

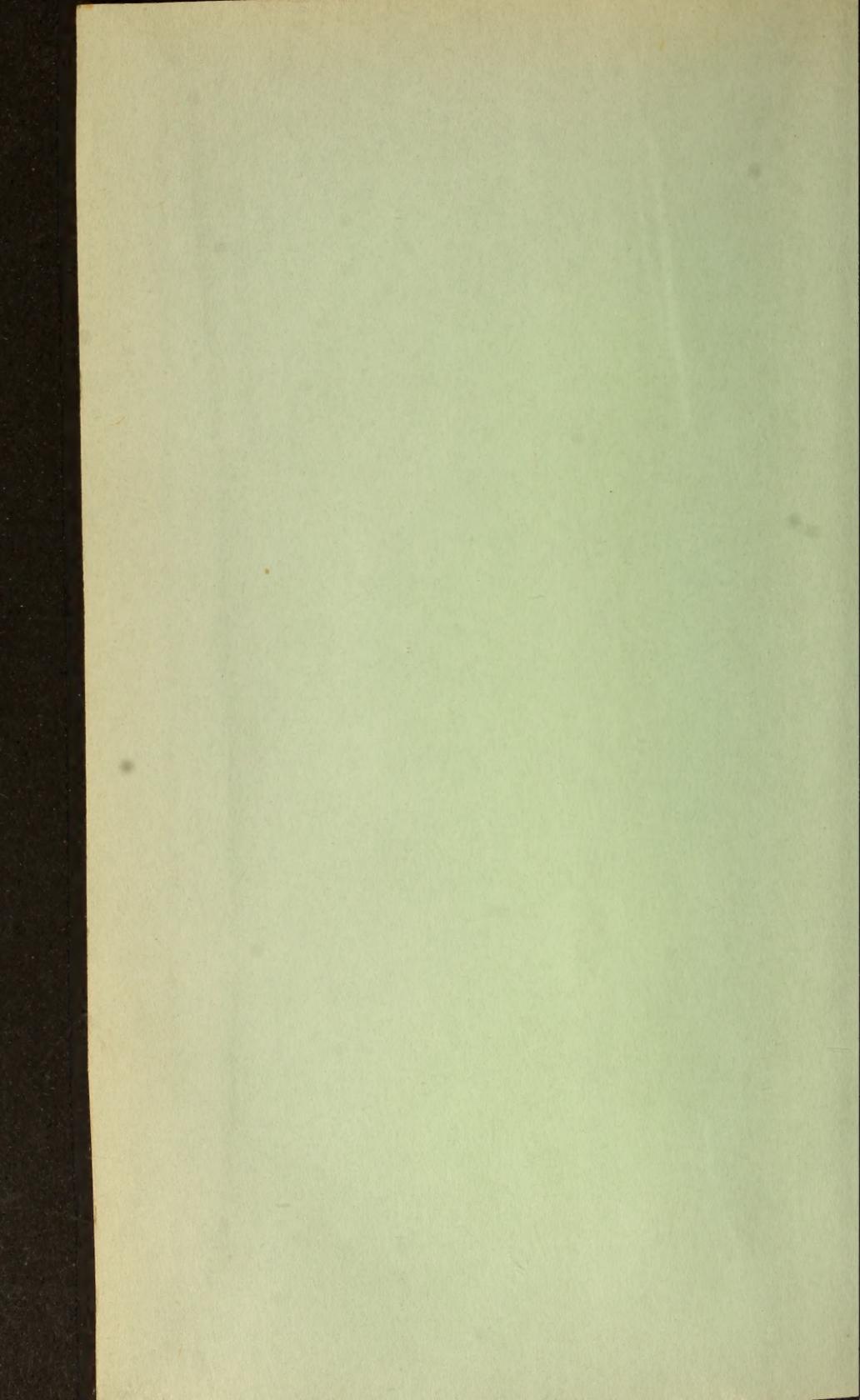
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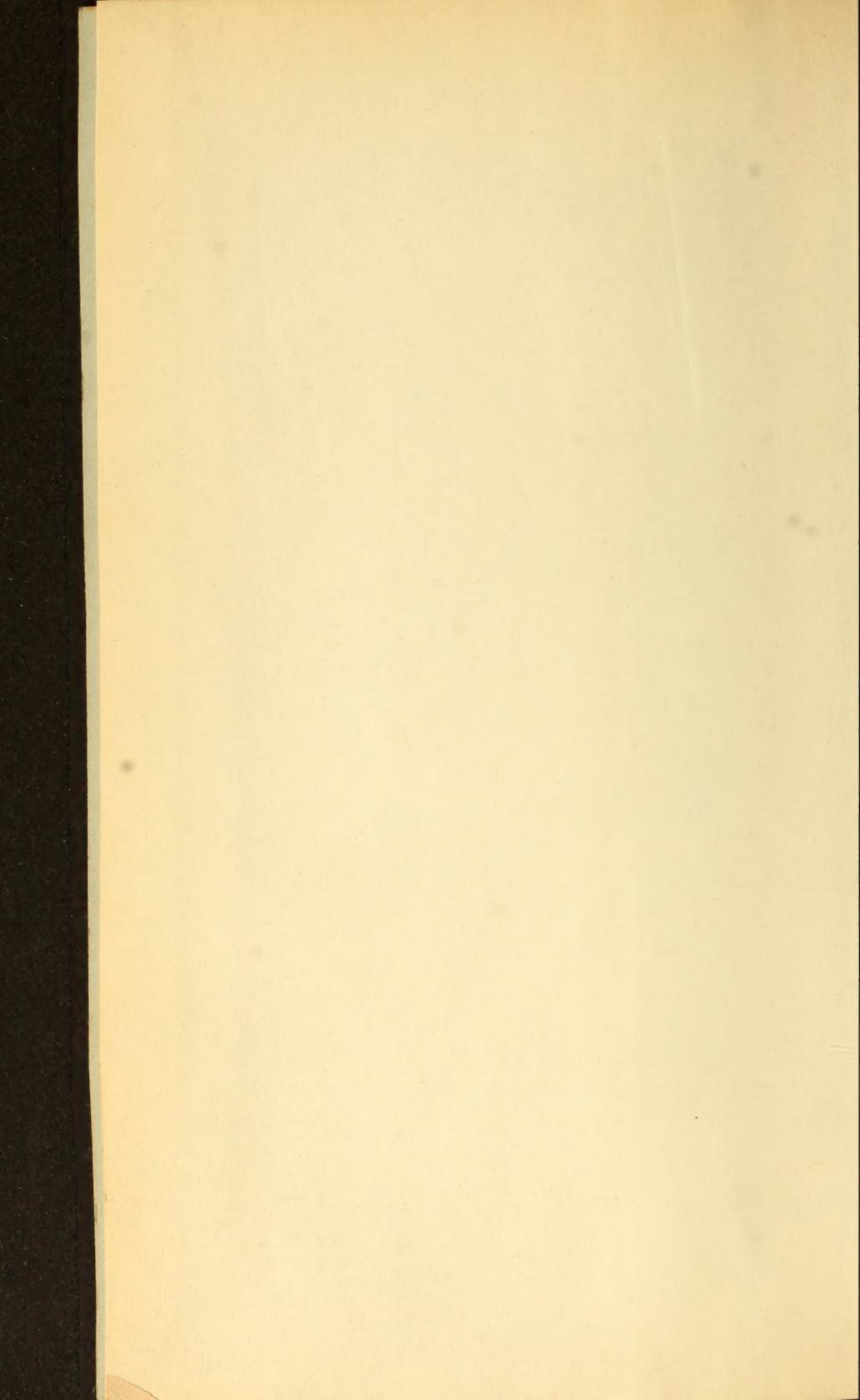
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THE
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CONDUCTED BY

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“Nec araneorum 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.

CONTENTS OF VOL. XXXIV.

(FOURTH SERIES.)

NUMBER CCXXVII.—JULY 1867.

	Page
Prof. R. Bunsen on a Method of determining the Specific Gravity of Vapours and Gases. (With a Plate.)	1
Sir William Thomson on Vortex Atoms	15
Archdeacon Pratt on Professor Stokes's Proof of Clairaut's Theorem	25
Dr. Atkinson's Chemical Notices from Foreign Journals	26
Mr. H. A. Newton on certain recent Contributions to Astro-Meteorology	34
Messrs. J. A. Wanklyn and R. R. F. Davey on Sir Benjamin Brodie's "Ideal Chemistry"	50
Mr. J. J. Waterston on the Change that would be superinduced upon an Elliptic Orbit if the intensity of the force of Gravity were influenced by the centripetal velocity of the Orbital Body	55
Mr. W. Ellis's Inquiry as to whether the tendency to Dispersion of Cloud under a Full Moon in any way depends on Lunar Influence	61
Proceedings of the Royal Society:—	
Dr. W. J. Macquorn Rankine on a Property of Curves	65
Proceedings of the Geological Society:—	
The Earl of Selkirk on ancient Sea-marks on the Coast of Sweden	67
The Duke of Argyll on a Posttertiary Lignite in Argyllshire.	67
Mr. W. S. Shea on recent discoveries of Gold in New Brunswick	68
Mr. W. Wheelwright on the Discovery of Coal in the Andes.	68
The Rev. P. B. Brodie on the Presence of Purbeck Beds at Brill, Buckinghamshire	68
Mr. H. W. Bristow on the Lower Lias of Glamorganshire.	69
Mr. C. Moore on Abnormal Conditions of Secondary Deposits when connected with the Somersetshire and South Wales Coal-basins	69
The Rev. P. B. Brodie on the Drift in a part of Warwickshire.	70
Mr. W. B. Dawkins on the Dentition of <i>Rhinoceros leptorhinus</i> (Owen)	70
Mr. J. W. Judd on the Strata of the Lincolnshire Wolds.	71
Researches on the Absorbing-Action which certain Volatile Liquids and their Vapours exert on the Heat from a Lamp with a Glass Chimney, by M. P. Desains	72

	Page
On the Spectrum of Mars, with some Remarks on the Colour of that Planet, by William Huggins, F.R.S.	74
On a New Method of determining the Resistance of Galvanic Circuits, by Prof. V. Waltenhofer.....	77
Researches on the Variation in the dispersion of Liquids under the influence of Heat, by M. J.-B. Baille	79

NUMBER CCXXVIII.—AUGUST.

Mr. H. Wilde's Experimental Researches in Magnetism and Electricity. (With a Plate.)	81
Dr. J. C. Draper on an Apparatus for the Detection of Adulterations in Coffee.....	104
The Rev. J. B. Harbord on the Conic Theory of Heat, in connexion chiefly with the Metamorphosis of Matter	106
Prof. C. Bohn on Negative Fluorescence and Phosphorescence.	108
Mr. J. Croll on the Change in the Obliquity of the Ecliptic, and its Influence on Climate	127
Dr. A. C. Brown on Sir Benjamin Brodie's System of Chemical Notation	129
Mr. C. Tomlinson on the so-called "Inactive Condition of Solids"	136
Mr. J. P. Harrison on the Moon's Influence over Cloud.....	143
Proceedings of the Royal Society:—	
Mr. H. C. Sorby on a Definite Method of Qualitative Analysis of Animal and Vegetable Colouring-matters by means of the Spectrum-Microscope	144
Some Observations on Glass Tears, by E. Reusch	166
Experiments on the Magnetism and Diamagnetism of Gases, by M. J. Chautard	168

NUMBER CCXXIX.—SEPTEMBER.

M. L. Schwendler on testing Telegraph-Cables during the process of Sheathing	169
Prof. J. D. Dana on a Connexion between Crystalline Form and Chemical Constitution, with some inferences therefrom..	178
The Rev. J. B. Harbord on the Conic Theory of Heat considered in connexion with General Sensation and the three senses of Touch, Taste, and Smell	185
Mr. G. J. Stoney on the Connexion between Comets and Meteors.	188
M. G. Van der Mensbrugghe on the Tension of Liquid Films..	192
Sir David Brewster on the Radiant Spectrum	202
Dr. A. Naumann on the Specific Heat of Gases for Equal Volumes under Constant Pressure	205
Mr. W. Ellis on the Influence of the Full Moon on Cloud....	218
Dr. Atkinson's Chemical Notices from Foreign Journals	220

	Page
Mr. C. Tomlinson on the so-called "Inactive" Condition of Solids	229
Proceedings of the Royal Society:—	
Mr. A. Claudet on a Self-acting Focus-Equalizer	231
Prof. Stokes on the Internal Distribution of Matter which shall produce a given Potential at the Surface of a Gravitating Mass	235
Mr. T. Graham on the Occlusion of Hydrogen Gas by Meteoric Iron	239
On a Photometer for Measuring the Transparency of the Air, by M. A. de la Rive	241
On the Laws which govern the general distribution of Heat over the Earth, by Pliny Earle Chase	244
On the spontaneous Change of a Liquid Cylinder into Isolated Spheres, by Dr. Félix Plateau	246

NUMBER CCXXX.—OCTOBER.

Mr. A. B. Northcote on the Water of the River Severn at Worcester	249
Mr. C. W. Siemens on a Resistance-measurer.	270
Mr. J. Douglas on the Source of Muscular Force	273
Dr. E. Lommel on the Theory of the Evening Glow and analogous Phenomena	275
Dr. Rankine on the Approximate Drawing of Circular Arcs of given Lengths.	284
M. L. Lorenz on the Identity of the Vibrations of Light with Electrical Currents	287
Prof. Lielegg on the Spectrum of the Bessemer-flame.	302
Proceedings of the Royal Society:—	
Mr. G. J. Stoney on the Physical Constitution of the Sun and Stars	304
Prof. H. E. Roscoe on the Chemical Intensity of Total Daylight at Kew and Pará in 1865-67	313
Dr. T. Andrews on the Identity of the Body in the Atmosphere which decomposes Iodide of Potassium with Ozone	315
Proceedings of the Geological Society:—	
Mr. R. Etheridge on the Physical Structure of North Devon.	317
Sir W. E. Logan on new Specimens of <i>Eozoon</i>	318
Dr. Dawson on Fossils recently obtained from the Laurentian Rocks of Canada	318
Mr. W. Whitaker on Subaërial Denudation	319
Captain T. A. B. Spratt on the Bone-caves in Malta	320
Mr. R. Tate on the Lower Lias of the North-east of Ireland	321
Mr. F. M. Burton on the Rhætic Beds near Gainsborough.	321
Note on the Electrical Condition of the Terrestrial Globe, by A. de la Rive	322

	Page
On Brewster's Neutral Point, by Pliny Earle Chase	325
Note on the Force which the Muscle of a Frog can develop in contracting, by M. J. Rosenthal	325
On the Absorption of Obscure Heat, by M. P. Desains	327
On the Change of Obliquity as a cause of Change of Climate, by J. Carrick Moore	328

NUMBER CCXXXI.—NOVEMBER.

Mr. D. Forbes's Researches in British Mineralogy	329
Mr. G. W. Keely on a new Attempt to determine the Resultant of two Pressures on a Fixed Point.	354
Mr. H. C. Sorby on the Colour of the Clouds and Sky	356
Mr. J. K. Laughton's Inquiry into the Evidence on which the Theory of the Circulation of the Atmosphere is based.	359
Dr. Rankine on Isoperimetric Regular Polygons	365
M. B. Riemann's Contribution to Electrodynamics.	368
Dr. A. Naumann on the Velocity of Atomic Motion	373
Mr. F. A. Tarleton on the Figure of the Bullet which experiences the least resistance from the Air	377
Dr. Rankine on the Approximate Rectification of Circular Arcs.	381
Dr. Gerlach's Contribution to the Mechanical Theory of the Electrical Current	382
Proceedings of the Royal Society:—	
Sir W. Thomson on a Self-acting Apparatus for multiplying and maintaining Electric Charges.	391
Proceedings of the Geological Society:—	
Mr. H. B. Medlicott on the Alps and the Himalayas	396
Mr. D. Mackintosh on the Terminal Curvature of Slaty Laminae in West Somerset.	397
Messrs. P. M. Duncan and J. Thomson on <i>Cyclocyathus</i> . .	398
Dr. Dawson on a new Pulmonate Mollusk	398
Mr. J. W. Salter on some tracks of <i>Pteraspis</i> (?)	398
Mr. J. W. Salter on a new <i>Lingulella</i>	399
Mr. G. Busk on the Dentition of Fossil Bears.	399
Dr. J. Haast on the Geology of the Province of Canterbury, New Zealand	399
The Rev. J. H. Timins on the Chemical Geology of the Malvern Hills	400
Mr. T. M. Hall Townshend on the Relative Distribution of Fossils throughout the North Devon series	400
Mr. W. R. Swan on the Geology of the Princes Islands ..	401
Mr. C. Collingwood on the Sulphur Springs of Northern Formosa	401
Mr. G. B. Stacey on the Geology of Benghazi.	401
Mr. E. Thornton on the Existence of large Coal-fields in the Province of St. Catherine's, Brazil.	402

	Page
Mr. G. Maw on the White Clays of the Lower Tertiaries.	402
Mr. S. V. Wood, Jun., on the Postglacial Structure of the South-east of England	402
Investigations on the Influence of Heat on the Mechanical Force of Frogs' Muscle, by M. Chmoulevitch	403
On the Intensity of the Solar Radiation, by M. J. L. Soret ..	404
On System in Mineralogical Nomenclature, by J. D. Dana ..	407

NUMBER CCXXXII.—DECEMBER.

Prof. A. de la Rive on Michael Faraday, his Life and Works..	409
Dr. W. M. Watts on the Spectrum of the Bessemer-flame. (With a Plate.)	437
Dr. C. Collingwood on a Horizontal Rainbow observed at Sea. (With a Plate.)	440
Chief Justice Cockle on the Conversion of Integrals	442
Mr. J. K. Laughton on the Natural Forces that produce the Permanent and Periodic Winds	443
Mr. J. Croll on certain Hypothetical Elements in the Theory of Gravitation and generally received conceptions regarding the Constitution of Matter	449
Mr. J. J. Sylvester's Thoughts on Inverse Orthogonal Matrices, Simultaneous Sign-successions, and Tessellated Pavements in two or more colours	461
Mr. C. H. Bennington's Description of a New Photometer ..	475
Notices respecting New Books:—Prof. W. A. Norton's Treatise on Astronomy, Spherical and Physical	477
Proceedings of the Geological Society:—	
Mr. A. Tylor on the Amiens Gravel	479
Mr. S. V. Wood and the Rev. J. L. Rome on the Glacial and Postglacial Structure of Lincolnshire and South-east Yorkshire	480
Mr. N. Whitley on Glacial Markings in the Valley of the Exe	481
Mr. A. B. Wynne on Disturbance of the Level of the Land near Youghal	481
On the Passage of Projectiles through Resisting Media, by M. Melsens	481
On Phosphorescent Light, by Dr. Kindt in Bremen	484
On the Influence of various Coloured Rays on the Decomposition of Carbonic Acid in Plants, by M. Cailletet	485
On the Interference-colours of Radiant Heat, by H. Knoblauch.	487

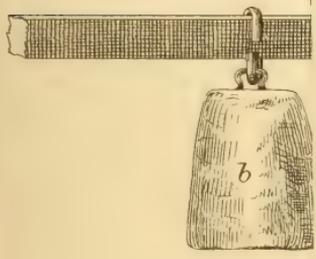
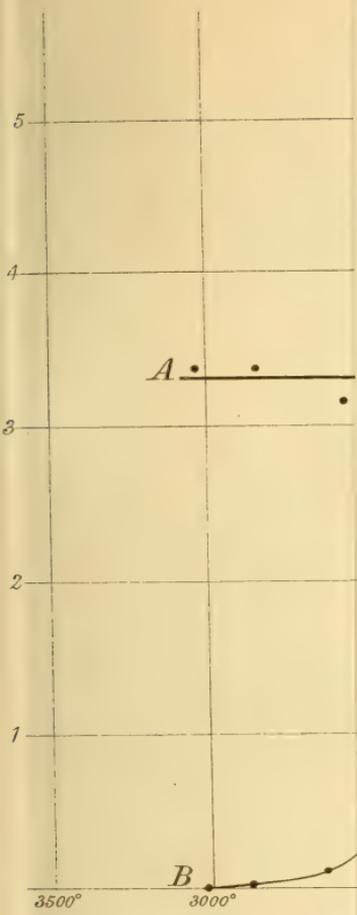
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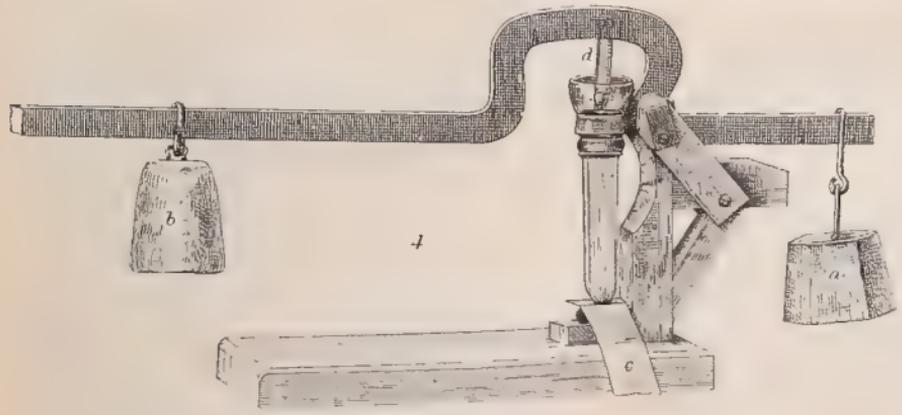
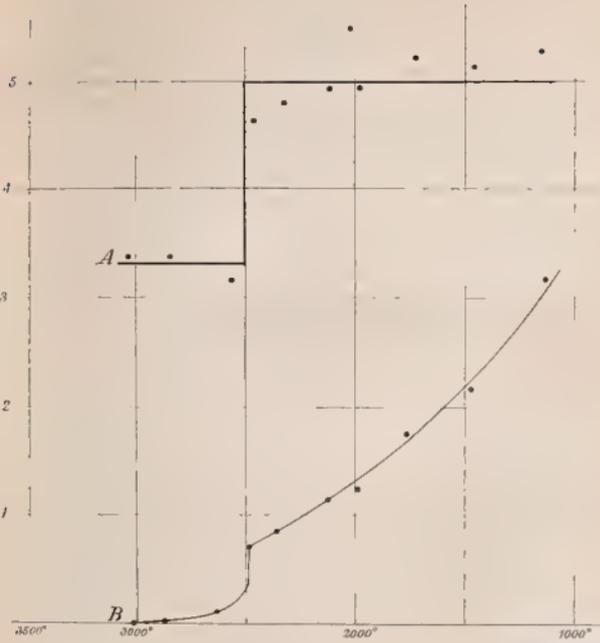
Prof. R. Bunsen on the Temperature of the Flames of Carbonic Oxide and Hydrogen. (With a Plate.)	489
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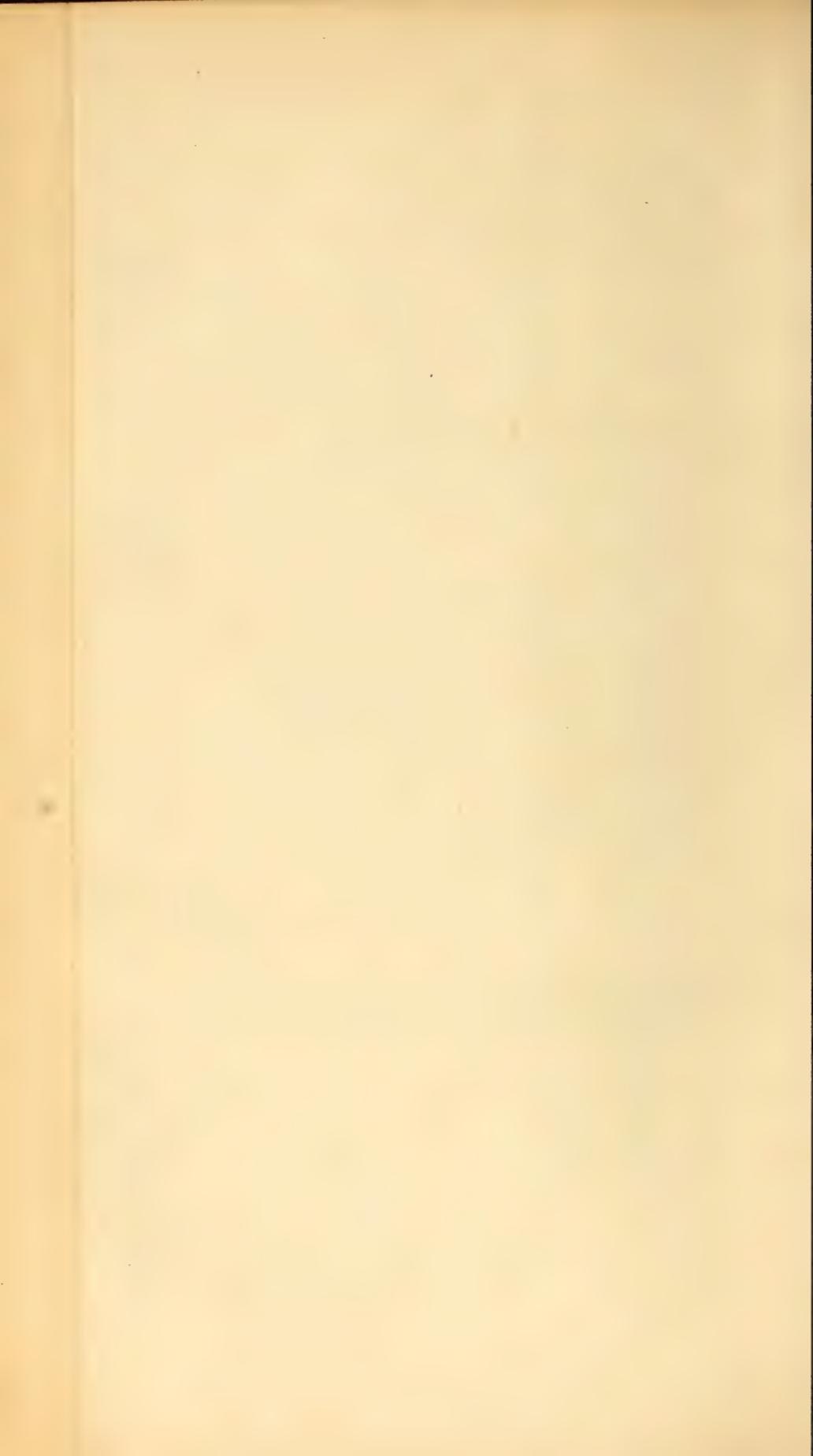
	Page
Mr. G. J. Stoney on the Solar Eclipse, August 1868	502
Dr. Atkinson's Chemical Notices from Foreign Journals	506
Mr. T. Gaffield on the Action of Sunlight on Glass	514
Prof. J. C. Pogendorff on a new Electrical Phenomenon of Motion	533
Notices respecting New Books :—	
Mr. C. H. H. Cheyne, "The Earth's Motion of Rotation, including the Theory of Precession and Nutation"	538
Mr. R. A. Proctor's Sun-views of the Earth, or the Seasons Illustrated	539
Proceedings of the Royal Society :—	
Dr. A. W. Hofmann on the History of Methylic Aldehyde.	540
Dr. J. D. Everett's results of Observations of Atmospheric Electricity at Kew Observatory, and at Windsor, Nova Scotia	543
Mr. J. H. N. Hennessey on a supposed Connexion between the Amount of Rainfall and the Changes of the Moon.	544
Proceedings of the Geological Society :—	
Mr. H. A. Nicholson on the Graptolites of the Skiddaw Series	546
Mr. P. M. Duncan on the Fossil Corals (<i>Madreporaria</i>) of the West-Indian Islands	546
Sir J. Lubbock on the Parallel Roads of Glen Roy.	547
Mr. C. Collingwood on the Geological Features of the Northern part of Formosa and the adjacent Islands; and on some Sources of Coal in the Eastern Hemisphere	548
Electroscopic Notices, by Professor Pogendorff	549
On the relative Magnitude of Molecules, by Dr. Alexander Naumann	551
On the Polarization of the Electrodes, by M. J. M. Gaugain.	553
Index	555

PLATES.

- I. Illustrative of Professor R. Bunsen's Paper on a Method of determining the Specific Gravity of Vapours and Gases.
- II. Illustrative of Mr. H. Wilde's Experimental Researches in Magnetism and Electricity.
- III. Illustrative of Dr. Watts's Paper on the Spectrum of the Bessemer-flame, and Dr. C. Collingwood's on a Horizontal Rainbow observed at Sea.
- IV. Illustrative of Professor R. Bunsen's Paper on the Temperature of the Flames of Carbonic Oxide and Hydrogen.







THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[FOURTH SERIES.]

JULY 1867.

I. *Method of determining the Specific Gravity of Vapours and Gases.* By R. BUNSEN, F.R.S., &c.*

[With a Plate.]

THE method of determining the specific gravity of gases and vapours described in the following communication does not depend upon any new principle, but, on the contrary, differs from the mode usually employed only in practical details. The object of this method is to attain the maximum amount of accuracy by the employment of the most convenient and simplest experimental means and of the smallest number of observations. It depends, in the first place, upon the ready preparation of glass vessels which possess equal capacities within one-hundredth of a cubic centimetre, and whose weights are the same to within a fraction of a milligramme; secondly, upon the employment of an unalterable perfectly air-tight stopper of very simple construction, by means of which a vessel which has been once for all weighed, may be used as frequently as may be wished for the determination of the specific gravity of gases and vapours; and thirdly, upon the possibility of arranging an air-bath of tolerably large dimensions in which almost perfectly constant temperatures can be kept up for any desired length of time.

In order to prepare vessels of equal weight and capacity, a number of large glass tubes (numbered with a diamond) are melted off a tube having a diameter of 25 millims. and a thickness of 1.3 millim., and one end of each tube is rounded in the flame. Each tube is then partly filled with an equal volume of

* Translated by Prof. Roscoe, F.R.S., from *Annalen der Chemie und Pharmacie*, vol. cxli. p. 273.

water, say from 170 to 200 cub. centims., poured in from an accurate burette. The height of the water in the tubes is then marked by a chalk line, and the tubes, having been emptied and dried, are drawn out before the blowpipe to long ends about 100 millims. in length, and having a diameter in the thickened glass of rather less than 1 millim. The end is then cut off square with a diamond, and the edges rounded in the flame. Plate I. fig. 1 represents such a vessel.

Supposing that each of the vessels I., II., III.,... has had its weight in grammes R_1, R_2, R_3 determined, it is filled with water by means of a hollow thread of glass connected to a funnel by a caoutchouc tube. Let the weights of the tubes thus filled with water be W_1, W_2, W_3, \dots ; then the capacity V_1, V_2, V_3, \dots of the vessels in cubic centimetres is

$$\begin{aligned} \text{I.} & \quad . \quad . \quad W_1 - R_1 = V_1, \\ \text{II.} & \quad . \quad . \quad W_2 - R_2 = V_2, \\ \text{III.} & \quad . \quad . \quad W_3 - R_3 = V_3, \\ \text{IV.} & \quad . \quad . \quad W_4 - R_4 = V_4. \end{aligned}$$

If V_4 turns out to have the smallest volume, the capacity of the other vessels, in order to be made equal to V_4 , must be diminished:

$$\begin{aligned} \text{I.} & \quad \text{by} \quad V_1 - V_4, \\ \text{II.} & \quad \text{by} \quad V_2 - V_4, \\ \text{III.} & \quad \text{by} \quad V_3 - V_4, \\ \text{V.} & \quad \text{by} \quad V_5 - V_4. \end{aligned}$$

This diminution of bulk is effected as follows:—A glass rod is drawn out to fine solid threads of such diameter that they can be passed through the capillary openings into the glass vessels. If s represent the specific gravity of the glass from which these threads are made, we have only to allow the following weights of given lengths of the threads to fall into the vessel to insure that they shall possess the same capacity:—

$$\begin{aligned} s(V_1 - V_4), \\ s(V_2 - V_4), \\ s(V_3 - V_4). \end{aligned}$$

In the following Table the calibration of five such vessels is given:—

No.	Weight of vessel with air, R.	Weight of vessel with water, W.	Capacity of the vessel, V.	$V - V_4$.	$s(V - V_4)^*$.	$V - s(V - V_4)$.
	grms.	grms.	cub. cent.	cub. cent.	grms.	cub. cent.
I.	55·8237	234·053	178·2293	1·6051	4·0808	176·6242
II.	57·2438	235·807	178·5632	1·9390	4·9297	176·6242
III.	56·4273	233·255	176·8277	0·2035	0·5174	176·6242
IV.	55·3758	232·000	176·6242	0·0000	0·0000	176·6242
V.	51·6725	231·020	179·3475	2·7233	6·9238	176·6242

The weight of the vessels with the solid glass threads is now

$$\begin{aligned} \text{I.} & \quad \cdot \quad \cdot \quad R_1 + s(V_1 - V_4), \\ \text{II.} & \quad \cdot \quad \cdot \quad R_2 + s(V_2 - V_4), \\ \text{III.} & \quad \cdot \quad \cdot \quad R_3 + s(V_3 - V_4). \end{aligned}$$

Amongst these $R_2 + s(V_2 - V_4)$ is found to be the greatest. Hence the weights of the other glass vessels must be subtracted from this in order to learn the weights of the glass which has to be added to each vessel, so that they all may possess the above weight. These weights are made out of glass rods whose finely drawn-out ends are broken off, or added to by fusion in the flame until they are sufficiently heavy; they are then inscribed with the number of the vessel to which they belong. The following Table contains the adjustment of the weights for the foregoing vessels:—

No.	R.	$s(V - V_1)$.	$R + s(V - V_1) = \Pi$.	$\Pi_2 - \Pi$	$\Pi + (\Pi_2 - \Pi)$.
I.	55·8237	4·0808	59·9045	2·2690	62·1735
II.	57·2438	4·9297	62·1735	0·0000	62·1735
III.	56·4273	0·5174	56·9447	5·2288	62·1735
IV.	55·3758	0·0000	55·3758	6·7977	62·1735
V.	51·6725	6·9238	58·5963	3·5772	62·1735

When any one of these adjusted vessels is mentioned, the corresponding glass weights $\Pi_2 - \Pi$ are always supposed to be included. The numbers in the last vertical column do not represent the weights of the separate vessels with the greatest degree of accuracy, as no allowance is made for variation during the weighings in the amount of atmospheric moisture and density. For the purposes of the following investigation, it suffices to determine with the utmost accuracy the differences of weight which the separate vessels possess. This is accomplished as follows:—Vessel I. is laid on the right-hand pan of the balance, vessel V. on the left-hand pan, and the position of equilibrium of the balance ascertained; V. is then replaced by each of the

* $s = 2·5424$.

other vessels, and the differences of weight of each ascertained. If the unknown weight of V. be represented by T_v , the successive differences by Δ_{II} , Δ_{III} , Δ_{IV} , and the weight of the corresponding vessels by G_{II} , G_{III} , G_{IV} , we have

$$\begin{aligned} G_{II} &= T_v + \Delta_{II} \\ G_{III} &= T_v + \Delta_{III} \\ G_{IV} &= T_v + \Delta_{IV} \\ G_v &= T_v. \end{aligned}$$

In order to determine Δ , which is seldom more than a fraction of a milligramme, it is best not to wait until the beam has arrived at a position of equilibrium, but to calculate the corresponding deviation of the pointer from the observation of a number of oscillations. Each of the vessels is filled, before the weighing, with the air of the balance-case by sucking the air out with a long hollow thread of glass, then wiped with a linen cloth, and quickly drawn through the flame of a non-luminous gas-lamp to get rid of any adhering electricity, and then weighed after waiting three-quarters of an hour, the vessel I. being always placed as tare on the right-hand pan.

The deviations from the vertical of the downwards directed pointer of the balance are taken to be positive when they occur to the left of the observer, and negative when they occur to his right. If $a_1, a_2, a_3 \dots$ represent the consecutive maxima and minima of deviation, obtained in this way, in the vibrations of the beam, we have, for the position of equilibrium of the pointer,

$$r = \frac{1}{n} \left(\frac{a_1 + a_2}{2} + \frac{a_2 + a_3}{2} + \dots + \frac{a_n + a_{n+1}}{2} \right).$$

In all the weighings seven vibrations were observed with the telescope, and the readings were commenced when the maximum deviation of the pointer was between +5 and +6. This is seen in the following example:—

$$\begin{array}{r} +5\cdot9 + 0\cdot75 \\ -4\cdot4 + 0\cdot45 \\ +5\cdot3 + 0\cdot70 \\ -3\cdot9 + 0\cdot45 \\ +4\cdot8 + 0\cdot70 \\ -3\cdot4 + 0\cdot50 \\ +4\cdot4 \\ \hline r = \frac{3\cdot55}{6} = +0\cdot592. \end{array}$$

The exactness of the single weighings may be judged of from the regularity of the differences in the second vertical column, since all errors are there shown as irregularities in the oscillations which were brought in by vibration of the ground, draughts of air, and unequal heating of the balance. It is an essential condition for the accuracy of the weighings to perform them according to a system by which all errors arising from alterations in the balance which do not occur within two closely consecutive weighings shall be fully eliminated. Let r_2, r_3, r_4, r_5 represent the position of the pointer when the balance is in equilibrium, calculated each time from the observed oscillations when the left pan is weighed successively with the vessels II., III., IV., V., whilst the vessel I. is placed on the right pan as a tare. If we take one r (in the foregoing example r_5) as a point from which the oscillations are to be reckoned, we have $r_2 - r_5, r_3 - r_5, r_4 - r_5$ as the deviations, expressed in divisions of the scale, which the several vessels produce when the beam is in equilibrium.

If we have estimated, once for all, the weight δ , which on balancing with one of the vessels corresponds to a deviation of one division of the scale, then we obtain the differences of weight Δ which the separate vessels show with the vessel V. from the equation

$$\Delta = \delta(r_n - r_5).$$

Three weighings made at different times gave for δ

	0·00079	gram.
	0·00071	"
	0·00073	"
Mean . .	0·00074	"

And as differences of weight between V. and II., III., IV.,

+0·00005	-0·00031	-0·00060
+0·00000	-0·00026	-0·00070
-0·00005	-0·00032	-0·00074
0·00000	-0·00030	-0·00068
	$= \Delta$ II.	$= \Delta$ III.
		$= \Delta$ IV.

In order to close the vessels air-tight, glass tubes are used made somewhat narrow in the middle, and about 50 millims. long and 5 millims. wide (fig. 2, $\alpha\alpha$). Their inner walls are lined with unvulcanized caoutchouc, and the end is closed with a glass stopper prepared from glass rod at the blowpipe. The tubes are lined with caoutchouc in the following manner:—A thread of strong saddler's twine is bound round the end of a seamless caoutchouc tube, which must be tolerably thick in the

body, and somewhat thicker than the glass tube which it is to line. After the portion of tubing which projects beyond the knot on the thread has been cut away with scissors, the glass tubes are strung on to the thread. If the thread is now pulled tight while the extremity of the caoutchouc tube is held fast, the latter is lengthened out and becomes so thin that the glass tubes can be conveniently pushed over it at measured distances from each other. If the extended tube is now allowed to regain its original position, it is pressed with such force against the interior of the glass tubes that perfect contact is formed where the glass and caoutchouc touch. Such caoutchouc tubes, surrounded on all sides by glass, scarcely change their weight at all in the air; and they form, when the one end is closed with its glass stopper, and the other with the fine point of the glass vessel, a perfectly air-tight connector capable of being readily opened at any time. In order that these connectors of caoutchouc may be weighed together, their weight must be equalized. Each one is distinguished by the number of the vessel to which it belongs, and the adjustment for weight is made in the following manner:—

Suppose we have five such connectors, I_k , II_k , III_k , IV_k , V_k , prepared of nearly the same size, the stoppers to each, made out of heavy glass rods (fig. 2, γ), must not at first be made to end in a ball, as represented in the figure, but in a long drawn-out glass thread. By breaking off and again melting the glass thread, it is easy to obtain the connectors with their stoppers of nearly equal weight. Connector I_k is now placed on the right pan as tare, and connector V_k is laid on the left; the position of equilibrium of the balance is observed by the oscillations, and the differences in weight which II_k , III_k , IV_k show with V_k are accurately estimated in the manner described above for the vessels themselves. If T_5 be the unknown weight of V_k , Δ_2 , Δ_3 , . . . , the differences of weight between V_k and the separate connectors, then the weight g of the same is

$$\begin{aligned} II_k & \cdot \cdot \quad g_2 = T_5 + \Delta_2, \\ III_k & \cdot \cdot \quad g_3 = T_5 + \Delta_3, \\ IV_k & \cdot \cdot \quad g_4 = T_5 + \Delta_4. \end{aligned}$$

The weighings obtained in this manner gave

-0.00059	-0.00060	-0.00041
-0.00062	-0.00046	-0.00043
-0.00070	-0.00055	-0.00053
-0.00040	-0.00043	-0.00033
Mean <u>-0.00056</u> = Δ_2	<u>-0.00051</u> = Δ_3	<u>-0.00043</u> = Δ_4

Among the glass vessels there is always one (in this case the vessel IV.) which contains no glass threads. This one serves for the reception of the gas or vapour whose specific gravity is to be determined.

The vessel which contains the fewest glass threads (in this case III.) serves for the reception of dry atmospheric air, with which the gas or vapour is to be compared. The vessel V., which contains most glass threads, is completely freed from air by means of a mercurial air-pump and then sealed up. The vessel II., containing air in any condition is sealed up, and serves in all cases as the tare on the right pan of the balance; but it is still more convenient to exhaust this vessel also and seal it up, in order to make all the weighings on one and the same balance-pan. Lastly, the vessel I. is kept in reserve, to be ready if one of the others should break or become useless. Let us suppose that IV. contains the gas or vapour to be investigated, that III. is filled with dry atmospheric air, and that both are closed with the caoutchouc connectors when at one and the same unknown pressure and at one and the same unknown temperature; then the difference of weight between the vacuous vessel V. and the vessel IV. filled with gas or vapour, divided by the difference of weight between the same empty vessel V. and the vessel III. filled with dry air, is the specific gravity sought.

It is convenient to make all weighings according to one and the same method, in order to avoid confusion and errors in calculation. Each single weighing consists of three elements:—

(1) The use of the weight p , for weighing to a centigramme.

(2) The use of the milligramme rider on the divided scale m , for weighing to a milligramme.

(3) The observations of oscillations δr , for weighing to a tenth of a milligramme.

Suppose, whilst II_k and II. are weighed as tare on the right pan, and IV^k and IV., containing the gases or vapours weighing P_g , are laid on the left pan, that the values of the three elements are $p_t, m_g, \delta r_g$, in which, when the weights are laid on the right pan, the value of p_t is negative, when they are laid on the left pan it is positive. When m is placed on the right arm of the beam it is positive, when on the left it is negative, and the deviations of the vertical pointer are to be considered positive towards the left and negative towards the right. The weight G calculated for the zero of the scale is then

$$G = T_v + \Delta_{IV} + T_5 + \Delta_4 + p_t + m_g + \delta r_g + P_g.$$

If IV_k and IV. be now replaced by the vacuous vessel V. and V^k , and the three new elements p_v, m_v , and δr_v again determined for the weight G equal to the former one, we have the

following expression, supposing that p_t (as is most convenient) lies for this second weighing on the balance:—

$$G = T_v + T_s + p_t + m_v + \delta r_v + p_g,$$

and consequently, by subtraction from both equations, the weight P_g , viz. the weight of gas or vapour contained in the vessel IV.,

$$P_g = p_g + m_v - m_g + \delta r_v - \delta r_g - \Delta_{IV} - \Delta_4.$$

Just in the same way the weight of P_1 is found, which is that of the vessel III., containing dry air at equal temperature and pressure:—

$$P_1 = p_1 + m_v - m_1 + \delta r_v - \delta r_1 - \Delta_{III} - \Delta_3.$$

Now, since the weights of equal volumes of vapours or gases, at a sufficient distance from their points of condensation, and when both are at one and the same temperature and under one and the same pressure, are proportional to their densities, it follows that the desired specific gravity of the gas or vapour is given by the equation

$$S = \frac{P_g}{P_1}.$$

When one does not possess the means of completely pumping the air from the vessel V., it is necessary to calculate the weight λ of the residual air, which is deduced from the volume of the vessel, the temperature, and the height of the column of mercury in the manometer at the time of sealing up. The equation is then

$$S = \frac{p_g + m_v - m_g + \delta r_v - \delta r_g - \Delta_{IV} - \Delta_4 + \lambda}{p_1 + m_v - m_1 + \delta r_v - \delta r_1 - \Delta_{III} - \Delta_3 + \lambda}.$$

When the vessel V., containing 176.6 cub. centims., was sealed up, the temperature stood at 18° C., and the height of the manometer 0^m.003 of mercury; therefore

$$\lambda = 0.00085 \text{ gm.}$$

The connectors I_k and V_k of the vessels I. and V. only play the part of weights during the weighing; so that it is convenient to replace them from the very first by glass weights, which are less changeable, and by means of which new connectors can be adjusted to the proper weight for the vessels III. and IV. It is especially advisable, when the connectors III_k and IV_k have been in contact with dry gases or vapours, to weigh them before each new experiment, and, when necessary, to provide them with a new caoutchouc lining.

The thermostat, or air-bath for constant temperature, in which the vessels with dry air or gases or vapours can be heated to one and the same temperature, is represented in fig. 3.

A is a cylinder of sheet-copper 40 centimetres long, closed at the lower end; its section is an ellipse, having a horizontal major

axis of 8 centimetres, and a minor axis of 5.5 centimetres. From this cylinder, at seven places equally distant from each other, project copper wires 7 to 8 millimetres thick, which are riveted and brazed into the copper cylinder.

The wires pass through the lamp-chimneys in such a manner that they lie at right angles to the vertical axes of the cones of the non-luminous flames, and are placed at the point where the temperature is highest and most uniform. By this arrangement it is seen that the temperature of the interior of the copper vessel is rendered very nearly independent of the height of the flame, and is almost entirely regulated by the distance of the lamps from the cylinder. To avoid the irregularities in the distribution of heat conducted in this way, a somewhat smaller copper vessel of the same shape, also closed below, is firmly connected to the outer one, in such a manner that a thin layer of air separates the one from the other.

The inequalities in the heating of the outer copper jacket are so completely dispelled by radiation from this layer of air, that, when the lamps are properly placed, the interior of the inner copper jacket retains a constant temperature for a very considerable time, as soon as an equilibrium is established between the heat conducted by the wires and that which passes off by the air. The upper ends of both copper vessels can be closed with covers pierced with three corresponding holes, through two of which project the points of the glass vessels which are being heated, and through the third a thermometer. In order to obtain as constant a temperature as possible, the apparatus must be carefully screened from draughts of air and other cooling or heating influences, and the heights of the flames kept nearly equal by means of a gas stopcock or by a regulator. If the pressure of gas is only equal to a water-column of $\frac{3}{4}$ of an inch in height (the pressure which the gas-establishments usually pledge themselves to maintain), the lead pipe which leads the gas to the chief tube of the apparatus must be scarcely less than an inch in diameter, so that the flames may reach a height sufficient to keep both the copper rods in the middle part of the flame, and not to have the upper rod heated only by the extreme point of the flame. The flames can be arranged in three different positions equidistant from the copper vessel. These are recognized by notches on the conducting-rods. The three positions represent the temperatures 123°·6, 144°·6, and 176° C. If the conducting-rods be surrounded by a second row of flames, the temperature rises to 210°; and if a greater number of conducting-rods are employed, it is easy to obtain constant temperatures above 300° C.*

* I use constantly, for chemical operations which require a constant

On noticing that the temperature begins to vary from $0^{\circ}\cdot 1$ to $0^{\circ}\cdot 2$, it is sufficient, in order to regain the original temperature, slightly to move one of the lamps. That this is the case may be seen from the following observations, in which neither the stop-cock was altered nor any special regulator employed.

First arrangement of the lamps.			Second arrangement of the lamps.		
h	m	$^{\circ}$	h	m	$^{\circ}$
8	0	70 C.	10	0	143.0 C.
	5	55.0		5	144.6
	10	93.8	10*		144.9
	15	111.8	15		144.7
	20	119.6	20		144.4
	25	122.2	25		144.1
	30	123.1	30		144.5
	35	123.3	35		144.5
	40*	123.4	40		144.4
	45	123.3	45		144.4
	50	123.2	50		144.4
	55	123.2	55		144.3
9	0*	123.4	11	0	144.3
	5	123.2			
	10	123.2			
	15	123.2			
	20*	123.4			
	25	123.5			
	30	123.2			
	35	123.2			

The vessels to be placed in the thermostat are filled in different ways, according as a gas or a vapour is being examined. Gases are filled in as follows:—

The vessels III. and IV., which are to receive the gas and dry atmospheric air, are placed in the thermostat, where they rest on a wire frame and are surrounded on all sides by a layer of air. The thermostat is then closed with its cover, and the holes from which the capillary tubes of the vessels project are stopped up by corks or cylinders of chalk, which have first been bored through, and then cut up from end to end, so as to form two halves capable of sliding over each other. A thermometer for observing when the temperature becomes stationary is placed in the third hole, which is closed up in a similar manner. The

temperature, thermostats like the one here described, in which the heat is introduced from the sides.

* The asterisks denote the times when an alteration in the lamps was required.

atmospheric air and the gas to be investigated are separately led through the caoutchouc tubes *a, a*, and, after being dried in the chloride-of-calcium tubes *d, d*, pass out at the exit tubes *c, c*. They then pass by means of two of the capillary glass tubes (fig. 5), which fit air-tight into the connectors *c*, down to the bottom of the vessels, through their somewhat wider capillary necks. In order to do this safely and conveniently, the glass thread is laid on the board *b*, which is of the same inclination as the thermostat, and then allowed to glide into the glass vessel; its pliability allows it then to be readily connected with the drying-tube *d* by means of the caoutchouc connector *c*. When the atmospheric air has been completely driven out of the vessel by the dried gas, the glass thread is, in the same way as before, withdrawn. If this is done slowly, and the gas is heavier than air, enough flows from the wide part of the glass thread, out at its lower end, to replace the space in the vessel occupied by the glass thread before its removal. But even when this is not the case, a serious error is not to be apprehended, since the volume of the glass vessel 176 cub. centims., compared with that of the glass thread by which these 176 cub. centims. would be diminished, amounts only to 0.09 cub. centim.; so that the error would amount to not more than $\frac{1}{10,000}$ of the volume of the gas.

If the apparatus is used for vapour-density determinations, then dry atmospheric air is led, in the way just described, into the proper vessel, and the liquid whose vapour is to be examined is poured into the vessel used for gases by means of a funnel with a capillary stem; it is then placed in the thermostat. The capillary tube projecting from the thermostat is then provided with the arrangement represented at B, fig. 3, which is intended to regain the evaporated liquid without loss, and also to form a self-closing stopper which can cut off the interior of the vessel from the outer air. Fig. 6 shows the arrangement in its details. *a* is a thick platinum-wire which does not quite fill up the capillary of the vapour-vessel; *b* a tube pushed over both platinum-wire and capillary, serving to lead off the vapours. When the stream of vapour enters from *f* into the wider tube *b*, it causes a suction of air at the opening of the tube *c*, which effectually prevents the dropping away of liquid from that point; *d* is a solid copper wire placed in front of the cork, hammered out at *d*, and fitted closely round the glass tube, while it is kept in position by the little weight β . By means of the moveable side-lamp attached to the thermostat, this copper wire is heated to such a point that a hissing noise is produced when it is moistened at *d* with a drop of the fluid which is being experimented on.

This heated wire prevents the condensation of liquid in and

behind the cork, which would render the results of the experiment completely illusory. The vapours from the liquid which was boiled in the thermostat pass through the tube *b* into the cooling-apparatus represented in fig. 3; and they condense at *f*, in consequence of the radiation of heat from the platinum-wire, and form a thin layer of liquid which prevents the entrance of air into the vapour-vessel. When the evaporation is nearly done, the bubbles of vapour pass through this layer of liquid very slowly; and when it is complete these bubbles cease entirely. The layer of liquid is then drawn up between the glass and platinum-wire into the capillary of the vapour-vessel, and forms there a thread of liquid which acts as a vapour-tight stopper. There is no danger of this thread of liquid being pushed forward and destroyed by the expansion of the vapour heated from its boiling-point to the temperature of the thermostat, since all vapours entering from the vapour-vessel into the capillary are condensed there, and thus serve only to increase the length of the thread of liquid. On the other hand, the thread cannot be sucked back beyond the cork from cooling of the vapour, since it must approach the heated part of the tube *d*, and cannot pass that point without itself producing vapour which would condense in its original position between *c* and *f*. If the thread is pushed forward by degrees as the temperature rises, we may conclude with certainty that the vapour is rendered impure by a permanent gas. This may result either from some atmospheric air not having been expelled when the tube was filled, or from the decomposition of the vapour into permanent gases. The temperature to which a vapour may be heated without decomposition is shown at once by this indication.

The vapour-stopper which has been described renders the estimation of vapour-densities a very convenient and certain operation. The vessel containing the liquid to be evaporated has only to be placed in the thermostat next the one containing dry air; the apparatus may then be left without supervision until the thread of fluid is formed in the capillary tube. The vessel filled with dry air is then connected with a chloride-of-calcium tube, and the apparatus again left to itself for an hour and a half; the thread of liquid is now expelled as quickly as possible by heating between *d* and *c*, and, lastly, the caoutchouc connectors are fastened on. The vessels need now only be wiped, drawn through the flame, allowed to cool for two hours, and the difference of weight from the vacuous vessel determined on a good balance.

The method which has been described has the advantage that, by the use of the same vessels, the specific gravity of both gases and vapours is obtained by simple estimation of two differences

of weight, without the volume, the pressure, and the temperature of these gases and vapours being known. By means of this method the errors of weighing, arising from the hygroscopic nature of the glass, from the variable amount of aqueous vapour in the balance-case, and from changes of temperature and pressure, are satisfactorily eliminated. The degree of exactitude of which the method is capable may be judged from the following experiments, which were performed with a balance weighing up to 100 grammes. I give first of all six carbonic-acid determinations, in which both the gas and the dry atmospheric air were freshly filled-in each time, and the temperature of the thermostat varied to between 10° C. and 15° C.

No.	Experiment I.	Experiment II.	Experiment III.	Experiment IV.	Experiment V.	Experiment VI.
p_g	+0.3200	+0.3200	+0.3200	+0.3300	+0.3200	+0.3200
r_v	+0.595	+0.196	-0.104	+0.066	-0.508	-0.262
r_g	-0.019	-0.516	-0.400	+0.950	+0.141	+0.079
m_g	+4.0	+4.0	+3.4	+4.2	+5.4	+5.0
m_v	+7.0	+6.0	+9.3	0.0	+9.4	+9.0
Δ_{IV}	-0.00068	-0.00068	-0.00068	-0.00068	-0.00068	-0.00068
Δ_1	-0.00043	-0.00043	-0.00043	-0.00043	-0.00043	-0.00043
λ	+0.00085	+0.00085	+0.00085	+0.00085	+0.00085	+0.00085
p_1	+0.2100	+0.2100	+0.2100	+0.2100	+0.2100	+0.2100
r_v	+0.516	+0.409	-0.508	+0.354	+0.016	-0.250
r_g	+0.975	+0.253	-0.025	-0.466	-0.387	+0.050
m_1	+4.0	+4.0	+3.2	+4.4	+6.0	+5.4
m_v	+6.0	+5.0	+6.6	+6.0	+7.0	+7.0
Δ_{III}	-0.0003	-0.0003	-0.0003	-0.0003	-0.0003	-0.0003
Δ_3	-0.00051	-0.00051	-0.00051	-0.00051	-0.00051	-0.00051
δ^3	+0.00074	+0.00074	+0.00074	+0.00074	+0.00074	+0.00074

From these data for calculation, the following values are got for the specific gravity of carbonic acid :—

I.	1.525
II.	1.525
III.	1.528
IV.	1.529
V.	1.528
VI.	1.529

In none of the experiments did the weight of carbonic acid exceed 0.35 gm. According to Regnault's classical researches carried out with more than 19 grms. of carbonic acid, the specific gravity of this gas was

1.52901.

The same vessels were employed for the determination of the

density of ether vapour which was heated up to 143° C. The vessels were filled anew in each experiment.

	Experiment VII.	Experiment VIII.	Experiment IX.
p_g	+0.3700	+0.3900	+0.3800
r_v	+0.317	+1.150	+0.525
r_g	-0.471	-0.558	+0.458
m_g	+0.6	+8.7	0.0
m_v	+7.1	0.0	+3.5
Δ_{IV}	-0.00068	-0.00068	-0.00068
Δ_4	-0.0002	-0.00060	+0.0003
λ	+0.00085	+0.00085	+0.00085
p_1	+0.1500	+0.1500	+0.1500
r_v	-0.250	-0.150	-0.667
r_1	-0.208	+0.691	+0.541
m_1	+9.4	+0.4	+0.3
m_v	+6.0	0.0	0.0
Δ_{III}	-0.0003	-0.0003	-0.0003
Δ_3	+0.00023	+0.00023	+0.00023
δ	+0.0005	+0.0005	+0.0005

These weighings give the following result for the specific weight of ether vapour:—

VII.	2.569
VIII.	2.563
IX.	2.565

The following two estimations of aqueous vapour, which was also heated to 143° C., were also conducted in the same vessels. The amount of vapour weighed was only about 80 milligrammes.

	Experiment X.	Experiment XI.
p_g	+0.0800	+0.0800
r_v	+0.608	-0.550
r_g	+0.573	+0.591
m_g	+2.2	+3.4
m_v	0.0	0.0
Δ_{IV}	-0.00068	-0.00068
Δ_4	-0.00451	-0.00271
λ	+0.00085	+0.00085
p_1	+0.1400	+0.1300
r_v	-0.116	+0.291
r_1	+0.050	+0.091
m_1	+6.4	+1.1
m_v	0.0	0.0
Δ_{III}	-0.00030	-0.00030
Δ_3	+0.00111	+0.00108
δ	+0.0005	+0.0005

The specific gravity of the vapour of water calculated from these elements is

X.	0.629
XI.	0.622

It is necessary to know the exact temperature to which the vapour was heated, since, as is well known, the specific gravity of vapours depends directly on the temperature. This is best calculated from the weight of the dry atmospheric air P_1 contained in the vessel III., whose volume V is known from the calibration, while the height of the barometer has been noted at the closing of the vessel.

If we call the coefficient of expansion of air α , and that of the glass β , then

$$\frac{0.76 \times 773 P_1}{VP} = A,$$

and the temperature sought is

$$t = \frac{A-1}{\beta - \alpha A}.$$

II. On *Vortex Atoms*.

By Professor Sir WILLIAM THOMSON, *F.R.S.**

AFTER noticing Helmholtz's admirable discovery of the law of vortex motion in a perfect liquid—that is, in a fluid perfectly destitute of viscosity (or fluid friction)—the author said that this discovery inevitably suggests the idea that Helmholtz's rings are the only true atoms. For the only pretext seeming to justify the monstrous assumption of infinitely strong and infinitely rigid pieces of matter, the existence of which is asserted as a probable hypothesis by some of the greatest modern chemists in their rashly-worded introductory statements, is that urged by Lucretius and adopted by Newton—that it seems necessary to account for the unalterable distinguishing qualities of different kinds of matter. But Helmholtz has proved an absolutely unalterable quality in the motion of any portion of a perfect liquid in which the peculiar motion which he calls “*Wirbelbewegung*” has been once created. Thus any portion of a perfect liquid which has “*Wirbelbewegung*” has one recommendation of Lucretius's atoms—infinity perennial specific quality. To generate or to destroy “*Wirbelbewegung*” in a perfect fluid can only be an act of creative power. Lucretius's atom does not explain

* Communicated by the Author, having been read before the Royal Society of Edinburgh.

any of the properties of matter without attributing them to the atom itself. Thus the "clash of atoms," as it has been well called, has been invoked by his modern followers to account for the elasticity of gases. Every other property of matter has similarly required an assumption of specific forces pertaining to the atom. It is as easy (and as improbable—not more so) to assume whatever specific forces may be required in any portion of matter which possesses the "Wirbelbewegung," as in a solid indivisible piece of matter; and hence the Lucretius atom has no *prima facie* advantage over the Helmholtz atom. A magnificent display of smoke-rings, which he recently had the pleasure of witnessing in Professor Tait's lecture-room, diminished by one the number of assumptions required to explain the properties of matter on the hypothesis that all bodies are composed of vortex atoms in a perfect homogeneous liquid. Two smoke-rings were frequently seen to bound obliquely from one another, shaking violently from the effects of the shock. The result was very similar to that observable in two large india-rubber rings striking one another in the air. The elasticity of each smoke-ring seemed no further from perfection than might be expected in a solid india-rubber ring of the same shape, from what we know of the viscosity of india-rubber. Of course this kinetic elasticity of form is perfect elasticity for vortex rings in a perfect liquid. It is at least as good a beginning as the "clash of atoms" to account for the elasticity of gases. Probably the beautiful investigations of D. Bernoulli, Herapath, Joule, Krönig, Clausius, and Maxwell on the various thermodynamic properties of gases, may have all the positive assumptions they have been obliged to make, as to mutual forces between two atoms and kinetic energy acquired by individual atoms or molecules, satisfied by vortex rings, without requiring any other property in the matter whose motion composes them than inertia and incompressible occupation of space. A full mathematical investigation of the mutual action between two vortex rings of any given magnitudes and velocities passing one another in any two lines, so directed that they never come nearer one another than a large multiple of the diameter of either, is a perfectly solvable mathematical problem; and the novelty of the circumstances contemplated presents difficulties of an exciting character. Its solution will become the foundation of the proposed new kinetic theory of gases. The possibility of founding a theory of elastic solids and liquids on the dynamics of more closely-packed vortex atoms may be reasonably anticipated. It may be remarked in connexion with this anticipation, that the mere title of Rankine's paper on "Molecular Vortices," communicated to the Royal Society of Edinburgh in 1849 and 1850, was a most suggestive step in physical theory.

Diagrams and wire models were shown to the Society to illustrate knotted or knitted vortex atoms, the endless variety of which is infinitely more than sufficient to explain the varieties and allotropies of known simple bodies and their mutual affinities. It is to be remarked that two ring atoms linked together, or one knotted in any manner with its ends meeting, constitute a system which, however it may be altered in shape, can never deviate from its own peculiarity of multiple continuity, it being impossible for the matter in any line of vortex motion to go through the line of any other matter in such motion or any other part of its own line. In fact, a closed line of vortex core is literally indivisible by any action resulting from vortex motion.

The author called attention to a very important property of the vortex atom, with reference to the now celebrated spectrum-analysis practically established by the discoveries and labours of Kirchhoff and Bunsen. The dynamical theory of this subject, which Professor Stokes had taught to the author of the present paper before September 1852, and which he has taught in his lectures in the University of Glasgow from that time forward, required that the ultimate constitution of simple bodies should have one or more fundamental periods of vibration, as has a stringed instrument of one or more strings, or an elastic solid consisting of one or more tuning-forks rigidly connected. To assume such a property in the Lucretius atom, is at once to give it that very flexibility and elasticity for the explanation of which, as exhibited in aggregate bodies, the atomic constitution was originally assumed. If, then, the hypothesis of atoms and vacuum imagined by Lucretius and his followers to be necessary to account for the flexibility and compressibility of tangible solids and fluids were really necessary, it would be necessary that the molecule of sodium, for instance, should be not an atom, but a group of atoms with void space between them. Such a molecule could not be strong and durable, and thus it loses the one recommendation which has given it the degree of acceptance it has had among philosophers; but, as the experiments shown to the Society illustrate, the vortex atom has perfectly definite fundamental modes of vibration, depending solely on that motion the existence of which constitutes it. The discovery of these fundamental modes forms an intensely interesting problem of pure mathematics. Even for a simple Helmholtz ring, the analytical difficulties which it presents are of a very formidable character, but certainly far from insuperable in the present state of mathematical science. The author of the present communication had not attempted, hitherto, to work it out except for an infinitely long, straight, cylindrical vortex. For this case he was working out solutions corresponding to every

possible description of infinitesimal vibration, and intended to include them in a mathematical paper which he hoped soon to be able to communicate to the Royal Society. One very simple result which he could now state is the following. Let such a vortex be given with its section differing from exact circular figure by an infinitesimal harmonic deviation of order i . This *form* will travel as waves round the axis of the cylinder in the same direction as the vortex rotation, with an angular velocity equal to $\frac{i-1}{i}$ of the angular velocity of this rotation. Hence, as the number of crests in a whole circumference is equal to i , for an harmonic deviation of order i there are $i-1$ periods of vibration in the period of revolution of the vortex. For the case $i=1$ there is no vibration, and the solution expresses merely an infinitesimally displaced vortex with its circular form unchanged. The case $i=2$ corresponds to elliptic deformation of the circular section; and for it the period of vibration is, therefore, simply the period of revolution. These results are, of course, applicable to the Helmholtz ring when the diameter of the approximately circular section is small in comparison with the diameter of the ring, as it is in the smoke-rings exhibited to the Society. The lowest fundamental modes of the two kinds of transverse vibrations of a ring, such as the vibrations that were seen in the experiments, must be much graver than the elliptic vibration of section. It is probable that the vibrations which constitute the incandescence of sodium-vapour are analogous to those which the smoke-rings had exhibited; and it is therefore probable that the period of each vortex rotation of the atoms of sodium-vapour is much less than $\frac{1}{25}$ of the millionth of the millionth of a second, this being approximately the period of vibration of the yellow sodium light. Further, inasmuch as this light consists of two sets of vibrations coexistent in slightly different periods, equal approximately to the time just stated, and of as nearly as can be perceived equal intensities, the sodium atom must have two fundamental modes of vibration, having those for their respective periods, and being about equally excitable by such forces as the atom experiences in the incandescent vapour. This last condition renders it probable that the two fundamental modes concerned are approximately similar (and not merely different orders of different series chancing to concur very nearly in their periods of vibration). In an approximately circular and uniform disk of elastic solid the fundamental modes of transverse vibration, with nodal division into quadrants, fulfils both the conditions. In an approximately circular and uniform ring of elastic solid these conditions are fulfilled for the flexural vibrations in its plane, and also in its transverse vibrations perpendicular to

its own plane. But the circular vortex ring, if created with one part somewhat thicker than another, would not remain so, but would experience longitudinal vibrations round its own circumference, and could not possibly have two fundamental modes of vibration similar in character and approximately equal in period. The same assertion may, it is probable*, be practically extended to any atom consisting of a single vortex ring, however involved, as illustrated by those of the models shown to the Society, which consisted of only a single wire knotted in various ways. It seems, therefore, probable that the sodium atom may not consist of a single vortex line; but it may very probably consist of two approximately equal vortex rings passing through one another like two links of a chain. It is, however, quite certain that a vapour consisting of such atoms, with proper volumes and angular velocities in the two rings of each atom, would act precisely as incandescent sodium-vapour acts—that is to say, would fulfil the “spectrum test” for sodium.

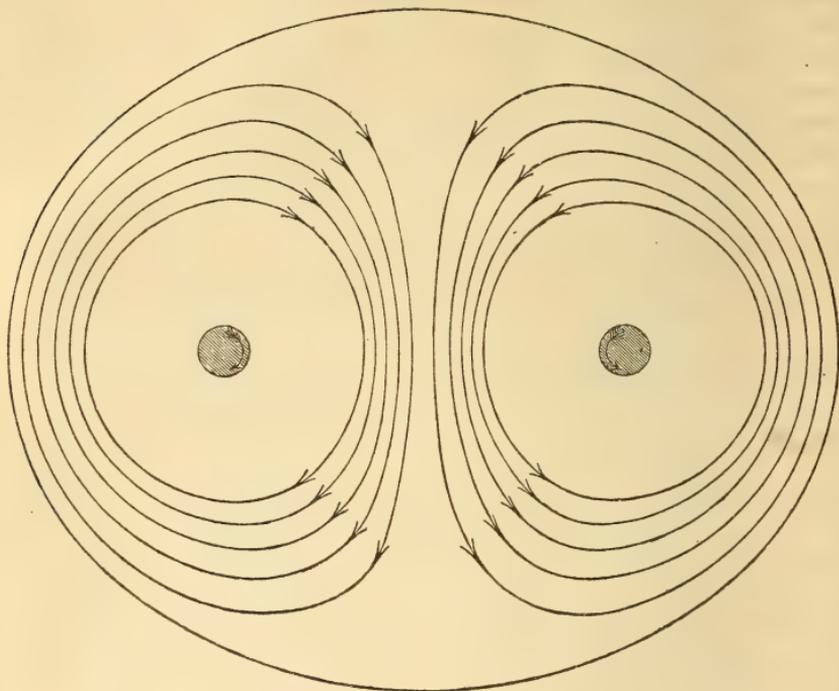
The possible effect of change of temperature on the fundamental modes cannot be pronounced upon without mathematical investigation not hitherto executed; and therefore we cannot say that the dynamical explanation now suggested is mathematically demonstrated so far as to include the very approximate identity of the periods of the vibrating particles of the incandescent vapour with those of their corresponding fundamental modes at the lower temperature at which the vapour exhibits its remarkable absorbing-power for the sodium light.

A very remarkable discovery made by Helmholtz regarding the simple vortex ring is that it always moves, relatively to the distant parts of the fluid, in a direction perpendicular to its plane, towards the side towards which the rotatory motion carries the inner parts of the ring. The determination of the velocity of this motion, even approximately, for rings of which the sectional radius is small in comparison with the radius of the circular axis, has presented mathematical difficulties which have not yet been overcome †. In the smoke-rings which have been actually observed, it seems to be always something smaller than the velocity of the fluid along the straight axis through the centre of the ring; for the observer standing beside the line of motion of the ring sees,

* [Note, April 26, 1867.—The author has seen reason for believing that the sodium characteristic might be realized by a certain configuration of a single line of vortex core, to be described in the mathematical paper which he intends to communicate to the Society.]

† See, however, note added to Professor Tait’s translation of Helmholtz’s paper (Phil. Mag. 1867, vol. xxxiii. Suppl.), where the result of a mathematical investigation which the author of the present communication has recently succeeded in executing, is given.

as its plane passes through the position of his eye, a convex* outline of an atmosphere of smoke in front of the ring. This



convex outline indicates the bounding surface between the quantity of smoke which is carried forward with the ring in its motion, and the surrounding air which yields to let it pass. It is not so easy to distinguish the corresponding convex outline behind the ring, because a confused trail of smoke is generally left in the rear. In a perfect fluid the bounding surface of the portion carried forward would necessarily be quite symmetrical on the anterior and posterior sides of the middle plane of the ring. The motion of the surrounding fluid must be precisely the same as it would be if the space within this surface were occupied by a smooth solid; but in reality the air within it is in a state of

* The diagram represents precisely the convex outline referred to, and the lines of motion of the interior fluid carried along by the vortex, for the case of a double vortex consisting of two infinitely long, parallel, straight vortices of equal rotations in opposite directions. The curves have been drawn by Mr. D. M'Farlane, from calculations which he has performed by means of the equation of the system of curves, which is

$$\frac{y^2}{a} = \frac{2x}{a} \cdot \frac{N+1}{N-1} - \left(1 + \frac{x^2}{a^2}\right), \text{ where } \log_e N = \frac{x+b}{a}.$$

The proof will be given in the mathematical paper which the author intends to communicate in a short time to the Royal Society of Edinburgh.

rapid motion, circulating round the circular axis of the ring with increasing velocity on the circuits nearer and nearer to the ring itself. The circumstances of the actual motion may be imagined thus:—Let a solid column of india-rubber, of circular section, with a diameter small in proportion to its length, be bent into a circle, and its two ends properly spliced together so that it may keep the circular shape when left to itself; let the aperture of the ring be closed by an infinitely thin film; let an impulsive pressure be applied all over this film, of intensity so distributed as to produce the definite motion of the fluid, specified as follows, and instantly thereafter let the film be all liquefied. This motion is, in accordance with one of Helmholtz's laws, to be along those curves which would be the lines of force, if, in place of the india-rubber circle, were substituted a ring electromagnet*; and the velocities at different points are to be in proportion to the intensities of the magnetic forces in the corresponding points of the magnetic field. The motion, as has long been known, will fulfil this definition, and will continue fulfilling it if the initiating velocities at every point of the film perpendicular to its own plane be in proportion to the intensities of the magnetic force in the corresponding points of the magnetic field. Let now the ring be moved perpendicular to its own plane in the direction *with* the motion of the fluid through the middle of the ring, with a velocity very small in comparison with that of the fluid at the centre of the ring. A large approximately globular portion of the fluid will be carried forward with the ring. Let the velocity of the ring be increased; the volume of fluid carried forward will be diminished in every diameter, but most in the axial or fore-and-aft diameter, and its shape will thus become sensibly oblate. By increasing the velocity of the ring forward more and more, this oblateness will increase, until, instead of being wholly convex, it will be concave before and behind, round the two ends of the axis. If the forward velocity of the ring be increased until it is just equal to the velocity of the fluid through the centre of the ring, the axial section of the outline of the portion of fluid carried forward will become a lemniscate. If the ring be carried still faster forward, the portion of it carried with the india-rubber ring will be itself annular; and, relatively to the ring, the motion of the fluid will be backwards through the centre. In all cases the figure of the portion of fluid carried forward and the lines of motion will be symmetrical, both relatively to the axis and relatively to the two sides of the equatorial plane. Any one of the states of motion thus described might of course be produced either in the order described, or by first giving a velocity to the

* That is to say, a circular conductor with a current of electricity maintained circulating through it.

ring and then setting the fluid in motion by aid of an instantaneous film, or by applying the two initiative actions simultaneously. The whole amount of the impulse required, or, as we may call it, the effective momentum of the motion, or simply the momentum of the motion, is the sum of the integral values of the impulses on the ring and on the film required to produce one or other of the two components of the whole motion. Now it is obvious that as the diameter of the ring is very small in comparison with the diameter of the circular axis, the impulse on the ring must be very small in comparison with the impulse on the film, unless the velocity given to the ring is much greater than that given to the central parts of the film. Hence, unless the velocity given to the ring is so very great as to reduce the volume of the fluid carried forward with it to something not incomparably greater than the volume of the solid ring itself, the momenta of the several configurations of motions we have been considering will exceed by but insensible quantities the momentum when the ring is fixed. The value of this momentum is easily found by a proper application of Green's formulæ. Thus the actual momentum of the portion of fluid carried forward (being the same as that of a solid of the same density moving with the same velocity), together with an equivalent for the inertia of the fluid yielding to let it pass, is approximately the same in all these cases, and is equal to a Green's integral expressing the whole initial impulse on the film. The equality of the effective momentum for different velocities of the ring is easily verified without analysis for velocities not so great as to cause sensible deviations from spherical figure in the portion of fluid carried forward. Thus in every case the length of the axis of the portion of the fluid carried forward is determined by finding the point in the axis of the ring at which the velocity is equal to the velocity of the ring. At great distances from the plane of the ring that velocity varies, as does the magnetic force of an infinitesimal magnet on a point in its axis, inversely as the cube of the distance from the centre. Hence the cube of the radius of the approximately globular portion carried forward is in simple inverse proportion to the velocity of the ring, and therefore its momentum is constant for different velocities of the ring. To this must be added, as was proved by Poisson, a quantity equal to half its own amount, as an equivalent for the inertia of the external fluid; and the sum is the whole effective momentum of the motion. Hence we see not only that the whole effective momentum is independent of the velocity of the ring, but that its amount is the same as the magnetic moment in the corresponding ring electromagnet. The same result is of course obtained by the Green's integral referred to above.

The synthetical method just explained is not confined to the case of a single circular ring specially referred to, but is equally applicable to a number of rings of any form, detached from one another, or linked through one another in any way, or to a single line knotted to any degree and quality of "multiple continuity," and joined continuously so as to have no end. In every possible such case the motion of the fluid at every point, whether of the vortex core or of the fluid filling all space round it, is perfectly determined by Helmholtz's formulæ when the shape of the core is given. And the synthetic investigation now explained proves that the effective momentum of the whole fluid motion agrees in magnitude and direction with the magnetic moment of the corresponding electromagnet. Hence, still considering for simplicity only an infinitely thin line of core, let this line be projected on each of three planes at right angles to one another. The areas of the plane circuit thus obtained (to be reckoned according to De Morgan's rule when autotomic, as they will generally be) are the components of momentum perpendicular to these three planes. The verification of this result will be a good exercise on "multiple continuity." The author is not yet sufficiently acquainted with Riemann's remarkable researches on this branch of analytical geometry to know whether or not all the kinds of "multiple continuity" now suggested are included in his classification and nomenclature.

That part of the synthetical investigation in which a thin solid wire ring is supposed to be moving in any direction through a fluid with the free vortex motion previously excited in it, requires the diameter of the wire at every point to be infinitely small in comparison with the radius of curvature of its axis and with the distance of the nearest of any other part of the circuit from that point of the wire. But when the effective moment of the whole fluid motion has been found for a vortex with infinitely thin core, we may suppose any number of such vortices, however near one another, to be excited simultaneously; and the whole effective momentum in magnitude and direction will be the resultant of the momenta of the different component vortices each estimated separately. Hence we have the remarkable proposition that the effective momentum of any possible motion in an infinite incompressible fluid agrees in direction and magnitude with the magnetic moment of the corresponding electromagnet in Helmholtz's theory. The author hopes to give the mathematical formulæ expressing and proving this statement in the more detailed paper, which he expects soon to be able to lay before the Royal Society.

The question early occurs to any one either observing the phenomena of smoke-rings or investigating the theory,—What conditions determine the size of the ring in any case? Helm-

holtz's investigation proves that the angular vortex velocity of the core varies directly as its length, or inversely as its sectional area. Hence the strength of the electric current in the electromagnet, corresponding to an infinitely thin vortex core, remains constant, however much its length may be altered in the course of the transformations which it experiences by the motion of the fluid. Hence it is obvious that the larger the diameter of the ring for the same volume and strength of vortex motions in an ordinary Helmholtz ring, the greater is the whole kinetic energy of the fluid, and the greater is the momentum; and we therefore see that the dimensions of a Helmholtz ring are determinate when the volume and strength of the vortex motion are given, and, besides, either the kinetic energy or the momentum of the whole fluid motion due to it. Hence if, after any number of collisions or influences, a Helmholtz ring escapes to a great distance from others and is then free, or nearly free, from vibrations, its diameter will have been increased or diminished according as it has taken energy from, or given energy to, the others. A full theory of the swelling of vortex atoms by elevation of temperature is to be worked out from this principle.

Professor Tait's plan of exhibiting smoke-rings is as follows:—A large rectangular box, open at one side, has a circular hole of 6 or 8 inches diameter cut in the opposite side. A common rough packing-box of 2 feet cube, or thereabout, will answer the purpose very well. The open side of the box is closed by a stout towel or piece of cloth, or by a sheet of india-rubber stretched across it. A blow on this flexible side causes a circular vortex ring to shoot out from the hole on the other side. The vortex rings thus generated are visible if the box is filled with smoke. One of the most convenient ways of doing this is to use two retorts with their necks thrust into holes made for the purpose in one of the sides of the box. A small quantity of muriatic acid is put into one of these retorts, and of strong liquid ammonia into the other. By a spirit-lamp applied from time to time to one or other of these retorts, a thick cloud of sal-ammoniac is readily maintained in the inside of the box. A curious and interesting experiment may be made with two boxes thus arranged, and placed either side by side close to one another or facing one another so as to project smoke-rings meeting from opposite directions—or in various relative positions, so as to give smoke-rings proceeding in paths inclined to one another at any angle, and passing one another at various distances. An interesting variation of the experiment may be made by using clear air without smoke in one of the boxes. The invisible vortex rings projected from it render their existence startlingly sensible when they come near any of the smoke-rings proceeding from the other box.

III. *On Professor Stokes's proof of Clairaut's Theorem.*
 By Archdeacon PRATT, F.R.S.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

ABOUT a year ago I expressed an opinion in your Magazine, that Professor Stokes's proof of Clairaut's theorem is not more general than Laplace's. In this I was wrong. My showing you in few words how I came to make this mistake will illustrate the great disadvantage under which those labour who are far away by thousands of miles from intercourse with the leaders of scientific thought, and from libraries where books and journals of the higher branches of mathematical investigation can be regularly consulted.

Several years ago Professor Stokes wrote two papers on Clairaut's theorem and attractions,—one of them in the Cambridge and Dublin Mathematical Journal, and the other in the Cambridge Philosophical Transactions. In the first paper he announced that the second was under preparation. But in the second there is no allusion to the first. He was kind enough to send me copies of both; but the second only reached me. The first, by some accident which it is impossible at this distant date to trace, never came to hand, and I did not know of its existence. As to any allusions I saw elsewhere to the new investigation, I had no reason for supposing that what I had received (viz. the paper in the Cambridge Philosophical Transactions) did not contain everything. It was the other paper which gave a complete view of the new method. In the paper which did reach me there was a step which I thought inconclusive. But I see now that this arose from my misconceiving the author's meaning, from which I should have been saved had I possessed the first paper. Subsequent correspondence with Professor Stokes brought to light that there was a paper missing. I have made every effort to get a copy and have at last succeeded, but only at the end of last March! I take this early opportunity of recalling the opinion which I expressed.

On the same authority I must yield the point that the sphere is the only surface which regulates the redistribution of the mass so as to leave the external attraction unaltered. Complicated as is the function in page 263* of your Number for April last, an *à priori* demonstration by Professor Stokes, which I have just received from him, shows that it must be capable of being made

* For $\frac{1}{r^{i+3}}, \frac{1}{a^{i+3}}, \frac{r}{a}$ read $r^{i+3}, a^{i+3}, \frac{a}{r}$.

independent of i for other surfaces besides the sphere, impossible though it be to see through the labyrinth of such a function. (In the paper dated March 23, the quantities $\phi_1 \dots$ are all functions of i , and it is only the i th that is equal to zero.)

J. H. PRATT.

Calcutta, May 18, 1867.

IV. *Chemical Notices from Foreign Journals.*
By E. ATKINSON, Ph.D., F.C.S.

[Continued from vol. xxxiii. p. 454.]

M. SORET, as the result of some experiments on ozone*, assigned to this substance a density once and a half that of oxygen. He has endeavoured† to control this result by a determination based on the unequal diffusion of gases of different densities.

Imagine two vessels superposed, separated by a diaphragm perforated by a hole which can be opened or closed at pleasure, and suppose at starting that the lower vessel contains a definite mixture of oxygen and chlorine, while the upper one contains pure oxygen. If communication be established, some of the chlorine will diffuse through, and after a certain time its quantity may be determined. If now the experiment is made with a mixture of oxygen and ozone in the same proportions, a certain quantity of ozone will diffuse into the upper vessel; if this quantity be less than the quantity of chlorine, it may be concluded that the ozone is less dense. If the duration of the experiment were very short, the quantities diffused would be inversely as the square roots of the densities of these gases.

The construction of an apparatus for this experiment presents several difficulties, owing to the circumstance that neither mercury nor water among liquids, nor any organic substances or metals among solids, could be used.

For diffusion-vessels, two large glass tubes about 45 millims. internal diameter were used, the ends of which were closed by glass plates. Their capacity was about 250 cubic centims. The glass plates were long and rectangular in shape, and perforated by a hole of suitable size and dimensions. The ends of the apparatus could thus be closed; or by sliding the plate so that the hole coincided with the interior of the vessel the closing was only partial, and by these holes gas could be allowed either

* *Phil. Mag.* S. 4. vol. xxxi. p. 82.

† *Comptes Rendus*, May 6, 1867.

to enter, to emerge, or to diffuse. By means of springs, partly of brass and partly of platinum, the plates were kept pressed against the ends, but not so as to prevent the possibility of sliding them. The edges were lubricated with sulphuric acid, which prevented any escape of gas, provided the pressure did not exceed certain limits which were never attained in the experiments.

When one of the vessels had been filled with oxygen, and the other with oxygen mixed with a small quantity of chlorine or ozonized oxygen, the first vessel was placed on the second. They were separated by the two glass plates in contact, between which a drop of sulphuric acid had been placed. Communication was then established between the two vessels by sliding the plates until the holes coincided. Diffusion then commenced; the aperture through which it was effected was 5 millims. in diameter; and it was continued for forty-five minutes, at the expiration of which time communication was intercepted between the two vessels.

The proportion of chlorine or of oxygen contained in each vessel at the end of the experiment was determined by passing the gas through iodide of potassium. The disk which closed each tube at its upper extremity consisted of a thick piece of glass with a conical hole, into which fitted the carefully ground conical end of a delivery-tube. When the vessels after diffusion had been closed, each was placed over a vessel of sulphuric acid, into which the lower end dipped to the depth of from 2 to 3 centims. The delivery-tube having been fitted to the upper disk, the latter was slid in such a manner that one of the ends of the delivery-tube was under solution of iodide of potassium, while the other end was over the interior of the vessel. By means of a glass rod the lower disk was slid away under sulphuric acid so as to open the glass vessel below, and a current of air was admitted so as to drive the gas through the solution of iodide. The quantity of iodine liberated was determined in the ordinary manner. The sum of the quantities of chlorine or of ozone found in the two vessels gave the quantity of gas in the lower vessel at the commencement of the experiment; the quantity found in the upper vessel represented that which was introduced by diffusion. The following results were obtained with mixtures of chlorine and oxygen.

Diffusion of Chlorine.

Quantity of chlorine mixed with oxygen in the lower vessel before the experiment, V.	Quantity of chlorine passed by diffusion into the upper vessel in 45 minutes, v.	Ratio, $\frac{v}{V}$.	Difference between the value of v found directly, and calculated from the mean of experiments, d.
cub. cent.	cub. cent.		cub. cent.
3.10	0.74	0.2387	+0.04
4.27	1.01	0.2365	+0.04
6.64	1.48	0.2230	-0.03
10.34	2.34	0.2263	-0.01
11.18	2.51	0.2245	-0.025
17.91	4.05	0.2261	-0.015
53.44	12.13	Mean ratio 0.2270	

The constancy of the ratio $\frac{v}{V}$, and more especially the smallness of the differences in the fourth column, show that the quantity of chlorine diffused is proportional to the quantity of chlorine mixed with oxygen in the lower vessel at the beginning of the experiment.

Working with the mixtures of oxygen and ozone obtained directly by electrolysis, the following results were obtained* :—

Diffusion of Ozone.

V.	v.	$\frac{v}{V}$.	d.
cub. cent.	cub. cent.		cub. cent.
4.68	1.29	0.2756	+0.02
9.13	2.45	0.2683	-0.02
9.49	2.53	0.2660	-0.04
10.89	3.03	0.2782	+0.08
12.71	3.40	0.2675	-0.04
46.90	12.70	Mean ratio 0.2708	

* The volumes of ozone are calculated on the assumption that the volume of ozone is double the volume of the quantity of oxygen absorbed—that is, assuming the density 1.658 for ozone. Moreover, whatever be the hypothesis, the ratio $\frac{v}{V}$ is not modified.

Here, as in the case of chlorine, it is seen that the quantity diffused is proportional to the mass of ozone contained in the lower vessel at the commencement of the experiment. But the diffusion of ozone is more rapid than that of chlorine, and it must therefore be concluded that the density of ozone is less than that of chlorine.

For each cubic centimetre of chlorine originally contained in the lower vessel, 0.227 cubic centim. penetrate into the upper one in forty-five minutes, whereas for each cubic centimetre of ozone 0.271 cubic centim. pass. The ratio of these numbers, $\frac{0.227}{0.271} = 0.8382$, is very nearly as the inverse ratio of the square root of the densities, assuming that the density of ozone is one and a half that of oxygen. That is to say, $\frac{\sqrt{1.658}}{\sqrt{2.44}} = 0.8243$.

The ratio is nearer unity than the theoretical one, as ought to be the case. Soret concludes from these experiments, and from analogous ones made with carbonic acid, that the *density of ozone prepared by electrolysis is once and a half that of oxygen*.

Carré has given to the French Academy* a description of some new apparatus for producing cold; they are based on Leslie's well-known experiment, and are on such a scale and so certain in their effects as to render them practically useful.

The first problem to solve was the construction of a simple and inexpensive air-pump which, while easy to construct and work, should produce an exhaustion up to a millimetre without being altered by acid vapours. Such a one was exhibited to the Academy; it had cost 60 francs, and had been at work without repair for eighteen months; and it produced ice with acid diluted to 52 degrees.

The second problem was to combine a mode of contact of aqueous vapour with acid which, without appreciably diminishing the gaseous flow, would enable the acid to be diluted so as to obtain the maximum effect. Two principal methods led to this result: the first consists in making a thin stream of acid pass through a tube through which circulates aqueous vapour; the second consisted in terminating the delivery-tube of this vapour by a moveable horizontal jet which skims the surface of the bath of acid, and which ascends in proportion as the acid increases in volume by absorbing moisture.

* *Comptes Rendus*, May 6, 1867.

The receivers for acid, formed of an alloy of lead and antimony, stand without change a pressure of five to six atmospheres, while the charge to which they are exposed never exceeds one atmosphere. There is no doubt that they can resist for a number of years the attack of sulphuric acid; it covers them at once with a coating of sulphate, which almost entirely prevents any further erosion.

The pump, which is made of copper, is protected against the action of sulphurous acid, always disengaged from acid recently introduced, by an arrangement which constantly and necessarily keeps its sides coated with oil: this, so far from hindering its work, makes it as perfect as possible. The valves are moved mechanically, and cannot get out of order.

These arrangements furnish apparatus which can retain a vacuum for many months, and produce 2 or 3 kilogrammes of ice for a kilogramme of acid at 66 degrees, which is only taken out when dilution is reduced to about 52 degrees. Freezing generally commences three or four minutes after exhausting; if cold water of 3 or 4 degrees is wanted, two minutes are sufficient; and agitation for a few seconds restores the air it has lost *in vacuo*.

Besides sulphuric acid, which is the most economical agent, various hygrometric substances may be used, among which caustic potash and soda are distinguished by the intensity of the cold they produce.

Von Pettenkofer and Voit* have made a series of experiments on the elimination of carbonic acid, and the absorption of oxygen in the waking state and during sleep. These experiments were made by means of the apparatus of which a brief account has already appeared in this Journal†. They made determinations of the products of respiration, which exhibit a great difference between the respiration of a man *working* and *resting*, and between a man in the *waking* and in the *sleeping* state. The subject of the experiments was a powerful working man, 28 years old. During the day of rest and of work he had exactly the same food, with the exception that when he was at work 600 grms. more of water were consumed. The numbers obtained are given in the following small Table:—

* Munich Academy, November 10, 1866. *Zeitschrift der Chemie*, January 1867.

† *Phil. Mag.* S. 4. vol. xxi. p. 544.

July 31, 1866. Day of rest.

Time of day.	Eliminated			Absorbed oxygen.	For every 100 parts of oxygen absorbed there are in the carbonic acid separated
	Carbonic acid.	Water.	Urea.		
From 6 A.M. to 6 P.M.	532.9	344.4	21.7	234.6	175
From 6 P.M. to 6 A.M.	378.6	483.6	15.5	474.8	58
In 24 hours	911.5	828.0	37.2	708.9	94
August 3, 1866. Working-day.					
From 6 A.M. to 6 P.M.	884.6	1094.8	20.1	294.8	218
From 6 P.M. to 6 A.M.	399.6	947.3	16.9	659.7	44
In 24 hours	1284.2	2042.1	37.0	954.5	98

It follows from these results that the elimination of carbonic acid is greater at day than during the night; further, that on a working-day the quantity of carbonic acid and water eliminated is far greater than on a day of rest, while the quantity of oxygen absorbed on both days is pretty equal. In return, however, for this the quantity of oxygen absorbed during the night which follows the working-day is greater. The authors deduce therefrom the following conclusions:—The oxygen absorbed during the night is, so to speak, stored up to be used on the following day for the complete oxidation of the food to carbonic acid. The oxygen in the water excreted is, for the most part, already contained in the mixed food used as diet. If a man works during the day, the excretion of carbonic acid and the consumption of the previously accumulated oxygen is considerable; the oxygen consumed is replaced during sleep by an increase in the quantity inspired, and thus the material for new exertions of force is again collected. On a day of rest the excretion of carbonic acid and of water is smaller, but still considerably greater than corresponds to the quantity of oxygen which is taken in during breathing. The absorbed oxygen therefore remains several hours in the body, where it probably furthers a slow oxidizing process before it is again excreted as carbonic acid and water. Both on the day of rest and in the night following the day, the quantity of carbonic acid inspired is small and equable, corresponding to the small and equable muscular motions made during sleep. The exhalation of aqueous vapour and the perspiration, however, are far greater in the night after the working-day than after the day of rest; this probably brings about the com-

plete cooling of the body. The equality in the quantity of urea separated on the day of rest and on the working-day confirms the observation made by Voit in the case of the dog, that increase in muscular work produces no increase in the decomposition of albumen.

These experiments acquire confirmation from some which Henneberg made in 1865 with two oxen in the respiration-apparatus at Weende. He only observed during twelve hours of daytime, and found that the quantity of oxygen taken in at daytime is not sufficient to explain the great quantity of excreted carbonic acid. In twenty-two experiments with different kinds of food, he obtained for 100 parts of inspired oxygen 131, 173, 145, 193, 163, 132, 259, 222 parts of oxygen in the exhaled carbonic acid. The circumstance that the apparatus at Weende can only be used in summer, and that he was prevented from prosecuting the experiments in the summer of 1866, were the reasons that Henneberg was hindered in experimentally demonstrating the cause of its abnormality.

In proving the necessity of a store of oxygen in the body for the exertion of mechanical force, the authors made two experiments with patients suffering from *diabetes mellitus* and *leukæmia lienalis*. Persons suffering from these diseases have always good appetite, but even with the richest and most nourishing food have not the smallest feeling of power, and are not refreshed by sleep. Experiment showed that in the case of these there is by no means such a difference of exhalation of carbonic acid and absorption of oxygen between day and night as in the case of healthy subjects. Those suffering from leukæmia absorb less oxygen at night than at day.

The authors think, and undoubtedly with reason, that the study of the products of respiration of man and animals will contribute to an accurate knowledge of the process of nutrition, both in the healthy and in the sick subject.

Hoppe-Seyler* has made some experiments on the diffusion of cane-sugar, grape-sugar, albumen, and gum in water. A solution of these bodies was placed in a glass vessel with plane parallel sides in such a manner under water as to avoid any mixing. The progressive diffusion was determined in different layers by means of a polariscope. Complete experiments were in fact only made with cane- and grape-sugar; an extremely slow diffusion was observed in the case of gum and albumen. The author sums up his principal results as follows:—

(1) The velocity of the motion of diffusion which a body exhibits in a liquid is, apart from its chemical affinity and the co-

* *Zeitschrift für Chemie*, January 1867.

hesion of its particles, almost entirely dependent on the composition of adjacent layers of liquid. An action at a distance or an action of mass could be observed here just as little as in chemical processes.

(2) Owing to the quick decrease of the velocity of this motion, an equalization in the composition of two diffusible layers of liquid, lying one upon the other, is only attained in several months or even years, if the height of each of these layers amounts from 1 to $1\frac{1}{2}$ decimetre. According to the author's experiments, this cannot be doubted in the case of sugar; the diffusion of salts in liquids is probably more rapid.

(3) The penetration of particles of sugar in its diffusion through water is not appreciably hindered by the concentration of the solution already formed. The author considers it probable that in concentrated layers viscosity hinders diffusion; on the other hand, the attraction of particles of sugar for particles of water is always less, the greater the quantity of water that already surrounds the former, so that these opposing actions produce an equalization of the velocity of diffusion in concentrated and dilute solutions.

(4) Cane-sugar and grape-sugar exhibit under similar circumstances almost equal diffusive velocity in water. Serous albumen and gum diffuse in water with extreme slowness.

Hoppe-Seyler has found indium* both in Wolfram of unknown origin, and in a specimen from Zinnwald. From a preliminary determination he found in the former 0.0228 per cent. oxide of indium. The Wolfram contained some zinc, but no sulphur, and therefore contained no blende. For its preparation, the mineral may either be digested with hydrochloric and nitric acids, or be fused with carbonate of soda, experiments having shown that in this way there is no appreciable loss of indium. The indium is separated from the other metals contained in Wolfram by the usual methods.

Richter†, the discoverer of indium, gives the following method of obtaining the metal:—The residue left after dissolving zinc in sulphuric acid contains indium and various other metals. This is dissolved in nitric acid, and the solution, mixed with sulphuric acid, is evaporated; into this solution, which must be largely diluted and but slightly acid, sulphuretted hydrogen is passed, which precipitates the indium along with cadmium and copper.

This precipitate is dissolved in hydrochloric acid, precipitated by ammonia, and this treatment repeated until all cadmium and zinc are separated from indium. The last quantities of iron which remain mixed with indium are removed by a partial precipitation by ammonia and carbonate of soda.

* Liebig's *Annalen*, vol. cxl. p. 247.

† *Comptes Rendus*, April 22, 1867.

The oxide of indium, dried and placed in a porcelain crucible, is reduced by hydrogen or by coal-gas, and the metal melted under a layer of cyanide of potassium.

The metal is white, with a shade like that of platinum; it is extremely soft and ductile; its specific gravity at 20° C. is 7.15, and its equivalent = 35.9 (H=1). The hydrated oxide is completely precipitated from its solutions by ammonia and by potash; the presence of tartaric acid prevents this precipitation.

The oxide is of a dark brown while hot, but of a straw-colour when cool. Sulphuretted hydrogen precipitates very little indium from a concentrated acid solution; it is, however, precipitated in great part from very weak and slightly acid solutions: the precipitation is almost complete in the acetic solution. Sulphuret of indium is of a beautiful yellow colour like that of sulphuret of cadmium.

The salts of indium are colourless. The chloride obtained by passing chlorine over heated oxide is extremely volatile; it gives colourless crystalline plates, and is extremely hygroscopic.

The spectrum of indium, which led to the discovery of the metal, has two lines:—one of a deep blue; the other weaker, in the violet.

V. *On certain recent Contributions to Astro-Meteorology.*

By H. A. NEWTON*.

1. *Radiant-Points, or Radiant Areas.*

FOR more than twenty years a Committee of the British Association for the Advancement of Science has annually made an extended report on "Observations of Luminous Meteors," in which have been given in detail the times, paths, physical appearances, and other phenomena of meteors seen during the years immediately preceding. No special effort has been made until recently to elaborate this mass of material. R. P. Greg, Esq., of Manchester, assisted by Mr. A. S. Herschel, at last undertook the task of plotting the paths contained in the voluminous records. By this means they have determined between fifty and sixty radiants for different periods of the year. Professor E. Heis of Münster has in like manner, from observations made by himself and his assistants during a period of twelve years, deduced a similar and, to a considerable extent, a corresponding series of radiant-points. The latter series is in the Monthly Notices of the Royal Astronomical Society, vol. xxiv. p. 213. Both series are in the Report of the British Association for 1864, and in the Proceedings of the British Meteorological Society for January 18, 1865. From the latter source we give the following Table:—

* From Silliman's American Journal for May 1867.

Comparison of the Epochs and Positions of Radiant-points of Shooting-stars, concluded independently by R. P. Greg, Esq., and Dr. E. Heis.

Reference number.	From observations contained in the British Association Catalogues, &c., 1845-63. (R. P. Greg.)					Observed at Münster, 1849-61. (E. Heis.)			
	Epochs in their order of commencement.	No. of meteors mapped.	Distinctive numbers (Greg).	Position of radiant.		Distinctive letters.	Right ascension.	North declination.	Epochs to the nearest half-month.
				Right ascension.	North declination.				
1	Dec. 20-Jan. 30	20	ii	22°	75°	A ₁	29°	50°	Jan. 1-15.
2	Dec. 20-Jan. 30	13	ii a	5	85	A ₂	15	63	Jan. 16-31.
3	Dec. 21-Feb. 4	28	iii	68	17	N ₁	285	84	Jan. 1-15.
4	Jan. 2-Jan. 3	52	i	234	51	N ₂	0	90	Jan. 16-31.
5	Jan. 2-Feb. 4	30	iv	133	40	AG ₁	235	52	Dec. 16-31.
6	Jan. 5-Jan. 25	15	iv a	173	32	K ₃	242	51	Jan. 1-15.
7	Feb. 4-Feb. 26	36	v	147	34	K ₁	166	52	Jan. 16-31.
8	Feb. 7-Feb. 26	20	vi	136	70	? M ₂	150	60	Feb. 1-14.
9	Feb. 9-Feb. 17	13	vii	76	40	MG ₁	130	63	Feb. 15-28.
10	Feb. 10-Mar. 17	21	viii	168	9	M ₃	65	51	Feb. 1-14.
11	Feb. 11-Mar. 16	10	viii a	37	1	M ₁	91	37	Feb. 15-28.
12	Feb. 19-Feb. 26	10	vi a	220	84	A ₃	170	11	Feb. 15-28.
13	Mar. 3-Mar. 27	11	xiii	44	72	S ₂	178	7	Mar. 1-15.
14	Mar. 3-Mar. 31	30	ix	145	67	S ₃	173	23	Mar. 16-31.
15	Mar. 3-Mar. 31	18	x	186	58	SG ₁	0	90	Feb. 1-14.
16	Mar. 12-Mar. 20	20	xii	223	39	N ₃	250	83	Feb. 15-28.
17	Apr. 1-June 2	52	xi	194	52	N ₄	340	80	Mar. 1-15.
18	Apr. 2-May 1	20	xiv	189	4	M ₅	125	52	Mar. 1-15.
19	Apr. 8-May 28	20	xix	227	-8	M ₆	140	50	Mar. 16-31.
20	Apr. 12-Apr. 13	17	xvi	276	26	? M ₂	140	50	" "
21	Apr. 16-May 3	30	xv	96	87	MG ₂	160	53	Apr. 1-15.
22	Apr. 19-Apr. 20	25	xvii	282	33	M ₇	150	61	Apr. 16-30.
23	Apr. 25-June 4	28	xviii	255	48	M ₈	194	5	" "
24	Apr. 30-June 4	15	xx	243	20	DG ₁	265	83	Apr. 16-30.
25	May 9-June 3	16	xviii a	277	42	DG ₂	218	20	May 1-31.
26	May 9-June 4	8	xxi	286	21	Q ₁	292	15	June 1-30.
27	May 29-June 17	18	xxii	336	45	D	332	60	May 1-31.
28	June 1-June 30	9	xx a	236	30	B ₁	333	42	June 1-30.
29	June 1-June 30	12	xxiii	300	85	B ₂	242	12	" "
30	July 2-July 24	51	xxiv	291 to 313	53 to 43	Q ₂	290	80	May 1-31.
31	July 10-Aug. 6	26	xxvii	257	13	N ₉	150	83	June 1-30.
32	July 20-Aug. 4	46	xxv	359	70	N ₁₀	315	54	July 1-15.
						B ₃	262	12	July 1-15.
						N ₁	20	85	" "
						N ₀	337	86	July 16 to Au. 15*

* Radiants marked thus are extracted from the work, by Dr. Heis, entitled *Die periodischen Sternschnuppen* (4to. Coln, 1849).

TABLE (continued).

Reference number.	From observations contained in the British Association Catalogues, &c., 1845-63. (R. P. Greg.)					Observed at Münster, 1849-61. (E. Heis.)			
	Epochs in their order of commencement.	No. of meteors mapped.	Distinctive numbers (Greg).	Position of radiant.		Distinctive letters.	Right ascension.	North declination.	Epochs to the nearest half-month.
				Right ascension.	North declination.				
33	July 22-Aug. 10	70	xxvi	344 ^o to 327	12 ^o 10	T ₁	314 ^o	15 ^o	Aug. 16-31.
34	July 29-Aug. 22	123	xxiv a	302 to 288 and 298	44 42 58	B ₄ B ₀	306 302	59 65	Aug. 16-31. July 16 to Au. 15*.
35	Aug. 6-Sept. 10	80	xxix	0	90	N ₁₂ N ₁₃	295 130	79 84	Aug. 16-31. Sept. 1-15.
36	Aug. 7-Aug. 16	...	xxviii	45 to 20	55 62	A ₀	50	51	July 16 to Au. 15*.
37	Aug. 17-Sept. 12	9	xxvii a	245 to 262	5 12	Q ₃	262	12	July 1-15.
38	Aug. 17-Sept. 30	18	xxiv b or xxx a	282	42	B ₅	293	57	Sept. 1-15.
39	Aug. 17-Sept. 30	150	xxx i. e. 314 to 347 and 333 to 333	333 52 47 41 62 13	50 52 47 41 62 34	EG E? A ₁₁ A ₁₂ A ₁₃	330 35 44 51	50 63 63 61	Oct. 16-31. Sept 1-15. Sept. 16-30. Oct. 1-15.
40	Aug. 18-Sept. 29	27	xxx or xxx b	13	34	R ₁ R ₂	53 46	35 37	Sept. 1-15. Sept. 16-30.
41	Aug. 22-Nov. 5	27	xxxii	1	15	T ₂ T ₃ T ₄	343 1 3	10 11 11	Sept. 1-15. Sept. 16-30. Oct. 1-15.
42	Sept. 6-Nov. 23	18	xxxiv	22	-9	TG U	10	-11	Oct. 16-31.
43	Sept. 20-Oct. 11	35	xxxv	83	48	AG ₂			
44	Sept. 25-Oct. 10	16	xxvi	51	84	N ₁₄	65	84	Sept. 16-30.
45	Sept. 27-Nov. 2	67	xxxiii	14	58	A ₁₄ A ₁₅	20 25	42 40	Oct. 16-31*. Dec. 1-15*.
46	Oct. 3-Oct. 20	11	xxxvii	140	45	LG ?L ₁	115	55	Dec. 1-15*.
47	Oct. 4-Nov. 10	35	xxxviii	45	33	R ₃	45	32	Oct. 1-15.
48	Oct. 18-Nov. 3	30	xxxix	83	12	O			
49	Oct. 20-Nov. 21	33	xl	91	56	F	75	40	Oct. 16-31*.
50	Oct. 31-Dec. 9	14	xliii	139	7	LH			
51	Nov. 1-Nov. 23	75	xli	16	49	AG ₃			
52	Nov. 7-Nov. 15	...	xlvi	153	22	L ₀	150	28	Nov. 1-30*.
53	Nov. 23-Dec. 9	9	xliv	279	56	DG ₃			
54	Nov. 24-Dec. 10	37	xliv	59	58	A ₁₆	37	59	Dec. 16-31.
55	Nov. 26-Dec. 30	84	xlvi	96	36	G			
56	Nov. 27-Dec. 19	10	ylvi	157	71	?L ₁ KG	115	55	Dec. 1-15*.

Total meteors, 1746 ; days, 1655 ; meteor-showers, 56.

The general results of Mr. Greg's investigations as regards *meteoric showers* are thus summed up in the Report of the Committee on Luminous Meteors.

"They appear to endure for almost any period, from twenty-four hours to eight or possibly ten weeks, differing from one another in richness or intensity of display. In some there appears to be a tendency to maximum display on particular days, as, for example, *xlvi*, lasting from November 26th to December 30th; but the most abundant display occurs from December 9th to 13th. In others no such maximum can be perceived. Their number, of fully fifty as yet ascertained, will probably not be much exceeded, unless by short-lived showers, and by others whose radiants culminate just before dawn. There is no confusion or chance in their return, but, on the contrary, the showers are very regularly recurrent every year, and, allowing a *radiant region* of 10° to 15° in diameter for each, the so-called "sporadic" meteors will become extremely scarce now that the principal showers and their radiants have been pointed out. A well-marked instance of long persistence and remarkable for having its radiant very small and fixed is the shower of August 6th to September 10th, No. *xxix*. The great majority have at the present time been as clearly defined (as regards the time of their occurrence, duration, and positions of their radiants) as in the case of the older and better-known showers of August and November. On the average of many years the radiant regions of a few are, however, still very extensive. In all, a plane, oval, or double-headed region of radiation appears to represent the conditions of the showers more correctly than a point. This elongation of the radiant region is in most cases perpendicular to the ecliptic, or parallel to the *via lactea*, in or near which the greater number of the radiants in the latter half of the year are placed. The meteors of particular showers vary in their distinctive characters, some being larger and brighter than others, some whiter, some more ruddy than others, some swifter, and drawing after them more persistent trains than those of other showers. Their connexion with the epochs and directions of large meteors still remains to be established."

From a private letter by Mr. Greg to Mr. B. V. Marsh, we learn that some minor changes are found necessary, by further observation and investigation, in the duration of the showers and the places of the radiants. Mr. Greg's charts containing the paths of nearly two thousand shooting-stars are about to be published by the British Association. We may hope to receive them within a few months. We evidently need these charts in order to discuss intelligently this important subject. While waiting for them, however, one or two remarks may not be out of place.

That the so-called sporadic shooting-stars should belong largely to rings or streams, as do the August and November meteors, is in the present state of our knowledge probable, or

at the least is not improbable. The reasonings of M. Schiaparelli, which will be spoken of further on in this article, strengthen this probability.

But we meet with some difficulty in accepting the proposition that a ring or stream may be of such breadth as to require eight or ten weeks for the earth to traverse it—that is, that the ring may extend 60° or 70° along the ecliptic; or rather, if there were so broad a ring or stream, it would not appear to have a radiant area so small and so well marked as to be detected.

The position of the radiant indicates that point of the heavens from which the *relative* motion of the meteoroids with reference to the earth is directed. This direction is the resultant of two *absolute* motions—that of the meteoroids and that of the earth. If either of these should change, the place of the radiant will change.

But during these eight or ten weeks the direction of the earth's motion would change 60° or 70° . If the direction of the meteoroids' motions were supposed parallel throughout the breadth of the stream, yet this change in the direction of the earth's motion alone appears to necessitate a change of the position of the radiant by a distance on the heavens of not less than 30° or 40° .

But for a group of such thickness we can hardly suppose the absolute motions of the meteoroids parallel throughout its breadth. Each meteoroid must move about the sun in its own orbit; and though this is not entirely inconsistent with a parallelism of the paths where the group crosses the ecliptic, yet such a case is extremely improbable. Again, it appears necessary that a meteoroid which is now on one side of the stream should be after half a revolution on the opposite side. Hence we might reasonably expect that at the centre of the stream we should find their paths crossing each other at large angles, angles comparable in magnitude to the 60° or 70° which measures on the ecliptic the breadth of the stream. Such divergence of directions of the individual members of the group would make the existence of an apparent radiant of moderate area impossible.

Again, a stream whose thickness is so great may be expected to have also large breadth in direction of the radius-vector. This again would make the parallelism of the paths, and consequently the apparent radiation improbable.

In fact a ring of such enormous thickness as to require two months for the earth to cross it, would seem not only to lose all the essential characteristics of a distinct group, but also to be unable to manifest its existence by a constant and small radiant area.

The conclusions of Mr. Greg and Dr. Heis are derived almost

entirely from meteors seen in evening hours. But the phenomenon of radiation caused by parallelism of absolute motions should be more distinctly evident as the radiant is nearer the meridian. This occurs in general in the morning hours.

While, then, the existence of rings or streams is *à priori* probable, and so the existence of radiants for *very short* periods is to be looked for, yet the series now proposed will doubtless undergo essential changes as we accumulate observations, or else some other cause than ring-formations be found to account for the radiation.

There should in any case be a tendency to a radiation, both from the zenith and from the point to which the earth is moving—hence from the region lying between these points, *i. e.* from the north-east quarter of the heavens.

2. Influence of the August and November meteors upon the temperature of the atmosphere.

Erman early asserted the existence of periods from the 5th to the 12th of February, and from the 10th to the 13th of May. These he attributed to the influence of the August and November meteors, assuming that they passed at those epochs between the earth and the sun. To the same cause he attributed certain dark days, and other appearances, said to have occurred in the years A.D. 1106, 1206, 1208, 1706, and 1547.

In a series of papers read before the Paris Academy of Sciences, and published in the *Comptes Rendus**, M. Ch. Ste.-Claire Deville has given the results of an elaborate investigation of the alleged abnormal changes of temperature in these months, as well as those alleged for corresponding days of August and November.

M. Faye, in response to the first of these papers, shows conclusively that the dark days &c. adduced by Erman cannot be referred with any probability to the meteors as their cause.

In his earlier papers, M. Ste.-Claire Deville undertakes to show that there are periodic variations of the temperature of the critical days in February, May, August, and November that correspond to secular maxima and minima of the August and November meteors. For the August meteors he assumes a maximum in 1847 or 1848, relying upon the assertion of M. Coulvier-Gravier.

The existence of a maximum for the meteors in or near those years is in itself exceedingly doubtful, and the evidence adduced by M. Deville to prove corresponding changes of temperature is also very far from satisfactory.

* Vol. lx. pp. 577, 655, 696, 909; vol. lxi. pp. 5, 61, 350; vol. lxii. pp. 1054, 1149, 1209; and lxiii. p. 1030.

M. Deville's own conclusions, as given in his later papers, themselves throw doubt upon the existence of any important connexion between the meteors and the temperature of the air. He finds that there is an accordance between the movements of the thermometer in the several months, February, May, August, and November, and that the 12th day of each of these months is the critical day of a marked inflection. But this accordance (*or solidarity*) between the several months is entirely lost when the days are so combined as to compare, not the days of the same name in the month, but those corresponding to points 90° apart on the ecliptic. But in what possible way can the meteors create oscillations in the temperature of the atmosphere, whose maxima and minima shall in November be one or two days *before* we reach them, shall in May be at the time of the passage of the earth across the plane of the November group, in August shall be one or two days *after* the shower, and in February five or six days after the earth crosses the plane of the August ring?

3. *The paths and probable origin of Shooting-stars.*

The most important recent contribution to the theory of shooting-stars is by M. Schiaparelli, of the Brera Observatory at Milan. It is contained in a series of five letters to Father Secchi, published in the *Bullettino Meteorologico* of Rome*.

By a course of reasoning similar to that which led the writer to the same conclusion†, he argues that the mean velocity of the meteoroids is considerably greater than that of the earth in its orbit. Hence their orbits are in general long ellipses or parabolas.

Assuming then (which is not improbable) that the meteoroids form in the planetary spaces a multitude of currents or continuous rings, having all possible inclinations to the ecliptic, he proceeds to inquire in what way so singular a form of grouping of cosmical matter could have been produced.

Notwithstanding the uncertainty of the determinations of the velocity of the solar system and of the stars in space, he considers it reasonable to assume that the velocities relative to the sun of the various bodies which are scattered through stellar spaces are comparable in magnitude to those of the planets in their orbits.

Suppose now one of these bodies, a comet for instance, to come by its proper motion so near to the sun that solar attraction far exceeds the attraction of the stars, and yet to be at such

* Vol. v. Nos. 8, 10, 11, 12; and vol. vi. No. 2.

† Silliman's American Journal, vol. xxxix. pp. 205-7; and Mem. Acad. of Nat. Sciences, vol. i. pp. 309-311.

a distance that its annual parallax is but a few seconds. Its orbit about the sun will then be a conic section. Suppose now the sun to be at rest, and regard the relative velocity of the comet as the real velocity. Let fall a perpendicular from the sun upon the direction of the comet's motion. The area described about the sun in a unit of time will be equal to one-half the product of this perpendicular into the comet's velocity.

As, however, the velocity is in general comparable in magnitude to the planetary velocities, and the perpendicular is in general enormously greater than the distances of the planets from the sun, it follows that the areas described in a unit of time are in general very much larger than corresponding planetary areas. But these areas are as the parameters of their corresponding orbits. Hence in general the orbits of such comets would have enormous dimensions in every direction, and the bodies themselves would remain invisible to us because of their great distance from the earth.

In two cases only would they come within our field of vision. When the perpendicular is very small, a hyperbolic orbit would result, differing in general not much from a right line. As the radius of the sphere of our vision is only about four times the distance from the earth to the sun, this case would rarely happen.

The second case is when the relative motion of the comet is very small, that is, when the comet and sun are moving in nearly parallel paths with nearly equal velocities. An orbit very nearly a parabola would result. This case would likewise be very rare, since of all possible velocities of the bodies in the stellar spaces, very few would be nearly equal and parallel to that of the sun.

The parabolic form of the cometary orbits should not, then, surprise us: this is not the only possible form; on the contrary, it is one that is very rare; but owing to the nature of comets, and to our feeble powers of vision, we can see only those which describe just these orbits. Nor is there reason for wonder that the planes of their orbits have no relation to the plane of the ecliptic.

The characteristics peculiar to planets can be accounted for by their formation in the solar system; those peculiar to the orbits of comets depend on the manner in which the sun attracts them to itself from the depths of space. The latter, then, should have an origin foreign to our system. To which of these two classes belong the shooting-stars? Are they planets, or are they comets?

The planetary hypothesis has hitherto been favoured by astronomers. The ring theory, however, as M. Schiaparelli believes, leads to serious difficulties respecting the origin of the shooting-stars, whether the rings be regarded as integral parts of the

solar system from the beginning, or whether they be supposed to have come from space, being attracted hither by the sun.

There are two arguments which show that the meteoroids should be classed with the comets as originally strangers to our system, rather than with the planets. The first is that their orbits seem to be inclined at all angles to the ecliptic*; the second that their velocity requires a long if not a parabolic orbit.

Admit, then, that the shooting-stars come from stellar space, and we have no longer closed rings of short period, but streams of a parabolic form, for which the period of revolution, if there is one, is very great. Now this parabolic current, which at first seems so strange a form, is not only possible, but it is the *only* form in which a cosmical cloud drawn from stellar space by solar attraction can approach the sun and become visible to us.

To prove this, he supposes a cosmic cloud of the size of the sun to be at first at a distance equal to 20,000 (the mean distance from the earth to the sun being unity). The rarity of the matter in this cloud is regarded as such that the mutual attraction of the particles may be disregarded. This cloud of particles is then supposed to have a velocity (relative to the sun) perpendicular to a line drawn to the sun, $\frac{1}{20000}$ of the mean velocity of the earth, or about 100 yards a minute.

The several particles will move in elliptic orbits about the sun, but these ellipses will not be exactly equal. M. Schiaparelli shows that a cloud, of spherical form at first, would be deformed little by little, and ultimately drawn out so as to have a very small transverse section. It will now lie along a parabolic arc, of which the sun is the focus. When the particle originally at the centre of the globe reaches its perihelion, at a distance from the sun equal to $\frac{1}{2}$, the anterior portion of the group will have passed its perihelion 193.5 days, and have already crossed in its outward course the orbits of the minor planets. The end of the

* That the inclinations of the orbits of the meteoroids are of all magnitudes, M. Schiaparelli infers, principally, from the positions of the radiant given by Messrs. Greg, Herschel, and Heis. Although these radiant are open to criticism, yet the above conclusion is undoubtedly true. It is readily seen that if the orbits of the meteoroids are but little inclined to the earth's orbit, the apparent paths of the meteors should themselves rarely be seen to cut the ecliptic, and that the paths produced forward should rarely cut the ecliptic above the horizon. In other words, the paths as seen in the sky should, with rare exceptions, appear to lead away from that circle. So far as my own observation is concerned, I find no such regularity. The meteors go toward the ecliptic as often as from it. Apparent radiation from points of such considerable latitude as those given in the Table on pp. 35, 36, shows also that many of the orbits of the meteoroids, at least, are inclined at large angles to the earth's orbit,

group will not have reached these orbits in its descent to the sun. The whole extends along 267° of anomaly on the parabolic orbit. The breadth of the group at its centre, in the direction of the radius vector, will be 96 metres, and in the direction perpendicular to the plane of the orbit 37 kilometres. Its density at this place will be 400 millions of times the density of the original group.

If the primitive form of the group was other than spherical, the same reasoning would apply and similar results follow.

The apparent diameter of the assumed globe, as seen from the sun, is about $0''.1$. But if the original cloud be supposed to have an apparent diameter of one minute, the resulting stream would require 636 years to pass the perihelion. Some of the nebulae have apparent diameters exceeding that of the sun. A globe of such a size ($1924''$) would be transformed into a parabolic stream which would require more than 20,000 years to pass the perihelion. The cross section of this stream would be much greater than in the preceding case, but yet not so great but that the earth might cross it in a few hours, or at most in one or two days. We may, then, without extravagant hypotheses account for the existence of meteoric currents which have been observed for hundreds or thousands of years, like that of August.

For facility of reasoning, the original position of the supposed spherical group was placed at the aphelion of a long ellipse. But results similar in kind, and comparable in degree, would result had the group been supposed at any point of a conic section of very great length.

If the orbit were an ellipse, the original form of the cloud would never be regained. At each perihelion passage the length of the stream would be increased until it formed a closed circuit. The stream would be at first periodic, but finally constant. If the orbit were open, there would be a single passage only.

The supposed cloud was deemed to exert no sensible attraction upon its particles. To justify such an assumption, M. Schiaparelli makes an estimate of the density of the August ring. For the mean distance from each other of the bodies of the August group, he obtains by computation about 100 (geographic) miles. This is obtained by quite arbitrary suppositions, but the result is very nearly that which is obtained by computations from the best data in our possession (see Silliman's Journal, vol. xxxix. p. 207, combined with observations at Hartford in 1863, when six observers counted 153 different meteors in a half hour *).

* The corresponding mean distance from each other of the members of the November group, where we crossed it last year, is 30 or 40 miles.

For the weight of the individual meteoroids he assumes one gramme, relying upon the conclusions of Mr. A. S. Herschel, who compared the light of the meteors with the light of a candle, and hence inferred their weight. The estimate seems too small; for some of the trains fill cubic miles of space with matter of sufficient consistency to form a cloud visible for minutes (see Silliman's Journal, vol. xliii. p. 86). Yet the probable size of these bodies is so small that M. Schiaparelli's reasoning is still conclusive.

To each sphere whose radius is fifty miles, he assigns, therefore, 1 gramme of matter. The cloud first supposed had only $\frac{1}{400000000}$ the density of the resultant stream at the perihelion. Suppose, however, the space originally occupied by the meteoroids of the August stream to be only one million times that now filled at the place where the earth traverses it. To each gramme of matter would originally have belonged, in that case, a volume equal to that of a sphere 10,000 miles in diameter.

He then shows that a spherical group of bodies, each body weighing 1 gramme, whatever be the dimensions of the group, must have at a distance from the sun equal to the earth's mean distance, a density such that the mutual distances of the members shall be less than 1.86 metre (2 yards), in order that the attraction of the sun may not dissolve the group. If the mutual distances of the members exceed 1.86 metre, the sun acts to separate the individuals from each other, not at the surface simply, but throughout the whole extent of the system.

But if the mutual distances are, as before determined, 100 miles, the dissolving power of the sun is 10^{15} times the mutual attraction of the particles. In like manner the dissolving-power of the sun's attraction upon a group of similar bodies distant 20,000 miles from the sun, the mutual distance of the bodies being 10,000 miles, is 125,000,000 times the attraction which the group has for one of its particles. This latter force then may be safely neglected. The dissolution or deformation of the system must, moreover, begin much further away from the sun than the assumed position of the cosmic cloud, out even in the stellar spaces. It can enter the solar system only as a parabolic current.

Even if we suppose a group that is tolerably dense approaching the sun, as, for instance, a comet without a nucleus, there is a certain limiting distance within which the differences of solar attraction tend to dissolve it. If such a group passes this limit in its descent to perihelion, the members will be scattered and the original formation will never be restored. We have thus a most singular effect of attractive force, namely, the dispersion of a system that lacks coherence.

If now a dense cloud of bodies is supposed to pass near one

of the larger planets, its orbit will be changed, and may become one of short period, like those of certain comets. If, moreover, its perihelion distance is less than the distance at which solar attraction disintegrates the groups, the cloud is dissolved into independent particles. Diversity of planetary perturbations produces in the orbits of these particles a variety of elements, especially variety in the periodic times. The group is gradually lengthened along the ellipse, and after a certain number of revolutions the cloud becomes a continuous ring. The meteors of November belong to such a group while the ring is partially formed. The August meteors probably represent a group after it is transferred into a continuous ring.

M. Schiaparelli gives a summary of the consequences which result from the preceding discussions in the following propositions, which establish the basis for a new theory of falling stars.

I. Matter is disseminated in celestial space in all possible grades of division. The first grade consists of the larger stars, either isolated or collected in systems of few members. The second is made up of large agglomerations of small stars (the *star dust* of Herschel), into which many nebulae are seen to be resolved by large telescopes. Then follow smaller bodies, which are invisible except when they approach the sun under the form of comets. Finally, the last grade consists of cosmical clouds composed of very minute elements, which have a weight comparable to that of objects which we are accustomed to handle or transport on the earth.

“ II. This last class of bodies may have been formed in space by the local concentration of the celestial matter in a manner analogous to the crystallization of substances chemically dissolved in liquids. From what occurs in these crystallizations we are even led to think that such a form of aggregation is much more probable and more frequent than the others, which take place by large masses. Hence the volume occupied by the cosmical clouds may be a notable fraction of the stellar space.

“ III. The movements of such clouds among the bodies of the universe are comparable to those of the fixed stars, and are probably due to analogous causes. When any one of them enters the sphere of attraction of the sun, it cannot be visible to us unless its orbit relative to this great luminary is a very greatly elongated conic section.

“ IV. Whatever may be the form and extent of a cosmical cloud, it cannot (with very rare exceptions) penetrate to the interior of the solar system unless it has been transformed into a parabolic current, which may consume years, centuries, and myriads of years in passing, part by part, its perihelion, forming in space a river whose transverse dimensions are very small with respect to its length. Of such currents, those which are encountered by the earth in its annual motion are rendered visible to us under the form of showers of meteors diverging from a certain radiant.

“ V. The number of meteoric currents crossing the spaces of the solar system at all possible distances and in all directions is probably very great. The exceeding rarity of the matter contained in them allows these currents to intersect mutually without causing any disturbance to one another. They may undergo progressive transpositions and deformations in space, like rivers which slowly change their bed. They may be interrupted, and thence become double or multiple, and they may even in particular circumstances become closed elliptical rings. The November meteoroids are apparently portions of such a ring in process of formation.

“ VI. The cosmical clouds having short periods of revolution around the sun, by which some are inclined to explain the appearance of shooting-stars, cannot have a permanent existence without violating the known laws of universal gravitation.

“ VII. The matter of the parabolic currents, after having passed the perihelion, returns into space in a state of dispersion greater than that which it had before the passage. In particular cases, as when the current meets a planet, very great perturbations may ensue, and a separation of some of the meteoric stars into special orbits. Such stars from that moment may be called truly *sporadic*.

“ VIII. Thus the meteoric stars, and other celestial products of analogous nature, which in past ages were commonly regarded as atmospheric phenomena, which Olbers and Laplace first ventured to make to come from the moon, and which at a later period were raised to the dignity of members of the planetary system, truly belong to the category of the fixed stars; and the name *falling stars* expresses simply and precisely the truth respecting them. These bodies have the same relation to comets that the small planets between Mars and Jupiter have to the larger planets. The smallness of the mass is in each case compensated by the very great number.

“ IX. Since we may safely regard it as certain that falling stars, bolides, and aerolites differ in nothing except their magnitude, we may conclude that the matter which has fallen from the sky is a fragment of that of which the stellar universe is formed. And as in such matter there is no chemical element that is not found upon the earth, the similarity of composition of all the visible bodies in the universe, already rendered probable by researches with the spectroscope, acquires a new argument for its credibility.”

The further question is then broached whether it is necessary for the original cloud to be made up of such small elements; whether, for example, it may not consist of a moderate number of comets. In such a case, we ought to meet from time to time with orbits somewhat unlike, which intersect each other in the depths of space, but which lose the characteristics of a common system, owing to considerable intervals between the times of perihelion passage.

The question is not a new one. Prof. Hoek of Utrecht has found several double and triple systems of comets*, which at a

* Monthly Notices, vol. xxv. p. 243; and vol. xxvi. pp. 1 & 204.

remote epoch were near each other in space, although their perihelion passages have differed by years. The most notable of these systems is that of the comets 1860 III., 1863 I., 1863 VI., which at the end of the year 760 A.D. were at distances from the sun respectively 600.00, 600.42, and 600.25, and distant from each other 12.8, 16.3, and 8.1. To a spectator from the sun, their apparent maximum distance was $1^{\circ} 33'$. Hoek believes that the comets of 1677 and of 1683 belong also to this system. The antecedent probability of a chance coincidence like that of these comets is exceedingly small.

We find thus analogies between the systems of shooting-stars and systems of comets. Can we not imagine mixed systems in which a cloud of meteoroids are grouped in space about one or more larger nuclei—that is, about one or more comets?

If a system of this kind is drawn from solar attraction into the figure of a parabolic current, the parabola described by the principal body (or those described by the principal bodies) should evidently differ very little from the line along the centre of the parabolic stream; consequently the preceding question is answered affirmatively whenever we find a meteoric current forming a parabola identical in magnitude and position with any parabolic cometary orbit. In such a case the comet will evidently form part of the current, and be one of its elements.

To test this question, M. Schiaparelli computed the following elements of the mean orbit of the August meteors, supposing it a parabola, assuming the radiant to be R. A. 44° , N. Dec. 56° , and the time of crossing the centre of the group in 1866 to be Aug. 10.75. With these elements are placed those of the comet 1862 III., according to the latest determination of Dr. Oppolzer*.

	Elements of August meteors.	Elements of comet 1862 III.
Longitude of perihelion	$343^{\circ} 38'$	$344^{\circ} 41'$
Longitude of node	$138^{\circ} 16'$	$137^{\circ} 27'$
Inclination	$64^{\circ} 3'$	$66^{\circ} 25'$
Perihelion distance	0.9643	0.9626
Motion	retrograde.	retrograde.
Perihelion passage	July 23.62	Aug. 22.9, 1860.
Period		123.4 years.

These elements do not differ by quantities greater than can be accounted for by the want of precision in the data for computing the paths of the meteors. We come thus to the unexpected conclusion, *that the great comet of 1862 is nothing else than one of the August meteoroids, and probably largest of them all.*

At the time of announcing this relation of the comet of 1862 to the August meteors, M. Schiaparelli found no comet having similar relations to the November meteors. But upon

* *Astr. Nach.* No. 1384.

the publication by Oppolzer shortly after* of the corrected orbit of the comet 1866 I., the resemblance of its elements to those of the orbit of the November group, on the supposition of a period of 53.25 years, was strikingly manifest, attracting at once the notice of several astronomers †. The following comparative elements are given by Schiaparelli (*Bullettino Meteor.* Feb. 28, 1867).

	November meteors.	Comet 1866 I.
Perihelion passage	Nov. 10.092, 1866.	Jan. 11.160, 1866.
Passage of descending node.	Nov. 13.576	
Longitude of perihelion ..	56° 25'9	60° 28'0
Longitude of ascending } node	231° 28'2	231° 26'1
Inclination	17° 44'5	17° 18'1
Perihelion distance	0.9873	0.9765
Eccentricity	0.9046	0.9054
Semi-major axis	10.340	10.324
Periodic time	33.250	33.176
Motion	retrograde.	retrograde.

The comet of Tempel, he adds, not only describes, therefore, the same orbit as the November group of meteoroids, but is in the same portion of the orbit as, and probably nearer, the head than the tail of the group ‡. The observations of November 13th, 1865, showed that the earth traversed the anterior portion of the swarm at that time. Two months later the comet passed the node; and ten months afterwards, the earth, returning to the node, encountered a dense portion of the stream §.

The comet 1862 III. (sometimes called 1862 II. by not counting Encke's comet), which seems thus to have such interesting relations with the August meteors, was discovered on the evening of the 18th of July, 1862, by Mr. H. P. Tuttle at Cambridge, Mass., and a little later on the same evening by Mr. Thomas Simons at Albany, New York ||. It was first seen in Europe on the 22nd of July, and remained visible more than two months. At its brightest its nucleus was equal to a star of the second or third magnitude, and its tail, according to some observers, was as much as 25° in length. The changes that took place in the coma and tail were quite remarkable, and were carefully observed. They will now possess a double interest.

* *Astr. Nach.* No. 1624.

† Peters, *Astr. Nach.* No. 1624; Oppolzer, *ibid.* No. 1626; Schiaparelli, *ibid.*

‡ The computed elements of the comet of 1366, though very uncertain, resemble those of comet 1866 I., and may belong to the same body. There was a remarkable star-shower in 1366, shortly after the computed perihelion passage of the comet.

§ The total length of the stream would exceed 500,000,000 miles.

|| Mr. Swift of Marathon, New York, claims to have seen it two or three days earlier; but he made no announcement of the discovery, supposing it to be another comet.

The telescopic comet 1866 I. was discovered by Tempel on the 19th of December 1865, and was visible about a month. Its minimum distance from the earth's orbit was '00660, about twice and a half the distance from the earth to the moon. This distance for Tuttle's comet is '00472, or about 430,000 miles.

4. Age of the November Group of Shooting-stars.

In the Paris Academy of Sciences, January 21, 1867, Le Verrier spoke of the November meteors (*Comptes Rendus*, vol. lxiv. p. 94). Inasmuch as the group is not a complete ring, he argues that it is of comparatively recent formation, having come into the solar system and been turned into its present orbit within a few centuries.

Now a body coming from a great distance and so having a great velocity in the vicinity of the earth, could not be thrown into an orbit nearly circular by the feeble action of the lower planets. Computation leads to this result, which is fully confirmed by the fact that the swarm passes every 33 years near the earth, and yet returns at *regular* intervals.

Assuming, then, an orbit whose period is $33\frac{1}{4}$ years, whose perihelion distance is 0.989, viz. the earth's distance from the sun on the 14th of November, and assuming the position of the radiant to be long. 142° , N. lat. $8\frac{1}{2}^\circ$, he computes corresponding elements.

The group, when it came into the system, could not be thrown into its present orbit except by a powerful perturbing cause, as was the case with the comet of 1770. Moreover comets so acted upon that the newly acquired orbit has a small perihelion distance, return necessarily to the orbit of the disturbing body, just as the comet of 1770 returned to Jupiter. We cannot help, then, being struck with the circumstance that the November group extends to the orbit of Uranus and a very little further, and that these orbits intersect, very nearly, just after the group passes its aphelion, and above the plane of the ecliptic.

The question then arises whether the group and Uranus have ever been together at this point. By calculation it is found that no such meeting could have taken place since the year 126 of our era, and that by a change of the computed node for that epoch of $1^\circ 48'$, and by placing the perihelion 4° from the descending node in November, the group would then actually strike the planet Uranus. These two changes are not greater than the possible errors of our observations.

Le Verrier's researches further show that a globular group one-third of the diameter of Uranus (more or less) might at that time have been then thrown into a shape and an orbit which

should by this time give all the phenomena of the November meteors. Its previous orbit might have been an ellipse, a parabola, or an hyperbola. Its motion might even have been direct in an elliptic or parabolic orbit.

In the course of future time, he argues, the phenomena will extend over a larger and larger number of consecutive years, diminishing at the same time in intensity. But no change in perihelion distance will make them disappear entirely. Even if this group again meets Uranus, the planet can act only upon a part of its matter, and cannot throw it all into a new orbit as Jupiter did the comet of Lexell.

These reasonings of Schiaparelli and Le Verrier have certainly great force, and make it probable that of the five possible periodic times of the November meteors that of 33.25 years is the true one. The strongest objection to this conclusion is that the radiant in November does not seem to be a point, but rather a small area. This area cannot be of great breadth in latitude, since the ring is only twenty-five or fifty thousands of miles in thickness. If, as observations seem to require, the radiant extends in longitude two or three degrees only, then the lines of apsides of the orbits of the several members of the group differ considerably. In this case it is more reasonable to suppose the orbits themselves grouped about an exact circle than about a long ellipse.

If upon examination it shall be found that the centre of the radiant area was decidedly more or less than $89\frac{1}{2}^{\circ}$ from the sun on the morning of the 14th of November last, then this objection will lose much of its force. Again, if there shall not be seen on the morning of May 12, 1867, between 1 o'clock and dawn, a few scattering members of the November group, radiating from a point 180° from Leo, this also will tend, to a certain extent, to strengthen Schiaparelli's reasonings.

VI. *Observations on Sir Benjamin Brodie's "Ideal Chemistry."*

By J. A. WANKLYN and R. R. F. DAVEY*.

SIR B. BRODIE read a paper before the Royal Society last year "On the Calculus of Chemical Operations." As will be gathered from the paper, which will be found in the *Philosophical Transactions* for 1866, p. 781, it claimed to inaugurate a new era in chemical philosophy. The Chemical Society was specially invited to attend the Meeting of the Royal Society at which the paper was read. A year afterwards (*i. e.* quite recently) Sir B. Brodie propounded the doctrines of "Ideal Chemistry" to one of the largest Meetings of the Chemical Society.

* Communicated by the Authors.

From the paper in the Philosophical Transactions just referred to, and from this discourse to the Fellows of the Chemical Society, we derive the following view of the chemical world:—

Atoms, molecules, and the like Sir B. Brodie proposes to ignore—neither affirming nor denying the existence of atoms, but simply ignoring the question. The chemical purposes which, according to him, are served by the conception of atoms are much more efficiently served by the following conception:—Chemical substances are to be referred to a standard volume—the litre. If this litre be empty, it is the unit of space. The unit of space is conceived as operated on by symbols of operation, and thereby converted into the standard volume or unit of different elements or compounds. Thus according to this conception the chemical unit of a substance, whether simple or compound, is generated by operating on the litre of space by an appropriate symbol or symbols of operation. In accordance with this notion α is the symbol of that operation by which the litre of space becomes the litre of hydrogen. By operating upon the litre of space by a single symbol of operation (it may be once or it may be twice, or even oftener), a litre of an element in the gaseous state is obtained. By operating on the litre of space with different symbols of operation, a litre of a compound results. We notice that a certain confusion is introduced by making (for brevity, as the author says) the same symbol stand for the *operation* and the *litre already operated upon*,— α being at once the symbol of the operation by which the empty litre becomes a litre of hydrogen and the symbol of a litre of hydrogen.

Here we make the remark that modern chemists will see in the standard litre a counterpart to the *standard two volumes which a molecule occupies*, and will recognize in the symbol of operation a kind of counterpart to the *atom* of an element. We confess we cannot see the advantage to be expected from ejecting the ordinary atomic hypothesis in favour of an hypothesis which is certainly false; for certainly reactions take place between, for instance, cubic millimetres of matter; certainly the litre of matter is not the unit which takes part in a chemical action; and really the vital and essential thing in the ordinary chemical system is the *ratios*.

Long sections are devoted to the development of a very bizarre algebra designed for the solving of a very simple arithmetical problem; *e. g.*,

$$x + 0 = x,$$

$$x1 = x,$$

$$x - x = 0.$$

Although the usual arithmetical meaning may be given to the signs, such as + and -, still on inspecting the following equations it will be seen that a new interpretation must be given of the combination of symbols.

$$xy = x + y \text{ (where } xy \text{ means } x \text{ "jointly" with } y\text{),}$$

$$\frac{x}{y} = x - y.$$

This introduction of a second signification imparts great apparent profundity, if not confusion, in the mathematical portion of the calculus. Thus, because "the symbol 1, regarded as a numerical symbol, possesses the property given in the equation

$$x1 = x" \text{ (i. e. } x1 \text{ meaning } x \text{ multiplied by 1),}$$

"symbol 1 is selected as the chemical symbol of no weight regarded as a component of a compound weight." The author, however, is not satisfied with the above proof that 1 shall equal 0; and fearful that his readers should mistake an inference for an assumption, he adds, "If, in the equation $xy = x + y$, $y = 1$, $x1 = x + 1$; whence since $x = x1$ and $x - x = 0$, we infer that

$$0 = 1."$$

In fact if 1 is assumed to equal 0, we can obtain such equations as the above ($x1 = x + 1$), from which the necessary inference shall not differ greatly from the original assumption. This may serve as a sufficient illustration of the algebra of the 'Chemical Calculus.'

On page 795 we are startled by the following statement:— "No uniform meaning has hitherto been attached to the symbols + and - in chemistry, notwithstanding their constant use. The prevalent opinion seems to be in favour of the use of the symbol + as the symbol of mechanical mixture. . . . A similar uncertainty prevails in the use of the symbol of identity. The symbol = is sometimes employed in chemistry as the symbol of numerical equality, at other times as the symbol of chemical transmutation."

These extracts afford a not unfair sample of the kind of reasoning to be met with in this paper. We reply to them (although at the risk of being considered tedious), that although the one side of an equation may be the other side which has undergone transmutation, yet it is not the = which expresses the transmutation, but the different arrangement of the symbols expresses the transmutation. And similarly the +, although it is often written between things in mechanical mixture, does not express such mixture, but is always used by chemists in its strictly algebraical sense.

Further on in the paper, Sir B. Brodie gives examples of the new formulæ of a great variety of chemical substances. We refer the reader to page 833, where he will find the following examples of Brodie formulæ for well-known organic compounds:—

	Brodie.	Ordinary.
Carbon	κ^y	C^n
Acetylene	$\alpha\kappa^2$	$C^2 H^2$
Marsh-gas	$\alpha^2\kappa$	$C H^4$
Olefiant gas	$\alpha^2\kappa^2$	$C^2 H^4$
Benzole	$\alpha^3\kappa^6$	$C^6 H^6$
Carbonic oxide	$\kappa\xi$	$C O$
Carbonic acid	$\kappa\xi^2$	$C O^2$
Alcohol	$\alpha^3\kappa^3\xi$	$C^2 H^6 O$
Ether	$\alpha^5\kappa^4\xi$	$C^4 H^{10} O$
Allylic alcohol	$\alpha^3\kappa^3\xi$	$C^3 H^6 O$
Benzylic alcohol	$\alpha^4\kappa^7\xi$	$C^7 H^8 O$
Glycol	$\alpha^3\kappa^2\xi^2$	$C^2 H^6 O^2$
Glycerine	$\alpha^4\kappa^3\xi^3$	$C^3 H^8 O^3$
Anhydrous acetic acid	$\alpha^3\kappa^4\xi^3$	$C^4 H^6 O^3$
Acetic peroxide	$\alpha^3\kappa^4\xi^4$	$C^4 H^6 O^4$
Lactic acid	$\alpha^3\kappa^3\xi^3$	$C^3 H^6 O^3$
Tetrachloride of carbon	$\alpha^2\chi^4\kappa$	$C Cl^4$
Chloride of ethylene	$\alpha^3\chi^2\kappa^2$	$C^2 H^4 Cl^2$
Chloroform	$\alpha^2\chi^3\kappa$	$C^2 H Cl^3$
Chloride of acetyl	$\alpha^2\chi\kappa^2\xi$	$C^2 H^3 O Cl$
Chloroacetic acid	$\alpha^2\chi\kappa^2\xi^2$	$C^2 H^3 Cl O^2$
Trichloroacetic acid	$\alpha^2\chi^3\kappa^2\xi^2$	$C^2 H Cl^3 O^2$
Chlorocarbonic acid	$\alpha\chi^2\kappa\xi$	$C O Cl^2$
Iodide of ethyle	$\alpha^3\omega\kappa^2$	$C^2 H^5 I$
Chloriodide of ethylene	$\alpha^3\chi\omega\kappa^2$	$C^2 H^4 Cl I$
Cyanogen	$\alpha\nu^2\kappa^2$	$C^2 N^2$
Hydrocyanic acid	$\alpha\nu\kappa$	$C H N$
Methylamine	$\alpha^3\nu\kappa$	$C H^5 N$
Kakodyl	$\alpha^7\rho^2\kappa^4$	$As^2 C^4 H^{12}$
Cyanide of kakodyl	$\alpha^4\rho\nu\kappa^3$	$As C^3 H^6 N$
Iodide of phosphotetrethylum.	$\alpha^{11}\omega\phi\kappa^8$	$P C^8 H^{20} I$
Mercuric ethide	$\alpha^5\kappa^4\delta$	$Hg C^4 H^{10}$

The right-hand column, containing the usual chemical symbols, has been inserted for the sake of facilitating comparison.

If the reader will make an examination, he will observe that whenever C occurs in the old formula of a substance κ occurs in the new formula. Whatever coefficient C has in the old formula, that same coefficient κ has in the new. So in like manner O of the old formula is replaced by ξ and with the same coefficient. In short, the new notation is the old one translated into Greek,

the only exception having reference to that which concerns hydrogen. With regard to hydrogen, which is thus in an altogether peculiar predicament, the rule for deducing the coefficient of α (the Brodie-symbol for hydrogen) from the old formula is this. Add to the number of atoms of hydrogen in the old formula one for each atom of chlorine or other halogen or nitrogen, &c., and divide the sum by 2, and the result will be the appropriate coefficient for α .

There is just one other difference between the new and the old formulæ, viz. in the new formulæ the arrangement of symbols is *alphabetic* according to the order of the Greek alphabet.

As Sir B. Brodie says, he might have represented the unit-volume of hydrogen by α^2 , *i. e.* the standard litre of hydrogen by α^2 instead of by α , as he has actually done. Had he done so, the new formulæ would have been absolutely the old formulæ translated into Greek, and we may add, would have been in accordance with the facts of the science.

Sir B. Brodie has chosen, for reasons which he does not disclose, to represent the molecule and the atom of hydrogen as identical. As a consequence of this mode of representation he is straightway landed at the discovery that chlorine itself is a chemical compound of hydrogen.

The following is no misrepresentation of the reasoning by which this conclusion is reached:—

The least quantity of hydrogen which can exist in my ideal world of litres is one litre of hydrogen weighing 0.089 grm., and a litre of a compound gas, if it contains any hydrogen at all, must contain not less than 0.089 grm. of hydrogen. A litre of hydrochloric acid gas certainly contains hydrogen, for we can extract hydrogen from it; therefore it must contain at least 0.089 grm. of hydrogen.

Now our modern chemical analysis extracts from a standard litre of hydrochloric acid gas 0.0445 grm. of hydrogen, and also another substance which we call chlorine, and presume to be an element. But matter is never lost, and therefore the chlorine extracted from the hydrochloric acid must contain the missing 0.0445 grm. of hydrogen.

In conclusion we beg to offer our congratulations to Sir B. Brodie that he has taken for the modulus of his system the symbol of hydrogen. Suppose, in an evil hour, he had taken ξ as the modulus, and made ξ the symbol of the operation by which the unit of space is converted into the unit of oxygen (chemists have before now been misguided enough to make oxygen their standard, we instance the Tables of equivalents wherein oxygen O=100). Mark the consequences which Sir B. Brodie has escaped. The litre of oxygen ξ is the least quantity of oxygen

which can exist ; and if a litre contain any oxygen at all, it must contain 1.430 grm. of oxygen. In a litre of steam there is certainly oxygen, there must therefore be 1.430 grm. of oxygen. Unfortunately a litre of steam weighs only 0.805 grm. Would not the modulus ξ lead us straight to phlogiston, which, as we all know, is blessed with a negative weight, and which would figure as α in steam with the negative weight $-.625$?

Chlorine a compound, phlogiston—behold the results of the Chemical Calculus.

VII. *On the Change that would be superinduced upon an Elliptic Orbit if the intensity of the force of Gravity were influenced by the centripetal velocity of the Orbital Body.* By J. J. WATERSTON, Esq.*

THE law of gravitation, as applied to the planetary motions, assumes that the power acts with a velocity that is practically infinite, that the acceleration of a body descending when its motion is in the same direction as the acting force, is exactly the same as the retardation of the same body ascending when its motion is contrary to the acting force.

To state this in its potential aspect : a body descending towards the sun at the earth's mean distance through a mile of its radial distance acquires an addition to its square velocity that amounts to 7 millionths (of a square mile), and in ascending through the same it loses the same amount. This result is held not to be influenced by the velocity with which the body moves through that mile. It may be an inch per second, or it may be 100 miles per second, the accession to the *vis viva* or square velocity at the end of the mile is the same, viz. 7 millionths.

The maximum velocity with which the earth approaches the sun, or the rate at which the radius-vector diminishes, is about one-third of a mile per second ; so that in three seconds it diminishes one mile, and in that time acquires the increment of square velocity 7 millionths (of a mile) ; *i. e.* a mechanical or work-force to this amount is transferred to each ton of its mass from some unknown source.

This is equal to the work performed by the same ton descending through 3.25 feet at earth's surface.

If this gradual appearance of *vis viva* at uniform rate in respect to radial distance passed through (but increasing as the inverse square of that distance) is a transference and not a genesis of force, the amount transferred must diminish as the velocity of the motion in the direction of the transference increases.

* Communicated by the Author.

Let U be the velocity with which the agent of gravitation acts, and w the velocity of the descending body in the direction of its action; then, if by supposing U to be doubled the force of gravitation is doubled, it is evident that U diminished to $U - w$ (which it is by the motion of the descending body) diminishes the force by $\frac{w}{U}$ part. Again, if U is supposed by doubling to quadruple the force of gravitation, U diminished to $U - w$ diminishes the force by $\frac{2w}{U} - \frac{w^2}{U^2}$. Thus upon whatever power of U the force of gravity may be supposed to depend, the decrement of force is always in the simple ratio of w to U , so long as w is small in comparison to U .

An orbital body in descending from aphelion to perihelion acquires centripetal velocity w , which attains its maximum value when radius-vector is perpendicular to axis, thence diminishes to zero at perihelion. From aphelion to perihelion the force of gravity is thus to be viewed as having a decrement, the amount of which is proportional to w , increasing with it to a maximum and diminishing to zero at perihelion. On the ascending side of the orbit the force of gravity is to be viewed as having an increment which passes through the same changes as the decrement, and has the same maximum value. Let us suppose that maximum value to be equal to 100ω . The decrement of gravitation caused by w increasing to $w + \omega$ is $\frac{\omega}{U}$. This is equal to the decrement that would be caused by the sun's mass losing a proportionate part $\frac{\delta m}{m} = \frac{\omega}{U}$. Hence the proportionate increase of semi-major axis is $\frac{\delta a}{a} = \frac{\omega}{U} \cdot \frac{2a - r}{r}$.

In the same way the increment of gravitation caused by w diminishing from w to $w - \omega$ is $\frac{\omega}{U}$, and the proportionate decrease of a is $\frac{\delta a}{a} = \frac{\omega}{U} \cdot \frac{2a - r_1}{r_1}$. This is negative and larger than the preceding, because the point in the orbit where w diminishes is nearer the perihelion than the point where the same value of w is found increasing; hence $\frac{\omega}{U} \left(\frac{2a - r_1}{r_1} - \frac{2a - r}{r} \right)$ is the value of the ordinate to w as abscissa. The area resulting from such ordinates is the integral value of the proportionate decrement of semi-major axis made good in the descending semi-revolution.

In the ascending semirevolution gravitation is augmented.

From perihelion to the maximum of w the augmentation increases from zero to $\frac{100\omega}{U}$, and from the maximum to aphelion the augmentation diminishes again to zero. Thus the integral for the ascending half is merely a repetition of the integral for the descending half.

Let c represent the orbital velocity in a circle with radius a ; and let $\tan p = \frac{\sqrt{e^2 - (a-r)^2}}{r}$; then shall $w = c \tan p$ (as shown in Appendix) $\omega = dw = c \cdot d \tan p$; and

$$\frac{\omega}{U} \left(\frac{2a-r_1}{r_1} - \frac{2a-r}{r} \right) = \frac{c \cdot d \tan p}{U} \left(\frac{2a-r_1}{r_1} - \frac{2a-r}{r} \right).$$

The relation between r_1 and r through p , which is common to both, enables this expression to be integrated, as shown in the Appendix. Twice the integral gives the proportionate decrement of major axis $\left[\frac{\delta a}{a} \right]$ made good during one revolution. From this we obtain the proportionate decrements of minor axis, excentricity, and period,—also the change in perihelion distance, which is an increment:—

$$\left[\frac{\delta a}{a} \right] = -2 \frac{c}{U} \cdot \pi \cdot \frac{ae^2}{b^3},$$

$$\left[\frac{\delta b}{b} \right] = \frac{1}{2} \left[\frac{\delta a}{a} \right],$$

$$\left[\frac{\delta e}{e} \right] = \left[\frac{\delta b}{b} \right] \left(\frac{a^2}{e^2} + 1 \right), \text{ or } \left[\frac{\delta e}{e} \right] = \left[\frac{\delta b}{b} \right] \left(\frac{a}{e} - \frac{e}{a} \right),$$

$$\left[\frac{\delta p}{p} \right] = - \left[\frac{\delta b}{b} \right] \cdot \frac{p}{e},$$

$$\left[\frac{\delta t}{t} \right] = \frac{3}{2} \left[\frac{\delta a}{a} \right].$$

On examining these expressions, it will be found that when e is small with reference to a , as in the planetary orbits, there is little change in axes or period, but the excentricity decrement is $\frac{a}{2e}$ time the decrement of a .

The following is computed for the earth's orbit, taking $U =$ one million, which is tantamount to assuming gravitation to be reduced one millionth for each mile of centripetal velocity:

$$\left[\frac{\delta a}{a} \right] = -.0000000334; \quad \left[\frac{\delta e}{e} \right] = -.000001016; \quad \delta t = -1^s.58$$

The following is computed for an ellipse with a same as our planet, but with $\frac{e}{a} = \cdot 99$, and U as above :

$$\left[\frac{\delta a}{a}\right] = -\cdot 042; \quad \left[\frac{\delta p}{p}\right] = +\cdot 00021; \quad \delta t = -23 \text{ days.}$$

This example shows that a periodic comet with small perihelion distance and large excentricity is favourable for discovering the absolute value of U . δt , computed for one revolution of Halley's comet with U as above, gives 70 days as the decrement of period.

The following is computed for Encke's comet :—

$$\left[\frac{\delta a}{a}\right] = -\cdot 00037; \quad \left[\frac{\delta e}{a}\right] = -\cdot 00038; \quad \left[\frac{\delta p}{p}\right] = +\cdot 00003;$$

$$\delta t = -0^d \cdot 66.$$

It is admitted that the period of Encke's comet is shortened half a day by some unknown cause; so if the finite value of 1·32 million miles is assigned to U , we have a sufficient cause for the decrement. On the other hand, the excentricity of the earth's orbit would diminish by this cause $\cdot 0000008$, which is double the amount of decrement assigned to it by the secular variation : also there would be a shortening of the period of Halley's comet by 53 days. These, it may be presumed, are both inadmissible.

It would be interesting if physical astronomers could assign a limit ; for it cannot now be believed that the living force, which is apparently generated and lost by means of gravitation, is absolutely created and destroyed. That would be to make gravitation an exception to the general rule of conservation. Besides, it is demonstrated that space is filled with a material instrument that is the carrier of living force outwards ; so may that or another even more subtle and dominant be the source from which descending matter acquires the *vis viva* that it carries centripetally inwards.

APPENDIX.

To prove $w = c \cdot \tan p$.

g = feet fallen through in one second at earth's surface.

$4g$ = feet square velocity acquired in falling through one foot.

$\frac{4g}{5280^2}$ = miles square velocity acquired in falling through one foot.

$\frac{4g}{5280}$ = miles square velocity acquired in falling through one mile.

$$\frac{4g}{5280} = \left\{ \frac{1}{3956} - \frac{1}{3957} \right\} G.$$

Hence

$$G = \frac{4g}{5280} \times 3956^2 = \text{dynamical constant of } \left. \begin{array}{l} \text{gravitation having reference to the earth's} \\ \text{volume and density as unity} \end{array} \right\} G. \quad (23)$$

$S = 355000$ the sun's mass (earth unity).

$$\sqrt{\frac{GS}{2a}} = c = \text{orbital velocity in circle radius } a \text{ (miles)}. \quad (26)$$

$$\frac{a}{2} \sqrt{\frac{GS}{2a}} = \text{area described per second in circle.}$$

$$\frac{b}{2} \sqrt{\frac{GS}{2a}} = \text{area described in ellipse.}$$

v = velocity, and r = radius vector at a point in elliptic orbit.

$$v = b \cdot \sqrt{\frac{GS}{2a}} \cdot \frac{1}{r \cos \frac{1}{2} \epsilon}, \text{ in which } \epsilon = \angle SEF \text{ (fig. 1)}. \quad (27)$$

$$w = v \sin \frac{1}{2} \epsilon = b \cdot \sqrt{\frac{GS}{2a}} \cdot \frac{1}{r} \cdot \tan \frac{1}{2} \epsilon \quad \dots \quad (28)$$

Because the product of the perpendiculars from the two foci of an ellipse to a tangent at any point is equal to the square of the semi-minor axis

$$\frac{(2a-r)r}{b^2} = \sec^2 \frac{1}{2} \epsilon,$$

and

$$\tan \frac{1}{2} \epsilon = \sqrt{\frac{(2a-r)r}{b^2} - 1}, \quad \dots \quad (29)$$

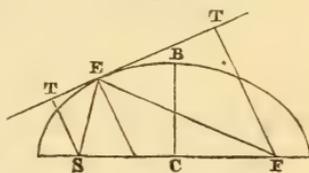
and

$$\frac{b}{r} \cdot \tan \frac{1}{2} \epsilon = \frac{\sqrt{e^2 - (a-r)^2}}{r} = \tan p.$$

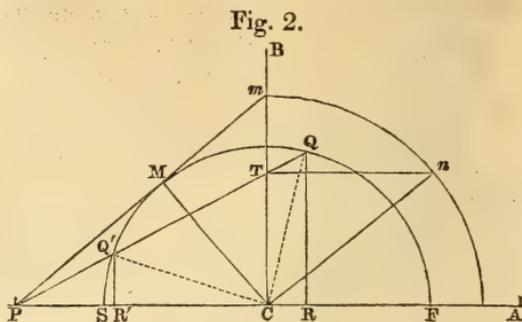
Hence

$$w = c \tan p. \quad \dots \quad (30)$$

Fig. 1.



The following construction (fig. 2) shows p and its relation to the elements of the ellipse. $PC = a = CA$, $SC = e = CM = CF$. Draw PT cutting semicircle in Q' and Q . Draw $Q'R'$, QR , \perp to PA , $\angle TPC = p$, $PR' = r_1$, $PR = r$,



$QR = \sqrt{e^2 - (a-r)^2}$ and $\frac{QR}{PR} = \tan p$; hence $r^2 \sec^2 p = 2ar - b^2$, and

$$r = a \cos^2 p \left\{ 1 + \sqrt{1 - \frac{b^2}{a^2 \cos^2 p}} \right\} = a \cos^2 p (1 + q), \quad (31)$$

$$r_1 = a \cos^2 p (1 - q). \quad (32)$$

This enables $\frac{1}{U} \cdot c d \tan p \left\{ \frac{2a-r_1}{r_1} - \frac{2a-r}{r} \right\} = \left[\frac{\delta a}{a} \right]$ to be integrated as follows. Substituting in it the values of r and r_1 given in (31), (32) and reducing, we obtain

$$d \left[\frac{\delta a}{a} \right] = \frac{c}{U} \cdot d \tan p \left\{ \frac{4q}{\cos^2 p (1-q^2)^2} \right\} = \frac{c}{U} \cdot d \tan p \cdot \frac{a^2}{b^2} \cdot 4q,$$

because $(1-q^2) = \frac{b^2}{a^2 \cos^2 p}$; and since $q = \sqrt{1 - \frac{b^2}{a^2 \cos^2 p}}$,

$$d \left[\frac{\delta a}{a} \right] = \frac{c}{U} \cdot d \tan p \cdot 4 \frac{a}{b} \sqrt{\frac{e^2}{b^2} - \tan^2 p} = \frac{c}{U} 4 \frac{a}{b} \cdot d \cos \phi \sin \phi \frac{e^2}{b^2},$$

because maximum value of $\tan p$ is $\frac{e}{b} = Cm$ (fig. 2), and

$$\tan p = \cos \phi \frac{e}{b}.$$

In fig. 2 the angle TCn is ϕ . Hence for one complete revolution,

$$\left. \begin{aligned} \left[\frac{\delta a}{a} \right] &= - \frac{c}{U} 4 \frac{a}{b} \cdot \frac{e^2}{b^2} \times \text{area of semicircle radius unity} \\ &= - \frac{c}{U} 2\pi \frac{ae^2}{b^3}. \end{aligned} \right\} \quad (33)$$

In the same way the integration of

$$d \left[\frac{\delta b}{b} \right] = \frac{c}{U} d \tan p \left\{ \frac{a}{r_1} - \frac{a}{r} \right\}$$

is effected, and thence is derived the changes in the other elements of the elliptic orbit as given above.

Inverness, September 5, 1866.

VIII. *Inquiry as to whether the tendency to Dispersion of Cloud under a Full Moon in any way depends on Lunar Influence.*

By WILLIAM ELLIS, F.R.A.S.*

IN addition to some general belief in the power of the moon to disperse cloud, the tendency to disappearance of cloud under a full moon is also spoken of by the philosophers Humboldt, Herschel, and Arago. By Herschel ('*Outlines of Astronomy*,' seventh edition, page 285) it is believed to be a consequence of lunar action, and Arago also (*Astronomie Populaire*, book xxi. chapter 33) appears to consider such influence probable. To question the existence of this power when supported by such testimony may therefore seem presumptuous; and yet I cannot conceal from myself the fact that many years' experience as an astronomical observer at the Royal Observatory at Greenwich has impressed me with the idea that at that place the evening tendency to clearance is not confined to the period of full moon, but exists to an equal extent at all times, leading to the conclusion that the phenomenon does not depend on lunar action.

But there is also evidence that the disappearance of cloud is not confined to the full-moon period. Admiral FitzRoy (*Weather Book*, page 256) says that in fine weather there is a general tendency towards a disappearance of clouds soon after evening, "whether the moon is visible or not, whether full, or near any other period." And the question is not whether the tendency exists, but whether any portion of it at full moon is due to lunar action—whether the tendency to dispersion of cloud in the evening is greater at the time of full moon than at other times, or greater than at the time of new moon. For if not greater, then the added lunar influence at the time of full moon produces no effect, or no lunar influence exists.

I had often wished to investigate this matter for myself; and it seemed to me that the observations made at the Royal Observatory, Greenwich, from the years 1841 to 1847, might be used for such a purpose. I am not aware that they have before been so employed. They were made continuously at intervals of two hours, excepting on Sundays and a few other days; and the series comprises in all $86\frac{1}{2}$ lunations. The amount of cloud at each observation was estimated, 0 being supposed to represent a sky free from cloud, and 10 one wholly cloudy.

Now in the separate Greenwich volumes the mean amount of cloud, at each two hours, is given for every month of the year. Combining together these results for the whole of the seven years, the following Table, showing the average diurnal inequality, is found. The observations were made at the even hours of Göttingen astronomical time, for which I have substituted Greenwich time.

* Communicated by the Author.

TABLE I.—Mean Amount of Cloud from all the Two-Hourly Observations made from 1841 to 1847 at the Royal Observatory Greenwich.

Greenwich Astronomical Time.	Mean Amount of Cloud.
h m	
23 20	7·14
1 20	7·11
3 20	7·00
5 20	6·61
7 20	6·23
9 20	6·04
11 20	6·14
13 20	6·41
15 20	6·76
17 20	6·84
19 20	6·89
21 20	7·10
Mean.....	6·69

It thus appears that, on the average of the whole period, the mean amount of cloud at Greenwich diminishes from 23^h 20^m to 9^h 20^m, then increasing again to 23^h 20^m, or the sky is more clear by night than by day. Now, if the moon at full has any peculiar power to disperse cloud, the mean amount of cloud at night at full moon ought to be less than the mean amount at night for the whole period, and still less than the mean amount at night at new moon. Grouping together the observations made (through the whole seven years) on the five consecutive days of each lunation having full moon occurring on the middle day, the two-hourly means of this collection of observations were found; and similarly for new moon. The resulting numbers are contained in the following Table, to which those previously found are added for comparison.

TABLE II.—Mean Amount of Cloud at Full Moon, for the whole period, and at New Moon, 1841 to 1847.

Greenwich Astronomical Time.	At Full Moon.	For the whole period.	At New Moon.
h m			
23 20	7·01	7·14	7·45
1 20	7·03	7·11	7·23
3 20	6·87	7·00	7·12
5 20	6·63	6·61	6·95
7 20	6·34	6·23	5·97
9 20	5·99	6·04	5·94
11 20	6·25	6·14	5·75
13 20	6·50	6·41	6·00
15 20	6·86	6·76	6·33
17 20	6·91	6·84	6·43
19 20	7·00	6·89	6·68
21 20	7·35	7·10	7·17
Means.....	6·73	6·69	6·59

On the average of twenty-four hours, the mean for full moon scarcely differs from that for the whole period, or from that at new moon. The slight difference that exists is against the supposed influence. And comparing the night hours only, the means are (including from 7^h 20^m to 17^h 20^m) for full moon 6.48, for the whole period 6.40, for new moon 6.07. These differences are also against the supposed influence; or the night is least clear at the time of full moon.

A further discussion of the full moon observations was made. The numbers in Table II. in the column for full moon, depend on observations made on 435 days. The observations on these days were now arranged according to the time of moonrise on each day, and the means taken. Before, however, taking the means, the observations were corrected for the general diurnal inequality. For the average of the whole year, corrections such as would be given by Table I. would be proper to use. But as the days were not quite symmetrically distributed over the twelve months of the year, and as the inequality is greater in summer than in winter, corrections were determined independently for each month. The means finally found remain affected by very nearly the whole of the full moon influence, if any exists.

TABLE III.—Mean Amount of Cloud from observations on 435 days at Full Moon.

Time as referred to moonrise.		Mean amount of cloud.
Four hours	} before moonrise ... {	6.69
Two hours		6.66
	At moonrise.....	6.73
Two hours	} after moonrise..... {	6.63
Four hours		6.87
Six hours		6.69
Eight hours		6.65
Ten hours		6.82
Twelve hours		6.67

The means of successive values taken twice give 6.69, 6.69, 6.72, 6.77, 6.73, 6.71, 6.75. The second value is that corresponding to moonrise; or, the general or solar inequality being removed, there appears to be no trace of lunar influence.

Again, if the full moon tends to disperse cloud, its amount ought to be least at the full-moon quarter. In the Greenwich volumes, the mean amount of cloud is given for every day as deduced from the two-hourly observations. Grouping together all the days of new moon with the days one day, two days, and three days before, and one day, two days, and three days after

(and similarly for first quarter, full moon, and last quarter), the following results were found:—

For the seven days having	$\left\{ \begin{array}{l} \text{New Moon} \\ \text{First Quarter} \\ \text{Full Moon} \\ \text{Last Quarter} \end{array} \right\}$	occurring on the middle day	$\left\{ \begin{array}{l} \text{the mean daily amount of cloud} \\ \text{'' '' '' ''} \\ \text{'' '' '' ''} \\ \text{'' '' '' ''} \end{array} \right\}$	= 6.61.
				6.72.
				6.76.
				6.67.

The slight difference between the numbers is against the full-moon influence, confirming a previous result.

From the observations made at Greenwich during seven years, it thus appears that the moon has not that power to disperse cloud which has been ascribed to her.

Supposing the result found for Greenwich to be also universally true, it may then be asked how arises any general belief in the moon's influence. To answer this, let us refer to Table II. It there appears that the ordinary evening change is *from a cloudy state of sky to a clear state of sky*, also that the change is not confined to any one part of the lunation, neither is it greater at full moon than at other times. Towards midnight, and after midnight, the ordinary tendency is again to cloud. Now the period of clearance corresponds, *at the time of full moon and about that time only*, to the period comprised between the rising and advance of the moon towards the meridian. There is thus, about the time of full moon, some appearance of *cause and effect*; for as the moon ascends above the horizon, so does the sky become, on the average, finer. Further, the change from a cloudy to a clear state of sky is more likely to attract attention near the time of full moon; for with no moon present the general darkness is little altered, but with the moon above the horizon, dissipation of the evening cloud so entirely changes the aspect of nature, that the contrast between the previous dulness and present splendour of the night is very striking. So that the moon may, by her presence, first attract attention to the evening tendency to clearance, and then, by her accident of position (the coincidence of her time of rising with the usual time of clearance), suggest the idea that to her influence is the effect remarked due.

Such speculation, however, assumes my own result to be true; and so far as number of observations can give certainty, it ought to be good. But the opposite conclusion of Sir J. Herschel is stated by him (see his 'Familiar Lectures on Scientific Subjects,' p. 147) to be also the result of "long-continued and registered observation." And it seems difficult to suggest any probable explanation of the discordance. My own result confirms my own previous impression, but, being opposed to that of Sir J. Herschel as well as to the belief of many people, it may be considered to require confirmation by discussion of observations made at other

places before it can be received as generally true. This seems indeed but reasonable. At present, therefore, I will only ask that my result may be looked upon as a protest against the opposite and more generally received opinion.

Greenwich, June 22, 1867.

IX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from vol. xxxiii. p. 554.]

May 9, 1867.—Lieut.-General Sabine, President, in the Chair.

THE following communication was read:—

“On a Property of Curves which fulfil the condition $\frac{d^2\phi}{dx^2} + \frac{d^2\phi}{dy^2} = 0$.”

By W. J. Macquorn Rankine, C.E., LL.D., F.R.S.S.L. & E.

1. In a paper “On Stream-Lines,” published in the Philosophical Magazine for October 1864, I stated, and, in a Supplement to the same paper, published in the Philosophical Magazine for January 1865, I proved the proposition that “all waves in which molecular rotation is null begin to break when the two slopes of the crest meet at right angles.”

2. I have now to state the purely geometrical proposition of which that mechanical proposition is a consequence. *If a plane curve which fulfils the condition $\frac{d^2\phi}{dx^2} + \frac{d^2\phi}{dy^2} = 0$ cuts itself in a double point, it does so at right angles.*

3. The following is the demonstration. It is well known that the inclination of any plane curve to the axes at an ordinary point is given by the equation

$$\frac{d\phi}{dx} dx + \frac{d\phi}{dy} dy = 0;$$

also that at a double point $\frac{d\phi}{dx}$ and $\frac{d\phi}{dy}$ both vanish, so that the inclinations of the two branches to the axes are given by the two roots of the quadratic equation

$$\frac{d^2\phi}{dx^2} \cdot dx^2 + 2 \frac{d^2\phi}{dx dy} \cdot dx dy + \frac{d^2\phi}{dy^2} \cdot dy^2 = 0;$$

whence it follows that the product of the two values of $\frac{dy}{dx}$, which are the two values of the tangent of the inclination to the axis of x ,

is $\frac{\frac{d^2\phi}{dx^2}}{\frac{d^2\phi}{dy^2}}$. In a curve which fulfils the before-mentioned condition,

the value of that product is -1 ; and when such is the case with the *Phil. Mag.* S. 4. Vol. 34. No. 227, July 1867. F

product of the tangents of two angles, the difference of those angles is a right angle; therefore the two branches cut each other at right angles. Q.E.D.

4. The proposition just demonstrated is so simple and so obvious, that I was at first disposed to think it must have been known and published previously; and had I not been assured by several eminent mathematicians that it had not been previously published to their knowledge, I should not have ventured to put it forth as new.

Supplement to the preceding Paper.

Professor Stokes, D.C.L., has pointed out to me an extension of the preceding theorem, viz. *that at every multiple point in a plane curve which fulfils the condition $\frac{d^2\phi}{dx^2} + \frac{d^2\phi}{dy^2} = 0$, the branches make equal angles with each other*; so that, for example, if n branches cut each other at a multiple point, they make with each other $2n$ equal angles of $\frac{\pi}{n}$.

The following appears to me to be the simplest demonstration of the extended theorem. At a point where n branches cut each other the following equation is fulfilled by all curves:

$$\left(dx \frac{d}{dx} + dy \frac{d}{dy}\right)^n \phi = 0.$$

Let θ be the angle made by any branch with the axis of x ; then

$$\left(\cos \theta \frac{d}{dx} + \sin \theta \frac{d}{dy}\right)^n \phi = 0.$$

But in a curve which fulfils the equation $\frac{d^2\phi}{dx^2} + \frac{d^2\phi}{dy^2} = 0$, we have

$$\frac{d}{dy} = \sqrt{-1} \cdot \frac{d}{dx};$$

whence it follows that in such a curve the equation of a multiple point of n branches is

$$\left\{ (\cos \theta + \sqrt{-1} \cdot \sin \theta) \frac{d}{dx} \right\} \phi = 0.$$

Choose for the axis of x a tangent to one of the branches at the multiple point. Then it is evident that the preceding equation is satisfied by the $2n$ values of θ corresponding to the $2n$ th roots of unity, that is to say, by

$$\theta = 0, \quad \frac{\pi}{n}, \quad \frac{2\pi}{n}, \quad \&c., \quad \dots \quad \frac{(2n-1)\pi}{n};$$

therefore the n branches make with each other $2n$ equal angles of $\frac{\pi}{n}$.

Q.E.D.

GEOLOGICAL SOCIETY.

[Continued from vol. xxxiii. p. 555.]

March 6, 1867.—Warrington W. Smyth, Esq., M.A., F.R.S.,
President, in the Chair.

The following communications were read :—

1. “ On ancient Sea-marks on the coast of Sweden.” By the
Rt. Hon. the Earl of Selkirk, F.R.S., F.G.S.

This paper contained a detailed description of some observations made in the month of July 1866, upon certain marks placed so as to show the level of the sea on the coast of Sweden, which were seen by Sir Charles Lyell thirty-two years ago, and which were supposed to indicate a gradual and equable rise of the land of about three feet in a century. Two of these marks were off the harbour of Gefle, and one on the Island of Gräsö, off Öregrund, on the east coast of Sweden; the rest were on the west coast, a little to the north of Göteborg.

The conclusion arrived at was that these marks do not afford any very certain proof of such rise of the land,—the fluctuation of the level of the water being so great that any difference of the level of the land in thirty-two years is lost in comparison with the daily and weekly changes owing to shifts of wind and other causes affecting the *water*, not the land. The marks off Gefle gave most indication of a change of level; but there were various elements of uncertainty connected with them.

2. “ On a Post-Tertiary Lignite, or Peat-Bed, in the District of Kintyre, Argyllshire.” By His Grace the Duke of Argyll, K.T., D.C.L., F.R.S., F.G.S.

A section of the Bed appeared in a bank cut through by a small stream near the village of Southend. The bank appears to belong to the “ old coast-line,” which is so well-marked a feature around most parts of the west coast of Scotland. The Peat at the point described is 3 feet 9 inches thick; above it is a bed of fine clay, from 13 to 14 inches thick, containing hazel-nuts, followed by a bed of fine yellow sand 4 feet thick, which is succeeded by a bed of coarse gravel, with small boulders of the thickness of 14 feet.

About 400 yards further up the stream there is a bed of fine black-blue clay with Mussel-shells.

These beds appear to furnish evidence of some five or six different changes of level. (1) The Peat-bed has been depressed under shallow and very muddy water, depositing the bed of fine clay. (2) A further depression has subjected this mud to an inroad of the sea, bringing with it the sand which overlies the clay. (3) A further depression, or possibly a partial elevation, exposing the same surface to some strong current or littoral action, has brought down upon it the bed of coarse gravel. (4) All these beds have been consolidated and reelevated above the sea. (5) Another depression has enabled the sea to erode the valley of which the “ old coast-line” forms the boundary, and in which this section is exposed. A long period

seems to have followed, during which this "old coast-line" formed the Coast of Scotland; and during that period the upper Mussel-bed seems to have been deposited. (6) A final elevation of the land has determined the present coast-line, and left the old one as it now appears—subsequently modified by atmospheric action, and cut through by streams. All these changes have occurred during what, geologically, must be called the existing period, as the vegetable remains in the peat, and in the clay, seem to be all referable to existing species.

March 20.—Warrington W. Smyth, Esq., M.A., F.R.S.,
President, in the Chair.

The following communications were read:—

1. "Report on recent discoveries of Gold in New Brunswick." By W. S. Shea, Esq.

Mr. Shea gave in his report a detailed account of his explorations into the gold-bearing gravels of certain river-valleys in the counties Victoria, Northumberland, Carleton, and York, in Central New Brunswick. He had been enabled therefrom to draw the following inferences:—(1) that the gold in these alluvial deposits is derived from the quartz-veins penetrating the rock of the district; (2) that the gravel, which contains pebbles of all sizes, was derived from the disintegration of the rocks of the district; and (3) that, judging from the richness in gold of paying drift in California, it is probable that these auriferous gravels will pay also.

2. "On the discovery of Coal on the Eastern slope of the Andes." By W. Wheelwright, Esq.

In this paper the author reported the occurrence of beds of Coal on the eastern slope of the Andes, between the cities of Cordova and San Juan, about 25 leagues east of the latter city.

3. "On the presence of Purbeck Beds at Brill, Buckinghamshire." By the Rev. P. B. Brodie, M.A., F.G.S.

The existence of the Purbeck-beds at Brill not having yet been accurately determined, the author recorded their occurrence in that locality, which he had been able to ascertain by finding blocks containing several species of Purbeck freshwater shells, especially *Paludina subangulata* and *Cyrena media*.

4. "On the Lower Lias, or Lias-conglomerate of Glamorganshire." By H. W. Bristow, Esq., F.R.S., F.G.S., of the Geological Survey of Great Britain.

The object of this communication was to show that the more or less conglomeratic strata immediately underlying the ordinary Lias limestone on the coast of Glamorganshire, between the River Ogmore and Dunraven Point, do not belong to the Rhætic series, as has been supposed, but to the Lias.

The Sutton-stone of Sutton and the so-called Southerndown series (seen in the cliffs under the hamlet of that name) constitute, in reality, but one series, the stratigraphical position of which is

proved by reference to the sections at Cwrt, near Langan, at St. Mary Hill Common, west of Tyrmynydd, and at the Stormy Cement-works, where it immediately overlies the Rhætic series; while its palæontological affinity with the Lias is proved by the occurrence throughout its entire thickness of the most characteristic shells of that formation, viz. *Gryphæa incurva*, *Ostrea liassica*, *Lima gigantea*, and *Ammonites*.

These conglomeratic strata extend over a large area from Sutton to beyond Cowbridge; and wherever the Lias rests upon Carboniferous Limestone or Magnesian Conglomerate, it assumes an abnormal lithological character, and becomes more or less conglomeratic. The replacement, in this district, of the ordinary calcareous and argillaceous sediments of which the Rhætic and Liassic series are usually composed, in the former case by sandstones, and in the latter by conglomerates, indicates that they are the near-shore and shallow-water deposits of an area that now forms part of South Wales, and which was partially undergoing slow depression during the deposition of the Lias.

After noticing the occurrence of lead-ore (Galena) in these beds, the term Lias Conglomerate, originally applied to them by Sir Henry De la Beche, was proposed as being sufficiently precise and distinctive, the name *Infra-lias*, by which they are sometimes denoted, being objected to as being both vague and misleading.

5. "On Abnormal conditions of Secondary Deposits when connected with the Somersetshire and South Wales Coal-basins; and on the age of the Sutton and Southerndown series." By Charles Moore, Esq., F.G.S.

The author first described the geological constitution of the Mendip Hills, which, in his opinion, were upheaved by the intrusion of a basaltic dyke (now noticed for the first time) during the period of the Upper Trias. The Mendip chain proved an island barrier to the incursion of the deeper sea-deposits of the south, on which lived the *Microlestes* and other terrestrial animals. Along the south side of this barrier shore-deposits were formed, the "Carboniferous Limestone" constituting the floor of the ocean at that time.

He then instituted a comparison between the Rhætic and Liassic formations within and those without the Somersetshire coal-basin. The thickness of the beds, from the Trias to the Inferior Oolite, was stated to be, outside the coal-field, 3320 feet, whilst inside it was only 169 feet. These results were obtained from an examination of numerous sections, which were described in detail by the author.

After considering the horizontal deposits beyond the Mendips, and the unconformable conditions within the coal-basin, the author discussed the abnormal conditions which are presented by deposits of the same age when they are intimately connected with the "Carboniferous Limestone." In the Charterhouse Lead-mine a deposit of clay 12 feet in thickness, and containing Liassic shells, was stated to occur at a depth of 260 feet in the Carboniferous Limestone.

Among the organic remains, three species of terrestrial shells,

referable to the genera *Helix*, *Vertigo*, and *Proserpina*, and a *Chara*-seed, were discovered.

The author concluded by pointing out the peculiarities presented by the Liassic strata in Glamorganshire, with special reference to the stratigraphical position of the Sutton Stone and the Conglomerates of Brocastle &c.

April 3, 1867.—Warrington W. Smyth, Esq., M.A., F.R.S.,
President, in the Chair.

The following communications were read:—

1. “Remarks on the Drift in a part of Warwickshire, and on the evidence of glacial action which it affords.” By the Rev. P. B. Brodie, M.A., F.G.S.

The later Drift-deposits in the district treated of occur along the valley of the Avon, and consist of the usual sands and gravels, with Mammalian remains; but as yet no Flint Implements have been discovered in them. The author enumerated the different kinds of rock represented by pebbles or fossils in the gravel, and referred to the abundance of flints and the occasional occurrence of pieces of chalk in the gravel as proof that their mode of conveyance was by icebergs, unless it be conceded that the Cretaceous formation at one time had a much further extension northwards. The abundant quartzose pebbles occurring in the drift of Warwickshire have recently yielded fossils identical with those occurring in the pebbles at Budleigh Salterton; and the author suggests that they had a similar origin to those in Devonshire.

2. “On the dentition of *Rhinoceros leptorhinus*” (Owen). By W. Boyd Dawkins, Esq., M.A. (Oxon.), F.G.S.

The Pleistocene species of Rhinoceros in Britain are four in number:—*R. tichorhinus*, Cuv.; *R. megarhinus*, Christol; *R. Etruscus*, Falc.; and *R. leptorhinus*, Owen [= *R. hemitechus* of Falconer]. The last of these is characterized by the possession of a *partially ossified* septum between the nares, and by the slenderness of its bones. In common with the other three, it was bicorn. Its upper molar series, as compared with the megarhine, is characterized by the following points:—by the rugosity of the enamel surface; by the development of a *third costa* on the *posterior area* of Pm. 3, 4; by the concavity of the base of the *external lamina*; and by the more vertical direction of the inner side of the *colles*. The absence of the *anterior combing-plate* and the stoutness of the guard are among the points that separate it from the tichorhine molars. The species does not seem to have existed in Britain before the great glacial epoch, the remains from the Forest-bed attributed to it by Professor Owen, viewed by the light of other specimens, turning out to belong to *R. Etruscus*. It is associated with the tichorhine species in Wookey Hole Hyæna-den, with that and the megarhine in the Lower Brick-earths of Crayford, in Kent. In a word, there is ample evidence to prove that it was coeval with the Mammoth and tichorhine Rhinoceros, that it ranged from Yorkshire, through the eastern counties, into South Wales, and the south-west of

England, and that it was very inferior to those animals in point of number. Its nearest living analogue is the bicorn Rhinoceros of Sumatra. The dentition both of the tichorhine and leptorhine species agrees remarkably in one point, that it is more specialized, or, in other words, more closely allied to that of living forms than the megarhine,—a fact that seems to the author to imply that both came into being *after* the less specialized *R. megarhinus* had ceased to exist.

3. "On the strata which form the base of the Lincolnshire Wolds." By John W. Judd, Esq., F.G.S.

After giving a sketch of the previous very scanty literature of the subject, the author proceeded to describe the outcrop and the various outliers and inliers of the "Hunstanton Red Limestone," which in this district serves as a well-marked datum line in the series of strata. It was shown that this bed, while maintaining much uniformity of lithological and palæontological characters, undergoes a regular attenuation southwards, being 30 ft. thick at Speeton, 14 ft. and upwards in Lincolnshire, and 4 ft. at Hunstanton, thinning out entirely about 12 miles south of the last-mentioned place.

In the second part of the paper a general sketch of the Chalk formation in Lincolnshire was followed by detailed descriptions of a number of red beds, previously confounded with the Hunstanton Limestone, but now shown to be intercalated in the series of the Lower Chalk. A typical section made at Louth was then compared with the grand natural section at Speeton Cliffs, as described by the Rev. T. Wiltshire.

The extensive deposits underlying the Hunstanton limestone were described as follows:—1st, ferruginous sands (unfossiliferous); 2nd, a series of limestones, sandstones, and clays, containing a large and interesting suite of fossils with an undoubted Neocomian facies, but presenting greater affinities to the faunas of certain continental deposits than to that of the English Lower Greensand; for this formation the provisional name of "the Tealby Series" was proposed; 3rd, another and thicker series of sands. In its northward development, the Tealby series was described as furnishing beds of ironstone (often of Oolitic structure and of considerable economic value), and finally as graduating into the upper part of the Speeton clay; while in tracing it southwards it is found to become almost wholly arenaceous.

The author gave lists of the fossils of the different beds, and described the numerous faults &c. of the district, which he illustrated by a map and numerous sections. He concluded with some remarks on the age of the various beds, and on the causes of the remarkable red colour of some of them.

An appendix, containing remarks on some of the fossils, showed that the following well-known species of Sowerby, *Ammonites plicomphalus*, *Pecten cinctus*, and *Lucina crassa*, are Neocomian, and not Jurassic forms as has hitherto been supposed.

X. *Intelligence and Miscellaneous Articles.*

RESEARCHES ON THE ABSORBING-ACTION WHICH CERTAIN VOLATILE LIQUIDS AND THEIR VAPOURS EXERT ON THE HEAT FROM A LAMP WITH A GLASS CHIMNEY. BY M. P. DESAINS.

I ASK leave from the Academy to communicate the results of a series of experiments which I have undertaken with the view of studying the comparative actions which a very volatile liquid and its vapour exert under comparable conditions on the same calorific radiation.

I worked with ordinary ether, formic ether, and bisulphide of carbon. The source of heat was a lamp with a glass chimney. The first two of these substances exercise a considerable absorption on the radiation from the lamp; and, comparing the manner in which they act under the two physical conditions, I found that a column of a given weight and section of each of these substances exercises the same radiation and absorption, the intensity of which is independent of the physical condition of the medium. The liquid column is very short, and that of the vapour very long, but they produce the same effect.

To prove this fact, I took (1) a copper tube blackened inside, about a metre long and a decimetre in diameter; it is closed by glass plates, and provided with stopcocks and with a jacket by means of which water can be kept at a given temperature; (2) a trough closed by clear glass, having the same section as the tube, and such a thickness that the quantity of liquid which can fill it is not enough to produce as much vapour as would saturate the tube at the temperature at which it is to be worked. The tube and the trough being ready, are placed behind each other, their axes being in the same right line. The tube is heated to a suitable temperature, 38° for ordinary and 57° for formic ether; the air is expelled and a small quantity of vapour is formed, so as to have an internal pressure of a few centimetres; and then the source of heat is allowed to radiate through the system of the tube, the empty trough, and the lenses, whose object is to increase the thermoscopic effect. The thermoscopic action is measured. Let D be this value after making all corrections. The experiment is recommenced after filling the tube with ether. A considerably smaller action is observed, which I call D' . The difference arises from the absorption which the liquid ether exerts on the radiation. That being done, the ether of the trough is brought into the tube and the trough replaced in its original position. When vaporization has taken place, it is ascertained whether the glasses have remained perfectly bright. Care is also taken that the pressure in the tube is less than that of saturation; and the thermoscopic action is measured afresh.

It is found that it is virtually equal to D' , which establishes the truth of the proposition announced.

The following are a few numbers.

Ordinary Ether.

The tube contains the vapour of ether at a pressure of 0.07 metre.

Radiation through empty trough	28	Absolute loss	9.9
„ trough full of ether	18.1	Relative loss $\frac{9.9}{28}$	= 0.35

Ordinary Ether (vapour).

Trough empty.

Radiation through tube containing vapour under a pressure of 0.06 metre	} 28.6	Absolute loss	10.2
		Relative loss	0.356
Radiation through tube containing in addition the ether of the trough vaporized	} 18.4		

Formic Ether.

The tube contains the vapour of ether under a pressure of 0.08 metre.

Radiation through empty trough	27.6	Absolute loss	7.3
„ full trough	20.3	Relative loss $\frac{7.3}{27.6}$	= 0.27

Formic Ether (vapour).

Trough empty.

Radiation through tube containing vapour at a pressure of 0.075 metre	} 29	Absolute loss	8.5
		Relative loss	0.29
Radiation through tube containing in addition the ether of the trough vaporized	} 20.5		

In another series of experiments on formic ether, the relative loss due to absorption of the liquid was 0.28, and that produced by vapour 0.27.

Professor Tyndall, in his researches on the absorption of heat by gaseous substances, had dwelt upon the extreme energy which ether vapour exerts upon obscure radiation. My experiments show that the absorption exercised by this body on the rays of a lamp is also very strong.

A natural consequence of the preceding was that the heterogeneous radiation which I used, when it traversed a tube full of ether under a pressure near that of the atmosphere, was more transmissible through liquid ether than it was when it had only passed through vapour under a few centimetres pressure. In fact I have seen the absorbing-action of ether almost double in consequence of a diminution of about 0.58 metre in the pressure of the vapour of ether in the tube.

Finally, by working under different pressures, but always with the heterogeneous radiation to which the bad condition of the atmosphere has for some time condemned me, I have been able to ascertain that ether vapour follows the law long known as that of the decrease of relative losses. By increasing the pressure from 10 to 100

centimetres, for example, I have been able to ascertain that the magnitude of the absorption due to the first 10 centimetres was greater than that of the ten following, and so on. I do not dwell upon this point, for I was not able to work with homogeneous rays.

I have not spoken in this note of the experiments I have made with bisulphide of carbon. Its action, whether liquid or gaseous, was so small that I could not deduce anything definite from its study as regards the principal fact to which I call the attention of the Academy.—*Comptes Rendus*, May 27, 1867.

ON THE SPECTRUM OF MARS, WITH SOME REMARKS ON THE
COLOUR OF THAT PLANET. BY WILLIAM HUGGINS, F.R.S.

On several occasions during the late opposition of Mars, I made observations of the spectrum of the solar light reflected from that planet.

The spectroscope which I employed was the same as that of which a description has appeared in my former papers*. Two instruments were used, one of which is furnished with a single prism of dense glass, which has a refracting angle of 60°. The other instrument has two similar prisms.

In a paper "On the Spectra of some of the Fixed Stars," by myself and Dr. W. A. Miller, we state that on one occasion several strong lines of absorption were seen in the more refrangible parts of the spectrum of Mars.

During the recent more favourable opportunities of viewing Mars, I again saw groups of lines in the blue and indigo parts of the spectrum. However, the faintness of this portion of the spectrum, when the slit was made sufficiently narrow for the distinct observation of the lines of Fraunhofer, did not permit me to measure with accuracy the position of the lines which I saw. For this reason I was unable to determine whether these lines are those which occur in this part of the solar spectrum, or whether any of them are new lines due to an absorption which the light suffers by reflexion from the planet.

I have confirmed our former observation that several strong lines exist in the red portion of the spectrum. Fraunhofer's C was distinctly seen, and its identity determined by satisfactory measures with the micrometer of the spectrum-apparatus. From this line the spectrum, as far as it can be traced towards the less refrangible end, is crossed by dark lines. One strong line was satisfactorily determined by the micrometer to be situated near C, at about one-fourth of the distance from C to B. As a similar line is not found in this position in the solar spectrum, the line in the spectrum of Mars may be accepted as an indication of absorption by the planet,

* "On the Spectra of some of the Fixed Stars," *Phil. Trans.* 1864, p. 415. During my prismatic researches I have tried, and used occasionally, several other arrangements for applying the prism to the telescope. Some of these instruments are fitted with compound prisms, which give direct vision. I have not found any apparatus equal in delicacy and in accuracy to that which is referred to in the text.

and probably by the atmosphere which surrounds it. The other lines in the red may be identical, at least in part, with B and *a* and the adjacent lines of the solar spectrum.

On February 14, faint lines were seen on both sides of Fraunhofer's D. The lines on the more refrangible side of D were stronger than the less refrangible lines. These lines occupy positions in the spectrum apparently coincident with groups of lines which make their appearance when the sun's light traverses the lower strata of the atmosphere, and which are therefore supposed to be produced by the absorption of gases or vapours existing in our atmosphere. The lines in the spectrum of Mars probably indicate the existence of similar matter in the planet's atmosphere. I suspected that these lines were most distinct in the light from the margin of the planet's disk; but this observation was to some extent uncertain. That these lines were not produced by the portion of the earth's atmosphere through which the light of Mars had passed, was shown by the absence of similar lines in the spectrum of the moon, which at the time of observation had a smaller altitude than Mars.

I observed also the spectra of the darker portions of the planet's disk. The spectrum of the dark zone beneath the Southern Polar spot appeared as a dusky band when compared with the spectra of the adjoining brighter parts of the planet. This fainter spectrum appeared to possess a uniform depth of shade throughout its length. This observation would indicate that the material which forms the darker parts of the planet's surface absorbs all the rays of the spectrum equally. These portions, therefore, should be neutral, or nearly so, in colour.

I do not now regard the ruddy colour of Mars as due to an *elective* absorption—that is, an absorption of certain rays only, so as to produce dark lines in the spectrum.

Further, it does not appear to be probable that the ruddy tint which distinguishes Mars has its origin in the planet's atmosphere; for the light reflected from the polar regions is free from colour, though this light has traversed a longer column of atmosphere than the light from the central parts of the disk. It is in the central parts of the disk that the colour is most marked. If indeed the colour be produced by the planet's atmosphere, it must be referred to peculiar conditions of it which exist only in connexion with particular portions of the planetary surface. The evidence we possess at present appears to support the opinion that the planet's distinctive colour has its origin in the material of which some parts of its surface are composed. Mr. Lockyer's observation that the colour is most intense when the planet's atmosphere is free from clouds, obviously admits of an interpretation in accordance with this view.

This opinion appears to receive support from the photometric observations of Seidel and Zöllner, some of the results of which I will briefly state.

These observations show that Mars resembles the moon in the anomalous amount of variation of the light reflected from it as it increases and decreases in phase—also in the greater brilliancy of

the marginal portions of its disk. Further, Zöllner has found that the albedo of Mars (that is, the mean reflective power of the different parts of its disk) is not more than about one-half greater than that of the lunar surface. Now these optical characters are in accordance with the telescopic observation that in the case of Mars the light is reflected almost entirely from the true surface of the planet. Jupiter and Saturn, the light from which has evidently come from an envelope of clouds, are, on the contrary, less bright at the margin than at the central part of the disk. These planets have an albedo, severally, about four or three times as great as that of the moon*.

The anomalous degradation in brightness of the moon at the phases on either side of the full, as well as the greater brilliancy of the limb, may be accounted for by the supposition of inequalities on its surface, and also by a partly regular reflective property of its superficial rocks. Zöllner has shown that if these phenomena be assumed empirically to be due to inequalities, then the angle of mean elevation of these inequalities must be taken as 52° . On the same hypothesis the more rapid changes of Mars would require an angle of 76° †.

It appears to be highly probable that the conditions of surface which give rise to these phenomena are common to the moon and to Mars. The considerations referred to in a former paragraph suggest that these superficial conditions represent peculiarities which exist at the true surface of the planet. In this connexion it is of importance to remark that the darker parts of the disk of Mars gradually disappear, and the coloured portions lose their distinctive ruddy tint, as they approach the limb.

The observations of Sir John Herschel‡ and Professor G. Bond§ give to the moon's surface a mean reflective power similar to that from a "grey weathered sandstone" rock. Zöllner has confirmed this statement. According to him,

The albedo of the Moon	=	·1736	of the incident light.
" " Mars	=	·2672	" "
" " Jupiter	=	·6238	" "
" " Saturn	=	·4981	" "
" " White paper	=	·700	" "
" " White sandstone	=	·237	" "

From this Table it appears that Mars takes in for its own use ·7328 of the energy which it receives as light. Jupiter's cloudy atmosphere, nearly as brilliant as white paper, rejects more than six-tenths of the light which falls upon it. Therefore less than four-tenths

* *Photometrische Untersuchungen*, von Dr. J. C. Zöllner. Leipzig, 1865.

† *Ibid.* pp. 113, 128.

‡ *Outlines of Astronomy*, p. 272.

§ "On the Light of the Moon and Jupiter," *Memoirs of the American Academy*, vol. viii. p. 222. In the same memoir Professor G. Bond estimates the albedo of Jupiter to be greater than unity. This estimate would require the admission that Jupiter shines in part by native light.—*Ibid.* p. 284.

of the light which this distant planet receives is alone available for the purposes of its economy.

The photographic researches of Mr. De la Rue and others show that the rays of high refrangibility, which are specially powerful in producing chemical action, are similarly affected*. At present we know nothing of the reflective power of the planets for those rays of slower vibration which we call heat.—*Monthly Notices of the Royal Astronomical Society*, March 8, 1867.

ON A NEW METHOD OF DETERMINING THE RESISTANCE OF GALVANIC CIRCUITS. BY PROF. V. WALTENHOFER.

The methods of determining the electromotive force and the resistance of metallic circuits afford a very high degree of certainty and accuracy, while the present methods of measuring the resistance of galvanic elements are very faulty. Even under the most favourable circumstances they are destitute of accuracy, and in many cases are quite useless. The latter is more especially the case with inconstant elements, in regard to which Poggendorff has shown that the use of Ohm's method yields greater values for the internal resistance the greater the external resistances; so that the entire method in such cases becomes quite deceptive.

It has been hitherto assumed that this peculiar deportment of inconstant elements arises from polarization, inasmuch as from one experiment to the next it acts unequally in opposition to the electromotive force of the element. But the author has calculated that this assumption is inadequate for explaining the said phenomenon, since polarization, so far as its dependence on the intensity of the current is understood, must behave quite differently when the resistance of the circuit is constant. The author concludes herefrom that the changes of resistance in question which are observed in the case of Ohm's method are not simply apparent, produced by polarization, but must be occasioned by an actual dependence of the resistance of the circuit on the intensity of the current,—which then is easily explained, taking into account the manifest dependence of the so-called resistances of passage (*Uebergangswiderstande*) on the intensity of the current.

If this assumption be correct, there must be circuits whose internal resistance is less with increasing external resistances (that is, with decreasing intensity), as well as those in which the opposite is the case, according as the resistances of passage, conditioned by the chemical nature, and arrangement of the materials of the element, occasion one deportment or the other.

Without entering here more minutely upon this point, it may be sufficient to remark that experimental investigations on these and other important points in the theory of the circuit presuppose the possibility of measuring the resistances of galvanic circuits quite independent of the influence of polarization,—which of course is only possible with very small intensities. But if these very small inten-

* Professor G. Bond states that "the moon, if the constitution of its surface resembled that of Jupiter, would photograph in *one fourteenth* of the time it actually requires."—*Ibid.* p. 223.

sities be produced by the application of correspondingly larger external resistances, and thus Ohm's method be used for ascertaining the comparatively very small internal resistances, a simple calculation shows that unavoidable errors of observation by no means permit the desired certainty of results.

The design of rendering possible such investigations has led the author to find a method for determining the resistances of circuits which corresponds to the requirements mentioned—a method, that is, which permits the use of very small intensities without the use of very great inter-polar resistances. It depends on the following principle.

If the element under investigation be joined in the inverse direction with another of greater electromotive force, and an extra circuit be introduced, we have three paths for the current between two nodal points, as in Poggendorff's compensation method. If the resistances in the paths of the stronger circuit, the weaker one, and the lateral circuit be respectively denoted by α , β , and γ , and the corresponding intensities by A, B, and C, and assuming that with any values of α , β , and γ , where B will in general be different from *nil*, by a small variation in α a corresponding alteration of the intensities be effected, we arrive by direct deductions from the principles of Ohm's law at the equation

$$\beta dB = \gamma dC,$$

or, if the currents running parallel with A be denoted as positive, and thus C as negative, at the equation

$$\beta dB = -\gamma dC.$$

Denoting by C_0 the value which C has when $B=0$, integration leads to the relation

$$\beta B = \gamma(C_0 - C).$$

If by compensation of the circuit investigated B has been made $=0$, and thus $C=C_0$, and thereupon by a very small alteration of α the equilibrium has been restored, $C_0 - C$ and B give the changes in the current in the circuits γ and β , and the above relation expresses in the form

$$\beta = \gamma \frac{C_0 - C}{B}$$

the principle that the quotient of the alterations in the current observed after removing compensation in γ and β , multiplied by the resistance of the extra circuit, gives directly the resistance β and the desired resistance of the circuit.

This method, then, is distinguished from all previous ones, and especially from Ohm's method, inasmuch as it enables the resistance of the circuit to be investigated in the vicinity of its point of compensation, and permits the use of extremely small intensities without using great resistances; it thus answers the requirement of investigating the resistance of a circuit as independent as possible of the disturbing influence of polarization—that is, under circumstances in which polarization is reduced to a minimum.

For measuring B, a multiplier graduated by Poggendorff's method may be used; for measuring $C_0 - C$, a Gaugain's tangent compass. The accurately measured γ of the extra circuit remains unchanged, while α is measured by a rheocord.—*Wiener Berichte*, vol. xiv. (1867).

RESEARCHES ON THE VARIATION IN THE DISPERSION OF LIQUIDS UNDER THE INFLUENCE OF HEAT. BY M. J.-B. BAILLE.

In a previous research I had investigated the influence of heat on the refraction and dispersion of solids whose expansion is regular. The great distinction which this research received from the Academy led me to extend these investigations to liquids. Several researches had already been made on this subject, among others those of Messrs. Dale and Gladstone (1860) and M. Fouqué (1867); but these physicists had more especially in view the refraction of liquids; and as in my previous investigations I had arrived at the conclusion that the dispersion of solids increases with heat, I wished to ascertain if the dispersion of liquids was also subjected to some special law.

The apparatus and the method I used have already been described. Since, from the nature of the case, it was necessary to work rapidly, the approximation is not so close as in my former experiments. I have used the indices of the three Fraunhofer's lines C, D, and F, and have obtained a large number of results, among which I will cite the following:—

Distilled Water.

It was first ascertained that the refractive index diminishes progressively from 0 to 100 degrees, and that the variation of this index remains very feeble up to 6 degrees, when it commences to increase with the temperature. The vicinity of the point 4 degrees, corresponding to the maximum density, did not appear to influence the refractive index, as has been found by M. Jamin and Messrs. Dale and Gladstone.

	C_n .	D_n .	F_n .	$F_n - C_n$. Units of the fifth place.
0°	1.33251	1.33482	1.33897	646
2.00	1.33248	1.33479	1.33894	646
4.50	1.33247	1.33479	1.33894	647
5.25	1.33243	1.33475	1.33890	647
8.00	1.33231	1.33461	1.33874	643
15.25	1.33165	1.33392	1.33799	634
Omitting the intermediate determinations.				
100.00	1.31799	1.31943	1.32284	485

It is seen from this Table that the dispersive power measured by the difference between the extreme indices is constant, or even slightly increases, from 2 to 5 degrees, and that from this point it diminishes regularly and pretty rapidly.

Though these numbers differ somewhat from those of Messrs. Dale and Gladstone, this constancy of dispersive power up to 5 degrees is undoubted; for it is found in their numbers—a coincidence the more remarkable that they have not noticed this feature.

Sulphuret of Carbon.

	C_n .	D_n .	F_n .	$D_n - C_n$. In units of the fourth order	$F_n - C_n$. In units of the fourth order.
14°	1.6213	1.6309	1.6556	96	247
25°	1.6156	1.6248	1.6492	92	244

It is thus seen that under the influence of heat the index of sulphuret of carbon considerably diminishes. The dispersive power

also diminishes, but diminishes in an irregular manner. It is in fact seen that the green of the spectrum between F and D diminishes less than the red part between D and C, even in absolute value and though it is more spread out: the green contracts less, then, than the red.

This fact is again met with in Dale and Gladstone's numbers, although they are different from mine. With their numbers it is seen that between A and D the dispersive power diminishes continuously, and between D and H this power seems to remain constant, or to diminish very little towards 30 degrees. It is moreover seen that from former experiments Mr. Barlow had found that the dispersion of bisulphide of carbon increased with the temperature (1829).

Bisulphide of Carbon saturated with Sulphur.

	C_n	D_n	F_n	$D_n - C_n$	$F_n - D_n$
14.	1.6809	1.6917	1.7202	108	285
24.	1.6733	1.6835	1.7118	102	283

The introduction of sulphur into bisulphide has the effect of increasing the refractive index and the dispersive power. Under the influence of heat these elements diminish, and the irregularity of the variation of the dispersive element is even more sensible than in the preceding substance.

With bisulphide almost saturated with phosphorus it seems that while the red part contracts, the green part of the spectrum expands. $F_n - D_n$ varies from 298 to 309, from 15 to 25 degrees of temperature.

From the whole of these results presented by these three liquids, it may be concluded that the dispersion of bisulphide of carbon varies irregularly under the action of heat, and that the green part contracts much less than the red part; if the bisulphide be rendered more refracting and dispersing by solution of sulphur or of phosphorus, this irregularity will be more and more pronounced, so that we might arrive at a liquid in which the two parts of the spectrum would vary in contrary directions.

Glycerine.

	C_n	D_n	F_n	$D_n - C_n$	$F_n - D_n$
8.	1.46591	1.46796	1.47368	205	572
99.	1.44246	1.44454	1.44976	208	522

This substance seems to differ from the preceding substance. Here the dispersive power still varies in an irregular manner; but the red part remains equally dispersed (the preceding numbers only giving a slight augmentation), and the green part contracts very appreciably.

I have also worked with many other liquids, and have ascertained that they may always be ranged in one of the preceding groups. Each liquid seems to behave in a special manner. In the greatest number of cases, such as aqueous solutions, alcohol, &c. the dispersion diminishes regularly, and the two coloured parts mentioned here contract almost equally; the dispersion of others, such as bisulphide of carbon, anhydrous chlorides, varies irregularly, and the green part of the spectrum contracts much less than the red one; in others, finally, like glycerine and hydrochloric acid, the dispersion still varies irregularly, but the red part contracts less than the green one.—*Comptes Rendus*, May 20.

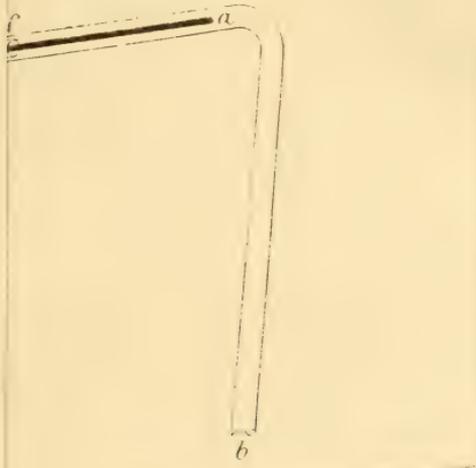


Fig. 4.

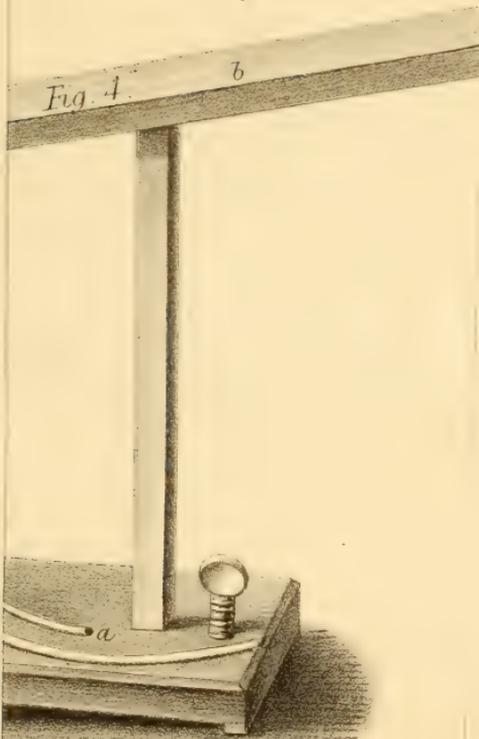
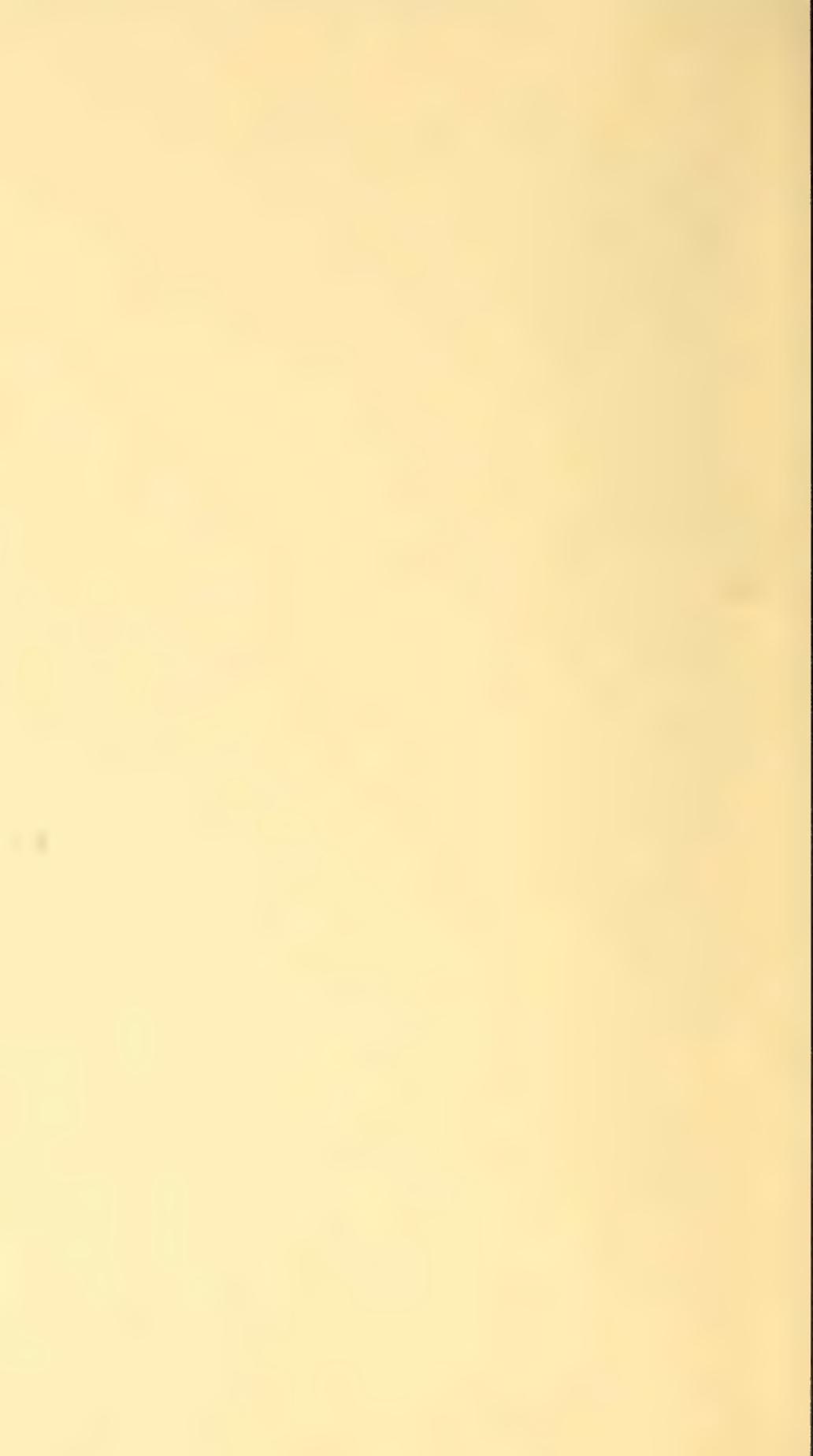
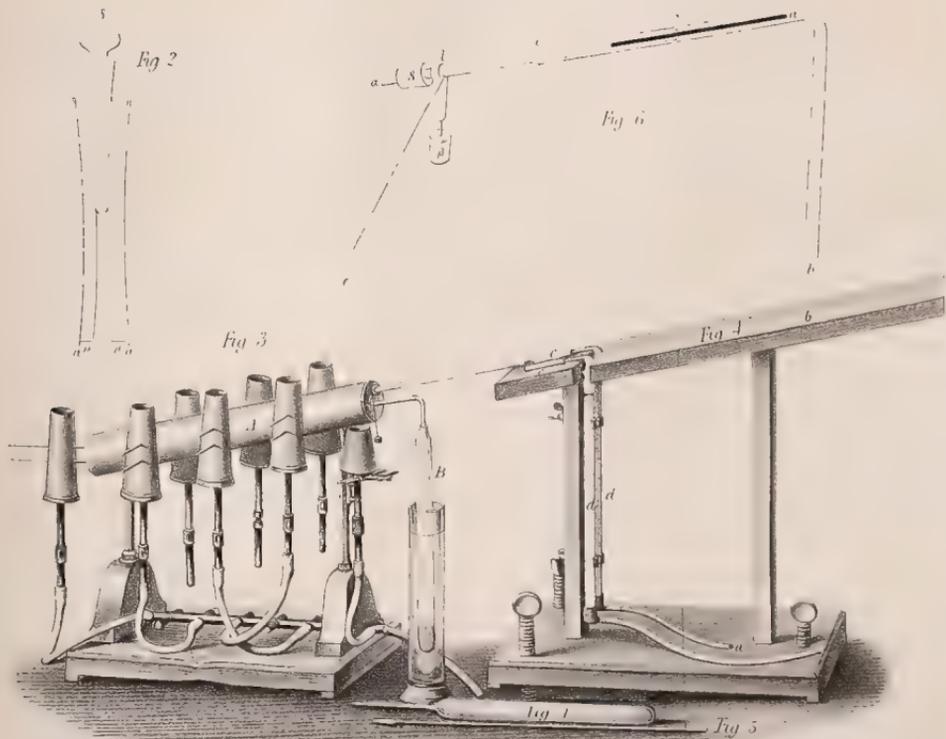
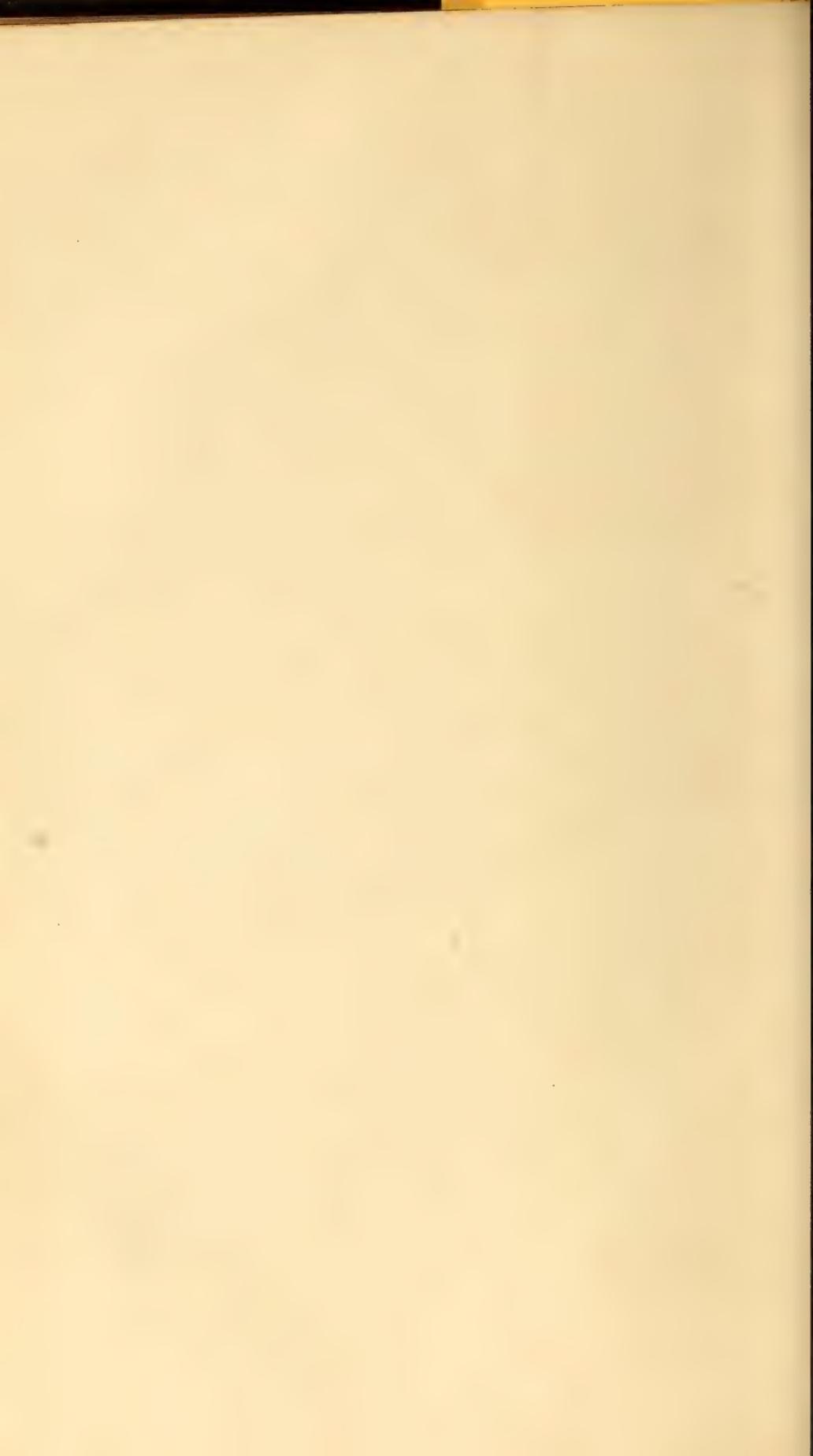


Fig. 5.







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[FOURTH SERIES.]

AUGUST 1867.

XI. *Experimental Researches in Magnetism and Electricity.*
By H. WILDE, Esq.*

[With a Plate.]

§ 1. *On some new and paradoxical Phenomena in Electro-magnetic Induction, and their relation to the Principle of the Conservation of Physical Force.*

1. **T**HE principle of the conservation of force, as I apprehend it, is the definite quantitative relation existing between all the phenomena of the universe whatsoever, both in direction and amount, whether such phenomena be considered in the relation of cause and effect, or as antecedent and consequent events.

2. In the particular application of this principle to the advancement of physical science, and also to the invention of new engines and machinery to meet the progressive requirements of society, problems not unfrequently present themselves which involve the consideration of static and dynamic force from several different aspects; and the solution of these problems often brings out results which are as surprising as they are paradoxical. Of such cases, in which the idea of paradox alluded to is involved, may be mentioned the one contained in the 36th Proposition of Newton's *Principia* (Book 2, Cor. 2)†, and in D. Bernoulli's *Hydrodynamica*, p. 279; in which the repulsive force of a jet of water issuing from a hole in the

* From the Philosophical Transactions for 1867, Part I., having been read April 26, 1866.

† *Principia*, 2nd Edition.

bottom or side of a vessel with a velocity which a body would acquire in falling freely from the surface, is equal to the weight of a column of water of which the base is equal to the section of the contracted vein, and about twice the height of the column which produces the flowing pressure,—the *static force* of reaction being thus double that which, without experiment, had been predicted*. An instance in which the quantity of *dynamic force* is increased paradoxically may be seen in that curious and useful piece of apparatus the injector, by means of which a boiler containing steam of high pressure is able to feed itself with water through a hole in its shell, though this hole is open to the atmosphere, or the steam from a low-pressure boiler is enabled to drive the feed-water through a hole (also open to the atmosphere) into a high-pressure boiler. Although, when rightly interpreted, these examples of paradox, as well as many others of a similar character, are in strict accordance with the principle of conservation, yet they are at the same time contrary to the inferences which are generally drawn from analogical reasonings, and to some of those maxims of science which are framed for the instruction of the unlearned. As the examples cited are only adduced for the purpose of illustrating some analogous phenomena observed in connexion with certain combinations of static and dynamic force in molecular mechanics which form the subject of the present research, it is not my intention to enter into the rationale of either of them, but to direct attention to some new and paradoxical phenomena arising out of Faraday's important discovery of magneto-electric induction, the close consideration of which has resulted in the discovery of a means of producing dynamic electricity in quantities unattainable by any apparatus hitherto constructed.

3. If round a piece of iron forming the armature of a permanent magnet a quantity of insulated wire be wound at right angles to the line which joins the poles of the magnet, and if the free ends of the wire be connected together directly, or indirectly by the interposition of some conductor, a momentary wave of electricity, as is well known, is generated in the wire every time the armature is suddenly removed from the magnet, or suddenly approaches it; and the wave of electricity generated by the removal of the armature moves in the opposite direction to that generated by the approach of the armature. With a description of this simple experiment, Faraday announced (in 1831) the discovery of magneto-electricity †, which was found to possess all the distinguishing characteristics of electricity derived from any other source.

* *Principia*, 1st Edition, Book 2. Prop. 37.

† Philosophical Transactions, 1832, vol. cxxii.

4. The force of a permanent magnet is usually estimated by the weight which is required to separate the armature or sub-magnet from its poles; and if the question were asked, for the first time, what relation existed between the sustaining-power of an electromagnet excited by means of a magneto-electric machine, and the sustaining-power of the permanent magnet from which the electricity was derived, it would probably be answered, that since the permanent magnet was the primary cause of the phenomena, the electromagnet would possess, at the most, no greater sustaining-power than the permanent magnet. This, however, is not the case; for I have found that an indefinitely small amount of magnetism, or of dynamic electricity, is capable of inducing an indefinitely large amount of magnetism, and, again, that an indefinitely small amount of dynamic electricity, or of magnetism, is capable of evolving an indefinitely large amount of dynamic electricity.

5. That Faraday himself stood on the threshold of this discovery will be obvious from the following observations made by him in a paper "On the Physical Character of the Lines of Magnetic Force"* , in which, when speaking of the magnet as a source of electricity, he says, "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 develop in an iron core magnetic 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 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 phenomena presented to us shall become *one*."

6. As the investigations which led to the paradoxical conclusions enunciated above (4) were not originally intended for publication, but were undertaken for my own instruction, I find that it will be much more convenient to describe the experimental results in a more methodical manner than that in which they were obtained.

7. The numerical determinations, derived from the experiments to be described, will be given with sufficient exactitude to allow of a comparison being made between them and those of other experimentalists. Other quantitative determinations will, for the present purpose, be sufficiently expressed by the terms

* Philosophical Magazine, June 1852, par. 3273

“greater” and “less,” as attention will be chiefly confined to a description of well-defined phenomenal effects.

8. Though I make use of certain conventional terms in connexion with the subject of these researches, it is not thence to be inferred that I hold to the opinion that specific entities distinct from ordinary matter are concerned in the production of phenomena of any kind whatever.

9. The apparatus with which the experiments were made is shown in Plate II. figs. 1–10. Two blocks of cast iron, A, A, of the form shown in figs. 1, 2, 3, and two pieces of brass, B, B, of the same length as the cast-iron blocks, were bolted together at the top and bottom with small brass bolts, in such a manner as to form a compound hollow cylinder of brass and iron, hereafter called the magnet-cylinder. A smooth and parallel hole C, $1\frac{1}{8}$ inch in diameter, was bored through the magnet-cylinder; and the thickness of the brass packings, B, B, separating the iron sides of the cylinder from one another, was three-quarters of an inch, or nearly half the diameter of the hole. Two pillars of wrought iron, D, D (fig. 3), were screwed into the cast-iron projections E, E (figs. 1, 2, 3) at each end of the magnet-cylinder, for the purpose of holding the cross heads F, F, fig. 3. These cross heads were made of brass, and were bored out concentrically with the hole through the magnet-cylinder, so as to form suitable bearings in which the journals of an armature might revolve.

10. The armature, which was of the same form as that used by Siemens (figs. 4, 5, 6, 7), was made of cast iron, and was turned parallel throughout its entire length, and about one-twentieth of an inch less in diameter than the hole in the magnet-cylinder, for the purpose of allowing it to revolve inside the cylinder in close proximity to it, but without touching it. The thickness of the rib G, uniting the segmental sides of the armature (figs. 4, 5, 7), was one-quarter of an inch. Two brass disks or caps, H, H', having concentric prolongations for holding the steel journals I, I, were fitted by means of screws, one at each end of the armature. A pulley, K, for driving the armature was fixed upon the cylindrical axis of the cap H'; and upon the axis of the cap H at the other end of the armature, a commutator, L, L', of hardened steel was fixed.

11. About 163 feet of copper wire 0.03 of an inch in diameter, insulated with silk, was wound upon the armature (fig. 6) in the direction of its length. The inner extremity of the wire was placed in good metallic contact with the armature, and its outer extremity was connected with the insulated half L' of the commutator by means of a clip and binding screw. Bands, encircling the armature at intervals, and sunk below the surface of

the iron in grooves turned out for their reception, prevented the convolutions of insulated wire from flying out of position by the centrifugal force attending their rapid revolution. The armature is represented complete in fig. 6, and in section in fig. 7.

12. A number of exactly similar permanent magnets (of the form shown in fig. 1), 8 inches long, were made from bars of steel 1 inch wide and a quarter of an inch thick, and the distance between the inner edges of the polar extremities of the magnets was a little less than 2 inches. The magnets weighed about one pound each; and they received very nearly equal degrees of magnetism, which enabled them to support a weight of about ten pounds each.

13. On each side of the magnet-cylinder was a flat raised surface, M, M, figs. 2, 3, extending the whole length of the cylinder between the projections E, E. These surfaces were planed parallel with each other and with the axis of the magnet-cylinder. When the magnets, the legs of which were somewhat less than 2 inches apart, were sprung upon the cylinder in the position shown in fig. 1, they were held in sufficiently good contact for the magnetism to diffuse itself equally throughout the entire mass of the cylinder; the two cast-iron sides of which, consequently, formed the poles of the magnets. On the lower part of the magnet-cylinder four projections or feet, N, N, N, N, figs. 2, 3, were cast, by means of which it was bolted firmly to a wooden base.

14. When the armature was made to revolve rapidly in the interior of the magnet-cylinder, waves of magneto-electricity were generated in the coils by the reversals of the magnetism in the rib G; and from the peculiar construction of the cylinder and armature, two waves of electricity, moving in alternate directions, were generated for each revolution of the armature.

15. The rapid succession of alternating waves thus generated could be taken from the machine as an intermittent current moving in one direction, by means of two steel springs (shown in the perspective drawing, fig. 10), when they were made to rub against the opposite sides of the commutator L.

16. The waves of electricity could also be taken in alternate directions from the machine when required, by adjusting the springs so as to rub against the unbroken cylindrical part of each half of the commutator.

17. The springs were placed in metallic connexion with the respective polar terminals of the machine, and to these terminals wires were attached for making the necessary connexions with those of a galvanometer or of an electromagnet.

18. The first series of experiments with the magneto-electric machine thus described, was made for the purpose of ascer-

taining what influence the number of magnets on the cylinder had upon the quantity of electricity generated by the machine, as indicated by the galvanometer.

19. During these investigations, the armature of this machine was driven by steam-power at a constant velocity of three thousand revolutions (equivalent to six thousand waves of electricity [14]) per minute. The direct current from the machine (15) was transmitted through one of Pouillet's galvanometers of tangents, constructed by Ruhmkorff, which was placed beyond the influence of the magnetism of the machine. The resistance of the galvanometer-coils was so small, in proportion to the resistances of the other circuits employed in these researches, as to render it unnecessary to take it into account.

20. Four permanent magnets (12) were placed successively upon the magnet-cylinder at a distance of half an inch from one another, and as each additional magnet was placed upon the cylinder the deviation of the galvanometer-needle was read off after it had taken up a steady position. The results of these experiments are shown in the following Table.

TABLE I.

Number of magnets on cylinder.	Deviations of galvanometer.	Tangents of deviations.
1	29·25	0·56
2	52·00	1·28
3	62·75	1·94
4	67·75	2·44

21. In making these experiments, which have often been repeated at different times, it was invariably found that, when only one magnet was on the cylinder, the quantity of electricity generated by the machine was proportionately less than when two or more magnets were placed on the cylinder. This deficiency appears to me to be due to the small amount of magnetism of a single magnet having to diffuse its influence through the comparatively large masses of iron of which the cylinder and armature were composed. After making allowance for this discrepancy, together with errors of observation, it will be seen from an inspection of the above Table, that, within the limits of these experiments, the quantity of electricity generated in the wire surrounding the armature of the magneto-electric machine is in direct proportion to the number of magnets on the magnet-cylinder, or to the quantity of magnetism induced in it.

22. A second series of experiments was made with the view of ascertaining the relation existing between the lifting-power of the permanent magnets on the magnet-cylinder, and that of an electromagnet excited by the electricity derived from the magneto-electric machine. In these investigations the apparatus shown in fig. 8 was used, which consisted of a small electro-magnet, made by bolting to an iron block, forming the base of the electromagnet, two plates of iron 6 inches long, 3 inches wide, and $\frac{3}{8}$ ths of an inch thick. The inside distance between the two plates was about 2 inches; and the polar surfaces of the magnet were truly planed, as was also the under surface of the keeper or submagnet used in connexion with it. This submagnet was made of a small block of iron about 3 inches square and 1 inch in thickness, and was connected, by means of a link and swivel, to one end of a scale-beam supported at its centre by an upright pillar. From the other end of the beam depended a scale-pan, which was weighted so as to exactly counterbalance the weight of the submagnet. The stand supporting the scale-beam was firmly bolted to an iron lathe-bed, as was also the electro-magnet, which was placed in a vertical position beneath the submagnet.

23. Around each side or plate of the electromagnet, a length of 100 feet of insulated copper wire 0.05 of an inch in diameter was coiled, and the ends of the wires were so arranged that they could, at pleasure, be coupled up so as to form a single circuit of 200 feet, or a double circuit of 100 feet in length. One foot of the wire on the armature of the magneto-electric machine had a resistance equal to 3 feet of the single wire on the electro-magnet.

24. Experiments were made, in the first instance, for the purpose of ascertaining what influence the number of magnets on the magnet-cylinder had upon the attractive force mutually exerted by the electromagnet and the submagnet. As the scale-beam was of too delicate a construction to allow of the submagnet being placed in direct metallic contact with the electromagnet, a piece of thin cardboard was fastened upon the poles, by means of gum. The wires of the electro-magnet were coupled up so as to form a double circuit 100 feet in length, the resistance of which was about one-tenth of the resistance of the armature circuit. The electromagnet was excited by the direct current from the magneto-electric machine. The submagnet was then placed upon the covered poles of the electro-magnet, and small weights were introduced into the scale-pan of the balance until the submagnet was separated from the electromagnet.

25. In order that a more rigid comparison might be made

between the quantities of electricity derived from the magneto-electric machine and the amount of the attractive force mutually exerted by the electromagnet and the submagnet, the particular experiments, the results of which are given in Tables I. and II., were made simultaneously, the tangent galvanometer at the same time forming part of the same metallic circuit as the electro-helices and the wire surrounding the armature.

26. Coordinate results, such as are shown in Tables I. and II., were, however, obtained, whether the first and second series of experiments were made either together or separately.

TABLE II.

Number of magnets on cylinder.	Weight, in ounces, required to separate submagnet from electromagnet.	Squares of magnetic force of the magnet-cylinder.
1	2.50	2.50
2	11.25	10.00
3	24.00	22.50
4	38.00	40.00

27. The results arrived at, as shown in the above Table, are somewhat remarkable, and have amongst themselves a well-defined ratio, such as would hardly have been expected from a bare consideration of the nature of the magnetism of the permanent magnet; for when one magnet was placed on the cylinder, the weight required to separate the submagnet from the electromagnet was 2.5 ozs. It might therefore have been expected that two magnets placed on the cylinder would have induced a double amount of magnetic force in the electro-magnet, whereas the force required to detach the submagnet was equal to a weight of 11.25 ozs., or was a little more than quadrupled. From a further comparison of the numbers contained in the Table, it will be seen that (within the limits and conditions of these experiments, and after making allowance for a certain degree of imperfection in them) the amount of magnetism induced in the electromagnet, as measured by the weight required to separate the submagnet, is as the square of the inducing magnetism of the permanent magnets of the electro-magnetic machine.

28. Experiments were then made for the purpose of ascertaining to what extent an alteration in the length and section of the wires surrounding the electromagnet would influence the quantity of magnetism induced in it. The electrohelices were therefore coupled up so as to form a single circuit 200 feet in

length (23), and its resistance was about four-tenths of that of the wire surrounding the armature. The experiments were conducted in the same order as those in the preceding series, the thin cardboard being still interposed between the submagnet and the electromagnet (24); and the following Table contains the results obtained.

TABLE III.

Number of magnets on cylinder.	Weight, in ounces, required to separate submagnet from electromagnet.	Squares of magnetic force of magnet-cylinder.
1	5.00	5
2	28.00	20
3	76.00	45
4	144.00	80

29. From a comparison of the numbers in this Table with those in Table II., it will be seen that the ratio of increase, as well as the amount of the magnetism induced in the electromagnet, has been considerably augmented by an increase in the length of the electromagnetic circuit.

30. Experiments were also made with the submagnet in direct contact with the electro-magnet without the interposition of the cardboard, the arrangement of the electrohelices remaining the same as in the preceding experiments (28). The small scale-beam and stand were removed from the lathe-bed, and were replaced by a stronger apparatus of a similar construction. The results of these experiments are shown in the following Table.

TABLE IV.

Number of magnets on cylinder.	Weight, in pounds, required to separate submagnet from electromagnet.	Squares of magnetic force of magnet-cylinder.
1	31.50	31.50
2	98.00	126.00
3	150.50	283.50
4	178.50	504.00

31. From an examination of the results of these experiments, it will be seen that when the submagnet was in direct contact with the electromagnet, the force required to separate them was very greatly increased; but the ratio of this increase, as measured by the same means as in the former experiments (22), is very considerably diminished; for when one magnet was placed

on the cylinder, the addition of a second magnet increased the sustaining-power of the electro-magnet by 66·5 lbs., whereas when three magnets were placed on the cylinder, the addition of a fourth magnet was only attended by an increase of 28 lbs. in its sustaining-power.

32. But the most extraordinary fact brought out in connexion with the latter series of experiments is the development of a much greater amount of magnetism in the electromagnet than that which existed in the permanent magnets employed in exciting it; for while the four permanent magnets on the cylinder were only capable, collectively, of sustaining a weight of about 40 lbs., the electromagnet, as will be seen from the Table, would sustain a weight of 178·5 lbs.

33. In order that this remarkable property might be exhibited in a more striking manner, a large electromagnet was constructed by screwing into a heavy iron block, 6 inches in thickness, two cylinders of wrought iron 24 inches in length and $3\frac{1}{2}$ inches in diameter. Round each of these cylinders an insulated strand of copper wires, each 950 feet in length and 0·15 of an inch in diameter, was wound from end to end of the cylinders in several concentric layers, and the two electrohelices were coupled up so as to form one continuous helix 1900 feet in length. The cylindrical poles of the electromagnet were $8\frac{1}{2}$ inches distant from centre to centre, and were furnished with a suitable submagnet, which was connected by means of a link with a strong lever, for the purpose of measuring the amount of force necessary to separate the submagnet from the electromagnet.

34. When the four permanent magnets (20) were placed on the cylinder of the magneto-electric machine, and the electricity from it was transmitted through the electromagnetic helices, a weight of not less than 1088 lbs. was required to overcome the attractive force of the electromagnet, or twenty-seven times the weight which the four permanent magnets used in exciting it were collectively able to sustain. It will, however, be shown hereafter (77) that this difference between the sustaining-power of a permanent magnet and that of an electromagnet excited through its agency, great as it is, is very far from reaching the limits to which it can be carried.

35. The question now arose, how the results obtained from these experiments were to be reconciled with the principle of the conservation of force, since it is now generally held by physicists that the calorific, magnetic, and other properties of the electric circuit are correlated, both in direction and amount; and to admit the coexistence of any one of these properties along with the others in a greater or less degree, under like

conditions, would involve the idea of the miraculous or the paradoxical.

36. In experimenting with the magneto-electric machine, it was found that the dead point of the armature, or that position during its revolution in which no electricity is evolved, varied with the length or the resistance of the wires which joined the poles of the machine. It therefore became necessary to change the position of the commutator on the armature-axis to suit the different circuits through which the electricity was transmitted, so that the burning effects of the spark at the junction of the two halves of the commutator might be avoided when the direction of the current was changed.

37. When the commutator was properly adjusted to the resistance of the wires surrounding the electromagnet (33), I observed that so long as the magneto-electric machine was allowed to run without its poles being connected, either with the wires of the electromagnet or any other conductor, a brilliant star of light appeared at the points where the springs were in contact with the commutator (15); but when the poles of the machine were connected by means of a short piece of wire, the bright light at the commutator instantly disappeared. It was also observed that when the poles of the machine were connected with the long helices of the large electromagnet, a perceptible interval of time elapsed before the light at the commutator disappeared. Besides this, it was also observed that at the moment of breaking contact between the wires of the electromagnet and the poles of the machine, a much more brilliant spark appeared at the points of separation, and a much more severe shock was felt when the body formed part of the circuit, than could be produced by the direct action of the machine alone. The latter effects could not, however, be obtained until a certain interval of time had elapsed after contact had been made between the electro-helices and the wires of the machine.

38. Moreover I found that a spark could be obtained from the wires surrounding the electromagnet even after they had been entirely disconnected from the machine. This curious result was obtained by first holding the free extremities of the wires surrounding the electromagnet, one in each hand, in contact for a few seconds with the respective polar terminals of the machine, and, while contact was still maintained, bringing the ends of the electrohelices into metallic contact with each other, so that they formed a continuous metallic loop or closed circuit. The loop so formed was then suddenly removed from the polar terminals, and while thus entirely disconnected from them the ends of the loop were suddenly separated, and a bright spark appeared at the point of disjunction. With a larger and more

powerful electromagnet (67, 68), a bright spark was in like manner obtained twenty-five seconds after all connexion with the magneto-electric machine had been broken*.

39. None of the effects described, such as the great sustaining-power of the electromagnet above that of the permanent magnets (34), the increased brilliancy of the spark at the point of disjunction of the wires (37), or the spark from the electrohelices after all connexion with the magneto-electric machine had been broken (38), were observed when the alternating current from the machine (16) was transmitted through the electrohelices instead of the direct current from the commutator (15). Under these conditions it was found that neither the small electromagnet (22, 23) nor the large electromagnet (33) would sustain even a weight of 1 lb.

40. It was at first thought that the great difference observed between the sustaining-power of the electromagnet and that of the permanent magnets which excited it might be due to the helices surrounding the electromagnet absorbing or retaining the electricity transmitted through them in a static condition, in the manner observed in insulated submarine and subterranean telegraph wires, an investigation of which phenomenon, as it was manifested in gutta-percha-covered wires submerged in a canal, and in similar wires laid underground between London and Manchester, was made by Faraday in 1853†.

41. For the purpose of ascertaining whether this view of the subject was correct, a very small and delicate electromagnetic balance was constructed, similar in principle to the one shown in fig. 8. The small electromagnet, fixed beneath one end of the balance, was coiled with a strand of insulated copper wires 6 feet in length and 0.15 of an inch in diameter. The submagnet was prevented from coming into contact with the poles of the electromagnet by means of regulating-screws. Other regulating-screws limited the movements of the balance in the opposite direction; and the distance of the submagnet from the electromagnet could also be adjusted, by means of the same regulating-screws, to suit the different quantities of electricity transmitted through the electrohelices.

42. This electromagnetic balance was placed in the middle of the circuit of the electromagnet (33), *i. e.* at the point where the two electrohelices were joined together. The poles of the magneto-electric machine were then connected with the free extremi-

* Since this paper was read I have discovered that Dr. Page, in 1851, succeeded in obtaining a spark from an electromagnet, coiled with a very long wire, half a second after all connexion with the battery had been broken.—Silliman's *American Journal of Science*, vol. xi. p. 88.

† Proceedings of the Royal Institution, January 20, 1854.

ties of the electrohelices for a few seconds; and after the spark from the commutator had disappeared, the submagnet was so counterpoised by means of small weights as to respond immediately to the attractive force of the small electromagnet placed beneath it, so long as the electricity from the machine was transmitted through the helices of the large electromagnet; but when contact with the machine and the electrohelices was broken, it was observed that the submagnet was instantly withdrawn from the poles of its electromagnet by the weights placed in the scalepan at the opposite end of the balance.

43. The apparatus being thus arranged, it would follow that if the charge in the wire surrounding the electromagnet were identical with that which is observed in insulated submarine-telegraph cables, a certain interval of time would elapse, after contact with the magneto-electric machine had been made, before the balance in the middle of the circuit would respond to the attractive force of the small electromagnet placed beneath it. On making the experiment it was found that when contact was made with the machine, 1.5 second elapsed before the balance responded to the attractive force.

44. When placed in the middle of the circuit of a larger and more powerful electromagnet (67, 68) excited by the same magneto-electric machine (18), the electromagnetic balance did not respond to the attractive force until an interval of 15 seconds had elapsed.

45. But if the retardation of the current, as indicated by the balance when placed in the middle of the circuit, had been the effect of an accumulation of static electricity in the electrohelices, it would also have been instantly attended by a rush of the full current of electricity into the helices at the beginning of the circuit, such as was observed in the before-mentioned experiments made by Faraday (40). On making the experiment, however, this was not found to be the case; for when the balance was removed from the middle and placed at the beginning of the electromagnetic circuit, the wires being again joined up so as to form a continuous helix as before (33), it was still found that 1.5 second, and with the large electromagnet (67, 68) 15 seconds, elapsed before the electricity acquired sufficient power to bring down the submagnet of the balance.

46. When the large electromagnet (67, 68) was excited by the electricity from a larger and more powerful machine (63) driven at a velocity of 2000 revolutions (equivalent to 4000 waves) per minute, instead of that from the small magneto-electric machine, which produced 6000 waves per minute, an interval of only four seconds elapsed before the balance responded to the attractive force of its electromagnet.

47. Moreover the direction of the current in the electrohelices, as shown by the galvanometer, was the same *after* as it was *before* connexion with the electromotor was broken; whereas had the current, as shown by the spark obtained (38), been the result of a static charge of the kind observed in insulated telegraph wires, the electricity would have discharged itself, when the place of disjunction was at the electromotor, in the opposite direction to that in which it entered the electrohelices.

48. The conclusions drawn from a consideration of these experiments are therefore opposed to the supposition that the effects described are the consequence of a static charge of the kind observed to be retained by insulated submarine and subterranean telegraph wires; but some of the phenomena described—such as the retardation of the current when contact was made with the magneto-electric machine (43, 45), and the exalted electrical condition of the wire surrounding the electromagnet, as shown by the voluminous spark seen and the severe shock felt when contact with the machine was broken (37)—are identical with the phenomena of electric induction observed by Dr. Henry* and investigated by Faraday with the aid of the voltaic battery, and which form the subject of his Ninth Series of Researches in Electricity†.

49. That an electromagnet possesses the power of retaining a charge of electricity in a manner analogous to that in which it is retained in insulated submarine cables and in the Leyden jar, but not identical with it, is evident from the appearance of a spark at the point of disjunction of the wires a considerable time after all connexion with the electromotor has been cut off. The production of this spark appears to me to arise from the comparatively slow manner in which large masses of iron return to their normal condition after having attained a highly exalted degree of magnetism—the rate of decrease, however, being sufficiently rapid to allow the induction-current to manifest itself in the electrohelices, with a decreasing intensity, simultaneously with the decreasing flux of magnetism in the iron itself.

50. It is this important retentive property of the electromagnet which maintains its attractive force unimpaired, notwithstanding the intermittent character of the electricity transmitted through the electrohelices; for, as is well known, no current whatever is produced from the armature of the magneto-electric machine when in certain positions during its revolution. These positions correspond in some measure with the dead points of the crank of a steam-engine, the fly-wheel of which performs the same function *dynamically* as that which the electromag-

* Silliman's American Journal of Science, 1832, vol. xxii. p. 408.

† Philosophical Transactions, 1835, vol. cxxv.

net performs *statically* in the case of the magneto-electric machine.

51. That the charge retained by the electromagnet is, as has already been observed, much more powerful than that which the magneto-electric machine is of itself capable of producing, is evident from the severe shock which is felt when the body forms part of the circuit, and also from the more voluminous spark which appears at the point of disjunction of the wires when contact with the machine is broken.

52. That this increase of electric force in the electromagnet is the consequence or effect of a certain number of electrical waves transmitted through the electrohelices, and succeeding each other with sufficient rapidity to sustain the increasing flux of magnetism in the iron, is manifest from the time which elapses before the electricity transmitted through the helices attains a permanent degree of intensity, and before the electromagnet acquires its greatest amount of magnetism (45)*.

53. That the length of time which was observed to elapse, and the number of waves which required to be transmitted through the electrohelices before the current from the magneto-electric machine attained a permanent degree of intensity, and the electromagnet acquired its greatest amount of magnetism, are dependent upon the magnitude of the waves of electricity transmitted through the electrohelices, is evident from the fact that the same degree of intensity of the current (as measured by the balance), and the same amount of magnetism in the electromagnet, were obtained with a much smaller number of waves, and in a shorter time, from a large electromotor, than could be obtained with a much greater number of waves from a small electromotor (46). These observations will be further confirmed by experiments to be hereafter adduced.

54. The cause of the great difference between the attractive force of a permanent magnet and that of an electromagnet excited through its agency, and also the agreement of the phenomena with the principle of the conservation of force, now become sufficiently manifest to render it unnecessary, at the present time, to institute a more rigorous comparison between the quantities of magnetism and electricity of the magneto-electric machine, and the quantities of the same forces developed in the electromagnets (23, 33). The general conclusion which may, however, be drawn from a consideration of the preceding experiments is, that when an electromagnet is excited through the agency of a permanent magnet, the large amount of magnetism manifested in the electromagnet simultaneously with the small amount manifested in the permanent magnet is the constant ac-

* Philosophical Transactions, 1846, p. 6.

companionment of at least a correlative amount of electricity evolved from the magneto-electric machine, either all at once in a large quantity, or by a continuous succession of small quantities (45, 46),—the power which the metals (but more particularly iron) possess of accumulating and retaining a temporary charge of electricity, or of magnetism, or of both together (according to the mode in which these forces are viewed by physicists), giving rise to the paradoxical phenomena which form the subject of this research*.

§ 2. *On a new and powerful Generator of Dynamic Electricity.*

55. The fact that a large amount of magnetism can be developed in an electromagnet by means of a permanent magnet of much smaller power having been established, and as from the first series of experiments (Table I.) it was shown that definite quantities of magnetism are accompanied by the evolution of proportionate quantities of dynamic electricity, and since an electromagnet when excited by means of a voltaic battery pos-

* Since the publication of the abstract of this paper in the Proceedings of the Royal Society, my attention has been directed to several accounts of experiments in which electromagnets, excited by means of magneto-electric machines, have been made to sustain considerable weights. The most important of these accounts which have come under my notice, is one contained in Silliman's Journal of Science for 1845, vol. xlvi. p. 393, in which it is stated that Dr. Page, by means of a magneto-electric machine, made an electromagnet sustain a weight of 1000 lbs.

Another account to which I have been referred is contained in a 'Treatise on the Electric Telegraph,' by M. l'Abbé Moigno, Paris, 1849, in which it is stated (page 15, p. 72 in the second edition) that the Abbés Moigno and Raillard, by means of a small machine, made an electromagnet sustain a weight of 600 kilogrammes.

In neither of these accounts, however, does any direct comparison appear to have been made between the sustaining-power of the permanent and the electromagnets, as no mention is therein made of the sustaining-power of the permanent magnets, nor are they (the permanent magnets) specifically mentioned.

In a brief notice of my experiments which appeared in *Les Mondes* of September 6, 1866, of which journal M. l'Abbé Moigno is the editor, he gives what professes to be a quotation from his *Traité de Télégraphie Électrique*, in which he has introduced a statement specifying the sustaining-power of the permanent magnets used in his experiments, although no such statement is to be found in the treatise from which the quotation is taken.

Another discrepancy with reference to the account of Moigno's experiments also occurs in an article on "Wilde's Magneto-electric Machine," in the Quarterly Journal of Science for October 1866, in which the writer would seem to have mistaken a small electromagnet (used only as an adjunct to a magneto-electric machine, and which Moigno states would only support a few grammes) for the permanent magnets which excited the electromagnet; and from this error it is made to appear that the permanent magnets used by Moigno would only sustain a few grammes.

sesses all the properties of a permanent magnet, it appeared reasonable to suppose that a large electromagnet excited by means of a small magneto-electric machine could, by suitable arrangements, be made instrumental in evolving a proportionately large quantity of dynamic electricity, notwithstanding the pulsatory character of the electricity transmitted through the wires surrounding the electromagnet.

56. Two magnet-cylinders of similar construction to the one already described (9) (figs. 1, 2, 3, 9), were therefore made, having a bore of $2\frac{1}{2}$ inches and a length of $12\frac{1}{2}$ inches, or five times the diameter of the bore.

57. As frequent mention will have to be made of the different-sized machines employed in these investigations, they will in future be distinguished by their calibre, or the diameter of the bore of the magnet-cylinder.

58. Each cylinder was fitted with pillars, cross-heads, and an armature similar to those already described (9, 10) (figs. 3, 6). Around each armature was coiled an insulated strand of copper wires 67 feet in length and 0.15 of an inch in diameter, the extremities of which were respectively connected with the two halves of a commutator fixed on the axis of each armature (10). Upon one of the magnet-cylinders (fig. 1) sixteen permanent magnets, of the form shown in the figure, 12 inches in length, were fixed. Each of the magnets weighed 3 lb., and would sustain a weight of 20 lb.

59. To the sides of the other magnet-cylinder, an end view of which is shown in fig. 9, two rectangular pieces of boiler-plate, O, O, $12\frac{1}{2}$ inches long, 9 inches wide, and $\frac{3}{8}$ of an inch thick, were bolted parallel with each other and between the iron packings P, P, P', P'. The upper extremities of these plates were united by means of a hollow bridge, Q, to which they were bolted, along with iron packings similar to those on their lower extremities. The bridge was made of two thicknesses of the same boiler-plate iron as that of which the sides were made; and for the purpose of ensuring good contact, its edges, as well as those parts of the sides of the rectangular plates in contact with them and with the magnet-cylinder, were planed to a true surface. An insulated strand of copper wires, 350 feet in length and of the same diameter as that on the armature (58), was coiled round each of the rectangular iron plates in a direction parallel with the axis of the magnet-cylinder. The two coils were united so as to form a single circuit 700 feet in length, the free ends of which were furnished with suitable terminal screws for the purpose of connecting them with the wires from the $2\frac{1}{2}$ -inch magneto-electric machine. A perspective view of this machine is shown in fig. 10, but on a much larger scale than the

magneto-electric machine which is placed on the top of it. The $2\frac{1}{2}$ -inch magneto-electric and electromagnetic machines were placed side by side upon a strong wooden base, and their armatures were driven simultaneously from the same driving-shaft, at an equal velocity of about 2500 revolutions per minute.

60. Experiments were then made for the purpose of comparing the quantities of electricity evolved from the magneto-electric and electromagnetic machines as measured approximately by their heating-effects. When the alternating waves from the magneto-electric machine were transmitted through a piece of No. 20 iron wire, 0.04 of an inch in diameter, a length of 3 inches of this wire was raised to a red heat.

61. The direct current (15) from the magneto-electric machine was then transmitted through the coils surrounding the iron plates O, O, which being united by the bridge Q, formed a powerful electromagnet similar in construction to that invented by Joule*, and having for its poles the two sides of the magnet-cylinder. When the alternating waves from this electromagnetic machine were transmitted through the same-sized iron wire as was used in the preceding experiment, 8 inches of it were melted, and a length of 24 inches was raised to a red heat.

62. A comparison of the heating-effects of the two machines, as found by these experiments, brings out the important fact that a much greater amount of electricity is evolved from the electromagnetic machine than is evolved simultaneously from the magneto-electric machine. Moreover, considering the smallness of the quantity of iron of which the armature was made (only five pounds), and of the copper wire surrounding it, the weight of which was only $3\frac{1}{2}$ pounds, the heating-effects of the electromagnetic machine are very remarkable. One of the most energetic generators of dynamic electricity is that invented by Grove; and it was found from experiments made with four new cells of this battery, the platinum plates of which were $6 \times 3\frac{1}{2}$ inches, with double zinc plates well amalgamated, and charged with concentrated nitric acid and a strong solution of sulphuric acid, that ten cells of this powerful arrangement would be required in order to produce the same heating-effects as those produced by the electromagnetic machine.

63. For the purpose of ascertaining in what ratio the power of the electromagnetic machine would be increased by an enlargement of its dimensions, a machine was constructed double the size of the one described (59), but of precisely the same proportions. The bore of the cylinder was 5 inches in diameter, and its length 25 inches. The copper wire strand surrounding the electromagnet was 1170 feet in length, and weighed about

* Phil. Mag. S. 4. vol. iii. p. 32.

390 lbs. The armature of this machine was coiled with an insulated copper strand 84 feet in length, the weight of which was about 28 lbs.

64. When the electromagnet of the 5-inch machine was excited by the $2\frac{1}{2}$ -inch magneto-electric machine, the armature of the 5-inch machine being driven at about 2000 revolutions per minute, the electricity from it melted 15 inches of No. 15 iron wire 0.075 of an inch in diameter. Now it was found that a piece of No. 15 iron wire 15 inches in length was about seven times the weight of 8 inches of the wire melted by the $2\frac{1}{2}$ -inch electromagnetic machine (61); and as the 5-inch machine was about eight times the weight of the $2\frac{1}{2}$ -inch electromagnetic machine, the increase of power of the 5-inch machine, as measured by its heating-effects, appears to me to be nearly in direct proportion to the increase of its cubical dimensions, after allowance has been made for the diminished speed at which the armature was driven, together with the small increase of power which might have been obtained had the electromagnet been excited by a more powerful magneto-electric machine.

65. For the purpose of learning to what extent the power of the electromagnetic machine might be increased by an exaltation of the magnetism of the electromagnet without changing the speed at which the armature was driven, the electromagnet of the 5-inch machine was excited by the direct current from the $2\frac{1}{2}$ -inch electromagnetic machine (59), the latter being in its turn excited by the $2\frac{1}{2}$ -inch magneto-electric machine (58). On making the experiment, it was found that although the magnetism of the electromagnet (63) was considerably increased, yet this increase was only accompanied by a very small additional quantity of electricity from the armature—thus showing that the full power of the 5-inch machine had been very nearly attained when its electromagnet was excited by the $2\frac{1}{2}$ -inch magneto-electric machine alone.

66. Having found that an increase in the dimensions of the electromagnetic machine was accompanied by a proportionate and satisfactory increase of the magnetic and electric forces, a 10-inch electromagnetic machine was constructed; and as its calorific and illuminating powers are very remarkable, some particulars respecting the machine, together with a few experiments made with it, may perhaps be found to possess some interest, especially for those physicists who are engaged in determining the quantitative relations existing between the various forces as manifested to the senses.

67. In describing the different parts of the machine, reference will still be made to figures 1-9, which have been drawn to a proportionate scale. A perspective view of the machine com-

plete is shown in fig. 10. Each side of the electromagnet O, O, fig. 9, is made of a plate of rolled iron 48 inches in length, 39 inches wide, and $1\frac{1}{2}$ inch in thickness. The wrought-iron bars P, P, P', P', bolted to the upper and lower extremities of the plates, are 6 inches wide and 2 inches thick. These bars are somewhat longer than the width of the plates, and are secured to the sides of the magnet-cylinder, with the plates between them, by means of iron bolts 1 inch in diameter. The bridge Q extends the whole length of the bars P', P', and is made of two plates of rolled iron 43 inches long, 16 inches wide, and $1\frac{1}{2}$ inch thick, separated from each other by an iron packing 3 inches in thickness, which makes the entire depth of the bridge equal to the width of the bars P', P'. The bridge is fixed between the side plates by means of long iron bolts 1 inch in diameter, extending from one side of the magnet to the other, as shown in the figure. All the component parts of the electromagnet which required to be fitted together were planed to a true surface, for the purpose of ensuring intimate ferric contact throughout the entire mass. The total weight of the iron of the electromagnet, exclusive of the magnet-cylinder, is a little more than 1.5 ton.

68. Each side of the electromagnet was coiled with an insulated conductor, consisting of a bundle of thirteen No. 11 copper wires, each 0.125 of an inch in diameter, laid parallel with each other, and bound together with a double covering of linen tape, after the manner adopted by Joule in the construction of his electromagnets*. The length of conductor coiled round each side of the electromagnet is 2400 feet, and the outer extremities of the coils are coupled up so as to form a continuous circuit 4800 feet in length. The inner extremities of the coils terminate in two insulated metal studs fixed upon the wooden top of the machine (fig. 10). The total weight of the two coils is 1.3 ton.

69. The magnet-cylinder consists of two masses of cast iron 50 inches in length, separated from each other by an interval of 5 inches by means of blocks of brass placed at intervals along the top and bottom of the cylinder. All the flat surfaces of the latter, as well as those of the brass blocks in contact with them, are truly planed, and the several parts of the cylinder are bolted together at the top and bottom by means of twelve copper bolts 1 inch in diameter. The bore of the magnet-cylinder is 10 inches, and its weight, when fitted up with iron pillars and brass cross-heads, is 1.1 ton.

70. The machine is furnished with two armatures—one for the production of “intensity,” and the other for the production of

* *Annals of Electricity*, vol. v. p. 472.

“quantity” effects. These armatures are made of cast iron, and are precisely alike in dimensions, as they were cast from the same pattern. The thickness of the rib G, fig. 5, uniting the segmental sides of the armatures, is 1.75 inch, and the latter are turned one-eighth of an inch less in diameter than the bore of the magnet-cylinder. A pulley, 10 inches in diameter, is keyed upon one end of each armature, and upon the other end is fixed a commutator, by means of which the waves of electricity from the armature can be taken, either in the same or in alternate directions as required (15, 16).

71. The intensity armature is coiled with an insulated conductor consisting of a bundle of thirteen No. 11 copper wires, each 0.125 of an inch in diameter, the same as that coiled round the sides of the electromagnet (68). The conductor is 376 feet in length and weighs 232 lbs., and is covered with a casing of wood extending the entire length of the armature, for the purpose of protecting it from external injury. Strong bands of sheet brass 1 inch in width encircle the armature at intervals of 6 inches, for the purpose of keeping the casing and the convolutions of the conductor in position during their rapid revolution (11). The total weight of this armature with all its fittings is 0.3 of a ton.

72. The quantity armature is enveloped with the folds of an insulated conductor consisting of four plates of copper, each 67 feet in length, 6 inches in width, and one-sixteenth of an inch in thickness. These plates are superposed in metallic contact with each other so as to form a single copper plate one-quarter of an inch in thickness, 67 feet in length, and nearly wide enough to occupy the entire width between the segmental sides of the armature. This division of the conductor into four plates was made for the greater convenience of bending it round the armature. The inner extremity of the conductor is held in intimate contact with the body of the armature by means of flat-headed screws; and the convolutions are insulated from one another by placing between them a band of thick cotton and india-rubber fabric, of the same length and width as the laminated conductor; and the edges of the latter are insulated from the sides of the armature by means of thin pieces of wood. The outer extremity of the conductor is terminated by a thick copper stud which connects it with the insulated half of the commutator fixed on the armature axis; and the convolutions are retained in position, by means of bands, in the same manner as those of the intensity armature (11, 71). The weight of the laminated copper conductor is 344 lbs., and the total weight of the armature is 0.35 of a ton.

73. The armatures were accurately balanced before being

placed in the magnet-cylinder, for the purpose of avoiding the excessive vibrations which were produced when they revolved at high velocities without being balanced. By means of a small carriage, the quantity and intensity armatures could easily be withdrawn from the magnet-cylinder, and interchanged, when required, in the course of a few minutes, the cross-head at the driving end of the machine being readily removeable for that purpose.

74. Though the total weight of the 10-inch electromagnetic machine complete is very considerable, being nearly 4.5 tons, yet its dimensions are comparatively small, since the entire length of the machine, including the brass cross-heads, is only 80 inches, its width 24 inches, and its height 60 inches.

75. Experiments were first made with the machine, for the purpose of testing its power when the large electromagnet was excited by magneto-electric and electromagnetic machines of various sizes. The 10-inch machine, as well as the machines used for exciting it, were all driven from the same countershaft by means of pulleys of suitable dimensions. The intensity and quantity armatures (71, 72) were driven at a uniform velocity of about 1500 revolutions per minute, by means of a broad leather belt of the strongest description. The springs for taking the electricity from the 10-inch machine were connected, by means of large copper conductors, with two insulated plates of copper let into the under side of an experimental table. On the upper side of this table were two moveable brass studs, sliding in good contact with the copper plates, and forming the polar terminals of the machine.

76. The quantity armature was first placed in the cylinder, and the springs were so arranged as to take the alternating currents of electricity from the polar terminals (16). The direct current from the small magneto-electric machine, having on its cylinder six permanent magnets, such as were used in the first series of experiments (12), was then transmitted through the coils of the electromagnet of the 5-inch electromagnetic machine (63); and the direct current from the latter was simultaneously, and in like manner, transmitted through the coils of the electromagnet of the 10-inch machine.

77. This combination of the machines, when in full action, was attended by the development of an amount of magnetic force in the large electromagnet far exceeding anything which had hitherto been produced, together with the evolution of a quantity of dynamic electricity from the armature so enormous as to melt pieces of cylindrical iron rod 15 inches in length, and fully one-quarter of an inch in diameter. With the same arrangement, the electricity from the quantity armature also

melted 15 inches of No. 11 copper wire 0.125 of an inch in diameter.

78. When the intensity armature (71) was placed in the cylinder, the combination of the machines remaining the same as in the preceding experiments (76), the alternating current from the armature melted 7 feet of No. 16 iron wire 0.065 of an inch in diameter, and made a length of 21 feet of the same wire red-hot.

79. The illuminating-power of the electricity from the intensity armature is, as might be expected, of the most splendid description. Two rods of gas-carbon, half an inch square, were placed in the carbon-holders of the beautiful apparatus for regulating the electric light, recently invented by M. Foucault, behind which was fixed a parabolic reflector 20 inches in diameter, so adjusted as to cause the rays of light to diverge from it at a considerable angle. When the electric lamp was placed at the top of a lofty building, the light evolved from it was sufficient to cast the shadows from the flames of the street-lamps a quarter of a mile distant upon the neighbouring walls. When viewed from that distance, the light was a very magnificent object to behold, the rays proceeding from the reflector having all the rich effulgence of sunshine.

80. A piece of the ordinary sensitized paper, such as is used for photographic printing, when exposed to the action of the light for twenty seconds, at a distance of 2 feet from the reflector, was darkened to the same degree as was a piece of the same sheet of paper when exposed for a period of one minute to the direct rays of the sun, at noon, on a very clear day in the month of March.

81. The extraordinary calorific and illuminating powers of the 10-inch machine are all the more remarkable from the fact that they have their origin in six small permanent magnets, weighing only 1 lb. each (12), and only capable, at most, of sustaining collectively a weight of 60 lbs.; while the electricity from the magneto-electric machine which was employed in exciting the electromagnet was, of itself, incapable of heating to redness the shortest length of iron wire of the smallest size manufactured.

82. The production of so large an amount of electricity was only obtained (as might have been anticipated by the physicist) by a correspondingly large expenditure of mechanical force, as the machine when in full action absorbed, as nearly as could be estimated, from eight to ten horse-power. When the $2\frac{1}{2}$ -inch magneto-electric machine (58) was substituted for the $1\frac{3}{8}$ -inch machine, in the combination before described (76), the magnetism developed in the electromagnet of the 10-inch machine was ex-

alted to such a degree that, although the strong leather belt from the main shaft, used for driving the countershaft, was 6 inches in width and acted upon a pulley 10 inches in diameter, it was scarcely able to drive the machine.

83. It was, however, found, as in the case of the 5-inch electromagnetic machine, that beyond certain limits a great augmentation of the magnetism of the electromagnet was only attended by a small increase of electricity from the armature (65). The results of a number of experiments, in which various quantities of electricity were transmitted through the coils of the electromagnet of the 10-inch machine, proved that when it was excited through the agency of the six permanent magnets, combined with the 5-inch machine (76), the maximum amount of electricity from the machine, when working at a speed of 1500 revolutions per minute, had been nearly obtained.

84. It was also found that the maximum amount of power, as measured by the quantity of wire melted, was very nearly obtained from the 10-inch machine when its electromagnet was excited by means of a 5-inch magneto-electric machine alone, instead of the combination of magneto-electric and electromagnetic machines used for that purpose (76).

85. When the electromagnet of the 10-inch machine was excited by means of the 2½-inch magneto-electric machine alone (58) (as shown on the wooden top of the machine in fig. 10), about two-thirds of the maximum amount of power from the 10-inch machine was obtained.

XII. *On an Apparatus for the Detection of Adulterations in Coffee.* By JOHN C. DRAPER, M.D., Professor of Natural History, College of the City of New York, and of Analytical Chemistry in the University of New York.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

College of the City of New York,
June 1, 1867.

HAVING been engaged during the past winter in the examination of specimens of adulterated coffee, I have contrived an instrument which has been of considerable practical value in enabling me to obtain satisfactory results with great rapidity. Thinking it may prove interesting to some of your readers I enclose a description of it, which was read at a recent meeting of the New York Academy of Medicine.

It consists of a tube (*a b*) 1 inch in diameter and 8 inches

in length, which terminates below in a narrow tube (*b c*) $\frac{1}{4}$ of an inch in diameter and 4 inches in length; the lower small opening (*c*) is closed by means of a cork to a certain point, from which it is graduated to tenths of a cubic centimetre: a number of these tubes, with a suitable stand and a small measure holding one cubic centimetre, complete the apparatus.

When it is to be used, the tube is closed below and filled to within half an inch of the upper extremity (*a*) with cold water (that has been boiled to expel the gas and kept in a well-stoppered bottle to prevent its reabsorption); a cubic centimetre of the sample of ground coffee under examination is then cautiously dropped on the surface of the water, when we should notice (1) whether it sinks quickly or slowly; (2) what tint and odour it imparts to the water; (3) the expansion in bulk as measured in the graduated small tube; (4) the change in colour of the grounds, and whether they collect loosely or not.



If it is good coffee and a pure article, it floats on the surface for a considerable period, and if freshly roasted and ground will often remain suspended for days if the tubes are kept free from agitation. In the majority of instances it falls in a few hours, and imparts to the water in the large tube a delicate amber tint and a faint coffee-like odour, each particle as it rests in the small tube retaining its outline, individuality, and colour, and the increase in bulk being about one-half a cubic centimetre.

If it is chicory it sinks instantly, nearly the whole mass tumbling into the small tube in a single minute, and even in its rapid passage through the water imparting to it a deep brown colour, and an odour somewhat like that of liquorice. The tint of the particles is at the same time altered greatly; the expansion of the mass is also very considerable, its bulk in a short time increasing to nearly two and a half cubic centimetres; and the particles, losing their individuality, seem to fuse together and form a mass almost without interstices.

By preparing mixtures of chicory and coffee in different proportions, we find that the first sinks rapidly, while the latter remains suspended for a considerable period, though a small portion may be carried down by the chicory in its sudden subsidence. After a time (which may be shortened if desired, by agitating the tube) the coffee sinks also; and by noticing the proportion of compact and loose sediment in the small tube, and the position of the line of meeting of the dark- and light-coloured solutions, we obtain an indication of the proportion of chicory to

coffee, which coincides very nearly with that employed in making the mixture. The tint of the fluid in the large tube and the increase in the bulk of the solid matter are also of use in arriving at a correct estimate. After the performance of a few experiments with mixtures of coffee and chicory in different proportions, a degree of expertness is obtained which enables the operator to form an accurate estimate of the amount of each ingredient in any sample that may be submitted for his examination.

Not only may coffee be thus to a great extent separated from any chicory with which it is mingled, but nearly all the other substances used for the purposes of adulteration, as exhausted coffee-grounds, wheat, &c., also subside rapidly with the chicory and collect in the small tube. If it is desired to determine the character of the matter that precipitates quickly we must resort to the microscope, when, by the nature of the cells and other structures found, we can soon decide upon the true character of the foreign substances. The presence of chicory is readily and satisfactorily proved; and if peas, beans, &c. are present, they may be easily distinguished by the peculiarities of the starch-cells of each. Such microscopic examinations are greatly facilitated by the use of the tubes I have described; for when the subsidence is complete, and we have read off the proportion of adulteration, it only remains to remove the cork gently from the small tube and allow a little of the lower portion of the sediment which contains the foreign matters to escape into a shallow dish, whence particles may be transferred to the microscope-slide; and if it is necessary, we may in this manner examine layer after layer of the deposit.

In the hope that the above communication may be found worthy of a place in your valuable Journal,

I am, Gentlemen,

Yours very respectfully,

JOHN C. DRAPER.

XIII. *The Conic Theory of Heat, in connexion chiefly with the Metamorphosis of Matter.* By the Rev. J. B. HARBORD, M.A.*

PROFESSOR TYNDALL remarks that "the ideas of the most well-informed philosophers are as yet uncertain regarding the exact nature of the motion of heat." The following idea I have not yet seen advanced; and as it is founded on the analogy of nature, and serves to explain and classify the various phenomena in this branch of physics, it may not be unworthy the consideration of the scientific world.

* Communicated by the Author.

The force which is the agent in the motion with which heat is now identified is believed, with reason, to be connected with the diffusion of highly attenuated matter throughout space. Adopting the terms of Sir John Herschel, on the relative motions of "æthereal molecules" and the "gross molecules" of sensitive matter seem to depend all the phenomena of heat. Two distinct sets of theories have been advanced:—those like that of Mossotti, which make the gross molecules centres of force; and that of Rankine, which makes the æthereal molecules the nuclei, and allots to the gross molecules the functions of an encircling atmosphere. The former theories conceive the æther to have a kind of motion analogous to the planetary orbital motion round the gross molecules, the latter theory ingeniously combines the two motions of waves and whirling. But on both hypotheses these orbits or these oscillations and vortices must have some typical form. Reasoning from analogy, these forms will be conic sections; and upon the kind of conic section, we conceive, depends the condition of the mass of matter,—the solid, liquid, and gaseous states corresponding respectively to elliptic, parabolic, and hyperbolic orbits. An addition of heat to a body accelerates the velocity of the revolving molecules; and on this increase relatively to the distance of the revolving molecules from the centre of force in the nucleus depends the change in the form of the orbits, and consequently the metamorphosis in the condition of the substance. As long as the space due to the velocity is less than the distance, the mass will be solid; while the space due to the velocity is equal to the distance, the mass will continue liquid; and when the space due to the velocity is greater than the distance, the mass will become gaseous. The velocity and the distance may increase together: if they do so at an equal rate, the size of the orbit will be increased, while its excentricity will remain unchanged, and the body, while continuing in the same physical condition, will expand equally in every direction, which is the case with liquids; if the velocity and distance increase, but not in the same ratio, the excentricity of the orbits will be changed as well as their size, and the body will generally expand unequally in definite directions and ultimately change its state from solid and liquid, for example.

The chief objection which may be urged to the above theory is in connexion with the liquid condition of matter. If our analogy hold, it may be said, the liquid state will be an unstable one, and a body will suddenly pass from solid to gas. The parabola being merely the limit between the ellipse and the hyperbola, no body can in reality continue to move in that form of orbit; but we know that many comets do move about the sun in ellipses and hyperbolas at very nearly the limiting state; and it may be

questioned whether external influences may not cause such bodies frequently to pass from one form to the other, and consequently through the typical parabolic orbit. And so the liquid condition of matter may perhaps be most scientifically described as "unstable." Many substances do pass immediately from solid to gas on an increase of temperature; and where this is not the case, liquids whose surfaces are in contact with gas undergo a constant evaporation, which is accelerated either by an increase of temperature or a decrease of external pressure—by anything, in short, which disturbs the equilibrium in the hyperbolic direction. In a similar manner, if the equilibrium be disturbed in the elliptic direction, the liquid will at once become solid. This is exemplified by the experiment of water reduced while in a state of perfect rest to a temperature many degrees below freezing-point: it still remains liquid till slightly agitated, when ice is suddenly formed.

During the metamorphosis of matter from solid to liquid and from liquid to gas a large amount of heat is expended. In ancient language caloric became latent; in modern phraseology it is held that this heat is consumed in conferring potential energy upon the atoms of the substance. If sensible heat is motion, latent heat is position. More definitely, according to our theory, the non-thermometric heat is expended in changing the form of the orbit from ellipse to parabola, from parabola to hyperbola.

XIV. *On Negative Fluorescence and Phosphorescence.*

By Professor C. BOHN*.

1. **I**T is well known that the phenomenon of fluorescence has been observed in the case of a great number of substances, and that it has been described by many physicists. But hitherto the law established by Professor Stokes†, according to which the refrangibility of the fluorescent rays never exceeds that of the rays which excite the fluorescence, has not been gainsaid. In fluorescence, as hitherto known, the process is generally regarded as consisting in the transformation of rays of greater refrangibility into rays of less refrangibility. In accordance with this conception, M. E. Becquerel‡ hoped to be able to detect by experiment the transformation of red rays of light into less refrangible rays of heat, but he did not succeed in doing so. In

* Translated from Poggendorff's *Annalen*, vol. cxxx. p. 367 (April 1867).

† "On the Change of Refrangibility of Light," § 80, *Phil. Trans.* 1852, and elsewhere.

‡ *Annales de Chimie et de Physique*, ser. 3. vol. lv. p. 5.

1859 M. Emsmann* suggested that it might be possible to detect cases of altered refrangibility of rays in which the change consisted in an increase, instead of in a diminution, of refrangibility. He proposed to call the already well-known phenomenon of a decrease of refrangibility "Positive Fluorescence;" and the converse phenomenon of an increase of refrangibility, which he regarded as possible or probable, he proposed to call "Negative Fluorescence." In the year 1861† he cited certain familiar facts (such as changes of colour consequent on elevation of temperature) as tending to establish the existence of this negative fluorescence; and he has recently‡ asserted that these phenomena afford an actual proof of such a transformation of rays.

Attempts have lately been made to detect experimentally the transformation of rays of heat into rays of light in accordance with negative fluorescence. Mr. Akin§ and Professor Tyndall|| consider that they have discovered and pointed out distinct phenomena of this kind. They have entered into an animated dispute, and are disagreed as to which of them was the first to make the discovery; they disagree also about the name by which the phenomenon in question ought to be denoted (Akin calls it "calcescence" and Tyndall "calorescence"), and are at one only in so far as they both consider that the phenomena of incandescence which they have either actually produced, or at least proposed to make the subjects of experiment, ought to be regarded as phenomena of the same kind as those of (positive) fluorescence—that is, as cases of "ray-transformation." In an abstract of the researches that have just been mentioned, which I gave in the Giessen "Annual Report"¶, I remarked that the transformation of rays of lower refrangibility into rays of greater refrangibility could be brought about much more simply than by the methods employed by Mr. Akin and Professor Tyndall. I mentioned that I had suspended fluor-spar freely by means of fine threads in the oven of a common house-stove, so that the mineral was exposed to the dark (slightly refrangible) rays of heat given out by the moderately heated sides of the stove, and that under these circumstances the fluor-spar, after

* Marbach's *Physikalisches Lexikon*, 2nd edit. by Cornelius, vol. vi. p. 1081.

† Poggendorff's *Annalen*, vol. cxiv. p. 651.

‡ Ibid. vol. cxxix. p. 352.

§ The Reader, Sept. 26, 1863. *Phil. Mag. S. 4.* vol. xxviii. p. 554; vol. xxix. pp. 28 & 136.

|| *Phil. Mag. S. 4.* vol. xxviii. p. 329. *N. Arch. Ph. Nat.* vol. xxii. p. 41. *Pogg. Ann.* vol. cxxiv. p. 36. *Phil. Mag. S. 4.* vol. xxix. p. 41. *Proc. Roy. Soc.* vol. xiv. p. 33. *Phil. Mag. S. 4.* vol. xxix. p. 218. *N. Arch. Ph. Nat.* vol. xxii. p. 133. *Proc. Roy. Soc.* vol. xiv. p. 476. *Phil. Mag. S. 4.* vol. xxix. p. 241. *Phil. Trans.* vol. lxx. p. 1.

¶ *Jahresbericht über die Fortschr. der Chemie*, &c. 1865, p. 80.

one or two minutes, became luminous—that is, gave out rays of higher refrangibility than those that it received. I mentioned also that when the crystal did not hang *free* in the air but was enclosed in a glass tube and put into the heated space, it did not become luminous till after about fourteen or eighteen minutes. As glass is known to exert very great absorptive action on dark rays of heat, I attributed the luminosity in this case to the heat received by the mineral through contact with the heated sides of the glass tube. I did not, however, neglect to state, on the occasion of making this communication, that the phenomenon which I had observed, as well as the phenomena of incandescence which Prof. Tyndall had produced at the focus of invisible rays of heat, and the incandescence of a platinum wire or piece of lime under the action of a hydrogen-flame, which of itself gives out almost exclusively dark rays of heat, differed from the phenomenon of fluorescence proper in a not unessential respect, inasmuch as the latter begin instantly the fluorescent substance is exposed to the exciting rays, whereas the former do not make their appearance until considerably later, in fact not until the body which exhibits them has become very hot. And I further pointed out that whereas fluorescence continues as long as the fluorescent body is exposed to the exciting rays, the phosphorescence of fluor-spar soon diminishes in intensity and at last ceases entirely, although it be continuously exposed to radiant heat.

I supposed at that time that the phosphorescence of fluor-spar would begin as soon in a space surrounded by plates of rock-salt as when it was separated from the radiating sides of the stove only by air, as was the case in one of my experiments, since rock-salt does not absorb rays of heat to a much greater extent than air does. I gave expression to this opinion (only in a letter, it is true) as early as March 1866, and suggested to a well-known physicist, who had plates of rock-salt at his disposal, to test my conclusion by experiment. So far as I am aware, this has not been done. In the meantime I have succeeded in obtaining a proper vessel of rock-salt, and have made the experiment myself. I have been thereby convinced that the phosphorescence of fluor-spar cannot be regarded in any sense as a phenomenon of negative fluorescence as understood by Emsmann (that is, as a direct transformation of rays of lower into rays of higher refrangibility), and that the experiments instituted and discussed by Messrs. Akin and Tyndall do not, any more than the facts cited by M. Emsmann, establish the fact of an increase in the refrangibility of rays*. I have become firmly

* In connexion with M. Emsmann's views, see M. Hagenbach's remarks in the Berlin Report on the Progress of Physics in 1861 (*Fortschritte d. Physik*, 1861, p. 270).

convinced that the emission of rays of greater refrangibility in the cases mentioned above was simply a consequence of the elevation of temperature which the various bodies underwent, and that it was entirely immaterial whether they were heated by dark rays or by luminous rays of heat, or by conduction, or in any other way. So far as I am aware, there is not a single phenomenon dependent on temperature in which the mode in which the temperature is raised, or the origin and particular character of the heat which causes it, exerts any recognizable influence.

In the following pages I will indicate the essential results of my experiments, and endeavour to establish the opinion I have expressed above, that no case of negative fluorescence, such as M. Emsmann originally supposed to be possible, has as yet been shown to exist.

2. I placed a piece of fluor-spar in the oven of a common stove, as I had done in my first experiments, supporting it so that it was not in conducting connexion with the stove, but received the radiation of the five black-leaded iron sides of the oven, which were heated to a point below redness, and thus gave out chiefly, if not entirely, invisible rays. Sometimes the perforated door of the oven was closed, so that a sixth heated surface radiated heat upon the crystal.

The first result was that different-sized pieces of the same crystal required to be suspended for different lengths of time in the heated space before they began to be luminous. Large pieces were always longer in becoming luminous than small pieces. A piece of from 7 to 8 cubic centims. in bulk did not become luminous even in half an hour, although its fragments very soon became luminous under the same circumstances.

If the rays underwent a change similar to that which constitutes fluorescence, it is obvious that pieces of the same substance must become luminous after being exposed for equal lengths of time to the action of the rays. Fluorescence is a process which takes place essentially at the surface of bodies; and, in the experiments that have just been described, equal surfaces were exposed to an equal number of equally intense rays. If the appearance of phosphorescence depends only upon temperature, the retarded luminosity of the larger pieces becomes very intelligible, since they are more slowly heated both superficially and throughout their mass.

3. In my next experiments I placed three pieces of the same crystal of fluor-spar, of nearly the same size and shape, in the heated space. One of them was freely suspended in the air by thin threads, the second hung freely in an enclosure constructed with plates of rock-salt, and the third in a similar envelope made of plates of glass. The "salt cell" and the "glass cell" were each

of them made of five plane-parallel plates of equal thickness, and formed three-sided prisms with square far-projecting bases. The three vertical sides were cemented on to the lower base-plate; a piece of a knitting-needle was placed at right angles across their upper ends, and from this the fragment of the crystal was hung by a thin cotton thread so that it did not touch the sides of the cell. The upper base-plate was laid on loose. Small holes were made above and below in the vertical sides so that the air could circulate through the cell; but the far-projecting terminal plates made it impossible for rays of heat emanating from the walls of the oven to reach the crystal inside without penetrating the solid substance of the envelope. The cells were supported on low tripods of thin iron wire, and the first of the three pieces of fluor-spar was hung from a support made of iron wire by a cotton thread of such length that it was in the same horizontal plane with the two pieces of crystal in the cells. The glass cell with its contents was first put in its place in the heated space, then immediately afterwards the salt cell, and lastly the piece of fluor-spar which was suspended freely in the air. I selected positions in the heated space for which the conditions as to radiation &c. were as little unequal as possible; but as small inequalities were unavoidable, the places of the several apparatus were interchanged in the various experiments. The several pieces of crystal were also interchanged, so that the one which had previously been in the salt cell came to be in the glass cell or in the free air, and *vice versa*. When the first apparatus was put in its place, the time was noted by the clock, and the seconds were counted to ascertain how much later each of the others came into its place. When the arrangement of the apparatus was completed, all the light in the room was extinguished, and the first beginning of luminosity was carefully watched for in the perfectly dark space. The time that elapsed between putting each piece of crystal in its place and the commencement of luminosity could be measured by a striking seconds-clock. I never allowed the pieces of fluor-spar which were to be used afterwards for further experiments to be phosphorescent for more than two minutes at the outside; and I endeavoured in most cases to remove them from the heated space as soon as possible after the luminosity was unmistakeably perceived.

4. If the crystals and the cells had the temperature of the room, namely about 18° C., before they were put into the heated space, the piece which was suspended without covering became luminous first, then considerably later the piece in the salt cell, and lastly, after a long additional interval, the piece in the glass cell. Thus, for example, in one experiment (in which the stove

was rather warm) phosphorescence began,

In the uncovered crystal after . . .	70 seconds.
In the crystal in the salt cell after . . .	170 „
In the crystal in the glass cell after . . .	330 „

I did not find that the times required by the several crystals in order to become phosphorescent were constant in different experiments, nor even that these times were in a constant ratio. But the uncovered crystal always became luminous first; then, after from two to three times as long an interval, the crystal in the salt cell began to shine, and that in the glass cell invariably last of all. Both cells always became very hot, so that in taking them quickly out of the heated space, in spite of a thick leathern glove, I generally burnt my fingers, and several times there was a perceptible smell of singed leather.

5. Very nearly an equal number of equally intense rays of heat are received in equal times by the uncovered piece of fluor-spar and by the piece in the salt cell. It is true that a small loss takes place owing to reflexion at the surfaces of the rock-salt plates and to absorption (8 per cent. according to the usual estimate founded on Melloni's experiments); but, on the other hand, the rock-salt itself becomes heated and radiates from a shorter distance upon the crystal, so as, no doubt, to make up for the loss. Seeing that rock-salt has very little, or almost no power of absorbing radiant heat, it gets heated chiefly by contact with the hot air. In consequence of this heating, one might be induced to believe that the crystal in the salt cell actually received *more* heat than the uncovered crystal. We must remember, however, that the radiating-power of rock-salt is excessively small, its absorbing-power being so very slight. Hence, if this phosphorescence was really an instance of negative fluorescence, the fluor-spar in the salt cell and that in the open air ought to begin to be luminous almost simultaneously. But all my observations contradict this in the most indubitable way. The crystal in the glass cell receives scarcely any rays of heat direct from the walls of the stove, since glass absorbs rays of the kind here concerned almost completely. The glass, however, becomes heated, partly by absorption and partly by contact with the hot air, and the glass plates themselves then radiate upon the crystal. The intensity of the radiation from the glass can never become equal to that from the sides of the stove, since the emissive power of glass is much less than that of graphite, with which the sides of the stove are covered; and, moreover, the temperature of the glass always remains very considerably lower than that of the stove itself. The maximum temperature of the glass, and consequently the maximum of its radiation, is certainly not

attained very quickly; and it appears, from observations to be communicated hereafter, that this maximum is not reached in $5\frac{1}{2}$ minutes (the time which elapsed, in the experiment recorded above, before phosphorescence began, while in other cases it occurred still more quickly). The radiation from the glass proceeds, however, from greater proximity than that from the walls of the stove. But it is impossible to obtain in this way any certain estimate of the quantitative ratio of the radiation upon the crystal in the glass cell as compared with that upon the uncovered crystal. It might therefore be more prudent not to insist upon the retardation of the phosphorescence in the glass cell as disproving the direct transformation of rays; but this retardation certainly does not speak *in favour* of ray-transformation. I may mention further that when the fire in the stove was kept so low that phosphorescence occurred slowly and with difficulty in the uncovered crystal and in that in the salt cell, it did not occur at all in the crystal in the glass cell, even when the latter had a more favourable position in the heated space than the others.

6. The elevation of temperature in the crystals of fluor-spar is owing to absorption of the heat radiated directly by the sides of the stove*, to contact with the continually renewed heated air, and, lastly, in the case of the pieces hanging inside the cells, to absorption of the heat radiated by the sides of the envelope. As the air can pass in and out of the cells only by a few small openings, it cannot change so easily and quickly as the air surrounding the uncovered crystal. The heating by conduction must therefore take place more slowly in the case of the crystals enclosed in the cells than in that of the one suspended in the open air. On the other hand, the heating by absorption of radiant heat must take place very nearly at the same rate in the case of this last crystal as in that of the crystal in the salt cell. If the luminosity is solely an effect of temperature, and independent of any direct influence of the radiation to which the crystals are exposed, the slower heating of the crystal in the salt cell explains why it is longer in becoming phosphorescent than the freely suspended piece. The heating due to contact with the hot air goes on at the same rate in the case of the equal-sized pieces placed in envelopes of the same size and shape; but the heating due to absorption is more rapid for the crystal in the salt cell than for that in the glass cell, inasmuch as the sides of the latter become heated and begin to radiate only gradually, whereas the rays emitted by the sides of the stove can pass through the plates of rock-salt and thus act directly upon the mineral. Hence the

* According to Melloni's experiments, a plate of fluor-spar absorbs about 60 per cent. of the invisible rays of heat.

retardation of the phosphorescence of the crystal in the glass cell might be anticipated, on my view of the phenomenon, as a necessary consequence.

7. In the experiments recorded in § 4, the fluor-spar and the cells were cold when they were put into the heated space; but I now arranged the experiments so that they were both at a high temperature before they were introduced. When one of the fragments had become luminous, I placed it with its holder *upon* the stove, where it was still kept warm, but where its luminosity soon ceased. In these new experiments the crystals began to shine much more quickly than when they were put in cold. But a more important result is that the pieces in the glass cell and in the salt cell became luminous almost simultaneously, often exactly at the same instant; and, indeed, on one occasion the piece in the glass cell began to glow before that in the salt cell. The previous heating thus did away with the retardation before observed.

8. When the crystals and the sides of the cells are already hot—already nearly as hot as they can become after equilibrium of temperature has been established between them and the oven—the further elevation of temperature is due much less to absorption of radiant heat than to contact with the heated air. The conditions of further heating are therefore equally favourable for the crystal in the salt cell and for that in the glass cell. Accordingly the observed fact that the luminosity of both crystals begins at about the same time agrees with my explanation of the phenomenon. If, in accordance with the opposite view, a direct transformation of rays takes place, the fluor-spar in the salt cell must always be ahead of the piece in the glass cell in beginning to glow, since it receives the radiation from the sides of the stove, whereas this radiation is cut off from the other piece by the glass.

9. A piece of easily phosphorescing fluor-spar was put cold into the cold salt cell and then transferred to the heated space. In two minutes the mineral had become luminous. The cold glass cell was now put into the oven, and the glowing fluor-spar was taken out of the salt cell and put into the glass cell. The luminosity ceased almost immediately, and did not begin again until after ten minutes. I then replaced the still shining crystal in the salt cell (which had cooled down in the meantime); here its luminosity ceased at first, but appeared again after $2\frac{3}{4}$ minutes.

10. If the luminosity were due directly to the rays received by the crystal, one cannot see why it should cease when the glowing crystal was transferred from the glass cell to the salt cell, since the rays of heat must have had freer access to it in the

latter than in the former. On the other hand, if elevation of temperature is the cause of the luminosity, the observed result of changing the place of the crystal is just what might have been expected. The heated mineral is cooled by radiation towards the cold sides of the little cell, whether they consist of glass or of rock-salt; and its temperature does not rise again sufficiently for the luminosity to begin again until the sides of the cell have become hot. The fact of the phosphorescence of the crystal in the glass cell having been interrupted for ten minutes proves how slowly the inner surfaces of the glass plates acquire the high temperature. Even although the temperature of the stove may have been higher in the experiments described in § 4 than during those described in § 9, the admissibility of the assertion made in § 5, to the effect that the glass plates had not attained their maximum temperature, and consequently their greatest emissive power for heat, after the lapse of $5\frac{1}{2}$ minutes, is established.

11. I transferred an already luminous piece of fluor-spar from one cell to another that was previously heated and standing in the flue of the stove. Generally no interruption of the phosphorescence occurred; a few times only there was a diminution of brilliancy. It made no difference in the result whether the crystal was changed from the glass cell to the salt cell, or *vice versa*. This observation is also very plainly in favour of the assumption that the luminosity is an affair of temperature, and that no direct transformation of rays takes place; for if it were otherwise, the phosphorescence must have been always more intense in the salt cell than in the other, in consequence of the ease with which the rays of heat could penetrate to the inside; while in the glass cell it must always have diminished, since the rays from the blackened sides of the stove could not pass through it, and the radiation from the sides of the cell itself is not, as we have already pointed out, a full substitute.

12. If a piece of fluor-spar is made to shine in the dark by heating it, either by laying it upon a hot conducting plate, or by exposing it while hanging in the badly conducting heated air to radiant heat, the brightness of the emitted light goes on for some time *increasing*; it then *diminishes*, until at last the faintest gleam disappears, although external circumstances have remained entirely unaltered. If the mineral has been very much heated, its power of becoming phosphorescent is thereby destroyed; renewed heating does not make it luminous any more. Nevertheless it is stated that the phosphorescent quality may be restored by moistening with water acidulated with hydrofluoric acid, or by the electric spark*.

* According to Placidus Heinrich, Gehler's *Physikalisches Wörterbuch*,

a certain limit, the mineral becomes luminous again under renewed heating, though perhaps not quite so easily as before. Now it has been asserted by some physicists that phosphorescence can be excited by heat only after previous exposure to light; others, on the contrary, regard any such preliminary insolation as unnecessary. M. Fiebig has briefly collected together these contradictory views*, and has arrived from experiments of his own, made with a piece of green fluor-spar, at the conclusion that phosphorescence cannot be excited by heat without the previous action of rays of light. But as the same piece of fluor-spar became repeatedly luminous, in my experiments described in §§ 9 & 11, without having been exposed to light in the intervals, I was induced to make a few special experiments in order to acquire further information as to the question of the possibility of phosphorescence by heat without previous exposure to light.

13. During a dark night I caused one and the same fragment of fluor-spar to phosphoresce forty times in succession by laying it on the moderately hot plate of a stove; and the phosphorescence was as bright and as easily produced the fortieth time as the first. No exposure to light took place at all: I sat the whole night in the dark. Every time, as soon as the luminosity had become quite distinct, I removed the piece of spar from the heated stove-plate and allowed it to cool down to the temperature of my hand on a small metal dish before making a new experiment. It continued luminous in the cold metal dish for nearly a minute on an average, and it appeared to me sometimes as though it shone, when first put into the metal dish, more brightly than it had been doing previously.

14. Two other fragments, one of them of about $1\frac{1}{2}$ cub. centim. and another smaller, were broken off the same crystal from which the piece just mentioned was taken. These likewise shone well, but I left them so long on the stove-plate that their luminosity ceased of itself. This took place in from forty to forty-five minutes. The stove had remained all the time at nearly the same temperature; and when the luminosity of these pieces came to an end, the piece above-mentioned began to glow after lying on the stove for one minute. I now allowed the pieces to cool. The stove had previously been gently heated; a fine cotton thread which was tied round the smaller piece was found, when examined the next day, unbrowned and in other respects uninjured. I now moderated the heat of the stove still more, and put in again the two pieces of fluor-spar, together with the one mentioned in § 13.

vol. vi. p. 246; Becquerel, *Traité d'Electricité*, vol. i. p. 421, according to Pearsal.

* Pogg. *Ann.* vol. cxiv. p. 292.

The last became luminous in 100 seconds; the smaller of the two which had previously spontaneously ceased shining became luminous in 160 seconds (more or less, for I was only counting by the beats of my pulse); but the larger piece did not begin to shine even after 25 minutes. By this time the smaller piece, after shining tolerably brightly, had again ceased of its own accord to be luminous. I took it out and let it cool. In the meantime the fire was increased, so that a cotton thread which was put into the heated space wound upon a knitting-needle was blackened, but still retained its continuity. The larger piece then at once began to shine, and remained splendidly luminous for more than an hour. It had not been at all exposed to light any more than the small piece; for when I removed this I found it by feeling for it with the tongs, guided by the faint gleam of a cigar. Moreover the small piece, which by this time was cold again, when put back into the heated space, soon became luminous again—that is, for the third time, after having twice ceased shining of its own accord, and without having been exposed to light in the interval. As I was trying to take out the larger piece with the tongs after its luminosity had died out, I let it slip, and immediately I remarked three small points which shone very brightly for a long time. They were small fragments separated by the slight fall of the piece which escaped from the tongs. I was able to move these luminous points about with the tongs, and one of them remained hanging on to them. This accidental observation consequently proved that, although the luminosity of a whole piece may have died out, its particles may still be capable of shining. But whether a renewal of the surface or a diminution of the mass is a necessary condition of this continued luminosity, I cannot say. After an interval of a quarter of an hour, during which the pieces had cooled down, they were replaced upon the stove-plate, which by this time was not quite so hot. The large piece soon became luminous, but the small one did not get luminous again in this way. My supply of fuel had come to an end, and I could not readily get any more; otherwise I should have been glad to have raised the temperature again a little. Just as day began to dawn in the east, the phosphorescence of the large piece had so far subsided that, although my eyes had become very sensitive, there remained only a barely perceptible glimmer; the small piece was not luminous; the piece mentioned in § 13 began in one minute to shine for the fortieth time. I now shut up all the pieces in the dark until the following night. The large piece, whose luminosity had already died out of itself three times, and which had not been again exposed to light, became luminous simultaneously with the piece which had previously been phosphorescent forty times, and with a perfectly fresh piece broken off

the large crystal, which had been exposed to light all day long. The luminosity was brilliant, and lasted about two hours. The small piece, however, did not become phosphorescent any more, even when I raised the temperature a good deal. The large piece, after ceasing spontaneously to shine four, and even five times, easily became brightly luminous again, and, indeed, the phosphorescence lasted longer each time; the temperature, however, required to be raised a little. The sixth phosphorescence did not come to an end, although I waited for it more than three hours. I now exposed the small piece to the light of a stearine candle, and then put it upon the stove-plate, but still it did not shine. I next exposed it for half an hour while cold to the light of a petroleum lamp, which, as is well known, is not very poor in the most refrangible rays; but even this treatment did not confer upon it the capability of again becoming phosphorescent. Exposure to the light of burning magnesium, which is very rich in the most refrangible rays, produced no greater effect. Lastly, I exposed the small piece for a whole day to diffused sunlight, and to a small extent to the direct rays of the sun, and tried again the following night to cause it to phosphoresce. Still it did not do so; whereas the large piece, which had not been exposed to light, easily became luminous. After the small piece had lain for two days more exposed to light, I examined it again. Placed on a plate, the temperature of which I gradually raised very considerably, it at last became luminous, but the light was very weak and soon died away. The power of becoming phosphorescent had accordingly been restored, though only to a small extent; but this was certainly due, not to the insolation, but to the long rest. The larger piece, which had not been exposed to light, shone much more strongly upon a plate that was not so much heated, and continued shining as long as I had patience to watch it. The piece that I had made phosphorescent forty times during the first night, and which was always removed from the hot plate immediately it had become luminous and carefully protected from light, I caused to become luminous by heat some dozens of times more during the second night, as well as several times during the third night. I could not perceive that its readiness to become luminous was in the least degree diminished. The small piece had evidently at one time been *too hot*, and its capacity of becoming phosphorescent had been thereby impaired. In other respects it remained externally unchanged, and, in particular, it still retained its yellow colour. From these observations I may therefore conclude that yellow fluor-spar can become phosphorescent by the action of heat *alone*, without any necessity for its previous exposure to light. Observations made by Von Grotthuss on a reddish-violet fluor-spar from Nertschink (pyrosmaragdite or

chlorophane) lead to the same conclusion. He found that this mineral could be rendered luminous by a very gentle heat after having been kept in the dark for months together. He kept one piece, that had been previously insolated, in the dark, and examined it from time to time by night. On the thirty-sixth night after the exposure to light, a faint luminosity (*Lichtschimmer*) was excited by a temperature of 40° R. On previous nights, even so high a temperature as this was not needed; the heat of the hand was sufficient. Von Grotthuss surprisingly concludes "that chlorophane, as well as other phosphorescent minerals, never become luminous except as a consequence of previous exposure to light, or, in other words, that during phosphorescence only that light is developed from them which they had absorbed during previous exposure to light at some time or other, and which had been incorporated with their ponderable matter"*.

I should have thought that, as the chlorophane received no new supply by further exposure, the store of "absorbed light" must have been exhausted by the thirty-sixth night, though this cannot have been the case, since the phosphorescence still occurred. In my experiments the larger piece was luminous altogether for nearly ten hours, for the most part shining very brightly without any fresh exposure to light. This long-continued and abundant emission of light would have exhausted a very considerable store, but yet I could not detect any diminution of capacity; at most a somewhat higher temperature was required in order to evoke the luminosity when only a short interval was allowed to elapse between two experiments; but even this difference disappeared if the mineral was left longer in repose: it then became luminous at the same time as a piece freshly broken off which had been exposed to light only a short time before.

I never heated the pieces of fluor-spar suspended in the cells so strongly as to cause an alteration of their properties, nor did I ever allow them to remain luminous long enough for their luminosity to die out of itself while the external conditions remained the same.

15. The stove made use of in this experiment has two ovens, placed one above the other like two stories. The bottom plate of the upper one is 31 centims. above that of the lower one, and the top plate of the whole stove is at the same distance above the bottom plate of the upper oven. This latter was used as the heated space in the experiments described in §§ 2, 4, 7, 9, and 11; and in general the fire was so regulated that the bottom plate of the lower, unused oven was slightly red-hot. With the

* Gehler's *Physikalisches Wörterbuch*, vol. vi. p. 252, quoted from Schweigger's *Journal*, vol. xiv. p. 154, and vol. vi. p. 172.

intention of bringing the top plate of the stove to about the same temperature as the bottom plate of the space into which the pieces of fluor-spar with their cells were introduced had previously possessed, I now increased the fire so much that the bottom plate of the upper oven became slightly red-hot. I then placed the crystals with their cells and the same tripods *upon* the stove. Here they did *not* become luminous, not even when I made the fire still stronger. Only a single mineral, a white fluor-spar with brownish-black specks, and with more of a fatty than the true vitreous lustre, which phosphoresced more easily than any of the other specimens that I examined—more easily even than a chlorophane belonging to the mineralogical collection of the University of Giessen, with which I had previously experimented—only this one piece became luminous when placed *upon* the stove, hanging freely in the air (not in a cell); and even it required to be brought within a quarter of an inch of the hot stove-plate. If the crystals had been so long inside the oven, where they received the radiation from five surfaces, as to have become luminous and were then placed *upon* the stove, they continued luminous for some time—as much as $2\frac{1}{2}$ minutes.

16. In these experiments the under surface of the crystal was exposed to just the same radiation as when it was inside the oven; but the other surfaces of the fluor-spar were there also exposed to strong radiation. If the phosphorescence consisted in a direct transformation of rays, and if it be further assumed that the radiation must attain a certain degree of intensity in order that the phenomenon may be perceptible, the crystals ought in the last arrangement to become luminous, at least, at their under surfaces. But as this was not the case, this phenomenon is evidence *against* the analogy of phosphorescence and fluorescence. On the other hand, if elevation of temperature is the only exciting cause of luminosity, this result cannot be expected to occur when the crystals are placed *upon* the stove; for in this case they are far from being as hot as when they are inside the oven. For, when placed *upon* the stove, the crystals radiate in all directions, except straight downwards, against relatively cold surfaces, and thus lose in these directions more heat than they receive. Moreover they are exposed to a current of air which ascends without any obstacle, and does not acquire an equally high temperature with the more confined air in the oven. The observation that pieces which are already hot enough to be luminous continue to shine for a short time when placed upon the stove where they can only cool slowly, is in perfect accordance with my view of the nature of this phosphorescence.

17. A thin rusty iron dish was placed upon a tripod in the oven, and a polished flat-iron upon another. When these had

remained an hour in the heated space, so that equilibrium of temperature had been reached, I threw small fragments of fluor-spar upon the dish and upon the flat-iron, and shaded them by means of a pill-box-lid from the heat radiated by the sides of the stove. In from one minute to one and a half, the fluor-spar upon the flat-iron began to be luminous; but the fragments upon the dish had not become luminous in five minutes, after which I was obliged to interrupt the experiment. I wanted to ascertain directly by means of this arrangement whether fluor-spar was capable of becoming phosphorescent at the temperature which bodies attain inside the oven, when they are not to any important extent in conducting communication with the sides, without being exposed to direct radiation. The experiment with the flat-iron answers this question in the affirmative, and that with the iron dish gives only an apparent negative; for the bright flat-iron, with its greater mass, attains, chiefly through contact with the hot air, a higher temperature than the small dish with its large surface, rendered a good radiator by its covering of rust. If I raised the temperature of the dish a little by laying it for a short time directly upon the hot stove-plate, the little pieces of fluor-spar which it contained at once became luminous.

18. Large pieces of fluor-spar were brought to a state of very vivid phosphorescence by being laid directly upon the hot stove-plate. When taken off and laid upon any cold support, they continue to shine for a longer or shorter time, until, as I suppose, their temperature has fallen below a certain limit. If the brightly shining pieces are thrown into cold water, their light is extinguished instantly.

19. According to M. Emsmann*, the difference between phosphorescence and fluorescence is analogous to the difference between the magnetic behaviour of steel and iron. Rays of light are supposed to exert an influence upon the arrangement of the atoms, and this influence is supposed to be capable of becoming strong enough to produce chemical action. This idea is due to Draper. The atomic motion which is superinduced by exposure to light is further supposed to give rise to vibrations of the æther and of the molecules of the substance, the result of which is the luminosity exhibited. Each substance is supposed to be endowed with a certain *coercive force*, which tends to keep its atoms in the particular arrangement which corresponds to the circumstances under which it is placed. In phosphorescent bodies this coercive force is regarded as strong, and in fluorescent bodies as weak. It is thus explained why phosphorescence does not begin until the exciting rays have acted for some time, and also why it continues after these rays have ceased to act. The experiment

* Poggendorff's *Annalen*, vol. cxiv. p. 654.

described in § 18, which most likely has been long ago made by others, is contrary to the assumption that any such coercive force exists. The fact that the intensity of the phosphorescence gradually decreases and at last becomes nothing, although the external conditions remain unchanged, and can be renewed only after a certain interval of repose, also speaks against M. Emsmann's view. Moreover the production of light at all, without the necessity for exposing the phosphorescent bodies to light, is opposed to a literal acceptance of M. Emsmann's view, although the statement of this view might easily be modified so as to be in harmony with this fact. But the single circumstance that many substances, including fluor-spar itself, are both fluorescent and phosphorescent, and therefore, according to this theory, possess at the same time a weak and a strong coercive force, is sufficient to show its inadmissibility.

20. We may still mention some phenomena of phosphorescence in which no coercive force is manifested. If a piece of fluor-spar is drawn along a hot plate without much pressure, a luminous streak appears *immediately*, and disappears again after some time, just as when a piece of phosphorus is drawn along the surface of a plate at a lower temperature. Chalk-marks also, when they are made upon a hot plate, show a phosphorescent light, as was first pointed out by Mr. Napier*. If a piece of fluor-spar is allowed to fall, even from a very small elevation, upon a hot plate, there is an immediate burst of light, which generally dies away again very quickly, especially if the temperature of the plate is rather high. It is evident that when a streak is made with fluor-spar, or when it is let fall, very minute particles are detached from it, which, in consequence of their smallness, rapidly acquire the temperature necessary to make them luminous. To this category also belongs a very beautiful phenomenon which I have repeatedly had occasion to observe. If a cold piece of fluor-spar is put into a heated oven, either freely suspended or laid upon the bottom, splendid luminous points not unfrequently shoot out like lightning from the crystal very soon after it is put in, and generally disappear again very quickly. This is a decrepitation of the crystal; small particles break away and then comport themselves like those separated by friction or by a blow. If coarsely powdered fluor-spar is thrown upon the hot stove-plate, the decrepitation is very brisk; there is a distinctly audible crackling, and small brightly luminous particles are scattered around.

21. The more attention I devote to the phosphorescence of fluor-spar, the more strongly am I impressed with the idea of some kind of chemical process taking place at the surface, pos-

* Phil. Mag. S. 4. vol. i. p. 432.

sibly a combustion, accompanied, like the slow oxidation of phosphorus, with evolution of light. The gradual extinction of the light would, upon such a view, have to be regarded as an exhaustion of the combustible material; and in the interval of repose needed for the reacquisition of the phosphorescent power, a new supply of this material might collect at a low temperature upon the surface, either by deposition from the air or from the interior of the mass of fluor-spar. I do not, however, wish to be understood as putting this forward as more than a possible suggestion.

22. On the other hand, I feel fully justified by the preceding observations in denying the analogy between phosphorescence and fluorescence. But if once the notion that the phosphorescence of fluor-spar is a direct transformation of rays of one kind into rays of greater refrangibility is given up or refuted, it is impossible to regard the phenomena of incandescence produced by Messrs. Akin and Tyndall as phenomena of the same kind as fluorescence.

Platinum wire becomes incandescent not only in a charcoal fire, or by the action of a galvanic current, or in the flame of a spirit-lamp, but also in the flame of hydrogen. The same applies to lime. Metals become incandescent and melt, and paper burns, if a certain temperature is reached, however the heat needed to cause the rise of temperature is produced.

Such, in my opinion, is the significance of the experiments of Messrs. Akin and Tyndall. The idea of negative fluorescence in their case is out of the question. If water is heated to the boiling-point, the properties of the hot water and of the steam are exactly the same, whether the heat requisite for this purpose was produced by the combustion of any substance whatever, or by the discharge of electricity, or by the transformation of mechanical work. It is impossible for any one to detect in pure warm water how and where it has got its heat—whether from a common fire or from the sun, in an iron boiler or in a porcelain dish, or inside a volcano—whether it was previously hotter or colder—whether it is condensed steam or melted ice. At a certain temperature chemical compounds are formed, and at another they are decomposed; the bulk and state of aggregation of bodies depends upon their temperature, other circumstances being always supposed constant; temperature also determines the kind and intensity of radiant heat, as well as incandescence and many other properties. But it is only the *temperature* which comes into account, never the origin of the heat which has produced this temperature. The manifold and great differences exhibited by different kinds of radiant heat disappear as soon as the heat passes from the dynamical into the statical condition (if I may be

allowed to make use of a mode of expression which is becoming somewhat obsolete). This statement makes no claim to novelty; I have myself made it as a passing remark a good while ago*. The phenomena of incandescence at the focus of pure rays of heat, rays of light and probably rays of heat of high refrangibility being excluded, amount to no more than a new argument in support of this old truth.

23. It is possible to give theoretical reasons in *favour* of the existence of a negative fluorescence—that is to say, of a process which *increases* the refrangibility of rays in the same way as it is *diminished* in the phenomena of fluorescence as hitherto observed.

Professor Stokes assumes that the vibrations of the æther which constitute the exciting light produce oscillations in the molecules of the fluorescent body which are not infinitely small; and hence it is concluded that their periods are longer than that of the exciting light. And as molecular movements give rise to vibrations of the æther of equal period with themselves, the dispersed light comes to be of less refrangibility than the exciting rays.

Professor Ångström makes the same assumption as Professor Stokes, but draws from the circumstance of the resulting molecular movements not being infinitely small the opposite conclusion†—namely, that oscillations of a higher order (that is, *the octave*) are thereby imparted to the æther. This would consequently come to be an increase of refrangibility, or a negative fluorescence.

M. W. Eisenlohr‡ considers that fluorescence is produced by the interference of the bluish-violet and ultra-violet rays. This interference is supposed to give rise to light of smaller refrangibility, just as combination-tones are of lower pitch than the two interfering tones. But we must remember, in opposition to this view, that, according to it, fluorescence could never be observed in a pure spectrum, since there rays of only *one* degree of refrangibility are to be found at the same place.

M. Lommel§ has adopted a combination of the theories of Stokes and Eisenlohr. The exciting rays are supposed to throw the particles of the body exposed to them into oscillations the rate of which depends upon the nature of the body. The rays emitted by the body in consequence of this action are supposed, like the rays given out by most luminous or radiating bodies, to be in general not homogeneous, but to consist of elementary rays

* Henle and Pfeufer's *Zeitschrift für rationelle Medicin*, ser. 3. vol. viii. p. 234.

† Pogg. *Ann.* vol. xciv. p. 162.

‡ *Ibid.* vol. xciii. p. 623.

§ *Ibid.* vol. xcvi. p. 642.

of various wave-lengths. Now, if the period of oscillation of the resulting rays is not too great for them to be perceptible by the eye, the body must phosphoresce; but if it is too great, the rays, which of themselves are invisible, might during the period of exposure combine with the impinging rays (of course after these have been reflected), and combination-colours might be so produced, which would be directly cognizable by the eye. The rate of vibration of the combination-ray (this is expressly stated) is supposed equal to the difference of the rates of vibration of the two conspiring rays.

24. If we are to accept the theoretical conceptions of M. Eisenlohr and, particularly, those of M. Lommel, the possibility, or rather the necessity, of an increase of refrangibility—that is, of a *negative fluorescence*, at the same time as the positive fluorescence, would be thereby conceded. The two physicists just named speak only of those combination-vibrations which were earliest discovered in acoustics, and are often spoken of as Tartini's combination-tones. Professor Helmholtz*, however, has shown that in addition to these deeper combination-tones, or "difference-tones," there are also higher combination-tones, "summation-tones," whose rate of vibration is the sum of the rates of vibration of the primary tones. If we extend this discovery to the oscillations of the æther, a ray of much greater refrangibility may be generated by the conjoint action of two rays of smaller refrangibility. For instance, from two ultra-red rays of but slightly different refrangibilities, light of about half the time of vibration (that is, indigo or violet light) might be produced. If vibrations of various kinds are produced in the molecules of the fluorescent body by the action of the exciting rays, but all or most of them of long period, it is not only possible but necessary that, if these vibrations are ultra-red and therefore of themselves invisible, visible light should be generated, even without the aid of the reflected rays, by the production of "summation-vibrations."

25. The experimental proof of negative fluorescence can be certainly given only by bringing some appropriate substance successively into the various parts of a very pure spectrum and carefully analyzing the resulting fluorescent light. Chlorophyll, guaiacum, and some other substances exhibit fluorescence even when exposed to light of low refrangibility, as the yellow or orange of the spectrum. I am not aware whether the red fluorescent light of chlorophyll and the so-called violet light of guaiacum have been accurately analyzed, attention being specially directed to ascertain whether this compound light does not contain rays of greater refrangibility than those constituting the

* Poggendorff's *Annalen*, vol. xcix. p. 497 (518).

exciting light. If I understand correctly Professor Stokes's statements concerning solution of guaiacum*, they already establish the existence of negative fluorescence; for the fluorescent light is said to be violet; it must hence contain rays of great refrangibility, while the fluorescence is described as beginning between Fraunhofer's lines D and b.

Aschaffenburg, February 1867.

XV. *Remarks on the Change in the Obliquity of the Ecliptic, and its Influence on Climate.* By JAMES CROLL †.

IN the Supplementary Number of the Philosophical Magazine for last month Mr. John Carrick Moore objects to the result stated in my paper on the Obliquity of the Ecliptic, that when the obliquity was at its maximum the annual amount of solar heat received by the poles was $\frac{1}{18}$ greater than at present. He questions the correctness of this fraction, on the ground that I have underestimated the amount of heat absorbed by the atmosphere in polar regions. But assuming that I have done so, how can this affect the result in question? Whether we suppose the quantity absorbed to be equal to 22 per cent. or to 99 per cent., or to nothing at all, the result in each case must be the same. For it holds true that when the obliquity was at its maximum, the poles would be receiving nineteen rays for every eighteen that they are presently receiving, however much those rays may have been weakened by absorption. And if so, then the quantity reaching the poles would be $\frac{1}{18}$ greater than at present.

But it is incorrect to say that I did not take into account the fact that the quantity of heat absorbed at the poles is greater than at the equator. (See page 434.) In page 442 I have shown that, supposing 75 per cent. of the sun's heat were cut off by the atmosphere, still the extra heat reaching the poles when the obliquity was at its maximum would be sufficient to melt a sheet of ice 470 feet thick in about 640 years.

Mr. Meech does not give the relative quantities of heat at the equator and poles when the obliquity is at its maximum, but, what is better, he gives us formulæ from which those quantities may be calculated for any amount of obliquity.

Mr. Moore says that the researches of Melloni and Tyndall show that "if a ray is made to pass through a number of transparent plates of the same substance and thickness, the proportion of the rays transmitted through the first plate is greater

* I have here consulted the *Fortschritte der Physik* for 1852, p. 263.

† Communicated by the Author.

than the proportion of those which, having emerged through the first plate, can pass through the second." I have always understood that those researches prove the very reverse, viz. that the percentage of heat transmitted through the first plate is *less* than that through the second.

I presume it may now be regarded as established that when a beam of light or of heat passes through any substance, those rays are cut off which agree in *period* with those of the molecules of the substance through which the beam passes, and that the principal part of the absorption takes place on the beam entering the substance, and that, when all the rays which agree in period are cut off (when the beam is "sifted"), those rays which remain pass onwards with but little obstruction. Then, if this beautiful theory be correct, it necessarily follows that all Tables calculated upon the principle that the amount of the sun's heat cut off in passing through the atmosphere is proportionate to the number of aërial particles that the rays have to encounter before reaching the surface of the earth must be "wholly erroneous."

But supposing it were the case that the annual amount of heat reaching the poles to that reaching the equator is actually no more than as 1 to 8, so much the better is it for my argument; for it would show that it is to ocean-currents, and not to the direct heat of the sun, that the polar regions are chiefly indebted for their temperature. If the absolute quantity of the sun's heat reaching the poles be only $\frac{1}{8}$ of that reaching the equator, then why is there only a difference of about 80° between the temperature of the poles and that of the equator? According to Mr. Moore's own calculations (Lyell's 'Principles,' vol. i. p. 294, new edition) there ought to be a difference of from 200° to 300° . Why, then, is not the mean annual temperature of the poles somewhere between 100° and 200° below zero, instead of at zero as at present? This comparatively high temperature of the polar regions must in such a case be chiefly attributed to the influence of ocean-currents; and if so, then to what an enormous extent would the climate of the arctic regions be affected by even a very slight deflection of the equatorial current of the Atlantic into the Southern Ocean, occasioned by a change in the excentricity of the earth's orbit.

XVI. *Remarks on Sir Benjamin Brodie's System of Chemical Notation.* By ALEXANDER CRUM BROWN, M.D., D.Sc.*

THE System of Chemical Notation introduced by Sir Benjamin Brodie, in his paper "On the Calculus of Chemical Operations," is so interesting from its apparent novelty, and from the great ingenuity its author has shown in its construction, that I hope the following remarks upon it may not be considered out of place.

In the introduction, the author is, I think, unnecessarily severe in his remarks upon those chemists who use atomic or even graphic formulæ. While there can be no doubt that *physical* research points to a molecular constitution of matter, it is perfectly indifferent to a *chemist* whether his symbols represent atoms or units; and graphic formulæ would be as useful as they now are, were it conclusively proved that matter is continuous.

In proceeding to the examination of the notation itself, it is necessary to observe, in the first place, that it is a system for expressing by a formula (1) the weight of a unit (or as most chemists would call it, a molecule) of a homogeneous chemical substance, and (2) the composition of the substance. It is therefore, as far as the author has developed it in his first paper, a system of empirical formulæ.

In Sections I.-VI. inclusive the author lays down the general principles applicable to the formation of such a system. These principles apply as well to the system at present in use as to that proposed (for H, O, N, C, &c. may be called prime factors as well as atoms), and one great benefit which we may expect from the publication of Sir Benjamin's paper is a greater consistency and uniformity in the use of symbols.

The assumption (as far as I can see quite unnecessary) of the distributive law of multiplication leads to some mathematical difficulties: thus $y + y_1 = yy_1$; but $x(y + y_1)$ is not equal to xyy_1 , for the former is $xy + xy_1 = x^2yy_1$. Now we know no analytical system in which $a = b$ does not imply $fa = fb$, f being a functional operator, unless f has more than one value (as $\sqrt{\quad}$, \sin^{-1} , &c.); and this is plainly not the case here.

In Section III. the author gives three modes in which the chemical symbol l may originate. I have some difficulty in seeing how these differ from one another. The first is $xy = x$, where y is the symbol of no weight; the second is $x^0 = 1$; and the third $\frac{x}{x} = 1$. Now the third seems to include the other two.

* Communicated by the Author.

For if $xy=x$ implies $y=1$, this can only be because $y = \frac{x}{x}$ and $\frac{x}{x}=1$. In a symbolical system in which this is not the case we cannot deduce $y=1$ from $xy=x$. Thus in the logical system of the late Professor Boole, $xy=x$ does not lead to $y=1$, but to $y = x + \frac{0}{0}(1-x)$.

The second case is $x^0=1$. As we can have no fractional indices, 0 cannot be defined as a limit; its only definition can be $n-n=0$; but here $x^0 = x^{n-n} = \frac{x^n}{x^n} = 1$, again the same as the third form of the symbol 1.

The subject of the numerical figures 0, 1, 2, 3, &c., considered as *chemical* symbols, is scarcely treated so fully as to make the author's meaning quite clear; judging from the context, I understand 0' to represent no space, 1' one litre of space, 2' two litres of space, and so on; and as the distributive law has been assumed, and as the author expressly states that the chemical and arithmetical symbols 0 and 1 are interchangeable, I understand that $x \cdot 0' = 0$, $x \cdot 1' = x$, $x \cdot 2' = x(1' + 1') = 2x$; that is, $x + x$ or two litres of x ; and so on. But if this be the case, the only practical use of the *chemical* symbols 2, 3, &c. is lost. For if we should be forced to admit units of substances occupying twice the volume of a unit of hydrogen (as we *may* be in the case of nitric oxide), we shall be unable to do so without fractional indices; whereas if $x \cdot 2'$ were interpreted to mean one unit weight of x put into two litres of space (at standard temperature and pressure), we might represent nitric oxide as a function of the chemical symbol 2'. This seems to me another disadvantage of the assumption of the distributive law.

In Section VII. the peculiarities of the author's system of notation make their appearance. We have, first, two assumptions, expressed in the words "The units of chemical substances . . . are integral compound weights." The two assumptions contained in this expression are, (1) that the unit symbol of a chemical substance never contains as a factor a *negative* power of a prime factor, and (2) that it never contains a *fractional* power. Both of these assumptions are common to this system and to that in ordinary use. The essential difference between the two systems lies in a third assumption. The author assumes that "the unit of hydrogen is a simple weight," or is to be expressed by a single prime factor. In the ordinary system the corresponding assumption is that the hitherto undecomposed substances (the so-called elements) are, to use the language of the author, simple with regard to one another, or that their sym-

bols are prime to one another. If we put this in place of the author's fundamental hypothesis and adopt all the rest of his reasoning, we shall arrive at a notation substantially the same as that which we now use.

This will be most distinctly seen by examining, first, those symbols in the determination of which the question of the divisibility of the hydrogen unit does not arise; and secondly, those in which it does.

Of the first kind we have the symbols of oxygen, sulphur, selenium, mercury, carbon, silicon, tin, zinc, and cadmium.

Of the second, we have hydrogen, chlorine, iodine, bromine, nitrogen, phosphorus, arsenic, boron, antimony, and bismuth. Silver, according to one hypothesis, belongs to the one set, and according to another, to the other.

In the first set we have uniformly

$$\begin{aligned} \text{O} &= 2 \cdot \xi, & \text{S} &= 2 \cdot \theta, & \text{Se} &= 2 \cdot \lambda, & \text{Hg} &= 2 \cdot \delta, & \text{C} &= 2 \cdot \kappa, \\ \text{Si} &= 2 \cdot \sigma, & \text{Sn} &= 2 \cdot \kappa, & \text{Zn} &= 2 \cdot \zeta, & \text{Cd} &= 2 \cdot \kappa_2. \end{aligned}$$

That is, there is only a difference of unit, but no difference of relative value. So far for the atoms or "prime factors." When we turn to the molecules or "units" we find the same thing. Oxygen, sulphur, and selenium are ξ^2 , θ^2 , and λ^2 , just as we have O^2 , S^2 , and Se^2 ; while zinc and cadmium are ζ and κ_2 , as we have Zn and Cd. The units of carbon, silicon, and tin are undetermined, and so are their molecules. In all of these cases the author has proceeded on the assumption that each element contains one peculiar prime factor not to be found in any other element; and as his fundamental hypothesis, the indivisibility of the hydrogen unit, does not affect his reasoning, he does not assume that they contain any other prime factor. His assumption is therefore here the same as ours; and his reasoning would have been the very same as it is, as far as the determination of the units and prime factors is concerned, had he taken hydrogen as α^n , n being any number whatever.

It is quite different when we come to the second class of elements. Taking chlorine as an instance, we have the equation, (unit of hydrogen)(unit of chlorine) = (unit of hydrochloric acid)². The first term of the equation must therefore be a square. This can only be the case (1) if (unit of hydrogen) and (unit of chlorine) be both squares, which is contrary to the author's assumption,—or (2) if (unit of chlorine) be a product of an odd power of (unit of hydrogen) and another factor which is a square; and this is contrary to the common assumption. Here, therefore, we part company: we adopt the simplest form of the first hypothesis and make (unit of hydrogen) = H^2 , and (unit of chlorine) = Cl^2 ;

the author adopts the simplest form of the second hypothesis, and makes (unit of chlorine) = $\alpha\chi^2$.

The same divergence of the results obtained under the two assumptions occurs in all the other members of this class. Thus

$$(\text{unit of nitrogen})(\text{unit of hydrogen})^3 = (\text{unit of ammonia})^2$$

in our system both of the factors of the first term are squares N^2 and H^6 . In the author's system the second factor cannot be a square, and therefore, as in the case of chlorine (unit of nitrogen), must contain as factors a square and an odd power of (unit of hydrogen); and here, as before, he takes the simplest form and makes (unit of nitrogen) = $\alpha\nu^2*$. In all the members of the second class we have:—

$$\begin{aligned} \text{H} &= 2 \cdot \sqrt{\alpha}, & \text{Cl} &= 2 \cdot \chi \sqrt{\alpha}, & \text{I} &= 2 \cdot \omega \sqrt{\alpha}, \\ \text{Br} &= 2 \cdot \beta \sqrt{\alpha}, & \text{N} &= 2 \cdot \nu \sqrt{\alpha}, & \text{P} &= 2 \cdot \phi \sqrt{\alpha}, \\ \text{As} &= 2 \cdot \rho \sqrt{\alpha}, & \text{B} &= 2 \cdot \beta_1 \sqrt{\alpha}, & \text{Sb} &= 2 \cdot \sigma \sqrt{\alpha}, \\ \text{Bi} &= 2 \cdot \beta_2 \sqrt{\alpha}. \end{aligned}$$

(In the case of silver we have two possible hypotheses—oxide of silver, AgO or Ag^2O ; according to the one we have $\text{Ag} = 2 \cdot \rho_1$, according to the other $\text{Ag} = 2 \cdot \rho_2 \sqrt{\alpha}$.)

Here again we have a difference of unit; reducing therefore to the same unit (either $w(\alpha) = 1$ and $\text{H} = 0.5$, or $w(\alpha) = 2$ and $\text{H} = 1$), we have, taking ϵ and E as general symbols of prime factors and atoms of the first class, and μ and M of the second (excepting hydrogen),

$$\text{E} = \epsilon,$$

and

$$\text{H} = \sqrt{\alpha}, \text{ and } \text{M} = \mu \sqrt{\alpha}.$$

It will further be observed that all the elements in the first class are artiad, all in the second perissad. So that it is true, as the author remarks, that this system falls with the law of even numbers; for if we have an odd number of perissads, we have an odd power of $\sqrt{\alpha}$.

As every perissad atom is the product of *one* peculiar prime factor and $\sqrt{\alpha}$, and as α does not occur in any other form (ex-

* There is a third and more general solution of these equations, which would give us as the symbol for hydrogen $\alpha\nu^2$, for chlorine $\alpha\chi^2$, for nitrogen $\alpha\nu^2$, &c. To determine the weight of the prime factors α , ν , χ , ν , &c., we have the equations $w(\alpha\nu^2) = 1$, $w(\alpha\chi^2) = 35.5$, $w(\alpha\nu^2) = 14$, &c.; that is, n equations to determine $n+1$ unknown quantities. Either, therefore, they must remain undetermined, or we must arbitrarily assume another equation. Sir Benjamin assumes $\nu = 1$, or $w(\nu) = 0$, and therefore unit of hydrogen = α ; we assume $\alpha = 1$, or $w(\alpha) = 0$, and therefore unit of hydrogen = ν^2 .

cept in hydrogen, where H is the product of 1 and $\sqrt{\alpha}$, it is obvious that twice the index of α in a unit symbol is the number of perissad atoms in the molecule. From this we can easily frame rules for translation.

1st. To translate from the ordinary notation into that of the calculus.

Let p be the number of perissad atoms in the molecule, then $\frac{p}{2}$ is the index of α in the unit symbol; then change the symbols for the atoms (exclusive of H) into the symbols for the corresponding prime factors (O^m into ξ^m , Cl^n into χ^n , C^r into κ^r &c.).

Examples.

Mercuric perchlorate, $HgCl^2 O^8$.

$p=2$, therefore index of $\alpha=1$, Symbol $\alpha\delta\chi^2\xi^8$.

Glycerine, $C^3 H^8 O^3$.

$p=8$, index of $\alpha=4$, Symbol $\alpha^4\kappa^3\xi^3$.

Cyanide of kakodyl, $As NC^3 H^6$.

$p=8$, index of $\alpha=4$, Symbol $\alpha^4\rho\nu\kappa^3$.

Sulphuric anhydride, SO^3 .

$p=0$, index of $\alpha=0$, Symbol $\theta\xi^3$.

Nitric oxide, NO .

$p=1$, index of $\alpha=\frac{1}{2}$, Symbol $\alpha^{\frac{1}{2}}\nu\xi^*$.

2nd. To translate from the system of the calculus into the ordinary notation.

Let r be the index of α , and q the sum of the indices of all the other prime factors corresponding to perissad atoms, then $2r-q$ is the number of H atoms. For the other elements, change the symbols for prime factors into the corresponding atomic symbols, retaining the index of each.

Examples.

Trichloroacetic acid, $\alpha^2\chi^3\kappa^2\xi^2$.

$r=2$, $q=3$, $2r-q=1$, Formula $HCl^3 C^2 O^2$.

Iodide of phosphotetreylium, $\alpha^{11}\omega\phi\kappa^8$.

$r=11$, $q=2$, $2r-q=20$, Formula $H^{20} I P C^8$.

Nitrate of ammonium, $\alpha^3\nu^2\xi^3$.

$r=3$, $q=2$, $2r-q=4$, Formula $H^4 N^2 O^3$.

Oxychloride of phosphorus, $\alpha^2\phi\chi^3\xi$.

$r=2$, $q=4$, $2r-q=0$, Formula $PCl^3 O$.

* Exceptional case, page 854.

Having now seen in what the two systems agree (or may by a very slight accommodation of terminology be made to agree) and in what they differ, we are in a position to estimate their relative advantages. We have seen that they are equally consistent when properly used, and equally capable of mathematical treatment (for the first six sections of the calculus can be applied to either). It is true that our system has an atomic origin and retains traces of this in its terminology; but it may be used, and has often been used, without any reference to any physical theory of matter, just as Sir Benjamin's system might have an atomic theory tacked on to it.

In comparing the two systems of notation I shall examine (1) the reasonableness of the fundamental hypotheses; (2) the amount of change of formulæ which would be involved in a change of the fundamental hypothesis; and (3) the convenience of the formulæ themselves. While greatly admiring the elegance, simplicity, and consistency of Sir Benjamin's system, I feel compelled to decide against it on all these points.

1. The fundamental hypothesis of the ordinary system is merely the statement of an experimental fact, namely, that we have not as yet been able to prove that any of the so-called elements contains a component common to it and to any other element. While this is no doubt a negative conclusion expressing merely our ignorance, it is surely better in the meantime to frame our notation consistently with it than to start from an assumption so purely arbitrary as that the unit of hydrogen is indivisible. Had we no choice but to select one element, and, without considering consequences, fix its symbol, the author's assumption would probably commend itself to us as the most convenient; but we have seen that we are not tied down to such a selection.

2. Should it be hereafter proved that some of the elements do contain common parts, the change in the case of the ordinary notation would be a simple one. If, for instance, it were proved, as many chemists have supposed, and as Sir Benjamin's brilliant researches on the organic peroxides have rendered it not unreasonable to suppose, that chlorine is the peroxide of murium, Cl would merely become MuO, and from being an atom would become a compound radical; and just as we now often write Me for CH^3 , or Bz for $\text{C}^7\text{H}^5\text{O}$, so we should, no doubt, often use Cl instead of MuO. In the same way, if chlorine were shown to contain hydrogen and to be analogous in composition, as it certainly is in some of its chemical relations, to the peroxide of hydrogen, we could express this relation by writing $\text{Cl}=\text{HX}$, and $\text{HCl}=\text{H}^2\text{X}$, and we should then have on a basis of fact the two analogous series H^2 , H^2X , H^2X^2 (hydrogen, hydrochloric acid,

chlorine), and H^2 , $H^2 O$, $H^2 O^2$ (hydrogen, water, peroxide of hydrogen), which the author now gives us on the basis of hypothesis. But if *hydrogen* were proved to be compound, a very large number of the author's formulæ would require to be essentially changed, and the meaning and weight of many of the prime factors would be affected. If, for instance, hydrogen were decomposed into two simpler substances, so that $\alpha = \eta\pi^2$, chlorine would become $\eta\chi_1^2$ instead of $\alpha\chi^2$, and χ_1 would not have the same weight as χ . The same change would require to be made on the symbols of all the elements of the form $\alpha\mu^2$. From this we see that the *stability* of the present system is greater than that of the system proposed.

3. The present formulæ express the composition of substances as far as that is known to us. Thus when we see C or H in a formula, we know that the substance represented will give us carbonic acid or water if subjected to suitable methods of oxidation; and we also know from the formula how much of each it will yield. When we see κ in a symbol, we also know that the corresponding substance will yield carbonic acid, but the presence of α does not, without an arithmetical operation, give us any information as to whether water can be obtained.

Another disadvantage of the new symbols is that, however carefully their author may guard against such a misconception, the symbols $\alpha\chi^2$, $\alpha\nu^2$, &c. will certainly be interpreted as meaning that chlorine, nitrogen, &c. contain hydrogen in the same sense as peroxide of hydrogen, $\alpha\xi^2$, does. It may be that they do; but it is surely well to make a sharper distinction than this between observed fact and speculation. To show that this is not an imaginary danger, I need only observe that the author himself refers to water, $\alpha\xi$, and to hydrochloric acid, $\alpha\chi$, as similarly composed "by two indivisible operations." The two operations referred to are obviously α and ξ in the case of water, and α and χ in the case of hydrochloric acid. Now α is the addition to a litre of space of a litre of hydrogen, and is an operation which may be regarded as one of the steps in the formation of a unit of water. It is, however, by no means *proved* that it is a step in the formation of a unit of hydrochloric acid; if a litre of hydrochloric acid does really contain a litre of hydrogen (and that remains to be proved), the hydrogen has not been put there by one indivisible operation, but by two. Further, ξ and χ are purely imaginary operations, ξ being the addition to a litre of space of a litre of an imaginary oxygen having half the density of the gas known by that name, and χ the similar addition of an unknown component of chlorine. I see no objection to the assumption of imaginary substances or operations; but a system which abounds in them and does not carefully distinguish them

from real substances and operations, can scarcely be said to be "the most real and most obvious expression of the facts with which the chemist deals."

It may be objected to the remarks here made that I have ascribed to the atomic symbols properties peculiar to the symbols of the calculus; that I have spoken of H and Cl as "factors" of HCl, and of H² as the "square" of H. I have done so to avoid sudden and disturbing change of language in passing from the consideration of the one system to that of the other, and I think I had a perfect right to do so. Functions are not defined or characterized by the names we may choose to give them, but by their action and effect; and as the figure 2 in the expression O² means exactly the same thing as the figure 2 in ξ^2 , if we call the one an index we may give the same name to the other. In the same way $\delta\theta$ means HgS, and the juxtaposition of the letters means the same thing in both cases; whether we call it multiplication or combination is a mere question of terminology.

XVII. On the so-called "Inactive" Condition of Solids.

By CHARLES TOMLINSON, F.R.S.*

IN the June Number of the Philosophical Magazine, p. 479, is a translation from the *Comptes Rendus* for November 19, 1866, of an interesting paper by M. Gernez, "On the Disengagement of Gases from their Saturated Solutions," in which it is stated that on stirring up Seltzer water, or an aqueous solution of carbonic acid with a solid rod, the rod loses its property of disengaging bubbles of gas after some time, and that the immersion of the body in water, the action of heat, and shelter from the air also render it inactive.

Some interesting experiments on this supposed inactive condition of matter were made by Herr Ziz, of Mayence, as long back as the year 1809, and were recorded by Herr Schweigger in 1815†. It was found that bits of iron wire, flint, glass, small coins, &c. cause supersaturated solutions of sulphate of soda to crystallize by acting as nuclei to which the crystals can attach themselves; but if previously wetted, these bodies become inactive. If thrown dry into a hot solution and allowed to cool with it they are also inactive, and may be shaken up in the solution without producing any effect. Air artificially dried was also inactive; but ordinary air produced crystallization (as it was thought) by introducing particles of dust. A dry nucleus that

* Communicated by the Author.

† "Ueber den Einfluss des Luftdruckes auf Krystallization der Salze," *Journal für Chemie und Physik*, vol. xv. pp. 160-171.

had been exposed to the air immediately produced crystallization; but if driven through the cork so as to touch the supersaturated solution, it did not induce crystallization. A knitting-needle is given as a convenient illustration of this phenomenon. The most efficient nucleus in inducing crystallization is a crystal of the salt itself; but the efflorescence from a solution into the neck of a phial contained in a large flask in which the air was dried by means of caustic potash, was repeatedly redissolved by agitating the vessel without inducing crystallization. An open vessel containing a supersaturated solution will remain liquid if lightly covered, as with a watch-glass. Ziz distinctly recognized the existence of two varieties of sulphate of soda with different fusing-points. Indeed the paper is in advance of the time at which it was written, and its merit has not, I think, been sufficiently recognized.

M. Henri Leowel has published a number of memoirs on supersaturated solutions*, in which he recognizes the distinction between active and inactive solids, or, as he terms them, *dynamic* and *adynamic*, in inducing crystallization; and he is of opinion that the cause of the difference is not mechanical, but *catalytic*. He found that if a glass rod be heated to 30° or 40° C., it could be used for stirring up a supersaturated solution without inducing crystallization. Rods of copper, iron, and zinc were passed through the corks used for closing flasks containing boiling solutions. As the solutions cooled, vapour condensed on the metal rods, which became dry in two or three days, the iron being oxidized; but they were all inactive. Metal rods heated to 100° C. in boiling water or otherwise were also inactive; if heated to from 150° to 200° C. and left to cool under cover they became inactive; but if exposed to air for some time they resumed the active condition in a quarter of an hour, or from that to an hour. Rods of glass, &c., kept in cold water become partially or wholly inactive, but regained their activity by exposure to the air so as to evaporate the water. The author does not pretend to explain these phenomena, but speaks of "that mysterious action" which the air and other bodies exert in inducing crystallization.

I think it would not be difficult to show that this so-called "inactive" condition of solid matter is simply a question of adhesion. The bits of iron wire, flint, glass, small coins, &c. in their dry state induce crystallization because there is adhesion between them and the supersaturated solution; that is, they are wetted by the solution; but if previously wetted with water they are not wetted by the solution when thrown into it, and consequently cannot act as *nuclei*, because the solution does not really

* The first memoir, "Sur la Sursaturation des Dissolutions Salines," is contained in the *Annales de Chimie et de Physique* for 1850.

come in contact with them. In like manner a glass rod made chemically clean will act differently from a rod that has been exposed to the air: the latter is said to be in an "active" condition; what then is the former? Is it "inactive" because it does not always do the work that is expected of it? But it may do other work, and prove itself a more efficient agent than the so-called "active" rod. This is particularly the case with the interesting experiments on the supersaturated solutions of gases already referred to. But what are the facts? A glass rod or other solid body put into soda-water or Seltzer-water disengages gas provided it has been previously exposed to the air, but not otherwise. It loses this property if kept for some time in the solution, or if previously immersed in water or heated, and hence it has been concluded that it is not the solids that disengage the gases, but the air in contact with them. It is assumed that a solid, whatever its polish, "is covered with roughnesses that form a sort of network of capillary conduits into which the surrounding gases penetrate and condense," and "the gas-bubbles thus imprisoned become the centres to which pass those which are dissolved." It is supposed that a lengthened immersion in water or exposure to heat renders the solids "inactive" by removing the air from their surface.

It is with diffidence that I venture to cast a doubt upon a theory supported as this is by ingenious reasoning and experiments, and the applause of the scientific world; but as the only object with which we cultivate science is, or ought to be, the discovery of truth, so the earnest student will not the less welcome her, even though she be not smiling on his pet theory. One of the best and wisest of French philosophers says, "Il ne faut pas juger de la nature selon nous, mais selon elle."

When a bottle of soda-water is uncorked and poured into an ordinary drinking-glass*, after the brisk effervescence is over, the sides and bottom of the glass are more or less covered with bubbles of carbonic acid. If the glass were chemically clean, these bubbles would form in much less quantity, or not at all.

Exp. 1. Two test-glasses, A and B, were selected and wiped out with a clean cloth. A was then filled with methylated spirits of wine, and rinsed out with water to get rid of the adhering spirit. A bottle of soda-water was then uncorked and gently poured into the two glasses. B was profusely covered with gas-bubbles. Not a single gas-bubble was seen on the surface of A.

* In opening a bottle of soda-water there is generally a waste of liquid at the moment the cork flies out, in consequence of the retroactive motion of the bottle. This may be prevented (unless the liquid be very highly charged with gas) by resting the bottle firmly and upright on a solid support while removing the cork.

Exp. 2. A glass rod and a platinum spatula that had been long exposed to the air were dipped first into A, then into B. Gas-bubbles were abundantly liberated from the surface of both. They were dipped into spirits of wine, rinsed in water, and then placed in A and B. Not a bubble of gas appeared on either surface except above the points at which the bodies had been made chemically clean; and there plenty of gas-bubbles appeared. Indeed it was accurately determined by the formation of those bubbles how far the rods had been dipped into the spirit.

Now in these cases, where the surface is made chemically clean, the attraction of adhesion between the liquid and the surface is perfect. The clean surface is completely wetted, and there is no liberation of gas*. But let the surface be not chemically clean (and it readily becomes so by even a few minutes' exposure to the air, or by handling, &c.), then the adhesion is imperfect; the liquid does not wet the surface completely, and the gas, yielding to the superior attraction of the solid, adheres to it. That is, where there is but small adhesion between the liquid and the solid, there may be strong adhesion between the gas and the solid; and hence it is that bodies not chemically clean are favourable for the liberation of gases from their solutions†.

But it is said that the condition of the surface as to roughness or smoothness has much to do with the action of the solid in liberating gas. I do not find such to be the case, provided the condition of chemical purity be attended to.

Exp. 3. A rat's-tail file, that liberated gas abundantly on being immersed in soda-water, was washed in spirit and rinsed in water; but it still liberated gas. It was then held in dilute sulphuric acid for about a minute until there was a decided smell of hydrogen; it still disengaged gas. It was then put into spirit and kept there during some hours. When immersed in fresh soda-water, not a single bubble of gas appeared on the surface of the file.

Exp. 4. The file was dried with a duster and drawn through the moist hand, when on being placed in soda-water it liberated bubbles of gas abundantly. It was now no longer chemically clean.

Exp. 5. Dry iron-filings gently placed in soda-water liberated an abundance of gas, and some of the filings were raised from the bottom to the surface by gas-buoys.

* I find spirits of wine more efficacious in cleaning the surfaces than sulphuric or nitric acid, ammonia, or caustic potash solution.

† A bottle of nitric acid that had become of a dark orange colour by exposure to the light, disengaged gas freely from the surface of a platinum spatula put into it. When the spatula was made chemically clean there was scarcely any discharge of gas.

Exp. 6. Iron-filings shaken up with spirits of wine, gently deposited on a fresh quantity of soda-water sank without liberating any gas.

But it will be said that the filings in exp. 5 were full of air, which was absent in the filings used in exp. 6. Now let us see whether air has really the function assigned to it of liberating gases from their solutions.

Among the apparatus of this College is a closed cylinder of fine wire gauze $2\frac{1}{2}$ inches long and 2 inches in diameter. Attached to the centre of one end is a wire stem 7 inches long, which is used for a handle. By means of this piece of apparatus a beautiful experiment on the adhesion of water to a solid is shown. The cylinder can be completely filled with water and carried about without its escaping. It is, however, necessary to the success of this result that the wire cage be chemically clean; accordingly we are in the habit of washing it in a weak solution of caustic potash before attempting to use it.

Exp. 7. The wire cage was washed in spirits of wine, then plunged several times into clean water, and placed, empty but wet, in soda-water. There was no escape of gas. The cage was lowered to the bottom of the solution so as to have a mass of air in the midst of it; but still no escape of gas, except from that portion of the wire handle that had not been dipped into the spirit and was not chemically clean. Soda-water gradually filtered into the cage, which was taken out three-fourths full of liquid, from which there was no escape of gas. It was repeatedly immersed in the solution, and when at length it was taken out and shaken, the stream of liquid falling into the liquid in the glass produced a slight effervescence.

I must claim for this experiment the conclusion that the presence of air is not concerned in liberating the gas from its solution.

Now let us see what is the action of the cage when not chemically clean.

Exp. 8. About half a drop of oleic acid was rubbed between the palms of the hands, and these were brought into contact with the cage. No sooner did the cage touch the soda-water than there was an audible boiling effervescence, which became more abundant on lowering the cage; even the sides and bottom of the glass, which before were chemically clean and were quite free from bubbles, became covered with large adhering bubbles.

But the action of heat, it is said, renders solids inactive by removing the gas from them by expansion. Of course, according to my view, the action of flame is to burn off impurities and so render the solid chemically clean. If the theory which I am opposing be true, a body raised in any way to a high temperature

will have its surface deprived of air, and so will not be in a condition to liberate gas. Let us see.

Exp. 9. A glass rod that had long been exposed to the air was put into olive oil, and this was raised to the temperature of 310° F. It was left in the oil about twenty minutes, then taken out and passed hastily through a duster to get rid of the excess of oil, and so plunged into soda-water. It was instantly and completely covered with bubbles,—so much so, that no part of the immersed portion was free from them.

Here the rod had not only lost its supposed film of air, but was covered with a thin layer of oil; it was, in fact, about as chemically unclean as it well could be, and in this state it exhibited the strongest adhesion for air, and little or none for water.

I cannot help thinking that in examining the phenomena in question sufficient attention has not been bestowed on the remarkable difference between a chemically clean and an ordinarily clean surface. A body that is capable of performing certain offices at one time becomes incapable of doing so at another, and the circumstances which lead to the altered condition are apparently insufficient to produce such marked differences. This has thrown an air of mystery over certain phenomena which seem to me to admit of very simple explanation. I believe that most, if not all, of the phenomena connected with the crystallization of supersaturated solutions depend on this principle of adhesion; but I propose to refer to this subject on another occasion. I must, however, insist on the necessity of distinguishing between the presence or absence of the condition of chemical purity of a surface as the key to the explanation of a large class of phenomena. I must further remark that it is difficult to maintain the condition of chemical purity for any length of time in apparatus with which one is working. A slight touch, an open door, a puff of smoke, a draught bringing dust is often sufficient to disturb it, as, for example,—

Exp. 10. A thick iron wire put into soda-water caused a copious disengagement of gas. The wire was wiped with a clean duster, it still produced the same effect; it was carefully polished with sand-paper, and still the same effect; it was dipped into spirit and washed in water, and when placed in soda-water not a bubble appeared on its surface; it was then wiped with what is usually called a clean duster, and on putting it into soda-water it immediately disengaged bubbles.

Exp. 11. A large fragment of flint disengaged gas from every part of its surface. It was broken into two pieces, and again put into the soda-water. Not a bubble of gas was liberated from the two fresh surfaces.

It may be objected to this experiment that the new surfaces

had not had time to become active by exposure to the air. I say that they were active, as active as Nature's chemically clean surfaces are, more active in fact than if they had been exposed to the air. It is true they did not disengage gas; the adhesion of the liquid was too perfect for that; and it was necessary to tame down the adhesive force in order to apply to them any one of what I cannot help thinking are the mistaken terms *active*, *catalytic*, and *dynamic*, as used with reference to this class of phenomena.

The ingenious experiment on which M. Gernez chiefly relies for the success of his theory, seems to me to require quite another explanation. A narrow tube, closed at one end and rendered "inactive" on the outside, was lowered full of air into the gaseous solution. "Gas adhered to the column of air which the tube contained, forming quickly a large bubble, which was disengaged; then another was produced, and so on. The gas formed, then, only at the point where the liquid touched the column of air." "From this experiment," says M. Gernez, "which I have varied in several ways, it may be concluded that air sets up the disengagement of carbonic acid." Had M. Gernez made the inside of his tube as "inactive" as the outside, I think he would have found that the column of air had nothing to do with the liberation of the gas.

Exp. 12. A narrow tube 11 inches long was kept for an hour in a quantity of spirits of wine 5 inches deep. It was then washed in water, and closed at the top with the finger, and so just dipped into a fresh quantity of soda-water. There was no disengagement of gas. The tube was lowered to various depths with the same result; the finger was then removed, so that the solution could enter the tube. There was no liberation of gas until the immersion of the tube just exceeded 5 inches; and then, at and about the boundary line marked by the spirit, there was a liberation of gas, both on the inside and the outside of the tube. The solution was 6 inches deep; the tube was chemically clean for 5 inches of its length both inside and out. Above this there was 1 inch of the tube that was not chemically clean, which liberated gas. The tube was taken out, wiped with a duster, drawn through the hand, and again inserted into the solution. The whole length of 6 inches was now covered with gas-bubbles, while a length of 5 inches on the inside was free from them as before.

I cannot resist the conclusion to which these experiments lead me, that the so-called "active" condition of solids in liberating gas from solutions is a lowering of the adhesive force of such solids for liquids, in consequence of want of chemical purity. Make the solids chemically clean, and the solution adheres to

them without any disengagement of gas; make them unclean, and then the adhesive force of the solid becomes more energetic for the gas than for the liquid, and there is a consequent separation of gas from the solution.

King's College, London.
July 1867.

XVIII. *On the Moon's Influence over Cloud.*

By J. PARK HARRISON, M.A.

To the Editors of the *Philosophical Magazine and Journal.*

GENTLEMEN,

IN Mr. Ellis's communication on the tendency to dispersion of cloud under a full moon, in the last Number of the *Philosophical Magazine**, sufficient account has not, I think, been taken of the probable effect of the interruption of continuous observation on more than 400 days in the seven years (1841-47) during which two-hourly observations were made at Greenwich. These, for all scientific purposes, blank days were not equally distributed over the lunation; ten would perhaps be found to occur at conjunction, and sixteen at opposition. And the imperfect means derived from three or four observations on these days cannot be "corrected" so as to make them available or admissible in an inquiry of this nature. The amount of cloud during sixteen or eighteen hours, for example on a day of full moon, cannot be supplied from the general mean of cloud for those hours on *other* days of the lunation.

In addition to this, Mr. Ellis has grouped several days together, and so merged and eliminated any distinctive character they may individually have possessed. It is quite true that the means of seven days at new and full moon, and at first and last quarter, at Greenwich, are nearly identically the same, and consequently approach very closely the mean of the whole lunation. But the curve of cloud which I constructed from the means of the more complete observations eight years ago†, for comparison with curves of mean temperature according to the age of the moon, shows that there certainly are clear and cloudy periods—a fact which receives confirmation from the results obtained by Schübler and Arago‡.

Thus, for example, at the period of last quarter, on the day itself and on the three days before and after it, the mean amounts

* *Phil. Mag.* July 1867.

† *Ibid.* March 1859.

‡ From long series of observations at Paris, Stuttgart, Munich, and Augsburg, the greatest number of clear days and the fewest rainy days were found to occur after last quarter. (*Annuaire du Bureau des Longitudes*, 1833, p. 161, &c. See also Smythe's translation of Arago's works.)

144 *Royal Society* :—Mr. H. C. Sorby on *Analysis of Animal*
of cloud are as follows :—

6·9, 6·9, 7·2, 6·3, 6·6, 6·4, 6·4.

The mean of the three days before last quarter is 7·0, whilst the mean of the remaining four days is 6·42. The mean of the seven amounts is 6·7.

So also at full moon the mean amounts of cloud are

6·6, 7·0, 6·6, 6·9, 6·2, 6·5, 7·1.

And here again, though the mean of the seven amounts is 6·7, a tendency to dispersion of cloud seems to be indicated after full moon. Indeed the amounts 6·2, at this period, and 6·3, at last quarter, are the minimum amounts of cloud in the lunation for the years 1841–47 at Greenwich.

Very remarkable conclusions were arrived at by Schübler from sixteen years' observations of the direction of the wind*. The south and west winds (which it is admitted are the most humid) were found to increase in frequency from new moon to the 2nd octant; whilst the last quarter was the epoch when these winds were at a minimum, and north and east winds reached their maximum.

Schübler's results were confirmed generally by Mr. Glaisher in the spring of this year, from a discussion of the observations of the self-registering anemometer at Greenwich for the years 1841–47†.

The number of hours during which the wind blows in any one direction is accurately registered by this instrument; whilst cloud-measurements cannot be considered more than approximately correct, and moreover leave the question of the height and density of cloud untouched.

Your obedient Servant,

Ewhurst, July 22, 1867.

J. PARK HARRISON.

XIX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 66.]

April 11, 1867.—Lieut.-General Sabine, President, in the Chair.

THE following communication was read :—

“On a Definite Method of Qualitative Analysis of Animal and Vegetable Colouring-matters by means of the Spectrum Microscope.”
By H. C. Sorby, F.R.S. &c.

1. *History.*

My attention was first directed to this subject by reading a report of Professor Stokes's very excellent lecture at the Royal Insti-

* *Annuaire*, p. 172.

† Proceedings of the Meteorological Society, March 20, 1867.

tution, Friday, March 4th, 1864*. It immediately occurred to me that a spectroscope might be combined with a microscope, and employed to distinguish coloured minerals in thin sections of rocks and meteorites. I was soon led to examine many other coloured substances, and found that the instrument is more useful in connexion with qualitative analysis when only very small quantities of material can be obtained.

2. *Apparatus.*

I have an ordinary large binocular microscope, and use an object-glass of about three inches focal length, corrected for looking through glass an inch thick, the lenses being at the top, so as to be as far as possible from the slit. This is placed at the focus; and between it and the lenses, at a distance of about half an inch from them, is a compound prism, composed of a rectangular prism of flint-glass, and two of crown-glass of about 61° , one at each end. This arrangement gives direct vision and a spectrum of the size most suitable for these inquiries, since a wide dispersion often makes the absorption-bands far too indistinct. In order to be able to compare two spectra side by side, a small rectangular prism is fixed over half the slit, and with the acute angle parallel to and just passing beyond it. This gives an admirable result, the only defect being that, when the spectra are in focus, their line of junction is some distance within it; and therefore to correct this I use a cylindrical lens of about two feet focal length, with its axis in the line of the slit, which can easily be fixed at such a distance between the slit and the prisms, as to bring the spectra and their line of contact to the same focus. In front of the slit, close to the small rectangular prism, is a stop with a circular opening, to shut out lateral light, and a small achromatic lens of about half an inch focal length, which gives a better field, and counteracts the effect of the concave surface of the liquid in the tubes used in the experiments, if they are not quite full. These are cut from barometer-tubes, having an internal diameter of about one-seventh of an inch, and an external diameter of about three-sevenths of an inch. They are made half an inch long, ground flat at each end, and fixed with Canada balsam on slips of glass two inches long and about six-tenths of an inch wide, so that the centre of the tube is about one-fourth of an inch from one edge. By this arrangement the liquid may be examined through the length of the tube by laying the slip of glass flat on the stage of the microscope, or through the side of the tube by placing the slip vertical and the tube horizontal. Cells of this size can be turned upside down and deposits removed without any liquid being lost; and the upper surface of the liquid is sufficiently flat, even when inclined at a considerable angle. If requisite, small bits of thin glass can be laid on the top, which are held on by capillary attraction, or may be fastened with gold-size, if it be desirable to keep the solution for a longer time. When the depth of colour is too great in the line of the length of the cell, we can at once see what would be the effect of about one-fourth of the colour

* *Phil. Mag.* S. 4. vol. xxvii. p. 388.

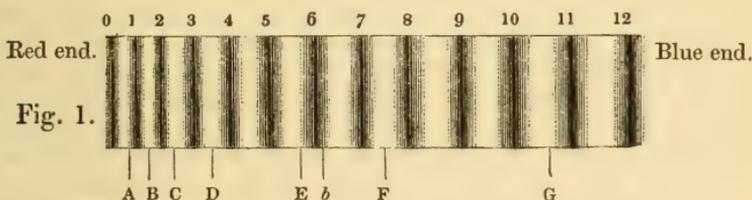
by turning it sideways; and thus we can save much time, and quickly ascertain what strength of solution would give the best result. Very frequently we obtain an excellent spectrum in one direction with one reagent, and in the other with another, without further trouble. I have constructed a small stage, too complicated to describe in writing, which enables me at once to examine solutions in two such tubes, either endways or sideways, and compare their spectra side by side, or to use test-tubes, or to fix the small apparatus which I have contrived for accurately measuring the spectra. This is of such great importance in these inquiries that I must describe it in some detail.

3. *Scale of Measurement.*

It consists of two small Nicol's prisms, and an intermediate plate of quartz. If white light, passing through two such prisms, without the plate of quartz, be examined with the spectrum-microscope, it of course gives an ordinary continuous spectrum; but if we place between the prisms a thick plate of quartz or selenite, with its axis at 45° to the plane of polarization, though no difference can be seen in the light with the naked eye, the spectrum is entirely changed. The light is still white, but it is made up of alternate black and coloured bands, evenly distributed over the whole spectrum. The number of these depends on the thickness of the depolarizing plate, so that we may have, if we please, almost innumerable fine black lines, or fewer, broader bands, black in the centre and shaded off at each side. These facts are of course easily explained by the interference of waves. It would, I think, be impossible to have a more convenient or suitable scale for measuring the spectra of coloured solids and liquids. If we use a micrometer in the eyepiece, an alteration in the width of the slit modifies the readings, and the least movement of the apparatus may lead to error, whereas this scale is not open to either objection. Besides this, the unequal dispersion of the spectrum makes the blue end too broad, so that a given width, as measured with a micrometer in the eyepiece, is not of the same optical value as the same width in the red. The divisions in the interference-spectrum bear, on the contrary, the same relation to the length of the waves of light in all parts of the spectrum, and no want of adjustment in the instrument alters their position. As will be seen from the drawing (fig. 1), the unequal dispersion makes the distance between the bands in the blue about twice as great as in the red. The perfection of a spectrum would be one in which they were all at equal intervals; but possibly no such uniform dispersion could be produced. By having a direct-vision prism, composed of one of flint-glass of 60° , and two of crown-glass of suitable angle, we can place it over the eyepiece, and may diminish the dispersion at the blue end, or increase that at the red end, by turning it in one position or the other, and thus see either end to the greatest advantage. It is, of course, very easy to draw spectra on this principle, and give all parts equal prominence, and not an unduly compressed red, and an unduly expanded blue end. Thus drawn, the spectra are far more uniform in many of their characters, and some general laws

are at once apparent that otherwise might have been entirely overlooked; and on this account I shall adopt this system in those figured in this paper. It is, in fact, merely representing the actual measurements by drawings, without being at the trouble of distorting them, so as to be like naturally distorted spectra.

Since the number of divisions depends on the thickness of the interference-plate, it became necessary to decide what number should be adopted. At first I thought that ten would be most suitable; but, on trying, it appeared to me too few for practical work. Twenty is too many, since it then becomes extremely difficult to count them. Twelve is as many as can be easily counted; it is a number easily remembered, gives sufficient accuracy, and has a variety of other advantages. With twelve divisions the sodium-line *D* comes very accurately at $3\frac{1}{2}$; and thus, by adjusting the plate so that a bright sodium-line is hid in the centre of the band when the Nicol's prisms are crossed, it is accurately at $3\frac{1}{2}$ when they are arranged parallel so as to give a wider field. The general character of the scale will be best understood from the following figure, in which I have numbered the bands, and given below the principal Fraunhofer lines.



The centre of the bands is black, and they are shaded off gradually at each side, so that the shaded part is about equal to the intermediate bright spaces. Taking, then, the centres of the black bands as 1, 2, 3, &c., the centres of the bright spaces are $1\frac{1}{2}$, $2\frac{1}{2}$, $3\frac{1}{2}$, &c., the lower edges of each $\frac{3}{4}$, $1\frac{3}{4}$, &c., and the upper $1\frac{1}{4}$, $2\frac{1}{4}$, &c. We can easily divide these quarters into eighths by the eye; and this is as near as is required in the subject before us, and corresponds as nearly as possible to $\frac{1}{100}$ part of the whole spectrum visible under ordinary circumstances by gaslight and daylight. Absorption-bands at the red end are best seen by lamplight, and those at the blue end by daylight.

On this scale the position of some of the principal lines of the solar spectrum is about as follows:—

A $\frac{3}{4}$	B $1\frac{1}{2}$	C $2\frac{3}{8}$	D $3\frac{1}{2}$
E $5\frac{1}{16}$	b $6\frac{3}{16}$	F $7\frac{1}{2}$	G $10\frac{5}{8}$

At first I used plates of selenite, which are easily prepared, because they can be split to nearly the requisite thickness with parallel faces; but I found that its depolarizing-power varies so much with the temperature, that even the ordinary atmospheric changes alter the position of the bands. Quartz cut parallel to the principal axis of the crystal is so slightly affected in this manner as not to be open to this

objection, but is prepared with far greater difficulty. The sides should be perfectly parallel, and the thickness about $\cdot 043$ inch, and gradually polished down with rouge until the sodium-line is seen in its proper place. This must be done carefully, since a difference of $\frac{1}{100000}$ inch in thickness would make it decidedly incorrect. I have prepared such plates, corresponding to my own, and placed them in the hands of Mr. Browning and of Messrs. Beck, so that any one wishing to adopt a similar scale may be able to do so more accurately.

The two Nicol's prisms and the intervening plate are mounted in a tube and attached to a piece of brass in such a manner that the centre of the aperture exactly corresponds to the centre of any of the cells used in the experiments, which are all made to correspond in such a manner that any of them, or this apparatus, may be placed on the stage and be in the proper place without further adjustment, which, of course, saves much time and trouble.

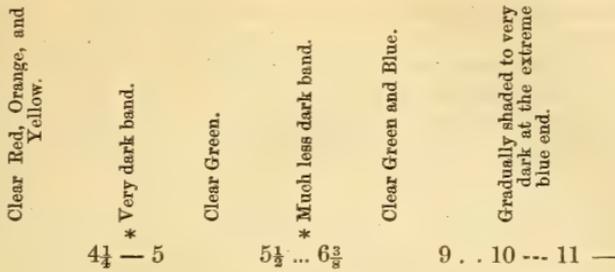
4. *Symbols used to describe Spectra.*

In order to describe spectra in my note-book or in print, I have devised a simple notation, employing types in constant use. Instead of writing an account of this system, I here give a printed illustration, which will show that by this means it is easy to give in a single line all the essential particulars which would otherwise require a long and tedious description, or a number of drawings and woodcuts. Without some such method of measuring and recording spectra it would be almost impossible to carry on extensive inquiries.

The intensity of the absorption is expressed by the following types:—

Not at all shaded	Blank space.
Very slightly shaded	. . . Dots with wide spaces.
Decidedly shaded	. . . Dots closer together.
More shaded	... Very close dots.
Strongly shaded, but so that a trace of colour is still seen	--- Three hyphens close.
Still darker	— Single dash.
Nearly black	—— Double dash.

Except when specially requisite, only the symbols . . . --- — are employed, for the sake of simplicity, and then as signs of the relative rather than of the absolute amount of absorption; and it is assumed that there is a gradual shading off from one tint to the other, unless the contrary is expressed. This is done by means of a small vertical line over the figure (see p. 150, No. 11), which shows that there is a well-marked division between them. Definite narrow absorption-bands are indicated by * printed over their centre. This will be better understood by a description of the spectrum of deoxidized hæmatin.



The following examples will show how simple or more complicated spectra may thus readily be printed and compared. I have chosen solutions of similar tint, in order to show that the spectra of those of nearly the same colour may be very different, or, if analogous, may differ in details easily expressed by the symbols. The colour of each is given after the name. Nos. 1, 8, 9, 10, 11, 12, and 13 can be kept for a long time sealed up in tubes; and the rest are easily prepared. I have in all cases chosen that strength of solution which gives either the most characteristic spectra or those best suited for comparison with other allied colours.

1. Cudbear in alum	(Pink)	3 8	11
2. Colour of Elder-berries with citric acid.	(Red Pink)	4 5 $\frac{1}{4}$ —8	— 9 11 —
3. Brazil-wood, with bicarbonate of ammonia.	(Pink)	4 $\frac{1}{2}$ — 5 $\frac{3}{4}$ 8	
4. Logwood, with bicarbonate of ammonia.	(Pink)	3 $\frac{5}{8}$ — 5 $\frac{1}{4}$ 7	

The next four are spectra of blood, produced by the successive addition of the various reagents, as in detecting fresh stains.

5. Fresh Blood.	(Pale Scarlet)	3 $\frac{1}{2}$ — 4 $\frac{3}{8}$ *	4 $\frac{3}{4}$ — 5 $\frac{1}{2}$ * 7 . . 8 . . 9 —
6. Citric Acid then added.	(Pale Brown)	1 $\frac{5}{8}$ 2 $\frac{1}{4}$ *	4 8 9 --- 10 —
7. Ammonia then added.	(Pale Brown)	3 $\frac{5}{8}$ 4 $\frac{3}{8}$ *	4 $\frac{7}{8}$ 5 $\frac{5}{8}$ * 7 . . 8 --- 10 —
8. Deoxidized hæmatin, from blood-stain 2 years old.	(Pink)	4 $\frac{1}{4}$ — 5 *	5 $\frac{1}{2}$ 6 $\frac{3}{8}$ * 9 . . 10 --- 11 —

With these may be compared the two spectra which more nearly resemble those produced by blood than any I have yet seen:—

9. Cochineal in alum.	(Pink)	3 $\frac{3}{8}$ — 4 $\frac{1}{4}$ 5 $\frac{1}{8}$ --- 6 $\frac{1}{8}$ 7 $\frac{1}{4}$ *
10. Alkanet-root in alum.	(Pink)	3 $\frac{1}{2}$ — 4 $\frac{3}{8}$ * 5 $\frac{1}{4}$ 5 $\frac{3}{4}$ *

The following spectra of compounds derived from chlorophyll are as complicated as any I have met with:—

11. Normal chlorophyll in alcohol. (<i>Deep Green</i>)	} $\frac{7}{8} - 2\frac{3}{8} - - 3\frac{1}{4} \dots 4\frac{1}{2} \quad 6\frac{1}{4} - - 7\frac{1}{2} -$
12. Ditto, as decomposed by acids, or as found in some leaves. (<i>Olive Green</i>)	} $1 - 2\frac{1}{8} \quad 2\frac{3}{8} - - 3\frac{3}{8} \quad 4\frac{1}{4} \dots 5\frac{1}{4} - - 5\frac{3}{4} \dots 6\frac{3}{8} - - 7\frac{3}{8} \quad 8\frac{1}{4} - - 9\frac{1}{4}$
13. Ditto, as decomposed by caustic potash, and then by hydrochloric acid. (<i>Red-Green, Neutral Tint</i>)	} $\frac{1}{4} - \frac{3}{4} \quad 1\frac{1}{8} - 1\frac{3}{8} \quad 1\frac{7}{8} - 2\frac{1}{8} \quad 4\frac{1}{2} - 5\frac{1}{4} \dots 9 - - 10 -$

5. *General Remarks on Absorption &c.*

It appears to me that in adopting the undulatory theory of light it greatly simplifies the subject before us if we, to some extent, make use of the phraseology of acoustics. And thus, for example, I shall speak of two absorption-bands that occur, one nearer the *red*, and the other nearer the *blue* end of the spectrum, as being relatively *lower* and *higher*. In a similar manner, if the addition of some reagent cause the absorption to increase towards the blue, and decrease towards the red, end, I shall describe it as *raising* the position of the absorption. We may also make some facts more intelligible by comparing them with the analogous phenomena of sound, and thus, for instance, may suppose that very narrow absorption-bands indicate that the ultimate particles of the substance will only take up vibrations of light of nearly one particular velocity, and that broad absorption-bands show that the particles have a much less definite rate of movement. Analogy would also lead us to infer that, when two spectra differ very decidedly, they must be due to different substances or to the same in a different condition; whereas, if two spectra agree, they may be the same substance, or two distinct substances whose different actions are made equal by particular circumstances. As an illustration, we may refer to a short string, which may give the same note as a longer whose tension is greater. For this reason we should be careful not to rely too much on one spectrum. If, however, we can produce some great physical change in both substances, and still their spectra remain the same under equal conditions, and if this occur uniformly in several different changes, we may conclude that they are identical. Hence the value of the various reagents named below. Many excellent illustrations of these principles could easily be given.

6. *General Method of Experiments.*

Since the spectrum-microscope enables us to use very small quantities, it appeared desirable to adopt such a method of research as would enable us to take full advantage of this circumstance, and to avoid as much as possible previous chemical manipulations. On this account I shall say nothing about modified chemical methods, which may, of course, be also employed when sufficient material is

at command. My aim has been to contrive a special system of qualitative analysis of coloured substances applicable to minute quantities, and as independent of general chemistry as the blowpipe method is in the case of minerals. I may here say that, in some very important practical applications to the detection of blood-stains, not above $\frac{1}{100}$ of a grain was at disposal, and yet perfectly satisfactory results were obtained.

I was led to study the colouring-matters of flowers, leaves, fruits, woods, and roots, because it appeared a most admirable field of inquiry to teach the general principles of the subject. The colours being so various, and occurring under such complicated conditions, I thought that if methods could be devised to distinguish those that are dissimilar and to prove the identity of those that are alike, even when mixed with coloured impurities, such principles could easily be applied to other inquiries. If the question were merely to distinguish or compare absolutely pure colouring-matters, there would be little or no difficulty; but it appeared to me that one great value of the method would be to be able to apply it at once to very impure and mixed materials. In such cases mere colour is of very secondary importance, since that may be totally changed by a very small amount of impurity.

7. *Preparation of Colours.*

If the petals, leaves, &c. of plants be crushed in water, it very commonly happens that the colour is rapidly decomposed and no clear solution can be obtained; but if crushed in a moderate quantity of spirits of wine, and the solutions squeezed out, filtered, and evaporated to dryness at a gentle heat, the colouring-matter does not decompose, even when redissolved in water and filtered to remove anything not soluble in that liquid. This clear solution should then be evaporated to dryness at a gentle heat in a small saucer, and kept *dry*; for then the colours often undergo no important change in the course of many months, whereas, when kept dissolved in water or alcohol, they may quickly decompose. I have thus prepared the colouring-matter of above a hundred different vegetable substances, some of which have become entirely changed, but a large number are apparently still unaltered. I have also kept a number of colours, sealed up in glass tubes, ready for direct examination, dissolved in alcohol, in strong syrup, or in alum. Many have decomposed; but many have kept perfectly well, or have merely faded, and still give excellent spectra after above a year. I have also prepared and kept in the same manner some animal colouring-matters, but comparatively few.

8. *Method of Examination.*

The coloured substances are examined, when dissolved in water, alcohol, or other solvent, in the small glass cells already described; and the various reagents are added and mixed by means of a moderately stout platinum wire, flattened at one end and turned up square, like a little hoe. I have made many experiments in order to ascertain what reagents are most serviceable in developing characteristic

spectra, and have at length concluded that for general purposes the following are the most convenient. Those which are solid are best kept in small bottles as coarse powder, and added to the small cells in a solid state, so that the quantity used may be more readily known.

9. *Reagents.*

Hydrochloric acid.
 Citric acid.
 Benzoic acid.
 Boracic acid.
 Bicarbonate of ammonia.
 Carbonate of soda.
 Diluted solution of ammonia.
 Caustic potash.
 Sulphite of soda.
 Sulphate of protoxide of iron.
 Alum.
 Iodine dissolved in alcohol.
 Bromine dissolved in water.
 Solution of hypochlorite of soda.
 Permanganate of potash.

This list might of course be very much extended, if we were to include such reagents as may be used in separating or decomposing colours by the ordinary chemical methods. In describing the effect of those named in this list, I feel that I could not avoid mentioning some well-known facts without breaking the thread of my argument; and therefore I trust it may not be thought out of place if I give a general account of the whole from the particular point of view required by the subject more especially before us.

The action of many reagents is so intimately related to different parts of the spectrum, as to show that there must be some connexion between so-called chemical reactions and optical phenomena. Not that their effect is absolutely the same in the case of all coloured substances, but generally only the *extent* differs, whilst the *character* of the change is uniform,—unless, indeed, decomposition take place; and even then it has a tendency to conform to a general law.

10. *Solvents.*

Water and alcohol are the most useful solvents, and the spectra of the two solutions of the same substance often differ most strikingly; in fact they often behave in other respects as if they were solutions of different substances. Sometimes the spectra are absolutely identical; but often well-marked narrow absorption-bands are seen in the alcoholic solution, where they are almost, or quite, invisible in the aqueous. Very commonly the same bands are seen in both, but not exactly in the same place, alcohol sometimes raising them to a higher part of the spectrum, and sometimes depressing them. Occasionally the spectrum of the dry material is like that of the alcoholic solution, and unlike that of the aqueous, as if the difference were due to the presence of water; but in other cases it

is unlike both. At all events the facts clearly show that a solvent has a most important action on the ultimate particles of the substance in solution, since it may produce a greater change in optical phenomena than even chemical combination. Undistilled hard water may act like a weak alkali.

11. Acids and Alkalies.

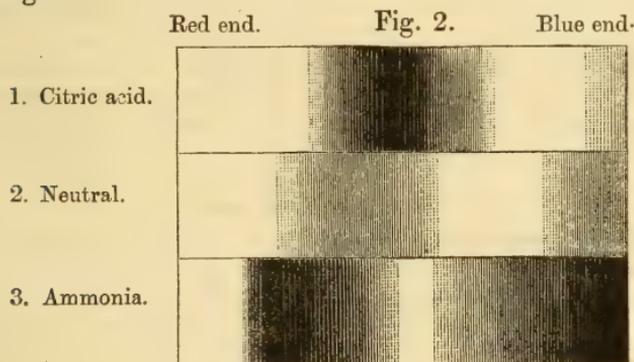
As far as optical phenomena are concerned, there is no absolute division between acids and alkalies; for we have every connecting link from the strongest acids to the strongest alkalies. In order to understand their action, it is most essential to distinguish between what may be called "general absorption" and "local absorption-bands." There may, perhaps, be no absolute line of division; but when seen to advantage they are affected in such a different manner that it is desirable to treat of each separately.

12. General Absorption.

As a good example of simple general absorption, we may take the crimson colouring-matter of the common Wallflower (*Cheiranthus Cheiri*), which is soluble in water, and, along with a yellow only soluble in alcohol, gives rise to the varied colours of the flowers.

When neutral, it is crimson		$2\frac{3}{4}$ 7	10 . . 11 - —
With ammonia, fine green	$1\frac{3}{4}$ — $4\frac{1}{2}$ 6		7 . . 8 - - 9 —
With citric acid, deep pink	$3\frac{1}{2}$ - $4\frac{1}{2}$ —		6 - - $8\frac{1}{2}$ 11 . . .

These facts will be better understood by means of the following drawing:—



whence it will be seen that citric acid raises and greatly increases the central absorption, and ammonia lowers and also increases it. At the same time the absorption at the extreme blue end of the spectrum is raised by the acid almost to beyond the range of vision, but lowered to the centre of the spectrum by ammonia. Acids and alkalies of intermediate character, as, for example, boracic acid and bicarbonate of ammonia, produce intermediate effects. These well-known phenomena may be looked upon as typical of acids and alkalies; but the extent of their action varies for each particular colouring-

matter, so that in some cases it is slight, and sometimes neither acids nor alkalis produce any effect. Their relative action on the central and upper absorption also varies very greatly in different colours. If there is no general absorption in the centre of the spectrum when the colour is neutral, but only an absorption at the blue end, acids and alkalis act on it in precisely the same manner as on the absorption at the blue end in the case just described, raising or lowering it to an extent varying greatly according to the substance; and the same may be said of any general absorption at the red end. The reverse certainly occurs when an acid is added to chromate of potash, or excess of ammonia to a salt of copper; and, according to Stokes (*Phil. Trans.* 1862, p. 609), alkaloid bases usually show this reverse action. It may depend on the different properties of two distinct *compounds*, which does not appear to be the cause of the phenomena now under consideration. In the case of all the vegetable colouring-matters which I have examined, the tendency of acids is to raise, and of alkalis to depress, the general absorption in each part of the spectrum—the extent of this action depending on the strength and quantity of the reagents, and on the nature of each colouring-matter; and thus we have a general rule, and not several, as commonly adopted by chemists, each of very limited application:—for instance, that vegetable blues are turned red by acids, and green by alkalis; and that vegetable yellows are reddened by alkalis. I may here remark that some colours would appear to be exceptions, if we did not remember that waves of light, or waves analogous to them, exist beyond the visible spectrum. Thus, for example, when alkalis are added to the yellow solution of Brazil-wood (*Cæsalpinia crista*), it is changed to pink, the absorption being so much lowered that the blues are transmitted—this clear space corresponding to what was probably a clear space beyond the blues visible under ordinary circumstances, but which would perhaps be seen if examined in the manner described by Stokes in his paper on the long spectrum of electric light*.

13. *Fading of Solutions.*

One striking peculiarity in the action of acids on the solutions of many vegetable colours is, that, when they are in a particular state of acidity, they fade to nearly or quite colourless, without there being any decomposition. This is especially the case with pink colours dissolved in alcohol. It occurs slightly with blue colours, and little, if at all, with yellows. The aqueous solutions change much more slowly, but more and more rapidly the more they are diluted, and frequently attain a permanent depth of colour which is dark or pale according as the solution is strong or dilute. Of course I here allude to the effect of the *same total amount of colour*, and not to the different effect of the same *quantity* of a strong or dilute solution. The alcoholic solutions obtained direct from the flowers often fade so rapidly, and become so nearly colourless, that any one

* *Phil. Trans.* 1862, p. 599.

might easily fancy that all the colour was lost by decomposition; and an evaporating dish containing it might appear merely filled with brownish alcohol, and yet on evaporation the whole dish might be covered with a fine deep colour. The same change may occur over and over again, the deep-coloured solution first obtained soon fading, and the colour being restored by subsequent evaporation.

When such a colour is dissolved in a little water and added to alcohol in an experiment-tube, the colour may at first be very deep, but may fade so rapidly that there is scarcely time to observe the spectrum before it passes into that molecular state which does not absorb any of the rays of light. The colouring-matter of the flowers of the red *Salvia* (*S. splendens*) is an excellent example. Neutral solutions do not undergo this rapid change; a different condition of acidity is requisite for different colouring-matters; and some do not change at all. A large excess of citric acid very often restores the intensity of the colour; and usually the absorption-bands are seen to the greatest advantage when the solution is in that state which rapidly fades; and by adding too much colour and watching whilst it fades, they may be seen and measured when at their best. This fading of a dark-coloured solution must not be confounded with the change which takes place on diluting some salts, as described by Dr. Gladstone in his paper on that subject*.

14. Absorption-bands.

Though acids and alkalies thus, to a greater or less extent, alter the position of the general absorption, they act very differently on the special, local absorption to which it is very convenient to restrict the term "absorption-bands." Since I shall often have to speak of their being at equal intervals, it would be well to say that I have found it convenient to construct a wedge-shaped piece of quartz, cut parallel to the axis of the crystal, and to use it along with two Nicol's prisms in such a manner that the spectrum may be divided into any requisite number of equal portions, by interference-bands situated in any requisite position. This of course avoids the errors which so often happen when we compare together measurements that cannot be made with very great accuracy.

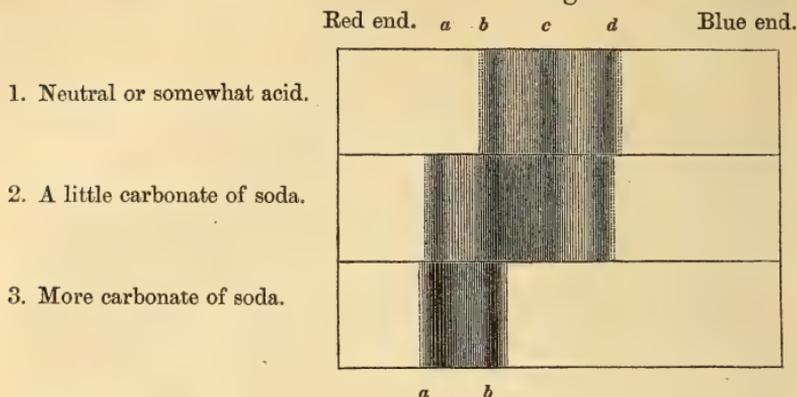
As an excellent illustration I select the colouring-matter of Alkanet-root (*Anchusa tinctoria*). It is insoluble in water, but is easily dissolved by alcohol, even when much diluted with water, and gives a clear pink solution. The spectrum is nearly the same when the colour is dissolved in absolute alcohol as when much water is present, only each of the absorption-bands is situated rather higher. Thus, taking the centres of the bands, we have—

	b.	c.	d.
Absolute alcohol	$4\frac{3}{8}$	$5\frac{7}{8}$	$7\frac{3}{8}$
Very dilute alcohol	$4\frac{1}{4}$	$5\frac{3}{4}$	$7\frac{1}{4}$

The general spectrum of the solution in dilute alcohol will be best understood from the following figure, No. 1 :—

* Quart. Journ. Chem. Soc. vol. xi. p. 36.

Fig. 3.



Acids produce no important change, and the effect of alkalis is best seen by gradually adding carbonate of soda. This alters the colour to a more and more blue purple, and the spectrum is changed in the manner shown in fig. 3. The three bands seen in the neutral solution may be referred to as *b*, *c*, and *d*; and their centres occur at equal intervals of about $1\frac{1}{2}$. When enough carbonate of soda has been added to make it slightly purple, a fourth band, *a*, makes its appearance, separated from *b* by the same equal interval of $1\frac{1}{2}$, whilst the other bands remain in the same position as at first, only modified in intensity. The band *a* becomes darker and darker as more carbonate is added, until, when the solution is a fine purple, it is as dark as the others (see No. 2); and on adding more carbonate it becomes still darker, and the bands *c* and *d* more faint, until the solution is a purple blue; and the spectrum has only the two well-marked bands *a* and *b*, shown by No. 3.

The bright blue colouring-matter of the flowers of *Lobelia speciosa* gives, when *neutral*, almost exactly the same spectrum as that of Alkanet-root when *alkaline* (No. 3), having two well-marked absorption-bands, whose centres are at $2\frac{3}{4}$ and $4\frac{1}{4}$; and on adding carbonate of soda, the upper one is gradually removed, and the centre of the lower is depressed to near $2\frac{1}{2}$. More or less similar results occur in the case of many other blue colouring-matters; and on adding a slight excess of acid the general absorption is raised, and other bands may be developed higher up, at equal intervals; but when a large excess has been added, they are lost in a strong general absorption. Too strong an alkali may also destroy narrow bands in a similar manner, as is well seen in the case of Brazil-wood. The neutral aqueous solution shows an absorption-band, made far more distinct by the addition of bicarbonate of ammonia, which makes it pink and very fluorescent. The spectrum is then

$$4\frac{3}{4}^* - 5\frac{3}{4} \dots 7;$$

but on adding excess of ammonia the solution ceases to be fluorescent, the narrow absorption-band is lost, and the spectrum becomes

$$3\frac{1}{2} \dots 4\frac{1}{4} - 8 \dots 9\frac{1}{2}.$$

Ammonia does not produce this effect when the colour is dissolved in alcohol, the solution remaining fluorescent and still giving the narrow band; and, as a general rule, that solvent greatly impedes or entirely prevents such changes, and on this account almost invariably shows absorption-bands to the greatest advantage.

We therefore see from the above examples that the absorption follows the general rule, and is raised by acids, and depressed by alkalis; but this only applies to the absorption when viewed as *a whole*, and not to the separate bands; for those reagents change their *intensity*, but not their *position*. In some cases, indeed, their position is slightly altered; so that perhaps it would be more correct to say that acids and alkalis may raise and depress the general absorption to an extent equal to a considerable fraction of its own great breadth; whereas they either do not change the position of narrow absorption-bands, or merely raise and depress them by a fraction of their own narrow width. It is their very definite position that makes them so useful in this method of analysis.

Unfortunately I have not hitherto been able to find a sufficient number of colouring-matters giving rise to three or more well-marked absorption-bands to warrant a general conclusion; and therefore it is perhaps premature to conclude that their centres always occur at *equal intervals*. At the same time it is certainly a very common fact. When the maximum point of transparency occurs between the different bands, there may be, as it were, a double interval; but then, sometimes, even this missing band may be seen under favourable conditions. A difference in the general absorption may also somewhat alter the apparent position of a band, if it is strongly shaded on one side and not on the other; and the presence of impurities may also modify the results; so that absolute accuracy cannot be expected in all cases; and occasionally very narrow bands occur which appear to belong to a second system. It must be borne in mind that the bands are equidistant only when measured by means of the interference-spectrum. Thus, in the case of Alkanet-root, when measured with a micrometer, instead of the intervals *a* to *b* and *c* to *d* being equal, they are related to one another about as $1\frac{1}{2}$ and 2; and thus the general law is entirely obscured. If subsequent research should prove that the bands are normally at equal intervals, it will be a fact of great value in deciding whether certain spectra are or are not due to a *mixture of colours*, since, if a band occurred at a perfectly unequal interval, it would show that there must be at least two substances. Even in the present state of our knowledge, any inequality should make us carefully search for some satisfactory reason for such a divergence from a common rule. My meaning will be better understood from the following examples:—

If a little of the colour of Brazil-wood be added to the solution of Alkanet-root, the bands are not altered, and are seen at $4\frac{1}{4}$, $5\frac{3}{4}$, and $7\frac{1}{4}$; but a little bicarbonate of ammonia develops a well-marked band whose centre is at $5\frac{1}{4}$, and therefore at an interval of 1 instead of $1\frac{1}{2}$. The same is also well seen in the case of a mixed solution of Brazil-wood and blue *Lobelia*. I therefore argue that if an un-

known substance gave rise to similar spectra, with bands at unequal intervals, we ought strongly to suspect either that it was naturally composite, or that some new compound had been formed by decomposition. As a very good illustration I may refer to the product of the action of acids on chlorophyll. The band in the red is not at an equal interval; but, on careful examination, it is seen to be made up of two bands, the upper of which is at an equal interval. I was not aware that these were due to two different substances, but was led to think it very probable; and Professor Stokes informs me that he has proved it to be the case. As an illustration of another kind of exception, I refer to the colouring-matter of the pink Stock (*Matthiola annua*). The aqueous solution shows two bands, whose centres are at about $3\frac{5}{8}$ and $5\frac{1}{4}$; and on adding ammonia the upper is removed, and the lower depressed to $3\frac{1}{4}$. In spirit of wine they are at 4 and $5\frac{1}{2}$, and ammonia develops a third at 3, which are *not* equal intervals. However, if absolute alcohol be used, the bands are at $2\frac{3}{4}$, $4\frac{1}{8}$, and $5\frac{1}{2}$, which are *equal* intervals; and thus we see that the abnormal inequality is due to the presence of water, which causes the spectrum to be as if due to a mixture of two colours, when in reality it is the same colour dissolved in two solvents.

In spectra showing one absorption-band, there is very commonly a general absorption extending from it towards the blue end; whereas it so seldom extends towards the red end that it is doubtful if it ever occurs in substances undoubtedly not a mixture of two colours. It can, however, so easily occur in mixed colours, that any substance giving rise to such a spectrum is probably a mixture. Many illustrations might be given; but I will select Brazil-wood, and the same artificially mixed with the colour of beet-root. Adding bicarbonate of ammonia to both, we have—

Brazil-wood alone $4\frac{3}{4}^* - 5\frac{3}{4} \dots 7$

Brazil-wood and beet-root $3\frac{1}{2} \dots 4\frac{5}{8}^* - 5\frac{3}{4} \dots 8$

Here, then, the shading below the absorption-band from $3\frac{1}{2}$ to $4\frac{5}{8}$ is evidence of the second colour; and if such a mixture had occurred naturally its mixed character might easily have been overlooked. I have found many cases similar to this, and had proved that they were mixtures before I was aware that the spectra indicated it. If these very common facts turn out to be general laws, we might thus detect at once the presence of as many as three different substances, or at all events might learn what further examination was desirable.

15. *Sulphite of Soda.*

Sulphite of soda is a most valuable reagent, and its action very remarkable. It enables us to divide colours into three groups, according as it produces a change in an ammoniacal, in an acid solution, or in neither. The action is related in a very simple manner to the spectra. Having added an excess of ammonia, there may be a well-marked broad absorption over more or less of the red, orange, yellow, and upper green, and above this a clear transparent space, limited

by a variable amount of absorption extending downwards from the extreme blue. Fig. 2 will illustrate my meaning. In the case of one group of colours, the addition of sulphite of soda almost immediately removes the detached broad absorption in the lower part of the spectrum, but leaves that at the blue end quite unchanged, or only slightly modified by the solution being made more alkaline. If, then, as in the case of magenta, there is no absorption at the blue end, sulphite of soda makes the solution quite colourless; whereas if the blues are absorbed, as in the case of the ammoniacal solutions of the colour of red roses and of some species of *Dianthus*, it changes the colour from green to yellow. If the absorption extends continuously down from the extreme blue to the orange, as often happens when ammonia is added to yellow colours, sulphite of soda produces no change. It is only when there is a more or less perfect *division* between the upper and lower absorption, that it has any effect; and then it simply and entirely removes the lower absorption. Some colours are changed immediately, even when a very small quantity of sulphite is added; but others require more, and change gradually, though still very soon.

16. *Groups A, B, and C.*

Colours which are thus altered when the solution is ammoniacal constitute my group A. Frequently, however, sulphite of soda does not remove the detached absorption when excess of ammonia is present, but does so when there is an excess of citric acid. These constitute my group B. As in the other group, any absorption which extends continuously from the extreme blue end is not altered, but the detached absorption in the green is almost immediately removed; and therefore a deep pink or red solution may at once become quite colourless, or only a very pale yellow; and in many cases this residual colour is due to some yellow colouring-matter mixed with the other. I have never seen a colour which was changed by sulphite when alkaline, and not when acid; and thus citric acid never restores the colour when it is added to the changed ammoniacal solution. Excess of ammonia usually restores the faded acid solution to nearly the original colour, and it is therefore not a case of actual decomposition, but merely the result of some remarkable molecular change. A third group of colours consists of those which are not almost immediately changed by sulphite of soda, either when alkaline or acid; and these I call group C. Some of them may fade on keeping several hours, and some do not fade even in several days; but they cannot thus be divided into two definite groups. When thus faded, ammonia does not restore the colour; and therefore it is evidently the effect of decomposition, and not like the mere molecular change met with in group B.

On the whole, the groups A, B, and C are remarkably distinct. There are, indeed, a few cases where the change takes place somewhat slowly; and a few scarlet colours do not show very distinctly the characteristic peculiarities of either B or C; but there are other very strong reasons for believing that some of these are really mix-

tures of different groups. Even if it should be found that perfectly simple colouring-matters may have, as it were, intermediate characters, such appear to be so rare that practically they may be classed with mixtures until some reason be found for classing them together as exceptions.

These reactions of sulphite of soda are so much interfered with by the presence of alcohol, that it should never be employed as a solvent, unless the substance is insoluble in water; and then it should be diluted as much as possible, since the ordinary spirit of wine with an equal quantity of water is the extreme strength admissible, and even that very much delays the reaction. The effect of various other reagents is also sometimes very different, according to the nature of the solvent.

The three groups A, B, and C differ in other particulars. It is easy to change A or B into C by various reagents which produce decomposition; but I do not know a case where C can be changed into A or B. Caustic alkalis usually soon decompose colours belonging to group A, when dissolved in water, but act slowly on those of groups B and C. Usually colours of group C are far more permanent than those of groups A and B; and to it belong most of the vegetable colours used in dyeing, and nearly all yellows.

17. *Other Reagents.*

Boracic Acid.—The chief value of this reagent is that it gives nearly the same spectrum as that of a neutral solution when added after the addition of a slight excess of ammonia. It should therefore be well fused in a platinum crucible and recrystallized, so as to be quite free from any stronger acid.

Sulphate of Iron.—Sulphate of the protoxide of iron is chiefly useful as a deoxidizing agent, in the case of blood and a few analogous substances, taking care to have citric acid present to prevent the precipitation of the oxide by ammonia*.

Alum.—Alum has a remarkable influence on some colours, and it has the property of gradually restoring many after they have passed into the faded modification. Many colours also may be kept for a long time dissolved in a strong solution, sealed up in tubes; and it is occasionally an excellent solvent for substances insoluble in either water or alcohol. The chief objection to it as a reagent is that the spectra are so much influenced by the presence of ammonia, even when neutralized by an acid, that it is almost impossible to compare together different substances under exactly the same conditions.

Iodine and Bromine.—Iodine dissolved in alcohol, and bromine in water, are useful in producing decompositions which may differ very considerably in colours otherwise very similar, as, for example, the yellow colouring-matters of the root of rhubarb and of turmeric. The iodine or bromine should be added in sufficient quantity, and then ammonia used to remove the excess, and thus avoid the effect of their own colour. The solution may then be made acid with citric acid, and should in both cases be compared with another tube to which no iodine or bromine has been added.

* See Stokes's Paper; Proc. Roy. Soc. vol. xiii. (1864) p. 355.

Hypochlorite of Soda.—This reagent, with or without the addition of citric acid, is sometimes useful, as, for instance, in detecting the adulteration of rhubarb with turmeric; but generally its action is too powerful and too uniform.

Pernanganate of Potash.—This also usually acts too powerfully on colouring-matters. The excess can easily be removed by sulphite of soda, which makes an alkaline solution pale yellow, but an acid solution quite colourless.

18. Grouping of Colours.

Having now considered some of the chief peculiarities of the most useful reagents, I proceed to describe what appears to me to be the most convenient method of dividing colouring-matters into groups and subgroups, so as to enable us to ascertain the nature of any particular substance under examination. The number of distinct coloured compounds met with in different plants is so great, that some such classification is imperative. In the first place, we cannot do better than divide them according as they are soluble in water or alcohol. This may be looked upon as a *chemical* division, and is very useful in practice. Thus—

Soluble in water and not precipitated by alcohol	Division 1.
Soluble in water and precipitated by alcohol	„ 2.
Insoluble in water but soluble in alcohol	„ 3.
Insoluble both in water and alcohol	„ 4.

Of course cases occur which cannot be unhesitatingly classed with any one of these; but they often form good practical divisions, and necessarily modify the methods requisite for further examination.

19. Method and Order of Experiment.

If a colour belongs to division 1, a small quantity, sufficient for three or four experiments, should be exposed to the vapour of ammonia in a watch-glass, until there is certainly no longer any *free acid*, and then gently evaporated, so that all excess of ammonia may be lost. If not thus made neutral we might be entirely misled; for some pink colours are blues reddened by an acid. A small quantity should then be dissolved in water in one of the small experiment-tubes and the spectrum observed. If too little colour has been added to give the characteristic spectrum, more should be introduced; but if any part is entirely absorbed, the cell should be turned sideways, in order to see whether or not some narrow absorption-band occurs there; and then it may be desirable to remove some of the solution, and fill up the cell with water. As a general rule, so much colour should be added as to make the darkest part of the spectrum decidedly shaded but yet not so black as to hide any narrow bands; and if any occur, the solution should be made of such a strength as to show them to the greatest advantage. This can easily be done, after a little practice, and is made much easier by being able to turn the tubes sideways. Having noted the spectrum of the neutral solution, a very small quantity of ammonia should be added, and then a decided excess, the spectra being examined to see if there be any

difference; for this is very often the case, and very important facts may be overlooked if too great an excess be added at first. The addition of a small bit of sulphite of soda then shows at once whether or not a colour belonging to group A is present; and on adding excess of citric acid we may also determine whether it chiefly belongs to groups B or C. Ammonia should then be added in excess, which may or may not restore it to the same state as before the addition of the acid. To another portion of colour carbonate of soda should be added, and then excess of citric acid, both spectra being carefully observed; and finally sulphite of soda, which definitely shows whether or not there is any other colour than one belonging to group C. Combining the results of the two sets of experiments, we may decide whether it belongs to groups A, B, or C, or is a mixture of any of them. If the substance is insoluble in water but soluble in alcohol, the same experiments should be made; only we must add the colour dissolved in alcohol to as much water as can be used without making the solution turbid, and must remember how much the presence of alcohol may interfere with the action of some of the reagents.

Another portion of the neutral colour should then be dissolved in as strong alcohol as will give a clear solution, and ammonia, benzoic acid, a little citric acid, and much of it added, one after the other, and all the spectra carefully observed, as well as any other facts which may present themselves.

By thus using three separate quantities of colour, and adding reagents one after the other, we may obtain about a dozen spectra, which may differ from one another in important particulars, or in some few cases may be all alike. The experiments are so easily made, that the whole series of twelve spectra may be seen in the space of five minutes; and the total quantity of material need not in some cases be more than $\frac{1}{10000}$ of a grain. The facts thus learned may show that for particular practical purposes some different method could be employed with advantage, and that only one or two simple experiments are needed. For example, suspected blood-stains should be treated in an entirely different manner, as described in my Paper on that subject*; and in examining dark-coloured wines, in order to form some opinion of their age from the relative quantity of the colour belonging to group C, gradually formed by the alteration of the original colouring-matter of the grape (belonging to group B), it is only requisite to observe the effect of sulphite of soda after the addition of citric acid. It would, however, extend this Paper beyond the limits I have prescribed to myself, if I were to enter into practical applications; and I shall therefore merely give a description of a convenient method of grouping the various colours.

20. *Subgroups.*

Since the narrow absorption-bands are decidedly the most important means of identification, it appears to me that we cannot do better than adopt subdivisions founded on *their number*. We may thus divide each group A, B, and C into subgroups, in which the

* Quarterly Journal of Science, vol. ii. p. 205.

neutral aqueous solutions exhibit 0, 1, 2, 3, &c. decided absorption-bands. Sometimes one of them may be so obscure that we may hesitate whether it should be counted or not; but practically this is no very serious objection, if we decide to reckon only distinct bands, and to look on the fainter as important merely in identifying individual colours. If no absorption-band can be seen in the neutral solution, we may take into account those seen when more or less ammonia is added; and if none occur in either case, we may make use of those seen in the alcoholic solution when neutral, and after the addition of ammonia. Whenever in this order of experiments the solution gives any decided absorption-band, the subgroup is determined; and it is only when none has been produced that the process must be carried further.

The general connexion of the subgroups will be best seen from the following Table:—

$$1, A \begin{cases} aq_0 \\ aq_1 \\ aq_2, \&c. \end{cases} \begin{cases} am_0 \\ am_1 \\ am_2, \&c. \end{cases} \begin{cases} al_0 \\ al_1 \\ al_2, \&c. \end{cases} \begin{cases} am_0 \\ am_1 \\ am_2, \&c. \end{cases}$$

The same system is applicable to each division, 1, 2, and 3, and to each group, A, B, and C. We can easily express the subgroups by using one or more of the signs *aq*, *am*, *al*, *am*, with a figure to indicate the number of bands in the first term that contains any, those before it being given to show the facts more clearly.

Each colour can be indicated by writing after the subgroup the characteristic spectrum, or, for the sake of simplicity, merely the position of the *centres* of the bands when they are seen as independent as possible of general absorption. If the centres of the bands are in different positions, the colours cannot be the same; but if they agree, it does not necessarily follow that they are the same: it is probable, but must be further proved by the correspondence of other spectra. As examples I give a few well-marked cases.

- Purple Pansy 1, A, $aq_0 am_1 (4)$.
- Crimson Rose 1, A, $aq_0 am_0 al_0 am_1 (2\frac{1}{2})$.
- Blue Lobelia (*L. speciosa*) 1, B, $aq_2 (2\frac{3}{4}, 4\frac{1}{4})$.
- Pink Stock (*Matthiola annua*) 1, B, $aq_2 (3\frac{5}{8}, 5\frac{1}{4})$.
- Several blue species of *Campanula* 1, B, $aq_1 (2\frac{3}{8}, 4, 5\frac{5}{8}, 7\frac{1}{4})$.
- Brazil-wood (*Cæsalpinia crista*) 1, C, $aq_1 (5\frac{1}{4})$.
- Logwood (*Hæmatoxylum campechianum*) 1, C, $aq_1 (4\frac{3}{8})$.
- Sandalwood (*Pterocarpus santalinus*) 3, C, $aq_2 (6, 7\frac{1}{2})$.
- Alkanet-root (*Anchusa tinctoria*) 3, C, $aq_3 (4\frac{1}{4}, 5\frac{3}{4}, 7\frac{1}{4})$.

21. Individual Colours.

Having then ascertained to which subgroup any particular colour belongs, it is in the next place requisite to determine what particular substance it is. When it gives rise to well-marked absorption-bands, this may be more or less definitely decided by their exact position and character—since they may of course occur in different situations, or vary much in absolute and relative breadth and in intensity. Thus, choosing closely related spectra, we have, for example:—

1, B, aq_2				
Blue <i>Lobelia speciosa</i>	$2\frac{1}{4}^*$	$3\frac{1}{4}$	$3\frac{3}{4}^*$	$4\frac{5}{8}$ 11 . . .
Pink <i>Matthiola annua</i>	$2\frac{3}{4}^*$	$4\frac{1}{4}$	$4\frac{1}{2}^*$	$5\frac{1}{2}$. . . 8 10 . . 11—

1, C, aq_1				
Logwood (<i>Hæmatoxylum campechianum</i>)	$3\frac{5}{8}^*$	$5\frac{1}{4}$	7	8—
Brazil-wood (<i>Cæsalpinia crista</i>)	$4\frac{1}{2}^*$	$5\frac{3}{4}$	7	8—

Such spectra are at once seen to differ most decidedly when compared side by side; and that the colouring-matters are entirely different is proved by other facts. If the absorption-bands agree very closely, we ought to compare other spectra before concluding that the substances are the same.

22. *Mixed Colours.*

Of course, if any impurity is present which absorbs that part of the spectrum where the characteristic bands occur, it may be difficult, or even impossible, to determine the nature of the substance; but the rest of the spectrum may be obscured, and the general colour entirely changed, without the least difficulty being thereby produced.

Thus, for example, on adding a solution of Saffron (*Crocus sativus*) to that of the blue *Lobelia*, the colour is changed from blue to a curious olive, and the spectrum becomes—

<i>Lobelia</i> and Saffron	$2\frac{1}{4}^*$	$3\frac{1}{4}$	$3\frac{3}{4}^*$	$4\frac{5}{8}$ 6½ . . 7—
<i>Lobelia</i>	$2\frac{1}{4}^*$	$3\frac{1}{4}$	$3\frac{3}{4}^*$	$4\frac{5}{8}$ 11 . . .
Difference				6½ . . 7—

If we did not know it, we might thus infer that they were the same substance, and only differed because one contained a yellow colour; and this conclusion would be borne out by adding to each citric acid and sulphite of soda, which make the *Lobelia* colourless, and leave the residual yellow colour, 6½ . . 7—, in the case of the mixture. The petals of very many flowers do really contain more or less of such a yellow, which appears to be that developed to a much greater extent in the stamens, &c.; and though this often modifies the general colour and the spectra, its presence may be recognized in a similar manner. Different species of *Dianthus*, various kinds of Roses, and *Digitalis purpurea* are good examples of one general colouring-matter modified in this manner. Its normal character is

$$1, A, aq_0 am_0 al_0 am_1 (1\frac{3}{4}^* - 2\frac{1}{4} \dots 4\frac{1}{2} \quad 11 \dots).$$

In studying mixed colours, so much depends on their special characters, that it would be difficult to give any other general rules; and particular cases do not form part of the plan of the present paper.

23. *Spectra with no Bands.*

The principal difficulty to be contended with in this method of qualitative analysis, is in the case of the subgroups where no decided absorption-bands can be developed by any of the reagents. They can easily be divided into the groups A, B, and C; but the difficulty

is to distinguish the separate colours, if we are not sure that they are pure and simple. Sometimes special facts may be of use; but, as a general rule, we are compelled to have recourse to the position and character of the general absorption. This requires a good deal of care, since a difference in the state of the solution may make the same colour differ more than two quite distinct colours. After trying a number of experiments, I find that the best spectra for comparison are those obtained by adding first a moderate excess of carbonate of soda, and then a considerable excess of citric acid. Both of these solutions change very slowly, and give well-marked spectra; whereas ammonia often causes decomposition, and weaker alkalis or acids give much more faint spectra, or such as rapidly fade. Closely related colours should be compared together, and made as nearly equal as possible after the addition of the carbonate, and then citric acid added in considerable, and nearly equal, excess. We thus can compare two different spectra; and even if the position of the absorption is the same in both cases, the relative intensity may vary considerably. Very closely allied colours may often be easily distinguished in this manner; and the only great difficulty is when coloured impurities are present. As an example, I give some colours belonging to subgroup 1, B, a_0, am_0, al_0, am_0 .

	Carbonate of soda.	Citric acid.
Petals of Wallflower (<i>Cheiranthus Cheiri</i>)	$2\frac{1}{4} \dots 5 \quad 8 \dots 9 \dots 10$ —	$3\frac{3}{4} \dots \dots 7$
Dark grapes	$2\frac{1}{4} \dots \dots 5\frac{3}{4} \dots 9 \quad 10 \dots 11$ —	$4\frac{1}{4} \dots \dots 8\frac{1}{2}$
Fruit of Elder (<i>Sambucus nigra</i>)	$2\frac{1}{2} \dots \dots 6 \dots 9 \quad 11 \dots$	$4\frac{1}{2} \dots \dots 8\frac{1}{2}$

The first differs entirely from the latter two; but they are so similar that it requires great care to be sure that they differ essentially. If it were quite certain that such colours were pure, it would not be difficult to distinguish them with confidence; but since they may contain coloured impurities, we must occasionally be content with results somewhat doubtful in questions of minute detail, which might not be of the least importance in some practical investigations.

24. Yellow Colours.

One of the best general methods of distinguishing yellow colours belonging to subgroup C, a_0, am_0, al_0, am_0 , or those with bands which are much alike, is to make them as nearly as possible of the same tint when neutral, and then to add excess of ammonia, which may make them very unequal. For example—

	Neutral.	Ammonia.
Yellow Dahlia (<i>D. variabilis</i>)	$8 \dots 9 \dots 10$ —	$3 \dots 4 \dots 4\frac{1}{2}$ —
Yellow Calceolaria (<i>C. aurea-floribunda</i>)	$7 \dots 9 \dots 11$ —	$6\frac{1}{4} \dots 6\frac{3}{4} \dots 7$ —
Saffron (<i>Crocus sativus</i>)	$7 \dots 8 \dots 11$ —	$7 \dots 8 \dots 11$ —

The action of ammonia thus shows that they differ very much, but at the same time the *Calceolaria* might be a mixture of the other two; and this would have to be decided by other facts.

25. *Fading of Group C.*

Sometimes in examining colours of group C, advantage may be taken of the different rate at which their acid solutions decompose and fade when a considerable quantity of sulphite of soda has been added to an acid solution. The two solutions should be made as nearly equal as possible in all respects; and then the rate of fading may prove that they are very different, or may show that one is a mixture. After fading, the addition of excess of ammonia may show valuable facts. For example, the colour of the root of the red beet (*Beta vulgaris*) is pink, but that of the leaves is red, the spectrum differing from that of the root merely in having the blue end much absorbed. On keeping acid solutions of both to which sulphite of soda has been added, that of the root becomes colourless, and that of the leaves yellow; and thus, considering that acid solutions of colours belonging to group C are very rarely pink, it is almost certain that the colour of the leaves is the same as that of the root, but modified by the yellow colour so common in leaves.

26. *Conclusion.*

Such, then, is a general outline of the method which I have hitherto found the most convenient in studying different colouring-matters, and for determining to what individual species any particular colour may belong. I need hardly say that it is just the sort of qualitative analysis to employ in detecting adulterations in many substances met with in commerce, as well as in inquiries where very small quantities of material are at command. By this method we might be able in a few minutes to form a very satisfactory opinion, or at least one that might meet all practical requirements; and even under unfavourable circumstances we might narrow the inquiry to a surprising extent; and if this can be said even now, surely further research cannot fail to make it most useful in cases where ordinary chemical analysis would be of little or no use.

XX. Intelligence and Miscellaneous Articles.

SOME OBSERVATIONS ON GLASS TEARS. BY E. REUSCH.

THE glass tears I have seen almost all contain larger or smaller hollow spaces. That these are essentially vacuous I ascertained in the following manner:—The external surface of a tear, the hollow space of which was pretty near the circumference, was removed by means of emery paper (emery with shell-lac); then, by means of a hard triangular steel borer, the outside coating was perforated under oil; at the moment of the perforation the hollow space became filled with oil, with the exception of a minute bubble, which is as likely to have arisen from air absorbed by the oil as from any gas previously contained in the tear.

The great force required to break off the tail seemed also remarkable. If its least thickness exceeded 2 to 3 millims., and if more-

over it was not very long, it could not be broken in the ordinary manner. I made a few experiments in order to ascertain the greatest tension at the time of the fracture. In an oval brass holder small holes were bored, through which the tail was introduced; the pear-shaped part projected sufficiently, and the part in the holder was fixed by means of gypsum. Two waxed threads fastened to the projecting part formed a small loop, on which, by means of a hook, a scale-pan could be suspended, into which shot fell in a thin stream from a small height. The following measurements were previously made:—the horizontal distance l of the hook from the holder; the axes a and b of the elliptical section in the neighbourhood of the holder, a being supposed horizontal. The experiment yielded the load P for the moment of the fracture. Denoting by R the greatest tension in the external parts of the section for the same moment, we have, by a known principle,

$$Pl = R \frac{\pi}{32} ab^2, \quad R = \frac{32Pl}{\pi ab^2}.$$

The following Table contains the results of three experiments, where R is given in kilogrammes to the square millimetre:—

	a .	b .	l .	P .	R .
	millims.	millims.	millims.	kil.	kil.
1. . . .	3·20	3·40	24	14	92·5
2. . . .	3·70	4·30	38	9·8	55·4
3. . . .	3·19	3·40	26·2	7·8	56·4

The values of R thus found are rather uncertain, but of a magnitude met with in Tables on the rigidity of metals only in the case of steel and iron.

Glass vessels containing water in which the pear-shaped part of a tear is held when it is broken off are, as is well known, frequently broken even when the sides are pretty thick. Still more interesting is the following experiment:—A glass tear is held by means of a lateral stand in the middle of a small opodeldoc bottle, and molten resin is poured into the glass. If now after the resin has quite solidified and cooled the projecting tail be broken off, the glass smashes with violence; the fragments of resin which have been in contact with the glass show deep fissures; but, what is the chief point, the glass tear itself forms a continuous whole, though rather loosely aggregated, which has detached itself like a kernel from the burst resinous shell. The surface of the pear, which of course has become opaque, exhibits a peculiar glitter. If the kernel be broken in pieces, a body is readily obtained consisting of a lower hemisphere and superposed cone, suggestive of certain forms of hail.

Parallel to the plane of contact of the cone there appears a tolerably regular separation in layers in the kernel; and since the surfaces of the cracks form in certain places small angles with the surface, the above-mentioned glitter is easily explained.

I restrict myself to the communication of the above facts. A perfectly satisfactory explanation of the causes of the hardness and brittleness of hardened glass and steel we do not possess, so far as I

know: as regards glass, Knapp's 'Chemical Technology' contains remarks from a professional point of view. Whether the explosive force of the bursting tears is a direct action of the tensions arising from sudden cooling, or whether the glass particles loosened by a wave of agitation impinge against each other like elastic bodies and are immediately repelled, is a further question the decision of which is closely connected with a clear view of the nature of hardened glass. More competent physicists may perhaps find themselves incited by my communication to devote their attention to the matter.—Poggendorff's *Annalen*, March 1867.

Tübingen, February 4, 1867.

EXPERIMENTS ON THE MAGNETISM AND DIAMAGNETISM OF
GASES. BY M. J. CHAUTARD.

All physicists are aware with what success the question of the magnetism and diamagnetism of gases has been treated and solved by Messrs. Faraday, Plücker, and E. Becquerel. My reason for advertising at present to a well-established scientific point is, that it has not entered into the course of instruction; and there are not, as far as I know, experiments which can be shown to a numerous audience, these phenomena being somewhat difficult of execution, while, on the other hand, they are never sufficiently marked to strike persons unfamiliar with these delicate investigations.

The use of soap-bubbles produced at the end of tubes of pipeclay has been successful, and in the case of *oxygen* has enabled me to obtain an energetic action which could be converted into a considerable oscillatory motion, by the successive magnetizations and demagnetizations of an electromagnet. I used Ruhmkorff's large electromagnet arranged for Faraday's experiments, and worked with a battery of twenty-five to thirty elements. The solution of soap is mixed with a certain quantity of glycerine (the proportions are the same as for Plateau's experiments); the tube of pipeclay is fixed in a clamp of such a height that the bubble formed at one end is above the poles of the magnet, and at a distance of 2 or 3 millims.; at the other end of the same tube is a caoutchouc tube connected with a bladder filled with oxygen: the experiment being ready, the bubble is illuminated by means of an oxyhydrogen light. A sort of magnetic gas-pendulum is thus formed, the movements of which in a large lecture-hall are as visible as those of the small iron pendulum.

Another perfectly easy experiment may be made with magnesium vapours, or rather the white cloud from burning magnesium. By burning the metal just below the conical polar extremities of the electromagnet, as soon as the current passes, the column of smoke divides laterally and takes the shape of a well-marked U. It is a very curious and easy lecture experiment.—*Comptes Rendus*, June 3, 1867.

Fig. 1

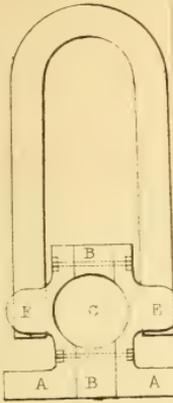


Fig. 10.

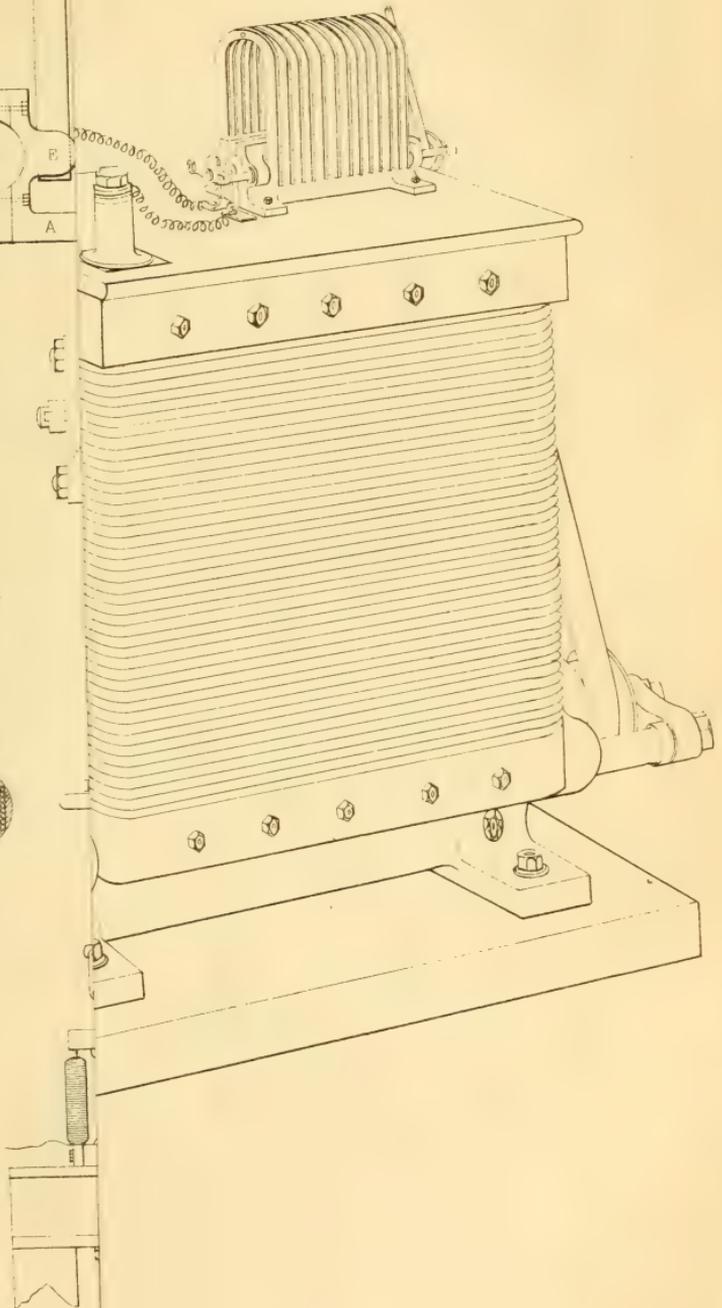
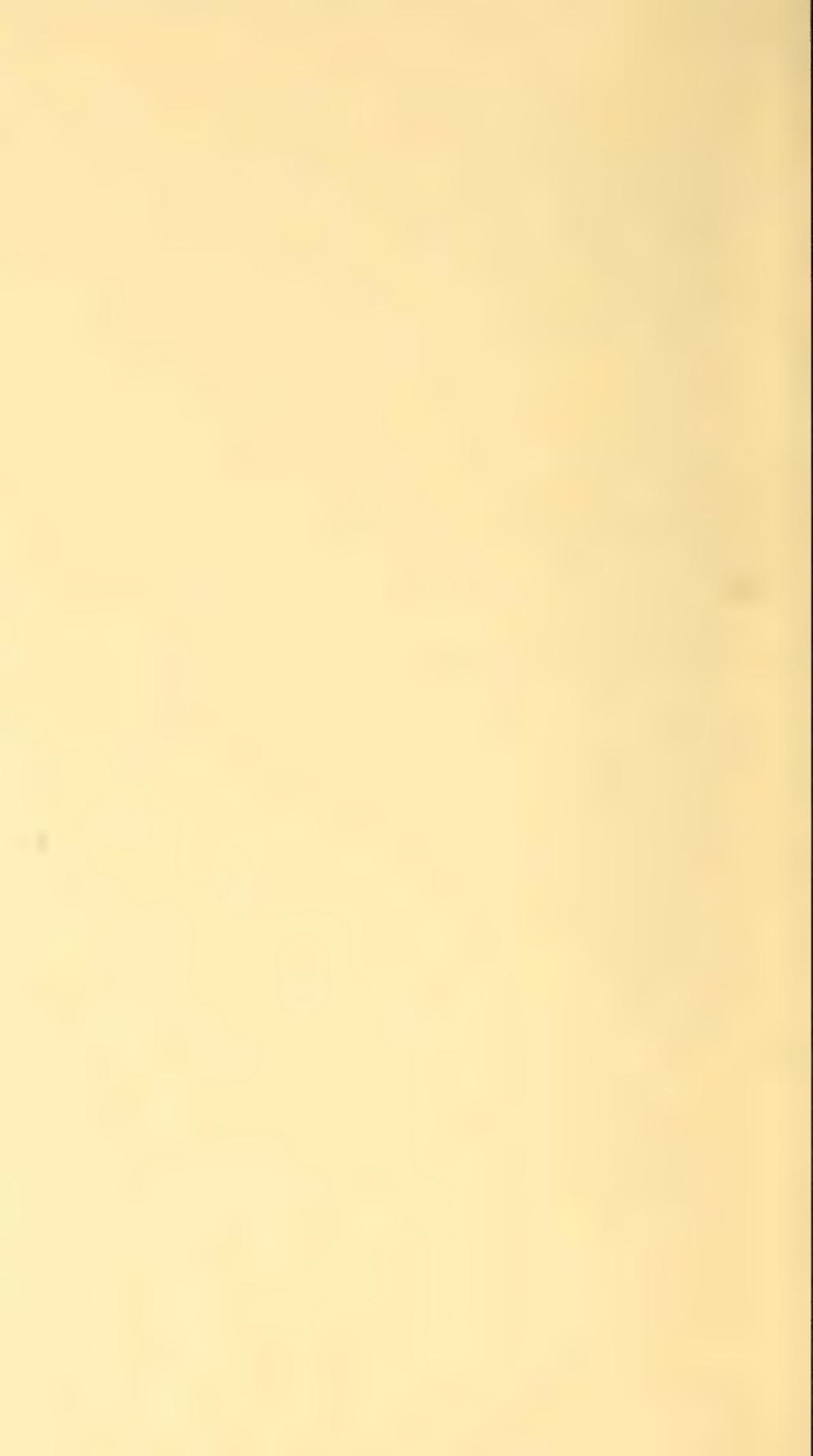


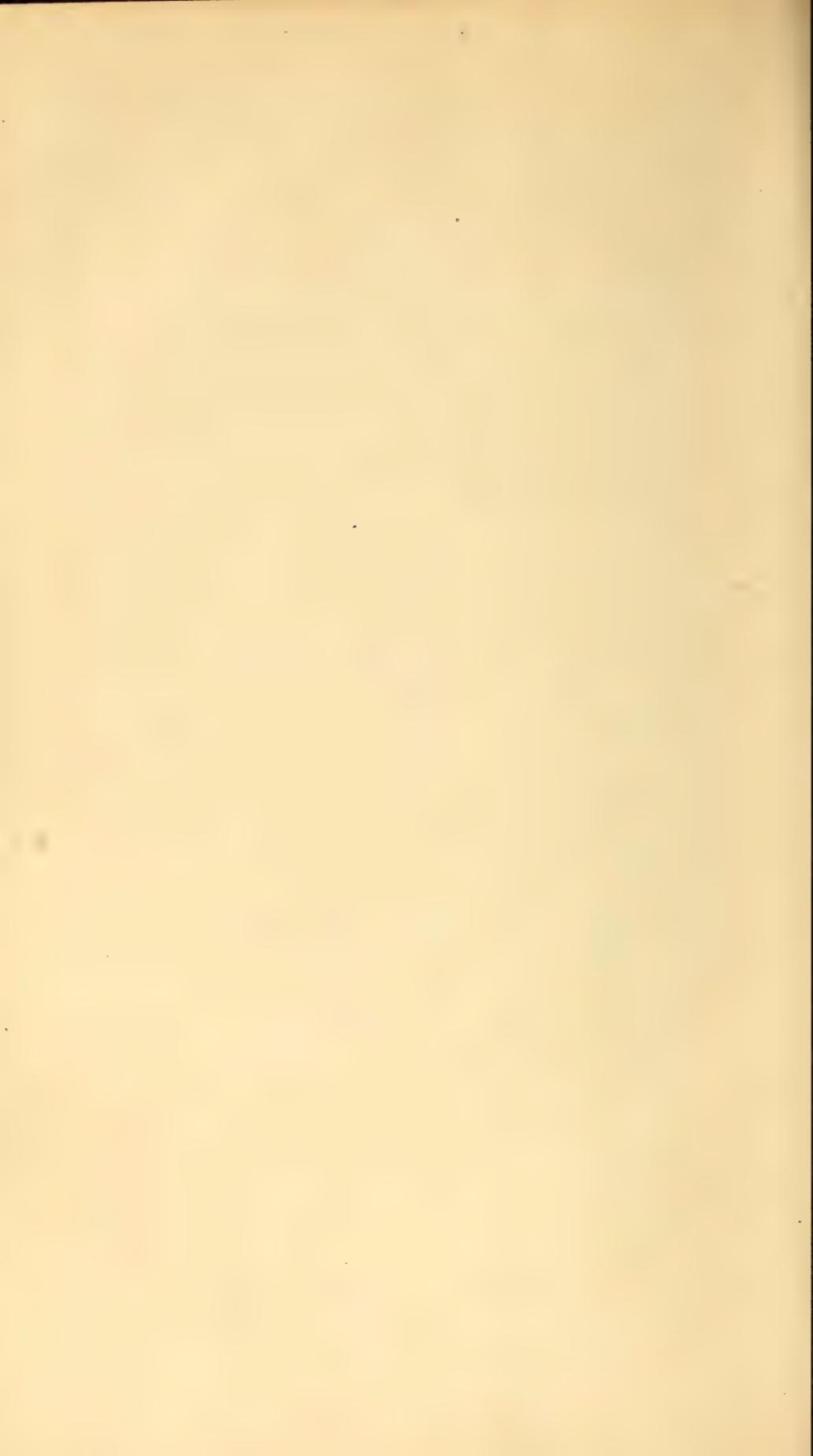
Fig. 8.



Fig. 9.







THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[FOURTH SERIES.]

SEPTEMBER 1867.

XXI. *On testing Telegraph Cables during the process of Sheathing.* By LOUIS SCHWENDLER*.

A TELEGRAPH cable, when complete, should be "*electrically normal*;" i. e. its resistances of conductor and insulator should correspond exactly with those of the different lengths of which it is composed, and which were tested before being joined together during sheathing.

The excellent method first suggested and applied by Messrs. Siemens in testing the Malta-Alexandria cable, gives us both the resistances of insulation and conduction for equal lengths of core at a standard temperature and pressure, expressed in the same unit, the period of time which elapses between connecting the battery to the cable and taking the reading being always the same. It has occurred to me, nevertheless, that at present we are not in a position to make a profitable use of these valuable tests to decide the normal condition of a cable satisfactorily. As regards the conductor, we are indeed able to do so; for not only can we measure its resistance very exactly, but we are also able to calculate the conductor resistance of the whole cable from those of its different parts; and if this calculated resistance (the temperature being taken into account) coincides with the measured resistance, we may consider the cable "*normal in its conductivity*."

But the same cannot be said of the insulation, which, however, is of greater importance. The insulation resistance of a cable

* Communicated by the Author.

is an unknown function of the time during which the battery circuit is closed and of the length, and also an unknown function of the temperature. We cannot, therefore, yet calculate the insulation of the whole from its different parts until we are in possession of these unknown functions. Thus at the sheathing-works, where the length of the cable increases daily and the temperature may also vary considerably, we are not enabled to judge whether the insulation be normal or not, in the same easy manner as we decided the question for the conductor. However, for some cables the dependence of insulation resistance on temperature has been determined empirically*; and with this function, and insulation-tests giving the maximum insulation for each length of cable, it would of course be possible to calculate the required insulation of the whole cable from its different parts reduced to a standard temperature. But such a calculation is always tedious; and maximum tests are required, which, especially for long cables, are not practicable at the sheathing-works. Besides this, I question if the calculation would be as exact as the high sensibility of the present testing-instruments and the great importance of the case itself require.

Here the electrician at the sheathing-works finds himself in a very unsatisfactory position. He has all materials collected with the greatest care during the manufacture of the core, he has all necessary means to measure even the smallest change in the insulation of the cable, but he is not able to use all this in such a way that he can say, without an elaborate calculation, at any moment during the sheathing-process, that the *insulation of the cable is normal*, i. e. corresponding with the measured conditions of its component parts.

On the other hand, we have a test for continuity during the sheathing-process by putting the conductor in circuit with an alarm which gives a signal as soon as the conductor breaks; while for insulation the only method which I have met with is that suggested by Robert Sabine†—to place a fixed point at some place on the galvanometer-scale, and let the pointer close the circuit of a delicate relay and battery whenever the deflection exceeds a certain amount. But the chief objection which I have to this is, that it cannot be applied simultaneously with the continuity-test.

* Experiments made by Messrs. Bright and Clark upon four coils of the insulated core destined for the Persian Gulf cable gave the empirical formula

$$R_t = R_\ell (0.8944)^{t-\ell},$$

nearly the same empirical curve as C. W. Siemens found for the Malta-Alexandria cable.

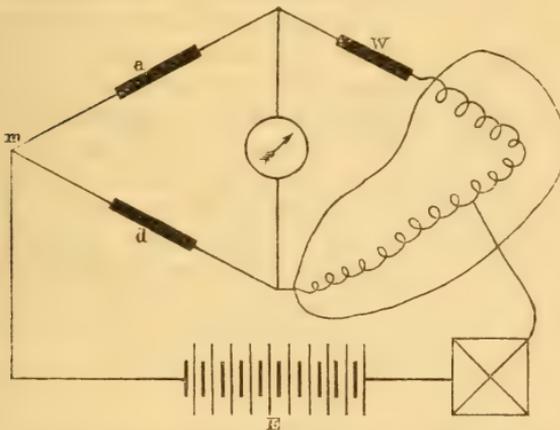
† The 'Electric Telegraph,' by Robert Sabine, page 404.

It will hardly be necessary to show that a permanent test for insulation during the hazardous process of sheathing would nevertheless be of far greater importance than the mere test for continuity.

Being entrusted at the cable-works of Messrs. Siemens with testing their cables, I began to search for a method which would not only give the insulation of a cable during manufacture at a glance, and be applicable by means of an automatic instrument during the sheathing-process, but be also on a level with the delicate instruments now employed for cable-testing.

This method, which I have had in practical operation for some time, is based on the well-known principle on which the determination of a fault depends when the two ends of a cable are at hand—known as the loop method. The two ends of the cable are connected with the ends of the galvanometer (forming two sides of a Wheatstone's balance), and the battery put in circuit, using the leakage of the whole cable as one entrance of the current, and the point *m*, fig. 1, as the other, *a* and *d* representing two branch resistances, and *W* an adjustable coil. The pole of

Fig. 1.



the battery connected with *m* is insulated completely from the earth. It is evident that, however the insulation of this cable be distributed throughout its length, there will always be for two given branch resistances *a* and *d* a fixed value of *W*, for which a balance in the galvanometer-circuit is established; and the magnitude of $\pm W^*$ indicates the position of that which we may call in future the "*resultant fault*," i. e. the fault resulting from all the partial faults of the whole cable. The resistance of this resultant fault is the absolute insulation of the cable. Supposing

* The minus value of *W* means, always, that *W* is to be placed at the other end of the cable before a balance can be established.

now that the cable be insulated equally at all points, and with a conductor also of homogeneous resistance, the resultant fault must evidently be in the middle, which would be indicated, in case we have $a=d$, by $W=0$.

In case a cable is not homogeneous, the resultant fault will not generally be in the middle; but knowing the order of the different parts, and their insulation and conducting resistances in the whole cable, and having besides a correct mathematical expression for the position of the resultant fault, which formula we shall develop afterwards, it will be possible to calculate this position with great accuracy. Knowing now the position of the resultant fault by a correct calculation as mentioned, and being at the same time able to test for this position exactly, the following rule may be set up as a necessary criterion for every *electrically normal cable*, assuming equal temperature throughout its length:—

The calculated position of the resultant fault must coincide with the tested position.

But by the scrupulous care bestowed upon the single lengths of core at the insulating-works, its manufacture has so much improved that the resistances of insulation and conductor come so nearly to a fixed standard at a certain temperature that we may in practice suppose a cable to be homogeneous, and, the temperature being the same throughout, express the above rule thus:—

A cable is electrically normal when the resultant fault lies exactly in the middle of the conductor.

It is true that this rule is necessary; but the fulfilment of the condition expressed does not give us entire security of the faultlessness of the cable—as, for instance, when two faults occur of equal magnitude and equally distant from the middle, which would evidently not affect the position of the resultant fault. The probability of this happening, however, is so small that the above rule will be sufficient in practice, especially when the present methods of testing insulation directly are applied in addition, and when an instrument which I have constructed for the purpose is employed, which is constantly testing the insulation and continuity of the cable during its sheathing.

The above method of testing cables during their sheathing, resting, as it does, on the test for homogeneity of the insulating covering (that is, on determining the position of the resultant fault by a simple balance in Wheatstone's diagram), has the following advantages:—

(1) This method furnishes practicable means by which it is possible to test the cables for insulation and continuity together permanently during manufacture. For this purpose the galvanometer in Wheatstone's diagram is used simply as a needle

relay. The instant a fault either in insulation or continuity occurs, the balance is disturbed and the needle closes the circuit of another battery containing an alarm so as to give a signal.

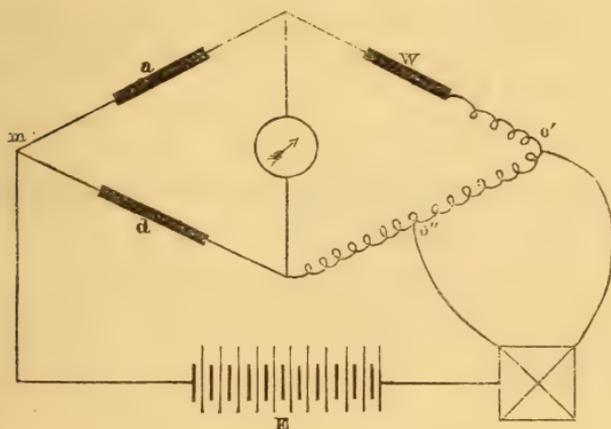
(2) The position of the resultant fault is independent of temperature so long as the latter remains uniform—also of the length of the cable and the time occupied in testing, *i. e.* of what is called electrification—and, further, of the electromotive force. Thus the position of the resultant fault may be calculated exactly and tested as nearly as the sensibility of the present galvanometers permit.

(3) By observing the position of the resultant fault every day and finding its maximum resistance, we are able to determine the position of a slight fault, which may afterwards occur, with great exactness.

As full proof of the existence of the above advantages is required, it will be necessary to enter into the theory of this method for testing cables; and this I shall endeavour to do as briefly as possible in the following.

Suppose, first, a cable to have only two faults, all other points being insulated absolutely; then by connecting such a cable according to fig. 1, we have fig. 2.

Fig. 2.

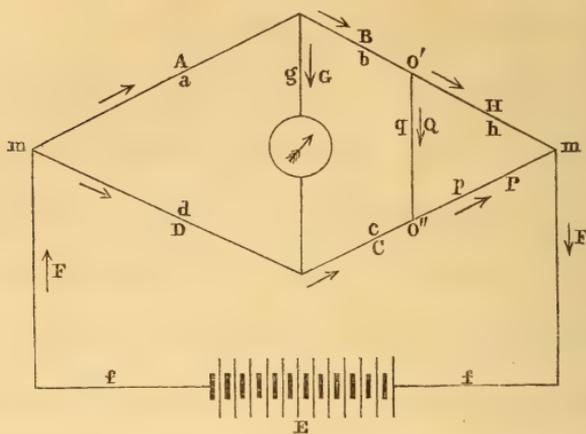


The current of the battery E enters through *m* on one side, and through *o'*, *o''*, representing the two faults, on the other.

Fig 2 is more clearly represented by fig. 3,—*a, b, c, d, g, f, h, p,* and *q* being the resistances of the different branches, and the respective capitals the different intensities, before balance in the galvanometer is established. These diagrams are evidently different from the one usually employed, where *o'* and *o''* fall together; and the first investigation will therefore be, what relation exists between the nine different resistances in case $G=0$,

or balance in the galvanometer is established. According to the two laws of Kirchhoff, we have eight independent equations be-

Fig. 3.



tween these different resistances and their respective intensities, viz. :—

$$\begin{aligned}
 A - B - G &= 0, & aA + gG - dD &= 0, \\
 C - D - G &= 0, & qQ + pP - hH &= 0, \\
 F - H - P &= 0, & gG + cC - qQ - bB &= 0. \\
 B - H - Q &= 0, \\
 Q - P + C &= 0, \text{ and}
 \end{aligned}$$

By eliminating seven of the intensities, with exception of F and G, and the latter developed, we have

$$G = F \cdot \frac{q(dh - ap) + (p + q + h)(bd - ac)}{(p + q + h) \{ g(a + b + c + d) + (a + d)(b + c) \} + q(p + h)(a + d + q)}$$

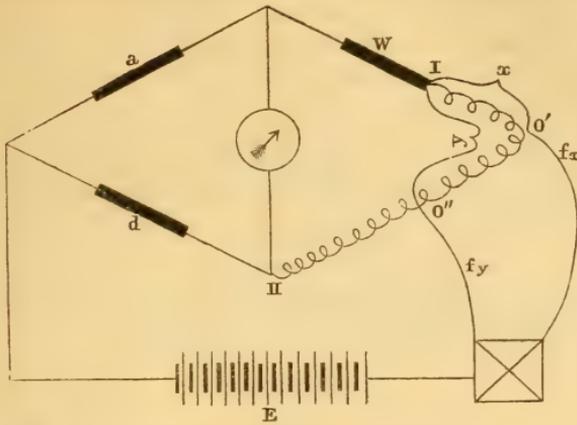
Supposing now $F > 0$, i. e. $E > 0$, we have to put, in case $G = 0$,

$$q(dh - ap) + (p + q + h)(bd - ac) = 0,$$

which equation gives the required general relation between the different resistances if balance is established; and it is evident that generally, when q , h , and p are definite quantities and larger than zero, the above equation is different from the simple law of Wheatstone's diagram. Reverting now to fig. 2, and calling the resistance of the fault in o' F_x , the resistance of the fault in o'' F_y , and x and y their respective distances, expressed also in resistances and measured from the same end I of the cable, we

have fig. 4. And supposing balance were established, we have,

Fig. 4.



in order to use the above equation, to substitute

$$\begin{aligned} b &= W + x, \\ c &= l - y, \\ h &= F_x, \\ p &= F_y, \\ q &= y - x, \end{aligned}$$

where l represents the resistance of the whole cable, thus:—

$$(y - x)(dF_x - aF_y) + (F_x + F_y) \{ (W + x)d - a(l - y) \} = 0.$$

Substituting $\frac{F_x}{F_y} = \alpha$ and developing x , we have

$$x = (1 + \alpha) \left(\frac{al - dW}{a + d} \right) - \alpha y, \quad \dots \quad (1)$$

which formula gives the position of *one* fault when the position of the other is known as well as the resistances of the two faults or the proportion of the two resistances only.

Proposing in formula (1) $\alpha = 0$, *i. e.* $F_x = 0$ or $F_y = \infty$, which is equivalent to having only *one* fault in the cable, we have

$$x = \frac{al - dW}{a + d}, \quad \dots \quad (2)$$

the known formula for the position of a fault if *only one* exists.

But where F_x and F_y are both definite quantities and larger than zero, we cannot put $\alpha = 0$, and therefore formula (2) would give quite another value of x than formula (1). Let us call this value z , and see what it amounts to in a case where we have in reality *two* faults. By comparing the two formulæ, it will be seen that as long as $y > x$, z is always larger than x and smaller than y , thus z indicates a position between the two faults o' and

o'' , where in reality no fault exists; but as W , the resistance at which balance in the galvanometer is established, must evidently be the same in both the formulæ, the balance would not be affected by moving the two faults from $o' o''$ to o ; and for this reason we may call that imaginary fault whose position is expressed by formula (2):—

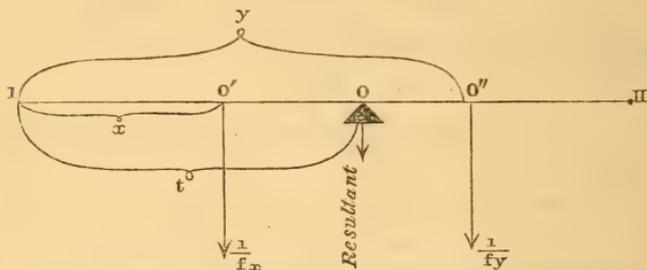
The resultant fault of the two real faults.

By eliminating W from both the equations, we have, by putting for α its value,

$$\frac{y-z}{F_y} = \frac{z-x}{F_x}, \dots \dots \dots (3)$$

which is a very simple and interesting relation between the two real faults and their resultant fault v : see fig. 5.

Fig. 5.



As $\frac{1}{F_x}$ and $\frac{1}{F_y}$ are the relative conductivities of the faults in o' and o'' , and calling in future the product of relative conductivity and distance from the resultant fault *the moment of a fault*, we may define, according to equation (3), the position of a resultant fault as follows:—

The resultant fault is that point for which balance of the moments of all faults is established.

The resultant fault of a cable is therefore a point similar to the centre of gravity; and we may infer directly that all formulæ which give the positions of the centre of gravity are applicable for the position of the resultant fault, substituting only for weight conductivity.

But it will be better to find the general formula for the position of a resultant fault directly, without referring to such analogy. If we develop z from equation (3), we have

$$z = \frac{yF_x + xF_y}{F_x + F_y}, \dots \dots \dots (4)$$

by the aid of which we can calculate the position of the resultant fault for any number of real faults, if their positions and resistances are known. Supposing a cable having n faults, of

which the resistances are $F_1, F_2, \dots F_n$, and their respective distances (expressed in resistance also) from the same end of the cable $x_1, x_2, \dots x_n$, then we may calculate by formula (4) the position of the resultant fault of the first two faults F_1 and F_2 ; from the first resultant fault and the third real fault F_3 , we may calculate in the same way the second resultant fault, and so on. At last we have calculated the position of the $(n-1)$ th resultant fault, which is in fact the resultant fault of all n faults. To have an algebraical expression for this, we will call P the product of the resistances of n faults, and F_x the resistance of a single fault whose distance from the one end of the cable is x ; and calling z the distance of the resultant fault of all n faults, expressed in resistance and measurement from the same end of the cable, we have

$$z = \frac{\sum_{x=x_1}^{x=x_n} \left(\frac{Px}{F_x} \right)}{\sum_{F_x=F_1}^{F_x=F_n} \left(\frac{P}{F_x} \right)}, \dots \dots \dots (5)$$

which may be easily calculated.

Supposing now the insulation of a cable to be a certain function of x the resistance of the conductor, which may be expressed by $f(x)$, then the resistance of a fault in each point of the cable will be

$$F_x = \frac{f(x)}{dx};$$

and thus we have, according to formula (5),

$$z = \frac{\int_0^l \frac{dx}{f(x)} \cdot x}{\int_0^l \frac{dx}{f(x)}}, \dots \dots \dots (6)$$

l being the resistance of the conductor of the whole cable.

If now a cable is insulated equally at all points, we have to substitute in formula (6)

$$f(x) = \text{constant};$$

thus

$$z = \frac{\int_0^l x dx}{\int_0^l dx} = \frac{l}{2};$$

or the resultant fault of a cable with uniform insulation is in the middle of the conductor resistance, which was evident *a priori*.

Having now all necessary formulæ, I may proceed to give the proofs of the advantages named under 1, 2, and 3, which shall follow in a subsequent article.

XXII. *On a Connexion between Crystalline Form and Chemical Constitution, with some inferences therefrom.* By JAMES D. DANA*.

AMONG oxides, the protoxides, like the metallic elements, are characteristically *isometric*† in crystallization. The sesquioxides are as characteristically *hexagonal*, this being the form of the sesquioxides of iron, aluminium, and chromium. The deutoxides are typically *tetragonal*, as seen in the deutoxide of tin (tin-ore) and of titanium (rutile and anatase). There are other forms among protoxides, sesquioxides, and deutoxides: for example, ZnO is hexagonal; TiO² in Brookite, and MnO² in pyrolusite are orthorhombic; but these cases, as the following observations make apparent, may be regarded as a consequence of polymerism—a principle that has been recognized by others as underlying dimorphism.

* Reprinted from a separate impression, communicated by the Author, from Silliman's American Journal for July 1867.

† I propose to employ in the forthcoming edition of my 'Mineralogy' the terms *Isometric*, *Tetragonal* (having a square base), and *Orthorhombic* (erect on a rhombic base), in place respectively of *Monometric*, *Dimetric*, and *Trimetric*. *Monometric* describes a line better than a cube; the hexagonal prism is as much *dimetric* as the square prism; and the oblique prisms are as truly *trimetric* as the right rhombic. It is very desirable that the technical terms of science should be uniform over the world, as far as possible, and that authors should be willing to yield their own usage for the sake of uniformity. The terms adopted appear to be the best that have been proposed, and have already extensive use in Europe. *Isometric* is Hausmann's term; *tetragonal* and *hexagonal*, with *rhombic*, are employed by Naumann. Mohs's terms *pyramidal* for the tetragonal system, and *prismatic* for the orthorhombic, are exceedingly bad, as there are pyramids among isometric, orthorhombic, and hexagonal forms, as well as the tetragonal; and prisms in all the systems excepting the isometric.

There is additional reason for our proposed change, in the natural relations of the systems of crystallization; for the similarity in the names *monometric*, *dimetric*, *trimetric* (the latter two the *monodimetrische* and *trimetrische* of Hausmann) implies a fundamental relation in the forms, while the true classification is as follows:—(1) *Isometric*, including the isometric system, peculiar in the absence of double refraction or polarization; (2) *Isodiametric* (from *ἴσος*, *equal*, and *diameter*), including the tetragonal and hexagonal forms (alike named from the shape of the base), characterized by equal transverse axes or diameters, and uniaxial polarization; and (3) *Anisometric* (from *ἀνισος*, *unequal*, &c.), including the remaining systems, and distinct in having the axes or diameters all unequal, and biaxial polarization.

Monoclinic, *Diclinic*, *Triclinic* (from Naumann) I would retain, as they express admirably the relations of the systems. *Clinorhombic* is often used for the monoclinic system, and is well enough. But *clinorhomboidal* for the triclinic would not be desirable, as the French commonly use the word *rhomboidal* where others use *rhombic*; and the *diclinic* could have no corresponding name, unless it be *clinorectangular*, which would be very objectionable.

Regarding the atom of oxygen as double in its fundamental nature, the number of atoms of oxygen (or the negative element) in the protoxides is 2; in the sesquioxides 6, or a multiple of 3; in the deutoxides 4.

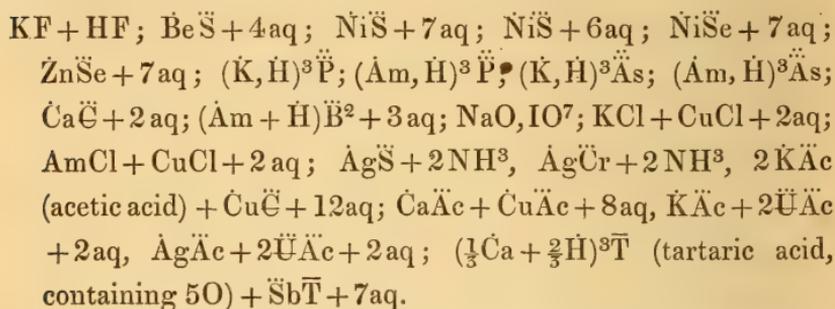
It appears, from a survey of all hexagonal and tetragonal compounds, to be a general fact that the number of atoms of the negative element is 3, or a multiple of 3, in the former; and 2, 4, or a multiple of 4, in the latter; and that consequently the hexagonal and tetragonal systems are based on these numbers respectively, their symmetry being a consequence of it.

1. *Tetragonal species, and the number 4.*—Among *unisilicates* (the silicates which have the ratio 1 : 1 between the oxygen of the bases and silica (SiO^2), and the number of atoms of oxygen 4, or its multiple) tetragonal species are common; while none occur among the *bisilicates*, in which the ratio is 1 : 2, and the number of atoms of oxygen is 3, or its multiple. There are none also among the anhydrous carbonates, which likewise have the oxygen-ratio 1 : 2. But among these bisilicates and carbonates there are examples of *hexagonal* species. The compounds $\text{Ca}\ddot{\text{W}}$ (Scheelite), $\text{Pb}\ddot{\text{W}}$ (Scheelite), $\text{Pb}\ddot{\text{Mo}}$ (Wulfenite), $\text{Y}^3\ddot{\text{P}}$ (xenotime) are tetragonal, the last having 8 of oxygen (or 16 if doubled) and the others 4. Matlockite ($\text{PbCl} + \text{PbO}$) is tetragonal, while $\text{PbI} + 2\text{PbO}$ is hexagonal, and $\text{PbCl} + 2\text{PbO}$ is orthorhombic. Cerasine ($\text{PbCl} + \text{Pb}\ddot{\text{C}}$) is tetragonal; and the number of atoms of the negative elements, O, Cl, is 4. Hausmannite is tetragonal, and, with the usual formula $\text{Mn}\ddot{\text{Mn}}$, has 4O. Yet the formula is better written $\text{Mn}^2\ddot{\text{Mn}}$; for this corresponds with its close relation in form to the RO^2 or deutoxide group, while $\text{Mn}\ddot{\text{Mn}}$ is a formula of the isometric spinel group. Similarly the tetragonal species chalcopyrite has the formula $2(\text{Cu}, \text{Fe})\text{S} + \text{FeS}^2$. Braunite, taking the most recent formula for it, that of Rammelsberg, $(\text{Mn}, \text{Si})^2\text{O}^3$, is apparently an exception. Its composition, as Rammelsberg shows, corresponds to $3\ddot{\text{Mn}} + \text{Mn} + \ddot{\text{Si}}$; and this formula has 12O, which is a multiple of 3, and satisfies the principle under illustration. But the true arrangement of the constituents makes it not a sesquioxide, as above, but a *deutoxide* like Hausmannite, which it approaches in its tetragonal form; for the formula may be $2\ddot{\text{Mn}}^2\ddot{\text{Mn}} + \ddot{\text{Mn}}\ddot{\text{Si}}$, which is equivalent to 2 of Hausmannite and 1 of a silicate analogous to the tetragonal species zircon ($\text{Zr}\ddot{\text{Si}}$)*. The deutoxide of manganese,

* Hausmannite approaches more closely the anatase form of TiO^2 than the rutile form, the angle between O and the plane made 1-i in anatase

MnO^2 (pyrolusite), is orthorhombic, and approximately isomorphous with orthorhombic TiO^2 (Brookite), the former having for the angles of the prism I , and the domes $1-\bar{i}$, $1-\bar{i}$, respectively $93^\circ 40'$, $104^\circ 22'$, $107^\circ 54'$; and the latter for the corresponding angles $93^\circ 16'$, $96^\circ 46'$, $99^\circ 50'$. MnO^2 in the tetragonal state is unknown except when it is in combination with 2MnO , as in Hausmannite. The protoxide of manganese, MnO , it may be remarked, is isometric, like MgO , it having been obtained artificially in octahedrons and cubo-octahedrons by Deville.

Among artificial compounds, there are the following tetragonal species all conforming to the principle stated:—



Omitting a few complex organic compounds, these are all the tetragonal species in the two volumes of Rammelsberg's 'Crystallographic Chemistry,' excepting $\text{Ag}\ddot{\text{C}}\ddot{\text{l}}$, Hg^2Cl . Other examples might be mentioned, but the above are fully sufficient.

The correspondence with the law for tetragonal species is so general that we may reasonably believe that the apparent exceptions, where the composition and crystallization are correctly given, may be brought into conformity to it by an application of one or the other of the following principles.

a. The principle of polymerism.— Hg^2Cl is Hg^2Cl^2 in the new system of chemistry; and if the whole is doubled, it becomes Hg^4Cl^4 , which is probably the true formula of this species in the tetragonal state, the only crystalline state yet known.

b. Part of the ingredients may be only accessory, or subordinate to a dominant part which determines the crystallization.—Water is commonly admitted to be present in this way in most of the compounds in which it occurs; although essential to the species, it is subordinate, crystallogenically at least, to the rest. Water is now believed to be not the only substance that may play the

being $119^\circ 22'$, and $O : 1$ (which might as well be $1-i$) in Hausmannite being $121^\circ 3'$. Braunite is much nearer cassiterite, rutile, and zircon, the corresponding angles for O on a pyramidal plane in these four species being respectively $135^\circ 26'$, $136^\circ 26'$, $137^\circ 40'$, $137^\circ 50'$. Thus the anatase and rutile form of TiO^2 are severally represented by Hausmannite and Braunite.

part of indifferentism in compounds; and many formulæ have of late been written by chemists admitting this. Apophyllite is a tetragonal species consisting of $\dot{R} + 2\ddot{S}i + 2\dot{H}$. Making the water basic, there is still no conformity to the type of either the unisilicates or bisilicates, the oxygen-ratio for the bases and silica being 3 : 4. If half the water be regarded as basic, and the formula be written $(\dot{R}, \dot{H})^2\ddot{S}i + \dot{H}\ddot{S}i$, it is made to consist of a dominant part which is a unisilicate analogous to the tetragonal species meionite, mellilite, &c., and a bisilicate which is a kind of opal or water-glass, well known to be a "colloid," or uncrystallizable, and which therefore might well have no effect toward modifying the crystallization as determined by the other part.

2. *Hexagonal species, and the number 3.*—Hexagonal species have been stated to occur among the sesquioxides (as Fe^2O^3 , Al^2O^3 , Cr^2O^3), the bisilicates (as in beryl, eudialyte, diopase, pyrosmalite, chabazite, Gmelinite), and the carbonates (in calcite, and the allied species), in which compounds the number of atoms of oxygen is 3 or a multiple of 3. Other examples are:—

Pyrrargyrite and Proustite, $3AgS + (Sb, As)^2S^3$, in which the number of atoms of sulphur is 6; Gibbsite, $\ddot{A}lH^3$; alunite, $\ddot{K}\ddot{S} + 3\ddot{A}l\ddot{S} + 6aq$; apatite, $3\ddot{C}a^3\ddot{P} + CaCl$; Coquimbite, $\ddot{F}e\ddot{S}^3 + 9aq$; $Al^2Cl^3 + 12aq$; $Mg\ddot{S} + 6aq$; $\ddot{A}l\ddot{S}^3 + 27aq$; $SrOS^2O^5 + 4aq$, and the corresponding salt of lime and of lead; $\ddot{K}\ddot{N}$; $\ddot{N}a\ddot{N}$; $(\ddot{C}a + \dot{H}^2)\ddot{C}$; $AgO, ClO^7 + 4\dot{H}$; $3NaCl + IrCl^3 + 24aq$; $KCl + 2MgCl + 12aq$; $MgCl + PtCl^2 + 6aq$.

The exceptions to the principle are to be accounted for in the same manner as those under the tetragonal system. Alongside of the hexagonal sesquioxides, Fe^2O^3 , Al^2O^3 , Cr^2O^3 , there is the hexagonal protoxide ZnO , similar in angle. Applying the principle of polymerism and writing the formula Zn^3O^3 , it then has, like the sesquioxides, 3 of O. This view of the protoxide is abundantly illustrated and sustained among the silicates. For the constitution of the larger part of them (garnet, scapolite, epidote, &c.) is based on the mutual replacement of 1 of sesquioxides (R^2O^3), and 3 of protoxides ($3RO$); and this mutual replacement signifies isomorphism of R^2O^3 and R^3O^3 . Again, graphite, or *hexagonal* carbon, has been shown to have its atomic weight nearly three times as great as that of ordinary carbon; and it is altogether probable, therefore, that in this hexagonal state carbon is C^3 , in accordance with the principle in view. Hexagonally crystallized water, on the same ground, is not HO , but H^3O^3 . ZnS occurs both in isometric and hexagonal forms;

and while the former may be simply ZnS, the latter should be $Zn^3 S^3$; and so for the hexagonal sulphide of Fe, Ni, Cd, we should have $Fe^3 S^3$ (troilite, pyrrhotine); $Ni^3 S^3$ (Millerite); $Cd^3 S^3$ (Greenockite); and similarly $Ni^3 As^3$ (copper nickel); $Ni^3 Sb^3$ (Breithauptite).

3. *Isometric system.*—The number of atoms of the negative element in isometric species appears to be either 1, 2, 3, 4, or a multiple of 3 or 4; and this diversity accords with the twofold nature of a cube; that is, (1) an equiaxial square prism, and (2) (if a diagonal be made vertical) a rhombohedron of 90° ; for it has this double relation to other forms. Accordingly, isometric forms occur among protoxides, protosulphides, protochlorides, &c.; also deutoxides; also unisilicates; in leucite, analcime; also in

$Mg^3 B^4$, or boracite; $\ddot{A}s$; $\ddot{S}b$; $\ddot{N}a\ddot{C}l$; $\ddot{K}\ddot{B}r$; $\ddot{N}a\ddot{B}r$; $\ddot{N}i\ddot{C}l + 6aq$;
 $Cu\ddot{C}l + 6aq$; $\ddot{C}o\ddot{B}r + 6aq$; $AmCl + SnCl^2$; $KCy + AgCy$;
 $3\ddot{N}a\ddot{C} + \ddot{C}r\ddot{C}^3 + 9aq$; the alums, which have 16O besides
 24O in the water; $\ddot{N}a\ddot{A}c + 2\ddot{U}\ddot{A}c$; $\ddot{N}a\ddot{W} + \ddot{W}\ddot{W}$.

Important chemical and crystallographic conclusions flow from the principle which has been explained, if it is sustained, as we believe, by the facts. A few only are briefly touched upon.

1. It follows that the hexagonal state of the *elements* may be one corresponding to $3R$, or $3nR$, that while zinc in the isometric state, if such exists (about which there is no doubt), is Zn, in the hexagonal it may be Zn^3 , the same state in which it exists in hexagonal oxide of zinc. So also Pd, As, Sb may represent the isometric state of the elements palladium, arsenic, antimony; but Pd^3 , As^3 , Sb^3 the hexagonal, and so for other cases.

2. The oxide of copper, CuO, which may also be written CuO^2 , is dimorphous, it occurring both in isometric and orthorhombic forms; and the orthorhombic form is closely isomorphous with TiO^2 in Brookite— $I : I$ and $I : \frac{1}{2}$ in the oxide of copper being respectively $99^\circ 39'$ and $126^\circ 29'$, and in Brookite $99^\circ 50'$ and $126^\circ 15'$. This relation to TiO^2 shows that the orthorhombic state of the cupric oxide should have the formula CuO^2 , or that of a deutoxide, and the isometric alone that of CuO. And it indicates further that the element copper may exist theoretically, if not actually, in two corresponding polymorous states.

3. As long since illustrated by Laurent, the protoxides RO, sesquioxides $R^2 O^3$, deutoxides RO^2 , and other grades of oxides RO^3 , RO^5 (and the same in corresponding chlorides, sulphides, &c.), in which 1 part of oxygen balances, in its affinity, 1, $\frac{2}{3}$, $\frac{1}{2}$, &c. parts of the basic element (as is seen on dividing by the num-

silicon has turned out to be only isometric silicon, we have no chance for a comparison, like that with respect to carbon.

Anatase is probably TiO^2 , and rutile Ti^2O^4 , the density of the latter being 4.2, of the former only 3.9. The relations of Hausmannite and Braunite (p. 179, note) accord with this, the latter containing *two of Hausmannite*. Brookite is intermediate in density, and in the temperature of origin, and hence may be $\frac{3}{2}(\text{TiO}^2)$. It would appear, therefore, that the species of *highest polymerous state*, rutile, forms at the highest temperature.

4. The views illustrated sustain the conclusion that the different states of elements represented above are fundamentally distinct—that Fe in the *alpha* state is related to all other metals that are in the same state, including K, Na (K^2 , Na^2 in the new system of chemistry), as well as Mg, Ca, &c.—that Fe, Cr, Co in the *beta* state are of the same group of elements with aluminium in alumina—that Fe, Mn, Cu, Pb in the *gamma* state should be classed with Ti, Sn.

5. Aid is given by the principle explained toward determining in many cases what are the accessory and what the dominant ingredients in a compound, and thence what should be regarded as its true constitution.

6. Crystallogeny hereby learns that *quadratic or tetragonal symmetry in crystals depends on quadratic symmetry, or the recurrence of fours, in the number of atoms of the negative part of a compound, and hexagonal symmetry, in like manner, on the presence of triads or hexads of the same atoms*. Moreover, on the view explained, the number of atoms of the more positive element or elements, in the simpler compounds at least, may be just equal to that of the negative. For since $3(\alpha\text{RO}) = \text{R}^3\text{O}^3$, $3(\beta\text{RO}) = \text{R}^2\text{O}^3$, and $3(\delta\text{RO}) = \text{RO}^3$, there are in these oxides as many atoms of αR , βR , δR as of O; and if the elements may exist in these divided states, they may thus make with the O the crystallogenic molecule.

The precise arrangement of the constituent atoms in a molecule subsisting in any case, and producing the characteristics and special dimensions of the crystal, yet remains to be explained. This much may be safely deduced:—that the negative atoms must be grouped (and, in the systems here referred to, under quadrate or hexad symmetry) at or toward one extremity of the molecule, and the positive at or toward the opposite; and that the molecule in this way derives its polarity—a characteristic abundantly manifested in the formation, the forms, and the physical natures of crystals, though not often apparent in mechanical effects, and which is in accordance with the most fundamental of nature's laws. The different constituent elements or parts of a compound may differ in *degree* of negativity or positivity; and even the

same element may be present in opposite states: such constituents would have their places accordingly, though with subordinate groupings, according to special affinities.

In order not to be misunderstood, I here state formally, what has been more than once implied in the foregoing, that while, according to the principle advanced, tetragonal and hexagonal forms depend on the numbers 4 and 3, as explained, the presence of these numbers by no means necessitates the occurrence of these forms. Multitudes of examples illustrate this: the dimorphism of TiO_2 is one. I would also remark that I express no opinion as to whether the molecule of a compound consists of the positive and negative atoms simply juxtaposed, or whether these so-called atoms are composed of particles, and there is a different disposition in the molecule,—and assert only that, whatever the fact on this point, there is tetragonal symmetry in the constitution of the molecule in the tetragonal system, and hexagonal in the hexagonal system.

I leave the subject here, without discussing at present the methods by which *orthorhombic* and *clinohedral* forms are produced,—only observing that orthorhombic and monoclinic forms occur under all numbers of atoms of the negative element, from 1 (or 2, as in sulphur) upward, and therefore, although polymerism may turn the 2 of sulphur (and, so, other numbers) into various multiples of the same, yet that the production of these forms does not depend simply on numbers.

XXIII. *The Conic Theory of Heat considered in connexion with General Sensation and the three senses of Touch, Taste, and Smell.* By the Rev. J. B. HARBORD, M.A.

HUYGHENS, in his advocacy of the undulatory theory of light, drew attention to the manner in which our various senses are affected by external objects, near and distant ones, pointing out as a probable conclusion that light is propagated in a similar manner as sound—by waves. Such an argument may not appear to be worth much now that the truth of the theory is established, but doubtless it helped at the time to mature and confirm a controverted hypothesis. While philosophers are still unagreed as to the exact nature of the motion of heat, it may not be altogether useless to direct attention to those of our faculties which take cognizance of heat and the effects of heat in the metamorphosis of matter. An examination of these will, I think, furnish considerable presumption in favour of such a theory as that of Rankine, which makes the gross molecules of substances revolve about undulating nuclei of æther.

* Communicated by the Author.

Phil. Mag. S. 4, Vol. 34, No. 229, Sept. 1867.

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What physiologists call general sensation or common sensibility is affected by radiant heat. This we know to consist of undulations of the æther, like those which convey the impression of light to the sight. They are freely propagated from a distance through space devoid of gross matter; and when a substance intervenes, whether this heat is reflected from it, transmitted through it, or absorbed in such a manner as to render the substance a new centre of radiation, in every case our common sensibility is affected at a distance. This confirms the idea that the æther retains its undulatory motion throughout its association with the gross molecules. The other part of the hypothesis is that these gross molecules have a kind of orbital motion round the æthereal undulating nuclei. We have submitted that these orbits are conic sections, and that upon the kind of conic section depends the physical condition of the material substance. Now, if it be true that the gross molecules revolve, and that their orbits undergo transformations as the condition of the substance is changed, we should expect that those senses which are specially adapted for the examination of the different forms of matter will require actual contact with the substance under examination. Undulations of gross molecules could be transmitted from a distance as in the case of sound; but the orbits of molecules which revolve can only be apprehended by close examination. Now we actually find that we have three cognate senses—touch, taste, and smell, severally adapted for the cognizance of matter in the solid, liquid, and gaseous states; and that with each of them contact of the nerve with the substance under examination is necessary. These two facts may require a little elucidation.

First, as to the several provinces of the three cognate senses. The sense of touch has reference to the solid condition of matter, the sense of taste to the liquid condition of matter, and the sense of smell to the gaseous condition of matter. The very form of the several organs might almost lead to this conclusion. The hand is formed to grasp at solids, and is like a basket for their transference, the mouth is shaped for the retention of liquids, and the nose can only be meant for the examination of gases. But let us direct our attention from the organs to the nerves. Putting aside the effect of sensible heat, which, as presenting a difference of temperature, is cognizable by all these three senses alike by the common sensibility which underlies them, we find that the nerves of touch are specially adapted to be acted on by matter in the solid state; those, for instance, terminating in the fingers require actual solid resistance to bring them into play. But is not, then, the sense of touch acted on by liquids and gases? The answer is—not *as* liquids and gases, but only by the resistance they present. Thus the hand placed on

the surface of water (putting aside the effect of sensible heat) only conveys information of the presence of the liquid by the resistance called into action by pressure. Then the molecules of the liquid seem to impinge on the sense of touch after the same manner as solids. And so, too, air in motion makes itself felt (putting aside the difference of temperature) by the resistance it opposes to the proper sense of touch. In addition to the above considerations, we may observe that it is by the sense of touch the mind is made acquainted with the size, form, and other external features of bodies, remarking that such attributes belong only to the solid condition of matter. Again, the sense of taste has especial reference to the liquid state of matter, its proper nerves being affected only by matter in that condition. Physiologists make the fundamental condition for the perception of taste to be the solution of the sapid substances in the secretions of the organs of taste. Substances to be tasted must either be in solution, or be soluble in the moisture covering the tongue; insoluble substances produce merely sensations of touch. Hence we should expect beforehand that the surface in which the sentient nerves of taste terminate should be moist in order that the action may be uniformly carried on. And this is the case. When the tongue and fauces are dry, sapid substances even in solution are with difficulty tasted. Finally, the sense of smell has reference to the gaseous condition of matter. It is only when a solid becomes volatilized that the olfactory nerves can be affected by it, and liquids can only reach this avenue of sensation in the shape of vapour. The nerves can only be impressed by substances in a state of extremely fine division in the atmosphere or gaseous exhalations. In the act of smelling the odorous matter is transmitted in a current through the nostrils. Thus this sense is brought into play by the ordinary respiratory movements, and the impression can be intensified by what is called sniffing. The manner, then, in which matter is conveyed to the nerves of smell is consistent with the view that this sense is specially intended for the gaseous condition of matter.

Secondly, in the case of all these three senses alike actual contact with the nerve by the substance under examination is necessary; we may, in fact, regard them as subsenses, and include them under the common heading of Tact. The prehensile shape itself of the proper organ of touch suggests that contact must take place before that sense can be called into play. We all know also that we cannot taste a thing except we put it into the mouth and a contact be effected with the gustatory nerves. Nor can the nerves of smell receive an impression from a remote substance through another, the matter to be examined must itself enter the organ: the olfactory nerves must actually be touched

by the material exhalations from the rose for the mind to become conscious of the sweetness of the flower; and a musk-rat's tail must part with a portion of its substance as long as it continues to convey an odorous impression through the nose to the brain.

The conclusion we draw from these facts is, as stated above, that the gross molecules of substances associated with æther transmitting radiant heat do not acquire an undulatory motion (which could be apprehended by the senses from a distance), but an orbital one. This orbit is such that it can pass through three several forms, which are distinct though cognate; and such are the conic sections—the ellipse, parabola, and hyperbola.

XXIV. *On the Connexion between Comets and Meteors.*

By G. JOHNSTONE STONEY, M.A., F.R.S.*

THE astonishing fact which Signor Schiaparelli brought to light some months ago, that there are comets moving in the tracks of the August and November meteors, compels us to infer that there is some intimate physical connexion between the two. In January last M. Leverrier pointed out that such a stream of meteors must have been in compact clusters when they underwent the great perturbations which brought them into permanent connexion with the solar system. And Mr. Graham has lately shown that the meteoric iron which reaches our earth had been at some previous time red-hot, and that when last red-hot it was acted on by hydrogen under considerable pressure—a pressure of perhaps six or more atmospheres. It is my present design to make use of these inferences as data, and to endeavour to trace by their help what the physical connexion between the comets and the meteors has been.

If interstellar space, external to the solar system, be, as is most probable, peopled with innumerable meteoric bodies independent of one another, a comet while outside the solar system would in the lapse of ages collect a vast cluster of such meteorites within itself. Each meteorite which approached the comet would in general do so in a parabolic orbit; and if it came near enough to pass through a part of the comet, this parabolic orbit would, by the resistance of the matter of the comet, be converted into an ellipse. The meteor would therefore return again and again, and on each occasion that it passed through the comet its orbit would be still further shortened, until at length it would fall in, and add one to whatever cluster had been brought to-

* From the Monthly Notices of the Royal Astronomical Society for June 14, 1867.

gether by the previous repetitions of this process*. In this way, a comet while moving in outer space, beyond the reach of the many powerful disturbing influences which prevail within the solar system, would inevitably accumulate within itself just such a globular cluster of meteorites as the November meteors must have been before they became associated with the solar system.

When this body of meteors, enveloped by their comet, swept past the planet Uranus in the year 126, they may have come so close that the comet brushed against the atmosphere of the planet. If this took place, the comet must have both received a motion of rotation and been retarded †. The meteorites at its centre retaining their speed would accordingly gradually pass out through it and leave it a little behind; and when all got so far from the planet as to be beyond its further influence, the comet would be found moving round the sun with a shorter periodic time than the meteors. This is in conformity with Dr. Oppolzer's determination of the periodic time of the comet, viz. 33·18 years, that of the meteors being 33·25 ‡.

* The behaviour described in the text is a consequence of the familiar formula for elliptic motion

$$V^2 = \frac{2\mu}{r} - \frac{\mu}{a},$$

since, if at any distance a resistance r be experienced, V is thereby diminished, and, as the formula must still hold good, a is also shortened.

† The cluster appears to have approached the orbit of Uranus from the outside, and, after passing the planet, to have described a relative orbit directed a little inwards towards the sun, but principally backwards, *i. e.* in a direction the reverse of the planet's motion, with a relative velocity greater than the velocity of the planet. It in this way acquired a slow absolute motion, which was directed both inwards and backwards, and was thus started in its retrograde orbit round the sun. A slight brush of the comet against the planet would both somewhat increase the curvature of the relative orbit, and slacken the comet's pace along it; and either of these effects would, under the circumstances which have been described, result in such a diminished absolute velocity as is attributed to the comet in the text.

‡ It should be remarked, however, that the comet seems to have fallen nearly a revolution behind the meteors since A.D. 126, *i. e.* in 52½ revolutions. If this be so, its periodic time must be less than Dr. Oppolzer's estimate, and is probably about 32·63 years—unless we may suppose that since its introduction into the solar system it has suffered a perturbation which has diminished its mean motion round the sun. Such a perturbation is not impossible; it would arise, for instance, if a swifter stream of meteors overtook the comet and passed through it; and it is easy to assure oneself that a swarm of meteors having the requisite direction and speed to behave thus may have been drawn into the solar system by any one of the planets Jupiter, Saturn, Uranus, or Neptune.

It is not without interest to observe that whether the periodic time of the comet be 33·18 years or less, it will, before its next perihelion passage, have been run into by the meteors. The effect of this would seem to be first to

The discovery of the Master of the Mint becomes now of exceeding interest, since it seems to show, first, that hydrogen is one of the constituents of comets; secondly, that the meteoric bodies he examined, when they originally joined their comet, fell in with a velocity sufficient to raise them by the friction they suffered to a red heat; thirdly, that the density of the comet was sufficient to occasion in front of the advancing meteorite a pressure of several atmospheres; fourthly, that when the meteors and the comet afterwards parted company, they glided asunder so quietly that the meteors were not again raised to any very high temperature; and, finally, that the friction they again encountered in passing through the earth's atmosphere was not sufficiently protracted to raise their internal parts to a red heat.

When the cluster of November meteors passed the planet which diverted them into the solar system, they were unequally acted on by it, the path of those which lay nearest being most bent. To this, as M. Leverrier has remarked, is to be referred their subsequently moving in slightly differing orbits with slightly different periodic times round the sun—which after the lapse of many revolutions has gradually extended them along their nearly common path, and will as time goes on still further lengthen out the stream. Hence the feeble gravity of the comet was not sufficient to restrain the meteors which were originally within it from yielding to these weak forces. The gravity of the comet accordingly cannot have been what kept the parts of its own mass from giving way to the same influences, and being (like the meteors) drawn out into a long thread. This is one of several considerations* which all point to the same conclusion—that a comet does not consist of matter merely held together by the mutual gravity of its various parts, but also coheres in virtue of some more powerful forces, perhaps not unlike those molecular forces which keep together the parts of a solid body.

It is remarkable that the principal meteoric streams which at the present day cross the earth's path have a retrograde movement, although the motion of most of the comets that are known to be periodic is direct. Perhaps this is to be accounted for by the earth's having exercised a more intense scattering in-

accelerate the comet at the expense of some of the *vis viva* of the meteors which pass through it, and finally, when the motion of the comet has been brought sufficiently into accordance with that of the meteors, to cause a gradual accumulation at the centre of the comet of those meteors which then happen to lie within the space occupied by it.

* Other grounds for this belief will be found in a Memoir on the Physical Constitution of the Sun and Stars, lately submitted to the Royal Society.

fluence upon whatever streams of meteors may have overtaken it than upon those which came in the opposite direction. The earth exerts an attraction which is competent to turn aside a meteorite of the former class through an angle of 50° , and to alter entirely its periodic time, but is too feeble to impress more than a trifling change on either the direction or period of bodies rushing past it with the speed of meteors moving in retrograde orbits*. Those chance metcors, therefore, which the

* The extreme cases will arise when a meteor passes the earth, either in precisely the same direction as the earth is moving, or in the opposite direction. In the former case it will approach the earth (assuming that the meteor moves in some large orbit, which is necessary, since it is only the great planets of the solar system, Jupiter, Saturn, Uranus, and Neptune, which can bring a swarm of meteors permanently in) with a relative velocity of about 0.4, in the latter case with a relative velocity of about 2.4 times the velocity of the earth in its orbit. Hence the relative orbit which the meteor will describe under the influence of the attraction of the earth will be hyperbolic; and the amount of deflection may be found as follows:—

From the equation of hyperbolic motion

$$V^2 = \frac{2\mu m}{r} + \frac{\mu m}{a}$$

it follows that

$$a = \frac{\mu m}{V^2},$$

where a is the semiaxis major of the hyperbola, μ the coefficient of attraction, m the mass of the earth, and V the velocity of the meteor at a sufficient distance from the earth to render $\frac{1}{r}$ negligible.

In the case of retrograde meteors

$$V = 2.4,$$

taking the velocity of the earth in its orbit as the unit of velocity. Again using the radius of the earth + the height of the earth's atmosphere as our unit of length, the distance of the sun is about 22400. Hence from the equation $V^2 = \frac{\mu M}{r}$ for the earth's motion round the sun, we find

$$\mu = \frac{22400}{M};$$

and, again, $\frac{M}{m}$ (the ratio of the sun's to the earth's mass) is about 324000.

Introducing these numbers, we find that

$$a = \frac{22400}{324000} \cdot \frac{1}{(2.4)^2} = \frac{1}{83}.$$

Now the deflection of a meteor's path in its relative orbit $= 2 \operatorname{cosec}^{-1} \frac{c}{a}$, and will of course be greatest when the meteor almost grazes the earth's atmosphere, *i. e.* when $c - a = 1$.

Therefore the maximum deflection $= 2 \operatorname{cosec}^{-1} 84 = 1^\circ 22'$. This is the

disturbing influence of the earth or other small planet has occasioned are probably, for the most part, the *débris* of streams which at one time were moving with a *direct* motion round the sun.

Since we have now abundant reason to believe that the great circular stratum within which the members of the solar system lie is traversed in all directions by numbers of these meteoric bodies, so vast that, as Professor Newton has computed, $7\frac{1}{2}$ millions large enough to be visible to the naked eye on a clear night, and 40 times that number of smaller ones, enter the earth's atmosphere daily, we are no longer called on to assume the existence of a resisting medium, or of a departure from the law of gravitation, to account for the retardation of comets. Meteors passing through a comet indifferently in all directions and with the same absolute speed, would operate upon it like a resisting medium.

XXV. On the Tension of Liquid Films.

By M. G. VAN DER MENSBRUGGHE*.

IN my first investigation "On the Tension of Liquid Films" †, I gave the laws which ought to be satisfied by the curve of a flexible inextensible thread without weight, and acted upon at its external surface simply by the contractile force of a liquid film in a state of equilibrium. It will be remembered that one of these laws is that the radius of curvature is the same at all points of the line in question. On the other hand, the osculating plane of the curve everywhere coincides with the tangent plane to the laminar surface. Now, if these two properties be combined, a

deflection as seen from the earth, and corresponds to an absolute deflection in space of $2^{\circ} 20'$.

On the other hand, in the case of a meteor overtaking and passing the earth,

$$a = \frac{22400}{324000} \cdot \frac{1}{(0.4)^2} = \frac{1}{2.314} ;$$

therefore the maximum deflection as seen from the earth

$$= 2 \operatorname{cosec}^{-1} 3.314 = 35^{\circ},$$

which corresponds to an absolute deflection of 50° .

The difference between these deflections far more than compensates for the circumstance that the earth would come across, and therefore have an opportunity of deflecting, about six times as many members of a retrograde swarm of meteors as of a similar one travelling in a direct orbit.

* Translated from the *Bulletin de l'Académie Royale de Belgique*, ser. 2. vol. xxiii. No. 5 (1867).

† *Bull. de l'Acad. Roy. de Belgique*, ser. 2. vol. xxii. p. 308. *Phil. Mag.* vol. xxxiii. p. 270.

very important conclusion follows, which has already been noticed by M. Lamarle* ; for, imagine that through a tangent to the curve a section is made normal to the surface. According to Meusnier's theorem, any oblique section whatever passing through the same tangent will have a radius of curvature equal to the projection, on the plane of this section, of the radius of curvature of the normal section. This being granted, if this theorem be applied to the section passing through the osculating plane of the curve, it is found that the normal section must necessarily have an infinite radius of curvature.

It follows thence that the curves formed by the flexible thread on liquid films are precisely those which M. Dupin has called *asymptotic lines*†, or those which Mr. Michael Roberts has called *generating lines*‡. There arises then the interesting question which forms the subject of the present research—that of ascertaining whether the properties of the lines of equilibrium of tension can be reconciled with the nature of the minimum surface operated upon.

We know that the surfaces of mean zero curvature may be represented, according to Monge, by the three following equations:—

$$\left. \begin{aligned} x &= u + v, \\ y &= \phi(u) + \psi(v), \\ z &= \sqrt{-1} \left\{ \int \sqrt{1 + [\phi'(u)]^2} du + \int \sqrt{1 + [\psi'(v)]^2} dv \right\} \end{aligned} \right\} . \quad (1)$$

where $\phi(u)$, $\psi(v)$ are any functions of the arbitrary quantities u and v . On the other hand, we have for the general equation of the asymptotic lines traced on minimum surfaces (see the memoir by Mr. Michael Roberts)

$$\frac{\phi''}{\sqrt{1 + \phi'^2}} du^2 = \frac{\psi''}{\sqrt{1 + \psi'^2}} dv^2; \quad . . . \quad (2)$$

ϕ' and ψ' , ϕ'' and ψ'' designate respectively the first and second derivatives of the functions $\phi(u)$ and $\psi(v)$. The question is to see in each particular case if the curves represented by equation (2) have or have not the same curvature at all points. In the first case the thread might be placed on the laminar surface without producing any deformation ; in the second case, on the contrary, the thread could not maintain itself on the film, or if it did, the form of the surface would necessarily be altered.

* Compare M. Lamarle's report on my first research (*Bull. de l'Acad. Roy. de Belgique*, ser. 2. vol. xxii. p. 272).

† *Développements de Géométrie*, p. 189.

‡ "On the Surfaces whose radii of curvature are equal but in opposite directions," *Journal de Liouville*, vol. xi. p. 302.

1. *Plane Surface.*

If we put $\phi''=0$, $\psi''=0$, we shall find, after having eliminated u and v in the equations (1), a result of the form

$$z = Ax + By + C;$$

consequently the minimum surface is then a plane. As the curvature of the lines formed by the thread must be constant, this latter will always assume a circular form. This I verified by numerous experiments in my first investigation.

2. *Skew Helicoid.*

Let us determine, in the second place, the functions ϕ and ψ . Taking

$$\phi(u) = \sqrt{-1} \sqrt{1+u^2},$$

$$\psi(v) = \sqrt{-1} \sqrt{1+v^2},$$

equations (1) give then, as we know,

$$z = -\text{arc tan } \frac{x}{y},$$

and represent therefore a skew helicoid; equation (2) becomes

$$\frac{du}{\sqrt{1+u^2}} = \pm \frac{dv}{\sqrt{1+v^2}}.$$

In order more easily to integrate this differential equation, let us put

$$u = \sqrt{-1} \sin \lambda, \quad v = \sqrt{-1} \sin \mu,$$

we shall thus have

$$x = \sqrt{-1} \{\sin \lambda + \sin \mu\},$$

$$y = \sqrt{-1} \{\cos \lambda + \cos \mu\},$$

$$z = -(\lambda + \mu),$$

$$d\lambda = \pm d\mu.$$

If we take the sign $-$, we have $\lambda + \mu = -z = \text{constant}$. We obtain thus all the rectilinear generating lines of the helicoid. If we take, on the contrary, the sign $+$, we find

$$\lambda - \mu = C; \quad x^2 + y^2 = -4 \cos^2 \left(\frac{\lambda - \mu}{2} \right) = -4 \cos^2 \frac{C}{2}.$$

Making $C = \pi + 2\alpha \sqrt{-1}$, we find

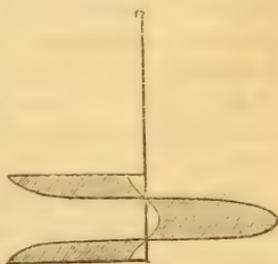
$$x^2 + y^2 = (e^\alpha - e^{-\alpha})^2,$$

a formula which represents, besides the axis of the helicoid, the totality of all the helices of the same pitch which can be traced on the surface; we know, on the other hand, that the helix has a constant curvature. Hence it follows that the lines of equi-

brium of tension on the skew helicoid are the rectilinear generating lines, the axis, and the helices of the same pitch as the directing helix*.

In order to submit these theoretical deductions to the test of experiment, I produced the surface in question in the shape of a film. M. Plateau, applying a general principle which he gave in one of his memoirs†, has produced this figure and pointed it out in a research which is as yet unpublished; but he has authorized me to use it and describe it here. A helix (fig. 1) is made of iron wire, the distance between the turns of which is 30 millims. for example, and which belongs to a cylinder of 45 millims. radius. Any number of turns of this helix may in general be taken; but for the present purpose it is best to work with only one turn. The ends of this latter are joined by straight iron wires to another straight wire, serving as axis of the helix and furnished with a prolongation, by which the skeleton can be easily immersed in the glycerine liquid. It is clear that we have obtained a closed contour belonging to a skew helicoid; and according to Plateau's principle we should obtain a helicoidal film when the skeleton is withdrawn from the liquid: this is confirmed in every respect by observation.

Fig. 1.



Since the verification of the theory requires in the present case very accurate experiments, I thought it necessary to give up using silk or cotton threads; for their weight, together with that of the liquid in which they are immersed, is too great to be disregarded; if, therefore, there were a change in the shape of a laminar surface, it might be either wholly or partially attributed to the disturbing cause arising from the weight of the moistened thread. Hence I have had recourse to threads out of a cocoon, the weight of which may be entirely neglected; these threads, to be sure, are not inextensible, and therefore short lengths only must be used.

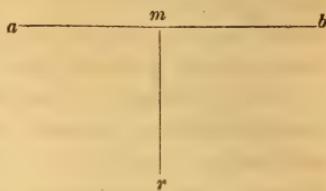
I pass now to the description of the experiments. And, first, I have never succeeded in reproducing the rectilinear generating lines of the helicoid: when, after having stretched a cocoon-thread in the direction of one of these generating lines, I produced a laminar surface, and then burst the portion of the film on either side of the thread, this always formed a concave line on

* This result is announced in M. Lamarle's report.

† 'Recherches expérimentales et théoriques sur les figures d'équilibre d'une masse liquide sans pesanteur,' 7th series, last paragraph (*Mémoires de l'Académie*, vol. xxxvi.).

the outside of the liquid film. This, indeed, should be the case; for let m (fig. 2) be a point of the thread ab supposed to be straight; this point is acted on by three forces, that is, by the two equal and opposite tensions of the thread, and by the resultant mr of all the elementary actions of the tension of the liquid on m , a resultant which is normal to ab . Now it is clear that these forces can never be in equilibrium unless the two former be infinite. As this condition cannot be fulfilled, the two elements terminating in m must necessarily bend towards the film. The same thing takes place at all points of the flexible thread; the curve obtained should be concave on the exterior of the figure, as I have always observed.

Fig. 2.



The same reasoning shows, again, why I have not been able to replace the solid skeleton by a stretched cocoon-thread; this, instead of remaining straight, always formed a helicoidal line which had an extremely feeble curvature.

It follows from the preceding that every line of equilibrium formed by a flexible thread and partially bounding a laminar surface must be concave on the outside; the curves of equilibrium which satisfy this condition are thus the only realizable ones; this remark must never be lost sight of in the present experiments.

I have effected the formation of a helix of equilibrium in proportion as experiment corresponds to all the restrictions of theory. I work in the following manner:—I begin by seeking the length of a spiral which would be traced on a cylinder of 6 millims. radius, for example, and which has the same pitch as the directrix helix of the skeleton I use (this pitch, measured by the cathetometer, is 31.1 millims.); the length in question is then equal to that of the hypotenuse of a right-angled triangle whose two other sides are respectively $\pi \times 12$ millims. and 31.1 millims., that is, to 48.9 millims. Consequently I take a cocoon-thread as exactly of this length as possible when moistened by the glycerine liquid, and by means of a little wax I fix the two ends at 6 millims. from the axis, at two points of the solid wires representing the extreme generators of the helicoid: in this operation care must evidently be taken not to attach the second end of the thread, before having made this describe an entire turn about the solid axis in the direction indicated by that of the director helix. I then immerse the skeleton in the glycerine liquid, and on withdrawing it I obtain a helicoidal film on which floats a cocoon-thread. By means of a point of filtering-paper I

then break the laminar portion comprised between the axis and the cocoon-thread, so that the concavity of the curve obtained shall be on the outside of the film; tension is then exerted along the entire length of the thread, which folds itself in a helix that appears perfect (fig. 1).

To ascertain if in this operation the remaining part of the surface is altered, I attach the stem of the skeleton to a support, and before breaking the film, and thus producing the helix, I arrange the flame of a candle so that by the reflexion of the rays on a point at a distance of 7 or 8 millims. from the axis I obtain a brilliant image in the focus of the telescope of a cathetometer. When this image is exactly in the centre of the cross wires, I form the helix as above and again observe the image of the flame; I observe that it is displaced, but to so small an extent that the deformation can be ascribed to the slight extensibility of the thread, which slightly mars the regularity of the curve. If, on the contrary, one of the ends of the cocoon-thread be ever so slightly removed to or from the axis without touching the other end, the image of the flame undergoes a considerable displacement, showing clearly that the film has undergone a very marked deformation. Hence I conclude that on a skew helicoid the only curves of equilibrium of tension at all realizable by experiment are helices (traced on cylinders) the common axis of which coincides with that of the surface in question.

I will here make two remarks. In the first place, I take the point where the rays of the candle impinge at 7 or 8 millims. from the axis (that is, close to the helix produced), so that, after breaking the laminar portion between the axis and the thread, I may be better able to ascertain the ultimate deformation of the surface: this precaution is necessary; for, as I shall afterwards show, the alteration of figure is greater the nearer we approach the cocoon-thread. In the second place, if, instead of bursting the portion of the film between the axis and the flexible thread, we break that between the latter and the director helix, we observe the thread suddenly thrown along the axis of the figure: this effect clearly arises from the fact that the tendency of the residual film to present the least possible surface can then be freely exerted at the moment the film is destroyed.

After having investigated the lines produced with cocoon-threads whose ends are fixed to two points of the solid skeleton, it may be proposed to produce in a helicoidal film an aperture bounded by a thread and forming a closed contour: the experiment always succeeds when the thread is not too long—though the film then no longer represents a helicoid, as can easily be ascertained by the method described above. It is proved in this manner that the deformation undergone by the film is the

more marked the more strongly curved the part in which the aperture is situated. The question suggested itself, if the film does not acquire a greater area in proportion as it is more profoundly altered, and hence does not oppose an increasing resistance to this augmented surface in proportion as this further removes the figure from the form of original stable equilibrium. This supposition has been justified by the following experiment. On the helicoidal film a cocoon-thread, 50 to 60 millims. in length, is arranged, the two ends of which have been tied; after having burst the film interior to this contour, the thread, by the effect of its weight, may be moved to all parts of the surface except near the axis of the helicoid: it may, indeed, be brought by force to this part of the film; but then as soon as it is free it moves from the axis, spite of its weight, and regains the portions with weak curvatures.

3. Catenoid.

If, as a third hypothesis, we put

$$\phi(u) = \sqrt{1-u^2}, \quad \psi(v) = \sqrt{1-v^2},$$

equations (1) represent, as we know, a catenoid, and the asymptotic lines are given by the following relation, where r, ω represent the polar coordinates, and α an arbitrary constant,

$$r = \frac{a}{2} (e^{\omega-\alpha} + e^{-\omega+\alpha}).$$

But by a general property demonstrated by Mr. Roberts, these lines cut the meridian curves of the catenoid at an angle of 45° *. Thus they are helicoidal curves, the spires of which are larger the further they are from the circle of the gorge; they cannot, therefore, give in all their points the same radius of

* See the memoir by Mr. Roberts, p. 312. By this property the above relation is directly obtained; for, on the one hand, the meridian catenary is represented by the relation

$$r = \frac{a}{2} (e^{\frac{z}{a}} + e^{-\frac{z}{a}}),$$

on the other hand the equation of the trajectory forming at each point an angle of 45° with the meridian line is given by the formula (see the memoir of the Abbé Aoust, "On the Trajectories which cut the Meridian Curves of Surfaces of Revolution at a constant Angle," *Liouville's Journal*, vol. xi. p. 184)

$$d\omega = \frac{dz}{r} \sqrt{1 + \left(\frac{dr}{dz}\right)^2} = \frac{dz}{a},$$

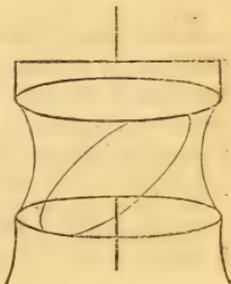
whence

$$d\omega = \frac{z}{a} + \alpha, \text{ and } r = \frac{a}{2} (e^{\omega-\alpha} + e^{-\omega+\alpha}).$$

curvature*, and consequently are not curves of equilibrium of tension. In order to verify this result by direct observation, I use the two rings described in my former note; they are 70 millims. in diameter; the upper one is provided with a suspender, the lower one resting on three feet. Suppose, for instance, the distance of these rings is 40 millims., I seek the length of the portion of the asymptotic line drawn from one to the other; for this purpose the portion of the meridian catenary comprised between the same rings may be divided by the cosine of 45° †; I thus obtain for the desired length nearly 63 millims. On the other hand, it is easily found that the meridian planes passing through the extremities of this portion of the asymptotic lines form an angle of about 86° .

This being granted, the following is the way in which the experiment is made. I fix the ring with a suspender to a support, and I make it quite horizontal; below it I place the ring with feet on a horizontal glass plate, so that the vertical distance of the rings is 40 millims., this distance being reckoned from the middle of the thickness of each wire. Then by means of a piece of wax I attach one end of a cocoon-thread, 63 millims. in length, moistened with glycerine solution, to a point in the upper ring, and the other to a point in the lower; I then fix the two ends of a second cocoon-thread of the same length to two points at a distance of 10 millims. from the first, and both situated on the same side of them. This being done, I bring the lower ring almost in contact with the upper one, and fill the intervening space with the glycerine liquid; I then carefully lower the tripod ring, taking care that the threads are completely immersed in the catenoidal film which is formed; when this ring is on the glass plate, I measure the diameter of the circle of the gorge of the film by means of a horizontal cathetometer. I then move the tripod ring 20 millims. from the upper ring, burst the film comprised between the cocoon-threads, and then very slowly lower the lower ring to the plate; I turn it about the axis of figure so as to establish an angular distance of 86° between the azimuths of the points of attachment of each of the two cocoon-threads. At this moment these form two curves of the same form, but arranged in inverse directions (fig. 3); these curves, as far as the eye can judge,

Fig. 3.



* In the report already mentioned, M. Lamarle gives as the value of this radius of curvature $T\sqrt{2}$, T being the part of the tangent to the meridian from the point under consideration to the axis of revolution.

† See the above-cited memoir of M. Aoust.

have the same curvature at all points. But the film is then no longer a catenoid; this is at once seen when the length of the line of gorge of the new figure is measured; I find that this length, which varies with the azimuths, may exceed by 2 millims. the diameter of the circle of the original gorge; the surface, therefore, is materially different from a catenoid*.

It remains now to be seen whether the deformation observed in a film after the breaking of a portion bounded by a flexible thread is simply local, or if it extends to the entire surface. In order to clear up this point, I made the following experiments:— I tied the ends of a moistened cocoon-thread so as to produce a closed contour 92 millims. in length; I laid it on a catenoidal film the diameter of the circle of the gorge of which I had found to be equal to 54.60 millims. Having then broken the inner film of the cocoon-thread, I saw it arrange itself in the direction of a skew curve, one part of which was very near the line of the gorge. I placed then the plane of symmetry of this curve successively in different azimuths, and noted each time, after measuring with a horizontal cathetometer, the breadth of the line situated in the plane of the circle of the original gorge. The following are the results obtained in this manner (the azimuth α of the plane of symmetry of the curve is reckoned from the vertical plane passing through the axis of the telescope of the cathetometer, and l denotes the corresponding breadth):—

α .	l .
	millims.
0	54.80
45	54.95
80	55.25
100	55.15
155	54.90
180	54.75

These numbers show very clearly that the deformation of the catenoidal surface is very considerable in the immediate neighbourhood of the cocoon-thread, and that it becomes less the greater the distance from this thread. This fully agrees with the experiments already described in reference to the skew helicoid.

It follows thence that, when a laminar portion limited by a flexible thread is broken, a new minimum surface is formed,

* I said in my former research that by the presence of a silk thread submitted to the tension of a catenoidal film, the surface did not seem altered; I had not made accurate measurements; moreover, if I had then observed a deformation, I should have attributed it, as I have already said, at any rate in part, to the weight of the moistened thread.

which obeys all the conditions of equilibrium previously enunciated, and sensibly coincides, at a greater or less distance, with the original surface.

I should have greatly liked to be able to apply the experimental proof to other surfaces, of mean zero curvature, but I have been prevented from this by the extreme complexity of the calculations; yet the examples investigated above will justify the following conclusions:—

Whenever a laminar surface bounded either wholly or partially by a flexible thread is broken, the original surface of the film is markedly altered when the asymptotic lines have not the same curvature at all points; this alteration takes place even when we try to make this thread coincide with an asymptotic line.

If, on the contrary, the asymptotic lines have everywhere the same radius of curvature, and the thread can moreover fold itself in their direction, the film undergoes but a very slight deformation, which appears to arise from the fact that the thread, not being extensible, yields a little to tension and ceases to coincide exactly with the asymptotic line.

In all cases the thread forms a curve whose radius of curvature appears on inspection to be the same at all points.

In terminating this note I may be permitted to point out a very curious effect of the tension of liquid films.

A plane liquid film is produced in a ring provided with a suspender, and this is placed in a vertical plane; a very thin hollow glass bulb, 8 to 12 millims. in diameter, and weighing 10 to 20 milligrammes, is then moistened with glycerine liquid; it is gently placed in contact with the lower part of the film; this immediately advances in the direction of the bulb and forms a curved surface, which rests on one side on the ring, and on the other on a small circle of the sphere parallel to the plane of this ring.

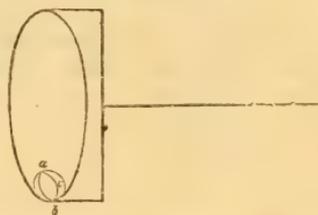


Fig. 4.

The centre of the bulb is gently moved towards this plane; as this takes place, the film moistens the sphere on a continually increasing circumference, which finishes by being the circumference of a large circle. If the bulb be allowed to rest on the solid contour and be left to itself, it remains perfectly in equilibrium (fig. 4). The ring may even be rapidly turned about its axis without bursting the film or detaching the hollow sphere.

If the bulb is ellipsoidal instead of being spherical, the ellipsoid turns until it is cut by the plane of the film in the direction of its maximum section, which, once plunged in this film, re-

mains constant there in spite of the rapid rotation of the ring. The least diameter of the ellipsoidal bulb which I used was 15 millims., and the maximum 20 millins.

I repeated the same experiment with a helicoidal film; the bulb remained at one point of the solid helix, the entire surface of which it could traverse without leaving the film when I turned the axis of the surface, keeping it horizontal. The termination of the laminar surface on the bulb was then a skew curve.

The following is what I imagine to be the explanation of the effects which I have noticed. As soon as the plane film touches the bulb, it fastens on to it in a section whose area expresses the diminution of the surface of this film; as this is subject in all parts to the force of contraction of the liquid, it tends to become as small as possible, which is the case when the section *acb* (fig. 4) immersed in the plane of the ring is a maximum; the equilibrium is then stable. The phenomenon is similarly explained in the case of a helicoidal film; the surface of intersection of the surfaces of the bulb and of the film is then a skew curve.

XXVI. On the Radiant Spectrum.

By Sir DAVID BREWSTER, K.H., F.R.S.*

I HAVE given the name of *Radiant Spectrum* to a phenomenon which I discovered in 1814, and which I described to this Society in the early part of that year.

It will be understood from fig. 1, which represents the brilliant radiation which surrounds a very small image of the sun, when it is formed either by reflexion or refraction, or otherwise.

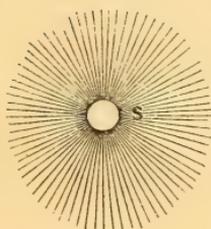


Fig. 1.

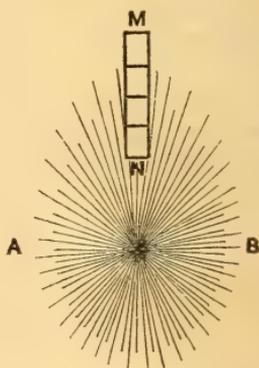
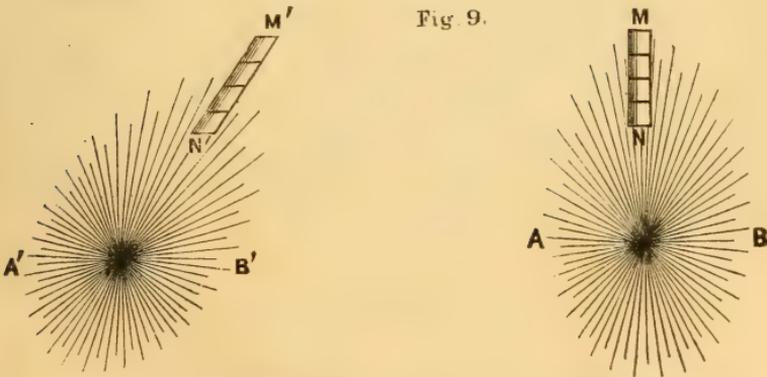
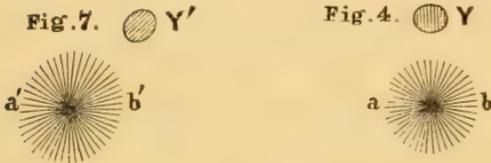
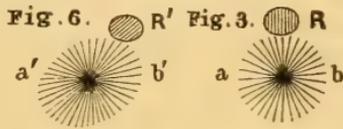


Fig. 2.

* From the Proceedings of the Royal Society of Edinburgh, Session 1866-67. Communicated by the Author.

If we now form a spectrum of this radiant image, either by a prism or by diffraction, we shall have the radiant spectrum shown in fig. 2, where MN is the spectrum of the small circular image S, and AB the spectrum of the radiation, the centre of which is beyond the violet, and nearly in the place where the intensity of the chemical or invisible rays is a *maximum*.

In order to analyze this compound radiation, let the image of the sun S, fig. 1, be taken from homogeneous *red* light R, fig. 3, and refracted by the prism; we shall have its radiation *ab* at a little distance from the bright portion R, as in fig. 3. In homogeneous *yellow* light Y, fig. 4, the radiation *ab* will be at a greater distance from Y than in the *red* light. In homogeneous



violet light V, fig. 5, the radiation *ab* will be at a greater distance from V than in the *yellow* light.

If we now refract laterally these homogeneous radiant spectra, fig. 3 will be changed into fig. 6, fig. 4 into fig. 7, and fig. 5 into fig. 8, thus proving that the radiant portion of the spectra consists of rays more refrangible than the portion R, Y, and V from which it is derived, and that the difference between the refractive indices of these portions increases with the refrangibility of the rays at R, Y, and V.

The compound-spectrum M N, A B, fig. 2, is therefore composed of all these separate spectra; and if we refract it laterally, as shown in fig. 9, we produce the oblique radiant spectrum M' N', A' B', thus proving that the radiant image consists of rays more refrangible than the homogeneous light from which it is derived.

In a rude experiment with a prism of flint glass, whose mean index of refraction was 1.596, the index of the extreme violet was 1.610, and that of the centre of the radiant image 1.640.

In the preceding experiments the radiation is produced by the action, on the retina, of the small and bright image of the sun; but the same results are obtained, and more distinctly exhibited, by placing a surface of finely ground glass either on the front of the prism, or behind it, and near the eye.

The existence of a radiant image beyond the violet end of the spectrum, as in fig. 2, is a fact difficult to explain. I have had an opportunity of describing or showing it to several distinguished philosophers—to the Marquis Laplace and M. Biot in the autumn of 1814, and more recently to others, by some of whom the experiments have been repeated; but no explanation of them has been suggested, excepting the untenable one that the separation of the radiant image from the ordinary spectrum might be the result of parallax.

A better theory, and one of great interest, if true, may be sought in the phenomena of fluorescence, discovered in sulphate of quinine by Sir John Herschel, and in fluor-spar and other substances by myself, and in the beautiful explanation of them by Professor Stokes. In this theory the invisible radiation of the chemical rays is rendered visible by being scattered by granular surfaces, just as the invisible chemical rays in the ordinary spectrum are rendered visible by being reflected and scattered by the particles of fluorescent bodies.

XXVII. *On the Specific Heat of Gases for Equal Volumes under Constant Pressure.* By Dr. ALEXANDER NAUMANN*.

§ 1. *On the relative magnitudes of the several quantities which make up the total specific heat of perfect gases, and on the connexion between the total specific heat and chemical composition.*

THE uninterrupted development of the Mechanical Theory of Heat has furnished continually increasing grounds of support for Clausius's view, that the thermal content of a perfect gas (that is, of one which obeys perfectly the laws of Mariotte and Gay-Lussac) is represented by the progressive motion of the molecules, and by the motions of the constituent atoms of the molecules. In particular the principle maintained by Clausius, according to which the *vis viva* of the progressive motion of the molecules is, in the case of all gases, proportional to the absolute temperature (counted from -273° C.), has received a beautiful experimental confirmation from the recent researches of O. E. Meyer † upon the internal friction of gases. This view likewise receives unquestionable, though less direct, confirmation from the immediate observation of similar molecular motions in liquids ‡.

When a perfect gas is heated under constant pressure, part of the heat imparted to it is employed in effecting the external work which accompanies the expansion of the gas: this portion may be called the *heat of expansion*. Another part serves to increase the *vis viva* of the progressive motion of the molecules: this we may call the *heat of molecular motion*. A third part increases the motion of the constituents of the molecules—that is, of the atoms inside the molecules: this may be called the *heat of atomic motion*.

The heat of expansion was proved experimentally by Dulong § to be the same for all gases under the same pressure, and under different pressures to be proportional to the pressure (though the reason of it was first pointed out by Clausius ||). For a pressure of 760 millims. of mercury and an elevation of temperature from 0° to 1° C. (which are the conditions that will be assumed in what follows when we have to deal with specific numerical statements), this heat of expansion amounts to 0.0691 ¶

* Translated from the *Annalen der Chemie und Pharmacie*, vol. cxlii. p. 265 (June 1867).

† Pogg. *Ann.* vol. cxxv. *passim*; vol. cxxvii. p. 253 (1865 and 1866).

‡ Chr. Wiener, Pogg. *Ann.* vol. cxviii. pp. 85-91.

§ Pogg. *Ann.* vol. xvi. p. 476.

|| Pogg. *Ann.* vol. lxxix. p. 397 (1850). [Phil. Mag. S. 4. vol. ii. p. 21; also 'Mechanical Theory of Heat' (Van Voorst, 1867), p. 42.]

¶ *Ann. der Chem. und Pharm.* vol. cxviii. p. 116.

thermal unit as appears from the mean value (0.23773) of the specific heat of the three permanent gases (oxygen, nitrogen, and hydrogen) and the ratio (1.41) of the specific heat under constant pressure to the specific heat at constant volume, as deduced from the velocity of sound* and other observations†. If γ' denote the specific heat of equal volumes of perfect gases under constant pressure, and γ the specific heat at constant volume (which latter is connected with molecular and atomic motions), the heat of expansion, namely $\gamma' - \gamma$, under equal pressure is a constant for all gases.

We will now in the first place show that the heat of molecular motion m bears a constant relation to the heat of expansion $\gamma' - \gamma$, and is consequently also a constant for equal pressure. According to Clausius‡, if K denote the *vis viva* of the progressive motion of the molecules, and H the total *vis viva* of the gas,

$$\frac{K}{H} = \frac{\frac{3}{2}(\gamma' - \gamma)}{\gamma}.$$

Consequently, if W represent the thermal equivalent of the unit of work, KW is the quantity of heat which produces the progressive motion of the molecules, and HW is the total quantity of heat in the gas, and

$$\frac{KW}{HW} = \frac{\frac{3}{2}(\gamma' - \gamma)}{\gamma}.$$

But since this relation is independent of the temperature, (if K_1 and H_1 represent the values corresponding to 1°C. , and K_0 and H_0 those corresponding to 0°C.) we have the equation

$$\frac{K_1 W}{H_1 W} = \frac{K_0 W}{H_0 W} = \frac{\frac{3}{2}(\gamma' - \gamma)}{\gamma},$$

whence

$$\frac{K_1 W - K_0 W}{H_1 W - H_0 W} = \frac{\frac{3}{2}(\gamma' - \gamma)}{\gamma}.$$

But $H_1 W - H_0 W$ is the specific heat at constant volume $= \gamma$; and $K_1 W - K_0 W$ is the portion of this heat employed in producing the progressive motion of the molecules $= m$. Hence we have

$$\frac{m}{\gamma} = \frac{\frac{3}{2}(\gamma' - \gamma)}{\gamma},$$

* Ad. Dronke, Pogg. Ann. vol. cxix. p. 393 (1863).

† Ann. der Chem. und Pharm. vol. cxviii. p. 113.

‡ Pogg. Ann. vol. c. p. 379 (1857).

and therefore

$$m = \frac{3}{2}(\gamma' - \gamma) \text{ and } \frac{m}{\gamma' - \gamma} = \frac{3}{2}. \quad (1)$$

Consequently the heat of molecular motion is to the heat of expansion in the constant ratio of 3 to 2. If the heat of expansion increases, in consequence of the gas being subject to greater pressure, the heat of molecular motion must increase also in the same ratio,—a result which also follows from the consideration that the number of molecules whose motion has to be augmented likewise increases, for equal volumes, proportionally to the pressure.

If the heat of molecular motion and the heat of expansion be deducted from the specific heat under constant pressure, the remainder constitutes the *heat of atomic motion*,

$$\alpha = \gamma' - \frac{3}{2}(\gamma' - \gamma) - (\gamma' - \gamma) = \gamma' - \frac{5}{2}(\gamma' - \gamma). \quad (2)$$

The simplest case that is theoretically conceivable is that the heat of atomic motion should be proportional to the number of atoms constituting a molecule—that consequently, if n be the number of atoms contained in a molecule, and a the quantity of heat required to increase the motion of *one* atom, we should have

$$\alpha = na.$$

Keeping in mind equation (1), the theoretical specific heat of gases can now be calculated, after determining the value of a , by the following equations:—

For constant volume,

$$\gamma = na + m = na + \frac{3}{2}(\gamma' - \gamma). \quad (3)$$

For constant pressure,

$$\gamma' = na + m + \gamma' - \gamma = na + \frac{5}{2}(\gamma' - \gamma). \quad (4)$$

If a gas departs notably from the laws of Mariotte and Gay-Lussac, the actual specific heat will be greater than the theoretical, since in such gases the existence of molecular attractions is indicated (for instance, by their greater compressibility), the gradual overcoming of which as the temperature rises will require an expenditure of heat. Hence, if the heat of atomic motion is deduced for such a gas, by subtracting the heat of expansion and of molecular motion from the specific heat as found by experiment, the value so obtained still includes the heat expended in operations which do not occur in the case of perfect gases. In order, therefore, not to find too great a value for the heat of mo-

tion of one atom ($a = \frac{\alpha}{n}$), we must start from the experimentally determined specific heats of the most perfect gases—that is, of oxygen, nitrogen, and hydrogen.

As a consequence of equation (4), which determines the heat of atomic motion, we have for the heat of motion of one atom

$$a = \frac{\gamma' - \frac{5}{2}(\gamma' - \gamma)}{n} = \frac{\gamma' - 0.17275}{n}, \quad . . . \quad (5)$$

where its numerical value (=0.0691) has been substituted for $\gamma' - \gamma$; while for γ' Regnault's experimental values, as given in Tables which follow lower down, and for n the number of atoms contained in a molecule must be taken. In this way closely accordant empirical values of a , the heat of motion of one atom, are obtained for those gases with diatomic molecules which nearly follow the laws of Mariotte and Gay-Lussac—namely,

	<i>a.</i>
For Oxygen, Θ^2	0.0339
„ Nitrogen, N^2	0.0320
„ Hydrogen, H^2	0.0316
„ Nitric oxide, $N\Theta$	0.0339
„ Carbonic oxide, $C\Theta$	0.0321
„ Hydrochloric acid, HCl	0.0303.

Moreover for a few other gases with triatomic, tetraatomic, pentatomic, and hexatomic molecules, the formation of which from their constituents, supposed in the gaseous state, takes place with condensation, we have—

	<i>a.</i>
For Hydrosulphuric acid, H^2S	0.0377
„ Ammonia, NH^3 *	0.0318
„ Marsh-gas, CH^4 †	0.0310
„ Ethylene, C^2H^4 †	0.0363

If we take into consideration the close agreement of these numbers relatively to the possible degree of accuracy‡ of the experiments, and remember that, as was shown above, the heat of expansion is to the heat of molecular motion in a simple ratio, the idea readily occurs, in relation to the value of the numbers

* Regnault is of opinion that his value for the specific heat of ammonia is “no doubt a little too low” (*Mém. de l'Académie des Sciences*, vol. xxvi. p. 162).

† Regnault could not get marsh-gas or ethylene perfectly pure. *Ibid.* pp. 139 & 141.

‡ On this point see the special description of Regnault's separate experiments, *op. cit. passim*.

that have been found, that a simple relation may also subsist between the heat of motion of one atom ($=a$) and the heat of expansion. The numbers calculated for the foregoing approximately perfect gases approach (in general more closely in proportion as Regnault's experiments are more trustworthy*, and the gas in question at the same time follows Mariotte's law more exactly) nearly the value $\frac{\gamma' - \gamma}{2} = \frac{0.0691}{2} = 0.0345$. Conse-

quently I consider myself justified in substituting this numerical value (which will hereafter have to be reduced to 0.03396), or in general $\frac{\gamma' - \gamma}{2}$, for a . According to equations (3) and (4), then, *there exists for all perfect gases a simple constant ratio among the three several parts of the specific heat under constant pressure referred to equal volumes, namely the heat of expansion, the heat of molecular motion, and the heat of atomic motion*—that is,

$$\gamma' - \gamma : \frac{3(\gamma' - \gamma)}{2} : \frac{n(\gamma' - \gamma)}{2} ;$$

or, since we have put $\frac{\gamma' - \gamma}{2} = a$,

$$2a : 3a : na = 2 : 3 : n.$$

Hence we have for the specific heat of all bodies in the state of ideally perfect gases :—

For constant volume,

$$\gamma = na + 3a = (n + 3)a ; \quad (6)$$

for constant pressure,

$$\gamma' = na + 3a + 2a = (n + 5)a. \quad . . . (7)$$

If in equation (7) we put for γ' the mean value ($=0.23773$) of the empirical specific heats of the approximately perfect gases with diatomic molecules (oxygen, nitrogen, and hydrogen), we have

$$0.23773 = (n + 5)a = 7a.$$

$$a = 0.03396 = 0.034 \text{ nearly.}$$

This numerical value introduced into equations (6) and (7) gives

$$\gamma = (n + 3) \cdot 0.034,$$

$$\gamma' = (n + 5) \cdot 0.034.$$

This specific heat γ' of equal volumes of perfect gases under

* *Op. cit.* p. 260.

whence, on the one hand,

$$\gamma' - \gamma = 0.06792 \text{ nearly;}$$

on the other hand,

$$\gamma' - \gamma = 0.0691.$$

From these values of $\gamma' - \gamma$ there follows further the value of the *mechanical equivalent of the unit of heat*, namely:—employing the first number,

$$A = \frac{1.000364166 \cdot 10334.5^*}{1.2932 \cdot 0.06792} = 428.47 \text{ kilogrammetres;}$$

employing the second number,

$$A = 425.33 \dagger \text{ kilogrammetres.}$$

The values of $\frac{\gamma'}{\gamma}$ and $\gamma' - \gamma$ are smaller, and that of A is greater, than those hitherto given; that is to say, the difference is in the direction which might be expected *à priori* when it is considered that the values hitherto deduced from observation have reference to gases which, although they approach nearly to the condition of perfect gases that is presupposed in the case of the values here deduced, do not quite attain to it.

In any case the value $\left(\frac{\gamma'}{\gamma} = 1.41\right)$ that has been found for the permanent, but not quite strictly speaking perfect, gases is at least not so well established, even for the ideally perfect gaseous state, as to overthrow the value $\left(\frac{\gamma'}{\gamma} = 1.4\right)$ deduced from the considerations above set forth. The approximate agreement of the values here compared, as well as the direction in which they differ, accordingly justify us in adhering to the values of the two specific heats expressed by the equations (6), (7), (8), and (9); and at the same time an additional confirmation is afforded to the views from which they are derived, respecting the molecular and atomic constitution of gases.

* 0.00364166 = coefficient of expansion of air.

10334.5 = pressure on 1 square metre in kilogrammes.

1.2932 = weight of 1 cubic metre of air in kilogrammes.

0.06792 = heat of expansion.

† Ad. Dronke, Pogg. *Ann.* vol. cxix. p. 399 (1863).

§ 3. Table of the numerical values of the specific heats of gases, and connexion of the differences between the values found and those calculated for the state of perfect gases with chemical composition.

In the annexed Table there are contained :—

In column 1, the *names* of the gases.

In column 2, their *chemical composition*.

In column 3, in general, the *theoretical specific gravities* given by Regnault*. These numbers were recalculated (by the formula $s = \frac{\text{molecular weight}}{28.94}$); and the number obtained was intro-

duced in place of the one given by Regnault whenever the difference between them was great enough to affect the third decimal place of the values in column 5. In order to distinguish the numbers thus substituted, they are given with only three places of decimals. For the specific heat of nitrogen, referred to volume, it is immaterial whether the specific gravity 0.9713 given by Regnault, or the theoretical specific gravity 0.968 be adopted, inasmuch as Regnault calculated it directly from the specific heats of oxygen and air, and then deduced from it the specific heat of the unit of weight.

In column 4, the value of the *specific heats of equal weights under constant pressure*, as given by Regnault.

In column 5, the *specific heats of equal volumes under constant pressure*, deduced from the numbers in the last column by multiplying by the specific gravities. The evidently erroneous† numbers 0.4106‡ and 0.4160§ given by Regnault for ethylene, are replaced by the right ones.

In column 6, the *theoretical specific heats of equal volumes, calculated for the condition of perfect gases* according to the equation

$$\gamma' = (n + 5) \cdot 0.034.$$

In column 7, the difference between the values given in columns 5 and 6.

In column 8, the *number of atoms* contained in a molecule.

In column 9, the *theoretical ratio of the two specific heats of equal volumes* (that is, the specific heat under constant pressure and that at constant volume), calculated for the condition of perfect gases according to the equation

$$\frac{\gamma'}{\gamma} = \frac{n + 5}{n + 3}.$$

* *Op. cit.* pp. 303, 311, 313, 318 (also Will, *Jahresbericht für Chemie*, 1863, p. 83 *et seq.*).

† Regnault, *op. cit.* p. 142.

‡ *Ibid.*

§ *Op. cit.* p. 318.

1. Name of gas.	2. Composi- tion.	3. Density.	4. Specific heats under constant pressure (according to Regnault).		6. Theore- tical specific heats of equal volumes in the state of perfect gases.	7. Differ- ence. Col. 5.— col. 6.	8. No. of atoms	9. Theore- tical ratio of the two specific heats.
			Equal weights.	Equal volumes.				
Oxygen	O ²	1.1056	0.21751	0.24049	0.238	0.002	2	1.4
Nitrogen.....	N ²	0.9713	0.24380	0.23680	"	-0.001	"	"
Hydrogen	H ²	0.0692	3.40900	0.23590	"	-0.002	"	"
Chlorine	Cl ²	2.4502	0.12099	0.296	"	0.058	"	"
Bromine	Br ²	5.529	0.05552	0.307	"	0.069	"	"
Nitric oxide	N ¹ O	1.0384	0.2317	0.2406	"	0.003	"	"
Carbonic oxide	C ¹ O	0.9673	0.2450	0.2370	"	-0.001	"	"
Hydrochloric acid ...	HCl	1.2596	0.1852	0.2333	"	-0.005	"	"
Carbonic acid.....	C ² O ²	1.5201	0.2169	0.331	0.272	0.059	3	1.333
Nitrous oxide.....	N ² O	1.5201	0.2262	0.345	"	0.073	"	"
Water	H ² O	0.6219	0.4805	0.299	"	0.027	"	"
Sulphurous acid.....	S ² O ²	2.221	0.1544	0.343	"	0.071	"	"
Hydrosulphuric acid	H ² S	1.1747	0.2432	0.286	"	0.014	"	"
Sulphide of carbon	C ² S ²	2.6258	0.1569	0.412	"	0.140	"	"
Ammonia	NH ³	0.5894	0.5084	0.300	0.306	-0.006	4	1.286
Terchloride of phos- phorus	PCl ³	4.751	0.1347	0.640	"	0.334	"	"
Chloride of arsenic	AsCl ³	6.272	0.1122	0.703	"	0.397	"	"
Marsh-gas	CH ⁴	0.5527	0.5929	0.3277	0.340	-0.012	5	1.250
Chloroform	CHCl ³	4.1244	0.1567	0.647	"	0.307	"	"
Chloride of silicon	SiCl ⁴	5.874	0.1322	0.777	"	0.437	"	"
Chloride of titanium	TiCl ⁴	6.572	0.1290	0.848	"	0.508	"	"
Chloride of tin	SnCl ⁴	8.970	0.0939	0.842	"	0.502	"	"
Methylic alcohol ...	C ¹ H ⁴ O	1.1055	0.4580	0.506	0.374	0.132	6	1.222
Ethylene.....	C ² H ⁴	0.9672	0.4040	0.3907	"	0.017	"	"
Chloride of ethyle ...	C ² H ⁵ Cl	2.223	0.2738	0.609	0.442	0.167	8	1.482
Bromide of ethyle ...	C ² H ⁵ Br	3.766	0.1896	0.714	"	0.272	"	"
Chloride of ethylene	C ² H ⁴ Cl ²	3.4174	0.2293	0.784	"	0.342	"	"
Alcohol	C ² H ⁶ O	1.5890	0.4534	0.720	0.476	0.244	9	1.167
Cyanide of ethyle ...	C ² H ⁵ N	1.9021	0.4262	0.811	"	0.355	"	"
Acetone	C ³ H ⁶ O	2.0036	0.4125	0.826	0.510	0.316	10	1.154
Benzole	C ⁶ H ⁶	2.6942	0.3754	1.011	0.578	0.433	12	1.133
Acetate of ethyle ...	C ⁴ H ⁸ O ²	3.0400	0.4008	1.218	0.646	0.572	14	1.118
Ether	C ⁴ H ¹⁰ O	2.5573	0.4797	1.227	0.680	0.547	15	1.111
Sulphide of ethyle ...	C ⁴ H ¹⁰ S	3.1101	0.4008	1.246	"	0.566	"	"
Oil of turpentine ...	C ¹⁰ H ¹⁶	4.6978	0.5061	2.378	1.054	1.324	26	1.069

As the Table shows, the difference between the observed and the calculated specific heat is not very great in the case of gases which have been ascertained to follow approximately the laws of Mariotte and Gay-Lussac; it becomes so in the case of gases which we have other reasons for believing depart considerably from these laws. Accordingly, the difference between the observed and the theoretical specific heats may be taken as a measure of the degree in which the gases in question respectively depart from the laws of Mariotte and Gay-Lussac under the

conditions in which their specific heats were determined. A further confirmation of this is afforded by the fact that the theoretically calculated specific heats are almost always smaller, sometimes considerably smaller, but never notably greater than the observed values; for the further a gas is from the perfect gaseous condition—the more molecular attractions have still to be overcome when it is heated,—the greater must be the excess of the specific heat as found by experiment over that calculated theoretically. In the case of chlorine, which in one of its compounds (hydrochloric acid) conforms to the above law in regard to its specific heat, we are certainly justified in concluding that in the free state it is comparatively far from following the laws of Mariotte and Gay-Lussac. Thus starting from the simpler relations, which in reality hold only approximately and in a minority of cases, we have a method of investigating the more complex relations presented by the majority of substances. Regnault himself takes as the basis of his reasoning the theoretical behaviour of ideal gases, in order to judge of the molecular state of the gases in accordance with their observed specific heats, in cases where a knowledge of the compressibility and expansion is defective or altogether wanting, as, for instance, in relation to carbonic acid* and the gases† enumerated in the second half of the above Table.

For *chemically allied bodies* containing equal numbers of atoms in a molecule, regularities are almost always observable. In such cases the difference between the observed specific heat and that calculated for the perfectly gaseous state is greater, and in general also the boiling-point is higher, in proportion as the molecular weight is greater, as is shown by the following Table (p. 215).

Since bodies of the same number of atoms and of corresponding chemical composition thus depart in general so much the more from the laws which apply to perfect gases as their molecular weight is greater, we may conclude that the mutual attractions of similar atoms which still subsist in the ordinary gaseous condition become stronger under otherwise identical conditions as the mass of the molecules increases—a result which is in perfect accordance with one of the fundamental laws of attraction.

* *Op. cit.* p. 299.† *Ibid.* pp. 319, 320.

Name of gas.	Composi- tion.	Number of atoms.	Molecular weight.	Difference between theoretical and ob- served specific heats.	Boiling-point.
Chlorine	Cl ²	2	71	0.058	Gas.
Bromine	Br ²	2	160	0.069	47°-63°
Carbonic acid	CΘ ²	3	44	0.059	Gas.
Sulphide of carbon	C ² S ²	3	76	0.140	48°
Ammonia	NH ³	4	14	-0.006	Gas.
Terchloride of phosphorus	PCl ³	4	137.5	0.334	78°
Chloride of arsenic	AsCl ³	4	181.5	0.397	133°
Marsh-gas... ..	CH ⁴	5	16	-0.012	Gas.
Chloroform	CHCl ³	5	119.5	0.307	62°
Chloride of silicon	SiCl ⁴	5	170	0.432	59°
Chloride of titanium	TiCl ⁴	5	190	0.507	135°
Chloride of tin	SnCl ⁴	5	259.6	0.502	120°
Chloride of ethyle	C ² H ⁵ Cl	8	64	0.167	11°
Bromide of ethyle	C ² H ⁵ Br	8	109	0.272	41°
Ether	C ⁴ H ¹⁰ Θ	15	74	0.547	34°-5
Sulphide of ethyle	C ⁴ H ¹⁰ S	15	90	0.566	91°
Water	H ² Θ	3	18	0.027	100° } Excep-
Hydrosulphuric acid	H ² S	3	34	0.014	Gas } tion.

§ 4. Comparison of the above method of calculating the specific heat of gases with those proposed by Buff* and Clausius†.

The equations arrived at for calculating the specific heats of perfect gases (namely,

$$\text{at constant volume} \quad \gamma = (n+3)a = (n+3) \cdot 0.034,$$

$$\text{under constant pressure} \quad \gamma' = (n+5)a = (n+5) \cdot 0.034$$

necessarily lead, in most cases, to results which differ notably from those which follow from a rule given by Professor H. Buff. This rule directs us to "take the specific heat of a simple gas at constant pressure as many times as there are gaseous volumes of the constituents which go to make up the compound, and then divide by the number of gaseous volumes occupied by the compound produced. If to the quotients so obtained the heat of expansion corresponding to the unit-volume (a quantity which is the same for all gases) be added, the specific heat under constant pressure is obtained"‡. According to the way in which

* *Ann. der Chem. und Pharm.* vol. cxv. p. 306; *Lehrb. d. physik. u. theoret. Chemie*, von Buff, Kopp, und Zamminer, p. 201.

† *Ann. der Chem. und Pharm.* vol. cxviii. p. 112.

‡ Buff has based this method (*loc. cit.* p. 305) upon the further hypo-

this rule is afterwards carried out, it must apply in general to such gases as are formed from their elementary constituents without condensation, and whose molecules consequently consist, according to the usual assumption, of two atoms. For every additional atom in the molecule, however, it must give too great a value for the specific heat by $\frac{m}{2} = \frac{3 \cdot 0 \cdot 034}{2} = 0 \cdot 051$; for, by the rule in question, half the heat of molecular motion (which is a constant quantity and independent of the number of atoms in a molecule) is reckoned for each atom that enters into the composition of the molecule. (In the case of gases which contain mercury, arsenic, or any other body of the same kind, the excess of the calculated value would bear a different but easily deducible ratio to the number of elementary atoms contained in one molecule.) It is doubtless to be attributed to this property of Buff's rule (namely, that it gives too great a value for the specific heat, except in the cases already specified, and one which is so much the more in excess in proportion as the number of atoms in a molecule, and therefore also in general the degree of departure from the laws of Mariotte and Gay-Lussac, is greater), that the values calculated by him agree with those observed by Regnault, and sometimes with surprising closeness, exactly for gases which are far from following the laws of Mariotte and Gay-Lussac, whereas this agreement no longer shows itself in the case of gases which follow these laws more exactly and at the same time contain more than two atoms in a molecule; for such gases the calculated values, in accordance with the above considerations, come out too high.

A method of calculating the specific heat of gases, which agrees in essential points with Buff's method, has also been proposed by Clausius*, to which of course the same remarks apply as have been made in regard to Buff's method. Clausius comes at last to the conclusion that the imperfection of the gaseous state is "certainly not quite sufficient" to account for the difference between Regnault's values and those which he calculated. I believe that I have ascertained the true reason of the discrepancy. The theoretical specific heat ought not, by the nature of the case, ever to be notably higher than the actual specific heat; and its

thesis that the specific heats of equal weights at constant volume are independent of alterations in the density of the gases, even when these alterations are caused by chemical action. But between mechanical and chemical condensation there is, however, this essential difference—that the former is not accompanied by any change in the number of molecules, whereas the latter is always accompanied by an alteration, namely a diminution of the number.

* *Ann. der Chem. und Pharm.* vol. cxviii. pp. 112 *et seq.*

defect, as compared with the actual value, must be so much the greater the further the gas is from the perfectly gaseous state. Since, now, both Clausius and Buff calculated the theoretical specific heat for a molecule of n atoms too high by a quantity which may be expressed generally by the formula $(n-2) \frac{\gamma' - \gamma}{2}$ in the case of bodies which, like water, hydrosulphuric acid, ammonia, (marsh-gas, ethylene*,) chloride of ethyle, alcohol, acetone, ether, and sulphide of ethyle, are not far enough removed from the state of perfect gases to counterbalance the errors of the mode of calculation, the surprising result comes out that the calculated specific heat is considerably higher than the observed specific heat. *In contrast with this, the specific heats calculated according to my method for the perfectly gaseous condition are never greater, to an extent worthy of consideration, than the observed specific heats*, as the above Table shows,—a circumstance which speaks in favour of the views here developed, and which, in view of many of the values calculated by Buff and Clausius, I think ought to be specially pointed out. In accordance with this, the specific heat of carbonic acid, which Regnault† found to increase with the temperature (a peculiarity which he considers it probable is shared by all the gases which are far from the condition of perfect gases), is greater than that calculated by my formula. Thus the lowest value observed was 0.2801, while the calculated value is 0.272.

§ 5. Minimum limit of specific heat.

The applicability of the equation $\gamma' = (n+5) \cdot 0.034$ to the calculation of the specific heat of bodies with monatomic molecules (of which mercury and cadmium are admitted to be examples) may be considered doubtful. It depends on the answer that is given to two questions:—Do the molecules hitherto called monatomic really consist of a single atom, homogeneous throughout its entire mass? and, Can we, in the case of such a molecule, talk of an atomic as distinguishable from the molecular motion? The former question, for the present at least, does not admit of an answer. But if it be answered in the affirmative, and the second in the negative, we get the expression $\gamma = 5 \cdot 0.034 = 0.17$, which must be the value of the lowest conceivable specific heat for the gaseous state. In any case, if it were experimentally ascertained that mercury or cadmium possessed this specific

* Neither of these gases could be obtained quite pure; but still the impurities could not, according to Regnault's statements (*op. cit.* pp. 139 *et seq.*), be sufficient to explain the great differences between the values calculated by Buff and Clausius and those observed by Regnault.

† *Op. cit.* p. 128, also p. 298.

heat, it would be at once the surest proof that the molecule of these two bodies consists of only a single atom, and that the molecules of most of the other undecomposed bodies consist of two atoms and not of a multiple of two atoms, and that those of phosphorus and arsenic consist of four atoms.

In the foregoing pages it has been shown that for the perfectly gaseous state the heat of expansion bears a simple constant ratio to the heat of molecular motion,—and also, on the basis of Regnault's experimental results, that the heat of atomic motion bears a simple ratio to the two already-mentioned parts of the specific heat, the heat of atomic motion being found proportional to the number of atoms in a molecule. Thus the ratio of the heat of expansion to the heat of molecular motion and to that of atomic motion has been found to be $= 2 : 3 : n$, and for the specific heat the general expression $\gamma' = 2a + 3a + na = (n + 5)a$ has been obtained; and this, when (from the results of Regnault's experiments) the value 0.034 has been found for a , reduces itself to the numerical expression $\gamma' = (n + 5)0.034$. Since the agreement between the numbers thus calculated and those deduced from experiment, is in general greater the nearer a gas approaches to the presupposed state of a perfect gas, and since also the numerical values of other kinds deduced from the equations that have been established agree with the results of experiment, the simple conclusions to which the foregoing considerations lead, and the consequences connected with them, appear to be sufficiently established.

Giessen, December 1866.

XXVIII. *Some further Remarks on the Influence of the Full Moon on Cloud.* By WILLIAM ELLIS, F.R.A.S.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

WILL you allow me space briefly to reply to the remarks made by Mr. J. Park Harrison on my paper on the Influence of the Full Moon on Cloud? What I wished to show, in the particular case which I investigated, was that no great effect exists, none that we can easily perceive. It is to be remembered that the language of Herschel and Arago is very strong on the point. Herschel, in his 'Familiar Lectures,' speaks confidently of the tendency of the moon "to clear the sky of cloud, and to produce, not only a serene, but a calm night, when so near the full as to appear round to the eye." And Arago says, "La lune mange les nuages." This does not indicate a

small effect, but rather that the nights near the time of full moon are much more clear than those on which no moon is present, as, for instance, the nights near to new moon. Now the numbers which I obtained gave no support to this idea; for, from the observations made during $86\frac{1}{2}$ lunations at Greenwich, the mean amount of cloud, as deduced from six observations taken at intervals of two hours on five nights at each full moon, was found to be 6.48, similar observations on five nights at each new moon giving 6.07 (a clear sky being represented by 0, and a sky wholly cloudy by 10). And I cannot help thinking that the cause of the apparent deception is as I have stated it in my paper printed in your July Number. But Mr. Harrison suggests that sufficient account has not been taken of the effect of the omitted (principally Sunday) observations. I may here state that from examination I do not find the omissions to occur in the irregular manner which Mr. Harrison has supposed. Now what would be the effect of supplying all the omitted observations? how would the numbers which I have quoted be affected? Practically one-seventh part of the observations are wanting, the omissions being pretty equally distributed between full and new moon. Suppose now each omitted observation to be supplied, and let means be taken on two suppositions, first, giving the omitted observations the extreme value 0, and, secondly, the extreme value 10. Such means are in each case the *least possible* and *greatest possible* mean values. But the one-seventh part of the observations is in each case a considerable number. It is therefore in a high degree probable that the means of the one-seventh part in each case will lie between the before-mentioned least possible and greatest possible mean values. We may therefore substitute for the omitted observations (instead of 0 or 10) the least possible and greatest possible mean values. Means being again taken, it is found that the numbers 6.48 and 6.07 could not be altered by more than 0.13. Admitting that the numbers may be altered by this amount, and supposing even that the first is diminished and the second increased, we should still have 6.35 for the mean state of the sky at night at full moon, and 6.20 for its mean state at night at new moon. These numbers not only negative any *great* effect, but also the existence of *any* full-moon influence.

Mr. Harrison further suggests that, by grouping several days together, I have "merged and eliminated any distinctive character they may individually have possessed." But I was not looking for small effects. The sky was said to be much clearer at night at the time of full moon. The seven years' Greenwich observations show that such peculiar effect does not exist. And this is all that I wished to prove.

I hope that I have now so far explained the special object of the paper printed in the July Number of the Magazine, and so considered Mr. Harrison's objections, that it will not be necessary for me again to address you on the same subject.

I am, Gentlemen,

Your obedient Servant,

WILLIAM ELLIS.

Greenwich, August 17, 1867.

XXIX. *Chemical Notices from Foreign Journals.*

By E. ATKINSON, *Ph.D., F.C.S.*

[Continued from p. 34.]

WHEN biniodide of phosphorus is treated with water, yellow flocculent masses are formed, which have always been assumed to be finely divided phosphorus. This substance has been investigated by Rudorff*, who has found that it is solid phosphuretted hydrogen. The analysis was effected by heating it in a known volume of an indifferent gas, by which it was decomposed into phosphorus, phosphuretted hydrogen, and hydrogen. The phosphuretted hydrogen was absorbed by a solution of copper. The hydrogen calculated from this, together with that obtained by direct measurement, gave the total quantity of hydrogen. The phosphorus was determined by oxidizing the powder with nitric acid and combining the phosphoric acid with oxide of lead. The results of the analysis gave 99 per cent. of phosphorus and 1.58 hydrogen; this agrees very well with the formula P^2H , which requires 98.41 phosphorus and 1.59 hydrogen.

The iodide of phosphorus yielded about 2 per cent. of its weight of this substance.

Weber has made the following observations † on the action of chloride of iodine on bisulphide of carbon. Terehloride of iodine acts energetically in the cold on bisulphide of carbon; a brown liquid is formed, containing chloride of sulphur, chloride of carbon, and a crystallizable combination of chloride of iodine and chloride of sulphur. The same products are formed when a current of chlorine is passed into a solution of iodine in bisulphide of carbon; the quantity of chlorine absorbed is very great, and causes a considerable increase of volume; at the same time there is a distinct increase of temperature: the reaction is finished when the liquid is of a wine-

* Poggendorff's *Annalen*, vol. cxxviii. p. 475.

† *Ibid.* p. 416.

red colour. On cooling, prismatic crystals are deposited, which are very large if the cooling is slow. These, which are but little soluble in the cold in bisulphide of carbon, contain almost all the iodine. To obtain the chloride of carbon dissolved, the liquid is agitated with water, which decomposes chloride of sulphur, and then with potass, over which the product is distilled; this is pure chloride of carbon, C^2Cl^4 .

Chlorine acting on solution of iodine produces chloride of iodine, which is decomposed as fast as it is formed, so that the iodine is constantly ready to take up fresh quantities of chlorine. A small quantity of iodine can thus bring about the decomposition of a large quantity of bisulphide of carbon by chlorine.

Terchloride of iodine (which dissolves in water) also acts upon bisulphide in the presence of water; the same crystals are formed; to collect them the liquid is decanted, and the crystals are dried in a current of dry chlorine. They are prisms, of an orange yellow, greatly resembling acid potassic chromate; but they are very deliquescent and produce a brown liquid, so that their angle could not be determined. Water decomposes them, with the separation of sulphur; bisulphide also decomposes them. Their composition is expressed by the formula $ICl^3 + 2SCI^2$; when heated they lose a little iodine.

Jaillard obtained a compound, by the action of chlorine on a mixture of 1 part of iodine and 2 of sulphur, to which he assigned the composition $ICl^3 + SCI^2$; Weber has prepared and analyzed this body, and has found that it is identical with the body described above.

Sulphurous acid and sulphuretted hydrogen do not act on each other if dry and at the ordinary temperature; in the presence of water they produce water and deposit sulphur. It is usually stated that the reaction is thus expressed, $2HS + SO^2 = 2HO + 3S$; it is, however, more complex: pentathionic acid is formed, which, in decomposing, liberates sulphur. The reaction has been investigated by De Luca and Ubaldini*. The following method was used, to avoid as far as possible the influence of heat on the deposited sulphur:—Two normal solutions were prepared, one containing a weight of 1.7 gr. of sulphuretted hydrogen in 613.5 cub. centims. of water, and the other 1.6 gr. of sulphurous acid in 54 cub. centims. of water, these proportions corresponding to the above equation. They were mixed in a stoppered bottle; a milky deposit was formed, from which the liquid became more or less clarified after

* *Comptes Rendus*, June 10, 1867.

being several times shaken with purified bisulphide of carbon. When the sulphide was separated and filtered, it was evaporated in the water-bath, and the sulphur weighed after being previously dried and melted.

The volume of the solution, the amount of agitation, the temperature of the liquid, the light, contact, time, excess of one or the other constituent, all exert great influence on the final result of this kind of experiment; so that in some cases about two equivalents of soluble sulphur are obtained, while in others insoluble sulphur is separated in greatest proportion.

In this reaction acids of the thionic series are formed; and according to M. Berthelot the sulphur separated from pentathionic acid should be the insoluble variety. Sulphurous acid tends to increase the stability of insoluble sulphur; and, accordingly, with an excess of sulphurous acid the proportion of insoluble sulphur is greater than when the substances used are in equivalent proportions.

A solution of common salt, shaken with the mixture of the two solutions, precipitates the suspended sulphur and clarifies the liquid. On subsequently oxidizing this liquid with chlorate of potass and hydrochloric acid, the sulphur of the thionic acid is changed into sulphuric acid, and may thus be determined.

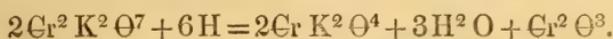
These researches show that the sulphur deposited by the reciprocal action of sulphurous acid and sulphuretted hydrogen consists of two varieties, one of which is soluble and the other insoluble in bisulphide of carbon, the proportion between these two varieties depending not only on the substances taken, but also on the conditions under which the action takes place. The experiments, finally, support the view that the atoms of which a simple substance is constituted may unite at the moment of separation and form complex molecules which produce all the phenomena that can be realized with the ordinary compound bodies to which we are accustomed to devote our attention from this point of view.

Wohler* makes the following observation on the extraction of thallium from a deposit from the sulphuric-acid manufactory of Ringkuhl, where iron-pyrites are used. The substance is exhausted with water acidulated with sulphuric acid, and the thallium precipitated as chloride by means of hydrochloric acid. The chloride is well washed with cold water, and converted into sulphate by heating it with concentrated sulphuric acid. The salt is dissolved in water and precipitated by zinc; but, to avoid the influence of the impurities usually present in zinc, the

* Liebig's *Annalen*, May 1867.

thallium solution is reduced by a simple voltaic element. For this purpose a short wide glass tube is covered at the bottom with a bladder, and, being filled with acidulated water, is immersed in the thallium solution, so that both liquids are on the same level. A zinc plate is immersed in the acidulated solution, and it is provided with a platinum or copper wire which is bent so that it dips outside in the solution of thallium. The reduction begins at once, and the wire is gradually covered with a beautiful crystallization of thallium, of which in a few days no traces are to be found in the solution. These crystals are washed, pressed, rapidly dried and fused to a regulus in a porcelain crucible with cyanide of potassium.

According to Otto*, crystallized oxide of chromium may be prepared with ease and in any quantity by the action of *hydrogen* upon *bichromate of potassium* at a high temperature, whereby the salt is decomposed into neutral chromate of potassium, water, and chromic oxide, according to the following equation:—



The dried and powdered salt is introduced into a tube of hard glass, which is gently heated in a suitable furnace, and a current of dry hydrogen is passed through; aqueous vapours soon make their appearance, and within a short time the reduction is complete. On cooling, the tube is broken and the dark-green slag placed in water, which dissolves the neutral chromate, leaving the oxide in the form of small green lustrous flitters.

Schiff†, in an investigation on the influence of various liquids on the division of phosphorus, has arrived at the following results:—

In many cases, but not always, the action of liquids in pulverizing phosphorus depends on their density.

The nature of the liquid has an influence even when its density is less than that of water.

Viscosity exerts great influence even with solutions of small density.

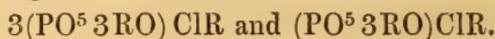
Liquids in which there is a slight disengagement of gas are especially adapted for pulverizing phosphorus.

Deville and Caron artificially reproduced apatite and Wagerite, which they regarded as types of two groups having re-

* Liebig's *Annalen*, April 1867.

† Liebig's *Annalen* (Suppl.), vol. iv. part 1.

spectively the general formula



In nature the arseniates accompany the phosphates, with which they are closely allied in composition and in crystalline form; pyromorphite, for instance, is accompanied, as Wöhler found, by a chlorarsenate of lead, to which he gave the name *mimetisite*, from its resemblance to chlorophosphate of lead. Lechartier* has succeeded in preparing a group of minerals isomorphous with the apatite and Wagnerite group, but containing arsenic instead of phosphoric acid. This was done by fusing the arseniates with excess of chloride of the same base; the arseniates dissolve, forming chlorarsenates, which crystallize in the fused mass when it begins to solidify. As in the case of apatite and Wagnerite, fluorine can either wholly or partially replace chlorine without an alteration in the general form.

The following minerals have been prepared:—

Arsenical Apatites.

Arsenical lime apatite . . .	$\left\{ \begin{array}{l} 3(\text{AsO}^5 \text{ 3 CaO}) (\text{CaCl}) \\ 3(\text{AsO}^5 \text{ 3 CaO}) \left(\begin{array}{l} \text{CaCl} \\ \text{CaF} \end{array} \right) \end{array} \right.$
Arsenical lead apatite . . .	
„ strontia apatite . . .	$3(\text{AsO}^5 \text{ 3 SrO}) (\text{SrCl}).$
„ baryta apatite . . .	$3(\text{AsO}^5 \text{ 3 BaO}) (\text{BaCl}).$

Arsenical Wagnerites.

Arsenical lime Wagnerite.	$(\text{AsO}^5 \text{ 3 CaO}) (\text{CaCl}).$
„ magnesia Wagnerite.	$\left\{ \begin{array}{l} (\text{AsO}^5 \text{ 3 MgO}) \text{ MgCl.} \\ (\text{AsO}^5 \text{ 3 MgO}) \left(\begin{array}{l} \text{MgCl} \\ \text{MgF} \end{array} \right) \end{array} \right.$
„ manganese Wagnerite.	

Fremy has described † a general method of obtaining, in the state of crystals, many substances which are usually amorphous. It consists in allowing the precipitations and decompositions to take place very slowly. Various modifications were used. In one series of experiments the substances were dissolved in solutions of different densities containing gum, sugar, gelatine, &c., and these separated by porous substances, or by unsized paper, which were only gradually penetrated by the liquids, and thus caused slow decompositions. In other cases endosmose was had recourse to, and the solutions separated by mem-

* *Comptes Rendus*, July 22, 1867.

† *Comptes Rendus*, vol. lxiii. p. 714. *Zeitschrift für Chemie*, Dec. 1866.

branes. Vessels of wood or of porous porcelain (unbaked), which allowed the substances to pass through very slowly, also gave excellent results. The author obtained by this method, and frequently in very beautiful forms, the sulphates of barium, strontium, and lead, carbonates of barium and lead, oxalate of calcium, borate and chromate of barium, magnesia, and the various sulphides. In the hope of obtaining crystals of quartz, solutions of alkaline silicates in porous vessels were placed in dilute acids, and also exposed to the action of carbonic acid. Silicates under these circumstances do not deposit gelatinous precipitates, but white crystalline masses hard enough to scratch glass. These crystals, however, were not quartz; for they were soluble in alkalis, contained water and a small quantity of soda which seems essential to their composition. The crystals from silicate of soda contained 68 silica, 5 soda, and 27 of water in 100 parts. Neglecting the small quantity of soda, the composition corresponds to the formula $\text{SiO}^3 + 2\text{HO}$.

The author's experiments furnish an elegant confirmation of Chevreul's view of the formation of crystallized oxalate of calcium in the cells of plants; for he succeeded in obtaining crystals of this kind when he allowed a soluble calcium salt to act through a membrane on a soluble oxalate.

Hörsæus has made* a series of experiments on the influence which various kinds of nutrition exert on the quantity of ammonia and of nitric acid which plants contain. The plants taken were the onion (*Allium cepa*) and the pea (*Pisum sativum*). The amount of ammonia and of nitric acid in onions could not be increased by corresponding mixtures of salts. The nitric acid which they exhibit in summer has been produced by oxidation of ammonia, and it is probable that in winter it again changes into ammonia. The numbers show clearly that they cannot assimilate nitric acid. While the roots, even of onions which had only grown in a solution containing ammoniacal salt, contained 0.5 and 0.3 per cent. of NO^5 , the onions themselves gave 0.084 per cent., the same quantity originally contained in them. Peas, on the contrary, can assimilate both nitric acid and ammonia, and more indeed than is necessary for their growth. In all experiments it was found that the greater the quantity of ammonia and of nitric acid, the worse appeared the growth of the plants, and the smaller the harvest of completely developed seeds capable of germination. Those which had grown in turf manured with nitrates, furnished three times as great a crop as in soil treated with ammonia salts. Manuring with both salts had no better

* *Arch. Pharm.* vol. cxvii. p. 237.

effect than with ammoniacal salts alone. The peas and onions treated with nitrates contained ammonia; those with ammonia contained nitric acid, and in larger quantity than before. This is a proof that ammonia is transformed into nitric acid.

Beyer has furnished a contribution on the metamorphosis of fruits during their ripening*. He took gooseberries from one and the same plant, and, commencing with the fruit when they were quite small, he examined them at intervals of three or four days, determining each time the sugar, free acid, nitrogen, fat, dry residue, ash. From these determinations he deduces the following conclusions:—

(1) Water diminishes during ripening; and consequently the amount of dry residue increases.

(2) Sugar increases constantly both in the fresh and in the dry residue.

(3) The proportion of acid is greatest in the middle of the development. The decrease towards the end of the ripening is inconsiderable in the fresh substance, but very considerable in the dry.

The mineral constituents diminish constantly in both cases.

Nitrogenous substances exhibit the same relations as free acid. They increase at first, then diminish; this diminution is very small in the fresh berries, but is very considerable if calculated on the dried substance.

The quantity of fat increases in the fresh substance; with the dry substance it is greatest during the middle, and then diminishes to an inconsiderable extent.

Kühne† deduces from his observations on hæmoglobine the following conclusions:—

Hæmoglobine, even when not combined with gases, is a body crystallizing in the rhombic system, which is more easily soluble than hæmoglobine containing oxygen.

Hæmoglobine is contained in blood in the free state, and not as alkaline solution. In lake-coloured blood it is present in a form differing from that in the corpuscles when dissolved in an alkaline liquid.

Hæmoglobine is deposited within globules in crystals if the solution in the current of blood-corpuscles be altered. It is separated from lake-coloured blood by neutralization of the alkaline serum. Oxygen may possibly bring this about indirectly.

Hoppe-Seyler's method of blood-analysis is but seldom applicable, because it presupposes a sinking of the blood-corpuscles be-

* *Arch. Pharm.* vol. cxxvi. p. 21. *Zeitschrift für Chemie.*

† *Arch. für path. Anat.* vol. xxxiv. p. 423.

fore coagulation has set in. To prevent the latter without decomposing hæmoglobine, &c., Masia* found it advantageous to mix 1 part of blood with 2 parts of a 4 per cent. solution of crystallized phosphate of soda. In twenty-four to forty-eight hours the clear layer acquires a height of 2 centims., and may be easily removed. Neither in it nor among the precipitated blood-corpuscles is any coagulation perceptible.

Favre† has made the following observations on thermal actions in chemical processes. For the purposes of the investigation he used the heat of a voltaic battery. Of two mercury thermometers, one very accurately measures the internal heat of a battery of five equal zinc-platinum elements. In the other, there are inserted, in a series, a zinc-platinum element of exactly the same dimensions as those in the other calorimeter, then a voltameter consisting of platinum foils of the same size as the zinc-platinum strips, and finally a voltameter with two exactly similar copper strips. The latter are immersed in a solution of sulphate of copper. Comparing the disengagement of heat observed in these various experiments, it is found that the battery yields the quantity of heat necessary for the decomposition of the bodies, and the separation of their constituents. This quantity of heat is greater than that which the same materials disengage in their combination. Hence at the moment of formation (*status nascens*) bodies possess an excess of heat, which they part with on entering into the ordinary condition in which we know them. We must assume, therefore, that the molecules concerned in chemical reactions, before entering into combination, or being given off in decomposition, undergo a change. Disengagements or absorptions of heat occur, which are quite independent of the thermal phenomena which accompany every chemical combination or decomposition. If the affinity is to be measured by the quantity of heat which the molecules exhibit in reactions, a conclusion can never be drawn as to the magnitude of the affinity from the permanence of compounds; for compounds whose constituents absorb in their separation the same amount of heat, will decompose more readily the more readily these constituents, once separated in the nascent state, disengage heat in their passage into the ordinary condition.

Preyer has described‡ a new analytical method for distinguishing colouring-matters, based on the phenomena which they pre-

* *Arch. für path. Anat.* vol. xxxiv. p 436.

† *Comptes Rendus*, vol. lxiii. p. 369.

‡ Schulze's *Archiv für mikro. Anat.* 1866.

sent when illuminated by homogeneous light. The stage of the microscope is illuminated by the various elementary rays of the spectrum.

Yellow light is bright enough for making the most delicate observations; the others range as follows in the order of decreasing intensity:—orange, red, green, blue, and indigo. Human blood, or the blood of frogs placed successively in these various rays, presents the following phenomena:—

In red light the oxygenated globules appear of an intense red; in orange light and in the least refrangible parts of yellow they are also red; in the most refrangible yellow they are only reddish. When the field passes into green, the globules entirely lose their red colour, and become completely black in the green nearest the yellow. In the extreme green they are a little brighter. In blue and violet the reddish tint reappears.

It follows thence that the globules contain a substance which absorbs green rays with the utmost energy; and as Hoppe-Seyler's hæmoglobine has its greatest absorption in the green, it is at least probable that this substance is hæmoglobine, thus furnishing a fresh proof that this body really represents the colouring-matter of the blood.

From these facts a very delicate method may be deduced for ascertaining the presence of blood. Three or four globules of the colouring-matter are placed on the stage, and it is observed whether they offer in the various homogeneous lights the phenomena of absorption corresponding to the rays of absorption of hæmoglobine—assuming in all cases that there is no other substance which behaves like hæmoglobine. Pigments which are similar in colour and in optical properties can also be distinguished by this method.

The substance is observed in crystals, or in amorphous masses, which may be spread on cloth. The same point of the field is fixed and the light varied; the tints are noticed in which the pigment loses its colour, deepens, becomes black, or retains its greatest intensity.

By this experiment a judgment can be formed of the appearance of a spectrum seen through a solution of a body. Thus the rays which produce the greatest obscuration will be either wholly or partially wanting in the spectrum; conversely, the spectrum of a colouring-matter being given, the presence of this colouring-matter can be demonstrated by the appearance it presents in the microscope.

Mitscherlich has described a new method of determining the composition of organic compounds*. He has succeeded in finding a method for determining directly all the elements of an or-

* Poggendorff's *Annalen*, April 1867.

ganic compound, including oxygen. The method for determining hydrogen and oxygen in an organic compound consists in heating it to redness in a current of chlorine, when the oxygen unites with the carbon in the substance, or that which has been added to it, while the hydrogen combines with the chlorine to form hydrochloric acid. The hydrogen is obtained by weighing the latter, while the oxygen is found from the carbonic oxide and carbonic acid formed.

He also describes a method for determining carbon, chlorine, bromine, iodine, sulphur, and nitrogen—a method for which he claims far greater accuracy and ease of execution. The determinations of chlorine, bromine, iodine, and sulphur are effected by the new method simultaneously with the determination of carbon and of nitrogen by a single analysis.

The principle of the method consists in volatilizing the bodies examined in a current of hydrogen, and burning the hydrogen along with the substances volatilized in it in pure oxygen in a special apparatus. The water formed is removed, and the other products (with the exception of nitrogen, which is determined by volume) are collected separately in weighed apparatus.

The author claims for his method, which he describes in detail, the advantages of being easy, certain, and rapid, and having the advantage over other methods of requiring little substance, and of being free from the sources of error to which they are liable.

XXX. On the so-called "Inactive" Condition of Solids.

By CHARLES TOMLINSON, F.R.S.

[Continued from p. 143.]

SINCE the date of my paper on the above subject in the Philosophical Magazine for this month, I have read some remarks in the *Comptes Rendus* on M. Gernez's theory of the disengagement of gases from their solutions, by those distinguished observers Messrs. Chevreul, Deville, and Matteucci. They not only assent to this theory, but refer to various phenomena on which, according to them, it throws light. M. Chevreul remarked that a large number of experiments had led him to admit the existence of a layer of air adhering to solid bodies exposed to the atmosphere; and he cites the experiment in which gas-bubbles are disengaged from the surface of a solid, plunged into a liquid, and exposed to an atmosphere that is being gradually rarefied by the air-pump. M. Deville considered the phenomena to be further illustrations of the theory of dissociation.

Premising that I do not consider the phenomena in question to fall under this beautiful theory, so far as I understand it, I beg to submit to the above-named leading European *savants* my theory, in a more precise form, and with some modifications.

A supersaturated solution of a gas with its upper surface freely exposed to the atmosphere is always giving off gas, either with effervescence or silently and imperceptibly. It does so because the excess of gas has but a slight adhesion to the liquid, and the air is virtually a vacuum for it—the only difference being that it would pass off into a real vacuum instantaneously. Now the remaining surface of the liquid, or that confined by the sides of the vessel, may be regarded as being in exactly the same condition, subject, however, to two modifications—(1) the state of chemical purity of their surface, and (2) the pressure exerted by them virtually on the liquid. (1) Suppose the vessel to be chemically clean. No gas will be disengaged and no bubbles will form on the sides, because the adhesion between the sides and the liquid is perfect; and therefore the sides may be regarded, *pro ratâ*, as merely a continuation of the liquid itself, and no bubbles will form there any more than in the central parts of the liquid. (2) But suppose the sides to be not chemically clean—to be dirty, in fact; adhesion is diminished or destroyed; and therefore the surface of the liquid next to such sides is virtually as free as its upper surface; bubbles consequently will form here, just as they do on the upper surface: but in the latter case they do not appear as bubbles (except in effervescence) because there is no pressure; the sides do exert pressure, and therefore bubbles are formed. Now it does not matter whether there be air or not between the sides and the liquid: there may be any kind of gas, possibly a vacuum, and the result will be the same. It is no function of air to induce the liberation of gas or the formation of gas-bubbles. It is really want of adhesion. Now apply this to the case of a so-called "inactive" glass rod, a coin, a fragment of flint, &c. A glass rod placed in the liquid does nothing more than form new sides, as it were, to the vessel, and its effect is merely that of the sides. If chemically clean, the rod will form no bubbles around it, and hence it is "inactive," because its adhesion is perfect. If dirty, the surface of liquid in contact with it will be as free, or almost so, as the upper surface. The same theory holds good, as I shall hereafter endeavour to prove, in the case of *nuclei*, in inducing crystallization in saline solutions. By means of this same theory I shall also attempt to reconcile the numerous and often conflicting statements respecting *ebullition*, and the effect of chemical purity or otherwise on the *boiling-points* of liquids,—all of which, I imagine, depend on the same law of adhesion, and have nothing to do with the air, except indirectly.

King's College, London,
August 20, 1867.

XXXI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 166.]

May 2, 1867.—Lieut.-General Sabine, President, in the Chair.

THE following communication was read:—

“Optics of Photography.—On a Self-acting Focus-Equalizer.”
By A. Claudet, F.R.S.

When a solid figure is brought too near the object-glass of a camera obscura, the difference of focus for its various planes is comparatively so great, that it is impossible that all the images should be equally well defined. Hence, in the case of photographic portraiture, there is a want of harmony in the representation of the various parts; some are too sharply delineated, and some others are confused in proportion as they are more and more distant from the plane in focus. But there is another defect which is the consequence of the difference of distance of the various planes bearing too great a proportion to the distance of the whole, which is that the nearest parts of the figure are too much enlarged, and the furthest too much reduced.

In a paper I read at the British Association at Nottingham in 1866, I proposed a plan to obviate these defects, which consisted in bringing all the planes consecutively into focus, by moving, during the exposure, the tube of the lens or the back frame of the camera; the consequence of which was, of course, that the planes were also during that movement brought out of focus, so that a sharp image of every plane was impressed upon a confused image; but they were all in the same degree in that mixed state, and the result was an equality of effect producing harmony in the whole, and that kind of softness in the picture so much approved by artists, as resembling, more than the sharpest photographs, the effect that they aim at producing.

The original simple idea of equalizing the focus of the various planes by moving either the frame holding the plate, or the tube of the lens, during the exposure had, it appears, occurred to several persons engaged in photographic pursuits (of which I was not aware before reading my paper); but it is certain that the plan had never been practically and generally adopted, and that, at all events, no specimens of the process had at any time been exhibited in public, probably because it presented several difficulties which could not be easily overcome. The greatest of these difficulties I soon found during my investigations, which was that, in changing the focal distances merely by moving the frame or the tube, the size of the various superposed images was unavoidably reduced or increased according to the alteration of focus during the movement applied.

Therefore I turned my attention to the means which might be found capable of avoiding this defect; and a fortunate idea presented itself, by which I found that it was possible to preserve the size of the various images during the adaptation of the focus to the different planes of the figure.

The desideratum was, when changing the focus, to increase the power of the double lens for the planes the most distant and to reduce it for the nearest planes. At first this seemed to be an impossibility. But in considering the subject attentively I was suddenly struck with the fact that, the power of any double combination of lenses being proportionate to the distance which separates the two lenses, greater when they are more separated, and smaller when they are less separated, it was possible to alter the power of the combination by changing the distance between the two lenses.

Therefore, if, instead of moving the whole tube containing the two lenses, we move only the back lens nearer the plate, when we want to focus for more distant planes, we increase at the same time the power of the double combination, and consequently the size of the image; and if we move the lens further from the plate, when we want to focus for the nearest planes, in doing so, by reducing the separation of the two lenses, we reduce the power of the combination, and consequently also the size of the image. This is a most fortunate property; for by this means it is possible not only to equalize the definition of the various planes, but at the same time to equalize the size of their images, and consequently to avoid the exaggeration of perspective by which the nearest planes are increased, and the furthest disproportionately reduced, a defect which is so detrimental to the appearance of large photographs.

I submitted my plan to M. Voigtlander, the celebrated optician, and I had the satisfaction to meet with his entire approbation. He found that I had solved the problem in a way which was perfectly correct and sufficient in practice. But wishing to investigate the question from a higher mathematical point of view, and being unable from indisposition to go himself into the subject, he charged his step-son, Dr. Sommer, Professor of Mathematics at the Carolinian College of Brunswick, well versed in all the questions of optical photography, to calculate the result of the gradual increase and reduction of the power of the double combination, in conjunction with the alteration of focus. Dr. Sommer entered thoroughly into the subject, and soon sent me a series of elaborate formulæ, showing that for all practical purposes in photography the movement of one of the two lenses, as I had proposed, fulfilled the object I had in view: still he found that a more scientific consideration of the subject called for a modification in my plan; which was that, instead of moving only one of the lenses, the same degree of their separation should be imparted by moving the two lenses in contrary directions from the fixed centre of the combination, and in different proportions, according to the distance of the object. These differential proportions were indicated in a table calculated by Dr. Sommer which he sent me.

This presented another difficult and unexpected problem, the solution of which was indeed most perplexing. But I did not like that it should be said that my plan was not completely in accordance with the mathematical laws of optics; and I set to work at finding a mechanical means by which I could avail myself of the scientific calculations of Dr. Sommer.

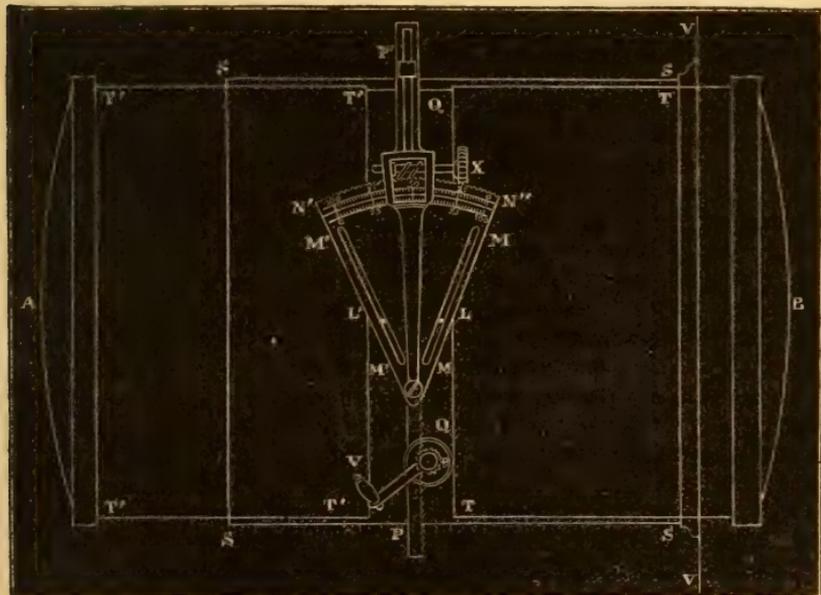
I have found such means; and it turns out indeed that by my mechanical construction the differential movement can be effected, not only as readily and easily, but with a greater command and steadiness than by moving only one lens. The following is a description of the arrangement:—

Description of the Focus-Equalizer.

The tube, containing at its two ends the lenses A and B, is divided into two parts, sliding in the principal tube SSSS fixed in the front of the camera at V V'.

Each tube has a strong pin, L and L'. These two pins are intended to push the tubes to and fro from the centre of the combination on the line P P' by means of the mechanical piece N N' N'' in the shape of a sextant, having two slits, M M and M' M', cut at an angle of 36°. Now the sextant, being mounted on a sliding bar Q Q', fixed in a socket holding to the tube SSSS at P P', can be made to move to and fro on the line P P' by means of a rack and pinion moved by a handle V on the axis R. While the sextant moves in the line P' P, the two slits will act on the two pins, and gradually increase the separation of the tubes; and on making the sextant move back from P to P', the slits will bring the two pins nearer each other, and decrease the separation of the tubes.

It will thus be easily understood how we can increase and reduce the separation of the two lenses from the centre of the combination; but we have now to explain how we can produce the differ-



ential movement according to the mathematical formulæ calculated by Dr. Sommer.

The arc of the sextant is divided into 100 parts, in two rows one
Phil. Mag. S. 4. Vol. 34. No. 229. Sept. 1867. R

against the other. The divisions on the outer limb have their zero on the left, and the 100 division on the right; on the inside limb the divisions are in a contrary direction.

By means of the endless screw X acting on the toothed edge of the sextant, it can be moved on its horizontal axis, so that any of its divisions may be brought under the index fixed on the middle bar Q Q'.

Now, supposing that by the table of Dr. Sommer the lens A for a certain distance of the object should move 0.235, and the lens B 0.765 of the whole space by which the lenses require to be separated or approximated, we turn the endless screw until the index is on the $23\frac{1}{2}$ division of the inside scale, and of course on the $76\frac{1}{2}$ division of the outside scale.

In that position of the sextant the slits M M and M' M', by means of the pins attached to the tubes of the lens A and of the lens B, will make them accordingly move—A in the proportion of 0.235, and B in the proportion of 0.765 of the whole space.

If for another distance the lens A should have to move 0.333, and the lens B 0.666, setting both limbs of the sextant to these divisions, the lens A will move $\frac{1}{3}$, and the lens B $\frac{2}{3}$ of the whole space.

If we wanted to move the two lenses in the same proportion, the sextant should be set so that the 50th division of both scales should be under the index.

And, finally, if, for the sake of comparative experiments, it were wanted to move only the lens A or the lens B, the slit of the pin for either and the zero of the scale should be placed under the index, by which that lens would be completely stationary, and the whole motion imparted to the other.

Latest Improvements, since the Communication to the Royal Society.

In the construction of the apparatus it was necessary to reduce to its minimum the friction of the various parts of the instrument and to render the action as smooth and easy as possible; otherwise the force necessary to overcome resistance would almost inevitably cause vibration of the whole apparatus, and thereby of the optical image on the sensitive surface, and consequently confusion in the ultimate formation of the photographic picture.

The lenses being fixed, each on its separate tube, one sliding in the other, in order to prevent the vibration of the two lenses during the movement, the tubes must fit very closely, in which case the friction is very great, and the evil described above occurs inevitably. To avoid this, the two tubes holding the lenses are made sufficiently different in size from the external tube, so as to leave a free space between them. Now a steady and regular movement is obtained by means of a slit made lengthwise in the top of the external tube, through which the pins attached to the two moving tubes holding the lenses can slide very freely. By means of a flat head placed at the top of each pin, and supporting the tube holding the lens, the whole friction is reduced to the bearing of the flat heads of the pins while they are sliding on the narrow strips forming the edges of the

slit; and that friction is so reduced that it is not even capable of preventing the lenses sliding by the force of their own gravity as soon as the whole tube is placed in the least degree in an inclined position. This is very advantageous; for it supplies the means (about to be explained) of rendering the whole instrument complete and perfect, making it self-acting with any velocity required, and in its regular action independent of any error in the appreciation of time during the exposure. All these advantages it might have been at first considered impossible to obtain; at the same time considerable difficulties are avoided in the working out of the plan of a moving focus which might have constituted a decided impediment to its adoption. But now that the movement can be imparted to the whole with the least power, we are able to communicate it with an ordinary clock work, and by means of a regulator to produce the separation of the two lenses required in any given time according to the exposure judged necessary beforehand, on account of the greater or less intensity of light.

Thus are effectually removed all the practical objections which at first were offered against the adoption of this great improvement in photography—as in the sequel it is sure to be found, although, like many other most useful and new inventions, it has been exposed to the opposition and sneers of those who, mainly influenced by prejudices, are always prompt to judge without taking the trouble of examining and understanding the conscientious labours which may have been performed at a great expense of money, time, and thought, and in the present case without any possible view of pecuniary remuneration, but only in the love of progress and for the honour of contributing to the advancement of the art.

May 16.—William Bowman, Esq., V.P., in the Chair.

The following communications were read:—

“On the Internal Distribution of Matter which shall produce a given Potential at the Surface of a Gravitating Mass.” By G. G. Stokes, M.A., Sec. R.S.

It is known that if either the potential of the attraction of a mass attracting according to the law of the inverse square of the distance, or the normal component of the attraction, be given all over the surface of the mass, or any surface enclosing it (which latter case may be included in the former by regarding the internal density as null between the assumed enclosing surface and the actual surface), the potential, and consequently the attraction, at all points external to the surface and at the surface itself is determinate. This proposition leads to results of particular interest when applied to the Earth, as I showed in two papers published in 1849*, where among other things I proved that if the surface be assumed to be, in accordance with observation, of the form of an ellipsoid of revolution,

* “On Attractions, and on Clairaut’s Theorem,” Cambridge and Dublin Mathematical Journal, vol. iv. p. 194; and “On the Variation of Gravity at the Surface of the Earth,” Cambridge Philosophical Transactions, vol. viii. p. 672.

Clairaut's Theorem follows independently of the adoption of the hypothesis of original fluidity, or even of that of an internal arrangement in nearly spherical strata of equal density.

But though the law of the variation of gravity which was originally obtained as a consequence of the hypothesis of primitive fluidity, and was afterwards found by Laplace to hold good, on the condition that the surface be an ellipsoid of revolution as well as a surface of equilibrium, provided only the mass be arranged in nearly spherical strata of equal density, be thus proved to be true whatever be the internal distribution, the question may naturally be asked, Does not the condition that the potential at the surface shall have its actual value require that the internal distribution shall be compatible with that of a fluid mass, or at any rate shall be such that the whole mass shall be arranged in nearly spherical strata of equal density? Such a question was in fact asked me by an eminent mathematician at the time to which I have alluded. I replied by referring to the well-known property of a sphere, according to which a central mass may be distributed uniformly over its surface without affecting the external attraction, by applying which proposition to a mass such as the Earth we may evidently, without affecting the external attraction, leave a large excentrically situated cavity absolutely vacuous, the matter previously within it having been distributed outside it. It is known further that the mass of a particle may be distributed over *any surface whatsoever* enclosing the particle without affecting the external attraction; and in this way we see at once that we may leave *any internal space we please*, however excentrically situated, wholly vacuous; nor is it necessary in doing so to introduce an infinite density, by distributing the whole mass previously within that space over its surface, since that mass may be conceived to be divided into an infinite number of infinitely small parts, which are respectively distributed over an infinite number of surfaces surrounding the space in question. These considerations, however, though they readily show that the internal distribution may be widely different from any that is compatible with the hypothesis of primitive fluidity, do not lead to the general expression for the internal density. Circumstances have recently recalled my attention to the subject, and I can now indicate the mode of obtaining the general expression required in the case of any given surface.

Let the mass be referred to the rectangular axes of x, y, z , and let ρ be the density, V be the potential of the attraction. Then for any internal point V satisfies, as is well known, the partial differential equation

$$\frac{d^2V}{dx^2} + \frac{d^2V}{dy^2} + \frac{d^2V}{dz^2} = -4\pi\rho, \quad (1)$$

or, as it may be written for brevity, $\nabla V = 0$. This equation may be extended to all space by imagining the body continued infinitely but having a density which is null outside the limits of the actual body; and by adopting this convention we need not trouble ourselves about those limits. Conversely, if V be a continuously varying

function of x, y, z , which vanishes at an infinite distance, and satisfies the partial differential equation (1), V is the potential of the attraction of the mass whose density at the point (x, y, z) is ρ ; or, in other words,

$$V = \iiint \frac{\rho'}{r} dx' dy' dz' \dots \dots \dots (2)$$

(where r is the distance between the points (x, y, z) and (x', y', z') , ρ' the density at (x', y', z') , and the limits are $-\infty$ to $+\infty$) is the complete integral of (1) subject to the condition that V shall vanish at an infinite distance.

This may be proved in different ways; most directly perhaps by taking the expression for the potential (U suppose) which forms the right-hand member of (2), substituting for ρ' its equivalent $-\frac{1}{4\pi} \nabla V'$, V' being the same function of x', y', z' that V is of x, y, z , and transforming the integral in the manner done by Green*, when we readily find $U = V$.

Suppose now that we have a given closed surface S containing within it all the attracting matter, and that the potential has a given, in general variable, value V_0 at the surface. For the portion of space external to S , V is to be determined by the general equation $\nabla V = 0$, subject to the conditions $V = V_0$ at the surface, and $V = 0$ at an infinite distance. We know that the problem of determining V under these circumstances admits of one and but one solution, though it is only for a very limited number of forms of the surface S that the solution can actually be effected. Conceive the problem, however, solved, and from the solution let the value of $\frac{dV}{d\nu}$ at the surface be found, ν being measured outwards along the normal. Now complete V for infinite space by assigning to the space within S any arbitrary but continuous† function we please, subject to the two conditions, 1st, that at the surface it is equal to the given function V_0 ; 2ndly, that it gives for the value of $\frac{dV}{d\nu}$ at the surface that already got from the solution of the problem referred to in this paragraph. This of course may be done in an infinite number of ways, just as we may in an infinite number of ways join two points in a plane by a continuous curve starting from the two points respectively in given

* Essay on the Application of Mathematical Analysis to the Theories of Electricity and Magnetism, Nottingham, 1828, Art. 3; or the reprint in Crelle's Journal, vol. xlv. p. 360.

† To avoid prolixity, I include in "continuous" the requirement that the differential coefficients of the function, to any order required, shall vary continuously. What that order may be it is perfectly easy in any case to see. We may of course imagine distributions in which the density becomes infinite at one or more points, lines, or surfaces, but so that a finite volume contains only a finite mass. But such distributions may be regarded as limiting, and therefore particular, cases of a distribution in which the density is finite; and therefore the supposition that ρ is finite does not in effect limit the generality of our results.

directions, which curve may be either expressed by some algebraical or transcendental equation, or conceived as drawn *liberâ manu*, and thought of independently of any idea of algebraical expression. The function V having been thus assigned to the space internal to S , the equation (1) gives, according to what we have seen, the most general expression for the density of the internal matter.

There is, however, no distinction made in this between positive and negative matter; and if we wish to avoid introducing negative matter we must restrict the function V for the space internal to S to satisfy the imparity

$$\frac{d^2V}{dx^2} + \frac{d^2V}{dy^2} + \frac{d^2V}{dz^2} \neq 0.$$

It is easy from the general expression to show, what is already known, that the matter may be distributed in an infinitely thin, and consequently infinitely dense stratum over the surface S , and that such a distribution is determinate.

We know that there exists one and but one continuous function applying to the space within S which satisfies the equation $\nabla V = 0$, and is equal to V_0 at the surface. Call this function V_1 . It is to be remarked that the value of $\frac{dV_1}{dv}$ at the surface is not the same as

that of $\frac{dV}{dv}$, V being the external potential, though V_1 and V are there each equal to V_0 . The argument, it is to be observed, does not assume that the two are different; it merely avoids assuming that they are the same; the result will prove that they cannot be the same all over S unless the density, and consequently the potential, be everywhere null, and therefore $V_0 = 0$. Now, attribute to the interior of S a function V which is equal to V_1 except over a narrow stratum adjacent to S , the thickness of which will in the end be supposed to vanish, within which V is made to deviate from V_1 in such a manner as to render the variation of $\frac{dV}{dv}$ continuous and rapid instead of abrupt. On applying equation (1), we see that the density is everywhere null except within this stratum, in which it is very great, and in the limit infinite. For the total quantity of matter contained in any portion of the stratum, we have from (1)

$$-\frac{1}{4\pi} \iiint \nabla V dx dy dz,$$

the integration extending over that portion. Let the portion in question be that corresponding to a very small area A of the surface S ; we may suppose it bounded laterally by the ultimately cylindrical surface generated by a normal to S which travels round the perimeter of A . Taking now rectangular coordinates λ, μ, ν , of which the last is parallel to the normal at one point of A , since ∇ is not changed in form by referring it to a new set of rectangular axes, we

have for the mass required

$$-\frac{1}{4\pi} \iiint \left\{ \frac{d^2V}{d\lambda^2} + \frac{d^2V}{d\mu^2} + \frac{d^2V}{d\nu^2} \right\} d\lambda d\mu d\nu.$$

Of the differential coefficients within brackets, the last alone becomes infinite when the thickness of the stratum, and consequently the range of integration relatively to λ , becomes infinitely small. We have in the limit

$$\int \frac{d^2V}{d\nu^2} d\nu = \frac{dV}{d\nu} - \frac{dV_1}{d\nu},$$

both differential coefficients having their values belonging to the surface. Hence we have ultimately for the mass

$$\frac{A}{4\pi} \left(\frac{dV_1}{d\nu} - \frac{dV}{d\nu} \right).$$

Hence, if ϖ be the superficial density, defined as the limit of the mass corresponding to any small portion of the surface divided by the area of that portion,

$$\varpi = \frac{1}{4\pi} \left(\frac{dV_1}{d\nu} - \frac{dV}{d\nu} \right), \dots \dots \dots (3)$$

which is the known expression.

In assigning arbitrarily a function V to the interior of S , in order to get the internal density by the application of the formula (1), we may if we please discard the second of the conditions which V had to satisfy at the surface, namely that $\frac{dV_1}{d\nu} = \frac{dV}{d\nu}$; but in that case to the mass, of finite density, determined by (1) must be added an infinitely dense and infinitely thin stratum extending over the surface, the finite superficial density of this stratum being given by (3).

We have seen that the determination of the most general internal arrangement requires the solution of the problem, To determine the potential for space external to S , supposed free from attracting matter, in terms of the given potential at the surface; and the determination of that particular arrangement in which the matter is wholly distributed over the surface, requires further the solution of the same problem for space internal to S . If, however, instead of having merely the potential given at the surface S we had given a particular arrangement of matter within S , and sought the most general rearrangement which should not alter the potential at S , there would have been no preliminary problem to solve, since V , and therefore its differential coefficients, are known for space generally, and therefore for the surface S , being expressed by triple integrals.

Instead of having the attracting matter contained within a closed surface S , and the attraction considered for space external to S , it might have been the reverse, and the same methods would still have been applicable. The problem in this form is more interesting with reference to electricity than gravitation.

“On the Occlusion of Hydrogen Gas by Meteoric Iron.” By Thomas Graham, F.R.S.

Some light may possibly be thrown upon the history of such

metals found in nature as are of a soft colloid description, particularly native iron, platinum, and gold, by an investigation of the gases which they hold occluded, such gases being borrowed from the atmosphere in which the metallic mass last found itself in a state of ignition. The meteoric iron of Lenarto appeared to be well adapted for a trial. This well-known iron is free from any stony admixture, and is remarkably pure and malleable. It was found by Wehrle to be of specific gravity 7.79, and to consist of—

Iron	90.883
Nickel	8.450
Cobalt	0.665
Copper	0.002

From a larger mass a strip of the Lenarto iron 50 millimetres by 13 and 10 millimetres, was cut by a clean chisel. It weighed 45.2 grammes, and had the bulk of 5.78 cubic centimetres. The strip was well washed by hot solution of potassa, and then repeatedly by hot distilled water, and dried. Such treatment of iron, it had been previously found, conduces in no way to the evolution of hydrogen gas when the metal is subsequently heated. The Lenarto iron was enclosed in a new porcelain tube, and, the latter being attached to a Sprengel aspirator, a good vacuum was obtained in the cold. The tube being placed in a trough combustion-furnace, was heated to redness by ignited charcoal. Gas came off rather freely, namely—

In 35 minutes	5.38	cub. centims.
In 100 minutes	9.52	„
In 20 minutes	1.63	„
In 2 hours 35 minutes	16.53	„

The first portion of gas collected had a slight odour, but much less than that of the natural gases occluded by ordinary malleable iron. The gas burned like hydrogen. It did not contain a trace of carbonic acid, nor any hydrocarbon vapour absorbable by fuming sulphuric acid. The second portion of gas collected, consisting of 9.52 cub. centims., gave by analysis—

Hydrogen.....	8.26	cub. centims	85.68
Carbonic oxide.....	0.43	„	4.46
Nitrogen	0.95	„	9.86
	<u>9.64</u>	„		<u>100.00</u>

The Lenarto iron appears, therefore, to yield 2.85 times its volume of gas, of which 86 per cent. nearly is hydrogen. The proportion of carbonic oxide is so low as $4\frac{1}{2}$ per cent.

The gas occluded from a carbonaceous fire, by iron, is very different, the prevailing gas then being carbonic oxide. For comparison a quantity of clean horseshoe-nails was submitted to a similar distillation. The gas collected from 23.5 grammes of metal (3.01 cub. centims.) was—

In 150 minutes	5·40	cub. centims.
In 120 minutes	2·58	„
In 4 hours 30 minutes	7·98	„

The metal has given 2·66 times its volume of gas. The first portion collected appeared to contain of hydrogen 35 per cent., of carbonic oxide 50·3, of carbonic acid 7·7, and of nitrogen 7 per cent. The latter portion collected gave more carbonic oxide (58 per cent.) with less hydrogen (21 per cent.), no carbonic acid, the remainder nitrogen. The predominance of carbonic oxide in its occluded gases appears to attest the telluric origin of iron.

Hydrogen has been recognized in the spectrum-analysis of the light of the fixed stars, by Messrs. Huggins and Miller. The same gas constitutes, according to the wide researches of Father Secchi, the principal element of a numerous class of stars, of which α Lyræ is the type. The iron of Lenarto has no doubt come from such an atmosphere, in which hydrogen greatly prevailed. This meteorite may be looked upon as holding imprisoned within it, and bearing to us, hydrogen of the stars.

It has been found difficult, on trial, to impregnate malleable iron with more than an equal volume of hydrogen, under the pressure of our atmosphere. Now the meteoric iron gave up about three times that amount, without being fully exhausted. The inference is that the meteorite has been extruded from a dense atmosphere of hydrogen gas, for which we must look beyond the light cometary matter floating about within the limits of the solar system.

XXXII. *Intelligence and Miscellaneous Articles.*

ON A PHOTOMETER FOR MEASURING THE TRANSPARENCE OF THE AIR. BY M. A. DE LA RIVE.

ALL inhabitants of valleys know that one of the most certain fore-casts of rain is the clearness, accompanied with a bluish colour, with which distant mountains are seen. This indicates the presence of great moisture in the air; but it may be asked why this moisture should facilitate the transmission of light, while it is well known to stop that of radiant heat, as is proved by Tyndall's beautiful experiments.

This phenomenon was already mentioned by De Saussure, in his *Essai sur l'Hygrométrie*. "When," he says, "the inhabitants of mountains see the air perfectly transparent, distant objects very distinctly, and the heavens of an extremely dark blue, they regard rain as very probable, though there appears no other sign. In fact," adds Saussure, "I have often observed that when for several days the weather is decidedly fine the air is not perfectly transparent; a bluish vapour is seen which does not arise from aqueous vapour (for it does not affect the hygrometer), but the nature of which is unknown to us."

This influence of moisture on the transparence of the air is also perceived on the surface of the sea on bright days. Thus in fine weather the chalk cliffs of Dover are seen from the French coast, a

distance of seven leagues. Humboldt observes that the peak of Teneriffe is visible at extraordinary distances, either immediately or even several hours after an abundant rain.

The cause of this phenomenon is not to be sought in an optical effect arising from a mixture of air and of aqueous vapour, as was formerly believed, but simply in the fact that aqueous vapour partially dissolves the impurities mixed with the air and makes it more translucent. This opinion, originally expressed by Colonel Jackson in 1832, always seemed well based, but it needed to be more precisely expressed and supported by experiment. M. Pasteur's remarkable researches, in showing that the atmosphere, more especially the layers nearest the soil, is filled with a mass of organic germs, seemed to furnish a key to these phenomena, at the same time that they have enabled me to explain the circumstances other than moisture which influence the transparency of the air.

All organic germs form, when the atmosphere is dry, a sort of slight mist which intercepts some of the light from distant objects; but when a general moisture sets in the mist disappears, either because the germs become transparent from absorbing aqueous vapour, or because the water which they have absorbed rendering them heavier makes them fall to the ground. This, as I think, is the most frequent cause of those striking changes in the transparency of the atmosphere which are sometimes manifested in the most unexpected manner, but which always coincide with variations in moisture.

Further, if the presence of aqueous vapour makes the atmosphere transparent when it contains organic germs, this presence is no longer necessary in the absence of these germs. This explains why mountains in winter are visible even when the air is very dry, why the air is transparent on snow fields, why, again, as has been observed by Humboldt, the atmosphere of the Peak of Teneriffe is transparent with an east wind which brings the African air; for this, having brought no organic exhalations from the deserts whence it comes, or from the sea over which it has passed, has no need of moisture to be transparent. It is, on the contrary, in the hot season and in the months in which organic life is most active that the air is most charged with this kind of dry vapour, which in serene weather diminishes so materially the visibility of distant objects.

These considerations have led me to the conclusion that it would be truly interesting to include the transparency of the atmosphere in the number of meteorological elements which are regularly determined, so as to establish precise relations between this particular element and others, such as pressure, temperature, degree of moisture, direction of the wind, and especially of the time of day and of the year (that is, the seasons). This kind of observations would be interesting, not merely as a question of pure science, but also perhaps for medicine, from the point of view of hygiene and epidemic maladies. It is probable, in fact, that the miasmata which M. Bous-singault, in a beautiful research published in 1834, had found to contain hydrogen, are due to these organic germs, whose presence in the atmosphere and falling to the ground would be indicated in a sufficiently exact manner by the greater or less transparency of the air.

I have sought, therefore, a means of estimating the degree of transparence of the air with ease and exactness ; and in this investigation I have been materially aided by Professor Thury of Geneva. Under his directions and by his plans has been constructed, by the Genevese Society for making physical instruments, the photometer for measuring variations in the transparence of the air, of which the following is an abridged description.

The instrument renders it possible to make a simultaneous comparative observation of two similar views placed at different distances. The difference between the optical images of these views expresses the effect produced by the interposition of a layer of air of known thickness equal to the difference in the distance of the two. The two images are made equal by means of an arrangement fitted to the instrument itself ; and the measure of the modification necessary to bring about this equality gives the difference in the brightness of the two images, and hence the effect of the interposition of the layer of air.

The two objects to be compared should be viewed by the same eye, in the same conditions, and in the same general direction ; and since a comparison of objects can only be made by that of their images, these should be quite like the objects they represent. Moreover any light foreign to the objects should be carefully excluded.

It has been attempted to realize these conditions by means of two telescopes, each having an object-glass, but with an eyepiece common to both. Each object-glass gives its image in half the field of the eyepiece. The optical axes of the two object-glasses form an angle which may vary from zero to 29° at the will of the observer. The luminous ray from the principal axis of each object-glass is brought into a direction parallel to the axis of the eyepiece by two successive total reflexions which it undergoes ; the first takes place in a moveable prism, and the second by a fixed one placed very near the focus of the eyepiece. The angular motion of the first prism is connected with that of the moveable part of the corresponding telescope, so that the angle described by the prism is always half that of the telescope. Thus whatever be the point to which the telescope is directed, the image of this point always occupies the centre of the eyepiece. What takes place with the luminous beam in one of the telescopes takes place in exactly the same manner with the other ; so that in the focus of the eyepiece there are two juxtaposed images.

On the other hand, the motion of a milled head placed in the hand of the observer makes the telescope describe equal angles with the axis of the eyepiece, which is that of the system ; and thus the two images are produced in identical conditions as regards reflexions in prisms. In order that the observer may at any time be certain that there is complete identity in the manner in which the two images are produced, the whole apparatus is capable of being turned by an angular motion of 180° about the common axis of the system. There is thus seen with one of the telescopes what was previously seen with the other. When there is a difference between the two images, the observer may easily convince himself that it is not owing to the instrument itself ; or if there are small differences

between the two halves of the instrument, which produce a corresponding difference in the images, they may be easily detected and allowed for.

It may be added that a diaphragm may slide at the focus of the eyepiece so as to uncover only equal portions of the two views, and that the adjustment is effected separately for the object-glasses and for each telescope.

The optical system must exclude both foreign colour and reflected light, conditions which are only realized by means of excellent object-glasses well achromatized and not presenting too pronounced colours of their own. The prisms must also be made of very pure glass, whose colour, as far as possible, should be complementary to that of the object-glasses. Lastly, the magnifying-power should be so small that the telescope may give its maximum light; for the greater the intensity of the pencil emerging from the eye-glass, the less perceptible will be the inevitable imperfections of the instrument. Hence the diameter of the ring of the eyepiece should be almost the same as that of the pupil. And in order not to introduce a variable element, the minimum (and not the mean) diameter should be taken. In the present apparatus the object-glasses are 54 millims. in diameter, and the magnifying-power is 22 times; so that 2.4 millims. is the diameter of the ring of the eyepiece, while the field of the eyeglass is $1^{\circ} 26'$.

The means by which the observer may modify one of the two images to make it equal to the other are those employed in the various photometers; and the present instrument enables any of them to be used at the will of the observer, and according to the object he proposes. The simplest is the use of diaphragms with variable apertures placed before the object-glass; this is what has been hitherto used, and it has given good results. Each of the accessories may be applied to the two telescopes alternately, or to both at once, as means of comparison and control.

The instrument may in case of need be used as a general photometer; and as it is provided with circles divided vertically and azimuthally, as well as a graduated arc to measure the angular distance of the two telescopes, as, moreover, either of these may easily be directed to the zenith, it constitutes also an astronomical photometer for measuring the lustre of the stars. Two portions of the sky may also be compared, as to the difference of lustre and colour between them, if care be taken to choose two portions where the atmospheric polarization is almost the same.—*Comptes Rendus*, June 17, 1867.

ON THE LAWS WHICH GOVERN THE GENERAL DISTRIBUTION OF
HEAT OVER THE EARTH. BY PLINY EARLE CHASE.

The principal elements of general thermometric variation are (1) the heat imparted by the sun, (2) terrestrial absorption and radiation, and (3) atmospheric currents. Of these three agencies the first is, in one sense at least, the chief, since it is the one on which the others depend; the second is mainly instrumental in modifying the

other two, and especially in retarding the daily and yearly changes; the third is a subject of hourly experience, and its meteorological importance is now generally recognized.

The amount of heat which is received directly from the sun evidently varies as the cosine of the zenith-distance, or the sine of the sun's altitude. In the daily distribution of temperature this is the most important element, as is evident from the tabular comparisons in my communication of September 21, 1866*. Absorption and radiation proceed at nearly uniform rates; therefore it may be assumed that their effects are approximately proportional to the time during which they operate. The average general variation which is due to the influence of the winds is a difficult point to determine; but the present investigation has led me to believe that it may be measured by the difference of *arc* (instead of the *sine*-difference) of the sun's meridian altitude. My reasons for this inference are the following:— (1) The general average temperature of the year often appears to vary very nearly as the arc in question; (2) it seems unreasonable to suppose that a variation of this character can be attributable either to the heat communicated by the sun or to terrestrial absorption and radiation; (3) the tendency of the air, so far as it is determined by the direct heat of the sun, is at all times toward that point of the earth's surface at which the sun is vertical, and we may readily believe that that tendency should be proportional to the distance, measured on a great circle of the earth, through which the air would be obliged to move in order to reach the subsolar point. This distance evidently varies as the *arc* of the sun's zenith-distance.

We have then three natural standards for admeasurement, by means of which, if we rightly eliminate special and limited perturbations, we may perhaps be able to determine the predominating influence, in many cases, both of local and of general thermal disturbance. In order to institute as broad a comparison as possible, I have adopted a method of elimination which may be illustrated by a single example.

The average monthly temperatures of the United States, as deduced from Professor Coffin's reductions, appear to be as follows:—

Jan. 28°352 Feb. 30°873 Mar. 39°049 Apr. 49°744 May 60°902 June 69°780
 July 75°640 Aug. 71°754 Sept. 65°643 Oct. 53°922 Nov. 42°350 Dec. 32°132

Averaging the temperature at equal intervals from January (taking the mean temperature of Dec. and Feb., of Nov. and March, &c.), we get the following results:—

Months from January.	0.	1.	2.	3.	4.	5.	6.
Average temp. ...	28°352	31°502	40°699	51°833	63°272	70°767	75°640
Monthly diff.....	3°150	12°347	23°481	34°920	42°415	47°288
Ratio of do.	·067	·261	·497	·738	·897	1·000
Ratio of diff. of } zenith-distance }	·069	·255	·500	·745	·931	1·000

The second of the above series of ratios (that of the differences in the arcs of the sun's zenith-distance) is based upon the following

* Proc. Amer. Phil. Soc. vol. x. pp. 261-269. See especially the observations at St. Bernard, and the general average of Table I. p. 267.

estimate of the average monthly increase of solar altitude at all places in the temperate zones:—

Months from winter solstice.	1.	2.	3.	4.	5.	6.
Increase of solar altitude ...	3°	12°	23 $\frac{1}{2}$ °	35°	43 $\frac{3}{4}$ °	47°
Ratio of increase	·069	·255	·500	·745	·931	1·000

If we allow about twenty-four days for the cumulative effects of increasing heat and cold, these ratios become properly comparable with the monthly ratios of temperature-variation, as in the following Table, which is compiled from the works of Dove and Guyot:—

Difference of time, months ..	1.	2.	3.	4.	5.	6.
Ratios of sines.....	·076	·284	·545	·784	·946	1
Ratios of arcs	·069	·255	·500	·745	·931	1
Northern hemisphere	·076	·259	·512	·753	·935	1
Southern hemisphere	·077	·281	·538	·763	·938	1
Arctic region	·071	·231	·479	·733	·934	1
Europe.....	·069	·239	·501	·738	·938	1
Asia	·097	·274	·535	·769	·934	1
North America ...	·055	·279	·494	·741	·917	1
South America ...	·077	·275	·501	·724	·936	1
Africa	·088	·267	·537	·761	·938	1
Australia	·081	·313	·618	·801	·913	1

An extensive series of comparisons* seems to warrant the following inferences, all of which are confirmed by other considerations:—

(1) Taking into view the entire land surface of the globe and the entire range of the year, the direct heat of the sun and the induced aërial currents appear to be about equally instrumental in determining fluctuations of temperature.

(2) The influence of the winds is most marked in the Northern and Western hemispheres; that of solar obliquity in the Southern and Eastern hemispheres.

(3) Where the sun's rays are least intense (as in the Polar Regions) and where the winds are most variable, the ratios exhibit the nearest parallelism to the increments of arc; but where the winds are most uniform (in and near the region of monsoons) they correspond more closely with the sinal increments.

(4) The general changes of temperature at midwinter and at the equinoctial seasons (when the sun's declination is changing most rapidly) are most dependent upon the local solar heat; the midsummer changes are more subject to the influence of the winds.

(5) The greatest conflict of opposing forces occurs during the sun's passage between the comparatively wind-governed Northern hemisphere and the sun-governed Southern hemisphere. This conflict is manifested in the spring and autumn rains.

(6) The closest and most general approximation of ratios is shown in the monthly-temperature change at midsummer, which corresponds almost precisely with the change of arc.—Silliman's *American Journal* for July 1867.

ON THE SPONTANEOUS CHANGE OF A LIQUID CYLINDER INTO ISOLATED SPHERES. BY DR. FÉLIX PLATEAU†.

In the second series of his researches "On the Figures of Equili-

* See Proceedings, &c., *loc. cit.*

† From a separate impression from the *Bulletin de l'Académie Royale de Bruxelles*, No. 7, 1867. Communicated by the Author.

librium of a Liquid Mass without Weight," my father showed that a very elongated liquid cylinder, or, more generally, any liquid figure one of whose dimensions is relatively considerable compared with the others, always changes spontaneously into a series of isolated spheres: it was on this principle that he based a theory of the constitution of liquid veins emitted from circular apertures. His experiments require particular instruments; I have accidentally hit upon a very simple method by which the phenomena may be studied without any special apparatus.

At the end of a cotton thread about 0.2 millim. in diameter and 50 centims. in length a weight of a few grammes is fixed; after having carefully moistened the thread by rubbing it in water, so as to remove adherent air, it is held by its free end, and allowed to sink in a vessel of water about 40 centims. in height. Being then held quite vertically, it is withdrawn with as uniform a velocity as possible, the lower end remaining immersed in the liquid: this operation should not occupy a longer time than five or six tenths of a second. The thread is seen to be covered on the length which has been immersed with a series of small elongated pearls of water pretty regularly arranged, the centres being about 5 millims. from each other.

If the thread is kept sufficiently motionless, the liquid pearls may remain in their relative positions for as much as ten seconds. Several of them then sink, and unite with those below, so that the distances between them become greater, while their diameter increases. This alteration, which ultimately brings all the liquid to the bottom of the thread, is at first slow, but afterwards increases as the pearls become larger.

When the thread is thicker and is taken out more rapidly, the liquid masses are larger, at greater distances, and the alteration begins almost immediately.

If we wish to have the phenomenon more regular, and also even more easily seen, a cotton thread with a weight is still used, but olive-oil is taken instead of water. In this case the thread, which has been well soaked and freed from air, is withdrawn with less rapidity; and when this is effected (save the lower end, which remains immersed), it is well to fix the free end to any suitable support which overhangs the vessel. The thread is then covered with pearls as in the previous experiment, but they are arranged with almost perfect regularity: their diameter is about 0.5 millim., the mutual distances of their centres 2.5 millims., and almost a hundred may be counted on a thread 25 centims. in length. This small and extremely delicate necklace remains without appreciable change for thirty seconds: the alterations it undergoes are slow and slight; and it is only after ten minutes that they appreciably modify the appearance of the whole.

If the capillary forces which determine the transformation of elongated liquid figures did not exist, the thread, which carries with it a certain quantity of liquid, would be seen to be covered with a liquid layer constituting a virtually cylindrical figure. But these forces acting continually, the liquid cylinder, in proportion as the thread is withdrawn, obeys their action, and the small pearls thus form

rapidly after each other during the ascent. These small masses tend to constitute spheres; but the thread which traverses them compels them to take a more elongated form, and they really constitute portions of the figure which my father has called the *unduloid*.

In the above experiments the change is gradual, as I have stated; but it is easy to make the experiment so that it takes place along the entire length of the thread. For this purpose a horizontal thread must be used instead of a vertical one. A thread about 20 centims. in length is stretched between the ends of a small wooden bow, of which it forms the string; and the liquid is poured on a plate. In this way, after the thread has been well moistened and immersed in the liquid, it may be withdrawn, being preserved in a horizontal position. The small pearls appear then all at once and remain for an indefinite period in their position, provided the thread be kept horizontal. With oil the result is as regular as in the case of a vertical thread, but with water the regularity is not all that could be desired. The small imperfections arise from inequalities in the thread. This is proved by the circumstance that, if we commence several times with the same thread, they are always produced at the same places.

My father has also shown that in the change of the cylinder into isolated spheres, the cylinder commences by dividing into alternately enlarged and contracted portions, and that the contractions deepen until rupture ensues, while the enlargements increase. In order to see this formation of enlargements and contractions, the method with the vertical thread may be modified as follows:—

Oil is used, but instead of a cotton thread a straight steel wire is taken—a knitting-needle, for instance, 0·8 millim. in diameter, and 25 centims. in length; the surface is made capable of being easily moistened, by being dipped in weak nitric acid; it is then well washed and perfectly dried. When the experiment is to be made, the wire is rubbed with a paper moistened with oil; it is then plunged *vertically* in the liquid and withdrawn in a space of time which should not exceed a minute and a half.

The surface at first is seen to be covered with an almost uniform layer of oil, a little thick at the lower end; after about a second this layer contracts at regular intervals, and the intermediate spaces enlarge so slowly that the phenomenon can be observed; the contractions then deepen, while the enlargements increase and separate from each other: there are thus formed separate masses, which, acted on by gravity, descend along the steel needle, and successively join the liquid in the vessel; there may be as many as fifty.

The contractions and enlargements commence to form at the bottom of the wire, and the change gradually ascends. If the phenomenon does not take place at all heights, it is due to the solid axis, which evidently hinders the transformation. This takes place in preference where the liquid is thickest (that is, down below), as may be easily proved by using a thicker steel wire—2 millims., for instance, in diameter; in this case the relation between the solid axis and the thickness of the layer of oil is so unfavourable that only traces of contractions and enlargements are obtained below.

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XXXIII. *On the Water of the River Severn at Worcester.* By
AUGUSTUS BEAUCHAMP NORTHCOTE, M.A., F.C.S., late Rad-
cliffe Fellow in the University of Oxford*.

INVESTIGATION into the constituents of the water of rivers is of considerable importance in chemical geology, as it not only affords some explanation of the action of meteoric water upon the rocks of the country which they drain, but also shows the nature and amount of the substances which they carry to the sea, by which both its composition and that of the sedimentary rocks now forming must be affected. To attain their full value, however, such investigations should be made upon a scale which it seldom falls within the power of an individual to accomplish: it is not sufficient to analyze a river-water at a given spot, even though this be done at different periods of the year; for rivers of considerable length must vary both in constituents and in their degree of dilution as their successive tributaries join them, since it is most unlikely that the rocks of the entire district which they traverse should present uniformity of composition. It is indeed an easy task to ascertain the substances which the water of a river pours into the sea, by analyzing a specimen taken at such a distance from the river's mouth as to ensure its freedom from admixture with sea-water; but to obtain anything approaching to a complete knowledge of the action which meteoric water exerts upon the rocks which form the river's watershed, water taken from various well-selected points ought to be

* Communicated by the Author.

Phil. Mag. S. 4. Vol. 34. No. 230. Oct. 1867.

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examined, notably before and after the accession of important tributaries, and before and after changes of stratum in the river's bed. From this point of view the series of analyses which I proceed to give is but a contribution to a knowledge of the constituents of the river Severn, for the specimens examined were taken almost exclusively at about a mile above the city of Worcester. I have, however, been enabled to make one analysis which bears upon the question of the influence of tributaries; and among the rest will be found one made in time of extreme drought and another immediately after a time of flood, which, from their thus presenting nearly the limits of the river's variation, have a certain value.

The channel of the Severn, in its course of 210 miles from the side of Plynllymmon to the sea, experiences many changes. Starting from the Silurian rocks of Montgomeryshire and cutting a patch of trap rock near the border, it enters the New Red Sandstone in Shropshire; at Shrewsbury it passes for a few miles through the Lower Red Sandstone of the Permian group and the neighbouring coal-field; it then passes again consecutively through the New Red Sandstone and Silurian rocks, and traverses in turn the great Shropshire coal-field and the overlying Permian Sandstone, until at about Stourport it passes from the coal-measures to the New Red Sandstone, through which and the Red Marl of the same system it flows to Worcester. At some distance below Worcester it cuts alternately the Lias and the Red Marl before reaching the Bristol Channel.

But although it thus passes through a considerable variety of rocks, yet I believe at no part of its course does it receive affluents which are likely to bring to it any constituent which it would not already have acquired as the Severn proper, for its principal tributaries drain the same formations as those through which the main stream runs. The original Hafren or Severn receives at Llanidloes the Clywedog, and before it leaves Montgomeryshire the Vyrnwy, both of which run exclusively through Silurian rocks; while its other principal tributaries, the Tern which falls into it below Shrewsbury, the Stour which joins it at Stourport, and the Salwarp which, under the name of Hawford Brook, flows into it about three miles above Worcester, begin and end their course in the New Red Sandstone. The composition of the river is probably more affected below Worcester by the junction of the Teme, which flows for many miles of its course through Devonian strata, and doubtless even more by the influx below Tewkesbury of the Avon, which passes chiefly through Lias; but these may be passed over as being without influence on the present inquiry.

The chief features, therefore, of the river's bed above Worcester

are the great preponderance of sandy and clayey rocks of various degrees of induration, and of the saliferous marl of the New Red Sandstone. Hence the river before it leaves Montgomeryshire, where its channels have been confined to rocks of a more insoluble and harder character, and where its sources have been recruited from a more abundant rainfall, is of necessity purer than at any other part of its course. The difference between its purity in this county and at Worcester is well shown by contrasting the results obtained by the late Dr. R. D. Thomson* with my own observations. Dr. Thomson examined the Vyrnwy and its most important tributary the Banw at points not far from their origin, the Clywedog and the Hafren or Severn at Llanidloes, and the Severn lower down at Caer Sws, soon after the accession of streams which must evidently have brought to it a purer water. His results are as follows :—

	Grains in the gallon.	
	Total impurity.	Organic impurity.
River Vyrnwy at Pont Hagel	2·60	·72
„ Banw at one mile below Can Office	2·84	1·16
„ Clywedog at Llanidloes Bridge	6·80	1·40
„ Severn and Afon Dulas at Llanidloes Bridge	9·92	1·96
„ Severn at Caer Sws	7·24	1·44

With these I will now compare the maximum and minimum amounts of dissolved matter which I have found in the Severn at Worcester :—

	Grains in the gallon.	
	Total impurity.	Loss by ignition.
River Severn at about a mile above Worcester after drought, July 1866	24·23	1·74
Ditto after flood, February 1867	11·65	·88

The circumstances, however, in which the water at Worcester contains the above minimum of dissolved matter are quite exceptional, and in all ordinary conditions of the river I have found it to contain from 21 to 22 grains in the gallon. From the want of complete analyses of the river at higher points, it is impossible to trace the entrance of the constituents which cause this great increase of impurity; yet when we consider that for almost its entire course through Shropshire and Worcestershire the Severn forms the principal drain of the New Red Sandstone and of the

* Quoted by Mr. Bateman in his pamphlet on the supply of water to London from the sources of the Severn, November 1865.

salt-deposits which that system contains, it becomes evident that the constituent added in largest quantity must be chloride of sodium; and from the analyses which follow it will be seen that at almost all ordinary times this salt constitutes from three- to four-tenths of the entire solid residue. That no portion of this salt can be derived from admixture with sea-water is obvious; for in addition to the existence of numerous locks upon the river, the length of the Severn below Worcester is 68 miles, if King Road be taken as the point at which it falls into the Bristol Channel.

The Severn for some distance both above and below Worcester has cut for itself a deep channel in the Red Marl through which it flows; and from the softness of its high banks, a slight rise in the level of the river is sufficient to render the water turbid. Dr. Nash, in his 'History of Worcestershire,' written in 1781, deduces the river's name from this circumstance. "The Severn," he says, "in Latin Sabrina, is so called from Sabr *sand*, Sabrin *sandy**, because this river is often muddy, especially when hasty rains come from the Welsh mountains." Since the time at which this was written the increased navigation and the employment of small steamboats upon the river must have augmented this turbidity; and from the well-known persistence with which clayey matters remain suspended in liquids, it can occasion no surprise that the water of the Severn at this part never clears beyond opalescence. Of this suspended matter I have examined two portions, collected just below the surface in midstream:—one from water taken on the 12th of April, 1866, from the immediate neighbourhood of the waterworks, when after some days of comparative clearness the river became suddenly turbid; the other from water taken from a different and higher part of the river, nearly four miles above Worcester; this was collected on the 14th of February, 1867, after the subsidence of the flood which followed the melting of last winter's snow, and when the river had just withdrawn within its banks. These are therefore specimens of detritus formed under extreme conditions; and the similarity of their composition is very striking, no greater difference occurring than I have found to exist between the deposits formed in two bottles which were filled consecutively from the same part of the river. In analyzing these sediments, after drying at 140° C., they were digested with warm and very dilute chlorhydric acid for about an hour: the amount of its solvent action will be seen below, and may be taken as in some sort representative of the effect which a body of water aided by carbonic acid would exercise upon the suspended matter. It is worthy of remark that the whole of the organic matter of the sediment ap-

* Quoting Bullet, *Dictionnaire Celtique*.

peared to pass into the chlorhydric solution; for the latter blackened considerably by evaporation to dryness, and the undissolved sediment, when dried at 140° C., presented the same weight as it did after subsequent ignition. The separation of the iron from the mixed ferric and aluminic oxides was effected by the action of ammoniac sulphhydrate after addition to the chlorhydric solution of much citric acid and excess of ammonia. The other determinations were made by the methods in ordinary use. The sediment of April contained a trace of manganese, and that of February small quantities of potassium, sodium, and carbonates. 1000 cub. centims. of the water of April contained .0798 grm., and of the water of February .0511 grm. of suspended matter, of which the following Table shows the percentage composition:—

Constituents of the Sediment from the River Severn above Worcester, calculated in 100 parts of the dry substance.

	April 12, 1866.				February 14, 1867.		
	I.	II.	III.	Mean.	I.	II.	Total.
The portion soluble in dilute hydrochloric acid contains of							
Silica	1·886	1·956	1·921	2·040
Alumina	2·358	2·391	2·374	3·061
Ferric oxide.....	4·717	4·782	4·749	4·285
Lime	1·584	2·191	1·887	1·371
Magnesia.....	·679	·627	·653	·881
Organic matter				11·584			11·638
[loss on ignition]	6·878	6·878	4·838	4·838
The portion insoluble contains of	81·602	81·300	81·451	82·730	82·730
Silica		49·043	55·918
Alumina		16·848	14·898
Ferric oxide.....		11·717	8·775
Lime		1·230	·913
Magnesia.....		2·635	·881
Potash and soda.....		traces
				99·913			99·206

The suspended matter of the Severn bears a certain resemblance in composition to that of the Rhine at Bonn, as analyzed by Bischof on the 24th of March, 1851, when the river was swollen and turbid; and it does not differ greatly from that of the mud of the Nile, near Cairo, which was analyzed by Johnson. Bischof has remarked that these deposits have a general resemblance to the constitution of clay slates, which, although of various

ages, present a tolerable uniformity of composition; and he conceives that the detritus which these rivers are carrying to the sea will pass, in lapse of time, into rocks of like character when the necessary indurating and cementing processes shall have been passed through*. Such, therefore, may be the formation now in progress at the mouth of the river Severn.

My attention was first drawn to the water of the Severn in the year 1857, about which time new waterworks for the supply of Worcester from this source came into operation; my intention then was to analyze it at intervals of three months during the year 1857-58; but circumstances obliged me to content myself with making incomplete analyses. The results then obtained are, however, of interest, as they afford various points of comparison with the water of the past year, which I have had more favourable opportunities of examining.

On the 12th of April, 1866, I collected a specimen from the middle of the river just above the place at which the waterworks draw their supply; this point is about a mile above Worcester. After a period of dry weather, rain had fallen on the preceding day, and a rise of 4 inches in the level of the river showed that rain had also fallen up the country. The river was muddy, and the portion collected deposited the sediment of which the composition has been given; the water remained opalescent, in which state it was analyzed, for ordinary filtration or rest for a moderate length of time had scarcely any effect in clearing it. When seen through a depth of 2 feet in a vessel of 3 inches diameter standing upon a white surface, it was of a slight yellowish-brown colour; and it was feebly alkaline to test-paper.

On the 23rd of July, 1866, a specimen was sent me from the same part of the river. There had been continued dry weather almost throughout the course of the Severn for more than a fortnight, and at no time during the previous month had the river risen 2 inches. After rest, the water deposited a very trifling sediment, and remained slightly opalescent; when seen through a depth of 2 feet, it was of a very decided yellowish-brown colour; it was alkaline to test-paper.

On the 30th of October, 1866, a specimen was collected for me from the middle of the river at Bailey's Ferry, which is about 500 yards higher up the river than the waterworks above mentioned. Early in the month of October the river had been exceedingly turbid, and had at one time risen 7 feet above its average height; but it had fallen and cleared gradually up to the time at which the specimen was taken. The water deposited a sediment in quantity sufficient for determination, and remained

* Bischof, Chem. and Phys. Geology (Cav. Soc.), vol. i. pp. 122-124 & 133; and vol. iii. p. 67.

opalescent, and of a distinctly yellowish-brown colour when seen through a depth of 2 feet. It also was alkaline.

At this time I became aware that Hawford Brook or the river Salwarp, which falls into the Severn nearly 2 miles above the points at which the previous specimens had been taken, receives the surplus water of the Droitwich Canal, which renders it occasionally so salt as to destroy the freshwater fish which it contains. This little stream being thus evidently an important contributor of the chloride of sodium which the water at Worcester contains in such abundance, I was desirous of comparing the composition of the river at points above and below its influx; and for the opportunity of doing this I am indebted to the kindness of Martin Curtler, Esq., of Worcester. On the 14th of February, 1867, Mr. Curtler filled several bottles for me from midstream at a point rather more than half a mile above the confluence of Hawford Brook with the river, and then driving down the river's course he collected another specimen at Bailey's Ferry. The difference between these may therefore be expected to express the exact influence of Hawford Brook. For about a fortnight previous to this time the whole of the country bordering the Severn about Worcester had been under water, in consequence of the melting of the accumulated snow; but on the 14th the river had just retreated to its channel; it was still very turbid, and both specimens on standing deposited about an equal amount of sediment. An analysis was made of the deposit yielded by the water taken above Hawford Brook, which has been already compared with the sediment of April. Both portions of water remained opalescent, and of about the same tint of yellowish brown as the water of April, and they both had an alkaline reaction.

The analysis of these waters has been conducted according to the usual methods. In the water of April, which I collected myself, I precipitated the carbonic acid by calcic chloride and ammonia; the result was similar to that which I obtained in September 1857, and showed that in the Severn, as in many mineral waters, the total quantity of carbonic acid present falls rather short of the amount required to meet the demands of those bases which must be calculated as acid carbonates. The deficiency of carbonic acid is doubtless in this case supplemented by the considerable amount of silicic acid which is present, of which the greater part is in combination or solution—although a certain portion doubtless exists as an ingredient of the suspended matter, which gives rise to the opalescence of the water. This opalescence wholly disappears after protracted rest; but the great diminution of organic matter which accompanies the clearing leads to the supposition that, probably in the form of an

iron or aluminium compound, the organic matter itself contributes to the opalescence. The October water, after a lapse of six months, lost more than half its organic matter, as determined by loss on ignition, and became clear and bright, although still slightly coloured; but the water of April, after standing for a year, became colourless, as well as perfectly transparent, whilst its organic matter was reduced to one-fourth of its original amount. In this water I determined the silicic acid, and also the ferric and aluminic precipitate which ammonia produces after removal of silica; and the comparison which follows exhibits the diminution of these constituents:—

	Silica.	Alumina.
Water of April 1866 . .	·0102	·0040, means per 1000.
„ „ 1867 . .	·0070	·0020

This, however, is but a single experiment and may err on the side of deficiency; at all events, to show how slowly any such deposition takes place, I may observe that the determinations of silica made in the October water, a month after it was received, amounted to ·0140 and ·0121 in the 1000, whilst in two bottles which had remained unopened for three months ·0130 and ·0150 in the 1000 were the quantities obtained. I may add that the determinations of these substances were always made after the waters had been allowed to stand for at least a month; and the silicic residues were in every case boiled twice with moderately strong chlorhydric acid before weighing.

The presence of so much silicic acid, part of which probably exists free or in unsaturated combination, must have a considerable effect on the determination of the solid residue of the water. I have made many of these determinations both with and without addition of disodic carbonate, and have found that although the former are invariably less than the solid contents of the water as deduced from analysis, yet the latter always present a still greater discrepancy. It is possible that by the influence of disodic carbonate decomposition and loss of the organic constituents of the water may go on more rapidly than by simple evaporation; and indeed the loss on ignition after addition of this salt I found to be rather less than when it had not been added; but I apprehend that the main cause of the difference between these determinations and the analytical results consists in the formation of silicates during the evaporation at the expense of previously existing carbonates.

The attempts to determine the amount of “organic matter” present in the water were necessarily confined to ascertaining the loss on ignition; for circumstances prevented my working on the spot, or receiving the waters sufficiently soon after collec-

tion to render the control by potassic permanganate of any value. The colour of the residues on evaporation was a yellowish brown, varying in strict accordance with the tint of the water from which they were obtained; they were dried at 140° C., cooled over oil of vitriol, and weighed. Upon gentle ignition they blackened, and after about two minutes burnt nearly white; they were then treated either with a saturated solution of carbonic acid for twenty-four hours, redried at 140° C., and weighed, or with a solution of diammonic carbonate for the same time, and very gently ignited before weighing,—either treatment being repeated until a constant weight was obtained. These two methods of restoring the lost carbonic anhydride give closely accordant results; but the former is preferable on account of the great difficulty of avoiding too strong an ignition.

All the waters contained traces of ammonia, and, with the exception of that of February, nitrates and traces of phosphates. The water of April was richest in these constituents, then that of October, whilst after the drought of July they were found in greatly diminished quantity, and after the floods of January and February the water contained only a trace of ammonia. A reference to a Table which follows, showing the rainfall about the sources of the river, may serve to explain this, as the few days' rain which will be seen to have occurred in April and October would doubtless flush all the drains which conduct sewage to the river, whilst in July they would probably have been for some time dry, and in February well cleansed by the flood occasioned by the long-continued thaw. Two of the waters were tested for nitrites, but without success. The ammonia I found to be conveniently obtained by a modification of the distillatory process: a litre of the water was placed in a flask of convenient size, which, after the addition of baric hydrate, was closed by an india-rubber cork pierced by two holes; through one of these a straight tube passed, the lower extremity of which was just closed by the liquid within; and in the other was inserted a Welter's tube with two capacious bulbs, which terminated immediately below the cork. In the latter tube four or five drops of pure chlorhydric acid diluted with an equal quantity of distilled water were placed. The liquid was then raised slowly to the boiling-point, and kept gently boiling for a quarter of an hour. Care must of course be taken to avoid rapid or long-continued ebullition, which might cause ejection of the contents of the bulbs, or render them replete with condensed water. At the close of the operation the bulb-tube may either be removed immediately, or the atmosphere of the flask first drawn through it, with the precaution of adapting a wash-bottle containing chlorhydric acid to the straight safety-tube to arrest any accidental ammonia. In

the analyses in question I simply removed the bulb-tube and evaporated its contents in a platinum dish, which was left over oil of vitriol for the same time, and weighed under precisely the same conditions before and after the evaporation. A litre of April water yielded thus $\cdot 0015$ grm. of ammoniac chloride, and the same quantity of October water gave $\cdot 0010$ grm., corresponding respectively to $\cdot 000476$ and $\cdot 000317$ grm. of ammonia (NH^3); in July the amount obtained was inappreciable, and, whilst the former residues gave good indications both with diplatinic chloride and Nessler's test, Nessler's test alone gave a faint reaction in this instance; and the same may be said also of the water of February. The presence of nitrates was detected in the usual way, by concentrating the water with addition of a little disodic carbonate, and applying the ferrous-sulphate test, the depth of colour which the different specimens yielded being in accordance with the quantitative results which are given below, and which were obtained by means of Pugh's process. The water of February gave no reaction with the ferrous-sulphate test, even when a litre was concentrated to a few cubic centimetres. This water also appeared to contain no trace of phosphates; for neither the concentrated water nor the mixed ferric and aluminic precipitate obtained from it yielded any reaction with the diammonic-molybdate test; whilst, when similarly treated, the ferric and aluminic precipitate from a litre of the waters of July, April, and October gave indications, slight in the case of the first, but quite distinct in those of the two latter waters. The waters of July and October contained traces of manganese, from which the others appeared to be perfectly free. The determinations of the other constituents do not need remark. After separation of silica and the precipitate by ammonia, the calcium was estimated as carbonate by ignition of oxalate, and the magnesium as pyrophosphate; the potassium was separated from the mixed chlorides as chloroplatinate; the chlorine and sulphuric acid were weighed as salts of silver and barium. The atomic weights* employed in obtaining the numbers which follow are those now in common use.

I have placed the analyses made in 1866-67 in two Tables, the first of which gives the constituents of the waters of 1866, whilst the second gives the comparative results obtained from the two specimens of February 1867; of the latter, I have placed the analysis of the water taken higher up the river first, by which the influence of a further course of three miles, and of the addition of Hawford Brook, is easily perceived. Although the time, being one of flood, was perhaps not the best adapted for ascertaining the change which the influx of Hawford Brook

* Odling's 'Practical Chemistry,' Tables, 1865.

would produce, yet its effect was even then sufficiently obvious. It nearly doubled the sodic chloride, and thus showed that although the river obtains much of this salt in the previous part of its course, yet in this respect the little river Salwarp is its most important tributary.

To each analysis I have appended the height of the river at Diglis lock on the days on which the various waters were taken; and for this and for other information I am indebted to the kindness of E. Leader Williams, Esq., C.E. to the Severn Commissioners. Diglis lock is about half a mile below Worcester Bridge; and the daily height of the river is read off upon its upper sill. At a height of 9 feet 5 inches the river is considered to be at low water, in winter or summer; and 12 feet is taken as the maximum winter height, except in times of flood.

Severn Water of 1866.

TABLE of Constituents calculated in 1000 parts of the Water.

	Water from immediately above the waterworks.						Water from Bailey's Ferry.		
	April 12, 1866.			July 23, 1866.			October 30, 1866.		
	I.	II.	Mean	I.	II.	Mean.	I.	II.	Mean
Dissolved matter :—									
Potassa	·0063	·0059	·0061	·0055	·0051	·0053	·0075	·0074	·0074
Soda	·0540	·0575	·0557	·0741	·0760	·0750	·0445	·0444	·0444
Ammonia.....			trace			trace			trace
Lime	·0672	·0660	·0666	·0694	·0679	·0686	·0722	·0722	·0722
Magnesia	·0164	·0169	·0166	·0209	·0207	·0208	·0187	·0184	·0185
Alumina &c.	·0059	·0030	·0040		·0020	·0020	·0025	·0035	·0030
Chlorine	·0607	·0597	·0602	·0842	·0846	·0844	·0420	·0416	·0418
Sulphuric anhydride.	·0353	·0350	·0351	·0399	·0419	·0409	·0415	·0422	·0418
Nitric	·0133	·0137	·0135	·0025	·0029	·0027	·0084	·0081	·0082
Phosphoric			trace			trace			trace
Carbonic	·0508	·0919	·0913			undetermined.		*·0757	·0757
Silicic	·0105	·0100	·0102		·0080	·0080	·0140	·0121	·0130
Loss on ignition	·0368	·0338	·0353	·0231	·0266	·0248	·0270	·0212	·0241
Residue obtained by direct evaporation. }	·3127	·3107	·3117	·3533	·3390	·3461	·3100	·3018	·3059
Suspended matter :—									
Inorganic	·0744		·0744			trace	·0162		·0162
Organic [loss on ignition]	·0054		·0054			trace	·0013		·0013
Spec. grav. of the water	1000·281			1000·366			1000·408		
Height of river-level at Diglis lock ... }	13 feet 6 in.			9 feet 6 in.			10 feet 9 in.		

* This determination was made immediately after the water was received.

Severn Water of 1867.

TABLE of Constituents calculated in 1000 parts of the Water.

	Water from above Hawford Brook.			Water from Bailey's Fer		
	February 14, 1867					
	I.	II.	Mean.	I.	II.	Mean.
Dissolved matter:—						
Potassa	·0058	·0056	·00570	·0068	·0066	·0067
Soda.....	·0109	·0108	·01085	·0200	·0193	·0197
Ammonia.....	trace	trace
Lime	·0380	·0379	·03795	·0393	·0385	·0389
Magnesia.....	·0090	·0088	·00890	·0095	·0095	·0095
Alumina &c.	·0050	·0049	·00495	·0078	·0086	·0082
Chlorine	·0140	·0141	·01405	·0217	·0223	·0220
Sulphuric anhydride	·0180	·0181	·01805	·0172	·0180	·0176
Nitric
Phosphoric
Carbonic	undeter- mined.	undeter- mined.
Silicic	·0075	·0080	·00775	·0079	·0086	·0082
Loss on ignition	·0109	·0112	·01105	·0132	·0119	·0125
Residue obtained by direct evaporation	·1439	·1403	·1421	·1664	·1664
Suspended matter:—						
Inorganic.....	·0487	·0487	·0431	·0431
Organic [loss on ignition],	·0024	·0024	·0029	·0029
Specific gravity of the water.	1000·127			1000·183		
Height of river-level at Diglis lock	17 feet 2 in.			17 feet 2 in.		

I have also determined the amount and composition of the precipitates formed upon concentrating the waters to about one-sixth of their bulk; they consist chiefly of carbonates, usually with a little admixture of silica, probably in the form of silicates. The water was concentrated in a platinum dish, the precipitate collected on a tared filter, the film adherent to the dish weighed, and, after solution in chlorhydric acid, added to the similar solution of the precipitate for analysis.

Precipitates produced by concentration of 1000 cub. centims. of the Waters to about one-sixth.

	April.	July.	October.	February.	
				Above Hawford Brook.	At Bailey's Ferry.
Total precipitate...	grm. ·0980	grm. ·0833	grm. ·0924	grm. ·0479	grm. ·0480
containing—					
Lime	·0515	·0450	·0483	·0245	·0246
Magnesia	·0049	·0050	·0038	·00098	·0020
Alumina &c. .	trace	trace	trace	trace	trace
Silica	·0058	undetermined	·0030

In the Table which follows I have given the results which I have already mentioned as having been obtained in 1857-58; although imperfect, they are of interest as affording points of comparison with the waters of the past year. So far as I can learn, periods of drought preceded the taking of all the waters, with the exception of a little rain which occurred high up the river towards the end of May 1858; and the height of the river, the concentrated condition of the three first waters, and the absence of sediment agree with these observations. These circumstances preclude a strict comparison with the waters of the past year. It may, however, be roughly remarked that the chloride and sulphate appear to have diminished, whilst the silica has increased since 1857; but even these changes may be entirely due to the altered conditions of the rainfall. The estimations of loss on ignition are undoubtedly too high; for the treatment with diammonic carbonate, as then adopted, was of too short duration fully to replace the lost carbonic anhydride.

With regard to the much debated question of the mode of combination in which the saline constituents should be assumed to exist in spring- and river-waters, it appears to be most desirable, in the absence of a definite knowledge of the chemical changes which occur among salts in solution, to endeavour to trace the source whence the different salts have been derived, and to consider that in their intermixture they preserve their original forms of combination. It is frequently taken for granted, for instance, that when solutions of calcic or magnesian salts meet with solutions of alkaline carbonates, a complete interchange of constituents takes place, however dilute the solutions may be; and the calcic or magnesian carbonate assumed to be formed is said to be dissolved either by the water, or by more or less carbonic acid, or by the agency of some solvent salt. Experiment, however, shows that, when carbonic acid and sol-

Severn Water of 1857-58.

TABLE of Constituents calculated in 1000 parts of the Water.

	Water taken in the neighbourhood of Bailey's Ferry.					
	September 1, 1857.			November 30, 1857.		
	I.	II.	Mean.	I.	II.	Mean.
Dissolved matter:—						
Soda				·0696	·0641	·0668
Lime	·0724	·0695	·0709	·0631	·0590	·0640
Magnesia	·0214	·0184	·0199	·0169	·0158	·0163
Chlorine	·0862	·0903	·0882	·0542	·0542	·0542
Sulphuric anhydride ..	·0480	·0502	·0491	·0407	·0386	·0396
Carbonic anhydride ..	·1008	·1015	·1011
Silica	·0021	·0021	·0036	·0042	·0039
Loss on ignition	·0282	·0323	·0302	·0626	·0542	·0584
Residue obtained by } direct evaporation. }	·3434	·3476	·3455	·2902	·2819	·2860
Height of river-level } at Diglis Lock ... }	9 feet 5 inches.			9 feet 11 inches.		
	March 1, 1858.			June 1, 1858.		
Dissolved matter:—						
Soda.....	·0840	·0807	·0823
Lime	·0621	·0595	·0608	·0408	·0408
Magnesia	·0157	·0169	·0163	·0115	·0115
Chlorine	·0738	·0753	·0745	·0345	·0350	·0347
Sulphuric anhydride ..	·0494	·0472	·0483	·0249	·0249
Carbonic anhydride
Silica	·0041	·0041	·0070	·0070
Loss on ignition	·0367	·0375	·0371	·0235	·0235
Residue obtained by } direct evaporation.. }	·3193	·3173	·3183	·1857	·1857
Height of river-level } at Diglis Lock ... }	9 feet 10 in.			10 feet.		

vent salts are excluded, 1000 cub. centims. of water may yet contain at least ·136 grm. of calcic sulphate unprecipitated for apparently any length of time in the presence of an atomic proportional, or ·106 grm. of disodic carbonate; and with calcic chloride the results are closely similar. Now, assuming complete interchange of constituents to take place, ·136 grm. of calcic sulphate will give rise to ·100 grm. of calcic carbonate; and as 1000 cub. centims. of pure water dissolve only ·035 grm. of the latter salt*, in the case in question a triple amount is

* Weltzien in *Bulletin*, 1866, 1^{re} semestre, p. 353, confirms this result obtained by Hofmann.

retained in solution by the sole additional agency of $\cdot 142$ grm. of disodic sulphate, which, regarded as a solvent, appears wholly inadequate. Parallel experiments made with magnesian sulphate and chloride exhibit more striking results; 1000 cub. centims. of water may contain $1\cdot 3$ grm. of magnesian sulphate, or $1\cdot 05$ grm. of magnesian chloride, perfectly unaffected, so far as precipitation is any evidence of chemical change, by the presence of the atomic proportional, or $1\cdot 167$ grm. of disodic carbonate. The peculiar proneness of magnesium to form double salts doubtless conduces to this result, which is probably arrived at by partial decomposition.

In the presence of these facts it appears unreasonable to assume that, in rivers which traverse gypseous deposits, decomposition of the calcic sulphate which they dissolve must immediately take place, if from another source they receive alkaline carbonates. Such a decomposition would undoubtedly occur, unless solvent agents intervened, whenever alkaline carbonates passed into a river which contained more calcic sulphate than $\cdot 136$ in the 1000 parts; but such rivers are of rare occurrence; a few small streams in the neighbourhood of Paris* contain more than this quantity, but they are, I believe, exceptional. The Thames, containing from $\cdot 04$ to $\cdot 08$ in the 1000, stands high in this respect among the great European rivers, and $\cdot 07$ is the largest amount I have found in the Severn; these quantities, however, fall far short of what might coexist in solution with an atomic proportional of disodic carbonate. If calcic sulphate decomposed after its entrance into a water, it would rather be a question whether it would not produce magnesian in preference to any other sulphate: it is known that by digestion† of magnesian carbonate with solution of calcic sulphate, double decomposition occurs; and it seems by no means improbable that the same change might take place in river-water. It is worthy of note that, in all the specimens of Severn water of 1866–67, the sulphuric anhydride is to the magnesia in as nearly as possible the atomic proportion; and when precipitates form in the greatly concentrated waters, they contain but little magnesia, the mass of which remains in solution together with the whole of the sulphuric anhydride.

But to assume changes of which no proof exists would be to depart from the principle in accordance with which I have arranged the results of the present analyses. This is to assign to the saline constituents those forms of combination in which they may be considered to have entered the water. The saliferous and gypseous marls of the New Red Sandstone have yielded the sodic chloride and calcic sulphate; and therefore I have expressed in these forms of combination all the sulphuric anhydride and,

* Bischof (Cav. Soc.) vol. i. p. 77.

† Ibid. p. 430.

when possible, all the chlorine; but in the water taken above Hawford Brook the sodium is insufficient to combine with the whole of the chlorine, and the small excess of the latter which remains I have combined with magnesium. In the other waters the chlorine is insufficient, and a little sodium remains; and this, together with the potassium, I have considered to exist as carbonate, derived from complex silicates by the action of carbonic acid. Perhaps these might more probably be regarded as silicates dissolved in carbonic acid; and it is possible*, as such solutions have the power of dissolving aluminic silicates, that the alumina present in the waters may have entered in this way. Silurian limestones may have furnished the calcic and magnesian carbonates with traces of ferrous and manganous carbonates, and perhaps the trace of phosphate which usually occurs in the waters, as this, from Forbes's analyses†, is a constant element in these rocks. Since, however, the phosphate does not appear in the water of February, which is also free from nitrates, this constituent may have a more direct origin in sewage. I have apportioned the nitric anhydride to the ammonia and to the lime, as forming the most probable combinations. The results thus arranged give the following composition for the solid residue of the different specimens of the water of the Severn:—

Severn Water of 1866-67.

TABLE of assumed Salts calculated in 1000 parts of the Water.

	Water from immediately above the waterworks,		Water from Bailey's Ferry.	Water from above Hawford Brook.	Water from Bailey's Ferry.
	April 12, 1866.	July 23, 1866.	Oct. 30, 1866.	February 14, 1867.	
Chloride of sodium ...	·0992	·1390	·0688	·0204	·0362
Chloride of magnesium	·0021
Sulphate of lime	·0596	·0695	·0710	·0306	·0299
Nitrate of ammonia ...	trace	trace	trace
Nitrate of lime	·0205	·0041	·0124
Phosphate of lime	trace	trace	trace
Carbonate of potassa...	·0089	·0077	·0108	·0083	·0098
Carbonate of soda	·0054	·0024	·0136	·0010
Carbonate of lime	·0627	·0689	·0692	·0452	·0475
Carbonate of magnesia.	·0348	·0436	·0388	·0168	·0199
Alumina &c.	·0040	·0020	·0030	·0049	·0082
Silica	·0102	·0080	·0130	·0077	·0082
Loss on ignition.....	·0353	·0248	·0241	·0110	·0125
	·3406	·3700	·3247	·1470	·1732
Residue obtained by } direct evaporation. }	·3117	·3461	·3059	·1421	·1664

* Bischof (Cav. Soc.), vol. ii. p. 65; and vol. iii. p. 89.

† Phil. Mag. S. 4. vol. xiii. p. 365.

I now give a Table which expresses in grains the amount of salts contained in the imperial gallon, or 70,000 grains, of the various waters :—

Severn Water of 1866–67.

TABLE of assumed Salts calculated as grains in the imperial gallon.

	Water from immediately above the waterworks.		Water from Bailey's Ferry.	Water from above Hawford Brook.	Water from Bailey's Ferry.
	April 12, 1866.	July 23, 1866.	Oct. 30, 1866.	February 14, 1867.	
Chloride of sodium ...	6·944	9·730	4·816	1·428	2·534
Chloride of magnesium.	·147
Sulphate of lime	4·172	4·865	4·970	2·142	2·093
Nitrate of ammonia ...	traces	traces	traces
Nitrate of lime	1·435	·287	·868
Phosphate of lime	traces	traces	traces
Carbonate of potassa...	·623	·539	·756	·581	·686
Carbonate of soda	·378	·168	·952	·070
Carbonate of lime	4·389	4·823	4·844	3·164	3·325
Carbonate of magnesia.	2·436	3·052	2·716	1·176	1·393
Alumina &c.	·280	·140	·210	·343	·574
Silica	·714	·560	·910	·539	·574
Loss on ignition.....	2·471	1·736	1·687	·770	·875
	23·842	25·900	22·729	10·290	12·124

I have, moreover, calculated the composition of 100 parts of the dry mineral residue of each of these waters (Table, p. 266). In so doing I have excluded the organic matter, and ought perhaps to have excluded the nitric anhydride as being also, as it were, an accidental constituent. The chlorine I have represented in combination; but the other constituents I have left uncombined. The carbonic anhydride has been introduced in quantity sufficient to combine with the free bases, and the silica is therefore considered to be free.

I have alluded to the rainfall along the course of the river as a point likely be of considerable interest in connexion with the constituents of the water. In my endeavour to obtain information on this head, I have been guided by Mr. Symons's annual rainfall report to a number of observers who have most courteously given me the information which I sought. It is difficult to ascertain what length of time the water from the different sources would take to reach Worcester; but as an average I am told that three days would suffice for the Welsh water, and about two or less for the water from the nearer sources. My infor-

Severn Water of 1866-67.

TABLE showing the Composition of the dry Mineral Residues of the Waters, calculated in 100 parts.

	April 12, 1866.	July 23, 1866.	Oct. 30, 1866.	Above Haw- ford Brook.	Bailey's Ferry.
				February 14, 1867.	
Chloride of sodium ...	32·49	40·27	22·89	15·00	22·53
Chloride of magnesium.	1·54
Soda	1·05	·40	2·66	·37
Potassa	2·00	1·53	2·46	4·20	4·17
Lime	21·81	19·87	24·02	27·87	24·21
Magnesia	5·44	6·02	6·16	5·89	5·91
Alumina &c.	1·31	·58	1·00	3·60	5·10
Sulphuric anhydride ...	11·50	11·84	13·91	13·23	10·96
Nitric anhydride	4·42	·78	2·72
Carbonic anhydride ...	16·64	16·37	19·86	23·01	21·65
Silicic anhydride	3·34	2·34	4·32	5·66	5·10
	100·00	100·00	100·00	100·00	100·00

mants, however, have kindly given me their observations for several days preceding those on which my specimens were collected; and these will give some general idea of the state of the river, so far as such observations, at present too widely scattered, can lead to any conclusion. I particularly regret not having been able to obtain more information with regard to the Welsh rainfall, as it is obvious that the single set of observations at Llanidloes can afford but a most imperfect indication of the influence of the Welsh water upon the river. The rainfall at Llanidloes may give the state of the Hafren; but it can afford no clue to the condition of the Welsh tributaries, and especially of the chief, the Banw and Vyrnwy, the sources of which are from 20 to 30 miles to the north, and which join the Severn about 55 miles below Llanidloes. Of the Shropshire rainfall I have a more complete account: that of Oswestry swells the little river Perry which falls into the Severn above Shrewsbury; and besides that of Shrewsbury itself, I have obtained the rainfall of Shiffnal, which supplies the Worfe, a little river which also joins the Severn; but of the rainfall which is likely to influence the Tern, its largest tributary in Shropshire, I have not been able to obtain any observations. The rainfall of Wolverhampton I have considered to be indicative of the probable condition of the Stour, and that of Bromsgrove of the Salwarp. In giving the following Table, I would express my thanks to T. F. Roberts, Esq., Rev. A. R. Lloyd, T. Howells, Esq., Rev. J. Brooke, H. Ward, Esq., and G. Dipple, Esq., for their respective observations:—

Rainfall about some of the sources of the Severn and of its tributaries.

Place	Llanidloes.	Oswestry.	Shrewsbury.	Shiffnal.	Wolverhampton.	Bromsgrove.
Above sea-level	750 feet.	270 feet.	200 feet.	450 feet.	520 feet.	273 feet.
1866.						
April 7.	·50	·30	...	·010
8.	·005
9.	·03	·20	...	·065
10.	·40	·55	...	·50	·07	·060
11.	·20	·02	·070
July						
18.
19.
20.
21.
22.
October						
25.	...	·64	·25	·30	1·08	·125
26.	·70	·40	...	·040
27.	·03	·17	·02	·080
28.	·10	·30	·15
29.	·30	...	·150
1867.						
February 9.	·01	·17	...	·080
10.	·27	·14	·29	·260
11.	·14
12.
13.	...	·85

I have found that the observations of rainfall for many of the places mentioned above do not go back so far as 1857-58, during which year the first analyses were made; but the observers at Oswestry, Shiffnal, and Bromsgrove have kindly furnished me with sufficient facts to show that at these places no rain fell for several days before the waters were taken in September, November, and March, and in May the only rainfall was at Oswestry.

A river must derive its water from two sources, one immediately, the other intermediately dependent upon rainfall. To the latter belong all the phenomena of springs, which frequently afford a very equable supply of water, and augment the bulk of a river in no inconsiderable degree; they are, however, connected with a rainfall of too uncertain remoteness in time, and perhaps in place, to admit of comparison with any given condition of the river. The immediate rainfall, on the other hand, produces not only a marked effect upon the height of the river at a given place, but also on the nature and amount of its constituents, dependent upon the geological character of the country in which the rain occurs. The analyses of 1857-58 exhibit this most di-

stinctly. It appears to have been a dry year; and on the four days on which the water was collected, the river was either quite or almost at low water. In September, November, and March its concentration corresponded with this state of things; but on the 1st of June, with a height of 10 feet, its dissolved matter had fallen to nearly the amount which it contained after the flood of February 1867, when the river had reached a height of more than seventeen feet. The explanation of this appears to be that, as from the observations at Shiffnal and Bromsgrove drought prevailed in those places, we may assume that over that district in general the same condition existed; whilst, on the other hand, from rain occurring towards the end of May at Oswestry, we may suppose that a purer and perhaps a Welsh water suddenly filled the river, and reached Worcester comparatively uncontaminated by the soluble or insoluble constituents of the New Red Sandstone. The conditions of the rainfall of 1866-67 were very different, and by no means so well calculated to place its influence in a striking light; still the details are explanatory of the state in which the river was on the days on which the specimens were taken. After a period of comparatively dry weather, rain probably fell, during the few days which preceded the 12th of April, over nearly the whole course of the Severn and its tributaries. This corresponds with the considerable height of 13 feet 6 inches which the river attained at Worcester, and it also explains how the river brought down at this time the largest amount of sediment which I have found it to deposit; for it is a matter of observation that the greatest quantity of suspended matter* is carried into a river during the first days of rain. The wide distribution of the rainfall, moreover, accounts for the considerable quantity of dissolved matter which this water contained; in this respect it affords a very useful contrast to the water of June 1858, and shows the great difference of result between a rainfall high up the river, and one which also extends over the district of the New Red Sandstone. The water of July had evidently been concentrated by evaporation during the prevailing drought, and it needs no further remark; but with regard to the water of October some few observations are required. It must be remembered that the river was very high at Worcester during the early part of the month, and had fallen gradually up to the time at which the water was collected: the reports of most of the observers along its course accord with this, and state that more rain had fallen earlier in October than during the day or two which could influence the water of the 30th. This, therefore, clearly accounts for the small quantity of sediment which this water yielded, for the first rain

* Bischof (Cav. Soc.), vol. i. p. 120.

had washed down all the looser materials of the river's banks ; and the same circumstance may perhaps explain why the more soluble salts, the chloride and nitrate, existed in less amount than in the water of April collected after more sudden rain, whilst the salts which would require a longer-continued action of meteoric water for their solution or formation, the calcic sulphate, the calcic, magnesian, and alkaline carbonates, and the silicates, were found in greater proportion. The conditions under which the water of February was collected have been sufficiently described. Rainfall could have had but little influence upon it, for the origin of the flood was the thawing of the snow ; it is, however, interesting to observe that the suspended matter then brought down was little more than half that produced by the sudden rain of April, and to see that the long-continued action of water acted upon the strata of the district in a more marked degree, but in the same direction, as was observed in October. For a comparison of these actions I would refer to the Table in which the percentage composition of the mineral residues of the waters is given, as that exhibits the mutual relations of the dissolved constituents more distinctly.

The few facts thus given show how greatly the character of a river-water depends on the amount, and even more on the locality of the rainfall. In the eight examinations of this water which I have made, the most unexpected variation which I have been able to trace is that of June 1858 ; but it is probable that such changes are of very frequent occurrence. This must be especially the case with such a river as the Severn, the tributaries of which, from the varying nature of their watershed and channels, necessarily come to it charged with very different kinds and quantities of dissolved matter. More might therefore be done in this instance to provide a town with a purer water than could be effected where the supply is drawn from a river which has its sources and its course in rocks of more equal solubility, and which is liable to no sudden accession of rainfall from a mountainous region. It would be necessary to ascertain the influence of all the principal tributaries on the constituents of the river ; and then, with a knowledge of the rainfall at the various points up the river by which these tributaries are supplied, the reservoirs might be filled at such times as would ensure the collection of a purer water. The requisite knowledge of rainfall might perhaps be in time obtained by an extended telegraphic system supplying a daily record which might be thus acted upon.

The Severn differs from most rivers in the larger quantity of chloride of sodium which it contains. Rivers in general are poor in this constituent, as the continued percolation of meteoric water has long since dissolved it from the majority of rocks and carried

it to the sea. It is probably being fast removed also from the New Red Sandstone; for although the analyses of February show that the Severn does dissolve this substance from the New Red Sandstone itself, yet they prove that it is chiefly from the Salwarp, which directly drains the Red Marl (in which the great deposits of rock-salt lie), that it acquires the large quantity of chloride of sodium which it contains at Worcester. With regard to its earthy carbonates it also presents some peculiarities. As contrasted with the Thames, it contains only about half the quantity of carbonate of lime, and about double the amount of carbonate of magnesia: the want of the former constituent is doubtless due to the comparative absence of limestone rocks from the upper channels of the Severn, while the magnesian salt probably arises from the decomposition of silicates, which may also be the source of the potassium which the water contains in rather large quantity.

XXXIV. *On a Resistance-measurer.*

By C. W. SIEMENS, F.R.S.*

FOR the measurement of small resistances the method formerly employed was that of the tangent galvanometer, which method is still valuable in the determination of resistances which are inseparable from a difference of electric potential, such, for instance, as a galvanic element.

In measuring wire-resistance, more accurate and convenient methods have been devised, amongst which that of the common differential galvanometer and that known as Wheatstone's balance hold the most prominent places.

But both these systems have disadvantages which render them insufficient in a great many cases. For instance, in the first method a well adjusted variable-resistance-coil is necessary, which, if the method is intended to be applicable between wide limits, will have impracticably large dimensions. The bridge method, though very beautiful, requires three adjusted coils, and frequently gives rise to calculation, which renders it unavailable for unskilled operators. The sine method, which is the most suitable for measuring great resistances, requires even a superior amount of skill and mathematical knowledge on the part of the operator. Many years' experience of these methods made me feel the want of an instrument which would, by its simplicity of construction and ease of manipulation, be capable of employment by an unskilled operator with a degree of correctness equal to that of the bridge method.

* Communicated through the Electric-Standard Committee.

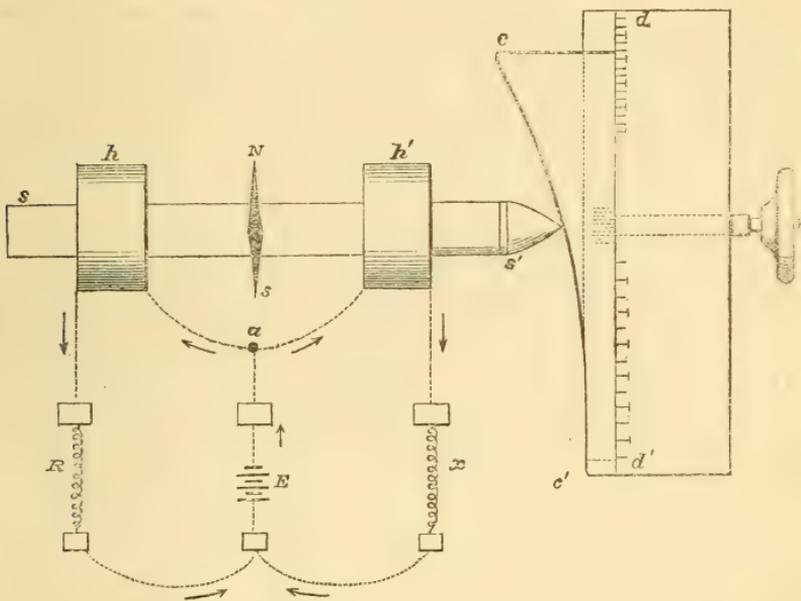
The conditions upon which such an instrument could be successful appeared to be the following:—

1. The employment of a zero method, by which the galvanometer-needle would always be brought to the direction of the magnetic meridian or the same given point upon the scale and, therefore, be independent of the unknown function of the angle of deflection.

2. The readings to be made upon a simple lineal measure divided into equal parts signifying equal units of resistance.

3. The employment of a single and unalterable comparison-resistance.

The apparatus constructed to fulfil these conditions is represented by the following diagram.



Two equal and parallel helices, h and h' , are fixed upon the common slide ss' , which moves in the direction of its length between guide rollers. This motion is effected by the end s' armed by a facing of agate, which presses against the face of the metal curve cc' . The latter is fixed upon a slide moving in a groove in the rule dd' , at right angles to the direction of ss' . The curve is moved in the direction dd' , by means of a milled head i , on the axis of which is a pinion gearing into a rack underneath the straight edge of the curve cc' . The rule dd' is graduated into equal parts; and opposite to the divisions is a nonius up the straight edge and the curve, to divide each degree into ten parts. Whenever the milled head i , therefore, is turned, the position of the curve is altered; and as the point s' of the bob-

bin-slide is pressed against it by means of a spring, the bobbin follows it in all its movements.

The wires of the two bobbins are connected together, in the common point a , with the pole of a galvanic battery E , the other pole being connected with two resistances R and x , and through these with the other ends of the galvanometer-helices. The resistance R is made constant, and adjusted so that when $x=0$ the index of the curve stands exactly opposite the zero of the graduated scale $d d'$, the unknown resistance being represented by x .

It is evident that, the resistance in the bobbins being equal, as also their dimensions and initial magnetic effects upon the needle suspended between them, if we make the resistance x equal to R , the currents in the two branches will be equal, and the magnet-needle therefore balanced between them only when the helices are equally distant from it. Should, however, either of these resistances preponderate, the strength of current in that branch will be lessened; and in order to reestablish the balance it will be necessary to shift the bobbins, approaching the one in which the weaker current is circulating towards the suspended magnet.

The instrument is erected upon a horizontal metal table standing upon three levelling-screws. The bobbins, with the suspended magnet, and dial-plate for observing the deflection and zero of the pointers, are contained in a glass case with glass cover, supported by four brass pillars. The instrument is supplied with terminals for the battery-connexions, and a current-breaker for interrupting the battery-circuit. Opposite to these are four terminal screws for receiving the ends of the resistances R and x , with contact-plugs between them in order to quickly establish a short circuit in case the operator should be in doubt towards which side he has to move the adjusting-curve. Two constant resistances accompany the apparatus,— R that which is used during the measurement, and a , a resistance of known value, which is introduced between the terminals x in order to enable the operator for his own security to make a control measurement by which he may convince himself of the adjustment of the instrument at any time. Another purpose of this resistance is to facilitate the readjustment of the zero-point, in case the galvanometer should at any time be cleaned or a new silk fibre put in.

In constructing the sliding curve of this instrument, it might be determined by calculation from the formula given by Weber for the deflection effect of a circular current of known dimensions upon a magnetic point, and from the given distance of the coils from each other. I prefer, however, in practice to determine the curve of each separate apparatus empirically, because it is not possible to coil a helix mathematically true, or to set it, when coiled, absolutely at right angles to the plane of its horizontal motion.

In the determination of each curve I use a delicately adjusted rheostat or scale of resistances in the circuit of x , giving it varying values corresponding to the equal divisions of the engraved scale, and constructing the curve according to the position which it is found necessary to give to the point s' in order to arrive at the magnetic balance. With each instrument it would be possible to have two values of R —one expressed in mercury and the other in B.A. units; and in order to measure at pleasure in either of these units, it would only be necessary to insert the one or other between the terminal screws for R .

The instrument has been found to be very convenient for the measurement of the wire resistances of overland lines, or for the reading of resistance thermometers; it reduces the operation to the observation of the zero position of a needle, and the reading upon a graduated scale, which can be performed by a person of ordinary intelligence without experience in electrical measurement. In accuracy and range it fully equals the bridge method, while as regards portability and cheapness of apparatus the advantages are decidedly in its favour.

XXXV. *On the Source of Muscular Force.* By JOHN DOUGLAS,
East-India Government Telegraph Department.*

THE attempts hitherto made to settle the question of the source or sources of muscular energy have consisted in calculating the quantity of muscle, the component nitrogen of which would equal either the nitrogen ingested or excreted during the performance of certain measured external work.

The consideration of the ingesta appears to promise the most reliable results if the nitrogen be reduced in quantity until just sufficient to support the body in health. In considering ingesta, it may be known for certain that all the effective nitrogen is included in the calculation; in considering excreta there is no such certainty. The data furnished by ingesta may be collected over a longer time than those furnished by excreta, the latter being obtained only by continued expenditure of time and labour on the part of the observer and subject or subjects under observation; and that generality so desirable in physiological inquiries is almost, if not quite, unattainable in the consideration of excreta, whereas, as shown below, it is easily attainable in considering ingesta.

The only source of the nitrogen of the muscles is the food, it being generally admitted that none is absorbed under ordinary circumstances by the lungs and skin. Observers agree on this point, although difference of opinion exists as to its evolution by those organs.

* Communicated by the Author.

It appeared to me possible that the natives of India might furnish a class of persons who habitually ingest the required minimum of nitrogenous food, and that the quantity of muscle formed under the most favourable suppositions from this might be insufficient to supply by its disintegration the internal work alone; the difficulties of time, generality, absolute determination of excreta, and determination of external work might in such a case be eliminated. After some inquiry amongst the native population, I thought it possible that the jail dietary-scales might furnish the data required, and with many advantages which could not be hoped for from inquiry amongst the free population. Through the kindness of Dr. Wilson, in medical charge of the Madras Penitentiary, I was supplied with the dietary-scales in use in that prison, and from this paper I shall select only two instances.

The items composing these diet-scales may be divided into two classes—(1) articles the same quantities of which are served out daily, and (2) articles which are varied in quantity on different days in the week. In the Table given below, of articles belonging to the first class the actual daily quantities, of those belonging to the second the average daily quantities calculated for one week are given:—

TABLE of Nitrogenous Food served out to classes of Prisoners in the Madras Penitentiary.

Articles.	Proportion of proteine compounds.	Brahmins.		Malays and Chinese.	
		Daily quantities of food.	Quantities of proteine compounds.	Daily quantities of food.	Quantities of proteine compounds.
		OZS.	OZS.	OZS.	OZS.
Rice	·08	25·000	2·000	26·000	2·080
Dholl	·24	2·857	0·686	0·857	0·206
Mutton	·24	1·889	0·453
Fish	·20	1·250	0·250
Tamarind	·04	·500	0·020	0·500	0·020
Curry powder	·04	·500	0·020	0·500	0·020
Vegetables	·03	2·571	0·077	2·571	0·077
Onions	·03	·500	0·015	0·500	0·015
Garlic	·03	·029	·001	0·029	0·001
Butter-milk or tyre } (curdled milk) ... }	·07	10·000	0·700	4·286	0·300
Total proteine compounds } }	3·519	3·422
Simple imprisonment prisoners, and prisoners for trial receive one-fifth less.....			·704	·684
		A	2·815	B	2·738

Assuming all the proteine compounds ingested in these cases to be assimilated, and transformed without waste into an equal weight of muscle, 1 grm. of muscle, giving by its disintegration 1848 metrekilogs. of force (Frankland, Phil. Mag. vol. xxxii. p. 188), the muscle so formed would in each case give—

A.	B.
Metrekilogs.	Metrekilogs.
146,842	143,335

the latter quantity being less by 16,677 metrekilogs. than the daily expenditure in the work of the circulation and respiration, given by Helmholtz and Fick at 160,012 metrekilogs (*Idem*).

It might be objected that the prisoners live in a higher temperature than the subjects observation on whose circulation and respiration furnished the above data ; but the quantity of clothing worn is so small, and loss of heat by evaporation and radiation consequently is so much greater in the one case than it was (probably) in the other, that the effect of this difference of temperature is probably neutralized at least by these attendant conditions. But if it be admitted that the difference of 10 per cent. can be so accounted for, the source of external work must be looked for elsewhere than in the disintegration of muscular tissue, as in this case the whole of the muscle formed from the food would be required to furnish the internal work alone. The proportions of proteine compounds given in the Table are above the truth, in many cases considerably so ; and the result is, the energy obtained must be considered an exaggeration.

Madras.

XXXVI. *Theory of the Evening Glow and analogous Phenomena.*
By Dr. E. LOMMEL*.

IN the following pages a theory of the evening glow and of analogous phenomena, based on the principle of the diffraction of light, is developed in an elementary form, the fundamental idea of which I have already stated and treated analytically on a former occasion†. I venture now to reproduce the theory in a new and what seems to me a more perfect form, and at the same time to discuss an objection which has been raised against it.

1. If a pencil of parallel luminous rays from a very distant luminous point meets a dark screen provided with a circular aperture, the elementary rays which each point of the aperture sends

* Translated from Poggendorff's *Annalen*, vol. cxxxi. p. 105 (June 1867).

† "Contributions to the Theory of the Diffraction of Light," Grunert's *Archiv*, part xxxvi. 1861.

in all possible directions into the dark space behind the screen may be considered to be grouped into an infinite number of pencils of parallel rays. The one which continues the incident rays is called *direct*, and the others *diffracted*; the angle which the direction of a diffracted pencil makes with that of the direct rays is called the angle of diffraction. If there is a lens behind the aperture (the object-glass of a telescope or the crystalline lens of the eye), it will condense the rays of each pencil in *one* point, which is obtained by drawing a line parallel to the direction of the pencil through the optical centre O, and measuring off on this line from the point O, towards the observer, a length equal to the focal distance of the lens. Hence the points of convergence lie upon a hemisphere drawn from the optical centre of the lens as centre and with its focal length as radius—that is, on the retina in the case of an eye adjusted for an infinite distance. In these points of convergence the rays of all the pencils interfere, in consequence of the difference of path which they have acquired owing to their inclination to the direct rays (the action of the lens does not appreciably alter the difference of path). The refracted rays, according to the magnitude of this inclination, sometimes completely extinguish, and sometimes more or less strengthen each other, and thus produce the well-known beautiful diffraction images. For the sake of more convenient investigation, each point of the hemispherical image may be supposed to be projected on the plane base of the hemisphere, and the same intensity of light be assigned to it there which it has in the original image. If the incident rays are normal to the plane of the screen, their point of convergence is projected exactly in the centre of that base. From known laws, which need not be developed here, the distances from the centre of the image of equivalent maxima and minima of luminous intensity, in the case of apertures of similar shapes, are directly proportional to the wave-length of the light used, and inversely proportional to corresponding dimensions of the aperture.

If, then, a diffracting aperture be made gradually smaller and smaller, the commencement of minimum of each colour will be gradually removed further from the middle of the image. Now, as the minima corresponding to the shorter wave-lengths are always nearer the centre of the picture than those belonging to the longer waves, the aperture may ultimately be supposed so small that the first minimum of any colour must commence just at the edge of the surface of the image—that is, for a diffraction-angle of 90° . Then all the less refrangible colours cease to have a minimum; but one still occurs for each of the more refrangible. If, for example, the breadth of a rectilinear slit be $= 0.0005888$

millim. (that is, equal to the wave-length of yellow sodium-light—line D in the solar spectrum), the minimum for this colour will be at the extreme edge of the surface of the image; there will be no minimum for red light; for violet, however (line H), a minimum will occur with a diffraction-angle of $42^{\circ} 18'$. Inasmuch, then, as the more refrangible rays approach their respective minima more rapidly than the less refrangible, the latter will predominate the more in diffracted light the further we are from the middle of the image—that is, from the place where the image of the source of light appears. In the above-mentioned example the ratio of the intensities of violet light (Fraunhofer's line H, wave-length = 0.0003963) and of red (line B, wave-length = 0.0006897 mil.), for the angles of diffraction $30'$, 1° , 5° , 10° , would be expressed by the numbers 0.9997, 0.9987, 0.9635, 0.8597*. These numbers show that the action is very feeble, especially for the diffracted rays nearest the direct ones; we shall, however, subsequently become acquainted with circumstances by which it is increased. We may, however, maintain that a white luminous point viewed through a very narrow aperture appears white indeed, but surrounded by an aureole of diffracted light which shows a reddish tint, though feeble and perhaps almost imperceptible.

2. If in a dark screen several equal apertures be arranged in irregular order, each of them will furnish in each direction of diffraction a resultant ray; all resultants of the same direction are equal to each other as regards intensity and mixture of colour; the phenomenon of interference which they produce upon the image will be occasioned solely by the differences of path which their inclination to the direct rays imparts to them. Owing to the irregularities in the arrangement of the apertures, the resultants will not, however, be able to neutralize one another in any direction of diffraction; and just as little will there be any direction in which they completely strengthen each other; there is no ground for supposing that in any one direction the degree of their concordance will be greater or less than in another. *The phenomenon of diffraction which a screen perforated several times produces is therefore essentially the same as that which one of its apertures would produce*, though the phenomenon occurs with an intensity increased proportionally to the square of the number of apertures †.

* These numbers are calculated from the well-known expression

$$\left(\frac{\sin \pi b \sin \psi \lambda^{-1}}{\pi b \sin \psi \lambda^{-1}} \right)^2,$$

where b denotes the breadth of the slit.

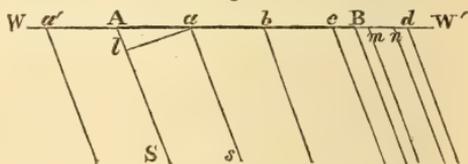
† The theory of sun and moon halos is, as is well known, founded on this reasoning.

If the individual apertures be as small as has been assumed above, even their equality is not necessary. In the case of larger unequal apertures, the diffracted light arising from one source of light will appear white, because the maxima of the various colours almost coincide, and thus give white from their mixture; but with such small apertures, only reddish tints can mix with reddish, and the action will be of the same quality as in the case of a single aperture. Hence, if a distant white luminous point be viewed through a screen which is perforated by very small arbitrarily arranged apertures, it appears white, but surrounded by reddish diffracted light; it is therefore immaterial whether the apertures be equal or unequal, provided their dimensions do not exceed a certain limit.

3. We will now return to the consideration of the action of an individual aperture, and compare it with the action of a small dark screen of the same form which stands in the path of the direct rays, or of the incident wave-plane.

We disregard in this the diffractive action of the aperture of the object-glass, or of the pupil, as compared with that which a small aperture or a small screen produces; this amounts to regarding this object-glass, and the incident wave, as unlimited on all sides.

Fig. 1.



Let A (fig. 1) be any given point at the edge of a small aperture, and AS a diffracted ray proceeding from it, and let a plane through AS cut the aperture along the straight line AB. To determine the resultant of all the diffracted rays parallel with AS proceeding from the band AB, we set off on AB equal pieces Aa, ab, bc, whose length is such that the difference of phase Al of the marginal rays AS and as is exactly a wave-length for every pencil of rays corresponding to one such division. The action of the complete pencils Aa, ab, bc then entirely disappears, because, in each, to any given ray another pencil may be assigned which is displaced towards it by half a wave-length; and there only remains the action of the imperfect pencil cB (the breadth of which in the figure is assumed to be less than $\frac{1}{2} cd$). If now, without any other alteration, in place of the small aperture a small screen coinciding with it be substituted, and if the same construction be made as before, AB represents the section of the small screen, while the straight lines AW and BW', supposed to be infinitely long, represent the section of the unlimited luminous wave. If now the straight line WW' be divided right and left, commencing at A as above, into equal parts Aa, ab, bc,

$c d . . .$, $A a'$, the rays proceeding from the parts of the wave AW and dW' produce no effect, and those incident upon the dark screen between A and B are quite kept off. Hence only the part $B d$ of the wave can come into operation. But if m is the centre of $c d$, and $d n$ be made equal to $B m$, the pencils $B m$ and $d n$ mutually extinguish each other, because they differ in their paths by half a wave-length; hence there merely remains the pencil $m n$, which is only distinguished from the pencil $c B$, remaining in the small aperture, by being displaced about $\frac{1}{2}\lambda$. Hence the pencil $m n$ will have the same amplitude but with opposite sign, and exactly the same intensity as the pencil $c B$. It is only when the ray $A S$ is at right angles to the plane of the wave (for the direct rays, that is), that the construction in question does not hold. But in this case all direct rays striking the object-glass directly will be concentrated in its focus.

Nothing in this conclusion is modified if in the figure we take $c B > \frac{1}{2} c d$ or $A a > A B$; and it remains equally applicable if we have to do, not with *one* aperture and with *one* screen, but with any given group of apertures or screens. Hence we propound the following principle:—

The phenomenon of diffraction produced by one dark screen, or by a group of dark screens, is quite identical with that arising from an aperture of the same shape, or a group of apertures, with the single exception of that point in which the direct rays unite; for all light is here collected which is not kept off by the small screen.

The action of a perforated screen is distinguished from that of a group of screens, which, so to speak, is the negative of the former, by the fact that in the first case both the direct and the diffracted light depend on the parts of the screen transmitting light, while in the second case the direct light depends on the bright, the diffracted on the dark parts. In the former case the diffracted light, as regards its intensity, conforms to the direct; in the latter case there is no such conformity. If the apertures of a dark screen be increased until, for instance, their total surface is equal to the contents of the part which remains dark, both the direct and the diffracted light increase in intensity; *but if, in like manner, the small screens of a group of small screens be increased, the diffracted light is thereby increased, while the direct is weakened.*

4. Hence, if a pencil of parallel rays (a plane wave) from an infinitely distant white point of light impinge on a (plane) group of very small dark screens, an eye behind the group will converge the direct rays to a white image of the luminous point, which appears surrounded by diffracted and, perhaps only imperceptibly, reddish-coloured light. The action of the group of screens is

thus to produce, along with the enfeebled direct white light, reddish-coloured oblique incident light. If these, before reaching the eye, again fall on a similar group of small screens, all (the direct as well as the diffracted) will again undergo the diffracting action. The direct rays in passing through the second group will diminish in intensity but not in whiteness; they will moreover give rise anew to the production of reddish diffracted light. The diffracted rays are again diffracted by the second group; of each diffracted pencil of rays one part is bent back in the direction of the direct rays. This had already suffered a loss of more refrangible rays by its first diffraction; on the second diffraction the more refrangible rays are again weakened to a greater extent than the less refrangible, its tendency to a reddish colour will therefore increase. With the direct light, now become weaker as compared with what it previously was, reddish light diffracted in the same direction will be associated, and thus make its tint reddish, though perhaps almost imperceptibly so. It is easily seen that similar reasoning applies to the diffracted pencil of rays which goes direct through the second group. Hence there is added to the light which is red from the first diffraction that which by the second diffraction has become parallel with it and is of a still deeper red.

By the addition of further groups of small screens parallel with the first, the original white direct light becomes more and more weakened, while there is continually being mixed with it more light which by repeated diffractions has become of a deeper red. The successive groups of screens act, so to speak, like sieves, which continually separate the transmitted light more and more perfectly from its more refrangible rays. *Hence the white point, viewed through a sufficient number of such groups of screens, not merely appears reddish of itself, but also appears surrounded by a still more strongly red-coloured aureole of diffracted light.*

If the rays, instead of being condensed by a lens, are received on a white screen, this, as is obvious, must appear as if illuminated with red light.

For the production of phenomena of diffraction it is unnecessary that the screens be perfectly opaque; it is enough that less light passes through them than by them. Thus the phenomena of halos, already mentioned, is produced by the diffracting action of fog-vesicles, which are themselves transparent.

5. The explanation of the red colour of the sun in rising and setting is now evident from what has been said. In the lower layers of the atmosphere a number of fine corpuscles are suspended; it is immaterial whether they be solid (as, for instance, organic and inorganic dust, the fine particles of carbon in smoke)

or liquid (water droplets or extremely minute water vesicles, as they are probably formed by incipient condensation of aqueous vapour). When the sun is high above the horizon, the groups of small screens which can be formed at right angles to its rays are not numerous enough to produce a perceptible action. But when the sun is near the horizon, its rays must traverse a sufficiently long path of the lower layers of the atmosphere (which are just those which contain such small corpuscles) to experience to an appreciable extent the diffracting action of the groups of screens which those particles can form. Hence each point of the sun must thereby appear reddish and surrounded by more strongly reddened diffracted light; for now, the red halos of adjacent points becoming superimposed, with the direct light of each point of the sun the diffracted light of the adjacent points will be associated and thereby, again, deepen its redness. Hence with a luminous surface *the red colour must be more strikingly conspicuous than with an isolated point of light.* While the luminous disks of the sun and of the moon shine on the horizon with a splendid orange-red, the reddish colour of rising and setting fixed stars is scarcely noticed. Distant white surfaces like the glaciers and the fields of snow of the Alps, and clouds near the horizon, when illuminated by the setting sun, often show a purplish-red colour, while a white wall in the neighbourhood only appears of an orange-red. The light reflected from the former must, before reaching our eyes, traverse a sufficiently thick layer of air to experience once more a diffracting action from the particles suspended in it. It has been hitherto left undecided whether solid corpuscles or fine vesicles of water exert the diffracting action. The solid particles of dust have doubtless a considerable part in it; but assuming, with Forbes, that it belongs mainly to aqueous vapour at the commencement of its condensation (that is, when it, though previously quite gaseous, begins to deposit liquid water in the form of extremely fine vesicles or droplets), our theory differs from his merely in explaining from the nature of light the property which aqueous vapours in the act of condensing have, of transmitting red rays more freely, and in not ascribing it to aqueous vapour alone, but equally to any other medium made turbid by fine particles. From the present theory, therefore, may be deduced the same conclusions as from that of Forbes—for example, as regards the meteorological significance of the redness of morning and evening.

Moreover it seems to me that the redness of morning and of evening is only to be ascribed to the action of aqueous vapour in the atmosphere when it appears especially brilliant and the whole morning or evening sky is of a fiery glow. If, on the contrary, the rising or setting sun simply appears as a reddish

disk like the moon in the horizon, I believe that the solid particles suspended in the atmosphere are adequate to explain this coloration. A similar appearance is exhibited by the sun also in the presence of a thick yellowish vapour (the *Höhenrauch*), even when it is high in the heavens. The red colour, too, which, according to travellers' statements, the sun exhibits when the simoom has raised the sand of the desert, belongs to this category.

The spectrum of the setting sun has been lately more accurately examined by Janssen. Towards the violet end it appears continually weakened; yet in the less refrangible parts, while the intensity of light is otherwise unweakened, dark lines occur along with the Fraunhofer's which are already there and have become more distinct. This latter phenomenon is, in my view, to be ascribed to the absorptive influence of aqueous vapour, while the general enfeeblement of the more refrangible rays depends upon the diffracting action of the finer particles of water and of dust.

By the same principles is explained the fact that imperfectly transparent media (those, that is to say, which are turbid owing to the admixture of very fine particles) transmit the less refrangible rays more easily than the others. Such media, for instance, are liquids rendered turbid by finely divided precipitates, smoky glass, milky glass, perhaps also dark opaque glass (which readily transmits the obscure thermal rays), smoke also, and a glass plate blackened with soot.

I also include here the observation first made by Hankel, that light which is reflected from a ground glass plate under a very oblique incidence appears of a reddish colour. If white light is incident on the plate under a smaller angle of incidence, the small depressions between the projections will act like small concave mirrors turned in different directions, and will disperse the rays of light accordingly. Hence an image of the source of light cannot be formed, and, in spite of numerous but irregular reflexions, the diffuse reflected light will appear white. But if the incident rays are greatly inclined to the plate, that light alone reaches the eye which is reflected from the tops of the projections that have by polish been brought into one plane, as is at once seen from inspecting fig. 2. Hence an image of the source of light, of a reddish shade, will be seen; for the slender pencils of light which are reflected from the tops of the projections will act just as if they had passed through very small apertures.

Fig. 2.



6. In the *Fortschritte der Physik* for 1861, in discussing the paper mentioned at the outset, the objection is urged against my idea that the red light of the setting sun is caused by the dif-

fractive action of small particles, that it is not clear what becomes of the complementary blue light; for if in other phenomena of interference colours be produced from white light, this is so effected that all the rays together (transmitted and reflected) give white again; in this case the light retained (reflected, absorbed) by the small screen is obviously white, and this should be the case with the whole of the transmitted light.

To this the following reply may be made:—The entire theory developed in the preceding is based on the hypothesis *that the laws of diffraction are quite independent of the magnitude of the apertures*, however small or great they be. I do not think that this assumption can be successfully contested; it has hitherto been always tacitly assumed; and I am acquainted with no fact that militates against it. But, admitting that the laws of diffraction hold for apertures however small, the principle that violet rays are more enfeebled than red ones is to be considered a necessary consequence of the undulatory theory.

It is true that in other phenomena of interference, for instance the colours of thin plates, the reflected together with the transmitted light give white. But this principle does not hold at all for phenomena of diffraction, be the diffracting apertures great or small; for here the light retained by the opaque parts is white, like the incident. Hence, as has been rightly observed, the diffracted would, collected, also give white. To this end it would be necessary that for an aperture in the form of a slit the integral

$$2 \int_0^{\frac{1}{2}\pi} \left(\frac{\sin \pi b \lambda^{-1} \sin \psi}{\pi b \lambda^{-1} \sin \psi} \right)^2 d\psi,$$

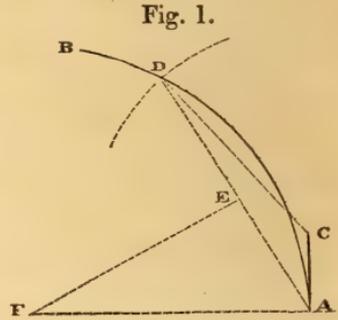
which for each colour represents the total intensity of the diffracted light, should be independent of λ , which is clearly not the case. Hence the above objection against the phenomena of diffraction cannot be sustained.

The facts enumerated in the preceding paragraphs, and conceived as phenomena of diffraction, have led Babinet to propound as a *postulate* the principle that rays of short are more readily destroyed than those of longer wave-length by obstacles which are not of a specific nature—that is, by fine corpuscles, no matter of what they consist. This principle is adduced in Billet, *Traité d'Optique Physique*, vol. i. p. 168, as “Babinet’s principle.” The present paper, as well as that previously cited, may be regarded as attempts to connect this principle of Babinet, which had not hitherto been demonstrated, with the fundamental laws of the undulatory theory.

XXXVII. *On the Approximate Drawing of Circular Arcs of given Lengths.* By W. J. MACQUORN RANKINE, C.E., LL.D., F.R.S.*

1. **RULES.**—The following rules for approximately drawing circular arcs of given lengths have not, so far as I know, been hitherto published.

Rule I. (See fig. 1.)—From a given point A, in the circumference of a given circle AB, to lay off an arc which shall be approximately of a given length. From A draw the straight tangent AC, equal to one-fourth part of the given length. About C, with a radius equal to three-fourths of the given length, draw a circular arc cutting the given circle in D. Then AD will be the arc required.



Rule II. (See fig. 1.)—To draw a circular arc which shall touch a given straight line AC at a given point A, shall subtend a given angle, and shall be approximately of a given length. Make AC equal to one-fourth of the given length; about C, with a radius equal to three-fourths of the given length, describe a circle; draw the straight line AD, making the angle CAD equal to one-half of the given angle, and cutting the circle in D; D will be the other end of the required arc. Bisect AD in E; draw the straight lines AF perpendicular to AC, and EF perpendicular to AD, meeting each other in F; that point will be the centre for the required arc AD.

2. *Extent of Errors.*—The arcs laid off according to the preceding rules are somewhat longer than the exact length given. The following are examples of the proportionate errors:—

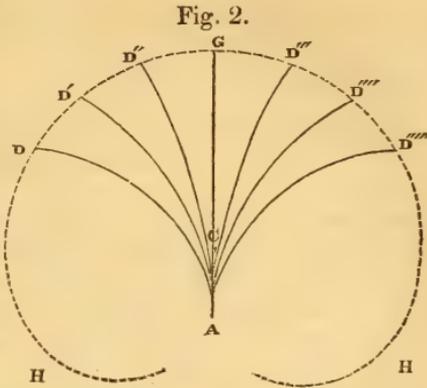
	Error in fractions of the arc, about
In laying off an arc subtending 30°	$\frac{1}{14800}$.
" " " 45°	$\frac{1}{2900}$.
" " " 60°	$\frac{1}{935}$.

The proportionate error varies nearly as the fourth power of the angle subtended by the arc, and diminishes very rapidly when that angle becomes small.

3. *Demonstration.*—In fig. 2, let a straight line AG and an

* Communicated by the Author, having been read at the British Association, Dundee Meeting, September 1867.

indefinite number of circles A D, A D', &c., all in one plane, touch each other at the point A. Let H G H be a curve cutting off equal lengths A G, A D, A D', &c. from the straight line and from all the circles. Take A G for the axis of *y* and A for the origin; let *a* be the common value of the equal lengths A G &c., and θ the angle in circular measure subtended by any one of the circular arcs A D &c. at its centre. Then the coordinates of the curve H G H have the following values:—



$$x = \frac{a}{\theta} (1 - \cos \theta); \quad y = \frac{a}{\theta} \sin \theta. \quad \dots \quad (A)$$

The radius of curvature of the curve H G H at the point G is the limit towards which $\frac{x^2}{2(a-y)}$ approaches indefinitely when θ diminishes indefinitely; and it is easily found by ordinary methods to be $\frac{3}{4}a$. Therefore in the neighbourhood of the point G the curve H G H approximates to a circle of the radius $GC = \frac{3}{4}a = \frac{3}{4}AG$.

4. *Method of Calculating Errors.*—The errors are calculated as follows:—If A D in fig. 1 were an arc exactly of the required length, the straight line joining C and D would be of the following length,

$$\sqrt{\left\{x^2 + \left(y - \frac{a}{4}\right)^2\right\}};$$

but its actual length is $\frac{3a}{4}$; therefore the absolute error is given by the following formula,

$$e = \frac{3a}{4} - \sqrt{\left\{x^2 + \left(y - \frac{a}{4}\right)^2\right\}};$$

and the proportionate error by the following formula,

$$\left. \begin{aligned} \frac{e}{a} &= \frac{3}{4} - \sqrt{\left\{\frac{x^2}{a^2} + \left(\frac{y}{a} - \frac{1}{4}\right)^2\right\}} \\ &= \frac{3}{4} - \sqrt{\left\{\frac{(1 - \cos \theta)^2}{\theta^2} + \left(\frac{\sin \theta}{\theta} - \frac{1}{4}\right)^2\right\}} \\ &= \frac{3}{4} - \sqrt{\left\{\frac{2 - 2 \cos \theta}{\theta^2} - \frac{\sin \theta}{2\theta} + \frac{1}{16}\right\}}. \end{aligned} \right\} (B)$$

When θ becomes a very small angle, the preceding expression is found, by developing $\sin \theta$ and $\cos \theta$ in powers of θ , to take the following approximate value,

$$\frac{e}{a} = \frac{\theta^4}{1080}, \dots \dots \dots (C)$$

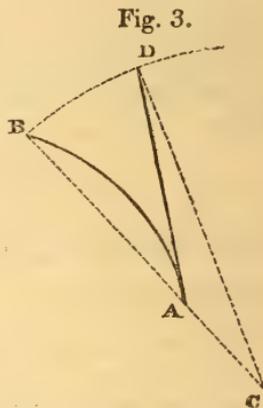
which varies, as already stated, proportionally to the fourth power of the angle subtended by the arc A D.

5. *Use of the Rules.*—Amongst the uses to which both the rules given in article 1 can be applied, there may be specified the setting out of arcs of given lengths upon the pitch-circles of toothed wheels. Rule I. is applicable also to laying off given distances upon railway-curves. Rule I. may be applied to other curves besides the circle, provided the deviations from uniformity of curvature are not great. The angular extent of the arcs to which the rules are applicable is limited by the degree of accuracy required in the measurement, regard being had to the relations between that angular extent and the extent of error, as stated in articles 2 and 4.

Glasgow University,
August 27, 1867.

APPENDIX.

Rule III. (See fig. 3.)—To rectify approximately the circular arc A B. Draw the chord A B: produce it, and make A C = $\frac{1}{2}$ A B; about C, with the radius C B, draw a circle; through A draw the straight tangent A D cutting that circle in D; A D will be approximately equal in length to the circular arc A B. The errors of this rule are approximately equal to those of the Rules I. and II., but of the contrary sign, the straight line A D being shorter than the arc A B.



XXXVIII. *On the Identity of the Vibrations of Light with Electrical Currents.* By L. LORENZ*.

THE science of our century has succeeded in demonstrating so many relations between the various forces (between electricity and magnetism, between heat, light, molecular and chemical actions), that we are in a sense necessarily led to regard them as *manifestations of one and the same force*, which, according to circumstances, occurs under different forms. But though this has been the guiding idea with the greatest inquirers of our time, it has been by no means theoretically established; and though the connexion between the various forces has been demonstrated, it has only been explained in single points. Thus Ampère has theoretically explained the connexion between electricity and magnetism, though he has not furnished a proof of the possibility of the peculiar molecular electrical currents (assumed by him) which in virtue of their own power are continuous; and, in like manner, Melloni was subsequently led step by step to the assumption of an identity of light with radiant heat. These theories are, however, quite isolated members of the great chain; and so far are we from being able to follow out theoretically the idea of the unity of force, that even now, half a century after Ørsted's discovery, the two electricities are regarded as electrical *fluids*, light as vibrations of *æther*, and heat as motions of the *molecules of bodies*.

Yet these physical hypotheses are scarcely reconcileable with the idea of the unity of force; and while the latter has had a signal influence on science, this can by no means be said of the former, which have only been useful inasmuch as they furnish a basis for our imagination. Hence it would probably be best to admit that in the present state of science we can form no conception of the physical reason of forces and of their working in the interior of bodies; and therefore (at present, at all events) we must choose another way, free from all physical hypotheses, in order, if possible, to develop theory step by step in such a manner that the further progress of a future time will not nullify the results obtained.

This idea is at the basis, not only of the present investigation, but also of my earlier researches on the theory of light †; and I am the more moved to adhere to it, that it shows in a remarkable manner how the results which I venture here to develop attach themselves to those I have formerly obtained, and go hand in hand with them. At the same time that I keep the investigation free from all physical hypotheses, I shall endeavour

* Translated from Poggendorff's *Annalen*, June 1867.

† *Phil. Mag.* S. 4. vol. xxvi. p. 81.

to demonstrate a new member in the chain which connects the various manifestations of the forces; I shall prove that in accordance with the laws for the propagation of electricity under the action of free electricity, and of the electrical currents of the surrounding media, which we can deduce from experiment, periodical electrical currents are possible which in every respect behave like the vibrations of light; from which it indubitably follows that *the vibrations of light are themselves electrical currents*.

We know that light is produced by a wave-motion with very rapid periodical motions which we may call vibrations. It is the peculiarity of these vibrations that they are at right angles to the direction in which the wave of light travels; and we may say that this peculiarity has not found a correct explanation in the theory of elasticity, or in the analogous one of Cauchy; for, apart from the fact that this theory necessitates the assumption of a special medium (the luminous æther, which moreover stands quite isolated and separate from any other observation or demonstrable connexion with other forces), even with this assumption, and the various hypotheses of Cauchy, it is scarcely possible to imagine a medium in which a wave-motion could travel without a trace of longitudinal vibrations. Convinced that this theory cannot give a real, but only a factitious explanation even of the peculiarity of light (the transverse vibrations), I had formerly drawn attention to the fact that variable electrical currents, which induce in closed conductors currents that are parallel with the original ones, are similar to the vibrations of light, which in a certain sense also induce parallel vibrations. But as the laws of induced currents, generally admitted and based on experiment, did not directly lead to the expected result, the question was whether it was not possible so to modify the laws assumed that they would embrace both the experiments on which they rest and the phenomena which belong to the theory of light.

Kirchhoff (*Pogg. Ann.* vol. cii.) has expressed the laws of the motion of electricity in bodies with constant conducting-power by the following equations,

$$\left. \begin{aligned} u &= -2k \left(\frac{d\Omega}{dx} + \frac{4}{c^2} \frac{dU}{dt} \right), \\ v &= -2k \left(\frac{d\Omega}{dy} + \frac{4}{c^2} \frac{dV}{dt} \right), \\ w &= -2k \left(\frac{d\Omega}{dz} + \frac{4}{c^2} \frac{dW}{dt} \right); \end{aligned} \right\} \dots \dots (1)$$

in which u, v, w are the components of the electrical density of the current in the point $x y z$, k the constant conducting-

power, c a constant, and

$$U = \iiint \frac{dx' dy' dz'}{r^3} (x-x') [u'(x-x') + v'(y-y') + w'(z-z')],$$

$$V = \iiint \frac{dx' dy' dz'}{r^3} (y-y') [u'(x-x') + v'(y-y') + w'(z-z')],$$

$$W = \iiint \frac{dx' dy' dz'}{r^3} (z-z') [u'(x-x') + v'(y-y') + w'(z-z')],$$

$$\Omega = \iiint \frac{dx' dy' dz'}{r} \epsilon' + \int \frac{ds'}{r} e',$$

in which u', v', w' are the components of the density of the current in the point $x' y' z'$, ϵ' the density of the free electricity in this point, e' the density upon the element of surface ds' , and r the distance of the points $x y z$ and $x' y' z'$.

These formulæ express that the components of the electromotive force in $x y z$, which according to Ohm's law are $\frac{u}{k}, \frac{v}{k}, \frac{w}{k}$, are a sum of two components of electromotive force,—one arising from the inducing action of free electricity, the other from the inducing action of the variable intensities of the current in all the elements of the body.

Kirchhoff has further expressed the relations between the components of the current and the free electricity by the two equations

$$\left. \begin{aligned} \frac{du}{dx} + \frac{dv}{dy} + \frac{dw}{dz} &= -\frac{1}{2} \frac{d\epsilon}{dt}, \\ u \cos \lambda + v \cos \mu + w \cos \nu &= -\frac{1}{2} \frac{de}{dt}, \end{aligned} \right\} \dots \dots (2)$$

in which $\lambda, \mu,$ and ν are the angles which the normal to the surface, directed inward, makes with the coordinate axes.

It is at once obvious that the equations (1), which are deduced in a purely empirical manner, are not necessarily the exact expression of the actual law; and it will always be permissible to add several members, or to give the equations another form, always provided these changes acquire no perceptible influence on the results which are established by experiment. We shall begin by considering the two members on the right side of the equations as the first members of a series.

By the equation

$$\bar{\Omega} = \iiint \frac{dx' dy' dz'}{r} \epsilon' \left(t - \frac{r}{a} \right) + \int \frac{ds'}{r} e' \left(t - \frac{r}{a} \right)$$

let a new function $\bar{\Omega}$ be defined, in which by the relations $\epsilon' \left(t - \frac{r}{a} \right)$ and $e' \left(t - \frac{r}{a} \right)$, where a is a constant, it shall be expressed that these are the same functions of $\left(t - \frac{r}{a} \right)$ as ϵ' and e' are of t in the above expression Ω . Now by the development of the series we have

$$\epsilon' \left(t - \frac{r}{a} \right) = \epsilon' - \frac{d\epsilon'}{dt} \cdot \frac{r}{a} + \frac{d^2\epsilon'}{dt^2} \cdot \frac{r^2}{a^2} \cdot \frac{1}{1 \cdot 2} - \dots,$$

$$e' \left(t - \frac{r}{a} \right) = e' - \frac{de'}{dt} \cdot \frac{r}{a} + \frac{d^2e'}{dt^2} \cdot \frac{r^2}{a^2} \cdot \frac{1}{1 \cdot 2} - \dots,$$

which series are inserted in the above equation, and this is then differentiated with respect to x . There is thus obtained

$$\frac{d\bar{\Omega}}{dx} \frac{d\Omega}{dx} + \frac{1}{2a^2} \frac{d'}{dt^2} \left[\iiint \frac{dx' dy' dz'}{r} (x-x') \epsilon' + \int \frac{ds'}{r} (x-x') e' \right] - \dots;$$

and if in this equation for $\frac{d\epsilon'}{dt}$ and $\frac{de'}{dt}$ the values given by equations (2) are substituted, we obtain by partial integration

$$\frac{d\bar{\Omega}}{dx} = \frac{d\Omega}{dx} - \frac{1}{a^2} \frac{d}{dt} \iiint \frac{dx' dy' dz'}{r} u' + \frac{1}{a^2} \frac{dU}{dt} - \dots, \quad (3)$$

U having here its previous meaning. Hence we may put

$$\frac{d\bar{\Omega}}{dx} + \frac{1}{a^2} \frac{d}{dt} \iiint \frac{dx' dy' dz'}{r} u' \left(t - \frac{r}{a} \right) = \frac{d\Omega}{dx} + \frac{1}{a^2} \frac{dU}{dt}, \quad (4)$$

where it is indicated by $u' \left(t - \frac{r}{a} \right)$ that u' is here a function of $\left(t - \frac{r}{a} \right)$ instead of t alone.

The right hand of this last equation is a series of which only the first two members are retained, and whose following members proceed by increasing powers of $\frac{r}{a}$. If a be assumed $= \frac{c}{2}$, both these members become the same as the expression between the parentheses in the first of equation (4); but now, according to Weber's determination,

$$c = 284736 \text{ miles,}$$

while the greatest value of r in the experiments has only exceeded

a few feet ; hence $\frac{r}{a}$ is an infinitely small magnitude. Hence the following members of the above series are everywhere quite inappreciable, provided only that the differential quotients of the components of the current of the second and third order be not very great as compared with the time, which comes into play here also.

Hence the equations for the propagation of electricity, as regards the experiments on which they rest, are just as valid as equations (1), if, by the aid of the equation (4) and its two analogous equations, the following form be assigned to them,

$$\left. \begin{aligned} u &= -2k \left(\frac{d\bar{\Omega}}{dx} + \frac{4}{c^2} \frac{d\alpha}{dt} \right), \\ v &= -2k \left(\frac{d\bar{\Omega}}{dy} + \frac{4}{c^2} \frac{d\beta}{dt} \right), \\ w &= -2k \left(\frac{d\bar{\Omega}}{dz} + \frac{4}{c^2} \frac{d\gamma}{dt} \right), \end{aligned} \right\} \dots \dots \dots (A)$$

where, for brevity's sake, we put

$$\begin{aligned} \alpha &= \iiint \frac{dx' dy' dz'}{r} u' \left(t - \frac{r}{a} \right), \\ \beta &= \iiint \frac{dx' dy' dz'}{r} v' \left(t - \frac{r}{a} \right), \\ \gamma &= \iiint \frac{dx' dy' dz'}{r} w' \left(t - \frac{r}{a} \right). \end{aligned}$$

These equations are distinguished from equations (1) by containing, instead of U, V, W, the somewhat less complicated members α, β, γ ; and they express further that the entire action between the free electricity and the electrical currents *requires time to propagate itself*—an assumption not strange in science, and which may in itself be assumed to have a certain degree of probability. For in accordance with the formulæ found, the action in the point $x y z$ at the moment t does not depend on the *simultaneous* condition in the point $x' y' z'$, but on the condition in which it was at the moment $t - \frac{r}{a}$; that is, so much time in advance as is required to traverse the distance r with the constant velocity a .

The constant a which enters into equations (A) should, from the foregoing, be made equal $\frac{c}{2}$; closer investigations, however,

will show that other values are also possible. The first equation (A) may also be written in the following manner:—

$$u = -2k \left(\frac{d\Omega}{dx} + \left(\frac{4}{c^2} - \frac{1}{a^2} \right) \frac{d}{dt} \iiint \frac{dx' dy' dz'}{r} u' + \frac{1}{a^2} \frac{dU}{dt} \dots \right),$$

which expression in the case of $a = \frac{c}{2}$ leads us back to the first equation (1), whereas, if a were assumed to be infinitely great, it would obtain just the form which would result from Neumann's electrodynamical theory. And as this theory also agrees with experiment, it is obvious that a is not defined by it, and must for the present be regarded as an indeterminate magnitude. Yet it must be very great, of the same order as c , to allow the following members of the series to be considered infinitely small. If, for instance, $a = \frac{c}{\sqrt{2}}$, the above equation will represent a mean between Weber's and Neumann's theories.

It now becomes necessary to obtain, in another manner, a determination of these undefined constants, and, if possible, seek a confirmation or correction of the results found. It might then be attempted, by using the indication of the formula, that electrical actions require time for their propagation, to find a probable hypothesis of the mode of action of dynamical electricity, by which results might be obtained similar to those already found. I have found that this may be effected in several ways; this method, however, quite loses its value, because its significance would entirely depend on finding an hypothesis which in and for itself is more probable than all others. After careful investigation of this point, I have completely given up the idea of getting any good from physical hypotheses; and we can only develop the consequences from the results found, and inquire whether this does not furnish an indication towards answering the question.

For a given function ϕ , provided the point $x y z$ is within the limits of the integral, we have

$$\left(\Delta_2 - \frac{d^2}{a^2 dt^2} \right) \iiint \frac{dx' dy' dz'}{r} \phi \left(t - \frac{r}{a}, x', y', z' \right) \Bigg\} = -4\pi\phi(t, x, y, z), \dots \dots \dots \Bigg\} \quad (5)$$

where

$$\Delta_2 \text{ is written for } \frac{d^2}{dx^2} + \frac{d^2}{dy^2} + \frac{d^2}{dz^2}.$$

The proof of this theorem, which moreover is not difficult to see, is found in my paper in Crelle's Journal, vol. lviii. By its aid

the equations (A) are transformed into the following differential equations:—

$$\Delta_2 u - \frac{1}{a^2} \frac{d^2 u}{dt^2} = 8\pi k \left(\frac{d\epsilon}{dx} + \frac{4}{c^2} \frac{du}{dt} \right),$$

$$\Delta_2 v - \frac{1}{a^2} \frac{d^2 v}{dt^2} = 8\pi k \left(\frac{d\epsilon}{dy} + \frac{4}{c^2} \frac{dv}{dt} \right),$$

$$\Delta_2 w - \frac{1}{a^2} \frac{d^2 w}{dt^2} = 8\pi k \left(\frac{d\epsilon}{dz} + \frac{4}{c^2} \frac{dw}{dt} \right),$$

with which is connected by (2) the equation

$$\frac{du}{dx} + \frac{dv}{dy} + \frac{dw}{dz} = -\frac{1}{2} \frac{d\epsilon}{dt}.$$

These equations are satisfied, for instance, by

$$u = e^{-hz} \cos p(\omega t - z), \quad v = 0, \quad w = 0, \quad . . . \quad (6)$$

where h, p, ω are constants between which the relations prevail,

$$h^2 a^2 = p^2 (a^2 - \omega^2) \text{ and } hc^2 = 16\pi k \omega. \quad . . . \quad (7)$$

From this preliminary treatment of equations (A), it is clear that *periodical* electrical currents are possible, that such ones travel like a *wave-motion* with the velocity ω , and, like light, make vibrations which are at right angles to the direction of propagation. If we assume thence that the vibrations of light themselves are electrical currents, ω expresses the velocity of light, while a is the velocity with which electrical action is propagated through space. It is manifest, further, from the latter equation, that, when the electrical conductivity k of the body is very small, the two velocities tend to become equal to one another.

The velocity with which in Weber's electrodynamical experiments the electrical action at a distance has passed from one conductor to another through the air, is according to this result the same as the velocity of light in air. But now, according to Weber's determination, $c = 284736$ miles, and therefore

$$\frac{c}{\sqrt{2}} = 201360,$$

a magnitude which remarkably agrees with the various determinations of the velocity of light; for they lie both above and below this value in such a manner that the present may be regarded as a new determination of the velocity of light, and not necessarily inferior in accuracy to any other. We have

therefore some reason for taking $a = \frac{c}{\sqrt{2}}$; and if this value $a\sqrt{2}$

be substituted for c in equations (A), the accuracy of this assumption is confirmed by the circumstance that the equations assume now a very simple form, and lead to exactly the same differential equations as those which I formerly deduced for the vibrations of light, with the addition of only a single member.

For, in accordance with equations (2), we have

$$\frac{d\epsilon'}{dt} \left(t - \frac{r}{a} \right) = -2 \left[\frac{\delta u' \left(t - \frac{r}{a} \right)}{\delta x'} + \frac{\delta v' \left(t - \frac{r}{a} \right)}{dy'} + \frac{\delta w' \left(t - \frac{r}{a} \right)}{\delta z'} \right],$$

where the differentiation in reference to x' , y' , and z' must be carried out in such a manner that r will be considered constant, and

$$\frac{d\epsilon'}{dt} \left(t - \frac{r}{a} \right) = -2 \left[u' \left(t - \frac{r}{a} \right) \cos \lambda + v' \left(t - \frac{r}{a} \right) \cos \mu + w' \left(t - \frac{r}{a} \right) \cos \nu \right].$$

If these values be substituted in

$$\frac{d\bar{\Omega}}{dt} = \iiint \frac{dx' dy' dz'}{r} \frac{d\epsilon'}{dt} \left(t - \frac{r}{a} \right) + \int \frac{ds'}{r} \frac{d\epsilon'}{dt} \left(t - \frac{r}{a} \right),$$

by partial integration and introduction of the designations α, β, γ we obtain

$$\frac{d\bar{\Omega}}{dt} = -2 \left(\frac{d\alpha}{dx} + \frac{d\beta}{dy} + \frac{d\gamma}{dz} \right).$$

Moreover from (5),

$$\frac{1}{a^2} \frac{d^2\alpha}{dt^2} = \Delta_2\alpha + 4\pi u,$$

and in like manner for β, γ . If now these values be substituted in the equations (A), after they have been differentiated in reference to t , and if $c = a\sqrt{2}$, we get

$$\left. \begin{aligned} \frac{1}{4k} \frac{du}{dt} + 4\pi u &= \frac{d}{dz} \left(\frac{d\gamma}{dx} - \frac{d\alpha}{dz} \right) - \frac{d}{dy} \left(\frac{d\alpha}{dy} - \frac{d\beta}{dx} \right), \\ \frac{1}{4k} \frac{dv}{dt} + 4\pi v &= \frac{d}{dx} \left(\frac{d\alpha}{dy} - \frac{d\beta}{dx} \right) - \frac{d}{dz} \left(\frac{d\beta}{dz} - \frac{d\gamma}{dy} \right), \\ \frac{1}{4k} \frac{dw}{dt} + 4\pi w &= \frac{d}{dy} \left(\frac{d\beta}{dz} - \frac{d\gamma}{dy} \right) - \frac{d}{dx} \left(\frac{d\gamma}{dx} - \frac{d\alpha}{dz} \right). \end{aligned} \right\} \quad (8)$$

Moreover we obtain directly from equations (A)

$$\left. \begin{aligned} \frac{dv}{dz} - \frac{dw}{dy} &= -\frac{4k}{a^2} \frac{d}{dt} \left(\frac{d\beta}{dz} - \frac{d\gamma}{dy} \right), \\ \frac{dw}{dx} - \frac{du}{dz} &= -\frac{4k}{a^2} \frac{d}{dt} \left(\frac{d\gamma}{dx} - \frac{d\alpha}{dz} \right), \\ \frac{du}{dy} - \frac{d}{dx} &= -\frac{4k}{a^2} \frac{d}{dt} \left(\frac{d\alpha}{dy} - \frac{d\beta}{dx} \right), \end{aligned} \right\} \dots \dots \dots (9)$$

by which equations α, β, γ may be eliminated from the previous equations (8) after they have been differentiated in reference to t . In this way the following equations are obtained:—

$$\left. \begin{aligned} \frac{d}{dy} \left(\frac{du}{dy} - \frac{dv}{dx} \right) - \frac{d}{dz} \left(\frac{dw}{dx} - \frac{du}{dz} \right) &= \frac{1}{a^2} \frac{d^2u}{dt^2} + \frac{16\pi k}{a^2} \frac{du}{dt}, \\ \frac{d}{dz} \left(\frac{dv}{dz} - \frac{dw}{dy} \right) - \frac{d}{dx} \left(\frac{du}{dy} - \frac{dv}{dx} \right) &= \frac{1}{a^2} \frac{d^2v}{dt^2} + \frac{16\pi k}{a^2} \frac{dv}{dt}, \\ \frac{d}{dx} \left(\frac{dw}{dx} - \frac{du}{dz} \right) - \frac{d}{dy} \left(\frac{dv}{dz} - \frac{dw}{dy} \right) &= \frac{1}{a^2} \frac{d^2w}{dt^2} + \frac{16\pi k}{a^2} \frac{dw}{dt}. \end{aligned} \right\} \dots (B)$$

These equations for the components of the electrical current agree fully with those which I have already found for the components of light up to the last member, into which the electrical conductivity k enters. This member indicates an absorption which will be greater the greater the electrical conductivity, and which is defined by the constant h in the equations (6) if in these $c = a\sqrt{2}$.

If k is very large as regards pa , equations (7) give

$$h = p = \frac{2\pi}{\lambda},$$

if λ denotes the wave-length of light; from which it follows that the amplitude of a ray of light which, for instance, has passed through a layer of a good conductor of electricity of the thickness of half a wave-length, is from (6) e^π times lessened, and the intensity reckoned proportional to the square of the amplitude, $e^{2\pi}$ or 535 times. This will be the case with all metals; for, according to Weber, the conductivity of copper is $\frac{1}{274100}$ in magnetic measurement, taking the millimetre and the second as units of time and length, and therefore $\frac{c^2}{8} \times \frac{1}{274100}$, or $283433a$ in mechanical measurement, a magnitude which is great as compared with $\frac{2\pi}{\lambda} a$. It is clear, however, that this result can only

be considered approximately correct, especially as a perfectly constant conductivity is presupposed, a homogeneity which does not in fact exist. The chief result, however, that *all good conductors of electricity absorb light to a great extent*, is in marked accordance with experiment.

When the electrical conductivity is very small, equations (7) give

$$h = 8\pi \frac{k}{a}$$

Now in the case of copper, whose conductivity has been given above, $\frac{k}{a}$ has been found equal to 283433; but for all transparent bodies the conductivity is millions of times as small as that of copper; and liquids form a marked exception, where the chemical activity and the mobility of the particles exert so great an influence on the determination of the conductivity in the proper sense that it becomes in fact impossible; we thus find that the conductivity of all other transparent media is *so many* millions of times as small as that of the metals that the coefficient of absorption h , as well as the last member of equations (B), will disappear, by which the latter become quite identical with the equations of light. Just as we can infer their opacity from the good conductivity of the metals, so from the very small transparency of a body we may conclude that, as compared with metals, it is *an extremely bad conductor* of the electrical current, a result which experiment has also fully confirmed.

The vibrations arising from equations (B) are transversal; and even if the member containing R be retained, longitudinal vibrations will not be possible. By differentiating the three equations (8) in reference to x , y , and z , and addition, we obtain

$$\frac{d\theta}{dt} + 16\pi k\theta = 0,$$

if

$$\frac{du}{dx} + \frac{dv}{dy} + \frac{dw}{dz} = \theta.$$

It is manifest from this that θ cannot be a periodical function of the time, from which it follows that longitudinal vibrations cannot take place. As, moreover, this equation shows that the value of θ diminishes as the time increases, and is independent of the components of all surrounding points, we are compelled to assume generally that $\theta = 0$; from which it follows, since

$$\theta = -\frac{1}{2} \frac{d\epsilon}{dt},$$

that in the interior of a body with constant conducting-power no

development of free electricity is possible. This result is different from that which Kirchhoff has deduced from the original equations (1)—that is, that in the interior of a conductor there is in general free electricity; but from the whole of the present investigation it will be clear that at all events this conclusion cannot be drawn with any degree of certainty.

Thus, after it has been proved that from equations (A), which embrace the laws of electrical currents that are in accordance with experiment, the differential equations (B) can be deduced, which show that electrical currents behave in every respect like the vibrations of light, the question arises whether, on the other hand, the laws of electrical currents can be deduced from the known laws of light. I shall now show that this is in fact possible, in such a manner that equations (A) can be again deduced from equations (B), provided the conditions be introduced into the latter which must be fulfilled at the limit of the body, and which we must know in order to deduce from the differential equations such others, which in a certain sense are their integrals. At the same time it will be seen that these limiting conditions are just the same as those I have already found (*Pogg. Ann.* vol. cxviii. p. 126; *Phil. Mag.* S. 4. vol. xxvi. p. 93) for the components of light; so that for this calculation we need make no other assumptions than just those which the theory of light gives.

For an element of the surface which is at right angles to the axis x , I have found that the magnitudes

$$u, \quad v, \quad \frac{du}{dy} - \frac{d}{dx}, \quad \frac{dw}{dx} - \frac{du}{dz}$$

on both sides of the element are equal; from this the limiting conditions for all other elements of the surface will be found, because the direction of the coordinate axis is arbitrarily chosen. These conditions are deduced from the differential equations of the components of light, which was possible in this case, because they held generally for all heterogeneous media, and they remained the same even after the members from the equations (B) containing the factor k had been added to the equations, a circumstance which is now seen to be necessary.

For a body whose conductivity is constant, which is surrounded by absolute non-conductors (no matter whether they really exist or not), the above-named magnitudes become zero on the surface of the body, since any electrical current is impossible in the entire insulating surface which surrounds the body.

We introduce now into equations (B), instead of u, v, w, x, y, z , the notation u', v', w', x', y', z' ; and first suppose $t - \frac{r}{a}$ substituted for t , where r denotes the distance of a fixed point $x y z$

from the point $x' y' z'$. The equations thus altered will hold good, provided the differentiation indicated on the left side be considered partial in such a manner that it is not effected as regards r . Both sides are afterwards multiplied by $\frac{dx' dy' dz'}{r}$, and the equations are integrated over the entire space of the body. By partial integration, for instance, the first member of the first equation with the previous notation becomes

$$\iiint \frac{dx' dy' dz'}{r} \frac{\delta^2 u' \left(t - \frac{r}{a} \right)}{\delta y'^2} = - \int \frac{ds'}{r} \frac{\delta u' \left(t - \frac{r}{a} \right)}{\delta y'} \cos \mu$$

$$+ \frac{d}{dy} \iiint \frac{dx' dy' dz'}{r} \frac{\delta u' \left(t - \frac{r}{a} \right)}{\delta y'}$$

in which the last member by repeated partial integration passes into

$$- \frac{d}{dy} \int \frac{ds'}{r} u' \left(t - \frac{r}{a} \right) \cos \mu + \frac{d^2 a}{dy^2}.$$

If now all the members on the left side of the equation in question be treated in the same manner, it will be found that, if all integrals are to vanish in reference to the surface of a body, we must have

$$\left(\frac{du'}{dy'} - \frac{dv'}{dx'} \right) \cos \mu - \left(\frac{dw'}{dx'} - \frac{dw'}{dz'} \right) \cos \nu = 0,$$

$$u' \cos \mu - v' \cos \lambda = 0, \quad u' \cos \nu - w' \cos \lambda = 0,$$

in which equations we suppose t again introduced instead of $t - \frac{r}{a}$, which is permissible, since the equations are valid for all values of t , and the differentiation is not to be effected in reference to r .

For an element at right angles to the x -axis (that is, for

$$\cos \mu = 0, \quad \cos \nu = 0),$$

these equations give

$$v' = 0 \quad \text{and} \quad w' = 0,$$

and the corresponding equations, which are obtained from the two other equations (B), and can be deduced from the above equations by changing the letters, give

$$\frac{du'}{dy'} - \frac{dv'}{dx'} = 0 \quad \text{and} \quad \frac{dw'}{dx'} - \frac{dw'}{dz'} = 0.$$

Hence, if the integrals in reference to the surface of the body are to disappear, they must for an element at right angles to the

x -axis fulfil just the very conditions which are deduced from the theory of light for this element; and since the direction of the axis is arbitrarily chosen, the same must hold good for all elements of the surface.

Thus, as, assuming these limiting conditions, the integrals to the surface disappear, by the above calculation, from the first equation (B) there results

$$\frac{d}{dy} \left(\frac{d\alpha}{dy} - \frac{d\beta}{dx} \right) - \frac{d}{dz} \left(\frac{d\gamma}{dx} - \frac{d\alpha}{dz} \right) = \frac{1}{a^2} \frac{d^2\alpha}{dt^2} + \frac{16\pi k}{a^2} \frac{d\alpha}{dt}.$$

If, in accordance with the earlier notation, we put

$$\frac{d\alpha}{dx} + \frac{d\beta}{dy} + \frac{d\gamma}{dz} = -\frac{1}{2} \frac{d\bar{\Omega}}{dt}$$

and further, on the right hand, in accordance with the general theorem (5),

$$\frac{1}{a^2} \frac{d^2\alpha}{dt^2} = \Delta_2\alpha + 4\pi u,$$

the equation takes this form—

$$\frac{1}{2} \frac{d^2\bar{\Omega}}{dx dt} = 4\pi u + \frac{16\pi k}{a^2} \frac{d\alpha}{dt}.$$

In an analogous manner the two other equations (B) give

$$\frac{1}{2} \frac{d^2\bar{\Omega}}{dy dt} = 4\pi v + \frac{16\pi k}{a^2} \frac{d\beta}{dt},$$

$$\frac{1}{2} \frac{d^2\bar{\Omega}}{dz dt} = 4\pi w + \frac{16\pi k}{a^2} \frac{d\gamma}{dt},$$

from the latter of which we obtain by the elimination of $\bar{\Omega}$,

$$\frac{dv}{dz} - \frac{dw}{dy} = -\frac{4k}{a^2} \frac{d}{dt} \left(\frac{d\beta}{dz} - \frac{d\gamma}{dy} \right),$$

an equation which is identical with the first equation (9); and both the others can be formed in a corresponding manner.

If now, by the aid of these equations,

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

be eliminated from equations (B), the first of these, after integrating in respect to t , will give

$$\frac{d}{dy} \left(\frac{d\alpha}{dy} - \frac{d\beta}{dx} \right) - \frac{d}{dz} \left(\frac{d\gamma}{dx} - \frac{d\alpha}{dz} \right) = -\frac{1}{4k} \frac{du}{dt} - 4\pi u,$$

which is identical with the first equation (8); and if here the last member, in accordance with (5), be put

$$-4\pi u = \Delta_2\alpha - \frac{1}{a^2} \frac{d^2\alpha}{dt^2},$$

and the designation $\bar{\Omega}$ be introduced, after another integration in reference to t we have

$$u = -2k \left(\frac{d\bar{\Omega}}{dx} + \frac{2}{a^2} \frac{d\alpha}{dt} \right).$$

Since we have $a\sqrt{2} = c$, we have returned to the first equation (A); and the two others may by analogy be deduced from this. The constants which should have been added in the two integrations in reference to t have been here omitted, because it is clear that such arbitrary constants would have no significance in the present case.

This result is a new proof of the identity of the vibrations of light with electrical currents; for it is clear now, not only that the laws of light can be deduced from those of electrical currents, but that the converse way may be pursued, provided the same limiting conditions are added which the theory of light requires. Thus we are in a position to deduce by calculation alone the inducing action of free electricity as defined by Kirchoff's equations (2), as well as the inducing action of variable electrical currents, both of which are contained in equations (A), by simply starting from those facts which are necessary to deduce the laws of light, and afterwards adding a single member to the differential equations found between the so-called components of light. This member expresses in a correct manner the absorption of light in good conductors of electricity, and disappears for perfectly transparent bodies.

Without dwelling more minutely on the consequences of the results obtained here, which manifestly lead us a step further towards developing the idea of the unity of force, and open a fresh field for future inquiries, I shall in conclusion call attention to the conclusions which we are entitled to draw with some degree of probability as to the mode of action of light, and how we are placed as regards the physical hypotheses concerning light.

Were we to attempt to represent the laws of electrical currents in such a manner that they would be generally valid for given *heterogeneous* bodies, and not merely for homogeneous bodies with constant conducting-power, this would appear to be best effected by starting from the differential equations, and regarding a and k as variable magnitudes. This would be more especially in agreement with the general equations found in the theory of light for heterogeneous media; besides, those limiting conditions which must be fulfilled for homogeneous bodies would then be contained in the differential equations and could be deduced from them. In this manner, however, a form corresponding in simplicity to the equations (A) could not be attained for heterogeneous bodies; and this must then be considered a special case obtaining alone for homogeneous bodies, while the differen-

tial equations would remain the original and only valid ones on which the physical explanation would have to depend. The theoretically important conclusion would thence follow which has been already indicated, that electrical forces require time to travel, and that these forces only apparently act at a distance (as would follow from equations (A) if they were regarded as the fundamental equations), and that every action of electricity and of electrical currents does in fact only depend on the electrical condition of the *immediately surrounding* elements, in the manner indicated by the differential equations (B). This is well known to be an idea indicated by Ampère, and which several physicists, more particularly Faraday, have defended.

The present general opinion regards light as consisting of backward and forward motions of particles of æther. If this were the case, the electrical current would be a progressive motion of the æther in the direction of the (positive or negative) electrical currents. But it is impossible that the same equations which theory deduces for very small displacements from equilibrium should hold good for all kinds of displacements whatever; and it just follows from the whole of this investigation that the same equations hold for both cases. Light cannot, therefore, consist of vibrations of the kind hitherto assumed; and this last consequence of the theory of æther makes it untenable.

There is, on the other hand, another conception of the nature of light-vibrations, to which I have already adverted*, and which perhaps now becomes more probable. For if we suppose light to consist of *rotating* vibrations in the interior of bodies, about axes which, according to the theory of electricity, we regard as directions of vibration, the electrical current is no translatory motion, but a rotation continued in one direction, and the axis of rotation becomes then the direction of the current. This rotation will only be continuous in good conductors, and the motion travel there in the direction of the axis, whereas it becomes periodical in bad conductors, and is propagated by what in electricity we call induction, in a direction at right angles to the axis of rotation. In this idea there is scarcely any reason for adhering to the hypothesis of an æther; for it may well be assumed that in the so-called vacuum there is sufficient matter to form an adequate substratum for the motion.

This hypothesis as to the nature of light and of electrical currents will probably, as science progresses, either assume a new form, or be totally rejected. But the result of the present investigation, that the vibrations of light are electrical currents, has been obtained without the assumption of a physical hypothesis, and will therefore be independent of one.

* Pogg. *Ann.* vol. cxviii. p. 113; *Phil. Mag.* S. 4. vol. xxvi. p. 82.

XXXIX. *On the Spectrum of the Bessemer-flame.*
By Professor LIELEGG*.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

TWO papers have been communicated to the Vienna Imperial Academy by Professor Lielegg, of the National Upper Practical School at St. Pölten, on the Spectrum of the Bessemer-flame, which contain an interesting examination of that spectrum, and therefore of that produced by the gas known as carbonic oxide, to the combustion of which the flame owes its origin. I beg to offer you the enclosed translations of the notices of those papers.

I am, Gentlemen,

Your obedient Servant,

W. T. LYNN.

Greenwich, September 21, 1867.

The flame which issues during a "charge" from the Bessemer-oven shows, when it is examined with a spectral apparatus of even the simplest kind, different bright lines which distinctly stand out from the continuous spectrum, which forms, as it were, the background.

Besides the lines due to sodium, lithium, and potassium, which are visible at the end of the period of slag-formation, groups of lines make their appearance during the combustion-period, which attain their greatest intensity of light at the commencement of the cooling-period. They extend from the sodium-line up to the blue strontium-line, or even a very little beyond it, and divide this space into four fields of equal size. The end of the first field, which is situated immediately close to the sodium-line, is indicated by a bright yellow line; but no other lines can be perceived in it, by reason of the extraordinary brilliancy of the light. The second field, in contact with this, lies in the greenish-yellow part of the spectrum, and contains, in its more distant half, three equally broad greenish lines, of which the third is the brightest, and defines the termination of the field. The third or following field contains four greenish-blue lines, of which the last but one is the brightest, and the last bounds the field; the lines are equally distant from each other, and occupy two-thirds of the field, so that between the third line of the second field and the

* Read at the Meeting of the Mathematical and Natural-Science Class of the Imperial Academy of Sciences at Vienna, January 31, 1867. Translated by W. T. Lynn, B.A., F.R.A.S.

first line of the third field an interval is left of the breadth of a third part of the whole. In the fourth field there are visible at nearly equally distant places four blue lines of equal breadth and brightness: in the violet part, with the exception of the potassium-line $K\beta$, no lines were observed. At the period of great vividness of the spectrum, the spaces between the lines of the third and fourth field appeared dark, and presented the appearance of absorption-streaks, the origin of which would moreover be explicable in the Bessemer-flame. On the other side of the sodium-line, nearly in the position of the orange-red calcium-line $Ca\alpha$, were visible two lines lying near together and not sharply bounded, which presented the appearance as if a broad bright streak were divided into two parts by a dark band in the middle.

At the end of the cooling-period the intensity of light of the groups of lines decreased, and shortly before the end of the "charge" all the lines of the third and fourth fields were no longer to be seen; the spectrum had nearly the same character as at the beginning of the combustion-period.

As the Bessemer-flame is formed principally by means of carbonic oxide, the groups of lines here described are also to be referred to this, their regular appearance during the combustion-period indicating the beginning of the real decarbonization; their increase in intensity up to the commencement of the cooling-period, and their remarkable fading off at the end of that period, ought to furnish appropriate ground to work upon for an investigation of the nature of the Bessemer-process.

These observations were made at the Bessemer-works of the Imperial Süd-Bahn Gesellschaft at Graz, permission to make use of which was most kindly granted to the author of this communication by the Director, Herr Hall.

Second Paper.*

This paper is an extension and completion of the previous one. It contains an accurate description of the spectrum of the Bessemer-flame, which consists of several groups of lines and isolated lines, the relative position of which was determined by measurement with a scale.

As the Bessemer-spectrum owes its origin to the combustion of carbonic oxide gas, it must be looked upon as also the spectrum of that gas; and as a spectrum of the flame of this has been hitherto unknown, it follows that a gap in the series of gaseous spectra is here filled up.

* Read June 6, 1867.

The appearance and disappearance of the lines is in close connexion with the periods of the "charge:" the commencement of the combustion-period, in which the decarbonization of the iron begins, as well as the end of the decarbonization, can be accurately determined by means of the spectral apparatus. But the occurrence of a group of lines and of an isolated line in the blue-violet part of the spectrum during the cooling-period, by which a particular state of this is indicated, must especially be noticed; and as these lines also again disappear before the others, their appearance and their disappearance, which invariably take place in the last five minutes of the charge, must acquire significance as an indication of its termination.

XL. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 241.]

June 20, 1867.—Lieut.-General Sabine, President, in the Chair.

THE following communications were read:—

"On the Physical Constitution of the Sun and Stars." By G. Johnstone Stoney, M.A., F.R.S., F.R.A.S.

An attempt is made in the memoir of which this is an abstract to take advantage of the insight we have gained within the last few years into the molecular constitution of gases, and the laws which regulate the exchanges of heat that take place between bodies placed in presence of one another, and to apply these new materials to the interpretation of the phenomena of the photosphere of the sun, the appearances presented during total eclipses, and the information about both sun and stars given by the spectroscope.

In an inquiry like this, where we are obliged to put up with such proofs as the materials at our disposal can supply, we must be content to accept results of every variety of probability, from that degree, bordering upon certainty, which commands an unhesitating assent, to that of which the chief scientific value is that it prompts to further investigation and points out a path. Those who read the memoir itself will best judge of the probability of each conclusion from the proofs laid before them; but in this sketch of its contents it may not be useless to indicate what is the value put upon each result by the author, since the proofs must in many cases be entirely omitted. It will be convenient to do this by numbers.

The probability 4, then, is to be understood to imply that the matter in hand appears to the author to be fully made out. He would, for example, assign this probability to the wave-theory of light, and to the main features of the theory of the molecular constitution of gases which have been worked out by Clausius and others within the last twenty years. The number 1 will be used where an hypothesis agrees so well with such of the phenomena as are known, that it is concluded that it must either be the true account of them or bear

some intimate relation to the true theory; 2 will indicate that we have good ground to conclude our hypothesis to be the true theory, although at the same time the evidence is too scanty or conflicting to free us from hesitation; 3 will indicate a proof so strong that we should be very much surprised if anything were eventually to disturb it; 4, as has been already stated, will mark a conclusion fully made out; and, to complete the series, 5 may be used for that demonstrative proof of which few subjects of inquiry are susceptible.

Observations with the spectroscope have made known to us that the sun's outer atmosphere (that is, the part of the atmosphere which extends outside the photosphere) is a mixture of many gases, amongst which hydrogen, sodium, magnesium, calcium, chromium, manganese, iron, nickel, cobalt, copper, zinc, and barium—all of them permanent gases in consequence of the temperature—have been detected. Now it is shown to be a necessary consequence of the molecular constitution of gases, that in such an atmosphere, decreasing in temperature from within outwards, the various constituent gases are not everywhere equally mixed, but that in the upper regions those which have the lightest molecules rise the furthest, so that the gases overlap one another in the order of the masses of their molecules (probability 5). It also follows from a consideration of the vapour-densities and atomic weights of the chemical elements, with probabilities which range from 4 to 1, that those which are present in the sun's atmosphere have molecules with masses increasing in the order in which their names have been printed above, the molecules of hydrogen being the lightest. This, then, is the order in which the boundaries of these gases would be met with in descending from the surface of the sun's atmosphere downwards.

This result is abundantly confirmed, and in its main features raised to probability 4, by observations with the spectroscope. Each constituent of the solar atmosphere is opaque to those rays which it emits when incandescent, and which constitute its spectrum. In this way all the light of these particular wave-lengths which has been emitted, either by the photosphere, or by the lower and more intensely heated strata of a gas in the solar atmosphere, is stopped in its passage outwards, and the gas substitutes for it the much more subdued light which emanates from its own upper and therefore coolest stratum. Now, if the view enunciated in the last paragraph be true, these outer layers of the respective gases, from which the rays as we see them come, must be at very various temperatures, that of hydrogen being the coldest, and the others in order after it. This is precisely in conformity with the observations. The rays of hydrogen, sodium, and magnesium emanate from a region so cold that the lines of these elements in the sun's spectrum are intensely black in whatever part of the spectrum they may occur; in other words, the light proceeding from the upper layers of these gases is so feeble that it is not in any perceptible degree luminous when placed in contrast with the intense background of light from the photosphere. On the other hand, calcium, iron, and the rest, while they produce only black lines in the violet and indigo, give rise to lines which are

sensibly less dark in the blue, and to lines which emit a still more considerable amount of light in the green, yellow, orange, and red, those colours in which a body gradually heated begins to glow.

A detailed scrutiny of the lines emitted by the various gases leads to several interesting results. Hydrogen and iron are the two most abundant constituents of the sun's outer atmosphere, and play in it the same part which nitrogen and oxygen do in the earth's. There is but the merest trace of sodium present. The other gases are met with in intermediate quantities. Again, barium cannot have a vapour-density so high as would appear at first from its atomic weight, and therefore probably belongs to the same class of elements as cadmium and mercury, which have vapour-densities half of what correspond to their atomic weights. To these several results we may attribute the probability 3.

The photosphere consists of two strata which may be distinguished. The outer of these is shown to be cloud in the ordinary sense of the word—that is, solid or liquid matter in a state of minute division, and denser than the part of the atmosphere in which it is dispersed (probability 3). This cloud is precipitated from its vapour by the chill produced by its own abundant radiation towards the sky, a chill which constitutes the shell of clouds a surface of minimum temperature considerably cooler than either the layer above it or the layer beneath (probability 3). The hotter layer which is outside the luminous clouds seems to have a depth somewhat greater than the length of the earth's radius (probability 2). Just outside it there is a second layer of luminous clouds, but so excessively thin that they can be seen only during a total eclipse, on which occasions a portion of them has been seen under the form of two arcs of cloud extending for some distance on either side of the points of first and last contact, where alone a sufficiently low part of the sun's atmosphere was disclosed (probability 3). Above these there soar other clouds raised by causes which will be referred to further on.

About the middle of the hot stratum over the photosphere there is a surface of maximum temperature, outside which the temperature decreases almost continuously to the limit of the iron atmosphere. A little outside this there is a second very feeble maximum, the temperature of which falls short of the heat of the flame of a Bunsen's burner; and outside this, through the immense height which is tenanted by sodium, magnesium, and hydrogen alone, the temperature goes on decreasing till it becomes excessively cold. These results are made out with probabilities 2 and 3.

Within the luminous clouds the temperature very rapidly waxes, and the density, too, appears to receive a nearly sudden increase. All gases with a vapour-density more than about eighty times that of hydrogen are imprisoned within the shell of clouds by the comparative chill which there prevails, cooperating with the intensity of the force of gravity exerted by the sun. Between the film of clouds and the stratum immediately beneath there are violent motions of convection, which both carry up fresh vapour to be condensed into cloud, and carry down the cloud into a region where it becomes mist and rain. It

is convenient to restrict the word *cloud* to cloud in that situation in which it can form, giving the names mist or rain to the cloud when carried down, either by currents of convection or by subsidence, into a position from which there is not that abundant radiation towards the sky which is essential to its forming. The clouds, in this restricted sense of the term, are everywhere of a gauze-like transparency, to admit of the copious radiation towards the sky which is requisite; and this enables spectators upon the earth to see through them the light emitted by the mist and rain beneath. This mist and rain seem everywhere, except in the solar spots, to be dense enough to be opaque, and therefore emit the maximum light corresponding to their temperature. This temperature is higher than that of the clouds; and accordingly the mist and rain constitute a background brighter than the luminous clouds.

Hence the finely-granulated appearance of the surface of the sun,—the currents of convection creating a kind of honeycombed structure in the stratum of clouds; the ascending currents carrying up hot vapours in which only excessively thin cloud can form, since under these unfavourable circumstances its lowest parts cannot tolerate even the slight obstruction to their radiating freely which a cloud of the average density would offer; and, on the other hand, the descending currents carrying down those portions which by prolonged radiation have cooled down abnormally, and thus become both more opaque by the condensation of more cloud, and less bright. Those portions which by the most persistent radiation cool down the most, seem to furnish the very dark specks which have been taken notice of by observers.

Hence also arises the gradation of light which is observed upon the sun's disk. In the middle of the disk we look vertically through the honeycombed structure which has been described, and see through it the brighter background almost without any intervening obstruction. But as we turn our eyes towards the margin of the disk, we look more and more obliquely across the columns, which progressively intercept increasing quantities of the brighter light from beyond, and substitute for them their own feebler radiations.

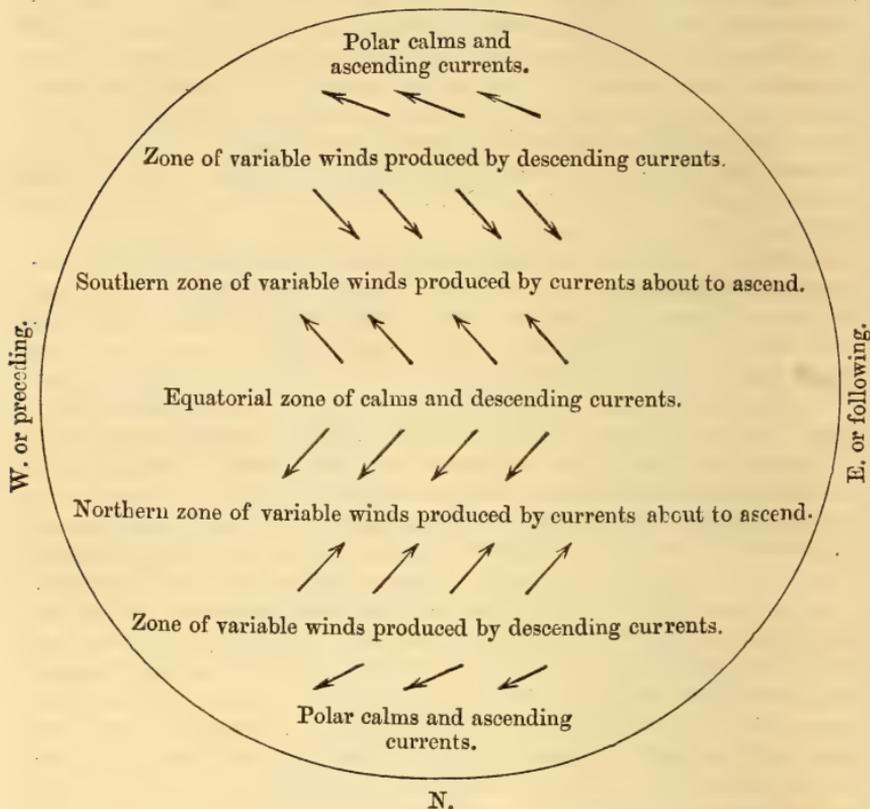
If by disturbances in the atmosphere the hotter stratum on either side is made in certain places to encroach upon the luminous clouds, they are unable to maintain in this situation as low a temperature as elsewhere, and therefore become abnormally thin. If this process is not carried so far as to put a stop to the incessant rain beneath the clouds, their increased transparency will give rise to a facula when the phenomenon takes place on a large scale, and to the coarsely mottled appearance of the photosphere when it presents itself in smaller patches. Hence we see why a facula retains its brightness up to the margin of the sun's disk, a phenomenon which is inconsistent with the usually received hypothesis that the gradation of light on the sun's disk is due to the absorption of the outer atmosphere. If the rain also cease we have the penumbra of a spot; if the cloud itself is dissolved away, we have its umbra.

The dark body which is disclosed in the umbra and penumbra of spots must be either an untarnished ocean of some highly reflecting

opaque substance, or a cloud of some transparent material which scatters light abundantly. Both hypotheses are fully considered.

To most of the foregoing conclusions relating to the photosphere and the adjoining parts we may safely accord the probability 3.

S.



We have strong reasons for suspecting that the luminous clouds consist, like nearly all the sources of artificial light, of minutely divided carbon; and that the clouds themselves lie at a very short distance above the situation in which the heat is so fierce that carbon, in spite of its want of volatility, and of the enormous pressure to which it is there subjected, boils. The umbra of a spot seems never to form unless when the region in which carbon boils is carried upwards, or the hot region above the clouds is carried downwards, so as to bring them into contact, and thus entirely obliterate the intervening clouds. It is, however, not safe to attribute to the results stated in this paragraph a probability of more than 1.

The trade-winds which blow over the surface of the photosphere are also inquired into. These seem to arise, as Sir John Herschel suspected, from the oblate form of the sun causing a difference in the escape of heat from his poles and equator. There are ascending currents at the poles, descending currents all round the equator.

This produces a region of equatorial calms bordered on either side by zones, in the northern of which south-east trades prevail, and in the southern, north-east. These are succeeded by variable winds in the regions of spots, beyond which the polar current blows over the surface of the photosphere in the form of a north-west trade in the northern hemisphere, and a south-west trade in the southern. In the region of spots, both the polar and equatorial currents make their way to a higher level, and in doing so heave up into a colder situation considerable portions of the upper layer of excessively thin cloud, that which is seen only during eclipses. This, though it may at first take place comparatively gently, will be succeeded by a violent upward motion, because the cloud when raised to a cool region will retain a temperature bordering upon that of the photosphere. When this occurs it will both produce the phenomenon of overhanging clouds seen during eclipses, and give rise to a violent cyclone in the regions beneath, immediately over the photosphere. There is no other part of the sun upon which these conditions prevail: hence the limitation of spots to two bands parallel to the equator. To these results we may assign the probability 2.

In the next branch of the inquiry we are obliged to have pretty free recourse to speculation; and the results, though there is much to be said for them, must be received with the caution which becomes us when we are not at liberty to award a probability higher than 1. We are forced to invoke an external agent to account for the periodicity of the spots; and that which is submitted as apparently the most probable, is a swarm of meteors like those which visit the earth in November every thirty-third year, but extended into a much longer stream. These while they pass through the sun's atmosphere would warm the upper regions above his equator, and thus tend to enfeeble the causes which produce the trade-winds. Hence upon each such visit, the trade-winds, the storms which result from them, and the spots which these occasion would all be moderated. It is remarkable that this hypothesis accounts also for the fact that spots prevail more in one hemisphere than the other, inasmuch as the meteors must act more on one hemisphere than the other, and lessen in it the causes that produce spots, unless we make the highly improbable supposition that the axis major of the orbit of the meteors lies just along the line in which its plane intersects the plane of the sun's equator. It is also very remarkable that the interval of time in which the spots go through their mutations, which we must of course adopt as the periodic time of the meteors in their orbit, assigns to them an aphelion distance outside and close to the orbit of one of the principal planets, Saturn. There is therefore very considerable ground to suspect that there is such a swarm of meteors which was diverted into the solar system by Saturn* at no very remote

* The attraction of Jupiter would also have been competent to divert a cluster of meteors into an orbit of the requisite form and dimensions; but the situation of the orbit would in that case have caused the meteors to cross the path of Jupiter, so that the planet would have acted ever since as a powerful dispersing agent; and it does not seem likely that such an influence has been in operation.

epoch—just as our November meteors were brought in by the planet Uranus in the year 126 of the Christian era.

Finally it is shown that an hypothesis which has found much and deserved favour of late years, that the heat expended by the sun is continually restored to him by the falling in of meteors which had been circulating round him, is no longer tenable.

The second part of the memoir treats of other stars. The differences in their appearances are found to depend mainly on differences in the force of gravity exerted at their surfaces. Where gravity on a star is feebler than on the sun, either from the mass of the star being less, or from its being so dilated by heat that its outer parts are further removed from its centre, gases which by reason of the mass of their molecules are imprisoned within the photosphere of the sun, will, when less attracted downwards, be able to stand the coolness of the shell of clouds and pass beyond them. Thus mercury, antimony, tellurium, and bismuth, all of which have too high a vapour-density to exist in the sun's outer atmosphere, show themselves in that of Aldebaran. Again, in these stars all the gases of the outer atmosphere expand until their upper layers, those from which their spectral lines issue, are cooler than on the sun. These spectral lines will accordingly be darker than on the sun, and as this will tell with most effect on the blue end of the spectrum, it will render the light from these stars ruddy.

On the other hand, those stars which, either from being of greater mass than the sun, or from being less hot in their internal parts, attract down the gases of their outer atmospheres with more force, constitute the class of intensely white stars with a somewhat violet tinge, of which Sirius and α Lyræ are examples. Several of the substances which in the sun's spectrum give rise to faint lines, are on such stars confined within the photosphere; and the lowest temperature which others of them can withstand is, by reason of the force with which they are attracted downwards, hotter than the corresponding temperatures of the sun. Hence the substances which on the sun cause his numerous dark lines—sodium, magnesium, calcium, chromium, manganese, iron—produce, in the spectrum of the star, lines equally numerous, but faint. There is but one exception to this. Hydrogen has a molecular mass so amazingly low (one twenty-third part of the mass of molecules of sodium, the nearest to it in this respect of the known constituents of stellar atmospheres), that there is probably no star which can exert a force of gravity so powerful as to compel hydrogen to limit itself to temperatures which show in any part of the spectrum a perceptible degree of brightness when placed upon the background of the photosphere. In all stars accordingly in which hydrogen appears at all, the four hydrogen lines are found intensely black.

We see, then, why solitary stars are found of some particular colours only. Stars which exert upon their outer atmospheres a force of gravity as great or greater than the sun's are white: those on which gravity is a less force are of some ruddy tint,—yellow, orange, or red. The foregoing results are adjudged to be of probability 4, that is, fully made out.

Those stars in which the force of gravity is *very much* less than on the sun appear to form a distinct subclass. The four hydrogen lines are not found in them, and at the same time new spectral lines, arranged in bands each of which is closely ruled and fades off on the less refrangible side, make their appearance. May we not here venture the suspicion that when gravity upon a star is below a certain limit, such conditions prevail as compel the hydrogen, which would otherwise be free, to enter into combination with some other element of low vapour-density, and that the resulting compound emits that spectrum of the First Order, as Plücker has called it, which we see?

To account for the colours of the companions of double stars we are again forced to enter upon speculative ground. If the sky be peopled with countless multitudes of dark stars, which as well as the small number that are visible, move only in virtue of their mutual attractions, it cannot be an absolutely unusual occurrence for two stars to come into collision. Whenever this happens, either the two stars emerge from the frightful conflagration which would ensue as one star, or, if they succeed in disengaging themselves, they will be found after the catastrophe moving in new orbits. If their previous courses had been parabolic, it can be shown that the new relative orbit will be elliptic. Hence they will return to the charge again and again, and at each perihelion passage there will be a fresh modification of the orbit. It is shown that these modifications will in some instances be such that the perihelion distance will be constantly on the increase, so that the stars will, in their successive perihelion passages, climb as it were asunder through one another's atmospheres. And the distance to which they will ultimately withdraw before they separate will of necessity be immense, since their atmospheres must have been dilated to a vast size by the friction to which they have been subjected. As the stars recede from one another the amount of heat which they generate at each perihelion passage is progressively less and less, until at length the atmospheres of the stars shrink in the intervals between two perihelion passages more than they expand when the brush takes place. When this happens the final separation of the two stars is imminent, and a new double star is on the point of being permanently added to the sky.

The astonishing appearances witnessed last year in T Coronæ seem to receive an easy explanation upon this hypothesis. They are exactly what we should expect upon the occurrence of one of the last perihelion passages that take place before two stars which are in the state of transition into a double star finally separate. The outer parts of the atmospheres becoming engaged would raise to incandescence the region in which hydrogen only is found, thus transforming what had previously been its four dark lines into intensely bright lines. At the same time the strata that lie further down would be very sensibly heated, though not to incandescence—quite enough, however, to lessen temporarily in a very material degree the extent to which they at other times subdue the light of the photospheres. This extent would of necessity have been very great, inasmuch as the enormous dilatation of the atmospheres must greatly enfeeble the force of gravity upon the outer strata of both stars.

Again, it follows as a consequence of this hypothesis that the circumstances which most favour the formation of a double star are when the two bodies that come into collision are of nearly equal mass. Such cases must be rare; but when they do occur, there is a very high probability that the issue will be a double star. This appears to account for the fact that a very remarkable proportion of double stars have constituents of nearly the same magnitude.

Another consequence is that when the stars are very unequal the companion will, as it plunges over and over again through the atmosphere of the primary, be gradually deprived of several of its lighter gases; so that when it finally gets clear it will not emit the principal spectral lines of a solitary star, but others which emanate from denser gases. This probably accounts for the blue, violet, and green colours which are found in the minute companions of double stars.

Another consequence is that the orbits of double stars will almost always have a considerable ellipticity.

Another consequence is that the conditions are likely not unfrequently to arise which would separate the companion into two or more fragments, and that when this happens the separate pieces will pursue paths which are distinct from one another and not far apart. This seems to account for such systems as γ Andromedæ.

When the same conditions act with unusual violence they would probably break up the companion into numerous fragments; and it is remarkable that they would at the same time be likely to cause the primary to throw off a number of rings. The fragments and the rings would move all in the same direction and nearly in the same plane, and each fragment would rotate rapidly in the direction in which it revolves in its orbit. When the fragments, as must generally happen, are of inconsiderable mass, their orbits would be almost certain to degrade from ellipses into circles before they got quite clear of the primary. Some would probably be found, when this happens, at the distance of the rings, others within the surface of the primary, none beyond both. Those within the surface of the primary would fall into him and be lost. But one that lay within a ring would gather by its attraction the ring round itself, and so become covered with an immense atmosphere with which it would continue to rotate while advancing in its circular orbit. If this rotation were sufficiently swift, the new planet would throw off rings which might afterwards condense into satellites, with this peculiarity, that they would always keep the same face turned towards the planet, and revolve round it in the same direction and nearly in the same plane in which the planet revolves round its sun.

The speculative element in this hypothesis is so considerable that perhaps we may not prudently yield to it a probability higher than 1. But an hypothesis which carries up so many of the main phenomena of nature to a single source, and which only asks us to admit what is not antecedently improbable, that the number of incandescent stars is but a small proportion of all that exist, seems nevertheless to deserve to be stated.

“On the Chemical Intensity of Total Daylight at Kew and Pará in 1865-67.” By Henry E. Roscoe, F.R.S.

This communication contains the results of a regular series of measurements of the chemical action of daylight, carried out at the Kew Observatory, through the kindness of Dr. Balfour Stewart, according to the method described by the author in the *Philosophical Transactions* for 1864, p. 605. The observations extend over a period of two years, from April 1, 1865 to March 31, 1867. The second part of the communication gives the results of observations upon the Intensity of the Chemical action of Sunlight under the Equator, made at Pará in latitude $1^{\circ} 28' S.$ during the month of April 1866.

I. *Kew Observations.*

The Kew measurements do not profess to exhibit the changes in chemical intensity which occur from hour to hour; but they give, with accuracy, the mean monthly chemical intensity, showing the rise and fall with the changing seasons of the year, and they enable us to deduce the mean monthly and yearly chemical intensities at Kew for 1865-67.

Tables showing the daily mean chemical intensity obtained from the daily observations, according to the method described in the above-mentioned paper, are given. The first result which these observations yield is that the mean chemical intensity for hours equidistant from noon is constant; that is, the mean chemical intensities are equal for equal altitudes of the sun; thus the mean of all the observations made about $9^h 30^m$ A.M. corresponds with the mean intensity at $2^h 30^m$ P.M.

	Mean of Times of Observation.	Mean Chem. Intensity.
Mean of 552 Morning Observations in 1865-67.	... $9^h 41^m$	0.105
Mean of 529 Afternoon Observations in 1865-67.	... $2^h 27^m$	0.107.

Hence the author concludes that, when the disturbing causes of variation in amount of cloud &c. are fully eliminated by a sufficient number of observations, the daily maximum of chemical intensity corresponds to the maximum of sun's altitude. The author then shows, from measurements made at varying altitudes of the sun at Heidelberg and Pará, that the relation between sun's altitude and chemical intensity may be represented by the equation

$$CI_a = CI_0 + \text{const. } a,$$

where CI_a represents the chemical intensity at a given altitude (a) in circular measure, CI_0 the chemical intensity at the altitude 0, and const. (a) a number to be calculated from the observations.

The agreement of the chemical intensities as found at Heidelberg with the calculated results is seen in the following Table:—

Phil. Mag. S. 4. Vol. 34. No. 230. Oct. 1867. Y

Altitude.	Chemical Intensity.	
	Found.	Calculated.
7 15.....	0·050.....	0·050
24 43.....	0·200.....	0·196
34 34.....	0·306.....	0·276
53 37.....	0·437.....	0·435
62 30.....	0·518.....	0·506

A similar relation is found to hold good for the Pará observations. Assuming the same relation to exist at Kew as at Heidelberg and Pará, the values of the mean monthly intensity at noon have been calculated from the observations at 2.30 and 4.30 P.M., and the mean monthly integrals of chemical intensity for each month, from April 1865 to March 1867 inclusive, have been obtained. Curves exhibiting the daily rise and fall for each of the twenty-four months, as well as a curve showing the biennial variation of chemical intensity for the same period, accompany the paper. The curve of yearly chemical intensity is found to be unsymmetrical about the vernal and autumnal equinoxes; thus in spring and autumn the results are as follows:—

1865 & 1867.	Mean Ch. Int.	1866.	Mean Ch. Int.
March 1867	30·5	March	34·5
April 1865	97·8	April	52·4
September 1865	107·8	September.....	70·1
August 1865	88·9	August	94·5

Or for 100 chemically active rays falling during the months of March and April 1865, 1866, and 1867 at Kew there fell in the corresponding autumnal months 167 rays, the sun's mean altitude being the same.

The author discusses the probable causes of this autumnal maximum; he finds that it is not due to variation in the amount of cloud, and believes that it is to be explained by a less amount of atmospheric opalescence in the autumn than in the spring.

The yearly integral for the twelve months January to March 1867 and April to December 1865, is 55·7, whereas that for the twelve months of the year 1866 is 54·7.

II. Pará Observations.

All the knowledge we possess concerning the distribution and intensity of the chemically active rays in the tropics is derived from the vague statements of photographers. According to their observations it appears that the difficulty of obtaining a good photograph increases as we approach the equator; and more time is said to be needed to produce the same effect upon a sensitive plate under the full blaze of a tropical sun than is required in the gloomier atmosphere of London. Thus in Mexico, where the light is very intense, from twenty minutes to half an hour is stated to be required to produce photographic effects which in England occupy but a minute. Hence the existence of a pecu-

liar retarding influence has been suggested which the heating and luminous rays are supposed to exert upon the more refrangible portions of the spectrum. The fallacy of these statements has been fully proved by a series of direct measurements of the chemical intensity of sunlight under the equator, made at Pará by Mr. T. E. Thorpe. The curves of daily chemical intensity given in the paper show that the activity of the chemical rays in the tropics is very much greater—on one day fifty-five times as great as in our latitudes; and these measurements prove that the reported failures of photographers cannot at any rate be ascribed to a diminution in the chemical intensity of sunlight. The following numbers give some of the daily mean chemical intensities at Pará compared with the same days in Kew:—

Daily Mean Chemical Intensity.			
1866.	Kew.	Pará.	Ratio.
April 6.....	28·6	242·0	8·46
„ 7.....	7·7	301·0	39·09
„ 9.....	5·9	326·4	55·25
„ 11.....	25·4	233·2	9·18
„ 20.....	38·9	385·0	9·90
„ 24.....	83·6	362·7	4·34

The measurements were made at Pará in the middle of the rainy season, and at very frequent intervals during the day; the curves show the enormous and rapid variation in intensity from hour to hour which the chemically active rays undergo under a tropical sun during the rainy season.

“On the Identity of the Body in the Atmosphere which decomposes Iodide of Potassium with Ozone.” By Thomas Andrews, M.D., F.R.S.

It was assumed for many years, chiefly on the authority of Schönbein, that the body in the atmosphere which colours iodide-of-potassium paper is identical with ozone; but this identity has of late been called in question; and as the subject is one of considerable importance, I submitted it lately to a careful investigation, the results of which I beg to lay briefly before the Society. The only property of ozone, hitherto recognized as belonging to the body in the atmosphere, is that of setting free the iodine in iodide of potassium; but as other substances, such as nitric acid and chlorine, which may possibly exist in the atmosphere, have the same property, no certain conclusion could be drawn from this fact alone.

One of the most striking properties of ozone is its power of oxidizing mercury, and few experiments are more striking than that of allowing some bubbles of electrolytic oxygen to play over the surface of one or two pounds of mercury. The metal instantly loses its lustre, its mobility, and its convexity of surface; and when moved about, it adheres in thin mirror-like films to the sides of the containing glass vessel. The body in the atmosphere acts in the

same way upon pure mercury ; but, from the very minute quantity of it which is at any time present, the experiment requires some care in order that the effect may be observed. On passing a stream of atmospheric air, which gave the usual reactions with test-paper, for some hours over the surface of mercury in a U-tube, the metal was distinctly oxidized at the end at which the air first came into contact with it.

This experiment, however, cannot be considered conclusive, as mercury will tarnish and lose its mobility under the influence of many bodies besides ozone.

It is well known that all ozone reactions disappear when ozone is passed through a tube containing pellets of dry peroxide of manganese, or other body of the same class. The same thing occurs with the substance supposed to be ozone in the atmosphere. About 80 litres of atmospheric air were drawn, at a uniform rate, through a tube containing peroxide of manganese, and afterwards made to play upon very delicate test-paper. Not the slightest coloration occurred, although the same paper was distinctly affected when 10 litres of the same air, without the interposition of the manganese tube, were passed over it.

But the action of heat furnishes the most unequivocal proof of the identity of the body in the atmosphere with ozone. In a former communication (*Phil. Trans.* for 1856, p. 12) I showed that ozone, whether obtained by electrolysis or by the action of the electrical brush upon oxygen, is quickly destroyed at the temperature of 237° C. An apparatus was fitted up, by means of which a stream of atmospheric air could be heated to 260° C. in a globular glass vessel of the capacity of 5 litres. On leaving this vessel, the air was passed through a U-tube, one metre in length, whose sides were moistened internally with water, while the tube itself was cooled by being immersed in a vessel of cold water. On passing atmospheric air in a favourable state through this apparatus, at the rate of three litres per minute, the test-paper was distinctly tinged in two or three minutes, provided no heat was applied to the glass globe. But when the temperature of the air, as it passed through the globe, was maintained at 260° C., not the slightest action occurred upon the test-paper, however long the current continued to pass. Similar experiments with an artificial atmosphere of ozone (that is, with the air of a large chamber containing a small quantity of electrolytic ozone) gave precisely the same results. On the other hand, when small quantities of chlorine or nitric acid vapour, largely diluted with air, were drawn through the same apparatus, the test-paper was equally affected, whether the glass globe was heated or not.

From these experiments I consider myself justified in concluding that the body in the atmosphere, which decomposes iodide of potassium, is identical with ozone.

GEOLOGICAL SOCIETY.

[Continued from p. 71.]

April 17, 1867.—Sir Charles Lyell, Bart., M.A., F.R.S.,
Vice-President, in the Chair.

The following communication was read:—

“On the Physical Structure of North Devon, and on the Palæontological Value of the Devonian Fossils.” By Robert Etheridge, Esq., F.R.S., F.G.S., Palæontologist to the Geological Survey of Great Britain.

The Lower, Middle, and Upper groups of sandstones and shales of West Somerset and North Devon were described in this paper as occurring in a regular and unbroken succession from north to south, namely from the sandstones comprising the promontory of the Foreland, at the base, to the grits and slates &c. overlying the Upper Old Red Sandstone of Pickwell Down to the south. The author was unable to see any traces of a fault of sufficient magnitude to invert the order of succession, or that would cause the rocks of the Foreland at Lynton to be upon the same horizon as those south of a line of high ground that passes across the county from Morte Bay on the west to Wiveliscombe on the east.

The Foreland grits and sandstones are overlain by the Lower or Lynton slates, and form a group equal in time to the Lower Old Red Sandstone of other districts, but deposited under purely marine conditions.

The author then showed that above the Lower or Lynton slates there is an extensively developed series of red, claret-coloured, and grey grits, from 1500 to 1800 feet thick; these form a natural and conformable base to the Middle Devonian or Ilfracombe group. The highest beds, containing *Myalina* and *Natica*, insensibly pass into the gritty and calcareous slates of Combe Martin, Ilfracombe, &c.; this Middle group Mr. Etheridge unhesitatingly regarded as the equivalent of the Torquay and Newton Bushel series of South Devon.

Mr. Etheridge gave detailed Tables of the organic remains of the two groups (the Lower, or Lynton, and the Middle or Ilfracombe), and collated with them those species found in equivalent strata in Rhenish Prussia, Belgium, and France. He was inclined to believe that these two marine fossiliferous groups represent in time the unfossiliferous Old Red Sandstone (Dingle beds) of Kerry, and the Glengariff and Killarney Grits of the south-west of Ireland.

The author then endeavoured to prove that the Pickwell Down beds are the true *Upper* Old Red Sandstone only, not the whole of the formation, as was lately proposed.

Arguments were also brought forward to show the probability of the Carboniferous slate (in part) and Coomhola grits being the equivalent of the English Upper Old Red Sandstone, or Upper Devonian, and that the North Devon beds only are to be regarded as the true type (to which the Irish must be compared), and not *vice versa*.

Physical and Palæontological evidence distinctly proves, the author stated, that the whole of the slates and limestones of Lee, Ilfracombe, and Combe Martin underlie the Morte Bay red sandstones.

The author compared the whole of the Devonian fauna of Britain with that of the Rhine, Belgium, and France, by means of a series of Tables based upon the British types. These marine Devonian species were compared with those of the Old Red Sandstone proper, the Silurian, and Carboniferous, and analyses were made of all the classes, orders, genera, and species, with relation to the groups of rocks in which they occur—the result being the conclusion that the marine Devonian series, as a whole, constitutes an important and definite system.

May 8, 1867.—Warrington W. Smyth, Esq., M.A., F.R.S.,
President, in the Chair.

The following communications were read:—

1. “On new specimens of *Eozoon*.” By Sir W. E. Logan, F.R.S., F.G.S.

Amongst several additional specimens of *Eozoon* which have been obtained during recent explorations of the Canadian Geological Survey, is one which was found last summer by Mr. G. H. Vennor in the township of Tudor, county of Hastings, Canada West. It occurred on the surface of a layer, three inches in thickness, of dark-grey micaceous limestone, or calc-schist, near the middle of a great zone of similar rock. This Tudor limestone is comparatively unaltered; and in the specimen obtained from it the skeleton of the fossil, consisting of white carbonate of lime, is imbedded in the limestone without the presence of serpentine or other silicate, a fact which the author regarded as extremely favourable to the view of the organic origin of *Eozoon*. Sir William Logan also described the nature and relations of the rocks of other localities which have recently yielded *Eozoon*, especially Wentworth, Long Lake, and Côte St. Pierre.

2. “Notes on Fossils recently obtained from the Laurentian rocks of Canada, and on objections to the organic nature of *Eozoon*.” By J. W. Dawson, LL.D., F.R.S., F.G.S.

The first specimen described in this paper was the one from Tudor referred to in the previous communication. Its examination had enabled Dr. Dawson to state that in it the chambers are more continuous, and wider in proportion to the thickness of the septa, than in the specimens found elsewhere, and that the canal-system is more delicate and indistinct. Without additional specimens the author could not decide whether these differences are of specific value, or depend on age, variability, or state of preservation; he therefore referred the specimen provisionally to *Eozoon Canadense*, regarding it as a young individual, broken from its attachment and imbedded in a sandy calcareous mud. Its discovery afforded him the hope that the comparatively unaltered sediments in which it has been pre-

served, and which have also yielded worm-burrows, will hereafter still more largely illustrate the Laurentian fauna. After giving short descriptions of new specimens from Madoc and from Long Lake and Wentworth, Dr. Dawson discussed the objections of Prof. King and Dr. Rowney to the view of the organic nature of *Eozoon*, and stated that those gentlemen had failed to distinguish between the organic and the crystalline forms, as was especially illustrated by their regarding the veins of crysotile as identical with the tubulated cell-wall of *Eozoon*.

3. "On Subaërial Denudation, and on Cliffs and Escarpments of the Chalk and the Tertiary strata." By W. Whitaker, Esq., B.A., F.G.S.

From the fact that escarpments differ from cliffs in all their chief features, the author inferred that the two could hardly result from the same action, but that, whilst the latter were made by the sea, the former seem to have been cut out by subaërial agents.

The chief contrasts between the two kinds of ridges are :—

(1) Escarpments always run along the strike. Cliffs rarely do so.

(2) The bottom of an escarpment is not at one level throughout. That of a sea-cliff is.

(3) At the foot of an escarpment one does not find a beach or other trace of the action of the sea, but often such débris as would be left by a slow and quiet denuding power.

(4) Two escarpments facing the same way often run near and parallel to one another for many miles. Not so with cliffs.

(5) The ridge of an escarpment is a nearly even line, and forms the highest ground of the neighbourhood. The top of a cliff is often very uneven and bordered by higher ground.

From an examination of escarpments of the Chalk and of the Tertiary beds, it was shown that though at first sight they might seem like old lines of cliff, yet a little study would destroy the fancied likeness, and it would be found that they are quite unlike cliffs *in the same beds*; for though, from their winding outline, these ranges of hills might remind one of some irregular coast, caused by rocks of different hardness wearing away at different rates, they have little in common with the far more even coast that is formed where there is but one kind of rock.

It was then pointed out that along the present coast the sea is not the only force engaged in the work of destruction, but that it is largely helped by atmospheric agents (the latter acting from above downwards, to detach and hurl down masses of rock, which the former, acting horizontally below, pounds down and sweeps away); and it was inferred that the joint action of the two kinds of force had a far greater effect than either alone.

In conclusion it was argued that, as deposits of great thickness (such as the Wealden beds) had been made by rivers, it must be allowed that (allowing for waste) still greater masses of rock had been destroyed by streams and by subaërial actions generally. The denuding power of the sea, however, was by no means denied, but

it was allowed that, as marine deposits much exceed in quantity those of freshwater origin, so the great denudations, the planings-down of vast tracts of which examples are given by unconformities, have been worked out by the action of the sea; but it was maintained that, on the other hand, the far smaller denudations and comparatively trifling irregularities of the surface (our hills and valleys) have been worn out by the long-continued action of rain, rivers, and ice.

May 22, 1867.—J. Carrick Moore, Esq., M.A., F.R.S.,
Vice-President, in the Chair.

The following communications were read:—

1. "On the Bone-caves near Crendi, Zebbug, and Melheha, in the Island of Malta." By Captain T. A. B. Spratt, R.N., C.B., F.R.S., F.G.S.

The Crendi (or Mahlek) Cavern is situated on the south coast of Malta. The flooring consisted of two distinct deposits, the lower being a stratum composed of a hard stalagmitic clay with rounded pebbles, and containing teeth and bones (unworn) of the *Hippopotamus* (*H. Pentlandi*). The upper stratum, also a stalagmitic deposit, contained bones of the *Myoxus Melitensis*, and of Birds, with some recent Land-shells.

The Zebbug Cavern, in the interior of the island, was, when discovered, filled with sandy clay containing subangular fragments of the rock, and bones of at least two species of Elephant, comprising a complete set of the teeth and tusks of the pigmy elephant, representing animals in every stage of growth, and part of the tusk of a much larger elephant. No remains of *Hippopotamus* were met with in this cavern; but a few bones of *Myoxus* (2 spp.), of Birds, and of a Chelonian were discovered in it.

The Melheha Cavern, at the north end of the island, contained a deposit with remains of the teeth and bones of the *Hippopotamus* only, and seemed therefore to represent the lower stratum of the Crendi Cavern.

From the fact that the deposits containing remains of the *Hippopotamus* were so distinct from those including the Elephant-remains, Captain Spratt inferred that these two mammals belonged to distinct geological epochs, the Elephant being the more recent.

As Malta and Gozo were probably elevated above the sea at the close of the Miocene period, it is very possible that the caverns, formed by the long action of the sea upon its cliffs, may contain the relics of animals of more than one, if not of each, subsequent geological period.

In respect to Dr. Leith Adams's discovery of remains of the Elephant in scattered débris of subangular fragments and red earth, filling fissures and hollows in various parts of Malta, Captain Spratt stated his opinion that the phenomena were produced by a "wave of translation," resulting from a sudden subsidence of the island below the sea at a very recent geological period, of too short a duration, however, to allow of any purely marine deposit being formed.

Captain Spratt discussed the evidence, afforded by soundings, of the former connexion of Malta with Europe and Africa, by land that

must have served as a means of migration for the cave-fauna. The submerged lands are indicated by the "Adventure Bank," between Tunis and Sicily; and by the "Medina Bank," a ridge connecting the south-east of Malta with Tripoli. An upheaval of about 200 fathoms would render both banks dry, with the exception of one or two narrow channels, which would not impede the larger animals from passing from one continent to the other at the seasonable times for migration.

2. "On the Lower Lias of the North-east of Ireland." By Ralph Tate, Esq., A.L.S., F.G.S.

The author described the Lias of Ireland as consisting of (1) the *Avicula-contorta* series, including a well-developed zone of *Avicula contorta* and the White Lias; (2) the Lower Lias, embracing the equivalents to the zones of *Ammonites planorbis*, *A. angulatus*, and *A. Bucklandi* of Great Britain, and a fourth zone (that of *Belemnites acutus*), representing that portion of the Lower Lias superior to the zone of *Ammonites Bucklandi*.

Mr. Tate stated that the principal portion of the Lias belongs to the zone of *Avicula contorta*, and that the greater portion of the Lower Lias is comprised in the zone of *Ammonites angulatus*. The remarkable and isolated mass of metamorphosed Lias at Portrush was referred to the "Planorbis series."

3. "On the fossiliferous development of the zone of *Ammonites angulatus* in Great Britain." By Ralph Tate, Esq., A.L.S., F.G.S.

In this paper the author recorded the discovery of a fauna, hitherto imperfectly known in this country, characterizing beds below the Limestone-series of the Lower Lias. It is exceedingly rich in fossils. Cephalopoda are few in number (about 8 species); Gasteropoda are very numerous and characteristic, there being about 50 species, the majority of which are new to Great Britain; the Corals are abundant and peculiar.

Ammonites angulatus was stated to occur at various places in Ireland; at Marton, Lincolnshire; in Warwickshire; in North Gloucestershire; at Brocastle and Sutton, in Glamorganshire; and in Dorsetshire.

The author further communicated the results of a critical examination of the Sutton-Stone fossils, which are:—(1) that the majority of the species are well-known Liassic forms of the continent, and occur in other parts of Great Britain; (2) that they incompletely represent the fauna of the zone of *Ammonites angulatus*; and (3) that the Sutton Stone is lithologically and palæontologically identical with the "Calcaire de Valogne," the stratigraphical relations of which have been well determined.

4. "On the Rhætic Beds near Gainsborough." By F. M. Burton, Esq., F.G.S.

Beds of the Rhætic series were stated to occur at Lea, two miles to the south of Gainsborough, and were described as consisting of more or less indurated and highly micaceous sandstones, alternating with black shaly clays, and containing two bone-beds.

The fossils are very abundant, and are those which are usually met

with in the "Avicula-contorta" zone of other parts of Great Britain. However, the right *ramus* of the lower jaw of an *Ichthyosaurus* was found in the lowest bed, lying on the blue Keuper Marl. Two interesting additions to the vertebrate fauna of this series are *Trematosaurus Alberti*, and *Lepidotus Giebeli*?

The author pointed out the correlation of these beds with those at Aust Cliff and other well-known localities in England, and their probable connexion with similar deposits in Ireland and on the continent; and he concluded by defining the surface-extent of this the most northern English deposit of Rhætic age as yet discovered.

XLI. *Intelligence and Miscellaneous Articles.*

NOTE ON THE ELECTRICAL CONDITION OF THE TERRESTRIAL GLOBE. BY A. DE LA RIVE.

IT is at present generally admitted that in the normal condition the atmosphere is charged with positive electricity, and that this electricity increases from the ground (where it is almost zero) to the greatest heights which can be attained. The terrestrial globe, on the contrary, is charged with negative electricity, as is proved by a variety of observations, direct and indirect; it is, moreover, a consequence of the presence of positive electricity in the atmosphere; for one of the electricities cannot manifest itself in the free state without the appearance of an equal quantity of the other kind.

At the place where the atmospheric air and the solid or liquid surface of the terrestrial globe are in contact, there is a layer of air in the neutral state, the two electricities neutralizing one another there, seeing that the cause (probably subterranean) which disengages them necessarily acts without interruption. This neutralization is of course facilitated on plains above the sea by the moisture, always more or less considerable, with which the layers of air in contact with the soil are charged. This, however, is not the case on mountains, and especially on the higher peaks; the dryness of the air must render the combination of the two electricities more difficult, and enable the negative on the ground and the positive in the air to acquire a tolerably energetic degree of tension. This is shown on the one hand by the strong positive electricity which air has at these great heights, and on the other hand by the attraction which the mountains exert on the positive clouds of the atmosphere in virtue of their negative electricity.

Now, what would take place if a metal plate, sunk in the ground on the plain, were connected by means of a telegraphic wire with a similar plate immersed in the soil of a high place? If the negative electricity with which the two places are unequally charged were in an entirely statical condition, the metallic wire becoming an integral part of the conducting layer of air which separates them, no dynamic phenomena would be observed. But there is a continuous flow of the negative electricity of the soil towards the positive of the air, which produces the neutral layer; it follows necessarily that there is a downward transport of negative electricity, or, what

amounts to the same thing, an ascending current of positive electricity, in the conducting wire which joins two places at different heights above the sea-level.

This furnishes the explanation of a phenomenon frequently observed, and under conditions quite free from objection, by M. Matteucci. He observed that in every mixed circuit, consisting of a layer of earth and a metal wire the ends of which are sunk in the ground, minute precautions being taken to avoid any thermal or chemical action, the wire is traversed by an electrical current whose direction is constant whenever the soils in which the ends are plunged are of unequal heights. The current ascends on the metal wire; its intensity increases with the length of the wire, and with the difference in level of the ends. M. Matteucci has convinced himself that at the higher station there were very intense indications of positive electricity, while these signs were feeble, or were entirely wanting, at the lower station.

All this, then, agrees with the theory which I have laid down; but to confirm the exactitude of the considerations on which it is based, I have tried to reproduce the phenomenon observed by M. Matteucci by means of a laboratory experiment. With this view I placed on an insulating support a sphere of about 30 centimetres diameter, made of porous earthenware, or of wood covered with bibulous paper, so as to have, by wetting the surface, a moist conductor representing the earth. I fixed to the highest part of the sphere, in contact with its moist surface, a small metal disk; a second one was arranged in the same manner at a distance of from 50 to 90 degrees from the first. I then joined the two disks by the wire of a galvanometer; no current showed itself, either positively or negatively, even when the insulated sphere was positively or negatively electrified. By means of an insulated rod I then suspended, at a distance of 2 to 3 centimetres above the sphere, a plate slightly concave on the lower side, and of such dimensions that it only covered a small portion of the sphere—that, namely, in the midst of which was the upper metal disk, and therefore not the portion in which the other disk was placed. The apparatus being thus arranged, I charged the sphere representing the earth with negative electricity from a machine, the positive electricity of which was led to the concave metallic surface representing the atmosphere. The galvanometer quickly indicated the existence of a current, the direction of which was from the lower to the upper disk; this current was perfectly regular, and lasted as long as the machine was at work. It is to be observed that the upper disk was in that part of the sphere where most negative electricity was accumulated, under the influence of the insulated positive plate, while the second disk was in the part withdrawn from this influence—that, therefore, in which the quantity of negative electricity was small and flowed out, in proportion as it was produced, into the surrounding air. The current proceeded, therefore, in the wire which joined the two unequally electrified portions of the negative sphere from the least-electrified portion to that which was more so, exactly as is the case in the natural phenomenon ob-

served by M. Matteucci, of which the experiment just described is a faithful representation.

When I produced a series of discharges, by bringing the positive plate too near the negative sphere below, I saw the needle of the galvanometer deflect, sometimes in one, and sometimes in the other direction, making very irregular movements, instead of preserving the constant deviation which it experienced when there was no discharge. This is another faithful representation of what takes place in nature—M. Matteucci having observed that during storms the oscillations of the needle of the galvanometer are sudden and frequent, while on calm and clear days (the only ones on which the normal phenomenon could be observed) the deflection of the needle remains almost constant.

I do not dwell upon the other phenomena of the same kind observed by M. Matteucci, such as the presence of very feeble and variable currents in a wire terminating in two points of the surface of the earth situate on the same parallel and on the same level; points whose electrical condition should obviously be the same, or only slightly differ in either direction, owing to accidental circumstances. The slight currents from south to north, again, which M. Matteucci has observed in wires placed horizontally in the direction of the meridian, and having their ends constantly in the ground, are probably due to the fact that the negative tension of the earth, other things being the same, gradually increases from the equator to the poles, where it is a maximum. This increase is a necessary consequence of the influence which should be exerted on the negative electricity of the earth by the great quantity of positive electricity accumulated in the atmosphere near the poles, owing to the trade-winds. The discharges which take place between the contrary electricities in the polar regions where they are accumulated, give rise, as I have shown, to the polar auroras and the electrical currents which accompany them—currents more powerful than those of which we have spoken, and which differ from them both in intensity and in direction.

The electrical phenomena which occur at the surface of our globe and in our atmosphere are sufficiently complex. There is first of all a general fact—that is, the accumulation, owing to the trade-winds, in the atmosphere of the poles of positive electricity, with which the air of the equatorial regions is constantly charged by the particles of aqueous vapour which rise from the seas. The influence of this electricity accumulates and condenses near the poles a great proportion of the negative electricity which the solid part of the globe possesses, at the same time that it in turn is condensed. The more or less frequent discharges that take place between the condensed electricities through the atmosphere, give rise to the polar auroras, whose appearance is always accompanied by electrical currents circulating in the ground; these currents show themselves either by their action on compass-needles, or by their transmission through telegraph-wires.

But, besides the general and dominant fact which we have men-

tioned, there are a great number of partial and local facts, arising from unequal tension in the more or less variably distributed electricity, whether negative or positive, with which the terrestrial globe and its atmosphere are respectively charged. Such are those which M. Matteucci observed, and those to which I have alluded in this note; such, moreover, are the ordinary storms, and all the varied phenomena which accompany them. The attraction of clouds by mountains, and the phenomena of phosphorescence which they sometimes present, arise from the same cause; and it is probable that many other natural phenomena, waterspouts for example, have the same origin.—*Comptes Rendus*, June 10, 1867.

ON BREWSTER'S NEUTRAL POINT. BY PLINY EARLE CHASE.

In the April Number of the Philosophical Magazine Sir David Brewster says, "Dr. Rubenson has never been able to see, even under the fine sky of Italy, the neutral point which I discovered under the sun, and which, I believe, has never been seen by any other observer than M. Babinet."

The point in question can be easily seen in Philadelphia on any clear day when the sun is more than 20° above the horizon, and I have reason to believe that it can be found with equal ease at many other places in the United States, although I have not been able to find any published observations except my own.

As all the phenomena of skylight polarization are very interesting, and as some of its laws are still imperfectly understood, others may perhaps be induced to turn their attention in this direction, so as to determine whether the difficulty experienced by European observers is owing to a higher latitude, to a moister atmosphere, or to some other cause.

A simple Savart polariscope is sufficient for making the observations. In positing Brewster's neutral point I have usually raised the lower sash of an attic window so that the bottom of the sash would screen the sun from the polariscope. I have thus been able, in every instance when the atmospheric conditions seemed favourable, to see very distinctly the neutral point, and the oppositely polarized bands above and below.—Silliman's *American Journal* for July 1867.

NOTE ON THE FORCE WHICH THE MUSCLE OF A FROG CAN DEVELOPE IN CONTRACTING. BY M. J. ROSENTHAL.

The height to which a muscle can raise a weight depends, as is well known, on the length of its fibres. On the contrary, the force of contraction, which is measured by the weight necessary to prevent contraction, only depends on the extent of the transverse section of the muscle, or on the number of fibres which compose it.

M. E. Weber of Leipzig measured this force, and found it equal to about 600 grammes for the unit of transverse section—that is, for a square centimetre of muscle. M. Schwann also has shown that this force is not constant in all cases, but depends on the state of contraction of the muscle—that is, that this force, being greatest in the

normal condition of the muscular fibre, diminishes as the muscle contracts, and disappears when the muscle has attained its maximum contraction.

M. Weber also measured the absolute force of the gastrocnemian and solear muscles of man, and found it equal to about a kilogramme for each square centimetre of muscle. M. Henke of Rostock, however, found an error of calculation in M. Weber's numbers; having repeated the experiments with the flexor muscles of the forearm, he found a higher number (that is, 8 kilogrammes) for each square centimetre of human muscle.

In my researches on muscular contraction I was led to repeat M. Weber's researches on the absolute force of the muscles of the frog. I used a method which is more completely free from the errors produced by the fatigue of the muscle. I have thus found values greater than those of M. Weber. The method is as follows:—

Imagine the muscle fixed at the upper end by a clamp so strong as not to yield to weights even heavier than those we use; we suspend to the lower end of the muscle a lever so light that its weight may be neglected. The axis of rotation of the lever is horizontal, and situated at one end; at the other end is a platinum wire resting on a plate of the same metal, and allowing the current of a Daniell's battery to pass round an electromagnet. The muscle being fixed in the middle of the lever, the clamp which supports it may be raised until the elasticity of the muscle just brings the lever in contact with the platinum plate. In the middle of the lever, just at the point at which the muscle is fixed, is suspended the plate of a balance, on which are placed the weights which are to measure the force of contraction. These weights do not stretch the muscle; for the lever rests on the platinum plate; but during contraction the muscle is compelled to raise these weights. Thus by gradually increasing the number of these weights a sufficiently great value is obtained to prevent contraction. So long as the weight has not reached this value, the muscle breaks the circuit at each contraction. When the current is broken, the contact of the electromagnet is withdrawn by a spring and strikes on a bell. The weight is thus easily found by which the current is not broken—that is, the weight sufficient to neutralize the motion produced by the contractile force of the muscle.

The absolute force of the muscle being thus found, it is necessary to measure its cross section; and this is effected by dividing the weight of the muscle by the length of the fibres multiplied by the specific gravity of muscle. My experiments have always been made with the muscles of the frog's thigh, which form a regular mass with parallel fibres. They yield better results than those used by M. Weber; for they present a greater mass, and withstand fatigue better. I found in this manner that the force of contraction of each square centimetre of frogs' muscle varies between 1·8 and 3 kilogrammes, a value far higher than that of M. Weber.

The absolute force of the gastrocnemian muscle of a frog of mean size varies between 1000 and 1200 grms. This enormous number is intelligible if we consider that the cross section is very large in proportion to its volume. We see thus that muscles are very perfect

machines, which, in proportion to their relatively small weight, develop a far greater force than machines constructed by human ingenuity.—*Comptes Rendus*, June 3, 1867.

INVESTIGATIONS ON THE ABSORPTION OF OBSCURE HEAT.

BY M. P. DESAINS.

On May 27 I had the honour of laying before the Academy the results of a series of experiments on the absorbing action which several volatile liquids and their vapours exert on the heat from a lamp with a glass chimney.

These experiments show that, in the case of the heat in question and the substances with which I worked, the absorption obeys a very simple law—that a column of any of these liquids of a given section and weight exerts an absorption which is independent of the physical condition of the medium. The liquid column is very short, and the gaseous one very long; but they produce the same effect.

It seemed important to ascertain whether this equality exists when the rays are far more absorbable than those of a lamp with a double draught, which have already passed through a considerable thickness of glass—whether it obtains, for instance, in the case of the radiation from a plate of copper heated to about 400° .

But here a purely experimental difficulty at once presented itself. This obscure heat is absorbed by glass to far too great an extent for me to dream of employing the glass plates I used in my former experiments. By the use of rock-salt Professor Tyndall was able to obtain good results. But although we need not exaggerate the fears which may arise from the facility with which this body absorbs aqueous vapour, I preferred to get rid entirely of this source of error; and fortunately I obtained from M. H. Soleil some very beautiful specimens of colourless fluor-spar, which were extremely useful to me.

Melloni's experiments have shown that a thickness of 2 millims. of this substance transmits about 50 per cent. of the heat from a blackened copper plate heated to 400° .

I used it for closing my tubes, and for constructing my lenses and troughs; and, thanks to its use, I have been able to ascertain that on the heat from a blackened copper plate heated to about 400° the ether exerts absorptions which are always independent of the physical condition of the bodies used. This absorption is considerable, amounting to about 92 per cent. in the case of a layer of liquid ether of 0.0017 millim. in thickness at 25° . In all cases, whether the ether be in the state of liquid or of vapour, the absorbent action is the same, provided the number of active molecules which the ray meets on its path is the same.

This characteristic of the absorption which the diathermanous bodies investigated in this research exert upon the heat is met with under different forms. Thus, for instance, in working with illuminating gas I ascertained that in a given tube a constant weight of this gas exerts an action completely independent of the greater or less quantity of air with which it is mixed. With the quantity of air introduced, the pressure of the internal elastic fluid changes; but, provided nothing escapes from the tube, the absorption remains un-

changed. The absorptive action of air is known to be so weak that it may be completely neglected. Lastly, comparing under several conditions the absorptive action exerted by columns of coal-gas having the same section and the same weight, I observed that they were the same, although their length and their densities were very different.

Tubes closed by plates of fluor-spar appear likely to be of great use in demonstrating the laws of the absorption of obscure heat.

In my experiments the galvanometric action produced by obscure radiation transmitted through a tube full of air 0·64 metre in length was 20° ; it sank to 11° when I used the coal-gas tube under a pressure of 0·76 metre. The action of the vapour of ether is far greater; a tube half as long would be sufficient to render it very distinct.

I may be permitted, in conclusion, to add that troughs closed by fluor-spar plates are, from their unalterability, very convenient in investigating absorption by liquids. Using them, I have ascertained that liquid chloride of carbon, with the same thickness, is more easily traversed by obscure heat than bisulphide of carbon itself.

Chloroform has great diathermaney, but less than the preceding. Benzole and glycerine, on the contrary, with a thickness of 0·01 metre, stop to a tolerably complete extent the heat from a plate of blackened copper heated to 400° .—*Comptes Rendus*, September 10, 1867.

CHANGE OF OBLIQUITY A CAUSE OF CHANGE OF CLIMATE.

BY J. CARRICK MOORE.

In the August Number of the Philosophical Magazine, Mr. Croll has pointed out a great inaccuracy which I had committed. Quoting from memory, I had misrepresented the results of Melloni's experiments, which show that when a ray of heat passes through a number of transparent plates, the principal absorption is effected by the first plate. But the main question to which I called attention, I submit, remains as it was. Mr. Croll asserts, on the authority of Mr. Meech's Tables, that if the present annual heat at the equator be represented by 365·24, the heat at the poles will be represented by 151·59; and if the obliquity were increased by $1^{\circ} 22' 34''$, by 160·04,—these two quantities being to each other as 18 to 19. What I contended for was that each of these last quantities was necessarily erroneous, as they make no allowance for the diminution of heat in passing through the atmosphere, which is known to be very great,—and that if we had the true quantities, their proportion might be very different. Melloni's experiments show that the absorption for each transparent substance that he tried was very different: he did not experiment upon a mixture of oxygen, nitrogen, and watery vapour, such as our atmosphere consists of. Mr. Croll says that whatever the absorption be, it will be the same for each obliquity, and therefore the proportion will be the same. But Tyndall has shown that watery vapour is the greatest absorber of all gaseous bodies. Now increase of heat is always attended by increase of vapour; and the additional thermal days at the poles from increased obliquity might therefore produce little or no change in the temperature of the poles, or in a very different proportion from 18 to 19.

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XLII. *Researches in British Mineralogy.*
By DAVID FORBES, F.R.S. &c.*

IN this present communication it is proposed to lay before the readers of the Philosophical Magazine the first part of a series of researches in British mineralogy which of late have engaged the attention of the author, who, being still occupied in their further prosecution, hopes from time to time to communicate in the pages of this Magazine the results of subsequent investigations.

In these papers it is intended, besides treating of the physical characters and chemical composition of the minerals under consideration, to pay especial attention, whenever it is practicable, to the study of their association, paragenesis, and mode of occurrence, as connected with the petrology and geology of their localities, in order thereby to elucidate as far as possible the origin and formation of the rock-masses or mineral veins in which they may happen to be imbedded.

As this department of mineralogy has hitherto attracted but little or no attention in England, it is thought necessary, before entering upon the description and particulars of the mineral species which are treated of in the present notice, to devote a few words to the consideration of the present state of British mineralogy, and more especially to bring forward some remarks on the great importance of the application of mineralogical investigation to the study of geological phenomena †.

* Communicated by the Author.

† The author would take the opportunity of stating that, although long engaged in mineralogical investigation (many of the results of which *Phil. Mag.* S. 4. Vol. 34. No. 231. Nov. 1867. Z

The high scientific position occupied by British mineralogists during the first third of this present century was one of which the country might justly feel proud; and the names of Wollaston, Hatchett, Jameson, Thomson, Phillips, Allan, Children, and others took their place amongst those of the most able representatives of the science at that period.

When, however, the actual state of mineralogical inquiry at this present time is reviewed, it cannot but be confessed that the many advances since made in the science have been almost exclusively due to the labours of continental investigators, and that British mineralogists have of late years neither done their fair share of the work, nor even sustained the brilliant reputation attained by their predecessors.

The more novel attractions of palæontological research on the one hand, and of organic chemistry on the other, appear to have diverted the exertions of many of those who otherwise would doubtless have done good service in the advancement of mineralogy; and it is to be feared that the British geologist of the present day not only frequently underrates the importance of a sound fundamental acquaintance with the elements of mineralogy, but also is not sufficiently impressed with the fact that such knowledge is absolutely essential to the proper and successful prosecution of petrological inquiry.

To say the least, it may be regarded as a misuse of valuable time, if not even termed presumption, when speculations altogether hypothetical in their nature are brought forward to explain the chemical changes which are supposed to have caused the appearance or changes of rock-masses in nature before the authors are in possession of accurate data to prove the mineralogical and chemical composition of the rocks themselves. And, here in England, it will not be denied that it is only in some very few instances (forming the exception to the rule) that at present there are at hand data, either mineralogical or chemical, sufficient to enable such generalizations to be attempted with any pretensions to soundness; for it is well known that only very few chemical analyses of British rocks have been made, but also that there is an equal want of either correct mineralogical determinations or chemical analyses of the British mineral species which combine to form the mass of even the most common rocks*.

have already appeared in this and other scientific periodicals), his residence and many years' travels in foreign parts have hitherto caused his attention to be more exclusively devoted to the mineralogy of such countries, and prevented him contributing his mite to British mineralogy.

* It must be admitted that England in this respect stands far behind the rest of Europe; France, Germany, Russia, and even the small kingdoms of Norway and Denmark are far in advance with regard to the knowledge of the chemical and mineralogical composition of their rocks.

No wonder, therefore, that the petrological nomenclature in present use is both indefinite and unintelligible, and, in short, altogether inadequate to the demands of the more advanced state of the other branches of geological inquiry, and, as a consequence thereof, that in geological surveys and maps it is common to find eruptive rocks of totally distinct mineral and chemical composition and age confounded with one another (as, for example, young diorites with old granites, young dolerites with old diabases, &c.), and in other cases to find rocks coloured and described by names which do not pertain to them (called, for example, syenites when quartz may not be even an essential constituent, diorites or greenstones yet not contain hornblende, talcose slates without talc, &c.). In fact the present state of classification and nomenclature of the eruptive rocks is such that it becomes quite impossible to know with any certainty what exact rock may be intended or mapped under the names generally in use.

Even in these days of intellectual progress, the old saying that "there is no royal road to learning" remains just as true as ever, and the discovery has not yet been made which would enable the child to read before first mastering the alphabet.

So even in geology; for however distasteful and laborious the task may be, it is nevertheless equally essential that the elements of mineralogy (physical and chemical) be acquired before the geologist is entitled to indulge in theoretical generalizations which, it will not even be disputed, must, from their very nature, be based upon such premises.

Mineralogy appears too often to be confounded with mere crystallography, a branch of applied mathematics undoubtedly of great service in the determination of crystallized mineral species, but which at the same time, especially when studied into its refinements, must be regarded more as of theoretical interest than of practical utility in extending our knowledge of the mineral constitution of the globe on which we live.

It must not be forgotten, therefore, that the study of mineralogy is not confined to the mere dry enumeration of mineral species, along with a description of their physical characters and chemical composition, which unfortunately is too often the entire substance of most text-books on mineralogy in general use, to the exclusion of any information calculated to elucidate the application of these data to the sciences of the geologist and miner, or to assist them in the discrimination and study of rock-masses in the field, and in the investigation of the occurrence and formation of those mineral veins and deposits which, when considered from an economical point of view, are of so immense an importance to the progress and welfare of the world in general.

The study of the mode of occurrence, association, and paragenesis of minerals, as well as of their origin and the relations which they bear to the geology of the matrix in which they are imbedded, is one of the highest importance and interest to the mineralogist. It is quite true, however, that the attention of most mineralogists has hitherto been all but exclusively devoted to the study of the crystallographic characters and chemical composition of mineral species, and has been but little directed to the investigation of the causes which have given rise to the appearances of the minerals themselves; for this reason, probably, it has become the general custom to regard the presence of most minerals* in rock-masses as merely accidental; and, with the exception of such as are products evidently formed from the alteration of other minerals by atmospheric or other sufficiently apparent action, few or no attempts have been made either to account for their origin, or to connect their appearance with the grander phenomena of nature which form the subject of geological inquiry.

The author, however, firmly believing that even the, to our eyes, apparently most insignificant phenomena in nature are never the result of chance, but are invariably due to the operation of definite laws (in many cases at present obscure, but which will ultimately be elucidated by the advances of scientific investigation), could not rest content with the explanation of the presence of such minerals as accidental, and therefore devoted himself to the study of this intricate subject. Although he himself considers the results as yet arrived at but very imperfect and incomplete, still he believes them to be so far satisfactory as to justify him in assuming the probability at least of certain general conclusions which these researches have, as it were, forced upon him.

Excepting only the smaller number of species which make up the bulk of rock-masses in general, it was found upon investigation that most other minerals, when occurring in eruptive rocks†, even when met with in the most widely separated parts of this

* This term is here employed more especially to indicate such mineral compounds as differ from the bulk of the rock-matrix.

† The present state of petrological nomenclature, already referred to, renders it necessary to define the exact meaning attached to any petrological term used in this communication, in order to avoid being misunderstood. By eruptive rocks it is here intended to designate all such rocks as by the naked eye, or, when fine-grained, under the microscope, are seen to possess a true crystallized structure, whilst at the same time, when studied in the field, they are found to intrude into or break through, and send ramifications, dykes or veins, into the neighbouring stratified or unstratified rock-masses. This term is here used quite independent of any theory (igneous or other) accounting for their origin or formation.

globe, present themselves under similar conditions, have the same associated minerals along with them, and that the eruptive rocks in which they occur, whenever the age of their intrusion could be satisfactorily ascertained, frequently, if not always, corresponded in geological chronology. This is more particularly the case with such minerals as have not been produced by a subsequent metamorphic action* (or alteration of previously existing species), and which may be assumed to be present in the rock in the unaltered condition in which they originally were developed *in situ*†.

Some, but comparatively few, minerals must be regarded as common to a variety of situations and circumstances, frequently occurring as normal constituents of very different rocks, although not really characteristic of any one precise rock or formation. Such minerals at times owe their appearance to chemical reactions and processes quite dissimilar to those which at other times have caused their formation‡.

Other minerals, the chemical elements of which, being common to the mass of the rock-matrix in which they occur, may merely indicate by their presence that they have been formed by the combination and rearrangement of these elements *in situ*, effected by the action of heat, water, or other agency. Epidote is generally an excellent example of this class, being most frequently found developed in rocks at their points of contact with eruptive intrusions, which seem to have affected the parts of the rock in juxtaposition sufficiently to cause its constituents to unite and form this mineral. In other cases such effects appear to be frequently brought about by the agency of water,—thus, for example, Vivianite, by the union of its constituents, phosphoric acid with oxide of iron &c.

The eruptive rocks, either directly, by enclosing them in their substance, or indirectly, by metamorphic action and the formation of metallic lodes, have supplied the mineralogist with by far the

* Melanterite (sulphate of iron), for example, is, when met with in nature, invariably the product of metamorphic action, and may be formed by the alteration of a variety of substances, even organic as well as inorganic; although usually the product of the oxidation of pyrites, its origin is not always referable to any distinct original mineral compound. Many hydrated silicates, some of which in nature are produced directly by igneous agency, may also be found as the result of the action of water on rocks which contain their chemical components, silica, alumina, lime, &c., as in the case of stilbite, scolezite, Heulandite, &c.

† Most sulphides, arsenides, antimonides and their compounds, some native metals, &c.

‡ Iron pyrites, quartz, &c., which may at times be either of igneous or aqueous origin. It seems very probable that even in such cases the more minute study of the characters, especially of physical structure, may lead to means of discrimination.

greater number of known mineral species; and in all parts of the globe the same or very analogous minerals are, as a rule, found to accompany the outbursts of similar eruptive rocks.

Although, as before stated, the appearance of such mineral compounds has been regarded as accidental and extraneous to the composition of the eruptive rock-mass as a whole, a closer examination leads to the conclusion, notwithstanding that they often bear so relatively small a proportion to the entire mass, that these minerals cannot but be intimately connected with the constitution and appearance of the rocks themselves; and it is anticipated that a more extended and accurate investigation of this subject will demonstrate, that, in like manner as the occurrence of certain fossils or groups of fossils enables the geological age of a sedimentary bed to be deduced, so will the presence of certain minerals or classes of minerals serve as a means of identifying the contemporaneous intrusions and outbursts of the eruptive rocks which at different geological epochs have disturbed the earth's external crust*.

The observations which immediately showed that the same mineral might frequently occur in two or more rocks of undoubtedly different character, composition, or geological age, instead of discouraging these anticipations, led, on the contrary, to a careful study of the physical characters and chemical composition of such minerals as were found to occur under the different circumstances here alluded to, in order to discover whether some characteristic differences not immediately visible to the eye might not still in reality exist.

These investigations were very tedious, and attended with many difficulties, since little or no reliable information on the subject was met with in works on mineralogy, and because in most cases it was found necessary to visit personally the localities of the minerals in question, both in order to procure authentic specimens and to make sure of the petrological relations of their occurrence; so that, notwithstanding much time has already been occupied in this inquiry, it can as yet only be regarded as in its infancy. As far, however, as the investigation has proceeded, the results already obtained appear most satisfactorily to indicate that, whenever the same mineral is present in two or more rocks of different geological age, it is usually, if not invariably, characterized in each case by certain peculiarities either in physical structure or chemical composition which serve to distinguish it under the different circumstances of occurrence.

It has long been known that certain species of felspar are cha-

* The author would here specially refer to his papers on the mineralogy of Chile, in his "Researches on the Mineralogy of South America," which have lately appeared in this Magazine.

racteristic of certain eruptive rocks but not of others. And amongst the numerous cases in which other minerals were found to differ when occurring under different circumstances may be mentioned the following.

Mica, when it occurs in granitic rocks*, was found, as a rule, to present itself as Muscovite, although in some special granite it also does occur as lepidolite. When in serpentines or metamorphic limestones it was, in all cases examined, phlogopite. When in zircon-syenites or miascite, it is present as astrophyllite or titaniferous mica; whilst in the volcanic rocks of Vesuvius, Laach, Baikal, &c. it occurs as Biotite.

Augite is found to present great differences in chemical composition and physical characters according as it is respectively found in serpentine, diabase, porphyrites, dolerites, metamorphic limestones, volcanic rocks and aërolites,—and may be in all these cases regarded as so many subspecies or varieties, admitting of easy and decided discrimination.

Garnet, when met with in granitic rocks, presents itself as almandine, iron garnet, or common garnet. When in connexion with dioritic eruptions and metallic veins it was found to be invariably as melanite, grossular, or colophonite. When in trap dykes it always presents itself as pyrope; and, as Durocher has observed, when in serpentine, it invariably occurs as magnesian garnet.

Leonhard, in his *Charakteristik der Felsarten*, has also shown that garnets, when in granite, always crystallize in trapezoedra, whilst when in the metamorphic schists it is invariably in the form of the rhombic dodecahedron. In like manner it was found that all the specimens of cuprite encountered in metallic lodes of the postoolitic period, were invariably crystallized as cubes, without any trace of bevelment or replacement of the angles, whilst those from lodes of the palæozoic period appeared invariably as octahedra in all the specimens examined.

Again, apatite, when occurring in granites, was invariably found to be fluor-apatite, but, when with the later diorites, was chlorapatite.

Hornblende differs in chemical composition and crystallization when respectively found *in situ* in diorites, zircon-syenites, porphyrites, or metamorphic schists.

Scapolite, a common mineral in granites, presents itself as meionite in volcanic rocks.

* By granitic is here understood the old acid rocks containing free quartz. Although not unfrequently used to denote structure, the author is not disposed to admit that there is any actual structure specially characteristic of granite when compared to many other coarsely crystallized eruptive rocks.

Olivine also differs in chemical composition, according as it is found in magnesian limestones, metamorphic schists, or volcanic rocks*.

And this list could easily be extended. Not only, however, do the same minerals when occurring in rocks of different nature possess distinct peculiarities which distinguish them from one another, but, as has been already shown (*Christiania Naturforskere Mødet Forhandlinger*, 1856†), this may even be the case when they occur in rocks of the same character but of different geological ages. Thus certain younger granites which are common in Norway, Sweden, and Russia (as well as in the United States of America) are characterized not only by the appearance of numerous mineral species containing the rare metals cerium, lanthanum, yttrium, &c., but also by several of the usual minerals (common to most granites) which otherwise do not contain any trace of these elements being found to have a portion of their ordinary bases replaced by the oxides of these metals. Thus, in this younger granite, epidote is represented by orthite, titanite often becomes yttrotitanite, garnets frequently contain several per cent. of yttria or oxide of cerium, apatite contains cryptolite (a phosphate of cerium), and the scapolite, according to Hermann's analysis, is hyposclerite (containing some 3 per cent. of oxide of cerium).

The results of the study of the mineral occurrence in eruptive rocks, along with that of the physical characters and chemical composition of these minerals, as well as a careful examination of the rocks themselves in the field in many parts of the world, have confirmed the author in the belief that such minerals as previously had been generally regarded as extraneous or accidental are in reality truly characteristic of the rocks themselves, and that even such common species as are usually recognized as rock-constituents also possess, when they occur in rocks differing widely in character and geological age, peculiarities either of physical structure or chemical composition which may serve to distinguish them from one another. This inquiry also led him to conclusions already made public, that:—

(1) Eruptive or intrusive rocks of identical mineral constitution have made their appearance or intrusion into the earth's crust at similar geological epochs.

(2) The minerals, or classes of minerals, accompanying or associated with such intrusive or eruptive rocks may serve as a

* Carbonate of lime is generally admitted to indicate differences in the origin of a rock in which it occurs, according as it is present in the form of calcite or Arragonite; and this is doubtless also the case with sulphate of lime when occurring respectively as gypsum or anhydrite.

† An abstract in the *Quarterly Journal of the Geological Society* for August 1858.

means of distinguishing the several eruptions in geological chronology.

(3) When the geological epochs of the appearance of two or more intrusive or eruptive rocks are known to differ, these rocks will then also be found to differ essentially in mineral constitution.

Unfortunately at the present time but few data are available for a more extended inquiry into this interesting subject, with a view to test or confirm the soundness of these conclusions.

Although mineralogists have accumulated a large amount of information regarding the crystallographic characters and chemical composition of minerals, it is but rare to find in works on mineralogy any definite statements as to their mode of occurrence, mineral association, or the nature and geological age of the rocks in which they may have been found imbedded. In carrying out such petrological inquiries, geology must necessarily go hand in hand with mineralogy and chemistry, and, whenever possible, the rock-matrix of a mineral should be referred to its corresponding geological epoch, so as thus to fix the age of the appearance of the mineral itself. As but extremely few such observations are at present on record, one of the main objects in thus directing attention to this most interesting and important branch of mineralogical science is to induce mineralogists and geologists to avail themselves of all the opportunities which may present themselves, in order to collect as many observations bearing upon this subject as possible, so as to serve as materials for future and more extended generalizations.

If, now, in order to inquire into how far the present state of British mineralogy can supply data for such investigations, the latest published compendium of British mineralogy* be consulted, it will at once be perceived, not only that there are but few allusions to the mode of occurrence, association, or geological relations of minerals, but that there is an equally remarkable want of chemical analyses of many of even the most common British mineral species.

According to this work, the total number of distinct British mineral species amounts to 241, of which number about one-half, or, more exactly, 121 have been analyzed (in this number, however, are included many analyses of mere varieties, in cases where no chemical analyses of typical species have been made). The total number of both British species and subspecies enumerated

* *Manual of the Mineralogy of Great Britain and Ireland*, by Greg and Lettson. Since the publication of this work many valuable additions have appeared, amongst which various papers by Haughton might specially be mentioned. As yet, however, these only exist scattered in scientific periodicals and in the Transactions of various learned societies.

is 306, of which only 142 have been analyzed,—the majority, or 164, not having been submitted to chemical examination.

If, now, the inquiry is made as to what species have been analyzed and what not, it will be learned with much surprise that very many even of the most common mineral constituents of rocks had not up to this date been examined. Thus, for example, not a single chemical analysis of the following common British species is to be met with:—hornblende, augite, orthoclase, Labradorite, albite, Saussurite, chlorite, talc, garnet, idocrase, tourmaline, olivine, epidote, bronzite, diallage, serpentine, beryl, Biotite, &c.

These facts speak too plainly for themselves to require any further comments; but, in addition, it must not be forgotten that the branch of applied mineralogy (petrology) is in no better position, since not only, with some very few exceptions, are there no analyses available to show the composition of British rocks, but there does not even exist in the language any work on petrology in which the chemical analyses of the rocks of other countries are even reproduced for comparison*.

May it not now be fairly asked whether the natural inference to be deduced from these facts is not, that it is high time for British mineralogists and geologists to set to work in order to supply these deficiencies before occupying themselves in propounding vague theoretical explanations to account for the origin and metamorphosis of rocks in the field?

In thus directing attention to the backward but, unfortunately, true state of the mineralogy and petrology of this country, the author fully believes that he is doing a service to British science, and trusts that the present communication will be accepted as a slight proof of his intention not to shirk a fair share of the work alluded to in the preceding paragraph.

Gold.

From the most ancient times native gold has been known to occur in small quantities in various part of the United Kingdom and Ireland; but as yet, however, no chemical analysis of specimens from any of the localities have been made, or at least recorded. The successful prosecution of gold-mining in Wales of late years has naturally tended to augment the scientific interest with which the occurrence of this noble metal is invested; and consequently, in the spring of this year, the author made a visit

* A translation of Cotta's 'Rocks Classified and Described' has recently appeared, which, although purporting to represent the present state of petrology, is so very far from so doing, that, besides being both defective and far behind date in many other respects, it does not even contain a single chemical analysis of a rock from any (even typical) specified locality.

to the principal gold-localities of Wales for the express purpose of obtaining information connected with the mineralogical and geological relations of the appearance of this metal in the rocks of the Principality*.

Gold from the Clogau Quartz Lode No. 2.

This specimen was taken from the stamps, and was the result of quartz mined from the backs of the lode, almost on the surface, and was in the form of minute flattened grains, nearly free from any mineral admixture, and of a fine yellow colour.

The specific gravity, taken with 34.08 grains free from any visible impurity, was found to be 17.26 at the temperature of 60° F.

The chemical analysis was conducted as follows:—A weighed amount of the gold was dissolved as far as possible in nitrohydrochloric acid and evaporated nearly to dryness in a water-bath, then redissolved in water slightly acidulated with hydrochloric acid, and filtered from the chloride of silver and insoluble quartz, which were determined conjointly and afterwards treated with ammonia, which by dissolving out the chloride of silver, left the quartz behind for estimation.

From the filtrate, after being heated to nearly boiling, the gold in its metallic state, along with any copper present (in the form of oxalate), was precipitated by the addition of a strong solution of oxalic acid. The metallic gold was then collected on a filter and washed, first with water and then with weak solution of ammonia, in order to dissolve out any oxalate of copper which might be present; these last washings with ammonia were collected separately and tested for copper.

The first filtrate and washings, which would contain any iron present in the gold, were now evaporated to dryness and the iron subsequently determined as usual.

34.08 grs. of gold afforded 4.19 grs. chloride of silver and 0.11 gr. of insoluble quartz along with 30.69 grs. of precipitated gold. These results tabulated will give the composition as follows:—

* The author would here express his obligations to Mr. Arthur Dean, the Director of the Vigna and Clogau gold mines, for the facilities afforded him in his examination, and for the valuable information which that gentleman's long practical experience in the district enabled him to communicate. The development of gold-mining in Wales may be entirely ascribed to the perseverance and exertions of Mr. Dean; and the author would also add his belief that, had the large amount of capital already invested in Welsh gold-mining companies been but legitimately and judiciously expended upon the mines themselves, several more of these undertakings would still be in existence.

Gold	30·69	90·16
Silver	3·15	9·26
Copper and iron . . .	trace	trace
Quartz	0·11	0·32
Loss in analysis . . .	0·09	0·26
	<u>34·04</u>	<u>100·00</u>

Another specimen of gold quartz from this lode was also submitted to examination, the quartz fragments being pounded for the purpose and washed in a batea by Mr. Melville Attwood. The gold obtained was in irregular flakes, apparently somewhat lighter in colour than the previously examined specimen, and upon experiment its specific gravity was found to be 15·62, a result evidently due to the gold flakes enclosing air-cavities. The analysis of this specimen was conducted in precisely the same manner as the other sample, and afforded when tabulated the following results:—

Gold	4·86	89·83
Silver	0·50	9·24
Iron	trace	trace
Quartz	0·04	0·74
Loss on analysis . . .	0·01	0·19
	<u>5·41</u>	<u>100·00</u>

In this case no trace of copper was detected.

The metals gold and silver are known to alloy with one another in all proportions when fused, and, being both monometric in crystallization, are doubtless isomorphic in their replacements. It is interesting, however, to observe that the above analyses closely agree with the formula Au^6Ag , which, supposing the equivalents of gold and silver to be respectively 196 and 108, would on calculation require a percentage composition of

Au^6Ag	Gold	90·88
	Silver	<u>9·12</u>
		100·00

It may, however, be remarked that in some of the gold-specimens shown to the author by Mr. Dean, there also appeared to be another alloy of gold and silver occasionally present in the quartz of this lode, which, as it is considerably lighter in colour, would most probably on analysis be found to contain a larger proportion of silver.

The accessory minerals found in the Clogau quartz lode associated with the gold were tetradymite, iron pyrites, chalcopyrite, galena, chlorite, calcite, dolomite, ankerite (?), chalybite, and

sulphate of barytes*. These minerals as well as the native gold itself are distributed very irregularly in the quartz of the lode.

When the quartz contains patches of calcite, dolomite, and chalybite, and includes splinters or fragments of the neighbouring clay-slate, it is regarded by the miners as more promising and likely to yield gold than when the lode consists of quartz alone.

Although the gold sometimes is found alone imbedded in the colourless quartz, it occurs more often in conjunction with more or less iron pyrites and the other above-mentioned metallic compounds, which usually occur as small patches, nests, or aggregations in the quartz.

When small pieces or splinters of the bluish-grey slate are found isolated and enclosed in the quartz of the lode, it is common to find the gold and other metallic minerals adherent to or crystallized on the under surface of such fragments. This mode of occurrence is very suggestive of crystallization from solution, where it is common to find the crystals developing themselves in preference on the sides of any foreign bodies which might happen to be floating about in the fluid—chips or straws for example. The adhesion of the gold and metallic minerals to the underside of such chips or fragments of slate might be supposed to indicate that the underside of such fragments had arrested the gold &c. in the act of being carried into the lode-fissure from below along with the stream of liquid quartz: this would be equally the case whether the quartz is imagined to have been injected as an aqueous solution, or in its known gelatinous state, or in a state of igneous or hydro-igneous solution.

The geological position of the Clogau quartz lode is in the Lower Silurian Lingula-beds, close to their junction with the Cambrian strata of the Geological Survey, on which these beds rest conformably; and in close proximity to the lode they contain the *Paradoxides Davidis* in abundance.

In these strata are seen numerous intrusive masses, and, as it were, sheets of true diabases (described and coloured by the Geological Survey as greenstones), apparently at first sight contemporaneous with the beds themselves, but which, although frequently found to follow the strike of the fossiliferous beds for long distances, and even at times to coincide more or less with their dip, will nevertheless be sooner or later observed to break through both the strike and dip of these strata and to be purely intrusive rocks.

* The above minerals were all found by the author. Mr. Readwin states the following also to occur along with the Welsh gold:—bismuth, bismuthine, grey copper, mispickel, orpiment, phosphat and arseniate of lead, native lead (?), platinum (?), tale, oxide of iron, rutile.

These diabases are composed of a felspar base in conjunction with diallage and chlorite, and frequently, owing to incipient decomposition of these silicates, effervesce slightly with acids, thus revealing the presence of a trace of carbonate of lime.

From the observations of the Geological Survey, it may be concluded that the intrusion of these diabases was not of later date than the Silurian period—which result is conformable with what is known as to the age of the appearance of similar diabasic rocks in other parts of Europe*.

The auriferous quartz lode at Clogau runs about 18° north of east and dips at a high angle (88°) to south, cutting through both the fossiliferous strata and the intruded diabases; it must consequently be of later geological age than both of these rocks, and not improbably is even younger than the Silurian formation as a whole; but at present no data are at hand to fix its exact age with any certainty.

The mining explorations carried on hitherto appear to indicate that this quartz lode is much richer in gold at the parts where it cuts through the Lower Silurian Lingula-beds, with their accompanying intrusive diabases, than in greater depth where it traverses the Cambrian grits.

The study of mineral lodes leads to the conclusion that their productiveness is in general much influenced by the nature of the strata through which they cut (by the "country," in miners' terminology); and although at present no satisfactory explanation has been brought forward to account for it, still it is generally admitted that hard siliceous strata (like the Cambrian grits) are, as a rule, unfavourable in this respect; it is inferred, therefore, that the superior productiveness of this lode in the Lingula-beds is due to this cause, and not to the accidental presence of the associated diabases in this precise locality, or, in other words, that the lode might be expected to prove equally rich in gold in

*. The term greenstone appears to be used by the Geological Survey quite indiscriminately, without reference to any distinction of geological age or mineralogical composition, as, for example, the above-mentioned diabases of Silurian age are in no ways distinguished on the map from the postcarboniferous dolerites equally common in Wales.

The very characteristic rock-species diabase does not even seem to have been recognized by the Survey, notwithstanding that it would be probably impossible to find better opportunities of studying it in all its variations of texture, crystallization, and mineral composition than in the immediate vicinity of Dolgelly. The coarse-grained varieties, with the felspar especially prominent, are quarried on the Towan road just beyond the town; the more chloritic ones are well seen in the rocks between Llanelyd and Tyn-y-Groes; calc-diabase occurs in the mountain-side immediately above Penmaen Pool; the schistose, close-grained, and compact varieties occur everywhere; and, lastly, the magnificent Uralite porphyry, as it has been termed, forms hills alongside of the river Mawddach before coming to Tyddynglwadis.

parts where it traversed Lingula-beds unaccompanied by such diabases.

If we could suppose the workings on the lode in such a mine carried down through the entire thickness of these Cambrian grits into softer and more congenial strata, it might even be anticipated that the lode might again be found quite as productive as before, irrespective of actual depth from the surface.

From the consideration of the general characters of these auriferous lodes and of the mineral association*, the author assumes the probability of these lodes pertaining to what he has termed the older or granitic intrusion of gold, which he supposes to have taken place some time between the Silurian and the Carboniferous period†; and it is also believed that these lodes have been injected from below, and not formed either by segregation or infiltration.

Stream-Gold from the River Mawddach.

Through the kind offices of Mr. T. A. Readwin a specimen of the gold-dust washed out from the bed of the River Mawddach, near Gwynfynydd, some eight miles above Dolgelly, was obtained and submitted to examination.

The portion received contained the native gold in the form of small flattened elongated spangles, in size from about a pin's head down to almost dust, and of a rich yellow colour; it was accompanied by abundance of fine black sand, supposed to be magnetic oxide of iron from its being strongly attracted by the magnet, but which on analysis turned out to be titanoferrite‡; it also contained small particles of quartz, slate rock, mica, and some small cubes and fragments of iron pyrites and galena.

The specific gravity of the gold, carefully separated from any other admixture, was found to be 15.79 at 60° F.; quantity employed 22.75 grs.

* Especially the presence of tellurium and bismuth compounds, which are usually found in the gold veins of the old or granitic class.

† The author (British Association Report for 1865, Sects. p. 52, and Geological Magazine, vol. iii. p. 385) classified the known auriferous veins as respectively pertaining to two distinct epochs:—(1) the older or granitic, having made its appearance between the Silurian and the Carboniferous period; (2) the newer or dioritic, about the Cretaceous period. In the case of the Clogau lode there is no visible granite known in the district; but nevertheless it is supposed to belong to the same system of metallic impregnation.

‡ In the qualitative or quantitative examination of this mineral the following process is employed:—A portion in finest powder is fused in a platinum spoon with about eight times its weight of bisulphate of potash until all the iron is seen to have been taken up. After being perfectly cold, the result is dissolved in a considerable amount of cold water; to the clear solution a few drops of nitric acid are added and the whole boiled for some time, when the titanous acid, if present, will be completely precipitated.

The analysis was conducted precisely as in the case of the specimens from Clogau previously examined.

20·16 grs. afforded 17·71 grs. metallic gold, 3·89 grs. chloride of silver, 0·09 gr. insoluble quartz, and 0·10 gr. sesquioxide of iron.

These results, when calculated, will give the composition as follows:—

Gold	17·71	84·89
Silver	2·92	13·99
Iron	0·07	0·34
Quartz	0·09	0·43
Traces of copper and loss	0·07	0·35
	<u>20·86</u>	<u>100·00</u>

Several of the larger spangles of gold appeared of a peculiarly rich colour, owing to their being coated with, as it were, a varnish or thin film of sesquioxide of iron, which persistently adhered to their surface, and probably was the source of part, if not of all the iron found by analysis. This coating was no doubt the cause of this gold appearing to the eye considerably richer than it actually turned out to be on assay.

Titanoferrite.

When engaged in the examination of the eruptive rocks of the South Staffordshire coal-field*, the author found that these basaltic or doleritic rocks, locally known by the name of Rowley Rag, invariably contained a small but constant amount of a heavy black metallic mineral, strongly attracted by the magnet, and which, therefore, was generally regarded as magnetite or magnetic oxide of iron.

As the analyses made of the rocks themselves always showed the presence of the comparatively rare element titanium, it at once appeared probable that this mineral would turn out to be titanoferrite—a compound of titanium, iron, and oxygen, the exact chemical constitution of which is not yet altogether agreed upon by mineralogists.

After a fruitless search in the various quarries of this rock, which are opened at many places along the range of the Rowley Hills, this mineral was nowhere found to present itself in specimens of a tangible magnitude, and was never observed except in microscopic grains disseminated throughout the rock itself. In order, therefore, to procure an amount sufficient for its examination and analysis, a perfectly fresh part of a basaltic column from the Turners Hill quarry (possessing a specific gravity of

* British Association Report for 1865, Sects. p. 53, "On the Igneous Rocks of South Staffordshire."

2.84) was first broken up in small pieces; and after well rubbing the pieces in a cloth and throwing away the dust in order to be secure against the introduction of any trace of metallic iron from the employment of an iron hammer, these were finely pulverized in a porcelain mortar. By means of a magnet, all attractable particles were then removed from this powder, and, after a careful washing in order to remove as much adherent earthy matter as possible, the metallic particles were then carefully dried, using alcohol, so as not to permit of their becoming oxidated, and reserved for examination.

The researches of Andrews having shown that the posteretaceous basaltic rocks of Antrim contain iron in the metallic state, a fact since confirmed by Pagels in the case of the basalt of Bärenstein in Germany, a portion of the mineral was treated with a solution of acid sulphate of copper, precisely as described by Professor Andrews, but no indication of the presence of metallic iron was discovered.

Although the metallic particles now obtained appeared to the naked eye as if quite free from any admixture, it was not found possible (notwithstanding several further attempts by repulverizing, extracting by the magnet, and again washing) to obtain the mineral perfectly free from adherent rock-matter, since under the microscope it appeared to the last as a black opaque mineral to which small particles of a colourless transparent silicate were attached.

In order, therefore, to arrive at as near an approximation to its true specific gravity as possible, the following method was adopted.

The specific gravity was first determined on a weighed amount of the mineral, which was then acted upon by boiling sulphuric acid until all the oxide of iron and titanitic acid was extracted, leaving the soluble siliceous matter behind, which was weighed, and subsequently its specific gravity was determined as usual. These data were found to be as follows:—24.82 grs. original mineral of specific gravity 3.65 afforded 8.84 grs. of rock admixture*, of specific gravity 2.601, and consequently, by the calculation

$$\frac{2.601 \times 3.65 \times 15.98}{(2.601 \times 24.82) - (3.65 \times 8.84)} = \frac{151.5085270}{32.29082} = 4.69,$$

the required specific gravity of the mineral.

The chemical examination was conducted as follows:—A weighed amount, previously reduced to impalpable powder, was repeatedly digested in fresh portions of sulphuric acid in a pla-

* This amount includes 1.17 gr. alumina, lime, and magnesia, found to be present in the course of the analysis.

tinum capsule until all the iron and titanium had been extracted; the whole was then poured into a large excess of cold water and filtered from the insoluble siliceous matter, which was determined as usual.

The filtrate was now supersaturated with ammonia, which precipitated all the titanitic acid, oxide of iron, &c.; without filtration the whole was then subjected to the action of a stream of pure sulphurous acid gas, which reduced the sesquioxide of iron to the state of protoxide and dissolved it, but ultimately also took up the titanitic acid and produced a clear solution. This solution, after boiling for some time, deposited the whole of the titanitic acid as a white precipitate, which, after ignition, was of a faint yellow colour from still retaining a trace of oxide of iron. From the filtrate the iron, manganese, alumina, lime, and magnesia were now determined as usual, and the results obtained were as follows:—Quantity of mineral employed 24·82 grs., insoluble residue obtained 7·67 grs., titanitic acid 5·43 grs., sesquioxide of iron 11·16 grs., manganoso-manganic oxide 0·18 gr., alumina 0·56 gr., carbonate of lime 0·76 gr., pyrophosphate of magnesia 0·13 gr.

Before the blowpipe this mineral is infusible and unaltered by the oxidating-flame; in a strong reducing-flame the sharp edges become somewhat rounded off, but no further change is perceptible. In the oxidating-flame, when treated with borate or phosphate, it merely shows the usual reactions of oxide of iron; but when the phosphate bead is treated for some time in a good reducing-flame, the glass on cooling has a brownish-red colour. When this glass is further treated with tin on charcoal in the reducing-flame, it acquires the reddish-violet colour characteristic of titanium. By fusion with bisulphate of potash, as before mentioned, the titanitic acid may be separated and examined.

The percentage composition of the mineral, calculated from the above data, will be as under:—

Titanic acid	5·43		21·87
Iron	7·81	} 10·39	} 31·47
Oxygen as loss 2·58			
			10·46
Protoxide of manganese	0·16		0·67
Alumina	0·56		2·25
Lime	0·42		1·71
Magnesia	0·19		0·67
Insoluble siliceous matter	7·67		30·90
	24·82		100·00

Deducting from these results the amount of the silicates, we find for the pure titanoferrite the following composition:—

Titanic acid	21·87	34·28
Oxide of iron	41·93	65·72
	<u>63·80</u>	<u>100·00</u>

And if the iron and titanium are supposed to be present respectively as titanic acid (TiO^2) and sesquioxide of iron (Fe^2O^3), according to the views of Rose and Berzelius, this composition closely approximates to the formula $\text{Fe}^2\text{O}^3\text{TiO}^2$, which, when the equivalents of titanium and iron are respectively considered to be 25 and 28, requires—

Titanic acid (TiO^2)	41	33·89
Sesquioxide of iron (Fe^2O^3)	80	66·11
	<u>121</u>	<u>100·00</u>

The minerals associated with this titanoferrite are seldom to be distinguished by the naked eye, but are easily recognized by a microscopic examination of thin sections of the rock itself*, and are seen to be:—a triclinic soda-lime felspar, probably Labradorite; augite in brown crystals often finely developed; and a small quantity of a green mineral having the same crystalline form as the augite, which may possibly be Delessite (a ferruginous chlorite), but is more probably seladonite, a variety (hydrous) of augite. In some specimens specks of iron pyrites are seen, and occasionally minute long prisms which are hexagonal in section and most likely are apatite, as by a chemical examination a small amount of phosphoric acid was found in some specimens. In some instances the rock contains small radiating crystallizations of a zeolitic mineral, occasionally from one-eighth to a quarter of an inch diameter, which, although not as yet examined, appears to be pectolite.

The geological age of the intrusion of these rocks is considered by Mr. Beete Jukes to be contemporaneous with that of the coal-measures, but is believed by the author to have been subsequent to the deposition and consolidation of these strata. It is not improbable, however, that it was previous to the formation of most of the Permian series; since at Marston Jabct in Warwickshire, and at the Holland Arms in Anglesea, eruptive rocks analogous to these are found traversing the carboniferous strata, but overlain by Permian beds, which consequently must be of later date.

The occurrence of these insignificant, almost microscopic black specks of titanoferrite in the substance of these rocks, should they even happen to attract the passing attention of the field geologist at all, would most probably be quite disregarded by him, in his

* *Vide* an article by the author "On the Microscope in Geology," in the Popular Science Review for October 1867, plate 17, fig. 5.

anxiety to occupy himself alone with what he would consider the broader views of Nature's phenomena*.

Just, however, as the mouse (in the old fable) proved of so great assistance by nibbling away at the net in which the captive lion was entangled, so in this instance also have we an example of how useful the study of such minute details in mineralogical investigation may be when applied to the explanation of the geological features of the district.

The chemical and microscopical examination of specimens of these rocks, taken from all the different eruptive bosses in Staffordshire (thirteen in number) which are coloured as greenstones on the map of the Geological Survey, as well as of many specimens taken from intrusive masses or dykes met with in depth in the coal-pits which do not make their appearance on the surface, proved most conclusively that titanoferrite was invariably present in all, and consequently that it is an essential constituent of the rock itself.

The chemical analysis and specific gravity of the mineral further showed it to belong to the variety of titanoferrite which usually accompanies the eruptive rocks of palæozoic age, but which is not as yet known in those of the later geological periods †.

As these rocks, as before mentioned, invariably contain the mineral species titanoferrite, it naturally follows that the presence of the comparatively rare chemical element titanium must be equally characteristic of their intrusion or appearance, since this element does not occur as a constituent of any other of the rocks of this district ‡. By determining, therefore, the presence of titanium, a means is at once obtained by which such rocks, even when altered by metamorphic action so as to be unrecognizable to the eye, can be distinguished, and by which also beds or deposits (such as tuffs, ashes, clays, &c.) formed from the breaking up and alteration of such rocks might be referred to their original source.

To illustrate the application of this in practice, a couple of instances may be cited.

In September 1855 the occurrence of a deposit of dark-red ferruginous clay in the outskirts of the town of Dudley, in ap-

* And doubtless also staunch in the orthodox belief "that mountains should not be looked at through microscopes."

† Titanoferrite, when found in modern lavas and in the younger basaltic rocks, occurs as iserine, monometric in its crystallization, and differing in the proportions of its chemical constituents, from when it presents itself as Ilmenite and other subspecies, which are rhombohedral and found in the older rocks of palæozoic age.

‡ Titanium has been detected in some clay ironstones and clays in minute quantity, but under circumstances which could not mislead the observer in such cases.

pearance totally different from any of the clays occurring either in the Carboniferous or Silurian formations of the neighbourhood, or even likely to have resulted from the alteration of any of the known strata in these formations, made the author conclude that this clay must have proceeded from the decomposition of the eruptive basaltic rocks themselves. Chemical analysis was resorted to in order to obtain evidence in support of or against this view, and appeared to be quite conclusive in its favour, since not only did the results of the analysis show that there was nothing, from a chemical point of view, to militate against the supposition that this clay had been of such origin, but the amount of titanitic acid which it contained seemed to decide the point most satisfactorily.

In another instance, in the same district, at the coal-pit No. 7, Highlanes, the thick coal was found to be traversed by a dyke of a white rock (the so-called white horse of the Staffordshire miners) which broke through it and sent ramifications into the coal.

The coal at points of contact with this rock was evidently greatly altered, having lost most of its volatile ingredients, and become, as it were, charred and nearly incombustible. The rock, on the other hand, so far from possessing the normal appearance of the eruptive dolerites of the vicinity, appeared in external characters to differ but little from a somewhat indurated greyish-white clay, and at first sight would be taken for anything but igneous. The chemical analysis showed its composition as follows:—

White-horse rock, pit No. 7, Highlanes. Specific gravity 2·81.

Silica	31·39
Titanic acid	1·02
Alumina	27·80
Lime	2·52
Magnesia	1·66
Soda	0·54
Protoxide of iron	16·13
Protoxide of manganese	0·09
Water	19·62
	<hr/>
	100·77

with traces of carbonic acid and potash, proving most satisfactorily its identity with the ordinary dolerite of the district, which evidently in this case had been altered *in situ* by the action of water. The microscopic examination also showed that enough of the crystallized structure still remained to prove its origin*.

* "The Microscope in Geology," Popular Science Review for October 1867, pl. 18. fig. 12.

At first sight it would seem that the resulting products in these two instances are very different, although undoubtedly both are derived from the alteration of the same rock-mass.

This apparent discrepancy, however, is easily accounted for. In the case of the first-mentioned dark-red ferruginous clay, the alteration, having taken place at the surface, under the full oxidating agency of the atmosphere, caused the whole, or at least the greater part of the iron present in the rock to be converted into sesquioxide or red oxide of iron, and thus accounts for the colour of the clay.

In the depths of the coal-pit, however, cut off from access to the air, the chemical changes would be very different. The water percolating slowly through the substance of the rock, besides causing it to become hydrated and disintegrated, would carry off a portion of its lime, magnesia, and alkali, and probably introduce a little carbonic acid to combine with a portion of the remainder of these bases; yet at the same time the deoxidating action of the carbonaceous matter in immediate contact (and what little air might be present would doubtless be highly charged with hydrocarbons) would prevent the iron from becoming more highly oxidized, and cause it to remain still as protoxide, as was found to be the case by analysis.

*Polytelite**.

(*Weissgiltigerz, Silberfahlerz.*)

Locality. Foxdale silver lead mine, Isle of Man†.

The occurrence of true silver ores in the United Kingdom is of such rarity as to invest the discovery of the present mineral, especially since it has been found in quantity sufficient to make it an object of commercial consideration, with great interest. Although the cupriferous tetrahedrite (occasionally containing traces of silver) has been found in small quantities at various localities in both England, Scotland, Ireland, and Wales, there is no analysis of true silver fahlerz or polytelite, or even occurrence of the mineral itself recorded, as far as the author has been able to ascertain.

The characters of this mineral are as follows:—massive; opaque; lustre metallic; colour brown-black; streak black to brown-

* So many different subspecies are comprised under the denominations tetrahedrite and fahlerz, that the name polytelite (from *πολυτελής, valuable*), given by Glocker to this mineral, is one which deserves to be retained as being characteristic of the species.

† The specimen examined was taken from the dressing-floors of this mine, by Mr. Melville Attwood, this summer. Mr. J. L. Thomas, the manager of this mine, has since informed me that this mineral was first observed early this spring, and occurred along with copper pyrites in the workings furthest to the east in the mine, where the lode runs out of the slates into the granite.

black; fracture subconchoidal, uneven, and granular; brittle; powder black; hardness 3.5, scratching calcite but not fluor. Specific gravity taken on 61.96 grains pure fragments was 4.97 at 60° F.

Before the blowpipe, on charcoal, it decrepitates, and then fuses to a globule which, when treated in the oxidizing-flame, covers the charcoal with a white sublimate of oxide of antimony; and still nearer to the globule sublimates of the oxides of zinc and of lead may be perceived, whilst sulphurous acid is evolved; with borax or charcoal the glass reacts for iron, whilst antimony; and sulphur are given off and by continued treatment in the oxidizing-flame a white metallic globule containing silver and copper is obtained, which, when treated with phosphate, shows the copper-reaction.

In the open tube sulphurous acid was evolved, and a white antimonial sublimate formed.

When a weighed quantity was fused with borax and lead, treated with the oxidizing-flame, and the resulting silver-lead cupelled, as in the ordinary blowpipe assay, the mineral yielded 13.9 per cent. of silver.

A preliminary qualitative examination having shown the mineral to contain sulphur, antimony, iron, silver, copper, zinc, lead, and a little insoluble quartz, the quantitative chemical analysis was conducted as follows.

A quantity of the finely pulverized mineral was introduced into a previously tared small glass-tube boat open at both ends, and weighed; it was then introduced (close up to the bend) into a hard-glass tube, the one end of which was cut off obliquely and bent so as to dip into a two-necked bottle containing a solution of tartaric and hydrochloric acids, whilst the other neck was provided with a bulb-tube inserted into the liquid only so far that the solution could only be forced up into the bulb, but must fall back again. A current of dry chlorine gas was now passed through; the arrangement and the part of the tube containing the mineral was then heated gradually until it appeared perfectly decomposed and converted into chlorides. The tube-boat was now drawn back with a wire out of the hard-glass tube in which it had been heated, and allowed to absorb moisture slowly by being kept for some hours in a glass beaker moistened on the sides with water. The hard-glass tube was now washed out with hot water into the solution of hydrochloric and tartaric acid contained in the two-necked bottle.

This operation consequently resulted in the formation of two products, respectively:—

A, a solid residue contained in the tube-boat, which contained all the silver in the form of chloride, the insoluble quartz, a

portion of the iron, and the greater part of the copper, zinc, and lead in the form of chlorides.

B, a solution containing all the antimony, along with part of the iron and a little zinc, copper, and lead, all in the form of chlorides, as well as all the sulphur contained in the mineral, which, however, in order to simplify the analysis, was neglected and determined separately upon another portion of the mineral.

This solution was now heated to boiling, and after being filtered from the floating flakes of sulphur, was precipitated by a current of sulphuretted hydrogen gas passed through it, by which the whole of the antimony, along with traces of copper and lead and some free sulphur, were thrown down. This precipitate was washed off the filter into a stoppered bottle and treated with excess of hydrosulphide of ammonium, which, by dissolving out the sulphide of antimony and free sulphur, left a minute quantity of the sulphides of copper and lead behind, which were added to the same sulphides obtained in the treatment of the solid residue A.

The filtrate, containing the antimony, was now supersaturated with sulphuric acid; and after the precipitated sulphide of antimony along with free sulphur had been collected on a filter previously dried at 212° F. and tared, they were dried at 212° and the weights determined.

A weighed portion of this was now fused with a mixture of four parts of nitrate of soda along with two parts of carbonate of soda in a platinum crucible, digested with water, the antimoniate of soda collected upon a filter, washed with dilute alcohol, and determined, the amount of antimony being then calculated from this result.

The filtrate from which the antimony had been precipitated by sulphuretted hydrogen, and which now contained the iron with a little zinc, was added to the similar product obtained in the treatment of the solid residue A.

The solid residue A was treated as follows:—Water acidulated with a little hydrochloric acid being added, all the copper, iron, zinc, and some of the lead dissolved, leaving the silver (as chloride), insoluble quartz, and a trace of chloride of lead behind; these were filtered off, well washed with water to dissolve out the trace of chloride of lead, and the chloride of silver, along with the insoluble quartz, then determined; and afterwards the former was dissolved out by ammonia, leaving the quartz behind, which was estimated.

The copper and lead in this solution were now precipitated by sulphuretted hydrogen, and the sulphides, added to those previously obtained (solution B), were converted into anhydrous sulphates, weighed conjointly; and the sulphate of copper being

subsequently dissolved out by water allowed the sulphate of lead to be determined.

The filtrate, which now contained the iron and zinc, was added to that previously obtained from solution B, and the whole precipitated by hydrosulphate of ammonia, after previous neutralization of ammonia. The sulphides thus obtained were oxidized by nitric acid and separated by ammonia, when both these metals were determined as usual.

To determine the sulphur, a separate portion of the mineral was acted upon by nitrohydrochloric acid, evaporated nearly to dryness, a small amount of free sulphur removed and estimated separately after deduction of a minute quantity of residue left upon ignition, and the solution, after filtration from insoluble matter, was precipitated by chloride of barium, and the amount of sulphate of barytes determined as usual. 20.01 grs. of mineral afforded 0.50 free sulphur, along with 36.41 grs. of sulphate of barytes; it consequently contained 27.41 per cent. of sulphur.

The other results obtained were as follows:—Mineral employed and treated by chlorine, 20.02 grs.; sulphate of lead obtained, 0.39 gr.; insoluble quartz, 0.07 gr.; chloride of silver, 3.61 grs.; sulphide of antimony with free sulphur, 13.22 grs.; antimoniate of soda from 4.04 grs. ditto, 2.88 grs.; oxide of zinc, 1.16 gr.; sesquioxide of iron, 1.38 gr.; sulphate of copper, 12.53 grs.

The above data will upon calculation show the percentage composition of the mineral to be as follows:—

Sulphur	27.48
Antimony	24.85
Silver	13.57
Copper	22.62
Iron	4.80
Zinc	4.65
Lead	1.43
Quartz	0.34
	<hr/>
	99.74

Arsenic and mercury were carefully sought for, but not found to be present in the mineral. Subtracting the amount of quartz and dividing the loss on analysis equally amongst the constituents, we have—

Sulphur	27.48	27.64
Antimony	24.85	25.00
Silver	13.57	13.65
Copper	22.62	22.76
Iron	4.80	4.82
Zinc	4.65	4.69
Lead	1.43	1.44
	<hr/>	<hr/>
	99.40	100.00

The result of this analysis differs but very little from the composition of specimens of this mineral from other localities, to which the formula $4(\text{Cu}^2, \text{Ag}, \text{Fe}, \text{Zn}, \text{Pb}) \text{S}, \text{Sb S}^3$ is attributed; it appears not unlikely, however, that in such metallic sulphosalts the copper may really be present as CuS . Taking the actual amounts of the metals found by this analysis and representing them as sulphides, the numerical results in both these cases will be as follows:—

Sulphide of antimony, Sb S^3	.	34.82	..	34.82
„ silver, AgS	.	15.67	..	15.67
„ copper, Cu^2S	.	28.51	CuS	34.26
„ iron, FeS	.	7.57	..	7.57
„ zinc, ZnS	.	7.18	..	7.18
„ lead, PbS	.	1.66	.	1.66
		<u>95.41</u>		<u>101.16</u>

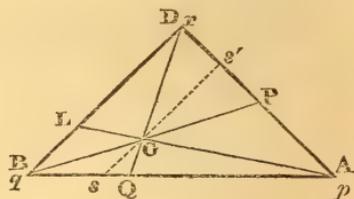
The minerals associated with this specimen were galena, chalcopyrite, iron pyrites, zincblende, quartz, dolomite, chalybite, and calcite.

The lode itself cuts through both the Lower Silurian (non-fossiliferous) slates and the eruptive granite, which latter made its appearance subsequently to the deposition of these beds.

This granite is identical in mineralogical composition with the auriferous granites of other parts of the world, being composed of orthoclase, Muscovite, and quartz; and traces of gold are reported to have been found in the gullies containing its *débris*, as well as in the quartz veins which are found in the slates in close proximity to it.

XLIII. *A new Attempt to determine the Resultant of two Pressures on a Fixed Point.* By G. W. KEELY, Esq.*

THROUGH the vertices A, B, D, of a triangle which is the half-base (right-angled at D) of a right square pyramid, let the directions of three forces, p, q, r , pass respectively, the forces being applied at the vertex of the pyramid. The total resultant will pass through some point G, determined by drawing in the plane of the base two straight lines from A and B, to the points L and P, through which are assumed to pass the resultants of q and r and of p and r ; and a third line, drawn



* Communicated by the Author.

through G from D, will, produced, determine the point Q, the place of the resultant of p and q , which point Q may be, if we please, stationary, while the points L and P will vary as r varies. If the units of linear and force magnitude are the same, and p , q , r are considered, two and two, sides of parallelograms, we shall have (by common geometry), if the directions of the diagonals and resultants coincide, $\frac{BQ}{AQ} = \frac{p}{q}$, &c. ; if not, we shall have

$$\frac{BQ}{AQ} = \frac{pe}{q}, \quad \frac{BL}{DL} = \frac{re'}{q}, \quad \frac{AP}{DP} = \frac{re''}{p};$$

e , e' , e'' being coefficients to be determined ; but since (by common geometry) the product of the three alternate segments LD, AP, BQ equals that of the alternates BL, DP, AQ, we have at once $e = \frac{e'}{e''}$; if e' and e'' have each different values, while r changes, let L be a point where e' has a maximum value, and P a point where e'' has a minimum value ; then if Q, and therefore e , and therefore $\frac{e'}{e''}$ are constant while r varies, it follows that neither e' nor e'' can, independently of each other, change ; nor can they, both together, become greater or less ; therefore each is constant for every value of r , and therefore *unity*, since we know the one is so when r equals q , and the other when r equals p ; therefore whatever the ratio of p to q , and whatever the angle between them, we have $\frac{BQ}{AQ} = \frac{p}{q}$.

That the diagonal and resultant are equal in magnitude is obvious, if we admit that if three forces in the same plane, acting on a point, are in equilibrium, either, reversed, is the resultant of the other two ; for if one is changed in magnitude to make it a diagonal, no other can afterwards, with that as a side, be made a diagonal without a change of direction.

Second Demonstration.

The construction and the distribution of the forces being the same, and $s s'$ being drawn, through G, parallel to BD, we have, by common geometry,

$$\frac{BQ}{AQ} = \frac{Bs}{SA} \cdot \frac{BD}{DI}; \quad \dots \dots \dots \quad (A)$$

we have also, as before,

$$\frac{BQ}{AQ} = \frac{BL}{DL} \cdot \frac{DP}{AP} \cdot \dots \dots \dots \quad (B)$$

First, let $p = q + r$ and q be successively equal to $r, 2r, 3r \dots nr$, n being any whole number; and we see by (A) that the resultant of p and q and the corresponding diagonal of the parallelogram, with those forces as sides, coincide whenever the forces have the ratios $2, 3, 4, \dots n$.

Second, let r be the unit of force, and p and q be expressed in terms of that unit which, whatever they are, will always make them as near to whole numbers as we please; then by the series above we have $\frac{BL}{DL} = \frac{r}{q}$, and $\frac{DP}{PA} = \frac{p}{r}$, whence by (B),

$$\frac{BQ}{AQ} = \frac{p}{q}$$

The proof for magnitude is the same as before.

Waterville, Maine, U.S.,
September 27, 1867.

XLIV. *On the Colour of the Clouds and Sky.*
By H. C. SORBY, F.R.S.*

THE appearance of Lommel's paper (*Pogg. Ann.* vol. cxxxi. p. 105), and of the translation in the last Number of this Magazine, induces me to publish an account of what I had written on the same subject. On February 19th of this year I gave a public lecture, and on March 5th I read a paper at the Sheffield Literary and Philosophical Society, on the Colour of the Clouds and Sky, and its connexion with the state of the weather. Within a few days of that date the following abstract was published in several of the local newspapers.

"The varied colours of the sky and clouds may be accounted for on very simple principles. The chief of these are that the clear transparent vapour of water absorbs more of the red rays of light than of any others, whilst the lower strata of the atmosphere, within no great distance of the surface of the earth, offer more resistance to the passage of the blue rays. This is especially the case at sunrise and sunset, and is probably due to impurities emanating from the earth. This effect is seen in a very intense form in the case of dark-coloured fogs, through which the red rays penetrate far more readily than the blue rays, so that the sun appears red. This is often due to the influence of a few hundred yards in thickness of such dense fogs; and the author thinks it probable that the same result would occur if the

* Communicated by the Author.

light passed through a hundred miles or more of air in which a comparatively small amount of fog was disseminated. By these two suppositions nearly all the facts of the case may be explained in the following manner:—The blue colour of the sky is due to the absorption of a considerable amount of the red light by the vapour of water present as *transparent gas* in the clear, pure atmosphere, far from the surface of the earth. If, however, minute particles of *liquid water* are present in the form of thin mist, the depth of this blue colour will be diminished; and hence in winter, and in cold countries, we have not the clear deep-blue sky of summer or of subtropical districts. The blue colour of distant mountains may also be explained in the same manner, the effect being due to the influence of the vapour of water contained in the air between the mountain and the observer, and also to some extent to the parts shaded from the direct white rays of the sun being often chiefly illuminated by the blue light of the sky. If the air be much charged with dry transparent vapour, the blue colour will be deeper; whereas if there be any mist or fog it will be obscured; and hence the blue colour is a sign of the air being loaded with vapour, which indicates rain. At sunrise and sunset the light of the sun would have to pass through about 200 miles of atmosphere within a mile of the surface of the earth, in order to illuminate a cloud a mile from the ground. In passing through this great thickness the blue rays are absorbed to a far greater extent than the red, and much of the yellow is also removed by a number of absorption-lines, probably due to the vapour of water. Hence clouds thus illuminated are red; but when the sun rises higher, the yellow light passes more readily, and the clouds become orange, then yellow, and finally white. Clouds in different parts of the sky, or at different elevations, might show these various colours at the same time. They would perhaps be detached, and the blue sky be seen between them; but this sky would also have its colour somewhat modified by the presence of very finely divided mist, which would reflect more or less red or yellow light; and hence the sky itself would be blue overhead, passing towards the sun into a peculiar green, and then into yellow or red. We can thus easily explain all the glorious phenomena of red and yellow clouds, lying, as it were, on a blue, greenish, or orange sky mixed with the dark and almost black clouds, which are either not illuminated by the sun, or are too thick to permit the light to pass through them to the observer. In order that these phenomena may occur, it is requisite that no great masses of clouds should block out the light of the sun; and therefore when we see a fine red sunrise, we may conclude that there are few clouds within a hundred miles or more towards the east; whilst, when

we have a fine red sunset, we must conclude that there are few clouds within a hundred miles or so towards the west. Since, then, the prevailing winds are from the west, and since rain very commonly depends on the advance of clouds from the west, red clouds at sunrise indicate that clouds are advancing from the west, and that therefore rain is probable; whereas a red sunset shows that there are few or no clouds in the far west, which by their advance could bring rain, and therefore it is a sign of fine weather. These are the chief facts connected with this subject; but there are besides a number of a more complicated character, not easily accounted for, unless we also take into account a variety of circumstances the full explanation of which would make this notice far too long."

In such an abstract I could only give a very general account of my views; and I purposed to publish a more complete paper when I had collected more facts, and had had the opportunity of reexamining some kinds of coloured clouds that I have not since seen. In the paper read at the Meeting, but not published, I contended that it is often mainly the minute particles of dust and fog which occur in the atmosphere, comparatively near the surface of the earth, that give rise to the red colour of the clouds at sunrise and sunset, and that therefore these magnificent colours, which we are apt to look on as so very celestial, are to a great extent due to the atmosphere being contaminated with terrestrial impurities.

I must confess that I was quite unacquainted with Lommel's previous more mathematical paper in Grunert's *Archiv* (1861, xxxvi.); but from what I have said it will be seen that previously to his paper in Poggendorff's *Annalen* I had been led to form very similar conclusions; and at the same time it appears to me that what I have said with reference to the effect of the clear transparent vapour of water will serve to remove a number of difficulties connected with the subject. I say this, notwithstanding the facts described by Janssen (Brit. Assoc. Report, 1866, Sects. p. 11), which I think admirably account for the blue colour of the sky, and for the greatly increased intensity of the narrow black atmospheric lines in the red end of the spectrum of morning and evening light, but do not explain why, independent of narrow black lines, the intensity of the blue end is so greatly diminished, which I attribute, with Lommel, to the presence of minute solid or liquid particles.

*XLV. An Inquiry into the Evidence on which the Theory of the Circulation of the Atmosphere is based. By J. KNOX LAUGHTON, B.A., R.N.**

THE commonly received theory of the trade-winds and of the circulation of air over the globe, which has been more fully enunciated and more elaborately argued out by Captain Maury than by any other writer, is, I conceive, open to very grave objections. The diagrams are so simple, and the scheme so complete in its details, that it obtains at once a ready assent to its truth; but further consideration leads to the question, How is it possible for a body of air whose density is (in round numbers) represented by 30 inches of mercury, to be forcibly displaced by a body of air which has a density of only 20 inches? or, conversely, How can heavy air rise through light and form an upper current above it? When, then, we find it asserted that such extraordinary transpositions do take place, whether in the latitude of 30° N. or S., or at the poles, we are not unnaturally led on to inquire into the evidence on which this assertion is founded, and on which, in point of fact, the whole system must be supposed to rest.

A distinctive and important feature of the scheme is the assumption that, between the tropics (or rather between the latitudes of 40°) and the poles, the prevailing winds are westerly and towards the pole; that is to say, are from the south-west in the northern, and from the north-west in the southern hemisphere. This is distinctly laid down by Maury in his "Diagram of the Winds" (Sailing Directions, vol. i. plate 3, eighth edition), as well as in the accompanying text. His words are:—

"From the parallel of 40° up toward the north pole the prevailing winds are the south-west passage winds; these, in the Atlantic, prevail over the easterly winds in the ratio of about two to one. Now, if we suppose, and such is probably the case, these westerly winds to convey in two days a greater volume of atmosphere towards the arctic circle than those easterly winds can bring back in one, we establish the necessity for an upper current," &c. (Sailing Directions, vol. i. p. 41.) But is this statement borne out by the evidence? The following is the mean of the observations tabulated by Howard (Climate of London, vol. i. p. 75) during a period of seventeen years:—

Between N. and W.	105		Between S. and W.	104
,, N. and E.	74		,, S. and E.	56

or, more shortly, winds

From the pole, Polar	179
To the pole, Equatorial . .	160

* Communicated by the Author.

This was the ratio observed, in a long series of years, in the neighbourhood of London. At stations further north, we find at Melville Island,

Polar winds	470
Equatorial	119

(Coffin's "Winds of the Northern Hemisphere," in Smithsonian Contributions to Knowledge, vol. vi.)

At Felix Harbour,

Polar winds	13347
Equatorial	7010

(Ibid.). And from Colonel Sabine's "Report of the Observations at Toronto in 1840, 1841, and 1842" I obtain the following:—

Winds blew from	Hours.	Pressure in pounds.
North	1245	1055·9
Between North and West	2411	3401·1
„ East	1999	1379·7
Total polar.....	5655	5836·7
South	688	189·6
Between South and West	2291	2196
„ East	1897	907·1
Total equatorial	4876	3292·7
West	749	613·3
East	979	752·4

It would be easy to give many more Tables all bearing the same witness; those I have given, however, are enough for my purpose. But I would call attention to the fact, which want of space has prevented my tabulating except in one instance, that the greater number of polar winds are also westerly, whereas the circulation of the air according to the received idea would necessarily cause the polar currents to be (as a rule) easterly. The air pressing to the equator from the pole, and deflected from its southerly course by reason of the rotation of the earth, must be deflected towards the west, and could not have an easterly tendency.

The assumption of certain belts or zones of calms is another important part of the scheme; and indeed we can quite understand how an interchange between the upper and lower currents, such as is shown in Maury's diagram already referred to, might produce such calms; but, as a point resting on evidence, the existence of such regions of calm round the poles, whether north

or south, is wholly hypothetical, and unsupported by any testimony whatever. They may be there, or they may not; it is impossible to say; and we might with as much reason, as far as evidence goes, assert the prevalence of furious hurricanes.

The calms which have been named the calms of Cancer come more directly within the limits of evidence, and the testimony of thousands of seafaring men who cross the tropic every year can readily be brought forward. Personal experience has, not unnaturally, considerable influence on the judgment of an inquirer; and I may perhaps be excused if I quote from my own journal that, in May 1856, H.M.S. 'Calcutta' (a rather dull sailing line-of-battle ship) ran from the Channel into the trades with a fresh northerly wind the whole way. Between the latitudes of 45° N. and 25° N she made never less, generally considerably more, than 150' a day, a distance which was for her a very good run. Our longitude was then, being outward bound, between 13° and 23° W. On the homeward passage, in July 1859, we found calms and very light winds through nearly our whole course from 30° N. 40° W. to 42° N. 32° W., and were a fortnight between these two positions.

La Pérouse, in August 1785, ran from Brest into the trade, in longitude 16° W., without suffering from calms, and with a prevailing wind from N.E. The expedition in search of La Pérouse, leaving Brest in the end of September 1791, found a fresh north-easterly wind in latitude 46° N., longitude 8° W., which carried them to Teneriffe, and thence into the trades in longitude 17° or 18° W.

Vancouver, under date April 1791, writes, "The wind, which had been generally from the west, veered round by the north as we advanced to the southward, and settled in the N.E. trade, accompanied with fine pleasant weather, which on the 26th, in latitude $30^{\circ} 54'$ N., afforded me an opportunity of obtaining several sets of lunar distances their mean result showed the longitude to be $16^{\circ} 21' 32''$." (Voyages, vol. i. p. 8.)

Cook's testimony is to the same effect. In 1772, he says, "We left Madeira on the 1st of August and stood to the southward, with a fine gale at N.E." And in 1776, "We passed Cape Finisterre on the afternoon of the 24th [July] with a fine gale at N.N.E. at four in the afternoon of the 31st we saw Teneriffe."

And leaving on one side special instances, which may perhaps be counted as exceptional, I would refer to the Tables of runs and crossings from the Lizard to the Line, given by Maury (Sailing Directions, vol. ii.), from which I have taken the following:—

Phil. Mag. S. 4. Vol. 34. No. 231. Nov. 1867. 2 B

From latitude.....	45° to 35°	35° to 30°	30° to 25°
Mean number of days .	5·1	3	2·5
Crossing the last-named latitude in west lon- gitude	} 16°	18°·8	21°·5

This Table, giving the means of a very large number of observations, shows no indications of any undue delay in the latitude where the zone of calms is laid down in the diagram. Neither does the Chart published by the Board of Trade show any such calms extending across the Atlantic, but represents them rather as confined to the immediate neighbourhood of the Western Islands. Further east, under the same latitude, we find a very marked preponderance of north-easterly winds; and further west, south-westerly winds seem to establish themselves, even in a lower latitude. The chart shows this, and the extracts of logs given in the 'Sailing Directions' (vol. ii. pp. 156-340) are witness to the same; and the whole body of evidence points to a *patch*, not a *zone* of calms in the North Atlantic. I would suggest that, in conceiving such a zone, geographers have been carried away by a fanciful tradition of ships becalmed for days and weeks in the *horse* latitudes. How the traditionary story of horses thrown overboard for want of water has arisen, I am unable to say; but I would call attention to the historical fact that the name was originally given as denoting the boisterous and tempestuous nature of the region which received it: this region was the part of the sea in the neighbourhood of Bermuda—the "still-veged Bermoothes" of our ancestors—and was called by the old Spaniards "el golfo de las yeguas" (the mare's gulf), in contradistinction to the trade-wind region, which was called "el golfo de las damas" (the ladies' gulf). (Humboldt's 'Personal Narrative,' by Williams, vol. ii. p. 8; Churchill's 'Voyages,' edit. 1704, vol. i. p. 658.)

The evidence which I have thus briefly adduced seems to me to lead to the conclusion that in the North Atlantic the air circulates in a sort of enormous whirlpool, the centre or vortex of which is, on the average, somewhere near the Azores, where it produces what may be considered a species of atmospheric Sargasso sea, and that towards the north or north-west of this circle the denser air from the pole keeps up a constant pressure, which bends down the south-westerly current, causing it to stream more from the west and afterwards from the north-west.

A great part of these winds, further deflected, forms the northerly and north-easterly winds on the western side of Europe; but a very large portion appears to sweep over the north of the Continent as far as the Ural Mountains, continuing in its east-

ward course to throw off branches to the southward, which, as violent gales in the Gulf of Lyons, or under the various names of *mistral*, *tramontana*, *bora*, *gregaglia*, and *levanter*, are well known to Mediterranean cruisers. These, again, there is fair reason to suppose, form the eastern wind which so largely prevails over the deserts of North-western Africa, and the hot wind charged with red sand so often felt near the Cape de Verde Islands, where it mingles with the trade, and is lost in the general system of circulation.

In comparison with the North Atlantic, other oceans may be considered as but little known, or not at all except in those parts where a quick-witted experience has mapped out the roads to distant ports. But examining the logs of ships making long voyages (and there can scarcely be a more valuable collection than that given in the 'Sailing Directions'), we find a mass of evidence amounting almost to satisfactory proof that the calm region in the South Atlantic is confined to the neighbourhood of Tristan d'Acunha, and towards the north-west quarter from that island, that in the western part of the ocean the south-east trade, in a comparatively low latitude (in 18° or 20°), very commonly changes to a north-easterly, and so, through north, to a north-westerly breeze, whilst on the eastern side the southerly or south-easterly wind frequently, and in the southern summer generally, carries homeward-bound ships from the Cape into the trades without hindrance from any intermediate calms. From the accounts of travellers in South America there is reason to believe that the trade-wind also recurves over the continent. The northern part of this huge mass of land is so flat that the influence of the tide is felt on the great rivers very far into the interior; the trade-wind blows as nearly as possible due east up the lower Amazon for a great part of the year, but on the upper Amazon it is never felt, whilst on the other hand it blows daily and with remarkable force up the Tapajos, a large tributary of the Amazon which flows from the south. (Bates, 'Naturalist on the Amazons,' vol. ii. pp. 143 *et seq.*)

Captain Maury represents the trade-wind as passing over the tops of the Andes and descending on Peru as a wind intensely dry. (Sailing Directions, vol. i. p. 25.) This is scarcely borne out by evidence. There is no proof that the trade reaches so far inland as even the foot of the mountains; these are, moreover, on that side exceedingly steep, and present a barrier to the passage of the wind which, in the absence of proof to the contrary, I believe to be insuperable. The force required to raise such a mass of air to such a height would be enormous; the necessary horizontal force acting against a very steep and very rough inclined plane would be almost beyond conception. But in point

of fact the unanimous testimony of reliable witnesses is strongly opposed to such a theory. At high elevations Humboldt found icy winds from the south; Condamine and Ulloa make mention of the same; nor on the west side do we find any notice of that excessive dryness or of the easterly winds which Maury describes.

The notes of Captain Betagh, written 150 years ago, so fully embody the evidence of all later observers that I give them in his own words, so far as they bear on the question.

“One would expect it much hotter thereabouts [near Lima] than it is, there being no proportion between the heat of this climate in America and the same degree of latitude in Africa; for which there are two reasons: the one is the cool temper of the air proceeding from the congealed snow on the mountains which diffuses itself every way; the other is the humidity of the vapours which hang over the plains, and which are so frequent that, when I came first to Lima, I often expected it would rain. These vapours are not so coarse, low, and gloomy as our fogs; nor separated above like our summer clouds, but an exhalation between both, being spread all round, as when we say the day is overcast; so that sometimes a fine dew is felt upon the outward garments, and discerned by the eye upon the nap of the cloth. . . . The south-west winds blow upon the Peruvian coast all the year round . . . these easy gales always bear away the vapours from the plains before they can rise and form a body sufficient to descend in a shower; but when they are carried further and higher they grow more compact, and at length fall by their own weight into rain.” (Pinkerton’s ‘*Voyages*,’ vol. xiv. p. 8.)

And Dampier some years earlier had written:—“On the coast of Peru these [southerly] winds blow 140 or 150 leagues off shore before you can perceive them to alter; but then as you run further off so the wind will come about more easterly, and at about 200 leagues distance it settles at E.S.E., which is the true trade.”

But, reverting to the Atlantic, the Board of Trade Chart of the coast of Brazil lends a strong sanction to the opinion that the S.E. trade recurves in the western part of the ocean; and Cook and Vancouver, at different times of the year, support it by their testimony with singular unanimity. The former says, “On drawing near that coast [of Brazil] we met with the wind more and more easterly, so that by the time we were in the latitude of 10° S. we could make a south-easterly course good. . . . The 6th of October [1776], being then in the latitude of $35^{\circ} 15'$ S., longitude $7^{\circ} 45'$ W., we met with light airs and calms by turns for three days successively.” And the other, “On the 9th [June 1791] we had reached the latitude of $19^{\circ} 47'$ S., lon-

gitude $27^{\circ} 27'$ W., approaching to the parallel of the islands Trinidad and Martin Vas. The wind now permitted our steering well to the eastward of south. . . . After crossing the tropic of Cancer [on the 12th] the wind became very variable, as well in point of strength as in direction, so that on the 28th we had only advanced to the latitude of $31^{\circ} 56'$ S., longitude $4^{\circ} 18'$ W."

I submit, then, that there are well-founded reasons for believing that in the South Atlantic, as in the North, the general tendency of the atmospheric currents is to circle round a central region in which calms and irregular winds prevail, and that in neither the South nor the North Atlantic is there any *zone* or *belt* of such calms extending over the whole width of the ocean.

In the Pacific the steady winds from the south-west down the coast of Chili or Peru, and from the north-west down the coast of California—winds which in both hemispheres veer through the bearing of their respective poles towards east, till on approaching the equator and at some distance from the coast they become the regular trades—seem at first to point to the conclusion that the atmosphere there also moves round in a pair of circles; but I have no evidence which will confirm such an idea by the completion of the circles on the western side of that vast ocean or over the continent of Asia. It would be foreign to my present purpose to enter into the peculiar nature of the winds that blow as well in the Western Pacific as in the Indian Ocean; but a very cursory examination will suggest that the same cause which produces the monsoons—which is, in fact, strong enough to divert the trades from their regular course—may also be strong enough to prevent their recurving in a manner similar to that which the concurrent testimony of many competent witnesses shows to take place in the North and South Atlantics; but as I have wished throughout this paper to base my argument entirely on positive evidence of facts actually experienced, rather than on any theory which might explain how or why such facts occur, or might say what might or ought to occur, I shrink from entering at present on this part of the question.

XLVI. *On Isoperimetric Regular Polygons*. By W. J. MACQUORN RANKINE, C.E., LL.D., F.R.SS.L. & E. &c.*

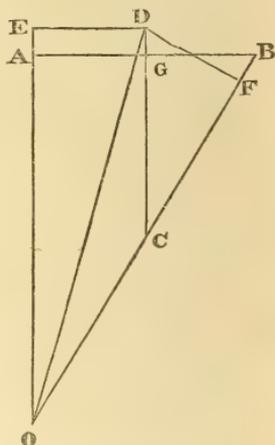
1. **T**HE process described in this paper may afford to students of mathematics an additional elementary method of testing for themselves the accuracy of the ordinary approxi-

* Communicated by the Author.

mations to the ratio which the diameter and circumference of a circle bear to each other.

2. PROBLEM.—Given any regular polygon: to construct a regular polygon of equal circumference and of double the number of sides.

3. Construction.—Let O be the centre of the given polygon, OA the radius of the inscribed circle, and OB that of the circumscribed circle (which we may call respectively the *internal radius* and the *external radius*), so that AB is a half-side of the given polygon. Bisect OB in C ; draw CD parallel to OA and equal to CB ; from D let fall DE perpendicular to OA produced, and DF perpendicular to OB ; join OD . Then will $OE=OF$ be the internal radius, and OD the external radius of the required polygon; and $DE=DF$ will be two of its half-sides.



4. Demonstration.—Let G be the point where CD cuts AB . Then because OB is bisected in C , and the triangles OAB and CGB are similar, AB is bisected in G ; and $DE=GA=\frac{1}{2}AB$. Also because $CD=CB$, and F and G are right angles, the triangles CGB and CFD are similar and equal; and $DF=BG=\frac{1}{2}AB$. So that DE and DF are two half-sides of a polygon having twice as many sides as the given polygon and of equal circumference.

5. Corollary.—Because the triangles CGB and CFD are equal, the rectangle $AEDG$ is the excess of the area of the quadrilateral $OEDF$ above that of the triangle OAB ; and if that rectangle be multiplied by the number of sides in the new polygon, the product will be the excess of the area of the new polygon above that of the original polygon. Hence it follows that if there be a series of regular polygons of equal circumference, each having double the number of sides of the preceding polygon, each polygon of that series is of a greater area than the preceding polygon; and the circle of equal circumference, being the limit towards which the polygons approximate, must have a greater area than any of them.

6. Formulae for calculation.—Let c be the common circumference of the polygons; let a denote the internal radius OA , and b the external radius OB of the original polygon; and let a' denote the internal radius OE , and b' the external radius OD of the new polygon. Let n and $2n$ be the numbers of sides in the

original polygon and the new polygon respectively. Then

$$b = \sqrt{\left(a^2 + \frac{c^2}{4n^2}\right)}, \dots \dots \dots (1)$$

$$a' = \frac{a + b}{2}, \dots \dots \dots (2)$$

$$b' = \sqrt{\left(a'^2 + \frac{c^2}{16n^2}\right)} = \sqrt{a'b}; \dots \dots (3)$$

and by repeating the calculations (2) and (3) again and again, the internal and external radii may be calculated of a series consisting of an indefinite number of polygons of equal circumference, each having double the number of sides of the preceding polygon. It is easy to see that the internal radii form an increasing series, and the external radii a diminishing series, both converging towards the radius of the circle of equal circumference as a limit.

7. The linear function of the internal and external radii which approximates nearest to the limit of both series is

$$\frac{a + 2b}{3}.$$

8. *Example.*—Taking the *semicircumference* of the polygons as the unit of length (that is, making $c=2$) and calculating to six places of decimals, we obtain the following results for a series of isoperimetric regular polygons of 6, 12, 24, and 48 sides:—

n.	a.	b.	$\frac{a+2b}{3}$.
6	0.288675	0.333333	0.318447
12	0.311004	0.321975	0.318318
24	0.316490	0.319221	0.318310 +
48	0.317855	0.318537	0.318310—

The reciprocal of 0.318310— to six places of significant figures is 3.14159+.

Glasgow University,
October 10, 1867.

XLVII. *A Contribution to Electrodynamics.*
By BERNHARD RIEMANN*.

I BEG to communicate to the Royal Society an observation which brings into close connexion the theory of electricity and of magnetism with that of light and of radiant heat. I have found that the electrodynamic actions of galvanic currents may be explained by assuming that the action of one electrical mass on the rest is not instantaneous, but is propagated to them with a constant velocity which, within the limits of errors of observation, is equal to that of light. On this assumption, the differential equation for the propagation of the electrical force is the same as that for the propagation of light and of radiant heat.

Let S and S' be two conductors traversed by constant voltaic currents but not moved towards each other; let ϵ be an electrical particle in the conductor S , which at the time t is in the point (x, y, z) ; ϵ' an electrical particle of S' , which at the time t is in the point (x', y', z') . As regards the motion of the electrical particles, which in each particle of the conductor is opposite in the negative to what it is in the positive, I assume it at each moment to be so distributed that the sums

$$\sum \epsilon f(x, y, z), \quad \sum \epsilon' f(x', y', z')$$

extended over all the particles of the conductor may be neglected as compared with the same sums if they are distributed only over the positively electrical, or only over the negatively electrical particles, as long as the function f and its differential quotients are constant.

This supposition can be fulfilled in various ways. Let us assume, for instance, that the conductors are crystalline in their smallest particles, so that the same relative distribution of the electricity is periodically repeated at definite distances which are infinitely small compared with the dimensions of the conductors; then, if β be the length of one such period, those sums are infinitely small, like $c\beta^n$, if f and their derivatives to the $(n-1)$ th degree are continuous, and infinitely small like $e^{-\frac{c}{\beta}}$ if they are all continuous.

Experimental Law of Electrodynamic Actions.

If the specific intensities expressed in mechanical measurement are u, v, w at the time t in the point (x, y, z) , parallel to the three

* Translated from Poggendorff's *Annalen*, No. 6, 1867. This paper was laid before the Royal Society of Sciences at Göttingen on the 10th of February 1858, by the author (whose premature death was such a loss to science), but appears, from a remark added to the title by the then Secretary, to have been subsequently withdrawn.

axes, and u', v', w' in the point (x', y', z') , and if r is the distance of the two points, c the constant determined by Kohlrausch and Weber, experiment has shown that the potential of the forces exerted by S upon S' is

$$-\frac{2}{cc} \iint \frac{uu' + vv' + ww'}{r} dS dS',$$

this integral being extended over the whole of the elements dS and dS' of the conductors S and S' . If, for the specific intensities of the current, we substitute the products of the velocities into the specific gravities, and then for the products of these into the elements of volume the masses contained in them, this expression passes into

$$\Sigma \Sigma \frac{\epsilon \epsilon'}{cc} \frac{1}{r} \frac{d d'(r^2)}{dt dt},$$

if the alteration of r^2 during the time dt , which arises from the motion of ϵ , be denoted by d , and that arising from the motion of ϵ' be denoted by d' .

This expression, by taking away

$$\frac{d \Sigma \Sigma \frac{\epsilon \epsilon'}{cc} \frac{1}{r} \frac{d'(r^2)}{dt}}{dt},$$

which disappears when summed with respect to ϵ , passes into

$$-\Sigma \Sigma \frac{\epsilon \epsilon'}{cc} \frac{d \left(\frac{1}{r} \right)}{dt} \frac{d'(r^2)}{dt};$$

and this again, by the addition of

$$\frac{d' \Sigma \Sigma \frac{\epsilon \epsilon'}{cc} r r \frac{d \left(\frac{1}{r} \right)}{dt}}{dt},$$

which by summing with respect to ϵ' becomes null, is changed into

$$\Sigma \Sigma \epsilon \epsilon' r r \frac{d d' \left(\frac{1}{r} \right)}{dt dt}.$$

Deduction of this Law from the new Theory.

According to the current assumption as to electrostatic action, the potential function U of arbitrarily distributed electrical

masses, if ρ denote their density at the point (x, y, z) , is defined by the condition

$$\frac{d^2U}{dx^2} + \frac{d^2U}{dy^2} + \frac{d^2U}{dz^2} - 4\pi\rho = 0$$

and by the condition that U is continuous and is constant at an infinite distance from acting masses. A particular integral of the equation

$$\frac{d^2U}{dx^2} + \frac{d^2U}{dy^2} + \frac{d^2U}{dz^2} = 0,$$

which remains continuous everywhere outside the point (x', y', z') , is

$$\frac{f(t)}{r};$$

and this function forms the potential function produced from the point (x', y', z') , if at the time t the mass $-f(t)$ is there.

Instead of this, I assume that the potential function U is defined by the condition

$$\frac{d^2U}{dt^2} - \alpha\alpha \left(\frac{d^2U}{dx^2} + \frac{d^2U}{dy^2} + \frac{d^2U}{dz^2} \right) + \alpha\alpha 4\pi\rho = 0,$$

so that the potential function from the point (x', y', z') , if the mass $-f(t)$ is there at the time t , becomes

$$= \frac{f\left(t - \frac{r}{\alpha}\right)}{r}.$$

If the coordinates of the mass ϵ at the time t are denoted by x_t, y_t, z_t , and those of the mass ϵ' at the time t' by x'_t, y'_t, z'_t , and putting for shortness' sake

$$\left((x_t - x'_t)^2 + (y_t - y'_t)^2 + (z_t - z'_t)^2 \right)^{-\frac{1}{2}} = \frac{1}{r(t, t')} = F(t, t'),$$

on this assumption the potential of ϵ upon ϵ' at the time t becomes

$$= -\epsilon\epsilon' F\left(t - \frac{r}{\alpha}, t\right).$$

The potential of the forces exercised by all the masses ϵ of the conductor S upon the masses ϵ' of the conductor S' from the time 0 to the time t becomes therefore

$$P = - \int_0^t \sum \sum \epsilon\epsilon' F\left(\tau - \frac{r}{\alpha}, \tau\right) d\tau,$$

the sums being extended over the entire masses of both conductors.

Since the motion for opposite electrical masses is opposite in each particle of the conductor, the function $F(t, t')$ by derivation with respect to t acquires the property of changing its sign with ϵ , and by derivation with respect to t' the property of changing its sign with ϵ' . Hence on the supposed distribution of the electricities, if derivation with respect to t be designated by upper, and derivation with respect to t' by lower accents, $\Sigma \Sigma \epsilon \epsilon' F_{n, n'}^{(n)}(\tau, \tau')$ distributed over all the electrical masses only becomes infinitely small as compared with the sum extended over the electrical masses of one kind when n and n' are both odd.

Let it now be assumed that during the time occupied in the transmission of the force from one conductor to the other the electrical masses pass over a very small space, and let us consider the action during a length of time compared with which the time of transmission vanishes. In the expression for P ,

$$F\left(\tau - \frac{r}{\alpha}, \tau\right)$$

can be replaced by

$$F\left(\tau - \frac{r}{\alpha}, \tau\right) - F(\tau, \tau) = - \int_0^{\frac{r}{\alpha}} F'(\tau - \sigma, \tau) d\sigma,$$

since $\Sigma \Sigma \epsilon \epsilon' F(\tau, \tau)$ may be neglected.

There is thus obtained

$$P = \int_0^t d\tau \Sigma \Sigma \epsilon \epsilon' \int_0^{\frac{r}{\alpha}} F'(\tau - \sigma, \tau) d\sigma;$$

or if the order of the integrations be inverted and $\tau + \sigma$ put for τ ,

$$P = \Sigma \Sigma \epsilon \epsilon' \int_0^{\frac{r}{\alpha}} d\sigma \int_{-\sigma}^{t-\sigma} d\tau F'(\tau, \tau + \sigma).$$

If the limits of the inner integral be changed to 0 and t , at the upper limit the expression

$$H(t) = \Sigma \Sigma \epsilon \epsilon' \int_0^{\frac{r}{\alpha}} d\sigma \int_{-\sigma}^0 d\tau F'(t + \tau, t + \tau + \sigma)$$

will be thereby added, and at the lower limit the value of this expression for $t=0$ will be taken away. We have thus

$$P = \int_0^t d\tau \Sigma \Sigma \epsilon \epsilon' \int_0^{\frac{r}{\alpha}} d\sigma F'(\tau, \tau + \sigma) - H(t) + H(0).$$

In this expression $F'(\tau, \tau + \sigma)$ can be replaced by $F'(\tau, \tau + \sigma) - F'(\tau, \tau)$, since

$$\sum \sum \epsilon \epsilon' \frac{r}{\alpha} F'(\tau, \tau)$$

may be neglected. An expression is thereby obtained as a factor of $\epsilon \epsilon'$, which changes its sign both with ϵ and ϵ' ; so that in the additions the members do not cancel one another, and infinitely small fractions of the individual members may be disregarded.

Hence, substituting $\sigma \frac{dd' \left(\frac{1}{r} \right)}{d\tau d\tau}$ for $F'(\tau, \tau + \sigma) - F'(\tau, \tau)$, and integrating with respect to σ , we obtain

$$P = \int_0^t \sum \sum \epsilon \epsilon' \frac{rr}{2\alpha\alpha} \frac{dd' \left(\frac{1}{r} \right)}{d\tau d\tau} d\tau - H(t) + H(0),$$

to a fraction which may be neglected.

It is easily seen that $H(t)$ and $H(0)$ may be neglected; for

$$F'(t + \tau, t + \tau + \sigma) = \frac{d \left(\frac{1}{r} \right)}{dt} + \frac{d^2 \left(\frac{1}{r} \right)}{dt^2} \tau + \frac{dd' \left(\frac{1}{r} \right)}{dt dt} (\tau + \sigma) + \dots,$$

and therefore

$$H(t) = \sum \sum \epsilon \epsilon' \left(\frac{rr}{2\alpha\alpha} \frac{d \left(\frac{1}{r} \right)}{dt} - \frac{r^3}{6\alpha^3} \frac{d^2 \left(\frac{1}{r} \right)}{dt^2} + \frac{r^3}{6\alpha^3} \frac{dd' \left(\frac{1}{r} \right)}{dt dt} + \right).$$

But here only the first member of the factor of $\epsilon \epsilon'$ is of the same order with the factor in the first constituent of P ; and this, on account of the summation with respect to ϵ' , yields only a fraction of it which may be neglected.

The value of P obtained from our theory agrees with the experimental one,

$$P = \int_0^t \sum \sum \epsilon \epsilon' \frac{rr}{cc} \frac{dd' \left(\frac{1}{r} \right)}{d\tau d\tau} d\tau,$$

if we assume $\alpha\alpha = \frac{1}{2}cc$.

According to Weber and Kohlrausch's determination,

$$c = 439450 \cdot 10^6 \frac{\text{millimetre}}{\text{second}},$$

which gives $\alpha = 192965$ miles in a second, while for the velocity of light Busch has calculated the number 193172 miles from Bradley's observation of aberration, and Fizeau has obtained the number 192757 by direct measurement.

XLVIII. *On the Velocity of Atomic Motion.*

By Dr. ALEXANDER NAUMANN*.

I HAVE shown in a previous communication† that the three component parts into which, with reference to their different functions, the specific heat of perfect gases under constant pressure may be resolved, namely the heat of expansion, the heat of molecular motion, and the heat of atomic motion, are to each other in the constant ratio $2 : 3 : n$, where n denotes the number of atoms contained in one molecule. But since the heat of expansion is expended in the performance of external work, the total thermal content of a gas is represented by the *vis viva* of the progressive motion of the molecules together with the *vis viva* of the motion of the atoms inside the molecules.

Now the absolute zero for perfect gases is fixed at $-273^{\circ}\text{C}.$, or more exactly at $-274^{\circ}\cdot6\text{C}.$ ‡, upon the assumption that the quantity of heat taken up by a gas is constant for equal increments of temperature. Hence it follows that the quantity of heat contained in a gas must be proportional to the absolute temperature, and that the *vis viva* of the progressive motion of the molecules is to the *vis viva* of the motions of the atoms inside the molecules in the same ratio as the heat of molecular motion to the heat of atomic motion—that is to say, in the ratio $3 : n$.

Let us first take the case of gases for which the relations that we have to investigate assume the simplest form. These are the simple gases containing in one molecule two similar atoms, as oxygen, nitrogen, hydrogen, for which the ratio above-mentioned = $3 : 2$, and the velocities of the two atoms (the nature of whose motion we shall have to consider more particularly hereafter) must be supposed equal.

Now let u be the mean velocity (expressed in metres per second) of the progressive motion of the molecules, and v the velocity of the two atoms. Then, since the sum of the masses of the two equal atoms is the same thing as the mass of the molecule, the two *vires vivæ* bear to each other the same relation as the squares of the velocities, and therefore

$$\frac{v^2}{u^2} = \frac{2}{3}, \text{ whence } v = u \sqrt{\frac{2}{3}} = 0\cdot8165u.$$

* Translated from *Ann. der Chem. und Pharm.* vol. cxlii. p. 284 (June 1867).

† *Ibid.* p. 265. *Phil. Mag.* S. 4. vol. xxxiv. p. 205.

‡ *Ad. Dronke, Pogg. Ann.* vol. cxix. p. 392 (1863).

But, according to Clausius*, the mean† velocity of the progressive motion of any gas-molecule is

$$u = 485^m \sqrt{\frac{273+t}{273\rho}}$$

where t denotes the temperature, and ρ the specific gravity of the gas in question. The absolute zero, which is here taken as 273° C. below the freezing-point, is, as was remarked above, more accurately 274.6° C. below the freezing-point; accordingly we have for the velocity of the atoms,

$$v^t = 0.8165 \cdot 485^m \sqrt{\frac{274.6+t}{274.6\rho}} \dots \dots (1)$$

For 0° , the equation becomes

$$v_0 = 0.8165 \cdot 485^m \sqrt{\frac{1}{\rho}} = \frac{396^m}{\sqrt{\rho}} \dots \dots (2)$$

Hence we get the following velocities for the motion of the atoms at 0° :—

	Velocity of an atom metres.	Velocity of a molecule (Clausius). metres.
For Oxygen . . .	376	461
For Nitrogen . . .	402	492
For Hydrogen . . .	1506	1844

Now of what kind is the motion of the atoms? A movement corresponding to that of the molecules, or, so to say, an indefinite wandering about within the sphere of their molecules, is inadmissible, if only on the ground that, if such were the case, metameric gases must be continually changing one into another, and we should obtain, by cooling, liquid mixtures of all possible metameric compounds, which is well known not to be the case. We are thus compelled to suppose an oscillatory movement of the atoms towards and away from each other. In order that this may occur, the mutual repulsion of two atoms must diminish, as the distance between them increases, more rapidly than their mutual attraction. The position of equilibrium, then, is the distance at which attraction and repulsion are in equilibrium and mutually neutralize each other. The velocity as above determined, which is to be taken into account in calculating the *vis viva* of the motion of an atom, is the velocity with which an atom passes its position of equilibrium, or its so-called *intensity of vibration*. As the temperature rises, the velocity of an atom in passing its

* Pogg. Ann. vol. c. p. 377 (1857). † Ibid. p. 372.

position of equilibrium increases, according to equation (1), in proportion to the square root of the absolute temperature; and the amplitude of vibration increases at the same time. (It may be remarked here, in passing, that it is evident from these considerations that every compound body must be capable of being decomposed by heat; for as the *vis viva* of the atomic motion continues to increase, and therefore also the amplitude of the vibrations of the atoms, the attracting forces will, after a certain limit, no longer be able to hold back the atoms within the sphere of the molecule.) The velocity of the two atoms after they have passed their position of equilibrium, moving towards each other, will be quickly but continuously diminished down to zero in consequence of the constantly increasing preponderance of the repulsion; the atoms then begin to move back, and under the influence of the repulsion, which acts as an accelerating force until they come to their position of equilibrium, they arrive there with their original velocity but moving in the opposite direction. This velocity now rapidly diminishes down to zero, as they recede further from each other, in consequence of the greater and greater preponderance acquired by the attraction; and then the opposite movement takes place in obedience to the same forces. When they reach the position of equilibrium, the atoms have again acquired their original velocity, to the square of which (the body being supposed always the same) the quantity of heat which presents itself in the form of atomic motion is proportional.

The conditions of the movement of the atoms are less simple when, as is for the most part the case, the molecule is composed of several dissimilar atoms. But since in these more complex cases also the total *vis viva* of atomic motion is proportional to the number of atoms, we must assume that the *vis viva* of the separate atoms is at least on the whole the same. The velocity corresponding to the thermal content, which expresses itself in the motion of an atom, is the velocity which the atom would have if the various attracting and repelling forces which act upon it mutually held each other in equilibrium; this is likewise the velocity which must be denoted as the intensity of vibration. Thus if v' and v'' represent these velocities for two atoms, and N' and N'' their masses, we have

$$\frac{1}{2} N' v'^2 = \frac{1}{2} N'' v''^2, \text{ whence } v'' = v' \sqrt{\frac{N'}{N''}}$$

But, according to equation (1), we have for hydrogen, whose specific gravity = $\rho = 0.0692$,

$$v_t = 0.8165 \cdot 485^m \sqrt{\frac{274.6 + t}{274.6 \cdot 0.0692}};$$

and therefore, for any other atom,

$$v_t'' = 0.8165 \cdot 485^m \sqrt{\frac{(274.6 + t)N'}{274.6 \cdot 0.0692N''}}$$

$\frac{N'}{N''}$, however, is also the ratio of the atomic weights; and since the atomic weight of hydrogen = 1, we have $\frac{N'}{N''} = \frac{1}{N''}$. Consequently, if N represents in general the atomic weight of any simple body referred to that of hydrogen as unity, the velocity of an atom of this body in gaseous compounds is represented by

$$v_t = 0.8165 \cdot 485^m \sqrt{\frac{274.6 + t}{274.6 \cdot 0.0692 \cdot N}} \quad \dots \quad (3)$$

For 0° C. this equation becomes

$$\begin{aligned} v_0 &= 0.8165 \cdot 485^m \sqrt{\frac{1}{0.0692 \cdot N}} = \frac{396^m}{\sqrt{0.0692 \cdot N}} \\ &= \frac{1506^m}{\sqrt{N}} \dots \dots \dots (4) \end{aligned}$$

Thus, for example, the velocity of an atom of carbon at 0° in a gaseous compound (such as marsh-gas, C H⁴, or ethylene, C² H⁴) would be

$$v_0 = \frac{1506^m}{\sqrt{12}} = 435^m.$$

Equation (3) expresses that *the same atom possesses in all its gaseous compounds the same velocity, and consequently the same intensity of vibration, at the same temperature.* The same equation expresses further that *the velocity or intensity of vibration is directly proportional to the square root of the absolute temperature, and inversely proportional to the square root of the atomic weight.*

There is as yet no conclusive reason for supposing that the velocity of the motion of the atoms in a molecule of liquid or solid bodies is different at the same temperature from what it is in gaseous bodies; and therefore it cannot for the present be asserted that the application of equation (3) to the atoms of liquid and solid bodies also is inadmissible.

Giessen, March 5, 1867.

XLIX. *On the Figure of the Bullet which experiences the least resistance from the Air.* By FRANCIS A. TARLETON, Fellow and Tutor, Trinity College, Dublin*.

TO find a surface of revolution whose volume and the diameter of whose base are given, and such that it may experience the least possible resistance in passing through the air in a direction parallel to its axis. The resistance of the air on any element of the surface is supposed to act in the normal to the element, and to vary as the square of the normal velocity.

Take the axis of revolution for axis of x , and let $2y_1$ be the diameter of the base, then

$$\int_0^{y_1} 2\pi y \kappa v^2 \left(\frac{dy}{ds}\right)^2 dy + a \int_0^{y_1} \pi y^2 \frac{dx}{dy} y_1 = \text{a minimum.}$$

Let

$$\frac{a}{2\kappa v^2} = b,$$

and

$$\int_0^{y_1} \left\{ y \left(\frac{dy}{ds}\right)^2 + by^2 \frac{dx}{dy} \right\} dy = \text{a minimum.}$$

Take the origin where the curve meets the axis of x ; then $x_0 = 0$ x_1 is wholly unrestrained. The Calculus of Variations gives

$$\left(by^2 - 2y \frac{dx}{ds^4} \frac{dy^3}{ds^4} \right)_1 \delta x_1 - \int_0^{y_1} \frac{d}{dy} \left\{ by^2 - 2y \frac{dx}{ds^4} \frac{dy^3}{ds^4} \right\} \delta x dy = 0.$$

Hence

$$\frac{d}{dy} \left\{ by^2 - 2y \frac{dx}{ds^4} \frac{dy^3}{ds^4} \right\} = 0;$$

$$\therefore by^2 - 2y \frac{dx}{ds^4} \frac{dy^3}{ds^4} = C;$$

but

$$\left(by^2 - 2y \frac{dx}{ds^4} \frac{dy^3}{ds^4} \right)_1 = 0;$$

$$\therefore C = 0.$$

Rejecting the factor $y = 0$, we have the equation

$$by = 2 \frac{dx}{ds^4} \frac{dy^3}{ds^4}$$

for the generating curve. Hence $y = \frac{2}{b} \sin \theta \cos^3 \theta$, if θ be used

* Communicated by the Author.

to denote the angle made with the axis of y by the tangent to the curve, and

$$dy = \frac{2}{b} (\cos^4 \theta - 3 \cos^2 \theta \sin^2 \theta) d\theta,$$

$$dx = \tan \theta dy = \frac{2}{b} \{ \cos^3 \theta \sin \theta - 3 \sin^3 \theta \cos \theta \} d\theta;$$

hence

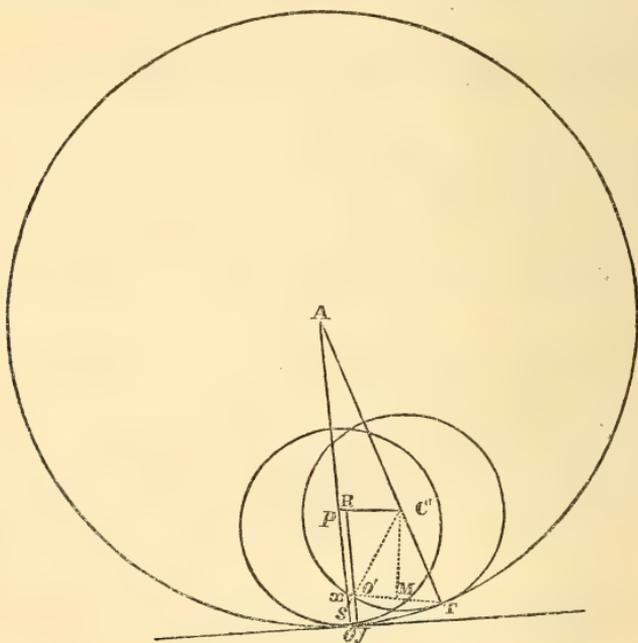
$$\begin{aligned} x &= \frac{2}{b} \left\{ -\frac{1}{4} \cos^4 \theta - \frac{3}{4} \sin^4 \theta \right\} + C' = \frac{1}{2b} \{ -\cos^4 \theta - 3 \sin^4 \theta \} + C' \\ &= -\frac{1}{8b} \{ 4 - 4 \cos 2\theta + 4 \cos^2 2\theta \} + C' = \frac{1}{2b} \cos 2\theta (1 - \cos 2\theta) + C''. \end{aligned}$$

Take the origin where $\theta = 90$, and let $\frac{1}{2b} = c$, then $C'' = 2c$, and we have, finally,

$$x = 2c + c \cos 2\theta (1 - \cos 2\theta),$$

$$y = c \sin 2\theta (1 + \cos 2\theta).$$

From these equations it can be proved that the curve is a hypocycloid, generated by a point fixed on the circumference of a circle rolling on the inside of another circle with three times as great a radius.



Take two positions of the moving circle, O O' being the posi-

tions of the generating point ; then the arc

$$TO = \text{arc } TO' ; \therefore \angle O' C' T = 3\angle O A T.$$

Take OA for axis of x , and the tangent to the two circles at O for axis of y , OA is clearly the tangent to the hypocycloid at the cusp O . The tangent at O' is perpendicular to $O'T$, and therefore parallel to $C'M$ drawn to bisect $O'T$ from the centre C' . Let the angle $OAT = \psi$, $\angle O' C' T = 3\psi$. Again, $\frac{3}{2}\psi = \angle M C' T = \psi +$ angle between MC' and $O'A$; therefore this latter $\angle = \frac{1}{2}\psi$. Hence the angle between the tangent at any point and the tangent at the cusp $= \frac{1}{2}\psi$. The coordinates of O' are $x = OX$ and $y = OY$,

$$x = OA - O'R - PA = 3r - r \cos 2\psi - 2r \cos \psi,$$

$$y = C'P - C'R = 2r \sin \psi - r \sin 2\psi ;$$

x may be written

$$4r - r(1 + \cos 2\psi) - 2r \cos \psi = 4r - 2r \cos \psi(1 + \cos \psi),$$

$$y = 2r \sin \psi(1 - \cos \psi).$$

Now let θ be the angle between the tangent at O' and the axis of y , then

$$\theta = 90^\circ - \frac{1}{2}\psi ; \therefore \psi = 180^\circ - 2\theta,$$

hence

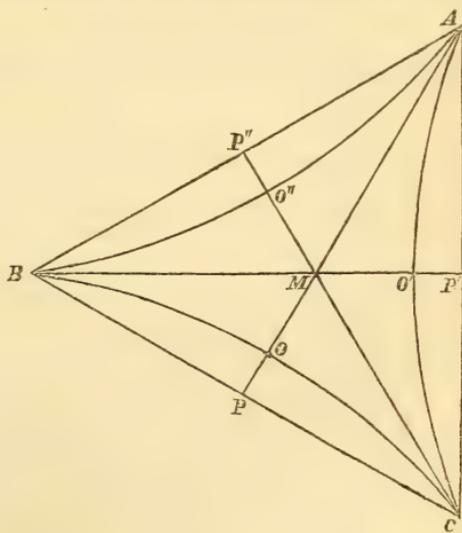
$$x = 4r + 2r \cos 2\theta(1 - \cos 2\theta),$$

$$y = 2r \sin 2\theta(1 + \cos 2\theta).$$

But these are the same equations as found for the generating curve of the solid of least resistance if $2r$ be assumed equal to c .

The shape of the curve is therefore evident. It consists of three symmetrical branches, and has three cusps ; moreover the tangent at a cusp is the axis of revolution of the required surface, and the lines joining the cusps form an equilateral triangle.

In the subjoined figure the origin may be supposed to have been taken at the point B . If the origin be moved to the point O' , we have



$$x = c \cos 2\theta(1 - \cos 2\theta),$$

$$z = c \sin 2\theta(1 + \cos 2\theta).$$

From these equations 2θ can be readily eliminated, and the equation of the curve is found to be

$$(x^2 + y^2)^2 + 2cx(3x^2 - 5y^2) + 12c^2x^2 - c^2y^2 + 8c^3x = 0.$$

If we refer the curve to the three lines joining the cusps, its equation becomes

$$a^2\beta^2 + \beta^2\gamma^2 + \gamma^2\alpha^2 - 2a\beta\gamma^2 - 2\beta\gamma\alpha^2 - 2\gamma\alpha\beta^2 = 0.$$

I had worked out these equations and had found the shape of the curve and several of its properties before I knew it was a hypocycloid. I mentioned some of its properties to Mr. Burnside, who said he thought it must belong to the cycloidal class. It was easy to see that, if this were so, one circle must have a circumference three times the other; and on investigation I easily proved, as above, that the hypocycloid generated as described coincides with the curve generating the surface of least resistance. One question still remains, Which branch of the curve is the one of least resistance? Is it that which meets the axis in a cusp and touches it there, or that which cuts it at right angles? To solve this question we must recur to the Calculus of Variations.

Let

$$V = y \left(\frac{dy}{ds} \right)^2 + by^2 \frac{dx}{dy}.$$

To apply Jacobi's test, we have

$$\frac{d^2V}{\left(\frac{dx}{dy} \right)^2} = - \frac{4}{b} \sin \theta \cos^9 \theta (1 - 3 \tan^2 \theta);$$

and taking the cusp B for origin from B to A, $\frac{d^2V}{\left(\frac{dx}{dy} \right)^2}$ is posi-

tive, and hence the resistance is a minimum; but if we take O' for origin, from O' to A $\frac{d^2V}{\left(\frac{dx}{dy} \right)^2}$ is negative, and the resistance

is a maximum. Hence one branch of the curve generates the solid of least resistance, and the other the solid of greatest resistance, the volume in each case being given.

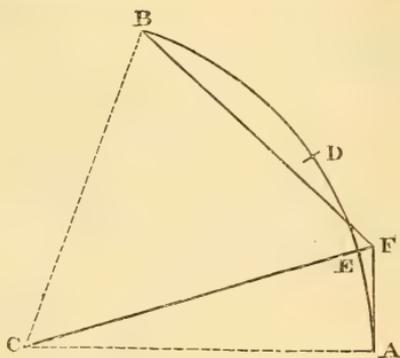
The foregoing investigation appears to possess some importance in connexion with the inquiry as to the shape best suited for the termination of long-range bullets. Part of it was published in Major Leech's 'Rifle Shooting in Ireland;' but I was so hurried in the preparation of my chapter in order not to delay the publication that I was unable to complete it as I should have wished.

L. *On the Approximate Rectification of Circular Arcs; being a second Supplement to a Paper read to the British Association.*
By W. J. MACQUORN RANKINE, C.E., LL.D., F.R.S.S.L. & E.*

1. **RULE.**—To rectify approximately the circular arc AB,

whose centre is at C.

Bisect the arc AB in D and the arc AD in E; so that $AE = \frac{1}{4} AB$. Draw the straight tangent AF and the radius CE, and produce that radius till it cuts the tangent in F. Draw the straight line FB. Then $AF + FB$ will be approximately equal in length to the arc AB.



2. **Error.**—The error is in excess; it is approximately one

four-thousandth part of the arc, for an arc equal in length to its own radius; and its value, in fractions of the length of the arc, varies nearly as the fourth power of the angle subtended by the arc.

3. **Demonstration.**—Take the radius CA as the unit of length; and let θ be the angle, in circular measure, subtended by the arc AB. Then $AF = \tan \frac{\theta}{4}$; and it is easily found by trigonometry that

$$FB = \sqrt{\left\{ \tan^2 \frac{\theta}{4} + 8 \sin^2 \frac{\theta}{4} \right\}};$$

consequently

$$AF + FB = \tan \frac{\theta}{4} \left\{ 1 + \sqrt{\left(1 + 8 \cos^2 \frac{\theta}{4} \right)} \right\}.$$

For brevity's sake, let $\tan \frac{\theta}{4}$ be denoted by t . Then, by developing the preceding expression in powers of t , we obtain the following series,

$$AF + FB = 4t \left\{ 1 - \frac{t^2}{3} + \frac{7t^4}{27} \text{ \&c.} \right\};$$

but by a well-known development we have

$$AB = \theta = 4t \left\{ 1 - \frac{t^2}{3} + \frac{t^4}{5} \text{ \&c.} \right\};$$

* Communicated by the Author.

therefore

$$\frac{AF + FB - AB}{AB} = \frac{8t^4}{135} \text{ nearly;}$$

which, when t is small, approximates to

$$\frac{\theta^4}{4320}$$

Glasgow University,
September 28, 1867.

Addendum.

The error of the rule in this second Supplement, in fractions of the length of the arc, when developed to two terms, is

$$+\frac{\theta^4}{4320} + \frac{101\theta^6}{3483648};$$

and the error of Rule III., in the Appendix to the paper as published in the Philosophical Magazine for October 1867, is

$$-\frac{\theta^4}{1080} - \frac{\theta^6}{54432};$$

therefore, to *four-fifths* of the length found by the rule with the positive error, add *one-fifth* of the length found by the rule with the negative error, and the sum will be an approximate length whose error, in fractions of the length of the arc, will be approximately

$$+\frac{17\theta^6}{870912}$$

Glasgow University,
October 16, 1867.

LI. *Contribution to the Mechanical Theory of the Electrical Current.* By DR. GERLACH*.

THE principle of the mechanical theory of heat, that in the transformation of heat into work or of work into heat the unit of heat always corresponds to a definite amount of work, has both theoretically and practically been confirmed in the process of electrodynamic heating; and in consequence thereof the hypothesis that electricity is a phenomenon of motion has acquired fresh support. Hence it may be anticipated that electrodynamic laws and those of the motion of solid substances will

* Translated from Poggendorff's *Annalen*, July 1867.

agree in many respects. The present research has the object of calling attention to such an agreement. I will endeavour at the outset, for the sake of the connexion, once more to solve a problem, though in a different manner, which, from the researches of Messrs. Thomson, Clausius, Holtzmann, Quintus Icilius and others, has long been considered settled.

When a hydro-electrical current has become constant, there must be a constant compensation in the motor in order to keep it up; and if the current performs neither external nor permanent internal work, but only develops heat, this heat is the equivalent of the compensation. In homogeneous batteries (such, that is to say, as are only distinguished by the number and magnitude of the elements united), in the solution of each atom of zinc (or when each atom of the electropositive body changes its condition) the same process is repeated. Hence the compensation, like the development of heat, is in all cases proportional to the consumption of zinc, and

$$W : W_1 = Z : Z_1.$$

In heterogeneous batteries dissimilar processes take place in the solution of each atom of zinc; and for this we have

$$W : W_1 = xZ : x_1Z_1,$$

where x and x_1 are constants to be independently determined.

The magnitude of the consumption of zinc may be easily determined. Let e be the electromotive force of an element, $r = l + \lambda$ its total resistance, $p = \frac{e}{l + \lambda}$ the intensity of the current, z the consumption of zinc; while these magnitudes are denoted, in the case of batteries, either by the corresponding large letters or by functions of the small ones. Let a battery consist in each case of mn elements, consisting of m cells each containing n pairs of plates, and let it be constant.

First of all we have in homogeneous batteries

$$e : E = 1 : m.$$

The consumption of zinc is proportional to the intensity of the current p ; hence Z is proportional to mP . Therefore

$$\frac{z}{p} : \frac{Z}{P} = 1 : m = e : E = rp : RP,$$

$$z : Z = rp^2 : RP^2;$$

further,

$$Z : Z_1 = RP^2 : R_1P_1^2. \dots \dots \dots (1)$$

For unequal batteries we have

$$Z : Z = p : p_1,$$

or

$$z : z_1 = \frac{p^2 r}{e} : \frac{p_1^2 r_1}{e_1};$$

hence

$$ez : e_1 z_1 = rp^2 : r_1 p_1^2.$$

Combining this equation with equation (1), we obtain by an easy calculation the proportion

$$eZ : e_1 Z_1 = RP^2 : R_1 P_1^2. \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Now, according to Joule and Lenz's law, the expression RP^2 denotes the relative quantity of heat which a battery disengages; hence, if c denotes the compensation, we have

$$eZ : e_1 Z_1 = RP^2 : R_1 P_1^2 = W : W_1 = C : C_1. \quad . \quad . \quad . \quad (3)$$

As in the following we shall only be concerned with ratios of the quantities of heat, it is unnecessary to add constants to the expressions eZ and RP^2 . Hence let eZ denote in each case the compensation, RP^2 the heat disengaged, and also let the consumption of zinc be indicated by the expression to which it is proportional.

If a phenomenon of motion be considered to exist in the current, $RP (= E)$, as regards its form, is analogous to the quantity of motion, RP^2 analogous to the *vis viva* of a moving body. I shall use the latter name for $RP^2 = L$, though it be nothing more than a similar designation for similar analytical expressions.

For a battery,

$$L = RP^2 = \left(\frac{m}{n} l + \lambda \right) \left(\frac{me}{\frac{m}{n} l + \lambda} \right)^2 = \frac{m^2 n e^2}{ml + n\lambda}.$$

If both poles be directly connected without any interpolar, $\lambda = 0$, and therefore $L = \frac{mne^2}{l}$. Here mn is constant, and therefore also L .

A number of elements joined in any manner to form a battery, or a given surface of zinc, represents, in the absence of an interpolar, a constant quantity of *vis viva*, which is first modified by the interposition of a resistance. In every individual element the quantity $\frac{e}{l}$ of zinc is thereby used, just the same quantity as in an isolated element closed without any interpolar. On introducing a conductor (λ), the total consumption of zinc is $\frac{m^2 n e}{ml + n\lambda}$, while $\frac{e}{l + \frac{n}{m} \lambda}$ is the consumption of an individual element. The

consumption of zinc of a single element with the resistance λ would be $\frac{e}{l+\lambda}$. Hence

$$\frac{e}{l + \frac{n}{m} \lambda} \begin{matrix} \leq \\ \equiv \\ > \end{matrix} \frac{e}{l+\lambda} \text{ according as } m \begin{matrix} \leq \\ \equiv \\ > \end{matrix} n.$$

If the consumption of zinc and the *vis viva* of mn elements joined as a battery are to be greater than those of mn isolated elements, each of which has the resistance λ , m must be greater than n . The *vis viva* and consumption of zinc of a battery attains a maximum if $n=1$, but a minimum if $m=1$. It follows by no means from this that the actions to be produced by a battery attain simultaneously their maximum or their minimum. This depends upon the distribution of *vis viva* between motor and conductor.

While L has a constant value in the case of a battery closed without an interpolar, $P = \frac{ne}{l}$ is variable, and hence the action on the conductor must also be variable. The impact of a moving body exhibits a similar difference in action if the *vis viva* is constant but the velocity variable; and therefore, on investigating this problem, results are to be expected which agree in form with the electrodynamic laws. As a series of analogies frequently serves to indicate the correct path in further investigations, I did not think it superfluous to discuss this problem more minutely. The supposed agreement has been fully confirmed, as will be seen from the following examples.

I α . An inelastic body a , with a velocity v , strikes centrally against an inelastic body b at rest. What is the loss A of *vis viva*?

We have

$$A = \frac{av^2}{2g} - \frac{1}{2g} (a+b) \left(\frac{av}{a+b} \right)^2 = \frac{1}{2g} \cdot \frac{b}{a} \cdot \frac{(av)^2}{a+b}.$$

I β . In a battery $\frac{E}{R}$ closed by one or more interpolars, the resistance λ is interposed. What is the decrease A of *vis viva*, or of the consumption of zinc*?

We have

$$A = \frac{E^2}{R} - \frac{E^2}{R+\lambda} = \frac{\lambda}{R} \left(\frac{E^2}{R+\lambda} \right).$$

* In dissimilar batteries eZ and RP^2 are proportional, in similar ones Z and RP^2 .

2. A body a , which strikes with the velocity v a body at rest, imparts to it the velocity $\frac{av}{a+b}$. What velocity would be produced by another body whose vis viva was x times as great as that of a ?

The vis viva of this body would be $\frac{xav^2}{2g}$; yet the factor x may be connected in the most varied manner with a and v . The most general form of the combination is attained by putting $x=mn$, and making the velocity n times as great; thus

$$\frac{xav^2}{2g} = \frac{ma}{n} \frac{(nv)^2}{2g}.$$

We have then $\frac{ma}{n}$ for the mass, mav the quantity of motion, and we obtain for the velocity c which is imparted to the body b the equation

$$c = \frac{mav}{\frac{m}{n}a+b} = \frac{mnav}{ma+nb}.$$

If $n=1$, then

$$c = \frac{xav}{ax+b};$$

and if $m=1$,

$$c = \frac{xav}{a+xb}.$$

These results express, as regards their form, the intensity of the current of a combination of mn elements, according as they are joined either to form a battery of m cells with n elements, or a battery of mn cells with single elements, or a single cell with mn pairs of plates. An increase in the size of the zinc plates corresponds to a smaller mass (density) with increased velocity; a larger number of cells to a greater mass, the velocity remaining unchanged; the electromotive force corresponds to the quantity of motion, the consumption of zinc without and with an interpolar to the vis viva before and after the impact.

As these fundamental examples, from a series of theorems belonging to one class, agree, the corresponding ones may be at once deduced by simple translation. In a battery we need only call $\frac{me}{\frac{m}{n}l}$ the velocity, $\frac{m}{n}l$ the density, me the quantity of motion

$\frac{mne^2}{l}$ the *vis viva* of the striking body, λ the body at rest,

$\frac{me}{n}$ the velocity communicated to it.

3 α . Two bodies a and a_1 , whose vires vivæ are equal, impart to a body at rest, b , the same velocity by an impact. How great is b ?

We must have

$$\frac{av}{a+b} = \frac{a_1v_1}{a_1+b},$$

or

$$ava_1 + avb = a_1v_1a + a_1v_1b,$$

while

$$av^2 = a_1v_1^2 \text{ or } v_1 = v \sqrt{\frac{a}{a_1}}.$$

Hence

$$ava_1 + avb = a_1va \sqrt{\frac{a}{a_1}} + a_1vb \sqrt{\frac{a}{a_1}},$$

$$b^2(a - a_1) = aa_1(a - a_1),$$

$$b = \sqrt{aa_1}.$$

3 β . Two batteries of equal vires vivæ, RP^2 and $R_1P_1^2$, yield the same current after interposing the resistance X . How great is X ?

From the preceding, as well as by an independent solution, we obtain $X = \sqrt{RR_1}$. The two batteries may be obtained by combining in various ways x equal elements, each of which has the internal resistance l . In one case $x = mn$, in the other $x = m_1n_1$; hence

$$X = l \sqrt{\frac{m}{n} \cdot \frac{m'}{n'}}.$$

4 α . Two bodies a and a_1 may strike with the velocities v and v_1 against a body b moving in the same direction with the velocity c , and both would produce by the impact the same velocity in b . What are the losses A and A_1 of vis viva experienced by a and b on the one hand, and a_1 and b on the other.

We have

$$\frac{av + bc}{a+b} = \frac{a_1v_1 + bc}{a_1+b},$$

or

$$aa_1v + abv + a_1bc + b^2c = a_1av_1 + a_1bv_1 + abc + b^2c,$$

or

$$aa_1v + abv - abc = a_1av_1 + a_1bv_1 - a_1bc.$$

Now

$$\begin{aligned} \Lambda : \Lambda_1 &= \left[av^2 + bc^2 - \frac{(av + bc)^2}{a+b} \right] : \left[a_1v_1^2 + bc^2 - \frac{(a_1v_1 + bc)^2}{a_1+b} \right] \\ &= \frac{ab}{a+b} (v-c)^2 : \frac{a_1b}{a_1+b} (v-c)^2 \\ &= \frac{v-c}{v_1-c} \cdot \frac{a(a_1+b)(v-c)}{a_1(a+b)(v_1-c)} \\ &= \frac{v-c}{v_1-c} \cdot \frac{aa_1v + abv - abc - aa_1c}{aa_1v_1 + a_1bv_1 - a_1bc - aa_1c}; \end{aligned}$$

hence

$$\Lambda : \Lambda_1 = (v-c) : (v_1-c).$$

If c is negative,

$$\Lambda : \Lambda_1 = (v+c) : (v_1+c);$$

and if $c=0$,

$$\Lambda : \Lambda_1 = v : v_1.$$

4β. Of two closed batteries, let one have the intensity $\frac{E}{R}$, the other the intensity $\frac{K}{W}$. Let both conducting wires be cut, and the ends of the one be joined either with the similar or the dissimilar ends of the other, by which the intensity $\frac{E \mp K}{R + W}$ is obtained. Moreover let the battery $\frac{E}{R}$ be replaced by another battery $\frac{E_1}{R_1}$, so that with a particular manner of joining the two the intensity in question is unchanged. What is then the ratio of the losses Λ and Λ_1 of vis viva which are experienced on the one hand by the batteries $\frac{E}{R}$ and $\frac{K}{W}$, and on the other the batteries $\frac{E_1}{R_1}$ and $\frac{K}{W}$.

From what has been said, as well as from an independent solution, it follows that

$$\Lambda : \Lambda_1 = \left(\frac{E}{R} \pm \frac{K}{W} \right) : \left(\frac{E_1}{R_1} \pm \frac{K}{W} \right)$$

if

$$\frac{E \mp K}{R + W} = \frac{E_1 \mp K}{R_1 + W},$$

and that

$$\Lambda : \Lambda_1 = \frac{E}{R} : \frac{E_1}{R_1}$$

if $\frac{K}{W}$ simply denotes a conductor.

If by B and B_1 we simply understand the diminution in the consumption of zinc, we have with similar batteries

$$A : A_1 = B : B_1.$$

5. The *vis viva* which after the impact of two bodies a and b is present in them is divided in the ratio of their masses. Hence analogy would seem to indicate that quantities of heat disengaged in the motor and in the conductor would bear the ratio $l : \lambda$. This conclusion corresponds to and confirms Joule's law. It has only been experimentally demonstrated with certainty in the case of solid conductors; so that a length of wire of the resistance ρ always develops the heat ρP^2 . It is, on the other hand, most probable that the law also applies to the motor; but it has not yet been confirmed experimentally. So far, therefore, there is no proper proof, but only grounds of probability: and the above analogy may be considered a further reason for probability; it is certainly not purely accidental.

6. If Joule's law be considered universally valid, we must expect for the developments of heat in the motor and conductor, as for the *vires vivæ* of two bodies impinging against each other, similar relations, similar maxima and minima. As the agreement cannot really be doubtful, it will be sufficient to confine myself to the disengagement of heat in the motor and conductor.

The magnitude of the *vis viva* depends in similar batteries only on the consumption of zinc, in dissimilar ones also on the factor $\frac{e}{e_1}$. As this factor may in each case be readily added, similar batteries may be presupposed in the following.

In a single cell the *vis viva* is divided over the motor and conductor in the ratio $l : \lambda$; in a cell with m times as large a surface, in the ratio $\frac{l}{m} : \lambda$. Hence by increasing the plates the *vis viva* is, as it were, taken out of the motor and transferred to the conductor, while by increasing the cells it chiefly accumulates in the motor. The greatest utilization of the heat, since it must take place in the conductor, is only attainable by an increase in the surface of the plates, or by increasing the resistance; yet in the latter case it may be connected with a simultaneous decrease of the *vis viva* and of the consumption of zinc.

If a battery be supposed unchanged, the conductor has its maximum *vis viva* as soon as its resistance equals that of the motor. On both sides of this resistance λ lie corresponding values $\lambda + x$ and $\lambda - y$, for which the *vis viva* is the same but the consumption of zinc different. It is therefore possible that with a smaller consumption of zinc a long wire might present the same appearance of ignition as is produced by a greater con-

sumption of zinc in a shorter wire of the same section. If the maximum development of heat in the conductor be denoted by h , the motor also develops the heat h , and the sum $2h$ is proportional to the consumption of zinc

$$\frac{m^2 ne}{ml + n\lambda} = \frac{m^2 ne}{2ml} = \frac{mne}{2l}.$$

If the resistance be increased to infinity, the consumption of zinc, like the *vis viva*, becomes null. But if, conversely, the resistance is null, $\frac{mne}{e}$ is the consumption of zinc. The disengagement of heat is then twice as great as in the previous case; it amounts to $4h$, and only occurs in the motor.

If the conductor λ be supposed unchangeable but the motor variable, a similar maximum is obtained. If the resistance λ be added to an element which has also the resistance λ , motor and conductor develop together a quantity of heat $2w$ proportional to the intensity $\frac{e}{\lambda + \lambda}$. If the plates be increased to infinity, $\frac{e}{\lambda}$ is the intensity, the disengagement of heat is double its previous amount, it amounts to $4w$, and only occurs in the conductor. If another conductor, of the resistance λ_1 , develops the heat w_1 when the intensity is $\frac{e}{\lambda + \lambda_1}$, $4w_1$ is the maximum. Now $w : w_1 = \frac{1}{\lambda} : \frac{1}{\lambda_1}$; hence $4w_1 = 4w \frac{\lambda}{\lambda_1}$. Therefore, if a is the heat which is developed in the conductor by an element of the external resistance $= 1$, and of the internal resistance $= 1$, $\frac{4a}{r}$ is the maximum heat which can be imparted to a conductor of the resistance r by a single element.

If m elements of the resistance $1 + 1$ be formed into a pile $\left(\frac{m}{m+1}\right)^2 a$ is the disengagement of heat in the conductor, whose maximum is, again, four times as great. For a conductor of the resistance r , $\left(\frac{m}{m+1}\right)^2 \frac{4a}{r}$ is the maximum; with an arbitrarily arranged battery the factor $\frac{e}{e_1}$ must be added to this expression.

The agreement which exists between the most important electrodynamic laws and those of the central impact of inelastic bodies is deduced from no hypothesis. I have referred to Joule's law, but it was not necessary: were it unknown, the expression

$\frac{RP^2}{e}$ would be proportional to the consumption of zinc, and denote an action which depends only on the consumption. Whatever be the internal process in the motor and conductor, the action perceived is quantitatively viewed as if a motor had exerted an impact on the conductor and both had behaved like completely inelastic bodies. Something in common both processes must have; and this testifies strongly to the actual presence of a *vis viva*, and of an acceleration whereby, owing to the resistance, the accelerated motion would soon be constant. The preceding seems to indicate that an increase of cells produces more powerful impacts, while an increase of the plates increases the quickness of their succession but not the intensity of the individual impulses, so that there would be an analogy to red and violet light. So long, however, as there are no further reasons, this notion remains a rather vague hypothesis.

LII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 316.]

June 20, 1867.—Lieut.-General Sabine, President, in the Chair.

THE following communication was read:—

“On a Self-acting Apparatus for multiplying and maintaining Electric Charges, with applications to illustrate the Voltaic Theory.”
By Sir William Thomson, F.R.S.

In explaining the water-dropping collector for atmospheric electricity, in a lecture in the Royal Institution in 1860, I pointed out how, by disinulating the water-jar and collecting the drops in an insulated vessel, a self-acting electric condenser is obtained. If, owing to electrified bodies in the neighbourhood, the potential in the air round the place where the stream breaks into drops is positive, the drops fall away negatively electrified; or, *vice versá*, if the air potential is negative, the drops fall away positively electrified. The stream of water descending does not in any way detract from the charges of the electrified bodies to which its electric action is due, provided always these bodies are kept properly insulated; but by the dynamical energy of fluid-motion, and work performed by gravity upon the descending drops, electricity may be unceasingly produced on the same principle as by the electrophorus. But, as in the electrophorus there was no provision except good insulation for maintaining the charge of the electrified body or bodies from which the induction originates, this want is supplied by the following reciprocal arrangement, in which the body charged by the drops of water is made the inductor for an-

other stream, the drops from which in their turn keep up the charge of the inductor of the first.

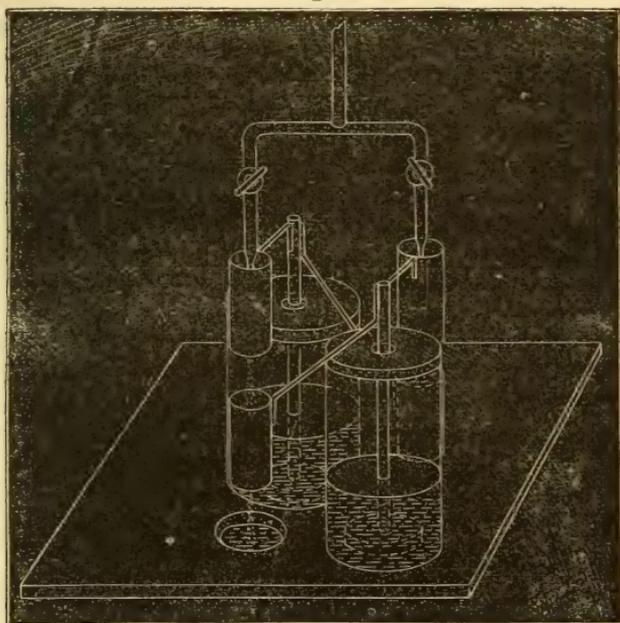
To stems connected with the inside coatings of two Leyden phials are connected metal pieces, which, to avoid circumlocution, I shall call inductors and receivers. Each stem bears an inductor and a receiver, the inductor of the first jar being vertically over the receiver of the second jar, and *vice versa*. Each inductor consists of a vertical metal cylinder (fig. 1), open at each end. Each receiver consists of a vertical metal cylinder open at each end, but partially stopped in its middle by a small funnel (fig. 1), with its narrow mouth pointing downwards, and situated a little below the middle of the cylinder. Two fine vertical streams of uninsulated water are arranged to break into drops, one as near as may be to the centre of each inductor. The drops fall along the remainder of the axis of the inductor, and thence downwards, along the upper part of the axis of the receiver of the other jar, until they meet the funnel. The water re-forms into drops at the fine mouth of the funnel, which fall along the lower part of the axis of the receiver and are carried off by a proper drain below the apparatus. Suppose now a small positive charge of electricity be given to the first jar. Its inductor electrifies negatively each drop of water breaking away in its centre from the continuous uninsulated water above; all these drops give up their electricity to the second jar, when they meet the funnel in its receiver. The drops falling away from the lower fine mouth of the funnel carry away excessively little electricity, however highly the jar may be charged; because the place where they break away is, as it were, in the interior of a conductor, and therefore has nearly zero electrification. The negative electrification thus produced in the second jar acts, through its inductor, on the receiver of the first jar, to augment the positive electrification of the first jar, and causes the negative electrification of the second jar to go on more rapidly, and so on. The dynamical value of the electrifications thus produced is drawn from the energy of the descending water, and is very approximately equal to the integral work done by gravity against electric force on the drops, in their path from the point where they break away from the uninsulated water above, to contact with the funnel of the receiver below. In the first part of this course each drop will be assisted downwards by electric repulsion from the inductively electrified water and tube above it; but below a certain point of its course the resultant electric force upon it will be upwards, and, according to the ordinary way of viewing the composition of electric forces, may be regarded as being at first chiefly upward repulsion of the receiver diminished by downward repulsion from the water and tube, and latterly the sum of upward

Fig. 1.



repulsion of the receiver and upward attraction of the inductor. The potential method gives the integral amount, being the excess of work done *against* electric force, above work performed *by* electric force on each drop in its whole path. It is of course equal to mV , if m denote the quantity of electricity carried by each drop, as it breaks from the continuous water above, and V the potential of the inner coating of the lower jar, the potential of the uninsulated water being taken as zero. The practical limit to the charges acquired is either when one or other of them is so strong as to cause sparks to pass across some of the separating air-spaces, or to throw the drops of water out of their proper course and cause them to fall outside the receiver through which they ought to pass. It is curious, after commencing with no electricity except a feeble charge in one of the jars, only discoverable by a delicate electrometer, to see in the course of a few minutes a somewhat rapid succession of sparks pass in some part of the apparatus, or to see the drops of water scattered about over the lips of one or both the receivers.

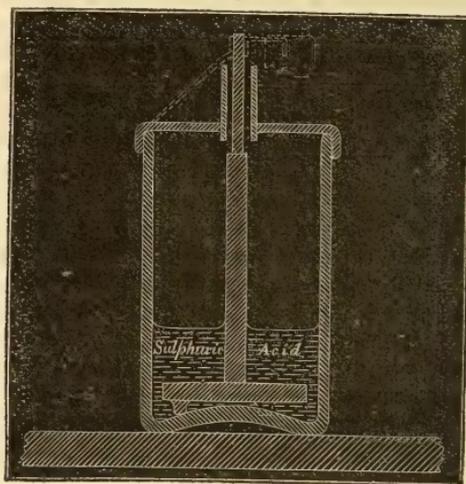
Fig. 2.



The Leyden jars represented in the sketch (fig. 2) are open-mouthed jars of ordinary flint glass, which, when very dry, I generally find to insulate electricity with wonderful perfection. The inside coatings consist of strong liquid sulphuric acid, and heavy lead tripods with vertical stems projecting upwards above the level of the acid, which, by arms projecting horizontally above the lip of the jar, bear the inductors and receivers, as shown in fig. 2. Lids of gutta percha or sheet metal close the mouth of each jar, except a small air-space of from $\frac{1}{8}$ to $\frac{1}{4}$ of an inch round the pro-

394 *Royal Society*:—Sir William Thomson on an Apparatus
jecting stems. If a tube (fig. 3) be added to the lid to prevent

Fig. 3.



currents of air from circulating into the interior of the jar, the insulation may be so good that the loss may be no more than one per cent. of the whole charge in three or four days. Two such jars may be kept permanently charged from year to year by very slow water-dropping arrangements, a drop from each nozzle once every two or three minutes being quite sufficient.

The mathematical theory of the action, appended below *, is particularly simple, but nevertheless curiously interesting.

The reciprocal electrostatic arrangement now described, presents an interesting analogy to the self-sustaining electromagnetic system

* Let c, c' be the capacities of the two jars, l, l' their rates of loss per unit potential of charge, per unit of time, and D, D' the values of the water-droppers influenced by them. Let $+v$ and $-v'$ be their potentials at time t, v and v' being both of one sign in the ordinary use of the apparatus described in the text. The action is expressed by the following equations:—

$$c \frac{dv}{dt} = D'v' - lv; \quad c' \frac{dv'}{dt} = Dv - l'v'.$$

If c, D, l, c', D', l' were all constant, the solution of these equations would be, for the case of commencing with the first jar charged to potential 1, and the second zero,

$$v = \frac{(c'\rho + l')\epsilon^{\rho t} - (c'\sigma + l')\epsilon^{\sigma t}}{c'(\rho - \sigma)}, \quad v' = D \frac{\epsilon^{\rho t} - \epsilon^{\sigma t}}{c'(\rho - \sigma)},$$

with the corresponding symmetrical expression for the case in which the second jar is charged, and the second at zero, in the beginning; the roots of the quadratic

$$(cx + l)(c'x + l') - DD' = 0$$

being denoted by ρ and σ . When $ll' > DD'$, both roots are negative; and the electrification comes to zero in time, whatever may be the initial charges. But when $ll' < DD'$, one root is positive and the other negative, and ultimately the charges augment in proportion to $\epsilon^{\rho t}$ if ρ be the positive root.

recently brought before the Royal Society by Mr. C. W. Siemens and Professor Wheatstone, and mathematically investigated by Professor Clerk Maxwell. Indeed it was from the fundamental principle of this electromagnetic system that the reciprocal part of the electrostatic arrangement occurred to me recently. The particular form of self-acting electrophorus condenser now described, I first constructed many years ago. I may take this opportunity of describing an application of it to illustrate a very important fundamental part of electric theory. I hope soon to communicate to the Royal Society a description of some other experiments which I made seven years ago on the same subject, and which I hope now to be able to prosecute further.

Using only a single inductor and a single receiver, as shown in fig. 1, let the inductor be put in metallic communication with a metal vessel or cistern whence the water flows; and let the receiver be put in communication with a delicate electroscope or electrometer. If the lining of the cistern and the inner metallic surface of the inductor be different metals, an electric effect is generally found to accumulate in the receiver and electrometer. Thus, for instance, if the inner surface of the inductor be dry polished zinc, and the vessel of water above be of copper, the receiver acquires a continually increasing charge of negative electricity. There is little or no effect, either positive or negative, if the inductor present a surface of polished copper to the drops where they break from the continuous water above: but if the copper surface be oxidized by the heat of a lamp, until, instead of a bright metallic surface of copper, it presents a slate-coloured surface of oxide of copper to the drops, these become positively electrified, as is proved by a continually increasing positive charge exhibited by the electrometer.

When the inner surface of the inductor is of bright metallic colour, either zinc or copper, there seems to be little difference in the effect whether it be wet with water or quite dry; also I have not found a considerable difference produced by lining the inner surface of the inductor with moist or dry paper. Copper filings falling from a copper funnel and breaking away from contact in the middle of a zinc inductor, in metallic communication with a copper funnel, as shown in fig. 4, produces a rapidly increasing negative charge in a small insulated can catching them below.

The quadrant divided-ring electrometer* indicating, by the image of a lamp on a scale, angular motions of a small concave mirror ($\frac{1}{3}$ of a grain in weight) such as I use in galvanometers, is very convenient for exhibiting these

Fig. 4.



* See Nichol's Encyclopædia, 1860, article "Electricity, Atmospheric;" or Proceedings of the Royal Institution, May 1860, lecture on Atmospheric Electricity.

results. Its sensibility is such that it gives a deflection of 100 scale-divisions ($\frac{1}{10}$ of an inch each) on either side of zero, as the effect of a single cell of Daniell's; the focusing, by small concave mirrors supplied to me by Mr. Becker, being so good that a deflection can easily be read with accuracy to a quarter of a scale-division. By adopting Peltier's method of a small magnetic needle attached to the electric moveable body (or "needle"), and by using fixed steel magnets outside the instrument to give directing force (instead of the glass-fibre suspension of the divided-ring electrometers described in the articles referred to), and by giving a measurable motion by means of a micrometer screw to one of the quadrants, I have a few weeks ago succeeded in making this instrument into an independent electrometer, instead of a mere electroscope, or an electrometer in virtue of a separate gauge electrometer, as in the Kew recording atmospheric electrometer, described in the Royal Institution lecture.

Reverting to the arrangement described above of a copper vessel of water discharging water in drops from a nozzle through an inductor of zinc in metallic connexion with the copper, let the receiver be connected with a second inductor, this inductor insulated; and let a second nozzle, from an uninsulated stream of water, discharge drops through it to a second receiver. Let this second receiver be connected with a third inductor used to electrify a third stream of water to be caught in a third receiver, and so on. We thus have an ascending scale of electrophorus action analogous to the beautiful mechanical electric multiplier of Mr. C. F. Varley, with which, by purely electrostatic induction, he obtained a rapid succession of sparks from an ordinary single voltaic element. This result is easily obtained by the self-acting arrangement now described, with the important modification in the voltaic element according to which no chemical action is called into play, and work done by gravity is substituted for work done by the combination of chemical elements.

GEOLOGICAL SOCIETY.

[Continued from p. 322.]

June 5, 1867.—Warrington W. Smyth, Esq., M.A., F.R.S.,
President, in the Chair.

The following communications were read:—

1. "The Alps and the Himalayas: a Geological Comparison."
By Henry B. Medlicott, Esq., A.B., F.G.S.

Current opinions on Alpine geology were first fully discussed by the author, especially as regards the abnormal nature of the actual boundary of the Molasse with the rocks of the higher Alps, including the explanation usually given of this phenomenon and of the contortion of the inner zone of Molasse—namely, the direct upheaval of the main mountain-mass. Mr. Medlicott then described some of

the sections exposed on the south flank of the Himalayas, and suggested a parallelism between them and those exhibited in the Alps. The clays, sands, and conglomerates of the Sivaliks are very like those of the Molasse; and in both regions the coarser deposits prevail towards the top. In the Himalayas also the younger Tertiary deposits almost invariably dip towards the mountain-range which they fringe, the plane of contact inclining in the same direction, and thus producing actual, though not parallel, superposition of the older rocks. All the arguments which have been used to prove prodigious faulting in the case of the Alps would therefore, the author stated, be quite as applicable to that of the Himalayas. But, as regards the latter range, Mr. Medlicott brought forward evidence which appeared to him sufficient to prove that the present contact of the Sivalik formation with the mountains is the original one, modified only by pressure, without relative vertical displacement, and that the *sinking* of the mountain-mass is the proximate cause of the contortions of the Tertiary strata. He then endeavoured to show that this explanation is equally applicable to the Alps, especially as it seems also to account for collateral phenomena which appear difficult of explanation consistently with the ordinary hypothesis; and he concluded by discussing the current theories of the formation of lake-basins, in relation to the more immediate subject of his paper.

2. "On some striking Instances of the Terminal Curvature of Slaty Laminæ in West Somerset." By D. Mackintosh, Esq., F.G.S.

Whilst engaged in investigating the nature and extent of oceanic and atmospheric denudation, and the origin of superficial accumulations in the West of England, the author observed, on the nearly level floor of one of the valleys which indent the Quantock hills, the laminæ of Devonian (Carboniferous?) Slate, very regularly and distinctly bent backwards towards the south.

Sections near Wiveliscombe exhibited some interesting instances of the curvature of the laminæ, the line of demarcation between the commencement of the curving back and the undisturbed mass of slate below being remarkably distinct and straight along the strike of the cleavage; but at right angles to the strike, the surface formed by the edges of the laminæ beneath, though as distinctly marked, is very uneven.

At Raleigh's Cross other instances of the curvature are seen. The most important fact in connexion with the sections was thus stated:—the bending and curving back over extensive areas has taken place on perfectly level ground, with a declivity instead of an elevation on the side whence the movement must have come.

An exaggerated continuation of the general curving back is seen at Gupworthy, which at first sight appeared to be part of a denuded anticlinal fold. Instances have also been exposed in a cutting of the Exeter and South Devon Railway near Plymouth.

In all parts of Devon, West Somerset, and Cornwall, if not in all districts where the slates are *flexible*, and where the cleavage-laminæ

dip at a considerable angle to the south, or where they are vertical, with an approximately east and west strike, similar appearances might be discovered, irrespective of the outline and inclination of the ground.

In the author's opinion the curving back of the slaty laminae in this district must have been produced by a great weight of solid matter propelled in a southerly direction.

June 19, 1867.—Warrington W. Smyth, Esq., M.A., F.R.S.,
President, in the Chair.

The following communications were read:—

1. "On *Cyclocyathus*, a new genus of the Cyathophyllidea, with remarks on the genus *Aulophyllum*." By P. Martin Duncan, M.B., Sec. G.S., and James Thomson, Esq.

A careful examination of a large series of sections and weathered specimens of Corals from near the base of the Carboniferous series at Lesmahago &c., which have hitherto been referred to the genus *Aulophyllum*, E. & H., has convinced the authors that *A. fungites* and *A. Bowerbanki* must be referred to a new genus, which they propose to name *Cyclocyathus*. It differs from *Aulophyllum* in having a more or less essential columella of a very complicated structure, while that genus was described by its authors as devoid of any such structure. One form in the series of specimens examined by Dr. Duncan and Mr. Thomson exhibited no columella, and the space which would otherwise have been occupied by it was covered in by successive tabulae; they therefore referred it to *Aulophyllum*, under the name of *A. Edwardsi*.

2. "On the discovery of a new Pulmonate Mollusk (*Conulus priscus*, P. P. Carpenter) in the Coal-formation of Nova Scotia." By J. W. Dawson, LL.D., F.R.S., F.G.S.

Some crushed specimens of the little shell described in this paper were found in a bed of clay, associated with numerous individuals of *Pupa vetusta*, between coals No. 37 and No. 38 of the Joggins section, during a search undertaken for the purpose of discovering, if possible, traces of land-animals in addition to the *Pupa* already known. On submitting these specimens to Dr. P. P. Carpenter, they were referred by him to the group *Conulus*, a subgenus of *Zonites*; and he has added to Dr. Dawson's paper a description of the characters which they present.

3. "On some tracks of *Pteraspis* (?) in the Upper Ludlow Sandstone." By J. W. Salter, Esq., A.L.S., F.G.S.

Impressions upon two slabs discovered by Mr. R. Banks in the Downton or Upper Ludlow Sandstones of Kington, Herefordshire, were described by Mr. Salter as the tracks of *Pteraspis* fish. His reasons for assigning this origin to the imprints were partly based upon their character, and partly on the fact that the *Pteraspis* was the only known creature of the period capable of making the im-

prints. He was of opinion that the tracks were produced by fish endowed with stiff defences to their pectoral or ventral fins. Whether the Pteraspis possessed such defences he was not aware, though he thought it not at all improbable.

4. "On a new *Lingulella* from the red Lower Cambrian rocks of St. Davids." By J. W. Salter, Esq., A.L.S., F.G.S., and H. Hicks, M.D.

Until very recently not a vestige of any organic remains had occurred in the red rocks of the Lower Cambrian series. The fossil now in question, which has been obtained from these deposits, is unquestionably a *Lingulella*; and although apparently of the same species as one which Mr. Salter names *Lingulella ferruginea*, very common in the lowermost of the layers which have yielded *Paradoxides*, this older form is distinguished by the varietal name of *L. ferruginea* var. *ovalis*, Hicks.

5. "Observations on certain Points in the Dentition of Fossil Bears, which appear to afford good diagnostic characters, and on the relation of *U. priscus*, Goldf., to *U. ferox*." By George Busk, Esq., F.R.S., F.G.S.

After noticing the difficulties which attend the study of the bones of the skeleton generally in Fossil Bears, and the somewhat confused and contradictory opinions as to the distinction of species which have arisen in consequence, Mr. Busk stated that his object in the present communication was merely to call attention to some points in the dentition which seemed to afford more certain and more readily ascertainable characters than could at present be drawn from the bones; and which, at any rate in the absence of other evidence, were a sufficiently sure guide in the distinction of species.

The characters derived from the teeth depend

1. On their dimensions, absolute and relative.
2. On their form.

It is not necessary, however, to consider all the teeth. The canines vary too much even within the limits of one species to be of much utility; and, with certain exceptions, the differences exhibited in the molars are not sufficiently marked to allow of their being employed.

The teeth upon which reliance is to be placed are the upper and lower fourth premolars, and the last molar in each jaw; and the distinctive characters of these teeth in *U. spelæus*, *U. priscus*, *U. ferox*, and *U. arctos* were pointed out.

It was also endeavoured to be shown that, in the size, proportions, and form of the teeth, no essential differences could be perceived between *U. priscus* and *U. ferox*; and the opinion was expressed that, so far as cranial and dental characters are concerned, those two species are at present indistinguishable.

6. "On the Geology of the Province of Canterbury, New Zealand." By Julius Haast, M.D., F.R.S., F.G.S.

The Southern Alps in the Province of Canterbury were stated to

form the eastern wing of a huge anticlinal of granites; on the western base of this central chain younger granites of secondary age support semimetamorphic strata. The eastern side of the arch consists of a succession of huge folds of dioritic sandstones, clayslates, and breccias, overlain by either Upper Devonian or Lower Carboniferous strata.

The Southern Alps are bounded on the eastern side by a belt of old Tertiary quartzose trachytes and pearlstones, on which repose extensive agglomeratic and tufaceous deposits (clays, sands, and lignite bands); these are succeeded by thick-bedded calcareo-arenaceous strata,—the whole broken through by Dolerites, which in their turn are covered by sand, clays, and thin limestones, probably of Pliocene age.

The author concluded by describing the Postpliocene moraine-accumulations which are found on both sides of the Southern Alps.

7. "On the Chemical Geology of the Malvern Hills." By the Rev. J. H. Timins, M.A., F.G.S.

From a detailed examination and analysis of the rocks composing the Malvern Hills, with a view to the determination of the chemico-physical processes which have contributed to their formation, Mr. Timins has arrived at the following general conclusions in regard to those rocks of eruptive origin:—(1) that, in the intrusive trap-rocks, the ratio of the oxygen of the silica to that of all the bases taken together, varies progressively from 5 : 4 to nearly 2 : 1; (2) that the relative proportions of the several bases vary considerably in different rocks, and often characterize particular localities; (3) that the chemical composition of the eruptive rocks does not vary according to their age; (4) that the atomic proportion of the silica to the bases is generally highest in the largest masses of trap, and lowest in the smallest masses; (5) that in the same masses of trap there is an appreciable increase in the silica towards their centres, and that the *primary source* of all the trap-rocks in the Malvern Hills was nearly a bisilicate, which, during the various processes by which it has been brought to the surface, has become united more or less with other substances, assimilating metallic oxides, lime, magnesia, or alkalis, according as one or another might be locally prevalent, just as, in modern times, the lava of Vesuvius takes up soda, and that of Etna lime; and (6) that in all the eruptive rocks the atomic proportion of the silica to the bases varies according to a common law.

8. "On the Relative Distribution of Fossils throughout the North Devon series." By Townshend M. Hall, Esq., F.G.S.

The author gave a table showing the relative distribution of the organic remains throughout the members of the Devonian system of North Devon. The subdivisions of the rocks employed by Mr. Hall are:—(1) The "Pilton Beds," highly fossiliferous; (2) "Cucullæa Zone," a term proposed in place of that of the "Marwood Beds," and suggested by the abundance of the shells of several species of *Cucullæa*, which are contained in the sandstone of the series; (3) the "Morthoe group," unfossiliferous; (4) the "Ilfracombe group,"

containing corals and several species of Brachiopods in good preservation; (5) the "Martinhoe group," unfossiliferous; (6) the "Lynton Beds" with few fossils; and (7) the "Foreland grits," which are without fossils, and the lowest of the series.

9. "On the Geology of the Princess Islands in the Sea of Marmora." By W. R. Swan, Esq.

Mr. Swan pointed out the existence of a considerable mass of Devonian strata, partly fossiliferous, in several of these islands, of an age different from that of the beds of the Bosphorus, which latter he has shown in a former paper to belong to the lowest of the Devonian series of the Rhine. The presence of remains of fish in the above strata, and of an ancient coral-reef in one of these islands (Andirovitho), was also noticed.

The rocks which form the remaining portions of these islands are (1) Trachytic, of younger age than the Devonian strata, and (2) Trappean, more recent than the Trachytic. The Quartz rocks, of which some of the islands are largely, and others entirely composed, are altered sandstones of Devonian age.

10. "On the Sulphur Springs of Northern Formosa." By Cuthbert Collingwood, M.B., F.L.S.

The sulphur springs are situated amongst the hills near Tamsuy in the north-eastern corner of the island of Formosa, and indicate the existence of volcanic action near the surface of the region, a phenomenon otherwise indicated by the frequent occurrence there of earthquakes. One spring possessed the character of a mountain-torrent, and had a temperature of about 130°. The spot containing most of the springs occupies about two acres of ground, is quite barren of vegetation, and is covered with low hillocks of friable rocks and débris, interspersed with shallow pits containing mud, sand, and sometimes water. From cracks and fissures in these depressions arose clouds of steam; and around them was strewn a quantity of sublimated sulphur, the yellow colour of which was visible from a distance.

11. "On the Geology of Benghazi, Barbary; with an account of the subsidences in its Vicinity." By George B. Stacey, Esq.

The town of Benghazi is built on a stratum of clay, which reposes on a sandy foundation. It is partially protected from the action of the sea by a reef of sandstone rocks projecting two or three feet above the water-level. Between the reef and the land the water is now about five feet in depth, while fifty years ago this area was dry land; remains of buildings are to be seen under the water. The author is therefore of opinion that the land is sinking regularly, and comparatively quickly. The fundamental rock of the country is a Tertiary limestone; and the author has obtained from the neighbourhood of Benghazi specimens of Corals, Echinoderms, Oysters, Pectens, and on the surface *Cardium edule*.

12. "Report on the Existence of large Coal-fields in the Province of St. Catherine's, Brazil." By Edward Thornton, Esq.

The existence of Coal in this district has for many years been an established fact; but no practical exploration had been made until the years 1861-63, when Viscount Barbacena, having purchased a tract of land containing the best seams, ascertained the existence of a series of coal-beds at nine different levels, underlying a sandstone formation, horizontally disposed, and varying in thickness from $1\frac{1}{2}$ to 10 feet. Analyses of specimens of the coal prove it to be of good quality, its profitable working depending solely upon the facilities for transport.

13. "On the Sources of the Materials composing the White Clays of the Lower Tertiaries." By George Maw, Esq., F.L.S., F.G.S.

In examining some of the light-coloured deposits occurring between the Boulder-clay and the Carboniferous Limestone of North Wales, the author was led to the conclusion that some of the beds of very white and pure clays that occur in "pockets" in the limestone could not have been derived from the mere mechanical degradation of any previously existing materials; analyses by Prof. Way and Dr. Voelcker showed that they contained similar proportions of silica and alumina as did the limestone, and Mr. Maw concluded that they were left behind after the dissolution of the calcareous matter of the limestone had been effected by carbonated water.

The Lower Tertiary deposits of Hampshire, Dorsetshire, the Isle of Wight, and Devonshire contain vast deposits of similar white clays, which the author believed to have had an analogous origin through a similar dissolution of the Chalk.

14. "On the Postglacial Structure of the South-east of England." By Searles V. Wood, Jun., Esq., F.G.S.

This paper was an outline of the principal points deduced by the author from his Geological survey of the country included in the Ordnance sheets Nos. 1 & 2, where the glacial clay approaches nearest to the Thames Valley beds, and from a survey on a smaller scale of the glacial beds over a much larger area. Both of his maps, with a manuscript memoir upon the subject, have been placed by him in the Library of the Society.

The author took up the structure at the southerly and westerly edges of the principal tracts of glacial beds, and in the parts where these are divided by great troughs of denudation; he showed the manner in which the denudation, commencing at the first upheaval of the glacial sea-bed, has descended through the Lower Tertiary and Secondary deposits, accompanied by the formation of successive gravel-beds during its progress. His conclusion was, that the removal of the Lower Tertiary strata over much of the South of England, and the excavation of the Weald Valley, as well as the great denudation which the Liassic, Oolitic, and Cretaceous beds have undergone in the west of England, are to be traced principally to the pro-

gress of the postglacial denudation. This denudation had its inception in the upheaval of a portion of the glacial sea-bed, and was accompanied by a long succession of subterraneous disturbances, which have brought up the Secondary and Tertiary rocks of the south and west to the elevations they now occupy, and left the early emerged portion of this sea-bed (represented by the detached tracts of the Upper and Middle Glacial formations) at lower levels than much of the denuded area of the south and west.

LIII. Intelligence and Miscellaneous Articles.

INVESTIGATIONS ON THE INFLUENCE OF HEAT ON THE MECHANICAL FORCE OF FROGS' MUSCLE. BY M. CHMOULEVITCH.

IN investigating the influence of temperature on muscles at rest, I was led to assume that all their physical properties experience great changes, even within very narrow limits of temperature. It was natural to assume *à priori* that these differences must also determine changes in their physiological functions. For this reason I decided to investigate the action of heat on the mechanical force of the muscle.

In making the experiment, the gastrocnemian muscle of the frog was fixed by its tendon in a vessel containing a solution of chloride of sodium (0.65 per cent.) at a given temperature. The upper insertion of the muscle was fixed to the short arm of a lever moving about a horizontal axis, the long arm of which marked upon a rotating cylinder the heights to which the weight applied to the same arm was raised. I obtained the following results:—

(1) The mechanical force of the muscle increases up to from 30° to 33° according to its length and tension.

(2) The increase in the height to which the weight is raised during the increase of temperature is greater the less the weight.

(3) In the case of each muscle there is a certain tension under which it retains the same length at different temperatures.

(4) When the temperature is raised to more than from 30° to 33° its mechanical work is seen to diminish rapidly; and a point is soon reached at which, while supporting a given weight, it no longer contracts; its work is then equal to zero; I designate it by the name *zero work*.

(5) Zero work is reached the sooner the greater the weight which the muscle supports. This proves that the muscle losing its property of contracting at certain temperatures is not due to a chemical action of temperature on the substance of the muscle, in which case the required temperature would be constant and would not change with the weight; it rather arises from a change in the purely physical relations of the molecules of the muscles produced by the high temperature. We have in support of this opinion the fact that

(6) The muscle need only be lowered in temperature to restore to

the molecules their normal ratios, and thus reimpart to a muscle the power of contraction. A coagulation, on the other hand, could never disappear instantaneously.

(7) Making a series of experiments on the same muscle, by heating it several times successively to the point of zero work and then cooling, I have observed that the greatest ordinate, that in which the mechanical force ceases to increase or to diminish, appeared in the next experiment at a lower temperature. I have found that this phenomenon is caused by the remarkable fact that the muscle is more rapidly exhausted at a higher than at a lower temperature. This I proved by putting to work two muscles of the same weight (as nearly as possible) under the same tension and the same irritation, but at different temperatures. At the commencement the ordinates at the higher temperature were then always greater than at the lower one; that is, the muscle raised the weight to a greater height: but the abscissa was always shorter at the higher temperature; this amounts to saying that then the muscle always ceased working sooner. In consequence of this fact,

(8) The total work of the muscle is always greater at a low than at a high temperature, all other conditions being equal.

(9) The increase in mechanical work during increase of temperature is due to the circumstance that the elasticity of the muscle at work increases with increase of temperature.—*Comptes Rendus*, August 26, 1867.

ON THE INTENSITY OF THE SOLAR RADIATION.

BY M. J. L. SORET.

The actinometer I use in these researches consists essentially of a thermometer, the blackened bulb of which is placed within an envelope also blackened. An aperture, 2 centims. in diameter, in this envelope gives entrance to a solar ray which falls on the bulb of the thermometer. The temperature rises until it loses, either by radiation or by contact with the surrounding air, as much heat as it receives from the sun.

In order to have the instrument always under the same conditions as regards radiation, the envelope is surrounded by melting ice, so that the thermometer is uninfluenced by the reverberation of adjacent bodies, or the radiation from various parts of the sky.

Contact with the surrounding air, although the thermometer is protected from the wind, contributes a notable proportion of the cooling, which varies and increases with the atmospheric pressure. If the same observations were always made in the same place, these variations might be neglected; but this is by no means so if we work at different heights. By direct experiments on the apparatus itself I have determined the influence which the barometric pressure exercises on the temperature of the thermometer. The in-

dications of the instrument may thus be corrected by calculation, and reduced to what they would have been if the thermometer had been surrounded by air under a pressure of 760 millims.

I think I may assume that, when this correction is made, the number of degrees above zero indicated by the thermometer gives a very approximate measure of the intensity of the direct solar radiation.

Observations made at Geneva.—A series of observations made at Geneva has shown me that the moisture of the atmosphere influences the direct solar radiation; in general, the more aqueous vapour there is in the air the less is the radiation. I will limit myself to adducing some examples.

Comparing the small number of observations which I have been able to make in winter with those of summer, we see that with equal radiation the height of the sun above the horizon was considerably greater in summer than in winter.

On several occasions a stronger radiation has been observed during dry than during moist weather, although the atmosphere was undoubtedly purer and more transparent in the second case than in the first. The presence, then, of solid or of liquid particles diffused in the air is insufficient to account for variations in the solar radiation.

With virtually equal solar heights the minima of radiation correspond to days on which the tension of aqueous vapour was strongest, and the maxima of radiation to the driest weather (during or immediately after strong north winds). Thus, for solar heights greater than 60° , the minimum temperature was $14^\circ.82$ on the 2nd of last June, the tension of aqueous vapour being 14 millims., after a series of moist days: it indicated a maximum of $15^\circ.93$ on the 10th of July, on the day after a strong north wind during which the temperature sank to 7° , and after a series of exceptionally dry and cold days.

Yet as these variations are not great, while fog exerts also an undoubted influence, it would be very interesting to have a long series of observations, the means of which would enable us to determine the exact relation between the moisture of the atmosphere and the intensity of the direct solar radiation.

The radiation is remarkably constant while the sun is at a great height above the horizon, and when the atmospheric conditions remain almost the same. Thus observations made between 11.30 A.M. and 1.30 P.M., the sun being more than 60° above the horizon, the sky clear, the tension of aqueous vapour 8 to 11 millims., excepting the case of strong north winds, have regularly given radiations comprised between $15^\circ.31$ and $15^\circ.59$. It may thus be assumed that the number $15^\circ.5$ represents the solar radiation of Geneva under these circumstances.

Observations at different heights.—The frequency of clouds on mountains renders it difficult to make observations at different heights; and thus I have only been able to make an inconsiderable number of them. Yet particularly in an ascent of Mont Blanc, on the 20th and 21st of July last, during exceptionally fine weather, I obtained results which, if I am not deceived, possess some interest.

Let us first take the observations made with the sun at a height of more than 60° above the horizon.

On the 20th and 21st of July, the state of the atmosphere being just that at which, as I have before said, the radiation at Geneva was $15^\circ.5$ of my actinometer, this number may be admitted for a height of 400 metres above the sea-level*. On the 20th of July, at the Glacier des Bossons, a height of 2500 metres, the thermometer of the actinometer indicated $18^\circ.63$. On the 21st of July, at 11.30 A.M., on the top of Mont Blanc, the thermometer of the actinometer marked $21^\circ.13$. These observations, being made under different atmospheric pressures, should be reduced to what they would have been had the thermometer been surrounded by air under a pressure of 760 millims. To render the results more comparable, the atmospheric thickness must in each case be calculated (product of the barometric height by the secant of the zenith-distance of the sun).

The following are the data :—

	Height.	Pressure.	Atmo- spheric thickness.	Radiation.	
				Cor- rected.	Uncor- rected.
	metres.	millims.			
Geneva	400	730.0	805	$15^\circ.50$	$15^\circ.34$
Glacier des Bossons.	2500	562.3	645	18.63	17.32
Mont Blanc	4800	424.6	473	21.13	18.62

It follows from these numbers, first, that under the observed conditions the ratio of the intensity of solar radiation on Mont Blanc to that at Geneva is about $\frac{6}{5}$. Thus the solar heat which has reached a height of 4800 metres through the upper layers of the atmosphere undergoes an absorption of $\frac{1}{5}$ in traversing the lower layers of the atmosphere to a height of 400 metres under an angle of 60° to 65° .

It will be observed, in the second place, that the increase of the radiation with the height is less rapid than the diminution of barometric pressure, and than the diminution of the atmospheric thickness. Taking the atmospheric thicknesses for abscissæ, and the corrected radiations as ordinates, three points are obtained representing the above three observations. If a curve be drawn through these three points, it will be seen to be concave towards the axis of the abscissæ†. This result is contrary to that which may be deduced

* It may perhaps be thought that, instead of adopting this figure, it would have been better to make simultaneous observations at the foot of the mountain; but, apart from the difficulty of this, I imagine measurements made in a narrow valley like that of Chamouni present fewer guarantees of accuracy.

† By prolonging this curve indefinitely, the point where it cuts the ordinate axis would correspond to the radiation for zero thickness. The number thus obtained would be about $20^\circ.5$, and would represent the in-

from Forbes's observations made in 1832 on the Faulhorn and at Brienz*.

I will further add two results flowing from observations made when the heights of the sun above the horizon were lower.

With equal atmospheric thickness the radiation at a greater height is undoubtedly stronger than at a lower height. Thus on the 20th of July, at 5.30 P.M., on the Grands Mulets (3000 metres), the atmospheric thickness being 1500, the corrected radiation was $15^{\circ}26'$. Now at Geneva, even under the most favourable circumstances, it scarcely reaches 14° . An observation made in winter on Mont Salem confirms this result, which is also opposed to what Mr. Forbes had found.

The diminution of the intensity of the radiation with the diminution of the height of the sun above the horizon is considerably less strong at a great height than on the plain; so that the ratio of the radiation observed at a higher station to that observed at a lower one is greater in the morning and evening than in the middle of the day.—*Comptes Rendus*, September 23, 1867.

ON SYSTEM IN MINERALOGICAL NOMENCLATURE.

BY J. D. DANA.

In an interesting paper on the above subject, which appears in Silliman's Journal for September 1867, the author arrives at the following conclusions:—

1. The only chance for uniformity in mineralogical nomenclature is in the general use of the termination *ite*.

2. This termination is the best that is available, because of (1) its original use and signification, (2) its early origin, (3) its almost universal adoption in the science.

3. If any other termination in addition is to be used, it should be so only *under system*; that is, it should be made characteristic of a particular natural group of species, and be invariably employed for the names in that group; and its use should not be a matter of choice or whim with describers of species.

As a matter of fact, *ine* has not been employed for any particular division of minerals, or with reference to any system, and it could not now be so restricted; and this being the case, it should be put out of the science as far as possible. It belongs by adoption and long usage to chemistry, and should be left to that science.

tensity of the radiation at the limit of the atmosphere. Of course this observation is only offered for what it is worth, and as an indication which may be interesting; it is in fact clear that, even assuming my observations to be absolutely correct, they by no means suffice to determine with any certainty the form of the curve representing the law of radiation, a law which must be very complex.

* Phil. Trans. for 1842, part 2, p. 225.

4. In order that the acquired uniformity may be attained, changes should be made in existing names when it can be done without great inconvenience.

Names like Quartz, Garnet, Gypsum, Realgar, Orpiment, with the names of the metals and gems, which are part of general literature, must remain unaltered. Mica and Felspar, equally old with Quartz, have become the names of *groups* of minerals, and are no longer applied to particular species.

Fluor was written Fluorite last century by Napione. Blende, although one of the number that might be allowed to stand among the exceptions, has already given place with some mineralogists to Sphalerite, a name proposed by Haidinger (because blende was applied also to other species) in 1845, and signifying *deception*, like Blende. Galena was written Galenite by von Kobell some years since. Orthoclase, Loxoclase, Oligoclase might be rightly lengthened to Orthoclasite, &c. But the termination *clase* (from the Greek for *fracture*) is peculiar to names of minerals, and the abbreviated form in use may be allowed to stand for species of the Felspar group. It seems better that it be avoided elsewhere. These remarks on special cases might be further extended; but it is unnecessary, as I shall soon present my views more fully in another place.

There are, of course, objections to all such change. But the number of names requiring it are comparatively few; and hardly any of them are over sixty years old, a short time compared with the future of the science. Mineralogy is yet in its formative period; it is far from being so stiffened with age as not to admit of progress in the direction contemplated.

With regard to the nomenclature of rocks, Professor Dana observes:—

It has become very desirable that this should have some point of difference from that of minerals. Names like Diorite, Dolerite, Eurite, Porphyrite, Andesite, Tonalite, Phonolite, Leptynite, Domite, Dunite, Amphibolite, Pyroxenite, Pegmatite, Hyperite, Itacolumite, Spilite, Ditroïte, Sanidinite, Phthanite, Nephelinite, Miascite, Itabirite, Aphanite, are fast multiplying, and have nothing in them to indicate whether they are to be looked for or not in a work on mineralogy. It is therefore here suggested, as an easy method of giving the names of rocks a distinctive feature, to substitute for *i* in the final *ite* the letter *y*. Thus Diorite, Eurite, Tonalite, &c. would become Dioryte, Euryte, TonalYTE, &c. The *y* is already in the name Trachyte. The change would not be necessary in the familiar names Granite and Syenite. In the German language the terminal syllable *lite*, when from the Greek *λίθος*, is written *lith*, as in Phonolith; but it would be no disadvantage to the language, or to its science, if the *h* should be left off here as elsewhere, and the *yte* (or *yt* in German) be accepted as simply a termination in obedience to system.

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SECURITY—Two Millions Four Hundred and Three Thousand Pounds Invested Assets, and Three Hundred and Twenty Thousand Pounds Annual Income.

BONUS—The Society being on the Mutual principle, the Assured share in the whole of the Profits.

A Bonus declared every Fifth Year. At the Division in 1864, a Bonus of Five Hundred and Six Thousand Pounds was added to the Sum Assured, making with those declared at previous Divisions, a total Bonus addition, since the commencement of the Society, of One Million Eight Hundred & Seventy-one Thousand Pounds.

Nine Thousand Three Hundred Policies now in force, assuring Eight Millions Four Hundred Thousand Pounds.

Since its foundation the Society has paid in Claims more than Three Millions sterling.

Assurances granted to the extent of Ten Thousand Pounds on a single life.

The sphere of the Society's operations limited to Great Britain.

Prospectuses and full Particulars may be obtained on application to

JOHN RALPH GRIMES,

SECRETARY,

Or to MESSRS. T. O. COOPER & SON, 3, BROWN'S BUILDINGS, LIVERPOOL.

ABSTRACT OF THE QUINQUENNIAL REPORT OF 1864.

Since the last Division, 2,641 Policies, assuring £2,050,788, have been issued, giving an annual average of 523 Policies of £777 each—a large and steady increase of the business.

The sum of £72,702 has been received during the five years in new Premiums, being at the rate of £14,540 a year.

The total income from Premiums, which in 1859 was £182,429, now amounts to £214,104, indicating an average annual increase of £4,385, after allowing for loss of income from discontinued Policies; while the gross income from all sources has increased at the rate of £10,230 per annum.

Claims have arisen during the five years on 794 Policies assuring £624,327, and carrying Bonuses to the amount of £116,899.

In addition to the Bonuses on Policies upon which claims have arisen, the sum of £87,149 has been paid as bonus in other ways, such as in reduction of bonus liability by cash payment, reduction of premium, purchase, &c., making a total of £204,48.

In the valuation of the Assets, an ample margin has been allowed for possible fluctuation of the Funds; and in the valuation of the Liabilities, the *risk* Premiums only have been taken into account, and no profit is declared by anticipation.

The Assets, consisting of Funded Property, Mortgages, Life Interests, and Reversions, Premiums due on 31st December (since paid), Interest accrued on Investments, Balance at Bankers, and in hand, amount to £2,315,129. 19s. 2d.

The Liabilities, consisting of the values of Policies and the Bonuses already declared, claims accrued in 1863 but due in 1864, commission, taxes, and sundry small accounts, amount to £1,964,739. 1s. 7d. There is, therefore, after making provision for every known liability, a surplus of £350,390. 17s. 7d.

The Directors recommend that £329,890 of this surplus be distributed as absolute Bonus; and that the remaining sum of £20,500. 17s. 7d. be retained for the payment of annual, contingent, and conditional Bonuses.

The sum of £329,890 will produce reversionary Bonuses amounting to £506,300, yielding a percentage ranging from 5 to 34, or 9½ on the average of the sums assured; and a percentage ranging from 26 to 160, or 59½ on the average, on the premiums received in respect of which the Bonus is allotted.

The Society now assures by 9,022 Policies the sum of £7,233,564, and has an Assurance Fund amounting to £2,272,385. 11s., and an Annual Income of £307,475. The large number of assurances in force affords a protection to the Society against those deviations from the average which attend a paucity of numbers, while the respective amounts of the capital and the income attest the firm and satisfactory condition of the Society.

The experience of the Society during this period (the fourth which has elapsed since paying off the Shareholders), has fully realised the expectations of the Directors, and they feel assured that the Members will share their satisfaction.

By order of the Board of Directors,

OLINTHUS GREGORY DOWNES,

Actuary.

WHOLE LIFE.

WITH PARTICIPATION IN PROFITS.

TABLE No. 1 A. EQUAL RATES OF PREMIUM. Annual Premiums required for an Assurance of £100 for the whole Term or Life.			
Age.	Annual Premium.	Age.	Annual Premium.
15	£ s. d. 1 10 8	33	£ s. d. 2 8 0
		34	2 9 5
16	1 11 5	35	2 10 11
17	1 12 3		
18	1 13 0	36	2 12 6
19	1 13 10	37	2 14 2
20	1 14 7	38	2 15 11
		39	2 17 9
21	1 15 5	40	2 19 9
22	1 16 3		
23	1 17 2	41	3 1 10
24	1 18 1	42	3 4 1
25	1 19 0	43	3 6 6
		44	3 9 0
26	2 0 0	45	3 11 9
27	2 1 0		
28	2 2 0	46	3 14 7
29	2 3 1	47	3 17 8
30	2 4 3	48	4 0 11
		49	4 4 4
31	2 5 5	50	4 7 6
32	2 6 8		

TABLE No. 1 B. Annual Premium payable during Ten Years only for an Assurance of £100 for the whole Term of Life.			
Age.	Annual Premium.	Age.	Annual Premium.
15	£ s. d. 3 14 11	33	£ s. d. 5 5 0
		34	5 7 1
16	3 16 5	35	5 9 5
17	3 17 11		
18	3 19 6	36	5 11 9
19	4 1 0	37	5 14 2
20	4 2 6	38	5 16 8
		39	5 19 3
21	4 4 0	40	6 1 11
22	4 5 6		
23	4 7 1	41	6 4 8
24	4 8 9	42	6 7 6
25	4 10 5	43	6 10 6
		44	6 13 10
26	4 12 1	45	6 17 4
27	4 13 9		
28	4 15 6	46	7 0 11
29	4 17 4	47	7 4 7
30	4 19 2	48	7 8 4
		49	7 12 4
31	5 1 1	50	7 16 5
32	5 3 0		

The advantages of Life Assurance may be thus briefly enumerated.

To provident persons Life Assurance presents the means of securing, by a small annual saving, a provision for their families, in the event of premature death.

It affords the opportunity of making a settlement prior to marriage.

It enables persons to raise money on life interests.

It secures sums of money contingent on parties coming of age.

It affords the opportunity of restoring to a family any amount of capital which the parent may have sunk on an estate, to hold it as tenant for life, or for two or three lives; an occurrence very common in many parts of England, particularly with respect to property held under the Church.

It affords facilities to debtors to satisfy their creditors; whilst to creditors it offers effectual, and, in many cases, the only means of security.

THE ECONOMIC LIFE ASSURANCE SOCIETY is constituted so as to afford these advantages to the public in their fullest extent, giving the advantages of the *mutual principle*, while supported by a large accumulated capital.

- Name, Profession, and Residence (in full) of the Party
who makes the Assurance
- Amount, and Term of Assurance, and by which Table
- Name and Rank, or Profession, of the Person whose
Life is to be Assured
- Place and Date of Birth
- Age next Birthday
- Present Residence
- Whether employed in any Military, Naval, or Pre-
ventive Service.....
- If resided abroad; where, and for what period?
- If any near Relative have died of Consumption?.....
- If had the Small Pox, or undergone Vaccination; and
which?
- If at any time afflicted with Gout, Asthma, Spitting
of Blood, Fits, Hernia, or any other disorder
tending to shorten Life?
- If the Life proposed for Assurance have been declined
at any Office
- Reference to two persons, to ascertain the present and
ordinary state of Health and Habits of the Per-
son whose Life is to be Assured

*Signature of the Person whose
Life is to be Assured.*

THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[FOURTH SERIES.]

DECEMBER 1867.

LIV. *Michael Faraday, his Life and Works.*
By Professor A. DE LA RIVE*.

SCIENCE has just lost one of its most eminent and faithful representatives. Faraday died on Sunday, the 25th of August 1867, at Hampton Court; he was born on the 24th of September 1791, at Newington Butts, near London. In 1804, at the age of thirteen, he was apprenticed to a bookbinder, in whose workshop he remained eight years. So many books passed through his hands that he could not resist the temptation of opening and reading some of them. These readings, performed in the evenings after the work of the day was finished, gave him a taste for study, and in particular for that of the sciences. The *Encyclopædia Britannica* first of all introduced him to some notions of electricity; and it was afterwards, from the works of Mrs. Marcet, that he derived his first knowledge of chemistry. His labours received their permanent direction from this opening; their essential objects were electricity and chemistry.

"Do not fancy," he said to me in a letter† of the 2nd of October 1858, in which he gives me these details, "that I was a profound thinker or a precocious child; I had merely a good deal of life and imagination, and the tales of the Thousand and One Nights pleased me as much as the *Encyclopædia Britannica*."

* Translated from the *Bibliothèque Universelle*, October 25, 1867, *Arch. des Sci.* pp. 131-176.

† This letter was addressed to me on the occasion of the death of Mrs. Marcet, and the notice which I was about to publish on this distinguished woman (see *Bibl. Univ. nouvelle série*, 1858, vol. iii.).

But what saved me was the importance I early attached to facts. In reading Mrs. Marcet's book on chemistry, I took care to prove every assertion by the little experiments which I made as far as my means permitted; and the enjoyment which I found in thus verifying the exactitude of the facts contributed essentially to give me a taste for chemical knowledge. You may therefore easily imagine the pleasure I experienced when I subsequently made the personal acquaintance of Mrs. Marcet, and how delighted I was when my thoughts went backward to contemplate in her at once the past and the present. Whenever I presented her with a copy of my memoirs, I took care to add that I sent them to her as a testimony of my gratitude to my first instructress."

"I have the same sentiments towards the memory of your own father," adds Faraday; "for he was, I may say, the first who encouraged and sustained me, first at Geneva when I had the pleasure of seeing him there, and afterwards by the correspondence which I regularly maintained with him."

Faraday here alludes to a journey in which he accompanied Davy to Geneva in 1814, and in which, during a stay which he made with his illustrious master at my father's, the latter quickly discerned the merits of the young assistant, and formed relations with him which were interrupted only by death. At the time when he travelled with Davy, Faraday was his assistant at the Royal Institution in London; and I must say that he has more than once expressed to me, both by letter and *viva voce*, his thankfulness to the eminent chemist, who had admitted him to one of his courses, and consented, after running through the notes of this course prepared by the young pupil, to take him for his assistant.

After the journey just referred to, Faraday, with the exception of rare and short absences, never again quitted the Royal Institution, where he had his laboratory and his residence. Married to a lady worthy of him, and who shared and understood all his impressions and all his sentiments, he passed a life equally peaceful and modest. He refused all the honorary distinctions which the government of his country wished to confer upon him; he contented himself with a moderate salary and with a pension of £300 sterling, which fully sufficed for his wants; and accepted nothing supplementary to this except the enjoyment, during the summer, in the latter years of his life, of a country house at Hampton Court, which the Queen of England graciously placed at his disposal.

Without children, a complete stranger to politics or to any kind of administration, except that of the Royal Institution, which he directed as he would have directed his own house,

having no interest but that of science, and no ambition but that of advancing it, Faraday was of all *savants* the one most completely and exclusively devoted to the investigation of scientific truth of which the present century offers us an example.

One may easily understand what must be produced under such circumstances by a life thus wholly consecrated to science, when to a strong and rigorous intellect is joined a most brilliant imagination. Every morning Faraday went into his laboratory as the man of business goes to his office, and then tried by experiment the truth of the ideas which he had conceived overnight, as ready to give them up if experiment said *no*, as to follow out the consequences with rigorous logic if experiment answered *yes*. His everyday labour experienced no interruption, except the few hours which he devoted from time to time to the exposition in the theatre of the Royal Institution, before an audience equally numerous and select, of certain parts of physics and chemistry. Nothing can give a notion of the charm which he imparted to these improvised lectures, in which he knew how to combine animated and often eloquent language with a judgment and art in his experiments which added to the clearness and elegance of his exposition. He exerted an actual fascination upon his auditors; and when, after having initiated them into the mysteries of science, he terminated his lecture, as he was in the habit of doing, by rising into regions far above matter, space, and time, the emotion which he experienced did not fail to communicate itself to those who listened to him, and their enthusiasm had no longer any bounds.

Faraday was, in fact, thoroughly religious, and it would be a very imperfect sketch of his life which did not insist upon this peculiar feature which characterized him. His christian convictions occupied a great place in the whole of his being; and he showed their power and sincerity by the conformity of his life to his principles. It was not in arguments derived from science that he sought the evidences of his faith; he found them in the revealed truths at which he saw that the human mind could not arrive by itself alone, even though they are in such great harmony with that which is taught by the study of nature and the marvels of creation. Faraday had long and justly perceived that scientific data, so moveable and variable, cannot suffice to give to man a solid and impregnable basis for his religious convictions; but he at the same time showed by his example that the best answer which the man of science can give to those who assert that the progress of science is incompatible with these convictions, is to say to them, And yet I am a Christian.

The sincerity of his Christianity appeared in his actions as much as in his words. The simplicity of his life, the rectitude

of his character, the active benevolence which he displayed in his relations with others, gained him general esteem and affection. Always ready to render services, he could quit his laboratory when his presence elsewhere was necessary to a friend or useful to humanity. We see him putting his knowledge under contribution both for inquiries upon questions of public health or industrial applications, and to give practical advice to an artisan or examine the discovery of a *débutant* in the scientific career. Only, as I have already said, with these exceptions, he made it a rule not to allow himself to be turned aside from the labours to which he had consecrated his life by occupations of another kind, or by those pretended duties of society which waste time, abridge intellectual life (already so short), and very often leave nothing behind them but emptiness and regret. It was not that he could not be eminently sociable when necessary, or that he did not allow himself some relaxations when, fatigued with work, he needed some repose. But these were only accidental circumstances in his life, which was so exclusively devoted to his laboratory.

The scientific career of Faraday was equally fortunate and complete. Named as early as 1823 a correspondent of the Academy of Sciences of Paris, he was called in 1844 by this same Academy to occupy one of its eight foreign Associateships, after having been associated successively with all the learned bodies of Europe and America. He was by no means insensible to these scientific honours, which he accepted with genuine satisfaction, whilst he constantly refused every other kind of honorary distinction.

But it is time to commence the more important part of this notice, that which is to be devoted to the examination of the works of Faraday. Only I may perhaps be allowed, before speaking of the works themselves, to say a few words of the manner in which Faraday worked.

Is it true that the man of science who wishes to interrogate nature must set himself face to face with his apparatus, make them act to derive facts from them, and wait until these facts have appeared, in order to deduce their consequences, and all without any preconceived idea? Most certainly the philosopher who could advance such an opinion has never experimented, and in any case this method has never been that of discoverers; it was assuredly not the one adopted by Faraday.

There is a second method also which was not his, although it is truly worthy of attention, and often fertile of results. This consists in taking up known phenomena and studying them with great precision, carefully determining all the elements and numerical data, so as to deduce therefrom the laws which govern them, and often

also to show the inexactitude of the laws to which they were supposed to be subjected. This method requires great previous study, great practical talent in the construction of apparatus, remarkable sagacity in the interpretation of the results furnished by experiment, and, lastly, much perseverance and patience. It is true that it leads with certainty to a result; and this is its good side; but the difficult conditions which it imposes are so many obstacles which prevent its being generally followed, except by the highest intellects.

A third method, very different from the last mentioned, is that which, quitting the beaten track, leads, as if by inspiration, to those great discoveries which open new horizons to science. This method, in order to be fertile, requires one condition—a condition, it is true, which is but rarely met with,—namely genius. Now this condition existed in Faraday. Endowed, as he himself perceived, with much imagination, he dared to advance where many others would have recoiled; his sagacity, joined to an exquisite scientific tact, by furnishing him with a presentiment of the possible, prevented him from wandering into the fantastic. Still always wishing for facts, and accepting theories with difficulty, he was nevertheless more or less directed by preconceived ideas, which, whether true or false, led him into new roads, where most frequently he found what he sought, sometimes indeed what he did not seek, but where he constantly met with some important discovery.

Such a method, if indeed it can be called one, although barren and even dangerous with mediocre minds, produced great things in Faraday's hands; thanks, as we have said, to his genius, but thanks also to that love of truth which characterized him, and which preserved him from the temptation so often experienced by every discoverer, of seeing what he wishes to see, and not seeing what he dreads.

The works which have issued from his brain, so well organized, are numerous and varied; they relate essentially, as we have already stated, to chemistry and electricity. Those on the latter subject are by far the most numerous and important; we shall therefore devote to them the greater part of this notice, after giving a summary exposition of the others.

I.

In 1816, Davy received a specimen of native caustic lime from Tuscany. He gave it to Faraday for analysis, and found that the account given was so perfect that he had it printed, and accompanied it with some observations. This success, by giving Faraday confidence in his own strength, encouraged him to attempt other original researches. He published (in 1817 and

1818) an investigation of the passage of gases through narrow tubes, from which it appeared that the velocity of the flow of elastic fluids does not depend upon their density alone, but also upon their individual nature. Various other points of chemistry and physics, besides those which had electricity and magnetism for their object, attracted his attention from time to time throughout the whole of his scientific career. Now we have a note upon the combustion of the diamond; then an investigation of the sounds produced by the combustion of gases, or by the superposition of a strongly heated iron rod upon a mass of copper at the ordinary temperature (Trevelyan's experiment); and then, again, researches upon the limit of vaporization, or upon the evaporation of mercury at low temperatures. We may notice two important memoirs—one upon the explanation of certain optical illusions produced by bodies in motion, the other describing some new acoustic figures proceeding from the vibrations of the stratum of air in contact with the surface of vibrating plates. His elegant discovery of *regelation* (that is to say, of the power possessed by two fragments of ice when brought together to become amalgamated by the fact of their simple contact at a temperature above 32° F.), followed into its consequences as it has been by Tyndall, has had a much greater influence than perhaps he ever expected. In all these notices, even the least important of them, we find an original idea, a new and striking point of view, which enables us at once to recognize Faraday. And, in connexion with this, how can we omit to mention his simple and clear explanation of table-turning, and the ingenious experiment by which he so clearly shows the muscular efforts made unconsciously by the persons who, by laying their hands upon the table, cause its movement?

Let us now dwell for a few moments upon some researches of longer duration, the publication of which preceded, and also in great part accompanied, his great works on electricity.

In 1820 Faraday described two new compounds of chlorine and carbon. One of them is solid, transparent, and colourless; it crystallizes in little prisms and in laminae, and is obtained by exposing to the direct action of the sun bicarbonated hydrogen gas with a large proportion of chlorine. The other contains less chlorine; it is liquid and colourless, possesses great density, and is prepared by passing the former through an incandescent tube, from which chlorine is set free. The discovery of these two compounds filled up an important gap in the history of chemistry.

Subsequently (in 1825), by the compression of the gas obtained from coal, Faraday obtained a new compound, which, no less interesting than the preceding from a scientific point of view, had besides a great industrial importance. This was a bicarburet

of hydrogen in a liquid state, which was found to be a mixture of several compounds endowed with various degrees of volatility, and which could be separated by distillation. Every one knows the advantage, in the production of colours, derived from this by the illustrious chemist Hofmann, when he extracted aniline from it.

The discovery of this bicarburet of hydrogen was only an incident in the researches which Faraday had undertaken in 1823, upon the condensation of gases into liquids. His mode of operation in this investigation consisted in placing, in one extremity of a recurved tube closed at both ends, the necessary ingredients for the production of the gas, and plunging the other extremity in a freezing-mixture. The gas, evolved in a closed space, speedily condensed into a liquid state in the refrigerated extremity of the tube. In this way chlorine, sulphurous acid, sulphuretted hydrogen, carbonic acid, protoxide of nitrogen, cyanogen, ammonia, and hydrochloric acid were successively reduced to a liquid state. With the exception of chlorine, all these liquefied gases were colourless and perfectly transparent; and all of them had a refractive power superior to that of water. The attempts made to reduce the other gases, especially hydrogen, oxygen, and nitrogen, to a liquid state were fruitless. Twenty years later (in 1844) Faraday resumed these experiments by directly condensing the gases by mechanical processes in very strong and hermetically sealed tubes, refrigerating them by means of the mixture of ether with solid carbonic acid produced by Thilorier's method. The condensation could be brought to fifty atmospheres, and the lowering of temperature to -166° F., or 110° C. below 0° . In this way Faraday succeeded in liquefying, besides the gases which I have already mentioned, olefiant gas, phosphuretted hydrogen, and arseniuretted hydrogen, as also fluosilicic acid; but he did not succeed in solidifying them. On the other hand, by applying his new process to the gases which he had previously liquefied, he brought them not only to a liquid state, but even to that of transparent and crystalline solids; hydrochloric gas alone of these latter would not become solid, whilst hydriodic and hydrobromic gas were successively liquefied and solidified.

It is easy to understand all the importance of an investigation the result of which was to modify completely the received ideas as to the constitution of the permanent gases by causing them to enter into the category of simple vapours; this was to introduce into molecular physics a new and important notion, the consequences of which have gradually unfolded themselves.

It is also to a question of molecular physics that we must refer the memoir on the relations of gold and the other metals

to light, published by Faraday in 1857. Among other interesting facts that this memoir contains, we shall cite that of a leaf of beaten gold, which, when placed upon a plate of glass, becomes perfectly transparent and colourless when it is brought to a high temperature, and which, when seen by transmitted light, resumes its green colour when it is subjected to strong pressure. A great number of experiments upon the pulverulent deposits of various metals obtained by electrical discharges transmitted through very fine wires, led to remarkable results as to the variations of colour arising from change in the molecular state of the same body. We also find in this memoir a detailed investigation of the various colours presented by different solutions of gold, and especially of the fine ruby-red tinge obtained by the solution of a quantity of gold which, if agglomerated into a single mass, would not occupy the seven-hundred-thousandth part of the volume of water which it colours. It is not necessary to dwell upon the interest presented by researches having for their object the study of the influence, still so imperfectly known, of the molecular structure of bodies upon their relations to light, and especially upon their transparency.

Among the numerous works of Faraday relating to the applications of science to the arts, we shall confine ourselves to citing his researches upon the manufacture of steel, and of glass for optical purposes, these being the most important.

It was by the analysis of the Indian steel called *wootz* that he was led, in concert with Stodart, to compose an alloy which had all the properties of this, by combining aluminium with iron and carbon. In a letter addressed in 1820 to Professor De la Rive*, he relates all the attempts made by his collaborateur and himself during two years of persevering labour, to discover the most satisfactory alloys. He indicates, as one of the best, that of rhodium and steel, and, as presenting curious peculiarities, that of steel and silver; this last alloy does not become a true combination unless the silver only forms $\frac{1}{500}$ part of it. Platinum, on the contrary, combines in all proportions with steel, but it does not furnish so good an alloy as rhodium and silver for the construction of cutting-instruments.

Although interesting in many respects, the results which Faraday obtained in his great investigation of the alloys of steel were not proportionate in their importance to the time and trouble which they cost him. We may say the same of the laborious researches upon the manufacture of glass for optical purposes, which he made a few years afterwards (in 1829). It was upon the initiative taken in 1824 by the Royal Society of London, which named a committee for the study of the improvement of glass

* See *Bibl. Univ.* (1820) vol. xiv. p. 209.

with a view to its optical use, that Faraday was called upon to occupy himself with it. Whilst he pursued the chemical part of these investigations, Dollond worked up the glass, and Herschel subjected it to the test of experiment. At the end of long and difficult experiments, Faraday ascertained that the greatest difficulty in the way of the fabrication of a good flint glass (that is to say, a very refractive glass) was the presence of streaks and striæ proceeding from a want of homogeneity, due, in its turn, to differences of composition between the contiguous portions of the same glass. The employment of oxide of lead in the composition of flint glass was the cause of this defectiveness, which could not be avoided even by making use of the most efficacious means of rendering the mixture perfect while in a state of fusion. Among the combinations tried, that of borate of lead and silica furnished a glass endowed with optical properties still more strongly marked than those of flint glass, and at the same time presenting a very uniform structure. This glass, which, on account of its great density (double that of flint glass), has been named heavy glass, is found unfortunately to have a slight yellowish coloration which renders it unfit for optical purposes: but the labour which Faraday devoted to its fabrication has not been lost; for, as we shall see hereafter, this same glass, in the hands of the talented experimenter, became the instrument of one of his most beautiful discoveries.

In the long and curious memoir which he published upon the fabrication of optical glass, Faraday gives a minute description of all the processes employed by him—of the construction of furnaces, selection of crucibles, means of heating, various artifices, such as the injection of platinum in powder into the fused glass to cause the disappearance of bubbles, &c. It is a genuine instruction in chemical manipulation, and, as it were, a complement to his *Treatise* on this subject, which was published in 1827, and has since gone through three editions. Only those who are called upon to experiment in the domains of physics and chemistry can appreciate the immense service which this treatise has rendered to them, by teaching them a multitude of processes of detail so valuable for them to know, and of which a description was previously nowhere to be found, so that every one was obliged to undergo an apprenticeship to them on his own account. It was necessary that a *savant* who for so many years had been struggling with the difficulties of experimentation, and who had been able to surmount them in so ingenious a manner, should give himself the trouble to describe the means which he had employed, so that his experience might be of service to others. Faraday was this *savant*, and his object was completely attained.

Here, perhaps, before proceeding to another set of subjects,

we ought to speak of certain of Faraday's theoretical ideas relating to general physics, and more especially to the nature of the forces, and their correlation to each other and to the essence of matter; but we prefer not to discuss the opinions emitted by him upon these questions until after the exposition of his works on electricity and magnetism. We must, however, at once admit that his views on these matters are very contestable, and that, if they inspired him to make experimental researches of the highest interest, this is a proof that, in the hands of a man of genius, even a bad theory may be the origin of the most beautiful discoveries.

II.

I pass now to the examination of those works of Faraday which relate to electricity and magnetism. It is not without embarrassment that I approach this examination; for these researches are so numerous that it would be necessary to extend this notice beyond all bounds in order to give only a simple analysis of them; and they are at the same time so varied, that it is impossible to explain them in the chronological order of their publication without confusion being the result. Thus, for example, the researches on induction are interrupted by others on electrochemical decompositions, to be afterwards resumed and completed. Each memoir certainly forms a complete whole; but one memoir is most frequently followed by another the subject of which is quite different. It seems as if the author, after having treated one question, found it necessary to recollect himself before resuming it, and to divert his mind from it, so to speak, by taking up some other kind of work.

It has therefore appeared to me that the best thing for me to do was to group all these various works under a few distinct heads, so as to be able to give their essence without requiring to enter into too many details. The first would include all the researches relating to electrochemistry; the second those which have for their object induction, whether electrodynamic or electrostatic; and the third the phenomena relating to the action of magnetism and dynamic electricity upon light and upon natural bodies in general. It is true that there are some works which elude this classification, as they will not enter into any one of our three divisions. But these are less important works, and such as were produced as occasions offered; that is to say, they are the fruit of some particular circumstance which attracted Faraday's attention to some special point. Such is, for example, the memoir which has for its object the investigation of the electrical properties of the *Gymnotus*,—and that devoted to the evolution of electricity by the friction exerted against solid bodies

by the globules of water or other substances carried up by vapour—experiments undertaken in consequence of the invention of Armstrong's machine. Lastly, there are others which only contain the more or less indirect consequences of the fundamental discoveries, which will be explained in one of the three subdivisions under which we have grouped them. We shall not dwell upon any of these, thinking that we may give a more exact and complete idea of all the progress which Faraday caused the science of electricity and magnetism to make by confining ourselves to pointing out in some detail the most prominent parts of his researches upon these subjects.

Faraday commenced with chemistry in his scientific career; it is therefore not surprising that he approached electricity by the study of electrochemistry. It was, moreover, towards electrochemistry that his attention must have been first directed in that laboratory of the Royal Institution which had witnessed the magnificent discoveries of Davy in chemical decompositions effected by the pile, and especially in the production of the alkaline metals. In taking up this subject, Faraday only followed the traditions left to him by his predecessor.

His researches upon the electrical conductivity of bodies constitute a first step in this path. The business was to ascertain whether, as was previously supposed, the presence of water is necessary to render solid bodies conductors, and whether solid non-metallic (and consequently compound) bodies can conduct electricity without being decomposed. Commencing with water, which is an insulator when solid and a good conductor in the liquid state, Faraday shows that a great number of compound substances are in the same case. Such are many oxides, some chlorides and iodides, and a multitude of salts, which do not conduct electricity in the solid state, but, without any intermixture of water, become excellent conductors when liquefied by heat, and are not decomposed by electricity with separation of their elements in the same way as aqueous solutions. To the list of these compounds Faraday adds that of those substances, either simple, like sulphur and phosphorus, or compound, such as the periodides and perchlorides of tin, and many others, which continue isolators when fused as well as in the solid state. In this first investigation, notwithstanding a great number of experiments in which he employed the influence of heat and of electricity of high tension in the study of the conductive power of solid bodies, he did not succeed in determining very accurately the conditions of electrical conductivity; he only ascertained that, with one exception, which he justly regards as only apparent, there is not a solid body which, on becoming conductive by its passage to a liquid state, is not decomposed by the electrical cur-

rent. We may add, so as not to return to the subject, that Faraday sometimes had doubts upon this point, and that he even thought that water could conduct electricity without being decomposed. Now experiment shows that in all cases, even those which appear most favourable to this opinion, electricity cannot be transmitted under any form through a compound liquid body without this body undergoing electrochemical decomposition.

As to the causes of conductivity, they are still far from being known; when we see bodies, such as the gases, becoming conductors when greatly rarefied, whilst under the ordinary pressure they are perfect insulators, we are compelled to come to the conclusion that the impossibility that we find of explaining this difference, as well as so many others presented in this respect by solid and liquid bodies, is due to the fact that we have not yet a correct notion of the molecular constitution of bodies. Perhaps the recent theories of several physicists, particularly that of Clausius, who regards the particles of bodies as being in a constant state of movement, may succeed in elucidating this subject, which is still so mysterious. Faraday himself had fully foreseen this relation between electrical conductivity and the ideas which we may form as to the nature of matter. In a remarkable article published in 1844 he showed, upon an experimental basis, that, in the theory according to which a body is regarded as consisting of atoms possessing weight separated from each other by larger or smaller intermolecular intervals, there are a multitude of facts, some of which can only be explained by assuming that the atoms are the conductors and the molecular space an insulator, and the others by supposing that the intermolecular space is the conductor and the atoms insulators—a contradiction which is inadmissible. He concluded from this that we must imagine matter to be continuous, or rather imagine the atoms to be simply centres of force, and consequently replace the atomistic by the dynamical theory. We shall often find traces of these ideas in the subsequent works of Faraday; for ourselves we cannot take this view. We are convinced that it is not by denying the existence of matter, properly so called, and admitting only that of forces, that we shall succeed in solving the difficulties under consideration and many others, but rather, following the example of Clausius and others, by modifying the ideas hitherto accepted as to the mode of constitution of bodies, and replacing them by others more in accordance with recent discoveries.

But we must return to electrochemistry. I have already said that Faraday first occupied himself with chemical decompositions effected by the electrical current. He commences by effecting the decomposition of water and of solutions by means of a jet of

ordinary electricity, rendered as continuous as possible by leaving a stratum of air interposed between the metallic points which convey and carry off the electricity from a machine, and a strip of moistened paper which this electricity traverses; he observes that the deposition of the elements separated from the decomposed liquid takes place against the surface of the air which is in contact with the paper. Then, investigating the decompositions effected by the pile, he examines the various explanations which have been given of this phenomenon, and concludes that it is much rather a chemical phenomenon than a truly electrical one. In other words, it is a peculiar form of affinity which, under the influence of electricity, is exerted between the neighbouring molecules, so that the decomposition is the easier in proportion as the affinity is stronger. He shows that the transfer of the elements can only take place between bodies the constituent parts of which have an affinity for each other; and if these elements separate in a free state against the surface of the metallic poles of the pile, this is because they cannot combine with the substance of these poles; for whenever this combination is possible, they are no longer set free. Water in some cases, air in others, as we have already seen, may serve as poles just as well as solid bodies. Faraday justly rejects the old idea of certain physicists who attributed electrochemical decompositions to the ordinary electrical attractions and repulsions exerted upon the elements of a conductive liquid by the voltaic poles immersed in it. The metallic wires, or other conductors, which transmit electricity into a liquid, are merely, according to him, the roads by which the electric current passes into the liquid; therefore, to exclude any idea of electrical tension which is more or less implied in the name *pole*, Faraday proposed to substitute for the denomination poles that of *electrodes*. He likewise applied the term *electrolysis* to the chemical decomposition effected by electricity, reserving that of *analysis* for the ordinary chemical decompositions in which electricity does not assist. Lastly, he gives the name of *electrolytes* to those compound bodies which are capable of being decomposed by the electric current.

After this preliminary and general study of the subject, Faraday enumerates the results which he obtained by submitting to electrochemical decomposition a very great number of compounds, some of them simple acids or simple bases, others saline combinations. He dwells particularly on the secondary effects often manifested in these decompositions, especially in the case of aqueous solutions, in which decomposition of the water and of the substance dissolved takes place at the same time. But the essential point of his researches is the law at which he arrived as to the definite nature of electrochemical de-

composition. He demonstrates, relying solely upon experiment, that the quantity of chemical action exerted by an electrical current is proportionate to the quantity of electricity constituting this current, and, further, that the same quantity of electricity, or the same current, decomposes chemically equivalent quantities of all the compound bodies through which it is passed. Thus, if we place one after the other, in the circuit of a voltaic pile, several pieces of apparatus arranged for the decomposition of water and for collecting the gaseous products of this decomposition, we find that in all, even when the degree of acidity of the water and the form and size of the electrodes are different in each, the same current traversing them for a given time produces the same quantity of gas, and consequently decomposes the same quantity of water. The quantity of water decomposed in a given time, appreciated by the quantity of gas evolved, is therefore the exact measure of the quantity of electricity which has produced this effect. Hence, like Faraday, we give the name of *voltmeter* to the very simple apparatus which holds acidulated water destined to be decomposed by the current, and by means of which the volume of gases set free by this current in a given time may be exactly measured.

The second principle, that the same quantity of electricity decomposes chemically equivalent quantities of all compound bodies, was demonstrated by Faraday by placing several different electrolytes one after the other in the same circuit, as, for example, acidulated water in a voltmeter, and protochloride of tin and chloride of lead in a state of fusion; and he obtains quantities of tin, lead, chlorine, hydrogen, and oxygen which are chemically equivalent. Then, rising from the effect to the cause, he comes to the conclusion that there is a perfect equality between the electricity which decomposes a body and that which is generated by the chemical action which produces the direct decomposition of an equal quantity of the same, or of a chemically equivalent quantity of some other body. He is thus led to pay attention to the theory of the pile, and to recognize that the power of this apparatus originates in chemical action, and not in the contact of two heterogeneous metals—a contact which is not necessary either to produce a spark or to cause a chemical decomposition.

He establishes, in the first place, that, either to effect a decomposition or to produce a spark, a plate of zinc immersed in acidulated water is sufficient without its being necessary to bring the zinc into contact with any other metal. He shows that in every pile the presence of an electrolyte (that is to say, a liquid susceptible of being decomposed) is indispensable for the evolution of electricity. Then, distinguishing in the electricity generated the intensity (or the tension) and the quantity, he studies

the circumstances, depending either on the nature of the chemical action or the number of voltaic pairs associated, which exert an influence on these two characters of the current. In a word, he establishes such a correlation between that which occurs in the interior of a pile, and that which takes place in the electrolyte interposed between the poles of this pile, that it is impossible not to admit (with him) that electrolytic decomposition is nothing but a form of chemical affinity transferred from the pile into the electrolyte decomposed.

Wishing to obtain an idea of the quantity of electricity which is associated with the particles of which matter is composed, he endeavours to estimate that which is necessary for the decomposition of a grain of water, regarding it, as he is justified in doing, as equivalent to that produced by the direct chemical action (of the acidulated water upon the zinc) which decomposes this grain of water. He arrives at this incredible result—namely, that this quantity of electricity, appreciated by the heat evolved by it in traversing a fine platinum wire, is superior to that manifested in 800,000 discharges of a battery of Leyden jars, charged by thirty turns of a powerful plate-machine, and consequently equivalent to that constituting a violent flash of lightning.

The researches of which I have been speaking were made in 1833, 1834, and 1835. I had previously paid attention to the same questions, and had arrived by somewhat different methods at the same conclusion with Faraday—namely, that it is in chemical action that resides the origin of the evolution of electricity in the voltaic pile. Faraday frequently alludes to my investigations in a very kind manner; and subsequently (in 1840) he wrote me a letter in which he said that, being a thorough adherent of the chemical theory, he had just attacked the question directly, as I had already done, by demonstrating that contact alone, if not accompanied by chemical action, is not a source of electricity. The memoir in which he probes this question to the bottom is the last which he devoted to this department of electricity. In it, by means of a multitude of ingenious experiments, he demonstrates that the presence of an electrolyte (that is to say, of a liquid which is at once a compound and a conductor of electricity) is indispensable for the production of electricity in a voltaic couple; he varies his experiments in a thousand ways, sometimes by exhausting the number of chemical compounds employed as electrolytes, sometimes by the intervention of temperature or of other agents; and he concludes by showing by general considerations the improbability of the existence of a force of contact.

We may say that this last work, a precious supplement to the preceding ones, has rendered perfectly evident the truth of the

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chemical theory. This theory, foreseen by Wollaston and Fabroni, but opposed by most of the physicists of the early part of the present century, had found a powerful argument in its favour in the beautiful experiments of the elder Becquerel upon the electricity developed by chemical actions. It was then (from 1825 to 1835) that, profiting by these experiments, and seeking, on my own part, to make others of the same kind although in a slightly different direction, I published several memoirs to support and render more precise the chemical theory of the voltaic pile. But I cannot but admit that we are indebted to Faraday for having based this theory upon irrefutable proofs, not only by the great number and variety of his researches, but especially by his beautiful discovery of the definite decomposing action of the electric current—a discovery which established between the external chemical action of the voltaic pile and the chemical action which takes place in the interior of this apparatus, a relation so intimate that it is impossible not to see in the latter the cause of the former.

III.

In 1831 Faraday discovered electrical induction; it is the most important, although perhaps not the most brilliant of his discoveries. Ten years before (in 1821) he had observed a perfectly new phenomenon in the science of electrodynamics,—that science which issued complete, as we may say, from the brain of Ampère, after Ørsted's discovery. Struck by the experiments of the great French physicist upon the mutual attractions and repulsions of electrical currents and magnets, Faraday was led, by theoretical ideas which were rather disputable and not very conformable to the principles of mechanics, to assume that an electric current must turn round the pole of a magnet with a continuous movement, and reciprocally that the pole of a magnet must in like manner turn round an electric current. He verified this double result by experiment; and Ampère soon showed its accordance with his theory, adding to it other facts of the same nature. It is not the less true that the discovery of a continuous movement of rotation due to the combined action of a magnet and an electric current was quite unforeseen, and at the same time very important; for up to that time there was no example of any such action in physics. It was a first step in the course which was to lead to the finding of a relation between mechanical movement and the molecular forces.

Arago (in 1824) was the first who directly established this relation, by his beautiful discovery of magnetism by rotation; for he showed that simple mechanical movement could render a body, in itself non-magnetic, capable of acting upon the magnet.

Faraday advanced still further in 1831, by discovering that it was sufficient to bring towards, or remove from, a metallic wire forming a closed circuit another parallel wire traversed by an electric current, or simply a magnet, in order to develop in the former wire an electric current. He discovered induction—that phenomenon which so many others had sought in vain, although suspecting its existence, but which he alone had succeeded in producing.

Let us dwell for a moment upon his fundamental experiment. Two metal wires covered with silk are rolled together round a cylinder of glass or wood; the two wires are thus isolated, and have all their spires approximate and parallel. An electric current is passed into one of these wires; immediately a current is manifested in an opposite direction in the neighbouring wire, the extremities of which are united by a galvanometer; but this current only lasts for a moment. The current passing through the first wire is interrupted; immediately another current is developed in the second wire, which is momentary, as in the former case, but directed in the same way as the producing current, instead of in the contrary direction. The momentariness of these two currents, and the fact of their alternately opposite directions, constitute the two important characters of this new mode of production of electricity.

Faraday did not stop at this. Starting from Ampère's idea that a magnet is only an assemblage of electric currents arranged round an axis in a manner very analogous to the circulation of an electric current through a metallic wire rolled into a coil, he tried the replacement, in his fundamental experiment, of the wire traversed by the current by a simple magnet. For this purpose he twisted a single wire instead of two into a coil round a glass or wooden tube; then he introduced a magnet into this tube, and ascertained that at this moment a momentary current is developed in the coil of wire, and that a second, equally momentary but in an opposite direction, is developed at the moment when the magnet is withdrawn. Here, therefore, was realized that production of electricity by magnetism which Faraday had long been seeking, convinced, as he was, that, as electricity produces magnetism, magnetism in its turn must produce electricity.

Is it necessary to follow Faraday in the multiplied experiments by which he demonstrates that the electricity developed by induction possesses all the properties of voltaic electricity, and of the ordinary electricity produced by machines—that it heats fine metallic wires, gives shocks, and even produces the spark? To produce an electric spark by means of the action of a simple magnet, is one of those striking facts which give to the discovery

leading to such a result a popularity, if I may venture so to express myself, which is reflected upon its author.

Faraday soon showed that terrestrial magnetism, like that of a magnet, can develop electric currents by induction in a metallic wire rolled into a coil or a circle, and actuated by a movement of oscillation in a plane perpendicular to that of the magnetic meridian. He found that it was not even necessary to employ metallic wires to ascertain the influence of the terrestrial magnetism upon the production of induced currents, but that it sufficed to set a metallic disk (of copper for example) in rotation in a plane perpendicular to the direction of the inclination-needle to find that it is traversed by electric currents passing from the centre to the circumference, or from the circumference to the centre, according to the direction of the rotation. Still more readily does the vicinity of a magnet to a similar disk set in rotation in any plane under the influence of this magnet develop in it induced currents, the presence of which, directly ascertained, explains in a perfectly satisfactory manner the phenomena of magnetism by rotation discovered by Arago.

These currents, although difficult to perceive, must nevertheless possess considerable power, since they can drag a rather heavy magnet by the action which they exert upon it. It is probable that this power is due less to their individual intensity than to their number, which appears to be very considerable. We may cite two examples which prove in a striking manner the energy which this mode of production of induced currents may acquire. The first is furnished by a curious experiment of Faraday's, in which, on causing a cubical mass of copper suspended by a thread between the poles of an unmagnetized electromagnet to turn upon itself, he saw this mass stop suddenly the moment he magnetized the electromagnet, in consequence of the magnetic action exerted by the currents which induction had set up in the copper. We find the second example in the fact observed by Foucault, of the sudden stoppage which is likewise experienced by a thick disk of copper set in rotation between the poles of an electromagnet the moment the latter is magnetized. This stoppage is such that it can only be surmounted by a considerable effort, and the disk itself becomes very strongly heated if the rotation be continued in spite of the resistance it meets with. In order that such a heating effect should be produced in a mass of such considerable size, and that we should experience an attractive action so strong on the part of the electromagnet, the induced currents thus produced must be of very great power—a power which they owe essentially to the excessive rapidity of the movement generating them.

I shall not follow Faraday through all his works upon induc-

tion which accompanied his fundamental discovery. I shall only refer to the fact that in 1834 he discovered a new important fact—namely, the production of an induced current in the very wire that conducted the inductive current, and which takes place at first at the moment when the latter current begins to circulate, and then at that when it ceases passing. If this wire is rolled in a coil round a cylinder of soft iron, the effect produced acquires great intensity by the fact of the alternate magnetization and demagnetization of the iron which accompanies the passage and interruption of the current in the wire. We all know the advantage that has been taken of this combination in the construction of very powerful apparatus. We also know how, from one improvement to another, we have come to find in induction, and consequently in the simple mechanical movement which gives birth to it, the most simple and economical principle for obtaining electricity, especially with regard to its application to therapeutics and illumination.

The discovery of electrodynamical induction (that is to say, the production of a current by the influence of an exterior current) led Faraday to examine more closely than had previously been done into the phenomenon of statical induction—that is to say, the development at a distance of tension-electricity in an isolated conductor by the influence of an electrized body. He ascertained, what no one had previously suspected, that the nature of the body interposed between the source of electricity and the conductor submitted to the action of this source had a great influence upon the effect produced—that, of the various bodies, some facilitated the development of electricity at a distance, whilst others completely stopped it. He named the former *dielectrics*; and he proved that these dielectrics, which are essentially resins, sulphur, shellac, oils of turpentine and naphtha, &c., enjoy this property of transmitting electricity by influence in different degrees, whilst there is not in this respect any difference between the gases, which have the same dielectric power whatever their nature or their density may be. On the other hand, none of the metals are dielectric; they are subject to the electrical influence, but do not transmit it.

From the investigation which we have just summarized, Faraday drew the conclusion that induction does not take place at a distance, but that it is effected by the intermediation of the particles interposed between the inductor and the inducted body. He assumed that these particles are polarized one after the other, which M. Matteucci afterwards demonstrated directly by experiment; that consequently the mode of propagation of electricity is the same in insulating as in conducting bodies; and that the various substances only differ from each other by the greater

or less facility or rapidity with which this polarization, necessary for the transmission of electricity, takes place in them. Then, passing from this to the analysis of the different modes in which electrical discharges take place, some obscure, others luminous, some electrolytic (that is to say, accompanied by the chemical decomposition of the conducting body), others disruptive (that is to say, effected by the mechanical disjunction of the particles of the interposed substance), he applied himself more particularly to the study of the various forms displayed by the electric spark in more or less rarefied gases. I should never have done if I were to attempt to explain all the experiments which he made to elucidate these different points and to arrive at an idea of the actual nature of the electric current. The identity of the current, whatever may be its origin,—that its production is due to polar forces which may exert a transverse action, as is the case in electrodynamical phenomena,—that these polar forces emanate from contiguous particles; such are the principles which Faraday endeavoured to establish as the consequences of his experimental researches, at the same time that he rejected the idea of actions at a distance, referring all electrical manifestations to the presence of ponderable matter.

Whether or not we completely admit all Faraday's ideas, it is impossible not to acknowledge the immense advance which he caused the theories of electricity to make, either by demonstrating by experiment the falsity of certain conceptions generally accepted up to his time, or by opening up perfectly new points of view as to the actual nature of electrical phenomena. We have just had the proof of this in the consequences to which he was led by his investigations on statical induction. His discoveries in electrodynamical induction have had still more important consequences, by introducing the notion of mechanical movement into the essence of electrical movement, and thus enabling Weber to combine, in an equally ingenious and satisfactory manner, the mechanical phenomena of electrodynamics, discovered by Ampère, with the electrical phenomena due to mechanical movement, discovered by Faraday.

Ampère and Faraday,—two names which will always be united by the intimate relation of their works to the history of the science of electricity, in which they have opened such new and vast horizons; and yet minds as dissimilar in their mode of proceeding as similar in the power of their genius. Both eminently endowed with that faculty of divination which generates great discoveries, but one of them, Faraday, arriving at them by impression, by a kind of instinct which never deceived him,—the other, Ampère, advancing with a more certain step, having as his instrument those calculations which he handled with such re-

markable ability, and thus arriving at results which he hardly required experiment to confirm, so certain was he that this would not contradict him.

IV.

I now pass to the last great series of Faraday's works. I have said, and, I think, proved, that induction was the most important of his discoveries; I must now say that the action of magnetism and electricity upon light was the most brilliant. Often the attempt had been made to see whether magnetism and electricity exerted any direct influence upon light; but these attempts had always failed. Investigators had operated upon luminous rays travelling in the air or in liquids, and endeavoured to act upon them, sometimes by strong magnets, sometimes by electric currents or by statical electricity; but these attempts had led to nothing, absolutely nothing. All these negative investigations have never been published, but they have nevertheless been made.

Guided by theoretical considerations upon the mutual correlation of the forces of nature, Faraday, after many fruitless attempts, succeeded in finding the connexion which exists between light and the magnetic and electric forces. Instead of taking an ordinary ray, he operated with a polarized ray; instead of acting directly upon this ray by means of a magnet, he submits it to the influence of magnetism whilst it is traversing a glass prism in the direction of its length. This prism, terminated by two square and parallel bases, the surfaces of which are well polished, and which are those by which the polarized ray penetrates and issues from the prism, is placed between the poles of an electromagnet in such a manner that its length and, consequently, the direction of the transmitted ray are parallel to the line joining the magnetic poles. Lastly, the polarized ray on issuing from the glass prism only reaches the eye after passing through a Nicol's prism which serves as an analyzer. It is also by traversing a Nicol's prism before penetrating into the glass prism that the ray of light is polarized; but this may be effected in any other manner.

It is well known that by turning the analyzing prism to a certain angle the polarized ray is extinguished, in such a manner that the brilliant spot is replaced by a black spot. If, after this operation has been effected, a strong electric current is passed through the wire surrounding the electromagnet, the black spot disappears and the bright one again makes its appearance. Then by turning the analyzing prism a little further in the same direction, the luminous ray is again extinguished, but this extinction ceases as soon as the magnetic action is suppressed by the

interruption of the current which magnetized the electromagnet. The action of magnetism therefore consists simply in causing the plane of polarization to turn by a certain angle, and to give artificially to the glass, while it is under the magnetic influence, a property which certain substances, such as quartz and essence of turpentine, possess naturally.

Any transparent substance, except gases, may serve, although in different degrees, as the *medium* for magnetism to act upon the polarized ray. But that by means of which this influence is best manifested is the yellowish heavy glass (borosilicate of lead) which Faraday obtained in his experimental researches upon the fabrication of glass for optical purposes. He happened to have at hand several specimens of this glass; and it was by using one of these for performing the experiment just described that he discovered the magnetic rotation of the plane of polarization, a phenomenon which would probably have escaped him if he had made use of ordinary glass at first starting. Thus the long and painful labours to which he had formerly devoted himself without any great success, in order to discover a glass fitted for the fabrication of lenses, were not lost to science, since they facilitated his enriching it with one of his finest discoveries.

Let us now study the new phenomenon a little more closely, so as better to show all its importance. Some substances, we have said, naturally possess the property of causing the plane of polarization of a polarized ray traversing them to rotate through a larger or smaller angle; some cause it to turn to the right, and others to the left of the observer. The discovery of Faraday was that the influence of magnetism or of electric currents developes this same property in nearly all transparent substances, but with this difference, that the direction of rotation of the plane of polarization depends only upon the position of the magnetic poles, or the direction of the currents with relation to the transparent substance. The law is that if the north pole of the electromagnet is placed on the same side as the observer who receives the ray into his eye, and consequently the south pole on the side by which the polarized ray enters into the substance, the rotation of the plane of polarization takes place, to the observer, from left to right. It takes place from right to left if the direction of the current, and consequently that of the magnetization, be changed. The action of the magnet may be replaced by that of a coil in the axis of which the transparent substance is placed. In this case, again, the rotation of the plane of polarization is very well observed when a rather strong current is transmitted through the wire of the coil; and the direction of the rotation is always the same as that of the current.

Thus, whilst in substances naturally endowed with circular polarization the rotation of the plane of polarization always takes place, according to the nature of the substance, either to the right or left of the observer, in Faraday's experiment the direction of this rotation only depends upon the direction of electric currents or the relative position of the magnetic poles, since it is completely independent of the position of the observer. These two kinds of action are therefore not identical, and we cannot say that by the influence of the magnet or of electricity we produce in all transparent bodies exactly the same property that certain substances naturally possess. Faraday well shows this difference by an experiment which consists in producing by an ingenious artifice the internal reflexion of the polarized ray upon the extreme surfaces of the prism; this may be done once or several times before the ray is allowed to escape, and doubles, triples, or quadruples the angle of rotation of the plane of polarization, according as the ray is reflected once, twice, or three times. But when, instead of the magnetic, we have to do with the natural rotatory polarization, the result is quite different, the return of the reflected ray neutralizing the effect which the direct ray had undergone while travelling in an opposite direction. In this case the angle of rotation of the plane of polarization reflected twice, and which consequently has three times traversed the transparent substance, is no greater than that of a ray which has only traversed it once.

The general phenomenon so unexpectedly discovered by Faraday has hitherto remained unexplained, notwithstanding many investigations, and especially the persevering and remarkable researches of M. Verdet.

It has not even been possible to connect it with some other property of bodies, although each substance has its specific magnetic rotatory power. Faraday, however, drew from it a general consequence which led him to another discovery, namely, that magnetism acts upon all bodies, since all transparent bodies may be modified under its influence sufficiently to acquire, in different degrees indeed, a power which they do not possess of themselves. The discovery to which I have just alluded is that as the magnet acts by attraction upon magnetic bodies, it acts also by repulsion upon all other bodies in nature. From this it results that whilst a rod of iron, or of some other magnetic substance, suspended between the poles of an electromagnet, places itself *axially* (that is to say, parallel to the line which joins the poles), a prism of heavy glass (the same, for example, which served for the experiments on light) places itself *equatorially* (that is to say, transversely to this line). A rod of bismuth is in the same case; and this metal and heavy glass are the sub-

stances on which this repulsive action of the magnet is most distinctly exerted ; but all bodies in nature which are not magnetic (and these are by far the most numerous) present the same property, although in various degrees. In this way Faraday comes to class all bodies under two heads :—those which are magnetic, or *paramagnetic*, as he calls them, such as iron, nickel, &c. ; and those which are *diamagnetic*, such as bismuth, antimony, heavy glass, &c. The character of the former is to be attracted by the magnet, that of the latter to be repelled by it. It is true that this repulsion, to become sensible, requires an enormous magnetic power, even in the case of bodies of which the diamagnetism is most strongly marked, whilst a very weak magnet is sufficient to betray its action upon the magnetic bodies, such as iron, steel, nickel, &c.

It therefore required very powerful means, such as Faraday employed, for the discovery of diamagnetism. Nevertheless a distinguished amateur in science, M. Lebaillif of Paris, had shown, as early as 1828, that a fragment of bismuth or antimony very evidently repels a delicately suspended magnetized needle when brought as near as possible to one of the poles of the needle but without touching it. Mr. Faraday was ignorant of this circumstance when he published his first work on diamagnetism. I immediately informed him of it, at the same time indicating the journal in which I had published M. Lebaillif's experiment, which I had witnessed at the time. He accepted my reclamation in the most amicable manner, and at once, with his usual good faith, recognized the priority of M. Lebaillif with regard to bismuth and antimony.

In the numerous researches which Faraday devoted (from 1845 to 1855) to diamagnetism and at the same time to magnetism, there are some important points which I must indicate. He discovered the remarkable influence exerted upon this kind of properties by the molecular constitution of bodies, and especially by crystallization. He showed, for example, that a crystallized lamina of bismuth or antimony can place itself axially between the poles of an electromagnet like a magnetic body, as well as equatorially, and that the position which it takes depends on the manner in which it is suspended relatively to the direction of its cleavage. He endeavoured to investigate the force which comes into play in facts of this order, which he names *magnetocrystalline* force ; whilst Plücker, on his part, widened its field by his beautiful and numerous researches on the manners in which crystals place themselves between the poles of an electromagnet ; and Tyndall, the worthy successor of Faraday at the Royal Institution, by his ingenious experiments analyzed the phenomenon in its generality and succeeded in connecting it,

in a perfectly satisfactory manner, with the laws which govern magnetism and diamagnetism. Subsequently Tyndall succeeded also in demonstrating, by a decisive experiment, that diamagnetism, like magnetism, is due to a polarity caused by the influence of the magnet in the diamagnetic body, but with this difference, that, instead of opposite poles, homonymous poles are developed by the poles of the magnet. Thus fell to the ground all the other more or less rash attempts at explanation which had been given of diamagnetism.

Another point which deserves attention is the investigation which Faraday made of the magnetism and diamagnetism of gases. He arrived at this curious result (observed likewise by Edmond Becquerel at the same time), that of all gases oxygen alone is magnetic, and this in a very marked degree, while all the other gases are diamagnetic. Considering the great part taken by oxygen in the composition of our atmosphere, he attempted to explain, by the magnetic properties of this gas combined with variations of temperature, the phenomenon of the diurnal variations of the magnetic needle which he traced over all parts of the surface of the globe. It is impossible for us not to regret a little the considerable time which he devoted to this investigation, especially as it appears to us very probable that it is not in the action of the atmosphere, but much rather in that of the earth itself, or perhaps even in that of the sun, that we must seek the cause of all the phenomena presented by the magnetic needle.

Lastly, a third point remains to be noticed, namely that which relates to the investigation of the magnetic field and of what Faraday denominates the lines of magnetic force. According to him, as we have already had occasion to remark, there is no such thing as action at a distance; consequently the magnetic field (that is to say, the space included between two approximated magnetic poles, such as those of a horseshoe magnet) is a medium from which, in every one of its points, forces emanate, the distribution and direction of which are indicated by the very regular arrangement affected by fine iron-filings placed in this space. The lines which he calls lines of magnetic force thus become visible and even tangible. But they exist none the less even when we cannot see them, and it is the displacements or modifications which they experience by the presence of a ponderable body in the medium in which they occur that give rise to all the remarkable effects of which the magnetic field is the scene. Such is, in a few words, Faraday's view upon this particular question.

We pass in silence over a multitude of interesting details upon diamagnetic polarity, upon the distinction to be set up between

magnetic and diamagnetic bodies, and upon the possible relation between gravity and electricity. In 1850 Faraday reverted to this question, which he had previously attempted but without success. We see that it is with regret that he is obliged to relinquish the discovery of this relation, which he had twice sought after; but with his usual good faith he admits that, although convinced that it exists, he was unable to find any fact to establish it. If experiment, which he knew so well how to employ constantly, gave him a negative response, would not this be because his point of view was not correct? and did not his error arise from his forming too vague ideas as to the transformation of forces, not taking sufficiently into account that it is the work effected by the force, and not the force itself, that must be considered in questions of this kind?

V.

We have passed in review the principal labours of Faraday; and it only remains for us, in order to complete this notice, to endeavour to form an idea of the special character of these labours, and of the influence which they have exerted on the progress of science.

The first character that strikes us is their number. What Faraday published in the form of memoirs from 1820 to 1855 is incredible. And what would it have been if, side by side with the multitude of experiments which he has made known, we placed in a parallel series those which he never published? It is true that if he has left them buried in his journal, it is because they gave him negative results; but from how many fruitless essays and erroneous attempts he would have preserved scientific men if he had not been so discreet!

A second character is the exactitude of the results obtained: I do not think that Faraday has once been caught in a mistake; so precise and conscientious was his mode of experimenting and observing. It must be admitted that in him the hand marvelously seconded the head; he was of remarkable dexterity, and possessed a practical talent, rare and precious in men of science, which enabled him, when necessary, to construct and modify his apparatus for himself, with the view of attaining with more certainty the desired result.

A third character, of quite a different kind and of much greater value, is the originality of the works of Faraday. A disciple of Davy, he undoubtedly shows traces of the school from which he came, especially in the choice of the subjects of which he treats; but he does not blindly follow either the method or the steps of his master, and, soon quitting the beaten track, he strikes out a

path for himself. What is this path? I shall be asked. This is not easy to say; but I will nevertheless attempt it.

At the commencement of the present century, thanks to the important works of which it had been the subject, the science of physics had acquired a character of precision and clearness which seemed almost to make of it a mathematical science. The fine treatise, in four volumes, on *Experimental and Mathematical Physics*, published in 1816 by M. Biot, gives the most correct and complete idea of the point at which this science had arrived. To the confusion which still reigned in the middle of the eighteenth century between the various departments of the science, to the ignorance which then still prevailed upon a great number of these departments, succeeded a clear and substantial analysis of all the phenomena, brought under simple and rigorous laws. Heat, light, electricity, and magnetism were regarded in it as so many distinct agents, having their special properties and obeying their own laws. Calculation was admirably fitted to these clear and precise conceptions; hence we find it greatly used, as witness the very title of M. Biot's treatise.

The great discovery of Ørsted (in 1820), upon the relations existing between electricity and magnetism, began to diminish confidence in this mode of considering the phenomena, a confidence which was already a good deal shaken by the researches of Fresnel and Arago upon light. The breach once opened, the fortress was soon entered; and among the most intrepid assailants Faraday figures in the front rank. By his researches on the condensation of gases, he shows that there is nothing absolute in the laws of Mariotte and Gay-Lussac and in the distinction so generally accepted between vapours and permanent gases. By his investigations upon voltaic electricity, he establishes between chemical affinity and the production of electricity a relation so intimate that it seems as if the one was only a form of the other. By his discovery of induction, he brings in mechanical movement as an important element in the production of electrical phenomena. By his experiments on the influence of the magnet and of electricity on polarized light, and by those which were the consequence of it, he opens to science a new path which no one had foreseen. He succeeds thus in establishing between the natural agents which we name light, heat, electricity, magnetism, chemical affinity, and molecular attraction, such intimate relations, such a connexion, that it is impossible not to think that we shall one day succeed in demonstrating that they are only different forms of the same agent. No doubt he is not the only one that has followed this path. Many others have brought in their contingent to this work of demolition and reconstruction; but he was one of the first, most active, and most persevering. There-

fore his works, I have no doubt, will always be regarded as corner-stones in the new edifice which we are now endeavouring to construct.

I designedly say, *which we are endeavouring to construct*; for we must carefully avoid thinking that it is already constructed. Since the fine discovery of the mechanical equivalent of heat, it seems as if everything had been said and everything were easily explained by means simply of a ponderable matter, an imponderable æther, and a mechanical impulse. Vulgarizers of science, more anxious to produce an effect than to remain faithful to scientific truth, proclaim a molecular system of the world destined to form a pendant to the *Mécanique Céleste* of Laplace. According to them, nothing is more simple, nothing clearer; attraction itself, which has been the object of the study of so many superior minds, is merely the effect of an impulse easy to understand. A dangerous illusion! which, if it succeeded in propagating itself, would be as fatal to the true progress of science as opposed to its useful diffusion; for it is especially upon those who take to themselves the high mission of popularizing science that it is imperiously incumbent to spread none but correct and well-founded ideas.

Let us not, however, exaggerate anything, or refuse to recognize in the too positive ideas which we have just combated that portion of truth which they may contain. With this purpose let us try, in conclusion, to lay down in few words the point at which, in our opinion, in the present state of science the important question of the unity of forces has arrived.

After having for a long time arrested the progress of science by abstract and general considerations upon the phenomena of nature, the philosophers finished by adopting, with Galileo, the experimental method, the only one that can lead with certainty to the discovery of the truth. A rigorous and profound analysis, placed at the service of this method, furnished certain and fundamental results. Reverting to a synthetic phase, many superior minds now seek by means of these tediously and painfully collected materials to reconstruct the edifice of which the raising was formerly attempted in vain. No doubt science has thus entered upon a fertile course, but only on condition of advancing with sure and consequently with slow steps. We speak of the unity of force, and of the transformation of forces one into the other; but do we know what are forces? do we know their nature? We have certainly proved transformations of movement, and shown that one work may change into another work, mechanical motion into heat, and heat into mechanical motion; these are, without doubt, the most important points gained by science, and enable us to get a glimpse of the existence

of a single cause manifesting itself in various forms. But it is a long way from this to the discovery of this cause, this single force. Shall we some day arrive at it? It is possible and even probable; and in this case the name and the works of Faraday will always remain associated with one of the greatest problems which the human mind can entertain.

LV. *On the Spectrum of the Bessemer-flame.*
By W. M. WATTS, D.Sc.*

[With a Plate.]

THE October Number of the Philosophical Magazine contains translations of two papers by Professor Lielegg, giving the results of his observations on the spectrum of the Bessemer-flame. As these results are published as entirely new and no mention is made of any prior observations, it is only right that attention should be called to the fact that as long ago as 1862 the same results had been obtained by Professor Roscoe, and were published in the form of a short preliminary notice in the 'Proceedings' of the Manchester Literary and Philosophical Society for February 24th, 1863. As the note is extremely short, I venture to transcribe it in full.

"Professor Roscoe stated that he had been for some little time, and is still, engaged in an interesting examination of the spectrum produced by the flame evolved in the manufacture of cast steel by the Bessemer process, on the works of Messrs. John Brown and Co., of Sheffield. The spectrum of this highly luminous and peculiar flame exhibits during a certain phase of its existence a complicated but most characteristic series of bright lines and dark absorption-bands. Amongst the former the sodium, lithium, and potassium lines are most conspicuous; but these are accompanied by a number of other, and as yet undetermined, bright lines; whilst among the absorption-bands those formed by sodium-vapour and carbonic oxide can be readily distinguished. Professor Roscoe expressed his belief that this first practical application of the spectrum analysis will prove of the highest importance in the manufacture of cast steel by the Bessemer process, and he hoped on a future occasion to be in a position to bring the subject before the Society in a more extended form than he was at present able to do."

In a lecture delivered before the Royal Institution (May 6, 1864) a year later than the communication quoted above, Dr. Roscoe described the Bessemer spectrum more fully, and pointed out the

* Communicated by the Author.

existence of lines produced by carbon, iron, sodium, lithium, potassium, hydrogen, and nitrogen.

An important practical result of the observations on which these communications were based, was the discovery that the exact point of decarbonization could be determined by means of the spectroscope with much greater exactitude than from the appearance of the flame itself, the change in which indicating the completion of the process is minute and requires a lengthened experience to detect with certainty. This method of determining the point at which it is necessary to stop the blast was indeed at that time (1863) in constant use at Messrs. Brown's works at Sheffield, and has since been introduced with equal success by Mr. Ramsbottom (at the suggestion of Dr. Roscoe) at the London and North-Western Railway Company's steel-works at Crewe.

I was at that time acting as assistant to Professor Roscoe, and in that capacity conducted a lengthened examination of the Bessemer spectrum at the works at Crewe. The results of that investigation were not published at the time, on account of their incompleteness; and I have since then continued in Glasgow the same research, which has now extended itself into an inquiry into the nature of the various spectra produced by the carbon compounds. These experiments are still incomplete; but under the circumstances, of the publication of Professor Liebig's papers, I have put together a few of the more important results obtained in the examination of the Bessemer-spectrum.

The changes which take place in the spectrum from the commencement of the "blow" to its termination are extremely interesting. When the blast is first turned on, nothing is seen but a continuous spectrum. In three or four minutes the sodium-line appears flashing through the spectrum and then becoming continuously visible; and gradually an immense number of lines become visible, some as fine bright lines, others as intensely dark bands; and these increase in intensity until the conclusion of the operation. The cessation of the removal of carbon from the iron is strikingly evidenced by the disappearance of nearly all the dark lines and most of the bright ones.

The spectrum is remarkable from the total absence of lines in the more refrangible portion; it extends scarcely beyond the solar line *b*.

Fig. 2 (Plate III.) represents the *general appearance* of the Bessemer-spectrum towards the close of the blow, drawn according to the plan proposed by Bunsen (Phil. Mag. vol. xxvi. p. 247). It must be remarked, however, that at the period of greatest intensity almost every bright band is seen to be composed of a great number of very fine lines.

The occurrence of *absorption*-lines in the Bessemer-spectrum is in itself extremely probable; and that this is the case appears almost proved by the great intensity of some of the dark lines of the spectrum. It was with this view that the investigation was commenced, with the expectation that the spectrum would prove to be a compound one, in which the lines of iron, carbon, or carbonic oxide &c. would be found, some as bright lines, others reversed as dark absorption-bands. To a certain extent this anticipation has been verified; but the great mass of the lines, including the brightest in the whole spectrum, have not as yet been identified.

In dealing with a complicated spectrum like that of the Bessemer-flame, it is indispensable that the spectrum should be actually compared with each separate spectrum of the elements sought. This was the plan actually pursued; the spectroscope was so arranged that the spectrum of the Bessemer-flame was seen in the upper half of the field of view, and the spectrum with which it was to be compared was seen immediately below. In no other way can any satisfactory conclusion be obtained as to the coincidence or non-coincidence of the lines with those of known spectra.

The spectrum of the Bessemer-flame was thus compared with the following spectra:—

- (1) Spectrum of electric discharge in a carbonic-oxide vacuum.
- (2) Spectrum of strong spark between silver poles in air.
- (3) " " iron "
- (4) " " iron poles in hydrogen.
- (5) Solar spectrum.
- (6) Carbon spectrum—oxyhydrogen blowpipe supplied with olefiant gas and oxygen.

The coincidences observed were, however, but very few, and totally failed to explain the nature of the Bessemer-spectrum. The lines of the well-known carbon-spectrum (given in fig. 1) do not occur at all, either as bright lines or as absorption-bands; nor was any coincidence observed between the lines of the Bessemer-spectrum and those of the carbonic-oxide vacuum-tube.

The lines of lithium, sodium, and potassium are always seen and are unmistakeable.

The three fine bright lines 73·7, 76·8, and 82 are due to *iron*. The red band of hydrogen (C) is seen as a black band, more prominent in wet weather.

After the charge of iron has been blown it is run into the ladle, and a certain quantity of the highly carbonized *spiegel-eisen* is run into it. The effect of the addition of the *spiegel-eisen* is the production of a flame which is larger and stronger when the blow has been carried rather far. This flame occasion-

ally gives the same spectrum as the ordinary Bessemer-flame ; but more commonly a quite different spectrum (fig. 3) is seen, which reminds one at first of the ordinary carbon-spectrum, but differs from it very remarkably.

In the carbon-spectrum, which is drawn (fig. 1), each group of lines has its strongest member on the left (*i. e.* less refrangible), and fades gradually away towards the right hand : in the spectrum of the spiegel-flame the reverse is the case ; each group has its brightest line most refrangible, and fades away into darkness on the least-refracted side. A comparison of the drawing of the spectrum of the spiegel-flame (fig. 3) with that of the Bessemer-flame (fig. 2), will show that they really contain the same lines ; but the general appearance of the spectrum is completely changed by alteration of the relative brightness of the lines. This was shown by direct comparison of the actual spectra.

There can be no doubt that the principal lines of the Bessemer-spectrum are due to carbon in some form or other. My own belief is that they are due to incandescent carbon-vapour. The experiments in which I am at present engaged have already shown the existence of *two* totally different spectra, each capable of considerable modification (consisting in the addition of new lines) corresponding to alterations in the temperature or mode of producing the spectrum, and each due to incandescent carbon. It is possible that the Bessemer-spectrum may prove to be a third spectrum of carbon, produced under different circumstances from those under which the ordinary carbon-spectrum is obtained ; and the intensity of the dark bands is more probably due to contrast with the extreme brilliancy of the bright lines than to their actual formation by absorption.

University Laboratory, Glasgow,
October 25, 1867.

LVI. *Account of a Horizontal Rainbow observed at Sea.*

By Dr. C. COLLINGWOOD, F.L.S. &c.*

[With a Plate.]

ON May 5th of the present year I witnessed a most remarkable phenomenon. I was in lat. $25^{\circ} 19'$ S. and long. $54^{\circ} 13'$ E., the weather very fine and bright, and we were sailing with a light breeze. Observing signs of a squall blowing up from the S.S.E. while I was sitting reading on deck, I got up and went to the port gangway to watch its approach. It was about half-past one P.M., and the sun was therefore in the N.N.W., exactly opposite the approaching squall, upon which was already developed a rainbow of low altitude (12° to 15°). While gazing at it my attention was arrested by a yellowish-

* Communicated by the Author.

brown haze upon the horizon immediately under the centre of the arc, which, although very faint, appeared from its position to have some connexion with the squall or with the rainbow; and I was thus induced to watch it attentively. At first it was a mere indefinite tinge of colour on the distant horizon, and for two or three minutes it seemed to undergo no change; but at length by slow degrees it increased in intensity, and then appeared to spread over the water, looking as though a cloud of reddish dust was hanging over the sea. For some minutes I was quite at a loss how to account for it, but carefully watched to see what would be the upshot. It now became rapidly intensified in brightness, and presently prismatic, and then slowly spread forward across the sea towards us, and at length presented the appearance of a brilliant horizontal bow lying upon the sea, its apex just capping the horizon, and its limbs seeming to fade away upon the water halfway between the eye and the horizon (Plate III. fig. 4). As the horizontal bow increased in intensity the vertical one gradually faded away, and quite vanished immediately after the former had reached its greatest brilliancy, which was most marked about the centre or apex.

In the horizontal bow the red colour was upon the outer or convex side, while in the vertical bow the red was on the inner or concave side. The *horizontal* bow was therefore the *primary* bow, and the vertical bow the secondary or reflected one. Hence we had the remarkable spectacle of a secondary bow appearing before the primary bow was at all developed, and fading in proportion as the latter reached its greatest intensity. The vertical bow, however, was always much less bright than the horizontal bow ultimately became. This latter, when once the prismatic colours became fully developed, seemed rapidly to approach us from the horizon, the limbs appearing to shoot forward, becoming broad, and spreading a wide coloured space upon the blue water on either side; and the bow, when complete, had somewhat of a horseshoe-shape, as though foreshortened. When it had reached its greatest intensity, being then of amazing brilliancy, it suddenly faded and disappeared, and the vertical bow, which had been growing very faint, disappeared at the same time. Throughout the whole duration of the phenomenon the apex of the horizontal bow maintained precisely its original position upon the horizon, namely where I had first been struck by the appearance of the luminous haze; and from the time I first observed this appearance till the time when the whole vanished was about ten minutes. During this time a small drizzling rain fell, which was scarcely sufficient to wet the deck, and the squally effect passed away to the S.W.

LVII. *Note on the Conversion of Integrals.*
*By Chief Justice COCKLE, F.R.S.**

ON reconsidering investigations suspended for some months and examining my paper "On the Conversion of Integrals" printed in the last July Number (Supplement), I perceive that I have improperly left v outside one of the brackets in equation (6) of page 538. The terms

$$-\epsilon v \{ \phi(x, n) - \phi(x, m) \}$$

should be replaced by

$$-\epsilon \{ \psi(x, n) - \psi(x, m) \},$$

where

$$\psi(x, v) = v\phi(x, v). \quad (23)$$

And I further perceive that throughout the numerators of (18) the quantity a can only have the value -1 ; so that (19) degenerates into

$$\frac{v}{x} \frac{d\phi}{dv} + \frac{\phi}{x} - \frac{df_2}{dv} = 0, \quad (24)$$

which is identical and useless for our purpose. It will be necessary to substitute for V its value as given by

$$V = \frac{v^{n-1}}{1+v^n}, \quad (25)$$

and to form the function

$$\frac{d\phi}{dx} + \epsilon v \frac{d\phi}{dv} + \zeta\phi,$$

which when developed will be a rational function of x, v, ϵ , and ζ ; and the possibility of satisfying (3) will depend upon our being able so to assign ϵ and ζ as to render this function a perfect differential coefficient with respect to v . In other words, we must be able to integrate the resulting rational fraction without completely decomposing its denominator or having to solve a certain trinomial equation. The question of the conversion of Boole's integrals is an important one. We know that the definite integrals of the quadratic cubic and quartic Boolean resolvents are equivalent to indefinite integrals, which become in fact algebraical expressions; and any general process by which their con-

* Communicated by the Rev. Professor Harley, F.R.S.

version should be effected would either conduct us to the expression of the roots of algebraic trinomials by means of indefinite integrals, or enable us to see why attempts to obtain such integrals fail when we pass the fourth degree*.

“Oakwal” near Brisbane, Queensland,
Australia, September 19, 1867.

LVIII. *On the Natural Forces that produce the Permanent and Periodic Winds.* By J. KNOX LAUGHTON, B.A., R.N.†

AS we examine further into the ordinarily received explanation of the trade-winds, we meet the distinct enunciation of the hypothesis that when air is expanded by heat, so that its volume is increased and its density diminished, colder and denser air will force itself in, and that with velocity enough to make a sensible current. This hypothesis is so thoroughly in accordance with our knowledge of the properties of fluids, with our daily experience, and with the principles on which coal-mines are successfully ventilated, that we accept it readily, and feel no doubt that colder air will force its way into the place occupied by the warmer, whether with a sensible velocity or not, if some stronger cause does not prevent it. But as a matter of fact, and especially in the phenomenon of the trade-winds, the air does not stream in towards the place of greatest heat. The temperature of the air over the sea on or near the equator, according to very numerous observations, is seldom much in excess of 82° F.; and at sea the decrease as we approach the poles is tolerably regular. On shore the case is widely different. The hottest parts of the world are in extratropical countries, where during the summer season the thermometer rises very far above 82° . During the summer months, the temperature over a large part of Africa to the north of the tropic, according to the observations of Dr. Barth, is seldom less than 90° , whilst it often rises to considerably more than 100° ; 112° is not unfrequent; and other travellers have noted much higher numbers. A similar degree of heat is found in Arabia and Persia. Humboldt gives $93^{\circ}.2$ F. as the

* Meditations of the night upon the developments indicated in the text have led me to the opinion that the definite integrals of the resolvents for the first four degrees inclusive will be convertible by a uniform process, that (unless there be some modification of this process) the conversion of those for the fifth and sixth degrees will depend upon the solution of a linear differential equation of the second order, and that the conversion of those for the seventh, eighth, and other higher degrees will depend upon the solution of linear differential equations of the third and other higher orders.—Brisbane, September 20, 1867.

† Communicated by the Author.

mean for July at Abousbeer, 111° as occasional in the Red Sea. In Central Australia, according to Captain Sturt, the thermometer rose every day to 112° or 116° , and on at least one occasion (on January 21st) to 131° in the shade; and the testimony of other explorers is to the same effect. Compared with such high temperatures, the 82° of the equator sink into insignificance; and it is clear that if the expansion of the air by heat produces a sensible wind, then we must find during the summer months a westerly wind prevalent on the coast of Africa between the latitudes of 20° and 30° N., between which parallels the great Desert is for the most part situated,—as also a south-easterly wind on the southern coast of Arabia, and a north-westerly on the north-west coast of Australia. These winds we do not find. I am at present unable to tabulate the winds on the Australian coast; but I have before me the star-charts published by the Royal Meteorological Institute of Holland, which show for the month of January a marked prevalence of winds from the W.S.W., blowing, that is, nearly parallel to the coast; whilst in February there is as marked a preponderance of winds from the S.W. The following Table (compiled from Maury's Pilot-Charts), in which, for the convenience of comparison, 1000 has been taken as the total number of observations in each case, will throw a clearer light on the winds that prevail on the coasts of the African and Arabian deserts during the months of June, July, and August:—

Latitude. Longitude.	Coast of the Sahara.			Coast of Arabia.			Coast of Guinea.	
	30° – 35° N. 10° – 15° W.	25° – 30° N. 15° – 20° W.	20° – 25° N. 15° – 20° W.	10° – 15° N. 45° – 50° E.	10° – 15° N. 50° – 55° E.	10° – 15° N. 55° – 60° E.	5° – 10° N. 10° – 15° W.	5° – 10° N. 15° – 20° W.
No. of obs.	75	115	48	165	123	65	296	1700
N.	266.7	139.1	270.8	36.4	24.4	4.1
N.N.E.	386.7	295.6	479.2	6.1	8.1	6.8	1.2
N.E.	80.0	208.7	62.5	60.6	24.4	2.4
E.N.E.	40.0	104.3	20.8	6.1	16.3	4.1
E.	26.1	6.1	16.3	4.1
E.S.E.	26.1	24.2	16.3	15.4	13.5	16.5
S.E.	24.2	56.9	6.8	21.2
S.S.E.	52.2	6.1	89.4	131.8	157.6
S.	97.0	97.6	15.4	226.3	294.7
S.S.W.	125.0	60.6	252.0	169.2	418.9	268.8
S.W.	8.7	315.1	284.6	769.2	91.2	86.5
W.S.W.	13.3	109.1	24.4	30.8	67.5	65.9
W.	8.7	66.7	24.4	21.2
W.N.W.	53.3	17.4	36.3	6.8	2.9
N.W.	40.0	34.8	78.8	3.5
N.N.W.	106.7	69.6	20.8	48.5	8.1	5.3
Calm.	13.3	8.7	20.8	18.2	56.9	30.4	40.0

Thus, then, according to the best evidence that can be produced, the theory already laid down is at variance with observation; and we are led to ask why should the Atlantic air from the latitude of 30° N., at a temperature which in summer may be put at 72° or 77°, rush southward to where the thermometer stands at 82°, instead of eastward to where the thermometer ranges about 100°. The aqueous vapour of the oceanic air is not sufficient to account for this, as is shown by the following Table of densities calculated approximately, on the supposition that the dew-point is 5° below the temperature of the air:—

Density of air.		Temperature (Fahr.).
Moist.	Dry.	
	1.0000	32°
.9184	.9259	72
.9086	.9174	77
.8987	.9091	82
	.9009	87
	.8928	92
	.8849	97
	.8772	102
	.8695	107
	.8620	112

The dew-point in the equatorial oceanic district is perhaps seldom so much as 5° below the temperature; Professor Daniell has estimated the difference as generally 2° or 3°; but, on the other hand, the desert air is by no means perfectly dry, so that there is probably very little error in the difference of densities as shown above. Now, independently of the evidence of the Pilot-Charts, no fact in meteorology is better known than the frequency of hot easterly winds on the northern part of the west coast of Africa: these winds, charged with sand from the desert, come off dry and scorching, streaming into a denser air, in apparent contradiction of the hydrostatic law.

Captain Maury considers that the rarefaction of the desert air makes itself manifest in the "African monsoons of the Atlantic;" but these winds (which I have shown in the last two columns of the foregoing Table) are not experienced for some degrees south of the southern border of the Sahara, which scarcely extends beyond the seventeenth parallel of latitude; and we cannot accept the conclusion that the Sahara will draw in the more distant and less dense air of the equator in preference to the neighbouring and denser air between the parallels of 20° and 30° N.

The expansion of the air over the desert of Gobi, and Central Asia generally, is laid down by Captain Maury as entirely, or in very great part, the cause of the monsoons of the Indian Ocean. Mr. Blodgett, in his interesting work 'On the Climatology of the United States,' and almost every other writer on the subject, have expressed the same opinion. But in opposition to this we have the positive assurance of Humboldt, after an almost exhaustive inquiry into the physical geography of Central Asia, that "the lofty chain of the Himalayas is a boundary to the climate of India: the monsoons, and the changes of dry and wet seasons which depend on them, do not extend beyond it." (*L'Asie Centrale*, vol. iii. p. 291.) "Thibet," he continues, "has a climate of its own, about which we as yet know only the extreme dryness, and that enormous difference of temperatures, day and night, summer and winter, which is peculiar to all tablelands where the air has but little density."

This is exactly what we might expect: a range of lofty mountains is a barrier that surface-wind is unable to cross; air cannot bound over peaks of great elevation; nor can the air which at great elevations has become extremely rarefied descend with a steady rush into the denser air of low-lying plains. The mean elevation of the Gobi desert, according to Humboldt, is, at the most, 4000 feet; whilst the central part of it is somewhat under 2500 feet. To whatever extent the air over this desert plain may be rarefied by the heat of summer, the diminution of its density can certainly never approach to an equality with that due to a height of from 20,000 to 30,000 feet.

In opposition to the views held by Captain Maury, Professor Dove considers that the monsoons are caused by the difference of the elastic force of the aqueous vapour in Siberia and India. (*Gesetz der Stürme*, p. 50, 3rd edit.) It is difficult to understand the force of his argument; for according to his own Tables the barometric pressure (that is, the entire elastic force of the atmosphere) is very nearly the same at the two places which he names in illustration of his meaning; these Tables, when reduced to English inches, give:—

	Mean for the year.	Variation for July.	Variation for January.
Calcutta.....	29·636	·303—	·387+
Barnaul.....	29·329	·354—	·301+

Elastic force will produce the same pressure, and will affect equilibrium in the same manner, whether it is the elasticity of dry air or of aqueous vapour; and, volume for volume, under equal temperatures, dry air is heavier than moist. It is impossible to

believe that such trifling variations as these, at such a distance apart, and on opposite sides of the Himalayas and the high tableland of Thibet, could produce such very marked results as the north-east and south-west monsoons, even if we were to set on one side the utter want of dependence that can be placed on comparative barometric observations so taken (*L'Asie Centrale*, vol. i. p. 80), and even if Humboldt's evidence as to fact was not conclusive. But since the Himalayas form an actual boundary to the monsoon region, the cause of the monsoons is clearly not to be sought either on the Gobi or at Barnaul.

If now we for a few minutes put on one side the theory that these constant winds, whether trade-winds or monsoons, are due to the expansion caused by heat, we cannot but be struck with a certain remarkable relation between the points to which they tend; these points are all distinguished by their excessive rainfall. The trade-winds in both oceans tend to the equator: the region of equatorial calms has received from all physical geographers the name of the *zone of constant precipitation*: Maury speaks of the rain as "so copious that the fresh water is sometimes found standing in pools on the sea." The south-east trade blows home on the coast of South America with great force as a wind due east; and the volume of water discharged into the sea by the Amazon and the Orinoco is a convincing proof of the enormous quantity which falls on the area of their drainage; whilst the succession of rainy seasons at different distances inland seems to bear on the changes of wind in the interior in a very extraordinary manner. Bates says that at Santarem "from August to February very little rain falls, and the sky is cloudless for weeks together, the fresh breezes from the sea, nearly 400 miles distant, moderating the great heat of the sun. The wind is sometimes so strong for days together, that it is difficult to make way against it in walking along the streets." (*Naturalist on the Amazons*, vol. ii. p. 13.) At Ega, on the contrary, "from the middle of October to the beginning of January the second wet season prevails," and "the second dry season comes on in January, and lasts throughout February." (*Ibid.* p. 223.) During this dry season there is a change in the state of things at Santarem: "the fine weather breaks up, often with great suddenness, about the beginning of February. Violent squalls from the west, or the opposite direction to the trade-wind, then occur. . . . They are accompanied with terrific electric explosions, the sharp claps of thunder falling almost simultaneously with the blinding flashes of lightning. Torrents of rain follow the first outbreak; the wind then gradually abates, and the rain subsides into a steady drizzle." (*Ibid.* p. 25.) The heaviest rains of the year fall (at Santarem) in April, May, and June;

at Ega in the same months; and the river reaches its highest level about the first week in June, the waters being then about 45 feet above their lowest level. Higher up still, the air would seem to attain a state of saturation unknown in any other country. "It was impossible (Bates says) at S. Paulo to keep salt for many days in a solid state. . . . Six degrees further westward, namely at the foot of the Andes, the dampness of the climate of the Amazonian forest region appears to reach its acme; for Poeppig found at Chincao that the most refined sugar in a few days dissolved into syrup."

On the other hand, on the east side of the Atlantic both northerly and southerly trades are most clearly drawn into Equatorial Africa, a country whose humid climate has proved so fatal to many of our countrymen; and in India we find the south-west monsoon pointing, in the same way, to the violent and heavy rains of the wet season.

The following Table of the rainfall at a few places in the localities I have named will not be uninteresting, as showing in figures the excessive nature of the precipitation to which I refer:—

	Yearly fall, in inches.	Remarks.
Doldrums of the Atlantic.....	224·6	Estimated by Maury.
Paramaribo (Guiana)	229·2	
San Luis de Maranhao	280	
Sierra Leone	189·6	
Mean of peninsula of Hindostan	117·5	Keith Johnstone.
Mean of North-east Provinces...	242·1	"
Mean of West Ghauts	175·2	"
Cherrapongi (N.E.)	610·3	
Bombay	110·3	Dr. Ives.
Mahabuleshwar	254	
Uttray Mullay	263	
Anjarakandy	116·3	Kaemtz.

Such figures tell their own tale: the elastic force of the aqueous vapour in the tropical atmosphere may be stated, in round numbers, as equivalent to 1 inch of mercury; and the sudden removal by precipitation of a very large proportion of this weight necessarily disturbs equilibrium. Where such a lessening of the weight of the atmospheric column takes place, air from the heavier columns of the neighbourhood must stream in, and that with a force bearing some proportion to the suddenness of the cause; so that when this cause is both sudden in point of time and comparatively confined in point of space, the effect becomes very clearly marked.

Mr. Thomas Hopkins some years ago, in an Essay on "The

Atmospheric Changes which produce Wind and Rain," called attention to the great effect which condensation must produce; but he attributes that effect to the further expansion of the air by the latent heat made manifest, rather than to the actual loss of weight, or elastic force, in the atmospheric column. This view has also been partially adopted in the later editions of the 'Physical Geography of the Sea,' by Captain Maury, who considers that the latent heat developed by condensation may give a further expansion to the tropical air. It seems to me, on the contrary, more probable that the effect is produced by the direct statical agency of diminished pressure, and that the heat evolved is simply thrown off into space through the very thin and clear medium of the upper atmosphere.

Whether this is the case, or not, it is difficult (perhaps impossible) to decide; but I submit that I have shown strong grounds for believing that, in whatever way it acts, sudden condensation of aqueous vapour is the principal cause of the trade-winds, of their inflection towards the west coast of Africa, and of the Indian monsoons, in opposition to the theory which would derive these winds from the mere expansion of the intertropical air by heat.

LIX. *On certain Hypothetical Elements in the Theory of Gravitation and generally received conceptions regarding the Constitution of Matter.* By JAMES CROLL*.

Gravitation.

NO future researches or discoveries in physical science will ever overturn Newton's grand theory of universal gravitation, or ever in the least degree shake universal confidence in its truth. It will stand as the immoveable foundation upon which the whole superstructure of physical science will for ever rest. Although the truth of everything that is really essential to the theory is established beyond the possibility of a doubt, yet there are certain hypothetical elements which have been unnecessarily associated with it, or rather included in it, which by no means can lay claim to be considered as established. I shall briefly refer to a few of these elements.

Gravity is commonly defined to be an "attractive force between the particles of matter varying inversely as the square of the distance." Or, as stated more fully, "every particle of matter in the universe attracts every other particle with a force varying inversely as the square of their mutual distances, and directly as the mass of the attracting particles." It will be seen at once

* Communicated by the Author.

that this definition contains something more than a mere statement of the facts determined by observation. It contains a hypothetical explanation of the facts.

Let A and B be two particles of matter. We know experimentally that they tend towards each other with a force inversely as the square of their mutual distance; but the ordinary definition of gravity goes further than this. It not only asserts that they tend towards each other, but it asserts that this force or tendency arises from A attracting B, and B attracting A. It asserts that B moves towards A *because* B is attracted by it.

It was demonstrated by Newton, and has been proved by general observation and experience, that bodies tend towards each other with a force varying inversely as the square of the distance, and directly as the mass of the bodies. But it never was demonstrated or proved by any one that the bodies *attract* each other. The thing which has been demonstrated is that B tends towards A: but the theory does not rest here; it goes on to account for this tendency by referring it to a hypothetical cause, viz. to the "attraction" of A. This, however, is a mere hypothesis and no way essential to the theory. All that the theory requires is that it be demonstrated that A tends to move towards B. It is not necessary that we should go beyond this, and attempt to explain the cause of this tendency.

Trifling as this assumption, included in the theory, may at first sight appear to be, it will be found that almost all the difficulties and objections which have been urged against the theory of gravitation are due, in some form or other, to that assumption. At the very outset we have the objection urged against the theory that it implies the absurdity of action at a distance. Now the mere facts of gravitation imply no such thing. That A and B placed at a distance should tend towards each other does not imply any action at a distance. A moves by virtue of a force, but it does not follow that this force is at a distance from A. But if we assert that A and B "attract" each other, then we imply action at a distance; for A is then affirmed to move in consequence of the force of B, and B in consequence of the force of A. "The very idea of attractive force," as Professor Brücke remarks, "includes that of an action at a distance."

No principle will ever be generally received that stands in opposition to the old adage, "a thing cannot act where it is not," any more than it would were it to stand in opposition to that other adage, "a thing cannot act before it is, or when it is not"*.

* For an account of the metaphysical origin of these adages, see a work by the author, 'Philosophy of Theism,' p. 112. Walford, Jackson, and Hodder. London, 1857.

pothetical part of the theory with the adage in question that led Sir Isaac Newton to suggest that gravity might be transmitted by means of an æthereal medium existing in space.

Another objection is this: if another particle C be placed beside B, it will be found that A will attract C with as much force as it does B, and yet continue to attract B the same as though C had not been added; and if we add another particle D, this particle will also be attracted with equal force, the other two remaining as strongly attracted by A as though D had never appeared. We might in like manner go on adding particle to particle to infinity, and still A would continue to attract each new particle as it appeared with as much force as though no other particle were in existence. In fact there is no limit to this attracting power of A. This is contrary to what we know of the character of force in every other department of nature.

Another objection also follows; when we add C to B and thus double the attraction, A doubles its force also and attracts them with as much force as they attract it. If D be added, the attraction is tripled, but A triples its force also; and we might proceed in this manner adding particle to particle until we had added to B every particle in the universe, and yet, strange to say, the single particle A would attract the entire universe with as much force as the universe attracted it.

The attraction theory is also in opposition to the principle of the Conservation of Force, as has been shown, I think, clearly by Faraday*. When a stone, for example, is thrown upwards from the earth, it not only loses all its motion, but it loses its attraction in proportion to the square of its distance from the centre of the earth. What becomes of the motion imparted to the stone? It is not transformed into attraction, for the attraction diminishes as well as the motion. When the stone again falls to the earth, it gains both motion and attraction. In the former case the attraction is said to consume the motion, and, instead of becoming stronger, becomes weaker in consequence; and in the latter case it imparts this same motion, and yet, after imparting the motion, it is actually found not only not to have lost, but to have gained force thereby. Faraday justly asks what becomes of the force or motion imparted to the stone? It is not converted into attraction, for the attraction becomes less instead of greater in consequence. And in the case of the falling stone, where does the motion come from? If the motion arises from the attraction of the earth, then there must be a certain amount of this attractive force converted into motion; and if so, the attractive force should be so far reduced; but instead of this, it is actually increased. There is, therefore, no account given

* *Phil. Mag.* for April 1857.

of what becomes of the motion externally imparted to the stone when thrown upwards, or where the increase both of attraction and motion is derived as it descends. If the attraction theory be correct, then there is a destruction of force in the one case, and a creation of force in the other; and if so, then the principle of conservation of force is violated.

Professor Brücke tries to answer Faraday's objections in the following manner:—"Let the mass A," he says, "be separated from the mass B by an external force; while this separation takes place the attraction diminishes, the attractive forces being in the inverse ratio of the squares of the distances. Where abides the force which is here destroyed? The reply is: If the mass A be left to itself, it moves back towards B, and when it has arrived at its original position it will be attracted by B with the same force as before; besides this, it has attained a velocity, half the square of which, multiplied by the mass of A, is exactly equal to the work which was formerly expended in removing it from B. There is therefore no force destroyed by the change which the external cause has wrought; but just as much force appears at the end as was expended in producing the change"*.

It will be easily perceived that this never touches the objection. Faraday will admit that when A *has returned to its original position*, it will have received back all the force that was lost. When A is removed from B by an external force, the motion disappears without producing any apparent effect; it does not appear under any other form of force; and when A approaches B, motion is produced without the expenditure of any force. There is therefore an unaccountable loss of force in the one case, and as unaccountable a gain of force in the other case. Consequently when A reaches B it will be in the same state as when it left; for the loss in the one case is compensated by the gain in the other. But this never explains what became of the force which was lost in the first case, or whence was derived the force which was gained in the latter case. If a man who had lost in some unaccountable manner £10 the one day and gained in an equally unaccountable way £10 the next day, were to inquire what became of the money lost on the first day, and whence came the money gained on the second, it would be no answer whatever to tell such an individual that he was just as rich at the end of the second day as he was at the beginning of the first. It would of course be a somewhat satisfactory answer to be told that the money gained the second day was what had been lost the first; and this no doubt is the idea Professor Brücke wishes to convey. When the stone is thrown upwards, he supposes that the motion or energy imparted becomes stored up in the

* Phil. Mag. February 1858.

stone under a new form, and when it returns to the earth the energy thus stored up is given back in the form of motion as before. To express the matter in more precise terms—when the stone is thrown up, the *vis viva* becomes less and less, and when it reaches the turning-point it is zero. But at this point the whole energy as a cause of motion is stored up, the *vis viva* is transformed into tension, *actual* or *kinetic* energy into *potential* energy.

That this is not a satisfactory explanation is evident. For when the stone is thrown upwards, the motion imparted gradually disappears. It is not converted into attraction, for the attraction, instead of being augmented by the loss of motion, is found to decrease also. Hence experience shows that, according to this theory, both the kinetic and the potential energy decrease as the stone rises.

No truth in physical science is now better established than that force is indestructible. If it ceases to exist under one form, it is because it has assumed some other form. Any conclusion which stands in opposition to this principle must be abandoned, whatever may be its claims for reception on other grounds. When, for example, we observe the loaded piston rising under the pressure of the steam, we at once conclude that the energy being manifested as mechanical work existed the instant before under the form of heat, and that the heat in turn existed previously as chemical affinity in the coal and the oxygen of the air. And again, the potential energy of the coal existed in some former age as sun-rays.

When we observe mechanical work performed by heat, or by electricity, or by magnetism, &c., we at once infer that there *must* have been a consumption of these forces corresponding to the amount of work performed; but, strange to say, although we are continually witnessing the mechanical effects produced by gravity, yet we are most reluctant to admit that the mechanical force manifested previously existed as gravity. When a stone, for example, falls to the ground, and by the concussion generates, say, 100 foot-pounds of energy in the form of heat, it is at once admitted that the 100 foot-pounds of energy appearing as heat was derived from the mutual attraction of the earth and stone. But how few will admit that there has been a consumption of gravity; and yet it is self-evident that if the total amount of the earth's gravity be as great after the stone has reached the ground as *before it commenced* its descent, then there must have been a creation of power. The principle of conservation necessitates us to conclude that when heat, *vis viva*, or work of any kind is produced by gravity, what we gain of actual energy in the form of *vis viva* &c. we must lose of potential energy in the form of gra-

vity. No one would for a moment think of denying the correctness of this mode of reasoning in regard to heat, magnetism, electricity, or any other form of force. Why, then, make an exception in the case of gravity? But more than this, the very idea of an exception is in itself absurd; it is nothing less than to make an exception in regard to a principle which we admit holds universally true.

To reconcile the common conceptions of gravity with that of conservation, it has been said that the potential energy of gravity does not simply consist in the tendency which bodies have of approaching to each other, but consists also in the distance through which that tendency is capable of continuing to act. For instance, when two bodies approach each other under the mutual influence of their gravity to one-half their former distance, their potential energies are diminished also to one-half (although their tendency to approach is not diminished), because the distance through which that tendency is now capable of acting is but one-half of what it was formerly.

The energy of a raised weight, for example, it is said is the product of the gravitation-pull upon it, and the distance through which this pull can act.

$$\left. \begin{array}{l} \text{Energy, or work performed} \\ \text{by descending weight} \end{array} \right\} = \left\{ \begin{array}{l} \text{Force, or pull of gravitation} \\ \text{upon the weight} \times \text{distance} \\ \text{passed over by descending} \\ \text{weight.} \end{array} \right.$$

Or,

$$\text{Energy} = \text{a force} \times \text{a length.}$$

It is certainly true that the amount of energy or work performed is proportionate to the pull of gravitation \times the distance through which the weight descends. But I am unable to perceive how this can meet Faraday's objection; for it seems perfectly evident that the mere change of relative position cannot constitute any form of force. Distance is a necessary *condition* to the transformation of the potential energy of gravity into the actual energy of *vis viva*, or of heat, &c.; but distance itself does not in any degree constitute this transformation. When bodies arrive at contact, there can be then no further transformation of potential energy into kinetic; not because the potential energy has been all consumed, but because the bodies are not in a *condition* to allow of any further transference. The *tendency* to approach, though increased to infinity, would not help in the least degree to produce any further transformation of potential energy into kinetic; for the thing wanted is not more potential force, but the necessary condition to transference. That which causes bodies mutually to approach with velocity and thus produce

actual energy is not the fact that they are *separated*, but the fact that they *tend* toward each other. Distance is a necessary condition to the action of this tendency, and, of course, the greater the distance the greater is the opportunity for acting; but when kinetic energy is produced in the form of *vis viva* or heat, &c., by the approach of bodies, the equivalent in the shape of potential energy lost is *tendency*, or *gravity*, not *distance*. The foot-pounds of kinetic energy produced existed previously in the statical condition of a *tendency* to approach, not in a mere *relation of coexistence in space*.

Let us take the case of the steam-engine. We have here also two elements, the potential and the kinetic. We have (1) the potential element consisting in the dead pressure of the steam in the interior of the cylinder, (2) the *vis viva* and mechanical work produced as the piston rises under the pressure. This is simply a transference of force from the one condition to the other. What we gain in *vis viva* and mechanical work we lose in pressure. But space is a *condition* as necessary to the transference of pressure into *vis viva* as of gravity into *vis viva*. No matter what the pressure may be, if the piston is at the end of its stroke, and has no further space through which to move, no transference can possibly take place. The equivalent gained in *vis viva* and mechanical work is wholly derived from the pressure of the steam, not from the space. Space is simply a *condition* in the transference. The matter is precisely the same in the case of *vis viva* generated and work performed by gravity. The actual energy of the falling stone must be entirely at the expense of the dead pull of gravity, the space being simply a condition in the transference. The same reasoning is equally applicable to the conversion of statical electricity into dynamical, or of magnetic force into mechanical work. Unless a path is opened up between the ends of the battery through which the forces may travel, no transference of statical into dynamical electricity can possibly take place. Unless the magnetic engine is allowed to move, the magnet does not lose any of its potential energy. In fact space is a necessary condition in the transformation of force under all circumstances. It seems to be metaphysically absurd to suppose that either *space* or *time* can be in the operations of nature anything more or less than simple *conditions*.

The work performed by a water-wheel, for example, is as really and truly derived from the pull of gravity as the work performed by the rising piston is from the pressure of the steam. And it is just as absurd to assert that the pull of gravity is not diminished by the motion of the wheel, as to assert that the pressure of the steam is not diminished by the rising of the piston.

It is, of course, perfectly true, as has been stated, that the work performed by gravity is in proportion to the pull of gravitation \times the distance through which the pull can act. And the work performed by the piston is in proportion to the pressure of the steam \times the length of stroke. But then, if space be nothing more than a condition in the operation, the energy must be derived from the pressure, not from the space. The gain of energy, or the work, would be in this case exactly equal to the loss of pressure or force, the space being simply the condition which allows the change from force to energy to take place.

Time and space are necessary conditions in all phenomena, whether of mind or matter, but they are mere conditions. We believe, in opposition to Kant, in the objective reality of time and space; but still, though space is an objective reality, a thing in itself, it can no more be converted into a force or an energy than it can be converted into a stone. The one supposition appears just as extravagant and absurd as the other. It is just as violent an assumption to suppose that time could be converted into *energy*, become an efficient cause in the performance of work, as that space could be so converted. Space has been eternally space, and can *absolutely* be nothing else.

But supposing that space could be something more than a mere condition in the transference of the force of gravitation into mechanical energy, still this would not reconcile the ordinary theory with the principle of conservation.

In the case of the loaded piston rising under the pressure of the steam, we have the pressure of the steam and length of space both diminishing as the *vis viva* or mechanical work increases. This is in harmony with the principle of conservation, for pressure or force diminishes as energy or work increases. But in the case of gravitation matters are reversed; for the force increases along with the work. As the weight descends and performs work, the pressure of the weight, the thing which performs the work, increases also. And when the weight is rising and energy diminishing, the force or pressure of the weight is not increasing but actually diminishing also.

This difficulty, along with all the others which we have been considering, will entirely vanish if we adopt the view of gravity which has been ably advocated by Faraday*, Waterston†, and other physicists, viz. that it is a force pervading space external to bodies, and that on their mutual approach this force is not increased as is generally supposed, the bodies merely pass into a place where the force exists with greater intensity; for in

* Phil. Mag. April 1857. Proceedings of the Royal Institution for 1855.

† Phil. Mag. S. 4. vol. xv.

such a case the intensity of the force, in the space external to any body, is inversely as the square of the distance from the centre of convergence of these lines of force. As the stone recedes from the earth, its *vis viva* is transferred to space and exists there as gravity. When the stone approaches to the earth, the force existing in space is transferred back to the body and appears again as *vis viva*.

"The integral gravitation," says Mr. Waterston, "is a function of space. . . . Each element of radial distance has associated with it a fixed element of mechanical force, to be given or taken from all bodies traversing it."

Matter.

Commonly associated with the hypothesis that the atoms of matter attract each other at a distance is another hypothesis, in regard to the physical nature of the atoms themselves.

The common conception of matter, which however is now beginning to be abandoned by many of our leading physicists and chemists, is that all matter consists of atoms essentially solid, indivisible, impenetrable, and infinitely hard. The conception is, that matter is solidity occupying space. It is almost needless to say that this conception is wholly hypothetical. It is not a simple representation of our experience of matter, but rather a hypothetical attempt at an explanation of the cause of that experience. What we experience is resistance. Matter offers resistance to the touch, and we say that it is hard. An atom or particle of matter will maintain length, breadth, and thickness against any pressure, however great, applied to deprive it of volume. Nothing can deprive the atom of the possession of a certain amount of space; hence it is asserted that it must be infinitely hard—in short, a part of space filled absolutely solid. This hypothesis also assumes the resistance thus offered by the atom to be purely static or passive resistance.

That this hypothesis is not necessary to account for our experience of matter will appear obvious from the following considerations:—Were a cubic inch of space to become, by some means or other, impenetrable (that is, were it to resist the approach of all bodies into it), even although it were *completely void*, this cubic inch of empty space would appear to the senses in every respect to be solid. And were a cube of what is considered solid matter of the same size placed beside it, we could not by any known means determine which of the two was the solid one.

All that is necessarily implied in matter, so far as what is called hardness or solidity is concerned, is that it is either a *power* of resistance in space, or a *substance* which manifests resistance as a property. If we consider this resistance to be an *effect*,

and not a property or quality, the most philosophic way is to say, with Faraday *, that the atom is simply a centre of force, and what we call matter is simply a power of resistance acting in a certain part of space, thus making no hypothetical statement of any kind regarding the nature of this cause or power.

But the hardness or resistance manifested to our experience is considered by those who adopt the ordinary theory to be a property or quality of a substance, not the effect of a cause. But this does not afford any warrant for assuming the existence of solid impenetrable atoms. It will not do to say that there can be no resistance without solidity. All that we require to affirm is that there must be a something possessed of the property or quality of resistance—a something which manifests itself as resistance in space. What we must believe is that there exists a substance or subject to which the resistance belongs.

The necessity for assuming the existence of a something to which these properties belong is purely metaphysical. The metaphysical necessity under which we lie obliges us to postulate the *existence* of a something; but it does not necessitate us to form any conceptions regarding the nature of this something. Its nature can only be learned by experience, through the properties manifested. If we experience resistance in space, then metaphysically we must assume the existence of a something which resists. This is all. We are not warranted from this property manifested to begin and speculate on the nature of this something. If it should manifest other properties than resistance, these other properties will give us further information regarding its nature. But if it does not manifest any other property than simple resistance, all that we can ever possibly say is that a something resists, but what this something actually is, further than a power of resistance, must in such a case remain for ever unknown. Some even believe that if you deprive matter of that imaginary quality called solidity you annihilate it altogether. Of course, if solidity be a property of matter, and you annihilate the solidity, you annihilate matter as a something existing *as a solid*. But this is not exactly what those to whom I refer mean. They mean that actual *existence* depends upon solidity, and that there can be no existent something manifesting itself in space as resistance unless it be in possession of this solidity.

It has been asserted that the idea of *vis inertiae* is irreconcilable with the hypothesis that matter consists of centres of force. It is certainly true that, whatever views we may adopt regarding the physical constitution of matter, *vis inertiae*, under some form

* Phil. Mag. for February 1844, and May 1846.

or other, must enter as an element into our theory. And it is also true that inertia, in the exact sense as understood by Newton, is not in all respects applicable to the theory of atoms being centres of force. But if we are allowed to differ from the ordinary views in regard to the constitution of matter, we are equally at liberty to differ in regard to our views of the nature of *vis inertiae*.

When a body is in motion its moving force is in proportion to the square of its velocity. The question arises, why is the motion of the body an energy? How is it that force can be stored up in the body under the form of motion? The answer, according to the ordinary view, is, because the body possesses *vis inertiae*. But this is simply saying in other words, an inert body in motion is a force or energy. According to the dynamical view the answer is equally the same; centres of force in motion are energies. In this respect *vis inertiae* must be regarded as a quality of matter, whatever our views may be. *A priori* it is just as natural to suppose that the motion of the one should be an energy as the motion of the other. A body in motion is a force or energy; but we are in profound ignorance of the reason why it is so. It is no answer to say that a body in motion is a force, because it possesses *vis inertiae*. This is merely asserting the fact, not giving the reason. We know from experience that a body possesses some unknown quality, by virtue of which it is, when in motion, an energy or force. Newton calls this quality *vis inertiae*, because, according to his idea of matter, a body is inert, being altogether destitute of active qualities. The advocate of the dynamical views, who does not regard matter as wholly inert, may, if he chooses, in conformity to common usages, designate this unknown quality by the term *vis inertiae*. The quality must have some name, and perhaps it is as well to abide by the old one. But if we imagine that when we assert that a body in motion is an energy because it possesses *vis inertiae* we convey to the mind some idea how it happens that a moving body is such, we certainly deceive ourselves.

Our knowledge of *vis inertiae* is exclusively derived from experience. No one could predict *à priori* that matter possesses *inertia*. The advocate of the old theory has therefore no warrant whatever to assert *à priori* that a centre of force in motion is not a force or energy by virtue of the motion. And if he has no warrant *à priori*, he has as little *à posteriori*; for how can he who maintains that *all* matter is essentially solid, prove experimentally that matter constituted of forces has no *vis inertiae*?

Although we are unable in the present state of our knowledge to explain fully how it happens that when, for example,

an atom A strikes a similar atom B at rest, it communicates to B its moving force, yet we believe that the dynamical theory will be found to penetrate deeper into the question than the old theory of inert solidity, as the following considerations may perhaps show.

If an atom A, perfectly elastic, moving with any given velocity, strikes a similar atom B at rest, it transfers its entire motion or force to B, and remains at rest itself. But no transference could possibly take place unless B offered resistance to A. Upon what principle, then, does B offer resistance to the advance of A? According to the ordinary view B is an inert solid, void of all power to offer any active resistance; but yet it does, nevertheless, offer resistance. According to the dynamical view, B is a point offering active resistance to every body which approaches within its sphere. When A reaches the place where resistance or repulsion commences, viz. the surface of B, then A meets with resistance as it advances into B and loses motion in consequence. But the motion thus lost by A is communicated to B. This transference goes on till both atoms have the same velocity. At this moment A has transferred to B the half of its moving force. But this condition of things cannot remain, for A has passed within the repulsive sphere of B (the sphere of its activity) and B within the repulsive sphere of A, and the consequence is, the two atoms must mutually repel each other; A will therefore still continue to push B forward. B, on the other hand, will continue to push A backwards until A is brought to rest; after this B will separate from A; but by this time B's velocity will be equal to that which A originally possessed.

Elasticity on the dynamical theory follows as a necessary consequence. But on the ordinary theory it is wholly inconceivable, if it be not contradictory. When A has communicated to B the half of its moving force, and the two atoms are moving forward with equal velocity, how, upon the ordinary theory, do they not continue to move side by side with equal velocity? How can B, an inert solid block, by means of *inertia*, without the exertion of any activity, begin now to act upon A so as to push it backwards and stop its motion?

LX. *Thoughts on Inverse Orthogonal Matrices, simultaneous Sign-successions, and Tessellated Pavements in two or more colours, with applications to Newton's Rule, Ornamental Tile-work, and the Theory of Numbers.* By J. J. SYLVESTER*.

PART I.—*Matrices and Sign-successions.*

1. A SELF-RECIPROCAL matrix may be defined as a square array of elements of which each is proportional to its first minor. When the condition is superadded that the sum of the squares of the terms in each row or in each column, or (which comes to the same) that the complete determinant shall be equal to unity, it becomes *strictly* orthogonal; but, by an allowable extension of language, any self-reciprocal matrix may be termed orthogonal when the epithet of *strictness* is withdrawn. The general notion is that of homographic relation between each element and its first minor, *i. e.* the relation $a + bx + c\xi + dx\xi = 0$ between the corresponding terms x and ξ of the matrix and its reciprocal. When $a=0$ and $d=0$, we have the case of orthogonality as above defined †. When $b=0$ and $c=0$, so that each term in either matrix is in the inverse ratio of its first minor, we fall upon what I call the case of inverse orthogonality.

This conception will be found to present itself naturally in the course of certain investigations connected with the calculus of sign-progressions suggested by the form of Newton's rule; and that calculus in its turn leads to a theory of tessellation highly curious in itself, and fruitful of consequences to the calculus of operations and the theory of numbers, furnishing interesting food for thought, or a substitute for the want of it, alike to the analyst at his desk and the fine lady in her boudoir.

2. In a strictly orthogonal matrix the $n^2 - 1$ equations resulting from the equal ratios above referred to, on account of the implications existing between them, really amount to no more than $\frac{n^2 + n}{2}$ independent conditions, leaving $\frac{n^2 - n}{2}$ of the n^2 terms arbitrary.

This law, which it would perhaps not be easy to obtain from a direct inspection of the equations, is an instantaneous consequence of the fact that a sum of the squares of n variables may be transformed into a sum of squares of n linear functions of the same by means of an orthogonal substitution,—and that, *vice versa*, such faculty of transformation is sufficient to establish the cha-

* Communicated by the Author.

† For a matrix of the order 2 the ratio of each element to its reciprocal in an orthogonal matrix is necessarily ± 1 . This is a case of exception, and may be disregarded. In all other cases the ratio can be varied *ad libitum*.

racter of orthogonalism in the matrix of substitution employed. Consequently the number of conditions to be satisfied is the number of terms in a homogeneous quadratic function of n variables, which is $\frac{n \cdot (n+1)}{2}$. In an *orthogonal* matrix (not *strictly* so) the number of implications is consequently

$$\frac{(n+2)(n-1)}{2}.$$

3. The problem of constructing an inverse orthogonal matrix of any order admits of a general and complete solution. It is to be understood in what follows, that the constant product of any term by its first minor is not to be zero; or, in other terms, the complete determinant of the matrix which is a sum of such products is not to vanish.

First, let us investigate the number of arbitrary elements which enter into any such matrix.

To fix the ideas, consider one of the third order, say

$$\begin{vmatrix} a & b & c \\ \alpha & \beta & \gamma \\ A & B & C \end{vmatrix}$$

and call the reciprocal matrix formed by its first minors

$$\begin{vmatrix} a_1 & b_1 & c_1 \\ \alpha_1 & \beta_1 & \gamma_1 \\ A_1 & B_1 & C_1 \end{vmatrix}$$

Then

$$\begin{aligned} aa_1 &= bb_1 = cc_1 \\ &= \alpha\alpha_1 = \beta\beta_1 = \gamma\gamma_1 \\ &= AA_1 = BB_1 = CC_1. \end{aligned}$$

These 8 equations are not independent; for we have

$$\begin{aligned} aa_1 + bb_1 + cc_1 &= aa_1 + \alpha\alpha_1 + AA_1 \\ &= \alpha\alpha_1 + \beta\beta_1 + \gamma\gamma_1 = bb_1 + \beta\beta_1 + BB_1 \\ &= AA_1 + BB_1 + CC_1 = cc_1 + \gamma\gamma_1 + CC_1; \end{aligned}$$

which 5 equations in their turn again are not independent, because the sum of the three groups written under one another on the left is equal to the corresponding sum on the right.

Hence we have implication upon implication, so that the number of independent equations is

$$(3^2 - 1) - (2 \cdot 3 - 1) + 1 = (3 - 1)^2;$$

and so in general for a matrix of the order n , the number of independent equations is $(n-1)^2$, leaving $2n-1$ of the elements arbitrary.

4. This result is easily verified. For, reverting to the example of the third order, if any inverse orthogonal matrix of that order is multiplied, term to term, by the following one,

$$\begin{array}{ccc} l\lambda, & l\mu, & l\nu, \\ m\lambda, & m\mu, & m\nu, \\ n\lambda, & n\mu, & n\nu, \end{array}$$

the product so formed will evidently retain its character unaltered, since each of the equal products will receive a constant multiplier, $lmn \cdot \lambda\mu\nu$.

The number of independent quantities thus introduced is 5, viz.

$$l\lambda; \quad \frac{m}{l}, \frac{n}{l}; \quad \frac{\mu}{\lambda}, \frac{\nu}{\lambda};$$

and so in the general case we can introduce $(2n-1)$ arbitrary elements. Thus, then, we may without any loss of generality regard only those matrices of the kind in question which are bordered horizontally and vertically by a line of positive units. From these *reduced* forms it is easy to pass to the general forms by term-to-term multiplication with a matrix of the kind above denoted. The question now becomes narrowed to that of determining the number and form of the reduced inverse orthogonal matrices of any given order n ,—a problem (if attacked by a direct method) involving the solution of $(n-1)^2$ equations between $(n-1)^2$ unknown quantities.

5. (1) Let n be a prime number. Write down the line of terms

$$1, \quad a, \quad a^2, \dots, a^{n-1},$$

and make a equal in succession to each of the $(n-1)$ roots of $\frac{x^n-1}{x-1} = 0$. The matrix so formed will be a reduced inverse orthogonal matrix of the n th order.

In the case of $n=3$, it is easy and will be instructive to verify this statement. Calling the required matrix

$$\begin{array}{ccc} 1 & 1 & 1 \\ 1 & a & b \\ 1 & c & d, \end{array}$$

we obtain the four equations

$$ad - bc = d(a-1) = c(1-b) = a(d-1) = b(1-c),$$

which are equivalent to the following,

$$ad = c = b, \quad bc = d = a.$$

Hence

$$a^2 d^2 = bc, \quad \text{or} \quad a^4 = a.$$

Hence rejecting the values $a=0$ and $a=1$, either of which

would cause the constant product to become zero, we have the two solutions,

$$(1) \quad a=\rho, \quad d=\rho, \quad b=\rho^2, \quad c=\rho^2,$$

$$(2) \quad a=\rho^2, \quad d=\rho^2, \quad b=\rho, \quad c=\rho.$$

There is thus but *one* single type of matrix of this order, viz.

$$\begin{matrix} 1 & 1 & 1 \\ 1 & \rho & \rho^2 \\ 1 & \rho^2 & \rho. \end{matrix}$$

(2) In like manner, for any prime number n there will be but a single type of matrix, the interior *nucleus* of which is a square matrix of the order $(n-1)$ made up of lines or columns of terms in which each line or column contains the $(n-1)$ powers taken in definite order of the $(n-1)$ prime roots of unity. That such a matrix is inversely orthogonal is not difficult of proof; but it is less easy to establish, what I have scarcely a doubt is true (but which I have not yet attempted to demonstrate), that such matrix, when its lines and columns are permuted in every possible manner, contains the complete solution of the corresponding system of $(n-1)^2$ equations. The number of distinct systems or roots satisfying these equations will be the number of distinct forms which can be obtained by permuting the lines and columns—in a word, the number of distinct *derivatives* (a word it will be found hereafter useful to employ) of any given phase of the nucleus. This number will be easily seen to be

$$(n-1) \cdot (n-2)^2 \cdot (n-3)^2 \dots 1^2;$$

for each derivative, when all the permutations are taken of the lines and of the columns, will appear n times repeated. For instance, if ρ be a prime fifth root of unity so that

$$\begin{matrix} \rho & \rho^2 & \rho^3 & \rho^4 \\ \rho^2 & \rho^4 & \rho & \rho^3 \\ \rho^3 & \rho & \rho^4 & \rho^2 \\ \rho^4 & \rho^3 & \rho^2 & \rho \end{matrix}$$

is the nucleus, if we take

the columns in the order 1, 2, 3, 4, rows in the order 1, 2, 3, 4,
 or " " 3, 1, 4, 2 " " 2, 4, 1, 3.
 or " " 2, 1, 4, 3 " " 3, 1, 4, 2,
 or " " 4, 3, 2, 1 " " 4, 3, 2, 1,

the resulting derivative is in each case the same. Thus, then,

when n is a prime number, the system of $(n-1)^2$ equations which give the terms of the nucleus admits of $\Pi(n-1) \cdot \Pi(n-2)$ systems of roots.

It will be seen that this law does not hold when n is a composite number, the rule for which I now proceed to state.

6. (1) I observe that there will be as many distinct types of solutions as there are distinct modes of breaking up n into factors*.

(2) Let $n = p \cdot q \cdot r \dots$ be one of the decompositions in question. Write down the disjunctive product

$$(1, a, a^2, \dots, a^{p-1}) \chi (1, b, b^2, \dots, b^{q-1}) \chi (1, c, c^2, \dots, c^{r-1}) \chi \dots$$

in which the terms are to follow any fixed law of succession. This will produce a line containing $p \cdot q \cdot r \dots$, i. e. n terms.

Let a, b, c, \dots respectively represent the p th, q th, r th, \dots roots of unity; by giving to each of these quantities successively its p, q, r, \dots values we shall obtain $p \cdot q \cdot r \dots$, i. e. n lines, constituting a matrix of the n th order; the totality of the matrices so formed contain between them the complete solution of the $(n-1)^2$ system of equations.

As an example let $n = 4$.

Here there are two modes of decomposition, viz.

$$n = 4, \quad n = 2 \cdot 2.$$

Let i, i' denote the two primitive fourth roots of unity, and denote negative unity by $\bar{1}$. The two types will be

$$\begin{vmatrix} 1 & 1 & 1 & 1 \\ 1 & i & \bar{1} & i' \\ 1 & \bar{1} & 1 & \bar{1} \\ 1 & i' & \bar{1} & i \end{vmatrix} \quad \text{and} \quad \begin{vmatrix} 1 & 1 & 1 & 1 \\ 1 & \bar{1} & 1 & \bar{1} \\ 1 & 1 & \bar{1} & \bar{1} \\ 1 & \bar{1} & \bar{1} & 1 \end{vmatrix}$$

The number of distinct derivatives of the nucleus of the first of these types is $\frac{(1 \cdot 2 \cdot 3)^2}{2}$, i. e. 18, the divisor 2 originating in the symmetry of the square in respect to its diagonals.

The number of distinct derivatives of the second type, which contains a higher capacity of symmetry than the former (i. e. a symmetry persistent under certain permutations of its constituent lines or columns), is 6.

The following Table, in which $+$ $-$ are substituted for $1, \bar{1}$, will make this evident.

* When n is the ν th power of a prime, the number of decompositions becomes the number of indefinite partitions of ν .

Phases of nucleus to type 2.2:—

— + —	+ — —	— — +
+ — —	— — +	— + —
— — +	— + —	+ — —
+ — —	— + —	— — +
— + —	— — +	+ — —
— — +	+ — —	— + —

Phases of nucleus to type 4:—

i — i'	— i' i	i' i —	— + —	+ — —	— — +
— + —	+ — —	— — +	i — i'	— i' i	i' i —
i' — i	— i i'	i i' —	i' — i	— i i'	i i' —
— i i'	i' — i	i i' —	+ — —	— + i	— — +
+ — —	— + i	— — +	— i i'	i' — i	i i' —
— i' i	i — i'	i' i —	— i' i	i — i'	i' i —

i — i'	— i' i	i' i —
i' — i	— i i'	i i' —
— + —	+ — —	— — +
— i i'	i' — i	i i' —
— i' i	i — i'	i' i —
+ — —	— + —	— — +

Thus, then, the total number of distinct solutions of our $(4-1)^2$, *i. e.* 9, algebraical equations applicable to this case is $18+6$, or 24. The formula $\Pi(n-1) \cdot \Pi(n-2)$ would give only 12. How it should happen that the order of the system of equations for different values of n is not an algebraical, but a transcendental function of n depending on the factors of which n is made up, will become less surprising when it is considered that the quantities equated to zero in any such system, although algebraical in themselves, are not analytical but *tactical* functions of n their degree.

7. It remains to assign the value of the constant product in the reduced form of matrix of the order n , or, which comes to the same thing, the value of the complete determinant of such matrix, which is obviously n times the former quantity.

(1) When n is undecomposed, the value of this determinant,

by virtue of a well-known theorem given years ago by Professor Cayley, for expressing the discriminant of an algebraical function as a determinant composed of powers of its roots, is easily recognized to be $i^{(n-1)(n-2)} n^{\frac{n}{2}}$, which we may call Δ_n .

(2) When n is decomposed under the form p, q, r, \dots , the corresponding determinant may easily be proved equal to

$$\Delta_p^{qr\dots} \cdot \Delta_q^{pr\dots} \cdot \Delta_r^{pq\dots} \dots$$

Hence the determinant in the general case is

$$(-)^{\phi} p^{\frac{pqr\dots}{2}} \cdot q^{\frac{qpr\dots}{2}} \cdot r^{\frac{rpq\dots}{2}} \dots = (-)^{\phi} n^{\frac{n}{2}},$$

where

$$\phi = n \sum \frac{(p-1)(p-2)}{p}.$$

Thus, if each term in any reduced inverse orthogonal matrix of the order n be divided by the square root of n , the fourth power of the resulting determinant is unity for all the types without distinction. If n is decomposed into μ equal factors p ,

$$\phi = \mu(p-1)(p-2)p^{\mu-1};$$

so that when $\mu > 1$, the determinant is $\pm i$ if $\mu \equiv 1 \pmod{2}$, and $p \equiv -1 \pmod{4}$, and is ± 1 in all other cases. When $\mu = 1$, its value is $(\pm i)$ if $p \equiv -1$ or $0 \pmod{4}$, and ± 1 in the other two cases. When n is undecomposed, the value of the constant product, which is $\frac{1}{n}$ of the determinant, takes the simple form

$$(i^{n-1} n)^{\frac{n-2}{2}}.$$

8. When n is a power of 2, the type corresponding to its decomposition into the equal factors 2 deserves especial consideration. In this type the only roots of unity which appear are 1 and $\bar{1}$; and as each of those numbers is its own arithmetical inverse, the matrix may be said with equal propriety to be inversely orthogonal or directly orthogonal, *i. e.* orthogonal in the sense conveyed in art. 1. Moreover, on dividing each term by \sqrt{n} , it becomes strictly orthogonal, since the sum of the squares of the terms in each row or column then becomes unity.

A very little reflection will make it clear, *à priori*, that using simply + and - in place of +1 and -1, the known theorems relating to the form of the products of two sums of 2, or of 4, or of 8 squares must exhibit instances of orthogonal matrices of this nature. Thus, to begin with the case of the equation

$$(\alpha^2 + \beta^2)(a^2 + b^2) = A^2 + B^2,$$

we may represent the values of A and B by writing the three matrices

$$\begin{vmatrix} \alpha & \beta \\ \alpha & \beta \end{vmatrix} \quad \begin{vmatrix} a & b \\ b & a \end{vmatrix} \quad \begin{vmatrix} + & + \\ + & - \end{vmatrix};$$

on multiplying these three together, term by term, we obtain

$$\begin{vmatrix} +\alpha a + \beta b \\ +\alpha b + \beta a \end{vmatrix},$$

where

$$+\alpha a + \beta b = A,$$

$$+\alpha b - \beta a = B.$$

Moreover the term-to-term product of the second and third matrix, viz. $\begin{vmatrix} a, & b \\ b, & -a \end{vmatrix}$, is an orthogonal matrix.

So again in the equation

$$(\alpha^2 + \beta^2 + \gamma^2 + \delta^2)(a^2 + b^2 + c^2 + d^2) = A^2 + B^2 + C^2 + D^2$$

the three matrices become

$$\begin{vmatrix} \alpha & \beta & \gamma & \delta \\ \alpha & \beta & \gamma & \delta \\ \alpha & \beta & \gamma & \delta \\ \alpha & \beta & \gamma & \delta \end{vmatrix} \quad \begin{vmatrix} a & b & c & d \\ b & a & d & c \\ c & d & a & b \\ d & c & b & a \end{vmatrix} \quad \begin{vmatrix} + & + & + & + \\ + & - & + & - \\ + & - & - & + \\ + & + & - & - \end{vmatrix}.$$

The resulting product,

$$\alpha a + \beta b + \gamma c + \delta d$$

$$\alpha b - \beta a + \gamma d - \delta c$$

$$\alpha c - \beta d - \gamma a + \delta b$$

$$\alpha d + \beta c - \gamma b - \delta a,$$

represents in its four lines the respective values of A, B, C, D. Moreover the matrix produced by the product of the second and third, *i. e.*

$$\begin{vmatrix} a, & b, & c, & d \\ b, & -a, & d, & -c \\ c, & -d, & -a, & b \\ d, & c, & -b, & -a \end{vmatrix}$$

is an orthogonal matrix. The same remarks apply to the representation of the product of two sums of eight squares under the form of a sum of eight. Omitting the first matrix, consisting of repetitions of one given set of eight letters, $\alpha, \beta, \gamma, \delta, \lambda, \mu, \nu, \pi$, the remaining two matrices may be written as below :

<i>a b c d l m n p</i>	+ + + + + + + +
<i>b a d c m l p n</i>	+ - - + + - + -
<i>c d a b n p l m</i>	+ + - - + - - +
<i>d c b a p n m l</i>	+ - + - + + - -
<i>l m n p a b c d</i>	+ - - - - + + +
<i>m l p n b a d c</i>	+ + + - - - + +
<i>n p l m c d a b</i>	+ - + + - - - +
<i>p n m l d c b a</i>	+ + - + - + - -

The lettered matrix forms (as in the preceding cases) a “conjugate system [in Cauchy’s sense] of regular substitutions.” The right-hand matrix, interpreting + and - to mean plus and minus units, is a direct and inverse orthogonal matrix corresponding to 8 represented as 2 . 2 . 2; the lines produced by the term-to-term multiplication of the three matrices gives the quantities A, B, C, D, L, M, N, P, which satisfy the equation

$$\Sigma A^2 = (\Sigma \alpha^2) \Sigma (a^2),$$

and the term-to-term product of the two matrices actually above written is an orthogonal matrix of the 8th order.

9. I now pass to another and more important illustration of such matrices, which presents itself in the application of Newton’s rule (or my extension of it) for finding a superior limit to the number of real roots in an algebraical equation. That rule deals with permanencies and variations of sign in two series of quantities. It will be more simple to consider the two simultaneous successions of signs obtained by multiplying together the signs of the consecutive terms in the series

$$f, f_1, f_2, \dots, f_n,$$

$$G, G_1, G_2, \dots, G_n.$$

We obtain in this way two series of *n* signs each, written respectively over one another; and the quantities with which the theory is concerned are the numbers, say π and ϕ , of compound signs $\begin{matrix} + \\ + \end{matrix}$ and $\begin{matrix} - \\ + \end{matrix}$ which occur in these simultaneous progressions: the *f* series and *G* series both consist of functions of *x*; the increase of π and the decrease of ϕ , when *x* ascends from one given value *a* to another *b*, each of them gives a superior limit to the number of real roots in *fx* contained between *a* and *b*.

It is of course obvious that π corresponds to the number of double permanencies, and ϕ to that of variation permanencies in

the original series of f 's and G 's. Now it appeared to me desirable, in the same way as double and higher orders of denumerants have been shown in my lectures on Partitions of Numbers to be expressible as linear functions of simple denumerants, so in like manner to get rid of compound variations and permanencies, and to express them, or at least their number, by means of simple variations or permanencies. This comes to the same thing as finding a means of making the enumeration of the four species of compound signs $\begin{matrix} + & | & - & | & + & | & - \\ + & | & + & | & - & | & - \end{matrix}$, in two simultaneous series of signs, depend on the enumeration of the simple signs $+$ or $-$ in those series themselves, or in series derived from them, or in the two sorts combined.

10. As a first step in the generalization of this question, let us suppose i series of simultaneous progressions of $+$ and $-$ signs giving rise to 2^i varieties of vertical combinations of sign. Now let the i given series be combined, r and r together, in every possible manner, where r takes all values from 0 to i , both inclusive.

When $r=1$, it is of course understood that the so-called combinations are the original i series themselves.

When $r=0$, it is to be understood that a series exclusively of the signs $+$ is intended.

When r is not 0, nor 1, let the r series corresponding to any r -ary combination be multiplied term-to-term together.

When $r=0$, the $+$ succession, and when $r=1$ the given n series are to be reckoned as the corresponding products. The number of series of signs so obtained will of course be

$$1 + i + \frac{i(i-1)}{2} + \dots = 2^i.$$

By the sum of any series let us understand the number of signs $+$ less the number of signs $-$. When the i given series are written over one another, each of the 2^i varieties of columns that can be formed of the signs $+$ and $-$ will occur a certain number of times. I shall show that these 2^i numbers are linear functions of the 2^i sums last mentioned. Of this theorem, on account of its importance, I shall give a rigorous proof.

As a matter of typographical convenience, I write the columnar combinations of sign in horizontal in lieu of their proper vertical

order, as, *ex. gr.*, $++-$ in lieu of $\begin{matrix} + \\ + \\ - \end{matrix}$, and, moreover, use such

horizontal line enclosed within brackets to signify the number of the recurrences of the corresponding combination; thus

(+ - - +) means the number of times the combination $\begin{pmatrix} + \\ - \\ - \\ + \end{pmatrix}$

occurs in four given simultaneous progressions. Again, as regards the sums, S will denote the sum of the line of *plus* signs, which is of course the same as the number of terms in each progression, *i* the number of columns, and $S_{p,q,r\dots}$ will denote the sum of the line formed by the multiplication together of the *p*th, *q*th, *r*th, . . . lines of the given *i* set of lines. This being premised, and using each of the symbols $\lambda_1, \lambda_2, \lambda_3, \dots$ to denote + or -, as the case may be, the number of recurrences of each species of combination in terms of the *sums* is expressed by the following formula,

$$[\lambda_1, \lambda_2, \dots \lambda_i] = \frac{1}{2^i} (S + \sum \lambda_p S_p + \sum \lambda_p \lambda_q S_{p,q} + \sum \lambda_p \lambda_q \lambda_r S_{p,q,r} + \&c.),$$

as I shall proceed to prove. But first, to make the meaning of this formula more clear, let us suppose $i=2$, the formula then gives the following equations:—

$$\begin{aligned} (+ +) \text{ i. e. the number of } & \left. \begin{array}{l} \text{combinations} \\ \left[\begin{array}{c} + \\ + \end{array} \right] \end{array} \right\} = \frac{1}{4} \{ s + s_1 + s_2 + s_{1,2} \}, \\ (+ -) \quad \text{,,} \quad \text{,,} & \left[\begin{array}{c} + \\ - \end{array} \right] = \frac{1}{4} \{ s + s_1 - s_2 - s_{1,2} \}, \\ (- +) \quad \text{,,} \quad \text{,,} & \left[\begin{array}{c} - \\ + \end{array} \right] = \frac{1}{4} \{ s - s_1 + s_2 - s_{1,2} \}, \\ (- -) \quad \text{,,} \quad \text{,,} & \left[\begin{array}{c} - \\ - \end{array} \right] = \frac{1}{4} \{ s - s_1 - s_2 + s_{1,2} \}. \end{aligned}$$

11. Now for the proof of the general formula.

For shortness call the quantity $S + \sum \lambda_p S_p + \sum \lambda_p \lambda_q S_{p,q} \dots$ (where the signs $\lambda_1, \lambda_2, \dots \lambda_i$ are all supposed to be given) E.

Let us consider the effect of the existence of any single column of signs $\mu_1, \mu_2, \dots, \mu_i$ in the given *i* progressions upon the value of E; besides contributing the signs $\mu_1, \mu_2, \dots, \mu_i$ respectively to the series $s_1, s_2, \dots s_i$ this column will contribute to the series

$$s_{\theta_1, \theta_2, \dots \theta_j}, \text{ the sign } \mu_{\theta_1}, \mu_{\theta_2}, \dots, \mu_{\theta_j}.$$

Hence altogether it will contribute to E

$$(1 + \lambda_1 \mu_1)(1 + \lambda_2 \mu_2) \dots (1 + \lambda_i \mu_i) \text{ units ;}$$

and thus the total value of E, depending on the entire number

of columns of all kinds, will be

$$\Sigma(\{(1 + \lambda_1\mu_1)(1 + \lambda_2\mu_2) \dots (1 + \lambda_i\mu_i)\} \cdot (\mu_1, \mu_2, \dots, \mu_i)),$$

where the λ system is given, but the μ system is variable.

But any factor $(1 + \lambda_q \cdot \hat{\mu}_q)$ is zero unless $\lambda_q = \mu_q$. Hence for any system of values of μ not coincident with the λ system, the corresponding multiplier of $(\mu_1, \mu_2, \dots, \mu_i)$ vanishes, and for that system it becomes 2^i . Hence

$$E = 2^i(\lambda_1, \lambda_2, \dots, \lambda_i),$$

as was to be proved.

12. These formulæ admit of a useful application to Newton's rule.

The two superior limits to the number of roots included between (a) and (b) which it (or my extension of it) furnished are $\Delta(+ +)$ and $-\Delta(+ -)$, where Δ refers to the ascent from a to b, and the series are those mentioned in art. 9. Hence, calling the two limits λ, λ' , remembering that s is constant,

$$\lambda = \frac{\Delta s_1 + \Delta s_2 + \Delta s_{1,2}}{4},$$

$$\lambda' = \frac{\Delta s_1 - \Delta s_2 + \Delta s_{1,2}}{4};$$

so that the limits are

$$\frac{1}{4}\{\Delta s_1 + \Delta s_{1,2}\} \pm \frac{\Delta s_2}{4}.$$

The mean of these is $\frac{1}{2}(\Delta s_1 - \Delta s_{1,2})$, which *à fortiori* is also a superior limit. Here s_1 refers to the series

$$f, f_1, f_2, \dots, f_n,$$

and $s_{1,2}$ refers to the series

$$fG, f_1G_1, f_2G_2, \dots, f_nG_n,$$

which I have called, in an article in this Magazine, the H series. If p is the number of permanencies in the f and ϕ in the H series, it is readily seen that

$$\frac{\Delta s_1 + \Delta s_{1,2}}{4} = \frac{\Delta f + \Delta \phi}{2}.$$

Hence, since λ and λ' are each of them superior limits, it follows as an immediate consequence that $\frac{\Delta f + \Delta \phi}{2}$ is so likewise;

but this assertion conveys no new information, and ought not to be treated as a new theorem, as I inadvertently stated it to be; the fact, however, of its being implied in what was previously known is so far from being immediately evident, that M. Angelo

Genocchi has followed me in regarding the theorem as an independent one, and devoted an article to the demonstration of it as such in the *Nouvelles Annales* for January of this year*.

13. The complete system of relations between the two sets of 2^i quantities given by the theorem in art. 10 may it is evident be expressed by means of the inverse orthogonal matrix (also orthogonal) whose type corresponds to 2.2.2... (i terms). Thus, *ex. gr.*, for the case of $i=3$, we may write—

	s	s_1	s_2	s_3	$s_{1,2}$	$s_{1,3}$	$s_{2,3}$	$s_{1,2,3}$
$2^3 . (+ + +)$	+	+	+	+	+	+	+	+
$2^3 . (+ + -)$	+	+	+	-	+	-	-	-
$2^3 . (+ - +)$	+	+	-	+	-	+	-	-
$2^3 . (+ - -)$	+	+	-	-	-	-	+	+
$2^3 . (- + +)$	+	-	+	+	-	-	+	-
$2^3 . (- + -)$	+	-	+	-	-	+	-	+
$2^3 . (- - +)$	+	-	-	+	+	-	-	+
$2^3 . (- - -)$	+	-	-	-	+	+	+	-

* If we call ν the number of real roots in f comprised between a and b , we know from Fourier's theorem that $\rho = \Delta p - 2\theta$, where θ is the number of times that an *even* change occurs in the value of the p as we pass from a to b , this change being always in the positive direction. And, again, as I have shown in the article in the *Philosophical Magazine* above referred to,

$$\rho = \frac{\Delta p + \Delta \phi}{2} - \mathfrak{S},$$

where \mathfrak{S} is the total number of times that ϕ undergoes a change within the same interval,—such change being always even, on account of the two terminals of the G series being both positive—the one extremity being a positive constant, and the other the square of f . This change, however, is sometimes additive and sometimes ablative, ϕ not necessarily *increasing* always (as p does) on ascending from a to b : thus the two unknown transcendents θ and \mathfrak{S} are connected by the simple relation

$$2\theta - \mathfrak{S} = \frac{\Delta p - \Delta \phi}{2}.$$

Of course each evanescence of a term in the f or G series between two terms of like sign is to be reckoned as a distinct *time* of change. I also make abstraction of the singular cases where several consecutive terms vanish together in either series.

The meaning of this Table is self-apparent. Thus, *ex. gr.*, if we wish to find the value of $(- + +)$, *i. e.* the number of recur-

rences of $\begin{vmatrix} \overline{+} \\ + \\ + \end{vmatrix}$ in the three given series, we read it off from the 5th line above and find it equal to

$$\frac{s - s_1 + s_2 + s_3 - s_{1,2} - s_{1,3} + s_{2,3} - s_{1,2,3}}{8}$$

The Table of signs itself is obviously the matrix corresponding to the product $2 \cdot 2 \cdot 2$.

From the fact of this Table being orthogonal, we infer that the two sets of quantities are (to a numerical multiplier *près*) the same linear functions, the first set of the second, and the second of the first.

14. The theorem of art. 10 may be extended to simultaneous progressions of signs denoting any root of $+$, as, *ex. gr.*, $+\rho, \rho^2$, where ρ is a cube root of $+$ instead of $+$ and $-$. Let each series be supposed to consist of q th roots of $+$, and let $(\lambda_1, \lambda_2, \dots, \lambda_i)$ denote the number of recurrences of the column

$\begin{vmatrix} \lambda_1 \\ \lambda_2 \\ \vdots \\ \lambda_i \end{vmatrix}$ in which each λ is some q th root of $+$; then there

will be q^i quantities of the form $(\lambda_1, \lambda_2, \dots, \lambda_i)$. Again, we may form series by combining together not merely the given i series themselves, but their squares, cubes, &c. up to the $(q-1)$ th powers, and form the term-to-term products of all the series entering into any such combination; in this way, including s (the series constituted exclusively of $+$ signs), we shall obtain q^i series, the general symbol for the sum of the terms in any one of which, when we substitute the roots of 1 for the corresponding roots of $+$, may be written $[s_1^{q_1}, s_2^{q_2}, s_3^{q_3}, \dots, s_i^{q_i}]$, where each s is a q th root of $+$, and each q with a subscript is some one of the numbers in the series $0, 1, 2, \dots, (q-1)$. If now we understand by the above bracket, when q_1, q_2, \dots, q_i are all zero, the value corresponding to s in the particular case previously considered, *i. e.* the number of terms in each series, the relation between the two sets of q^i numbers is given by the equation

$$(\lambda_1, \lambda_2, \dots, \lambda_i) = \frac{1}{q^i} \sum \frac{[s_1^{q_1}, s_2^{q_2}, s_3^{q_3}, \dots, s_i^{q_i}] *}{(\lambda_1^{q_1}, \lambda_2^{q_2}, \lambda_3^{q_3}, \dots, \lambda_i^{q_i})}$$

* The reader will please to observe that the terms included under the sign of summation are in general not real but complex numbers formed with the q th roots of unity. Their *sum*, however, is necessarily a real number, being the number of recurrences of the column of signs $\lambda_1, \lambda \dots, \lambda$

If we write out a Table expressing these relations in a manner similar to that employed for the particular case of $q=2$ in a preceding article, we shall obtain a square array of signs (q^i to a side) which will form an inverse orthogonal matrix corresponding to the type $q \cdot q \cdot q \dots$ (to i terms).

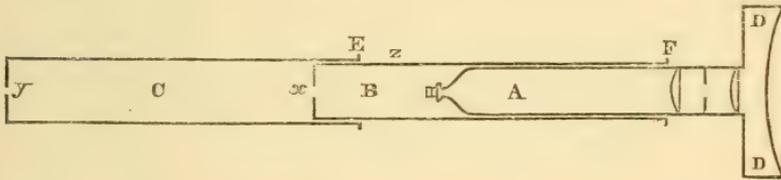
[PART II. to follow.]

LXI. Description of a New Photometer.
By C. H. BENNINGTON, M.A. Cantab.*

"LIGHT varies inversely as the square of the distance."
O is a pinhole in an opaque screen, papered and lighted behind. A scale extends 1 foot halfway from O to the observer. An opaline screen x slides on the scale.

Suppose when x is visible set at 1 foot from O, O	}	Times.
can be photographed in	}	1
∴ when x is only visible at $\frac{1}{12}$ of a foot from O, O	}	12 ²
can be photographed in	}	

My photometer is based on the above principle.



A is a small compound microscope† sliding tight in a tube B, at the end of which in a $\frac{1}{15}$ -inch diaphragm is an opaline micro-photograph x . B slides tight in another tube C which has a $\frac{1}{60}$ -inch diaphragm y . D is an eyepiece fixed to A, fitting the

in the given system of sign-progressions. The proof of the theorem is precisely the same as for the case previously considered, where $q=2$; viz. it may be shown that the sum above denoted by Σ is equal to

$$\Sigma \left\{ \frac{\left(1 - \frac{\lambda_1}{\mu_1}\right)^q \left(1 - \frac{\lambda_2}{\mu_2}\right)^q \dots \left(-1 \frac{\lambda_i}{\mu_i}\right)^q}{\left(1 - \frac{\lambda_1}{\mu_1}\right) \left(1 - \frac{\lambda_2}{\mu_2}\right) \dots \left(1 - \frac{\lambda_i}{\mu_i}\right)} \right\} [\mu_1, \mu_2, \dots, \mu_i]$$

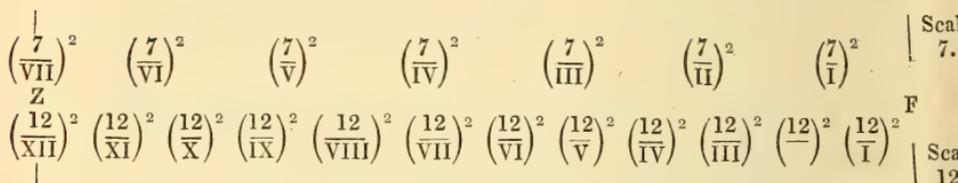
each of the q^i terms of which new sum vanishes except that one in which the variable μ system is identical with the given λ system of the q th roots of unity, for which term the fraction becomes equal to q^i .

* Communicated by the Author.

† The eye-tube of a small telescope answers the purpose.

eyeball to exclude stray light. F and E are the ends of B and C. Let A be fixed in B at the distance to see x , and let the time for photographing a house in full sunshine with a portrait-lens of full aperture be already known. Hang a white blind over one of the windows. Get the picture of the blind sharp in the centre of the focusing-screen, and fix y on it by means of a pneumatic holder. Draw B nearly out of C, and, having cleared the eye from impressions of light, make the observation. Push B in till x is just distinct, and write Z on B where E now stands. Divide FZ into six scales of XII., XI., X., IX., VIII., VII. equal parts, counting I. in each scale from F.

The 7 and 12 scales will here do for examples.



Now, to take a darker subject with the same or a slower lens and the same chemicals :—

Choose a half-lighted portion of the subject, not the darkest details nor the brightest, but a half light; put against it, parallel to the plate, enough white material to make a $\frac{1}{4}$ -inch spot on the focusing-screen. Fix the end y on the spot and make the observation; to do this set E at I; x will perhaps be too distinct, if so, turn E back till x is about as plain as before. Suppose E happens to stand at IV. scale 12, then if a picture can be taken in t (a known time) when x is distinct at Z, it can now be taken in $t \times \left(\frac{12}{IV}\right)^2 = t \times 9$.

C should be lined with black velvet. B and C must be just 4 inches long, with x and y quite central, and the plane of x in the end of B.

The light, be it strong or weak, must be constant, or the test fails.

It may be said that as a visual index it does not measure actinism; but I assume that the amount of actinism is always proportional to the amount of light, and I think that no practical error will occur if the observation be made in uncoloured daylight; and with care and practice it is quickly made, which is a very useful item in photometry.

Melton, Suffolk,
November 6, 1867.

LXII. *Notices respecting New Books.*

A Treatise on Astronomy, Spherical and Physical; with Astronomical Problems, and Solar, Lunar, and other Astronomical Tables. For the use of Colleges and Scientific Schools. By WILLIAM A. NORTON, M.A., Professor of Civil Engineering in Yale College. Fourth Edition. Revised, remodelled, and enlarged (pp. 560). New York. 1867.

WHEN composing the work before us, the author seems to have had in view the wants of the numerous class of readers who bring to the study of natural phenomena a knowledge of elementary mathematics only; this class we suppose to be more numerous in America than in England, though here it must be large and increasing. There cannot, we apprehend, be any question of the advantage gained by treating the subject mathematically in its elementary parts, and particularly in setting the student to work problems in astronomy; but if a strictly mathematical treatment is adopted throughout, the work is of necessity adapted for a small number of readers. It thus becomes a question to be settled by a sort of compromise, how far the strictly scientific treatment shall be carried, and where the popular or semipopular treatment shall begin. To effect the compromise satisfactorily, the author must know the wants and acquirements of the class he addresses; and this knowledge, as far as we can judge, Professor Norton possesses, and has used admirably. He has undoubtedly succeeded in producing a work which, if studied throughout, will put the reader in possession of an extensive acquaintance with the facts of the science, as well as with the power to work a large number of astronomical problems.

What the author has done and what he has not attempted we shall now show by briefly mentioning the contents of the volume. It consists of three parts—viz. Spherical Astronomy, Physical Astronomy, and Astronomical Problems. The contents of the first part may be inferred from its title; but we may mention that its contents illustrate the incompleteness which must attend the compromise the author has tried to effect, and the inconveniences which, at all events partially, it necessitates. Thus several of the questions connected with the parallax of heavenly bodies are discussed on pages 60–65; but the formulæ for parallax in right ascension and declination, in latitude and longitude, and the augmentation of the moon's semi-diameter, are transferred to the appendix. So with regard to eclipses; a large part of the subject is contained in Chap. XVI. pp. 190–197, including the process of calculating an eclipse of the moon. We get more about an eclipse of the moon on page 371, and, following it, an account of the calculation of an eclipse of the sun, pp. 375–392. Finally, in the appendix we have another long article, headed Calculation of an Eclipse of the Sun, pp. 426–431. It seems only fair to question whether the arrangement in this last case might not be amended.

The second part of the work, treating of Physical Astronomy, oc-

cupies only thirty-seven pages, and therefore is little more than a sketch, though doubtless very good so far as it goes. Its treatment is so characteristic of the work that we will mention its contents. In the first place it is shown that Kepler's laws follow from the law of universal gravitation, the proofs being substantially the same as those of Newton's 1st, 11th, and 15th propositions. The case of two reciprocally attracting bodies is then briefly considered. In the case of three bodies, expressions are found for the components of the disturbing force along the radius, tangent, and at right angles to the plane of the orbit respectively; but that point attained, the analytical treatment of the question stops. The modifications of the motion of the disturbed body under the action of these forces is only considered generally, the kind of effect produced by each being pointed out without any attempt to calculate its magnitude. The remainder of the part, comprising the questions of the densities of the Planets, of the form and density of the Earth, and of the Tides, is treated in an entirely popular manner. The last of these questions is gone into in very considerable detail, considering the space at the author's disposal, so far as the facts concerning the tides at particular places are concerned.

Perhaps the most valuable part of the work is the section devoted to the solution of astronomical problems; in every case worked examples are given, and the requisite Tables are contained in the volume. In no branch of mathematics is it more necessary than in astronomy that the student should be constantly referred back to actual numerical results. It is very possible for a person to have read a great deal about astronomy and yet to be puzzled when set to do a very simple numerical question; and, besides, numerical results impart a reality to knowledge which is not otherwise to be attained.

We ought to add that throughout the work accounts are given in considerable detail of astronomical facts and appearances (as in the case of the tides already mentioned), and that the physical theories used to account for these phenomena are briefly but fully stated. Such are the chapter on "Theories of the Evolution of Sidereal and Planetary Systems," and the account of the author's speculations on the formation of the tails of comets by the action of the sun's "repulsive force." These portions are very properly printed in a different type from the body of the work.

The treatise forms a handsome octavo volume; the printing is legible, and, so far as we have noticed, accurate. But exception must be taken to the illustrations, which in some instances are coarsely cut (*e. g.* figs. 19, 77, 95), and in others ill printed (*e. g.* figs. 76, 96). This is a defect, though perhaps a minor defect, and certainly will not seriously impair the usefulness of a very valuable work.

LXIII. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 403.]

November 6, 1867.—Warrington W. Smyth, Esq., M.A., F.R.S.,
President, in the Chair.

THE following communication was read:—

“On the Amiens Gravel.” By A. Tylor, Esq., F.L.S., F.G.S.

The author referred first to the prevalent views respecting the gravels of the Valley of the Somme, namely, (1) that there are two deposits of distinct age—the upper and the lower valley-gravels; (2) that the former of these is the older; (3) that the Valley of the Somme has been excavated to the depth of 40 or 50 feet since its deposition; (4) that both gravels contain bones of extinct animals, and implements of human manufacture, the lower gravels, however, containing the greater number of species of Mollusca, and the upper the greater number of flint implements; and (5) that the height (70 feet) of the gravels at St. Acheul above the present level of the Somme is much beyond the limit of floods, and that, therefore, they could only have been deposited before the river-channel was cut down to its present level. He then pointed out that the general effect of these views is to refer back the remains of man found at St. Acheul to an indefinite date, separated from the historical period by an interval during which the valley was excavated.

In former papers Mr. Tylor stated his belief that the upper and lower valley-gravels of the Somme are continuous and of the same age, which he considered to be close to the historical period. In this paper he stated facts which appeared to him to demonstrate the truth of his views, and described a number of sections near Amiens, in which the levels were laid down from an exhaustive survey by M. Guillom, Chief Engineer of the Northern Railway of France.

The conclusions he had thus been able to arrive at are the following:—(1) That the surface of the chalk in the Valley of the Somme had assumed its present form prior to the deposition of any of the gravel or loess now to be seen there; (2) that the whole of the Amiens valley-gravel is of one formation, of similar mineral character, contains nearly similar organic remains, and belongs to a date not much antecedent to the historical period; (3) that the gravel in the valley of the Somme at Amiens is partly composed of debris brought down by the river Somme and by the two rivers the Celle and the Arve, and partly of material from the higher grounds washed in by land-floods; (4) that the Quaternary gravels of the Somme are not separated into two divisions by an escarpment of chalk parallel to the river, as has been stated; (5) that the evidence of river-floods extending to a height of at least 80 feet above the present level of the Somme is perfectly proved by the gradual slope and continuity of the gravels deposited by them; and (6) that many of the Quaternary deposits in all countries, clearly posterior to the formation of the valleys in which they lie, are of such great dimen-

sions and elevation that they indicate a pluvial period just as clearly as the Northern Drift indicates a Glacial. This Pluvial period must have immediately preceded the true Historical period.

November 20, 1867.—Warrington W. Smyth, Esq., M.A., F.R.S.,
President, in the Chair.

The following communications were read :—

1. "On the Glacial and Postglacial Structure of Lincolnshire and South-east Yorkshire." By S. V. Wood, Jun., Esq., F.G.S., and the Rev. J. L. Rome, F.G.S.

The features of Yorkshire and North-east Lincolnshire having distinctive characters from those of Central and South Lincolnshire, the authors described the two areas separately. In the former, their coast sections exhibited the Glacial clay separated into two portions : of these the lower, which they identified with the ordinary (or upper) Glacial clay of the South, contains abundant chalk débris ; but the upper or purple portion (which was in places divided from the lower by sand and gravel beds) contains no chalk in the upper, and but little in the lower part of it, the place of the chalk being taken by swarms of Palæozoic fragments. The latter of these clays alone extends over the Wold-top at Speeton, and alone occupies the valley along the northern Wold-foot, and so away northwards to Scarborough and the Tees' mouth, from which the authors inferred that the north of England did not subside beneath the glacial sea until after the south had been submerged. The, so-called, Bridlington "Crag" was shown to be an intercalated bed in this purple clay. Both these clays were shown to be denuded, and their denuded edges to be everywhere covered by a much thinner Boulder-clay, that of Hessle, which wraps Holderness like a cloth, extending to altitudes of 150 feet, and running down the east of Lincolnshire to the Fen-border. This Postglacial Boulder-clay of Hessle is again cut through, and in those places covered by posterior beds of gravel, one of which (at Hornsea) contained fluviatile shells. At Hull this clay supports a forest, which is now submerged 33 feet below the Humber, the same submerged forest also occurring at Grimsby. The authors regarded the position of the sea during the Postglacial period as having been principally on the west of the Yorkshire and North Lincolnshire Wold until the formation of the gravel-troughs, cutting through the Hessle clay, and that its present position was connected with a recent westerly elevation and easterly depression.

The Glacial clay of Central and South Lincolnshire belongs to the chalky portion, from which all the superior or purple part of the formation has been denuded ; and the valleys of Central Lincolnshire were shown to be cut out of the Cretaceous series and Glacial clay as a common bed, the hills formed of the clay rising to elevations equal to the Wold in that part.

The Glacial clay of both areas was shown to be denuded westwards, and the denuded edges occupied with sands and gravels, termed by the authors denudation-beds.

2. "On supposed Glacial markings in the Valley of the Exe, North Devon." By N. Whitley, Esq.

Mr. Jukes having, in a late paper, mentioned some glacial grooves observed by him in the valley of the Exe, the author stated his opinion that the "Grooves" have been formed by the minor contortions of the strata, and not by glacial action.

3. "On Disturbance of the Level of the Land near Youghal, in the South of Ireland." By A. B. Wynne, Esq., F.G.S.

The object of this communication was to place upon record certain facts connected with alterations of level in the shore of the South of Ireland, near the town of Youghal. The occurrence of submerged peat beneath the Youghal strand shows that considerable alterations of level have taken place along the coast of Youghal Bay subsequently to the formation of the peat which so commonly covers the Glacial Drift of Ireland. The author also inferred that towards the close of the Glacial period the sea was further from the present land than it is now, that the land then sank to the depth of about 90 or 100 feet, and subsequently rose again, but not to its former level; and, in conclusion, he pointed out the probability of its undergoing depression at the present time.

LXIV. *Intelligence and Miscellaneous Articles.*

ON THE PASSAGE OF PROJECTILES THROUGH RESISTING MEDIA.

BY M. MELSENS.

I HAVE observed that a lead ball falling through the air into water carries with it a considerable quantity of air. In the case of a spherical ball 17 millims. in diameter, and weighing 29 grammes which fell through a height of a metre into a column of water, it was found that the volume of the air was about twenty times that of the ball. In making the experiment, it was found that part of the air was disengaged before reaching the bottom; but when it touched, a large bubble was seen to be disengaged, accompanied by a number of smaller bubbles. I imagine I am not far from the truth in assuming that half the air collected is carried to a depth of more than a metre.

I have endeavoured to investigate the course of the phenomenon in the case of a projectile discharged from a firearm, and passing into water after having traversed a layer of air of a few metres. The following is the arrangement of the experiment.

A metal cylinder placed horizontally is provided with two graduated vertical tubes and a lateral tubulure, by which it can be connected with a reservoir of water under a pressure of more than 1 metre. One of the ends of the cylinder is closed by a thin brass plate, the other by a very thick plate of lead. The apparatus and its tubes being filled with water, a pistol-bullet is fired against the brass plate; this is thereby perforated; the water of the cylinder, which is connected with the reservoir of water, jets through the aperture formed by the ball; and it is found that the volume of air which

is disengaged and is collected in the graduated tube is at least a hundred times as great as the volume of the projectile.

These experiments appear to me to prove that sufficient attention has not been devoted to the important part which air plays in the penetration of projectiles into resisting media, and during their passage through more or less thick solid plates.

I have made an extensive series of shots in plates of different kinds, using projectiles either of metal or of organic substances, as stearine, wood, caoutchouc, &c., varying the nature of the media to be penetrated and the substance of the projectile.

The following are some facts.

If a leaden ball be fired either with a great or a small velocity at an ordinary slate, the projectile traverses without smashing it; the round apertures produced are small, and but little different whatever be the velocity. A perfectly round hole may even be made, without breaking the slate, although the velocity of the projectile is inadequate to pass through the slate. This latter experiment requires that the slate be fastened in a wooden frame, or regularly supported in several points. If a lead or a cast-iron ball be fired from a pistol or a gun against a leaden plate, it is observed that the larger apertures in the lead correspond to the great velocities, so that the measurement of the diameter would furnish a datum for the velocity of the projectile. The plate remains flat for great velocities, but bulges out with small ones. Chipped edges are produced on the two sides of the plate: those on the side of the face struck are virtually perpendicular to the trajectory, those on the opposite face are parallel; in short, they are almost at right angles.

The results of the firing into plates of plastic clay are of the most unexpected character, and have surprised all the artillery officers who have witnessed them.

(1) With equal velocities the apertures are larger the thicker the plates traversed, up to a certain limit.

(2) The diameter of the circular apertures increases with the velocity of the projectile.

Thus a pistol-bullet (12 millims. in diameter and weighing about 10 grammes) produces in a very plastic plate of ordinary clay, when shot by a charge of powder of 0.150 gr., an aperture having twice the diameter of the ball; the same projectile shot by 2 to 2.5 grms. of powder produces such an aperture as would be attributed to a projectile from a field-gun.

The edges on both sides are so strongly jagged that it is difficult to say on which side of the plate the ball has struck.

This phenomenon is accompanied by one no less remarkable: part of the clay flies towards the firer, and is thrown several metres in the opposite direction to that of the projectile.

(3) Two plates of clay are taken, each about 15 millims. thick and 25 to 30 centimetres in diameter, and are cemented together by moistening their surfaces.

If a pistol-bullet be shot by 2 grms. of powder against these plates, the edges are formed as in the preceding experiment; but the two

plates separate partially by bulging from each other, in opposite directions, leaving a hollow between them like a double convex lens, the centre of which corresponds to the hole formed by the ball; the circular aperture due to the passage of the projectile attains more than 10 centims. diameter.

With thick clay plates the aperture has the shape of a truncated cone, but, contrary to what happens in the case of indefinite media, the base of the cone is on the side at which the ball emerges.

In large blocks an ellipsoidal or double conical shape is met with, and at a small depth the figure is met with deduced from the able analysis and the celebrated experiments of MM. Didon, Probert, and Morin.

The general shape and the greater or less degree of acuteness of the cone appear to depend on the density of the projectile. I have even produced cavities resembling a semiellipsoid of revolution, when, for instance, wooden balls were used, which burst.

I am indebted to the kindness of M. Regnault for having been able to repeat the experiments, originally made on common clay, with a mass of Sèvres porcelain prepared for hardening in the fire. I may be allowed to express my obligation to the able director of the Imperial Manufactory of Sèvres.

I have lately examined the shape of the aperture produced by lightning in a glass pane 4 millims. in thickness, and have resumed on this subject experiments which I made long ago. The results of these experiments are completely opposed to what is assumed with regard to the phenomena of the communication of motion*.

Hundreds of experiments, in fact, without exception, give the following results:—

(1) When a pressure is exerted on the centre of a square gradually increasing until it is broken, a certain number of slits are made which radiate around the centre of pressure; these slits are generally in almost straight lines.

(2) A cork bullet fired by a strong charge of powder against a pane coated on one side with gummed paper produces slits analogous to the preceding; these are sometimes intersected by slits, which indicate that the motion commenced at the point struck.

(3) If from the same pistol and with a small charge of powder a leaden bullet be fired against a pane suspended by metal wires, the glass is perforated and broken as in experiment No. 1.

(4) Other things being equal, if the charge of powder be increased to about 0.2 grm., the ball traverses the square, producing a greater or less aperture surrounded by short and almost rectilinear slits;

* The following extract is taken from M. Delaunay's excellent *Traité de Mécanique Élémentaire* (vol. i. p. 156, 3rd edition):—"A lead bullet gently projected against a window-pane would be repelled without producing fracture. If thrown strongly by the hand, it would pass through the square, producing a large number of slits which radiate round the hole through which it has passed. But if the bullet is fired from a gun, it will merely make a round hole in the pane; the rest of the pane will be unchanged."

the other parts of the square remain unchanged, and it continues suspended.

(5) If the charge be increased to 2·5 grms. for instance, all other conditions remaining the same, the square is smashed into a number of pieces which fall almost vertically. To ascertain the shape of the débris, a sheet of paper may be fastened on one or both of the faces of the glass: the principal phenomena are unchanged; and it is seen that with small velocities the hole produced in the paper is well defined and round without projections on the side struck, while with greater velocities the edges of the paper are strongly jagged on the two sides.

These experiments were made with squares of all sizes, varying in thickness between 1 and 5 millims., and either suspended, fixed, or cemented. They may even be inclined at an angle of 45° to the trajectory without materially altering the phenomena.

Comparing these experiments with those which prove the transportation of the air, we must conclude that the air which precedes the ball commences the action; and we may perhaps be able to prove that the square is perforated in certain cases before being actually reached by the projectile. However this may be, it seems to me that we have generalized too widely in the application of the celebrated experiment of the Abbé Camus*.

We frequently confound, in the question of the impact of solid bodies considered from this point of view, the effect of rupture, or the molecular motion communicated to the body struck, with the effects of the translation of matter in the direction of the projectile striking the body at rest, and the motion of translation of the whole of this body.

I am obliged to my friend Major Caron for having enabled me to show my experiments to several artillery officers and other French scientific men, after having arranged the apparatus in the chemical laboratory under the direction of the artillery committee.—*Comptes Rendus*, September 30, 1867.

ON PHOSPHORESCENT LIGHT. BY DR. KINDT IN BREMEN.

If a piece of chlorophane, which, heated in a tube, gives a green light visible even by daylight, be viewed in the dark through a spectrum-apparatus, homogeneous green only is seen. Phosphorite of Estremadura (which gives a yellowish light) shows green, yellow, and red. A green fluorspar from the Breisgau shows, on the contrary, two dark lines in the green, one of which is near the orange-red. Two other, bright-green fluorspars, of unknown localities, present the same phenomenon. The dark lines are as powerful as in solutions of didymium.—Poggendorff's *Annalen*, No. 5, 1867.

* A musket-ball piercing a piece of wood of considerable thickness without communicating to it any appreciable velocity (*Mémoires de l'Académie des Sciences*, 1738).

ON THE INFLUENCE OF VARIOUS COLOURED RAYS ON THE DECOMPOSITION OF CARBONIC ACID IN PLANTS. BY M. CAILLETET.

It has long been observed that the green parts of plants exposed to the direct rays of the sun possess the property of decomposing the carbonic acid in the air, and of disengaging an almost equivalent quantity of oxygen. In darkness the converse phenomenon is produced: the oxygen of the air is absorbed, and carbonic acid is disengaged, arising from the oxidation of part of the carbon of the plant.

In the experiments I have undertaken to ascertain the influence of the various coloured rays on the decomposition of carbonic acid by plants, I have endeavoured to attain as far as possible the conditions under which nature works.

I had to arrange my coloured glasses so as to prevent, by a considerable draught of air, the great increase of temperature produced in closed vessels exposed to the direct solar rays. Under a red glass I have observed the temperature rise to above 70° .

By preliminary trials I found that by taking some precautions the detached leaves acted on mixtures of gases as if they still adhered to the plant which produced them. I have also found that leaves of the same plant and of equal surfaces virtually decompose the same quantities of carbonic acid when they act upon the same gaseous mixtures exposed to the same source of light.

The absorption of carbonic acid, and the disengagement of oxygen more or less mixed with nitrogen, is met with exclusively in the green parts of vegetables; but it is indispensable that the organs be intact, for by crushing or even by rubbing them the property of absorption is destroyed. When a leaf is carefully cut into small fragments the decomposing action is still manifest; for each part, containing all the anatomical elements, acts like an entire leaf. A temperature of 10° to 15° C. is necessary for the manifestation of the decomposing action, but the rays of obscure heat are insufficient to produce it. This I have been able to ascertain by means of an apparatus which I owe to the skill of M. M. Alvergriat.

It consists of two concentric gas-jars of colourless glass sealed at the bottom. In the space between these two vessels of different diameter there is a concentrated solution of iodine in bisulphide of carbon. This screen only allows obscure heat to traverse it; and it can be ascertained that carbonic acid placed in the central gas-jar is not decomposed by the leaves, spite of the continued action of the sun's rays.

The different coloured rays have, on the contrary, a special action more or less energetic in the decomposition of carbonic acid. When tubes containing leaves of the same plant equal in surface are placed in the same gaseous mixture under bell-jars of different colours, after eight or ten hours' exposure to the sun, the quantities of carbonic acid left undecomposed are stated in the following Table.

Rays passing through	Air mixed with CO ² .			Observations.
	18 in 100.	21 in 100.	30 in 100.	
Iodine dissolved in bisulphide of carbon.	18	21	30	Photographic paper not blackened.
Green glass	20	30	37	
Violet glass	18	19	28	Chloride of silver slowly coloured.
Blue glass	17	16.5	27	
Red glass	7	5.50	23	Paper very rapidly blackened.
Yellow glass	5	1	18	
Ground glass	0	0	2	Neither paper nor chloride of silver mixed with nitrate blackened.
				Paper not blackened.
				Paper rapidly coloured.

The examination of this Table proves that the thermal rays, equally with the chemical rays, are without action on the strange decomposition of carbonic acid by vegetables which takes place under conditions quite different from those we can produce in our laboratories. But the forces which produce this decomposition act on the elements of this compound body dissolved in the liquids of the leaf, and we must avow our entire ignorance of the state in which these elements exist in the solution. It would appear, from an inspection of the numbers given in the Table, that the colours which are most active, from the chemical point of view, are just those which least favour the decomposition of carbonic acid.

I must particularly dwell upon the special and unexpected action of green light, whether this colour be obtained by a glass, by the leaves of vegetables, or by coloured solutions. Under its influence, carbonic acid does not seem to be decomposed at all, but, on the contrary, a new quantity of the gas seems to be produced by the leaves.

For if under a green glass bell-jar, and exposed to the direct rays of the sun, a gas-jar be placed containing a leaf in pure air, we obtain, after the lapse of a few hours, a quantity of carbonic acid little less than would be produced by the same leaf in total darkness.

It is possibly owing to this curious property of green light, which must in the long run produce the etiolation of the plants on which it acts, that vegetation is generally languishing and feeble under large trees, though the shade they produce is frequently not very intense.

The results of my experiments agree with the conclusions of the beautiful research published by MM. Cloez and Gratiolet on the vegetation of submerged plants. Working with gaseous mixtures, I have merely been able to confirm this curious property of green rays, which the authors could not have been led by their special researches to suspect.—*Comptes Rendus*, August 19, 1867.

ON THE INTERFERENCE-COLOURS OF RADIANT HEAT.

BY H. KNOBLAUCH.

The following are the chief results of an investigation of this subject on which Professor Knoblauch has been engaged.

(1) When two groups of rays meet under certain conditions, radiant heat differs in its properties after the meeting; for instance, as regards its property of traversing diathermanous bodies, it manifests an interference-colour.

(2) If this is produced in doubly refracting crystals placed between a polarizing agent (for example two Nicol's prisms), when the plate of crystal is fixed and one Nicol is rotated, the colour passes through white to the complementary.

(3) The maximum of these differences ensues if the polarizing arrangements are parallel in one case, and in the other make with each other an angle of 90° ,—the plate of crystal being fixed, for the most favourable case (that is, in the case of mica) at an angle of 45° between the original plane of polarization and that of the optical axis of the mica. The passage through absolute colourlessness takes place when the polarizing plates form an angle of 45° . That special shade of colour which, when the angular distance is again halved, occurs at $22^\circ.5$ in the case of one and at $67^\circ.5$ in the case of the other colour, is almost exactly halfway between the full degree of colour and absolute want of it, being, however, somewhat nearer the latter. When the rotation of one polarizing arrangement is from 90° to 180° , the complementary colour in a corresponding manner passes through want of colour at 135° , again into the original colour.

(4) On rotating the plate of crystal in its plane, when the principal sections of the Nicols are parallel, only one thermal colour occurs; when they are at right angles, the complementary colour; while when they form an angle of 45° , both thermal colours appear.

(a) In the first case the complete colour appears when the plane of the axes is arranged at 45° and 135° to the principal sections of the Nicols, and colourlessness at 0° (that is, when the three planes coincide), at 90° , and 180° . The shade of colour which occurs at the intermediate angles $22^\circ.5$, $67^\circ.5$, $112^\circ.5$, and $157^\circ.5$ is much nearer the point of colourlessness than the transition colour on rotating the polarizing arrangement.

(b) In the second case the full degree of saturation of the complementary colour is unaltered. The colour is only interrupted by the disappearance of the rays themselves, which ensues when the plane of the axes of the crystal is arranged at 0° , 90° , and 180° .

(c) In the third case the thermal colour mentioned under (a) occurs at its maximum when these planes are at $67^\circ.5$ and $157^\circ.5$. The complementary colour at $22^\circ.5$ and $112^\circ.5$ is observed in only one shade of colour, which is just as near colourlessness as that shade of the first colour mentioned in (a), and which occurs at $22^\circ.5$, $67^\circ.5$, $112^\circ.5$, and $157^\circ.5$. Colourlessness occurs when the crystals are in the positions 0° , 45° , 90° , 135° , and 180° .

(5) The interference-colour of heat can also be observed if, as in the case of mica, the substance of the doubly refracting crystal has a special absorptive power for certain thermal colours which increases with the thickness. A new field is thus opened for thermochroism in this observation.

(6) The phenomena of heat and light obey in this the same general laws. The former draw attention to facts which have hitherto been little observed, or not established with equal certainty. More especially, in this case the deportment of the total heat, consisting of invisible and visible rays, does not correspond to that of the visible part.

(7) The intensity of the heat which passes through two Nicol's prisms, whose principal sections form an angle ϕ , is proportional to $\cos^2 \phi$. Hence under an angle of 45° it is half as great as in the case of parallel principal sections.

(8) On interposing a doubly refracting plate between the two Nicols, the intensity is

$$I = i[\cos^2 \alpha \cos^2 (\phi - \alpha) + \sin^2 \alpha \sin^2 (\phi - \alpha)],$$

if i represents the heat which passes when the principal sections of the Nicols and the axial plane of the crystal are parallel, α the angle between this plane and the principal section of the polarizing Nicol, ϕ that between the principal sections of the two Nicol's prisms.

Among the numerous changes which here take place we must note as characteristic points:—

When the plate of crystal is fixed and the analyzing Nicol is rotated, the intensity may considerably alter—for instance, proportionally to $\cos^2 \phi$ when the axial plane coincides with the principal section of the polarizing Nicol ($\alpha = 0$); or it may be quite constant during that rotation when the axial plane is at an angle of 45° , and when at 135° to the principal section of the polarizing Nicol.

With the Nicol fixed and the plate of crystal turned,

(a) The principal sections being parallel, the position of the axial planes at $0^\circ, 90^\circ, 180^\circ$ produces the intensity i ; placing them at $22^\circ.5, 67^\circ.5, 112^\circ.5, 157^\circ.5$ $\frac{3}{4}i$; while at 45° and 135° we have $\frac{1}{2}i$.

(b) If the principal sections are at right angles, the first arrangement of the crystal, at $0^\circ, 90^\circ, 180^\circ$, transmits no heat; that at $22^\circ.5, 67^\circ.5, 112^\circ.5, 157^\circ.5$ $\frac{1}{4}i$; the last, at 45° and 135° , again transmits $\frac{1}{2}i$.

(c) The principal sections being at 45° , when the planes of the axes are at $0^\circ, 45^\circ, 90^\circ, 135^\circ, 180^\circ$, the intensity $\frac{1}{2}i$ is obtained; at $22^\circ.5, 112^\circ.5, \frac{3}{4}i$; and at $67^\circ.5$ and $157^\circ.5, \frac{1}{4}i$.

(9) All these phenomena of the colours and intensities of interfering heat-rays (which hold good even in detail) confirm the assumption of transverse waves of various length, by which the phenomena are explained.—Poggendorff's *Annalen*, May 1867.

Fig. 1.

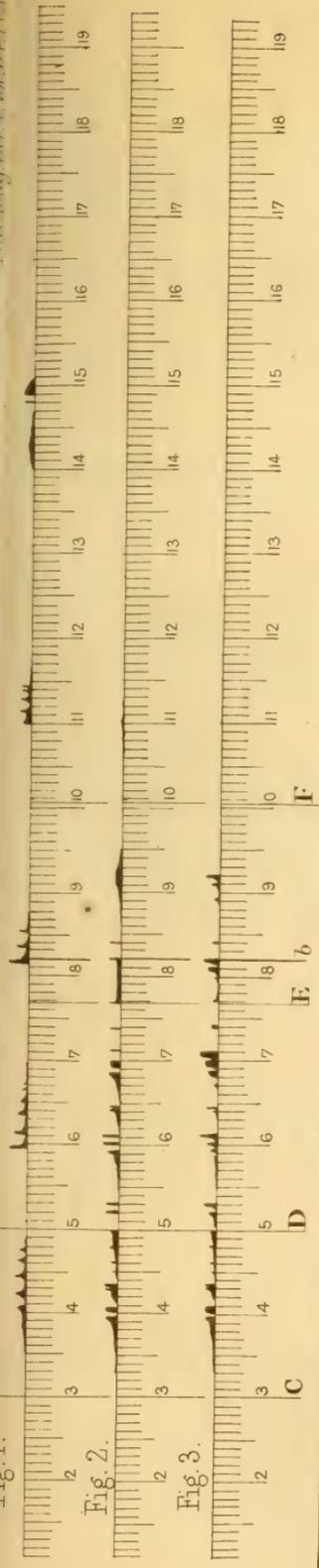
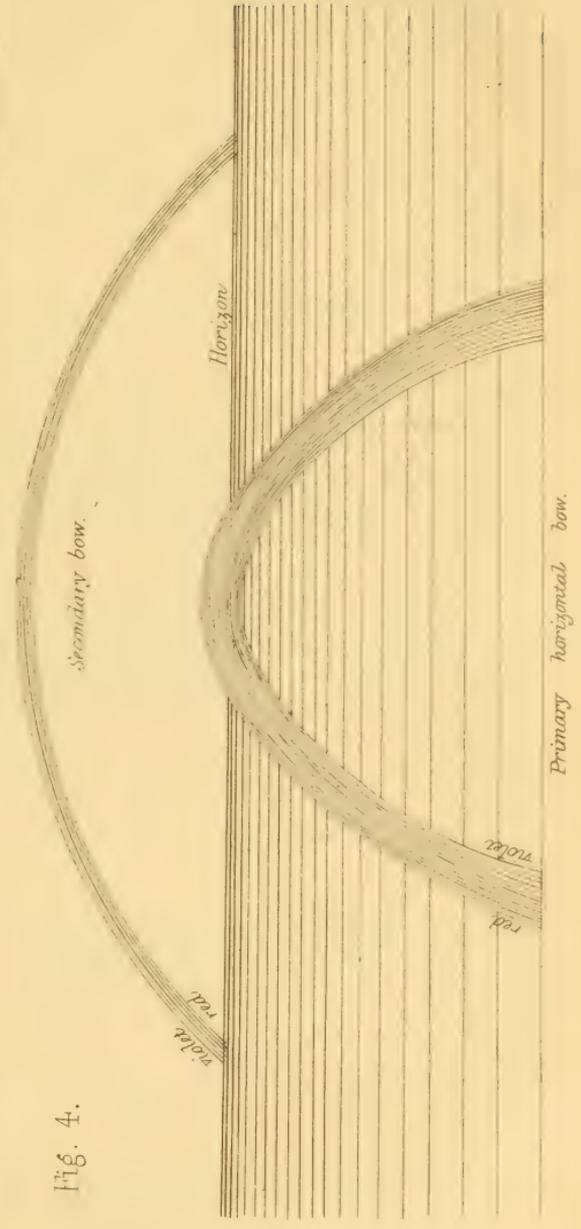
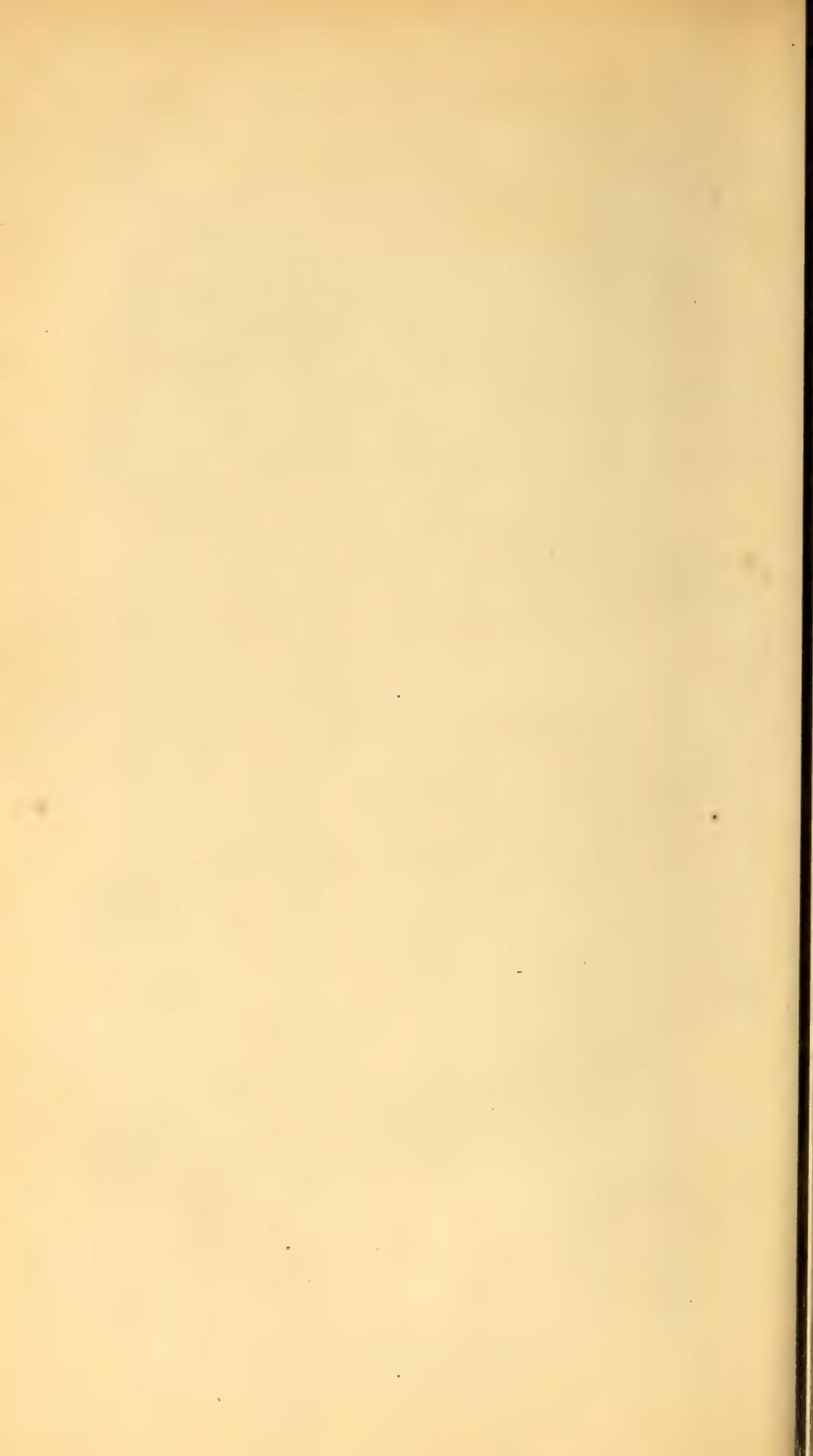


Fig. 4.





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LXV. *On the Temperature of the Flames of Carbonic Oxide and Hydrogen.* By R. BUNSEN*.

[With a Plate.]

WHEN a mixture of a combustible gas with oxygen is inflamed, an elevation of temperature occurs, which can be calculated from the heat of combustion of the gas and from the specific heats of the products of combustion, for every case in which the combustion of the mixture at the prevailing temperature is complete.

Given a mixture of h parts by weight of hydrogen, o parts by weight of oxygen, and n parts by weight of nitrogen; then (provided the combustion be complete, and oxygen be present in excess) hw units of heats will be produced, and the following products of combustion formed,

$$\frac{H+O}{H} h,$$

$$o - \frac{O}{H} h,$$

where H and O represent the atomic weights of hydrogen and oxygen, and w the heat of combustion of hydrogen. If we call the specific heat of these products of combustion by the series $\sigma_w, \sigma_o, \sigma_n$, then we have

$$t_1 = \frac{wh}{\left(\frac{H+O}{H}\right)h\sigma + \left(o - \frac{O}{H}h\right)\sigma_o + n\sigma_n} \dots (1)$$

This equation is simplified, as remarked, on the supposition that the mixture of gases h can be completely burned at the high

* From Poggendorff's *Annalen*, vol. cxxxi. p. 161; communicated by Professor Roscoe.

temperature t_1 . On the other hand, if only hx burn by the explosion and therefore $h(1-x)$ remain unburnt, then the equation is altered to

$$t_1 = \frac{wxh}{\left(\frac{H+O}{H}\right)hx\sigma_w + \left(o - \frac{O}{H}hx\right)\sigma_o + (1-x)h\sigma_h + n\sigma_n}, \quad (2)$$

where σ_h represents the specific heat at t_1 of the residual unburnt hydrogen. If we have

$$h\left(\frac{H+O}{H}\sigma_w - \frac{O}{H}\sigma_o - \sigma_h\right) = C,$$

$$h\sigma_h + o\sigma_o + n\sigma_n = D;$$

so we get

$$t_1 = \frac{hw x}{Cx + D}, \quad \dots \dots \dots (3)$$

$$x = \frac{t_1 D}{hw - t_1 C}. \quad \dots \dots \dots (4)$$

From the pressure P_1 to which the exploding mixture at the temperature t_1 is exposed, we obtain for t_1 (by the help of Mariotte and Gay-Lussac's laws) the further equation

$$(1 + \alpha t_1)PS = (1 + \alpha t)P_1S_1, \quad \dots \dots \dots (5)$$

in which α signifies the coefficient of expansion of the gases, t and P the temperature and pressure of the mixture at the closing of the exploding vessel, and S and S_1 the specific weight of the mixture before and after the combustion.

If we take the specific weights of the gases in the mixture and in the products of combustion, representing the specific heat of the same by the symbols s_w, s_o, s_n, s_h , and the total weight of the mixture of gases by G , then

$$S = \frac{G}{\frac{h}{s_h} + \frac{O}{s_o} + \frac{n}{s_n}},$$

$$S_1 = \frac{G}{\frac{1}{s_w} \left(\frac{H+O}{H}\right)hx + \frac{1}{s_o} \left(o - \frac{O}{H}hx\right) + \frac{1}{s_h} (1-x)h + \frac{1}{s_n} n};$$

or, more simply,

$$h \left(\frac{H+O}{Hs_w} - \frac{O}{Hs_o} - \frac{1}{s_h} \right) = A,$$

$$\frac{h}{s_h} + \frac{o}{s_o} + \frac{n}{s_n} = B,$$

$$S = \frac{G}{B},$$

$$S_1 = \frac{G}{Ax+B} \dots \dots \dots (6)$$

If we now substitute in equation (6) the value of x found in equation (4), and the value of S_1 in equation (5), so we have, when

$$P_1(1 + \alpha t) = Q,$$

$$t_1 = -\frac{E}{2} \pm \sqrt{F + \frac{1}{4}E^2},$$

where

$$E = \frac{AD + B\alpha hw - BC + \frac{CGQ}{PS}}{\alpha(AD - BC)}$$

$$F = \frac{\left(\frac{QG}{PS} - B \right) hw}{\alpha(AD - BC)}$$

The one of these values of t_1 is greater than that got from equation (1), and has therefore no importance for this problem. When the temperature of combustion t_1 is found, then we get from equation (4) the value of hx , i. e. the quantity by weight of hydrogen which was burned at the moment when the maximum temperature of the flame was reached; and consequently also the value of $h(1-x)$, i. e. the quantity by weight of hydrogen which had lost the power of burning at the high temperature t_1 .

All the numerical values are known, with the exception of P_1 , for mixtures of carbonic oxide or hydrogen with oxygen and nitrogen, which serve in the equations for the determination of t_1 and x . For mixtures of gases of this composition it is therefore only necessary to determine by experiment the value of P_1 .

The measurement of the pressure caused by an explosive mixture at the moment of maximum temperature can be approximately determined when the exploding-vessel is closed air-tight with a plate loaded with weights, by means of which the exact weight can be observed which can still be raised by the exploding gases. Such measures of pressure are only reliable within certain limits. For if an explosive mixture be diluted with a

gas which takes no part in the combustion, the celerity of the ignition becomes more and more diminished, until, when the mixture is on the point of burning, the progress of the ignition can be followed with the naked eye. If therefore the combustion is excessively slow, owing to the presence of foreign gases, the measurement of pressure is rendered completely erroneous, since the first portion of gas which is ignited is cooled down again before the remainder has become heated. In order to remove this source of error, it is necessary to avoid too great a dilution with foreign gases, to have the diameter of the column of gas to be burned not too great, and to cause the inflammation of the gases to be as instantaneous as possible. This last is best accomplished by sending a very powerful spark from a large Ruhmkorff's coil-machine through the entire length of the column of gases. The exploding-vessel (fig. 1, Plate IV.) which I employed was made of exceedingly thick glass, and contained a column of gas 8.15 centims. in height and 1.7 centim. in diameter, so that the entire length which the combustion had to travel was 8.5 millims., the distance from the path of the spark to the glass walls. The shorter the time in which the maximum temperature is reached in proportion to the duration of the cooling, the less is this source of error to be feared. It is necessary here to say a word on the celerity with which the propagation of the ignition takes place. This is estimated as follows: the explosive mixture is allowed to burn from a fine orifice of known diameter, and the rate of the current of gas carefully regulated by diminishing the pressure, to the point at which the flame passes back through the orifice and ignites the gas below it. This passing back of the flame occurs when the velocity with which the gaseous mixture issues from the orifice is inappreciably less than the velocity with which the inflammation of the upper layers of burning gas is propagated to the lower and unignited layers. It is then only necessary to measure directly the rate of the current of incombustible gas at the pressure under which the flame went back, in order to obtain with approximate exactness the rate of propagation of the ignition to which this rate of the current is equal. If we call the rate of propagation c , the diameter of the orifice in the thin plate from which the explosive gas issues d , and the volume of gas which streams in t seconds under each pressure from the orifice V , then

$$c = \frac{4V}{\pi d^2 t}.$$

An experiment with pure hydrogen explosive mixture gave the following numbers:—

$V = 963000$ cubic millims.; $d = 1.2$ millim.; $t = 25$ seconds.

The rate of propagation of the ignition in pure hydrogen mixture is therefore 34 metres in the second*.

It is to be supposed that the true rate found by the experiment is somewhat too high, owing to the cooling which the stream of gas is subjected to at the mouth of the exit-opening, and which makes the desired value too small.

Since the distance between the path of the spark and the sides of the glass vessel (constructed of the dimensions given above) is only 8.5 millims, we may conclude

That the time which the whole of the gas takes to burn completely, and therefore also to reach the maximum temperature, is not more than $\frac{1}{4000}$ part of a second.

The light of the spark was examined by means of a disk whose rate of rotation is known, and provided with radiating segments. Looking through this at a white surface illuminated by the light from the exploding gases, the duration of the illumination is found to be $\frac{1}{63}$ of a second.

Hence, whilst the maximum temperature is reached at the very most in $\frac{1}{4000}$ of a second, the temperature during $\frac{1}{63}$ of a second appears to fall so little that the flame still produces a dazzling light.

It is therefore clear that, in the case of pure detonating gas, a constant error in measuring the pressure is not to be feared. These relations are less favourable in the case of the detonating mixture with carbonic oxide, which possesses the very low rate of not quite 1 metre in the second, but, on the other hand, seems to cool very slowly indeed. Notwithstanding this, satisfactory results can be obtained by measurements of pressure of this gas, since we can scarcely doubt that the agreement between results with this gas and those with hydrogen does not depend simply on chance. The measurements of pressure were made in the following way:—The mixture of gases to be exploded is collected over mercury in the exploding-vessel, fig. 1, and filled up to a definite point denoted by a file-mark; the level of mercury in the vessel is then made equal to that in the trough, and the vessel is closed with the covering-plate, fig. 2. This consists of a thick disk of mirror-glass with polished surface. To this a second, iron plate is cemented, from which an iron pin passes for the reception of the spark through a hole in the glass plate.

* From these numbers we see that the velocities which are observed on the sun's surface in the case of certain progressive luminous effects occurring in given times, are altogether of a higher order than the velocities with which chemical combinations occur in gaseous mixtures. The hypotheses according to which these solar phenomena are explained by chemical processes must therefore undergo considerable limitation.

The glass plate is covered with a little olive-oil and then brought under the exploding-vessel, to the polished mouth of which it is made to adhere by gentle rubbing. The vessel thus closed air-tight is brought out of the trough, provided with the little glass ring, fig. 3, and arranged as seen in fig. 4. This glass ring, which is filled with water, is placed water-tight on the caoutchouc washer attached to the exploding-vessel. For the measurement of the pressure itself, the lever arrangement represented in fig. 4 is used. The fixed weight (*a*) serves to balance the longer arm of the lever, along whose divisions the weight (*b*) can be moved so as to be able to vary at pleasure the pressure with which the vessel is closed. The iron plate of the exploding-vessel, on which the rod (*d*) from the lever presses, is provided with a slight hollow at the centre, into which the conical point of *d* can be placed at all times in the same position. *c* is a strip of tinfoil which is in contact with a platinum wire sealed air-tight through the bottom of the exploding-vessel; and this wire is in contact with the mercury contained in the vessel. In order, then, to pass a spark through the whole column of gas, it is only necessary to connect one wire from the coil with the metal lever, the other with the tinfoil. When the pressure caused by the burning of the gas is less than that exerted by the lever-arm on the glass plate, the gas burns without any distinctly audible noise, and without setting the water in the glass funnel in motion; if the contrary is the case, the water is forcibly thrown out and a loud explosion occurs. The degrees of pressure by which, on the one hand, quiet burning, and, on the other, burning with explosion, occurs, are so distinctly marked, that the mean of a few experiments in which both points are noted may be considered to be the pressure sought. This is shown in the following experiments:—

Cyanogen with air : pressure, in atmospheres	{ 11·01 quiet burning, 10·90 loud explosion.
Carbonic oxide with air: pressure, in atmospheres	{ 7·34 quiet burning, 7·22 loud explosion.
Carbonic oxide with oxygen : pressure, in atmospheres	{ 10·20 quiet burning, 10·04 loud explosion.
Hydrogen with oxygen: pressure, in atmospheres	{ 9·56 quiet burning, 9·46 loud explosion.

It is scarcely necessary to say that the pressure to which the gases were subjected when the exploding-vessel was closed must be added to that exerted on the lever-arm. There is, besides, another correction to be made for the adhesion of the glass plate to the vessel. The pressure which this exerts must be overcome by the explosion, and it must therefore be added to that measured on the lever. The force of the adhesion, as is well known, is to

a great extent dependent on the time during which the pressure works. In order, then, to measure this force under the same circumstances as when the experiment is going on, the following plan was adopted. The pan of a balance was fastened by means of string to the adhering glass plate, and weights were placed on it by degrees until the point was reached at which, when the explosion-vessel was quickly raised, the adhering plate was removed without any marked raising of the balance-pan. The weight at which this occurred was then added to that measured on the lever-arm. The uncertainty which attends a correction made in the way described does not, however, affect the accuracy of the method, since the weight for this correction is only a very small proportion of the entire pressure which is measured. The exactness of measurements conducted in this way is only small; but, as the following experiments show, it is still great enough to show the remarkable laws which govern the combustion of gases. By a series of experiments the volume of gas in the exploding-tube was found to be 18.5 cubic centims. The cylindrical flame produced was 8.15 centims. in height and 1.7 centim. in diameter. For the first and second of the following seven experiments, a mixture was used of h parts by weight of carbonic oxide with o parts by weight of oxygen, which exactly serves for complete combustion. For the experiments 3 to 7 the same mixture was used with successive additions of n parts oxygen. The pressures P and P_1 are, in these and in all subsequent experiments; given in atmospheres.

	Exp. 1.	Exp. 2.	Exp. 3.	Exp. 4.	Exp. 5.	Exp. 6.	Exp. 7.
h	1.0000	1.0000	1.0000	1.0000	1.0000	1.0000	1.0000
o	0.5715	0.5715	0.5715	0.5715	0.5715	0.5715	0.5715
n	0.0000	0.0000	0.1850	1.4664	1.8620	2.9390	3.6960
P	0.9934	0.9804	0.9590	0.9658	0.9699	0.9763	0.9768
P_1	10.1197	10.5600	8.6940	8.1662	7.6057	6.5130	5.6867
$t^\circ \text{C.}$	4.5	10.0	10.7	8.5	8.8	8.6	10.0

The following values were found for the constants entering into the calculations:—

$$\begin{array}{lll}
 \sigma_h = 0.1753 & s_h = 0.9674 & w = 2403 \\
 \sigma_o = 0.1547 & s_o = 1.1056 & \alpha = 0.00366 \\
 \sigma_n = 0.1547 & s_n = 1.1056 & H = 14.0000 \\
 \sigma_w = 0.1702 & s_w = 1.5202 & O = 8.0000
 \end{array}$$

In the following experiments 8 and 9, instead of excess of oxygen, the mixture of 1 part by weight of carbonic oxide with 0.5715 part by weight of oxygen was mixed with increasing

excess of n parts by weight of carbonic oxide; and in experiments 10 and 11, instead of this last an excess of nitrogen was added:—

	Exp. 8.	Exp. 9.	Exp. 10.	Exp. 11.
h	1·0000	1·0000	1·0000	1·0009
o	0·5715	0·5715	0·5715	0·5715
n	1·0285	4·7444	1·8920	1·8920
P	0·9851	0·9791	0·9934	0·9802
P_1	8 7602	4 7008	7 2760	7 5720
$t^\circ \text{C.}$	8·0	7·7	4·5	10·0

For experiments 8 and 9,

$$\sigma_n = 0\cdot1753,$$

$$s_n = 0\cdot9674;$$

and for experiment 10,

$$\sigma_n = 0\cdot1717,$$

$$s_n = 0\cdot9713.$$

Of the following experiments 12 and 13 were made with pure electrolytic detonating gas, 14 with the same gases and nitrogen:—

	Experiment 12.	Experiment 13.	Experiment 14.
h	1·0000	1·0000	1·0000
o	8·0000	8·0000	8·0000
n	0·0000	0·0000	26·5000
P	0·9736	0·9736	7·9684
P_1	9·7300	9·5140	7·2512
$t^\circ \text{C.}$	5·0	10·0	10·0

The constants occurring in the equations used for calculating these experiments are—

$$\sigma_h = 2\cdot3910$$

$$s_h = 0\cdot06927$$

$$w = 29629$$

$$\sigma_o = 0\cdot1547$$

$$s_o = 1\cdot1056$$

$$\alpha = 0\cdot00366$$

$$\sigma_n = 0\cdot1717$$

$$s_n = 0\cdot9713$$

$$H = 1\cdot00000$$

$$\sigma_w = 0\cdot3621$$

$$s_w = 0\cdot62207$$

$$O = 8\cdot00000$$

The next Table contains the calculations of several experiments arranged according to the maximum temperature $t_1 - t$ which each gas-mixture reduced to 0° will reach when burned in a closed vessel*. Column III. gives the maximum temperatures for the mixtures mentioned in columns I. and II.

* The diminution which the heat of combustion suffers from the small loss of temperature (t) is omitted from the calculation as being inappreciable.

Numbers of the experiments.	I.	II.	III. $t_1 - t_2$	IV. x .	V. Mean.	VI. Variation from mean.				
2.	vol. CO	0.0000 vol.	3172° C.	0.351	0.3316	+0.0194				
	vol. O									
1.	vol. CO	0.0000 vol.	2893 "	0.319		0.3316	-0.0126			
	vol. O									
12.	vol. H	0.0000 vol.	2854 "	0.338			0.3316	+0.0064		
	vol. O									
13.	vol. H	0.0000 vol.	2833 "	0.336				0.3316	+0.0044	
	vol. O									
3.	vol. CO	0.1079 vol. O	2558 "	0.314					0.3316	-0.0176
	vol. O									
4.	vol. CO	0.6857 vol. CO	2471 "	0.460	0.3316					-0.0421
	vol. O									
8.	vol. CO	0.8554 vol. O	2325 "	0.478		0.3316				-0.0241
	vol. O									
5.	vol. CO	1.0861 vol. O	2117 "	0.490			0.3316			-0.0121
	vol. O									
11.	vol. CO	1.2563 vol. N	2084 "	0.515				0.5021		+0.0129
	vol. O									
14.	vol. H	1.2599 vol. N	2024 "	0.547					0.5021	+0.0449
	vol. O									
10.	vol. CO	1.2563 vol. N	1909 "	0.470	0.5021					-0.0321
	vol. O									
6.	vol. CO	1.7145 vol. O	1726 "	0.520		0.5021				+0.0179
	vol. O									
7.	vol. CO	2.1559 vol. O	1460 "	0.512			0.5021			+0.0099
	vol. O									
9.	vol. CO	3.1629 vol. CO	1146 "	0.527				0.5021		+0.0249
	vol. O									

From column III. we conclude—

(1) That a mixture of carbonic oxide and oxygen in the proper proportions is heated by combustion in a closed vessel from 0° C. to (mean of experiments 1 and 2)

3033° C.

In these experiments the variation of the single experiments from the mean was only 4.58 per cent. of the whole increase of temperature.

(2) That a mixture of hydrogen and oxygen in the proper proportions is heated by combustion in a closed vessel from 0° C. to (mean of experiments 12 and 13)

2844° C.

In these experiments the variation of the single experiments from the mean was only 0.388 per cent. of the whole increase in temperature.

(3) That a mixture of carbonic oxide and air in the proper proportions is heated by combustion in a closed vessel from 0° C. to (mean of experiments 10 and 11)

1997° C.,

with a variation in the single experiments of 4.41 per cent. of the whole increase of temperature.

(4) That a mixture of hydrogen with atmospheric air in the proper proportions (experiment 14) is heated by combustion in a closed vessel from 0° C. to

2024° C.

Columns IV. and V. show the very remarkable fact—

(5) That whether the mixture be pure carbonic oxide and oxygen, or hydrogen and oxygen, during the maximum temperature $t_1 - t$ (mean of experiments 1, 2, 3, 12, and 13) almost exactly one-third of the hydrogen or carbonic oxide present is burned, since

$$x = \frac{1}{3.015},$$

whilst the remaining two-thirds have lost the power of combining even when heated from 2558° C. to 3033° C.

(6) That when 1 volume of the same explosive mixtures is diluted with from 0.686 to 3.163 volumes of an incombustible gas, and in consequence the temperature of the flame sinks from 2471° C. to 1146° C., at all temperatures within these intervals almost exactly the half of the carbonic oxide or hydrogen is burned. The mean of all experiments gave

$$x = \frac{1}{1.992}.$$

In the remaining halves oxygen and carbonic oxide, or oxygen and hydrogen, had lost the power of combining with each other.

The products of combustion of a mixture of pure carbonic oxide and oxygen at 3033° C. consist of

Oxygen . . .	1 vol.
Carbonic oxide .	2 vols.
Carbonic acid .	1 vol.

In like manner, the detonating mixture of hydrogen and oxygen at 2844° C. gave

Oxygen . . .	1 vol.
Hydrogen . . .	2 vols.
Water-vapour .	1 vol.

Between the temperatures 2471° C. and 1146° C. a mixture of carbonic oxide and oxygen forms the following products of combustion :—

Oxygen . . .	1 vol.
Carbonic oxide .	2 vols.
Carbonic acid .	2 vols.

and the hydrogen and oxygen mixture at 2024° C.,

Oxygen . . . : 1 vol.
Hydrogen . . . : 2 vols.
Water-vapour . . : 2 vols.

The graphic representation A (fig. 5) derived from columns III. and IV. (in which the abscissæ represent the temperatures, and the ordinates the volumes of combustible gas obtained at these temperatures from 10 vols. of hydrogen or carbonic oxide) shows that the portion of the gas rendered incombustible by the gradually increasing temperature does not vary proportionally with the increase of temperature, but is represented by a broken line. The curve B (fig. 5) represents in the abscissæ-lines the temperature of the flame of the carbonic oxide detonating mixture, calculated upon the volumes of excess of gas added to 1 vol. of the mixture corresponding to the vertical ordinates.

Where two experiments were made with the same mixture of gases, the mean value is given. In curve B a turning-point is shown corresponding to the broken part of curve A; and this represents the temperature at which the portion of the gas undergoing combustion changes from one-third to one-half. When the result of these experiments is considered, it is clear that the phenomena of the combustion of gases depend upon circumstances totally different from those which were formerly supposed to regulate these actions.

When carbonic oxide and oxygen are exploded, and the temperature of the mixture in consequence raised from 0° to 3033° C., two-thirds of the carbonic oxide present remains in an unconsumed and incombustible condition, and the temperature 3033° C. is lowered by radiation and conduction to 2558° C. without any of the two-thirds carbonic oxide being burnt. If, however, the temperature sinks below this, a second burning begins, which prevents a further loss of heat by radiation and conduction, and restores the temperature to 2558° C., beyond which point it does not rise. There follows then a constant abstraction of heat from the temperature of 3033° C., which produces a steady temperature of 2558° C., until exactly the half of the carbonic oxide is burned; a third change now takes place by the cooling of the inflamed mixture to 1146° C.; and after this no combustion occurs. Since the gaseous mixture on cooling consists entirely of carbonic acid, these changes must be repeated at temperatures under 1146° C. until the last portion of the gas is burned. If carbonic acid is decomposed by slowly increased temperature into carbonic oxide and oxygen, in the same way as it is formed from a mixture of carbonic oxide and oxygen when losing temperature, a series of specific-gravity-determinations must give the

following values, according to the temperature at which each estimation was conducted* :—

At 0°	1.5202
Between 1146° and 2558° .	1.2162
Between 2558° and 3033° .	1.1402

In consequence of the decomposition of water-vapour, the specific gravity of the mixture would be found to be :

At 150°	0.6221
At 1874°	0.4977
At 2024°	0.4666

The discontinuous combustion of a uniform mixture of combustible gas with oxygen belongs to a class of phenomena which fall under a law pointed out by me in another place †, which is expressed as follows :—

That the quantities of compounds which, under favourable circumstances, form simultaneously in a perfectly uniform gaseous mixture, bear simple atomic relations to each other; and that these simple atomic relations undergo sudden alterations by the gradual addition of a third body, which does not affect the homogeneous nature of the mixture.

In order to explain this law more clearly, I here append some of the observations on which it is based, and which have been already published ‡.

If a light is applied to a uniform mixture of hydrogen, carbonic oxide, and oxygen which contains less oxygen than is necessary for the combustion of one of the other two gases, three results are possible; viz., the oxygen may burn only with the one, or only with the other, or with both together. Experiment shows that both burn; but it is also seen that the quantities of the two gases present in excess which the oxygen selects for combustion stand in a simple atomic relation to each other, and that these quantities of gas united to the oxygen do not regularly increase by gradual increase of one part of the mixture, but by intervals they spring suddenly from one simple atomic relation to another. The oxygen divides itself between the excess of hydrogen and carbonic oxide in the proportion represented by the following atomic numbers of the products of combustion :—



* When the effect produced by the slight deviation from Mariotte and Gay-Lussac's laws is neglected.

† Liebig's *Annalen*, vol. lxxxv. p. 137.

‡ Bunsen's 'Gasometry,' p. 256 &c.

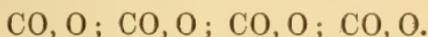
As up to the present time it has not been possible to prepare any single compound of carbonic acid and water, we are compelled to assume that even between the uncombined atoms certain predominant attractions, corresponding to simple atomic relations, are at work, but that we are unable to repeat the conditions under which the compounds corresponding to these relations are formed; so that the chemical attractions have the power of arranging the neighbouring atoms (whether they afterwards take part in the combination or not, and even before any combination of the system occurs) in simple atomic proportions, according to which combination can occur more easily than in any other. Bearing this in mind, and comparing it with what happens in the case of the combustion of gases, we find that we have to do with similar phenomena. If we suppose a number of carbonic-oxide and oxygen atoms,



to be exposed to a constantly increasing temperature, then a point must come when the first pair CO, O are united together and CO^2 produced. The result of the force between the atoms



must now be quite different from the original,



It may be that the two atoms of carbonic acid cause the neighbouring atoms to become carbonic acid between the temperature $3033^\circ C.$ and $2558^\circ C.$ If, then, the same action is repeated throughout the whole mass of the gas, only one-third of the gas can be burned to carbonic acid, as in fact the experiment shows. Why this attractive influence should be exerted under the existing conditions only on three pairs of atoms and no more, can as little be theoretically explained as the fact that it is possible to estimate *à priori* the number of atoms contained in any substance already well known. In order to conceive why under the temperature of $2558^\circ C.$ only one-half of the entire gas can be burned, we may suppose that the formation of a further atom of carbonic acid is again possible, and, as before, calls forth a new equilibrium of the forces which produces the union of another group of atoms,



in which the results of all the forces present, acting within the still lower temperature $2471^\circ C.$ to the lowest $1146^\circ C.$, cannot induce the formation of any more carbonic acid.

This explanation finds an important confirmation in the fact that not only the intervals of temperature between which the

different phases of combustion occur, but also the temperatures of ignition, depend upon the attraction which all the groups of neighbouring atoms, and therefore even those which take no part in the compound produced, exert upon each other. Every one knows that phosphorus begins first to unite with pure oxygen at from 20° C. to 30° C. to form phosphorous acid, that a slight addition of different hydrocarbons raises the temperature at which combination begins to above 30° C., and, lastly, that by an admixture of nitrogen the temperature at which the phosphorus begins to emit light is lowered much below 20° C.

It is scarcely necessary to remark here that the method which has been described for the estimation of the temperature of the flames of explosive gas-mixtures is applicable to those alone in which only two elements enter into combustion, since we are not in a position to indicate in more complicated cases which of the different possible products of combustion has been formed during the successive phases of the combustion. On the other hand, by the aid of the subjoined formula we can calculate the temperature t_1 of the flames of explosive mixtures in which only two elements are burned, when first we determine by experiment, once for all, the coefficient $\frac{m}{n}$, made up of small whole numbers, which represents the fraction of the whole gas-mixture which is combustible within certain intervals of temperature,

$$t_1 = \frac{\frac{m}{n} H w}{\sigma_2 \frac{m}{n} (H + O) + \sigma_1 \left(1 - \frac{m}{n}\right) H + \sigma_0 \left(1 - \frac{m}{n}\right) O + \sigma_q q}$$

In this formula the roman capital letters represent the atomic weights of the gases burned together, σ_0 , σ_1 , σ_2 , σ_q the specific heats of the single gases, and q the quantity by weight of any incombustible gas which has been added, expressed in terms of the unit employed in H and O.

LXVI. *On the Solar Eclipse, August 1868.*

By G. JOHNSTONE STONEY, M.A., F.R.S.*

IN May last I sent a memoir to the Royal Society on the Constitution of the Atmospheres of the Sun and Stars, which contained, in an Appendix, suggestions relative to observations of much interest which might be made during the eclipse of

* From the Monthly Notices of the Royal Astronomical Society, December 13, 1867.

August 1868, and for which there will not occur so good an opportunity for several years afterwards. An abstract of this communication has been published in the Proceedings of the Royal Society, and reprinted in the Philosophical Magazine; but the memoir and its appendix are not yet in type. As so great a delay has occurred, I fear lest the suggestions to which I have referred may not appear in time to be of use, and I therefore venture to submit them to the Royal Astronomical Society.

In the memoir it is shown that the gases which constitute the solar atmosphere range to unequal heights, and that the order in which the outer boundaries of those which are known succeed one another is probably the following:—hydrogen extending furthest; then, in order after it, sodium and magnesium; calcium; chromium, manganese, iron, cobalt, and nickel; and lastly, copper, zinc, and barium. These, and whatever other gases exist in the sun's atmosphere, intercept all light coming from beyond them of the refrangibilities which correspond to their spectral rays, and substitute for it the feebler light which emanates from their own upper strata. In this way they produce those dark lines in the spectrum which are so familiar, and the various intensities of which depend in great measure upon the different temperatures of the upper layers of the gases in which they have their origin.

Now, if the corona which is seen during a total eclipse is caused, as we must presume, by the sun's enormous outer atmosphere projecting beyond the disk of the moon, the light which reaches us from it is probably in part borrowed light, and in part due to the atmosphere being itself self-luminous. Borrowed light, however it may originate*, will give a spectrum resembling the sun's, whereas any light emitted by the solar atmosphere in virtue of its being incandescent will consist of bright rays in the same positions as some of Fraunhofer's lines. Accordingly, when examined through a spectroscope adapted to an equatorial telescope †, much of the light of the corona may be found to resolve

* Borrowed light may come either from a mist of solid or liquid particles in the sun's atmosphere, or because some of the gases are sensibly coloured, or from irregular refractions occasioned by the troubled condition of the sun's atmosphere, but will not in any considerable degree arise from the illumination of the earth's atmosphere. It is from the absence of this last and chief source of borrowed light that the corona comes into view, and observations during an eclipse are so precious.

† The equatorial stand is not essential, as it would be enough to provide a side reflector with a slit in it, through which the rays to be observed in the spectroscope shall pass; and the rest of the image being viewed in the reflector by an assistant, he could by hand movements sufficiently direct the telescope. A telescope of considerable aperture and focal length would be best.

itself into a multitude of bright lines, the brightest being coincident with the faintest of Fraunhofer's lines. If this should prove to be the case, and if the observer could train himself to distinguish in the hurry and under such novel circumstances* the lines of the different gases, it would even be possible to ascertain how high in the sun's atmosphere each reaches, by using a curved slit, and noting the moment at which each set of lines is obliterated by the advancing moon. This would be a determination of exceeding interest. The observations should commence immediately after the beginning of totality and be kept up to the end of it, as it is only from situations close to the sun's disk that the brightest lines can come.

Directly outside the photosphere there lies a stratum of the sun's atmosphere which is still hotter than the photosphere; and on the outer boundary of this hot region there appears to be a shell of excessively faint cloud, part of which is to be seen in Mr. De la Rue's photographs of the eclipse of 1860. It probably extends the whole way round the sun. It is therefore very desirable that this faint shell, which seems to lie at a distance of 8 or 10 seconds of space from the edge of the sun's disk, should be observed, both from a central station and from stations close to the northern and southern limits of totality, so as to ascertain whether, as we have reason to presume, it is continuous round the disk. For this purpose telescopes of moderate power, and any way mounted, would suffice.

It is not likely that there will be many spots; but if any should present themselves upon the edge of the disk within a week of the eclipse, they should be observed for some days before and after, with a view to learning whether they are related to ascending clouds in the way pointed out in the memoir. These observations could probably be best made at Kew; but to guard against bad weather it might be advisable to associate with Kew some other observatory. Possibly there may be records in existence for instituting this inquiry in regard to the eclipse of 1842, 1851, or 1860. It is perhaps not impossible that the heavy mass of cloud marked gg^1 in Mr. De la Rue's diagram of the eclipse of 1860 may have been the source of a cyclone which occasioned the neighbouring group of spots. If so, the cloud must have afterwards drifted a good deal towards the pole (which is the direction in which the probable course of the solar trade-wind would have carried it), and also come through the rotation of the sun into a position in which it was much foreshortened.

* One of the most novel of these circumstances is that the spectrum of each gas will present itself with inverted intensity, the brightest lines obtained by artificial means being in general those which are dimmest in the corona, and *vice versa*.

Finally, it is evident from the observations of the eclipse of 1860, that flame-like protuberances are formed of various materials. Probably they are most of them very attenuated mists of solid or liquid particles; but it is also possible that there may be some in a gaseous state. It is, in fact, shown in the above-mentioned memoir that if two of the gases of the solar atmosphere are kept asunder by the temperatures which prevail in its low-lying strata, but can unite into a compound gas under the diminished temperature and pressure which prevail at greater heights, or *vice versa*, the gas which in such cases presents itself only at great altitudes will comport itself, in many respects, like a cloud, and in particular will become intensely heated, and emit the rays which constitute its gaseous spectrum. A similar state of things would result from such a change in the spectrum emitted by a constituent of the sun's atmosphere as we know from Plücker's experiments can take place in nitrogen and other gases. It is desirable, therefore, that the spectra of the protuberances should be examined, if this prove practicable, in order to determine whether they all resemble the solar spectrum, as they must if these clouds are all mists, or some of them consist of bright lines, as they will if gaseous. For such observations a telescope whose motions are under the control of an assistant looking in sideways would probably be best. I think, however, that the persons who are willing to devote themselves to such delicate observations would do well to prepare for them by staying for some time beforehand and up to the commencement of totality in the subdued light of a darkened chamber.

Shortly after the time of the solar eclipse the morning zodiacal light will be visible in great splendour to the members of the expedition; and it is very much to be desired that the opportunity should not be lost of obtaining a careful spectral examination of it by experts. This would not require any telescope*; but a binocular spectroscope, consisting of two similar instruments placed parallel to one another, would double the light†.

* That is, provided the collimator is sufficiently long to render the angular aperture of its lens, viewed from the slit, not more than the angular breadth of the zodiacal light at the part of it observed. Otherwise there will be advantage in placing a lens, which need not be achromatic, in front of each collimator.

† A similar arrangement might be used with a telescope upon all objects which are not stellar—the two spectroscopes being provided with collimators of unequal length and inclined to one another so as to be directed towards two parallel slits brought as close as possible together, and the light being diverted up one of the collimators either by total reflexions, as in binocular microscopes, or by a small achromatized prism. In this way the light would be nearly doubled—an effect which could not be produced by an enlargement of the telescope. By moving the achromatized prism

LXVII. *Chemical Notices from Foreign Journals.*
By E. ATKINSON, Ph.D., F.C.S.

[Continued from p. 229.]

WE have given an account, in a former notice*, of the experiments of Fittig and others on the formation of the hydrocarbons of the aromatic series, the general result of which was to show that the higher members might be obtained from the lower by substituting radicals of the ethyle series for hydrogen. Thus toluole, $\text{C}^7 \text{H}^8$, was found to be methylphenyle, $\text{C}^6 \text{H}^5$; xylene, $\text{C}^8 \text{H}^{10}$, to be ethylphenyle, $\left. \begin{array}{l} \text{C}^6 \text{H}^5 \\ \text{C}^2 \text{H}^5 \end{array} \right\}$; and so forth.

Fittig and Ernst showed that the hydrocarbon *methylxylene*, $\left. \begin{array}{l} \text{C}^8 \text{H}^9 \\ \text{C} \text{H}^3 \end{array} \right\}$, which was obtained by treating a mixture of bromide of methyle and brominated xylene with sodium, was identical with cumole, $\text{C}^9 \text{H}^{12}$, as was evidenced by a comparative examination of some of the derivatives of the synthetically prepared body with those of the cumole extracted from coal-tar.

By the action of sodium on a mixture of bromide of ethyle and monobrominated xylene, the same chemists obtained *ethylxylene*, $\text{C}^8 \text{H}^9 (\text{C}^2 \text{H}^5)$, or, as it may also be considered, *ethyl-dimethylated benzole*, $\text{C}^6 \text{H}^3 \left\{ \begin{array}{l} \text{C} \text{H}^3 \\ \text{C} \text{H}^3 \\ \text{C}^2 \text{H}^5 \end{array} \right.$. This hydrocarbon differs from any known one of the same composition.

Fittig and Bigot† have prepared some other hydrocarbons by this process. Amyltoluole, $\text{C}^{12} \text{H}^{18} = \text{C}^6 \text{H}^4 \left\{ \begin{array}{l} \text{C} \text{H}^3 \\ \text{C}^5 \text{H}^{11} \end{array} \right.$, is obtained by the action of sodium on a mixture of bromide of amyle and brominated toluole. It is a body of pleasant odour, which has a constant boiling-point, 213° . Nitric acid acts upon it very energetically, and forms a nitro-compound, *dinitroamyl-*

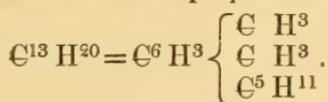
the image in one spectroscopie could be made to shift laterally; and it is likely that the position of coincidence of the two spectra would be readily seized, even with extremely faint spectra. To secure this motion of the images with the instrument for observing the zodiacal light, the prisms of the spectroscopes might be placed a little out of the position of minimum deviation, and one of them so mounted as to admit of rotation through a small angle.

* See Phil. Mag. S. 4. vol. xxix. p. 311.

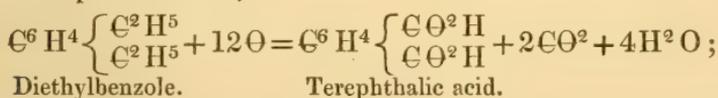
† Liebig's *Annalen*, vol. cxli. p. 161.

toluole, $\text{C}^{12} \text{H}^{16} (\text{N}\Theta^2)^2$. Oxidized with chromic acid, *amyltoluole* yields acetic acid and terephthalic acid, $\text{C}^8 \text{H}^6 \Theta^4$.

The same chemists have also prepared *amylxylole*,



Fittig and König have investigated the action of oxidation on ethylbenzole, $\text{C}^6 \text{H}^5 (\text{C}^2 \text{H}^5)$, which is isomeric with xylene, and of diethylbenzole, $\text{C}^6 \text{H}^4 \begin{cases} \text{C}^2 \text{H}^3 \\ \text{C}^2 \text{H}^3 \end{cases}$. The former, by chromic acid and also by nitric acid, yields benzoic acid, whereby it is distinguished from xylene; the latter, when oxidized by chromic acid, yields terephthalic acid,



while if nitric acid be used, the action only extends to one atom of ethyle, and there is formed a new acid which is isomeric with xylylic acid,



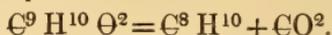
This new acid, which Fittig and König call *ethylbenzoic acid*, crystallizes in laminæ which melt at 110°C .

Under the direction of Fittig*, Ahrens and Mattheides have made an extensive series of comparative experiments on the derivatives from the xylene of coal-tar and those from methyltoluole. The result of these investigations has shown that in the nitro-compounds more especially there is a great difference, and that this difference extends to the bases which result from these nitro-compounds by reduction. The dibromo-compounds, too, which have the same formula, $\text{C}^8 \text{H}^8 \text{Br}^2$, exhibit a small but distinct difference in fusing-point. When treated with oxidizing agents, both yield identically the same products:—with dilute nitric acid, toluylic acid; and with chromic acid, terephthalic acid. It would seem that the difference of the two hydrocarbons only appears when other bodies are substituted for the hydrogen.

Fittig has described another hydrocarbon which is isomeric with xylene. Mesitylenic acid, described below (p. 510), exhibits the greatest analogy with benzoic acid, with which it is homologous. When mixed with three times its weight of caustic lime and

* *Zeitschrift für Chemie*, vol. iii. p. 523.

heated, it decomposes at a comparatively low temperature, in accordance with the equation



This new hydrocarbon, which Fittig calls isoxylol, is easily obtained quite pure. Its nitro-derivatives exhibit the greatest analogy to the nitro-derivatives of the xylol from coal-tar. Yet when isoxylol is oxidized, its behaviour is totally different. Xylol is easily oxidized by nitric acid to toluylic acid; isoxylol is scarcely, if at all, acted on by this agent; while sulphuric acid and bichromate of potassium, which rapidly oxidize xylol to terephthalic acid, slowly oxidize isoxylol to an acid identical in composition with phthalic and terephthalic acids, but quite different in properties. This acid he names *isophthalic acid*. The difference is also met with in the salts of the acid.

In order to prepare a compound which should be analogous to silicon-methyle, or ethyle, Friedel and Ladenburg* tried the action of chloride of carbon, $\text{C} \text{Cl}^4$, on zincethyle, but were as unsuccessful in obtaining definite results as had been Rieth and Beilstein before them. By acting upon zincethyle, however, with methylchloracetole †, $\text{C}^3 \text{H}^6 \text{Cl}^2$, a better result was obtained. The reaction, which does not occur at ordinary temperatures, is extremely violent when once commenced, and requires great care to control it. The residual mass, containing much zincethyle, is treated with a large quantity of water, and, after acidifying with hydrochloric acid, the supernatant liquid is removed, appropriately purified, and rectified. It boils between 85° and 90° , and was found, on analysis, to have the formula $\text{C}^7 \text{H}^{16}$; from this and

its mode of formation it must have the constitution $\text{C} \begin{cases} \text{C} \text{H}^3 \\ \text{C}^2 \text{H}^5 \\ \text{C}^2 \text{H}^5 \\ \text{C} \text{H}^3 \end{cases}$.

Rieth ‡ describes a method of preparing acetylene, in which an accident that not unfrequently happens in a Bunsen's burner, namely the striking down of the flame, is turned to account.

The light of a Bunsen's burner is caused to strike down, and then the supply of gas is increased; over the burner is suspended a glass funnel, and by means of an aspirator the products of combustion are drawn off: they are rich in acetylene; and on allowing the gases to bubble through four Woulfe's bottles containing ammoniacal solution of silver, even the last gave a copious

* *L'Institut*, No. 1716.

† *Phil. Mag.* S. 4. vol. xxix. p. 306.

‡ *Zeitschrift für Chemie*, vol. iii. p. 598.

precipitate. Indeed it was found that one aspirator was insufficient; and accordingly another aspirator and series of Woulfe's bottles were fitted on to the funnel, so that the current divided into two parts. It was thus possible to collect 100 grammes of the silver compound in twelve hours. It was found to have all the properties described by Berthelot. A series of experiments by Rieth showed that it was the ethylene gas to which the formation of acetylene was mainly due.

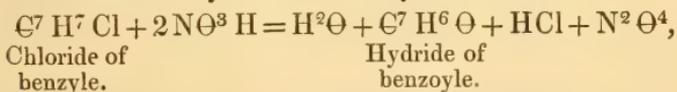
If a flask holding about 200 or 300 cub. centims. be held over such a prepared lamp for a short time, and as much concentrated solution of subchloride of copper be added as moistens the whole inside of the flask, it becomes at once sufficiently dark brown to be opaque.

Wurtz has applied Fittig's reaction to the preparation of methylallyle*, having found that it is formed in very small quantities by the action of zincmethylene on iodide of allyle:—

A mixture of iodide of methyle, $C^2 H^3 I$, and iodide of allyle, $C^3 H^5 I$, with twice the volume of dry ether, was heated with sodium in a closed tube. When the action was over, the tube was carefully opened and the gases which it contained were condensed in a tube placed in a freezing-mixture. The resultant, treated with bromine, yielded a bromide of butylene, $C^4 H^8 Br^2$, which is readily decomposed by sodium with the formation of a body which has the same composition as butylene, though probably it is only isomeric, seeing that it boils at a point lower than that assigned to butylene.

The product obtained by distilling toluene in a current of chlorine, and known as chlorinated toluole, $C^7 H^7 Cl$, is, as Cannizzaro discovered, identical with the hydrochloric ether of benzylic alcohol, $C^7 H^7 \Theta$. Lauth and Grimaux have investigated this substance†, and have described a method for its preparation by which about 90 per cent. of the toluole used is obtained without much difficulty.

When this chloride of benzyle is heated with dilute nitric acid, it is converted into hydride of benzoyle, $C^7 H^6 \Theta$,



a considerable quantity of benzoic acid being formed at the same time, owing to the further oxidation of the hydride of benzoyle. This reaction would furnish an industrial process for the

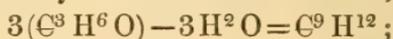
* *Comptes Rendus*, vol. lxiv. p. 1088.

† *Ibid.* vol. lxiii. p. 918.

preparation either of oil of bitter almonds or of benzoic acid, if ever these substances were required in large quantities.

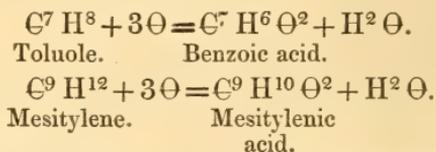
If chloride of benzyle be treated with water and freshly precipitated oxide of lead, it is converted into benzylic alcohol, $C^7 H^8 \Theta$. When chloride of benzyle is treated with phenylate of potassium, a double ether is formed, crystallizing in small nacreous scales, which have the composition $\left. \begin{array}{l} C^7 H^7 \\ C^6 H^5 \end{array} \right\} \Theta$.

The substance mesitylene, $C^9 H^{12}$, has so far an interest that it furnishes a well-marked instance of the direct formation of a substance belonging to the aromatic series of hydrocarbons from a substance belonging to the fatty-acid group; for it is obtained from acetone by the withdrawal of water,



and some chemists have on theoretical grounds been disposed to doubt whether it ought to be classed with the aromatic hydrocarbons. Fittig* has published the first part of a complete investigation of the subject. He describes the preparation of mesitylene, which is effected by heating acetone with sulphuric acid; he also prepared several of its derivatives, such as the nitro-compounds, some of which had already been prepared by other chemists. He has investigated further the action of oxidizing agents upon it—and first of all, of chromic acid. This was effected by heating mesitylene with a mixture of chromate of potassium and sulphuric acid. An acid distillate passed over which contained acetic acid; while if the oxidation was interrupted before all the mesitylene had disappeared, a small quantity of a new acid was found to exist in the oxidizing-mixture, which could not, however, be procured in a state fit for analysis.

When oxidized with nitric acid, mesitylene is converted into *mesitylenic acid*, which crystallizes from alcohol in beautiful colourless transparent monoclinic prisms, and melts at 166° . It begins to sublime before reaching its melting-point; and heated more strongly, it passes over without decomposition. It has the formula $C^9 H^{10} \Theta^2$, and bears to mesitylene the same relation that benzoic acid bears to toluole.

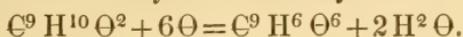


The salts it forms with the alkaline earths and sodium, silver, and lead all crystallize. When treated with nitric acid, it yields

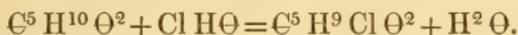
* Liebig's *Annalen*, vol. cxli. p. 129.

nitromesitylenic acid, $\text{C}^9 \text{H}^9 (\text{N}\Theta^2) \Theta^2$, which crystallizes and also furnishes crystallizable salts.

When mesitylenic acid is treated with chromic acid, it yields a new acid which Fittig calls *trimesic acid*, $\text{C}^9 \text{H}^6 \Theta^6$. This is probably the same as that which is formed in small quantity when mesitylene is directly oxidized. It crystallizes from hot water in pretty thick, colourless and transparent hard prisms. Heated in a glass tube it begins to melt at a temperature which is certainly not below 300° ; but before the entire mass is melted the substance volatilizes without change. Trimesic acid is a strong tribasic acid, which forms with barium a very characteristic salt. Its formation from mesitylenic acid may be thus written:—



Schlebusch has examined the action of hypochlorous acid on some of the fatty acids*. With valerianic acid hypochlorous acid in solution unites directly, in the manner expressed by the equation



The acid which is thus formed, chlorovalerianic acid, remains mixed with some unaltered valerianic acid, and their separation is attended with considerable difficulty; but in many cases the presence of unaltered valerianic acid is no very great objection.

When the mixture of the above acids was treated with caustic baryta, valerolactic acid ($\text{C}^5 \text{H}^{10} \Theta^3$) was obtained; and when the same mixture dissolved in absolute alcohol was saturated with ammonia, butalanine ($\text{C}^5 \text{H}^{11} \Theta^2 \text{N}$) was formed.

Schlebusch also tried the action of hypochlorous acid on palmitic acid, and finds that it takes place in accordance with the equation



It would thus seem that as we go higher in the series of fatty acids the facility of substitution increases.

Bayer has investigated† the two products of condensation of acetone—oxide of mesityle and phorone. They are most easily prepared by saturating acetone with hydrochloric acid gas, leaving the mixture for some days, and then treating it carefully with alcoholic potash: a mixture of the two is liberated, and they are separated by fractional distillation,—oxide of mesityle boiling at 130° , and phorone at 196° .

When oxide of mesityle, $\text{C}^6 \text{H}^{10} \Theta$, is treated with perchloride

* Liebig's *Annalen*, vol. cxli. p. 322.

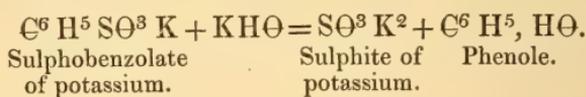
† Ibid. vol. cxl. p. 297

of phosphorus, it is converted into bichloride, $C^6 H^{10} Cl^2$. This is a dense oil, which by the action of caustic potash is converted into a chloride, $C^6 H^9 Cl$.

When oxide of mesityle is treated with sodium-amalgam it is reduced, and there is formed some *mesitic ether*, $C^6 H^{11} \left. \begin{array}{l} C^6 H^{11} \\ C^6 H^{11} \end{array} \right\} \Theta$. This is a colourless liquid boiling at 206° , and with a strong odour of camphor. It is decomposed partially by distillation, giving some products of condensation.

What has hitherto been known as phorone has been an impure product. It is best prepared by rectifying that part of the original product which passes between 180° and 205° . It crystallizes in long friable prisms of greenish-yellow colour, and with an odour like geranium. Treated with perchloride of phosphorus this body yields chloride of phoryle, $C^9 H^{13} Cl$, which, treated in turn with potash, regenerates phorone.

Simultaneously, but quite independently of each other, Wurtz and Kekulé have described a reaction by which the aromatic hydrocarbons may be converted into their corresponding phenoles*. When sulphobenzolic acid, or one of its salts, is fused with caustic potash, it splits into sulphurous acid (which combines with the alkali) and the corresponding phenole. The decomposition is best effected at a temperature of from 250° to not exceeding $300^\circ C.$; and the residual mass on being treated with hydrochloric acid liberates quantities of sulphurous acid, while phenole floats on the acid solution. It is readily obtained pure, and its quantity is almost the theoretical amount. The decomposition may be thus written :



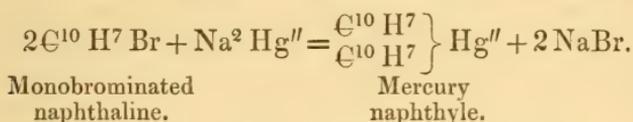
Wurtz has also found that sulphonaphthalic acid undergoes a similar change, yielding sulphite of potassium, and probably naphthylic alcohol.

Kekulé has observed that the sulpho-acids of phenole by similar treatment undergo the same reaction and furnish derivatives of benzole in which two atoms of hydrogen are replaced by two of hydroxyle. One of the two isomeric modifications of sulphophenylic acid passes into pyrocatechine, and the other into resorcine—



* *Comptes Rendus*, vol. lxiv. pp. 748 & 752.

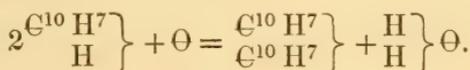
Otto and Morries describe the formation of a mercury compound of naphthaline. When to monobrominated naphthaline dissolved in benzole sodium amalgam is added, a violent reaction ensues; the bromine is taken away, but, instead of the two rests, $\text{C}^{10} \text{H}^7$, uniting together to form a dinaphthyle analogous to dibenzyle and ditolyle, mercury takes its place and a mercury naphthyle is formed, in accordance with the following equation:



Mercury-naphthyle is easily prepared in large quantities. It forms colourless needles which are readily soluble in hot benzole, but very little so in alcohol, and not at all in water. They melt at 248° . Iodine unites directly with it, forming a compound crystallizing in beautiful satin scales.

Lossen* has made a very exhaustive examination of the products of oxidation of naphthaline. By the action of a boiling solution of permanganate of potassium it yields phthalic acid, $\text{C}^8 \text{H}^6 \text{O}^4$. Laurent had stated that by the action of a mixture of sulphuric acid and bichromate of potassium, a new acid, *naphthesic acid*, was formed, to which he assigned the formula $\text{C}^{10} \text{H}^6 \text{O}^4$. Lossen has ascertained that this substance is really the anhydride of phthalic acid, $\text{C}^8 \text{H}^6 \text{O}^3$, and that this substance and phthalic acid are the main products of the oxidation of naphthaline by the above agents.

A mixture of manganic oxide and sulphuric acid yields principally phthalic acid; but there is formed at the same time some quantity of the body *dinaphthyle*, $\text{C}^{20} \text{H}^{14}$, which is produced from naphthaline by direct oxidation,



This body melts at 154°C ., is quite volatile, and boils at a temperature higher than that of boiling mercury. From solution in ether-alcohol it crystallizes in regular octahedra. It forms with bromine *dibromodinaphthyle*, $\text{C}^{20} \text{H}^{12} \text{Br}^2$, a compound of great stability, and when bromine is in great excess *hexabromodinaphthyle*, $\text{C}^{20} \text{H}^8 \text{Br}^6$. By the action of strong nitric acid on *naphthyle*, $\text{C}^{20} \text{H}^{10} (\text{N}\Theta^2)^4$, tetranitrodinaphthyle is formed.

* Liebig's *Annalen*, October 1867.

LXVIII. *On the Action of Sunlight on Glass.*

By THOMAS GAFFIELD*.

THE great attention now given to all the phenomena connected with light and heat may awaken some interest in the experiments in which I have been engaged for the past four years on the subject named at the head of this article. Perhaps I cannot better commence my essay than by quoting, from the 'Proceedings of the Nat. Hist. Society of Boston' (vol. ix. p. 347), an account given before that Society of my experiments in 1863, and after I had been engaged in them only a few months.

"He believed that his experiments in connexion with the subject were original as to their method and their extent, although it had long been observed in Europe that colourless or light-coloured plate glass had turned to a purple hue by exposure to intense sunlight. One case is cited † of a change to a gold colour; and one experiment recorded by Dr. Faraday ‡, some forty years ago, proving that a light purple changed to a darker hue after eight months' exposure.

"Other experiments are on record showing the action of glass of different colours as media in the transmission of light and of heat, but none, with the above exception, showing the effect produced on the glass itself.

"An experience of some twenty years in the window-glass business had only presented a few isolated cases of supposed change of colour from this cause, which were attributed to some obvious defect in an article of inferior manufacture; but within a short time he had heard of the change of colour in an article of superior manufacture, in a quantity of white plate glass, of which some lights had been broken out of a window in which they had been exposed to the sun.

"This fact coming to his knowledge led him to try an experiment with several specimens of plate, crown, and sheet glass during the month of July last, which proved that a month's exposure to a hot sun would change the best white French plate and all white sheet glass, such as is used for photographs and engravings, to a colour containing more or less of a yellow hue. The dark green and dark blue or bluish green did not experience any change; but any hue which approached a white, whether bluish, greenish, or yellowish white, turned to a yellowish colour.

"A second series of experiments, commenced in July, and continued three months on some thirty specimens from France, England, Belgium, Germany, and the United States, only con-

* From Silliman's American Journal for September and November 1867.

† Journal of the Society of Arts for February 15, 1854.

‡ Chemical Researches. London, 1859, p. 142.

firmed the results of the first ; and a daily examination at first, and afterwards from week to week and month to month, revealed the interesting fact that even after a single day's exposure to a July sun the change of colour will, in some instances of the lightest hues, commence.

“So remarkable was the change in a week, affecting nearly all the light-coloured glasses, that he commenced a third experiment on the 6th of August which should speak for itself. He then exhibited to the Society ten pieces of French white plate glass, 4 by 2 inches in size (all of which were cut from the same sheet), one of which showed the original colourless glass, and the others exhibiting the change of hue towards yellow, after exposure respectively of one, two, and four days, one, two, and three weeks, one, two, and three months.

“The changes in the first four days were slight ; but the last specimens were so yellow as to exhibit a contrast very marked, and excited the interest of all the members present. That the colour permeates the body of the glass and is not confined to the surface, or produced by reflexion therefrom, has been conclusively proved by grinding off about one-sixteenth of an inch from both surfaces and the four edges of a duplicate exposed specimen which, after repolishing, still exhibited the same yellow colour.

“The glasses exposed were all what are called colourless window-glasses, although they varied in tinge and hue from the whitest French plate to the darkest-green English sheet glass.

“An experiment for four months, from July to November, on really *coloured* glasses, red, green, yellow, blue, and purple, showed no change except in the purple, which became slightly darker.

“The experiments were carried on upon a rough plate-glass roof nearly horizontal, and which received the rays of the sun during the greater part of the day. In all cases strips corresponding to those exposed, and cut off from the same pieces, were placed in the dark to be compared with the other specimens after exposure.

“It will be noticed that the dark-green, blue, and bluish-green did not change. The colour of the Belgian sheet (called German or French by glass-dealers in America), a yellowish or brownish green, did not change ; and these were the only exceptions. All plate glasses changed, except an inferior blue quality and a superior crystal plate of a greenish colour, made in Germany, and at the only factory which has not given up the use of potash for soda-ash.

“It is possible that a longer exposure, of a year, or of years, might change every colour in some degree.

“His inquiries, since he instituted these experiments, have brought out some fine specimens of Belgian sheet glass from a house built three years ago, which had changed in some instances to a golden and in others to the well-known purple hue.

“It is his intention to pursue the experiments further, with a view to ascertain the effects of sunlight during each month and season of the year—and also whether exposure to heat, air, or moisture alone, out of the direct action of the sun’s rays, will produce any corresponding change.

“Mr. Gaffield does not propound any theory to explain these changes of colour, which under our sunny skies probably take place much more rapidly than in the different and less clear atmosphere of England.

“Some writers point to the presence of oxide of manganese in the original composition of window-glass, and some to the oxide of iron, as a chief cause.

“Some writers have peculiar theories about the different classes of the sun’s rays. Some may think the change referred to a molecular or chemical one; and others, wiser than the rest, refrain from any explanation, waiting for a larger multiplication of experiments and a greater accumulation of facts before educating any satisfactory law of nature which governs these curious and interesting phenomena.

“Mr. Gaffield makes no pretensions to any discoveries, unless it be to the very rapid change in glass observed in our climate in July, but only gives the result of his experiments in the hope that the great interest now manifested in the subjects of light and heat may lead others to examine the matter, to repeat the same experiments in other countries, and to give the world the result of their researches, and enable the learned and scientific men of the age to explain this remarkable power and action of the sun’s rays.

“It should be remembered that he submitted his specimens to the most severe tests, by placing them where they received reflected as well as transmitted light and heat. The change in glass when glazed in the windows of our dwellings and stores is so much more gradual that it very rarely attracts the attention of observers, except in the marked variation from white to purple.”

In accordance with the intention above expressed, I have continued my experiments on this interesting subject, and under different heads will now give some account of their method and results.

My first experiment was with pieces of glass 4 by 6 inches placed in a sash 6 by 60 inches long, in the grooves of which

the specimens were placed, the sash being fastened together by wooden pins, and placed on a nearly horizontal rough plate-glass roof which received the direct rays of the sun during the greater part of the day. In my next experiment I placed some of the specimens directly on the rough plate glass. These were carried on in summer and autumn. It was necessary to make a different arrangement for winter, when the fall of snow and the formation of ice might interfere with the full success of the experiments. I concluded that the best size for specimens was 4 by 2 inches; and I made some wooden boxes about $\frac{3}{8}$ inch deep, $4\frac{1}{4}$ inches wide, and of a length to fit the sills of windows facing the south in the upper story of a Boston house. One of the windows was three-sided, looking east, south, and west. In these boxes (painted white) my specimens were placed in a nearly horizontal position, side by side, and (after the loss of a few pieces blown away by hurricanes and squalls) were secured in their positions by cords of twine or slight copper wire fastened at each end. I have arranged boxes on the roof in front of this window also, some being of greater depth, in which to place pieces of plate and rough plate about 1 inch in thickness. All of these boxes are provided with covers, which are placed over the glass and fastened by buttons on the occurrence at any time of a snow-storm. At all other times the glass is exposed. Holes in proper places in the boxes allow the rain which falls to pass off easily. Every piece of glass is carefully marked by a diamond in an upper corner with its name, and, if necessary, with some abbreviation descriptive of the experiment to which it belongs. This precaution is necessary to prevent mistakes when a comparison is to be made of several kinds and colours, of exposed and unexposed specimens. In all cases, where an experiment has to be made with any description of glass, the pieces (4 x 2 inches in size) must all be cut from the same sheet, as there is frequently a slight difference in the shade of sheets from the same factory, arising from the difficulty of having the materials in their manufacture, and the circumstances attending the melting, blowing, or casting precisely the same. Two pieces or more should be laid aside to show the original colour, and to compare with the others, and thus to show the changes produced by exposure to sunlight for days, weeks, months, or years. At the commencement of an experiment, say, for instance, the exposure of white plate glass for from one to twelve months, a neat paper box is provided, $4\frac{1}{4}$ inches long, $2\frac{1}{8}$ inches deep, and wide enough to hold fourteen pieces (two of the original colour and twelve exposed specimens), and a description of the contents is marked on its cover on an adhesive label. At the end of each month a piece is withdrawn from exposure, carefully cleaned,

and marked either with a diamond or by an adhesive label, and placed in the box. As "order is heaven's first law," it is peculiarly necessary in all observations upon the actions of heaven's brightest luminary. By observing the above directions much time will be saved, and at the conclusion of an experiment everything is in shape for exhibition to friends at home, or students at a lecture-room.

Of course a *perfect* arrangement could only be made when a perfectly flat roof or platform in an open field could be provided and the sunlight could act with full force during every hour and minute of the day. But mine was sufficiently near this point to show very interesting results.

In one of my earliest experiments I kept a record of the changes going on in the various kinds of glass, at first from day to day, and afterwards from week to week and month to month. The following is a specimen:—

Kinds of glass.	Original colour before exposure.	Colour after one day's exposure.	Here were inserted other columns to show effect of exposure for 2 and 4 days, for 1, 2, and 3 weeks, for 1 and 2 months.	Colour at end of experiment (3 months).
French white plate. English crown glass. French white sheet. Belgian sheet. American sheet.				

I might give the names of all the different kinds of glass which I have exposed to sunlight; but I refrain because, for the reason given previously, I have found different specimens from the same manufactory of plate, of crown, and of sheet glass sometimes to differ in shade, sometimes in result of exposure, and sometimes in both.

I will, however, give below (not naming the particular manufactories) a general description of my experiments in 1863, when an exposure of thirty-three specimens for a few months in summer and autumn showed the following results.

The first Table shows that twenty-four kinds were found to be easily affected, and exhibits the time at which the change in colour was first observed. The second Table shows nine kinds which did not change in colour during the same time.

TABLE of Glasses easily changed.

Kinds of glass.	Original colour before exposure.
5 of French white plate.	White, with light bluish-green tinge.
2 of German "	" " "
1 of English "	" " "
5 of English plate.	Yellowish green.
1 of Belgian rough plate.	White, with slight green tinge.
3 of French white sheet.	White, with light bluish-green tinge.
1 of Belgian "	White, with light yellowish-green tinge.
4 of English "	{ One light yellowish-green, and three light bluish-green tinge.
2 of American "	White, with light blue-green tinge.
—	
24	

The time at which change of colour was first observed.									Colour after exposure.
Days.			Weeks.			Months.			
1	2	4	1	2	3	1	2	3	
1	...	4	Yellow or yellowish green.
1	1	" " "
1	" " "
	4	1	Deeper yellow tinge.
	...	1	Yellowish-green tinge.
3	" " "
1	" " "
	1	...	1	1	...	1	" " "
1	...	1	" " "
8	5	6	1	3	...	1			

TABLE of Glasses not changed in three months.

Kinds of glass.	Original colour of glass.
1 kind of German crystal plate.	Light green.
1 " German plate.	Dark blue.
1 " English rough plate.	Dark green.
1 " English crown.	Light green.
1 " American "	Bluish green.
1 " American sheet.	Dark bluish green.
1 " English "	Dark green.
1 " Belgian "	Yellowish or brownish green.
1 " French "	" " "
—	
9	

Subsequent experiments with five of these kinds (all which I could conveniently obtain) showed that an exposure of a year, or even less, would change all but an ordinary kind of American sheet, which was of a dark bluish-green colour.

The experiments which speak for themselves are the most satisfactory ones—that is to say, where one has not only the record made at the end of each month, but a piece of glass taken in and laid aside at the same time to show the actual colour produced, and the truth of the records. It is very interesting to witness any one of these series of specimens showing, as in one of white plate, a gradual change (commencing in a day or a few days in summer) from greenish or bluish white to a yellowish white, or light yellow, a deep and deeper yellow, until it becomes a dark yellow or a gold colour,—and in some Belgian sheet specimens a gradual change (commencing in a few weeks in summer) from brownish yellow to deeper yellow, yellowish pink, pink, dark pink, purple, and deep purple.

There are several kinds of glass in which no perceptible change took place in three months, which were very sensibly affected by an exposure of a year.

Experiments.—I have given a general account of my first experiments in 1863, and a portion of the Tables kept in my journal at that time. I might have given names and results in full, and shown the actual effects and shades of colour produced by exposure for a few months on some thirty kinds of glass. But in my case, as in many novel and original investigations, the results of first experiments, and the theories based upon them, were modified by subsequent ones. I supposed that many kinds of glass not changed in three months would not change at all, that all which changed would take a yellowish colour, unless by exposure of many years. that no colour but some shade of yellow or pink would ever be produced in any kind by exposure to sunlight.

The experiments of 1864 and the two following years proved to me that nearly every kind of window-glass I had exposed could be changed in one year, that a rose or pink colour (or some tint approaching them) could be produced in various kinds in a few months, and that some kinds of greenish-white glass would, after exposure, assume a bluish tint or bluish white.

It may seem singular for one who has been a glass dealer and manufacturer like ourselves, thus to advertise what may be called a defect in his own wares. It might seem unkind to other manufacturers to expose the defects of their productions literally before the light of day. But my scruples were all removed when I noticed, in a late communication of Pelouze (see *Comptes Rendus*, January 14, 1867), the following statement:—“ I do

not believe that there exists in commerce a single species of glass that does not change its shade in the sunlight." As all manufacturers are in the same category, it will do no harm for me to repeat, as the result of my experiments, what Pelouze says, and to affirm that a longer or shorter exposure to the direct action of the sun's rays will probably change in some degree the colour of all, or nearly all, kinds of window-glass.

I subjoin an account of an experiment carried on for one year with nine different kinds of glass. These represent plate, crown, and cylinder glass, the manufactures of both hemispheres and almost every shade and colour of what are called colourless glasses.

I name only the nationalities, and not the particular manufactories of the glasses in any of my Tables. I have this information recorded in my journal, and shall be happy to communicate it to any who may desire it.

Memorandum of nine different kinds of Glass exposed from January 12, 1866, to January 12, 1867.

Kind of glass.	Colour before exposure.	Colour after exposure.
French white plate.	Bluish white.	Yellowish colour.
German crystal plate.	Light green.	Bluish tinge.
English plate.	" "	Yellowish green.
English crown.	" "	Light purplish colour.
Belgian sheet.	Brownish yellow.	Deep " "
English sheet.	Dark green.	Brownish green.
American crystal sheet.	Light bluish white.	Purplish white.
" "	Lighter bluish white.	Light yellowish green.
American ordinary "	Bluish green.	No change.

The colours named above are given from an observation of the glass edgewise, by which one can see a body of colour 2 or 4 inches in depth, whereas the usual thickness of the glass varies from $\frac{1}{4}$ to $\frac{1}{2}$ of an inch, and shows its colour easily only by placing a white curtain or paper behind it.

I have tried several experiments showing the effect of sunlight during each month and each season of the year. At the end of the year, by the comparative depth of yellow or purple colour produced in the various specimens, one can see the comparative actinic power of the rays during each month and season. The results proved that the actinic effect increased from January to July, and decreased after that month. The greatest effect during any season was observed in the summer, the least in winter; and that in spring and autumn was about alike, and midway between that of summer and winter.

Crystal or lead glass and a piece of optical glass, having pro-
Phil. Mag. S. 4. No. 233. *Suppl.* Vol. 34. 2 M

bably very little, if any, manganese, changed not in two years. Perhaps a longer exposure may produce some change.

A rough piece of light-coloured window-glass metal changed to a yellowish colour in a year.

Coloured glasses after two or three years' exposure showed no perceptible change in any instance, except a slight one in a single specimen of purple. Perhaps an exposure of many years may make a change in some other colours.

I have made experiments with artificial heat on glass in various ways, from exposure to the heat of a cooking-range oven to that of a glass-stainer's kiln, without any change of colour in the common colourless window-glasses, while the same or similar specimens exposed to sunlight have been nearly all changed in a few months.

Specimens exposed in hot water for a month, indoors and out of sunlight, experienced no change of colour; while similar ones exposed during the same length of time in the bottom of a dish filled with two or three inches of water out of doors, and to the direct rays of the sun, experienced a decided change, though only about half as much as when directly exposed out of the water.

Being convinced that air, moisture, and artificial heat do not cause any change of colour, our experiments indicate that the change is effected by the actinic rays of the sun alone.

This actinic effect is cut off in some degree by every medium, by water as stated above, and even by clear glass, as a specimen exposed inside of a window or under another piece of thin colourless glass shows only about one-half as much change as that exposed outside of the window or with no covering of glass over it. The amount cut off by colourless glass and by coloured glass differs greatly with the difference of colour.

The comparative power of glass of different kinds to transmit the actinic rays I have tested by placing underneath pieces of each kind, pieces of easily changing glass (white plate or Belgian sheet glass), exposing them one year, and noticing at the end of that period the comparative depth of the yellow or pink colour to which the under pieces had changed. The result of my experiments proved that the most easily transmissive of the colourless glasses were the English crown, French plate, two kinds of white crystal sheet made in Massachusetts (from the celebrated Berkshire white sand), the New Jersey sheet glass, one kind of English plate, and one kind of Belgian sheet, and about in the order in which I have named them.

Of the coloured glasses, the blue transmitted the most, the purple less, the red and orange the least—the glasses under these two and the yellow and green showing little or no change.

This last experiment proves the propriety of the preference given by photographers to blue glass for skylights, because it transmits the blue rays, which exert the most actinic power. But it may be added, that a colourless white glass, or bluish white (if one which will not change by sunlight to a yellow or rose-colour, owing to the presence of manganese, or any other cause), is especially good, as it will transmit all the rays, and among them the actinic or blue ones. In proportion as any kind changes to a yellow or rose-colour, it will lose its power of transmission and its value as photographic glass. I have seen specimens of the two kinds of white crystal sheet made in Massachusetts, before alluded to, which answered the demands of photographic artists. Of foreign glass, I have noticed a fine bluish-white sheet, made lately without manganese, from a certain excellent manufactory in Belgium, and one kind of English crown glass.

Should plate glass be required, the most permanently enduring, or least likely to assume a yellow colour, are a superior kind of white plate made by the French and Belgian Plate-Glass Companies, and an excellent quality of German crystal plate made at a long-established factory in Hanover.

I desire to say here, however, that it is not the place where any glass is made which determines its good character, but the actual constituent materials and the superiority of its manufacture.

Manufacturers are frequently changing their mixture or "batch;" so that any results given with one set of samples might differ from those made with another set from the same manufacturers. For this reason, in noticing any differences which may occur in experiments made by any of our readers, this fact should be considered as an explaining cause.

I have seen specimens of glass from a factory which changed to a yellowish tinge in a few months, others which changed to a purplish hue, and still others from the same factory which hardly changed at all. A difference in the mixture (or batch, as it is termed) makes a difference in the tinge of the specimens from the same factory, both before and after exposure to sunlight. The chief points for photographers are to get glass made from as pure materials as possible, of as light a colour as practicable, and free from oxide of manganese. A glass like either of those named above as most easily transmitting the actinic rays might be good for one year or more, and then become very much injured for photographic effects by the change of colour to yellow or pink by sunlight.

Any photographer can make these observations practical by testing the action of sunlight for six months, or a year, on all

the specimens offered him for sale. And all manufacturers can make them practical by making their glass of pure materials, which will not have to be "doctored," to use the glass-makers' term for the use of manganese; or by allowing the glass to assume its natural colour, even if it be a little blue or green, rather than to run the risk of its subsequent change to yellow or purple by exposure to sunlight.

In the *Comptes Rendus* for January 14, 1867, Pelouze says (and we believe he is the first and only writer who has made this observation):—

"Exposure to red heat decolorizes the glasses which have been made yellow by sunlight; or, to speak more exactly, they retake the light-green shade which they had before exposure. A second exposure to sunlight produces a second coloration similar to the first; and a red heat makes it disappear again. These phenomena can be reproduced indefinitely. The glass preserves its transparency and does not give place to any striæ or bubbles." He also says:—

"I possess specimens of glass rendered violet by sunlight. All present the property of being decolorized by heat. A temperature of 350° is not sufficient. It is necessary to have that employed in the reheating of glass in general; and that is in the vicinity of red heat. The glass decolorized by heat, when exposed to sunlight, retakes the amethyst colour which it acquired the first time, loses it anew when it is heated; and these curious phenomena can be reproduced without cessation."

In confirmation of this most interesting statement of Pelouze, I have exposed in a glass-stainer's kiln several specimens of glass which had been changed by the action of sunlight, some to a yellow and some to a purple colour. The exposure to an extreme red heat made the glass assume, some a white, some a yellowish white, and some a green colour, which were probably the original colours. These specimens were taken from windows where they had been exposed from a few years to more than half a century. Further experiments, which I have already commenced, will show whether we can reproduce the exact original colours by heat after they have been changed by exposure to sunlight.

We have in the same kiln exposed some dozen original and unexposed specimens of what are called colourless window-glasses of different kinds and shades of colour, and found them unchanged in the slightest degree by the action of great heat, while similar specimens have been changed in a few days, weeks, or months by the simple action of the sun's rays. Fifteen specimens of really coloured glasses (red, green, yellow, &c.) have been exposed in the same way without any change of colour,

except a very slight one in a few specimens which were burnt or overheated.

In Poggendorff's *Annalen* for May 1839, is recorded the following interesting fact by A. Splittgerber :—

“ I would mention a curious fact in which the sunbeams have, if I may say so, done something in the art of penmanship, not only on the surface, but by inscribing characters through the body of the glass ; and though the matter is based upon causes well known by experience, yet there has probably never before been so striking an instance of their effect known. I am in possession of a plate of glass which was used as a window-pane for more than twenty years, and on which was an inscription in gold letters. This inscription was taken off by grinding the plate on both sides, and polishing it so as to have a new surface. When the glass had been polished, the inscription could again be clearly seen. The parts which had been under the letters remained white, while the remainder of the plate had assumed a violet tint in consequence of the manganese it contained—a colouring which permeates the whole mass, as the grinding of the surface proved. The uncovered part of the plate, especially when laid upon a white background, shows the clearly readable characters.”

The same, or a similar instance, is related by Dr. Hermann Vogel in the *Photographische Mittheilungen*, September 1866.

Desiring to produce a similar result, we made an inscription on a piece of Belgian sheet glass in part with gold- and silver-leaf, and in part with black and white paint. The gold- and silver-leaf were soon washed off, but the black- and white-painted letters remained, and being removed after an exposure of nearly two years, the words stood out in clear contrast and full proportions—the inscription being in the original colour of the glass, and the surrounding portions having been changed by the action of the sunlight to a purple colour.

A very interesting experiment can be made to show the gradually increasing effect of the sunlight on glass, by taking a piece of easily changing glass, say 4×20 inches, painting black a strip 4×2 inches at each end to preserve the original colour, and then exposing the strip to sunlight. At the end of one, two, four, six, eight, and ten months, one, two, and three years respectively, cover with black paint a strip 4×2 inches, and at the end of three years remove all the paint, and you will have in a single piece of glass the original colour and all the gradations of change effected by exposure from one to thirty-six months. I have made a similar one with Belgian sheet glass exposed nearly two years. It is one of those interesting experiments which speak for themselves and defy suspicion or contradiction.

I have made an experiment for one year with two kinds of easily changing glass out of doors, and out of the direct rays of the sun, and found that they were both slightly affected and changed towards a yellowish colour. I did not expect any change, but can perhaps properly account for it on the ground that it was the result of the action of diffused sunlight. It is barely possible that the sun may for a few minutes in some days of the year have cast some reflexions when I was not present in the dark corner in which I placed my specimens.

It may be that the action of the sun's heat produced the slight effect noticed. If so, it would be an interesting confirmation of Tyndall's experiments, and of his theory of the correlation of forces. I do not consider my single experiment entirely conclusive, and shall make others which will give us more material for proper theories and conclusions.

The experiments which I have carried on for four years embrace one specimen of optical glass, a few kinds of flint glass, and glass ware, sixteen kinds of French, Belgian, German, and English plate glass, four kinds of American, English, French, and Belgian rough plate, two of American and English crown glass, ten kinds of American, Belgian, French, and English white sheet glass, four kinds of American, Belgian, and English ordinary sheet glass, fifteen kinds and shades of English coloured glass, four of opaque, white-enamelled, and ground glass, and one piece of the rough metal of American sheet glass,—in all, about sixty varieties.

I have watched and recorded in some experiments the results from day to day, in others from month to month and season to season. I have now commenced a series in which I may record results from year to year for ten years or more. In these it may be found that specimens of what are called colourless glasses changed to a yellow colour by exposure for a year, may by much longer exposure be turned to a yellowish pink and a purple. And some which have been entirely unaffected may be affected by an exposure for ten or twenty years. Perhaps some of the coloured glasses may show signs of a change of hue or shade.

These new experiments include rough and polished plate, crown, cylinder, ground, enamelled, and coloured glass. I have also begun to expose *under* several of these kinds of glass pieces of easily changing glass, which I shall take in from year to year, these *under* pieces showing the power of these glasses above them to transmit the actinic rays.

The most easily changing glasses are a certain kind of white plate, which changes from a white to a yellowish colour, and a certain kind of Belgian sheet, which the manufacturers used to make of a brownish yellow (they now make it of a bluish or

greenish hue, and it is not so easily changed), which changes to a flesh-colour or a pinkish hue. I have accordingly taken these two kinds for my *under-glass* experiments. Under each of several kinds, to be exposed from one to ten or twenty years, I have placed pieces 4×2 inches of the white plate. I shall take in one piece at the end of the first, second, third, fourth, sixth, and tenth year. These six lights will show the increased action of the transmitted rays from year to year. By comparing the different series with each other, one can perceive the comparative actinic power of each kind of glass, or rather their comparative power of transmitting actinic rays.

Another interesting *under-experiment* is the following. I have placed under one piece of each kind of glass exposed a piece of easily changing glass, which I shall take in at the end of the year.

At the beginning respectively of the second, third, fourth, sixth, and tenth years I shall place under the same piece another strip of 4×2 -inch glass, taking in each piece at the end of the year of its exposure. This series will show the diminishing or increasing power of the glasses under which they have been exposed to transmit the actinic rays,—in other words, will show whether exposure to the sun increases or diminishes the actinic power of the glasses exposed, and renders them better or worse for photographic purposes.

I have taken a piece of 4×18 of easily changing white plate, painted with black paint two inches of each end to preserve the original colour, and exposed the piece. At the end of the year I shall paint over two inches more of the glass. At the end respectively of two, three, four, six, and ten years I shall paint over two inches more. At the end of this time, or a longer term, I shall remove all the black paint, and on one light I shall have all the grades of changed colour and shade produced by their different lengths of exposure. I shall lay aside one piece of 4×18 white plate, taken from the same sheet with the exposed light, in order to compare the original with the changed specimen.

I have painted and exposed, just in the same manner as above described, a piece of 4×16 of easily changing Belgian sheet glass.

To show a speaking proof of the painting-power of the sunlight, I have taken a piece of 4×6 Belgian sheet and covered it with a thin plate of brass having the following letters cut out of it: T. G., Jan. 1, 1867. I have taken another piece, 4×6 Belgian, and stuck on, with gum shellac, the two letters T. G. After exposure of one year or more the removal of the brass plate and letters will show in the former case rose- or purple-coloured letters on a brownish-yellow ground, and in the latter brownish-yellow letters on a rose- or purple-coloured ground.

A similar experiment to the above I have commenced with two pieces of white plate and the simple letters T. G. without the date. The result of the experiment in a year or more will be to show in one case yellowish letters on a light-coloured ground, and in the other light-coloured letters on a yellowish ground.

I have thus given as briefly as possible, and yet as fully as desirable, an account of my past and present experiments. New ones are suggested from year to year. I trust that this interesting field for observation and experiment may be worked in other countries. There is ample room for research in the application of chemical knowledge, of qualitative, quantitative, and spectral analysis, and of photogenic tests, to discover the exact action and causes of the interesting effects of the sun's rays which have here been noticed.

Theories.—The interesting phenomena of which I have given an account have given rise to many theories to account for their cause. Some attribute them to the presence of oxide of iron, and some to oxide of manganese. Exactly how the change takes place is a question on which writers differ, although it is my opinion that the precise explanation can only be given after a multiplication of experiments and a thorough examination of exposed and unexposed specimens of glass by quantitative and qualitative analysis, and perhaps by spectral analysis and observation of effects, or photogenic tests*.

We will briefly state the part which the oxides of iron and manganese play in glass-making. In almost all kinds of window-glass, and in some poorer qualities of flint glass and glass-ware, materials are used which are not perfectly and chemically pure. The sand, the carbonate or sulphate of soda, and the lime, one or all, contain slight impurities of iron, the protoxide of which gives glass a green colour. To correct this after the batch is partially melted, a little oxide of manganese, called *glass-makers' soap*, is put into the crucible or glass-pot; some of the oxygen of the manganese flies off to the iron and converts the protoxide into peroxide of iron. The peroxide gives a yellowish colour to the glass; and this, being complementary to the natural pink of the manganese, is neutralized, and the glass is thereby made of a light colour. When the sunlight acts

* Since writing the above, by the kindness of Mr. John A. Whipple (the distinguished photographer of Boston) I have been enabled to show, by the comparative darkening of sensitive paper under several exposed and unexposed specimens, the effect of exposure to sunlight for one year. The loss of actinic power, or power to transmit the actinic rays, was in proportion to the change of colour. This was in some varieties of glass quite perceptible, but in all will be more so after an exposure of several years.

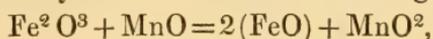
upon glass thus made, the nice equilibrium between the oxygen of the iron and the manganese is disturbed, and sometimes the yellow and sometimes the pink or purple colour is produced. I have produced all shades of the purples, running from pale lavender into the lilac, mulberry, flesh, amethyst, rose, violet, pink, and deep purple. I have produced, or seen, specimens showing all shades of the yellow, from the brownish yellow up to the brightest gold-colour; and I have several series of specimens in which the green has gradually changed into the yellow, and the yellow gradually run into the pink and purple.

Pelouze, in an article in the *Comptes Rendus* of January 14, 1867, sets forth the following theory:—

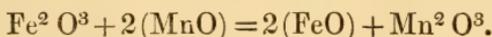
“There is in glass coloured yellow in sunlight some protoxide of iron and sulphate of soda. Light provokes between these matters a reaction, from which results peroxide of iron and sulphide of sodium. The heat brings about an inverse reaction, and reproduces sulphate of soda and protoxide of iron. From thence comes the return of the glass to its primitive colour. Analysis comes to the aid of this theory, in demonstrating in glass rendered yellow by sunlight the presence of an infinitely feeble but still very sensible proportion of a sulphide, whilst the reactions do not show the slightest trace in the same glass before its exposure.

“It may be asked why glasses coloured by the reduction of the sulphate, or the direct introduction of a sulphide into their mass, resist an equal or superior heat to that which produces the decoloration of glass become yellow in sunlight. Here is the answer. In glass made yellow at a high temperature by the reduction of sulphate, the iron is found in a state of protoxide, which cannot react in any degree upon the sulphides. Therefore the glass remains coloured. In glass made yellow by sunlight the iron is peroxide, and in consequence in a condition to change the sulphide into sulphate when we expose the glass to the action of heat.”

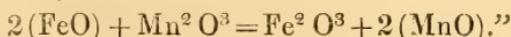
In reference to glass made violet-colour by sunlight, he says, “The coloration seems to be due to the fact that the peroxide of iron gives up a part of its oxygen to the protoxide of manganese, conformably to one of the two following equations:—



or



“The reheating of glass (that is to say, the action of a temperature of red heat) produces an inverse reaction, which explains the decoloration, in which we have



Bontemps, in an interesting article in the *Comptes Rendus* of February 4, 1867, attributes the changes in colour to the presence of oxide of manganese. He also remarks that he thinks that the violet colour occurs only in glass in which the silicates have a base of potash, and the yellow in cases where soda is used. I doubt the correctness of this opinion, as I am quite sure that I have several specimens coloured violet which contain no potash at all in their composition.

I have also specimens which are coloured both yellow and violet in the same piece—the yellow portion in one case having been produced by a certain length of exposure, and then having been covered with black paint while the violet portion was produced by longer exposure.

As before stated, I have noticed changes in what are called colourless glasses, from light colours approaching white to yellow and pink or purple. I have noticed also a change in a few specimens from a light-green to a bluish shade. The former may be accounted for by the presence of manganese, a very minute proportion of which oxide will have a sensible decolorizing effect in a crucible of melted glass-metal. Pelouze's theory of the peroxidation of the iron may have some weight in the determination of the cause of the yellow or pink colour by the action of sunlight. But I know not how to account for the change of one specimen each of plate, crown, and sheet glass in my possession, from a greenish white to a bluish tinge not mingled with either yellow or purple.

I have been pleased to find the interest taken in these experiments by photographers, who have long noticed that they could take better pictures under a newly-glazed skylight than under one which had long been exposed to sunlight. The cause of this change is that the slightest yellow colour interferes with the transmission of the actinic rays, and a very deep shade will cut them off in a very great degree. My experiments with glasses under other glasses proved which was best for photographers' use, information which all can gain by exposure of the specimens of various manufacturers which may be offered them. The most pure glasses of light-green or bluish-white colour are the best for photographers; and when I say *pure* glasses, I mean those most free from oxide of iron or manganese, but especially of the latter, which I think is the cause of nearly all the changes which I have observed. Mr. J. W. Osborne, of New York, the gentleman who has done so much to bring the art of photolithography to perfection and into practical use, writes as follows:—

“I believe your researches will prove of much practical importance, and I wish the glass-manufacturers could be got to

give serious attention to the subject. It takes but a very slight tinge of yellow to cut off twenty-five per cent. of the actinic rays. I am forced to work under glass because of the protection from the wind and weather, but in doing so I sacrifice, in any case, much of the light. I have to increase the time of exposure proportionately. With the best of glass the loss from reflexion and absorption is considerable. This may be fearfully increased by the colour; and if that undergoes a continual change for the worse, the state of things is exceedingly unpleasant. I was warned by a photographic friend in England to take care of a certain kind of cheap French glass, made for glass rooms, which turned yellow; but I had no notion that the evil was so general as you appear to have found it. The subject is of such vital importance to photographers, that I intend drawing the attention of that friend to what you have done."

It may seem strange that so long an experience in the window-glass business had not at an earlier period drawn my attention to the subject of this article. But my experience is not singular. In conversation with many glaziers and glass-dealers I have seldom found one who was aware of the great change of colour effected by sunlight. Few have supposed that specimens were to be found in any other windows in our city than those on Beacon Street facing Boston Common and the south, and exposed to the full force of the sun's rays. But having my attention now particularly directed to the subject, I never pass a window without detecting where any considerable change has been effected. I have found them in all portions of the city, and most generally in those positions which face the east and south. The colour is most easily detected when the glass has been ground or enamelled, or where a white window-curtain forms a good contrasting background. Many people suppose that the very distinctly marked purple plate glass in Beacon Street was imported of this colour, and that it is now no longer made, and hence the reason why the windows looked like checkerboards when broken lights were replaced by those of the usual light-greenish colour of plate glass.

On this point I have convinced many who had held contrary views, by showing pieces from which the putty on the edges has been removed, and displaying beneath the original colour. When the putty covered the glass, the sun's rays could not reach it, and the colour was unchanged. Such specimens are quite interesting. I have had many conversations with old glaziers, dealers and consumers of glass, and also with those who stain and enamel it. I have thus gathered specimens of various kinds and colours, and of differing lengths of exposure, from old win-

dows. The oldest specimen was one of crown glass, set in a church in Lexington, Massachusetts, in 1794, from which the windows were removed in 1846, and since used as covers for hotbeds. The original colour, found by removing the putty from the edges, was a light green; and the present, after seventy-three years' exposure, is a purple colour. I have never yet met any one who has seen glass in original imported packages of the purple colour made by exposure to sunlight; and until I do, I shall adhere to my opinion, that all purple or rose-coloured glass which is seen in our city windows was made so by said exposure. I have very fortunately found an octogenarian who has furnished me with some glass which was imported, he thinks, from some part of Germany, which is of a light-green or yellowish-green colour. The glass was imported more than thirty years ago. Much of that which has been set in his windows facing the south is now purple. An experiment with the original glass, commenced this summer, showed a perceptible change in colour in one day; and in two weeks the change towards purple was so marked that I have no doubt that this colour will be distinctly visible in less than a year. If two years were occupied in the erection of the Beacon Street houses, or any others in which the plate glass purpled by sunlight is found (as I am informed was the fact in some cases), then the result of the single experiment named above is a sufficient reason for the mistaken belief of many occupants and owners today, that the glass was purple when imported. It was probably changed from yellowish green to purple before the houses were finished and the owners had taken possession.

The action of sunlight which I have spoken of in this article must not be confounded with that called "rust" or "stain," which is occasioned by exposure to the weather, and manifests itself in two ways:—first, by a disintegration and roughening of the surface, sometimes producing all the effects of ground glass; and secondly, by an apparent formation of an infinitesimal coating of oxide on the surface, on which the play of light gives all the colours of the rainbow, as with the action of light on the infinitesimal grooves of the surface of mother-of-pearl. This is simply surface-action, whereas the action of sunlight permeates the whole body of the glass wherever the rays directly strike it.

The writer of this article (compiled in the midst of the busy duties of mercantile and official life) makes no pretension to accurate scientific knowledge, but gives the results of his observations and methodical experiments on a well-known phenomenon, in the hope that they may add some mite to the sum of human knowledge, and may stimulate and aid those who

are better versed in scientific studies to ascertain the causes and exact operations of this interesting power of the sun's rays to paint the products of art, as they do so beautifully and wonderfully the works of nature on the mountain, in the forest and field.

LXIX. *On a new Electrical Phenomenon of Motion.*

By J. C. POGGENDORFF*.

PHENOMENA of motion produced by the direct or indirect action of electrical currents have been frequently observed, and under the most varied forms; but I am not aware that mention has ever been made of that of which I am about to make a short report.

The success of the electroscopic experiments, the results of which I communicated to the Academy on the 18th of March, 1867 †, gave me occasion to observe them. Among others, I then occupied myself with the excitation of electricity on rubbing mercury against insulators. The enigmatical anomalies which were long ago observed by the older physicists, whose observations I found fully confirmed, raised the question whether they would occur in the absence of air; and I thus directed my attention to the *phosphorus mercurialis*, which in the earlier history of electricity played so interesting (I might almost say amusing) a part.

With this view I had some of them made—that is, exhausted tubes containing a certain quantity of mercury, and which, moreover, were provided either at one end or both with platinum wires for the purpose of investigating the electrical condition of the enclosed mercury.

The first tube contained only one wire. It was very luminous when shaken in the dark; and when the wire was applied to an electroscope, a strong divergence of the leaves ensued. The divergence was negative, and therefore normal.

In order to ascertain whether electrifying the tube altered this, I coated it towards both ends with a broad band of tinfoil, and placed this as an exhausted double jar across the electrodes of a Holtz's machine.

The current did not in the least alter the condition of the mercury; it was still negative. I thought, however, I observed, while the tube lay on the electrodes, a certain motion in the mercury—which, however, could have no definite character; for the current in such a jar is an alternating one.

Hence I had another tube made provided with platinum wires

* Translated from the *Berliner Monatsberichte*, June 1867.

† See page 549.

at both ends, and passed the current through the length of the tube, which, as far as possible, was placed horizontally. Here I again saw a motion of the mercury, more distinct and defined than before, but by no means so decisive as to justify me in saying it was real. The case was the same with a third, fourth, and fifth tube, though in the latter it assumed such a form as to convince me that I was not labouring under a mistake.

A *sixth* tube finally removed all doubt. This had been prepared with particular care. It had not only been most carefully exhausted, but the mercury in it had been kept briskly boiling for some time, so that when the tube was inclined it made a sharp crack in striking against the ends. The tube, moreover, was bent at right angles at a distance of about an inch from each end, so that the platinum wires were not touching the mercury; for I thought I had observed that the mobility of the mercury (probably owing to the formation of amalgam) was diminished by such a contact.

This tube, in which the mercury was extremely mobile, was hung by wire hooks to the electrodes of the machine in such a manner that the middle was perfectly horizontal. This could easily be effected by moving the electrodes, the mercury thread itself serving as level; the adjustment was altered until it remained at rest in a certain position of the tube, even when gently struck with a stick.

When this adjustment was effected the machine was set to work. As soon as the current passed through the tube the mercury moved, and travelled with considerable velocity from the negative to the positive pole. In whatever direction the current was sent the result was always the same; I never saw an exception.

Even in the fifth tube this travelling of the mercury towards the positive pole could not be mistaken; but, owing to an adhesion of the metal to the side of the tube, it often stuck, and had to be assisted by tapping: in the last tube this was not necessary. So long as the current continued, the motion was continuous and easy, and could be hindered or renewed at pleasure according as the current was opened or closed.

For this mobility of the mercury, it is necessary that when the tube is horizontal it does not occupy the entire section, which with the width taken is either not at all the case, or only so for short distances. That the tube is quite filled for a short length does not do much harm; it acts just like a cork, until the free thread has quite passed and taken it to the end; but for the most part I arranged the tube so that the mercury formed a thread or cylinder over which the current could pass.

In the experiment mentioned, this thread was 4 inches in

length. The horizontal part of the tube was a foot long, so that the thread had to move over a space of 8 inches; it took, on the average, two to three seconds, according to the intensity of the current.

As soon as the thread begins to move, it changes its shape. It becomes considerably longer. In my experiments the elongation amounted to an inch. If the current be only allowed to act intermittently, it is observed that at first the anterior end (that is, the one towards the positive end of the tube) begins to move, while the posterior one remains stationary. When the current is stopped, the posterior end draws itself after. The motion of the thread has in this case a great similarity to that of a leech.

It is also observed, during the movement of the thread, that on its surface it is in an oscillating condition; for a number of fine lines are observed, which obviously originate in the reflexion of light from so many undulating crests and hollows*.

The phenomenon of motion in question is by no means restricted to small quantities of mercury; on the contrary, small quantities of mercury do not stir, probably owing to very great adhesion to the side of the tube. If, by suitable shaking, small portions, of 2, 3, or 4 lines in length, be separated from the posterior part of the thread, on the passage of the current it is observed that these remain at rest, while the larger remainder executes its motion as before.

In the tube used, the mass of mercury set in motion amounted to an ounce. I doubt not that in a larger tube it might have amounted to a pound or more, for in my opinion the moving force is partly in the mercury itself.

For the production of this phenomenon it is not at all necessary to have an intense current or a great quantity of electricity. The current of a Holtz's machine is feeble compared with that of an induction-coil or a voltaic pile. Yet it can be materially weakened and still produce its action apparently as strong as without this enfeeblement. Before entering the exhausted tube I passed it successively through air for a distance of an inch, through a column of water 8 inches long, and through the wire

* If during the continuance of the current the tube be gently pressed between the fingers, a trembling motion is perceived, which, however, is more electrical than purely mechanical, for the tube acquires polar electricity. If during the passage of the current an electroscope be brought within half an inch, it diverges with positive electricity at the positive end, with negative at the negative end. Singularly enough, this divergence, at any rate in my machine, is not constant, but continually varying. I am inclined to ascribe this to the circumstance that the rotating disk of this machine is not quite plane, so that it is sometimes nearer and sometimes more distant from the stationary disk, by which the current necessarily undergoes a slight variation.

of an induction-coil 10,000 feet in length, without perceiving a decrease in the action. Just as little effect is produced by a shunt, if it is not too strong. The electrodes to which the tube hangs, if they terminate in points, may be brought within an inch from each other (when small sparks begin to pass) without causing the cessation of the phenomenon of motion in the tube.

Even the current of an ordinary electrical machine with a disk of only 15 inches set the mercury just as briskly in vibration as that of Holtz's machine; the motion was not, however, so continuous, but was more by starts. And it was the end of the machine joined with the conductor (that is, with the positive pole of the machine) towards which the mercurial thread moved.

I also produced the phenomenon with a Holtz's machine in conjunction with a Leyden jar. The tube was suspended with one end to the positive electrode, and with the other to an insulated brass stand to which the negative electrode was approached within about half an inch. It was interesting to observe that sometimes before a spark of the jar passed, therefore while the positive electricity flowed through the electrode in the formation of this spark, the mercury in the tube began to move towards the positive pole, or, rather, towards the more positive part of the positive electrode, and when the spark passed remained almost stationary.

An induction-current appears to act like the current from the machine, but, in spite of its great quantity, far more feebly than the latter. I have been unable to arrive at certain conclusions in the matter. In order to avoid the injurious influence which platinum wires, under the influence of the induction-current, exert upon mercury, I had made six exhausted mercurial tubes provided with aluminium wires. With the current of the machine all of them produced good effects, though not so good as the tube mentioned before; but a powerful induction-current had only a doubtful action, in spite of the fact that it filled them with a white stratified or granulated light visible in day-time, none of which appeared with the current of the machine*. In the tube which I have mentioned, and which I did not like to expose to an induction-current, with several discharges the mercury jerked every time towards the positive side.

In this phenomenon the quantity of electricity is less important than what is called intensity or tension; and it is thereby

* Aluminium wires cannot, however, be recommended for these tubes. In a few days the mercury in these tubes loses its mobility, becomes viscous, drawn out into threads, adhering in parts to the glass, and therefore incapable of showing distinctly the phenomenon of motion. Tubes with platinum wires are not exposed to this deterioration, provided they are not exposed to an induction-current.

distinguished from similar phenomena which are produced by voltaic currents in liquids, and especially from the motion of mercury in reference to which P. Erman made a communication to our Academy as long ago as 1808*, and also from those which Professor Quincke accurately examined in 1861.

I think it probable, however, that the phenomenon I have examined depends essentially on the same principle as that investigated by the last-named physicist; and I am not at all reluctant to adopt, at all events for the present, the theory he has propounded.

According to this theory, before any motion of the enclosed substance, it becomes electrified by contact with what is next to it; and the current acts on the substance thus electrified, carrying it to the positive or negative pole according as the preceding electricity was negative or positive.

By contact with glass, mercury becomes negative, and was so in fact in the tube mentioned as having acted so well. Hence there was nothing enigmatical in its transference to the positive pole. If this view be correct, the conclusion seems admissible that the transfer of mercury may occur in any quantity. I must, however, remark that in some of the tubes the mercury had a positive reaction, even before I had been able to observe a corresponding motion towards the negative pole. It may be that it was negative during the current.

* This phenomenon, which differs from ours in that the motion of the mercury is reversed (that is, from the positive to the negative pole) has been recently described by M. Daniel as new (*Phil. Mag.* vol. xxxiii. p. 482), whereas he can only claim to have first produced it by the current of the induction-coil and of the magneto-electrical machine, by which he states it is more easily produced than by the voltaic current. I have repeated part of his experiments, and have found that with the induction-current the motion of the mercury towards the negative pole is unaltered, whether spring-water or water mixed with a small quantity of alkali or of acid be used as a conducting liquid. I observed at the same time that particles which had detached themselves from the oxide formed at the anterior end of the mercurial column, moved on the surface of the mercury in the opposite direction, apparently towards the positive pole, but really towards the posterior end of the column, which must of necessity be negative. By separate openings and closings of the circuit I found that the opening current exerts a stronger action than the current on closing—which explains why, when a mechanical make and break is used even without introducing a layer of air, the motion of the mercury ensues in the direction mentioned. If only the current on opening be used, at each impulse a wave is seen proceeding on the surface of the mercury from the positive to the negative pole, which has just the appearance of the column being pushed from behind, whereas in the phenomenon in the exhausted tube the mercury appears as if attracted in front. By the current of the electrical machine, both Holtz's and the ordinary one, I did not succeed in producing any motion of mercury in liquids.

I will mention, in concluding, that the phenomenon of motion in question is accompanied by a pretty luminous appearance, which, however, is only visible in the dark.

At the negative end of the tube, as far as the platinum wire projects, a beautiful yellow fluorescent light appears in the glass, and the same at the end of the mercurial column turned towards the positive pole, which travels with it through the entire tube as far as the positive bend, which, when the mercury reaches it, also shines very brilliantly. The rest of the tube, excepting the mercury, is filled with beautiful violet but unstratified light. It is remarkable that as soon as the mercury has reached the positive bend, the yellow light on the negative leg loses its lustre considerably, but soon acquires it again when the positive electricity is conducted away. Up to a certain degree its brilliance increases, if the current, before entering the tube, has to strike across a small layer of air. All tubes do not show the yellow fluorescent light to the same extent, even if they are of the same kind of glass*.

LXX. *Notices respecting New Books.*

The Earth's Motion of Rotation, including the Theory of Precession and Nutation. By C. H. H. CHEYNE, M.A., of St. John's College, Cambridge. (Pp. 52.) London and Cambridge: Macmillan and Co. 1867.

THIS little treatise consists of two parts. In the first the author shows that the motion of a body about its centre of gravity can be determined by the following process, which is, in fact, an application of Lagrange's theory of the Variation of Arbitrary Constants. If the constants introduced into the equations for determination of the motion when the body is acted on by no forces are treated as arbitrary functions, this solution can be adapted to the case in which the body is acted on by "disturbing" forces. The author shows that the differential coefficients of these functions with respect to the time can be expressed in terms of a single function (V), and that they exactly correspond to those used in the planetary theory for determining the disturbed motion of a planet. This result, which is perfectly general, having been obtained, the author proceeds in

* According to Morren's statement (*Ann. de Chim. et de Phys.* ser. 4. vol. iv. p. 365), uranium is present in all German glass, and the yellow fluorescent light is produced by this uranium. The great intensity of this light, which I have seen occur under very varied circumstances, rendered me suspicious of this statement, and I therefore requested Dr. Stahlschmidt to analyze the glass of the above tubes. This he has kindly done, and has found that the glass in question, which is Thuringian, does not contain the least trace of uranium; but it does, in addition to some iron, contain a not inconsiderable quantity of copper, although it is quite colourless.

the second part to solve these equations in the particular case of the motion of the earth under the action of the sun and moon, the approximation being carried so far as is needed for the proof of the formulæ commonly given for Precession and Nutation. The treatise is purely mathematical, and, from the nature of the subject, abstruse. It is clearly and elegantly written, and well worth the attention of any one interested in the question which it discusses.

Sun-views of the Earth, or the Seasons Illustrated. By RICHARD A. PROCTOR, B.A., F.R.A.S. London: Longmans, Green, and Co. 1867.

A sun-view of the earth means a perspective projection of the earth, the sun's centre being the point of sight and the plane of the picture at right angles to the line joining the sun's centre with that of the earth; of course such a projection is practically orthographic. Mr. Proctor's book consists mainly of twelve plates, one for each month, on days so chosen as to show the earth when in the solstitial and equinoctial points. Each plate contains the four views corresponding to 6 A.M., noon, 6 P.M., and midnight, Greenwich solar time. Each view is in fact a small map of a hemisphere, $2\frac{3}{4}$ inches in diameter. The whole series shows extremely well the general distribution of land and water on the earth's surface far better than can be done by any single map. This, however, is not the main object of the book, which is to enable a learner to understand the way in which the various regions of the earth are presented to the sun at different times of the year. The plates are extremely good, and show what is intended as well, perhaps, as anything of the kind could do. In addition to these twelve plates there are two—one showing five "sun-views" of the British Isles for different times of the year, the other showing the daily motion of the earth in her orbit for the year 1867, and the positions of the centre and major and minor axes of the orbit. There is prefixed to the work an introduction of four pages, explaining the plates and suggesting points illustrated by them.

In addition to the above work, Mr. Proctor has also published four separate plates. The first is a chart of all stars of the zodiac of magnitudes superior to the 6th; the second and third show the orbits of the larger and smaller planets, their major axes, lines of nodes, &c.; the fourth gives a series of eighteen views of Mars as seen at different dates, together with a chart of Mars based on twenty-seven drawings by Mr. Dawes. It is by no means clear whether the eighteen views are copies of actual drawings; we could have wished that Mr. Proctor had left the point clear of doubt; for if, on the one hand, they are copies of actual drawings, they completely justify the chart; if not, we are left entirely without the means of estimating the value of the chart.

LXXI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 396.]

November 21, 1867.—Lieut.-General Sabine, President, in the Chair.

THE following communication was read:—

“Contributions to the History of Methylic Aldehyde.” By A. W. Hofmann, LL.D., F.R.S.

“The aldehyde of the methyl-series is not known;” all the chemical manuals say so, and for the last twenty years my students have been duly informed thereof. It will scarcely appear strange that more efforts to become acquainted with that body should not have been made, since the masterly picture which Liebig has delineated of the aldehyde *par excellence* embraced as it were the history of the whole class, and of course also of the aldehyde in question. Nevertheless methylic aldehyde deserves our consideration for more than one reason. As one of the simplest terms of the monocarbon-series, occupying a position intermediate between marsh-gas and carbonic acid, as a link of transition connecting methylic alcohol and formic acid, as either aldehyde or acetone, according to the point of view from which we look upon it, the compound CH_2O illustrates a greater variety of relations than any one of the higher aldehydes. But in addition to the interest with which the methyl-compound has thus always been invested, this substance possesses special claims upon our attention at the present moment. Our actual method of treating organic chemistry for the purposes of instruction almost involves the necessity of starting from the methyl-series. The simplest of aldehydes thus acquires quite an especial importance; and all those who, like the author of this note, are engaged in teaching, cannot fail to have sadly missed a compound which is the carrier of such varied and interesting considerations.

The desire which I have frequently felt in my lectures of developing the idea of the genus aldehyde, when speaking of the methyl-compounds, has more than once induced me to attempt the preparation of methyl-aldehyde, but it was only at the conclusion of my last summer course that I succeeded, to a certain extent at all events, in attaining the object of my wishes.

A substance possessing the composition and the properties of methylic aldehyde is formed with surprising facility if a current of atmospheric air, charged with the vapour of methylic alcohol, be directed upon an incandescent platinum spiral.

The bottom of a strong three-necked bottle, of two litres capacity, is covered to the height of about five centimetres with moderately warm methylic alcohol. The first neck is provided with a tube descending to the very surface of the liquid; into the second is fixed a loosely-fitting cork, which carries the platinum spiral; the third one, lastly, communicates with the upper end of a condenser, the lower end of which is fastened into a two-necked receiver. This receiver

is in its turn connected with a series of washbottles, and the last of these communicates with a water-jet aspirator, by which a current of air can be sucked through the whole system.

The apparatus being disposed in this manner, the platinum spiral is heated to redness and introduced into the three-necked bottle. After a few minutes the flameless combustion of the methyl-alcohol begins to manifest itself by the evolution of a vapour powerfully affecting the nose and eyes. Gradually the temperature of the apparatus rises, and soon droplets of a colourless liquid are condensed in the receiver. The formation of methyl-aldehyde is now fairly proceeding, and if the current of air be appropriately adjusted, the platinum spiral remains incandescent for hours and even for days. There is no difficulty in collecting from 50 to 100 grammes of a liquid rather rich in methyl-aldehyde.

Instead of establishing the current of air by a water-jet aspirator, a pair of bellows may be conveniently employed. I have often used with advantage the bellows of an ordinary glass-blowing table. This mode of proceeding is more particularly adapted to the requirements of the lecturer, who is thus enabled, by simply accelerating the movement of the foot, to enliven the combustion, so as to keep the whole spiral in a state of incandescence. By thus proceeding it happens, however, occasionally that the gaseous mixture in the three-necked bottle is fired; but these explosions are perfectly harmless, the whole effect being the forcible ejection of the loosely-fitting cork which carries the platinum spiral.

The liquid which is being collected in the receiver has all the properties which theory assigns to the aldehyde of the methyl-series, or, more properly speaking, to its methyl-alcoholic solution. When rendered slightly alkaline by a few drops of ammonia, and mixed with nitrate of silver, it yields, on gently warming, a silver mirror of irreproachable perfection, which is indeed more readily and more certainly produced than with the aldehyde of the ethyl-series. The reduction in this case is the result of two consecutive reactions; in the first stage the aldehyde yields formic acid, which in the second stage is converted into water and carbonic acid.

On heating the methyl-alcoholic solution of the aldehyde with a few drops of a fixed alkali, the liquid becomes turbid on ebullition, acquires a yellowish coloration, and soon deposits droplets of a brownish oil, possessing in the highest degree the peculiar odour of ethyl-aldehyde-resin.

After the observation which I have mentioned, it was scarcely doubtful that the product of the slow combustion of methylic alcohol contained the aldehyde of this alcohol in considerable proportion. Nevertheless it appeared necessary to fix the nature of this compound by some numbers. The commencement of the vacations being at hand, there was but little hope of preparing the liquid in sufficient quantity for the purpose of obtaining the aldehyde, which will probably be found to be either gaseous at the common temperature or extremely volatile, in a state of purity for analysis. Under these circumstances I have been compelled to limit myself to the prepara-

tion of an easily accessible derivative of methyl-aldehyde possessing a characteristic composition, and the analysis of which would not be less conclusive than that of the aldehyde itself. The slight solubility and the powerfully crystalline tendencies of the sulphaldehyde of the ethyl-series could not fail to indicate the direction in which I had a right to hope that the object which I was aiming at might be accomplished.

If a current of sulphuretted hydrogen be passed through the methyl-alcoholic solution of methyl-aldehyde, the liquid becomes turbid after a few minutes, and on allowing the saturated solution to stand for some hours, a body of an alliacious odour begins to be separated at the bottom of the flask. If the liquid be now mixed with half its volume of concentrated hydrochloric acid, and heated to ebullition, it becomes limpid, and solidifies on cooling into a mass of felted needles of dazzling whiteness. These needles fuse at 218° ; they are volatile without decomposition. Slightly soluble in water, they are more readily dissolved by alcohol, and still more so by ether. For the purpose of analysis they were recrystallized from boiling water, in order to exclude free sulphur, with which they might have possibly been contaminated. The numbers obtained in the analysis of the crystals unmistakably establish their nature. The white crystals, as might have been expected, have the composition of the sulphaldehyde of the methyl-series,



The analysis of the sulphur-compound fixes, of course, the presence of the corresponding oxygen compound among the products of the slow combustion of methylic alcohol.

A more minute examination of methylic aldehyde and its derivatives remains still to be made. It will be absolutely necessary to isolate the oxygen-term and to determine its vapour-density, in order to ascertain its molecular weight. If we remember the facility with which the aldehydes are polymerized, the question presents itself, whether the aldehyde formed by the slow combustion of methylic alcohol is represented by the formula



or a multiple thereof. A similar remark applies to the sulphur-derivative. It deserves, moreover, to be mentioned that a compound isomeric with methylic aldehyde, the dioxymethylene ($\text{C}_2\text{H}_4\text{O}_2$) of M. Boutlerow, is known already; also that a sulphur-compound of the formula



has been obtained by M. Aimé Girard, who observed that bisulphide of carbon is reduced by the action of nascent hydrogen with disengagement of sulphuretted hydrogen.

In the course of next winter I propose to perform some further experiments on the product of the slow combustion of methylic alcohol for the purpose, if possible, of isolating methylic aldehyde in a state of purity, and of thus completing this inquiry.

December 5.—Dr. William Allen Miller, Treasurer and Vice-President, in the Chair.

The following communication was read :—

“Results of Observations of Atmospheric Electricity at Kew Observatory, and at Windsor, Nova Scotia.” By Joseph D. Everett, D.C.L., F.R.S.E., Assistant to the Professor of Mathematics in the University of Glasgow.

The paper commences with an account of the concluding observations taken by the author at Windsor, N.S., of which the previous portion has already been published in the ‘Proceedings,’ vols. xii. & xiv.

It then goes on to describe the self-recording apparatus employed at Kew Observatory for the observation of atmospheric electricity, and the method of procedure employed in measuring and reducing the curves thus obtained, this portion of the work having been performed in the Physical Laboratory of the University of Glasgow.

Tables are given showing the mean hourly values of the electrical potential for each month, and the mean monthly values are hence derived. These values for Kew are compared with the corresponding values for Windsor, N.S., and remarkable differences are shown to exist between the curves, both diurnal and annual, for the two places.

The hourly means at Kew for the mean of the year are represented by the following numbers :—

23 ^h	0 ^h	1 ^h	2 ^h	3 ^h	4 ^h	5 ^h	6 ^h	7 ^h
1·91	1·96	1·92	1·93	1·95	2·08	2·29	2·58	2·86
8 ^h	9 ^h	10 ^h	11 ^h	12 ^h	13 ^h	14 ^h	15 ^h	16 ^h
2·96	2·93	2·74	2·42	2·12	1·86	1·68	1·58	1·54
17 ^h	18 ^h	19 ^h	20 ^h	21 ^h	22 ^h			
1·52	1·64	1·96	2·26	2·28	2·13.			

These numbers indicate a principal maximum between 8^h and 9^h, and a secondary maximum between 20^h and 21^h. At Windsor, on the other hand, the mean potential about 9^h was in every month, without exception, less than at the other principal times of observation, viz. about 21^h and 14^h.

The following Table shows the ratio of the mean monthly to the mean annual potential for the whole series of observations at both places :—

		Kew.		
June	1862	·770	June 1863	·681
July	„	·773	July „	·643
Aug.	„	·836	Aug. „	·685
Sept.	„	·845	Sept. „	·854
Oct.	„	·981	Oct. „	1·000
Nov.	„	1·600	Nov. „	1·390
Dec.	„	1·188	Dec. „	1·460
Jan.	1863	1·033	Jan. 1864	1·226
Feb.	„	1·333	Feb. „	1·263
March	„	1·160	March „	1·375
April	„	·920	April „	·831
May	„	·672	May „	·549

Windsor, N.S.

Oct. 1862	·832	Oct. 1863	1·033
Nov. „	·766	Nov. „	·949
Dec. „	1·010	Dec. „	1·110
Jan. 1863	1·057	Jan. 1864	1·125
Feb. „	1·432	Feb. „	?
March „	1·396	March „	1·416
April „	1·023	April „	1·026
May „	·796	May „	·985
June „	·720	June „	·799
July „	·755	July „	·885
Aug. „	·952	Aug. „	(·862)
Sept. „	·985			

The last step in the reductions consisted in expressing the variations, both diurnal and annual, at Kew, and the annual variations at Windsor, by the first two terms of an harmonic series.

In the case of the diurnal variations at Kew, the amplitudes of the two terms were nearly equal, but the epoch was much more uniform in its values (whether in comparing one year with the other or in comparing one month with another in the same year) for the second term than for the first.

In the case of the annual variations, the amplitude of the second term at Kew was almost inappreciable, while at Windsor it was greater than that of the first term.

December 12.—Lieut.-General Sabine, President, in the Chair.

The following communication was read:—

“On a supposed Connexion between the Amount of Rainfall and the Changes of the Moon,” being an extract of a Letter from J. H. N. Hennessey, Esq., First Assistant on the Great Trigonometrical Survey of India, to General Sabine, R.A., Pres. R.S.

Allow me now to say a few words in connexion with the enclosed paper. There appears to prevail a belief, more or less popular, to the effect that more rain falls at “the changes of the moon” than on the intermediate days of a lunation. As I happened to possess a record of the rainfall at the office of the Superintendent of the Great Trigonometrical Survey of Mussoorie, extending over thirteen consecutive years, I obtained Colonel Walker’s permission to make use of the register, in connexion with this popular belief.

The results tabulated have been obtained by employing an *average daily* fall as the means for comparing the fall at “the changes” with that at intermediate intervals. The method of calculation adopted is explained in the footnote to the Table. The annual average result may be stated thus:—

At “the changes” of the moon the *mean daily* fall of rain is 0·466 inch.
 Between “the changes” of the moon the mean daily fall is .. 0·525
 which is in opposition to the popular belief on the subject. I en-

close the Table, on the chance of its proving sufficiently interesting to be noticed.

Average daily fall of rain between successive quarters and at each quarter of the moon from 1st of May to 31st of October of each year, measured at the Office of the Superintendent of the Great Trigonometrical Survey of India. The office stands in Mussoorie, on the most southern range of the Himalaya Mountains, lat. N. 30° 28', long. E. of Greenwich 78° 7'; height above mean sea-level 6500 feet.

Year.	Average Daily Fall.								Total Fall from May 1 to October 31.
) to ☉	☉	☉ to ☽	☽	☽ to ☊	☊	☊ to ☉	☉	
	inch.	inch.	inch.	inch.	inch.	inch.	inch.	inch.	inches.
1854	·644	·374	·813	·176	·630	·096	·512	·621	100·72
1855	·456	·204	·360	·918	·311	·356	·753	·733	85·85
1856	·732	·745	·703	·237	·397	·588	·347	·340	93·28
1857	·280	·319	·794	1·013	·521	·136	·368	·606	88·27
1858	·402	·448	·485	·298	·518	·157	·705	·373	84·61
1859	·665	·263	·253	·642	·306	·253	·570	·583	78·31
1860	·356	·228	·430	·719	·564	·205	·301	·073	65·81
1861	·685	·678	1·014	·372	1·332	·287	·577	·855	141·16
1862	·611	·620	·513	·651	·364	·852	·645	·530	93·91
1863	·348	·342	·862	·932	·511	·595	·291	·546	93·03
1864	·762	·409	·545	·292	·394	·328	·237	·352	82·19
1865	·543	·235	·276	·120	·443	·526	·518	·785	76·37
1866	·135	·360	·402	·580	·636	·809	·452	·483	81·15
Means of } columns }	·509	·402	·573	·535	·533	·399	·483	·529	89·589

General mean of ☉ ☽ ☊ ☉ 0·466 inch.

General mean of ☽ to ☉, ☉ to ☽, ☽ to ☊, ☊ to ☉ 0·525 „

Note.—The rainfall during the *preceding* twenty-four hours was measured daily at mean noon. Suppose $m_1, m_2, m_3, m_4, m_5, m_6, m_7, m_8, m_9$ to denote nine such consecutive measurements of daily rainfall, registered at Mussoorie mean noon, respectively on the 1st, 2nd . . . 9th of the month, and that the moon entered her first quarter at an hour nearer to noon of the 1st than to the preceding or succeeding noons. In this case the arithmetical mean of m_1 and m_2 has been entered in column ☽ as the average *daily* fall at the first quarter. Similarly, if full moon occurred nearest to noon of the 8th, the quantity $\frac{m_8 + m_9}{2}$ has been reckoned as the average *daily* fall at

full moon; and $\frac{m_3 + m_4 + m_5 + m_6 + m_7}{5}$ represents the average *daily* fall from ☽ to ☊. The foregoing Table has been prepared under these conditions by Baboo Dwarkanath Dutt, Computer to the Great Trigonometrical Survey of India.

GEOLOGICAL SOCIETY.

[Continued from p. 481.]

December 4, 1867.—Robert Etheridge, Esq., F.G.S.,
in the Chair.

The following communications were read:—

1. "On the Graptolites of the Skiddaw Series." By Henry Alleyne Nicholson, D.Sc., M.B., F.G.S. &c.

The author first described the geological relations and distribution of the Skiddaw Slates, and noticed their correspondence with the Quebec Group of Canada, and then gave a description of the Graptolites found in these rocks. The genera and their distinguishing characters are the following:—

1. *Dichograpsus*, Salter (3 species): possesses a frond repeatedly dichotomous from a basal stipe into 8, 16, or more branches, each with a single row of cells, the lower part of the stipe being enveloped in a corneous cup.

2. *Tetragrapsus*, Slater (3 species): possesses a frond composed of four simple stipes, arising from a non-celluliferous funicle, which bifurcates at both ends.

3. *Phyllograpsus*, Hall (2 species): differs from the last in possessing a frond composed of four simple stipes united back to back by their solid axes.

4. *Didymograpsus*, M'Coy (7 species): the frond consists of two simple stipes springing from a mucronate radicle, which may be rudimentary or apparently absent.

5. *Diplograpsus*, M'Coy (4 species): two simple stipes, united by their solid axes into a celluliferous frond furnished with a radicle at the base.

6. *Graptolites* vel *Graptolithus*, Linn. (4 species): simple stipe, with a single row of cells on one side, and a small, generally curved, radicle at the base.

7. *Pleurograpsus*, Nicholson (1 species): celluliferous branches derived from a main celluliferous rhachis.

2. "On the Fossil Corals (*Madreporaria*) of the West-Indian Islands.—Part IV. Conclusion." By P. Martin Duncan, M.B., Sec. G.S.

In this communication the author concluded his series of memoirs on the Fossil Corals of the West Indies with a description of the Miocene corals from St. Croix, Trinidad, and with some supplementary remarks on the species described in his former papers from St. Domingo, Jamaica, and Antigua, including notices of new species from those islands. He also gave a complete and revised list of all the fossil corals he had described from the West Indies, including 5 species from Cretaceous strata, 4 species and 1 variety from Eocene deposits, and 102 species and 26 varieties from the Miocene formation, making a total of 111 species and 27 varieties. Of the Miocene species 11 still exist, namely, 6 in the Caribbean

Sea only, 3 common to that sea and the Pacific Ocean, and 2 in the Pacific Ocean and Red Sea, but not in the Caribbean. Twelve other species are common to European deposits and the West-Indian Miocene, 10 being of the same age in both hemispheres, while 2 occur in the Lower Chalk in Europe. These 23 species being deducted from those of the West-Indian Miocene, a large characteristic fauna still remains; and Dr. Duncan showed that the recent representatives of the characteristic genera composing it are for the most part inhabitants of the Pacific and Indian Oceans, the Red Sea, and the Australian waters, and that their Tertiary congeners are found in Europe, Australia, Java, and Sinde. Of the 14 genera thus enumerated, 8 are not represented in the recent coral-fauna of the Caribbean Sea.

Jamaica has yielded the only known Cretaceous and Eocene corals; and Dr. Duncan stated that the former are identical with European Lower-Chalk species, and that the latter are similar to species from the London Clay, the Bracklesham Beds, and the Paris Basin.

Dr. Duncan then mentioned several curious facts in the distribution of the West-Indian corals, both fossil and recent, and especially the circumstance that whilst Jamaica, San Domingo, and Guadeloupe present solitary species mixed with those indicating shallow water and a reef, Antigua and Trinidad offer for consideration only reef-species. In conclusion the author drew attention to the confirmation by subsequent discoveries of his theory of an Atlantic archipelago, which he had put forward in his earlier papers.

December 18, 1867.—Warrington W. Smyth, M.A., F.R.S.,
President, in the Chair.

The following communications were read:—

1. "On the Parallel Roads of Glen Roy." By Sir J. Lubbock, Bart., F.R.S., Pres. Ent. Soc.

The author did not enter into the question as to the manner in which the valleys were filled with water, but, assuming that the "roads" or "shelves" represent ancient water-margins, he attempted to point out the manner in which they were produced.

The theory of Macculloch, which has been adopted by Darwin, Lyell, and Jamieson, is, that the matter brought down by frost, rain, &c. from above was arrested by the water, and heaped up by the action of the waves. If this were the true explanation, however, Sir John argued that the roads would form an excrescence on the slope of the hill, which they do not, that their breadth must vary considerably, that the slope of the roads would be towards the hill, and that the roads would be widest where the inclination of the hill is less than usual, and where streams bring down matter from above, whereas, on the contrary, in these places the roads disappear.

In opposition to this theory, Sir John then argued that the action of the waves under such circumstances would be to throw matter down, and not up. Given a slope of angular débris standing at the angle of repose, partly in air and partly in water, the angle will be

about the same throughout, because the angle at which matter will stand depends partly on gravity and partly on friction. Now, as long as the water is at rest, the equilibrium in water remains as in air; but as soon as the water is agitated, the friction is diminished, and the angle of repose becomes less. In other words, the pebbles are set in motion, and roll down the hill,

This explains the equal width of the roads, because the new angle of repose being equal throughout, and the depth to which the agitation extends being also equal, the width of the road must be equal also; and when once the new slope of repose was acquired, the hillside would again be in a condition of equilibrium, and the road would receive no further enlargement however long the water might stand at the same level. This explains why there are no roads when the natural rock appears, or when the hillside is less steep than usual, whereas if the roads were due to a heaping-up action, of course in places where the sides were more shelving the roads would be better marked. We can also thus understand why there are no rolled pebbles on the roads; and lastly, as the lower line of the roads marks the depth to which the water was disturbed, we can see why the roads become narrower wherever they are steeper than usual.

§ Finally, the vertical height of the roads (that is to say, the vertical difference between their upper and lower lines) gives the measure of the depth to which the water filling the valleys was agitated, and affords thus an additional argument in favour of its having been that of a lake, as in a tidal sea the width of the roads must have been much greater than it is.

2. "Remarks on the Geological Features of the Northern part of Formosa and the adjacent islands." By Cuthbert Collingwood, M.B., F.L.S.

The west coast of Formosa is flat, consisting of low alluvial plains, with a few hills, some of which approach the coast; a range of mountains runs nearly through the island. Near Tamsuy, on the right bank of the river, is a thick deposit of clay, containing boulders on which the author could detect no traces of glacial striæ. Higher up the river, on the north side, hills containing sulphur-springs rise from the plain. On the north-east side of the island, sandstone extends from Masou peninsula, north of Kelung, to Petou Point on the south-east. The harbour of Kelung is a spacious excavation in the sandstone, which is hollowed out into numerous caves; and Dr. Collingwood states that the land is slowly rising, blocks of water-worn coral being found above high-water mark. Sano Bay, the only harbour on the east coast, is protected by a reef composed of trap-rock. The Pescadores are of volcanic origin, and are composed of basalt. The author then describes Hai-tan, and the islands of Craig and Agincourt, which lie to the north of Formosa, as well as the Pinnacle Islands, lying still further north.

3. "On some Sources of Coal in the Eastern Hemisphere." By Cuthbert Collingwood, M.B., F.L.S.

1. *Kelung, Formosa*.—The coal is found in depressions in red

sandstone, and is of comparatively recent origin. It is light, burns very rapidly, gives out great heat, produces 50 per cent. of ash, and forms considerable quantities of clinker.

2. *Labuan, Borneo*.—Several seams of coal crop out conspicuously near the coast, the lowest being 11 feet 4 inches in thickness. It is heavy, close-grained, fast-burning, and gives out considerable heat; it is of very recent date,—dammara resin and leaves of recent trees being found associated with it.

3. *Diu, Saghalien*.—Coal excellent, burns quickly, with little ash. Presents a fracture similar to Welsh coal.

4. *Japan*.—The author describes coal from several localities in Japan as bright, clean, and resembling Sydney coal, but having a tendency to form clinker. He concludes with a description of some coal from Ivanai, Nippon, which is very clean, highly bituminous, burns with a flame in the flame of a candle, and would probably be valuable as a gas-producing material.

LXXII. Intelligence and Miscellaneous Articles.

ELECTROSCOPIC NOTICES. BY PROFESSOR POGGENDORFF.

PYROXYLINE, or gun-paper, besides its great inflammability, is well known to have the property of becoming strongly charged with negative electricity by friction with most organic substances. A strip of this paper drawn between the fingers is therefore in many cases a very useful electroscope. With negatively electrified bodies this electroscope can be used under all circumstances; for it is repelled by them; but it can only be used with positively electrified bodies if their electricity is so strong that it can act at considerable distances; for when near, the paper is attracted even by indifferent substances, owing to inductive action.

This objection led the author to consider whether it was not possible to find a substance which would furnish an equally trustworthy electroscope for positive electricity. At first it was imagined that the beard of the Chamois (*Gemsbart*), recommended by M. von Kobell, might prove available; and it may indeed be used, but, apart from the circumstance that this substance is not at the disposal of every physicist, it is both too fragile and too rigid. In the course of his investigations the author found that there is more than one substance with the desired properties and within every one's reach. He was led to examine the deportment of metals when rubbed with certain insulators, and he obtained certain points of view which invested this well-worn subject with fresh interest. The insulators investigated were the following:—Ebonite, gutta percha, caoutchouc, patent caoutchouc (*Patentgummi*), waxed cloth, white wax, resin, shellac, sealing-wax, sulphur, amber, copal, silk, gun-paper, colloidion, and gun-cotton. And the metals and conductors rubbed were graphite, carbon (gas-carbon), platinum, gold, palladium, silver,

mercury, mercury-amalgam, tin, bismuth, antimony, copper, zinc, cadmium, iron, lead, aluminium, and magnesium.

Although, from causes hitherto unexplained, individual exceptions occur, the author thinks himself entitled to lay down the general rule that the so-called electronegative metals, platinum, gold, palladium, silver, &c., render the above insulators *positive* by friction; while the electropositive metals, zinc, cadmium, iron, &c., put them in the negative condition. An almost never-failing example is ebonite; rubbed, or rather gently stroked, with platinum it becomes positive, while with zinc or iron it is negative.

The action of *amalgam* is especially remarkable. So far as the author's observations extend, it makes all insulators, without exception, positive. Even gun-paper and collodion, two of the most negative substances known, present no exception; and though it is sometimes difficult to make gun-cotton positive, it is soon found that this arises more from the fibrous structure than from the nature of the substance. At all events it can be made positive.

At first the author used Kienmayer's amalgam rubbed in upon greased leather. It is in this case difficult to avoid points of amalgam being rubbed off and placed on the insulator. Yet this rubbing off, to which the action might be ascribed, principally occurs at first; the more the amalgam is dried in upon the leather, the less it rubs off; in eight to fourteen days it entirely ceases to do so, provided the insulator has a smooth surface and is gently rubbed. This rubbing off is quite avoided if the coating of mirrors be used instead of solid amalgam; the insulator is then to be gently rubbed between the coated faces of two pieces of mirror.

A certain indicator of positive electricity can thus be easily constructed from pyroxyline paper. Among other methods, this is attained by gently pressing a strip of paper for a time between two plates of ebonite which have been rubbed with fur. The strip then becomes positive by induction. Instead of the paper a thin strip of ribbon may be advantageously used.

Ebonite, which rubbed with fur becomes so strongly negative, when rubbed with amalgam attains a degree of positive electrification which is little or not at all inferior to that of glass. Hence electrical machines with disks of ebonite may be used. The author constructed such a machine, and at first the action was equal to that of glass; but it rapidly diminished, which he is inclined to ascribe to the circumstance that the amalgam, owing to the sulphur in the ebonite, appears to undergo a gradual decomposition. It was also found that flaps of oiled silk could not be used; for they made the ebonite negative, and thus weakened and even inverted the action of the rubber, while they increase it in the case of glass. Hence, to avoid this evil, flaps of gun-paper would have to be used, which, indeed, serve very well in the case of glass disks.—*Berliner Monatsbericht*, March 1867.

ON THE RELATIVE MAGNITUDE OF MOLECULES.

BY DR. ALEXANDER NAUMANN.

If η denotes the coefficient of friction, m the molecular weight, and u the molecular velocity of a gas, r the semidiameter of the molecule assumed to be spherical, according to O. E. Meyer*,

$$\eta = \frac{mu}{r^2\pi}.$$

Now the same physicist† has deduced the coefficients of friction of several gases from the values of their coefficients of transpiration found experimentally by Graham. But as molecular weights have but relative values, the previous equation can also only be used for deducing relative values of the molecular sections $r^2\pi$; in what follows, these are given for those gases whose coefficients of friction are known.

Let η and η_1 be the coefficients of friction of different gases, m and m_1 the molecular weights, u and u_1 the molecular velocities, r and r_1 the molecular radii; we have from the above equation,

$$\frac{\eta}{\eta_1} = \frac{\frac{mu}{r^2\pi}}{\frac{m_1u_1}{r_1^2\pi}} = \frac{mur_1^2}{m_1u_1r^2},$$

from which is obtained for the relative magnitudes of the molecular sections,

$$\frac{r^2}{r_1^2} = \frac{mu\eta_1}{m_1u_1\eta} \dots \dots \dots (1)$$

Now if τ and τ_1 are the absolute temperatures of the gases, we have, since the *vires vivæ* of molecular motions are proportional to the absolute temperatures,

$$\frac{mu^2}{m_1u_1^2} = \frac{\tau}{\tau_1} \text{ and } \frac{u}{u_1} = \frac{\sqrt{m_1\tau}}{\sqrt{m\tau_1}}.$$

Introducing this value into equation (1) gives

$$\frac{r^2}{r_1^2} = \frac{m\eta_1\sqrt{m_1\tau}}{m_1\eta\sqrt{m\tau_1}} = \frac{\eta_1\sqrt{m\tau}}{\eta\sqrt{m_1\tau_1}} \dots \dots \dots (2)$$

If the gases are compared at the same temperatures (the melting-point of ice for instance) $\tau = \tau_1$, equation (2) passes into

$$\frac{r^2}{r_1^2} = \frac{\eta_1\sqrt{m}}{\eta\sqrt{m_1}} \dots \dots \dots (3)$$

* Poggendorff's *Annalen*, vol. cxxv. p. 597 (1865).

† *Ibid.* vol. cxxvii. p. 378 (1866).

If in the case of hydrogen, for which the molecular radius is smallest, we take $r_1=1$, remembering that $m_1=2$, and $\eta_1=0\cdot000134$, equation (3) assumes the form

$$r^2=0\cdot0000948 \frac{\sqrt{m}}{\eta} \dots \dots \dots (4)$$

$$r=\sqrt{0\cdot0000948 \frac{\sqrt{m}}{\eta}} \dots \dots \dots (5)$$

$$r^3=0\cdot0000948 \frac{\sqrt{m}}{\eta} \sqrt{0\cdot0000948 \frac{\sqrt{m}}{\eta}} \dots (6)$$

The values adduced in the last three columns of the following Table have been calculated by these three equations.

Names of the gases.	Composi- tion.	No. of atoms.	Mole- cular weight.	Coefficient of friction.	Ratios of the		
					Mole- cular sections.	Mole- cular radii.	Mole- cular vo- lumes*.
Hydrogen	H ²	2	2	0·000134	1	1	1
Oxygen	O ²	2	32	306	1·75	1·32	2·32
Nitrogen	N ²	2	28	267	1·88	1·37	2·57
Chlorine	Cl ²	2	71	210	3·80	1·95	7·41
Carbonic oxide	CO	2	28	266	1·88	1·37	2·59
Nitric oxide	NO	2	30	269	1·93	1·39	2·68
Hydrochloric acid	HCl	2	36·5	225	2·54	1·59	4·06
Carbonic acid	CO ²	3	44	231	2·72	1·65	4·49
Nitrous oxide.....	N ² O	3	44	231	2·72	1·65	4·49
Sulphuretted hydrogen	H ² S	3	34	188	2·94	1·71	5·04
Sulphurous acid.....	SO ²	3	64	200	3·79	1·95	7·38
Ammonia	NH ³	4	17	156	2·50	1·58	3·96
Cyanogen	C ² N ²	4	52	155	4·41	2·10	9·26
Marsh-gas	CH ⁴	5	16	174	2·18	1·48	3·22
Chloride of methyle ...	CH ³ Cl	5	50·5	167	4·03	2·01	8·10
Ethy ene.....	C ² H ⁴	6	28	158	3·17	1·78	5·65
Chloride of ethyle	C ² H ⁵ Cl	8	64·5	153	4·97	2·23	11·09
Methylic ether	C ² H ⁶ O	9	46	148	4·34	2·08	9·05

It follows from equation (4), that bodies which with equal molecular weights have also equal coefficients of friction, like nitrogen and carbonic oxide, nitrous oxide and carbonic acid, have also equal relative numbers for their molecular magnitudes. Ethylene has, it is true, the same molecular weight as nitrogen and carbonic oxide, but a different coefficient of friction, and therefore a different relative molecular magnitude. With all their other differences, the molecular magnitudes of sulphurous acid and chlorine agree very closely, as

* By molecular volume is to be understood the space occupied by a molecule, into which other masses cannot penetrate without destroying the previous condition of the molecule.

M. Matteucci has recently called to mind an experiment which he made in 1838, and on which he considers himself justified in assuming that the polarization arises from the gas adhering to the electrodes. I think that the polarized metals should be regarded as fugitive combinations of the metals and the gases; and I assume that in polarization-elements, as well as in Grove's gas-battery, the electromotive force is the affinity exercised on one of the elements of the water by the gas united in some special way to a metal; there is, however, a distinction to be drawn between the combination produced under the influence of a current and that formed simply from the affinities of the metals put in juxtaposition.

As I have already pointed out, a platinum plate immersed in a saturated solution of oxygen develops no electromotive force, while a platinum plate polarized by oxygen may develop in contact with pure or acidulated water a force equal to 193; platinum, therefore, under the influence of a current can form with gases combinations different from that which they form simply in virtue of their affinities. Let me add that platinum is the only body with which gas-couples have hitherto been constructed, while all the metals may be polarized by hydrogen when they are used as cathodes in the decomposition of water.

The distinction I have drawn appears to me useful in explaining how two contrary polarizations may apparently superpose themselves on one and the same electrode. Imagine acidulated water to be decomposed by using two platinum electrodes A and B: if the current be passed for ten minutes in one direction and then be turned for an instant in the other, and then by means of a convenient commutator the polarized electrode be rapidly connected with a galvanometer, at first a transitory deflection in one direction is obtained, followed by a permanent one in the other. This fact, which may be readily confirmed, may, I think, be explained in the following manner:—When a current of tolerable duration passes through a liquid from A to B, the electrode B is polarized by the hydrogen, the liquid which bathes the plate A becomes charged with oxygen, the liquid surrounding B is charged with hydrogen. When the direction of the current is inverted for a moment, the existing polarizations are destroyed and inverse polarizations produced, but the condition of the liquid layers surrounding the electrodes is not appreciably altered. It thus happens that at the moment communication is made with the galvanometer, the electrode A, polarized by hydrogen, is immersed in a solution of oxygen, and that, on the contrary, the electrode B, polarized by oxygen, is immersed in a solution of hydrogen. The current which is observed at first is due to polarization properly so called; but as this polarization is very feeble, it is annulled in a few seconds; and when the plate B is deprived of the oxygen it had absorbed under the influence of the current, it combines, owing to its affinity solely, with a portion of the hydrogen contained in the surrounding liquid, and thus forms a *gas-couple*.—*Comptes Rendus*, September 9, 1867.

INDEX TO VOL. XXXIV.

- ACETYLENE**, on a new method of preparing, 508.
- Ahrens (M.)** on the derivatives of xylole and methyltoluole, 507.
- Air**, on a photometer for measuring the transparence of the, 241.
- Albumen**, on the diffusion of, in water, 32.
- Andrews (Prof. J.)** on ozone in the atmosphere, 315.
- Arcs**, circular, on the approximate rectification of, 284, 381.
- Astro-meteorology**, on certain recent contributions to, 34.
- Atkinson (Dr. E.)**, chemical notices by, 26, 220, 506.
- Atmosphere**, on the influence of the August and November meteors on the temperature of the, 39; on the existence of ozone in the, 315; on the theory of the circulation of the, 359.
- Atomic motion**, on the velocity of, 373.
- Baille (J.-B.)** on the variation in the dispersion of liquids under the influence of heat, 79.
- Bayer (M.)** on oxide of mesityle and phorone, 511.
- Bennington (C. H.)** on a new photometer, 475.
- Bessemer-flame**, on the spectrum of the, 302, 437.
- Beyer (M.)** on the metamorphosis of fruits in ripening, 226.
- Bigot (M.)** on the hydrocarbons, 506.
- Blood**, on a new method of analysis of, 226.
- Bohn (Prof. C.)** on negative fluorescence and phosphorescence, 108.
- Books**, new:—Norton's Treatise on Astronomy, 477; Cheyne's "The Earth's Motion of Rotation," 538; Proctor's Sun-views of the Earth, 539.
- Brewster (Sir D.)** on the radiant spectrum, 202; on the neutral point of, 325.
- Brodie's (Sir B.)** system of chemical notation, remarks on, 50, 129.
- Brown (Dr. A. C.)** on Sir B. Brodie's system of chemical notation, 129.
- Bullet**, on the figure of the, which experiences least resistance from the air, 377.
- Bunsen (Prof. R.)** on a method of determining the specific gravity of vapours and gases, 1; on the temperature of the flames of carbonic oxide and hydrogen, 489.
- Cailletet (M.)** on the influence of various coloured rays on the decomposition of carbonic acid in plants, 485.
- Cane-sugar**, on the diffusion of, in water, 32.
- Carbonic acid**, on the influence of various coloured rays on the decomposition of, in plants, 485.
- Carbonic oxide**, on the temperature of the flame of, 489.
- Carré (M.)** on an apparatus for the production of cold, 29.
- Chase (P. E.)** on the laws which govern the general distribution of heat over the earth, 244; on Brewster's neutral point, 325.
- Chautard (J.)** on the magnetism and diamagnetism of gases, 168.
- Chemical notices** from foreign journals, 26, 220, 506.
- processes, on thermal action in, 227.
- Chlorovalerianic acid**, 511.

- Chmoulevitch (M.) on the influence of heat on the mechanical force of frogs' muscle, 403.
- Chromium, on crystallized oxide of, 223.
- Clairault's theorem, on Prof. Stokes's proof of, 25.
- Claudet (A.) on the optics of photography, 231.
- Climate, on the influence of a change in the obliquity of the ecliptic on, 127, 328.
- Cloud, on the moon's influence on, 61, 143, 218, 544.
- Clouds, on the colour of, 275, 356.
- Cockle (Chief Justice) on the conversion of integrals, 442.
- Coffee, on an apparatus for the detection of adulterations in, 104.
- Cold, on an apparatus for the production of, 29.
- Collingwood (Dr. C.) on a horizontal rainbow observed at sea, 440.
- Colouring-matters, on the analysis of, 144, 228.
- Comets and meteors, on the connexion between, 188.
- Croll (J.) on the change in the obliquity of the ecliptic and its influence on climate, 127; on the theory of gravitation and the constitution of matter, 449.
- Crystalline form and chemical constitution, on a connexion between, 178.
- Curves, on a property of certain, 65.
- Dana (Prof. J. D.) on a connexion between crystalline form and chemical constitution, 178; on the system of mineralogical nomenclature, 407.
- Davey (R. R. F.) on Sir B. Brodie's "Ideal Chemistry," 50.
- De la Rive (A.) on the electrical condition of the terrestrial globe, 322; on a photometer for measuring the transparency of the air, 241; on M. Faraday, his life and works, 409.
- De Luca (M.) on the action of sulphurous acid on sulphuretted hydrogen, 221.
- Desains (P.) on the absorbing action of volatile liquids and their vapours on the heat from a lamp with a glass chimney, 72; on the absorption of obscure heat, 327.
- Douglas (J.) on the source of muscular force, 273.
- Draper (Prof. J. C.) on an apparatus for the detection of adulterations in coffee, 104.
- Ecliptic, on the change in the obliquity of the, and its influence on climate, 127, 328.
- Electric charges, on an apparatus for multiplying and maintaining, 391.
- Electrical condition of the terrestrial globe, on the, 322.
- currents, on the identity of, with the vibrations of light, 287; on the mechanical theory of, 382.
- phenomenon of induction, on a new, 533.
- Electricity, experimental researches in, 81; on a new and powerful generator of dynamic, 96; results of observations of atmospheric, 543.
- Electrodes, on the polarization of the, 553.
- Electrodynamics, contributions to, 368.
- Electroscopic notices, 549.
- Ellis (W.) on the tendency to dispersion of cloud under a full moon, 61, 218.
- Ethylbenzoic acid, on the preparation and constitution of, 507.
- Evening-glow, on the theory of the, 275, 356.
- Everett (Dr. J. D.) on results of observations of atmospheric electricity at Kew and at Windsor, Nova Scotia, 543.
- Faraday (M.), life and works of, 409.
- Favre (M.) on thermal action in chemical processes, 227.
- Fittig (M.) on the hydrocarbons, 506; on mesitylene, 510.
- Fluorescence, on negative, 108.
- Forbes's (D.) researches in British mineralogy, 329.
- Fremy (M.) on the preparation of crystals, 224.
- Friedel (M.) on zincethyl, 508.
- Fruits, on the metamorphosis of, during maturation, 226.
- Gaffield (T.) on the action of sunlight on glass, 514.
- Galvanic circuits, on a new method of determining the resistance of, 77.
- Gases, on a method of determining the specific gravity of, 1; on the

- magnetism and diamagnetism of, 168; on the specific heat of, for equal volumes under constant pressure, 205.
- Gaugain (J. M.) on the polarization of the electrodes, 553.
- Geological Society, proceedings of the, 67, 317, 396, 479, 516.
- Gerlach (Dr.) on the mechanical theory of the electrical current, 382.
- Glass, on the action of sunlight on, 514.
- Glass tears, observations on, 166.
- Gold, analyses of British, 338.
- Graham (J.) on the occlusion of hydrogen by meteoric iron, 239.
- Gravitation, on certain hypothetical elements in the theory of, 449.
- Gravity in relation to centripetal velocity, on the intensity of, 55.
- Grimaux (M.) on chloride of benzyle, 509.
- Gum, on the diffusion of, in water, 32.
- Hæmoglobine; researches on, 226.
- Harbord (Rev. J. B.) on the conic theory of heat, 106, 185.
- Harrison (J. P.) on the moon's influence over cloud, 143.
- Heat, experiments on the absorption of, 72, 327; on the variation in the dispersion of liquids under the influence of, 79; on the conic theory of, 106, 185; on the mechanical theory of, 205; on the laws which govern the general distribution of, over the earth, 244; on the influence of, on the mechanical force of frogs' muscle, 403.
- , radiant, on the interference-colours of, 487.
- Hennessey (J. H. N.) on a supposed connexion between the amount of rainfall and the changes of the moon, 544.
- Hofmann (Dr.) on methylic aldehyde, 540.
- Hoppe-Seyler (M.) on the diffusion of cane-sugar, albumen, and gum in water, 32; on the occurrence of indium in Wolfram, 33.
- Hosæus (M.) on the nutrition of plants, 225.
- Huggins (W.) on the spectrum of Mars, and on the colour of that planet, 74.
- Hydrocarbons, synthetical researches on the, 506.
- Hydrogen, on the occlusion of, by meteoric iron, 239; on the temperature of the flame of, 489.
- Indium, on the occurrence of, 33.
- Integrals, on the conversion of, 442.
- Iodine, on chloride of, 220.
- Isoperimetric regular polygons, on, 365.
- Isophthalic acid, 508.
- Keely (G. W.) on the resultant of two pressures on a fixed point, 354.
- Kekulé (Prof.) on the conversion of the aromatic hydrocarbons into their corresponding phenoles, 512.
- Kindt (Dr.) on phosphorescent light, 484.
- Knoblauch (Prof. H.) on the interference-colours of radiant heat, 487.
- König (M.) on ethylbenzoic acid, 507.
- Kühne (M.) on hæmoglobine, 226.
- Ladenburg (M.) on zincethyle, 508.
- Laughton (J. K.) on the theory of the circulation of the atmosphere, 359; on the natural forces that produce the permanent and periodic winds, 443.
- Lauth (M.) on chloride of benzyle, 509.
- Lechartier (M.) on the preparation of a group of minerals isomorphous with the apatite and Wagnerite group, 224.
- Lielegg (Prof.) on the spectrum of the Bessemer-flame, 302.
- Light, on the identity of the vibrations of, with electrical currents, 287; on phosphorescent, 484.
- Liquid films, on the tension of, 192.
- Liquids, on the variation in the dispersion of, under the influence of heat, 79.
- Lommel (Dr. E.) on the theory of the evening-glow and analogous phenomena, 275.
- Lorenz (L.) on the identity of the vibrations of light with electrical currents, 287.
- Lossen (M.) on the products of oxidation of naphthaline, 513.
- Magnetism, experimental researches in, 81.
- Mars, on the spectrum of, 74.
- Masia (M.) on a method of blood-analysis, 227.

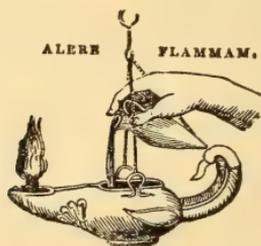
- Matter, on the internal distribution of, 235; on the constitution of, 449.
- Mattheides (M.) on the derivatives of xylole and methyltoluole, 507.
- Melsens (M.) on the passage of projectiles through resisting media, 481.
- Mesitylene, researches on, 510.
- Meteoric iron, on the occlusion of hydrogen gas by, 239.
- Meteors and comets, on the connexion between, 188.
- Methylalloy, on the preparation of, 509.
- Methylic aldehyde, contributions to the history of, 540.
- Mimetisite, on the artificial production of, 224.
- Mineralogical nomenclature, on the system of, 407.
- Mineralogy, researches in British, 329.
- Minerals, on the artificial production of certain, 224.
- Mitscherlich (M.) on a new method of determining the composition of organic compounds, 228.
- Molecules, on the relative magnitude of, 551.
- Moon, on rainfall in relation to changes of, 544.
- Moore (Carrick) on the change of obliquity as a cause of change of climate, 328.
- Morries (M.) on mercury-naphthyle, 513.
- Motion, on a new electrical phenomenon of, 533.
- Muscular force, on the source of, 273.
- Naphthaline, on the products of oxidation of, 513; on a mercury compound of, 513.
- Naumann (Dr. A.) on the specific heat of gases for equal volumes under constant pressure, 205; on the velocity of atomic motion, 373; on the relative magnitude of molecules, 551.
- Newton (H. A.) on certain recent contributions to astro-meteorology, 34.
- Northcote (A. B.) on the water of the Severn at Worcester, 249.
- Oil of bitter almonds, on a new method of preparing, 510.
- Organic compounds, on a new method of determining the composition of, 228.
- Orthogonal matrices, thoughts on inverse, 461.
- Otto (M.) on crystallized oxide of chromium, 223; on mercury-naphthyle, 513.
- Ozone, on the density of, 26; on the existence of, in the atmosphere, 315.
- Pettenkofer (Prof.) on the elimination of carbonic acid, and the absorption of oxygen in the waking state and during sleep, 30.
- Phenole, researches on, 512.
- Phosphorescence, observations on, 108.
- Phosphorus, on the influence of various liquids on the division of, 223.
- Phosphuretted hydrogen, on solid, 220.
- Photography, on the optics of, 231.
- Photometer, on a, for measuring the transparence of the air, 241; on a new, 475.
- Plants, on the nutrition of, 225; on the influence of various coloured rays on the decomposition of carbonic acid in, 485.
- Plateau (Dr. F.) on the spontaneous change of a liquid cylinder into isolated spheres, 246.
- Poggendorff (Prof.) on a new electrical phenomenon of motion, 533; on substances suited for electroscopes, 549.
- Polytelite, description and analysis of the new British mineral, 350.
- Pratt (Archdeacon) on Prof. Stokes's proof of Clairaut's theorem, 25.
- Preyer (M.) on the analysis of colouring-matters, 228.
- Projectiles, on the passage of, through resisting media, 481.
- Radiant-points, observations on, 34.
- spectrum, on the, 202.
- Rainbow, on a horizontal, 440.
- Rankine (W. J. M.) on a property of certain curves, 65; on the approximate drawing of circular arcs of given lengths, 284, 381; on isoperimetric regular polygons, 365.
- Resistance-measurer, on a, 270.

- Reusch (E.) on glass tears, 166.
- Richter (M.) on the preparation of indium, 33.
- Riemann (B.) on electrodynamics, 368.
- Rieth (M.) on a new method of preparing acetylene, 508.
- Roscoe (Prof. H. E.) on the chemical intensity of total daylight at Kew and Pará, 313.
- Rosenthal (J.) on the force which the muscle of a frog can develop in contracting, 325.
- Royal Society, proceedings of the, 65, 144, 231, 304, 391, 540.
- Rudorff (M.) on solid phosphuretted hydrogen, 220.
- Schiff (M.) on the influence of various liquids on the division of phosphorus, 223.
- Schlebusch (M.) on the action of hypochlorous acid on some of the fatty acids, 511.
- Schwendler (L.) on testing telegraph-cables during the process of sheathing, 169.
- Shooting-stars, on the paths and probable origin of, 40; on the age of the November group of, 49.
- Siemens (C. W.) on a resistance-measurer, 270.
- Sky, on the colour of the, 275, 356.
- Solar eclipse, August 1868, on the, 502.
- radiation, on the intensity of, 404.
- Solids, on the so-called "inactive" condition of, 136, 229.
- Sorby (H. C.) on the analysis of animal and vegetable colouring-matters by the spectrum-microscope, 144; on the colour of the clouds and sky, 356.
- Soret (M.) on the density of ozone, 26; on the intensity of solar radiation, 404.
- Spectrum, on the radiant, 302; of the Bessemer-flame, on the, 302, 437.
- microscope, on the analysis of animal and vegetable colouring-matters by the, 144.
- Stars, on the physical constitution of the, 304.
- Stokes (Prof.) on the internal distribution of matter which shall produce a given potential at the surface of a gravitating mass, 235.
- Stoney (G. J.) on the connexion between comets and meteors, 188; on the physical constitution of the sun and stars, 304; on the solar eclipse, August 1868, 502.
- Sulphur, on some varieties of, 221.
- Sun, on the physical constitution of the, 304.
- Sylvester (J. J.) on inverse orthogonal matrices, tessellated pavements, ornamental tile-work, and the theory of numbers, 461.
- Tarleton (Prof. F. A.) on the figure of the bullet which experiences the least resistance from the air, 377.
- Telegraph-cables, on the testing of, during sheathing, 169.
- Thallium, on the preparation of, 222.
- Thomson (Prof. Sir W.) on vortex atoms, 15; on a self-acting apparatus for multiplying and maintaining electric charges, 391.
- Titanoferrite, analysis of British specimens of, 344.
- Tomlinson (C.) on the so-called "inactive" condition of solids, 136, 229.
- Ubal dini (M.) on the action of sulphurous acid on sulphuretted hydrogen, 221.
- Vapours, on a method of determining the specific gravity of, 1.
- Van der Mensbrugge (G.) on the tension of liquid films, 192.
- Voit (M.) on the elimination of carbonic acid and the absorption of oxygen in the waking state and during sleep, 30.
- Voltaic theory, illustrations of the, 391.
- Vortex atoms, on, 15.
- Waltenhofer (Prof. v.) on a new method of determining the resistance of galvanic circuits, 77.
- Wanklyn (Prof.) on Sir B. Brodie's "Ideal Chemistry," 50.
- Water of the Severn at Worcester, on the, 249.
- Waterston (J. J.) on gravity in relation to centripetal velocity, 55.
- Watts (Dr. W. M.) on the spectrum of the Bessemer-flame, 437.
- Weber (M.) on the action of chloride of iodine on bisulphide of carbon, 220.

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- Winds, on the natural forces that produce the permanent and periodic, 443.
- Wöhler (Prof.) on the preparation of thallium, 212.
- Wurtz (M.) on the preparation of methylallyle, 509; on the conversion of the aromatic hydrocarbons into their corresponding phenoles, 512.
- Zincethyle, researches on, 508.

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II. On Vortex Atoms. By Professor Sir WILLIAM THOMSON, F.R.S.	15
III. On Professor Stokes's Proof of Clairaut's Theorem. By Archdeacon PRATT, F.R.S.	25
IV. Chemical Notices from Foreign Journals. By E. ATKINSON, Ph.D., F.C.S.	26
V. On certain recent Contributions to Astro-Meteorology. By H. A. NEWTON	34
VI. Observations on Sir Benjamin Brodie's "Ideal Chemistry." By J. A. WANKLYN and R. R. F. DAVEY	50
VII. On the Change that would be superinduced upon an Elliptic Orbit if the intensity of the force of Gravity were influenced by the centripetal velocity of the Orbital Body. By J. J. WATERSTON, Esq.	55
VIII. Inquiry as to whether the tendency to Dispersion of Cloud under a Full Moon in any way depends on Lunar Influence. By WILLIAM ELLIS, F.R.A.S.	61
IX. Proceedings of Learned Societies:—	
ROYAL SOCIETY:—Dr. W. J. MACQUORN RANKINE on a Property of Curves which fulfil the condition $\frac{d^2\phi}{dx^2} + \frac{d^2\phi}{dy^2} = 0$	65
GEOLOGICAL SOCIETY:—THE EARL OF SELKIRK on ancient Sea-marks on the Coast of Sweden; THE DUKE OF ARGYLL on a Post-Tertiary Lignite in Argyllshire; Mr. W. S. SHEA on recent discoveries of Gold in New Brunswick; Mr. W. WHEELWRIGHT on the Discovery of Coal in the Andes; The Rev. P. B. BRODIE on the Presence of Purbeck Beds at Brill, Buckinghamshire; Mr. H. W. BRISTOW on the Lower Lias of Glamorganshire; Mr. C. MOORE on Abnormal Conditions of Secondary Deposits when connected with the Somersetshire and South Wales Coal-basins; The Rev. P. B. BRODIE on the Drift in a part of Warwickshire; Mr. W. B. DAWKINS on the Dentition of <i>Rhinoceros leptorhinus</i> (Owen); Mr. J. W. JUDD on the Strata of the Lincolnshire Wolds	
	67-71
X. Intelligence and Miscellaneous Articles:—	
Researches on the Absorbing-Action which certain Volatile Liquids and their Vapours exert on the Heat from a Lamp with a Glass Chimney, by M. P. Desains	72
On the Spectrum of Mars, with some Remarks on the Colour of that Planet, by William Huggins, F.R.S.	74
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Researches on the Variation in the dispersion of Liquids under the influence of Heat, by M. J.-B. Baille	79

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XI. Experimental Researches in Magnetism and Electricity. By H. WILDE, Esq. (With a Plate.)	page 81
XII. On an Apparatus for the Detection of Adulterations in Coffee. By JOHN C. DRAPER, M.D., Professor of Natural History, College of the City of New York, and of Analytical Chemistry in the University of New York	104
XIII. The Conic Theory of Heat, in connexion chiefly with the Metamorphosis of Matter. By the Rev. J. B. HARBORD, M.A.	106
XIV. On Negative Fluorescence and Phosphorescence. By Professor C. BOHN.	108
XV. Remarks on the Change in the Obliquity of the Ecliptic, and its Influence on Climate. By JAMES CROLL	127
XVI. Remarks on Sir Benjamin Brodie's System of Chemical Notation. By ALEXANDER CRUM BROWN, M.D., D.Sc.	129
XVII. On the so-called "Inactive" Condition of Solids. By CHARLES TOMLINSON, F.R.S.	136
XVIII. On the Moon's Influence over Cloud. By J. PARK HARRISON, M.A.	143
XIX. Proceedings of Learned Societies:—	
ROYAL SOCIETY:—Mr. H. C. SORBY on a Definite Method of Qualitative Analysis of Animal and Vegetable Colouring-matters by means of the Spectrum Microscope	144
XX. Intelligence and Miscellaneous Articles:—	
Some Observations on Glass Tears, by E. Reusch	166
Experiments on the Magnetism and Diamagnetism of Gases, by M. J. Chautard	168

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XXI. On testing Telegraph Cables during the process of Sheathing. By LOUIS SCHWENDLER	page 169
XXII. On a Connexion between Crystalline Form and Chemical Constitution, with some inferences therefrom. By JAMES D. DANA.	178
XXIII. The Conic Theory of Heat considered in connexion with General Sensation and the three senses of Touch, Taste, and Smell. By the Rev. J. B. HARBORD, M.A.	185
XXIV. On the Connexion between Comets and Meteors. By G. JOHNSTONE STONEY, M.A., F.R.S.....	188
XXV. On the Tension of Liquid Films. By M. G. VAN DER MENSBRUGGHE.....	192
XXVI. On the Radiant Spectrum. By Sir DAVID BREWSTER, K.H., F.R.S.	202
XXVII. On the Specific Heat of Gases for Equal Volumes under Constant Pressure. By Dr. ALEXANDER NAUMANN.....	205
XXVIII. Some further Remarks on the Influence of the Full Moon on Cloud. By WILLIAM ELLIS, F.R.A.S.	218
XXIX. Chemical Notices from Foreign Journals. By E. ATKIN- SON, Ph.D., F.C.S.	220
XXX. On the so-called "Inactive" Condition of Solids. By CHARLES TOMLINSON, F.R.S.....	229
XXXI. Proceedings of Learned Societies:—	
ROYAL SOCIETY:—Mr. A. CLAUDET on a Self-acting Focus- Equalizer; Prof. STOKES on the Internal Distribution of Mat- ter which shall produce a given Potential at the Surface of a Gravitating Mass; Mr. T. GRAHAM on the Occlusion of Hy- drogen Gas by Meteoric Iron.....	231-241
XXXII. Intelligence and Miscellaneous Articles:—	
On a Photometer for Measuring the Transparence of the Air, by M. A. de la Rive	241
On the Laws which govern the general distribution of Heat over the Earth, by Pliny Earle Chase	244
On the spontaneous Change of a Liquid Cylinder into Isolated Spheres, by Dr. Félix Plateau.....	246

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XXXIV. On a Resistance-measurer. By C. W. SIEMENS, F.R.S. 270

XXXV. On the Source of Muscular Force. By JOHN DOUGLAS, East-India Government Telegraph Department 273

XXXVI. Theory of the Evening Glow and analogous Phenomena. By Dr. E. LOMMEL 275

XXXVII. On the Approximate Drawing of Circular Arcs of given Lengths. By W. J. MACQUORN RANKINE, C.E., LL.D., F.R.S. 284

XXXVIII. On the Identity of the Vibrations of Light with Electrical Currents. By L. LORENZ 287

XXXIX. On the Spectrum of the Bessemer-flame. By Professor LIELEGG 302

XL. Proceedings of Learned Societies :—

ROYAL SOCIETY:—Mr. G. J. STONEY on the Physical Constitution of the Sun and Stars ; Prof. H. E. ROSCOE on the Chemical Intensity of Total Daylight at Kew and Pará in 1865–67 ; Dr. T. ANDREWS on the Identity of the Body in the Atmosphere which decomposes Iodide of Potassium with Ozone 304–316

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Note on the Electrical Condition of the Terrestrial Globe, by A. De la Rive 322

On Brewster's Neutral Point, by Pliny Earle Chase 325

Note on the Force which the Muscle of a Frog can develop in Contracting, by M. J. Rosenthal 325

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XLIII. A new Attempt to determine the Resultant of two Pressures on a Fixed Point. By G. W. KEELY, Esq.	354
XLIV. On the Colour of the Clouds and Sky. By H. C. SORBY, F.R.S.	356
XLV. An Inquiry into the Evidence on which the Theory of the Circulation of the Atmosphere is based. By J. KNOX LAUGHTON, B.A., R.N.	359
XLVI. On Isoperimetric Regular Polygons. By W. J. MACQUORN RANKINE, C.E., LL.D., F.R.S.S.L. & E. &c.	365
XLVII. A Contribution to Electrodynamics. By BERNHARD RIEMANN	368
XLVIII. On the Velocity of Atomic Motion. By Dr. ALEXANDER NAUMANN.	373
XLIX. On the Figure of the Bullet which experiences the least resistance from the Air. By FRANCIS A. TARLETON, Fellow and Tutor, Trinity College, Dublin	377
L. On the Approximate Rectification of Circular Arcs. By W. J. MACQUORN RANKINE, C.E., LL.D., F.R.S.S.L. & E. &c.	381
LI. Contribution to the Mechanical Theory of the Electrical Current. By Dr. GERLACH	382
LII. Proceedings of Learned Societies :—	
ROYAL SOCIETY :—Sir W. THOMSON on a Self-acting Apparatus for multiplying and maintaining Electric Charges, with Applications to illustrate the Voltaic Theory	391–396
GEOLOGICAL SOCIETY :—Mr. H. B. MEDLICOTT on the Alps and the Himalayas; Mr. D. MACKINTOSH on the Terminal Curvature of Slaty Laminæ in West Somerset; Messrs. P. M. DUNCAN and J. THOMSON on <i>Cyclocyathus</i> ; Dr. DAWSON on a new Pulmonate Mollusk; Mr. J. W. SALTER on some tracks of <i>Pteraspis</i> (?) in the Upper Ludlow Sandstone, and on a new <i>Lingulella</i> from the red Lower Cambrian Rocks of St. David's; Mr. G. BUSK on the Dentition of Fossil Bears; Dr. J. HAAST on the Geology of the Province of Canterbury, New Zealand; the Rev. J. H. TIMINS on the Chemical Geology of the Malvern Hills; Mr. T. M. HALL on the Relative Distribution of Fossils throughout the North Devon series; Mr. W. R. SWAN on the Geology of the Princess Islands; Mr. C. COLLINGWOOD on the Sulphur Springs of Northern Formosa; Mr. G. B. STACEY on the Geology of Benghazi; Mr. E. THORNTON on the Existence of large Coal-fields in the Province of St. Catherine's, Brazil; Mr. G. MAW on the White Clays of the Lower Tertiaries; Mr. S. V. WOOD, Jun., on the Postglacial Structure of the South-east of England	396–403
LIII. Intelligence and Miscellaneous Articles :—	
Investigations on the Influence of Heat on the Mechanical Force of Frogs' Muscle, by M. Chmoulevitch	403
On the Intensity of the Solar Radiation, by M. J. L. Soret.	404
On System in Mineralogical Nomenclature, by J. D. Dana.	407

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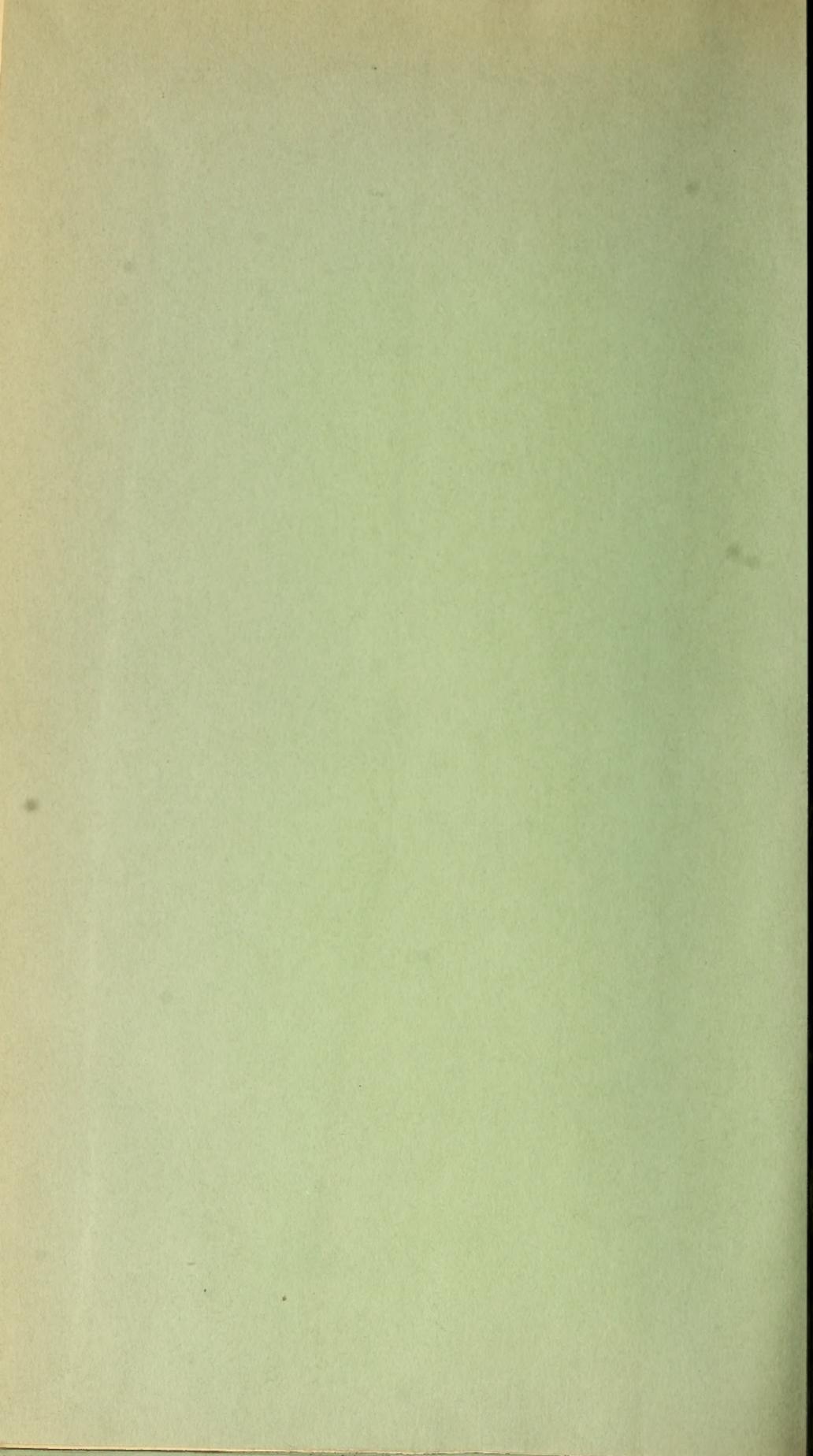
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