unnecessary to employ the oil recommended by Lenz as an additional resisting medium. Nor did I find it necessary to make use of any verniers or micrometers; the only assistance to the naked eye being a glass prism, to look through horizontally when the pointer could not be well viewed from a vertical position. I found no difficulty in reading off the angles of deflection to 2 or 3 minutes of a degree. Those who are accustomed to work with galvanometers will admit, that it would be useless to attempt to arrive at greater accuracy by the employment of means which must necessarily increase the time occupied in the observations.

The galvanometer was adjusted in the plane of the magnetic meridian, by changing its position until a current passing in one direction could produce a deflection of the needle, exactly equal in extent to the deflection on the other side of the meridian occasioned by a current of the same intensity, but passing in the opposite direction. After the galvanometer had been thus adjusted with very great care to the magnetic meridian, it was found that the glass pointer stood 30′ from zero. This error arose from the difficulty of cementing the pointer to the needle so as to be exactly in the same plane with the magnetic axis of the latter; but it did not give rise to any serious inconvenience, as it only made it necessary to affect the observed deflections with an increase or diminution of 30′, according to the direction of the current. I may mention in this place, that in every observation the position of *both* ends of the pointer was noted.

Since the force of torsion of the filament of silk is so trifling that six complete twists of it only produce a deflection of the magnetic needle amounting to 12′, and since the length of the magnetic needle is only $\frac{1}{24}$th of the diameter of the coil, it could hardly be doubted that the tangents of the angles of deflection represent pretty accurately the intensity of the transmitted electricity. I have nevertheless made experiments in order to prove my galvanometer. Sixteen large cells of Daniell's pile having been arranged in a series of four, the deflection of the needle under this voltaic force was ascertained for both sides of the meridian. The mean of the deflections produced by each separate cell was also noted. It is evident that the resistance of sixteen cells in a series of four is equal to that of a single cell; consequently the deflections produced by the above arrangements ought to indicate currents whose ratio is as 4 to 1. Experiments of this kind having been repeated, with proper precautions against any alteration in the intensity of the battery cells, it was found that $\frac{1}{200}$dth had to be added to the tangent at a deflection of 55°, $\frac{1}{120}$th at 82°, &c.

My thermometers were constructed by a method very similar to that employed by Regnault and Pierre*. The calibre of the tube was first measured in every part by passing a short column of mercury along it. The surface of the glass having then been covered with a thin film of bees-wax, the portions of tube previously measured were divided into an equal number of parts by a machine constructed for the purpose. The divisions were then etched by means of the vapour of fluoric acid. Two thermometers were employed in the present research, in one of which the value of each space was $\frac{1}{18 \cdot 14}$, in the other $\frac{1}{23 \cdot 38}$ of a degree Centigrade. A practised eye can easily estimate the tenth part of each of these spaces; consequently I could by these thermometers observe a difference of temperature not greater than 0°·005.

The voltaic pile that I made use of was one of very large dimensions, each cell being 2 feet high and 5 inches in diameter. The internal arrangements of the cells were similar to those of the ordinary pile of Daniell.

5. I shall now proceed to describe my experiments on the heat evolved by currents traversing metallic wires. The apparatus used consisted of a wire of pure silver, 8 metres long and about 0·6 of a millimetre in thickness, coiled upon a thin chimney-glass, the several coils being prevented from touching one another by means of silken threads. The ends of the silver wire were connected metallically with two thick copper wires, the ends of which dipped into cups of mercury. The coil, thus mounted, was immersed in a jar of tinned iron, capable of containing two pounds and a half of water. In order to prevent, as well as possible, the influence of the surrounding atmosphere in raising or depressing the temperature of the water, the sides and bottom of this jar were made hollow by soldering two jars of unequal magnitude within each other. Fig. 4 represents a section of this double can, *aa* being the hollow part between the internal and external cans. The positions of the coil, thermometer and stirrer, are also shown in the same figure.

At 7 o'clock A.M., Sept. 4, 1844†, having filled the jar with $2\frac{1}{2}$ lbs. of distilled water, I immersed the coil of silver wire into

* *Annales de Chimie et de Physique*, 1842, vol. v. p. 428, note.

† My object in being so particular as to the dates of the experiments, was to eliminate the effects of any variation in the intensity of the earth's magnetism. In the subsequent series of experiments I have not always thought it necessary to mention these dates, but I have nevertheless used the same precaution in all of them.

it, and caused it to form part of a circuit in which a pile consisting of sixteen of the large Daniell's cells in series and the galvanometer were placed. The circuit remained closed for exactly five minutes, during which time the deflections of both ends of the pointer of the galvanometer were observed three times. The mean of all the observations, no two of which differed from each other more than a few minutes of a degree, when properly corrected for the error in the position of the pointer, was 72° 17′. The increase of the temperature of the water, ascertained with all proper precautions in stirring, &c., was indicated by 81·8 divisions of the scale of the thermometer, each division corresponding to $\frac{1}{18\cdot 14}$th of a degree Cent. The temperature of the room was 2°·07 Cent. lower than the mean temperature of the water.

As soon as the experiment just described was finished, another was performed in exactly the same manner; only the direction of the current was reversed, in order that the deflections might be observed on the other side of the meridian.

At 7 o'clock P.M. of the same day, two experiments were again made in the manner above described; but in these the quantity of electricity passed through the silver wire was only about half as much as before; five cells of the pile being now employed, instead of sixteen as before.

On the morning of September 5, two experiments of the same kind were made with a pile consisting of two cells in series; and on the evening of the same day, two experiments were made using only one of the constant cells.

On the two succeeding days all the above experiments were gone over again in the reverse order, beginning with one cell and ending with sixteen. In this way I sought to get rid of the mischievous effects of any change in the intensity of the earth's magnetism during the experiments.

The table of these results which I subjoin will easily be understood by means of the headings of the columns; and the only thing, therefore, which it will be necessary for me to say in explanation of it is, that the last column contains the results of observation corrected for the cooling or heating effect of the surrounding air. The amount of this correction was estimated by simple and decisive experiments, and was in no one instance found to exceed one-tenth of the quantity of heat evolved, even in the experiments with one cell, in which the heat evolved was least.

Table I.

Date of the experiments.	Number of cells in the pile.	Deflections of the needle of the galvanometer.		Corrected tangents of the mean deflections.	Square of the corrected tangents reduced to facilitate comparison with column 6.	Heat evolved in 5′ in divisions of the thermometer.	
Sept. 4, 7 a.m.	16	72° 17′	mean 72° 46′	3·2428	87·24	81·94	mean 87·24
Sept. 4, 7½ a.m.	16	73 1½				89·13	
Sept. 7, 7 p.m.	16	72 47				87·97	
Sept. 7, 7½ p.m.	16	72 58				89·90	
Sept. 4, 7 p.m.	5	61 8½	60 50¼	1·8015	26·92	27·08	26·56
Sept. 4, 7½ p.m.	5	61 26				27·98	
Sept. 7, 7 a.m.	5	60 19				25·39	
Sept. 7, 7½ a.m.	5	60 28				25·79	
Sept. 5, 7 a.m.	2	41 8	40 41	0·8634	6·18	6·23	6·03
Sept. 5, 7½ a.m.	2	41 7				6·27	
Sept. 6, 7 p.m.	2	40 5				5·76	
Sept. 6, 7½ p.m.	2	40 23				5·87	
Sept. 5, 7 p.m.	1	25 57	25 11½	0·4721	1·85	2·01	1·71
Sept. 5, 7½ p.m.	1	25 50½				1·83	
Sept. 6, 7 a.m.	1	24 40½				1·49	
Sept. 6, 7½ a.m.	1	24 18				1·53	
1	2	3		4	5	6	

In order to carry on the experiments with electric currents of feebler tension, I now introduced into the circuit an electrolytic cell, consisting of two plates of zinc immersed in a solution of sulphate of zinc. The results thus obtained are arranged in the following table. In order to collect an appreciable quantity of heat, the experiments were carried on for an hour with the lower intensities, and for half an hour with the highest intensity of current; I have, however, reduced all the results to five minutes, in order that they might be more readily compared with those of Table I. Each of the results given in Table II. is the mean of four experiments tried at different times, according to the principles which guided me in the former experiments.

Table II.

Number of cells in the pile.	Quantity of zinc deposited on the negative electrode per 5 minutes in milligrammes.	Deflections of the needle of the galvanometer.	Corrected tangents of the deflections.	Squares of the corrected tangents reduced to facilitate comparison with column 6.	Heat evolved per 5 minutes in divisions of the thermometer.
4	143	19° 22′	0·3527	1·03	0·96
2	82	11 14½	0·1991	0·33	0·29
1	44	6 2½	0·1059	0·09	0·09
1	2	3	4	5	6

By comparing the last two columns of the foregoing table with each other, we see that throughout a very extensive range of electric intensities the heat evolved in a given time remains proportional to the square of the quantity of transmitted electricity.

6. Having thus succeeded in giving another proof of the law of voltaic heat as far as regards a change in the intensity of the current, we may now proceed to consider the effects produced by a change in the resistance of the wire. It will not be necessary for me to enter very largely upon this part of the subject, inasmuch as it has long been admitted by philosophers that the heat evolved by a current of given intensity is proportional to the resistance of the wire. I will, however, give one series of experiments, in which I have compared a wire of mercury with the coil of silver wire used in the previous experiments. The comparison of a fluid with a solid metal was, I thought, eminently calculated to test the accuracy of the law.

A glass tube, 157 centimetres long and about 2·3 millimetres in internal diameter, was fashioned into a spiral, as represented in fig. 5. The tube was filled with mercury as high as the bulbs *aa*. Connexion could be established between the pile and the spiral by means of the copper wires *bb*, which dipped as far as the centre of the bulbs *aa*.

The coil of mercury, thus prepared, was immersed in 2 lbs. 11 oz. of water contained in a double-cased can, similar to the one I have already described, and a current from a pile of five cells was transmitted through it for ten minutes. The heat evolved, the temperature of the room, and the deflections of the galvanometer during the experiment, were carefully noted. Eight of these experiments were made, in four of which the deflections were on one side, and, in the other four, on the other side of the meridian.

Four experiments were made in a similar way with the coil of silver wire. In order to avoid the effects of any change in the intensity of the earth's magnetism, these four experiments were alternated with those made with the mercury coil. The thermometer used in all the experiments was one of great accuracy; and each division of its scale corresponded to $\frac{1}{23 \cdot 33}$ of a degree of the Centigrade scale.

Table III.

	Deflections of the galvanometer.	Corrected tangents of the deflections.	Squares of the corrected tangents.	Heat evolved per 10′ in divisions of the thermometer.	Difference between the mean temperature of the water and of the room.
Experiments with the mercury spiral.	60° 24′	1·7696	3·1315	45·5	0·61° C. +
	60 56	1·8086	3·2710	45·0	1·97 +
	57 20½	1·5683	2·4596	34·8	0·80 +
	58 17	1·6266	2·6458	38·4	0·80 +
	58 30½	1·6409	2·6926	40·1	0·87 −
	59 48	1·7271	2·9829	43·8	0·84 −
	57 11	1·5584	2·4286	35·2	1·11 −
	56 30	1·5184	2·3055	34·7	0·84 −
		Mean	2·7397	39·69	0·05 +
Experiments with the coil of silver wire.	55 54½	1·4847	2·2043	42·6	0·04 −
	56 19½	1·5083	2·2750	44·2	0·75 +
	54 38	1·4159	2·0048	39·4	0·40 −
	54 52	1·4282	2·0398	39·5	0·19 −
		Mean	2·1310	41·425	0·03 +

The resistance of the mercury wire in comparison with that of the silver wire, was found by ascertaining the intensity of the current produced by a pile of five cells:—1st, when the pile was in direct communication with the galvanometer; 2nd, when the resistance of the circuit was increased by the addition to it of the coil of silver wire; and 3rd, when the mercury wire was substituted in the circuit for the silver wire. Calling the intensity of the current in the first instance A, and the resistance y; in the second instance B, and the resistance $1+y$; and in the third instance C, and the resistance $x+y$, we have, by the laws of Ohm and Pouillet,

$$x=\frac{\mathrm{B(A-C)}}{\mathrm{C(A-B)}}.$$

The observations from which I have deduced the constant quantities of the above formula are arranged in the following table. In these experiments the precaution was taken that the temperature of the water in which the coils of mercury and silver were immersed should be as nearly as possible the same as in the experiments of Table III., in order to obviate the possibility of an alteration of the resistance arising from an alteration of the temperature of the metals. I may mention also, that each of the recorded deflections is the mean of two observations, one on one side, and the other (by reversing the direction of the current) on the other side of the magnetic meridian. The effect of any change in the intensity of the pile during any of the experiments was carefully guarded against by a repetition of each experiment in the reverse order; *i. e.* beginning with current C and ending with current A, and then taking the mean of the two sets of observations.

Table IV.

No. of experiment.	Deflection with resistance y.	Corrected tangent of deflection or A.	Deflection with resistance $1+y$.	Corrected tangent of deflection or B.	Deflection with resistance $x+y$.	Corrected tangent of deflection or C.	Resistance of mercury spiral or $\frac{B(A-C)}{C(A-B)}$.
1	73° 48′	3·4635	57° 10′	1·5574	60° 56′	1·8085	0·74771
2	71 10	2·9492	55 2	1·4370	58 39	1·6501	0·74814
3	72 25	3·1753	56 21	1·5098	59 54½	1·7346	0·75292
4	75 24	3·8630	58 37	1·6475	62 25	1·9242	0·74927
5	76 11	4·0916	58 47	1·6583	62 42	1·9477	0·75015
					Mean...............		0·74964

On multiplying 0·74964 by 2·7397, the square of the intensity of the current to which the mercury wire was exposed (see Table III.), we obtain 2·0538, a quantity which ought to be proportional to the heat evolved, if our law be correct. From Table III. we see also, that, in the case of the silver wire, the square of the current multiplied by its resistance (which we called unity) is 2·131, while the heat evolved was 41·425. Hence we have for the heat which ought to have been evolved by the mercury spiral,

$$\frac{2{\cdot}0538}{2{\cdot}1310}\times 41{\cdot}425=39{\cdot}924.$$

Referring again to Table III., we find that the heat actually evolved was 39·69. The difference between this number and the result of theory, trifling as it is, is almost entirely accounted for by the circumstance, that the capacity for heat of the mercury spiral exceeded that of the coil of silver wire by a quantity equal to the capacity of 5·64 grms. of water. Hence we must apply a correction of $\frac{1}{222}$ to the observations with the mercurial apparatus. This brings the heat actually evolved up to 39·868, a quantity differing from 39·924, the theoretical result, only by 0·056 of a division of the thermometer, or 0°·0024 of a degree Centigrade.

7. Having thus given fresh proofs of the accuracy of the law of the evolution of heat by voltaic electricity, we may now proceed to apply it in order to determine the quantity of heat evolved in chemical combinations. The following is an outline of my process:—I take a glass vessel filled with the solution of an electrolyte, and properly furnished with electrodes. I place this electrolytic cell in the voltaic circuit for a given length of time,

and carefully observe the quantity of decomposition and the heat evolved. By the law of Ohm I then ascertain the resistance of a wire capable of obstructing the current equally with the electrolytic cell. Then, by the law we have proved, I determine the quantity of heat which would have been evolved had a wire of such resistance been placed in the circuit instead of the electrolytic cell: this theoretical quantity, being compared with the heat actually evolved in the electrolytic cell, is always found to exceed the latter considerably. The difference between the two results evidently gives the quantity of heat absorbed during the electrolysis, and is therefore equivalent to the heat which is due to the reverse chemical combination by combustion or other means.

Having thus given a short outline of the process, I shall at once proceed to describe the experiments in detail.

1st. *Heat evolved by the Combustion of Copper.*

I took a glass jar, fig. 6, filled with 3 lbs. of a solution consisting of 24 parts of water, 7 parts of crystallized sulphate of copper, and 1 part of strong sulphuric acid. In this solution, two plates, one of platinum, the other of copper, were immersed, each being connected by means of a proper clamp with a thick copper wire passing through a cork in the mouth of the vessel, and terminating in a mercury cup *a*. A very delicate thermometer, each of whose divisions was equal to $\frac{1}{23\cdot38}$ of a degree Cent., was also fixed in the cork so as to have its bulb nearly in the centre of the liquid. Lastly, a glass stirrer *b* was introduced.

The experiments were conducted in the following manner:—A pile consisting of four large cells of Daniell (*a*, fig. 7) was connected with the galvanometer *b* by means of two thick copper wires, one of which was continuous, while the other was divided at the mercury cups *cc*. The connexion between these mercury cups was first established by means of a short thick copper wire, and the deflection of the needle noted. The quantity of current indicated by this deflection I shall call A. The thick copper wire was now removed from the cups at *cc*, and the standard coil of silver wire (immersed in water to keep it cool) was put there instead, and the deflection again noted. The current observed in this second instance I shall call B. The coil of silver wire was now removed, and the electrolytic cell above described being put in its stead, electrolysis was carried on for exactly 10′ of time, during which the deflections of the needle were noted at equal intervals of time. The current indicated by the mean of these observations I shall call C. Currents B and A were then again observed in the reverse order; and the mean of these and the former observations taken, so as to obviate the effects of any change that might be occurring in the intensity of the pile.

The temperature of the solution was observed, with the usual precautions, immediately before and after the electrolysis was carried on. The amount of electrolysis was obtained by weighing the negative copper electrode before and after each experiment.

Putting x for the resistance of a metallic wire capable of retarding the passage of the current equally with the electrolytic cell, and calling the resistance of the coil of silver wire unity, we have, as in the case of the coil of mercury,

$$x=\frac{(A-C)B}{(A-B)C};$$

this value, multiplied by C^2, gives $\frac{(A-C)BC}{A-B}$ for the calorific effect of the current C passing along a wire whose resistance $=x$.

The calorific effects of the standard coil of silver wire were ascertained by experiments made on the day before, and on the day after the experiments on electrolysis were performed. In this way I sought, as before, to avoid the injurious influence of a change, either in the intensity of the earth's magnetism, or in the resistance of the standard coil. The standard coil was immersed in a light tin can containing 2 lbs. 12 oz. of distilled water. The thermometer employed was that used in the experiments of electrolysis.

Table V.—Experiments on the Electrolysis of the Solution of Sulphate of Copper, with a pile of 4 Daniell's cells.

Current A.		Current B.		Current C.		Difference between the mean temperature of the solution and that of the room.	$\frac{A-C}{A-B}\times BC$.	Heat evolved in 10' in divisions of the thermometer.	Copper deposited on the negative electrode in grammes.
Mean deflection.	Corrected tangent.	Mean deflection.	Corrected tangent.	Mean deflection.	Corrected tangent.				
73° 31′	3·4006	53° 52′	1·3764	35° 55½′	0·7275	1°·24 C. −	1·3223	20·4	0·5686
74 59	3·7510	54 40	1·4176	36 53	0·7535	0·43 −	1·3722	19·4	0·5777
75 22	3·8538	54 45	1·4220	37 18	0·7650	0·23 −	1·3817	19·45	0·5881
75 12	3·8084	54 47	1·4237	38 26	0·7968	1·78 +	1·4326	17·4	0·6153
					Mean	0·03 −	1·3772	19·162	0·5874
							*Corrected for difference 0°·03 −	19·113	

* The corrections I have applied to the quantities of heat evolved were derived from experiments on the cooling of the liquids reduced by the law of Leslie to the difference between the mean temperature of the liquids and that of the room. The signs + or − signify that the temperature of the liquid is greater or less than that of the room.

Table VI.—Experiments on the Heat evolved by the Standard Coil. Pile of 4 cells.

Mean deflection of the needle of the galvanometer.	Corrected tangent.	Difference between the mean temperature of the water and that of the room.	Square of the corrected tangent.	Heat evolved in 10′ in divisions of the thermometer.
51° 7′	1·2462	1°·08 C. −	1·5530	30·0
51 18	1·2544	0·03 +	1·5735	31·0
53 38	1·3648	0·19 +	1·8627	37·1
54 47	1·4239	0·90 +	2·0275	36·9
Mean		0·01 +	1·7542	33·75
Corrected for the difference 0°·01+				33·76

In order to compare the results of the above tables, it now became necessary to ascertain the capacity for heat of the jars of liquid employed in the experiments. This was done in one or two instances by the method of mixtures. The jar along with its contents was heated to a certain point, and then having been immersed in a large can of cold water, the capacity was determined by the decrease of temperature in the former and the increase in the latter. I felt, however, that this plan was on several accounts incapable of giving results of extreme accuracy, and had therefore recourse to a method founded upon the law of the development of heat by electricity. The spiral glass tube (fig. 5), filled with mercury, was immersed up to the bulbs *aa* in the jar whose capacity for heat was to be determined. A current of electricity was then passed through the mercury for a given time, and the heat thereby evolved was observed with the usual precautions. The capacity of the jar and its contents was of course directly proportional to the square of the intensity of the current, and inversely to the increase of temperature.

Table VII.—Experiments on the Heat evolved by the Mercury Spiral in the jar of Solution of Copper used in the experiments of Table V. Pile of 4 cells.

Mean deflection of the galvanometer.	Corrected tangent.	Difference between the mean temperature of the solution and that of the room.	Square of the corrected tangent.	Heat evolved in 10′ in divisions of the thermometer.
57° 34′	1·5815	0°·05 C. +	2·5011	39·1
58 10½	1·6193	1·59 +	2·6221	37·0
58 17	1·6261	1·23 −	2·6442	43·4
59 0	1·6725	0·37 −	2·7973	41·3
Mean		0·01 +	2·6412	40·2
Corrected for difference 0°·01+				40·216
Corrected for capacity				40·622

Table VIII.—Experiments on the Heat evolved by the Mercury Spiral in the can of water used in the experiments of Table VI. Pile of 4 cells. 2 lbs. 11 oz. of water in the can.

Mean deflection of the galvanometer.	Corrected tangent.	Difference between the mean temperature of the water and that of the room.	Square of the corrected tangent.	Heat evolved in 10′ in divisions of the thermometer.
57° 26′	1·5734	1°·16 C. −	2·4756	35·6
57 27	1·5744	0·25 +	2·4787	36·6
59 11½	1·6853	1·01 −	2·8402	40·3
58 27	1·6367	1·85 +	2·6788	38·2
Mean.........		0·02 −	2·6183	37·675
Corrected for difference 0°·02 −				37·65
Corrected for capacity				36·99

Besides the correction on account of the difference between the mean temperature of the liquid and that of the room in which the experiments were made, it was necessary to supply the second correction given in the above tables, on account of the capacity for heat of the jars being necessarily somewhat different from what it was in the experiments of Tables V. and VI. In Table VI. the can contained 2 lbs. 12 oz. of water, and the coil of silver wire; whereas it contained 2 lbs. 11 oz. and the coil of mercury in Table VIII. Again, in the experiments of Table V. there were 3 lbs. of solution of copper along with the platinum and copper electrodes; whereas in those of Table VII. the mercury coil was substituted for the electrodes, whilst the weight of the solution was two grammes less than before. It would be tedious and unnecessary to give in detail the various reductions demanded by these circumstances; suffice it to say, that the calculations were founded upon the best tables of specific heat, and were made with the most scrupulous care.

The tin can containing (as in the experiments of Table VI.) the coil of silver wire and 2 lbs. 12 oz. of water, was found by careful calculations to be equivalent in its capacity for heat to 1283·7 grms. of water; consequently from Tables VII. and VIII. we obtain for the capacity of the jar of solution used in the experiments of Table V.,

$$\frac{2{\cdot}6412}{2{\cdot}6183} \times \frac{36{\cdot}99}{40{\cdot}622} \times 1283{\cdot}7 = 1179{\cdot}2.$$

Referring now to Tables V. and VI., and remembering that

23·38 divisions of the scale of the thermometer employed are equal to one degree of the Centigrade scale, we obtain for the quantity of heat due to $\frac{A-C}{A-B} \times BC$,

$$\frac{33{\cdot}76}{23{\cdot}38} \times \frac{1{\cdot}3772}{1{\cdot}7542} \times 1283{\cdot}7 = 1455^{\circ}{\cdot}3.$$

The quantity of heat actually evolved will be

$$\frac{19{\cdot}113}{23{\cdot}38} \times 1179{\cdot}2 = 963^{\circ}{\cdot}99.$$

Subtracting the latter from the former result, we obtain 491°·3 as the quantity of heat absorbed in the electrolysis of a quantity of sulphate of copper corresponding to 0·5874 of a gramme of copper. The quantity of heat absorbed per gramme of copper deposited will therefore be 836°·4.

Two other series of experiments conducted in precisely the same manner, excepting that in the former of the two the specific heat of the solution was obtained by the method of mixtures, gave, for the absorption of heat per gramme of copper deposited, respectively 856° and 796°·5. The mean of the three results is 829°·6.

The above quantity of heat is that absorbed in separating the copper and oxygen gas from a solution of sulphate of oxide of copper. It is therefore necessary to subtract the absorption due to the transfer of the sulphuric acid from the oxide of copper to water, in order to obtain the heat absorbed in the decomposition of oxide of copper into metal and oxygen gas. For this purpose, 8 grammes of oxide of copper, prepared by adding potash to a solution of the sulphate of copper, and then carefully washing and igniting the precipitate, were thrown into an acidulated solution of copper similar to that used in the above experiments, the capacity for heat of which had been previously ascertained. The mean of four experiments, tried in this way with every possible precaution, gave 236° as the heat due to the solution of 1·252 gramme, the quantity of oxide corresponding to a gramme of copper.

829°·6 − 236° = 593°·6 = the quantity of heat absorbed in the decomposition of oxide of copper into copper and oxygen gas, and which ought therefore to be the quantity of heat evolved by the combustion of a gramme of copper.

Combustion of Zinc.

My experiments on this metal were similar to those on copper; they will not therefore require a very detailed description. The solution employed was one consisting of 3 parts of crystallized sulphate of zinc and 8 parts of water, weighing 3 lbs. 2 oz. The electrodes were plates of platinum and zinc, each plate exposing an active surface of about 8 square inches. At the conclusion of each experiment, oxide of zinc was thrown into the solution to replace that removed by electrolysis, in order to prevent the zinc electrode from being acted upon by free acid.

Table IX.—Experiments on the Heat evolved by the Electrolysis of Sulphate of Zinc. Pile of 7 cells.

Current A.		Current B.		Current C.		Difference between the mean temperature of the solution and that of the room.		$\frac{A-C}{A-B} \times BC$.	Heat evolved in 10′ in divisions of the thermometer.	Zinc deposited on the negative electrode in grammes.
Mean deflection.	Corrected tangent.	Mean deflection.	Corrected tangent.	Mean deflection.	Corrected tangent.					
74° 10½′	3·5500	61° 46′	1·8722	35° 31′	0·7167	0°·83 C.	−	2·2659	32·2	0·5797
75 55½	4·0133	63 9	1·9858	35 14	0·7092	0·35	+	2·2951	30·0	0·5647
75 18	3·8356	62 30	1·9311	37 1½	0·7573	0·40	−	2·3638	31·3	0·6010
75 32½	3·9025	62 47	1·9546	36 56	0·7548	0·84	+	2·3841	30·5	0·5991
				Mean		0·01	−	2·3272	31·0	0·5861
						Corrected for difference 0°·01 −			30·984	

Table X.—Experiments on the Heat evolved by the Standard Silver Coil. Pile of 5 cells.

Mean deflection.	Corrected tangent.	Difference between the mean temperature of the water and that of the room.		Square of the corrected tangent.	Heat evolved in 10′ in divisions of the thermometer.
53° 58½′	1·3820	1°·15 C.	−	1·9099	36·3
53 48	1·3731	0·25	+	1·8854	37·0
56 26½	1·5150	0·44	−	2·2952	45·9
57 4½	1·5520	1·24	+	2·4087	44·4
	Mean.........	0·025	−	2·1248	40·9
		Corrected for difference 0°·025 −			40·869

The following tables give the results of the experiments for ascertaining the capacity for heat of the jar of solution.

Table XI.—Experiments on the Heat evolved by the Mercury Spiral in the jar of Solution of Sulphate of Zinc used in the experiments of Table IX. Pile of 5 cells.

Mean deflection.	Corrected tangent.	Difference between the mean temperature of the solution and that of the room.	Square of the corrected tangent.	Heat evolved in 10′ in divisions of the thermometer.
61° 17½′	1·8355	0°·48 C. −	3·3691	51·1
60 30	1·7768	1·61 +	3·1570	45·6
58 19	1·6287	0·47 −	2·6527	42·7
59 18½	1·6935	1·11 +	2·8679	40·5
Mean		0·442 +	3·0117	44·975
Corrected for difference 0·442 +				45·700
Corrected for capacity				46·269

Table XII.—Experiments on the Heat evolved by the Mercury Spiral in the can of water used in the experiments of Table X. Pile of 5 cells. 2 lbs. 11 oz. of water in the can.

Mean deflection.	Corrected tangent.	Difference between the mean temperature of the water and that of the room.	Square of the corrected tangent.	Heat evolved in 10′ in divisions of the thermometer.
60° 21′	1·7659	0°·66 C. −	3·1184	44·4
60 24	1·7696	1·25 +	3·1315	45·1
58 44½	1·6561	1·17 −	2·7427	40·7
59 16½	1·6913	0·51 +	2·8605	40·4
Mean		0·018 −	2·9633	42·65
Corrected for difference 0°·018 C. −				42·628
Corrected for capacity				41·881

From the last two tables we obtain for the capacity of the jar of solution used in the experiments of Table IX.,

$$\frac{3{\cdot}0117}{2{\cdot}9633} \times \frac{41{\cdot}881}{46{\cdot}269} \times 1283{\cdot}7 = 1180{\cdot}9.$$

From Tables IX. and X. we obtain for the quantity of heat due to $\frac{A-C}{A-B} \times BC$,

$$\frac{40{\cdot}869}{23{\cdot}38} \times \frac{2{\cdot}3272}{2{\cdot}1248} \times 1283{\cdot}7 = 2457^{\circ}{\cdot}7.$$

And for the actual quantity of heat evolved during electrolysis,

$$\frac{30{\cdot}984}{23{\cdot}38} \times 1180{\cdot}9 = 1565^{\circ}.$$

Hence $2457^{\circ}{\cdot}7 - 1565^{\circ} = 892^{\circ}{\cdot}7 =$ the quantity of heat absorbed in the electrolysis of a quantity of sulphate of zinc corresponding to 0·5861 of a gramme of zinc.

The quantity of heat absorbed by the electrolysis of a quantity of sulphate of zinc corresponding to a gramme of zinc will therefore be 1523°·1.

The results of two other series of experiments, conducted in precisely the same manner as that I have just given, were 1547° and 1619° respectively. The mean of the three results is 1563°.

The heat absorbed by the transfer of the sulphuric acid from the oxide of zinc to the water was ascertained in the following manner. A solution of zinc similar to that employed in the experiments was acidulated with about 10 grammes of sulphuric acid. 7·9 grms. of oxide of zinc (prepared by igniting the carbonate) were thrown into this solution; and the heat evolved by its union with the free sulphuric acid was carefully ascertained, and properly corrected for the influence of the atmosphere. The capacity for heat of the jar of solution was then ascertained by the method of electrical currents. This being done, a fresh quantity of oxide of zinc was thrown into the solution, and the heat evolved again observed. The mean of the two experiments gave 378° for the quantity of heat evolved by the solution of 1·242 grm., the quantity of oxide of zinc corresponding to a gramme of zinc.

$1563^{\circ} - 378^{\circ} = 1185^{\circ}$, the quantity of heat absorbed in the decomposition of oxide of zinc into zinc and oxygen gas; and which ought therefore to be the quantity of heat evolved by the combustion of a gramme of zinc.

Combustion of Hydrogen Gas.

The apparatus employed in the experiments on hydrogen is shown in fig. 8. *a* represents a glass jar nearly full of a solution consisting of six parts of water and one of strong sulphuric acid, and containing platinum electrodes; *b* represents a glass tube for conveying the mixed gases to the pneumatic trough *c*. The glass stirrer *d*, being inserted in the small cork *e*, can, when not in use, be made perfectly tight by inserting the latter into the large cork which stops up the mouth of the jar. A coating of a viscid solution of rosin in turpentine was applied wherever it appeared necessary, in order to ensure perfect tightness. The quantity of mixed gases evolved was ascertained by the weight of water displaced in the bottle *f*; and hence the weight of liberated hydrogen was computed with the assistance of the best tables, regard being paid to the temperature of the gas, its hygrometric state, the barometric pressure, &c.

Table XIII.—Experiments on the Electrolysis of Dilute Sulphuric Acid, spec. grav. 1·103. Pile of 6 cells.

Current A.		Current B.		Current C.					
Mean deflection.	Corrected tangent.	Mean deflection.	Corrected tangent.	Mean deflection.	Corrected tangent.	Difference between the mean temperature of the solution and that of the room.	$\frac{A-C}{A-B}\times BC$.	Heat evolved in 10′ in divisions of the thermometer.	Hydrogen liberated from the negative electrode in grammes.
73° 35½′	3·4170	59° 26′	1·7020	58° 14′	1·6234	1°·30 C. –	2·8897	40·6	0·03978
75 41	3·9429	60 57	1·8098	60 31	1·7780	0·34 +	3·2658	37·8	0·04212
76 57	4·3412	62 0	1·8906	61 19	1·8374	0·71 +	3·5492	41·7	0·04372
76 3	4·0508	61 13	1·8298	60 50	1·8011	1·85 +	3·3382	38·8	0·04411
					Mean	0·40 +	3·2607	39·725	0·04243
						Corrected for difference 0°·40+		40·381	

Table XIV.—Experiments on the Heat evolved by the Standard Silver Coil. Pile of 5 cells.

Mean deflection.	Corrected tangent.	Difference between the mean temperature of the water and that of the room.	Square of the corrected tangent.	Heat evolved in 10′ in divisions of the thermometer.
56° 12′	1·5012	0°·25 C. –	2·2536	42·1
55 40	1·4714	1·53 +	2·1650	41·1
55 37	1·4687	0·53 –	2·1571	43·4
56 27	1·5155	1·05 +	2·2967	41·7
	Mean	0·45 +	2·2181	42·075
		Corrected for difference 0°·45+		42·642

Table XV.—Experiments on the Heat evolved by the Mercury Spiral in the jar of Dilute Sulphuric Acid used in the experiments of Table XIII. Pile of 5 cells.

Mean deflection.	Corrected tangent.	Difference between the mean temperature of the solution and that of the room.	Square of the corrected tangent.	Heat evolved in 10′ in divisions of the thermometer.
60° 15½′	1·7594	0°·46 C. –	3·0955	49·9
60 39½	1·7883	1·59 +	3·1980	46·0
59 34½	1·7117	1·55 –	2·9299	46·6
58 52½	1·6648	0·38 +	2·7716	43·9
	Mean	0·01 –	2·9987	46·6
		Corrected for difference 0°·01–		46·584
		Corrected for capacity		47·164

Table XVI.—Experiments on the Heat evolved by the Mercury Spiral in the can of water used in the experiments of Table XIV. Pile of 5 cells. 2 lbs. 11 oz. of water in the can.

Mean deflection	Corrected tangent.	Difference between the mean temperature of the water and that of the room.	Square of the corrected tangent.	Heat evolved in 10′ in divisions of the thermometer.
59° 39′	1·7168	0°·47 C. −	2·9474	41·9
59 46½	1·7255	1·35 +	2·9774	42·6
59 10	1·6841	1·01 −	2·8362	42·8
59 37½	1·7151	0·74 +	2·9416	40·5
Mean		0·152 +	2·9256	41·95
Corrected for difference 0°·152 C. +				42·141
Corrected for capacity				41·402

From the above tables we obtain for the capacity for heat of the jar of dilute sulphuric acid used in the experiments of Table XIII.,

$$\frac{2{\cdot}9987}{2{\cdot}9256} \times \frac{41{\cdot}402}{47{\cdot}164} \times 1283{\cdot}7 = 1155.$$

For the quantity of heat due to $\frac{A-C}{A-B} \times BC$,

$$\frac{42{\cdot}642}{23{\cdot}38} \times \frac{3{\cdot}2607}{2{\cdot}2181} \times 1283{\cdot}7 = 3441^\circ{\cdot}8.$$

And for the actual quantity of heat evolved in the electrolysis,

$$\frac{40{\cdot}381}{23{\cdot}38} \times 1155 = 1994^\circ{\cdot}9.$$

Hence $3441^\circ{\cdot}8 - 1994^\circ{\cdot}9 = 1446^\circ{\cdot}9$, the quantity of heat absorbed during the electrolysis of a quantity of sulphate of water corresponding to 0·04243 of a gramme of hydrogen.

The quantity of heat absorbed by the electrolysis of a quantity of sulphate of water corresponding to a gramme of hydrogen will therefore be 34101°.

Two other series of experiments conducted in precisely the same manner, excepting that in the former of the two the capacity for heat of the jar of dilute acid was obtained by the method of mixtures, gave 34212° and 32358° respectively, as the heat absorbed per gramme of hydrogen liberated. The mean of the three results is 33557°.

A small portion of this quantity of heat absorbed is that due to the removal of water from the dilute acid; but the correction on this account is so exceedingly small as to be hardly worth applying. Subtracting 4°, however, on this account, we obtain 33553° as the quantity of heat absorbed during the electrolysis of water, which ought therefore to be equal to the quantity of heat evolved by the combustion of a gramme of hydrogen gas.

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8. By the inverse method of electrical currents, then, we have found that the quantities of heat evolved by the combustion of copper, zinc and hydrogen, are respectively 594°, 1185°, and 33553°. These quantities agree so well with the results obtained by Dulong, that I think I may assume that the principles admitted in this paper are demonstrated sufficiently to justify me in making them the basis of a few concluding observations.

The fact that the heat evolved in a given time by a metallic wire is proportional to the square of the quantity of transmitted electricity, proves that the action of the current is of a strictly mechanical character; for the force exerted by a fluid impinging against a solid body obeys the same law. Now I have shown in previous papers*, that when the temperature of a gramme of water is increased by 1° Centigrade, a quantity of *vis viva* is communicated to its particles equal to that acquired by a weight of 448 grammes after falling from the perpendicular height of one metre. Hence the mechanical force of a voltaic pile may be calculated from the heat which it evolves.

Hence also may the absolute force with which bodies enter into chemical combination be estimated by the quantity of heat evolved. Thus, from the data already given, the *vis viva* developed by the combustion of a gramme of copper, a gramme of zinc, and a gramme of hydrogen, will be respectively equivalent to the *vis viva* possessed by weights of 266112, 530880, and 15031744 grammes, after falling from the perpendicular height of one metre.

LXX. *The Bakerian Lecture.—Contributions to the Physiology of Vision.—Part the Second. On some remarkable, and hitherto unobserved, Phænomena of Binocular Vision (continued). By* Charles Wheatstone, *F.R.S., Professor of Experimental Philosophy in King's College, London*†.

[With a Plate.]

§ 17.

In § 3. of the first part of my "Contributions to the Physiology of Vision," published in the Philosophical Transactions for 1838‡, speaking of the stereoscope, I stated, "The pictures

* Philosophical Magazine, S. 3. vol. xxvii. p. 206.

† From the Philosophical Transactions for 1852, part i.; having been received and read by the Royal Society January 15, 1852.

‡ Reprinted in our April Number.—Ed. *Phil. Mag.*

will indeed coincide when the sliding pannels are in a variety of different positions, and consequently when viewed under different inclinations of the optic axes; but there is only one position in which the binocular image will be immediately seen single, of its proper magnitude, and without fatigue to the eyes, because in this position only the ordinary relations between the magnitude of the pictures on the retina, the inclination of the optic axes, and the adaptation of the eye to distinct vision at different distances, are preserved. The alteration in the apparent magnitude of the binocular images, when these usual relations are disturbed, will be discussed in another paper of this series, with a variety of remarkable phænomena depending thereon."

In 1833, five years before the publication of the memoir just mentioned, these yet unpublished investigations were announced in the third edition of Herbert Mayo's "Outlines of Human Physiology" in the following words:—"Mr. Wheatstone has shown, in a paper he is about to publish, that if by artificial means the usual relations which subsist between the degree of inclination of the optic axes and the visual angle which the object subtends on the retina be disturbed, some extraordinary illusions may be produced. Thus, the magnitude of the image remaining constant on the retina, its apparent size may be made to vary with every alteration of the angular inclination of the optic axes."

I shall resume the consideration of the phænomena of binocular vision with this subject, because the facts I have ascertained regarding it are necessary to be understood before entering on the new experiments relating to stereoscopic appearances which I intend to bring forward on the present occasion.

Under the ordinary conditions of vision, when an object is placed at a certain distance before the eyes, several concurring circumstances remain constant, and they always vary in the same order when the distance of the object is changed. Thus, as we approach the object, or as it is brought nearer to us, the magnitude of the picture on the retina increases; the inclination of the optic axes, required to cause the pictures to fall on corresponding places of the retinæ, becomes greater; the divergence of the rays of light proceeding from each point of the object, and which determines the adaptation of the eyes to distinct vision of that point, increases; and the dissimilarity of the two pictures projected on the retinæ also becomes greater. It is important to ascertain in what manner our perception of the magnitude and distance of objects depends on these various circumstances, and to inquire which are the most, and which the least influential in the judgements we form. To advance this inquiry beyond the point to which it has hitherto been brought, it is not sufficient to content ourselves with drawing conclusions from observations on the circumstances under which vision naturally occurs, as preceding writers on this subject mostly have done, but it is necessary to have more extended recourse to the methods so successfully employed in experimental philosophy, and to endeavour, wherever it be possible, not only to analyse the elements of vision, but also to recombine them in unusual manners, so that they may be associated under circumstances that never naturally occur.

The instrument I shall proceed to describe enables these abnormal combinations to be made in a very simple and effectual manner. Its principal object is to cause the binocular pictures to coincide, with any inclination of the optic axes, while their magnitudes on the retinæ remain the same; or inversely, while the optic axes remain at the same angle, to cause the size of the pictures on the retinæ to vary in any manner.

Two plane mirrors inclined 90° to each other are placed together and fixed vertically upon a horizontal board. Two wooden arms move round a common centre situated on this board in the vertical plane which bisects the angle of the mirrors, and about $1\frac{1}{2}$ inch beyond their line of junction. Upon each of these arms is placed an upright pannel, at right angles thereto, for the purpose of receiving its appropriate picture, and each pannel is made to slide to and from the opposite mirror. The eyes being placed before the mirrors, the right eye to the right mirror and the left eye to the left mirror, and the pannels being adjusted to the same distances, however the arms be moved round their centre, the distance of the reflected image of each picture from the eye will remain exactly the same, and consequently its retinal magnitude will be unchanged. But as the two reflected images do not occupy the same place when the pictures are in different positions, to cause the former to coincide the optic axes must converge differently. When the arms are in the same straight line, the images coincide while the optic axes are parallel; and as they form a less angle with each other, the optic axes converge more to occasion the coincidence. When the arms remain in the same positions, while the pannels slide towards or from the mirrors, the convergence of the optic axes remains the same, but the magnitude of the pictures on the retinæ increases as the distance decreases. By the arrangement described, and which is represented by figs. 1 and 2, Plate XII., the reflected pictures are always perpendicular to the optic axes, and the corresponding points of the pictures, when they are exactly similar, fall upon corresponding points of the retinæ. The instrument has an adjustment for otherwise inclining them if it be required.

Let us now attend to the effects produced. The pictures being fixed at the same distance from the mirrors, there is a certain adjustment of the arms at which the binocular image will appear of its natural size, that is, the size we judge the picture itself to be when we look at it directly; in this case the magnitude of the pictures on the retinæ and the inclination of the optic axes preserve their usual relation to each other. If now the arms be moved back, so as to cause a less convergence of the axes, the image will appear to increase in magnitude until the arms are in a straight line and the optic axes are parallel; and, on the other hand, if the arms be moved forwards, so as to form a less angle, the optic axes will converge more, and the image will appear gradually smaller. In this manner, while the retinal magnitude remains the same, the perceived magnitude of the binocular object varies through a very considerable range.

The instrument being again adjusted so that the image shall be seen of its natural size; on sliding the pictures nearer the mirrors its perceived magnitude will be augmented, and on sliding them from the mirrors it will appear diminished in size. During these variations of magnitude the inclination of the optic axes remains the same.

The perceived magnitude of an object, therefore, diminishes as the inclination of the axes becomes greater, while the distance remains the same; and it increases, when the inclination of the axes remains the same, while the distance diminishes. When both these conditions vary inversely, as they do in ordinary vision when the distance of an object changes, the perceived magnitude remains the same*.

Before I proceed further it will be proper to explain the meaning of some of the terms I employ. I call the magnitude of the object itself, the real or objective magnitude; the magnitude of the picture on the retina, the retinal magnitude; and the magnitude we estimate the object to be from its retinal magnitude and the inclination of the optic axes conjointly, I name the perceived magnitude. I do not use the term apparent magnitude, because, according to its ordinary acceptation, it sometimes means what I call retinal, and at other times what I name perceived magnitude.

We have seen in what manner our perception of magnitude is modified by the new associations which this instrument enables us to form; let us now examine how our perception of distance

* Several cases of the alteration of the perceived magnitude of objects are mentioned by Dr. R. Smith (Complete System of Optics, 1738, vol. ii. p. 388, and rem. 526 and 532); and Dr. R. Darwin (Philosophical Transactions, vol. lxxvi. p. 313) observed that when an ocular spectrum was impressed on both eyes it appeared magnified when they were directed to a wall at a considerable distance. The facts noticed by these authors are satisfactorily explained by the above considerations.

is affected by them. If we continue to observe the binocular picture whilst it apparently increases or decreases, in consequence of the inclination of the optic axes varying while the magnitude of the impressions on the retinæ remains the same, it does not appear either to approach or to recede; and yet if we attentively regard it in any fixed position, it is perceived to be at a different distance. On the other hand, if we continue to regard the binocular picture, enlarging and diminishing in consequence of the change of retinal magnitude while the convergence of the axes remains the same, we perceive it to approach or recede in the most evident manner; but on fixing the attention to it, when it is stationary, at any instant, it appears to be at the same distance at one time as it is at another.

Convergence of the optic axes therefore suggests fixed distance to the mind; variation of retinal magnitude suggests change of distance. We may, as I have above shown, perceive an object approach or recede without appearing to change its distance, and an object to be at a different distance, without appearing to approach or recede; these paradoxical effects render it difficult, until the phænomena are well apprehended, to know, or to express, what we actually do perceive.

It is the prevalent opinion that the sensation which accompanies the inclination of the optic axes immediately suggests distance, and that the perceived magnitude of an object is a judgement arising from our consciousness of its distance and of the magnitude of its picture on the retina. From the experiments I have brought forward, it rather appears to me that what the sensation which is connected with the convergence of the axes immediately suggests is a correction of the retinal magnitude to make it agree with the real magnitude of the object; and that distance, instead of being a simple perception, is a judgement arising from a comparison of the retinal and perceived magnitudes. However this may be, unless other signs accompany this sensation the notion of distance we thence derive is uncertain and obscure, whereas the perception of the change of magnitude it occasions is obvious and unmistakeable.

To see, in their full extent, the variations of magnitude exhibited by the instrument I have described, it is necessary to attend to the following observations.

As the inclination of the optic axes corresponding to a different distance is habitually, under ordinary circumstances, accompanied with the particular adaptation of the eyes required for distinct vision at that distance, it is difficult to disassociate these two conditions so as to see with equal distinctness the binocular picture when the optic axes are parallel, and when they converge greatly, although the pictures remain, in both cases, at the same distance from the eyes. The adaptation is, therefore, not entirely dependent on the divergence of the rays of light which proceed from the object regarded, but also, in some degree, on the inclination of the optic axes. I have acquired by practice considerable power of adjustment, or rather disadjustment, of the eyes, and can, without having recourse to artificial means, see the binocular picture distinctly when its perceived magnitude is widely different. Those to whom such an effort is painful may employ short-sighted spectacles to see the binocular picture when the eyes converge within the limit of distinct vision for the distance at which the pictures are placed; and long-sighted spectacles when the eyes converge beyond that limit, or become parallel.

There is a means of avoiding to a very considerable extent the influence of the adjustment of the eyes, and thereby enabling the pictures to be seen distinctly within the entire range of the inclination of the optic axes. This is by looking at the reflected images in the mirrors through two very minute apertures, not larger than fine pin-holes, placed near each eye, and illuminating the pictures by a very strong light; sunshine in the middle of the day answers the purpose very well. By this expedient the divergence of the rays of light is greatly diminished, and the adaptation of the eyes does not materially influence the result.

§ 18.

Leaving this subject, I will now revert to the stereoscope and its effects.

Since 1838 numerous modifications of the stereoscope have occurred to me, and several ingenious arrangements have also been proposed by Sir David Brewster and Professor Dove; but there is no form of the instrument which has so many advantages for investigating the phænomena of binocular vision as the original reflecting stereoscope. Pictures of any size may be placed in it, and it admits of every kind of adjustment.

I have constructed a very portable reflecting stereoscope which is represented at fig. 3. The sides fold over the mirrors, and the mirrors then fold into a box, which is not larger than six inches in any of its dimensions. To avoid the second feeble reflexion from the anterior surface of the silvered glass, which has a bad effect when the attention is attracted to it, I have sometimes employed reflecting prisms. The reflecting surfaces of the prisms should be silvered in order to obviate the unequal brightness of the field of view on each side of the limit of total reflexion; and as it would be too costly to employ very large prisms, they should have an adjustment to accommodate their distance to the width between the eyes of the observer.

I have, for many years past, employed also another means to occasion, without any straining of the eyes, the coincidence of the pictures so that the image in relief shall appear of the same magnitude and at the same distance as the object which they represent would do if it were itself directly regarded. In this apparatus, prisms being employed to deflect the rays of light proceeding from the pictures, so as to make them appear to occupy the same place, I have called it the refracting stereoscope.

It is represented by fig. 4. It consists of a base 6 inches long and 4 inches broad, upon which stands an upright partition, 5 inches high, dividing it equally; this partition is capable of extension by means of a slide to double the length, and carries at its upper extremity a board placed parallel to the base, and of the same dimensions. In this upper board there are two apertures an inch square, one on each side of the partition, the centres of which are $2\frac{1}{2}$ inches from each other; in these apertures are fixed a pair of glass prisms having their faces inclined 15°, and their refractive angles turned towards each other. The stereoscope pictures are to be placed on the base, and their centres ought not to exceed the distance of $2\frac{1}{2}$ inches.

A pair of plate-glass prisms, their faces making with each other an angle of 12°, will bring two pictures, the corresponding points of which are $2\frac{1}{2}$ inches apart, to coincidence at a distance of 12 inches, and a pair with an angle of 15° will occasion coincidence at 8 inches.

The refracting stereoscope has the advantage of portability, but it is limited to pictures of small dimensions. It is well suited for Daguerreotypes, which are usually of small size, and, on account of the nature of their reflecting surface, must be viewed in a particular direction with respect to the light which falls upon them; whereas in the reflecting stereoscope it is somewhat difficult to render the two Daguerreotypes equally visible. For drawings and Talbotypes it however offers no advantages, though it is equally well suited for them when their dimensions are small.

Stereoscopic drawings afford a means of illustrating works with figures of three dimensions, instead of with mere plane representations. Works on crystallography, solid geometry, spherical trigonometry, architecture, machinery, &c., might be thus rendered more instructive, from the perfect counterpart of the solid figure seen from a single point of view being represented instead of merely one of its plane projections. For this purpose the corresponding binocular figures must be engraved in parallel vertical columns, and their coalescence may be effected by viewing them through a pair of prisms, similar to those employed in the refracting stereoscope, placed in a frame at the proper distance from each other. If the engravings should be less than $2\frac{1}{2}$ inches apart, the prisms may be dispensed with by persons who have command over the adaptation of their eyes, particularly if they be short-sighted.

§ 19.

At the date of the publication of my experiments on binocular vision, the brilliant photographic discoveries of Talbot, Niepce and Daguerre, had not been announced to the world. To illustrate the phænomena of the stereoscope I could therefore, at that time, only employ drawings made by the hands of an artist. Mere outline figures, or even shaded perspective drawings of simple objects, do not present much difficulty; but it is evidently impossible for the most accurate and accomplished artist to delineate, by the sole aid of his eye, the two projections necessary to form the stereoscopic relief of objects as they exist in nature with their delicate differences of outline, light and shade. What the hand of the artist was unable to accomplish, the chemical action of light, directed by the camera, has enabled us to effect.

It was at the beginning of 1839, about six months after the appearance of my memoir in the Philosophical Transactions, that the photographic art became known, and soon after, at my request, Mr. Talbot, the inventor, and Mr. Collen (one of the first cultivators of the art) obligingly prepared for me stereoscopic Talbotypes of full-sized statues, buildings, and even portraits of living persons. M. Quetelet, to whom I communicated this application and sent specimens, made mention of it in the Bulletins of the Brussels Academy of October 1841. To M. Fizeau and M. Claudet I was indebted for the first Daguerreotypes executed for the stereoscope. The beautiful stereoscopic representations of statuary, architecture, machinery, natural history specimens, portraits of living persons, single and in groups, &c., which have recently been produced by M. Soleil and M. Claudet, are now too well known to the public to need more than a slight reference to them.

With respect to the means of preparing the binocular photographs (and in this general term I include both Talbotypes and Daguerreotypes), little requires to be said beyond a few directions as to the proper positions in which it is necessary to place the camera in order to obtain the two required projections.

We will suppose that the binocular pictures are required to be seen in the stereoscope at a distance of 8 inches before the eyes, in which case the convergence of the optic axes is about 18°. To obtain the proper projections for this distance, the camera must be placed, with its lens accurately directed towards the object, successively in two points of the circumference of a circle of which the object is the centre, and the points at which the camera is so placed must have the angular distance of 18° from each other, exactly that of the optic axes in the stereoscope. The distance of the camera from the object may be taken arbitrarily; for, so long as the same angle is employed, whatever that distance may be, the pictures will exhibit in the stereoscope the same relief, and be seen at the same distance of 8 inches, only the magnitude of the picture will appear different. Miniature stereoscopic representations of buildings and full-sized statues are therefore obtained merely by taking the two projections of the object from a considerable distance, but at the same angle as if the object were only 8 inches distant, that is, at an angle of 18°.

To produce the best effect, it is necessary that the pictures be so placed in the stereoscope that each eye shall see its respective picture at the proper point of sight: if this condition be not attended to, the binocular perspective will be incorrect.

For obtaining binocular photographic portraits, it has been found advantageous to employ, simultaneously, two cameras fixed at the proper angular positions.

I subjoin a Table of the inclinations of the optic axes which correspond to different distances; it also shows the angular positions of the camera required to obtain binocular pictures which shall appear at a given distance in the stereoscope in their true relief.

Inclination of the optic axes	2°	4°	6°	8°	10°	12°	14°	16°	18°	20°	22°	24°	26°	28°	30°
Distance in inches..........	71·5	35·7	23·8	17·8	13·2	11·8	10·1	8·8	7·8	7·0	6·4	5·8	5·4	5·0	4·6

The distance is equal to $\frac{a}{2}$ cotang $\frac{\theta}{2}$; a denoting the distance between the two eyes, and θ the inclination of the optic axes.

§ 20.

As the inclination of the optic axes diminishes by the removal of an object to which they are directed to a greater distance, not only does the magnitude of the pictures projected by it on the retinæ proportionately diminish, but the dissimilarity of the pictures becomes less. The difference of distance between any two points of each of the pictures will diminish until the projections become sensibly similar. Under the usual circumstances attending the vision of a solid object placed at a given distance, a particular inclination of the axes is invariably accompanied by a specific pair of dissimilar projections; and if the distance be changed, a different inclination of the axes is accompanied by another pair of projections; but, by means of the stereoscope, we have it within our power to associate these circumstances abnormally, and to cause any degree of inclination of the axes to coexist with any dissimilarity of the two pictures. To ascertain experimentally what takes place under these circumstances, M. Claudet prepared for me a number of Daguerreotypes of the same bust, taken at a variety of different angles, so that I was enabled to place in the stereoscope two pictures taken at any angular distance from 2° to 18°, the former corresponding with a distance of about 6 feet, and the latter with a distance of about 8 inches. The effect of a pair of near projections seen with a distant convergence of the optic axes, is to give an undue elongation to lines joining two unequally distant points, so that all the features of a bust appear to be exaggerated in depth. The effect, on the contrary, of a pair of distant projections, seen with a *near* convergence of the axes, is to give an undue shortening to the same lines, so that the appearance of a bas-relief is obtained from the two projections of the bust. The apparent dimensions in breadth and height remain in both cases the same.

§ 21.

To reproduce the conditions of the binocular vision of a solid object as completely as possible by means of its two plane projections, it is necessary, as I have before stated, that the projections shall be such as correspond exactly with the inclination of the optic axes under which they are viewed. I have already shown in § 20 what takes place when this condition is not strictly observed, and I may add, that the mind is not unpleasantly affected by a considerable incongruity in this respect; on the contrary, the effect in many cases seems heightened by viewing the solid appearance, intended for a determinate degree of inclination of the axes, under an angle several degrees less; the reality is as it were exaggerated. When the optic axes are parallel, in strictness there should be no difference between the pictures presented to each eye, and in this case there would be no binocular relief; but I find that an excellent effect is produced when the axes are nearly parallel by pictures taken at an inclination of 7° or 8°, and even a difference of 16° or 17° has no decidedly bad effect.

This circumstance enables us to combine the ideal amplification arising from viewing pictures placed near the eyes under a small inclination, or even parallelism, of the optic axes mentioned in § 17, with the perception of solidity arising from the dissimilarity of the projections; for this purpose, the pictures in the refracting stereoscope, or their reflected images in the reflecting instrument, must be viewed through lenses the focal distance of which is equal to the distance between them and the pictures; the perceived magnitude of the binocular image will increase with the nearness of the pictures, and depends almost entirely on the disassociation of the retinal magnitude from its usually accompanying inclination of the optic axes, the actual magnifying power of the lenses having a very small influence.

The sole use of the lenses is to render the rays of light parallel, which it is necessary they should be for distinct vision when the optic axes are parallel. When the reflecting stereoscope is employed, this means of magnifying the effect is not of much utility, as pictures of any size may be adapted to that instrument. But in the case of the refracting stereoscope it may be advantageously made use of. By combining lenses with the refracting stereoscope, described in § 18, Daguerreotypes somewhat wider than the width between the eyes may be employed. Sir David Brewster has used, to effect the same purpose, semi-lenses with their edges directed towards each other, which serve at the same time to render the rays less convergent and slightly to displace the pictures towards each other. Two corresponding Daguerreotypes, each not exceeding in breadth the width between the eyes, being placed close to each other, and viewed with lenses of short focal distance, will, even without the aid of the prisms, give an apparently highly magnified binocular image in bold relief.

There is a peculiarity in such images worthy of remark; although the optic axes are parallel, or nearly so, the image does not appear to be referred to the distance we should, from this circumstance, suppose it to be, but it is perceived to be much nearer, and indeed more so, as the pictures are nearer the eyes, though the inclination of the optic axes remains the same, and should therefore suggest the same distance; it seems as if the dissimilarity of the projections, corresponding as they do to a nearer distance than that which would be suggested by the former circumstance alone, alters in some degree the perception of distance.

I recommend, as a convenient arrangement of a refracting stereoscope for viewing Daguerreotypes of small dimensions, the instrument represented, Pl. XII. fig. 4, shortened in its length from 8 inches to 5, and lenses of 5 inches focal distance placed before and close to the prisms.

§ 22.

I now proceed to another subject—to the consideration of those phænomena which I have termed Conversions of Relief.

In § 5 of my first memoir I noticed the remarkable circumstance, that when the drawing intended to be seen by the right eye is presented to the left eye in the stereoscope, and *vice versâ,* a totally different solid figure is perceived to that seen before the transposition. I called this the converse figure, and showed that it differs from the normal figure in the circumstance, that those points which appear the most distant in the latter, appear the nearest in the former.

The pictures being, in the first place, presented directly to their corresponding eyes, as in the refracting stereoscope, and exhibiting therefore the resultant image in its normal relief, the conversion of the relief may be effected in three different ways,—1st, by transposing the pictures from one eye to the other, as mentioned above; 2ndly, by reflecting the pictures, while they remain presented to the same eye, as in the reflecting stereoscope; and 3rdly, by inverting the position of the pictures without transposing them.

The following considerations will explain the cause of the conversion of relief in the preceding cases.

If two different objects, or parts of an object (fig. 5*a*), have a greater lateral distance between them on the right-hand picture than that which they have on the left-hand picture, the optic axes must converge more to make the left-hand than to make the right-hand objects coincide, and the left-hand object will appear the nearest.

If the pictures be now transposed from one eye to the other (fig. 5 a'), the greatest distance will be between the corresponding points of the picture presented to the left eye; the optic axes must therefore converge less to make the left-hand objects coincide, and the right-hand object will appear the nearest.

If the pictures, remaining untransposed, be each separately reflected (fig. 5 *b*), the relative distances of the corresponding objects remain the same to each eye, and the left-hand object will still appear nearest; but in consequence of the lateral inversion of the objects in each picture by reflexion, that which was previously on the left will now be on the right, and therefore the object which before appeared nearest will now appear furthest.

When the pictures are turned upside down, still remaining untransposed (fig. 5 *c*), the objects are reversed with respect to the right and left, in the same manner as they are when reflected, and the lateral distances between the objects remaining the same to each eye, precisely the same conversion of relief is produced as in the preceding case, except that the resultant image is inverted. The diagram (fig. 5) represents all the possible changes of the two binocular pictures; those marked N show the normal relief, and those marked C the converse relief.

But it may be asked why, if the reflexion or inversion of the binocular pictures of an object gives rise to the mental idea of the converse relief, the same converse relief is not observed when the object itself is reflected in a mirror, or inverted. The reason is this; that in the former cases the projections to each eye are separately reflected or inverted, still remaining presented to the same eye, whereas, by the reflexion or inversion of the object itself, not only are the projections reflected or inverted, but they are also transposed from one eye to the other; and these circumstances occurring simultaneously reproduce the normal relief.

Fig. 6 will render this evident in the case of reflexion: A is the object, B its reflexion in the mirror CD; RB and LB are the directions in which the right and left eyes view the reflected image respectively, and *l*A and *r*A the directions in which the eyes would view the corresponding face of the object directly.

In the case of an inverted object, it is obvious that that projection which was before seen by the right eye must be seen by the left eye, and the contrary.

It is possible to make this normal or converse relief appear while one of the pictures remains constantly presented to the same eye. This result may be thus obtained. Having taken a photograph of the object, which should be one the converse of which has a meaning, take two others at the same angular distance (say 18°), one on the right side, the other on the left side of the original. Of the three pictures thus taken, if the middle one be presented to the right eye, and the left picture to the left eye, a normal relief will be seen; but if the right picture be presented to the left eye, the other remaining unchanged, a converse relief will be seen. In like manner, if the middle picture be presented to the left eye, and the right picture to the right eye, a normal relief will appear; but if the left picture be presented to the right eye, the converse relief will present itself. It must be observed, that the normal and converse reliefs, when the same picture remains presented to the same eye, belong to two different positions of the object.

§ 23.

Hitherto I have taken into consideration only those cases of the conversion of relief which are exhibited by binocular pictures in the stereoscope, when they are transposed, reflected or inverted; I shall now proceed to show how phænomena of the same kind may be elicited by regarding objects themselves, by means of an instrument adapted for the purpose. As this instrument conveys to the mind false perceptions of all external objects, I have called it the Pseudoscope. It is represented by fig. 7, and is thus constructed: two rectangular prisms of flint glass, the faces of which are 1·2 inch square, are placed in a frame with their hypothenuses parallel, and 2·1 inches from each other; each prism has a motion on an axis corresponding with the angle nearest the eyes, so that they may be adjusted that their bases may have any inclination towards each other; and the frame itself is adjustable by a hinge at *a*, in order to bring the prisms nearer each other to suit the eyes of the observer.

The instrument being held to the eyes, and adjusted to an object, so that it shall appear single, each eye will see a reflected image of that projection of the object which would be seen by the same eye without the pseudoscope. This is exactly the contrary of what occurs when the eyes regard the reflected image of an object in a looking-glass; the left eye then sees the reflected image of the right-hand projection, and the right eye the reflected image of the left projection, as shown by fig. 6.

Plane mirrors cannot be substituted for the reflecting prisms, for this reason; the refraction of the rays of light at the incident and emergent surfaces of the prisms enables the reflexion of an object to be seen when the object is even behind the prolongation of the reflecting surface, as shown at fig. 8, and thus the reflected binocular image may be seen in the same place as the object itself, whereas the images cannot be made by means of plane mirrors thus to coincide.

When the pseudoscope is so adjusted as to see a near object while the optic axes are parallel, to view a more distant object with the same adjustment, the axes must converge, and the more so as the object is more distant; all nearer objects than that seen when the axes are parallel, will appear double, because the optic axes can never be simultaneously directed to them. If this instrument be so adjusted that very distant objects are seen single when the eyes are parallel, *all* nearer objects will appear double, because the optic axes can never converge to make their binocular images coincide. If the attention is required to be directed to an object at a particular distance, the best mode of viewing it with the pseudoscope is to adjust the instrument so that the object shall appear at the proper distance and of its natural size. In this case the more distant objects will appear nearer and smaller, and the nearer objects will appear more distant and larger.

In ordinary vision, whenever the distance of an object varies, the magnitude of the picture on the retina, and the degree of convergence of the optic axes, always maintain a constant relation to each other, both increasing or decreasing together; and the perceived magnitude, suggesting to the mind the real magnitude of the object, in consequence thereof remains the same. The instrument I described in § 17 shows what illusions arise when the usual relations of these elements of our perceptions are disturbed, by causing one to remain constant while the other varies. The pseudoscope exhibits the still more curious illusions which result from combining these elements inversely; so that as an object becomes nearer, its larger picture on the retina is accompanied by a less convergence of the optic axes. With the pseudoscope we have a glance, as it were, into another visible world, in which external objects and our internal perceptions have no longer their habitual relation with each other.

I will now proceed to describe some of the illusions produced by the aid of this instrument. Those which may be strictly designated conversions of relief, in which the illusive appearance has the same relation to that of the real object as a cast to a mould, or a mould to a cast, are very readily perceived. I must however remark, that it is necessary to illuminate the object equally, so as to allow no lights or shades to appear upon them, for their presence has a considerable influence on the judgement, and is one of the principal causes of the perception of the proper relief when a single eye is employed.

The inside of a tea-cup appears as a solid convex body; the effect is more striking if there are painted figures within the cup.

A china vase, ornamented with coloured flowers in relief, presents a very remarkable appearance; we apparently see a vertical section of the interior of the vase, with painted hollow impressions of the flowers.

A small terrestrial globe appears as a concave hemisphere; on turning it round on its axis, it was curious to see different portions of the spherical map appear and disappear in a manner that nothing in external nature can imitate.

A bust regarded in front becomes a deep hollow mask; the appearance when regarded in profile is equally striking.

A framed picture hanging against a wall, appears as if imbedded in a cavity made in the wall.

A medal, or the impression of a seal, is perfectly converted into a representation of the die from which it has been struck; and, on the other hand, the mould or die of a medal, or an engraved seal, becomes a *fac-simile* of the medal or raised impression. It will also be observed, that if the medal be placed on a flat surface, as a sheet of paper, it will appear sunk beneath the surface; and if it be placed in a hollow of the same size, it will appear to stand above the surface as much as it actually is below it.

These appearances are not always immediately perceived; and some much more readily present themselves than others. Those converse forms which have a meaning, and resemble real forms we have been accustomed to see, are those which are the most easily apprehended. Viewed with the pseudoscope, notwithstanding the inversion of the pictures on the retina, the natural appearance of the object continues to intrude itself, when sometimes suddenly, and at other times gradually, the converse occupies its place. The reason of this is, that the relief and distance of objects is not suggested to the mind solely by the binocular pictures and the convergence of the optic axes, but also by other signs, which are perceived by means of each eye singly; among which the most effective are the distributions of light and shade and the perspective forms which we have been accustomed to see accompany these appearances. One idea being therefore suggested to the mind by one set of signs, and another totally incompatible idea by another set, according as the mental attention is directed to the one and abstracted from the other, the normal form or its converse is perceived. This mental attention is involuntary; no immediate effort of the will can call up one idea while the other continues to present itself, though the transition may be facilitated by intentionally removing some of the signs which suggest the preponderating idea; thus the converse form being perceived, closing either eye will most frequently cause an instant reversion to the normal form; and always, if the monocular signs of relief are sufficiently suggestive.

I know of nothing more wonderful, among the phænomena of perception, than the spontaneous successive occurrence of these two very different ideas in the mind, while all external circumstances remain precisely the same. Thus a small statuary group, an elegant and beautiful object, without any apparent cause becomes converted into another totally dissimilar object uncouth in appearance, and which gives rise to no agreeable emotions in the mind; yet in both cases all the sensations that intervene between objective reality and ideal conception continue unchanged.

The effects of the pseudoscope I have already mentioned, may be strictly called conversions of relief, because the illusive appearance is in each case the converse impression of the relief of the real object. If, however, the object consists of parts detached from and behind each other, the preceding term is inappropriate to denote the effects which result, but the more general expression conversion or inversion of distance may be employed to designate them. I proceed to call attention to a few such effects.

Skeleton figures of geometrical solids, as cubes, pyramids, &c., readily show their converse.

Two objects at different distances, being simultaneously regarded, the most remote will appear the nearest and the nearest the most remote.

An ivory foot-rule, held immediately before the eyes a little inclined to the horizon with its remote end elevated, appears inclined in the opposite way, its nearer end elevated, and as if the observer were looking at its lower surface. Its form also undergoes a change. Since the nearest end, the retinal magnitude of which is the largest, appears farthest from the eyes, and the nearest end, the retinal magnitude of which is greatest, appears near the eyes, the rule will no longer be perceived to be rectangular, but trapezoidal. If the rule be placed horizontally, and it be regarded with the pseudoscope at an angle of 45°, it will appear with the form just described standing vertically.

Any object placed before the wall of a room will appear behind the wall, and as if an aperture of the proper dimensions had been made in the wall to allow it to be seen; if the object be illuminated by a candle, its shadow will appear as far before the object as in reality it is behind.

The appearance of a plant is very remarkable; as the branches which are furthest from the eye are perceived to be the nearest, those parts which are actually obscured by the branches before them, appear broken away and allow the parts apparently behind them to be seen. A flowering shrub before a hedge appears to be transferred behind it; and a tree standing outside a window may be brought visibly within the room in which the observer is standing.

I have before observed, that the transition from the normal to the converse perception is often gradual; I will give one instance of this as an illustration. The object was a page of medallions embossed on card-board, and the raised impressions were protected from injury by a thick piece of mill-board having apertures in it made to correspond to each medallion. The page was placed horizontally, illuminated by a candle placed beyond it, and looked at through the pseudoscope at an angle of 45°; for the first moment the page appeared as it would have done without the instrument; soon after the medallions appeared level with the upper surface, and the shadows on the upper parts of the circular apertures were converted into deep depressions as if cut out with a tool; they next, from horizontal, became vertical, each standing erect on the horizontal plane, and immediately afterwards the reliefs were all changed into hollows; finally, the page itself stood vertical, but with that change of form which I indicated in the case of the rule, the upper edge appearing much shorter than the lower edge: the series of changes being now complete, the final form remained constant as long as the object was regarded.

In endeavouring to analyse the phænomena of converse perception, it must be borne in mind that the transposition of distances has reference only to distances from the retinæ, not to absolute horizontal distances in space. Thus, if a straight ruler be held in the vertical plane perpendicular to the optic base, and also inclined 45° to the horizon so that its upper end shall be the most distant, when the eyes are directed horizontally towards it, the rule will appear exactly in the converse position. If the rule be now removed lower down in the same vertical plane, its inclination remaining unchanged, so that to look upon it the plane of the optic axes must be inclined 45°, it will appear unaltered in position, because its two pictures are parallel on the retinæ, and the optic axes would require the same convergence to make the upper and lower ends coalesce. The rule being removed still lower down, instead of its position being apparently reversed, it will appear to have a greater inclination on the same side than the object itself has. In the first case the more distant end is actually furthest from the eyes; in the second, the near and remote ends are equally distant; and in the third the nearest end is most distant.

Attention to what I have just stated will explain many anomalous circumstances which occur when the eyes are differently directed towards the same object. It may also be necessary to remark, that the conversion of distance takes place only within those limits in which the optic axes sensibly converge, or the pictures projected on the retinæ are sensibly dissimilar. Beyond this range there is no mutual transposition of the apparent distances of objects with the pseudoscope; a distant view therefore appears unchanged.

Some very paradoxical results are obtained when objects in motion are viewed through the pseudoscope. When an object approaches, the magnitude of its picture on the retinæ increases as in ordinary vision; but the inclination of the optic axes, instead of increasing, becomes less, as I have already explained. Now an enlargement of the picture on the retina invariably suggests approach, and a less convergence of the optic axes indicates that the object is at a greater distance; and we have thus two contradictory suggestions. Hence, if two objects be placed side by side at a certain distance before the eyes, and one of them be moved forwards, so as to vary its distance from the other, its continually enlarging picture on the retina makes it appear to come towards the eyes, as it actually does, while at the same time it appears at every step at a greater distance beyond the fixed object; from one suggestion the object appears to approach, from the other to have receded. I again observe that retinal magnitude does not itself suggest distance, but from its changes we infer changes of distance.

I have hitherto only described the pseudoscope constructed with two reflecting prisms. This is the most convenient apparatus for effecting the conversion of distance and relief that has occurred to me; but other means may be employed, which I will briefly mention.

1st. Two plane mirrors are placed together so as to form a very obtuse angle towards the eyes of the observer; immediately before them the object is to be placed at such distance that a reflected image shall appear in each mirror. The eyes being placed before and a little above the object, must be caused to converge to a point between the object and the mirrors; the right-hand image of the left eye will then unite with the left-hand image of the right eye, and the converse relief will be perceived. The disadvantages of this method are that only particular objects can be examined, and it requires a painful adaptation of the eye to distinct vision.

2ndly. Place between the object and each eye a lens of small focal distance, and adjust the distances of the object and the lenses so that distinct inverted images of the object shall be seen by each eye; on directing the eyes to the place of the object, the two images will unite, and the converse relief be perceived. As the rays of light proceeding from the images have a greater divergence than those which would proceed from the point to which the optic axes are directed, long-sighted persons will see the binocular image more distinctly by wearing a pair of short-sighted spectacles. In this experiment the field of view is very small, on account of the distance at which it is necessary to place the lenses from the eyes; but I have been enabled in this manner to see beautifully the converse relief of a small ivory bust and of other small objects, which, however, should be inverted in order to see them direct.

3rdly. The inverted images of the lenses, instead of being received immediately by the eyes, as just described, may be thrown on a plate of ground glass, as in the case of the ordinary camera-obscura, and may be then caused to unite by the means employed in any form of the refracting stereoscope.

§ 24.

The cases of the conversion of relief when the object is regarded with one eye only, some of which were known more than a century ago, were taken into consideration and endeavoured to be explained by me in § 11 of the first part of this memoir, and Sir David Brewster* has published some interesting and instructive observations on the same subject; I will therefore not revert to this matter here, but only to say that I have myself never observed the conversion of relief when looking with both eyes immediately on a solid object, and if it has been observed by others under such circumstances, I should be inclined to attribute the effect to an inequality in the impressions on the

* Transactions of the Royal Society of Edinburgh, vol. xv. p. 365 & 657.

two eyes so that one only is attended to. But the plane shaded representation of a solid object, the relief of which is not very deep, may easily be made to appear at will, either as the solid which it is intended to represent or as its converse, even when both eyes are employed. This effect is strikingly observed in the glyptographic engravings of medals of low relief, and depends entirely on whether the light is so placed that it would cast the same shadows on the real object as are represented in the picture, or that it would cast shadows in the opposite direction. In the former case the picture appears with the relief it was intended to suggest; in the latter with the converse relief. I have observed similar effects with Daguerreotypes of medallions and cameos, and with carefully shaded drawings of simple objects.

LXXI. *Geometry and Geometers. Collected by the late* THOMAS STEPHENS DAVIES, *F.R.S.L.&E.&c.**

No. X.

[Continued from p. 290.]

THERE is another ground of embarrassment to the young mathematician in forming his estimate of the ancient geometry. It is the want of proper discrimination between *classes of propositions* which are in themselves of essentially distinct characters. This is traceable to our very elements; for even the first three books of Euclid comprise indiscriminately almost every kind of proposition—determinate and indeterminate. I need only refer to Mr. Potts's "Appendix," before referred to (p. 289), for proof of this; for it will there be seen how diversified are the propositions as to logical character, which con-

* Communicated by James Cockle, Esq., M.A., Barrister-at-Law, who adds the following note:—

[" The above autograph of the late Professor Davies (for this addition to which I am responsible) constitutes the residue of the paper of which the remaining portion appeared in the April Number of this Journal. I have now communicated to the Philosophical Magazine for publication all the manuscripts of my late friend, which Mrs. Davies has confided to me. But I have no doubt that, in the ample store which I believe still remains in her hands, much will be found of the working of his genius—much that, while it reminds science of the loss she has sustained, will render important advantages to mathematical literature, and prove worthy of the name and reputation of the departed philosopher.

"JAMES COCKLE.

"2 Pump Court, Temple,
May 11, 1852."]

stitute our first "Elements." In the more extended classes of research, however, this becomes much more embarrassing; and it is to be regretted that no single work in which the different classes of geometric research are intelligibly defined, can be pointed out. With one more source of difficulty this formidable list will be concluded; though others, and those not of a minor character, might have been added.

The great object of the ancient geometers appears to have been the *solution of problems*; and hence the investigation of theorems held no importance in their estimation, further than as they were subsidiary to the demonstration of the constructions arrived at, or in the analyses by which those constructions were obtained. Instead, therefore, of investigating the properties of figures and classing them according to any rule (good or bad), only those were recorded that became subservient to some step or other in the construction of a problem. This is strikingly manifested in the seventh book of the Mathematical Collections of Pappus; where we see given as isolated propositions many theorems which form parts of the most beautiful and interesting classes of research that have been yet discovered. That wonderful work of M. Chasles (*Aperçu Historique*) bears witness to this in almost every page, and it prevents the necessity of my adducing illustrative examples in this paper.

It will probably be objected that the *arbelon* and some other speculations mentioned by Pappus, as well as some of the minor works of the ancients which have reached us, contravene this view of the leading objects of the Greek Geometry. I know of none of those ancient works, however, in which I cannot trace the ultimate object to be the solution of some specific problem or class of problems; and so far I see no force in such an objection. As regards any of the sets of properties mentioned by Pappus, we must recollect that he wrote and "collected" long after the period when geometry could be said to "flourish" in the school of Plato —long after the decadence of pure geometry amongst the Greeks. The arbelon is itself, beyond being "pretty and curious," mere geometrical trifling; just the kind of speculation that might be supposed to be indulged in the age when the weak Proclus presided over that once illustrious school. Nothing of this kind appears to have engaged the attention of geometers during the period of Apollonius and Archimedes: even the various curves that were devised by the ancients were not devised for the purpose of investigating their properties, but of solving some intractable problem by means of them. The conic sections come the nearest to claiming an exemption from this general rule: but though many properties are given by Apollonius, the immediate application of which to constructive purposes might not readily strike the mind, yet so many of them are subsidiary to the demonstration of properties which have that undoubted purpose, as to require little concession on this point. Besides, my remarks are more immediately made in reference to the propositions of *plane geometry*; and I think we may infer that if such *classes of properties* had been investigated, the good taste and judgement of Pappus would have led him to substitute them for the *arbelon* at least*.

* It is usually stated that the several treatises enumerated by Pappus in the celebrated preface to his seventh book, "were written with a view of facilitating the study of the geometrical analysis." High as is the authority with which this opinion is enforced, I can only adopt it in a very modified sense of the terms employed—and in a sense too, which its supporters do not seem to include in their mode of understanding the statement. It is only in the light of their forming *exemplars* of the geometrical analysis, that I can view it as approaching to the fact; although I should not, perhaps, dispute the question if it were stated that these treatises are in the main, solutions of the problems in which the analysis of other problems often terminate. My principal objection to this latter view would be, that though analyses do often terminate in one or other of these problems, they as often do not; and that even if they were found by experience to do so still more frequently, there appears to be no reason why other classes of problems may not present as much variety in respect to this circumstance as those upon which the Greeks happened to spend their powers presented of frequency.

That Euclid's Data and his Porisms were subservient to analysis, and intended to be so, there cannot exist a moment's doubt. Like his Elements they are intended to be *subsidiary*, and appear to have no other object. The treatises of Apollonius, on the contrary, can only be viewed as final and complete, each in itself: the complete enumeration of the varieties of case and circumstance, and the solution of each in succession, is the obvious *end* of his undertaking—not the *means* of getting to something else beyond it. Indeed, we may ask, *to what purpose could these solutions have been rendered subservient in the cultivation of analysis?* I cannot form the least conjecture as to how they can be so employed. We are also compelled to ask what could have been the nature of those problems which required such an immense amount of preparation as these treatises would imply, even supposing we could see how to apply them? It is strange that no single hint should have escaped the pen of Pappus on this topic, had there been such wonderful problems or classes of problems. To me, therefore, every one of the treatises of Apollonius appears to have nothing further to do with analysis, than as far as analysis might have been employed in obtaining the constructions; even this being an assumption for which it might be difficult to furnish convincing authority. Our views would be much more in keeping at all events with the disputational character of the intercourse of the geometers of those times, did we believe that the analysis was *always concealed*, and only the construction and demonstration given.

LXXII. *On the Puzzle of the Fifteen Young Ladies. By the* Rev. Thomas P. Kirkman, *M.A., Rector of Croft with Southworth, Lancashire.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

While I fully appreciate the analytic value of Mr. Spottiswoode's observations on my problem of the fifteen young ladies in your May Number, I shall hope for his pardon if I say, that, so far as I can discover from what he has written, his solution, like my own and all that I have yet heard of, is accomplished simply by the rule of thumb. When he has supplied the demonstration, that from his seven groups, each of eight terms, not one *must*, but one *can*, be selected "in such a manner that no combinations recur," I will confess that all the tentative process is avoided.

I do not believe, although I am far from denying, that 63 young ladies can be handled day after day like the 15. That this can be done with $5 \times 3^{m+1}$ young ladies, I have proved at p. 259, vol. v. of the Cambridge and Dublin Mathematical Journal.

The following arguments in support of the opinion that the problem cannot be generalized for the case of $3n$ young ladies, n being a prime number greater than 5, may be deserving of attention, although I do not offer them as a demonstration of the negative.

Let it be required to march out day by day in threes, until every pair have walked together, all the $3n$ ladies,

$$\begin{array}{ccc} a_1 & a_2 & a_3 \\ b_1 & b_2 & b_3 \\ c_1 & c_2 & c_3 \\ \vdots & & \\ \mathrm{N}_1 & \mathrm{N}_2 & \mathrm{N}_3, \end{array}$$

consisting of three sisters a, three sisters b, three sisters c, &c., n being a prime number.

As the data are symmetrical in $a, b, c \ldots \mathrm{N}$, and there is nothing in the restriction, that each pair shall walk once and once only together, which is unsymmetrical, and as the whole column is to walk out every day, it is to be expected that the sum of the columns will be also symmetrical in these n letters. The families will exhibit no special preferences or dislikes towards each other, when we consider the letters apart from the subindices. Now the number of triplets possible with n things is less than that of those which must be employed in the columns to be added to the given one. We have a right to expect that the pair ab will be associated equally with the remaining letters; that is, the number of triplets to be added, which is $\frac{1}{6}3n(3n-1)-n$, will be divisible by that of those possible with n symbols, which is $\frac{1}{6}n.\overline{n-1}.\overline{n-2}$; in other words, 9 is divisible by $n-2$, which confines n to the values 3, 5, and 11. The force of this reasoning lies in the position, that there cannot be less than n families, all being symmetrical.

The problem can be solved for the two first values of n; but I doubt greatly its solvability for $n=11$.

There are 11×15 triplets possible with 11 things, and 15 columns of 11 triads are required to be added in the solution; thus we may safely predict that every triplet of the 11×15 will be once employed. And it is reasonable to anticipate, on account of the symmetry to be expected, that the 15 columns will fall into groups of one or more columns, which can all be formed from the first added group by cyclical permutation either of n, or of $n-1$, or of $n-2$ letters; for to suppose such permutation to be made with less than $n-2$ letters, would involve the admission that some one triplet of the 11×15 would be unaffected by it, which is next door to absurd. The only groups into which the 15 columns of letters, considered apart from subindices, can fall, are groups of 1, or of 3, or of 5, if all is symmetrical; and these cannot be produced from each other by cyclical permutation of 11, nor of 10, nor of 9 letters. I venture to affirm, though I do not pretend to have demonstrated, that the problem cannot be solved for n a prime number greater than 5.

The following is a better arrangement than that which I have before given:—

$a_1a_2a_3$	$a_1b_1c_1$	$a_1d_1e_1$	$a_1b_2d_2$	$a_1e_2c_2$	$a_1b_3e_3$	$a_1c_3d_3$
$b_1b_2b_3$	$a_2b_2c_2$	$a_2d_2e_2$	$a_2b_3d_3$	$a_2e_3c_3$	$a_2b_1e_1$	$a_2c_1d_1$
$c_1c_2c_3$	$a_3d_3e_3$	$a_3b_3c_3$	$a_3e_1c_1$	$a_3b_1d_1$	$a_3c_2d_2$	$a_3b_2e_2$
$d_1d_2d_3$	$b_3d_1e_2$	$d_3b_1c_2$	$b_1e_2c_3$	$e_1b_2d_3$	$b_2c_3d_1$	$c_2b_3e_1$
$e_1e_2e_3$	$c_3d_2e_1$	$e_3b_2c_1$	$d_1e_3c_2$	$c_1b_3d_2$	$e_2c_1d_3$	$d_2b_1e_3$

The second and third groups of added columns, looking at letters apart from subindices, are made by cyclical permutation of cde in the first. The subindices are made by cyclical permutation of 123, under all the letters $bcde$, the second and third groups from the first.

LXXIII. *Early Egyptian Chemistry.* *By* W. Herapath, Esq.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

WHILE engaged in unrolling a mummy at the Bristol Philosophical Institution lately, I elicited a few chemical facts which might probably be interesting to some of your readers. On three of the bandages were hieroglyphical characters of a dark colour, as well defined as if written with a modern pen; where the marking fluid had flowed more copiously than the characters required, the texture of the cloth had become decomposed and small holes had resulted. I have no doubt that the bandages were genuine, and had not been disturbed or unfolded: the colour of the marks were so similar to those of the present "marking-ink," that I was induced to try if they were produced by silver. With the blowpipe I immediately obtained a button of that metal; the fibre of the linen I proved by the microscope, and by chemical reagents, to be linen; it is therefore certain that the ancient Egyptians were acquainted with the means of dissolving silver, and of applying it as a permanent ink; but what was their solvent? I know of none that would act on the metal and decompose flax fibre but nitric acid, which we have been told was unknown until discovered by the alchemists in the thirteenth century, which was about 2200 years after the date of this mummy, according as its superscription was read. A very probable speculation might be raised upon this to account for the solution of the golden calf by Moses, who had all his mundane knowledge from the Egyptian priests. It has been supposed that he was acquainted with and used the sulphuret of potassium for that purpose: how the inference arose I know not; but if the Egyptians obtained nitric acid, it could only have been by the means of sulphuric acid, through the agency of which, and by the same kind of process, they could have separated hydrochloric acid from common salt: it is therefore more probable that the priests had taught Moses the use of the mixed nitric and hydrochloric acids with which he could dissolve the statue, rather than a sulphuret, which we have no evidence of their being acquainted with.

The yellow colour of the fine linen cloths which had not been stained by the embalming materials, I found to be the natural colouring matter of the flax; they therefore did not, if we judge from this specimen, practise bleaching. There were in some of the bandages near the selvage some twenty or thirty blue threads; these were dyed by indigo, but the tint was not so deep nor so equal as the work of the modern dyers; the colour had been given it in the skein.

One of the outer bandages was of a reddish colour, which dye I found to be vegetable, but could not individualize it; my son Mr. Thornton J. Herapath analysed it for tin and alumina, but could not find any.

The face and internal surfaces of the orbits had been painted white, which pigment I ascertained to be finely powdered chalk.

I am, Gentlemen,

Yours respectfully,

WILLIAM HERAPATH.

Mansion House, Old Park,
Bristol, June 10, 1852.

LXXIV. *Proceedings of Learned Societies.*

ROYAL SOCIETY OF EDINBURGH.

Dec. 15, 1852.—ON the Quantities of Mechanical Energy contained in a Fluid Mass, in different states, as to Temperature and Density. By Professor William Thomson.

Let p be the pressure of a fluid mass when its volume and temperature are v and t respectively, and let $\mathrm{M}dv+\mathrm{N}dt$ be the quantity of heat that must be supplied to it to augment its volume by dv and its temperature by dt. The mechanical value of the work done upon it to produce this change, is the excess of the mechanical value of the quantity of heat that has to be added above that of the work done by the fluid in expanding, and is therefore

$$\mathrm{J}(\mathrm{M}dv+\mathrm{N}dt)-pdv.$$

It was shown in the author's paper on the Dynamical Theory of Heat, that this expression is the differential of a function of v and t, so that, if this function be denoted by ϕ, we have

$$\phi(v,t)=\int\{\mathrm{JM}-p)dv+\mathrm{N}dt\}.$$

This function would, if the constant of integration were properly assigned, express the *absolute quantity of mechanical energy contained in the fluid mass*. Failing an *absolute* determination of the constant, we may regard the function ϕ as expressing the mechanical value of the whole agency required to bring the fluid mass from a specified *zero* state to the state of occupying the volume v and being at the temperature t. In the present paper some formulæ are given, by means of which it is shown that nearly all the physical properties of a fluid may be deduced from a table of the values of ϕ for all values of v and t; and experimental methods connected with the experimental researches proposed in the author's last paper, are suggested for determining values of ϕ for a gaseous fluid mass.

On a Mechanical Theory of Thermo-Electric Currents.

It was discovered by Peltier that heat is absorbed at a surface of contact of bismuth and antimony in a compound metallic conductor, when electricity traverses it from the bismuth to the antimony, and that heat is generated when electricity traverses it in the contrary direction. This fact, taken in connection with Joule's law of the electrical generation of heat in a homogeneous metallic conductor, suggests the following assumption, which is the foundation of the theory at present laid before the Royal Society.

When electricity passes in a current of uniform strength γ through a heterogeneous linear conductor, no part of which is permitted to vary in temperature, the heat generated in a given time is expressible by the formula

$$A\gamma+B\gamma^2,$$

where A, *which may be either positive or negative, and* B, *which is essentially positive, denote quantities independent of γ.*

The fundamental equations of the theory are the following:—

$$F\gamma=J(\gamma\Sigma a_t+B\gamma^2) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (a)$$

$$\Sigma a_t=\Sigma \alpha_t(1-\epsilon^{-\frac{1}{J}\int_T^t \mu dt}), \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (b)$$

where F denotes the electromotive force (considered as of the same sign with γ, when it acts in the direction of the current) which must act to produce or to permit the current γ to circulate uniformly through the conductor; J the mechanical equivalent of the thermal unit; $\alpha_t\,\gamma$ the quantity of heat evolved in the unit of time in all parts of the conductor which are at the temperature t when γ is infinitely small; μ "Carnot's function" of the temperature t *; T the temperature of the coldest part of the circuit; and Σ a summation including all parts of the circuit.

The first of these equations is a mere expression of the equivalence, according to the principles established by Joule, of the work, F γ†, done in a unit of time by the electromotive force, to the heat developed, which, in the circumstances, is the sole effect produced. The second is a consequence of the first and of the following equation:—

$$\phi.\gamma=\mu\Sigma\alpha\,\gamma.(t-T), \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (c)$$

where ϕ denotes the electromotive force when γ is infinitely small, and when the temperatures in all parts of the circuit are infinitely nearly equal, This latter equation is an expression, for the present circumstances, of the proposition ‡ (first enunciated by Carnot, and first established in the dynamical theory by Clausius) that the obtaining of mechanical effect from heat, by means of a perfectly reversible arrangement, depends in a definite manner on the transmission of a certain quantity of heat from one body, to another at a lower temperature. There is a degree of uncertainty in the present application of this principle, on account of the conduction of heat that must necessarily go on from the hotter to the colder parts of the

* The values of this function, calculated from Regnault's observations, and the hypothesis that the density of saturated steam follows the "gaseous laws," for every degree of temperature from 0° to 230° Cent., are shown in Table I. of the author's "Account of Carnot's Theory," Transactions, vol. xvi. p. 541.

† See Philosophical Magazine, Dec. 1851, "On Applications of the Principle of Mechanical Effect," &c.

‡ "Dynamical Theory of Heat" (Transactions, vol. xx. part ii.), Prop. II. &c.

circuit; an agency which is not reversed when the direction of the current is changed. As it cannot be shown that the thermal effect of this agency is infinitely small, compared with that of the electric current, unless γ be so large that the term $B\gamma^2$, expressing the thermal effect of another irreversible agency, cannot be neglected, the conditions required for the application of Carnot and Clausius's principle, according to the demonstrations of it which have been already given, are not completely fulfilled: the author therefore considers that at present this part of the theory requires experimental verification.

1. A first application of the theory is to the case of antimony and bismuth; and it is shown that the fact discovered by Seebeck is, according to equation (c), a consequence of the more recent discovery of Peltier referred to above,—a partial verification of the only doubtful part of the theory being thus afforded.

2. If $\Theta\gamma$ denote the quantity of heat evolved [or $-\Theta\gamma$ the quantity absorbed] at the surface of separation of two metals in a compound circuit, by the passage of a current of electricity of strength γ across it, when the temperature t is kept constant; and if ϕ denote the electromotive force produced in the same circuit by keeping the two junctions at temperatures t and t', which differ from one another by an infinitely small amount, the magnitude of this force is given by the equation

$$\phi = \Theta\mu(t'-t) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (d)$$

and its direction is such, that a current produced by it would cause the absorption of heat at the hotter junction, and the evolution of heat at the colder. A complete experimental verification of this conclusion would fully establish the theory.

3. If a current of electricity, passing from hot to cold, or from cold to hot, in the same metal produced the same thermal effects; that is, if no term of $\Sigma\alpha_t$ depended upon variation of temperature from point to point of the same metal; we should have, by equation (a),

$$\phi = J\frac{d\Theta}{dt}(t'-t); \text{ and therefore, by } (d), \frac{d\Theta}{dt} = \frac{1}{J}\Theta\mu.$$

From this we deduce

$$\Theta = \Theta_0 \epsilon^{\frac{1}{J}\int_0^t \mu dt}; \text{ and } \phi = (t'-t)\mu\Theta_0 \epsilon^{\frac{1}{J}\int_0^t \mu dt}.$$

A table of the values of $\frac{\phi}{\Theta_0(t'-t)}$ for every tenth degree from 0 to 230 is given, according to the values of μ*, used in the author's previous papers; showing, that if the hypothesis just mentioned were true, the thermal electromotive force corresponding to a given very small difference of temperatures, would, for the same two metals, increase very slowly, as the mean absolute temperature is raised. Or,

* The unit of force adopted in magnetic and electro-magnetic researches, being that force which, acting on a unit of matter, generates a unit of velocity in the unit of time, the values μ and J used in this paper are obtained by multiplying the values used in the author's former papers, by 32·2.

if Mayer's hypothesis, which leads to the expression $\frac{\mathrm{JE}}{1+\mathrm{E}t}$ for μ, were true, the electromotive force of the same pair of metals would be the same, for the same difference of temperatures, whatever be the absolute temperatures. Whether the values of μ previously found were correct or not, it would follow, from the preceding expression for ϕ, that the electromotive force of a thermo-electric pair is subject to the same law of variation, with the temperatures of the two junctions, whatever be the metals of which it is composed. This result being at variance with known facts, the hypothesis on which it is founded must be false; and the author arrives at the remarkable conclusion, that *an electric current produces different thermal effects, according as it passes from hot to cold, or from cold to hot, in the same metal.*

4. If $\vartheta(t'-t)$ be taken to denote the value of the part of Σa_t which depends on this circumstance, and which corresponds to all parts of the circuit of which the temperatures lie within an infinitely small range t to t'; the equations to be substituted for the preceding are,

$$\phi=\mathrm{J}\frac{d\Theta}{dt}(t'-t)+\mathrm{J}\vartheta(t'-t), \quad \ldots \ldots \quad (e)$$

and therefore, by (d),

$$\frac{d\Theta}{dt}+\vartheta=\frac{1}{\mathrm{J}}\Theta\mu \quad \ldots \ldots \quad (f)$$

5. The following expressions for F, the electromotive force in a thermo-electric pair, with the two junctions at temperatures S and T, differing by any finite amount, are then established in terms of the preceding notations, with the addition of suffixes to denote the particular values of Θ for the temperatures of the junctions.

$$\left.\begin{aligned}\mathrm{F}&=\int_{\mathrm{T}}^{\mathrm{S}}\mu\Theta dt=\mathrm{J}\left\{\Theta_{\mathrm{S}}-\Theta_{\mathrm{T}}+\int_{\mathrm{T}}^{\mathrm{S}}\vartheta dt\right\}\\&=\mathrm{J}\left\{\Theta_{\mathrm{S}}\left(1-\epsilon^{-\frac{1}{\mathrm{J}}\int_{\mathrm{T}}^{\mathrm{S}}\mu dt}\right)+\int_{\mathrm{T}}^{\mathrm{S}}\vartheta\left(1-\epsilon^{-\frac{1}{\mathrm{J}}\int_{\mathrm{T}}^{t}\mu dt}\right)dt\right\}\end{aligned}\right\}\ (g)$$

6. It has been shown by Magnus, that no sensible electromotive force is produced by keeping the different parts of a circuit of one homogeneous metal at different temperatures, however different their sections may be. It is concluded that for this case $\vartheta=0$; and therefore that, for a thermo-electric element of two metals, we must have

$$\vartheta=\Psi_1(t)-\Psi_2(t),$$

where Ψ_1 and Ψ_2 denote functions depending solely on the qualities of the two metals, and expressing the thermal effects of a current passing through a conductor of either metal, kept at different uniform temperatures in different parts. Thus, with reference to the metal to which Ψ_1 corresponds, if a current of strength γ pass through a conductor consisting of it, the quantity of heat *absorbed* in any infinitely small part PP' is $\Psi_1(t)(t'-t)\gamma$, if t and t' be the temperatures at P and P′ respectively, and if the current be in the direction from P to P′. An application to the case of copper and iron is made, in which it is shown that, if Ψ_1 and Ψ_2 refer to these metals respectively, if S be a certain temperature defined below (which, according to Regnault's observations, cannot differ much from 240° Cent.), and if T be any lower temperature, we have

$$\int_T^S \{\Psi_1(t)-\Psi_2(t)\}dt=\Theta_T+\frac{1}{J}F,$$

since the experiments made by Becquerel lead to the conclusion, that at a certain high temperature iron and copper change their places in the thermo-electric series (a conclusion which the author has experimentally verified), and if this temperature be denoted by S, we must consequently have $\Theta_S=0$.

The quantities denoted by Θ_T and F in the preceding equation being both positive, it is concluded that *when a thermo-electric current passes through a piece of iron from one end kept at about* 240° *Cent., to the other end kept cold, in a circuit of which the remainder is copper, including a long resistance wire of uniform temperature throughout, or an electro-magnetic engine raising weights, there is heat evolved at the cold junction of the copper and iron, and (no heat being either absorbed or evolved at the hot junction) there must be a quantity of heat absorbed on the whole in the rest of the circuit. When there is no engine raising weights in the circuit, the sum of the quantities evolved at the cold junction and generated in the "resistance wire" is equal to the quantity absorbed on the whole in the other parts of the circuit. When there is an engine in the circuit, the sum of the heat evolved at the cold junction and the thermal equivalent of the weights raised, is equal to the quantity of heat absorbed on the whole in all the circuit except the cold junction.*

7. An application of the theory to the case of a circuit consisting of several different metals shows that if

$$\phi(A,B),\ \phi(B,C),\ \phi(C,D)\quad .\ .\ .\ .\quad \phi(Z,A)$$

denote the electromotive forces in single elements, consisting respectively of different metals taken in order, with the same absolute temperatures of the junctions in each element, we have

$$\phi(A,B)+\phi(B,C)+\phi(C,D)\quad .\ .\quad +\phi(Z,A)=0,$$

which expresses a proposition, the truth of which was first pointed out and experimentally verified by Becquerel. A curious experimental verification of this proposition (so far as regards the signs of the terms of the equation) was made by the author, with reference to certain specimens of platinum wire and iron and copper wires. He had observed that the platinum wire, with iron wires bent round its ends, constituted a less powerful thermo-electric element than an iron wire with copper wires bent round its ends, for temperatures within atmospheric limits. He tried, in consequence, the platinum wire with copper wires bent round its ends, and connected with the ends of a galvanometer coil; and he found that, with temperatures within atmospheric limits, a current passed from the copper to the platinum through the hot junction, and concluded that, in the thermo-electric series

$$\overset{+}{\text{Antimony}},\ \text{Iron},\ \left\{\begin{matrix}\text{Copper,}\\ \text{Platinum,}\end{matrix}\right\}\ \overset{-}{\text{Bismuth}},$$

this platinum wire must, at ordinary temperatures, be between iron and copper. He found that the platinum wire retained the same properties after having been heated to redness in a spirit-lamp and cooled again; but with temperatures above some limit itself considerably below that of boiling water, he found that the iron and platinum constituted a more powerful thermo-electric element than the iron and copper; and he verified that for such temperatures, in the platinum and copper element the current was from the platinum to the copper through the hot junction, and therefore that the copper now lay between the iron and the platinum of the series, or in the position in which other observers have generally found copper to lie with reference to platinum. A second somewhat thinner platinum wire was found to lie invariably on the negative side of copper, for all temperatures above the freezing-point; but a third, still thinner, possessed the same property as the first, although in a less marked degree, as the superior limit of the range of temperatures for which it was positive towards copper was lower than in the case of the first wire. By making an element of the first and third platinum wire, it was found that the former was positive towards the latter, as was to be expected.

In conclusion, various objects of experimental research regarding thermo-electric forces and currents are pointed out, and methods of experimenting are suggested. It is pointed out that, failing direct data, the absolute value of the electromotive force in an element of copper and bismuth, with its two junctions kept at the temperatures 0° and 100° Cent., may be estimated indirectly from Pouillet's comparison of the strength of the current it sends through a copper wire 20 metres long and 1 millimetre in diameter, with the strength of a current decomposing water at an observed rate; by means of determinations by Weber, and of others, of the specific resistance of copper and the electro-chemical equivalent of water, in absolute units. The specific resistances of different specimens of copper having been found to differ considerably from one another, it is impossible, without experiments on the individual wire used by M. Pouillet, to determine with much accuracy the absolute resistance of his circuit; but the author has estimated it on the hypothesis that the specific resistance of its substance is $2\frac{1}{4}$ British units. Taking ·02 as the electrochemical equivalent of water in British absolute units, the author has thus found 16300 as the electromotive force of an element of copper and bismuth, with the two junctions at 0° and 100° respectively. About 154 of such elements would be required to produce the same electromotive force as a single cell of Daniell's, if all the chemical action in a Daniell's battery were electrically efficient A battery of 1000 copper and bismuth elements, with the two sets of junctions at 0° and 100° Cent., employed to work a galvanic engine, if the resistance in the whole circuit be equivalent to that of a copper wire of about 100 feet long and about one-eighth of an inch in diameter, and if the engine be allowed to move at such a rate as by inductive reaction to diminish the strength of the current to the half of what it is when the engine is at rest, would produce mechanical effect at the rate of about one-fifth of a horse-power. The electromotive force of a copper and bismuth element, with its two junctions at 0° and 1°, being found by Pouillet to be about $\frac{1}{100}$ of the electromotive force when the junctions are at 0° and 100°, must be about 163. The value of Θ_0 for copper and bismuth, according to these results (and to the value 160·16 of μ at 0°), or the quantity of heat absorbed in a second of time by a current of unit strength in passing from bismuth to copper, when the temperature is kept at 0°, is $\frac{163}{160 \cdot 16}$, or very nearly equal to the quantity required to raise the temperature of a grain of water from 0° to 1° Cent.

ROYAL INSTITUTION OF GREAT BRITAIN.

April 23, 1852.—On the Analogies of Light and Heat. By the Rev. Baden Powell, M.A., F.R.S., &c., Savilian Professor of Geometry, Oxford.

The researches of Sir W. Herschel, Sir J. Leslie, M. De la Roche, and others, long since established the existence of well-marked differences in character, not only between the radiation from the sun and that from terrestrial sources, but even among these latter, according as the source was luminous or not; and this especially as regarded its transmissibility through various screens and the absorptive effect of different surfaces.

But the most striking peculiarity in the radiation from flame was established by Sir W. Herschel and afterwards extended to gas-lights by Mr. Brande, in that, even at considerable *distances*, after passing through a *thick glass lens*, without heating it, the concentrated rays produced *heat on a blackened thermometer* at the focus, exactly as in the case of the solar rays.

This pointed to a peculiar distinction (also recognised by Sir J. Leslie), and showed that *the mere proportion* of heat transmitted by a screen (as in De la Roche's experiments) was not the essential characteristic, but that further distinction as to the *specific nature* of the rays, was wanted. This want it was attempted in some measure to supply in some experiments by the author of this paper (Phil. Trans. 1825), in which the *character of the different rays as to* TRANSMISSIBILITY through screens was examined IN COMBINATION *with the conditions of the* ABSORBING SURFACE.

This last is a point even yet little understood; but thus much is clear:—

(1) A certain peculiarity of *texture* in the external lamina is favourable to the absorption of radiant heat, probably in all cases.

(2) *Darkness of colour* is peculiarly favourable to the effect *for the sun's rays*, and wholly overrules the first condition.

In terrestrial *luminous hot bodies* it does so to an extent sufficient to give very marked indications. But this (as the author showed, in the experiments referred to) applies to *that portion only* of the compound rays, which is also *transmissible* through glass; the non-transmissible portion is subject wholly to the former condition, as are *all* the rays from *non-luminous* sources (as was shown by Leslie and others).

Hence the distinction of *at least two species* of heating rays emanating at the same time from the same *luminous* source.

From the neglect of this distinction much confusion has been kept up; and statements involving such confusion have been repeated from one elementary treatise to another.

Again, notwithstanding that the experiments of Leslie and others on the *absorption* of heat from *non-luminous* sources, as well as those of Professor Bache on the *radiation from surfaces*, demonstrate that the effect has *no relation* whatever to *colour*, yet the contrary assertion has been often persisted in.

Again, "dark heat" is often spoken of without recollecting that rays of the very same quality and properties exist in the compound radiation from *luminous* sources.

The conclusions drawn from later experiments (performed with all the advantages derived from the beautiful invention of the thermo-electric instrument of Nobili), in many instances, are still vague, from want of attention to the distinction of *different species of heat* emanating at the same time from the same source.

Melloni, in a most extensive and valuable series of experiments, taking as the sources of heat successively flame, incandescent metal, boiling mercury, and boiling water, and applying in each instance a long series of substances as screens, estimated the proportion of rays out of 100 stopped, which was very different for each screen and each source: evincing wide differences in "*diathermaneity*," while *rock-salt* alone was almost totally "diathermanous" to rays from all sources alike.

But we must still ask, what *species* of rays were those respectively stopped and transmitted? To take the *per-centage* simply is ambiguous; the body of rays is not homogeneous; the property of transmissibility should be viewed in combination with other properties of the specific rays, such as those evinced in their relations to the texture or colour of the absorbing surface.

Nor is the ambiguity removed, though the difference of *source* is specially referred to, if the heterogeneity of rays from the *same* source be overlooked. The mere classification of sources into *luminous* and *non-luminous* will not suffice; still less a reference to their *temperatures*, it being perfectly well known that the *temperature of luminosity* is very different for different substances *.

Again, Melloni has shown that the *diathermaneity* is not proportional to *transparency*, by a classified series of transparent screens with the *lamp*.

It must however be recollected that the term "diathermaneity"

* References in detail to all the different researches here mentioned, will be found in the author's two Reports on the state of our knowledge of Radiant Heat in the British Association Reports, 1832 and 1840.

is applied indiscriminately to a heterogeneous body of rays; out of which *some species of rays* are entirely stopped, others entirely transmitted; and the great differences in "diathermaneity" for heat from different sources, which Melloni has also established, are nothing else than *absorption of* PECULIAR *rays* by each medium, not more anomalous than the corresponding absorptions of *luminous* rays by different transparent media so little as yet reduced to law.

While *rock-salt* is analogous to colourless media for light, *alum* on the other hand is totally impermeable by heat from dark sources, and partially so by rays from the lamp; that is, wholly impermeable for that portion of the rays which are of the *same kind* as those from non-luminous sources, and permeable to the others.

By other sets of experiments Melloni showed that rays from the lamp transmitted in different proportions by various screens and then equalized, were afterwards transmitted by *alum* in equally various proportions; or as he expresses it, "possess the diathermancy peculiar to the substances through which they had passed."

But this implies no new property communicated to the rays. It shows that as *different specific rays* out of the compound beam were transmitted in each case by the first screen, alum, though impervious to the lower heating rays, is permeable by these higher rays; and in different degrees according to their *nature*; an effect simply dependent on the heterogeneity of the compound rays from a lamp.

Again, with differently coloured glasses peculiar differences of diathermaneity were exhibited with rays from a lamp, incandescent metal, and the sun; but not more various or anomalous than the absorption of specific rays of light.

And besides considerations of this kind, it must always be borne in mind that a *blackened surface* (like that which was used in all these experiments) itself is *unequally absorptive for the different rays.*

The solar heat being freely transmissible through all colourless transparent media along with the light, there would be no peculiar advantage in experimenting on the solar spectrum formed by a rock-salt prism. Melloni however with such a prism, on interposing a thick screen of water, found the most heating rays (*i. e.* those at or beyond the red end) intercepted, as they are known to be by water; and this caused the position of the *relative* maximum to be apparently shifted higher up in the spectrum, even to the position of the green ray.

On the other hand, many coloured glasses, he found, absorbed the rays in various proportions, yet they left the point of maximum heat unaltered; *i. e.* though variously absorptive for the higher rays, they were not of a nature to stop the lower, or most heating rays.

One result indeed is recorded which seems at variance with all other experiments on the solar rays: a peculiar green glass (tinged by oxide of copper) was found to absorb so entirely all the most heating rays that the remaining portion produced no heat, though when concentrated by a lens they gave a brilliant focus. Speaking generally, however, these experiments only confirm what is on all hands admitted, viz. that the *illuminating* and *heating* powers follow very different laws with relation to the different rays.

The grand discovery by Melloni of the true REFRACTION OF HEAT, even of that kind which constitutes the whole radiation from dark sources, by means of the *rock-salt* lens and prism, and its extension by Professor Forbes to the determination of the *index of refraction* (μ) for the most heating rays from all sources, both luminous and non-luminous, gave the first actual proof of the real analogy of the propagation of heat by waves in an etherial medium: which was further carried out when it was shown from Cauchy's theory that for different wave-lengths (λ) there must be in every medium a certain *limit of all refrangibility*: that is, as we suppose (λ) to increase, large changes in (λ) will give continually smaller changes in (μ), and when (λ) is very great compared with (Δx) the intervals of the molecules, then the index (μ) assumes its limiting value, which is not greatly below that for the extreme red ray, and with this, the index for the lowest heat coincides.

This is seen directly from the formula *

$\frac{1}{\mu^2} = \mathrm{P} - \mathrm{Q}\left(\frac{\Delta x}{\lambda}\right)^2 + \mathrm{R}\left(\frac{\Delta x}{\lambda}\right)^4$ —&c., which, when we suppose $\left(\frac{\Delta x}{\lambda}\right) = 0$, will have for its limiting value $\left(\frac{1}{\mu}\right) = \sqrt{\mathrm{P}}$.

The results from observation for rock-salt compared with this theory, are as follows:—

Rock-Salt.

Rays.	μ.	
	Obs.	Theory.
Mean light	1·558	
Red ray	1·540	
$\lambda = \cdot 000079$	. .	1·529
Dark hot metal	1·528	
Limit	. .	1·527

But it is to the capital fact established by Professor Forbes, of the *polarization* of heat from *dark* sources (for with *luminous* sources little doubt could exist), with all its remarkable train of consequences, that the complete analogy with light is seen in the most uninterrupted point of view;—the transverse vibrations, the depolarization, the consequent interferences, the production of circular and elliptic vibrations under the proper conditions,—to those familiar with the wave-theory present an irresistible accumulation of proof of the identity of the rays of heat with a succession of waves in an etherial medium; exhibiting different properties in *some* dependence on their wave-lengths.

* See the author's Treatise " On the Undulatory Theory applied to the Dispersion of Light," &c. London, J. W. Parker, 1841, pp. 71–122.

Among the most recent researches on the subject are those of Mr. Knoblauch (of which a translation is given in Taylor's Foreign Scientific Memoirs, Part xviii. and xix.), and they are not to be surpassed for extent and accuracy of detail.

One series is devoted to the examination of the alleged differences in radiation of heat *proportioned to the temperature* of the source. This, as before observed, is an untenable hypothesis, but Mr. Knoblauch distinctly refutes it by a series of experiments on alcohol flame, red-hot metal, hydrogen flame, and an Argand lamp, whose *temperatures* are in the order of enumeration beginning with the highest; but the power of their heat to penetrate screens is found to follow exactly the reverse order. And even with lower stages of heat, the effects bear no proportion to the *temperatures* as such. Hence the effect is evidently not due to a mere extrication of the heat of temperature, but is of a peculiar kind. In a word, agreeably to the preceding remarks, the different species of rays, more or less compounded together in the several cases, exhibit their diversities of character in developing heat by their absorption. One very peculiar result is, that platinum, at a stage intermediate between red and white heat, transmits through all the screens employed rather less heat than when at a red heat. That is, these intermediate rays are of such a wavelength as to be subject to a peculiar absorption by these screens; while at the same time possibly less of the former may be emitted.

In another section Mr. Knoblauch adverts to the effects of surfaces on the absorption of rays, and particularly remarks (p. 205), "The experiments of B. Powell and Melloni have shown that one and the same body is not uniformly heated by rays from different sources, which exert the same direct action on a blackened thermoscope;" a statement which does not very intelligibly express any conclusion of the author's. Mr. Knoblauch however supports it by elaborate experiments, showing, as might be anticipated, that an Argand lamp affects a surface of carmine less, and one of black paper more, while a cylinder heated to 212° affects the carmine more and the black paper less.

Another extensive series, on the effect of surfaces on radiation, is directed to show that the effect is independent of the source whence the heat so radiated was originally obtained.

Among the very multifarious results referring to screens and surfaces obtained by Mr. Knoblauch, it can here only be remarked that none of those varied facts appear to present anything *at variance* with the principles here advocated, while in the general conclusions which he indicates at the close of his memoir, the author, though professedly avoiding all hypothesis, yet distinctly intimates his conviction of the heterogeneity of the heating rays increasing as the condition of the source rises in the scale from a low heat up to luminosity or combustion: and that the diversities of heating effect on different media are due to a selective absorption of particular species of rays, from peculiarities in the nature of those substances, and analogous to the absorption of particular rays of light by coloured media.

It must not however be omitted to notice, however briefly, another recent set of researches of high interest, those of M. Silberman; in which (among others) the very remarkable fact is established, that on transmitting a narrow ray of heat from a heated wire, through *rock-crystal*, there is a singular difference according as the ray passes *parallel* or *perpendicular* to the axis of the crystal: the effect being indicated by having the further side of the crystal coated with a fine composition of wax, the portion of which in the direction of the ray is melted in a *circular* form in the first instance and in an *elliptical* in the second.

The general fact of the heterogeneity of heating rays, especially from luminous sources, is fully recognised by Melloni as in some sense the conclusion from all his experiments.

The hypothesis that this heterogeneity consists simply in differences of wave-length would seem a probable one; though it is still possible, as Professor Forbes suggests, that some other element may also enter into the conditions.

This view has been extended by M. Ampère so as to refer both luminous and heating effects to the *same* rays:—a view controverted by Melloni, chiefly on the ground, evinced by several classes of experiments, that *the intensity of the heating effect* (especially in the solar rays) *follows no proportion to that of illumination*; an argument which really amounts to little, unless the theory obliged us to infer that the amount of illumination must follow the *same law* as that of heat; which it manifestly does not; since the nature of the effect in the one case is wholly dependent on the unknown constitution of the optic nerve; according to which some precise proportion of the impinging vibrations, with a particular wave-length, is that which gives the greatest perfection of *vision*; while for *heat* the effect has no reference to such peculiar conditions, but is dependent in some way on longer wave-lengths, and probably more simply connected with the intensity or amplitude of the vibrations.

On this theory our view of the case would be thus:—

A body heated below luminosity begins to give out rays of large wave-length only. As it increases in luminosity it continues to send out these, and at the same time others of diminishing wave-lengths, till at the highest stage of luminosity it gives out rays of all wave-lengths from those of the limit greater than the red end of the spectrum, to those of the violet end, or possibly less.

Rays of all these species are transmissible and refrangible by rock-salt; and many of them with numerous specific distinctions by other media.

They are all *more* or *less* capable of exciting *heat* when absorbed or *stopped*; though in some the effect is perhaps insensible. Both this property and that of their transmissibility seems to depend in some way *on the wave-length*, though in no simple ratio to it.

The absorptive effect due to *texture* of surfaces has some *direct* relation to the magnitude of the wave-length, especially near the limit; while that due to *darkness of colour* is connected with shorter wave-lengths, such as belong to rays within the limits of the *light spectrum*: and in any case when a ray impinges on any absorbing substance, its vibrations, being stopped, communicate to the molecules of the body vibratory movements of such a kind as constitute heat of temperature.

The peculiar molecular constitution of bodies which determines their permeability or impermeability to rays of any species, gives rise to all the diversities of effect, whether luminous or calorific. We thus escape all such crude ideas, at once difficult and unphilosophical, as those either of two distinct material emanations producing respectively heat and light, or of a conversion of one into the other; and obtain a view far more simple and consistent with all analogy.

LXXV. *Intelligence and Miscellaneous Articles.*

DR. KEMP'S PATENT FOR A NEW METHOD OF OBTAINING MOTIVE POWER BY MEANS OF ELECTRO-MAGNETISM.

MY invention of a new method of obtaining power by means of electro-magnetism consists of the mode hereinafter described of combining apparatus to be actuated by electro-magnets. And in order that my invention may be most fully understood and readily carried into effect, I will proceed to describe the means pursued by me. I so arrange electro-magneto apparatus that a series of electro-magnets are caused to act in succession by their armatures on the same bar or instrument, and by such bar or instrument I give motion to fluids in order to obtain and communicate power thereby. To accomplish this object the armatures of several electro-magnets are fixed to stems, and the stems of the armatures are to be free to move through the bar or instrument which carries them. For the purpose of enabling the armatures to be acted on in succession by their magnets, I make the stem of the armature which is to be first attracted somewhat longer than the next in succession, by which means the first armature will be as near as may be to its magnet; and the next armatures being more and more distant from their electro-magnets, therefore when the first armature has been attracted by its electro-magnet, the others will be moved nearer to their electro-magnets, and will consequently be brought into the most advantageous position to be attracted thereby when their turns come.

Thus, supposing it to be determined that each armature shall be attracted through a quarter of an inch by its electro-magnet, and that there are to be eight electro-magnets to act on the same bar or magnet, the first armature before being attracted would be at a distance of a quarter of an inch from its electro-magnet; the second would be half an inch from its magnet; the third three quarters of an inch from its magnet, and so on; whereby the eighth armature would be two inches from its electro-magnet, and these differences of distance are to be obtained by the stems (by which the armatures are connected to the bar or instrument) being made shorter and shorter. By this arrangement it will be evident that if electric currents be caused to pass in succession to the coils of the several electro-magnets, and in such manner that the currents of electricity having caused the first electro-magnet to attract its armature, are cut off therefrom, and caused to pass to the next electro-magnet, and so on in regard to the eight electro-magnets and their armatures; each armature before being attracted will have been brought by the movement of the bar or instrument to within about a quarter of an inch of its electro-magnet, the bar coming at each step of its movement nearer and nearer to the electro-magnets, which it is enabled to do by the stems of those armatures which have been previously attracted, being enabled to slide back freely through the bar or instrument which carries them. The stems of the armatures are enabled to draw the bar or instrument towards the electro-magnets (when their armatures are attracted by reason of the stems having projecting heads or end), which prevent the stems from being drawn through the bar or instrument which carries them, whereby, when all the electro-magnets have attracted their armatures, the bar or instrument will have been moved two inches or other distance according as arrangement is made for each of the electro-magnets to act through a less or larger space than a quarter of an inch. It will be evident that this bar or instrument may be arranged to give motion to machinery in various ways; but I believe the most convenient mode of applying the power thus derived from electro-magnets, will be found to be to affix one bar or instrument, such as herein described, to one end of the rod of a piston working in a cylinder, and another such bar or instrument to the other end of the piston-rod, the piston being in the middle of the piston-rod, and the piston-rod working through stuffing-boxes on the covers at either end of the cylinder. Each such bar or instrument is to be fixed in the manner of a cross-head to the piston-rod, and to be guided in its movement to and fro, and is to be provided with armatures on stems as herein described, and sets of electro-magnets to attract the same, and capable of being brought into action in succession, as above explained, and as will be readily understood by workmen accustomed to making electro-magneto apparatus; by which means the piston in the cylinder may be moved first in one direction and then in the other. In order that the armatures may be in a position to act correctly, the ends of their stems, when being moved back towards the cylinder, should come against a stop or stops to move the heads or enlarged ends of the stems to the bar or instrument which carries them; they will thus be brought into position to be again acted on by their electro-magnets so soon as the electro-magnets have, by attracting their armatures, drawn the piston, as far as it can go, in the other direction. As a piston, by such means, cannot with convenience be caused to move through an extended length of space, the cylinder is to be of comparatively large diameter to its length, and at either end it is to have passages for the water or other fluid (contained in the cylinder) to pass into and from the ends of another cylinder of less diameter, but of proportionably greater length, in which a piston also works; and I prefer that the piston-rod of such second cylinder should also work through stuffing-boxes at either end of that cylinder; such piston-rod communicating the power obtained (by the means above described) by a connecting-rod and crank from one end of the piston-rod, or by other suitable means of communicating power from a piston, may be employed. From the above description it will be understood that great power may be obtained from a series of electro-magnets, each attracting its keeper or armature, and consequently moving the piston through only a small space; and such power being exerted over a large area of piston, moving a fluid and forcing it into a cylinder of smaller diameter, will cause the piston of that second cylinder to be moved through a longer stroke in proportion to the different capacities of the cylinders, and the piston of the second cylinder will consequently be moved at a greater speed than that in the larger cylinder, and the pressure per square inch on the smaller piston will be the same as that on the greater piston. All which will be readily understood by a workman acquainted with the pressure of fluids put in motion by one piston, and caused to act on another; and it will be at once perceived that the action will be the reverse of that in Bramah's Press, wherein the water is put in motion by the power used acting on a piston or plunger of comparatively small diameter, and the water is caused to act on and to move a piston of much larger diameter. Whereas, in the present invention, a series of electro-magnets are caused to act in succession on a bar or instrument, as above explained, in such manner that when combined with a comparatively large piston the power will, by driving or forcing the water or fluid with a cylinder of less diameter and of greater length, cause the piston therein to be moved with less power, but with greater speed. And it will at once be understood that the power obtained will depend on the effort each magnet is capable of exerting; for it will be evident that the actual force which is kept up to and given off from the piston in the small cylinder will be equivalent to that exerted by one of the magnets, in attracting or drawing its armature through a comparatively small space.—*Repertory of Patent Inventions*, February 1852.

ELECTRO-CHEMICAL RESEARCHES ON THE PROPERTIES OF ELECTRIFIED BODIES. BY MM. FREMY AND BECQUEREL.

For several years the attention of chemists and physicists has been directed to the very remarkable modifications which certain bodies present when submitted to the action of a moderate temperature. We know that, under this influence, sulphur and phosphorus acquire new properties. We propose to investigate whether electricity, like heat, can change the physical and chemical properties of different bodies. We must examine, in the first place, into the singular effects presented by oxygen in various circumstances, and referred to the formation of what has been called *ozone*; this body appears to be produced in all cases in which oxygen is submitted to the influence of electricity.

Without wishing to cast doubt upon the sagacity of those who have examined into the properties of ozone, it cannot be denied that there still exists great uncertainty in the minds of chemists and philosophers as to the interpretation of the phænomena observed; we have therefore thought that it was important to submit these phænomena to new experiments.

We will confine ourselves here to reproducing some of the facts mentioned in the memoir which we have the honour to present to the Academy.

1. After going over all the experiments made on *ozone*, mentioning in particular the important researches of Schönbein, Marignac and De la Rive, we have examined, first the oxidizing properties of the oxygen procured by the decomposition of water by the galvanic pile; the result of these researches is that the pile cannot be employed to determine the nature of *ozone*, because the active principle is found only in very small proportion in the oxygen of the pile. We have therefore been obliged to study successively all the methods which can be employed to electrify oxygen.

2. The arc which is formed upon the interruption of the voltaic circuit does not appear to modify the oxygen in the same manner as the ordinary spark, because the elevation of temperature which accompanies it probably destroys that which the electricity might produce; but according to our observations, this arc may determine the combination of gases amongst themselves, acting thus as spongy platinum and as electricity; under its influence we have combined nitrogen and oxygen directly, to form nitric acid, nitrogen and hydrogen to produce ammonia, and sulphurous acid and oxygen to form anhydrous sulphuric acid.

3. The spark proceeding from currents of induction, and produced by means of the ingenious apparatus lately constructed by M. Ruhmkorff, acts like the spark of the ordinary machines, and has enabled us to repeat, without fatigue, all the experiments made with the machine.

4. Pure oxygen, enclosed in glass tubes together with a band of starched and iodized paper, was electrified by means of a series of sparks striking the outer surface of the tube; the paper began to become blue after the passage of a few sparks. This colorization depends on the electrization of the oxygen, and not on the decomposition of the iodide; for no effect takes place when the iodide is placed in hydrogen and operated on. This fact is so much the more remarkable, as the oxygen is electrified without the intervention of metallic wires, and consequently without the presence of particles transported by the electric spark.

5. Oxygen, prepared by the most different modes, such as the calcination of the oxides of manganese, mercury or silver, by the decomposition of chlorate of potash, or of water by means of the pile, acquires a very distinct odour, and strongly marked oxidizing properties when it is subjected to the influence of electricity; these properties are manifested by oxygen as pure as it is possible to obtain it. The oxygen thus electrified loses its oxidizing properties when exposed to iodide of potassium, but it regains its odour and chemical activity when again electrified; this experiment may be repeated indefinitely on the same gas.

All these facts show that the oxidizing power of electrified oxygen is not due to the presence of a foreign body contained in the gas:

the following experiments were directed to rendering a given volume of oxygen entirely absorbable whilst cold by mercury, silver, or iodide of potassium.

6. When pure and dry oxygen is enclosed in a series of glass tubes and subjected to the action of electric sparks, if after a time we break one of the extremities of these tubes to ascertain the volume of gas which has become immediately absorbable by alkaline iodide, we shall find that during several hours the modification increases in proportion to the time of electrization, and that afterwards it appears to diminish, probably because the spark destroys that which at first it produces.

7. The difficulties presented in the preceding experiment induced us to study the deportment of electrified oxygen with certain absorbing bodies capable of immediately seizing the modified oxygen and of withdrawing this gas from the decomposing action of the excess of electricity; we therefore passed a series of electric sparks into small eudiometric tubes full of moist oxygen, and placed over either mercury or a solution of iodide of potassium, or containing in their interior a moistened leaf of silver: we then saw the oxygen become absorbed in a regular manner by the action of the electric spark, and in many experiments obtained a complete absorption.

8. Lastly, to get rid of all doubts about the particular activity imparted to oxygen by the electric spark, we wished to verify the preceding experiments in closed tubes. We therefore introduced into tubes filled with pure oxygen some iodide of potassium and moistened silver. We submitted these tubes for several days to the action of electricity; the spark, which, during the first days was very brilliant, became paler and paler, and presently almost invisible. At this moment, on breaking the tubes under water, we saw this liquid rush into their interior and fill them entirely, thus showing that a vacuum had been produced, and consequently that the oxygen had become completely absorbable without heat, by the silver and iodide of potassium. We must add, that, to render these experiments decisive, we had previously ascertained—1st, that pure water, the surface of glass and the platinum wires conducting the spark, could not absorb oxygen; 2nd, that water is not necessary to develope the activity of oxygen, but to cause the active oxygen to react upon metals or iodide of potassium; 3rd, that the electric spark does not decompose the iodide of potassium.

We think therefore that we have shown, by rigorous experiments, that oxygen, under the influence of electricity, can become completely absorbable in the cold by iodide of potassium and several metals, such as mercury and silver.

These facts confirm the last researches of MM. Schönbein, Marignac and De la Rive, and show that electricity, in acting upon oxygen, developes properties in it which did not exist before its influence; we propose therefore simply to give the name of *electrified oxygen* to the gas, which, having been submitted to the action of electricity, acquires a particular state of chemical activity, and to abandon the name of *ozone*, which expresses the idea of the transformation of the oxygen into a new body.—*Comptes Rendus*, March 15, 1852, p. 399.

ON THE ALLOTROPY OF SELENIUM. BY M. HITTORF.

It is well known that selenium is softened by heat, becomes semifluid at 212°, and melts at a few degrees higher. In cooling, it becomes viscous, thickens more and more, like wax, and then solidifies into a reddish mass, with a shining surface and a conchoidal and vitreous fracture. Berzelius had already observed, that when the cooling takes place very slowly, the selenium acquires a reddish colour, a rough surface, and a dull granular fracture, but that it loses this appearance when it is melted again and cooled rapidly.

The author, having attentively studied these phænomena, has found that they are due to the existence of an allotropic modification of selenium analogous to that presented by sulphur, and evidenced by the fact that the crystallized selenium melts without any previous softening, but at a temperature of 422°·6 F.

When the substance is melted and the temperature raised, for instance to 428°, and left to cool with a thermometer immersed in it, the temperature is seen to descend gradually without the thermometer becoming stationary, without its even being possible to observe a single point where its cooling appears to slacken until it has attained the temperature of the medium. At the same time the selenium passes through all degrees of viscosity until at about 122° it entirely solidifies into a resinous mass. In these circumstances the substance has therefore solidified in the amorphous state without having lost its latent heat of fusion; and it may retain it indefinitely, for it persists in this amorphous state at the ordinary temperature. But it passes into the crystalline state when kept for some time at a temperature between 176°–422°, and it then parts with its latent heat.

Between 176° and 212° the transformation requires several hours; and in this case the disengagement of heat which accompanies it is not appreciable. Between 257° and 356° it is very rapid; and if we operate upon 20 grms. of selenium, which are heated in an oil-bath, a thermometer immersed in the interior of the substance, after having attained the temperature of the bath, will rapidly exceed it, and will rise from 70° to 90° above it for some minutes. This phænomenon is still more striking when a hot air-bath is substituted for an oil-bath. In one experiment, in which the bath was heated to 266°, the author observed the thermometer, after having risen slowly to 257°, ascend suddenly to between 410° and 419°.

When the selenium is employed in the state of powder, its metamorphosis is more rapid. In this case, even in a bath heated merely to 212°, the crystalline state may be developed so rapidly that the heat rises from 45° to 52° above that of the interior.

These phænomena are exactly similar to those which are presented by sulphur. When this substance is strongly heated, and it is then suddenly cooled, it is converted into an amorphous, soft and elastic mass. At the ordinary temperature it passes gradually, but very slowly, from this amorphous state into the hard and crystalline state in which it is ordinarily met with. At a temperature approaching 212°, it passes in a few minutes into this state; and it is well known that M. Regnault noticed in this case the temperature of the soft sulphur rise spontaneously to 232° in a chamber heated to 208°. The only difference which exists between these two bodies is, that with sulphur the ordinary temperature is sufficiently near that at which the change of state takes place for the transformation to be gradually effected; whilst in the case of selenium, it is requisite to raise it to a more elevated temperature in order to cause it to pass from the amorphous into the crystalline state.

In many chemical reactions the selenium is separated from its solutions in the form of an amorphous red powder. It is obtained thus by precipitating selenious acid by sulphurous acid, by the chlorides of tin, zinc, iron, or by exposing a solution of hydroselenic acid to the atmosphere, or by diluting with water a solution of amorphous or crystallized selenium in concentrated sulphuric acid. These precipitates have only to be exposed to the action of the solar rays to make them gradually pass into the crystalline state.

In other circumstances selenium may be precipitated at the ordinary temperature in the crystalline state; for instance, when solutions of seleniuret of potassium or ammonium are exposed to the air.

Selenium exhibits very different densities in its two allotropic modifications. In its amorphous state the specific gravity is 4·26–4·28, whilst in the crystalline it is 4·80; its conductibility for electricity likewise varies considerably; the vitreous selenium insulates almost perfectly, whilst the crystalline substance is a very excellent conductor. In this state it exhibits a very curious phænomenon, that of the resistance decreasing in proportion as the temperature rises, provided its point of fusion be not attained.

The author concludes his memoir by some observations on the analogy which exists between the phænomena of allotropy presented by sulphur and selenium and that which M. Schrœtter has published regarding phosphorus; he thinks that the latter chemist commits a mistake in considering the red phosphorus as amorphous; and although we have not yet been able to obtain this modification in the crystalline state, he believes that it is the state of phosphorus corresponding to the crystalline sulphur and selenium, and that very probably this body, in passing into that state, likewise disengages a quantity of heat.—Poggendorff's *Annalen*, lxxxiv. p. 214.

METEOROLOGICAL OBSERVATION. BY P. J. MARTIN.

The perpendicular column of light seen in the horizon at sunset in April, as described by your Orkney correspondent, was also seen in this part of Sussex. I did not get sight of it more than once; because, supposing it to be of a transient and local character, I did not look for it again. Here it was singularly vivid, and faded gradually away, or rather followed the sun, as described by your friend and the correspondents of the *Times*. It had none of the character of the zodiacal light, but rather looked like the columnar prolongation of the sun described by oriental travellers as frequent in the east; and it immediately suggested to my mind (as it seems to have done to some of the above-mentioned observers) the columnar light in Martin's "Exodus," the "pillar of fire" moving before the Israelitish host.

Pulborough, June 4, 1852.

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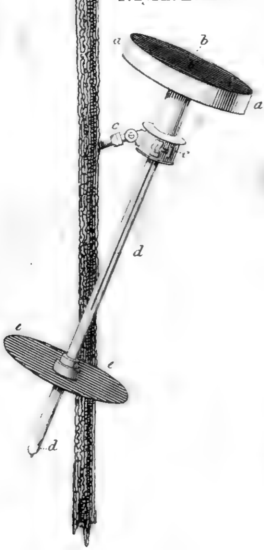

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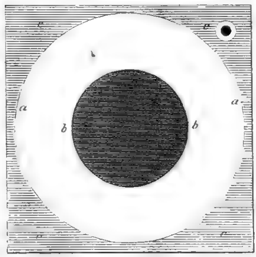

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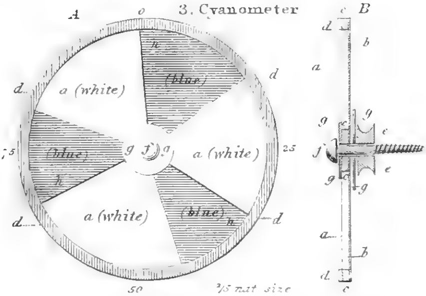

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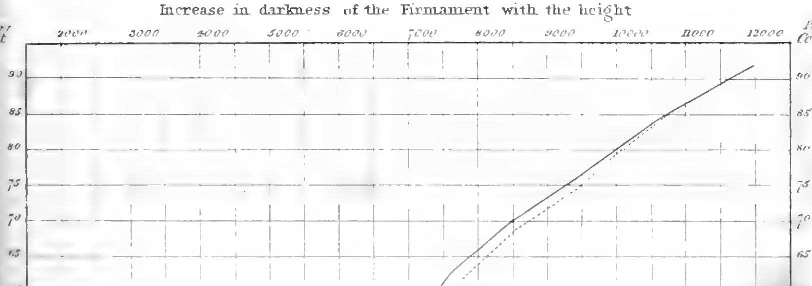
Increase in darkness of the Firmament with the height



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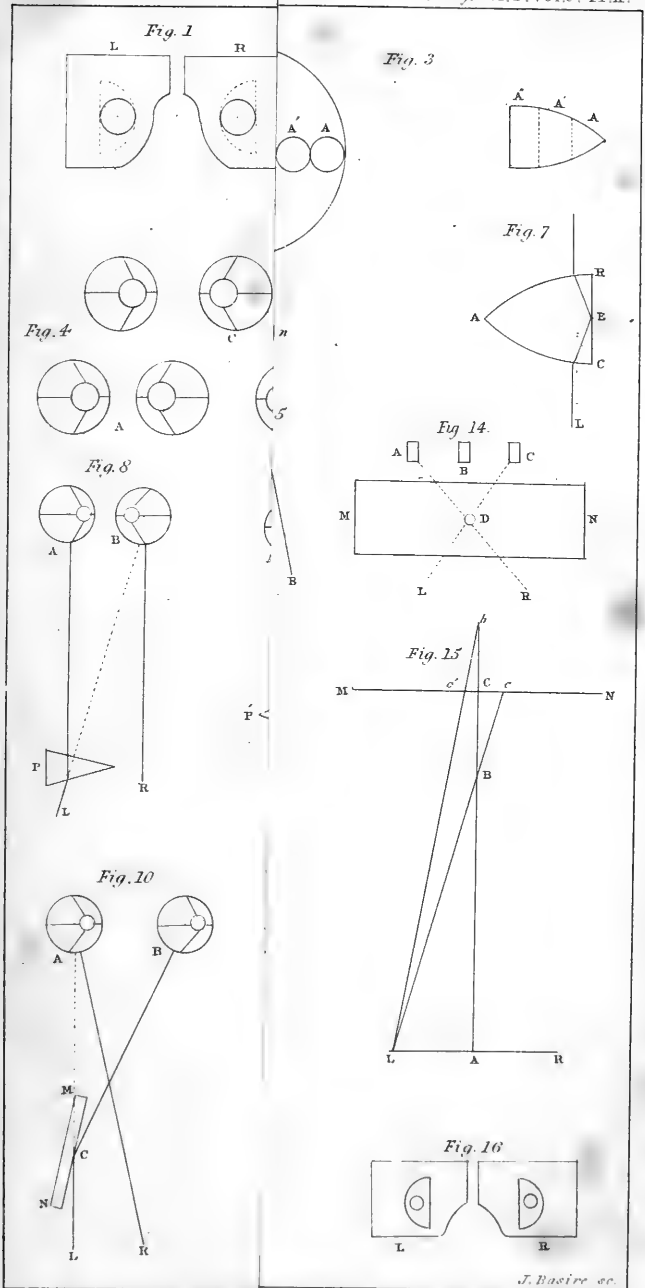
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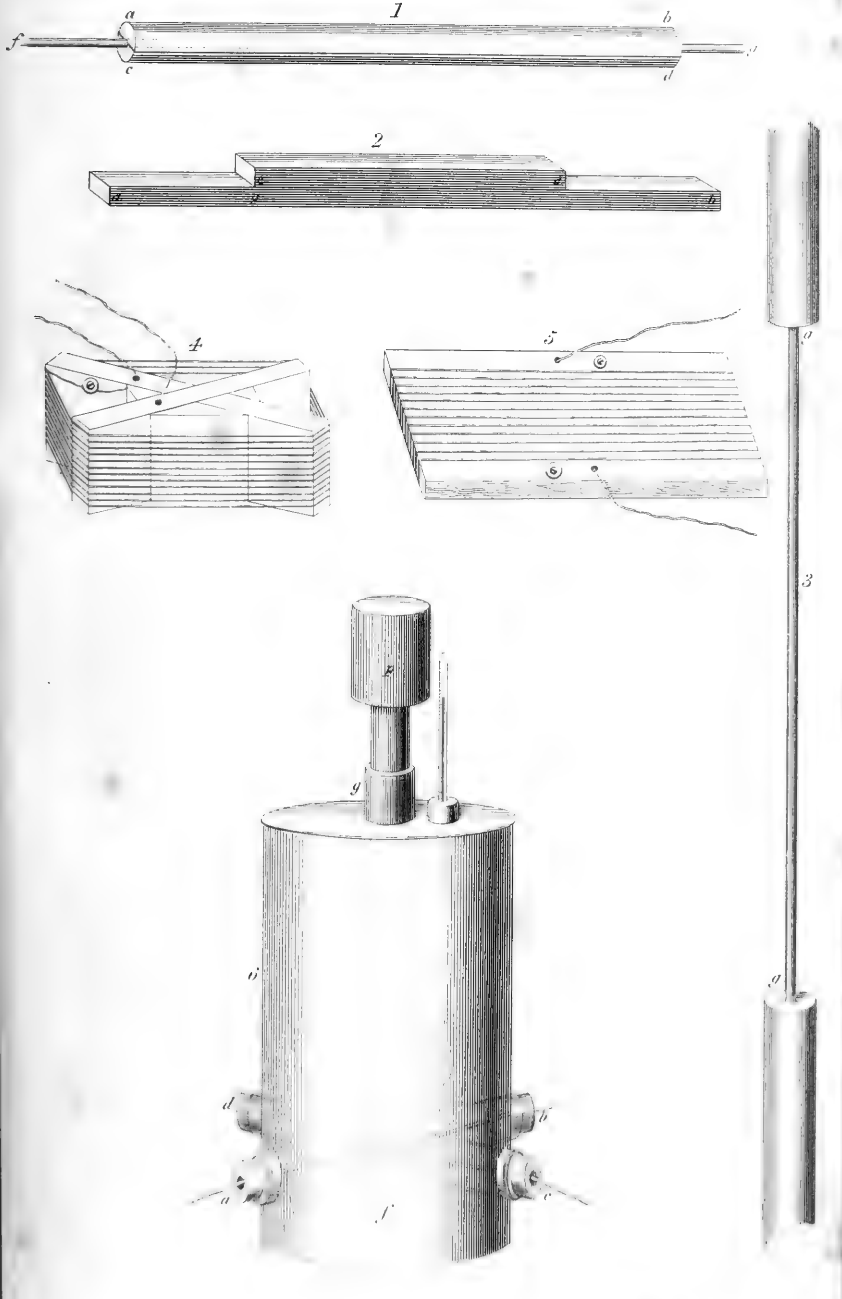
Sir David Brewster's Stereoscopes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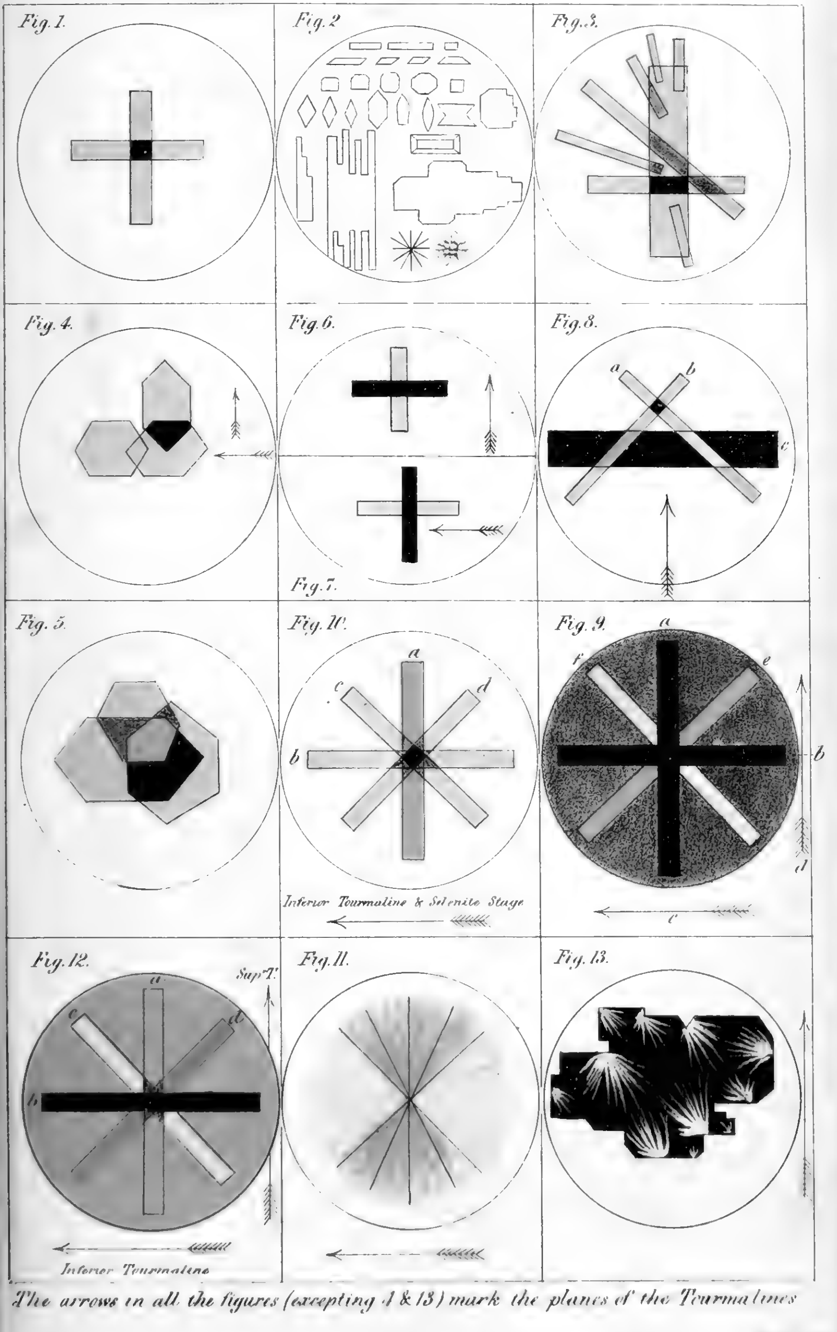
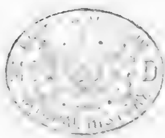


The arrows in all the figures (excepting 1 & 13) mark the planes of the Tourmalines

J. Basire, Lith.



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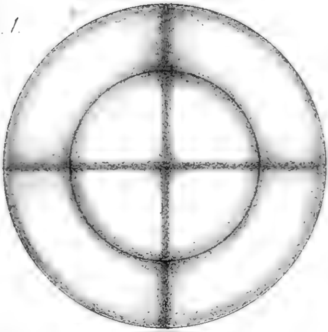

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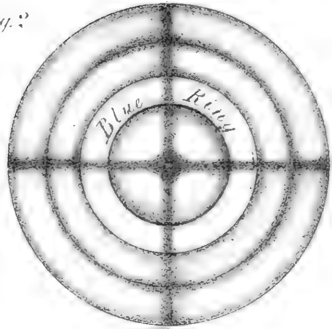

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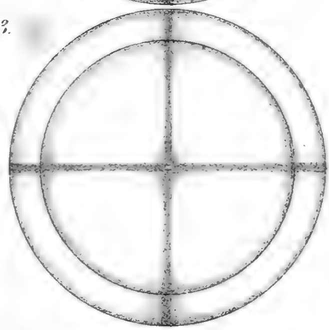

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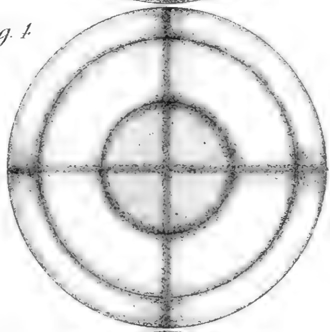

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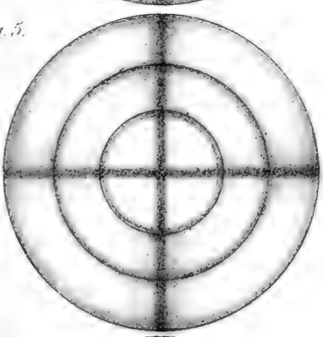

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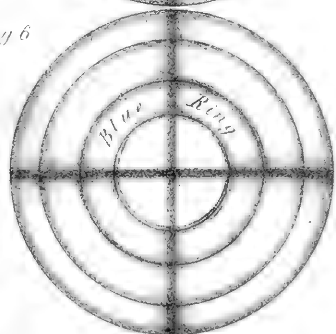

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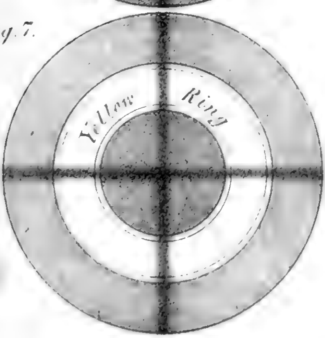

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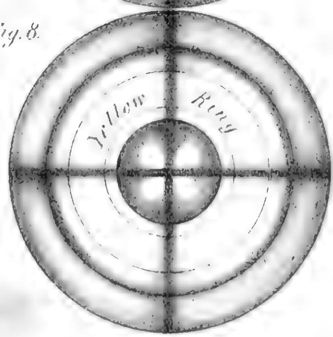

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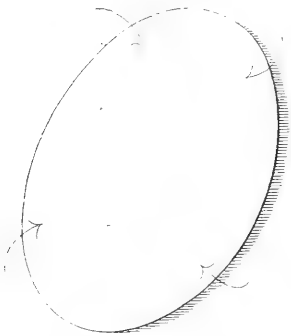

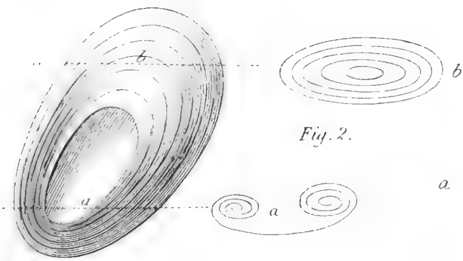

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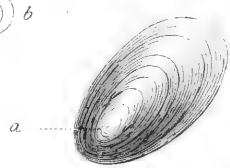

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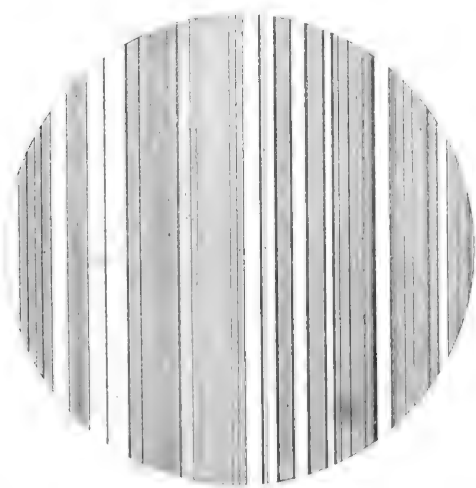

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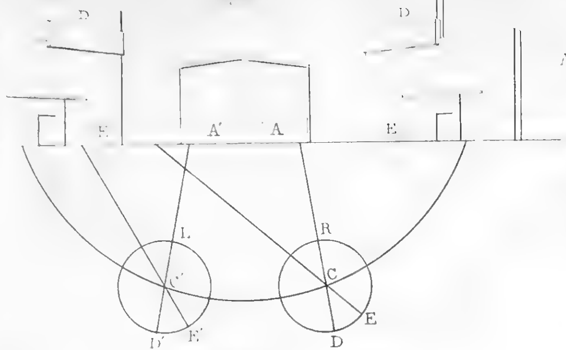

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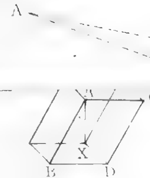

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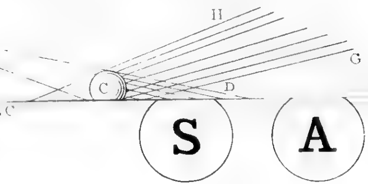

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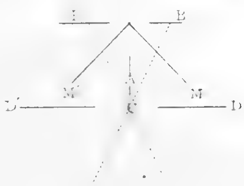

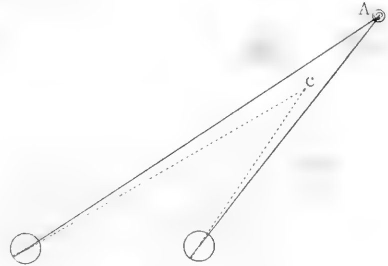

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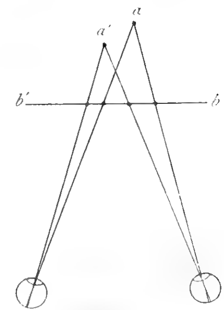

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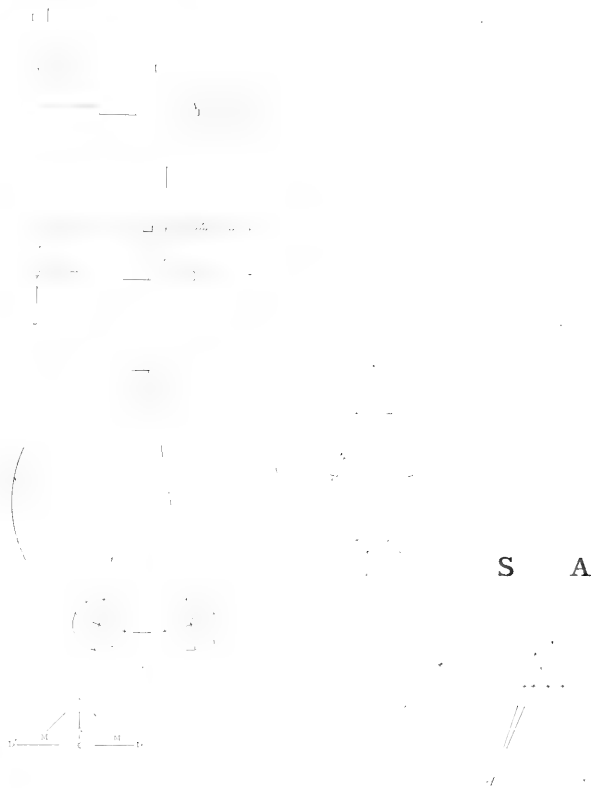
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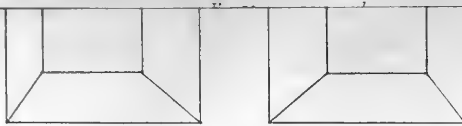

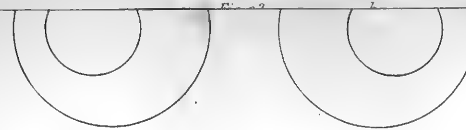

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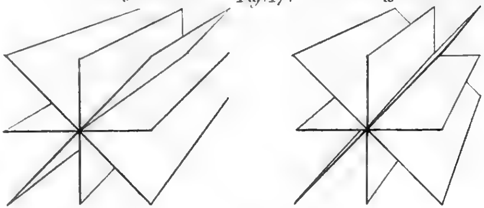

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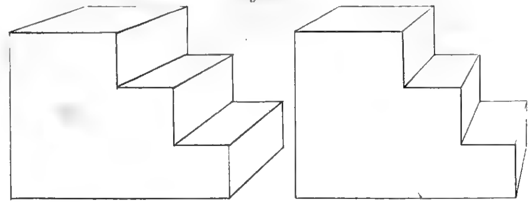

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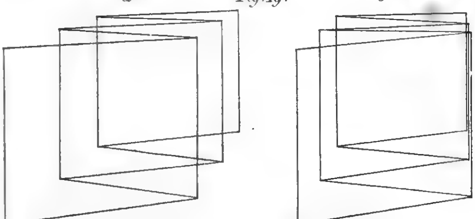

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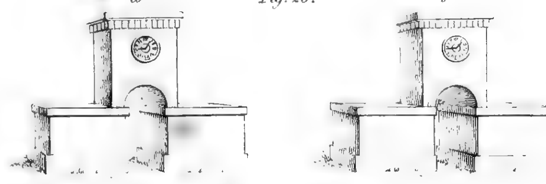

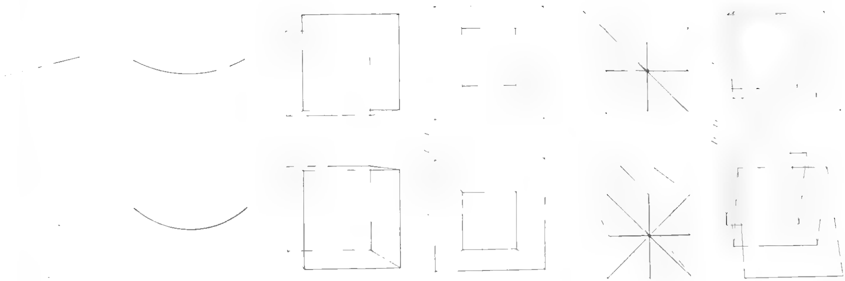

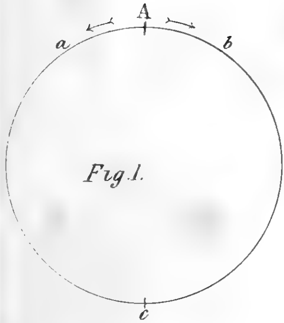

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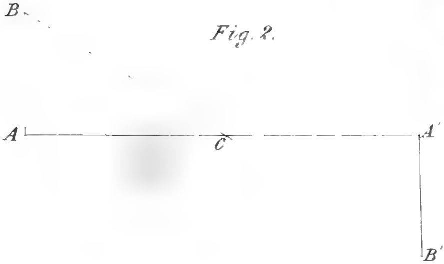

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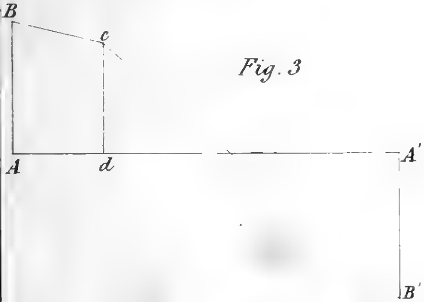

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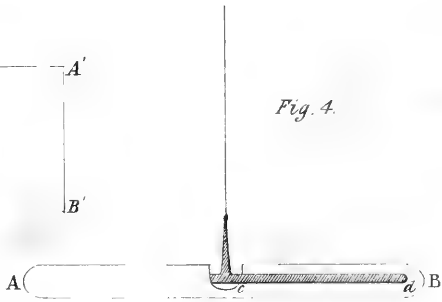

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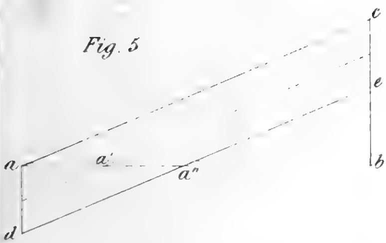

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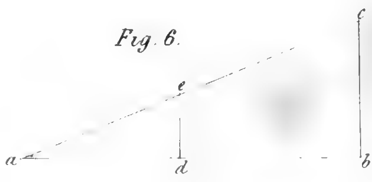

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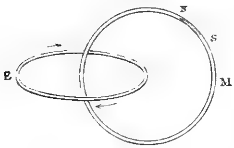

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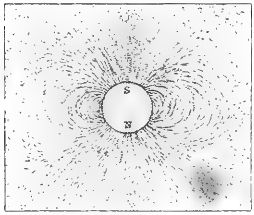

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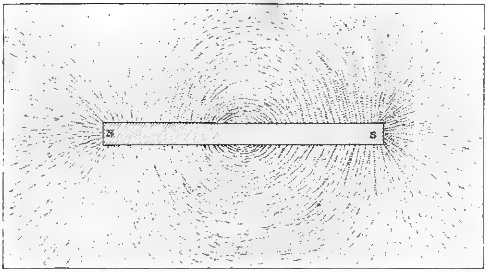

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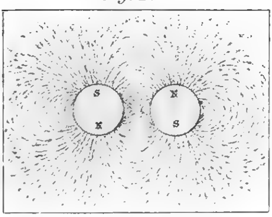

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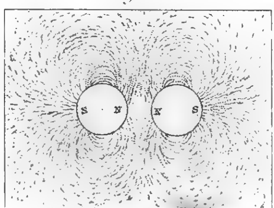

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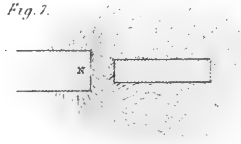

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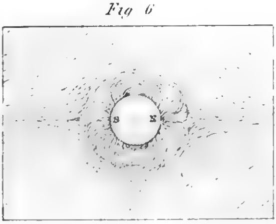

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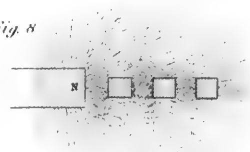

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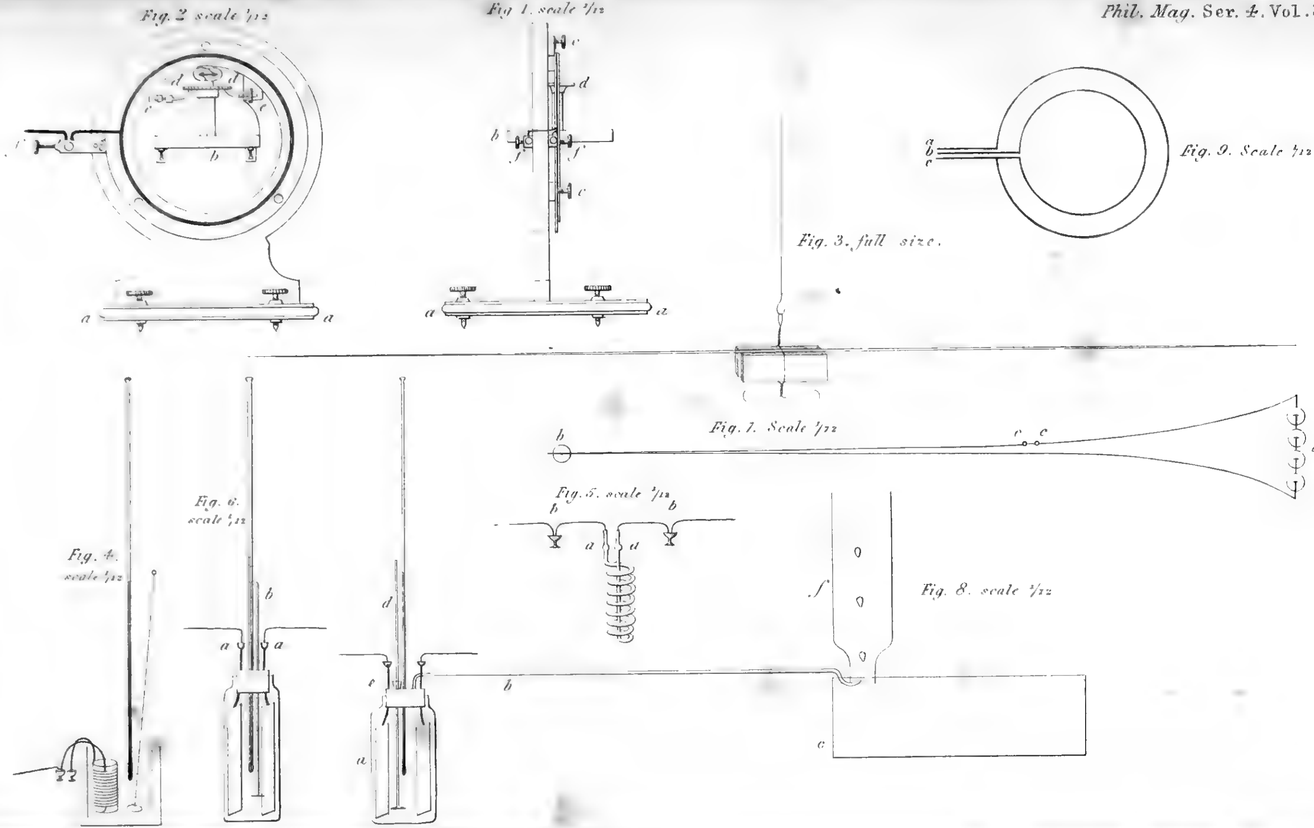
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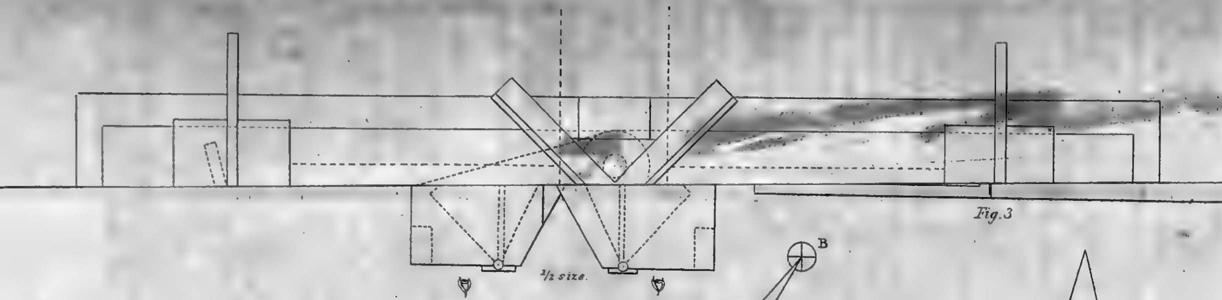

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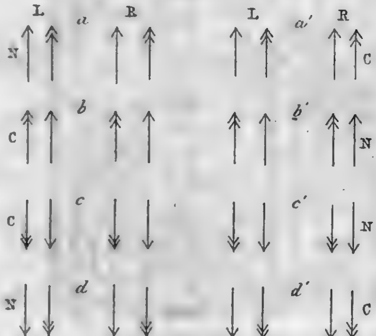

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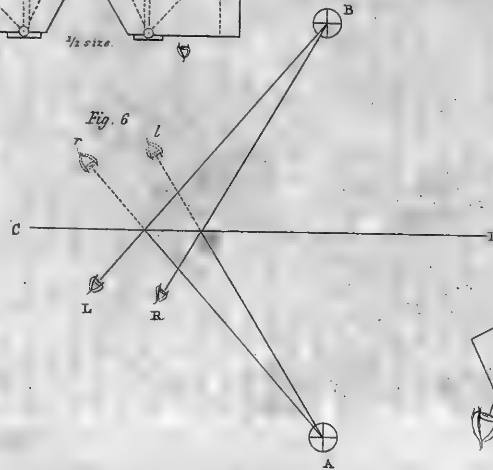

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